

# A Transatlantic History of the Social Sciences

Robber Barons, the  
Third Reich and the  
Invention of Empirical  
Social Research

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Christian Fleck

Translated from the German by Hella Beister

B L O O M S B U R Y   A C A D E M I C

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# INTRODUCTION

From the middle of the nineteenth century, ocean liners crossed the North Atlantic on a regular basis. In the early days, this meant a two-week voyage, but by 1900 the fastest liners could make the journey in five days, and by the time ocean liners had become outmoded as a means of transatlantic transport, this had been reduced to just over three days. The first transatlantic flight was made in May 1919; the 'flying boat', however, still had to make three stops on its way from New York to Plymouth. From 1930, Zeppelin-type aircrafts operated regular transatlantic passenger flights. The first telegrams were transmitted in 1858; by 1919, more than a dozen submarine transatlantic telegraph cables were operating. By 1927, technical improvements allowed for transatlantic telephone calls. In the same year, the first shortwave radio programme started operations in the Netherlands, contributing to cultural exchange in its own way. In 1933, Germany upgraded its two-hours-per-day shortwave radio programme to a twelve-language around-the-clock service. Special efforts were made by the Nazis to broadcast their radio propaganda to the United States of America (whereas the legendary 'Volksempfänger' – German for 'people's receiver' – was not equipped for shortwave radio reception, making transatlantic radio-listening a one-sided affair). After the Second World War PanAm, having realized its first commercial air-boat flight in May 1939 – a twenty-nine-hour endeavour – began to offer a regular transatlantic service by turboprop aircraft. In the late 1950s, jet planes started to operate, inaugurating the era of mass (air) tourism which now carries thousands of passengers in both directions every day.

The acceleration, if any, of the transatlantic exchange of ideas cannot be measured as precisely as the speed of travel of emigrants, tourists, letters, telegrams and electronically transmitted communications or entertainment products. There is no doubt, however, as to the existence of such an exchange of ideas. The present book is concerned with the transatlantic transfer of money, people and institutions, and its impact on the development of empirical social research. In this process, mutual enrichment between Europe and the United States was often simply a byproduct of activities that served other purposes. Thus, American philanthropic foundations' allocation of research funds to European beneficiaries was instrumental to the implementation of research that was 'made in the USA'.

The twentieth century also saw an increase in researchers' regional mobility. Initially, this was mainly a matter of voluntary, temporary displacements of individuals who had been granted some kind of scholarship. However, the

number of those who were displaced against their will soon dramatically increased. The emigration of intellectuals and scholars as well as of people who, for the time being, were neither of these, but would become so after their escape, contributed significantly to transatlantic enrichments.

In the relevant literature, however, this bi-directional interplay is mostly described as one-way traffic. As often as not, authors will present the trajectories of those they choose as the heroes of their tales in narratives of high drama. Thus, an individual arriving at his destination as a missionary of some arcane teachings may appear to have single-handedly built a utopian communitarian settlement, such as New Harmony. Narratives of how someone was prevented from pursuing his or her path in the New World are no less frequent. In this case, latter-day admirers deplore the injustice incurred by their hero. However, these dramatic narratives about heroes do little to help us understand how institutions come into being and how they change. On the other hand, failing to account for individuals would be no less of a reduction.

In the twentieth century, the social sciences underwent a kind of crystallization process in which the familiar sub-disciplines of today became differentiated. This crystallization occurred thanks to the predominance of one particular methodological orientation, modelled after that of physics. According to this school of thought, there would someday be a science of social facts that, due to its use of exact methods, would be able to present verifiable results which, in turn, would yield a theory of society through accumulation of verified knowledge. Those who participated in this endeavour did away with teleological and holistic ideas, and expounded their meta-theoretical concepts with such verve and persuasion that even the sceptics were carried away. Even its confirmed opponents had to grapple with this powerful project or had to pay tribute to it with criticism. This orientation of social research to a hypostasized model of physics resulted in the predominance of an empiricist approach and in the vilification of everything else as 'armchair research'.

By the late 1950s, the canon of social science disciplines had been determined, and the methodology that regulated them had been programmatically laid down, becoming the binding norm. The third culture (Lepenies 1985) had acquired pride of place.<sup>1</sup> Even if one does not share the belief that the scientific construal of the social sciences is the only road to salvation, one cannot deny that in the middle of the twentieth century it was the dominant model, or paradigm, to take up a term that came into fashion in the 1960s. From the United States, where it had initially become predominant, this conception of the social sciences spread to other parts of the world and, ultimately, gained worldwide acceptance. Philanthropic foundations set up by immensely rich magnates, the so-called 'robber barons' of the late nineteenth and early twentieth century, functioned as the catalysts, not limiting themselves to activities in their own country, but promoting research in other countries as well, especially in those European nations that used to be regarded as scientific leaders. Thus, these philanthropic organizations contributed to the global predominance of the new empirical brand of the social sciences.

This tide of scientific exports began a few years before the Nazis took over, and continued until the outbreak of the Second World War, with Germany eventually no longer among its beneficiaries. In Europe, the Rockefeller Foundation as well as its predecessors and counterparts sponsored what they called ‘realistic’ research – a label they invented for what we today call empirical research. They invited promising young researchers to come to the United States and familiarize themselves with what was going on there. All these activities gradually evolved into a transatlantic exchange, and but for Nazi interference, enrichment might have been *mutual*. Before the onset of Nazi policies of exclusion and persecution of those who were deemed undesirable on racial or political grounds (most of whom took refuge abroad) individuals, funds and ideas had migrated back and forth between Europe and the United States. As the sphere of Nazi rule stabilized and expanded, this migration increasingly became a one-way flow, i.e. an escape to the United States.

The last phase was the evacuation of those who had become stuck in Marseilles, where a young German helped to distribute ‘affidavits’ to his fellow sufferers and, having finished this job, escaped to Norway where he boarded a steamer to New York thanks to the last pre-Second World War fellowship awarded to a European by the Rockefeller Foundation. In Marseilles, he had called himself Beamish; in Oslo, he boarded the ship as one Albert Otto Hirschmann; and once in the United States, he dropped the last letter from this surname. In the decades to come, he richly contributed to more than one field of the social sciences. If he had been arrested and turned over to the Nazis by Vichy agents, he would in all likelihood have gone to his death in Auschwitz or, even before that, been beaten to death in a Gestapo cellar.

Having been socialized in the Second Austrian Republic, I have learned to be wary of any claims to ownership by our German neighbours of what is actually Austrian. Alpine national sentiment is roused when one of ‘Us’ is claimed by the Germans. Yet, since Austrian nation-building is a comparatively recent endeavour, Austrians are fervently devoted to the task of securing their national heritage and do not at all like to be told that in doing so, they in turn tend to be selective: ‘To be sure, Hitler is German and Beethoven is Austrian.’ For all such irony, however, it cannot be denied that differences between the two German-speaking countries exist, especially from the perspective of the history of science. Even my rather weak (or so I feel) version of patriotism was strongly tested each time that, in talking to emigrated German-speaking social scientists or on reading their texts, I noticed that they, too, said ‘Germans’ when they meant ‘Austrians’. Maybe it was the thrall of some residual nationalism that spurred me to dig further into the differences between the Germans and the Austrians. It is up to the reader to decide whether this project is justified.

However, a clarification on my part seems called for. With regard to the structure and organization of the academic world, and especially of universities, the commonalities between both nations are plain to see. University structures, models for academic careers and intellectual styles of discussion are so much alike that to see them as a unit seems quite appropriate. In order

to avoid having to specify this commonality again and again in so many words, I will rely on a typology that was proposed by Johan Galtung (Galtung 1981) to distinguish between four intellectual styles: 'Saxonic' is his term for the British and American style, 'Nipponic' for that of Japan, 'Gallic' for the ways of France and 'Teutonic' for the academic mores in German-speaking countries. Whenever the latter are referred to, I will use Galtung's term. On the contrary, where commonalities are evoked that do not pertain to the *academic* sphere but are rooted in the family likeness between both national *cultures*, I will use the terms 'German-speaking' or 'German-language'. When speaking of 'Austria', one might as well say 'Vienna' in many cases, since the hegemony of that former metropolis of the Habsburg Empire was indeed such as to have the 'province' pale beside it. That said, since this is a structural trait of Austrian society, especially as compared to the German situation, the name of the country is preferred.

With that, the subject of the following study is outlined. The present book is about the evolution of four phenomena, with special emphasis on their mutual interdependencies: (1) the emergence of empirical social research; (2) the role, in this, of the funding provided by American foundations; (3) a collective biography of those German-speaking social scientists who were active in the period between the 1920s and the 1950s; with (4) special emphasis on the differences between those who emigrated and those who stayed in their home country, and between Germans and Austrians.

A 'collective biography' is a highly prestigious but rarely used procedure in the social sciences and humanities. In the late sixteenth century, the term 'prosopography' was used for the first time to refer to the comprehensive description of an individual's physiognomy in view of elucidating his or her character. Later, the term was primarily understood to refer to descriptions of groups of individuals, and since Theodor Mommsen, prosopography has been part and parcel of the research techniques of scholars of ancient history, where it refers to the whole set of individuals mentioned in the corpus of antique inscriptions. Due to its reference to groups, the term 'prosopography' is also used to highlight the characteristics shared by a group of individuals. An early example of group pictures showing people who share the same trade was done, at an interval of three decades, by the Flemish painter Dirck Jacobsz: the two paintings of the members of the Amsterdam Shooting Corporation (*Group Portrait of the Amsterdam Shooting Corporation*, 1532 and 1561, respectively, both at the Hermitage in St Petersburg). The historical propinquity of the appearance of both the term 'prosopography' and these pictures of a (vocational) group suggests an emerging interest, at the time, in the representation of trades and professions. The seventeen men portrayed by Jacobsz, as well as the seven men of the later picture, show little individuality. They strongly resemble each other in their clothing, posture and features, while as a group they convey the specific impression of homogeneity based on their occupational background that is so peculiar to both pictures. In painting, the portraits of homogeneous members of a vocational group soon gave way to the more individualized portraits of individual representatives of this group. In Rembrandt's

*Night Watch*, painted almost a century after Jacobsz, the officer posing in the foreground stands out clearly against the other members of the company.

The fact that collective biographies – as prosopographies, to avoid confusion with what is done in ancient history, may also be called – are rarely found has to do with the numerous difficulties that have to be mastered. Since the data one would like to use for such a portrait are very often not available, authors of collective biographies have thus far tended to focus on constellations that are well documented. This, in turn, is primarily true for individuals who were prominent at some point during their lifetimes. Robert K. Merton, in his doctoral thesis, proposed one of the very first sociological collective biographies, analysing the members of the early Royal Society (Merton 1938). Some time before, Edgar Zilsel had analysed Giorgio Vasari's biographies of artists in a similar attempt to demonstrate the amalgamation of the craftsman and the artist resulting in the novel type of the scientist which, as he saw it, had emerged in the Italian Renaissance (Zilsel 1926). Later, collective biographies of members of parliament, high officials and other members of clearly defined groups were published. The members of the respective occupational groups are characterized by the fact that their careers have a common peak and are traced back, so to speak, to their roots.

Unfortunately, this approach means ignoring all those who failed to reach such a peak in their careers. Among the about 800 'sociologists' whom I analysed, there are quite a few who failed to rise to the higher academic ranks. Many of them had no *oeuvre* to leave behind; some disappeared from the scene after a few years, others engaged in other careers or were prevented from pursuing the careers they had planned and hoped for. The advantages of this approach are immediately evident, but so are the problems involved: those who were left behind, were disappointed, or were ignored do not leave many traces.

Inclusion of the 'home-guard', as they were so aptly called by Everett Hughes (Hughes 1959, 572), suggested itself from the start since, up to now, the literature has made a point of analysing the two groups separately. The home-guards who had stayed put and the refugees who left are two elements of the same generation and thus were confronted with the same major historical event but chose different ways of coping with it. Relying on this generation terminology, proposed by Karl Mannheim, solves a problem which the studies that confine themselves to one of these two groups eliminate by drawing a definitional dividing line. Those who, by definition, are not part of the target group are, as a consequence, not treated in the study.

My account is primarily based on archival data. This historical material has a number of advantages as compared to the sources sociologists usually draw on for their conclusions: namely, what can be found in the archives was written at that time and for that particular archive, and the authors were not biased by the questions pursued by sociologists today. Archival material differs from the material most commonly relied upon in sociology (i.e. computerized data sets and written records of observations or interviews) in that it is less clean. While this can be taken quite literally, it is primarily true in a figurative sense. The data one



looks for, and needs, are hidden in a mountain of odds and ends. Still, the most likely reason for sociologists to keep their distance from archives and archival material is that, in an (otherwise rather rare) reluctance to indulge in disciplinary expansionism, they regard this field as the domain of historians.

An unwelcome side-effect of sociologists' dread of archives is that sociological studies tend to be short-winded and to address long-term trends by either relying exclusively on official statistics or making do with interpretations obtained from historical works. Thus, the sociological community is split into the large majority of those who strictly limit themselves to the present, which can be explored by primary surveys, or to such periods as are covered by official data, i.e. mass statistics, and into the small minority of historical sociologists who bury themselves in libraries and transpose what they read into their own universe of sociological concepts. Middle-range evolutionary processes, notably those of institutions on the one hand, and of generations on the other hand, are located exactly between these two poles of sociological work.

The actors that are the object of this account do not belong to the great majority of the population, which typically does not leave much behind. In the course of their career, researchers tend to fill a lot of paper, some of which is available in print, while the bulk is never published because it consists of preparatory or accessory work or has failed to convince the 'gate-keepers' of the market of academic publications. Legacies of researchers are of primary importance for the historiography of scientific disciplines and are therefore treated with some care if the deceased enjoyed some celebrity. All those who were left to merely cherish hopes for fame are lucky to have been spared the revelation of how quickly their fame faded. Indeed, the usual repository for the legacies of third-rate to last-rate researchers is the container of some disposal service charged with waste-paper collection.

The merciless attitude currently shown by the world of science towards those of its members whose contributions have failed to be well received finds its match in the not very noble-minded stance taken by universities and other institutions when it comes to the preservation of the memories of their former collaborators. For many of the sociologists (male and female) who are mentioned on the pages that follow, nothing remains today, only a single generation after their demise, besides what they managed to publish during their lifetimes. For many, there is neither a photograph nor a letter, or rather, to be more precise: all this may well be somewhere, but not in those places where one might reasonably look for it.

During my search for forgotten legacies or other documents left behind by sociologists, my impression was that the social sciences are a lot more callous in their dealings with the historical material of their discipline than their neighbouring sciences or other branches of intellectual life, which tend to set great store by anything written. Sociologists' collective lack of interest with respect to the preservation of the data of their own work and of the papers of their colleagues may well be a consequence of their fixation on the present.

After his demise, Paul F. Lazarsfeld's office had to be cleared. Some of his papers were handed over to the Rare Book and Manuscript Division of the Butler Library of the Columbia University, where they still are today. The rest, which someone must have deemed unworthy of preservation, were dumped in the middle of Lazarsfeld's office, with everyone free to help him or herself. His large collection of papers, with personal inscriptions from the authors, was thus lost, as well as the volumes of all the journals he had taken so much care to have rebound for his library.

In 1945, the 'Committee for the Study of Recent Immigration from Europe' research group conducted a survey by questionnaire; due to the technical options of the pre-computer era, only two-dimensional cross tabulations were published (Davie 1947). At the time, social researchers had, at best, access to Hollerith tabulating machines for processing their data, while most of the time they were reduced to doing their calculations by hand or with the help of a slide rule. Re-analyses of studies like these would of course have been helpful for my own work, but Davie's original data are lost.

Contrary to a widespread notion among sociologists that is based on Max Weber's definition of bureaucracy, state archives are, notwithstanding the norm of the written form, of very limited value, at least for the period that is of interest here. Rather, when getting in touch with the universe of Austrian archivists, one learns a new word: '*skartiert*' (which can be roughly translated as 'unavailable as missing or destroyed'), their term for records that are quite simply no longer there. These include, for example, the records of the offices of the first three post-Second World War education ministers. It is anyone's guess as to why this should be so.

University archives are more rewarding in this respect. In the Teutonic world, this primarily means information on students – i.e. courses followed, examinations taken, name of examiner, marks received – while the universe of academic personnel, assistants and professors is less well documented. Personnel records mainly consist of the official correspondence between an employee and others in his or her chain of command. American university archives, however, are different: they rarely provide information on students but quite frequently contain the legacies of former professors.

Compared to the Teutonic science administration, the Rockefeller Archive Center, open since 1974, is paradise. Internal communication by notes and letters among the various foundations of the Rockefeller family started very early, and officers were required to keep diaries to record their contacts with third persons. These records, almost none of which ever seems to become '*skartiert*', allow the researcher not only to get an overall idea in a very short time, but also to achieve a detailed reconstruction of how views changed, and why, and which decisions were reached. Just as the Rockefeller Foundation has contributed, through its funding activities, to the shaping of the emerging social sciences, the Rockefeller Archive Center has for years and years shaped the historiography of the sciences. The Teutonic disregard of the information that lies dormant in American archives has produced some strange effects.

As an illustration, a separate chapter will be dedicated to the American side of the legendary *Studies in Prejudice*, the history of which has to date been exclusively written on the basis of the material that is available at the Horkheimer-Archiv in Frankfurt.

For the *Studies in Prejudice* chapter (Chapter Six), as elsewhere in the present book, the presentation is done from an American perspective, and quite deliberately so, in order to counterbalance the ethnocentric view of the presumed advantages of Teutonic science that prevails in German-language historiography.

The account is historical but tries to draw, as far as possible, on systematic insights of sociology. For this, Andrew Abbott has coined the label of 'narrative positivism' (Abbott 2001a; Abbott 2001b), which I readily adopt because of its provocative concision, a step all the easier to take as there is no obligatory commitment to any sectarian school bound up with it. Whenever possible in the analysis of historical events, I tried to bring to bear insights from specialized subfields of sociology, from migration research to the sociology of organizations to the sociology of science. In this, some eclecticism was inevitable, but the general perspective that was adopted has been defined as follows by one of the ancestors of sociology: 'Men make their own history, but they do not make it as they please; they do not make it under self-selected circumstances, but under circumstances existing already, given and transmitted from the past' (Marx 1852). That opportunity structures constrain our action is one of the few insights of the social sciences to which even laymen will subscribe. While sociologists in recent times have tended to emphasize individual freedom of choice, analyses of social life in dictatorships or under conditions of forced migration, as well as of the process of assimilation to which immigrants are exposed, have for a long time been dominated by the opposite view. In the following, I will try to steer clear of these extreme positions by focusing on the interaction between these supporting or constraining structures and the individual freedom of action.

Chapter One gives an overview of the shift of the hub of the science system as a whole to the United States that occurred over the course of the twentieth century, a pattern that also holds true for the social sciences and that began to dynamically unfold at about the same time the power structure of Central Europe shifted towards dictatorship. The predominance of the American scientific system is further due to the fact that the United States was the first country in the world to undergo a considerable expansion of its system of tertiary education, which in turn called for change in the recruitment of junior scientists, and to see the emergence of new institutions dedicated to the advancement of the sciences.

In Chapters Two and Three, two variants of science sponsorship are analysed in some detail: post-doctorate grants and institutional support. Developments in Germany and Austria are analysed in a comparative perspective, as well as the way American foundations reacted to the handing-over of power to the Nazis.

Chapter Four is an attempt at a collective biography of German-speaking social scientists, based on an analysis of the data available for about 800 individuals.

The German-Austrian comparison is based on contemporary socio-demographic variables and is supplemented by an analysis of academic trajectories in these two countries as well as in the United States, the country that accommodated the largest number of academic emigrants. In addition, an attempt is made to factor in the reputation gained by these social scientists.

In the subsequent chapters, the presentation proceeds on a lower level of aggregation. Doing research in terms of projects is a truly American invention. Chapters Five and Six provide a detailed analysis of two projects where emigrated German-speaking social researchers played a leading role. The Princeton Radio Project marked Lazarsfeld's entry into the American world of science. While he finally succeeded, in some way or other, in overcoming all the obstacles he met with, he never managed to convince those who funded this study that his collaborator Theodor W. Adorno was indeed capable of doing research work that would justify further support. Adorno and the emigrated Frankfurt School (formally, the Institut für Sozialforschung or Institute of Social Research) eventually got a second chance when the American Jewish Committee (AJC) proposed employing Max Horkheimer to direct a project which was eventually published as the five tomes of *Studies in Prejudice*. The most important study of this series was *The Authoritarian Personality*, the genesis of which is described in Chapter Six. This analysis is based on archival material of the AJC, as yet neglected by existing studies.

Chapter Seven, the final chapter, presents the accounts of foundation officers and American guest professors concerning their experiences in Germany and Austria after the Second World War. Its aim is to show how large a gap there was, by then, between American social scientists and the achievements and competencies of those of their colleagues who had lived through the years of Nazi rule in their home countries. Differences in emigrants' readiness to return to one of the successor nations of the Third Reich strongly contributed to the marked differences between the situations of the social sciences in Germany and Austria.

Since the role of financial support is of major importance in the present account, an attempt is made, in an appendix for those interested in the issue, to represent income differences over time for academics among the countries that are included in the present analysis.

# 1

## THE BUILDING OF AN AMERICAN EMPIRE

How did the United States become the global hub of scientific research? There is no denying that this is its position at the beginning of the twenty-first century. What is controversial, however, is whether this 'domination' is legitimate and rests on properly scientific grounds, or whether it is just a side-effect of the economic and military success that the United States achieved over the course of the last century. Occasionally this new centre is said to exclude its competitors (either intentionally or out of ignorance); sometimes it is diagnosed with initial signs of decline. Still, German authors who study the United States system of higher education like to point out that it was modelled on the German university. While obviously true for the late nineteenth century, this resemblance is as obviously untrue for the decades that followed. Within a very short time, the American post-secondary education system and the American way of organizing research developed a quite distinct profile. The apprentice outdid the master, becoming the global model.<sup>1</sup> European researchers concerned with exile and emigration, cultural history and the history of science sometimes suggest that this scientific and cultural pre-eminence was primarily due to the impact of those thousands of scholars, intellectuals and artists who had come to the United States as refugees from Europe. Rudolf Carnap, Ernst Cassirer, Erwin Chargaff, Max Delbrück, Albert Einstein, Enrico Fermi, Kurt Gödel, Walter Gropius, László Moholy-Nagy, John von Neumann, Otto Preminger, Leo Szilard, Alfred Tarski, Victor Weisskopf, Hermann Weyl, Billy Wilder ... – the list could easily be continued. However, as I will show in the following, the pre-eminence of the American system of science and its institutions was not only attained quite independently of any impact by emigrants from Europe but, on the contrary, offered them the very opportunities that allowed them to establish themselves (in many cases quite successfully) in the first place.

### THE EXPANSION OF TERTIARY EDUCATION

In the first half of the twentieth century, all nations which had systems that differentiated scientific research and teaching faced similar problems. As an

immediate consequence of the process of modernization and the extension of civic rights that went along with it, there was the issue of granting access to higher education to previously excluded social strata, mainly – at least initially – the female half of the population. In democratic countries, it was no longer possible, and often undesirable, to defend the exclusiveness of access to education horizontally – against women – as well as vertically – against those strata that would later be described as the ‘educationally disadvantaged’. Sooner or later, one or more of these groups had to be granted access to higher education. Traditionally stratified societies (*Ständische Gesellschaften*), in contrast, were slower to give up practising social discrimination.

Since their earliest days, the American colonies and, later, united independent states conceived of themselves as a refuge for those whose freedom was compromised elsewhere. This came to be expressed iconographically in the Statue of Liberty in New York Harbor, a gift of French admirers of the American idea of freedom. If, initially, colonists were mainly non-conformist members of the elites of their countries of origin, the new immigrants of later periods formed a population that collectively adhered to the idea of upward social mobility as a promise to, or even a certainty for, the next generation. Educational institutions were the vehicles of this ascent.

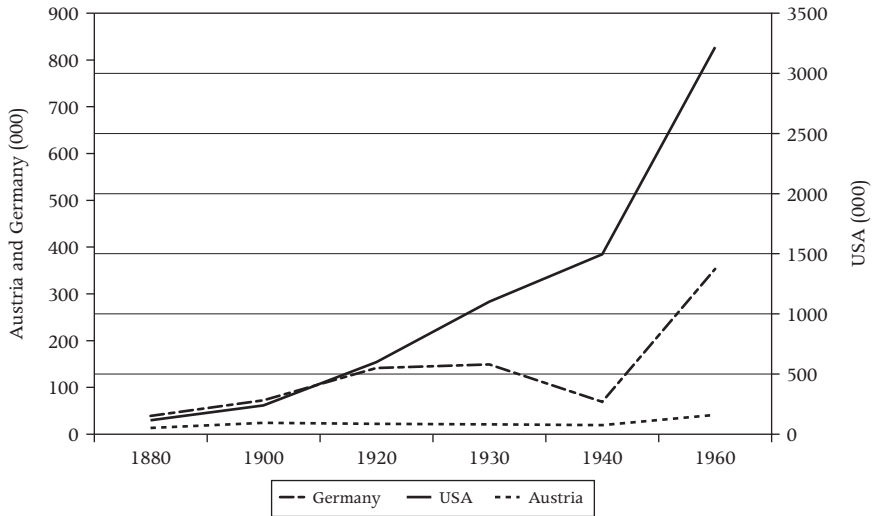
Even after the end of the American Civil War, the system of higher education in the United States was a rather unsophisticated affair. Those who sought a scientific education had to go to Europe to get it. Between 1815 and 1914, more than 10,000 Americans obtained their doctorate at a German university (Brubacher and Rudy 1997: 175), among them some of the early sociologists, such as Robert E. Park, as well as many American philosophers and virtually all of those who made up the founding generation of American psychology. Not surprisingly, Johns Hopkins University, which was founded in 1876 and was the first US university to establish doctoral studies, did so by adopting the German model. It was Johns Hopkins, again, that later introduced the new type of academic teacher, who did both teaching and research, to the colleges and universities that had been set up at the end of the nineteenth century. Johns Hopkins became the model for both the reorganization of existing colleges and the foundation of new universities, among them, most notably, the University of Chicago. John D. Rockefeller Sr donated \$2 million (\$4.7 billion in 2010) to found it. In operation since 1892, this new university in no time became the leading centre of research and education for the new group of disciplines called the *social sciences*. Albion W. Small, the founding professor of sociology there and a Johns Hopkins graduate, had studied in Germany for a time and formed, with John Dewey, Thorstein Veblen, James R. Angell and others, a group of professors who departed from the German model with their stronger commitment to the practical application of knowledge. This commitment resulted in their being praised by pragmatist William James as ‘more consistently pragmatist than his own pragmatism’ (Brubacher and Rudy 1997: 186).

A problem all university systems had to solve was the status of the education they offered. Should the universities that educated for the sciences be

complemented by universities dedicated to vocational education, e.g. the technical or educational professions? Should the education offered by the establishments of the tertiary education system be hierarchically structured – for instance, by implementing a separation such as that between colleges charged with undergraduate education and special university-status ‘graduate schools’ for further education? Should new professions be allowed the same type of programme as the older medical or law schools? Finally, how much education (or training) was needed in the first place? For most of these questions, no definite answers could be found, and solutions changed with the decades. Different national provisions for the education of elementary school teachers and social workers are an impressive case in point. While traditionally stratified societies looked down on such education as mere vocational training and disposed of it by confining it to special institutions outside of the traditional universities, the young democracies routinely integrated it into their college and university programmes. In contrast, ‘home economics’<sup>2</sup> in the United States illustrates how problems of education were dealt with in a society that had already subscribed to the idea that formal education was a source of socio-economic distinction, and disapproved of any display of class arrogance towards inferior forms of work.

In the middle of the twentieth century, the ever increasing proportion of young people staying for longer periods in educational institutions led to a rise in the age of entry into the labour market, which in turn reduced the numbers in the workforce. This was a welcome effect at a time that was still experiencing the impact of the Great Depression. Around 1900, while the proportion of those between age five and nineteen who were in primary and secondary education was about the same in most rich countries of the time,<sup>3</sup> the numbers of those in post-secondary education differed widely. In 1880, the absolute number of university students in the United States was almost three times that of Germany or Austria, and twenty years later the figure was only slightly higher. In the years that followed, however, the number of university students underwent almost exponential growth in the United States, while numbers in Germany remained stagnant. In 1930, the number of American university students was seven times the number of German or Austrian students; due to the Second World War, this ratio rose to a gigantic 21:1 by 1940 before going back, in 1960, to the still high ratio of 9:1 (Figure 1.1).

Since the layout of national educational systems differs widely and the quality of available data leaves much to be desired, the best way to do a long-term comparison of several countries is to cross-reference the number of individuals in post-secondary education, (i.e. university students) to the size of the general population of their respective countries. With these two sets of data, one can be reasonably sure that they were established in similar ways and are not too strongly biased. Table 1.1 shows these comparative data for four measurement points in the twentieth century. In the second half of the twentieth century, Austrian numbers keep declining towards the back positions (just as those of the three other countries that were in top positions in the 1920s, Czechoslovakia,



**Figure 1.1** Development of the No. of Students in United States, Germany and Austria

or the Czech Republic, Switzerland and Hungary), whereas the English-speaking countries outside of Europe (United States, Canada, Australia and New Zealand) could maintain or even boost their leading position. Until 1970, Japan is among the countries with the highest proportion of students, while Israel maintains her front position and Argentina is for a certain time surprisingly close to the top. In the classical European science nations – France, Germany and Great Britain – the number of students is consistently lower than the mean of those 30 or so countries included in the comparison.

The countries exposed to a prolonged period of communist rule show a remarkable development of their own. While in 1950 and 1970, the communist countries are quite close to the top, they all – with the exception of Russia – lose this position after the downfall of the communist system. Unhampered by market constraints and funding problems, in the forty years of their existence these countries pursued an extensive educational policy, which may have been in part a systematic labour market policy as well. The high proportion of qualified personnel that migrated from these countries to the West after 1989 highlights the international competitiveness of education under state socialism.

In Austria, the First Republic was heir to an over-sized university system housing a disproportionate number of young people who sought academic education as a matter of family tradition. The result was an ‘over-production’ of under-employed talents because governmental retrenchment policies prevented any increase in employment openings. The universities were denied further appointments, and non-university research was as negligible then as it would be later.

In the United States, the number of university students, and of Ph.D.s, rose steadily. While in 1850, there were no more than eight ‘graduate students’ in



**Table 1.1** Number of Students per 100,000 Inhabitants, 1920, 1950, 1970, 2000

Country (Year) 1920 unless specified	Per 100,000 Inhabitants	Country (Year) 1950 unless specified	Per 100,000 Inhabitants	Country (Year) 1970 unless specified	Per 100,000 Inhabitants	Country (Year) 2000	Per 100,000 Inhabitants
USA	566	USA	1764	USA	3713	Russia	4948
New Zealand (1921)	322	Japan	980	Japan	3280	USA	4676
Austria (1923)	298	Argentina (1960)	872	Israel (1972)	3097	Spain	4571
Japan	285	Soviet Union	597	Canada (1971)	2216	Australia	4414
Canada (1921)	262	New Zealand (1951)	567	Sweden	1486	New Zealand	4372
Czechoslovakia (1921)	208	Bulgaria (1946)	558	New Zealand (1971)	1292	Israel	4365
Switzerland	178	Poland	500	Finland	1277	Argentina	4267
Hungary	161	Canada (1951)	457	Yugoslavia (1971)	1272	Norway	4252
<b>Mean</b>	<b>160</b>	Israel (1948)	432	Bulgaria (1975)	1226	Ireland	4246
Romania (1930)	158	<b>Mean</b>	<b>398</b>	<b>Mean</b>	<b>1111</b>	Poland	4088
France (1926)	152	Australia (1947)	396	France (1968)	1088	Canada	3901
Sweden (1925)	150	Hungary (1949)	353	Argentina	1082	Sweden	3888
Australia (1921)	147	Austria (1951)	327	Italy (1971)	1036	Portugal	3722
Denmark (1925)	146	France (1946)	324	Australia (1971)	972	Denmark	3541
Germany (1925)	143	Czechoslovakia	315	Austria (1971)	840	Belgium	3459
Great Britain (1921)	138	Romania (1948)	307	Denmark	838	<b>Mean</b>	<b>3446</b>
Italy (1921)	135	Italy (1951)	303	Romania (1966)	796	Great Britain	3400
Poland (1921)	130	Finland	300	Netherlands	785	France	3393
Soviet Union (1925)	112	South Africa (1951)	291	Belgium	778	Bulgaria	3338
Belgium	126	Switzerland	271	Czechoslovakia	739	Austria	3217
Spain	110	Netherlands (1947)	270	Ireland (1971)	729	Japan	3143
Finland	108	Ireland (1951)	253	Germany	679	Netherlands	3068

Ireland (1926)	108	Yugoslavia (1948)	252	Norway	594	Italy	3067
Bulgaria (1926)	99	Sweden	233	Portugal	571	Hungary	3029
South Africa (1921)	96	Germany	230	South Africa	558	Czech Republic	2473
Argentina (1914)	95	Belgium (1947)	215	Switzerland	515	Switzerland	2160
Yugoslavia (1921)	93	Denmark	213	Mexico	514	Romania	2018
Netherlands	83	Great Britain (1951)	206	Spain	500	Mexico	1963
Norway	68	Spain	184	Great Britain (1971)	493	South Africa	1492
Mexiko (1921)	67	Norway	165	Hungary	433	Germany	1449
Portugal	49	Portugal	161	Poland	300		
		Mexico	66	Soviet Union	143		

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Source: Mitchell (1982); Mitchell (1992); Mitchell (1993). In the following cases data for students were not available for the census year: Yugoslavia 1923, Argentina 1913, Mexico 1924, Great Britain 1922, Bulgaria 1948, Soviet Union 1925, Hungary 1950. The numbers for students for the Soviet Union are: for 1925 Woytinsky 1928; for 1950 *Yearbook of the United Nations* 1950; for 1950 *Yearbook of the United Nations* 1970. Numbers for 2000 students: UNESCO 2003.

all of the United States, their number had risen to 6,000 by 1900, and to eight times as many by 1930. In the years following the foundation of the Johns Hopkins University, the number of Ph.D.s was about three dozen, by the end of the First World War it was already 562 per year, and within the next six years it doubled, reaching about 3,000 per year in 1940 (cf. Brubacher and Rudy 1997: 193; Thurgood *et al.* 2006).

## GROWING DEMAND FOR ACADEMIC TEACHERS

Growing numbers of students usually lead to an increase in the size of faculties and, at least in the short run, in opportunities for those who aspire to an academic teaching career. It is in the nature of things that the increase in the number of degrees lags behind the increase in the number of students. An educational system that is closed to influx from the outside (i.e. does not allow for immigration) will necessarily result in an inadequate student-teacher ratio, as measured by the number of students to be supervised by one teacher. Educational systems are known for their capacity to put up with prolonged periods of overworking their faculties. The negative effects of the ensuing deficits in research output tend to register only with a certain delay.

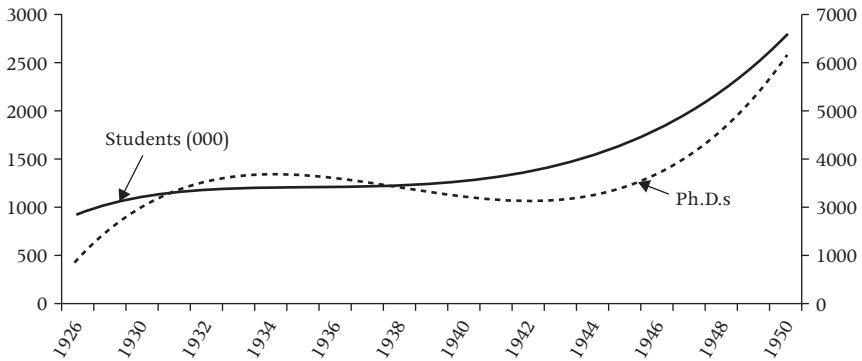
While in Germany and Austria in the first half of the twentieth century there was an increase in the numbers of both university students and university teachers, the latter did not increase at the same rate. In the United States, in contrast, student expansion was accompanied by a disproportionate increase in the size of faculties. Supervision ratios may be taken as an indicator. For Germany, these are available for 1880 and 1930 (Titze 1987). In 1880, the best student-professor ratio is 4.4:1 at the University of Kiel, with the University of Berlin last but one with, even then, a ratio of 15.4:1. Half a century later, the top ratio is 10.4:1 for the small University of Giessen (with a faculty of only 177); Berlin ranges in the lower half at 19:1; and the most inadequate ratio is found at the University of Cologne with 29.1:1. For Austrian universities, the ratio is 10.5:1 in 1900, rising to 12.9:1 at the beginning of the First Republic (1920) and starting to re-decline in 1924 to the level of 10.4:1 that was maintained until 1933. In the following years, the number of students to be supervised by one professor keeps declining, reaching its lowest level in 1937 with 8.9:1. In the United States, the supervision ratio was essentially stable between 1930 and 1940, whereas from 1938 onward, there was a decline in the number of those who qualified for a professorship, approximately defined by the number of individuals having obtained the highest academic degree, the Ph.D. (Table 1.2; cf. Thurgood *et al.* 2006: 7, 14).

The reason for this marked difference between the two German-language countries is no doubt to be found in the special characteristics of the Habsburg Empire, where the German-speaking population formed the elite and was, thus, also more strongly represented in higher education. After the end of the Dual Monarchy in 1918, what had once been the educational institution for the multinational body of civil servants for a multi-ethnic empire became a relatively overpopulated university whose graduates were no longer absorbed by the

**Table 1.2** Students and Teaching Staff, United States, 1900-50

Year	Students	Teaching Staff	Supervision Ratio
1900	237,000	23,868	9.9
1910	355,000	36,480	9.7
1920	597,000	48,615	12.3
1930	1,100,000	82,386	13.3
1940	1,494,000	110,885	13.5
1950	2,444,000	190,353	12.8

Source: Snyder 1993: 75, Table 23 (author's calculations).

**Figure 1.2** Development of the No. of Students and Ph.D.s, United States 1926-50

Source: US Bureau of the Census 1964.

bureaucracy of the residual state that went by the name of Austria. This reduced absorption capacity of a severely downsized bureaucracy, in conjunction with the de facto abolition of the military as an alternative career for the graduates of secondary education, can be assumed to have caused an increase in the afflux to, and time spent in, the universities.<sup>4</sup> There, students faced a comparatively large and almost constant number of faculties, which also was a legacy of the ancient Empire. Various reports refer to the high average ages of faculties, a result of long continuance.

In the United States, there was a short period in the 1930s where the size of faculties failed to keep up with the rapid growth of the number of students (Figure 1.2). The usual way of meeting excess demands is to rely on import. In the present context, this is of some relevance because the refugees from Europe who had come after 1933 now provided the human capital the American universities could draw upon. Between 1930 and 1950, the number of university teachers doubled. However, different cohorts of immigrants faced different opportunities for establishing themselves.

For those refugees – primarily those from Austria – who had come to the United States as late as at the end of the 1930s, and for those Germans who had

somehow managed to get through in the years between 1933 and 1938, conditions were comparatively favourable.<sup>5</sup> For those who had already arrived in 1933, coping with the enforced delay in their careers was easier for the younger than for the older ones. Having been socialized in the Teutonic model, on the one hand they were more or less prepared for this career latency, and on the other found it easier to overcome their competitive disadvantages in terms of culture (of science) than the older refugees since they could opt for a second round of university studies. This was an outlook doubly barred to those of their comrades in misfortune who were their seniors in age as well as careers: for someone who had been a lecturer or even a professor in Europe, taking a second undergraduate degree was more or less unthinkable from a subjective point of view,<sup>6</sup> while positions to be appointed to were few in the early 1930s. For those among the older ones who could not rely on support by some relief action committee, opportunities on the regular academic job market were next to zero. The few exceptions are just this: exceptions.

Since the late 1930s, the problem for many of the younger immigrant scholars no doubt was not so much finding a job but adapting to the cultural characteristics of the American system of science. The number of German-speaking sociologists who had come to the United States as refugees from the Nazis and were successful in their careers can be estimated at over 200; when they engaged in their career, there were no more than 50 American graduate sociologists to compete with (Riley 1960; Turner and Turner 1990). Growth in the number of students and the size of faculties varied between disciplines. In the 1930s and the 1940s, the overall proportion of social science graduates was stagnant while the proportion of Ph.D.s in psychology, sociology and political science increased, as compared to those in history and economy (Daugherty 1948).

## DIFFERENTIAL CAREER PATHS FOR JUNIOR SCIENTISTS

The growing number of university students also led to an increase in the opportunities for engaging in a scholarly career. This career, however, is informed by other factors. University professors are the narrow peak of the system of science, and students are its broad base. Whoever wanted to gain access to this system had to start at the bottom, pursue their course of studies and then, maybe, work their way up – slowly in one culture, somewhat faster in the other. There is no ‘lateral entry’ in the academic world.<sup>7</sup> For many decades, one-man representation was the prevalent model for most scientific fields in Germany and Austria. One man – and for once, there is no need to bother about a choice of words that might discriminate against women – represented the entire field at his university and, thus, determined curricular content. This constellation was full of consequences. The steep hierarchy of the academic universe did not allow for ‘peers’ at the full professor’s side, but only, in the best of cases, for aspirants to his succession. This is the humus in which myriad small groups have had to thrive, conceiving of themselves as ‘schools’.

Because of the small number of ‘formalized’ positions (i.e. those guaranteeing a regular income), full professors tended to foster only a small number of individuals, and in most cases only one, as their potential successor. If this individual happened to leave, or die, before the position reserved for him became vacant, this small universe cracked. Unexpected opportunities for promotion opened up for outsiders or for individuals whose socialization for the sciences had taken place elsewhere (which in this world was more or less the same thing). But early deaths were few, and academics seeking urban exodus rare.

This account is at odds with the self-image cultivated by Teutonic academics throughout the twentieth century and, thus, calls for a more detailed argumentation. There is no doubt that students often turn to more than one university in the course of their studies. This fact of student mobility seems to have given rise to the collective view that German-speaking academics showed an intense regional mobility. It was supported by pieces of popular wisdom pretending, for instance, that one had to accept whatever first position one was offered, regardless of where one might intend to end up. This view, however, is not borne out by statistical facts. The biographical data provided by *Kürschmers Deutscher Gelehrten Kalender* allow for an assessment of the scope of German academic mobility. Between 1926 and 1950, there were six editions of *Kürschmers* where sociology was registered as a discipline in its own right. During this period, 282 men and 7 women opted for sociology as at least one of several disciplinary identities. Among 147 sociologists who appear in more than one volume of the *Kürschmers*, only 54 report a change of regional affiliation. This is a full third of those who might have migrated, but only one-fifth of the overall population. During the whole period under examination, 36 of those who were mobile went to other locations within the German Reich and 18 went abroad and can therefore be included in the group of emigrant sociologists.<sup>8</sup> Thus, only 36 of 289, or 12 per cent, can be considered a living proof of Teutonic academic mobility in this turbulent quarter of a century. All the others pursued a career at the university where they had got their first position, unless they died prematurely or withdrew from academic life.

In pre-First World War Europe – and in part even after the war – sustenance for the time between graduation and access to a professorship had to be met by one’s own means. This career pattern was highly selective on the social level. In the United States, the idea that well-to-do families should invest part of their money into the (far from certain) future of their offspring well beyond graduation seems to have been non-existent. The pensioner scholar<sup>9</sup> (who after the hyper-inflation in the wake of the First World War had become extinct at least in Central Europe) and the private lecturer pursuing a bread-and-butter profession in a non-scholarly environment while waiting for an academic opening, were replaced by the post-doc, a young person who was being paid for acquiring research competencies and who, due to flat hierarchies and the larger overall number of positions, could after one or two years take up regular academic activities. This threshold was later lowered to the level of Ph.D. students, ‘graduate students’, thus completing the career pattern and pushing it to a limit

where incompatible role expectations made conflicts inevitable. Paradoxically, US university students today organize themselves in unions not as students, but in terms of their role as low-paid substitute teachers.

Even Max Weber was impressed by the American career model, which he discussed in *Wissenschaft als Beruf* (Weber 2002). Its main characteristic was – and is – that comparatively young people are given admission to the ‘faculty’, where they have to prove their worth not as assistants of their seniors in age and status but as masters of their own fate: ‘publish or perish’ is much more of an imperative for beginners than for those who have tenure. Thanks to the volume of the academic market, those who fail at one location may find a niche somewhere else. Regional and status mobility sometimes interact here in ways that may strike the European observer as strange. The early career of Robert K. Merton is a case in point. Having graduated from Temple University in his hometown of Philadelphia, he went on to Harvard on a scholarship. There he received his Ph.D. in 1936 and attracted attention with his study on ‘The unanticipated consequences of purposive social action’ (Merton 1936). In the three years that followed, he worked at Harvard on the lowest levels of academic employment, as a tutor and instructor. Between 1939 and 1941, he was a member of the Department of Sociology at Tulane University, in New Orleans, where – skipping the level of assistant professor – he was appointed associate professor. During his three-year stay at this less elite university, he rose to be a full professor and chairman of the department. In 1941, he gave up this position which, while high-ranking on the internal scale, was marginal in a regional perspective, to become an assistant professor at the Ivy League Columbia University in New York City.

In the middle of the twentieth century, leaving one university for another that was less prestigious, or more remote from the cosmopolitan centres, was as much a characteristic of the American system of higher education as the skipping of rungs in the career ladder. For example, Lewis A. Coser, Alvin Gouldner and C. Wright Mills obtained their first positions as (assistant) professors even before they had finished their graduate studies (of course, at other universities than those where they were still pursuing their studies). Moreover, changing disciplinary affiliations was comparatively easy since it was pre-formed by the non-specialist character of undergraduate studies: Robert E. Park’s many years of journalistic activities were no more a handicap than a non-existent Ph.D. was for Daniel Bell – he was appointed professor, anyway. David Riesman transferred from a professorship at the Faculty of Law to a sociological one, and when Paul Lazarsfeld was appointed associate professor of sociology at Columbia in 1941, he still thought of himself as a psychologist. Career paths such as these were possible because there was such a strong demand for professors. In the 1940s and 1950s, the academic market in the United States was a ‘seller’s market’.

A major effect of the pronounced market structure that determined the allocation of human capital in the American educational and science system is that novices are put under strong pressure to present proofs of their competence

within a rather short time. This is counterbalanced by openings on the faculty level that do not depend on whether there is actually a vacancy or the present jobholder agrees to a partial cooptation. Upgrading the position of an assistant professor to that of an associate and, later, full professor is a matter of the performance shown by the holder of the position rather than of staffing schedules.

In the Teutonic culture of science, there is no competition, no openings for meritocratic promotion and temporary downward mobility, and no cooperation among peers. The loneliness of the full professor, resulting from structural constraints and ideological hypostasis, may in some cases have increased his willingness to engage in interdisciplinary cooperation since communication on a national or even international level with peers in his own field remained episodic for various reasons. The often ridiculed conference tourism of our day was as yet nonexistent.

### INSTITUTIONAL INNOVATIONS

An innovation that turned out to be truly momentous was the department structure adopted by American universities. Unlike the Teutonic model of the professorial chair, the department consists of a stratified but nevertheless egalitarian group of teachers. Members differ in rank on the formal as well as on the pay level, but all are part of the 'faculty' and are called 'professors', and are often even addressed as such by students. Access to higher positions and better pay is based on individual performance. A new 'professor' is not recruited by the holder of a chair but by the head of the department, for whom the potential appeal to students of the education offered is a factor to be reckoned with when he composes his team. The result very often is that a wider range of orientations is represented within a field. Thus, this system allows for more diversity in the composition of a faculty than the Teutonic system does, where the obligatory first step for whoever wanted to engage in a scientific career was (and in many places doubtless still is) to be recruited as an assistant by one of the established faculty members, and according to this member's personal preferences.

One of the side-effects of the department structure is that it constitutes a barrier against the formation of schools around a single leader. Only in their very first years, the American schools that have become known as such in the history of sociology congregated around single men (William S. Sumner at Yale, Albion W. Small at Chicago and Franklin H. Giddings at Columbia), and even these could only hope for permanence if their founders managed to achieve a certain balance between shared beliefs and diversity among the members of the second generation. While Small was successful in doing so in Chicago, schools at Yale and Columbia universities disintegrated when their founders left. In the United States, one-man schools like those in Germany existed only at relatively minor or rather peripheral universities (George A. Lundberg at the University of Washington in Seattle, Howard Odum in North Carolina, Emory Bogardus at the University of Southern California), while larger departments typically became bi- or even multi-polarized after a few years, which frequently led to sharp rivalries: Robert Lynd vs Robert MacIver



at Columbia, Pitirim A. Sorokin vs Talcott Parsons vs George C. Homans at Harvard. Successful former antagonists such as Merton and Lazarsfeld contrast with failed efforts, as in the case of Parsons and Samuel Stouffer. In between, there is the standard case of the more or less heterogeneous department, such as at Berkeley in the 1950s, where symbolic interactionist Herbert Blumer worked side by side with historical sociologist Reinhard Bendix and Lazarsfeldians Seymour Martin Lipset and Martin Trow (Platt 1996).

Departments were first introduced in the newer American universities, while traditional universities like Harvard were comparatively late in adopting them. As a result, the number of representatives of the same discipline who were concentrated at one place increased, which in turn entailed a certain pressure for cooperation, since pursuing purely individualistic strategies in one's dealings with the administration meant that one was collectively worse off. Departments whose members had fallen out with one another risked being closed down and, at any rate, had their funds reduced (for examples see Abbott 1999 and Bulmer 1986). The fact that the department was the organizational level for allocating funds imposed cooperation and compromise among its members and ruled out individual free-rider behaviour, if nothing else because students, via tuition fees, acted as a regulative. Even though students never actually gained the exclusive position that should have been theirs by the rules of model economy, they still had a certain influence on the course of events since, as consumers, they had a market to choose from. In Hirschman's terminology they had the option of exit, but not of voice (Hirschman 1970). In order to appeal to students, the university had to guarantee minimum diversity, which in turn resulted in more opportunities for outsiders to establish themselves.<sup>10</sup> Flat hierarchies and the absence of personal dependencies (such as the dependency on a full professor at German and Austrian universities) further contributed to internal diversity (a term used by Bulmer to characterize inner department diversity) (Bulmer 1986).

Even though Europe has seen similar (but historically very rare) cases of 'motley' constellations of faculties, this diversity rarely became productive because differences in status were strongly accentuated and there was no culture of cooperation to rely on. In First-Republic Vienna, Othmar Spann and Max Adler temporarily engaged in a heated rivalry for students' attention, albeit under very unequal conditions, since Spann was the chairman and Adler just an associate professor without civil-servant status.

American universities soon abandoned the combination of teaching and research that is characteristic of the German model for a teamwork model where junior scientists are specifically recruited as collaborators in a research project and work under the supervision of a senior scientist (for a European perspective on this, see Adorno 1972a). This model seems to have emerged during the First World War when even social scientists were required to engage in teamwork to serve the United States government and were put under severe time pressure to produce results. By the late 1920s, this type of collaboration had become so much the routine procedure that the committee on

'Recent Social Trends' set up by President Herbert Hoover (President's Research Committee on Social Trends and Mitchell 1934) needed a mere twenty-four months to publish their final report (a book of several hundred pages, supplemented by half a dozen book-length special reports). Reviews of this book repeatedly pointed out the novel character of this way of doing research.<sup>11</sup>

At the same time, the 'Notgemeinschaft deutscher Wissenschaft' (Association for the Emergency Funding of German Science) was reflecting on 'collective research' (see Chapter Three). In the autobiography of Friedrich Schmidt-Ott, the *éminence gris* of German research policies, there is a revealing remark on how this 'collective research' used to be initiated: 'In most cases, proposals were submitted by individual eminent scholars, described and explained in individual memoranda and discussed in special committees which, after endorsement by the expert committee and the steering committee [of the 'Notgemeinschaft'], were also in charge of supervising their execution' (Schmidt-Ott 1952: 212).

In this corporate model, participation was limited to the high-ranking members since only these were represented in the respective committees. There they were of course confronted with competitors, but policies were routinely oriented to mutual non-interventionism. Thus, if one member out-rivalled another at one point, he had to give way to the losing party next time. In the Teutonic model, one had to find a field and establish a monopoly on it, often in the form of a long-term solo editorial project. It is hardly surprising, then, that no other science culture has produced so many 'complete works' that were never *really* completed, even as new ones continued to be launched. This is the exact opposite of teamwork, as described in the foreword of *Recent Social Trends of the United States*:

The investigators were recruited with the advice of officers of the Social Science Research Council, of universities and other scientific institutions. Frequent progress reports were made by them and staff conferences were held from time to time as the researches progressed. Preliminary drafts of chapters were submitted for criticism as to accuracy and freedom of bias. In published form the chapters represent not only a treatment of the factors of social change, but an attempt to coordinate and integrate the evidence into a useful whole. (President's Research Committee on Social Trends and Mitchell 1934: xciii)

As a consequence of the contract form of American (social science) research, the social form of the 'project' became the predominant organizational mould for conducting research. Its major elements are: external funding by organizations not necessarily interested in the material results but which, as with philanthropic foundations, may promote non-specific interests; limited temporal horizon of the research; collaborators who are specifically recruited for a project; supervision by an advisory board composed of prominent scholars and representatives of the public; and pressure for success in order to qualify for further funding.

Research was subjected to a new temporal regime because funds, whether provided by foundations or allocated by the universities themselves, were almost always bound up with a time limit, and decisions on renewals, extensions and new applications were also reached on the basis of previous results. As a major way of structuring research, the 'invention' and large-scale introduction, in the United States, of time horizons within which projects were socially expected to produce presentable results was a byproduct of external funding. Combined with the regulative effects of the obligation to submit a report and of having past achievements evaluated, it revolutionized scientific research.

One could escape the stress of teaching in the mass university and of being submitted to administrative control – both of which had already been identified, with some concern, by Weber as special features of the American system of higher education – by exiting from the university system and establishing oneself in the non-university sector. There were special research centres that had been set up by a number of scientific academies, and where there were no such institutions or the existing institutions were too insignificant, private sponsors stepped in to set up non-university research centres. Finally, starting in the late 1880s, there was a quick succession of specialized research institutions created by industry to serve as industrial laboratories.<sup>12</sup>

With the creation of non-university research institutions, funding issues became more pressing. Nations with strong central governments and few or no private sponsors felt the need to justify the way funds were distributed since acceptance for preferential treatment in a universe of nominal peers (full professors) was hard to come by. As recently as the late 1920s, Augustus Trowbridge, travelling Europe on behalf of the Rockefeller Foundation, was surprised to find more than one Nobel laureate rather poorly equipped, in local comparison, with instruments, laboratory equipment and the like (Kohler 1991: 150–56). In a country such as the United States with its huge private fortunes, the first task for scholars was to persuade potential private sponsors that what they were doing was worth funding. Of course, the 'utility' this science was supposed to have was not an invention of those days, but it was then that it became a criterion for the allocation of funds. Since there was no central state administration or funding of universities in the United States, new actors appeared, encouraged by a cultural climate of scientific optimism: foundations assumed steering functions. Foundation officers, often endowed with far-reaching authority, decided on the allocation of funds and the development of research fields.

The American educational and science systems not only expanded earlier and faster than elsewhere, but also implemented a number of institutions that quickly spread to other science systems. One of these is the sabbatical (Eells and Hollis 1962), a free year with half one's pay available not only to senior scientists, used as an opportunity to dedicate oneself to the completion of a larger individual project, unmolested by day-to-day university routines. The first sabbatical was granted in 1880 by the president of Harvard University to a philologist, and by the turn of the century this policy had become accepted

as the rule for all the so-called research universities of the American northeast that competed with Harvard because otherwise they would have risked a brain drain. By the end of the twentieth century, the sabbatical had become a more or less worldwide institution (Geiger 1986: 75).

As such, one might say with regard to the United States, as a variation from the term coined by Rudolf Hilferding to describe the capitalism of that time, that this was 'organized' science, developed in the first third of the twentieth century in the course of a comprehensive social experiment. There is a pattern here that seems familiar in the history of social theories in the United States: a strong affinity to social Darwinism. This common thread in American social theories has been attributed to the fact that the immigrant society offered opportunities for the many and exorbitant material success for the few. As far as I know, however, none of the authors who have dealt with the American spirit and its notions of *exceptionalism* and *destiny* as a critique of ideology have ever considered the idea that a quasi-Darwinist pattern might also be detected in the way institutions were formed. Since the end of the Civil War – before which one can hardly speak of a system – the United States science system has been oriented to a diversity that was free to evolve and combine into endlessly novel forms. In the competition with other models, this diversity soon turned out to be its strongest asset.

## THE EMERGENCE OF NEW SCIENTIFIC DISCIPLINES

The national counterpart to the creation of the department on the level of individual universities was the creation of professional organizations. Combined with the opportunities for and pressure to mobility, this led to a standardization of the social and cognitive gestalt of disciplines. Being a member of the department of  $x$  and the professional organization of  $x$  encouraged a person to see him or herself as a member of the discipline of  $x$ . As the annual conventions of the professional associations began to function as a job exchange and, thus, as a transfer point for junior scientists, the result was a cognitive and social standardization of the respective disciplines. Thus, at the beginning of the twentieth century, the United States had already established the very structures which Europe came to adopt only with a considerable delay. On the local level of a department, the advantage of diversity seems evident at least to students since it makes for a wider range of the education offered. But diversity seems to be an advantage for research as well – provided one is willing to accept concepts such as critical mass and density of communication as factors that are conducive to innovation (Hage 1999; Hage 2000; Hollingsworth and Hollingsworth 2000; K.H. Müller 2000).

A non-intended side-effect of small social universes is that they generally lack a body that represents the interests of disciplines still in the making. The associations of German physicians and jurists, but also the *Deutscher Historikerverband* (German Historical Association), were founded because of, among other reasons, the growing numbers of members of these professions. Thanks to their large membership, these quasi-professional organizations

could effectively present their demands in public, while the valiant pioneers of the new discipline of sociology had to be content with filing petitions to these pressure groups and were unable to directly approach the sovereign or the administration.<sup>13</sup> This absence of a professional or trade organization was also one of the reasons that prevented the emergence of an exchange of ideas among colleagues who were not personally acquainted. In the personal space of a full professor, new faces seldom appeared, a fact that could be conveniently justified by the romantic notion that friendships had to be established on a life-long basis and, therefore, were few in number.

Decisions about the fate of new disciplines were rare, while decisions about the fate of aspiring assistant professors were a regular event, namely in the cabinets of regents or in the antechambers of ministers. These discussions were not open to everybody, probably not even to every full professor if his university happened to be a provincial one. Since the proponents of anything that was innovative, uncommon and, therefore, highly suspect were rarely allowed to climb the internal status ladder to reach the rung of a dean or rector, they were barred from access not only to the salons, but also to the antechambers of power.<sup>14</sup> By relying on mediators – power brokers in Eric Wolf's terminology<sup>15</sup> – one could perhaps influence personnel decisions but surely not explain, let alone influence, something as incomprehensible as the social or cognitive gestalt of a new discipline and its potential for development.

Characteristically, innovation within this system was possible only indirectly via personnel decisions that had the support of those 'in high places'. Joseph Schumpeter owed his appointment to the University of Graz – by no means the biggest hit in the lottery of positions – to an intervention from the top which the emperor had surely not been given to sign without the help of a power broker (Swedberg 1991). Even in republican times, disciplinary policy was pursued in terms of personnel policy. The Prussian Minister of Education, Carl H. Becker, who would have liked to see sociology established as a proper discipline because it was an adequate means of 'civic education', had to rely almost exclusively on an appointment policy in pursuing this aim: 'While in England and America universities are based on foundations and are free to handle appointments and economic conduct, universities in Prussian Germany are state organizations ... Everywhere, professors are appointed ... by the government. It fills the vacancies and is theoretically empowered – and has more than once acted accordingly – to appoint faculty at its own discretion, disregarding the universities.'<sup>16</sup>

It was only at the bigger universities that there were opportunities for cooperation among the representatives of a field and, thus, for the formation of what is referred to in innovation research as critical mass – a term which, however, fails to specify its volume. A closer look at the development of the (social) sciences in the German-language area provides some confirmation for this: the Vienna School of the Theory of Marginal Utility only flourished when a second generation had emerged and, with it, the number of chairs held by its

proponents at the University of Vienna had increased. The Logic Empiricists of the Vienna Circle favoured an atypical collaboration among the representatives of various disciplines by opening the Circle to participation from outsiders. In Frankfurt, competition between the followers of Mannheim and those of the Institut für Sozialforschung (Institute of Social Research) is said to have inspired discussions in the early 1930s, while the presence of a large number of representatives of various disciplines united by the bonds of friendship similarly contributed to a fruitful dialogue. No such thing can be reported for the smaller universities where, as often as not, teaching was assured by mavericks who expounded on their very special versions of the respective disciplines. Adolph Günther in Innsbruck is a case in point, as well as Johann Plenge in Münster (see Stölting 1986: 129–33), or Richard Hildebrand, predecessor of Joseph Schumpeter at the University of Graz and dubbed the ‘miniature Herostratus’ (Seidl 1982). The one-man constellation implied that there was a tacit division of labour between the professors at the bigger universities, who did research as well as teaching, and the professors at provincial universities, who in most cases did only teaching but could compensate for this by being the local ‘top dog’.

At the structural level, the system of German-language universities was the equivalent of the German mini-state system that prevailed prior to the foundation of the Reich in 1871. Under conditions of low student numbers it operated well enough, but to glorify it would merely be to indulge in nostalgia. References to the highly productive and pioneering research achievements that were accomplished at the time have to be relativized in two respects. The achievements of the Vienna School of Medicine, the Berlin physicists and the Göttingen mathematicians, on the one hand, were not realized within this structural pattern but under conditions that were an anticipation of later developments, and were an exception in Teutonic science. The memorable achievements of German-language sociology, on the other hand, were realized under conditions one hesitates to describe as being conducive to creativity since they were, for a large part, due to the work of private scholars such as Georg Simmel, Robert Michels, Wilhelm Jerusalem, Gustav Ratzenhofer, Karl Renner, Edgar Zilsel and Alfred Schütz, or of ‘also-sociologists’ (academics with a strong, or primary, involvement in other disciplines) such as Hans Kelsen, Joseph Schumpeter or Emil Lederer, whose sociological achievements, at any rate, were not produced in an institutional environment that was favourable to sociology.

## INTERNATIONAL DISTRIBUTION OF SOCIAL SCIENTISTS

In the last decade of the nineteenth century, René Worms (1869–1926) founded the Institut International de Sociologie (IIS). This institution, while largely bypassed in the literature, is interesting in two respects: set up in 1893, it was the first international (multi-language) organization to bring together social scientists under the label of sociology, and an analysis of its membership provides insights into the history of the institutionalization of sociology. Until the

beginning of the First World War, the IIS regularly organized international congresses of sociology, the proceedings of which were published in its *Annales* of the IIS. Worms was also the editor of the *Revue Internationale de Sociologie*.<sup>17</sup>

IIS membership was limited, as was customary for academies and similar institutions, to 100 full members and associated members, respectively,<sup>18</sup> and since Worms could not win over all his colleagues for his undertaking, it does not reflect the entire range of the sociologists who were working around 1900. The most notable absence was that of the followers of Émile Durkheim. Still, the French were the largest national group, and French was the lingua franca. The United States provided only 14 IIS members (6 per cent) and, thus, was the sixth largest group – behind Russia! A change of name from ‘International’ to ‘European’ would seem to have been indicated, but since no such change occurred, this rather suggests that at the time the Europeans conceived of themselves as the unquestionable centre of the whole scientific world.

The scarce biographical data<sup>19</sup> that were published in the register of members reveal that those who reported sociology as their professional identity already were the largest group, followed by the economists and the representatives of the juristic disciplines (including political science), all of whom, however, also conceived of themselves as sociologists. At any rate, the fact that they were members of the IIS strongly suggests this type of partial identity.<sup>20</sup>

The biographical data further include information on whether or not IIS members were affiliated with a university, which in turn allows for conclusions as to the different degrees of institutionalization attained by sociology. While 35 (of a total of 37) jurists, 44 (of 47) economists and 22 (of 25) philosophers held a university chair, only 26 (of 49) of those who reported a sociological position did. This is even more striking when countries are compared: even in 1913, there still was no German member who could report both an exclusively sociological identity and a university position; of the 12 British members who identified themselves as sociologists, only 2 were affiliated with a university. Table 1.3 shows the cumulative data for the different countries of origin. What is striking is the large proportion of IIS members with a university affiliation in Italy, Belgium and Germany, which in the latter case, however, was primarily due to the so-called also-sociologists, as mentioned above.

In the early 1920s, there were about 200 international sociologists from a wide range of nations and disciplines. In some countries, the new subject was taught at the universities under the name of sociology, in others its efforts to gain ‘cognitive’ independency remained controversial, while in still others attempts at university establishment failed altogether. In many countries, the national sociological associations were dominated by also-sociologists. These did not always evolve to be ‘pure’ sociologists, nor did their associations evolve to be professional organizations. Whatever the individual reasons for this unwillingness, or powerlessness, to impose sociology as an independent university discipline (see Käsler 1981; Fleck 1990a), by around 1910 American sociology had already taken the lead. Sociology existed as an independent

**Table 1.3** IIS Members, Affiliation and Country (in %)

Country	University based	Not university based	Total	No.
France	9.2	7.4	16.6	38
England	7.4	7.4	14.8	34
Italy	10.9	3.5	14.4	33
Germany	7.4	0.9	8.3	19
Other European	3.9	3.1	7.0	16
Russia	3.5	3.1	6.6	15
USA	5.2	0.9	6.1	14
Belgium	5.2	0.9	6.1	14
Spain	4.8	0.9	5.7	13
Austria	3.1	0.9	3.9	9
South America	2.6	1.3	3.9	9
Hungary	2.6	0.4	3.1	7
Switzerland	2.2	0.4	2.6	6
Asia Minor	0.9		0.9	2
<b>Total</b>	69.0	31.0		229

Source: *Annales de l'institut international de sociologie* 1913.

discipline with, what is more, sociologists teaching at prestigious universities. They had gained, via their professional organization, a distinct position that was unmistakable in the concert of the social sciences and unchallenged by the neighbouring disciplines. Even in these early days, the sociologists of the country that later provided the model for the discipline were otherwise distinguished than their European colleagues: 9 of the 14 American IIS members were full professors of sociology.<sup>21</sup> No other country had a similar number of scholars who called themselves sociologists and were established as such at a university.<sup>22</sup> Before the First World War – after it, the IIS became more and more insignificant – the social science elite that was brought together in the IIS was not yet concentrated in terms of regions, and still extremely diverse in terms of disciplines.

### NEW DONORS, NEW DECISION-MAKERS

It was the immense wealth of the United States that made foundations possible, in the first place. This wealth was accumulated under cultural and economic conditions that were – and still are – characteristic of the American society and need not be described in detail here. Visiting foreigners observed them with amazement, Americans glorified and vilified them. Phrases like 'self-made millionaire', 'robber baron' and 'big spender' became household words not only in English-speaking countries.

The religious roots of philanthropy have been studied in much detail. The set of institutional conditions that allowed for the rapid acquisition of wealth, and the normative orientations that went along with them, are the reasons



why the United States is the wealthiest country of the world and, at the same time, has one of the least 'inclusive' systems of social security. John Kenneth Galbraith's decades-old diagnosis of 'private wealth and public poverty' still holds true. But since our concern here is not with present-day issues of social policies but with the emergence of a new way to promote the sciences, issues that belong to an analysis of society will not be addressed unless they are relevant for an understanding of scientific institutions.

Philanthropy is an ambiguous term, and its literal meaning – love of human beings – does not help much either when it comes to defining what it is supposed to mean. In one sense it describes every form of charity by individuals to other individuals or social groups. Due to the successful application of this form of altruism, the United States refrained from developing a system of social security such as those implemented by all other modern states to ensure the (re) distribution of wealth as well as of social risks. Charity is something between individuals, between those who give because they feel a moral obligation to do so and those who receive and who may count on being given to when they are in need – but with the power of definition lying not with the needy but with those who give. When Alexis de Tocqueville returned from his journey to the United States in the fifty-fifth year of its existence as a republic, he even claimed that a 'general compassion for the members of the human race'<sup>23</sup> would arise as soon as democracy would abolish the barriers of rank and the privileges that went along with them. He seems to have been right in at least one point, since American philanthropists, as we will see, often enough extended their activities beyond their own country to support needy foreigners.

In the course of the last two centuries, American philanthropists served religious, aesthetic, contemplative and cognitive needs, the Church, education, culture and science. In the beginning, there was the funding of the Sunday-school activities of some religious denomination by its members, but after the Civil War the focus shifted to the education of the great-grandchildren, still called 'negroes' at the time, of the slaves towed in from Africa. The extended system of public libraries that helped immigrants of every level of education in their struggle to find their bearings in the New World was almost entirely the result of private initiative. Today, philanthropists fund museums, concerts and research centres, and support efforts to rebuild an open society in countries formerly under communist rule (Nielsen 1972; Nielsen 1989; Lagemann 1999).

Any survey of the history of philanthropist activities in the United States reads like a chronology of the values that prevailed at different times. Philanthropy took a decisive turn when, with the emergence of mass society, social problems multiplied and every clear-sighted observer had to acknowledge that charitable donations could at best serve as palliatives to these ills but were powerless to eliminate them. The charity of former times, whose aim had been to help individuals cope with needs, was superseded by a philanthropy that used scientific findings and sought to foster them. Since foundation officers were university graduates trained to think in chains of causation, they were no longer oriented to

the immediate relief of distress but to the detection of its roots. The new goals were public health, medical research, and campaigns for disease and epidemics control, rather than the care for orphans or the wars against alcoholism and prostitution, which used to be seen as a healthcare issue. This also was the beginning of the liaison between affluent donors and scientists. Didn't the scientists promise to reveal the sources of diseases? So, why not use the same scientific approach to reveal the sources of those *social* evils which, at the time, were framed as a social pathology? Moreover, the widespread belief, stimulated by scientific achievements, that if only scientific research was permitted to go on long enough it would in the end also detect the laws that governed the social sphere, acted as a catalyst (Karl and Katz 1981: 243–4).

Around 1900, the first industrial magnates turned to the sciences as the 'needy' ones of modernity, and at the same adopted – by their own account at any rate – the tools of science in order to invest their donations with similar accuracy in meeting their targets. The two best-known philanthropists were Andrew Carnegie and John D. Rockefeller, two prototypical self-made men. From early youth both had donated part of their incomes, which at the time had by no means already reached notable heights, to charities and later continued to do so on a correspondingly larger scale. The part of their fortunes that was dedicated to charities was so large that new forms of organization had to be found.

Distributing one's wealth according to scientific standards as Carnegie and Rockefeller did was in the spirit of the age, so to speak; donating to the sciences was more of a surprise. The long history of philanthropy shows that in choosing the institutions to be supported, the owners of large fortunes often paid tribute to what were defined as key problems by their society and culture. They rarely acted as social innovators, leaving this part to the recipients of their funds or to one or the other of their foundation officers. Anyhow, in twentieth-century America, a widespread belief in the sciences prompted these two magnates to transfer part of their fortunes to foundations that in some way or another began to act as institutions for the advancement of science.

Carnegie, having risen to become the country's leading steel industrialist and multi-millionaire, made redistribution his personal philosophy and, since he liked to put pen to paper, preached it at some length. At the age of fifty-four, he declared that 'the man who dies rich dies in shame,' as if he had already been aware of the Kwakiutl Indian institution of potlatch (in which possessions are given away or destroyed to enhance prestige). Rather than entrust an immense fortune to inept heirs (or to donate it by testamentary bequest to some vague charities), Carnegie argued that every millionaire having reached the second half of his life should give his fortune to the public. Entitled the *Gospel of Wealth*, Carnegie's pamphlet found entrance not, it is true, in world literature, but in the history of philanthropy. One of its arguments is that it takes an expert to redistribute wealth just as it takes one to accumulate it. Until his death in 1919 at age eighty-four, the eccentric Scottish-born immigrant placed over \$300 million (\$3.7 billion in 2010) in the various

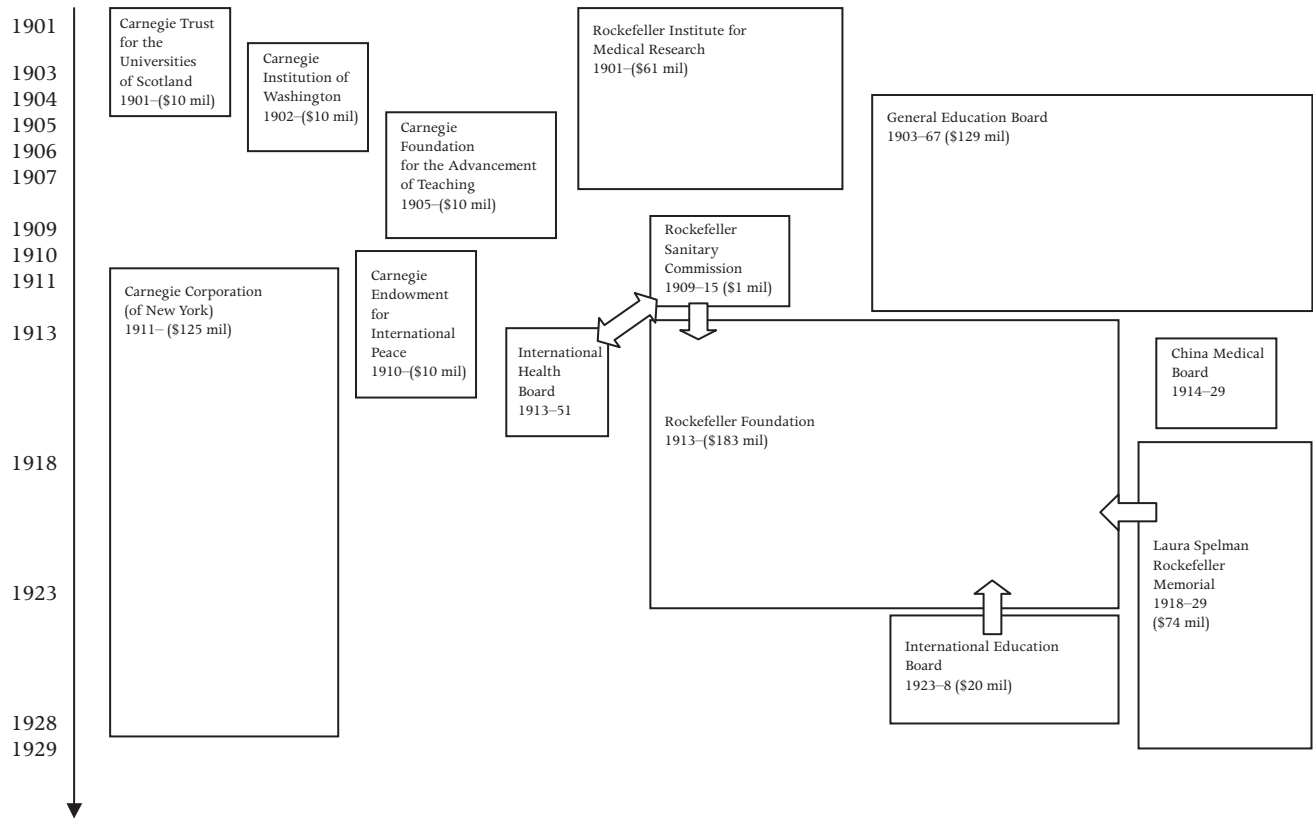
foundations that bore his name. But he came to learn that donating money was a difficult job. When he died, there still were millions of dollars left, even though he had begun to distribute his riches rather frantically in his last years.

Carnegie was an example that inspired. Among those who initially imitated and later competed with him, the one figure that stood out was Rockefeller, who had become rich in the petroleum industry. His reputation as an industrialist was even worse than that of Carnegie; since neither had bothered much with morals or the law when making their money, they were far from being popular with or respected by the general public. When they started to distribute their wealth, many people suspected that they did so only to embellish, disguise or even promote their entrepreneurial concerns. In the long run, however, both have become firmly rooted in the collective memory of Americans for their donations rather than for the way they accumulated their wealth.

For the present purpose, there is no need to go into the details of the philanthropic empires of Carnegie and Rockefeller (see Figure 1.3); it is quite enough to describe the problems that arose in the context of the advancement of science and to highlight the mechanisms that proved to be useful in this business.<sup>24</sup> Initially, Carnegie and Rockefeller took an active part in their foundations, which was by no means something all donors did. Unlike in business life, where they had ultimately relied on their personal intuitions, these new activities often required them to rely on the judgement of others. Philanthropists at the time sought to avoid public controversies about the institutions or measures they chose to support, which led to less confidence and more reserve in their actions in the world of science sponsorship.

Carnegie's funding of public libraries – he promised to pay the cost of setting up a library for every community that would provide a plot of land and assume the cost of current operations – occasionally met with resistance,<sup>25</sup> but the obstacles that had to be overcome were clearly outlined and the benefit of a library was unquestionable. In an American society where equal opportunities for starting out in life were highly valued, the diffusion of knowledge to all who were interested in it was much more evident than in the more traditionally stratified societies of Europe, which were much more reluctant to give up privileged access.

When scholars began to apply, foundations not only had to learn to distinguish between charlatans and experts but also to come to decisions about which fields of science were worth supporting. Prior to this period, philanthropic donations had been dedicated to more or less clearly outlined concerns which – and this, again, is specific to the United States – were supposed to either combat an evil on the local level, known to and deplored by everybody, or to satisfy some secondary needs, and thus to enable all members of the community to derive a clearly defined benefit. Scientific research and even teaching (which Carnegie had also sought to improve through a foundation) were no longer local concerns, but national and even international ones. When Rockefeller entered the scene as a big spender, his initial activities still evolved entirely in the local tradition: he



**Figure 1.3** Andrew Carnegie's and John D. Rockefeller's Foundations (Selection)

Note: Title of foundation, founding year-final year, initial trust property; area proportional to trust property.

Source: Fosdick 1952; Lagemann 1983; Lagemann 1989.

donated money to a Baptist college in Chicago in order to found a university. That this would become the University of Chicago was not foreseeable. When local charitable concerns such as these were replaced by the general advancement of science, neither the definition of the object to be advanced nor the group to be favoured were clear, and the pompous mottos both foundations had given themselves were not least a result of this state of affairs. The Carnegie Corporation (CC), Carnegie's last and biggest foundation set up in 1911 with \$125 million in capital (\$2.8 billion in 2010), was supposed to 'promote the advancement and diffusion of knowledge and understanding among the people of the United States'. The Rockefeller Foundation (RF), established two years later with an initial capital of \$183 million (\$3.9 billion in 2010), was dedicated to 'the Well-Being of Mankind Throughout the World' (Geiger 1986: 143).

Big donors such as these two industrialists faced the problem of deciding what (else) they wanted to promote. The decisions reached in the end – for instance, with Rockefeller, Christian missionary work in China, various forms of medical and public health activities, support for war victims in Europe, or fighting disease in Central America – reflect the personal preferences and influence of advisors with a long-standing experience in philanthropic matters. When the entire territory was covered, so to speak, and there were still millions waiting to be donated, Carnegie and Rockefeller both looked beyond the small circle of advisors and ended up with the vague aim of advancing science. But in doing so, they also delegated part of their discretionary competencies to those who, as members of professional groups, were closer to the potential applicants than to the social worlds of the donors.

Foundations of philanthropists that are dedicated to such a wide range of purposes were, and are, organized like firms. The founder and namesake of the foundation – or later his heirs – would have a seat on the board of trustees and, in most cases, exercise an influence that went well beyond a mere vote. For support, they recruited intimates and celebrities. The former were supposed to help them reach decisions, the latter to function as experts in carrying them out. The founders, however, did not act as the president of the board or the foundation. Initially, this was a charge for one of the intimates, and a full-time job at that. As foundations evolved, presidents were selected very much like CEOs. Neither the founders nor the presidents were authorized to make decisions without consulting others. In some of the foundations established by Carnegie and Rockefeller, their influence was rather reduced, which was due not only to their old age but also to the fact they were no longer interested in the current affairs of these institutions. For individual decisions, the trustees who laid down the general guidelines (and in the early days also were responsible for day-to-day management) had to rely on the expertise of others unless they wanted to fall prey to lobbyists, insinulators and applicants.

As the philanthropic empires expanded, more and more people were involved in decision-making and, unsurprisingly, more and more debates arose. What, then, were the structured forms that crystallized? One of the problems to be solved was information: how should a decision be reached, and what prior

knowledge was needed to reach it? Another one was that the organizational structure of the foundations called for change because the work required could no longer be done as a part-time job. But who was to decide? Three options suggested themselves: obtaining expert opinion, delegating the decision to external sources or recruiting a new type of foundation officers.

The simplest case was for a foundation to ask, and pay, a renowned expert to do a survey, and to reserve its decision on what was to be done (i.e. what was to be sponsored) until an opinion was delivered, even if recommendations voiced by experts were not always acted upon. Considerably more influence was given up when foundations, following European examples, established research institutions of their own. The Rockefeller Institute for Medical Research (founded in 1901) and the Carnegie Institution of Washington (founded in 1902) were endowed with sizeable capital, the interests of which served to cover the cost of daily affairs (Geiger 1986: 59–93; Kohler 1991; Lagemann 1989).

For Carnegie's foundations, success only came when, after a long time, they helped to set up two new social science institutions. During this long incubation period, however, external political conditions as well as the staff composition of the Carnegie Corporation had drastically changed. Carnegie's role in setting up the National Bureau of Economic Research or the Brookings Institution is not evident in their names. Whether this was due to the amount of funds allocated or to the controversial nature of the disciplines represented by these two institutions remains an open question, but in the light of some comparable cases, the latter seems more likely. These two institutes were the first social science research centres that owed their existence to funding by a philanthropist. Therefore, we will discuss their emergence in some detail here.

Captains of industry did not feel that the knowledge that was the basis of their success still needed discovering. Only its diffusion seemed something of a problem. Malevolence and lack of education were taken to be the reasons why their perspective on the world of money making was not shared by everybody. So they readily took to the idea of popularizing the insights that guided their action. Thus, at a conference that brought together the wealthiest and most important industrialists in 1912, the founder and president of AT&T declared that a 'constant chain of correct information' should be directed at those who were most exposed to the influence of demagogues and agitators, and most ready to succumb to it (cited in Lagemann 1989: 53). Sympathizing with this view, John D. Rockefeller Jr, who had also been present at the conference, charged one of his officers to sound out possible ways of action. But having consulted the economists at various universities, this officer came up with the suggestion that an institute be set up in order to do basic research in economics. This, the researchers felt, was much more important than propaganda. Since no compromise could be reached between the advocates of propaganda, mainly found among the senior advisors in philanthropic affairs, and the junior academic economists who argued for setting up the research centre, the initiative was adjourned.

The Russell Sage Foundation, which since its founding in 1907 has been dedicated to the improvement of social work and the funding of social surveys to this end, did not strike the industrialists as a possible model. For a number of years, empirical economic research remained a domain of researchers at universities. In 1914, in the wake of an industrial conflict on the Colorado prairie that had turned to bloodshed when gunmen, hired by a company in which Rockefeller had a major share, had murdered strikers and their families, Rockefeller charged a Canadian with conducting a scientific study on 'industrial relations', a term that came to be used for the first time in those days. Because of the obvious link between the violent repression of the strike and this assignment, reactions among the public as well as on Capitol Hill were extremely hostile, recalling the populist criticism that had flared up during the anti-trust suit against the flagship of the Rockefeller empire. Due to this episode, Rockefeller abstained from sponsoring the controversy-prone social sciences for the next ten years.

Carnegie or, rather, the leading officers of his foundations, were less averse to economics and saw the danger that arose from such initiatives as rooted for by the political critics of plutocracy. One suggestion was to strengthen public control of the foundations. It took the political caesura of US entry into the First World War and the subsequent shake-up of many institutions, as well as the innovations brought about by war administration, to make possible the foundation (and funding) of the two research institutes mentioned above.

Carnegie Corporation funding began in 1920 for the National Bureau of Economic Research and in 1922 for the Institute for Governmental Research, which later became known as Brookings. In both cases, cooperation between scholars, industrialists and government officials during the First World War seem to have acted as a stimulant. After the end of the war, the idea of maintaining some of the research institutions that had been set up under the auspices of the federal government rather imposed itself, primarily for those institutions dedicated to statistical work. Emphasizing the fact-finding aspect of economic research, a proposal was drawn up that was acceptable to all concerned (Lagemann 1989; Grossman 1982; Bulmer and Bulmer 1981).

The long delay that characterized the social sciences' entry into the sphere of large-scale funding by foundations and, ultimately, the way to success of those who had ventured to promote it, was due to the fact that in the world of philanthropic organizations a number of changes had occurred that, in sum, contributed to facilitate the establishment of the social sciences. The traditional trustees, who were businessmen or even clergymen, had seen their influence more and more reduced in favour of individuals coming from the world of science. Scholars, or former scholars to be more exact, made their entry not only to the boards of trustees, but also to the various levels of foundation staff. This shift in the social composition of the main bodies also entailed a shift in the foundations' aims. While the funding of the natural and medical sciences, besides the actual benefits these disciplines might offer, had to a certain extent been geared to the instruction of and effect upon the lay public, this aspect

faded into the background when scholars in the foundations sought to improve the working conditions of their colleagues at universities and research centres. After the end of the First World War, the advancement of science was intensified. The shift from propaganda to basic research as the maxim for funding decisions was accompanied, on the part of the scholars, by a neutral, impartial, exclusively science-orientated attitude which was more and more taken for granted and made it easier for its advocates as well as for the foundations to appease contradictory or admonitory voices since there was no risk of triggering public controversy.

The fact that decisions were no longer made by the founders or their advisors but were delegated to the representatives of the profession that was to benefit was explicitly acknowledged when the foundations started to collaborate with organized groups of scholars. A committee that had been established during the First World War as an advisory body to the government evolved to become the National Research Council (NRC) which, due to its energetic protagonists, soon gained acceptance with their scientific colleagues and, what is more, could convince the foundations to carry on with their activities after the end of the war. The NRC was not only allotted large sums of money, or promised such sums for the future, but was also left free to decide on how to distribute them. In this case, the foundation actually was reduced to a kind of holding company that had no say in the operative part of the business. There was not much left of Carnegie's initial idea that a rich man should, in the second half of his life, become an 'administrator of surplus wealth' (cited in Bremner 1988: 102). The place of the self-assured super-rich dilettante was increasingly taken by well-paid employees who, before entering the world of philanthropic foundations, had come to know or even excelled in the world of science. Among the first presidents of the CC as well as the RF, there are former professors of sociology, such as George Vincent (RF, 1917–29), of psychology, such as James R. Angell (CC, 1920–21), and of mathematics, such as Max Mason (RF, 1929–36), all of whom also acted, before or after, as university presidents.

After the First World War, the number of staff at the sub-presidential levels also increased; almost all of these men and women were university graduates, and some of them had been more or less intensely engaged in scientific work themselves. They were, thus, much more familiar with the frame of mind of their clients. They had their own notions of the development the sciences should take, and a clear idea of the way in which foundations could contribute to this development.<sup>26</sup> We will meet some of them on the pages that follow.

Until the United States entry into the Second World War in 1941, some of these novel formations proved to be highly efficient; afterwards, the state became involved as an actor in the system of science and transformed it from top to bottom. In a sense, the United States became much more European after this, which some have attributed to the influence exercised by the intellectuals among the refugees from Hitler. After a review of some of the points that have been briefly evoked here, it will be easier to see whether or not this is true.



Hardly anybody will deny that after the Second World War, the United States became a world power on the military as well as the political level – and that, at the end of the century, it was the only one left. Most commentators date the ascent of the American science empire to this period as well. With respect to top achievements, as reflected, for example, by the number of Nobel Prize laureates, this is no doubt true. But on the level of institutions, the American science system was superior to the European one even in the interwar period. The two main driving forces were the rapid increase in the number of students in tertiary education and the willingness of private financiers to donate their fortune to the sciences. The basis for both was the belief in the utility of scientific research. The social sciences benefited from this utilitarian and science-friendly climate and, more specifically, from the fact that conditions for innovations to emerge and to impose themselves were more favourable in the United States than in Europe. The pioneers of American sociology, it is true, were no match on the intellectual level for their European contemporaries, if being awarded the honorary title of a '*Klassiker*' or a 'great' is taken as a criterion for someone's admission to the collective memory of the discipline – the only American from this generation who was admitted to the temple of the immortals was Charles H. Cooley.<sup>27</sup> But unlike their European colleagues who, with the exception of Durkheim, were institutionally impotent, the founding fathers of American sociology managed to participate in the establishment of the American science empire and even to derive an above-average benefit from the new institutions. These new innovations included features as diverse as the sabbatical with regular payment for university professors, paid work opportunities for graduates, departments and professional organizations that were more than intellectual debating societies, project-based organization of research and scholars who worked as experts in the new philanthropic foundations or acted as their advisors. These smaller or bigger innovations have as yet not been given much attention in the literature on the institutionalization of sociology, although there can be no doubt that they largely contributed to the success of the American world of science.

Finally, it has to be emphasized that the United States was much quicker to adjust to new conditions, no doubt also because there was no central state relying on national legislation to control the intricately linked worlds of higher education and research. As a result, innovations were pitted against each other in a qualifying competition that produced the very diversity which, in turn, allowed others to adopt the most successful among them. One need not share the belief that the United States system of science has come up with an adequate solution for every problem. But it can hardly be denied that this was the system where the broadest range of responses was put to the test. Some of the institutional innovations that proved to be successful in the United States have been adopted in other parts of the world. It is not an exaggeration to say that the United States has been – and remains – *the* laboratory for science policies in the twentieth century.

# 2

## FELLOWSHIPS AND WHAT THEY ENTAILED

Among the various activities pursued by American foundations, fellowship programmes may seem to be the least spectacular ones. Material support for gifted but needy students is no doubt an institution as old as the university itself. Traditionally, students were supported to help them finish their studies. What was new about the grants awarded by foundations is indicated by the very term ‘fellowships’, which soon became quite common, marking as it did the difference from ‘scholarships’ that were granted for educational purposes.

### A NEW WAY TO SUPPORT JUNIOR SCIENTISTS

The idea of awarding one-year fellowships to university graduates was voiced, advocated and finally realized by one of the first American Nobel laureates, Robert Andrews Millikan (1868–1953). In 1923, Millikan was awarded the Nobel Prize in physics for his experimental proof of Einstein’s photoelectric equation. He won recognition for being one of the first scholars to excel in a new role in the scholar’s role set: along with a certain number of other science entrepreneurs who were indefatigable in their efforts to improve scientific infrastructure, he devoted himself to the organization of research. Besides his full professorship in Chicago, Millikan was one of the directors of the National Research Council (NRC) that had been established during the First World War, where he helped to develop anti-submarine devices. He also used his stay in Washington, DC to lobby for the creation of a grants programme designed to help junior scholars avoid the deficits of his own education (Millikan 1950). Millikan, whose political stance seems to have been a rather conservative one, later got involved in another new international scene, the League of Nations’ Committee on International Cooperation, and eventually helped co-found the California Institute of Technology (Caltech) in Pasadena. The grants-for-graduates project was rejected by the Carnegie Corporation,<sup>1</sup> but aroused the interest of the officers of the Rockefeller Foundation (RF). In 1919, the RF allotted half a million dollars (\$6.1 million in 2010) in total to the NRC to be spread over a five-year period.

In the years after the end of the First World War, the philanthropic activities of the Rockefeller family had become so diversified that the family members themselves and their advisors, for whom this used to be a side job, had already given up part of their influence in favour of salaried foundation officers. The turn towards the social sciences in one of the Rockefeller foundations in the 1920s was, thus, due to the commitment of junior officers who succeeded in getting the trustees' assent, rather than to any initiative of the founding family. The person who played an outstanding role in this was Beardsley Ruml.

## A NEW DIRECTOR

Ruml, the grandson of a Czech immigrant, was born in the Midwest. After his undergraduate studies at Dartmouth College, one of the leading American 'liberal arts colleges', he studied psychology under James R. Angell (1869–1949) at the University of Chicago. He wrote his Ph.D. thesis on the reliability of psychological tests for measuring intellectual capacities. During the First World War, Ruml collaborated with Angell, John B. Watson and other psychologists at the War Department to develop a test to be used for recruiting military personnel. After the end of the war, he and some other members of the group set up the first private firm to offer psychological counselling for industry.

At age twenty-five, Ruml entered the world of philanthropic organizations as an assistant to Angell, who served for a short time as President of the Carnegie Corporation. At age twenty-seven, Ruml already was Director of the Laura Spelman Rockefeller Memorial (LSRM), a foundation of the Rockefeller family dedicated to the memory of the late wife of John D. Rockefeller, Sr. During its first years, the Memorial had sought to sponsor activities in domains bound up with the deceased's concern for the well-being of women and children. Accordingly, during the first four years, only 0.005 per cent of the budget was spent on scientific research (Bulmer and Bulmer 1981: 352). The analysis of the Memorial's archival material led the Bulmers to a conclusion that was not very flattering: 'The programme was lacking in clear direction and was undistinguishable' (Bulmer and Bulmer 1981: 353). With Ruml's appointment as a full-time director, this rapidly changed. Within a very short time, he transformed the smallest of the Rockefeller foundations (with a trust property of \$74 million, about \$1 billion in 2010) into the most important institution for the advancement of the social sciences not only in the United States, but in Europe as well. The eminent Robert M. Hutchins (1899–1977), long-term President of the University of Chicago, even referred to Ruml as 'the founding father of the social sciences in the USA', although this may give him a bit too much credit (Johnson 1945).

In 1928, before the merger of the Memorial and the RF, Ruml stepped down as director to engage in other activities. After a short interlude as Dean of the Division of Social Sciences at the University of Chicago, he returned to New York where he eventually became chairman of Macy's, the department store. During the New Deal, he served as Director of the New York Federal Reserve Bank and entered US financial history as the man who invented a new

method of collecting income taxes. 'Ruml's method of tackling problems is to sit in a chair and do nothing ... With his mind released from ordinary influence, he can command wider vistas of fact and theory than when methodically studying a subject,' one reads in an article about him entitled, significantly enough, 'National Idea Man' (Johnson 1945).

At barely 30, Ruml, with some flourish, submitted his ideas for the social sciences to the trustees of the Memorial:

All who work toward the general end of social welfare are embarrassed by the lack of knowledge which the social sciences must provide. It is as though engineers were at work without an adequate development in the sciences of physics and chemistry ... production from the universities is largely deductive and speculative, on the basis of second-hand observations, documentary evidence and anecdotal material. It is small wonder that the social engineer finds this social science abstract and remote, of little help to him in the solution of his problems.<sup>2</sup>

There was nothing new or original about this, but the way it was proclaimed sounded a new note of self-assurance of the social sciences. Ruml's experience as an army psychologist and industrial counsellor provided him a powerful rhetoric that was obviously convincing. His approach proved to be effective since the addressees of his memorandum, while they may not have believed him, did nothing to prevent him from putting his plans into action, either.

Although Ruml did his best to achieve a balance of power between the social sciences and the hard sciences, there was no evidence for him to rely on in his dealings with the patrons that could substantiate his claim of equal economic utility for both science realms. The new 'realistic' social sciences – for a long time this was the qualifying adjective that firmly pointed out the kind of social sciences they had in mind – were not geared to boosting productivity and, ultimately, profit.

In contrast to the dealings with the trustees, where the motto of the RF – 'the well-being of mankind throughout the world' – was frequently evoked in some paraphrase or other, communication within the Foundation and with external scholars much more strongly relied on properly scientific arguments: the fellowships were to serve the advancement of the best and prevent junior faculty members from abandoning research because of their teaching load (Mitchell 1926).

## NEW FIELDS FOR INTERNATIONALISM

While detailed reasons had been given to the trustees of the Foundation in favour of the social sciences as a field of funding activities by 1922, few words had been devoted to the innovation that was perhaps more spectacular, i.e. awarding fellowships to foreigners with the explicit aim of improving conditions in their home countries. This seems odd since it clearly contrasted with the localist tradition of philanthropist donations and, thus, should have required an explicit rationale. One explanation might be that for the RF, it

could have been subsumed under its Christian missionary activities, mainly represented by its extensive China programme (Ninkovich 1984). On the other hand, its activities to combat epidemics were driven by an internal logic to look beyond the borders of the United States.

The decisive factor seems to have been the attitude of American intellectuals who were interested in international affairs. In the early 1920s, the general feeling among them led to the creation of another initiative. Immediately after the signing of the ceasefire that marked the end of the First World War, two influential trustees of the Carnegie Endowment for International Peace, Elihu Root, former Secretary of State under Theodore Roosevelt, and Nicholas M. Butler, President of Columbia University and later Nobel Peace Prize laureate, suggested founding an organization dedicated to the promotion of international understanding. US intervention in the First World War, they argued, had been driven by moral rather than nationalist or imperialist reasons, and this commitment was now to be continued.

At the same time, European intellectuals were fighting over whom to blame, and to what degree, for the war, and what could be done to foster or stem social revolution – depending on one's convictions. Throughout all these turbulences in and around Europe, the small number of Americans who were interested in foreign affairs cultivated a paternalist attitude towards European issues that had emerged from the war. First, foundations like those of Rockefeller and Carnegie had provided the old continent with humanitarian aid; then the United States had also helped on the military level; and now they wanted the role of peacemaker. With an enthusiasm that, given the former isolationism, might seem compensatory, this small group began to advocate international commitment. This, however, spectacularly failed when in 1920 both houses of the United States Congress voted down President – and former professor of political science – Woodrow Wilson's motion that the United States join the League of Nations.

Refusing defeat, the losers began to build the institutions outside the sphere of influence of the political establishment. The Institute of International Education (IIE), an initiative of Root and Butler, developed a remarkable exchange programme for foreign university students and professors, inviting them to the United States to study the inner workings of the new world power. The direction of the new institute was in the hands of Stephen Duggan, one of the most active liberal internationalists among the American university professors. IIE funding was ensured by the Carnegie Endowment for International Peace.

At the time, Americans doubtlessly felt superior to Europeans in many ways and wished to demonstrate that this feeling of superiority was culturally as well as politically well founded. But even though they wished to be admired, they were not quite sure of the admirers' free will to do so. Pilgrimages to the Mecca of Western civilization were not yet undertaken on the pilgrims' own initiative and expense, but on invitation. Rather than simple generosity, this was primarily a manifestation of a lack of self-assurance on the part of the young nation.

The scholars who came as refugees from the Nazis after 1933 directly benefited from the internationalist spirit of their American colleagues that had emerged more than a decade previously, as well as from the institutions this spirit had sparked, because institutions like the IIE were the core element of the relief organizations for emigrants.

## AID TO EUROPEANS AND TO THE NEW SOCIAL SCIENCES

In 1924, the Memorial's Executive Committee authorized the funding of its first fellowships for social scientists. The sums dedicated to sponsoring European and American social research were roughly the same – about half a million dollars (\$6.2 million in 2010) each, over a five-year period.<sup>3</sup> Once the programme was authorized, issues of implementation had to be discussed.<sup>4</sup> Experiences previously gained in other Rockefeller philanthropic institutions were drawn upon, with the result that funds, for instance, were allocated over a longer period of time, and to self-governed committees. In strict analogy to the procedure relied on for NRC fellowships in the natural sciences, the Social Science Research Council (cf. Fisher 1993) was established, and charged with the selection of fellowship candidates.

In preparation for the fellowships to be granted to Europeans, Guy Stanton Ford, a historian who at the time was a Memorial officer, charged two professors and colleagues of his to go to Europe and look for candidates. These were John J. Coss, philosopher at Columbia University, and Frank Aydelotte, pedagogue and President of Swarthmore College and, later, of the Institute for Advanced Study at Princeton. Nominees-to-be were, primarily, economists, sociologists and political scientists, but eligibility also extended to historians, psychologists and anthropologists: 'Nominations may also be made in the case of a man or woman of very unusual qualifications in philosophy, geography, law and education, when the specific subject of study bears immediately on the principal subjects mentioned above.'<sup>5</sup> Thus, the disciplinary field for the recruitment of fellows was staked out for years to come. It is noteworthy that in doing so, the Memorial anticipated an internal differentiation and external delimitation of the social sciences which, actually, was instituted only much later.<sup>6</sup> The focus on the three core disciplines of economics, sociology and political science decisively influenced the development of the social sciences.<sup>7</sup>

However, the choice of disciplines is rather less of a surprise than the list of the countries to be included in the programme. On their very first exploratory trip, the professors not only visited the states which, along with the United States, were among the victors of the recent world war, but also those that had been enemy countries, such as Germany and Austria.

'Inductive' research, as defined by the Memorial, was also the decisive criterion for the support dedicated to research projects. Ruml and company were of the opinion that this style of research was still far from dominant in the United States. On the contrary, financial support for American institutions was also framed to help develop this type of empirical research.

This initial setting of priorities may well have been what ultimately won over the senior Rockefeller trustees who had for such a long time hesitated to (re-) enter the field of social research. To solicit their approval, Ruml used strong words which today seem to reveal a rather naive view of the potential of the social sciences. However, simple but strong beliefs may sometimes be necessary to get something new under way: 'Social knowledge is not a substitute for social righteousness; but unless we are ready to admit that the situation is utterly hopeless, we must believe that knowledge is a far greater aid to righteousness than is ignorance.'<sup>8</sup>

In the hands of 'competent technicians', the gain in knowledge would lead to 'substantial control'; it should perhaps be noted that at the time, 'social control' was understood to be the self-regulation, without state control, of the society (Janowitz 1975).

### EUROPEAN ADVISORS

Since the Memorial did not want to take on the task of selecting the European fellows itself, nor to rely on applications, local advisors were appointed and, in the following years, acted as its representatives in the countries chosen. In this, the traditional reluctance to cooperate with foreign governments that was characteristic of American foundations played as strong a part as the positive experience of working with the NRC. Avoiding publicity as much as possible was a way of keeping Europeans' animosities against American donors at bay and, thus, from counteracting the plan. This procedure quickly resulted in nominations and clear responsibilities.

Initially, responsibility for the quality of the holders of fellowships lay exclusively with the respective representatives or, in the case of Germany and Britain, with a committee of only a few members. This again was due to previous experiences gained in the United States where, rather than entrust the funds for fellowships to the heads of departments, selections had from early on been left to the discretion of independent advisors (Kohler 1991).

In France, this representative was Charles Rist (1874–1955), Professor of Political Economics at the University of Paris. In Austria it was Alfred Francis Pribram (1859–1942), Professor of Medieval and Modern History. In Czechoslovakia there were two representatives: Alfred Amonn (1883–1962), economist at the German University of Prague, for the German-speaking candidates, and Joseph Macek (1887–1972), Professor of Economical Theory at the Hohe Handelsschule and, later, the Technical University of Prague, for those who spoke Czech.

The Vienna and Prague advisors eventually ceased to fulfil their functions, although it was at different times and for different reasons. Amonn left for a three-year stay as a visiting professor in Tokyo, an appointment Ludwig von Mises had previously turned down. On his return from the Far East, he accepted a professorship at the University of Berne, where he became Rector in 1949/50. In 1926, due to his long absence abroad, Amonn was replaced as a RF representative by Franz Xaver Weiss (1885–1956), economist at the Prague

German University. The three other advisors were victims of the political situation: Pribram was dismissed from his Vienna chair in 1938 and managed to escape to Britain with the help of RF officers. Macek, who for a certain time had also been a Member of Parliament, emigrated to the United States after the communists took over in 1949 and, in spite of his advanced age, was appointed Full Professor at the University of Pittsburgh. Weiss had to leave Prague in 1939 for Britain where he lived as a freelance author until his death (Hagemann and Krohn 1999a: 737–8).

The German committee included the following members:

- Friedrich Schmidt-Ott (1860–1956), jurist, collaborator of Friedrich Althoff, Prussian politician committed to cultural and educational issues, and co-founder of the Kaiser-Wilhelm-Gesellschaft (known today as the Max Planck Society). Between 1917 and 1918, he was the last Royal Prussian Minister of Cultural Affairs, and in the Weimar Republic he was one of the leading founding fathers of the Notgemeinschaft der Deutschen Wissenschaft (Association for the Emergency Funding of German Science), acting as its President between 1930 and 1934. Between 1920 and 1945, he also was a member of the supervisory board of the IG Farben conglomerate.
- Paul Fridolin Kehr (1860–1944), full professor at the universities of Marburg and Göttingen, director of the Prussian Historical Institute in Rome since 1903, General Director of the Prussian State Archives in Berlin between 1915 and 1926, the year of his retirement, as well as Director of the Kaiser Wilhelm Institute for German History and President of the Central Board of Management of the Monumenta Germaniae Historica.
- Albrecht Mendelssohn-Bartholdy (1874–1936), grandson of the composer, full professor of Civil and Foreign Law and Comparative Law at Leipzig and Würzburg and, from 1929, at the University of Hamburg; from 1925 also acting as an arbitrator at the Hague Court of Arbitration and as a German delegate to the League of Nations. He was forced to emigrate in 1934.
- Hermann Oncken (1869–1945), historian, full professor at Giessen, Heidelberg and Munich, and, from 1927, at the Friedrich-Wilhelms-Universität at Berlin. In 1905/06 he was exchange professor at Chicago, and during his time in Berlin was also a member of the Historical Committee of the Reich.
- Hermann Schumacher (1869–1952), born in South America, several times abroad for extended educational journeys in his early years, which also led him to the United States. After full professorships at Kiel and Bonn, he became full professor for Political Science at the University of Berlin. He was the first Kaiser-Wilhelm (Visiting-) Professor to teach at Columbia University at New York and had himself referred to as the ‘German editor of the 16-volume “Encyclopedia of the Social Sciences” that is being published’.<sup>9</sup>
- Acting as the Secretary: historian Dr August Wilhelm Fehling, who had graduated from the University of Rostock and whose main job was that of a ‘referent’ for the Notgemeinschaft.



Apart from Kehr and Oncken, all the Committee members had been to the US.<sup>10</sup> They held key positions in the German science system and were highly respected. All members of the Committee were more or less involved in German politics. This was particularly obvious in the controversy about war guilt. As early as in 1915, Schmidt-Ott, a lifelong monarchist, had participated, along with Oncken, Schumacher, Otto Hintze and Friedrich Meinecke, in the publication of what he called a 'factual scientific account entitled "Germany and the World War"' (Schmidt-Ott 1952: 143). After 1933, the national conservatives among the members of the Committee parted company. Mendelssohn-Bartholdy was among the first victims of Nazi anti-Jewish policies. Others tried to come to terms with the new rulers, as Heiber has shown, using Schumacher as an example (Heiber 1991: 343). Schmidt-Ott lost his function as President of the Notgemeinschaft, but remained otherwise unmolested. Oncken became the object, after one of his lectures, of attacks by his former student, Walter Frank, and subsequently had to retire against his will.<sup>11</sup>

In the present context, the political involvements of the members of the German Committee are of less interest than their preferences in terms of a policy for science. They were not young any more, and in spite of their transatlantic experience they did not really seem predestined as proponents of the new 'American' social sciences. Kehr and Oncken were traditional historians, and Schumacher was explicitly hostile to the theory of political economics. Mendelssohn-Bartholdy was the only one who advocated a more open attitude towards recent scientific developments.

In Britain, historian James R.M. Butler was nominated as an advisor and charged with setting up the committee that was to support him. In the beginning, however, the committee was not very active.<sup>12</sup> It was not until 1931 that the list included six more members:

- James R.M. Butler (1889–1975) was the son of the Master of Trinity College, Cambridge, where he himself was schooled and, after the First World War, acted as a tutor to the future King George VI. In the 1920s, Butler stood as a Member of Parliament for Cambridge University; his views were those of an independent liberal. At the same time, he held a lectureship in history at Trinity College and, in 1947, was appointed Regius Professor of Modern History. Butler primarily distinguished himself as a military historian and as one of the chief editors of a forty-one-volume *British Military History*, to which he contributed two volumes on the first half of the Second World War. He was knighted in 1958. An obituary reads: 'He never married, and his college and university always stood first in his interests and affections' (*Dictionary of National Biography* 1986: 114).
- Alexander Morris Carr-Saunders (1886–1966) attended the elite school of Eton and read zoology at Magdalen College, Oxford. He then turned to biometrics, which he studied under Karl Pearson, and became the Secretary of the Eugenics Education Society. After the First World War, he worked as a demonstrator for zoology in Oxford, but soon became interested in demographic studies which ultimately earned him the appointment to the first Charles Booth Chair in Social

Sciences in Liverpool. In 1937, he succeeded William Beveridge as Director of the London School of Economics, and held that appointment until 1965. He was knighted in 1946.

- Henry Clay (1883–1945) had graduated from Oxford and worked in private welfare organizations and as a lecturer in adult education. As a result of this activity, he wrote an introduction to political economics for the general reader. In 1922, he was appointed Stanley Jevons Professor of Political Economics at the University of Manchester, but later changed to hold a chair of social economics. In 1930, he became involved with Bank of England work; he was critical of John Maynard Keynes's economic ideas. In the late 1930s, he contributed to the establishment of the National Institute of Economic and Social Research, where for twelve years he was in a leading position. In 1944 he became head of Nuffield College in Oxford, created with a donation by Lord Nuffield and dedicated to the advancement of the social sciences. He was knighted in 1946. Clay's political outlook was that of a nineteenth-century liberal.
- Charles Samuel Myers (1873–1946) read physiology at Cambridge and as a young man participated in an anthropological expedition. Later he turned to experimental psychology. In the early 1920s, he went to London where he founded the National Institute of Industrial Psychology and the *British Journal of Psychology*. Myers gained international recognition as a psychologist and was also active in Jewish philanthropic concerns.
- Josiah Charles Stamp (1880–1941) left school after compulsory education and joined the civil service as a boy clerk in the Inland Revenue Department. He acquired knowledge in political economics by private reading, published articles in economic journals and studied economics as an external student. Stamp quit the civil service to start a second career as a manager, first in the Imperial Chemical Industries and later in a railroad company. In 1928, he was appointed a director of the Bank of England and for many years was a member of the steering committee of the London School of Economics (LSE). He became Baron Stamp of Shortlands in 1938, and died during German bombing in April 1941.
- John Leofric Stocks (1882–1937) graduated from Oxford and became Professor of Philosophy at the University of Manchester in 1924. His philosophical publications are concerned with ancient philosophy and moral theology. In 1935, he stood as an unsuccessful Labour candidate.
- Arthur Keith (1866–1955) began his academic career as a surgeon, but soon turned to anthropology and as a proponent of Darwinist ideas got involved in a contemporary dispute about the dating of the age of homo sapiens. In 1913 he became President of the Royal Anthropological Institute for four years, and after that Professor of Physiology at the Royal Institution. Assuming leading positions in the British Association for the Advancement of Science, the Royal Society, and as Rector of the University of Aberdeen, he was an honoured and influential representative of British science, and was knighted in 1921.

- From the start, economist Noel F. Hall (1902–83) had been appointed Secretary to the Committee, for which he was paid £400 per year by the RF.<sup>13</sup>

The British Committee differed in its composition from its German counterpart in several respects. In addition to members of the traditional British upper class, there was at least one social climber. The constitution of the members of the British Advisory Committee was not exclusively centred on the academic world but more clearly included elements of civic society, and it also represents a broader political spectrum than the German mandarin Committee did. The scientific disciplines represented by the British advisors are closer to the RF preference for the so-called inductive approach than those of the very traditionalist German historians and political scientists.

The small group of RF advisors very aptly illustrates the national scientific styles that prevailed in Europe at the time. Johan Galtung's ironical division of the world of science into the four parts of Saxonic, Teutonic, Gallic and Nipponic styles, however, is too broad to be able to account for the subtle distinctions that can be shown even among the German-speaking professors (Galtung 1981). A minority of the members of the German Committee belonged to the group of German scholars that have been described as modernists by Fritz Ringer (Ringer 1983). Two of them – Schumacher and Oncken – are explicitly mentioned in his study. By contrast, Schmidt-Ott, the most influential one in terms of science policies, is rarely referred to in the historical literature. The German advisors were Teutonic in two respects: first, in their strong affinity not only to the German national state, but also to its autocratic interpretation, and second, in their allergic reaction to the RF officers' democratic egalitarianism.

## THE FIRST EUROPEAN FELLOWS

Potential fellows were required to be college or university graduates and to 'have given evidence of exceptional intellectual distinction and capacity to do original research'.<sup>14</sup> They received a one-year fellowship of \$1,800 (\$22,000 in 2010), and the Memorial provided the travel costs and tuition fees. In the first year, there was an English language course of three or four months at the LSE for those fellows who did not speak English.<sup>15</sup> A certain amount of thought was dedicated to the issue of whether or not spouses or family should be included in the fellowship. In the end, the Foundation decided on a very restrictive ruling: spouses and other family were to stay at home because they would keep the fellows from achieving the 'fullest realization of their fellowship opportunity'. This ruling was repeatedly discussed among RF officers as well as with the fellows. In later years, enforcement does not seem to have been very strict – with the minimum requirement, however, that spouses spoke English.<sup>16</sup>

The first sixteen<sup>17</sup> fellows, who later were commonly called Rockefeller fellows, came from only four of the five countries in question. The German Committee appears to have been unable to agree on a candidate. Table 2.1 shows some details of this cohort, their origin, their education prior to the fellowship and the subjects they intended to work on during their fellowship.

**Table 2.1** European Fellows 1924-5

Name (country, year of birth)	Religion	Status	Graduation year	Grade	University	Discipline	Research topic (duration)	Place to study	In 1950 active as
Blackwood, Beatrice (UK, 1889)	Presbyterian	Single	1920	MA	Oxford	Anthropology	Interracial correlation of mental and physical characters of women (3 years)	Princeton, Minnesota, Santa Fe, Yale	Lecturer in Ethnology, Oxford
Bossavy, Marthe L. (Fr, 1894)	N/A	Single	1916	Agrégé	Paris	Sociology	Education of immigrant children in USA, Americanization methods (2 years)	N/A	In convent
Brown, Sybil C. (UK, 1899)	Protestant	N/A	N/A	BA	London	Sociology	Children's courts, probation systems, industrial and reformatory schools (3 years)	New York School of Social Work, Harvard	Tutor in Mental Health, LSE
Bühler, Charlotte (Aus, 1893)	Evangelical	Married	1918	Dr phil.	Munich	Psychology	Social development of children (1 year)	Teachers College Columbia	Asst. Prof. Clinical Psychology, Univ of Southern California
Duncan, George A. (Ire, 1902)	Presbyterian	Single	1923	LLB	Dublin	Economics	Rural industrialization (2 years, actually 1 year)	North Carolina	Prof. Polit. Economy, Dublin

*(Continued)*

**Table 2.1** (Continued)

Name (country, year of birth)	Religion	Status	Graduation year	Grade	University	Discipline	Research topic (duration)	Place to study	In 1950 active as
Evans, A.D.Meurig (UK, 1902)	Protestant	Single	1923	BA	Cambridge	Law	International law: interpretation of treaties (2 years)	Harvard, Lyon	Liverpool
Ficek, Karel F. (Czech, 1903)	Catholic	Married during fellowship	1924	Handelsingenieur	Handelshochschule, Prague	Economics	Recent developments in business and industrial organization and their social implications (3 years)	Chicago, Harvard, LSE, Frankfurt	State Dept of Labor, Albany, New York
Finer, Herman (UK, 1898)	N/A	N/A	1923	DSc	London	Political Science	Comparative constitutional legislation (1 year)	USA, France, Germany, Italy	ILO, Montreal, CDN
Fritscher, Ludwig (Aus, 1890)	Catholic	Married, after selection	N/A	Dr.rer. pol.	Berlin	Economics	Correlation between agriculture and industry (1 year)	Washington, DC	Banker, Vienna
Grierson, John (UK, 1898)	Protestant	Single	1923	MA	Glasgow	Sociology	Immigration and social problems; influence of press and films on public opinion (2 years; 5 years)	Chicago, New York	Controller of Films for British Government, London
Jones, Idris D. (UK, 1899)	Church of England	Married	1924	BA	Oxford	Political Science	Law of nature in the Middle Ages (1 year)	Harvard	Died 1947

Philip, André (Fra, 1902)	Protestant	Married	1922	Jur. D	Sorbonne, Paris	Economics	Employee representation and industrial relations (2 years)	Columbia, Wisconsin	Chambre des Députés, Paris
Pollak, Heinrich (ČSR, 1899)	N/A	Single	1921	Dr jur.	German Univ Prague	Economics	Labour union organization, integration of industry (3 years)	Wisconsin, Harvard, Columbia, Brookings Institution	Commercial work in Prague
Salvesen, Harold K.(UK, 1897)	Presbyterian	Single	1923	BA	Oxford	Economics	Theory of international trade, banking and currency (1 year)	Harvard	Edinburgh
Teilhac, Ernest (Fra, 1901)	Catholic	N/A	1922	Jur. D	Bordeaux	Economics	Legal status of trusts, monetary situation of the US and financial theories of US economists (2 years)	Harvard, Columbia	Prof. Polit. Economy, Beirut
Voegelin, Erich (Aus, 1901)	Protestant	Single	1922	Dr.rer. pol.	Vienna	Political Science	General problems of government, theory of social organization, theory of law and government (3 years)	Columbia, Harvard, Paris	Prof. of Govt., Louisiana State Univ, Baton Rouge, LA

Source: The Rockefeller Foundation, Social Science Fellowship of the Rockefeller Foundation 1924–1932, Paris 1933, RG 1.2, Box 50, Folder 382, RAC, and Rockefeller Foundation 1951. Bühler and Finer were later given additional fellowships. Pollak is only mentioned in Kittredge's report, RF RG 1.2, box 50, folder 383, RAC.

The further careers of the fellows who benefited from the opportunity to do at least one year of research work (ten of them got a one-year renewal, and seven of them could even stay for three years),<sup>18</sup> primarily in the United States, are informative with respect to national political and cultural particularities. Only just over half of the European fellows of the first cohort continued to do research work while the other half pursued career paths for which the fellowships had not been conceived.

Two of the Austrians, Charlotte Bühler and Erich Voegelin, gained fame far beyond the boundaries of their home country and their mother tongue, and certainly not only because they later were among those who had to flee from the Nazis. Voegelin's fellowship was renewed twice, and Bühler received a new 'Special Fellowship' in 1934/5; both were also among those who were granted further support by the Foundation. Bühler used her stay in the United States to familiarize herself with behaviourism. In the following one-and-a-half decades, she established an intense transatlantic exchange of ideas and people. In the early 1930s, Bühler was the contact, at the Psychological Institute of the University of Vienna, for junior RF fellows from other European countries who came to Austria to pursue their studies.

Voegelin, who was eight years her junior, developed his intellectual potential only after he had emigrated to the United States. Until 1938, he was one of the few members of the groups around Hans Kelsen, Othmar Spann and the Vienna school of economics who was also welcome in the enemy camps. A lecturer until 1935, and an associate professor after this, his role at the university was marginal.

The influence exercised by the other fellows who pursued a scholarly career remained more or less 'local' in comparison. Ernest Teilhac, for many years a university teacher in Beirut, published a book right after his stay in the United States on Jean-Baptiste Say and, later, a book on the pioneers of economic thought in nineteenth-century America, which was translated into English (Teilhac 1927; Teilhac 1928; Teilhac 1936). Herman Finer had published, even before taking up his fellowship, a study on how the French, German and American governments worked (Finer 1921), and continued to focus on American issues in a perspective of comparative political science. He later went to Canada and then to the United States where he held a professorship of political science at the University of Chicago. Anthropologist Beatrice Blackwood was highly respected among her colleagues for her work on Melanesia and Guinea. She was Curator of the Pitt Rivers Museum in Oxford and taught at Oxford University. André Philip obtained his *agrégation* (the French type of permission to teach at a university, comparable to the German *Habilitation*, the importance of which is explored in Chapter Four) for political economics right after his return from the United States. Like some of his former fellows,<sup>19</sup> he wrote a book (Philip 1927) and, later, published treatises on political economics. In the 1930s, he was a university professor in Lyon. After the German invasion, he joined the French Resistance and for a certain time was in exile in London. After the end of the Second World War, he was a

Socialist member of parliament and, several times, a minister. In the 1950s, he was appointed to a chair at the University of Paris. In spite of his long list of publications, he seems to have thought of himself as a politician rather than a scholar. At the time of his death in 1970, he was responsible for development issues at the Organisation for Economic Co-operation and Development (OECD). Former priest Ludwig Fritscher turned to the private sector after his return, where he rose to become a member of the board of a large Vienna bank and president of the Vienna stock exchange during the Nazi period. The career of sociologist Marthe Bossavy, who had already obtained her *agrégation* before taking up her fellowship, took a somewhat unusual turn – she entered a convent on Long Island in 1928.

The first fellows, while not very precise in stating their research interests, invoked some more or less vague reference to the United States in most cases. The French economists, along with those fellows who wanted to study immigration problems, stand out by their rather clear-cut subjects. That Charlotte Bühler's subject exactly corresponded to one of the initial missions of the Memorial is doubtlessly not due to some strategic use of prior knowledge on her part; but the coincidence made it easier for her to be brought into contact with the right people.<sup>20</sup>

Within the first cohort, response to the idea underlying the establishment of the programme – namely, providing Europeans with additional educational options – varied widely. Most of the British fellows evidently saw the fellowship as a way of enhancing one's education as a gentleman before engaging in the hard business of making money, rather than as a professional preparation for a career as a scientist. Still, some of them would choose the latter, after all.<sup>21</sup>

The proportion of female fellows in the first cohort was comparatively high<sup>22</sup> – and they all were rather young or, at any rate, had just graduated.<sup>23</sup>

## THE FIRST AMERICAN FELLOWS

A comparison between the European and the American fellows, who received financial aid from the same organization but were selected by different bodies, will allow for a more precise view of the commonalities and the differences between the social sciences on both sides of the Atlantic. Members of the selection committee of the Social Science Research Council were, in the first year, political scientist Charles E. Merriam (1874–1953), University of Chicago, sociologist F. Stuart Chapin (1888–1974), University of Minnesota, and economist Wesley C. Mitchell (1874–1948), Columbia University (Mitchell 1926). On average, American fellows were four years senior to their European colleagues. The proportion of female fellows was somewhat smaller, but the age distribution was broader. Two of the Americans had been born abroad. Like their European counterparts, most of them had graduated two years earlier at the most, and on both sides a certain number of non-graduated candidates were awarded fellowships, as well. Seven Europeans, but only two Americans, were economists. No anthropologists and psychologists, whose professional associations were members of the SSRC, were included in this first-year cohort, in contrast to graduates



**Table 2.2** American Fellows 1925–6

Name (country and year of birth)	Married	Graduated	Grade	University	Discipline	Position 1924	Research topic	Place of Study	In 1939	In 1950
Bernard, Luther Lee (USA, 1881)	1925	1910	Ph.D.	Chicago	Sociology	Prof. Uni. Minnesota	Development of social sc. in Argentina	Argentina	Prof., Washington Uni., St. Louis	Prof./lecturer, Penn State
Everett, Charles W. (USA, 1895)	1920	1931	Ph.D.	Columbia	English	Instr. Columbia Coll.	Bentham biography and edition	London	Asst. prof., Columbia	Prof. & head English, Columbia College
Gosnell, Harold F. (USA, 1896)	1928	1922	Ph.D.	Chicago	Political Sc.	Instr. Chicago	Voting behaviour in several European countries	UK, Fra, Ger, Bel	Assoc. prof., Uni. Chicago	US Government + adjunct prof., American Uni. Washington, DC
Hansen, Marcus L. (USA, 1892)	-	1924	Ph.D.	Harvard	History	Ass. prof. Smith	Immigrants	Ire, UK, Switz, Ger	Died 1938	
Harris, Joseph P. (USA, 1896)	1918	1923	Ph.D.	Chicago	Political Sc.	Instr. Wisconsin	Voter registration	USA	Prof., Northwestern Uni.	Prof., UC Berkeley
Jaffee, William (USA, 1898)	1948	1924	Dr en Droit	Paris	Jurisprudence	Tutor City College NY	Industrial Revolution in France	Fra	Assoc. prof., Northwestern Uni.	Assoc. prof., Northwestern Uni.
Knight, Edgar W. (USA, 1886)	1916	1913	Ph.D.	Columbia	Education	Superintendent of schools	Folk high schools in Scandinavia	Den, Swe, Fin	Prof., Uni. North Carolina	Prof., Uni. North Carolina

Kuznets, Simon S. (Russia, 1901)	1929	1926	Ph.D.	Columbia	Economics	Grad. student	Trends in econ. theory, cyclical fluctuations	NYC	Prof., Uni. Pennsylvania	Prof., Uni. Pennsylvania
Malmud, Rose S. (Bessarabia, 1899)	-	1923	MA	Teachers Coll Columbia	Education	Teacher in English	Literacy	NYC	Instr. Gymnasium Tel Aviv	Teacher, Brooklyn
Martin, Thomas P. (USA, 1887)	1917	1922	Ph.D.	Harvard	History	Assoc prof. Uni. Texas	Anglo-American relations	UK	Librarian Lib. of Congress	Librarian LC, visiting prof. Bloomington, IN
Metzger, Hutzel (USA, 1894)	1922	1926	Ph.D.	Minnesota	Agricul. Econ.	N. Dakota Agricult. College	Rural price developments	Minnesota	Pres. St Paul Bank, Minnesota	Pres. St Paul Bank, MN
Mowrer, Ernest R. (USA, 1895)	1924	1924	Ph.D.	Chicago	Sociology	Ass. prof. Ohio Wesleyan	Family disorg. as a socially inherited behaviour pattern	Chicago	Assoc. prof. Northwestern Uni.	Prof. Northwestern Uni.
Mudgett, Mildred D. (USA, 1888)	1916	1924	Ph.D.	Columbia	Social Legislation & Statistics	Ass. prof. Uni. Minnesota	Legislation affecting pre-school child in Europe	UK, Fra, Ita, Scandinavia	Independent research	Independent research
Spero, Sterlin, (USA, 1896)	1921	1924	Ph.D.	Columbia	Political Sc.	Research fellow New School, NY	Negro in industry	NYC	Lecturer NYU	Assoc. prof NYU
Thomas, Dorothy Swaine (USA, 1899)	1935	1924	Ph.D.	LSE	Sociology	Res. ass. Fed Res Bank NY	Econ. factor in crime	NYC	Assoc. prof. Yale	Prof. Pennsylvanna since 1948

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from specialties (pedagogy, law, English literature) that were not represented at the Social Science Research Council (SSRC). Nine American fellows were married when they took up the fellowship, but only three of the first European fellows were married at the time of their nomination (Table 2.2).

The main difference between the two groups is that the vast majority of the American fellows held a university position when they took up the fellowship, while this seems not to have been the case for most of the Europeans (however, for many fellows, we lack information on this point). Thus, the fellowship programme in the United States was from the very start in accordance with the idea of the sponsors, i.e. to facilitate beginning a career for junior social scientists:

A newly-fledged doctor, appointed to a junior position in one of our departments, is usually assigned a heavy teaching schedule, when he neither knows thoroughly the subjects he has to cover, nor knows how to teach. During the years when he would be most likely to make discoveries, he is kept exceedingly busy mastering new subjects, marking papers, acquiring class-room skill, and often eking out an inadequate salary by non-scientific work. That is a most effective system for discouraging research. (Mitchell 1926: 606)

Eight Europeans were ‘drop-outs’, so to speak, but only two Americans, both of them female (Rose Malmud, a teacher, and Mildred Mudgett, marginalized as a social worker), and two others rose to top positions at the margin and outside of the world of science, respectively: Thomas Martin as a collaborator of the world’s biggest library, the Library of Congress, and Hutzel Metzger as a manager in the emerging non-profit sector.

Completing this survey of the success of the first cohort of American fellows, there are Simon Kuznets, who was awarded one of the first Nobel Prizes for economics in 1971, and Dorothy Thomas, one of the leading demographers and sociologists of her country and the first woman to be elected President of the American Sociological Association. Furthermore, Luther L. Bernard and Harold F. Gosnell were prominent figures in their disciplines in their time.<sup>24</sup>

The comparison between the two cohorts of social science fellows shows how advanced the United States was, in the mid-1920s, concerning the institutionalization of the social sciences and the professionalization of the way junior scientists were trained. This is further emphasized by the fact that the American fellows hardly ever had their fellowships renewed while this was often the case for those from Europe – probably also to prevent unemployment.<sup>25</sup>

## EUROPEAN EXTENSIONS

Even before there could be any conclusive experiences with the first fellowships, the Memorial extended its European programme to other countries. In the summer of 1925, another American professor, historian William E. Lingelbach (1871–62), was sent to Europe on a half-year exploratory journey. In the fall of the same year, as a result of his report and recommendations, the circle of national advisors, or representatives, was extended to Italy, the

Netherlands, Sweden, Norway and Denmark. In the following years, professors Luigi Einaudi (1874–1961), Torino, Johan Huizinga (1872–1945), Leiden, Gösta Bagge (1882–1951), Stockholm, Fredrik Stang (1867–1941), Oslo, and Hans V. Munch-Petersen (1869–1934), Copenhagen, were to various degrees involved in the development of the fellowship programme. They were about the same age as the advisors who had been previously nominated, and two of them later became very prominent: cultural historian Huizinga, for his reinterpretation of the late Middle Ages; and Italian economist Einaudi, in the 1930s, as a theorist and editor of a journal that received attention beyond Italian borders, and after 1945, due to a political career that culminated in his being elected President of the Italian Republic. In the 1920s, the advisors as well as other European scholars were invited to the United States, as Special Fellows, for stays of various lengths.

In the five years in which the Memorial was responsible for the fellowship programme, a total of 178 social scientists were selected. The largest group came from Great Britain (53),<sup>26</sup> followed by Germany (25), France (19), Czechoslovakia (14), Austria (12), the Netherlands (9), Italy (8), Australasia (6), Norway (5), the United States (4) and 12 other countries.

In the context of the later expulsion of Jewish scholars from the German Reich and from those parts of Europe over which it gained control, it may be of some interest that, for its internal evaluations, the RF also analysed the religious affiliation of their fellows. According to this analysis, 60 per cent were Protestants, 22 per cent were Roman Catholics and only 5 per cent were Jewish; 16 fellows provided no information on their religious affiliation, and one fellow declared himself to be an 'agnostic Jew'. The small number of Jews is most likely explained by the fact that this count covered religious rather than racial affiliation such as it was later invented by the Nazis.<sup>27</sup>

## A BRAIN DRAIN, INC.?

From the very beginning, the goal pursued by the Memorial officers had been to provide their fellows with the means to acquire additional expertise that would benefit their home countries. Enticement policies were neither advocated nor practised. However, return to the home country was a problem that kept worrying the Memorial, and later the RF. After two years, Ruml declared himself satisfied with the European programme, but had to admit that 'very few of the men have returned to their countries'.<sup>28</sup>

Analysing the fellows' migration flows, three periods can be defined and documented with data, each of them governed by different marginals. The first period spans the years before 1933. During this period, most of the fellows were nominated by the national representatives. The following period covers the years till the outbreak of the Second World War, which was also the end of the European fellowship programme. During this period, the officers of the RF Paris Office, former fellows and other advisors became more and more influential. Therefore, these two periods provide an excellent basis for a comparison of selection procedures. The third period, then, covers the years from

the end of the Second World War until the 1960s, when the fellowship programme was discontinued.

The brain drain into the United States already started before the Nazis seized power. The Great Depression after 1929 may have played a certain role in this, but cannot explain the exodus of those Memorial fellows who left their country before 1929. Czechoslovakia, Sweden, Italy, Denmark and Poland had virtually no losses by migration before 1932. The reason why there was no such exodus despite the fact that these states boasted large numbers of nominations must be sought in their structural and cultural conditions. The young states of Czechoslovakia and Poland appear to have offered job opportunities for their social science elites just as the two Scandinavian states did. Italian fellows struck the RF officers as particularly attached to their home country. And apparently, Italian fascists did not drive out their intelligentsia.

The United States and Switzerland benefited from migration which, in the latter case, was oriented to the Geneva-based institutions around the League of Nations rather than the Swiss universities. The only genuine migration magnet was the United States with at least twenty-one fellows (or 5 per cent of all fellows) going there to stay even before 1932.

The countries that show definite migration losses can be divided into two subgroups. On the one hand, there are the peripheral nations of the Anglophone world, such as Australia, New Zealand and Canada, where there were no language barriers for those fellows who were willing to migrate. On the other hand, the great traditional science nations of Great Britain, Germany, France, and the two successor states of the Habsburg Empire, Austria and Hungary, had to cope with an exodus of thirty fellows, which amounts to one out of seven nominees from these countries.

Interpretations of the impact this 14 per cent migrant drain may have had on the leading European science nations should nevertheless not be pushed too far. In the following, we will show how the migration profile changed during the next period, in which the pull of the new centre of the global science system was amplified by the push of political and economic factors. Before this, however, we will provide a first comparison between the two groups of German-speaking fellows.

## COMPARISON OF GERMAN-SPEAKING FELLOWS, I

Between 1924 and 1928, Pribram was the only representative in charge of the selection of Austrian fellows; later, he had to submit his proposals to the RF Paris Office, where they were acted upon in most cases. During the first five years he nominated four economists, four political scientists, one historian, one criminologist and one psychologist.<sup>29</sup> Pribram was comparatively misogynous: only two of the eleven nominees were women.

The German fellows who were selected by the mandarin Committee during the same period included thirteen economists, four sociologists, four historians, one political scientist, one anthropologist, one geographer and one psychologist. Only four of them were women.

Research topics as reported in the printed list of fellows are in general not very instructive. German economist Gertrud Beushagen, for instance, wanted to study the political organization of the English labour market and the problem of unemployment; Vienna criminologist Edgar Foltin was interested in the psychology of crime; and economist Andreas Predöhl wanted to analyse industrial centres and industrial development in the United States.<sup>30</sup> In spite of these rather frugal data, an attempt will be made to see if anyone, and if so who, lived up to the Memorial's expectations. Among the various criteria that were defined at various periods, there are three that can be put to the test by such data as exist. First, it should be possible, by considering entire career paths, to identify those fellows who rose to a leading position in their disciplines, or countries. To rule out idiosyncratic assessments, the opinions of competent members of the respective disciplines will be relied on as a complement to the views voiced by the RF officers. Inclusion in biographical directories and individual entries in encyclopedias provide something of a measure for the prestige accorded to potential candidates by competent members of their disciplines.<sup>31</sup>

Second, research topics pursued during the fellowship and subsequent publications should allow for an assessment of whether the fellow in question adopted the realistic, or inductive, style of research advocated by the Memorial.

Finally, the sponsor's satisfaction with a fellow's achievements can be used as a third criterion. Observations noted by the Foundation itself provide a kind of ranking of fellows during or after their research stay (there does not seem to have been a consistently applied system to determine the ranking, but nonetheless it provides a great insight into the RF's considerations). This is especially obvious in the positive case where the fellowship is the beginning of a more long-term cooperation with, i.e. support by, the RF.

The group of twenty-five German and twelve Austrian Memorial Fellows during this period included two Vienna economists, Oskar Morgenstern (reputation score 313) and Gottfried Haberler (reputation 316), who later enjoyed an unquestionable reputation as highly productive and innovative scholars in their field, while no such thing can be said for the thirteen German economists.

Among the political scientists, top ranking no doubt goes to Erich Voegelin (reputation 92) whose influence in the field of the theory of politics is, at any rate, superior to that of the other fellows from political science. Otto Vossler (reputation 11), who obtained his doctoral degree in Munich, and Erich Hula (reputation 10), born in Vienna and a former assistant of Hans Kelsen in Cologne, held professorships at important universities (Leipzig and Frankfurt and the Graduate Faculty of New School, respectively), but do not come near to Voegelin in terms of status. Vienna-born Peter Kuranda died very young, and Elisabeth Ephrussi-Waal's scientific career ended with her fellowship.<sup>32</sup> None of the political scientists can be said to have adopted the new behaviourist style of research.

Among the sociologists, high achievement was shown by Rudolf Heberle (reputation 11) in the field to which he had already turned during his research

stay in the United States – regional mobility – and by Gerhard Mackenroth (reputation 5), who did not leave Europe during his two-year fellowship because ‘his German professor felt strongly that he should be on hand when the decision [on his *Habilitation*] was made’.<sup>33</sup> Mackenroth, whose English was excellent,<sup>34</sup> had studied under Gustav Cassel in Sweden, among others, and wanted to go to the United States for a third year to pursue his studies with Chicago economists Frank Knight and Jacob Viner. Nothing came of it because his application for a third year was rejected. Heberle’s and Mackenroth’s style of research was empirical, but not very advanced.

Fellows from disciplines that had been rated of secondary or tertiary importance by the Memorial were historians Alfred Vagts (reputation 112) and Dietrich Gerhard (reputation 28) and, from Austria, Friedrich Thalmann (reputation 0). At the time of their fellowships, they were projecting work on issues that can only be described as highly conventional. Gerhard, who had been nominated by the editors of the journal *Historische Zeitschrift*, planned to work on ‘England and Eastern Europe at the beginning of the Oriental question 1774–98’. Vagts, who came from Mendelssohn-Bartholdy’s institute and had already been to Yale as an exchange student in 1924/5, did research on the ‘Diplomatic relations between America and Germany between 1888 and 1906’, and Thalmann dedicated his fellowship to the issue of ‘Relations between Great Britain and Germany in the 19th century, with special consideration of naval construction in both countries’. While Gerhard and Vagts had to emigrate after 1933, Thalmann stayed in Vienna where he was awarded a ‘grant-in-aid’ by the RF as a follow-up measure subsequent to his fellowship. All three historians were far from approaching historiography in a social science perspective. Gerhard gained a certain influence in the field on his return from his American exile and was appointed Director of the Max-Planck-Institut for History. Vagts did not return from exile.

The further career of Charlotte Bühler has already been described above. After her escape to the United States, it took a long time for her to regain her leading position as the mother of humanist psychology.<sup>35</sup>

Haberler and Morgenstern had definite leadership roles in their field, while Mackenroth, Predöhl (reputation 8), Gerhard and Vossler may even be credited with national leadership. But since assessments such as these are invariably biased, we will instead turn to the question of which of the fellows were deemed worthy of further support by the RF.

Interestingly, Haberler was not included in the group of those who were granted further aid, which may be due to the fact that after a short interlude at the League of Nations, he began teaching at Harvard University in 1936 and no longer needed support. Hula, by contrast, repeatedly needed help by the RF – and was granted aid to a satisfactory degree, first in Cologne and then on his way to exile. Morgenstern’s fellowship card reports an even longer cooperation with, and long-time material support by, the RF. During his activities as director of the Vienna Konjunkturforschungsinstitut (Institute for Business Cycle Research) he benefited from an institutional grant (see Chapter Three),

and after his escape from Vienna, his post at Princeton University was subsidized by an annual contribution of, initially, \$2,000 (\$30,000 in 2010) and, later, \$2,500 (\$36,000 in 2010). After the end of the Second World War, he was granted a number of smaller travel allowances by the RF.

Voegelin is another Austrian long-term recipient of grants-in-aid and refugee relief funds. In October 1933, a RF officer had a 'long talk with Voegelin' which resulted in the following rather disconcerting diary entry:

He has completed a two-volume History of Race Theory which is being published by a German firm, on the direct insistence of one of the high Nazi university professors. While the book is not orthodox in the Hitler sense it at least takes the race conception seriously as a political factor. It may lead to V[oeogelin]'s being called to Germany. He would accept despite preference for Austria.<sup>36</sup>

One year later, Voegelin's preferences had gained acceptance, and he submitted an empirical research project to the RF. Since he also referred to its empirical nature in the presence of another American visitor, the project was in all likelihood not conceived as such exclusively out of deference to the RF's well-known preference for 'inductive' research.<sup>37</sup> Voegelin wanted to study the 'social transformation which the war and the post-war situation has imposed on the City of Vienna'.<sup>38</sup>

Still in Vienna, Voegelin had received a monthly \$50 (\$770 in 2010) for a total of twenty months, and after his escape to the United States he was included in the relief measures for refugees from Hitler and received generous aid. After his return to Europe he seems to have become accustomed, so to speak, to this regular flow of money. In November 1958, a RF officer who went to see him in Munich reports:

[Voegelin is to me not] a prepossessing person. He seems to think in devious and politicking terms as though the devious were the inevitable. Doubtless, too, he is not characterized by complete selflessness. Even so, his idea that the academic powers in Germany must be presented with a *fait accompli* which they will then undertake the support of may make sense if one is willing to run the risk.<sup>39</sup>

One-and-a-half months later, the support of many years ended with the RF's refusal to subsidize the Munich Institute for Political Science that Voegelin was about to set up.

In continuing its support for Bühler, Hula, Morgenstern and Voegelin even after their fellowships terms had expired, the Memorial had focused on those Austrians among the German-speaking social scientists who had already benefited from privileged treatment. Three of these four could rely on further support after their escape to the United States, which highlights the close link between prior reputation and differential opportunities for establishment.

The German Memorial fellows also include refugees from Hitler, but not all of them belonged to the group of those who were supported by the RF. In the



case of anthropologist Paul Kirchhoff (reputation 0), for instance, the RF took an active interest for quite a time, but provided only short-term material support. Kirchhoff, who between 1928 and 1930 had used his fellowship to study anthropology under Alfred Kroeber, among others, had not returned to Germany after graduating but had tried to establish himself in Britain. Research stays in Africa and Australia were refused by the British authorities, which the RF files attribute to unfounded rumours about Kirchhoff's alleged link to the Communist party. Kirchhoff was subsequently awarded a small RF grant, beginning in October 1932, to finish a publication. When the grant ended, he and his wife subsisted on her earnings as a model. Later Kirchhoff tried to get a job in Paris, but failed for lack of funds. Despite the Polish anthropologist Bronislaw Malinowski's intercession and the RF officers' explicit willingness to come to his aid, no further support was granted. After a temporary stay in New York in 1935, he resurfaced in Mexico where he had found work in a museum. Kirchhoff later managed to obtain an appointment to a university and excelled as an anthropologist of Mesoamerica – a term he is said to have coined.

Rudolf Heberle was similarly dependent on long-term aid. While still in Kiel, he was awarded a generous grant-in-aid of \$2,500 (\$38,600 in 2010). He had expected to be able to survive as a lecturer in Nazi Germany, forfeiting hopes of promotion, but to his surprise was nominated for a professorship by the University of Kiel. This did not come off since proof of his Aryan descent was required and it turned out that a great-grandmother of his had been Jewish. Still, he considered staying in Germany and cherished hopes of getting a position within the framework of the Four-Year Plan (the Nazi party's framework for invigorating industry and infrastructure). Heberle adaptation to the new situation continued to the point of joining the Nazi brownshirts (Sturmabteilung or SA), a step he tried to conceal after his emigration.<sup>40</sup> In the summer of 1937, thanks to a small RF grant, he arrived in New York. In the following year, again thanks to a RF subsidy, he started teaching at Louisiana State University.

Sociologist Clara Liepmann (reputation 0) escaped to the United States where she worked for a certain time as a research assistant at the Russell Sage Foundation. After the end of the Second World War, she and her husband returned to Germany. Hans Staehle (reputation 78) and Alfred Vagts are another two refugees whose fellowship cards report contacts, but no material support. While Staehle did not depend on this because he worked, first, in positions close to the League of Nations and, later, in the world of the United Nations Organization, Vagts confined himself to the role of a private scholar, with only a short interlude as a guest professor at Harvard in 1938/9 and a membership at the Institute for Advanced Study in Princeton (1939–42). As the son-in-law of the famous (and well-to-do) American historian Charles A. Beard, Vagts could afford to dispense with academic positions. Until old age, he devoted himself to his historical studies on international relations, international law and the military (Vagts 1979; Epstein 1993).

Money has the well-known advantage of providing a simple measure for value. By applying this measure to the group of German and Austrian fellows who were supported by the RF after their fellowships had ended, one can assess their importance as perceived by the various foundation officers. Morgenstern's case can be taken as an illustration that scholarly greatness is not to be had without material foundations. In March 1938, he happened to be abroad and decided to stay there. Since he was not Jewish but the son of an illegitimate daughter of the 'Hunderttagekaiser' (the 'Emperor of the 100-day rule'), Frederic III, survival in the Third Reich would presumably have been possible. But then, 'Theory of Games and Economic Behaviour' would never have been published – given that it was at Princeton where Morgenstern elaborated this founding manifesto of game theory, in collaboration with John von Neumann who in the 1920s had studied in Germany due to a grant by the International Education Board. The book was not published before 1944, and even then only because the RF subsidized the printing costs.<sup>41</sup>

While Vienna representative Pribram had succeeded in finding promising candidates beyond the boundaries of his own field – seven of the eleven nominees were appointed to professorships at renowned universities – the group of the first German fellows turned out to be just average with respect to their future scientific productivity as well as to the positions they could secure. Only ten of the German fellows obtained professorships. The reasons for their relative insignificance can be found in a diary entry by John Van Sickle concerning a conversation he had had with Schumpeter in London in the autumn of 1931, in which the latter complained at some length about the activities of the German Committee:

S[chumpeter] is frankly dissatisfied with the fellowship administration in Germany. He feels that there are so many factions, especially in Economics, and that the gaps between them are so great, that it is impossible for a German to be impartial. Specifically he alleges that Schumacher who represents Economics on the German Committee, is so bitter against Spiethoff, Schumpeter's colleague, that candidates from his camp have no chance with the Committee. He cited one recent example without mentioning the name. In general he feels that the school of Mathematical Economics which he represents and which is so strongly developed in the States, cannot get a hearing in Germany because the older economists do not understand it and cannot appreciate its significance.<sup>42</sup>

In the same year, Morgenstern provided a further explanation for the difference between Germans and Austrians by pointing out to Tracy Kittredge that: 'a number of men who hesitated to accept fellowships for fear of losing their positions, are still where they were some years ago, while other men with less prospects at the time, but who had fellowships, have since been advanced to Professorships'.<sup>43</sup>

In this perspective, the situation of the Austrian candidates who otherwise could not have hoped to get a university position seems to have given them a relative competitive edge over those who preferred the limited appeal of

domestic security to the uncertain promotional openings of university studies abroad. Resemblance to the pattern of 'economic backwardness', as identified by Alexander Gerschenkron (Gerschenkron 1962), is obvious – and not very surprising given that Gerschenkron was one of the collaborators, before 1938, of the Konjunkturforschungsinstitut (Institute for Business Cycle Research) directed by Morgenstern. (This is not to say that Morgenstern was at the origin of Gerschenkron's insights but that there was some family resemblance between their analytical tools.)

The German mandarin Committee was obviously not capable of, or not interested in, finding the best junior scientists and nominating them as candidates for a fellowship; instead, they were content with rewarding their clientele.

## THE FELLOWSHIP PROGRAMME IN THE SHADOW OF NAZISM

In 1929, the reorganization of the Rockefellers' philanthropic activities became effective, and the foundations that had been independent were united with the 'mother foundation'. Administration of the fellowship programme was now a direct responsibility of the five RF Divisions (Public Health, Medicine, Natural Sciences, Social Sciences and Humanities)<sup>44</sup> or of its Paris-based European office. Although the advisors retained their functions until 1935 (in Great Britain and Germany until the end of 1936), decisions were, from 1935 onwards, increasingly made by the Paris RF officers.

RF policies in the European social science field were determined by two new officers: John V. Van Sickle, Assistant Director of the Social Sciences from the autumn of 1929, and Tracy Kittredge who was appointed his assistant two years later. Initially, Selskar M. Gunn, long-term officer at the Rockefeller foundations, had a strong say in the matter since between 1930 and 1932 he acted as the Associate Director for Europe of the Social Sciences Division.<sup>45</sup>

Van Sickle was no longer young when he obtained his degree in economics from Harvard, but this was because the First World War had interrupted his education, which had started in his home state of Colorado. After the end of the war, he lived in Vienna for a couple of years, first as a collaborator of the American Un-Official Delegation of the Austrian Section of the Reparations Commission and then, for three years, as an advisor to the Austrian government. A result of his stay was his doctoral thesis on 'Direct Taxation in Austria', published in 1931 in the Harvard Economics Series. In the same year, he started teaching as an economist at the University of Michigan, which he left four years later to take up his RF position. He retained this function for the next ten years, initially in Europe. When his RF activities ended, Van Sickle settled in the American South where he taught as a professor of economics, first at Vanderbilt University and then at a smaller college.

Tracy Kittredge, who was one year his senior, had studied anthropology, history and political science at Oxford and had transferred – after a brief teaching spell and work as a statistician and archivist in the United States – to Europe where he worked first as an assistant to the General Director of the League of Red Cross Societies and, later, as its General Secretary. Besides his enthusiasm for

writing reports and detailed memoirs decked out with statistics, he evidently also possessed diplomatic skills. At any rate, he served the RF for ten years.

The transfer of decision-making power in matters of fellowships to these two Americans who were little known in Europe met with resistance from the former fellows. This minor controversy illustrates well the difference between the science cultures of the old continent and the New World. In identical letters, fellows and ex-fellows appealed to the President of the Foundation, voicing their belief that it was impossible for anyone ‘who does not know (the candidates’) relative value, who has not followed them in their past academic work, who has not tested their capacities for real and earnest study’ to ‘make a good choice’. Foundation officers, they argued, were unable to come to a ‘comparative judgment upon all applicants’, and their decisions risked being influenced by the ‘too frequent rivalries unknown to the outside world’.

In his answer, RF President Max Mason,<sup>46</sup> refusing particularist concerns, wrote: ‘It is the custom in all of the fellowship programs to secure complete and adequate opinions on any application which is received. The officials of the Foundation will ... endeavour to keep in close touch with the outstanding scholars in this field, and particularly with the past fellows of the Foundation.’<sup>47</sup>

The group of those who had written from Europe included Austrian, French, Italian, Czech and Slovak fellows or ex-fellows, among them Morgenstern, Heberle and, interestingly, Mario Einaudi, the son of the Italian RF representative. Some of those who wrote were already full professors. What is remarkable about the letter is that its authors implicitly attribute their own nomination to the paternalist care of their elders rather than to their own achievements. The clientelism that predominated in continental European science prevented them from seeing themselves as winners in a fair competition. Was it a mere accident that there were no British fellows among the group of sceptics? Or was the fellows’ massive protest also a reflection of the norms of a stratified, hierarchical world that knew neither fair play nor the idea that later would be described by the – initially ironical – term of meritocracy?

The RF officers tried to keep track of the success of their fellows, as shown by the diaries and fellowship cards that were so painstakingly updated. From time to time, they produced summary reports for their New York superiors. Given their audience, they may well have tended to overstate success rather than failure. In one of their reports, ‘Social Science Fellowship Program in Europe – Rockefeller Foundation 1924–1938’, different groups of countries are explicitly compared. For its author, presumably Kittredge, two aspects were important: academic success and migration.

In the British isles and the Commonwealth, which together had nominated the largest number of fellows, ex-fellows initially rather strongly tended to exit from academic life, a tendency which, however, had been stemmed since 1929. In the late 1930s, when the report was written, the picture had changed, with the vast majority of British ex-fellows having obtained positions at universities or non-university research institutions. For Britain and Ireland, the rate of academic success, i.e. of ex-fellows having obtained positions in universities or research

institutions, rose from 71 per cent between 1924 and 1933 to 81 per cent between 1934 and 1938.

In France, the situation was unsatisfactory during the first five years, since only one-third of the fellows succeeded in the academy. This was attributed to the system of *concours d'agrégation* which seems to have implied that anyone aspiring to a professorship had better stick to the 're-formulation of traditional knowledge, which in turn, it was argued, may explain why in the 'past generation' only a small number of French economists 'have enjoyed international reputation for their personal contribution to knowledge'. If the rate of academic success was meagre – 37 per cent – during the first five years, it rose to 64 per cent in the next five years and to 67 per cent between 1934 and 1938.

In the Scandinavian countries, the fellowship programmes turned out to be very satisfactory. Of the 59 fellows, 13 were professors by the time of the report, 16 others held other university teaching positions and 4 former fellows had positions abroad. Thus, the rate of academic success had risen from an initial 59 per cent (1924–28) to 76 per cent (1929–33) and, finally, 92 per cent (1934–8). The great majority of Scandinavian fellows were economists, among them two future Nobel laureates, Ragnar Frisch and Gunnar Myrdal.

For the Central European countries, only part of the analysis was done. The text is less detailed than the others but nevertheless includes contemporary qualitative assessments. In all three successor states of the Habsburg Empire, there had been a surplus supply of qualified individuals, but only four ex-fellows had managed to obtain a professorship: anthropologist Jirí Malý in Prague, political scientist Zdenek Peska in Bratislava, agrarian economist Jan Dokládál in Brno and economist Theodor Suranyi-Unger in Szeged.<sup>48</sup>

One third of the fellows appointed from Central Europe are now established abroad, 17 in the USA and 7 in Europe. This large proportion of emigrants is due partly to the uncertainties of academic careers in these countries, and partly to political developments. Thus, for example, of 29 Austrian fellows, 16 are now abroad (of whom 12 had emigrated before the Anschluss) ... The individual capacities of these fellows are in many cases of such recognized distinction that many of them have had no difficulty in obtaining university or other appointments abroad.<sup>49</sup>

The author of this report does not get around to the analysis, announced in the opening part of the report, of the German and the Italian fellows. We will try to at least partially make up for this by providing an analysis similar to the one presented for the Memorial fellows. But before doing so, we will address the issue of how the handing-over of power in Germany to the Nazis impacted the fellowship programme.

As early as in the spring of 1933, Van Sickle voiced concerns about the three main German RF confidants, Bernhard Harms, Ernst Jäckh and Alfred Weber, who had been dismissed from their functions.<sup>50</sup> The issue of whether the German advisory committee should remain in office was discussed at some length between the New York headquarters and the Paris Office. Change for reasons of age seemed to suggest itself, but was put off in order to avoid giving

the wrong signals. Furthermore, there was concern about the dramatically reduced opportunities for Jewish candidates to be nominated by the German committee. In his talks with Kittredge in January 1935, exiled former German advisory committee member Mendelssohn-Bartholdy advocated discontinuing the fellowship programme in favour of supporting ex-fellows who had fallen into disgrace in Germany or had left the country. In the spring of 1935, the RF finally decided to discontinue cooperation with Dr August Wilhelm Fehling, the Secretary of the German committee, by the start of the coming academic year. This did not mean that the allocation of fellowships also stopped, but the few individuals who were awarded fellowships after that were no longer in Germany when they were nominated.

## COMPARISON OF GERMAN-SPEAKING FELLOWS, II

Between 1929 and 1941, all told, seventeen Austrian and fifty-three German social scientists were selected for a fellowship; three of the Germans were already in exile when they took up their fellowships: Albert O. Hirschman, Jakob Marschak and Svend Riemer. In the 1950 Directory, their countries of residence are correctly reported as the United States, England and Sweden, so they are most likely to have been classed accordingly in the internal analysis.

What immediately strikes one is the proportion of Austrians and Germans. If the high proportion of Austrians in the cohort of the Memorial fellows could be attributed to the fact that the RF Vienna representative, Pribram, showed more initiative than the German committee, this explanation is ruled out after 1929. There were now too many actors involved in the selection process to allow for the initiative of any one of them to be decisive.

The unequal distribution stands out even more clearly if, for the sake of comparison, the number of fellows is referred to the population from which they were recruited: students and faculties. In 1930, the 23 German universities had just under 100,000 students for about 5,000 faculty members. At the same time, Austria's three universities had about 15,000 students for almost 1,500 faculty members whose supervisory load was, thus, only half of what their German Reich colleagues had to shoulder.

Further information can be obtained by comparing the university sites in the German-language area to the proportion of fellows coming from them. Had the fellows been justified in raising the alarm about reorganization and the disempowerment of national advisors? Six German universities (among them Breslau, Göttingen and Münster) 'produced' no fellows at all, and the minor Austrian universities had hardly any effect on the overall picture. In the two periods between the two world wars that are considered in the present context, twenty-three future fellows had graduated from the University of Vienna. This was the largest number, by far, in the whole German-language area. Berlin was second with twelve future fellows, followed by Hamburg (eight) and Heidelberg, Jena, Kiel and Munich (seven each). The order does not reflect the distribution of students across universities.

In the years after 1929, the RF had partly revised their goals for their fellowship programme. Not least as a reaction to the Great Depression, new priorities for support were set that privileged research projects aiming at the 'stabilization of economic processes' and this priority was also, at least partially, binding for the fellowship programme. A second priority aimed at improving research on international relations. As a parallel to the setting of priorities, the policies initiated by Ruml, which aimed at the systematic advancement of social science research centres, were maintained until 1934. This also included awarding fellowships to a number of junior scholars. The third change concerned the 'maturity' of the persons eligible for a fellowship. Support for very young scholars who sometimes had not even graduated was discontinued because the new priority was to reach those who, in the case of Germany and Austria, could be described as '*Habilitanden*', i.e. as junior scholars preparing their qualification for a teaching career in higher education.

The mean age of 'ordinary' fellows was thirty-two years when they took up their fellowship, while their predecessors from the Memorial period had been four years younger, on average.<sup>51</sup> Average time since their doctorate, which all fellows were supposed to have obtained, was seven years, in which some of them had taken their first steps towards an academic career or, like most Vienna fellows, had made do with marginal jobs or even, as noted on Gustav Seidler's fellowship card, had stayed 'at home' and had made a living by doing translations.

For the purpose of comparison, but also because the new RF officers did not differ from their predecessors in this point, the same three criteria for assessing the success of the fellowship programme shall be used for the period between 1929 and 1941. Who succeeded in obtaining a leading role on a disciplinary or national level? Had the 'new' style of doing research been learned or adopted? And did the RF look upon their achievements as justifying further support?

Among the economists, Fritz Machlup (reputation 389), Albert O. Hirschman (reputation 374) and Jakob Marschak (reputation 90) gained the highest reputation in their discipline, followed – at a considerable distance – by two Germans, Erich Schneider (reputation 29) and Friedrich A. Lutz (reputation 16), and two Austrians, Paul Rosenstein-Rodan (reputation 24) and Gerhard Tintner (reputation 22).<sup>52</sup> All of them can be said to have fulfilled the second criterion, enriching economic research by their empirical and theoretical contributions. Was this also due to the fact that they all belonged to the group of those who were in exile during Nazi rule? No answer to this question even remotely meets the prerequisites for a causal explanation. Did creative individuals tend to be first in going into exile, or was the creative potential of the 'home-guard'<sup>53</sup> so deeply impaired by the system of bribery and terror established by the Nazis that it could not be regenerated after liberation from dictatorship? Were there (sub-)cultural commonalities among the productive social scientists who became outlaws at the will of the Nazis which they were not even conscious of, since they no longer wanted to be Jews? Or had, inversely, the experience of a liberal culture, a life in freedom, the friendly welcome by colleagues and other

conditions not only allowed them to enjoy the fact that they were still alive but also provided the necessary impulse for them to give full scope to their creative potential? Or was the relative marginality of the first years in exile, which for some of them became their second home country, an incentive to prove their worth to others or to strive even harder out of gratitude for this 'second chance' (Mosse 1991)? Regardless of which answer one would like to give to these questions, it is advisable to first gain some insight into the facts.

When it comes to international reputation, the person who is to be named first, and at a considerable distance from all the others, is Paul Lazarsfeld (reputation 959). Alexander von Schelting (reputation 0) seems to have little benefited, in a professional perspective, from his fellowship. But his situation was such that it certainly did not allow for the carefree leisure of a research stay. In June 1933, he was granted a fellowship which he was to take up in October of the same year. When he was about to leave, publication of the *Archiv für Sozialwissenschaft und Sozialpolitik* (*Archive for Social Science and Social Policy*) of which he was the editor was halted. As a result, von Schelting asked for his fellowship to be postponed since he was indispensable at the Mohr Siebeck publishing house. In December 1933, Kittredge was informed that von Schelting's *Habilitation* was jeopardized because of Carl Brinkmann's negative attitude. In April of 1934, von Schelting reported to the RF that he would come to New York in autumn. On his arrival in New York, he had to see the dentist almost immediately and was diagnosed with suppurating teeth. The RF paid the considerable dental bill of \$225 (\$3,500 in 2010). During the remaining ten months of the year 1935, von Schelting journeyed across the United States, obviously in search of a job rather than as a learner. In Wisconsin, he became interested in the life of the farmers and workers, and would have liked to study this in more detail provided the Foundation would give him an opportunity to do so. Stacy May, Assistant Director of the Social Sciences Division of the RF in New York, and in charge of the American fellows, refused to renew the fellowship and recommended that von Schelting return to Germany and reapply for a new scholarship for 1936/7. Back in Germany, von Schelting again worked at the Siebeck publishing house and reapplied for a second fellowship year in February 1936, but only one month later informed Kittredge that his situation had drastically changed for the worse. His employer and sponsor Siebeck was suddenly deceased. His openness in his conversation with Kittredge did not pay off: 'Asks for assistance in obtaining position in U.S. or for a 2nd fellowship appointment (TBK [Kittredge] replied that RF would not grant 2nd fellowship).'

Had von Schelting failed to read, or forgotten, the application rules for fellowships? The return to the home country after the end of the fellowship was not only an important point with the Foundation but also conformed to US immigration rules. Fellows were regularly informed of this before they applied. Half a year later, von Schelting was back in New York, and the RF paid Columbia University a three-year salary subsidy subject to the provision, which was usual at the time, that Columbia was expected to subsequently employ him on a regular basis. A letter from another RF fellow, Arvid Brodersen



(reputation 5), to Joseph Willits, then head of the Social Sciences Division, informed the RF on the further course of von Schelting's life: he had returned to Europe in 1939, only to be stranded in Switzerland.<sup>54</sup> His focus had meanwhile shifted towards the history of ideas. For 1951–3, he was awarded a combined fellowship by the Humanities and the Social Sciences Divisions of the RF.

Among the political scientists of this cohort, Leo Strauss (reputation 249) is the only one to come near to Lazarsfeld, Machlup and co. The case of Strauss is instructive in more than one respect. Born of orthodox Jewish parents in provincial southern Germany, he studied philosophy under Ernst Cassirer in Hamburg and worked in a small non-university, the Akademie für die Wissenschaft des Judentums (Academy for Jewish Studies) in Berlin, before taking up his fellowship. It is hard to find another fellow as contrary to Ruml's programme for the new social sciences and their practical relevance for the shaping of modern life. The fellowship he was awarded, as well as its one-year renewal, allowed him to leave Germany before power was handed over to the Nazis, and Strauss never returned. His tremendous success as the founder of conservative political theory in the United States did not come about until he was appointed to a professorship in Chicago in 1949. Until then, he had managed to stay alive thanks in no small part to repeated financial support by the RF, first in England, and from 1937 in New York.<sup>55</sup> The fact that a man of Strauss's philosophical stance was welcomed even by an institution such as the New School for Social Research in New York, which was deeply committed to the New Deal and to a liberal worldview, is one of the peculiarities of the period. Arguably, it cannot be understood unless one takes into account the mutual encouragement of immigrant solidarity, on the one hand, and the willingness to help, which sometimes changed to admiration, of the American intellectuals and their internationalist outlook in the New Deal period, on the other hand. More recently, Strauss has gained dubious posthumous celebrity since he was a mastermind for and a teacher of the ideologues of President George W. Bush. Unsurprisingly, his complex early years are not adequately appreciated in the many publications dedicated to his person and his work.

When it came to helping a brilliant man continue in his work, the RF officers more than once ignored their own principles. Their justification for this was given in a formulation by one of the legendary officers of one of the early Rockefeller foundations, Wicliffe Rose of the International Education Board; he termed it 'making the peaks higher' (Geiger 1986; Kohler 1978; Kohler 1991). However, the officers never went so far as to let their role as sponsors of the sciences be transformed into that of an uncompensated enticement or escape agent. Von Schelting and Strauss are only two cases out of a long series of former or current fellows who were supported only after they had managed to come to the United States on their own, or with the aid of some relief committee for refugees from the Nazis. What has been said with respect to Strauss is more or less true for two other German philosophers, as well: Karl Löwith (reputation 38) and Leo Kohn (reputation 5), whose research topics and later

works, while not that radically contrary to modernity and empirical social research, were not close to it, either.

As for the jurists, there is not much to say about them in the present context. The two Austrian political scientists, Josef Kunz (reputation 95), an expert in international law who would even at the time have to be characterized rather as an expert in international relations, and Joseph Fürth (reputation 15), jurist and co-founder of the Vienna 'Geistkreis' (the 'Mind Circle' of leading thinkers including Morgenstern and Voegelin), continued their careers after their flight rather on the sidelines of academia.

Siegfried F. Nadel (reputation 69) as well as Christoph Fürer-Haimendorf (reputation 15) had remarkable careers. They studied in London under Malinowski before pursuing field studies in Africa and India, respectively.<sup>56</sup> Nadel was definitely quicker than the conservative Fürer-Haimendorf to break away from the habits of thought of the Vienna teachers and their 'Kulturkreis' school doctrine. Both became professors at important universities (cf. Fürer-Haimendorf 1990; Spencer 2000).

As for the future cultural anthropologists, London certainly suggested itself as the place to go, but still the Eurocentrism of some of the German fellows is somewhat surprising. Only one out of two Germans in this cohort went to the United States to study or do research, in contrast to two out of three Austrians. That German economists wanted to study public finances in Italy, for example, was not necessarily due to any emulation on the individual level of the political affinity between the two politically converging dictatorships, as is shown by the tragic case of Gerhard Dobbert. Dobbert was arrested during his fellowship and committed suicide after his release. As for two other economists who went to Italy for their studies, Harold Fick and Woldemar Koch (both reputation 0), 'family resemblance' in politics is what seems to have motivated their choice of country. The same is most likely true for Hans Freyer's assistant Franz Haufe (reputation 0) who was drawn to Romania where he wanted to study the life and culture of the peasants under Demetre Gusti, later discredited because of his Nazi sympathies. At least eighteen Germans were found, according to RF information, to have held positions in Germany during the Third Reich and to have in most cases been able to keep them after the war had ended. The proportion of Austrians who survived dictatorship and war in the 'Volksgemeinschaft' (people's community) was one out of five and, thus, considerably lower. There was only one, if at all, among them who exposed himself as a Nazi partisan. And conversely there was no one equal to August Lösch (reputation 27), as an outright opponent to the Nazis, who died in 1945 from exhaustion due to repeated imprisonment.

With the political situation in Germany steadily deteriorating and the willingness of the RF to support German institutions and fellows similarly on the decline, measuring satisfaction with the work of the fellows in this cohort, as expressed by subsequent 'grants-in-aid', is not possible without certain reservations. Some of the emigrants who had received RF support have already been mentioned. Among those who had stayed in Germany, only economists

**Table 2.3** Country of Residence, 1970, of RF Fellows, Nominated before 1941 and after 1947 (%)

Country of Origin before Second World War	Country of residence 1970						Total (n)
	G	A	Switz.	USA	Other EU	Rest of the World	
Germany	63			20	9	7	64
Austria		24		68	4	4	25
Switzerland			67	33			6
Others		6	6	65	18	6	17
<b>Total</b>	34	7	5	41	9	6	102
<b>%</b>	33	7	5	40	9	6	100

Source: Rockefeller Foundation 1951; Rockefeller Foundation 1972 (author's calculation).

Hermann Bente (reputation 2) and Leo Drescher (reputation 1), both associated with the Kiel Institut für Weltwirtschaft (Institute for the World Economics), received support between 1934 and 1936 (Drescher emigrated to the United States in 1939). After 1945, economists Walther Hoffmann (reputation 82), Friedrich Lutz (reputation 16), Erich Schneider (reputation 29) and historian Egmont Zechlin (reputation 19) received financial aid.

Three Austrians were granted RF subsidies before their emigration in order to complete the studies they had initiated during their fellowships: Leo Gross (reputation 82), Josef Kunz (95) and Paul Rosenstein-Rodan (24). Among those who were supported after the end of the war were criminologist Roland Grassberger (reputation 2) and Ernst John (reputation 0), one of the leading figures at the Vienna Wirtschaftsforschungsinstitut (Institute for Economic Research) at the time. Aid was also granted to Christoph Fürer-Haimendorf and Leo Gross. Paul Lazarsfeld's Radio Research Project, which will be dealt with in more detail in Chapter Five, was better endowed by far.

I do not know how to explain the small proportion of women in this cohort. The fact that there are only four women among the seventy fellows can therefore only be mentioned as such without elaboration.

The concern voiced by the former fellows in their letter to the President of the RF, i.e. that the elimination of national advisors would lead to unfavourable outcomes, was obviously unfounded. On the contrary, it was only when RF officers were increasingly involved in the selection of the fellows that outsiders such as Hirschman and Strauss seem to have stood a chance at all. Similarly, in the Austrian case, the former fellows seem to have had a positive influence on the quality of the nominees. As a procedure, applying impersonal criteria, on the one hand, and soliciting opinions on candidates by as many persons as possible, on the other hand, was clearly superior to the particularistic policies of the German mandarin Committee. A final overview of the fellows of the post-Second World War period may yield some further insights.

**Table 2.3** (Continued)

Country of Origin after Second World War	Country of residence 1970						Total (n)
	G	A	Switz.	USA	Other EU	Rest of the World	
Germany	86			9		6	35
Austria		82		9		9	11
Switzerland	22		67			11	9
Others	17			17	33	33	6
<b>Total</b>	33	9	6	5	6	6	61
<b>%</b>	54	15	10	8	10	10	100

### COMPARISON OF GERMAN-SPEAKING FELLOWS, III

One hundred and ninety-three individuals could be included in the following analysis; a few of them were awarded RF fellowships more than once (which will not be detailed in the following analysis). Gender proportions are massively one-sided: after the Second World War, only three female fellows (economists Eva Bossman and Elisabeth Liefmann-Keil, and sociologist Renate Mayntz) were selected, whereas in the interwar period, which was generally less favourable to female scientists, there had been nine female fellows.

In the prewar period, 23 per cent of all fellows came from Austria, 58 per cent from Germany and 4 per cent from Switzerland. In addition, there were 19 other fellows (14 per cent) who were classed with other countries but who had scientifically relevant links to Germany or Austria, either as students or graduates from a university, or because this was where their workplace was. In the postwar period, the number of Austrian fellows plummeted from 30 to 11, reducing the proportion of Austrians to 17 per cent. A contemporary observer and ex-fellow laconically diagnosed the Austria of the 1950s as having 'no brains, no initiative, no collaboration'.<sup>57</sup> While 'no brains' is not borne out by a detailed examination of the 11 RF fellows, it is certainly true that, in terms of reputation, the majority of Austrian postwar fellows did not come near their prewar compatriots (see Chapter Four).

The available data allow for some insights into the further careers of the fellows. Table 2.3 lists those who were alive in 1970 and whose place of residence was known to the RF. A comparison of the country of 'origin' and the country of 'residence' clearly shows that Switzerland had the same proportion of returns for both periods: two out of three fellows from Switzerland had returned to live there. For the two German-language countries, a quite different picture emerges: almost two out of three prewar fellows had returned to Germany, in contrast to only one out of four Austrians who had lived abroad for at least one year before the 'Anschluss'. Of the twenty-five Austrians on whom data are available, nineteen had become ex-Austrians. Whether an explanation

for this disproportionately large permanent emigration should be sought in the obvious push factor – i.e. the two dictatorships – or, rather, in the pull factors of the United States system of science remains an open question. A comparison with the eagerness bordering on localism with which Austrian postwar fellows came home (some of them even breaking off their stay prematurely to do so!) suggests that explanatory priority should be given to the immediate danger to which those who were undesirable on political or racial grounds were exposed. On the other hand, one might argue that the proportion of Jewish fellows was very small and that only few of the future fellows had been conspicuous for their political activism. Thus, considering that three-quarters of those prewar fellows from Austria who left the country for good cannot be characterized as cases of emigration due to immediate danger, explanations for this brain drain will have to be sought in the material and cultural conditions.

The European fellowship programme of the RF, both before and after the Second World War, provided openings in terms of further post-graduate studies for talented junior scientists, at a time when the Teutonic system of science offered particularly little institutional support. For the vast majority of them, these fellowships were probably what allowed them to remain in the field of science, in the first place.

All in all, the Memorial and RF fellowship programme was a great success. The selection procedures demonstrate that nomination by established committees clearly resulted in the selection of more mediocre fellows than nomination via the more complex procedure that was used after 1931. This latter procedure anticipated elements of the ‘peer review’ which proved, at least at the time, to be the better method. The foundation officers, disinterested in particularistic constraints, were able to spot a much larger number of promising candidates, and their strategy of seeking a variety of opinions turned out to be a much more effective means of evaluation. The comparison between the German and the Austrian postwar fellows, however, shows that even the best selection procedures will be ineffective when the pool from which one chooses is almost empty.

# 3

## INSTITUTIONAL SUPPORT IN EUROPE

The fellowship programme was only part of the Rockefeller Foundation's activities and, considering the sums involved, not even its most important one. From the start, Beardsley Ruml's vision was to support promising centres of social science research – 'up to a dozen or fifteen world centers for social sciences' – by providing them with funds to be spread over a period of several years that would allow them to improve their infrastructure, buy books or journals, recruit additional personnel, or release researchers from routine tasks and immerse themselves in their scientific work. Again, European institutions were among the earliest beneficiaries. A much larger share of this programme, however, was dedicated to US institutions, and negotiations between the Foundation and its US beneficiaries were much less complicated and long-winded in the homeland than in Europe.<sup>1</sup>

The European research centres that were supported as early as 1925 included, in Britain, the London School of Economics and Political Science (LSE), the Royal Anthropological Institute and the National Institute of Industrial Psychology, while beneficiaries on the continent were the Institut für Auswärtige Politik (Institute for Foreign Policy) in Hamburg, founded in 1923 by Albrecht Mendelssohn-Bartholdy who was also its director at the time, and the Institute for Social Sciences at the University of Stockholm. The Notgemeinschaft der deutschen Wissenschaft (Association for the Emergency Funding of German Science) received funding to buy books. They all were granted sums between \$17,000 and \$75,000 (\$212,000 and \$935,000, respectively, in 2010), with the exception of the LSE, which received more than double the amount of the second largest European beneficiary. Compared to the sum allotted to the University of Chicago in the same year, however – a quarter of a million dollars over a three-year period (\$3.1 million in 2010), the support granted to the Europeans was rather moderate.

On the financial level, different options were implemented: large-scale support was partly donated as an endowment, and partly provided as a basic subsidy or an overall budget that was to be spread over a previously defined period and from which funds could be drawn. American institutions were the

only ones to benefit from so-called 'matching funds', i.e. a form of funding that required recipients to raise additional funds from other sources while the Memorial committed itself to equal the sums thus raised, with a ceiling amount previously agreed upon. Owing to this arrangement, the Local Community Research Committee of the University of Chicago, for example, was able to receive \$180,000 (\$2.2 million in 2010) in extra money between 1923 and 1931 in addition to the basic support of \$451,000 (\$5.5 million in 2010) (Bulmer 1986: 141). Support schemes that spread over several years had to be authorized by the Board of Trustees (for the Memorial) or the Executive Committee (for the RF); the allocation of 'grants-in-aid' was left to the discretion of the officers.

Like the fellowships, institutional support was focused on fostering a 'realistic approach', i.e. the type of research that would later be known as empirical social research. This type of social research was expected to improve traditional philanthropy – 'an appeal to tradition, expediency, or intuition' – by providing 'more understanding': 'The interest in science [sic] was an interest in one means to an end, and the end was explicitly recognized to be the advancement of human welfare.' Sponsoring the social sciences would help philanthropy to gain 'greater knowledge as to social conditions, a better understanding of social forces, and a higher objectivity in the development of social policy.' Due to a lack of funds, research in terms of an 'immediate personal observation of the social problems or social phenomena which were under investigation' had so far failed to emerge. Therefore research had remained 'speculative, bibliographical, or merely literary in character'.<sup>2</sup>

Ruml's second concern was to encourage cooperation among the representatives of different fields. He considered the current division of the 'social sciences' – economics, political science, sociology, anthropology and psychology – to be 'artificial' and of limited efficiency for conducting 'social research'. What was needed was a 'synthesis ... from which a new and more fruitful division of labor may be achieved'. With his memorandum, Ruml wanted to counteract the emerging differentiation of the 'social sciences' in the United States. In Europe, any differentiation of the social sciences was still a long way off.

In a closed meeting of Memorial officers in 1927, a controversy arose between Ruml and Edmund E. Day,<sup>3</sup> the new Memorial officer who shortly thereafter would become his successor as head of the Social Sciences Division of the RF, about the issue of what later would be called interdisciplinarity. Day, feeling that Ruml's high opinion of cooperation between disciplines was somewhat exaggerated, asked Ruml why he thought institutions that were dedicated to a single discipline were inefficient. Interestingly, Ruml responded by referring to a European case to illustrate his criticism:

B.R. [Beardsley Ruml] said that a general development was ... not wholesome to preclude varied contacts of disciplines, which would be apt to happen because of the artificial academic categories ... He spoke of the Institute [für Weltwirtschaft und Seeverkehr] at [the University of] Kiel as being an example of a fine research institute in its field, which lacks effectiveness because of having few contacts.<sup>4</sup>

In the last sentence, Ruml gives away his actual motive for denouncing the division of the social sciences as 'artificial'. His *bête noire* was not so much that lines were being drawn between individual disciplines, but that social scientists tended to be excessively single-minded and compartmentalizing.

After Ruml had left, and the Memorial and the RF had merged, policies were reoriented along new lines. Rather than evoking cooperation as merely an ideal, the Foundation now sought to stimulate it more directly by focusing its support. Still, the rhetoric of 'cooperation' kept resurfacing in the papers and communications of RF officers. And critics of the cooperative form of conducting research soon also appeared on the scene. In a widely read magazine, Harold Laski attacked the new form of science which, he said, relied on the 'description and tabulation of fact' rather than on the personal philosophy of the individual researcher.<sup>5</sup>

In the transitional period between Ruml's charismatic Memorial leadership and the rational administration of support as practised by Day and his new staff, the aversion to radical change in the way research funds were allocated was particularly manifest. This seems well worth examining from the perspective of organizational sociology. From this perspective, an element that is particularistic in two respects comes to light.

On the one hand it can presumably be explained as a long-term effect of the tradition of private philanthropy, where the social ties between benefactor and beneficiary had always gone beyond the granting of material support.

On the other hand, this reticence to change reflects a respect for the approach of former collaborators. Their philosophies regarding sponsorship were not simply dismissed when they retired but continued to act as a kind of legacy. In a bureaucratic form of organization, this is something rather unexpected since the ideal of impersonal functioning has always implied that its executive staff is standardized as well, and is confined to the relatively narrow range of tasks circumscribed by their social roles, which makes it easier for them to be replaced. In the formational phase of the 'general-purpose' foundations, by contrast, neither the goals nor the criteria to be applied had been fixed to the point of making interpretational efforts by individual actors unnecessary.

But rapid change was also constrained by the foundation's clients and their expectations. A client who had been selected because he was in need of, as well as eligible for, support could rightly expect not to be arbitrarily denied this status just because the foundation personnel had changed. In the early years of philanthropic foundations, decisions were geared to the support of scholars who were deemed deserving of it, rather than to impersonal research projects. If someone was considered promising in terms of further creativity and productivity, support was almost certainly continued. Thus, requests for support were hardly ever rejected in these early years since potential applicants were much more likely to be dissuaded at a preliminary stage from applying at all. Andrew Abbott points out that even in the 1950s, when Everett C. Hughes was the editor of the *American Journal of Sociology*, this still was the customary practice in the academic world (Abbott 1999). But eventually, even



the RF turned to the new universalistic paradigm of 'peer review'. A major reason for this change, as indicated by Abbott and supported by statistics, was the quantitative growth of the discipline and the consequent impossibility of knowing all of its members personally. The shift to an anonymous evaluation of the quality of a paper – and, one may add, of a research project – was also a consequence of the fact that decision-makers no longer knew the author or applicant as a person.

The problems faced by RF officers with their German clientele after 1933 perfectly illustrate the pitfalls of the particularistic routine for sponsoring research.

## EUROPEAN BENEFICIARIES – AN OVERVIEW

Between 1924 and 1941, the date when the European sponsoring programme was more or less suspended due to the United States entry into the war, funds committed to social science research centres in Europe amounted to about \$10 million (about \$150 million in 2005), according to my own estimations. To get an idea of the scale of the spending, one may point out that, after 1929, this was about the same amount the five divisions of the RF could annually dispose of for authorizing new grants. The amount dedicated to the European social sciences was thus only a negligible part of RF spending.

From the recipients' perspective, however, things looked quite different. In the early 1930s, \$10 million would, for instance, have covered one year of wages for 20,000 Austrian scholars or a one-year study stay in the United States for 5,000 European fellows.<sup>6</sup>

The largest sums were allotted to Britain, whose social science institutions received about 40 per cent of the money dedicated to Europe, followed by Switzerland with about 25 per cent. Support for France was remarkably high, amounting to about 17% of the sum total, whereas support for German and Austrian institutions was comparatively moderate (8 per cent and 2 per cent, respectively). All the other European countries shared another 8 per cent.

Under Ruml, the LSE had already been identified as a potential European centre, and William Beveridge, who was its director at the time, made the best of this opportunity (Beveridge 1955). The LSE was the only European university that was granted a 'research fund', otherwise the privilege of American institutions. In addition, the LSE was given considerable funds to acquire land, construct buildings and enlarge its library. Funds to the LSE were remitted under at least seventeen different project titles, and amounted to at least \$1 million (\$15 million in 2010). Among the projects that benefited from these subsidies was a rather disconcerting one entitled 'human genetics', an offshoot of one of Beveridge's more bewildering ideas and part of his effort to further the development of 'social biology'. On the other hand, the grants also served to finance a nine-volume *New Survey of London Life and Labour*. Their main effect, however, was to facilitate the research work of scholars such as Malinowski, Laski, Edvard Westermarck, Raymond Firth, Lionel Robbins and R.H. Tawney. The last grant that was remitted to the LSE in the period under

consideration served to compensate for the loss in tuition fees that was a consequence of the Second World War (Fisher 1980; Bulmer 1982; Bulmer *et al.* 1991; Dahrendorf 1995).

The attempt to induce the two leading English universities to enhance their social science activities was less satisfactory. In spite of intense RF pressure, Cambridge refused point-blank to open up to the new social sciences,<sup>7</sup> and Oxford hesitated to do so until 1934 (Rockefeller Foundation 1934: 177). Oxford social sciences were further boosted when in 1937 Lord Nuffield donated £1 million (about \$3.6 million at the time, \$53 million in 2010) for the establishment of a Graduate School of Social Studies, and the RF was delighted to help. Cooperation with the two Royal Institutes – for Anthropology and for International Relations – was more of a success.<sup>8</sup> The best-known collaborator of the latter institute was Arnold J. Toynbee. In 1929, the National Institute of Industrial Psychology, founded in 1921 and generously supported under Ruml, was granted funds for two more years.<sup>9</sup>

In the 1930s, funds were allotted to the establishment of an international network of economic institutes dedicated to the promotion of business cycle research – albeit under the label of ‘Social Security’. The University of Manchester, the London and Cambridge Economic Service of Great Britain and the newly founded National Institute of Business Cycle Research succeeded in being included in this network, along with the Institute for Statistics in Oxford and, of course, the LSE. Some of these institutions had received support by the RF programme for ‘Economic Planning and Control’ before. The Service, whose office was at the LSE and whose president was A.M. Carr-Saunders, who was appointed Director of LSE in 1937, had since its foundation in 1923 published a kind of business barometer, the sale of which had so far served to finance its activities (Rockefeller Foundation 1937: 244–6).

The National Institute for Business Cycle Research was founded in 1937 and was subsidized by three English foundations – among them the Pilgram Trust and the Leverhulme Trust – as well as by the RF, which contributed \$150,000 total (\$2.2 million in 2010) over a period of five years. Its mentor was former LSE director and current private scholar Beveridge who for many years had been on excellent terms with the various Rockefeller foundations and had already benefited from RF support for the gigantic compilation on the history of prices and wages that had been completed under his direction.<sup>10</sup>

The sums reported for Switzerland are somewhat misleading since a considerable part of the funds were attributed to the Financial Section and the Economic Intelligence Service of the League of Nations in Geneva. Support was granted for the publication of laws concerning the finance and banking sector ‘of all the nations of the world having stabilized monetary systems’ and for a compilation of international agreements on the double taxation of enterprises with a transnational scope of action (Rockefeller Foundation 1929: 228–9, 253). In the 1930s, Gottfried Haberler was one of the collaborators of this division. The Institut Universitaire des Hautes Études Internationales, founded in 1927, was similarly considered worthy of support. Two years after its foundation, it received a

ten-year subsidy of \$850,000 (\$10.5 million in 2010). The third institution to be supported in Switzerland was the Jean-Jacques Rousseau Institute in Geneva, where pedagogues and psychologists were conducting research, among them Jean Piaget.<sup>11</sup>

In France, the Memorial and the RF were rather at a loss to identify institutions that qualified for funding. Four research institutions – a documentation centre as well as Institutes for Ethnology, International Law and Public Law based in Paris – each had to be content with a one-time grant, which suggests that they failed to satisfy their donors. The Centre d'Études de Politique Étrangère and the University of Paris received funding twice, the latter being expected to become the home of realistic social sciences. Cooperating partners in this endeavour were Lucien Lévy-Bruhl and Marcel Mauss, among others. This effort seems to have been no more successful than the attempt to convince 'Gallic' jurists of the merits of empirical political science.

The most important French ally for support given to institutions was economist Charles Rist at the University of Paris, who from the very start had acted as a fellowship advisor for the Memorial. In 1933, many years of negotiations resulted in the establishment of a non-university Institute for Economic and Social Research.<sup>12</sup> It was dedicated to research in fields that had so far been 'little cultivated' in France, i.e. industrial organizations, the social and economic structure of France, and the development of the agrarian sector and markets, in addition to the obligatory business cycle research. The data and documents investigated and compiled at the institute's documentation and publication centre were to be made available to the public (Rockefeller Foundation 1933: 238–9).

The first Scandinavian institution to be included in the circle of 'European centers of social science research' was the Copenhagen Institute of Economy and History. Its director was Hans Vilhelm Munch-Petersen, who also acted as a fellowship advisor. Later, the institute was also supported as an institution that did research on international relations (Rockefeller Foundation 1930: 220; and Rockefeller Foundation 1936: 248–9). The Institute for Economics at the University of Oslo, which had been founded in the 1920s and was later headed by 1927/8 Memorial fellow (and 1969 Nobel laureate for economics, together with Jan Tinbergen) Ragnar Frisch, qualified for funding because it brought together businessmen and university teachers in a concerted effort to study economic problems and because it 'combines in an interesting and unusual way inductive and deductive methods in economic research' (Rockefeller Foundation 1931: 240).

The Institute for Social Sciences at the University of Stockholm, directed by Swedish fellowship advisor Gösta Bagge, benefited from massive long-term support. It also received funds for construction works, for setting up a social science library, and for other infrastructural projects. Among the research projects that were supported was a guest professorship for American sociologist Dorothy Thomas 'to introduce a specifically American method of attack upon social problems'. Later, the Stockholm Institute, no doubt under the influence,

among others, of Dorothy Thomas and her husband W.I. Thomas, who were engaged in transcontinental cooperation with the Myrdals, also took up demography and migration research.<sup>13</sup> At the time, the RF observed, and welcomed, a 'slight trend toward interchange of personnel in Scandinavia countries' which was attributed, as its most likely source, to the support given to the three sites in a 'well rounded program, and through interchange, in greatly increasing opportunities for trained investigators'.<sup>14</sup>

The group of institutions involved in business cycle research later included a number of institutes at the University of Louvain in Belgium, the Netherlands Institute for Economics, with Jan Tinbergen among its leading scholars, and the Statistical Institute for Economic Research in Sofia, Bulgaria.

In 1935, the RF started funding two institutions in Poland, the University of Lwów and the Polish Academy of Sciences in Krakow. In 1939, due to the German aggression against Poland, both programmes came to an abrupt stop.

Beginning in 1932, a negligible sum from the programme was channelled to Budapest for public administration, while the Romanian Institute for Social Sciences in Bucharest, a non-university institution directed by sociologist Demetre Gusti, who was affiliated with the local university, received at least three 'grants' starting in 1931 (Rockefeller Foundation 1931: 242).

Reviewing these three decades, it is striking to see the rather strong continuity of the ties that had been established, via the first grants, between the officers of the philanthropic enterprise in New York (and, later, its Paris Office) and their European clients.<sup>15</sup> Long after Ruml had left, the contacts he had established remained – as well as, at least until the outbreak of the Second World War, the problems he had pointed out in his memorandum after only one year of European involvement:

Here are the first tentative steps toward the building up of the dozen or fifteen world centers for social sciences ... Only one such center can be said to have clearly emerged, the LSE. The necessity of centers in Paris and Geneva is clearly felt but the problem of method is equally obscure. In Germany, the question is one of choice among a number of strong centers, Berlin, Kiel, Hamburg, perhaps Heidelberg and Cologne. In Austria, the Vienna situation is baffling and it is unlikely that steps can be taken for some years. In Scandinavia, the U of Stockholm stands out prominently.<sup>16</sup>

In 1935, the support programme dedicated to the centres of social science research was discontinued, albeit with a five-year transition period (Rockefeller Foundation 1935: 194). At the time, the RF had already virtually stopped operating in Germany. How this came about will be discussed in the following section.

## THE GERMAN CASE

As long as the Memorial's European programme lasted, German research institutions received varying amounts of money for varying periods of time. The above quotation from Ruml's memorandum shows that if, in 1925, no decision

had as yet been reached concerning the German centre or centres, the Memorial was nonetheless bent on setting one up.

One of the first institutions to receive grants for conducting concrete research projects, aside from the Institute for Foreign Politics in Hamburg referred to above, which distinguished itself primarily by its nomination of fellows, was the *Notgemeinschaft der deutschen Wissenschaft* (Association for the Emergency Funding of German Science) that had been founded after the First World War with the aim of aiding the ailing universities and libraries (Marsch 1994). At a very early stage, scholars in America had set up relief committees to support the country their ancestors had come from. There was, for instance, the Society for German and Austrian Science and Art directed by German-born anthropologist Franz Boas, and the so-called 'Professorentisch' (literally 'professors' table') in Vienna that was sponsored by the Quakers to counteract the severe food shortage.<sup>17</sup> But soon, material help was re-channelled towards the funding of scientific research. Initially, this primarily meant subsidies for buying books and meeting the costs of publications.<sup>18</sup>

The first large research grant was allotted, via the *Notgemeinschaft*, to a group of German 'anthropologists' who received the handsome sum of \$125,000 (about \$1.5 million in 2010), to be spread over a five-year period, for a 'Survey of the German Population.' From the perspective of the history of science, this research project is important in more than one respect. It was the first cooperative effort (a so-called 'Gemeinschaftsarbeit') of German scholars who went by the title of social scientists in the RF papers, and it brought together individuals who, due to their activities during the Nazi era, have acquired a questionable reputation. In this case, transatlantic cooperation between the RF and the *Notgemeinschaft* was characterized by misunderstandings rather than by the mutual intellectual and institutional enrichment it was supposed to foster, and its organizational and financial management was somewhat peculiar, to say the least.<sup>19</sup>

The idea of applying for financial aid to the RF had been suggested to the Germans by Ruml during one of his trips to Germany. In September 1929, the President of the *Notgemeinschaft*, Schmidt-Ott, submitted a detailed proposal to the RF. In New York, his German letter was translated, and a comparison of the documents shows that due to differences in language as well as in meaning, misunderstandings arose from the very start. Of course, what was termed 'Anthropologie' in one scientific culture could be translated as 'anthropology', but this by no means ensured that both terms described the same thing. In the 1929/30 annual report of the *Notgemeinschaft*, the research project was classified with the cooperative projects in medicine.<sup>20</sup> All the explanations provided by the Germans concerning the eugenicist perspective of their research project, and all the elements indicating, in a none too covert way, that this research group was a far cry from any social science approach, were without comment ignored by the RF in New York. Instead, the paper submitted to the RF board that had to authorize the grant gave much weight to the only passage of the six-page letter that referred to the influence of 'social and economic

conditions', thereby subjecting the proposal of the Notgemeinschaft to yet another interpretation. While the German application stated that 'in addition to anthropological surveys' the project would include 'a study of the population in a genealogic and historical context' and, in doing so, would take into account social and economic conditions, the RF paper levelled out the hierarchical research design and, instead, focused on details of survey techniques:

The object of the study is to find a scientific basis for judging the population both as to anthropological composition and as to social-economic relationships. Knowledge as to the recurrence of degenerative traits and the distribution of hereditary pathological attribute should result from the study proposed.

The entire area of Germany will be covered ... through the choice of sixty typical regions as points of attack. Industrial areas, remote agricultural areas, mountain areas, certain urban areas will be included. Regions of various racial or religious composition and regions differentiated by historical events will be studied.

The same documents further specify that the research project would be organized in terms of individual studies to be conducted or directed by the anthropological institutes of the nearest universities. These research directors were to cooperate via the Notgemeinschaft and the Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik (Kaiser Wilhelm Institute for Anthropology, Human Genetics and Eugenics). The Notgemeinschaft reported that in the previous year, \$25,000 (\$310,000 in 2010) had been spent on 'preliminary work' in thirty-seven regions, and that they would try to ensure the funding of the study for all sixty regions.<sup>21</sup> The research conducted so far was described as follows: 'Several related lines of investigation which are intimately related to the general plan have already been uncovered. Data of psychiatric and biological value have resulted from study of the convict prison of Staubing in Bavaria, for instance, and suggest specific criminological research.'

The RF officers, in turn, informed their superiors on what they felt to be the importance of this '*Gemeinschaftsarbeit*' (collaborative work): 'This program of research appears a most promising venture in co-ordinating the work of German universities and scientists. While the chief emphasis is upon anthropological investigation, there is a broad concept of interests involved.' This statement was echoed in Schmidt-Ott's letter of thanks in which this leading German science administrator emphasized that he expected 'support for the idea of cooperative research projects also for other social science fields'.

Reference to the social sciences is primarily found in Schmidt-Ott's covering letters and letters of thanks, but not in the comments on the actual proposal. Why the RF, given its concern with 'realistic research', considered this project as deserving of support remains unclear. Two main factors seem to have been decisive for the emergence of this misalliance. Approval was granted at a time when the Memorial and the RF were deeply involved in the merging process, and the reorganization of responsibilities and personnel that went along with it obviously resulted in a temporary confusion in the administrative order of

the Foundation. In addition, the prestige enjoyed by His Excellency Schmidt-Ott among German scholars seems to have cast a spell on his visitors from overseas, as well. When the internal organizational processes had ended and the RF had reconsolidated, none of its officers showed much interest in the project. Thus, for five years, the Notgemeinschaft received subsidies from New York which amounted to 3 per cent of its annual budget, but it was not subjected to the otherwise customary supervision by RF officers.<sup>22</sup> Nothing happened until the German side submitted a request to have the remaining funds reallocated as subsidies for the publication of further volumes of the series they had started. When, in addition, the director of the '*Gemeinschaftsarbeit*' enquired if the 'grant' could be renewed for five more years, Paris-based Van Sickle rejected the request without even bothering to consult New York. This was in April 1933, and one would like to think that the belated change of mind was due the changed political situation. However, there is little to go on for this interpretation.

This research network, repeatedly and rather grandiosely passed off as a '*Gemeinschaftsarbeit*', was directed by Eugen Fischer who since 1927 was a professor of anthropology at the University of Berlin as well as Director of the Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik that had opened in the same year. Fischer was one of the leading German exponents of eugenics, racial biology and racial hygiene. While in his early writings he had still argued a positive stance to racial mix, he later became an advocate of the doctrine of racial purity. He did not, however, join up with the ruthless racists of the *völkisch*-Nordic conviction. Fischer and most of his colleagues remained aloof, at least formally, from the Nazi party for a very long time, insisting on their allegedly purely scientific approach to racial issues. This did not, however, keep Fischer from assuming the post of a Rector of the University of Berlin and, subsequently, from becoming a willing executor of the *Gleichschaltung* (forced conformation) of his university (Weingart *et al.* 1988; Deichmann 1992). Fischer refrained from any explicitly anti-Semitic passages in his writings, conforming in this with a Notgemeinschaft ruling that requested its anthropologists to guarantee – in the time before the handing-over of power to the Nazis – that their publications would not be used in the public debate on the Jewish question (Weindling 1989: 467).

Fischer's favourite research methods were comparisons of blood groups and family reconstructions, both of which were supposed to enable him to determine whether illnesses and criminal behaviour were hereditary. His survey population was to have been the 'native population' of rural districts that had a high degree of endogamy or, in the terminology used at the time, racial purity. Fischer successfully fought off alternative suggestions to use occupational groups as a survey population.

Since Fischer, rather than amalgamate different disciplinary perspectives, sought to rally a number of comrades-in-arms, it seems more adequate to say that this '*Gemeinschaftsarbeit*' was cooperation based on a shared-paradigm and was, moreover, an assembly of high-level university members. However,

this paradigm group was not entirely homogeneous in its scientific orientation. Racial anthropologists whose concern was anatomy worked side by side with researchers in the field of ethnogeny, who were more strongly oriented to the natural sciences and, more particularly, to biology. Research on blood groups provided a convergence point for both orientations, bridging the gap between those who were primarily interested in morphological and anatomical issues and those who focused on the identification of hereditary factors.

A group that had initially been a rival, but had in the end also gained access to the American coffers, was centred on Ernst Rüdin, Professor of Psychiatry at the University of Munich. Rüdin and his colleagues were more interested in criminological and so-called crimino-biological issues. One of the anthropologists who rallied around Rüdin and also profited by this opportunity was Oscar Vogt, Director of the Kaiser-Wilhelm-Institut für Hirnforschung (Kaiser Wilhelm Institute for Brain Research), who fell from grace after 1933. He had examined Lenin's brain after the latter's death, he had subsequently maintained his contacts to his Soviet colleagues, and his wife was French – all of which aroused Nazi suspicions. Karl Saller, the only German anthropologist who after 1945 gave a self-critical account of the Nazi period (Saller 1961), was similarly marginalized after 1933. A content analysis of the seventeen volumes of the *Deutsche Rassenkunde* (German Ethnography) shows that almost half of it is taken up by physical anthropology (Table 3.1).

**Table 3.1** Content Analysis of *Deutschen Rassenkunde* (German Ethnogeny)

Topics	Pages	%
History	225	12.1
Ethnology	150	8.1
Sociology, Demography	166	8.9
Geography	47	2.5
Pre-History	30	1.6
Economy	30	1.6
Physical Anthropology (incl. Physiognomy)	891	47.9
Methods of <i>Rassenkunde</i> (Racial Anthropology)	71	3.8
Racial Types ('Dinarisch' etc.)	12	0.6
Investigations of Blood Groups	10	0.5
Photographs	228	12.3
Total	1860	100

Source: Fischer 1929–38. Author's calculations.

Beginning in 1932, the '*Gemeinschaftsarbeit*' was reinforced by two other race researchers whose work was particularly infamous in the eyes of latter generations but who otherwise had little in common: Hans F.K. 'Rasse'-Günther ('Race-Günther') at the University of Jena, who belonged to the small group of ruthless racists among the university teachers; and Otmar Freiherr



von Verschuer, who directed the division of human heredity at Fischer's Kaiser-Wilhelm-Institut für Anthropologie, menschliche Erblehre und Eugenik. The latter was for a long time primarily perceived as a scholar. Only after it was discovered that Adolf Mengele had been one of his collaborators was this image marred. It is still is a controversial issue in the literature whether or not it was by order of Verschuer that Mengele conducted his murderous research on twins in the concentration camp of Auschwitz.

In the present context, the fate of the German racial anthropologists under Nazi rule is of little relevance since our concern here is with the relations of researchers within the group and with the main sponsor, the RF. Not all of those who rallied around the 'anthropological survey' contributed to the publications that resulted from this large-scale project. Rather, there is a considerable discrepancy between the subsidies provided by the RF and the publications, as revealed by a comparison between the approximately \$100,000 (\$1.6 million in 2010) accounted for between 1931 and 1934 and the seventeen volumes of the *Deutsche Rassenkunde*.

Most of the volumes were published before the handing-over of power to the Nazis. Surprisingly, even the volumes that were published after 1933 refrained from the political kowtow to the new rulers that was customary in other publications of those years. Apparently, the authors did not feel the need to demonstrate their ideological kinship by including martial quotations from Hitler. Not so for Schmidt-Ott, their long-term spokesman in the apparatus of the German science administration; as head of the Notgemeinschaft he did feel this need, as witnessed by his deep reverence to the Führer in the Annual Reports.<sup>23</sup>

In the preface to the first volume, Fischer defines the general principles and also refers to the way it came into being. Without the 'generous aid of the Notgemeinschaft, the work would never have been realized,' for which thanks were due in particular to the 'Honoured President, Excellency Dr. Schmidt-Ott'. Here as elsewhere in his preface, no reference, be it ever so vague, is made to the massive support provided by the RF. This contrasted with the annual reports of the Notgemeinschaft that between 1930 and 1933 explicitly evoked RF support for the association in general and, more specifically, for its racial research.

A content analysis of all the volumes clearly shows how far away German ethnogeny was from the social sciences (Table 3.1). The bulk of the published texts deals with physical anthropology, and only a negligible part of them is concerned with social science issues and reflections in the widest sense. That RF aid is passed over in silence before the outward world indeed suggests a systematic effort to distort the facts and blur the traces, which met with a like-minded response in the literature concerned with the history of the sciences.<sup>24</sup>

In spite of RF interest in German social science, its local social science sponsorship programme was very slow to gather momentum. In 1928, the Heidelberg Institute, which at the time was directed by Alfred Weber, Carl Brinkmann and Emil Lederer, was granted \$60,000 (\$746,000 in 2010) over a five-year

period as a subsidy to its general research programme. Their programme was clearly demarcated by Lederer: studies were to focus on the contemporary economic structure of Europe, especially on the economic bases for establishing industries, on European trade relations with non-European countries and on capital movement in the postwar era.<sup>25</sup>

In the summer of 1929, on his journey to Europe with Chicago political scientist and long-term RF advisor Charles Merriam, Ruml had already devised a plan to sponsor the establishment of a Kaiser Wilhelm Institute for International Relations in Berlin. One year later, nothing had come of it, partly because the German government had cut down its science expenditure, but mainly because of the open question of who was to be the director of the new institute: 'Unfortunately no one has emerged as the obvious leader'.<sup>26</sup> Again, one month later, the issue was discussed in private with the Secretary of the Notgemeinschaft, Fehling:

F. said that time was not yet ripe but that he thought it soon would be as an important person had consulted him confidentially about a plan he had been formulating for some time past and which he had discussed with the Minister of Education. The man is well known to the RF, but Fehling preferred not to mention his name.<sup>27</sup>

A few months later, the secret about the unknown person had been revealed:

Telegraphed from frontier to Fehling who met me at station Berlin in order to discuss present prospects re Institute of International Studies in Berlin. F. stated that the project referred to at our last meeting was sponsored by Prof. Jäckh. J. wants to call his Institute, 'Institute of Peace', and will have nothing to do with the KWG [Kaiser-Wilhelm-Gesellschaft] because the very name smells of militarism ... It would probably help precipitate things if F. authorized to say casually in response to German inquiries, that it is his understanding that the RF interested not merely in a center for International Studies at Geneva but also in a number of strong national centers.<sup>28</sup>

The episode, the rather detailed account of which we owe to Van Sickle's otherwise so concise diary, is an apt illustration of the long-winded and complex functioning of the Teutonic science culture. Not much in this reminds one of the ideal of a scholarly republic of seekers of truth enjoying equal rights. The idealized view of forms of interaction that were obviously oriented to the no less idealized image of Athenian democracy had little in common with the actual situation. Intricate networks of administrative and political power had to be taken into account, competences were but vaguely defined, and whoever wanted to start something new was obliged to find advocates, intermediaries and power brokers who were in a position to submit his idea to those in power, the scope and composition of the latter's portfolios being unknown to anybody outside the chancelleries of his Excellency.<sup>29</sup> There were no clear procedures for applying to one of the funds that was charged with administrating the finances dedicated to the sciences, nor did it

seem appropriate to propose new ideas when one had them. Rather, it was appropriate to wait for the right moment. The entrenched confrontation of republicans and mandarins, of democrats and those who grieved after the fallen monarchy, had crystallized into habitual barriers that kept scholars apart and were not at all easy to overcome. If, to boot, the topic at stake was a politically sensitive one, such as international political research, and funds of American plutocrats were involved, failure was predetermined.

Instead of founding a new institute for international studies, which in July 1930 had turned out to be definitely impossible,<sup>30</sup> the RF awarded a \$25,000 five-year period grant (\$318,000 in 2010) to the Notgemeinschaft for research on international relations.<sup>31</sup> The committee, as the annual report of the RF notes in its habitually amiable and none-too-specific manner, 'consists of scholars from various fields and represents the beginning of a promising program in cooperative research upon international questions, in which German jurists, economists, political scientists, sociologists, and publicists will participate' (Rockefeller Foundation 1931: 249). Soon after the first instalment had been remitted to Berlin, a conflict with another recipient of RF aid appeared. The Deutsche Hochschule für Politik (German Academy for Political Science), directed by Ernst Jäckh, the hapless founder of an Institute for Peace, planned to extend the academy's research activities to the field of international relations. Selskar M. Gunn, Associate Director of the Social Sciences Division in Europe, who had either not read the diary of his colleague Van Sickle or was still maintaining an unbroken optimism with respect to cooperative research, tried to deal with the Berlin problems the American way: 'Personally he [Gunn] thought it would be desirable if the Hochschule could absorb the committee as it had good men on its staff and was working in close collaboration with scholars in various German universities.'<sup>32</sup> Apparently, this cooperation never came about.

In November 1932, Excellency Schmidt-Ott assembled a select group of social scientists at the Berlin Hotel Continental to have lunch with Kittredge. Those present were Hermann Schumacher, a member of the German fellowship committee, Herbert von Beckerath, from Bonn, Arnold Bergsträsser, from Heidelberg, and Arnold Wolfers, from the Deutsche Hochschule für Politik: 'In general the program has taken the form of a series of studies of the world trade and of the effects of governmental intervention in the economic field, on movements of capital and goods. The committee on international relations has thus far avoid [sic] any attack upon outstanding controversial questions of Germany's foreign policy.'<sup>33</sup> In Heidelberg, the situation seems to have been less complicated. Arnold Bergsträsser, an Assistant at the Institut für Sozial- und Staatswissenschaften (Institute of Social and Political Science), had applied to the RF for the first time in February 1928. On his first visit to Heidelberg, Van Sickle was rather pleased. In August 1932, Bergsträsser sent him the first volume that had been completed thanks to RF support: *Local Economic Policy*. Half a year later, Alfred Weber, the institute's senior scientist, reported in person – albeit in German – to Van Sickle on the work that had been done so far.

Reliance on the German language by almost all German applicants as well as by the Notgemeinschaft Secretary Fehling is one of the peculiarities of the German clientele of American philanthropy. Unlike most Austrians who in their second letter to RF representatives, at the latest, started to write in English – and, in doing so, saved the Paris Office the trouble of having the documents translated before forwarding them to New York – the Germans, although most of them were quite proficient in foreign languages, held on to their mother tongue. Was it the fading pride of the once leading nation of science, was it simple thoughtlessness, or was it the fear that no other language would allow them to phrase their request in adequate terms?<sup>34</sup>

Towards the end of February of 1933, Van Sickle was again in Heidelberg, negotiating on a further three-year subsidy. Although the request had his backing as well as that of the other European officers, nothing came of it. When Nazi dictatorship had been established, a number of New York trustees vetoed any further funding of German institutions. In the end, their scruples, first no doubt drawn primarily from public opinion, determined RF policies in general: individual grants, yes; institutional support, no.

In the seven years between the launching of the European programme and the end of the Weimar Republic, neither the Memorial nor the RF had managed to find German cooperation partners for the planned German centre. Had they contacted the wrong people? Was the number of scholars interested in, or considered capable of, the realistic type of research too small, or was seven years too short a period for such an undertaking, given that after the spring of 1933, RF ‘field officers’, while steadfast in their intentions, came up against the veto of the New York headquarters?

An answer to this question may be found in the impression a benevolent American visitor could not help but receive of German social sciences shortly before Hitler was appointed Chancellor of the Reich. In the summer of 1932, Tracy Kittredge had written a detailed report on the situation of the social sciences in Germany. One-third of it is dedicated to a description of the German system of higher education and the internal workings of its universities, and to a comparison with its French and, of course, American counterparts. This is what he wrote about the career paths of junior scientists, for instance:

The German system of appointment has of course its weak points as well as its strong ones. Thus, admission to the first rung of the academic ladder – the Privatdozentship – is not based, as in France, upon perfectly free and fair competition open to all who satisfy certain minimum requirements, but upon invitation to compete by a professor. This practice makes possible the promotion of an excellent man, but it also opens the door to favoritism. In the present temper of Germany there is a real danger that this possibility of favoritism will react on the quality of the ‘Nachwuchs’ [younger generation] in the country. France has protected herself against this danger but at the expense of closing the door upon the really brilliant man who does not satisfy certain very formal requirements having little connection with capacity for research.<sup>35</sup>

Kittredge found German *Nationalökonomie* (political economics) ‘well developed’, and specialization farther advanced than in France, but noted that

German economists had until recently met with little approval abroad because of the prevalence of the Historical School. He saw the German development in sharp contrast to the Austrian school which, under Eugen von Böhm-Bawerk and Friedrich von Wieser's leadership, had achieved great brilliance and top-rank renown. Their 'weakness was just the opposite to that of the German Historical School – its impatience of the tiresome grubbing for facts, and its enormous reliance on theoretical analysis.' Among the economic institutes, those in Kiel und Heidelberg stood out; the former, he wrote, was 'a mecca for research workers interested in international economic problems'. The somewhat smaller Heidelberg Institute was covering almost the whole field of the social sciences: economics, political science, sociology, history, geography and law. Until Joseph A. Schumpeter's recent departure, the third most important centre of economic research had been Bonn. This was due in part to the scholarly excellence of the three directors of the Institute (Schumpeter, Arthur Spiethoff and Herbert von Beckerath), but mainly to their capacity for harmonious cooperation and their competence in drawing up a well-designed research programme.

Kittredge continues with a comment on a peculiarity of German research organizations which cannot be fully understood unless it is seen against the background of the United States structure of departments and research centres described in Chapter One: 'The situation here [i.e. Bonn], as at Heidelberg, affords a good illustration of the difficulty of operating research programs in Germany. Research Institutes are built up around one or two scholars with a flair for organisation and a capacity to raise money. When the leader dies or is called to another university, the Institute vegetates or disappears.' Kittredge goes on to give a short summary of German business cycle research as well as of research on public finances, transportation economics and rural economics.

In contrast to economics, *sociology* seemed much less developed, and its achievements much harder to assess, for someone who had been socialized in the American world of science: 'To understand many a German sociological article one must know well the entire history of German philosophy.' According to Fehling, Kittredge writes, there were 'about thirteen first-rate sociologists' in Germany. Leopold von Wiese of the Cologne Institute, who at the time was widely considered to be the most influential sociologist in the Weimar Republic, is only briefly characterized. 'No one of the Paris Office has as yet visited the Institute.' The first visit to Cologne took place towards the end of February of 1933, under highly adverse conditions, and nothing became of it.<sup>36</sup> Johann Plenge's *Forschungsinstitut für Organisationslehre* (Institute for Organization Research) in Münster disposed of 'appreciable research funds': 'Report is however, that the director has such a difficult character that cooperation with him is impossible.' The last institution referred to is the *Institut für Betriebssoziologie und Betriebslehre* (Institute for Organizational Sociology and Business Economics) of the Technical University in Berlin, under its Director Götz Briefs.

On all these scholars, Kittredge seems to have had only second-hand information, an assumption further confirmed by the fact that both Van Sickle's and Kittredge's diaries fail to allude to their names or to any visits to their institutes. Even for those scholars he deals with in more detail, Kittredge notes that his knowledge of them is rather unequal. By way of introduction, he writes: 'Professors Mannheim at Frankfurt, Andreas Walther at Hamburg, Freyer at Leipzig, all have seminars in which research is going on with very inadequate support. Heberle, a former fellow of the R.F. is developing at Kiel an interesting program on the border line between sociology and political science.' The Seminar für Soziologie (Seminar of Sociology) of the University of Frankfurt, under its director Karl Mannheim, was reported to have been very active, conducting research on the mechanisms of leadership choice in political parties, trade unions and the Catholic Church, on women in politics, on the sociology of the immigrant and on the effect of education on social status. His conclusion was that 'Professor Mannheim is one of Germany's leading sociologists, and the work here [Frankfurt] is said to be interesting and significant.'

The seminars directed by Andreas Walther and Hans Freyer, he went on to say, were of about the same size. Walther was interested in urban sociology, had been to the United States and was the co-editor of Emory S. Bogardus's *Journal of Applied Sociology*. 'It is only lack of funds that prevents him [i.e. Walther] from developing careful inductive studies.'<sup>37</sup> Freyer was said to be Germany's 'best know[n] young sociologist'. Thanks to RF funds, American literature was 'fairly well represented' in the library of his seminar. Contacts between the RF and Freyer must have existed even before Kittredge wrote his report, since on 31 July 1931, Van Sickle's diary in Paris reads: 'Dr. Hartshorne. SSRC fellow called to discuss his project. Advised him to call on Fehling and Hans Freyer of Leipzig with whom we had discussed his project.'<sup>38</sup> Van Sickle had met Freyer in May 1931 in Leipzig, had had tea with him at his home and had also visited his institute. The memorandum notes, among other things:

Freyer himself is one of the leading sociologists and has come into the field from philosophy. In his own work he is primarily concerned with philosophic and dialectic problems and the history of sociological thought. As Director of the Institute, however, he is directing students into research of a more realistic nature. He is undertaking comparative studies in city planning, regional planning and colonization. In the latter field he is particularly interested in the methods by which colonists are selected and the type of colonist that best succeeds.

Freyer is about 40 years old, lively, alert and very friendly. He was told of the proposed work of several of the SSRC fellows next year, and he expressed the greatest willingness to receive them and to aid them in any way in their work.<sup>39</sup>

The colonists Freyer proposed to study were probably part of the so-called *Binnenkolonisation* ('domestic colonization') which, given the Nazi slogan of '*Volk ohne Raum*' ('Nation without Space'), might have given rise to suspicions as early as 1931. Subsequent Nazi resettling campaigns benefited, as Götz Aly

and others have shown, from the preparatory work of the German '*Ostforschung*' ('Research of the East') (Aly and Heim 1991) – something Van Sickle could not know at the time.

Van Sickle's notes illustrate the consistent course of action taken by RF officers who travelled through Europe. Before funds were granted, candidates had to undergo a personal inspection by an RF officer. The impression thus received had at least as much weight for decision-making as the content of their projects.

Finally, almost every contact was used to open gates for protégés from both sides of the Atlantic, to encourage the exchange of ideas and to provide support. In latter-day terms, this was 'networking'.

Van Sickle and Kittredge, while not shying away from rightwing contacts, were hesitant in their dealings with the left-wingers. Kittredge's report includes a characterization of the Frankfurt Institut für Sozialforschung (Institute of Social Research). He highlighted its position between sociology and economics, praised its excellent library and considered it an 'effective center for research in labor problems' where a study on 'labor attitudes which is based on an elaborate questionnaire' was currently being conducted. For Kittredge, this study – obviously the study on blue-collar and white-collar workers in the pre-Nazi era which was published many years later (Fromm 1980) – was the first one in Germany to apply this survey technique.<sup>40</sup> Although comments on the political orientation of projects are rare in RF documents, there is a concluding remark about the Institut für Sozialforschung: 'It is regarded in Frankfurt as a breeding ground for communists and as being financed from Moscow.'

Although Van Sickle considered Frankfurt to be a 'very important university center', he advised against 'large aid': 'The atmosphere is international and Jewish. Many Jews on the faculty. From the point of view of research Frankfurt is of first importance. Any large aid here just now would be badly received by German public opinion. A series of small grants-in-aid might on closer examination prove advisable.'<sup>41</sup> As such, these sentences express blatant anti-Semitism. Given the context in which they were written, the reluctance thus expressed to support research done by Jews illustrates a basic dilemma of philanthropic activities abroad. How much allowance should be made for public sentiment in the recipient country, how much compliance should be given to their prevailing standards and prejudices? With respect to the advancement of science, this dilemma has undergone a slight shift in its emphasis: how could one make the 'peaks higher' without getting involved in public controversies? This was something every member of the Rockefeller empire was mortally afraid of since that disastrous attempt to promote the industrial interests of the founder by a form of ancillary research that turned out to be nothing but propaganda. In the years after 1933, this was a recurrent pattern, and we will see how the RF and its European officers coped with this delicate, as they saw it, problem.

A third social science discipline considered in detail in Kittredge's report is *political science*. Europe, he wrote, was lacking a real equivalent to American

political science, but German international law, public law and administrative law might be seen as those fields that most closely corresponded to 'political science'. The most important institute of international law was the Kaiser-Wilhelm-Institut für ausländisches öffentliches Recht und Völkerrecht (Kaiser Wilhelm Institute for International Law) in Berlin, under its director Viktor Bruns, which according to Kittredge had been established as part of German efforts towards a revision of the Treaty of Versailles. Institutes at the universities of Kiel, Frankfurt and Göttingen are mentioned in passing, while Hans Kelsen's Cologne Institute is presented in great detail. Kittredge reports that Kelsen had been promised sufficient funds when he was appointed, but had failed to receive them due to the present crisis. Kelsen and his assistant Erich Hula, a former RF fellow, had to make do with a poorly equipped library and, as a result, were handicapped in the proper conduct of their research.

Both are Austrian Jews, able, and more theoretically minded than their German colleagues. Their approach offers an interesting contrast to the prevailing German approach which starts from the assumption that international law has to be discovered and described by careful historical and inductive study of international agreements. To them law is an historical growth and not a product of pure reason. Kelsen maintains, on the other hand, that there is a uniformity of principles which makes international law of a higher order than any of the particular manifestations of law that may appear in the statute books of nations, or in the treaty compromises between nations.

German RF representative Fehling voted against support for Kelsen, arguing that in addition to Kiel and Hamburg, the two Berlin-based institutes, Kaiser-Wilhelm-Institut für ausländisches öffentliches Recht und Völkerrecht and Kaiser-Wilhelm-Institut für ausländisches und internationales Privatrecht (Kaiser Wilhelm Institutes for International Law and International Private Law),<sup>42</sup> were quite sufficient to cover the field.<sup>43</sup> Despite this, Hula was awarded a 'grant'.

The Deutsche Hochschule für Politik (German Academy for Political Science), which Kittredge hoped to see evolve into the 'most important center for research in Germany' as a result of the support granted in 1932, was favourably reviewed, as was Mendelssohn-Bartholdy's Hamburg Institut für Auswärtige Politik (Institute for Foreign Policy), which from the very start had benefited from RF support.

With respect to future RF policies in Germany, Kittredge concluded by discussing three variants of aid and its potential beneficiaries. Large long-term aid variants in Germany were bound to come up against the problem he had himself repeatedly addressed in the report: a single person leaving an institute could bring the whole work to a standstill. Therefore, aid should only be granted to those institutes that 'have gained sufficient momentum and built up such an equipment as to offer reasonable guarantees of permanence' – a category where Institut für Weltwirtschaft in Kiel (Kiel Institute for the World Economics) had a top position.



Minor aid – Kittredge gives the figure of \$3,000 (\$42,000 in 2010) of annual support – might be granted to six to ten one-man centres or institutes to enable them to conduct ‘post Ph.D. level’ research, improve the training of future professors, and promote ‘inductive as against speculative inquiries’.

The third variant would be to continue subsidizing committees charged with ‘specific pieces of research’, as had been done in the past for the anthropological survey and, more recently, for international relations. This kind of aid did not saddle the RF with any post-hoc obligations.

Before analysing RF activities in Nazi Germany, let us briefly consider the question of whether the RF’s perspective on the situation of the social sciences in Germany in the summer of 1932 was accurate. Although it is somewhat surprising that Werner Sombart is not mentioned in the RF documents, the picture is by and large consistent. Why Ferdinand Tönnies and Leopold von Wiese were ignored or underestimated is hard to understand given their actual position and their at least partial affinity to the inductive programme of the RF. Ignoring Theodor Geiger is a real oversight which seems to be due to the fact that the field of vision of the RF officers – and their German confidants – did not extend to the smaller universities. Due to the lack of political scientists who actually identified themselves as such, and due to the RF focus on international relations as the core element of political sciences, no final evaluation is possible. What is quite certain, in contrast, is that German psychology was completely ignored as part of the field of the social sciences to be considered.<sup>44</sup> More or less the same is true for philosophy and history. While a sizeable proportion of German fellows were recruited from both disciplines, their disciplinary environment was not taken into account. In the case of philosophy, this meant that contacts were established with Freyer but not with Martin Buber, Ernst Cassirer, Helmut Plessner, Hans Reichenbach or Paul Tillich.

A comparison with the situation in Austria – or the Netherlands, where the distinguished historian Johan Huizinga was the fellowship advisor – clearly shows that after 1929, the national Memorial representatives were decisive for the RF field officers’ perception of, or blindness to, the local characteristics of these disciplines. Professor of Modern History Pribram sponsored psychologists such as Charlotte Bühler or ethnologists such as S.F. Nadel, Robert Heine-Geldern and Christoph Fürer-Haimendorf, while the German Committee and its Secretary Fehling were unsuccessful in their selection of fellows as well as unwilling to look for candidates beyond the circles of those students who rallied around the influential mandarins.

Half a year after Kittredge’s report was written in 1932, any plans for enhancing RF commitment in Germany had become obsolete. As a result of Nazi accession to power, RF protégés were dismissed or driven into exile while RF officers found themselves saddled with the delicate task of deciding which of those scholars that did not leave Germany could still be sufficiently trusted to conduct research that was politically unbiased.

## THE NAZI PROBLEM

Even before January 1933, RF officers were attentive observers of political developments and public sentiment in the countries where the RF was operating. Discrimination against Jews, and also against women, was noted in diaries and other documents, as were the obstacles faced by those fellows who returned.

The first returning researcher whose concern over German developments was mentioned in Van Sickle's diary was a French fellow. Georges Gurvitch returned 'very depressed' from his stay in Germany and Italy:

Feels that Hitlerism is sweeping Germany and that union of Germany, Russia and Italy and a European War not unlikely – or else civil war in Germany. Fears for our whole western civilization. He regards the difference between the Social Democrats and the middle parties as practically unbridgeable. The S.Ds want economic and industrial democracy and will scrap the Republic if necessary to get it. The middle parties, on the other hand, want economic and industrial authority and will scrap the Republic if necessary. Meanwhile Hitlerism thrives. Northern Italy is full of Germans and they are almost all Hitlerites. 85 per cent of the student body at Heidelberg are fascists. Everywhere one talks of an early revision of the Peace Treaties – war is praised.<sup>45</sup>

Until January 1933, RF officers confined their observations to paper in order to keep one another, as well as RF officers in New York, abreast of developments in Europe. After that, decisions were increasingly influenced by their evaluation of the situation in Germany and the developments to be expected. Most of their interlocutors who had stayed in Germany did not seem overly concerned. At the end of February 1933, Van Sickle, somewhat enervated, notes:

[N]or have I found two people who agree as to the immediate future. Significant is it, however, that Professor Jäckh, a Left Liberal, and Professor Spiethoff (Bonn), a Right Conservative, are agreed that developments will be orderly. Spiethoff looks for a Right consolidation that will destroy communism, restore religion, and force Germany to live within its means. Jäckh looks for a Hitler Government with Papen and Hindenburg the real masters, capable of restraining the excess of National Socialism.<sup>46</sup>

A few weeks later, Van Sickle informed the Director of the Social Sciences Division in New York that the Paris Office was assailed by refugees enquiring about potential aid, and asked what he is supposed to do. In the same letter he also reported a proposal of the Secretary of the German Fellowship Committee arguing that it would be 'highly useful' to make 'some local grants which would not directly involve the Foundation' to support a number of 'outstanding German Jewish scholars' who had been dismissed.

He feels that such grants should only be made to men who are prepared to stick it out in Germany. He does not approve of the exodus so early in the struggle, and he is glad to note that many of the very best men are staying to see the matter through ... Fehling

states that the government has let it known that while it cannot officially retract and replace the evicted scholars upon the public pay roll, it would be far from adverse to seeing them taken care of through private or foreign resources.<sup>47</sup>

Of course this is plain nonsense – but was it so in the spring of 1933? There is no indication whatsoever that Fehling, after January 1933, took to ingratiating himself with the Nazis. Like many others who were not subject to direct persecution, however, his reaction was a mixed one, partly laying the blame, as people will do in times of confusion, on the victims themselves and on their illusionary hopes. Fehling's trust in the good will of the government and the Nazi elite (rather than in the strength of their political opponents, or goodness knows what) is something one is least inclined to hold against him given that he was raised in the frame of mind that is characteristic of the authoritarian state. That this sentiment could be contagious, however, becomes apparent in the last sentence of Van Sickle's letter to Day: 'The individual cases are extraordinarily depressing. We should not forget, however, that during the past fifteen years the Jewish liberal element has definitely favored in Germany, and that they have, as a result, attained to a situation which inevitably produced a reaction.'

Given the enormous efforts made not only by the RF as an organization but also by all of its officers to come to the aid of those who had been driven out of their country by the Nazis, it would be unfair to set too great a store by this remark. As an illustration of the, at least temporary, disorientation and the readiness to fall back on easy explanations that is typical for this kind of situation – and what could be easier than blaming the victims? It is nevertheless depressing.

The impression that the European RF officers were more or less at a loss as to what to do is further emphasized by the unusual frequency of letters being sent to New York. In the spring of 1933, Van Sickle was obviously near-desperate in his endeavour to find a way to continue support in Germany. In his letter of 8 May 1933, once more to Day, the questions raised clearly outnumber the suggestions made. Van Sickle was much more clearly aware than his German informants that there was no future in 'objective scientific social science research' in Germany. The government, he argued, was extremely nationalist and anti-liberal. Some means of publication might survive because the government was not interested in them, but what would become of the students, and would researchers dare to publish anything that was contrary to the 'prevailing religion' in the first place? Although the prestige enjoyed by the sciences in Germany was immense, they had surrendered completely and without protesting. If freedom of speech and academic freedom was suspended, the RF would have to discontinue its support, but a way should be found to make clear to all thinking Germans how severe a blow their country's science had received.

To withdraw now would not produce the desired effect. To the public at large and to many university professors such action would be tantamount to our saying that

the Jewish scholar and the Weimar Republicans alone had our confidence; that we doubt the ability of a 'pure' German to do objective work. Withdrawal, on the other hand, after a further cooperation would not be liable to such an interpretation.

As a conclusion, Van Sickle raises the question of whether RF policies towards Germany should be the same for all RF Divisions or whether each Division should decide for itself.<sup>48</sup>

The case of Germany's most important beneficiary of RF subsidies, the Institut für Weltwirtschaft at the University of Kiel, may serve as an illustration of how the RF was attempting to maintain its support. It also shows how hopes for niches where RF protégés in Germany might be able to survive were thwarted by the Nazis from both inside and outside the universities.

On 28 April 1933, Van Sickle reported to New York that the Director of the institute, Bernhard Harms, had got into trouble with local Nazis, but that the Prussian government and the government of the Reich were still supporting him. A few days later, Harms told Van Sickle over the telephone that he had been summoned to Berlin to explain his defence of his Jewish collaborators. Two days later, Harms again called Van Sickle to tell him that the Prussian authorities as well as those of the Reich had been 'entirely sympathetic'. A note of the same date says that Gerhard Colm had been relieved of his duties, but had not been dismissed, that Hans Neisser wanted to leave Germany and that Jakob Marschak had asked if it would be possible to transfer his RF-supported workplace to Geneva. Given Harms's reassuring information, Van Sickle suggested that the RF should make an 'emergency grant' to the Kiel Institute to help it overcome its budgetary problems, with explicit mention that they risked withdrawal, thus strengthening Harms's position.

In May, visitors from abroad affirmed that the political situation in Kiel was difficult, but that working conditions were good. At the end of the month, Harms was reported to have been confirmed in his position while a new man – 'Jessen, Nazi' – had been put in charge of finances and agriculture. Neisser was in England where he conducted research with funds from Kiel, and Colm, while relieved from his duties, was still paid by the institute. By mid-June, the RF Paris Office was informed that Harms had left Kiel, information which was corroborated a few days later by Harms himself who told Van Sickle that he had been dismissed, as of October 1933, from his position as a Director but would keep his professorship in Kiel, and that his successor, Jens Jessen, had the economic, organizational and human competencies needed for the job. In his report to New York, Van Sickle tried to make things seem not quite as bad. Harms, he wrote, had resigned of his own free will and had designed Jessen as his successor because the latter was the only Nazi-party member among the professors. Van Sickle took up negotiations with Jessen over ways to continue RF support. In this, Jessen tried to rope in Van Sickle by promising to hire a former RF fellow. After consultations with Day, Jessen was informed that previously approved annual installments would be continued, but that no 'emergency grant' would be given. Day approved a special fellowship for Harms.

In the meantime, Alva and Gunnar Myrdal had put in a three weeks' stay in Kiel and on their return to Sweden sent a 'strictly personal and confidential' detailed report to Van Sickle in Paris.<sup>49</sup> This six-page letter typed in narrow lines is important not only as a document by two eye-witnesses, but also, and no less so, because both Myrdals showed great sympathy for the Kiel Institute and their German colleagues but, unlike Van Sickle, were under no pressure to act.

Kiel, Königsberg and Breslau, they wrote, had been converted into 'political universities' by the Reichsministerium. The Kiel Institute would doubtlessly be of major importance, and to this end, a change of staff had occurred, not as a result of 'revolutionary action' but through regular dismissals. Most of these changes, however, would not take effect before the summer semester of 1934, and dismissal from Kiel did not necessarily mean that those concerned could not get a position elsewhere. One of the professors who had been dismissed from Kiel was criminologist Hans von Hentig, who had closely cooperated with the institute's sociologists. One of the professors who had been newly appointed, besides two or three rather less competent 'sympathizers', was Hermann Bente from the University of Königsberg. The Zentralstelle für Hochschulforschung (Centre for Higher Education Research), where Svend Riemer was working, would surely be closed down. The future of business cycle research was uncertain without its director Colm, but as to the fate of junior staff in general, nothing had as yet been decided.

After this overview, the Myrdals reported on 'face to face talks' with institute staff and characterized the personality of their interlocutors: 'As a rule people have been extremely open-hearted, both as to their opinions on the general situation, on their personal hopes and fears, and on their estimation of each other.'

Harms had been the only one with whom they had, for obvious reasons, avoided discussing these 'vital problems'. All the others had confirmed that he was courageously fighting for the freedom of the institute, 'even when facing the guns'. He had sacrificed his own position to help others survive which, however, had by now turned out to be of no avail. There was no way of knowing what he really thought about the situation. 'Anyway he is out of influence.'

They had had a 'frontal discussion' with the new man, Jessen. As a person, he had impressed them as 'honest but naive, a jugendbewegt puritan type'.<sup>50</sup> He was well capable of considering things from a moral standpoint, but this by no means made him less dangerous, because he was 'quite a man of principles', 'doctrinaire as to the Nazi ideas of race and nationale Gesinnung [views] as first requirements'.<sup>51</sup>

Gunnar Myrdal had taken a look at some of his publications and had discussed some technical issues with him. His conclusion is that 'compared with international standards' Jessen was 'not a very prominent scholar'. Jessen was an important man within the Nazi movement, an unofficial adviser of the Kultusministerium (Culture Ministry) and rather close to the Führer. Some considered him to be the 'Robespierre' of this revolution. He saw himself as one of the few men 'inside the policy of the new state'. His fanaticism kept him

from acting with caution and a long-term perspective; in the name of the 'holy ideal' he would be ready to execute any policies whatsoever. Since he was definitely more strongly committed to other ideals than academic freedom, and since he was capable of rationalizing any given motive, vengeance and a sense of inferiority being first among them, he would pass off even complete oppression as idealism. Since he had not previously held any position of power and was sticking to a moderate rhetoric, he might still grow with his new task.

The institute's Andreas Predöhl's scientific achievements were quite good, albeit in a very narrow field. He was a diplomat, the Myrdals said – somewhat later in the letter, the meaning of the term is specified: 'He is going to conform.' That he remained with the institute was seen as one of the positive points, but he was not a 'very strong guarantee'.

The two other professors at the institute were nice, but naive and had no backbone. Among the assistants, the Myrdals explicitly mention Heberle, Walter Egle, Walther Hoffmann and Riemer. Surprisingly, Heberle had so far not been harrassed. He was one of the few German sociologists who knew what data were. He would in all likelihood have to leave Kiel, but they thought he would still be able to find another position in Germany. Egle was, for the coming years, provided for thanks to a RF fellowship; he had accomplished some good work, and although he seemed to be rather desperate, he would be able to conform to the new Germany. Hoffmann was an intelligent junior economist who could be expected to be able to direct a programme of business cycle research. However, as a former socialist, he would come up against major difficulties.

Svend Riemer was 'somewhat of a discovery'. He was one of the most brilliant junior sociologists and, in addition, well-versed in economics and statistics. The Myrdals praised Riemer's competencies in much detail and warmly recommended him for a RF fellowship which, however, he was not awarded until five years later (see Chapter Two). Riemer emigrated in 1934 with the help of the Myrdals who, having felt from the start that this step was inevitable given his Jewish wife and his socialist past, employed him, as a first measure, at their Stockholm Institute. Riemer's manuscript on working-class university students that Alva Myrdal had read during her stay in Kiel in the summer of 1933 could no longer be published in Germany, but seems not to have been published as such by Riemer in exile, either.

All in all, Kiel presented itself to the Myrdals as staffed by a rump faculty, facing major succession problems because of the lack of acceptable Nazi economists, and housing a group of young people who risked forfeiting not only their jobs but their entire academic future. Academic freedom was jeopardized by political considerations on four levels: choice of research issues, choice of the facts and data to be published, choice of senior researchers and choice of scientific staff. 'This point about the Nachwuchs [younger generation],' the two Swedes add, 'we consider to be the most devastating element in the situation as its effects reach far into the future.' They did not feel called upon, they said, to proffer advice to Van Sickle and the RF, but a major reason for

maintaining RF commitment in Germany, as they saw it, would be the opportunity to bring to bear an influence on the scientific development in that country.

In the summer months of 1933, Jessen repeated his request for an 'emergency grant', and Van Sickle again informed him of RF refusal. Less than two weeks after this letter had been expedited from Paris, Harms got in touch after a three-month silence and urged Van Sickle to grant some kind of support to Jessen, saying that he had had a long talk with him and could 'entrust his life work to J. with full confidence'.

In the end, the RF trustees refused all payments to German institutions with direct links to the Nazi government. Financial aid to individuals, in contrast, was still possible.

When Van Sickle visited Berlin in February 1934, an agreement was reached with Fehling that Jessen should be prevented from making yet another attempt to obtain an 'emergency grant'. (This little episode illustrates the RF's preference for preventing potential applicants from applying rather than impress the public by large numbers of rejected applications, as many institutions for the advancement of science did in later years. Thus, they not only kept clear of the obviously undesirable and awkward situation of having to turn somebody down, but also acted economically with respect to their officers' workload.)

In 1934, Van Sickle transferred to the New York RF Headquarters where he worked until he left the Social Sciences Division in 1938. His tasks in Europe were taken on by Kittredge. In July 1934, the latter visited the Kiel Institute, where he also met Harms who had returned from his stay abroad – made possible by an RF fellowship – and who was still fighting for his institute. Jessen, he reported, had been suspended from his directorship, and there was a possibility that Predöhl would be appointed his successor. Predöhl would not accept this position unless he was given free rein in running the institute and its research programme, as well as granted a budget by the government which would be sufficient not only for maintaining the institute but also for conducting at least a limited research programme. A few days later, Predöhl, who already held the position of a pro-rector in Kiel, was indeed appointed Director of the institute.

Negotiations between Kiel and the Paris Office continued as before. Fehling campaigned for support. The new Director, RF ex-fellow Predöhl, reported on the friendly atmosphere of the talks conducted in Berlin about the institute. He proposed to recruit another RF ex-fellow, Gerhard Mackenroth, and shortly thereafter had to admit that Leo Drescher, also an ex-fellow and recruited on similar motives by his predecessor, could no longer be employed. Drescher himself reported difficulties concerning his *Habilitation*.

One cannot help thinking that some tacit consensus had been reached between the American patrons and their German clients on a way to let the Kiel Institute have at least part of the money the New York trustees had no longer been willing to grant. By limiting support to the extensible 'old boys' network' of Rockefeller fellows, they hoped to be able to maintain the quality of research and to establish a kind of reinsurance even though this implied creating a world of revolving doors.

All RF officers seem to have disregarded the tenor of the resolutions issued by the trustees of their foundation in numerous ways. In collaboration with the Director of the Kiel Institute, Predöhl, whose appointment to this position was surely not due to the fact that he had held a three-year fellowship by the American foundation beginning in 1925, they funnelled many minor sums to those ex-fellows who rallied around the Kiel Institute and accepted all new nominations. While between 1925 and 1932 there had only been three nominations of fellows by the Kiel Institute, the number doubled after 1933, which at any rate further contributed to easing the stress on the institute's budget since no salary needed to be paid while someone was abroad. These sums would have to be added to the direct financing if the real volume of RF support was to be established.

Second, the European RF officers ignored the Myrdals' suggestions about those junior collaborators of the institute, as well as those who had to leave after 1933, whom they thought particularly deserving of support. Riemer finally got a fellowship when he was in Stockholm, but this seems to be due to the activities of his Scandinavian patrons rather than to any specific support by RF officers. All the other names mentioned in the Myrdals' report disappeared from the scientific scene.

Third, Van Sickle, Kittredge and Day – the latter by not stopping his colleagues – also dismissed the Myrdals' misgivings about scientific content and research policies. After Jessen's retreat from the directorship of the Kiel Institute, Predöhl enjoyed the Paris RF officers' unconditional confidence. Only after the end of the Second World War is there a note on Predöhl's political stance. Talking to a RF officer, exiled RF ex-fellow Friedrich Lutz pointed out that Predöhl, probably not from his innermost conviction but due to sheer opportunism, had been a Nazi until the end.<sup>52</sup> This can be assumed to also apply to all the others who pursued a career in Kiel during the Nazi period, since this university was one of the strongholds of Nazi science policies (Janssen 2000).

Fourth, in the end, all these efforts were of little effect: a majority of Kiel economists emigrated, if not in 1933 like Colm and Neisser, then still in the 1930s as one by one, Rudolf Freund, Egle, Riemer, Drescher and Heberle left Germany. Among those who stayed, the founder and long-time director Harms, as well as Karl Stephans, who had worked in Kiel as a guest researcher for a certain time, died before the outbreak of the war.

The line of action followed by RF officers in Europe did not go unchallenged by their New York hierarchy. When in September 1937, Kittredge addressed a long letter marked 'personal,' and arguing for a continuation of RF activities in Germany, to Syndor H. Walker, who had served the Rockefeller foundations since 1924 after two years of working for a Quaker European aid programme, the differences of opinion became obvious:

The Paris officers were unanimously of the view that the Foundation's program should ignore, so far as possible, political considerations. Where interesting and important



work could be supported, the Paris officers were unanimous in urging that grants should be made for such work in Germany as well as in any other countries of Europe. The present situation probably renders inadvisable large grants, and necessitates particularly careful examination of individual proposals. The officers felt however, that it would be mistake to refuse assistance to work in Germany merely because the government happens to be of a type which does not win the personal approval of the Trustees and officers of the RF. It was agreed that the limitations on work in the SS field are greater than in other fields.<sup>53</sup>

In the margin, beside the sentence underlined by Walker, there is a handwritten comment: 'rather mild characterization'.

### THE AUSTRIAN CASE

In September 1930, John Van Sickle once again visited Vienna and dined with Ludwig von Mises at Restaurant Schöner. His diary provides the brief note: 'further discussion of the proposed Institute'. The discussion, as further diary entries on a talk with Alfred Pribram clearly indicate, was not about the future of von Mises's Konjunkturforschungsinstitut (Institute for Business Cycle Research), but about the founding of a new one. The project proposed by the two Vienna confidants had obviously strongly appealed to Van Sickle, since he noted a week later: 'I spent the remainder of the period in Lausanne working up a report on the proposed Viennese Independent Institute for Social Studies.'<sup>54</sup>

In a letter to Edmund E. Day in New York, Van Sickle reported on the initiative:

In view of the low level to which the university is sinking such a project is appealing – doubly so to me because of my affection to Vienna. [Pribram] has approached me confidentially, so that the matter is not yet on record. I have replied discouragingly as you will see from the enclosed copy of my letter, without completely closing the door.<sup>55</sup>

He tried to persuade Day to come to Vienna: 'It seems to me that you ought to know conditions there first hand.' At the end of September, he sent Pribram's proposal to Selskar M. Gunn, as well.<sup>56</sup> At the time, however, Van Sickle did not manage to convince his superiors. Another memorandum Van Sickle had sent to New York at the end of October was definitely rejected by Day:

your memorandum of October 27th ... Free University at Vienna. Gunn and I talked this item over at some length. It did not seem to either of us that it would be wise to provide Foundation support for such an organization in Vienna at this time ... I personally share your opinion that some of the best existing personnel, as well as some of the best traditions, are to be found in Vienna. But for the present it would seem unwise to make any move there which would not divorced from highly charged political implications.<sup>57</sup>

Half a year later, at least one of the superiors had changed his mind. In early May 1931, Gunn reported in detail on his visit to Vienna in the preceding

month. He informed Day that he had altered his opinion and now approved Van Sickle's suggestion of supporting the Vienna project. This shift, he said, was brought about by the fact that obviously no reasonable results could be expected from any direct dealings with the University of Vienna.<sup>58</sup> As a result, Day sent a telegram to Paris on 20 May: 'Van Sickle Approve further study situation Vienna but with careful explanations that no grant is assured stop.'<sup>59</sup>

Thus the Vienna researchers could now be encouraged to set up a request, which they did in less than two months. In early June 1931, Van Sickle was already back in Vienna to discuss the founding of the institute: 'The formal request for aid for the Institute of Social Sciences in Vienna will be forwarded very shortly.'<sup>60</sup>

The Austrians kept their promise, submitting a six-page 'Memorandum on the situation of Research in Social Sciences in Austria' well within the time limit they had been given.<sup>61</sup> The memorandum provides a detailed description of the situation in Vienna and explains the conception of the planned institute. The document is dated 'July 27, 1931', and is signed by Friedrich August von Hayek, Alfred Francis Pribram, Ludwig von Mises and Richard Reisch, all of whom except for Pribram were members of the Konjunkturforschungsinstitut.<sup>62</sup>

In a first step, the authors deplored that Austria lacked private sponsoring for the social sciences, while state funding was almost exclusively dedicated to university teaching. After the end of the war, due to general impoverishment and the influence of party politics, which presented a special danger for the social sciences, the situation had further deteriorated. What little means were available were administered by organizations with a more or less obvious political orientation, and with a tendency to dedicate them, understandably enough, to such ends as served their party line rather than abide by scientific criteria. At present, there was no support upon which independent and unprejudiced social science research could rely. The support given to the Konjunkturforschungsinstitut by the RF had enabled it to launch a number of promising studies, but the field was necessarily very narrow. Researchers in other fields of economics and, more generally, in the social sciences suffered from being unable to commit themselves exclusively to their research work.

This general and no doubt justified lament is followed by some explanations about the situation at the universities and in the various social science disciplines. The following issues were said to be promising for fruitful research:

- Social history: 'We still need a comprehensive work on the share which nobility, clergy, state-officials, bourgeoisie and labour had in the cultural and social development of the old Empire.'
- Sociology: serious studies addressing the problems arising from the 'racial and national mixture of population in Central Europe'.
- Economics: Vienna is a city that had produced the greatest number of theoreticians worldwide. But 'as soon as it comes to the question of verification of the theories', the lack of funds prevented them from pursuing their work.

- Political science: the transformation of an autocratic to a democratic community as well as its effects on social and economic policies were worthy of more in-depth studies.

For sociology and political science, the situation was particularly awkward since in both disciplines the lack of university professors tended to 'turn away' those young people who might be interested in the issues listed above 'because they do not find the slightest encouragement or aid in their study'.

The new institute was to make grants to young people to allow them to engage in full-time research work. An annual salary of \$500 (\$7,000 in 2010) each would be quite sufficient, and sponsorship should extend to two years for about twenty individuals. Considering how perfectly this proposal answered the conception initiated and pursued by Beardsley Ruml, one wonders whether its Vienna authors had not been given a hand in setting it up.

The next visit to Vienna by a RF representative did not occur until February 1932. The visitor was Gunn, accompanied by Van Sickle. In a three-page letter (dictated, but not revised, hence the spelling mistakes) to RF President Max Mason, Gunn reports his Vienna impressions:

So far we have met with nothing but gloom and almost despair on the part of some people [i.e. Prof. Pribram]. No one apparently can guess what will happen, and no ray of light is apparent. Pribram seems to think that ... Austria ... is doomed to become a 'bread and cheese' country, typically Balkan, and he sees its cultural development dwindle to a very low point ... The University has been having student troubles and has been shut for some time ... The difficulties were nationalistic and anti-Jewish in character ... Austria cannot afford to support three universities. Innsbruck [sic] and Gratz [sic] could be dispensed with as far as the needs of the country are concerned, but local pride, tradition, etc., keep them going despite the difficulties. There is overproduction of trained men in all directions. Seventy per cent of the doctors and lawyers are Jewish ... I see no feasible program for the Foundation at Vienna. Aid to certain institutes or individuals may be desirable on occasion in the future.<sup>63</sup>

Van Sickle and Kittredge persevered in their efforts to secure support for Vienna, and the impression one gets, though unfounded in terms of actual evidence, is that the two RF officers made common cause with their Vienna protégés against their Paris and New York superiors.

Charlotte and Karl Bühler succeeded in securing support on their own, independent of the more comprehensive plan. Charlotte Bühler could avoid waiting for the financial approval of the common institute but had not failed to evoke the possibility of cooperative work with, for example, the Vienna ethnologists.<sup>64</sup> This has repeatedly been described in detail and need not be reiterated here.<sup>65</sup> Charlotte Bühler could even win over Gunn who, although as a rule he was more hesitant, thought fit to point out in a letter to Day that 'they [the Böhlers] are a couple who it would be difficult to beat. Non-Jewish, but liberally minded on the Jewish question.'<sup>66</sup>

In the autumn of 1932, Kittredge stayed in Vienna for some time. He conscientiously informed the RF of Pribram's concern that the Austrian science system could no longer be maintained by its own means, and unfailingly stressed the latter's request: 'he hopes that the Foundation will continue its interest in the Social Sciences in Vienna and will find it possible, sooner or later, to make a substantial contribution toward maintaining and developing the work now being done'.<sup>67</sup>

The seizure of power by the Nazis in Germany brought little change, for the time being, to Austria. Van Sickle and Kittredge interviewed a number of Austrians about the future of economic research in Germany. Mises's comment on the situation in March 1933 is the typical expert's mix of clairvoyance and resentment: any intelligent economic research was likely to come to a standstill for at least one generation, while the Nazis would impose their own economic theories, which were based on incorrect premises. Intellectual freedom would be abolished and, more generally, a 'legal confiscation of Jewish property' by capitalizing on income tax regulations was to be expected.<sup>68</sup>

As a consequence of the change that had occurred in the neighbouring country, chances for setting up the new institute in Vienna even seem to have improved. In the beginning of October 1933, Van Sickle sent a long letter to Day in New York<sup>69</sup> reporting a talk he'd had with Pribram. Pribram had again referred to the proposal transmitted by Van Sickle two years previously and had insisted on his belief that this institute was 'the most constructive thing we could do down there'. This was followed by some very detailed propositions, beginning with the names for the institute's board of directors, which would include five persons:

- |       |   |
|-------|---|
| Aryan | Prof. Richard Rei[s]ch, former President of the Austrian National Bank, representing Economics. |
| Jew-  | Aryan Prof. Mises or Prof. Hans Mayer, representing Economics.                                  |
| Aryan | Prof. Karl Bühler, representing Psychology.   |
| Aryan | Prof. Verdross, representing Law and Political Science.   |
| Jew   | Prof. Pribram, representing Modern Social and Political History.                                |

According to Pribram, it is more than ever important that the Institute should be independent of the University. The majority of the professors there are frankly Nazi. The directors of the proposed Institute would all be members of the university, but they are all Liberals and independent. There would only be one or at the most two Jews in the Direction. It might be well to get in an Anthropologist-Sociologist of the Koppers-Schmidt group [the Catholic priests Wilhelm Koppers and Father Wilhelm Schmidt].

The letter is reproduced here not only literally, but in its graphic layout in order to illustrate how much store Van Sickle, and presumably also Pribram, set by racial affiliation and how far they were prepared to go in making allowances for the general atmosphere in Vienna.

Van Sickle asked Day to re-read his letter of June 1931, arguing that while he still felt that the objections raised against the Vienna plan were justified,

'subsequent developments, especially in Germany, may have rendered them somewhat less serious'. The situation in Vienna was such, he wrote, that 'we may be justified in backing frankly the minority liberal element'. He suggested that annual grants of \$10,000 (\$158,000 in 2010) should be made for 1934 and 1935 and that for this period – with the Psychologisches Institut and the Konjunkturforschungsinstitut still benefiting from their own 'grants' – support should primarily be channelled to research work that was not covered by the programmes of these two institutions. Meanwhile, a 'habit of collaboration' might develop, and decisions as to whether to transfer support from individual institutes to the one and only 'Social Science Institute' or, inversely, to 'scrap' the latter, could be suspended until then.

Only a few days later, Van Sickle travelled via Heidelberg to Vienna where he stayed for more than a week. His report to Day is optimistic: 'The case for increased support here [i.e. Vienna] seems as strong as does the case for decreased support in Heidelberg.'<sup>70</sup> During lunch at Sacher's with Pribram, Alfred Verdross and Ferdinand Degenfeld-Schonburg, an agreement had been reached to form a committee for administering research funds independently from the university. The letter does not specify who was to be a member of this committee, but there is reason to believe that the three guests at Sacher's saw themselves in this role. This assumption is further confirmed by the list of research fields that were to be covered that had not previously benefited from RF support: 'Constitutional and International Law, Modern Political and Social History and Economic Policy'. Why projects in these disciplines should be able to conduct 'joint research ... on Vienna – somewhat along Chicago lines' remains unclear. Chicago, the centre of sociological research in the 1930s, was associated with a style of research that focused on urban problems as the starting point for, and object of, detailed sociographic descriptions; in Vienna, only Paul Lazarsfeld's *Wirtschaftspsychologische Forschungsstelle* (Austrian Research Unit for Economic Psychology) was familiar with this kind of research.

In contrast, Van Sickle's attitude towards Othmar Spann was quite unambiguous. He reported that he had advised Pribram to define the social sciences in his 'letter of request' so as to preclude 'support of the pure Romanticism and the vituperative propaganda of Spann ... yet permit support of precisely defined problems by younger scholars of the Spann School'. Van Sickle noted that the whole proposal involved some 'distinct hazards' but saw 'deep personal animosities' as their only source – which is definitely wrong. It was certainly erroneous to suppose that the conflicts between the rival groups of scholars were based on personal differences alone and had nothing to do with politics. They had their deepest roots in the fact that the protagonists belonged to different ethnic and social groups, as the 'Aryans' in particular were pointing out with increasing violence, and these conflicts could no longer be resolved by a 'tactful and impartial committee'. The inclusion of Degenfeld-Schonburg, who only a few years earlier had been the object of a rather disparaging comment by Van Sickle, actually showed that apart from the few university professors with whom the RF was already in contact there

was nobody else who was not predisposed to anti-Semitism. In this respect, also, the view reported by Van Sickle that this was something like a violent conflict between schools was inaccurate. Degenfeld-Schonburg had been the very professor who had flatly informed young Fritz Machlup that he was opposed to his *Habilitation* (Craver 1986a). By including Degenfeld-Schonburg in the circle of the 'beneficiaries', Van Sickle hoped to make it harder for him to persevere in 'his present destructive opposition to all objective liberal research':

Thus, if one of his men receives committee support, it would be harder for him to characterize as 'stuff and nonsense' another piece of work accomplished under committee auspices by a man of the rival marginal utility school, and to oppose his 'habilitation' at the university. To do so would be an affront to the whole committee.

Even if, Van Sickle adds with some resignation, his worst misgivings should come true, the RF had achieved two major goals: 'We shall have aided the social sciences in Vienna over a critical period, and we shall be in a better position to know what to do in 1935 when our existing grants to the Psychological and Business Cycle Institute terminate.'

In November 1933, Van Sickle dispatched the documents that were needed for an application to be processed to New York. The request was for US\$13,000, or 65,000 Austrian Schillings (\$213,000 in 2010) to be spread over two years, i.e. until the end of 1935, to be granted to a 'Committee for Promotion of Social Science Research – Vienna'. The reasons given were, among others, that after a recent visit to Vienna by the Assistant Director for the Social Sciences, i.e. Van Sickle, the latter had reported that the situation in Austria was 'promising', that the economic situation had improved, and that in the current academic year, Austrian universities would host about 5,000 foreign students (as compared to 7,000 foreign students in Germany). Furthermore, there was 'an abundance' of scientific personnel.

Degenfeld-Schonburg, Verdross and Pribram were members of the Committee. One of Pribram's students, Friedrich Engel-Jánosi, was to be its secretary, and ethnologist Wilhelm Koppers was to be included as a further member.<sup>71</sup> The Committee was to draw up a 'general research program in the social sciences', complementing the work done at the Psychologisches Institut and the Konjunkturforschungsinstitut. The 'field of international relations' was explicitly mentioned. The beneficiaries of the 'useful emergency support of the social sciences in Vienna'<sup>72</sup> seem to have been neither intent on nor capable of engaging in the kind of social sciences that the RF meant to promote under the generic term of 'inductive research'. Van Sickle, for one, seems to have been aware of this, since on being summoned by New York to give a more detailed account of the projects to be conducted, he was uncommonly vague: 'The recent history of Vienna, i.e. from ca. 1800 to date ... Verdross states that the opening of the Ballhaus-Platz archives gives access to a mine of information regarding the diplomatic history of the pre-war period, and the formation of

international law.<sup>73</sup> The primary goal, however, was to help the 'former fellows' who were an 'elite of young scholars', which would be a 'capitalization of the very large sums we have invested in their formation'.

An examination of the list of former Austrian Rockefeller fellows helps to clarify whom Van Sickle may have had in mind. About twenty Austrian fellows had so far benefited from fellowships. Of these, a few were ineligible for reasons of quality. Others had just been granted a renewal of their fellowship (Hans Mars, Paul Rosenstein and S.F. Nadel). Still others had obtained more or less secure positions at home or abroad (Edgar Foltin, Ludwig Fritscher, Alexander Mahr, Ewald Schams, Erich Voegelin and Elisabeth Ephrussi-Waal), or worked at one of the two institutes that were supported independently of this new project (Charlotte Bühler, Oskar Morgenstern and Gottfried Haberler), leaving Erich Hula, Leo Gross and Friedrich Thalmann among the older ex-fellows and Joseph H. Fürth, Roland Grassberger, Berthold Löwenfeld and Karl Stephans among those recently returned as potential collaborators of the new institute.

All this suggests that at this point, it was no longer possible – even considering what the term was understood to mean in the early 1930s – to set up a research association that could have reasonably carried the name 'social scientific'. Due to the lack of opportunities for studies in sociology and political science that had already been pointed out in the original memorandum by Hayek and co., the range of scholars who might focus on social science research was more or less limited to the collaborators of the Konjunkturforschungsinstitut and the Psychologisches Institut. Considering the conception of science that predominated in the interwar period, one might add the idiosyncratic Vienna version of ethnology as an institution capable of producing talents. However, these three institutions already received RF support albeit, at least partly, a rather modest amount. Thus, the new Research Committee was compelled to either allocate additional funds to collaborators of these institutions or fall back on such competencies as were represented by the Committee members themselves: modern history or, more specifically, the history of diplomacy, and law, in its more or less traditional variants.

Actually, neither the Vienna aficionados among the RF officers nor the members of the new committee need to have bothered since, as we all know, the political history of Austria took a different turn. The main question had become whether there still was something like 'reasonable freedom of research' in the first place. In January 1934, the request for support disappeared from the agenda of the RF Executive Committee. Apparently, American observers were much more in awe of a Nazi seizure of power in Germany than of the suppression of social democracy that occurred soon after in Austria.<sup>74</sup>

Remarkably, Van Sickle did not give up but doggedly fought on for his Vienna project. As a result, a number of informative documents were drafted, and some remarkable judgements were written down. At the end of January 1934, French economist Charles Rist, having been asked for an opinion, said that he knew Dollfuss<sup>75</sup> well enough to feel that an authoritarian regime

headed by him would be 'compatible' with 'reasonable freedom of research'. Van Sickle, elaborating on Rist's argument, suggested that a distinction should be made between 'good and bad authoritarianism'.<sup>76</sup>

Less than two months later, Van Sickle asked Day whether it would seem inappropriate to resubmit the suspended Vienna project, and expounded his view of the situation in Austria – and it can be safely assumed that this view was shared, if not inspired, by some of his Austrian confidants. Van Sickle, not unreasonably expecting to see 'a dictatorship tempered with Schlamperei [sloppiness]', felt that under these conditions social science research could go on and, ergo, be subsidized.<sup>77</sup> However, the New York headquarters, not at all willing to invest in a situation as unstable as this, flatly refused to reconsider the Vienna request.<sup>78</sup>

In April, Van Sickle was back in Vienna, this time reporting not on dinners, but on compromises. Pribram, he wrote, was the only one to be utterly pessimistic, which had to be put down to his age, bad health and race – 'naturally the Jews are the most uneasy'. Hula and Karl Bühler were radiating optimism, the former expecting a reconciliation among the classes since the Austrians were 'very real Christians', the latter reporting small results with the new Vienna rulers which, however, presently turned out to be the effect of an intervention by a number of Catholic scientists around Koppers and Schmidt who had pleaded for tolerance with respect to Bühler's work. The scholars, described by a defiant Van Sickle as 'members of the self-constituted committee for administering a Rockefeller Foundation fluid research fund', expressed their sympathy for the removal of their request from the agenda: placed in his position, they would have done the same. But all of them, as the author of this for once rather succinct report emphasized, had been unanimous in thinking 'that within wide enough limits freedom of research is assured'.<sup>79</sup>

Within one year, support for the Psychologisches Institut and the Völkerkundeinstitut (Institute for Ethnology) was discontinued, leaving the Konjunkturforschungsinstitut as the only one that regularly received RF funds under authoritarian state rule.<sup>80</sup> Understandably enough, the former beneficiaries were not prepared to take this lying down, and some of the bolder ones even tried to submit new requests to the RF. Although the pattern of RF officers being favourably inclined towards their Vienna partners' requests, and invariably being turned down by New York headquarters, persisted for several years, the group of professors who wanted to be invited to dinner increased. On 10 March 1938, Kittredge reported from Vienna on the proposals that had been submitted to him during his recent stay and listed all those with whom he had had discussions. Besides the familiar names of Pribram, Degenfeld-Schonburg and Hans Mayer, and of those fellows who were still living in Vienna ('Fürth, Hula, Mahr, Voegelin etc.), there were the newcomers Otto Brunner, Alphons Dopsch, the Director of the Konsularakademie Friedrich Hlavač, Heinrich Srbik, Richard Strigl and Stefan Verosta. It appears that somehow word had got to Vienna that the RF had meanwhile established 'international relations' as a priority for its sponsoring activities. At any rate,



Dr Anna Selig came up with a proposal for a 'Vienna International Study Center' that was to do research on the relations 'between German and non-German populations of Central and Eastern Europe' and to provide 'students from the U.S.A. and Western Europe' with a venue for their studies. In his report, Kittredge went on to say that Selig had secured 'strong backing from Catholic and Liberal groups in Austria and other countries' for her project, as well as fifty rooms in a wing of the Salesian Seminary for housing it; funds for purchasing the library of Josef Redlich would come from the Carnegie Endowment.

A few days later all this had become obsolete, and the only problem left for the RF to solve in Vienna was to decide on how short a notice the small grants already approved should be discontinued. The last installment to the Konsularakademie in the spring of 1938 was already remitted in Reichsmark, and aid for the Konjunkturforschungsinstitut was discontinued as soon as it became evident that it would cease to be anything but a branch of the Berlin Institute.<sup>81</sup>

Rather than support institutions in Austria, the RF now turned to the support of Austrian scientists in exile.

Initially, the establishment of an interdisciplinary centre for social science research in Vienna had not failed because its conception had been deficient or because there was a lack of funds, but because the rival cliques had suspiciously watched over one another to prevent preferential treatment, and in so doing behaved as if the aid the RF was prepared to give had been the stakes in a zero-sum game (which was the case only for state-administered research funding). When they had finally agreed on how to divide the pie, RF assessment of the political conditions for long-term commitment in Austria was so negative that it no longer came as a surprise when the plan of establishing a second 'world centre' for social science research in Vienna was repeatedly adjourned and, finally, abandoned. The two RF officers who had come to like their Austrian protégés had initially euphemized their reports and were now confronted with an ever-growing throng of petitioners whose scholarly expertise they were no longer able to assess, contrary to what they used to do for their early Austrian – and indeed for all – contacts. They had secured some material aid for a number of former fellows in Austria, but the great majority of former fellows as well as their patron Pribram had to leave the country in 1938 to go into exile. There, some of them could benefit from the relations established during their fellowships and did not need to draw on special aid for refugees while others were provided with more or less prolonged support by the RF.

# 4

## IN THE SHADOW OF NAZI RULE: TWO GENERATION UNITS OF SOCIAL SCIENTISTS

At the end of the first third of the twentieth century, German-language sociology suffered a dramatic caesura. For a long time, René König's dictum that after 1933 the discipline had been 'brought to a brutal standstill' (König 1958: 14) was accepted as the ultimate description of the impact of the Nazis' rise to power on sociology. Helmut Schelsky's contrary opinion, at the time, that 'it was our sociology itself that had run out of subject matter, the melodies had all been played through, the fronts were consolidating, and little evolutionary momentum was left within the discipline itself' (Schelsky 1959: 36), did not register. Decades later the debate resurfaced under the new heading of 'sociology in National Socialism' (Rammstedt 1985; Klingemann 1996). At the end of his life, König vehemently protested against what he felt to be an exculpation of Nazi sociologists (König 1987). The debate, fraught with polemics, criticism and counter-criticism, carried on for some time and ran dry without any consensus being reached. The present study is not aiming for consensus either, but proposes a new perspective. Analysing the career paths of the sociologists of the time may indeed shed new light on the repercussions of the year 1933 (or, for Austria, 1938).

First of all, a collective biography of German-speaking sociologists should identify the commonalities as well as the differences that marked this generation. Karl Mannheim's concept of *Generationseinheit* (generation unit) seems the adequate tool for drawing a comparison between those who emigrated and those who did not. In his essay 'The Problem of Generations' (Mannheim 1952), first published in 1928, Mannheim distinguished between generation, social location of a generation and generation unit. Given the 'similarity of location ... within a social whole', Mannheim came to the conclusion that a generation tends to show something like a 'characteristic mode of thought and experience' (Mannheim 1952: 291). Often, however, the similarity of location as such does not suffice as a basis for postulating a generational bond. For the latter to emerge, there must be something like a common destiny. Within a

generational bond constituted by certain political and historical facts, there may be contrasting generation units, differing in their way of coping with the same historical and political macro-event: 'Youth experiencing the same concrete historical problems may be said to be part of the same actual generation; while those groups within the same actual generation which work up the material of their common experiences in different specific ways, constitute separate generation units' (Mannheim 1952: 304). In the present case, the concept of separate generations clearly suggests itself.

Secondly, a comparison between two science cultures that can be identified in terms of nation states is also attempted here: situations in Germany and Austria were different, but the common language certainly made the inhabitants of these countries more similar to each other than to the inhabitants of other states. Table 4.1 gives a preliminary idea of these differences. While the number of universities and students is rather close to the base value of the population, the other ratios clearly differ. In Austria there were more university teachers, and the 1938 dismissal rate for Austria was slightly higher than the 1933 rate for Germany. There are remarkable differences in the last two cells, but one has allow for the fact that there have as yet been few – indeed, hardly any – contributions by Austrians to the editorial decisions of the publications (Hagemann and Krohn 1999a; Hagemann and Krohn 1999b; Smelser and Baltes 2001) used to estimate the figures.

**Table 4.1** Ratios between Austria and Germany

For every 100 Germans account for ... Austrians	
Population (1930s)	10
Universities (1930s)	13
Students (1930s)	15
Teaching staff (1930s)	30
Dismissed professors (1933 and 1938, resp.)	34
Grantees of the Emergency Committee (1933-44)	20
Émigré Economists (1933-45)	43
Leading Social Scientists (20 <sup>th</sup> Century)	77

Sources: Population: Mitchell 1992. Universities, Students and Teaching staff: Titze 1987; Völlmecke 1979. Dismissed professors: for Germany - *A Crisis in the University World* 1935; for Austria - Archive of the Society for the Protection of Science and Learning 1938. Grantees of the Emergency Committee: Files of the Emergency Committee in Aid of Displaced German Scholars, New York Public Library, Rare Book and Manuscript Division, New York; Duggan and Drury 1948; émigré economists - Krohn and Hagemann 1999; leading social scientists - Smelser and Baltes 2001. Author's calculations.

Third, to allow for the effects of the change of political regimes to show, and for individual career paths to be traced, a period spanning three decades from the mid-1920s to the mid-1950s was chosen for analysis here. The starting point is

early enough, with respect to the handing over of power to the Nazis, to allow us to establish the positions held before change set in, and the end is late enough, with respect to the time when most of the emigrants had left the German-language area, to allow us to reconstruct their careers after displacement. It furthermore enables us to account for the dangers faced by non-emigrants, such as military service and imprisonment, likely to have caused breaks in their careers.

Fourth, the following specifically concerns sociology. This is easier said than done, for while sociology as an intellectual project was not at all uncommon in the German-language area, it did not exist as a formal discipline, i.e. in an institutionalized form. One thus comes up against a number of difficulties when trying to establish the boundaries of sociology as a discipline-in-the-making. Doing so horizontally among the social sciences – delimiting sociology from other disciplines – is difficult since the latter were far from being clearly outlined. In the German-language area, the emergence of the ‘third science culture’ (Lepenes 1985) was different and slower than that of other countries. As for the date that marks the beginning of sociology as a scientific discipline, there is no consensus at all.

## WHO IS A SOCIOLOGIST?

Right after the problem of how to delimit a discipline from its neighbours, there is the problem of whom to count among its members. Who makes up the very top of the status hierarchy is just as uncontroversial as the very bottom, the students. Full professors seldom become ignored, while the many graduates of a discipline are rarely given any more room in its history than is needed for some statistics. But what about those in between? Limiting investigations to sociologists with a university affiliation or, even more restrictively, to those who were actually qualified as lecturers or professors, would be too narrow a definition of the population, since it means excluding a number of scholars who today are definitely considered part of the history of sociology.

Nor does it help much to consult the ‘membership list’ of the Deutsche Gesellschaft für Soziologie (DGS, German Sociological Association, Austrians included) to identify German-speaking sociologists in the first half of the twentieth century. The DGS existed from 1910, but since it conceived of itself as an association of notables, eligibility for membership was severely restricted. And since there is no other membership list of the proto-discipline, the only way to establish the population is to reconstruct it on the basis of contemporaneous classifications and later delimitations. This may be done by (a) relying on self-reports; (b) using an independent definition as a basis for a reconstruction of the group; (c) referring to similar attempts by others; or (d) relying on reports by contemporary or latter-day peers.

### (a) Relying on Self-reports: Anyone Calling Him or Herself a Sociologist is a Sociologist

One way of defining sociology is to say that it is what sociologists do. So, as an analogy, one way to count the membership of this discipline may be to label as

sociologists all those who claim to be so. An excellent contemporaneous source for this is *Kürschners Deutscher Gelehrten Kalender*. Between 1926 and 1950, six issues were published of this 'Who's Who' of German-language scholars, and sociology is indeed included in the index of disciplines in the 1926 issue for the first time. A search of the indices of names in those six issues of the *Kürschners* results in a total of two hundred and seventy male and seven female (2 per cent) sociologists. Almost half of the names appear only once, one-fifth appear twice, and only thirty-five individuals (12 per cent) appear as often as four times or more within these twenty-five years. Several reasons may account for this marked discontinuity. Apart from the obvious – the exclusion of those scholars who had become undesirable for political reasons due to the change in regime – there is of course the natural mortality of the population, as well as the war-time death-toll (fifty-nine of those indexed had died by 1950, but for almost a hundred other individuals, no information on their year of death is provided). Another correlation that seems reasonably sound is the one between age and the frequency of appearances. The oldest subjects (those born before 1870) can be disregarded as well as the youngest (those born after 1901). In the three cohorts in between, the rate of those who appear up to three times in the *Kürschners* is 72 per cent for the oldest and 92 per cent for the youngest cohort.

For the 277 sociologists who had at least once wished to be reported, distribution over the six issues is curiously unbalanced. The highest frequency of sociology reported as a disciplinary affiliation is found in the 1935 issue; the lowest in 1950. The (multiple) disciplinary affiliations over a quarter of a century reflect a change in the position of sociology among the disciplines from which sociologists and also-sociologists (that is, those who reported sociology as one disciplinary affiliation among others) used to come: the three most frequently reported neighbour disciplines were economics (about one-fifth), philosophy (with an upward tendency, rising from one out of ten to one out of four reporting this discipline as their first affiliation) and law (with proportions fluctuating around 15 per cent). During these decades, German-language sociology was seen as part of the humanities, with its boundaries remaining unclear, because German economics was dominated by the Historical School and also conceived of itself as part of the humanities.

A contemporary source confirming this judgement is the *estschrift* (a book of articles honouring a colleague) presented 'in the name of German science' by Walter von Dyck and Adolf Harnack, among others, to Friedrich Schmidt-Ott in honour of his seventieth birthday in 1930 (Abb 1930). This memorial publication took the form of an outline of the achievements of German science in the preceding fifty years. Among the disciplines included, neither sociology nor economics are to be found, both being subsumed under the heading of *Staatswissenschaften* (political science), the achievements of which are portrayed by one of the members of the very committee that was charged with the selection of fellows for the Rockefeller Foundation (Chapter Two), Hermann Schumacher. While the latter believed that 'political economics is the core

element of *Staatswissenschaften*', he still felt *Staatswissenschaften* to be a quite adequate generic title for the discipline since in Germany 'economics were far from being just a matter of individuals but also a state responsibility'. In the more than twenty pages of his essay, sociology is mentioned only once, and even then only in passing:

Max Weber's peculiar role in *Staatswissenschaften* may perhaps be summed up by saying that in him, in a most precise way, the scientific aspirations of the time became a conscious endeavour. Deeply immersed in a number of individual tasks, he contemplated the question of what might be the real meaning and worth, in terms of knowledge, of scientific work in history, political economics, and sociology. [...] With considerable power, and often disconcerting boldness, he has extended his doctrine of '*verstehende* sociology,' primarily confined to rational action, to cover all domains of human community life, even religion and music (Schumacher 1930: 144).

Another remarkable finding is the near-total eclipse of psychology as an academic neighbour for, and origin of, also-sociologists. After the Second World War, the picture abruptly changed: now almost two in three sociologists included in the *Kürschmers* report sociology as their first or only disciplinary affiliation, followed by philosophy (one in four).

Of the individuals reported in the *Kürschmers*, 83 per cent were born in Germany, 12.5 per cent in Austria (both of them in their historical boundaries). 89 per cent had taken their doctoral degrees at German universities, as opposed to only 10 per cent at Austrian universities, suggesting an ongoing brain drain towards Germany during this period, which dates farther back than the period covered by the survey (three out of four doctorates had been taken before 1925). In all the *Kürschmers* issued within the period surveyed, biographical articles include an entry specifying the place of residence for the qualifying date. These entries reveal a constant proportion of 80 per cent Germans, as opposed to 13 to 19 per cent of sociologists working in Austria; the remaining few reported a place of residence abroad. For obvious reasons, the 1940 issue of the *Kürschmers* included only citizens of the Third Reich, with only three former Austrians with a Vienna address<sup>1</sup> who had wished to be included. In 1950, the proportion of Austrians is reduced to 10 per cent while the second largest group consists of scholars who lived in non-German-language countries. Of the latter, only two succeeded in reintegrating at a German university in the following years, but the fact that the others reported back, as it were, calls for closer examination. (These were Theodor Geiger, Hans Gerth, Gustav Gundlach, Rudolf Heberle, Paul Honigsheim, Ernest Manheim and Hellmut Plessner; not a single Austrian wished to be included in the first postwar *Kürschmers*.)

The distribution of sociologists by status position (Table 4.2) shows a consistent proportion of a quarter to almost one-third holding the top academic career level of full professor; when the range is extended to include the highest-but-one level as well (*außerordentliche Professoren* [extraordinary professors], not including guest and honorary professors, emeriti and retired professors), the proportion of professors rises to almost 50 per cent before the handing-over of

**Table 4.2** Sociologists in the *Kürschners*, by Status Positions (%)

Status Position	1926	1928	1931	1935	1940	1950
<i>o. Professor</i> (= Full Professor)	30	25	32	28	28	31
<i>ao. Professor</i> (= Associate Professor)	18	10	16	10	23	17
<i>Dozent</i> (= Lecturer)	25	17	17	18	15	14
Faculty, total	73	62	65	56	66	62
<i>Assistent</i> (= Assistant Professor)	0	1	1	2	6	7
Honorary-, Visiting-, Emeritus etc. Profs	2	6	8	14	17	14
Non-university occupations	25	41	27	28	11	17
N.	56	109	131	160	53	42

Sources: *Kürschners Deutscher Gelehrten-Kalender 1925-*. Author's calculations.

power to the Nazis. At the next lower level, the lecturers (*Dozenten*) show a steady increase in absolute numbers and a no less steady decrease in their share of the population. Between 1926 and 1935, however, the number of those who had qualified for an academic career (*Habilitation*) doubled. While the overall number of sociologists declined during the Nazi dictatorship, the rate of sociologists who were *habilitiert* steadily increased. Since the rate of those who are known to have emigrated was comparatively low in this group, the decrease must be attributed to the dwindling appeal of sociology during the Nazi period rather than to exile.<sup>2</sup>

#### (b) Reconstruction by Definition: You Are a Sociologist Because We Say So

Rather than relying on self-reports, another way to define a group is to rely on certain criteria that are simple and comprehensible. Given that the discipline under investigation was still in the making, so to speak, institutional criteria such as lists of university personnel are of little use, and reliance on information about graduates is impossible since there was no formal course of studies at the time. What seems uncontroversial, however, is to consider as members of a discipline-in-the-making all those who contributed to its corpus of literature. Thus, anyone having published at least one article or two reviews in one of the sociological journals between 1925 and 1955 shall be considered a member of the discipline. This assessment was initially based on all journals with a 'sociological' title or a review section with a specific sociological sub-section, all in all thirty-six journals,<sup>3</sup> but the first analyses resulted in such a high number of authors that the field had to be narrowed down. The journal sample was thus restricted to include only the data of 197 individuals who up to the point of their possible emigration had worked exclusively in Austria, and of 68 individuals (or 26 per cent of this group) who up to the point of their possible emigration had worked in both Austria and Germany. The latter, being citizens of dual nationality, are a hybrid case with respect to their national culture. For the present study, anyone who had lived or studied in Austria for at least two years is considered an 'Austrian'. The criterion for inclusion in the

sample is, therefore, based not on arbitrary characteristics such as place of birth or nationality, but on institutional affiliation.<sup>4</sup>

A corollary of this narrowing down of the analysis to journal articles and reviews might be that scholars who only wrote books are under-represented. However, it seems rather unlikely that the subjects concerned should confine themselves to this sort of text. By taking contributions to a journal as a criterion and, thus, lowering the threshold for inclusion, only those authors are ruled out whose merits as a writer did not pass the test of an editor or publisher's scrutiny. Furthermore, one may suppose that, in the beginning, emigrants found it easier to write an article in the foreign language than to publish a book.

The sample does not include the highly productive and successful so-called 'second generation', those who had to emigrate in childhood or adolescence and had therefore not been exposed to the influence of Austrian tertiary educational institutions (all those included were born before 1920).<sup>5</sup>

The selection criteria were designed to allow for as little bias as possible in favour of either of the two sub-populations (emigrants and non-emigrants). The main selection criterion – journal articles that were labelled as sociological by their contemporaries, and reviews of another author's sociological work – is very broad, which means that individuals are included who, in a different perspective, would not have been counted as sociologists. I feel, however, that this flaw is defensible since it does in a way reflect the facts about the sociology of those years.<sup>6</sup>

Checking the list of individuals included in the journal sample one finds that a number of names are missing. Psychologists who had only published in psychological journals are systematically under-represented; the same is probably true for some representatives of modern history since there were no journals that specialized in modern history during the period surveyed and the more general historical journals were not included in our analysis. Apart from the systematic discrimination against these two disciplines, however, no further discrimination should have occurred.

A collective biography that is not confined to the members of the academic establishment, or the elite will soon come up against a number of limiting factors that can hardly be overcome. Missing entries in biographical reference works, contradictory information in different sources and similar adversities tend to reduce the scope of analysis more than one would wish for. As a case in point, retrieving information on social backgrounds turned out to be particularly difficult.

The journal sample includes 200 emigrants, 5 scholars who endured long-term imprisonment in Nazi concentration camps and thus were prevented from emigrating (although most of them had been imprisoned on political grounds, all of them, with the exception of Eugen Kogon, had to wear the star of David in the camps), and 59 who remained at home, including 16 individuals who had at least temporarily been suspended from their positions during Nazi rule, and some who had been in prison for a certain time. They were discriminated against as losers in the competition for the favours of Nazi leaders



(Othmar Spann and his followers) or because they had exposed themselves in the authoritarian regime in Austria before 1938.

The high number of emigrants cannot be accounted for unless one keeps in mind that they became professional social scientists only after their escape from Europe. But for the annexation of Austria by German troops, heartily welcomed by the Austrians in March 1938, most of these later sociologists are quite unlikely to have taken to sociology. In the mid-1950s, there were probably five university teachers in Austria who were engaged in sociology in the broadest sense, but almost ten times as many former Austrians in positions of a 'full professor' of sociology at American colleges and universities. Since in our journal analysis special care was taken to preclude preferential treatment of one of either group, one is reasonably justified in saying that the ratio of 1:3 'home-guards' to emigrants correctly mirrors the scale of the displacement of intellectual potential.

### (c) Using Comparison: How Others Defined Sociology

In his doctoral thesis, Klemens Wittebur (Wittebur 1991) tried to come to a systematical identification of the group of sociologists in exile from Germany. He based his 'biographical cartography' on a very broad definition of sociology and used a multilevel criterion that covers the whole range from 'top' to 'bottom', from sociological chairs and inaugural dissertations to literary contributions. As a minimum condition for inclusion, candidates had to have graduated before emigrating and to have subsequently engaged in professional or literary work in Germany as sociologists in the broadest sense. Of course delimitations are easier among the high-level scholars than among those who were only included because their publications had been classified as sociological work. A certain amount of arbitrariness is unavoidable, but at least Wittebur went beyond high-level positions in his count of sociologists. He ends up with a total of 7 (or 5 per cent) female and 134 male sociologists. For obvious reasons, the 141 individuals reported by Wittebur also include 10 who may be counted as belonging to the group of emigrated Austrian sociologists.<sup>7</sup>

### (d) Relying on Peer Judgement: Experts Nominate Sociologists

Another way of identifying the population of German-speaking sociologists is to refer to nominations by experts. The rules governing their choice may be quite restrictive, as is the case for the *International Encyclopedia of the Social & Behavioural Sciences* (Smelser and Baltes 2001) which is limited to 140 all-time most important names, or rather liberal and inclusive, as is the case for a biographical reference work.

In the present context, a German publication called the *Internationales Soziologenlexikon* (ISL) (Bernsdorf and Knospe 1980–84) has the advantage that it was not compiled in a perspective of emigration research. Among the scholars included in the second edition, 292 were selected because they were born after 1850 and before 1920, and thus corresponded to the criterion of age used for the other samples. In spite of its cosmopolitan title, the ISL is more comprehensive than others in its charting of the German-speaking area. The sample obtained

from the ISL includes 275 male and 17 (6 per cent) female sociologists, 77 per cent of them German, 12 per cent Austrian, and 11 working in both or in other countries, primarily in Czechoslovakia; 122 individuals (42 per cent) of the sample are emigrants, with distinctly higher emigration rates for Austrians than for Germans (64 per cent and 39 per cent, respectively).

The comparatively smaller data set of German and Austrian Rockefeller fellows already referred to in Chapter Two was used as a supplementary source. For our purpose here, RF officers and advisers can be regarded as contemporary peers whose problem – unlike that of the latter-day peers who served as experts for the ISL and Smelser and Baltes's encyclopedia – was to find promising candidates. To a certain degree, this small sample counterbalances the age bias of all the retrospective selections. By 1941, eighty-three Germans, twenty-eight Austrians and eight scholars who could be assigned to both countries had been granted a fellowship by the Social Sciences Division of the Rockefeller Foundation (or its predecessor, the Laura Spelman Rockefeller Memorial), among them nine female scholars (8 per cent).

Due to their experience of life abroad, the former Rockefeller fellows and later emigrants can be viewed as a privileged group. After all, in 1950, of the seventy-one German and Austrian prewar fellows for whom some information is available on this point, thirty-two were in the United States, three in Great Britain, four in non-European countries and two in Switzerland. In contrast, only twenty-five (of forty-six) ex-fellows were back in Germany, and only five (of twenty-five) were back in Austria. The emigration rate (to non-German-language countries) for this group was 55 per cent (Rockefeller Foundation 1951).

Of the thirty-two individuals who had held Rockefeller fellowships between 1925 and 1941 and had later transferred to the United States, twenty had at some point taught as 'full professors' at universities or colleges (two each at Berkeley, Chicago, Princeton and UCLA), while of the twenty-seven former fellows who returned to Germany or Austria, nineteen Germans and only one Austrian were affiliated with institutions of tertiary education.

In 1972, the RF published another list of its former fellows. Of those sixty-one individuals who had taken up their fellowship before 1941, thirty lived in the United States, three in Great Britain and three in other countries (ten had died in the meantime). Twenty-four of them lived in Germany, and only one lived in Austria. The long-term emigration rate for Germany and Austria (excluding those who had died) was thus 41 per cent; the rate for Austria alone, however, was 95 per cent (Rockefeller Foundation 1972).

In the following analysis, data sets from different sources will be analysed as an aggregated set as well as, in certain cases, separately. All these samples together can be taken with reasonable certainty to provide an overview of the population of German-speaking sociologists in the 1930s and 1940s. Of course, there are a number of individuals who arguably belong to the population thus constituted but do not appear in any of these samples; since they were discovered quite accidentally, as it were, they will not be taken into account in the following.<sup>8</sup> It would turn things upside down to say that between 1925

and 1955, roughly speaking, there were 823 sociologists, male and female, in the German-language area. It is correct, however, to say that the population from which sociologists could be sampled – according to criteria that would have to be justified in each case – was at least that large.

For validating the five samples, another data set can be used. This is the reconstruction by David Vampola, Fritz Ringer and Philip Seidel in the *Göttinger Hochschullehrer-Untersuchung* (Göttingen Survey of University Teachers) that provides individual data of more than 13,000 German university teachers across all disciplines between 1864 and 1938. (As suggested by Ringer, this study is cited as the VRS [Vampola Ringer Seidel] Sample.)<sup>9</sup> The VRS Sample includes 51 social scientists who also figure in the Wittebur sample (36 per cent of all those included there), and 108 social scientists who figure in the ISL sample (or 37 per cent of this group of sociologists). That there is only a partial overlap is due to the fact that while the Göttingen study did a complete survey for each qualifying date, there were only nine such dates (Ringer 1993).

### ELEMENTS OF A COLLECTIVE BIOGRAPHICAL PORTRAIT OF GERMAN-SPEAKING SOCIOLOGISTS

A first comparative analysis of the five samples, or of the aggregate data set for which they provided the basis, is shown in Table 4.3. The proportion of female sociologists is highest in the journal sample (ten per cent), which can be explained by the fact that inclusion in this sample was based on the existence of publication in German- or English-language journals. Emigrated female scholars obviously met with favourable conditions in the United States, whereas the sample in which inclusion was most strongly oriented to Central European institutional conditions (*Kürschners*) is also the one that most strongly reflected discrimination against women. The markedly higher rate of female Rockefeller fellows (9 per cent), however, clearly shows that in the academic universe of the time, female scholars worth supporting could well be found.

**Table 4.3** Overview of the Samples

	Kürschners	Wittebur	ISL	RF	Journals	Total
Women (%)	2	5	6	8	10	7
Emigrants (%)	33	100	42	64	75	58
Austrians (%)	13	0	12	24	74	29
Double Citizens (%)	5	7	11	7	26	9
Year of Birth (Median)	1886	1897	1899	1901	1900	1898
Cases	277	141	292	119	265	826

Note: The difference of 268 between the sum of all five samples and the number of cases in the overall data set (826) results from elimination of double entries.

Sources: *Kürschners Deutscher Gelehrten-Kalender* 1925–; Wittebur 1991; Bernsdorf and Knospe 1980–84. Author's calculations.

The minute female share in sociology before the mid-1950s must be seen in the context of the development of women's access to higher education. By the mid-1920s, the rate of female students was 13 per cent for Germany and 15 per cent for Austria. The rates for other European countries were hardly higher, with the exception of Switzerland where women were allowed university access early on, while the rate of female students in the United States was 37 per cent even then. In 1950, the rate of female students was 21 per cent for Austria and 20 per cent for Germany.

The situation in Vienna in the interwar period was, if not exactly supportive of women, at least less prone to discrimination than elsewhere. It is a sociological truism that role models are of extraordinary importance to members of a group that wants to gain access to social areas previously closed to them. In this context, it is instructive that Charlotte Bühler, herself one of these role models, investigated the importance of heroic models for the project of biographical identity in her psychological lifespan research. Legends, however, are much less easy to circumscribe than true role models. While the latter show those who try to emulate them how to perform the role they are interested in, the former are as often as not idyllic figures and, while no doubt admirable, of little help when it comes to the next steps to be taken on one's way to unknown life-worlds.

Now, 'Frau Professor', as Charlotte Bühler was reverently addressed, was not the only female role model in Vienna. One of her predecessors, a legend already in the interwar period, was Eugenie Schwarzwald. In title-happy Vienna, she was addressed as 'Fraudoktor' and signed her name with 'FrDr'. A German philologist and graduate from Zurich University, she had founded a *Gymnasium* for girls in Vienna as early as at the times of the emperor. Until 1938, this school was something like an institutionalized role model for girls, its guiding principles quoted as follows by one of its former students: 'Here, girls were to learn everything that made up the knowledge of men and, at the same time, remain gentle, modest, girlish, housewifely' (Spiel 1989: 56). Many a later female student of the University of Vienna had been to this reform school, among them Else Frenkel-Brunswik, Hedda Bolgar and Lucie Stein-Varga, whose names figure in the journal sample.

A third female role model in Vienna was Käthe Leichter. Given that women were denied access to the University of Vienna at the time, she had taken her doctoral degree at Heidelberg. In the early 1920s, Leichter rejected an offer from Carl Grünberg to accompany him to Frankfurt to work at the newly opened Institut für Sozialforschung, since she had been offered a post as head of the new Department for Women's Affairs with the Vienna Arbeiterkammer (Chamber of Labour). A social democrat, her interest was in opening up a way for young women who lacked formal education to gain the kind of freedom such education would offer. Among the collaborators on her *Handbuch der Frauenarbeit* (Handbook of Female Labour; Leichter 1930) were a number of young women who rose to the political elite of postwar social democracy after being lucky enough to survive the Nazi dictatorship.

While the Vienna of 1930 offered at least some women-friendly niches, another variable is more difficult to establish with any certainty. By the definition of the Nuremberg Laws, Bühler, Leichter and Schwarzwald were Jewish. Leichter was murdered in 1942 on these grounds (Fleck and Berger 2000: 89). Schwarzwald managed to escape to Switzerland, in spite of her advanced age, where she died in 1940, and Charlotte Bühler was spared Gestapo treatment in the days of the *Anschluss* only because she happened to be abroad. What is much less clear is whether any of these three saw herself as Jewish before this civilizational caesura: Leichter had seceded from the Israelitische Kultusgemeinde in 1921 (Fleck and Berger 2000), Charlotte Bühler was of Protestant denomination, and Schwarzwald, while documented as being of 'mosaischen' (Jewish) faith, declared herself to be 'frankly anti-Semitic' (Schwarzwald and Deichmann 1988: 229).

These three women did not belong to any Jewish intellectual milieu, or whatever labels may have been in circulation. Moreover, there definitely was no such thing as a Jewish milieu, neither in fact nor in spirit. In Vienna as elsewhere, Jews congregated as Jews in the synagogues, and it is highly unlikely that what was being discussed in the synagogue had anything to do with the subjects for which the Jewish intellectual milieu was famous. Most of the fans of the soccer club Hakoah would have been Jewish, but there is no tradition telling us whether the players developed any specifically Jewish style. While with soccer – in spite of its having been discovered as a worthy object of cultural studies – nobody to my knowledge has ever ventured to see anything Jewish in the way the Hakoans kicked their ball, countless authors indulge in just these kind of questions as soon as they turn to the loftier products of intellectual life.

While there is no doubt as to the questions that need to be asked here, the same cannot be said for the validity of the answers that are currently given. To start with the simplest ones, is it true that there was a disproportionate share of Jews in certain intellectual domains? Is it, furthermore, true that these domains were among the innovative ones with respect to scientific discourse? Finally, is it true that certain intellectual products are clearly marked by Jewish thought, or at least show a stronger family likeness to the latter, as compared to the Christian worldview? While such questions can be more or less clearly put, no answer will ever be found since their very form is counterfactual.

In contrast, questions which aim to establish whether the share of Jews was larger in certain professions or scientific domains than in others seem more promising. But even here, while it seems quite likely that German philology or mechanical engineering were courses of study or fields of professional activity that were comparatively rarely chosen by Jews, one cannot entirely, and not even approximately, be sure of this. We know nothing about this because it has never been investigated, and even if attempts had been made, no satisfactory results would have been obtained since we simply do not have any data to go by. For whatever the answer might be, it cannot start out from the end of the causal chain by pointing out, for instance, that among those evicted by the Nazis in 1933 and 1938, respectively, the share of professors of German

philology was minute. It may well be that Jews who were interested in this discipline were at any rate prevented from reaching top positions, or that many Jews, aware of the discrimination that reigned in the field, chose to opt for their second favourite discipline in the first place. And, to make things still more complicated, aversion to Jewish candidates in German philology need not have been more intense than in, let's say, political economics or psychology. A lack of Jewish professors in a particular discipline may simply have been the result of the longevity of some professors. Where there are no vacancies, there also is no need to discriminate against those who are undesirable.

The term discrimination may be used when an individual is deprived of something because of his or her membership in a certain group, as measured by commonly shared normative standards. Cases in point are the many restrictions faced by blacks in the American South under segregation, or, more recently, the impossibility for a homosexual to become the leader of a Christian-conservative party. Though the term discrimination might also be used when members of a specific group are under-represented in certain professions or institutions, such as women among university professors, one feels uneasy with the idea that a prerequisite for reaching gender democracy should be a balanced rate, for example, of male and female prison inmates. This uneasiness can be transformed into a rational argument by pointing out that the term discrimination should only be used where groups are kept from obtaining something they want or, to say it even more philosophically, where someone is prevented from the pursuit of his or her happiness (with imprisonment counting as a pursuit of happiness only where it provides an escape from even more miserable conditions of life).

However, since this is discrimination by statistical means, so to speak – statistical because the proportion of group members among the excluded exceeds the proportion of group members in the waiting queue – not every member of the group discriminated against may refer to it in order to boost his or her individual demand for promotion to a desired position if the latter is bound up with certain requirements in terms of competencies, as is the case for professorships but not for seats on a bus.

Jon Elster (Elster 1983; Elster 1984) has pointed out that people who think they are denied access to something coveted and auspicious may accommodate, as in the fable of the fox and the sour grapes, by describing as undesirable that which is out of their reach. For example, we know that the non-admission of Jewish students to the duelling fraternities, which started by the end of the nineteenth century (Jarausch 1984), led only a minority of the excluded to respond by founding duelling fraternities of their own. Most male Jewish students are likely to have resorted to a sour-grapes strategy, whose byproduct in terms of cultural history was that they were quicker than their duelling colleagues to abandon the feudal idea of honour for a more universalistic idea.<sup>10</sup>

Evidence of discrimination in appointment policies is hard to find, and for a rather profane reason. At the time, appointments to professorships were not initiated by launching a job advertisement but by charging some members of

the faculty in question with gathering information on who might be eligible. For someone engaged in the history of science, this pre- (and, by the way, also post-) modern procedure has the disagreeable side-effect that appointment processes simply remained undocumented.

This deplorable lack of documents is not confined to the level of scientific micro-environments; it holds for official statistics as well. For Austria, which at the time was not yet called Austria, the 1910 census is the last one to include data on the rates of Jews in certain professions (cf. Rozenblit 1983). For the period that is of interest here – the subsequent years up to the seizure of power by the Nazis – no reliable statistics are available. Resorting to simple extrapolation can be ruled out for a number of reasons: as the monarchy was disintegrating, its populations (among them a particularly large share of Jews) were swept up in large-scale migration. In addition, the end of the monarchy was accompanied by a profound structural change within professional groups, with the result that Jewish professionals, mainly lawyers, doctors, engineers, architects and journalists, now faced competition with those who had themselves left or been dismissed from civil service positions or could no longer count on such positions as a career for their sons. Jewish fortunes, probably no less than non-Jewish fortunes, had been ruined by the First World War and the inflation years. But above all, Vienna Jews still sought to assimilate, so that even if there had been any official statistics, the proportions of Jews reported in them would not have mirrored actual social facts, especially with respect to those professional groups from which most university students were likely to come. If the Jewish lower-middle class was hit as hard by economic problems as the rest of the population, their children suffered from it no less than the non-Jewish population. The result would have been a decrease in the proportion of students from this social stratum, and the proportion of university students who did not complete their studies would very likely have risen as well as the proportion of those who, rather than stick to risky career aspirations that required long-term preparation, turned to career paths that promised short-term income maintenance. Evidence for all this is provided by individual cases, and there is no reason not to assume, with due caution, a more general trend.

Lacking official statistics, one has to make do with estimations. The proportion of Jews in the professions can be obtained by comparing the occupational statistics of three censuses. In Austria, two such censuses were taken in 1934 and 1951, and one was taken in May 1939 as part of the census of the German Reich, new territories included. The diminishing numbers of the working population between 1934 and 1939 are revealing since there is every reason to believe that those 'leaving' these occupations were those who had been labelled Jews by the Nazi race rulings and were, therefore, persecuted.

As an illustration, we may take the rather clearly defined (in terms of social statistics) professions of the *doctor*, the *lawyer* and the *journalist* (Fleck 2004). Estimations of the proportion of Jews in these professions before the *Anschluss* are a maximum of 15 per cent for doctors, a maximum of 60 per cent for lawyers

and probably one out of two journalists.<sup>11</sup> The picture obtained by comparing the various censuses clearly differs from the reports of contemporaries who were more or less unanimous in thinking that the proportion of Jewish doctors was higher. In any case, estimates suggesting that Jews made up about one-third of the members of the professions can be supposed to be quite realistic.

The information gap on the level of official statistics is matched by a corresponding gap on the level where one might attempt to find some data on the personal characteristics of the 823 sociologists. Even if they were registered in one of the reference books, these still provide little of the information one might wish for as a sociologist. Information that can be included in the most down-to-earth questionnaire and that respondents will usually not hesitate to answer is rare when one looks for it with respect to those who are long dead or, if still alive, far away. As for their ethno-religious or social backgrounds, the lack of data is almost total. The *International Biographical Dictionary of Central European Émigrés 1933–1945* (Röder and Strauss 1980–83), the *Bibliographica Judaica* (Heuer 1981–96) and the *Handbuch österreichischer Autorinnen und Autoren jüdischer Herkunft 18. bis 20. Jahrhundert* (Handbook of Austrian Authors of Jewish Origin, 18th–20th centuries; Blumesberger 2002) provide reliable information on whether an individual was Jewish.<sup>12</sup> The criteria used are broader than those relying on current denominations but narrower than the historically significant trait of whether or not an individual could fall victim to the Nuremberg Laws on race. Data on their denomination (in most cases data from their university years) could be obtained for 30 per cent of the 823 sociologists. Thirty-nine individuals (or 16 per cent of those for whom data are available) belonged to the Jewish religious community. Two hundred and sixty-six individuals are reported as Jewish in a broader sense in the three reference works, with distinctly higher proportions among the Austrians than among the Germans: Austrians make up 29 per cent of the entire data set, 55 per cent of them being Jewish, whereas Germans make up 62 per cent of the entire data set while only 45 per cent of them are Jewish. Given the numbers referred to above of the presumed proportion of Jews among doctors, lawyers and journalists (ranging from 15 per cent to 60 per cent), a Jewish proportion of one-third to one-half among German-speaking sociologists seems plausible.

The proportions of *emigrants* strongly vary between the samples, but these differences are relatively easy to explain: Wittebur documents only established German scholars who later became emigrants. The criteria for the journal sample include American publications, the main host country for scholars escaping the Nazis. In other words, emigrants from Austria encompass an extraordinarily large number of individuals who 'became sociologists' only after their emigration. The large proportion of emigrants is, thus, not a consequence of some sampling bias, but a spectacular result. If Wittebur had used sources from the host countries, he would probably have come up with larger numbers, too. Since, on the other hand, our more fine-tuned screening does not discriminate against the 'home-guard', the high Austrian emigration rate



cannot be explained away, even though it should not be taken as an exact measure.

The Rockefeller sample also includes a high proportion of emigrants – and this, again, is not due to any bias. The reason is no doubt bound up with American institutional conditions where the gates of academic institutions were clearly open to refugees, a fact which even Wilhelm Bernsdorf and Horst Knosp were compelled to at least partially acknowledge by opening up the ISL for emigrants in 1984. That both editors did not set great store by gender equity is manifest in the fact that they readily included twice as many females who remained in their country than female emigrants, even though there is no reason to suppose that visibility for female scholars among their male colleagues was any easier to achieve in the Federal Republic or in Austria. A comparison between the two groups shows that the ISL seems to have ignored emigrated female sociologists until their visibility could no longer be denied.<sup>13</sup> The fact that the various editions of the *Kürschmers* report only slightly less than one-third of emigrants also suggests that most of the emigrants could make a name for themselves only after their emigration.

Variations in the shares of Austrians in the samples are less sizeable. The lack of Austrians in Wittebur has to do with the simple fact that he chose not to include all German-speaking sociologists in his sample; he only looked for sociologists who went into exile from Germany. The comparatively large number of Austrians among the RF fellows reflects a fact that has been repeatedly emphasized in the present context, namely that Austria produced an incomparably higher number of talents in those years. Due to their stay abroad (planned to be temporary) they could build larger social networks, the 'weak ties' (Granovetter 1973) of which could later be reactivated in more than one way.

Economist Oskar Morgenstern's contacts to American economists, established during his fellowship stay in the mid-1920s and enhanced in the years to follow, resulted in an invitation to a half-year guest professorship at Princeton in 1938; that March, with little hesitation, he decided not to return to Vienna. Christoph Fürer-Haimendorf, a disciple of the Viennese ethnology, not only met his future wife during his fellowship stay in Britain but, through her, also came to realize his loathing for the Nazis:

Shortly before our wedding Hitler had occupied Austria, and Vienna was no longer the easy going city of my youth, where I had moved in cosmopolitan circles and my closest friends other than Austrians had been, Bulgarians, Poles, Hungarians, Greeks and other foreigners. I realized that neither Betty nor I would be happy in a country ruled by National Socialists. So we planned to leave as soon as possible for India and awaited further developments in Central Europe. (Fürer-Haimendorf 1990: 21)

Although Morgenstern and Fürer-Haimendorf may not have planned to leave their home country for good, it can be assumed in their cases, as well as in a number of others that are comparable, that they would certainly have gone abroad anyway at some point or other.

The German-language social scientists targeted in the present context belong to more than one *generation*; their years of birth span a period of seven decades. The various samples reflect different cohorts, as shown by their mean age: the *Kürschners* represents the oldest age groups, while all the other data sets focus on the younger ones. This is hardly surprising since the *Kürschners* is something like an ongoing chronicle of the personnel of academia which, after all, is dominated by the senior age groups. The differences in mean age between the samples clearly suggest that the emigrants belonged to the younger cohorts. This becomes quite manifest when the group of the emigrants is compared to the group that remained in their home countries: the mean difference in age between emigrants and the 'home-guard' across all samples is five years; in the journal sample, the mean difference is eleven years (*Kürschners*: three years; ISL: two years), and even among the Rockefeller fellows, a group that was quite homogeneous with respect to age, the mean difference of age between those who emigrated and those who did not is two years. If the great majority of emigrants in all samples were refugees from the Nazis, the difference in age as compared to the 'home-guard' should actually not be that large. It can therefore be assumed that the younger ones – provided the opportunity presented itself, and regardless of what other reasons they may have had for emigrating – were quicker to opt for emigration than the older ones.

A further differentiation among the population of German-language social scientists can be obtained by analysing the *places* from which they graduated (post-emigration grades excluded). In 1930, the number of sociologists taking their doctoral degree more or less corresponds, at most universities, to the proportion of the student population of the respective universities. These numbers, however, are lower in Munich and Bonn, and higher in Berlin, Heidelberg, Leipzig and Frankfurt. The four latter universities can be considered the hubs of social science training. With respect to Heidelberg, this is not at all surprising since this university, located in southwestern Germany, had a long-standing reputation as a stronghold of German social science; the strong presence of Frankfurt, however, is more of a surprise given the rather recent foundation of its university (in 1914), and particularly so when compared to Kiel which, while frequently pointed out in the literature as a university with a strong social science orientation, has only half as many students taking their doctoral degree there and being included in our samples. While the German social scientists grew up in a science culture with a federal structure, the Austrians were concentrated in the metropolis, Vienna.

The right-hand column of Table 4.4 shows the proportion of emigrants among those who took their doctoral degree at each of these universities. The university with the highest proportion of future emigrants among its graduates was the University of Frankfurt, where eight out of ten students who took their doctoral degree later went into exile, followed by Vienna and Heidelberg, where three-quarters of their respective 'sociological' graduates were lost to emigration. Assuming that Nazi persecution of Jews was the primary cause of emigration suggests that the proportion of Jews (or, more precisely, the

proportion of those who were predestined to fall victim to Nazi extinction policies) must have been particularly high at these three universities. By contrast, Berlin, which is often compared to Vienna in the literature, seems to have housed a lower proportion of Jewish students, since only 34 per cent of those who took their doctoral degree later emigrated – unless Jewish students in Berlin had felt more strongly attracted by other disciplines in the first place.

**Table 4.4** Ratio of Students per University, 1930 (Selection); Graduations by Members of the Samples; Ratio of Graduations to Country, Emigrants per University

University	Ratio of Students 1930 (%)	Graduations by Members of the Samples	Ratio of Graduations to Country (%)	Emigrants (%)
Vienna		175	88	77
Berlin	13	63	16	38
Heidelberg	4	56	14	75
Leipzig	7	38	10	39
Frankfurt	4	28	7	89
Munich	9	24	6	50
Cologne	6	22	6	41
Bonn	6	20	5	25
Freiburg	4	20	5	60
Tübingen	3	16	4	19
Kiel	3	13	3	54
All German		399	100	
Total		621		

Source: Students 1930 from Titze 1995; all other calculations by the author.

Interwar Vienna boasted a particularly high density of individuals and influences that were decisive for the socialization of social scientists (and future emigrants): there was more than one female role model and there was a broad range of educational options which, while at first glance do not seem to have been of first-rate sociological relevance, clearly proved to be fertile ground for a variety of social science orientations. Vienna's ethnology – highly traditional and, given its 'Kulturkreis' doctrine, even quite isolated – produced S. F. Nadel, who was of eminent importance for social science in spite of his untimely death, and exported Fürer-Haimendorf and Robert Heine-Geldern, who carved out careers for themselves in London and Washington, DC, respectively. The Austrian School of Economics, one of the earliest groups of German-language social scientists with a cosmopolitan orientation, not only produced a large number of talents but also exported virtually all of them, among them arguably the largest number of non-Jewish scholars from all the scientific domains considered in this context – what remained of economics in Austria after 1938 was scientifically irrelevant. Hans Kelsen, who departed in anger from Vienna to Cologne in 1929, also left behind a group of followers who

somehow managed to keep their heads above water until the Nazis took control of Austria, but did not sacrifice their intellectual life: Erich Voegelin, Erich Hula, Josef Laurenz Kunz, Leo Gross and William Ebenstein all worked mainly in the field of political science after their emigration to the United States, a speciality they had not even come close to knowing in Vienna (Söllner 1996). The group associated with the philosophers of the Vienna Circle and the mathematicians around Karl Menger not only produced some eminent scholars (such as Kurt Gödel) who are not at issue here, but also some philosophers and mathematicians such as Gustav Bergmann, Rudolf Carnap, Herbert Feigl and Menger himself, who later dealt with questions of a philosophy of the social sciences. Moreover, there were the psychologists mentioned above who, with few exceptions, distinguished themselves by combining their training in academic psychology (under the Bùhlers) with an interest in psychoanalysis.

What was the situation of those German social scientists who ended up as emigrants? Except for Berlin, no Vienna-like clusters of outstanding individuals seem to have existed. According to René König, who had studied in both cities (König 1984; König 1987), intellectual stimulation appears to have been less comprehensive and less appealing. There were hardly any sociologists at the University of Berlin, not in the sense that was current at the time, nor in any later sense. Each of the four *Kürschmers* editions published between 1926 and 1935 presents the same names of Berlin-based sociologists, supplemented by a growing number of scholars without a university affiliation.

In the larger disciplinary context of what was not yet called sociology, Berlin had some proponents of gestalt psychology who, unlike the Bùhlers in Vienna, were exclusively geared to their discipline. Berlin economics was controlled by staunch proponents of the Historical School who, due to their advanced age, were hardly practising anymore. There were no jurists with an interest in sociology, such as Kelsen, and ethnologist Richard Thurnwald was probably too often away on his expeditions – and, what is more, was only an associate professor – to have had a lasting influence on students. Berlin philosophy included a branch of neopositivism around Hans Reichenbach, but while this was obviously a potential force, it was just as obviously not exploited. The most innovative social science micro-environment was the Deutsche Hochschule für Politik (German Academy for Political Science), with many future emigrants among its members. Their orientation to the new empirical brand of social science, however, can be assumed to have occurred only after their enforced departure from Berlin (Söllner 1996). Significantly, the Akademie für die Wissenschaft des Judentums (Academy for Jewish Studies) where Leo Strauss worked between 1925 and 1932 was also an institution at the margins of the academic universe.<sup>14</sup>

The two other university sites known for their social science activities – Heidelberg and Frankfurt – are marked by an inverse asynchronicity of what was being done in the respective fields: while Heidelberg groaned under the oppressive weight of the great Max Weber, the two new Frankfurt institutions presumably did not exist long enough to develop a distinct profile. The Institut

für Sozialforschung (Institute of Social Research) had the additional handicap of preferring splendid isolation over the recruitment of students. In spite of Karl Mannheim, who contributed more to graduate output during his two-year stay in Frankfurt than Horkheimer (whose output in terms of doctorates was virtually zero throughout his first period of activity at the institute), the institute's overall 'productivity' on this measure was lower than that of Vienna, due to the very short time it was allowed to operate. That said, during their short Frankfurt intermezzo Mannheim and his assistant Norbert Elias also went a good way towards the integration of female students, albeit to a lesser degree than in the Vienna cases described above (Honegger 1990; Kettler and Meja 1993; Rubinstein 2000).

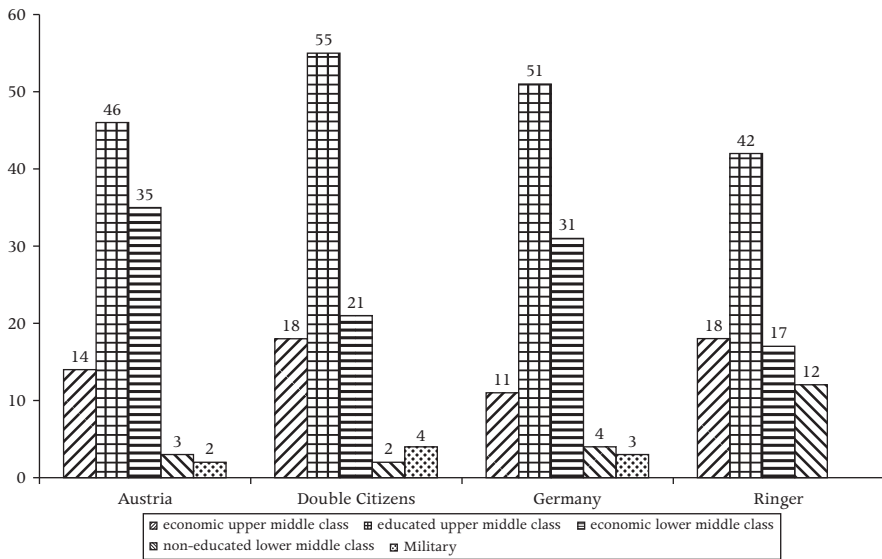
Displaced German social scientists' ties to their nation and culture seem to have been stronger than those of their Austrian counterparts, for they stuck to German as the colloquial language in their writings for much longer. One of the reasons for these marked differences is no doubt the way Jews experienced integration and assimilation in the Weimar Republic, as opposed to the Republic of Austria. The popular discourse of German-Jewish symbiosis can be taken to imply that German Jews wanted to be both German Jews and Jewish Germans. Their Jewish family background was marked by a strong identification with the German nation-state and, even more strongly, with German culture. That for many of them this proved to be their undoing after 1933 is a well-known fact. At any rate, there was no equivalent to this disposition among Vienna Jews.<sup>15</sup> Since at the time there was no such thing as an Austrian national consciousness (in terms of a self identity oriented to a nation-state framework) Vienna Jews could of course identify with German culture and recite Goethe, Schiller and Heine as well as the rest of their German contemporaries, but did not see this as a basis for building pride in Austria. To the young social scientists, old imperial Austria was, if not exactly hated, alien in many ways; the new Austria was identified, by those on the Left no less than by those on the Right, with the hypertrophic metropolis of Vienna, for which the former planned a Red future while the latter mourned for her faded glory. The rest of Austria was mountains and lakes, good for hiking trips and summer holidays.

The 'Vienna hydrocephalus', as the by then oversized capital for an under-sized republic was characterized at the time (and is so sometimes even today), was not only the European city – after Warsaw and Budapest (cf. Karady 1999: 33) – with the highest *proportion* of Jewish inhabitants, but this Jewish population was presumably also 'educationally advantaged', as this would later be called, or at any rate eager to be educated. This was true for the offspring of Jewish migrants from Eastern Europe as well as for those of well-established Jewish citizens. What else was there for them to do, after all, but to invest what remained of their melting economic capital in educational capital? In this, the key factor was probably not so much the hope for a 'return of investment' but the sheer lack of alternatives. Prolonged studies were one response to this, an extreme case in point being Bruno Bettelheim who spent the years between 1923 and 1937 studying philosophy and art history as a sideline to

his activities as a businessman. Others, too, can be said to have dedicated their early adulthood to the pursuit, at a rather leisurely pace, of scientific study, either as a sideline to a more or less reputable occupation, or in place of it. Alfred Schütz was a banker and, as such, lucky enough to dispose of a source of income that allowed him to pursue his interests as a philosopher and sociologist in his leisure time, which after all resulted in his *Phenomenology of the Social World*. Alexander Gerschenkron was surely not idle in the ten years between his doctoral degree and his emigration, but even his grandson was unable to find out, in his minute reconstruction of his grandfather's biography, what he actually did in those years (Dawidoff 2002). As for Hans Zeisel, we at least know that after graduating he earned his living first as a law clerk and then as a lawyer, and in his spare time engaged in political work, covered sports for the *Arbeiter Zeitung* (Workers' Daily) and engaged in further education in the social sciences.

These observations are confirmed by an analysis of the occupations of 350 Austrian emigrants (professional politicians not included). One out of five of these future emigrants had not yet finished their education, only one in five had to give up a university position, and almost one in seven reported journalist or writer as a profession. There were of course some Austrians who not only entered service as a journalist, but actually made a name for themselves in this line. Still, in terms of numbers they were no more than a handful (Gustav Stolper and Karl Polanyi being the best known among them). Both job titles are of almost as undetermined a nature as that of university student. Among the future emigrants, the estimated proportion of those whose occupational position was similarly transitory, or easy to abandon, amounts to almost 50 per cent. In opting for emigration, they could thus hope for a more promising future with respect to occupational security, in exchange for a poorly endowed present (Pinter and Scherke 1995). In Germany, where emigrated journalists such as Gustav Stolper, Siegfried Kracauer or Hans Gerth actually had to abandon a standing that was not as easy to give up as an occupation that served as a kind of social alibi, this seems to have been different.

For the VRS sample, Fritz Ringer attempted to analyse the social backgrounds of German professors. To this end, he used a four-level stratification scheme: university graduates, professionals, teachers in upper secondary school and upper-level civil servants were classified as the 'educated upper middle class' – the mandarins, in his terminology; industrialists and managers made up the 'economic upper middle class'; employees and intermediary civil servants were the 'non-educated lower middle class'; and small businessmen, shopkeepers and the like were categorized as the 'economic lower middle class' (Ringer 1993). In order to be able to compare our data with Ringer's findings on the occupations of the fathers of those professors whose first appointment to a German university occurred between 1864 and 1938, his classification was largely adopted (with fathers with a military occupation added as an additional category). The results, insofar as they can be supposed to reflect anything of social reality, suggest that the background of the social scientists in our sample is more clearly



**Figure 4.1** Comparison of the Social Background of German and Austrian Social Scientists (%)

Note: Ringer: Professors of the social sciences, appointed between 1864 and 1938; Germans, Austrians and Double Citizens: our calculation, born between 1860 and 1918. Classification according to Ringer 1993, in per cent of the different samples.

Sources: Ringer: data set David Vampola, Fritz Ringer and Philip Seidel, *Rekonstruktion der Göttingen Hochschullehrer Untersuchung von 1956 (VRS)*; Ringer 1993. Germans, Austrians and Double Citizens: author's calculations.

‘economic upper middle class’ and ‘non-educated lower middle class’ than that of Ringer’s mandarins (Figure 4.1). While the proportion of fathers with commercial and blue-collar occupations is somewhat lower, the proportion of fathers belonging to the educated middle class is somewhat higher. The first generations of professional sociologists clearly had a more distinct educated and middle-class background than the social science professors studied by Ringer.<sup>16</sup>

Two limitations need to be pointed out, however: The historical period covered by Ringer’s sample is longer, and the focus is on individuals who succeeded in obtaining a professorship, while the historical period covered by the samples in the present study is shorter (and the median of the year of birth is 1880 for Ringer and 1898 for the samples analysed in the present study), and the population here includes also those who failed to obtain a university affiliation.

### GERMAN VERSUS AUSTRIAN CAREER PATHS

Much more so than by any of these background variables, the everyday life of scholars is characterized by their striving for recognition. This is manifest in the struggle to have and maintain a position, on the one hand, and to gain a reputation, on the other. These are two key factors that need to be investigated in greater detail. I will first deal with the Teutonic situation in general, including

the changes this system had undergone since the nineteenth-century reforms. I will then examine the careers of the sociologists with a focus on how their life planning was informed by social opportunity structures, and finally explore the reputation gained by the population in question.

I will start out with some reflections on the German academic career model or, rather, on the absence of such a model. Max Weber, in his well-known dictum, characterized the career prospects of German junior scientists as a 'wild hazard' (Weber 2002: 477–8). Contemporaneous as well as later studies corroborate Weber's judgement by providing statistical evidence and certain details of social history (cf. Eulenburg 1908; Ferber 1956; Schmeiser 1994, on whose writings I draw in the following). For Weber, the two most notable characteristics of the German model were the absence of both a specific course of studies and a universalistic framework for selection procedures, which made life difficult for those who, simply feeling an inner urge to do so, chose science as a profession. Selection procedures of a less imponderable nature would have allowed candidates to roughly calculate the risk they were taking; that there were no such procedures turned the choice of science as a 'profession' into a game of chance. This indeed was a structural flaw and, as such, is of greater importance than the fact that access to the academic teaching profession could be denied on the basis of particularistic considerations (irritating as that was not only to Weber).

Beginning in the early nineteenth century, the stages that led to professorship were clearly defined at German and (with some minor variations) Austrian universities. After 1820, the simple fact of having graduated no longer opened up the way to an academic career. A further dissertation had to be submitted and another examination had to be passed before one was 'habilitated' – literally, 'to have been made able' – for academic teaching. Once this '*Habilitation*' had been institutionalized, the doctoral degree alone was a sufficient qualification only for professions outside the university. For a long time, however, risk remained predictable in terms of the time it took candidates to complete the *Habilitation* process, only a couple of years to be dedicated to the writing of the second dissertation. The real uncertainty was not the time they would have to invest – and finance – but whether there would be a department willing to grant them access.

Until the second half of the twentieth century, there was no substantial increase in the time between graduation and *Habilitation*. In the 1850s and 1860s, the average time it took a professors-to-be at a German university to cross the hurdle of *Habilitation* was 2.8 years. This average rose to 4.6 years in the two decades around the turn of the nineteenth century, and to seven years between 1910 and 1930. After 1945, the average time for candidates to reach the stage even of an assistant professor had become at least eleven years.<sup>17</sup> While prior to 1910 the average assistant professor had to shoulder this burden in his mere twenties (and even in the 1920s as a young man in his very early thirties), after the Second World War this status transition increasingly tended to coincide with another transition – that which would much later be called the 'midlife crisis'.<sup>18</sup>



In the nineteenth century, it took candidates seven to eight years, on average, to be made professors; between 1900 and the end of the First World War, this average rose to about ten years, and in the 1920s, there was another two-year rise (with differences between disciplines increasing over the decades). During the period analysed here, chances for obtaining a professorship deteriorated dramatically. While around 1900 the ratio of associate professors and lecturers to professors was 1.3:1, it kept increasing during the subsequent decades (1.5:1 in 1910; 1.7:1 in 1931). Ratios varied considerably between disciplines: they were highest in medical departments, where even in 1890 the ratio of candidates to professors was 2:1, and in the experimental sciences, with a ratio of 1.8:1, while ratios in the humanities remained relatively balanced during the whole period under examination, reaching 1.3:1 in as late as 1931. Conditions for professors-to-be were best in law departments where, during the whole period, there were fewer associate professors and lecturers (between 0.4 and 0.6) than full professors.

In the group of disciplines we are concerned with here, which have been classified as the social sciences by Ferber (1956) and, following him, Ringer (1993),<sup>19</sup> the ratios of associate professors and lecturers to full professors are much higher than for all disciplines: in 1900, the ratio was 6:1; by 1910, 2.75:1. While in most disciplines the situation eased up around 1920, the ratio in the social sciences decreased to a mere 1.8:1, only to rise again to 2.3:1 by 1931.

While jurists and graduates of the humanities could bridge the waiting time for a chair in a more or less convenient way by practising as lawyers, practitioners or teachers in upper secondary schools, respectively, social scientists were much worse off. A habilitated lawyer or doctor could also content himself with remaining a lawyer or a doctor in case his ambitions for an academic career were foiled (and traditionally historians have had the alternative role of archivist), but for social scientists whose career ambitions turned out to be unrealizable, no comparable professional alternatives presented themselves. If *Habilitation* was unattainable, the loss of status was accompanied by a very real loss of social standing. The fact that so many chose to pursue this thorny path in spite of all this is probably due to the appeal of the intellectual field in which sociology emerged as a discipline. The promise of sociology's founding fathers that theirs was a method offering undreamed of insights into social life obviously found listeners who willingly opened their ears to this siren song, most likely because of some prior experience that prompted them to search for answers to existential questions. This sociology-in-the-making promised no income or jobs, but, if not redemption – for this, the future sociologists will have been too agnostic – at least the key to an understanding of novel facts.

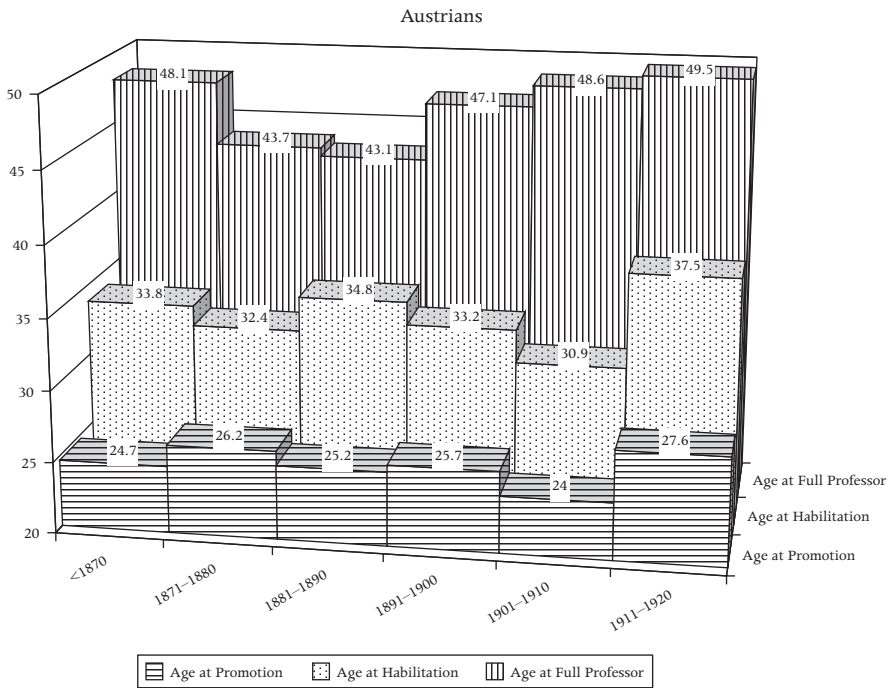
We will now turn to the careers of German-speaking sociologists, complementing their collective biographical portrait with a further facet.

The age at which the members of all the samples in this analysis took their doctoral degree<sup>20</sup> remained more or less unchanged for more than six decades. The mean age rose from 25.2 years (for those born before 1870) to 27.5 years (for those born between 1881 and 1890), then dropped again even below its initial

value: the average age for those born in the first decade of the twentieth century to receive their doctoral degree was 24.7 years, with a substantial rise (to 28.3 years) only for the youngest cohort. The time between doctoral degree and *Habilitation* was 7.6 years for the oldest cohort (the Germans Georg Simmel, Werner Sombart, Ferdinand Tönnies, Alfred Vierkandt, Alfred and Max Weber, and the Austrians Eugen Ehrlich, Sigmund Freud, Wilhelm Jerusalem and Friedrich Wieser), and in the next cohorts, the preparation time needed to become an unsalaried lecturer was longer by no more than half a year; only for the youngest cohort was this stage of academic life uncommonly long, at 12.3 years. Germans and Austrians show some commonalities, but also considerable differences. In both countries, the oldest and the youngest cohorts differ from the relatively clear trend observed for the four middle cohorts: Germans were consistently faster at getting their doctoral degree while Austrians were faster (not only than their German colleagues but also members of the older cohorts) in crossing the hurdle of *Habilitation*. Only the time to reach top-level positions grew longer and longer in both countries (Figure 4.2).

Since even a general comparison of the two German-language countries reveals a number of substantial differences, two further dimensions should be considered that are likely to have had an impact on careers, the relevance of which in the present context is undeniable: a comparison between the 'home-guard' and (future) emigrants, and a comparison between those we know to be Jewish and the others (who no doubt include further Jews, and certainly further victims of the Nuremberg Laws). The following analysis covers only the four cohorts of those born between 1871 and 1910 and does not include the oldest and the youngest cohorts (Figure 4.3). The age for taking the doctoral degree primarily reflects the difference between Germans and Austrians; on closer examination, however, the most significant difference is that between German Jews and German non-Jews, while the fact of later emigration does not yet weigh in at the time of graduation.

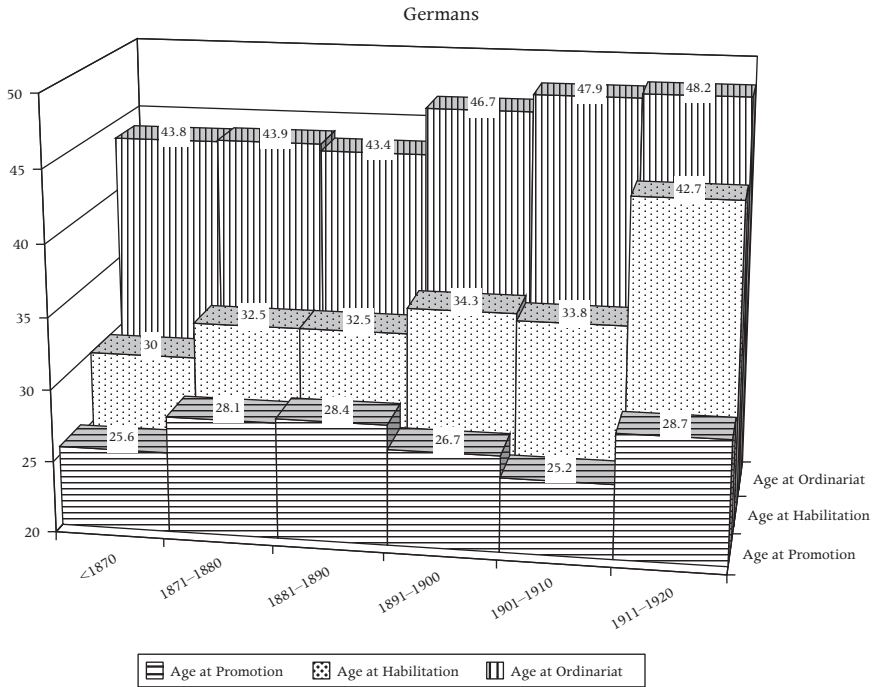
One would expect discrimination against Jews to show primarily at the second career level, since the status transition of *Habilitation* seems more open to discrimination than the doctoral degree. Actually, however, future emigrants were significantly faster to cross this hurdle than those who would stay, while Jews in both countries had to wait somewhat longer for *Habilitation* than the non-Jewish candidates who would share their fate. Ironically, this finding echoes the feelings at the time of all those who were not favourably inclined towards Jews (not to mention the ruthless anti-Semites who were unlikely to let their attitude be determined by experience anyway). For instance, when the young Fritz Machlup felt ready for the *Habilitation* process at Vienna, he was told by professor of economics Ferdinand Degenfeld-Schonburg, who did the honours with him, that he could not support his request because, given that everyone knew Jews to be precocious, it would be unjust to the non-Jews.<sup>21</sup> A differentiated examination of the career pace of the three groups suggests two conclusions: even before 1933 and 1938, respectively, future emigrants were faster in their careers than the future home-guard, and even



**Figure 4.2** Mean Age at Promotion, *Habilitation* and Full Professor, by Birth Cohort and Country

those individuals who had to wait longer for an appointment as assistant professor because of their ascribed Jewishness were faster to overcome the first hurdles of the academic career than non-emigrants. As a result, future emigrants may have been perceived as more successful even before they emigrated, and it does not seem at all far-fetched to suppose that this perceived superiority stirred up the envy of those who were less successful, an envy to which the Nazi regime gave leeway by opening up new opportunities for discriminating action. For many of the future emigrants, the prospect of purposeful eviction (denunciations of academic competitors were endemic after 1933) combined with the fact that there was no need for them to fear competition when it came to intellectual potential, may have made it easier for them, from a subjective perspective, to leave the Teutonic science culture.

The mean age for being made a full professor is forty-six for all sociologists in all the data sets included in the present analysis. In other words, it took them almost two decades and, thus, significantly longer than German professors in general, to rise from the bottom entry hurdle, the doctoral degree, to the top rung of the academic career ladder. In the oldest cohort, nine out of ten of those who were habilitated were appointed to a chair at some point in their life while the remaining one-tenth had to be content with the status of an associate professor. In the next cohort (those born between 1871 and 1880), the success rate fell to 78 per cent, with all but two of the remaining individuals

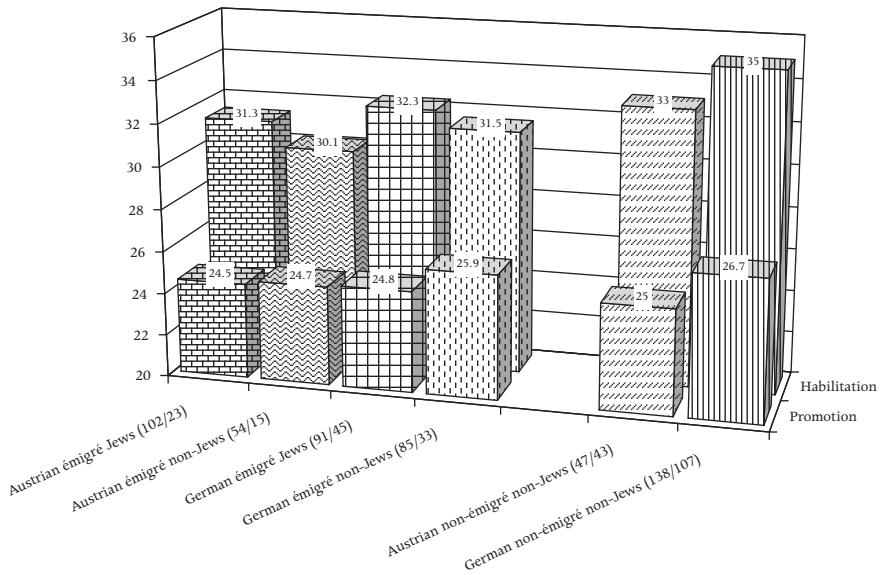


**Figure 4.2** *Continued*

Source: Author's calculations.

reaching at the second highest level. In the third cohort, the proportion of those who obtaining a full professorship was further reduced to 71 per cent, while one-fifth had to be content with the rank of assistant professor and seven remained stuck on the level of unsalaried lecturer. For two of the latter, we know that it was politics that ruined their careers: polyhistorian Otto Neurath, habilitated for economics in Heidelberg, not only lost his position as an lecturer there because of his participation in the short-lived Munich Soviet Republic (Münchener Räterepublik) but was henceforward perceived as a communist by the bourgeois public, a label which, as is often the case, was a misnomer but stuck anyway.<sup>22</sup> Oscar Ewald ruined his prospects at Vienna by more peaceful means: a pronounced pacifist, he did not fit in any academic teaching position. Among the rest of those 'eternal' unsalaried lecturers, economist Louise Sommer seems worth mentioning since in her case it was most likely gender discrimination that barred her from further promotion.

In the cohort born in the decade before 1900, already one out of three were failing to obtain a place in the academic sun: four in five succeeded in obtaining a chair while one out of seven were stuck as unsalaried lecturers or left the German university system. Those who failed to go farther than lectureship included the first scholars who became refugees from Nazi dictatorship: Rudolf Heberle, Felix Kaufmann, Josef L. Kunz and Jakob Marschak.



**Figure 4.3** Mean Age at Promotion and *Habilitation*, by Country, Status as Emigrants and Ethno-religious Affiliation, Birth Cohorts 1871–1910

Note: In parentheses absolute numbers of those holding Promotion/*Habilitation* for each group; 16 non-émigré Jews and 9 deceased before 1933 and 1938 respectively are excluded.

Source: Author's calculations.

For those born in the first decade of the twentieth century, reaching the professorial level was somewhat easier: due to a decline in the number of positions of associate professor, three out of four of them succeeded in being appointed to a chair. This cohort, however, includes the largest number of lecturers who got stuck at this level. A full third of them opted for emigration, where they succeeded in establishing themselves in the academic domain, provided they did not die prematurely. The war generation that came next again differs considerably from the more general picture: the cohort was smaller in number than that of the founding generation, which improved the chances for each of its members to spend old age as an emeritus, just as their fortunate elders had done. In this cohort, most of those who reached professorship came to be junior scientists under Nazi rule, such as Werner Conze and Elisabeth Noelle-Neumann, or were student soldiers, such as Friedrich Tenbruck and Ernst Topitsch.

## STRUCTURES OF OPPORTUNITY FOR NEW OCCUPATIONS

In a formal perspective, the development of the sociological personnel in Germany and Austria seems to be the very model of cyclic surplus production. The cohorts entering the scene at the peak of this cycle realize the lack of opportunities and respond by engaging in intensified competition or by exiting from the field. Both attitudes act as a deterrent for the next cohorts, thus reducing the number of candidates. This phenomenon can also be

described in terms of a collective inertia of ambition building. What does this mean? If science is seen as a field for exercising a profession, it is immediately evident that an interest in any such profession will, as a rule, emerge in specific phases of life; someone who is already well established in a profession is rather unlikely to seriously consider engaging in a different career, and the more rigid the rules that govern access to the profession, the less he or she will be inclined to change. Professional reorientation among academics in German-language countries is a historically rather recent phenomenon.

New professions rarely emerge on the initiative of individual pioneers, but rather require a coalescing group of people pushing for the recognition and practice of the new profession. If, in addition, the exercise of the profession to be created hinges on the approval of senior personnel or on the allocation of funds or positions through channels that are more or less impervious to outside influence, the risk is even greater, and the number of co-combatants smaller. At any rate, given that the chances for success for the new (proto-) professional group depend to a considerable degree on their respectability in terms of numbers, the pioneers will be well advised to assemble a group of people around – and, with respect to age, ‘below’ – them who share their occupational goals. But recruiting this group takes time, and with the group growing larger, its youngest members almost inevitably see their chances for advancement compromised. On the other hand, it also takes time, and an accumulation of experience, for the group to realize that the new professional option has meanwhile become blocked, and for this insight to spread.

The institution of the unsalaried lecturer can serve to illustrate this inertia of ambition building. Between 1873 and 1931, the number of unsalaried lecturers at German universities tripled while the number of professors doubled, a situation that since the days of Thomas Malthus (1766–1834) gives rise to uneasiness. Growing numbers of professorships are a necessary stimulus for ambition building among the next cohorts. Stagnating numbers at the top level tend to reduce the attractiveness of the field in any profession, while growth, as a rule, results in growing numbers of candidates. If, in addition, the cycle of replacement spans many years (in the period under investigation, a professor used to stay in place for about a quarter of a century), the youngest cohorts will necessarily see their ambitions frustrated.

While this is pure speculation, it nevertheless seems defensible in the present context if a tentative link is established between the widespread anti-Semitic and Nazi-friendly attitudes among German and Austrian junior academics in their frustrated hopes for advancement in the interwar period. In this perspective, all those eloquent laments over the difficult situation in the ‘Systemzeit’ (the Weimar Republic) suggest an unequivocal undercurrent of defamation aimed at the holders of positions that had turned out to be out of reach under the rule of law. A forced reduction of the pool of the holders of and candidates for these positions, in contrast, could open up the structurally barred horizons of advancement.<sup>23</sup> Those who obtained a university position during the Nazi period could hope to keep it for a quarter of a century, so is it just a historical

accident that they were themselves, at the end of their careers, confronted with yet another generation of rebels who attempted, now under constitutional rule, to get rid of a thousand years of stuffiness beneath academic gowns? It should be noted in passing that with some of these old men – senior by status and, by now, also by age – the response to the new challengers was rather emphatic.<sup>24</sup>

In any world it is mainly the young who look for something new and feel attracted to it. If a fairly satisfactory and secure professional position can only be obtained after a prolonged time lag, support from like-minded individuals is needed for candidates to seriously consider engaging in this arduous and uncertain path at all. However, the longer the preparation period, the smaller the number of initially shared ideas that will survive. In this respect, the Nazi seizure of power in German and Austrian universities was arguably more of a revolutionary event than the sinking in of sociological thought in the minds of the younger generation of the German educated middle-class.

For a new field of intellectual activity – as sociology around 1900 undoubtedly was – to emerge and translate into the professional ambitions of the younger generations, a number of intermediate stages have to be passed. If what is at stake really is a new science, the influence of academic teachers, intellectual discourse and the social networks, without whose action the appeal of these new options would never have crystallized in the first place, never act just on individuals, but necessarily on cohorts of such individuals. However, if the size of a group can be seen as a precondition for their discourse to gain cognitive density, the appeal of novelty tends to result in future bottlenecks that will constrain the opportunities for professional activities in the very field for which recognition was the object of the struggle.

There are two ways out of this quandary: increasing the number of professional positions, or invading and redefining other fields. The expansion of sociological university positions in the late 1960s and early 1970s in Germany, as elsewhere, can be taken as an example of successful establishment by expansion; the invasion by ethnogenists and folklorists of the positions vacated by evicted sociologists after 1933 is an example of the second alternative. If neither of these options – expansion or invasion – is available, a large number of the initial protagonists is left stranded, as it were, and has to consider professional reorientation. A case in point is the group of founding members of the Wiener Soziologische Gesellschaft (Vienna Sociological Society) who failed to obtain university positions as sociologists in Austria either before 1914 or after 1918. Some turned to completely different activities: Karl Renner, Julius Ofner and Michael Hainisch remained, or became, politicians; Rudolf Eisler, Rudolf Goldscheid and Rose Mayreder remained, or became, writers; still others returned to their own traditional disciplines (e.g. jurists Josef Redlich and Hans Kelsen, historian Ludo Moritz Hartmann). As a side-effect, Austrian universities in the First Republic completely lacked the continuity in academic personnel that is usually a result of mentor-disciple relations. In the 1920s, Vienna students who were in any way interested in sociology had to look for mentors at places other than the ruins of the Wiener Soziologische Gesellschaft.

In Hans Zeisel's 'History of Sociography', published as an appendix to *Marienthal: The Sociography of an Unemployed Community* (Lazarsfeld-Jahoda and Zeisel 1933) and obviously written with the aim of establishing an intellectual genealogy for his own work, the few Austrian contributions that might have been cited—for instance Ehrlich's ethnographical '*Rechtstatsachenforschung*' (research into legal facts) – are simply ignored, probably because Zeisel was not even aware of them. After 1945, the sociological achievements of the inter-war period met with a similar and even more drastic fate. They were unknown entities, and came to be known only through the works of others who exploited them. This is true for the Viennese sociology of knowledge as represented by Wilhelm Jerusalem and his (prematurely deceased) follower Ernst Grünwald, as well as for the methodological writings of Felix Kaufmann and, of course, for Alfred Schütz and *Marienthal*. They all arrived late, by way of detours and even then usually damaged in transit, at the places where they ultimately surfaced (cf. Lepenies 2006: 76–92).

The Teutonic university, due to the long transitional phase it imposed on its candidates for professorship, was particularly impervious to innovation, since new disciplines could emerge only when they could boast rapid success or were supported by protagonists with a lot of staying power and attractive alternatives to fall back on. For sociology, none of these favourable conditions can be said to have existed. Moreover, the economic basis that had throughout the nineteenth century provided the material underpinnings for the emergence of new disciplines or new candidates for high-level positions in existing disciplines, was in a shambles in the first third of the twentieth century. As private wealth evaporated as a result of the large-scale destruction of the First World War and postwar inflation, the '*Rentnerintellektuelle*' (private-income intellectual; Weber 1923: 173) disappeared from the scene.

This situation, already difficult enough, was further complicated when appointments ceased to be based on universalistic criteria, making it hard, and finally impossible, for Jews and other political undesirables to become professors. The menace of ostracism on the grounds of some ascribed characteristics seems to have kept a certain number of potential candidates from envisaging *Habilitation* at all. They dropped out even before coming up against the prejudices of the mandarins. In this respect, situations in Germany and Austria clearly differ. While German Jews succeeded in taking the hurdle of *Habilitation* until 1933, conditions for Austrian Jews were less favourable. For a small number of them, the short spring following the foundation of the Republic – glorified as the 'Austrian Revolution' – provided opportunities for advancement (Max Adler was habilitated during this period, Wilhelm Jerusalem was appointed professor shortly before he died), but the list of those who were somewhat younger but no less eligible for *Habilitation* is much longer: Alfred Schütz, Walther Eckstein, Edgar Zilsel and Karl Polanyi, all born before 1900, might well have tried to qualify but for their stigmatization as '*Ungerade*' (odd ones).<sup>25</sup>

This overview of the development of academic career prospects at German and Austrian universities highlights facts that the actors themselves were no doubt



roughly aware of at the time. Barred career horizons, particularistic selection criteria and the necessity of long-term reliance on other sources of income are conditions that are unlikely to have gone unnoticed, particularly by social scientists. But of course neither the contemporaries nor those judging from historical distance could tell to what degree these conditions acted as an instrument of selectivity by keeping potential candidates from considering an academic career at all. In this respect, the forced-migration policies pursued by the Nazi dictatorship allow for a number of significant insights, since the pressure to seek refuge abroad also opened up new opportunities for an academic career.

The deficiencies of the Teutonic academic career model, which made themselves felt in all disciplines, were multiplied in those cases where ascribed characteristics became effective reason for exclusion. In view of the onset of social science emigration in 1933 and 1938, respectively, it can be argued that the disadvantage of having not been successful in one's country of origin turned out to be an advantage because, after emigration, the level of aspiration was lower and social decline was thus subjectively less strongly felt, if felt at all. The Teutonic lone wolves lived by the dictate that their own activities and their entire personality<sup>26</sup> had to be attuned to the expectations of higher-status individuals who needed to be courted as future supervisors of their doctoral degree and, later, their *Habilitation*. In contrast, the task faced by academic neo-immigrants was to come to terms with the American world of 'teamwork' among individualists and to tend to their market value (i.e. to produce publications regularly and frequently) while cooperating, at least temporarily, with other projects.

## OPPORTUNITY STRUCTURES AND INDIVIDUAL LIFE PLANNING

What remains is the question of whether, and how, the new opportunity structures came to be reflected in individual life projects, how the more general expectations concerning professional opportunities translated into short-term life-planning routines, and which aspiration levels emerged with respect to the endowment, material and other factors of professional positions. Here, however, we come up against a definite lack of sources, which by the way is an astonishing fact in itself. Hardly any other status transition that has been institutionalized some place or another has been given so little attention. Very few people have written about the process of completing their inaugural dissertation in memoirs, and virtually none of those who failed to complete it have said a word.<sup>27</sup> We seem to be better informed about the Ashanti way of coping with status transitions than about those of our own ancestors and the members of our own tribe (for the Anglo-American science culture, see Becher 1989). Periods of latency within academic careers are themselves a case of socially expected duration, a concept first proposed by Robert K. Merton (Merton 1949), albeit without reference to scholars. Merton distinguishes between 'expected duration' and 'actual duration' since the specific attitudes of the members of a social group are informed by, among other things, their expectations in terms of duration. In a later elaboration of the concept of socially expected durations (SEDs), Merton

extends the scope of this concept of socially binding or culturally informed expectations beyond 'temporal durations' (Merton 1984). The expectations inherent in various types of social structures thus include phenomena such as the 'lame duck' pattern in American politics, limited or unlimited prison terms, durations of illness, limited durations of public appointments, academic or other tenure positions, teacher-student relationships in the sciences, status sequences and guidelines for succession in organizational management, and other status positions with an inherent temporal structuring.

Merton argues that the very omnipresence of SEDs may have contributed to sociologists' long neglect of them, a criticism which for him is also self-criticism. In the present context, reference to the long neglect of SEDs is important insofar as it can be assumed that the theoretical ignorance goes along with a practical ignorance in everyday life. In other words: something that seems unworthy of our attention as sociologists and, therefore, will not be analysed any time soon, is likely to be equally unworthy of our attention as actors in everyday life. The question, then, is how this is reflected in the attitudes of displaced scholars. In their attempt to gain a foothold in the foreign culture they arguably paid little attention to everyday routines they took for granted. In extreme cases, their European habituation to biographical waiting periods, perceived as something quite natural, may have led them to more or less ignore, in their work plans, the American norm of rapid presentation of samples of one's work. Of course, the repertoire of attitudes that was theirs from European times was radically challenged by the very circumstances that determined their lives. The necessity of unearthing a means of subsistence did not allow for choosiness. But we may well assume that focusing on the necessity of securing some kind of job or support prevented them from modifying habits that were at the very core of their socialization.

Some clues to an answer to this question are provided by sources such as letters dealing with research projects and work plans and, perhaps, the time needed to execute them; exposés submitted to aid committees in view of obtaining support; and the records of foundation boards or refugee committees dealing with the evaluation of candidates. The records of the Emergency Committee in Aid of Displaced Foreign Scholars (EC) are a rich case in point.

In May 1938, forty-five-year-old outside lecturer for modern history Friedrich Engel-Jánosi, who was still in Vienna at the time, wrote to a colleague in the United States:

it is impossible for me to continue my scientific work (in Vienna). Accordingly to this I am writing to you to ask if there would be any possibility of my obtaining a position in European history at your university or if this would be impossible ... If an ordinary professorship would be out of question I would be very much interested in obtaining a research assistantship, research fellowship, or any other position that would enable me to continue my scientific work.<sup>28</sup>

Engel-Jánosi soon abandoned the unrealistic hope for a professorship, but thanks to a donation by an Austrian count and the active recommendation of

his brother-in-law who lived in New York, Engel-Jánosi was made a research associate at Johns Hopkins University. He arrived in New York in April 1940 from England, where he had been living since the year before. He ignored the well-intentioned advice to shorten his name, but very soon managed to find a platform for one of his research subjects (Lord Acton) in some American and British journals. While he was still in his 'trail year' (as it was dubbed by one of the more peripheral figures involved, John Whyte, in 1938), which was made possible by a private donation, he found a job at another East Coast university. Here, subsidizing was no longer necessary: a recommendation letter states that he was 'a good Catholic (Jewish is only racial) and a fine representative of the best Austrian type' (Ernst Barker, 27 March 1940). Thus, the Catholic University in Washington, DC became Engel-Jánosi's address of many years. What contributed to his relatively uncomplicated establishment, besides his large network of personal contacts and his fitting into a (admittedly narrow) segment of the American academic world, was no doubt the fact that he rapidly engaged in English-language publication activities. At any rate, Kent Roberts Greenfield of the Johns Hopkins University history department found his 'record of activities' remarkable and wrote that 'his articles in English have been published as rapidly as possible, and once they have appeared should improve his chances of establishing himself in this country' (18 February 1941).

Edgar Zilsel's writing activities in his Vienna period, where he was teaching adult education classes, mainly consisted of the approximately forty reviews he wrote for the journal *Die Naturwissenschaften* (The Natural Sciences); in the two decades between his doctoral thesis and his emigration he published only a short polemical pamphlet on the religion of genius in 1918 (Zilsel 1990) and, in 1926, one large monograph on the emergence of the social character of the modern scientist (Zilsel 1926). A second volume was announced, but failed to materialize in the twelve years to come. After his escape to New York, Zilsel clearly kept working on the project but at the same time, contrary to his Vienna attitude, began to publish partial results of his studies, no doubt also because there was no other way for him to have his EC aid renewed. His suicide in 1944 may well have been prompted in part by his failure to integrate these pieces into a single comprehensive monograph.

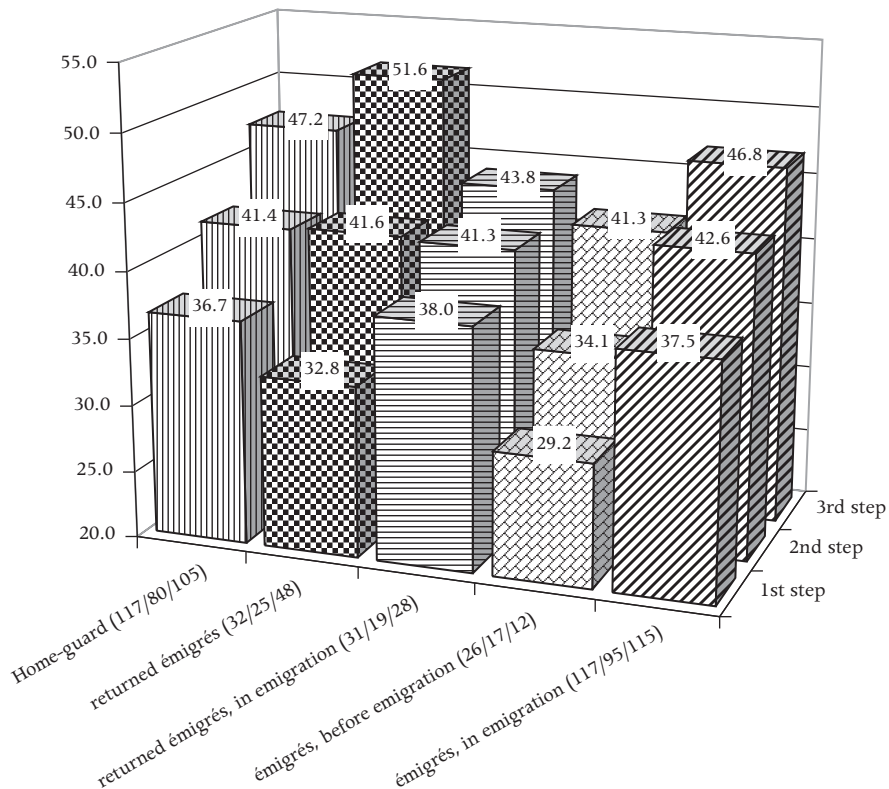
Paul Neurath was thirty when he arrived in New York in the early summer of 1941. After Austria's *Anschluss* in 1938, he had spent several months as a prisoner in the concentration camps of Dachau and Buchenwald. After regaining his freedom, he emigrated to Sweden where he worked as a metalworker. With a good bit of luck, he obtained a visa to the United States just in time to be on one of the last ships to cross the Atlantic. In Vienna, Neurath had completed seven years of law studies by taking his doctoral degree a few weeks before the Nazi invasion. In New York, he seized the opportunity to finally study sociology. In spite of his lack of experience in the field, his poor knowledge of English and having to work to support himself, he managed to complete his doctoral degree at Columbia University after two years of graduate studies. (Living on a

grant and jobs with Lazarsfeld's Bureau of Applied Social Research, Neurath was able to forego support from the EC or similar committees in aid of refugees.) Immediately after graduating, he started teaching statistics, first at the City College and later at Queens College. During his first two years in New York, Neurath was well aware that he had not much time to establish himself professionally in the way he wanted. He made optimal use of the opportunities he was offered. When the war ended, he was in a secure position and never for a moment considered returning to Europe (Neurath 2004).

These three cases show that refugees had to cope with very different adaptation problems. One of these problems was the time it took them to produce publications, and some of the newcomers seem to have been well aware of this. Since they all were novices in the United States academic field, those who were successful can be assumed to have learned about it from scratch, including those aspects which it took insiders such as Merton many years to realize in spite of their interest in the sociology of science.

This adaptation process is manifest also on a higher level of aggregation. A comparison of the career pace of those who pursued their careers exclusively in the Teutonic world with those who did so entirely or temporarily in the Anglo-Saxon world reveals significant differences not only between groups but also between the science cultures on either side of the Atlantic. For this comparison, the analysis was limited to those birth cohorts whose members were still in a position to complete further steps in their career in the 1930s and in the years that followed (those born between 1891 and 1920). Five groups can be distinguished: one group of 'home-guards' and four groups of emigrants. Among the emigrants, there were those whose stay in the United States was exile in the strictest sense (i.e. temporary), and those who stayed in the non-German-language area for good. Among those who returned to their home country, two subgroups can be distinguished: those who reached certain career levels in the Teutonic system before escaping from (or after returning to) their home country, and those who reached all of these levels during emigration. Similarly, the permanent emigrants can be subdivided into those who had already reached certain levels of their academic career before emigration and those who went through all of these stages as emigrants.

It has to be noted, however, that this analysis fails to take account of two important aspects. For the younger emigrants, it made sense to complete a second or sometimes even a third course of studies when arriving in the United States; access to scholarships was easier for young immigrants than access to university positions, and since many of the younger emigrants came to the United States prior to obtaining a European doctorate, continuing their studies was an obvious option. Since individuals who never held a university position are not included in the present analysis, the comparison is only between those who managed to obtain a university position at one of the three career levels in either the European or the United States system. A comparison of the mean age at which the members of all the five groups reached each of these three career levels is rather surprising (Figure 4.4).



**Figure 4.4** Mean Age at Three Career Steps of those Born between 1891 and 1920

Note: Home-guard: sociologists who never left the German-speaking environment. Returned émigrés: sociologists who moved upwards either before or after their emigration to Germany or Austria. Returned émigrés, in emigration: sociologists who moved upwards during emigration but returned afterwards to a German-speaking environment. Émigrés, before emigration: sociologists who moved upwards in Germany or Austria before their emigration. Émigrés, in emigration: sociologists who moved upwards only in non-German speaking countries.

1st step: *Habilitation* or assistant professor. 2nd step: ao. (*außerordentlicher*)/extraordinarius) Professor or Associate Professor. 3rd step: o. (*ordentlicher*/ordinarius) Professor or Full Professor.

In parentheses number of scholars in the three career steps.

Source: Author's calculations.

Sociologists who would eventually emigrate, but were also successful in the Teutonic system before (or after) doing so, reached all three career levels before emigration at a mean age that was significantly lower than that of sociologists who stayed in, or returned to, their home countries. The comparison of the career paths of future emigrants with those of the home-guard is particularly revealing in this respect because it can be argued that both groups competed against each other under conditions that were comparable on the individual level. In the Teutonic system, the group of future emigrants faced a greater risk of being discriminated against. Nevertheless, they were faster in accessing

top-level university positions than those who blended in more easily with the Teutonic world of science.

The eviction of professors by the Nazis opened up career horizons for those who were not targeted, which may have prompted some of them to take advantage of the new political setup to start scheming against those they wished to have dismissed. But neither the extensive clearance of the market that resulted from Nazi eviction policies nor the purposeful rancour that was brought to bear in individual cases was able to lever out the inertia of the Teutonic science system. With respect to Teutonic SEDs, the career model remained effective even for those who aspired to fill the vacant positions. The mean age for the members of this generation unit to reach the professorial level was 47.2, while all three groups of displaced scholars who managed to reach top-level academic positions in emigration did so at a lower age. The significant delays faced by those who chose to return after emigration, on the other hand, are arguably due to the fact that, for them, advancement to the third level was possible only after their return. In other words, after 1945, the Teutonic system primarily attracted those who, given their age, were already at their zenith. In contrast, the comparison of the two groups who made a career for themselves exclusively as home-guards or permanent emigrants clearly shows that those who were forced to emigrate were by the same token propelled into a kind of career elevator that took them up to the top floor of the academic world faster than their peers who stayed at home.

However, a perspective that does not take into account the question of who could obtain which position at which age may provide a more adequate explanation for this disconcerting finding: in both the Anglo-Saxon and the Teutonic science systems, there was a preferred age for candidates to access each of the three career levels – and at each of these stages, the Teutonic window of opportunity was geared to an older age group than the Saxon one, which clearly preferred its candidates to be younger.

For female emigrants, American conditions were even more favourable since more than twice as many, as compared to female non-emigrants, reached the top level. The small number of cases (nine female emigrants and four female non-emigrants who reached the top level as full professors) does not allow for any far-reaching interpretations, but there is a definite finding concerning age: among those who reached the top level and on whom data are available, the mean age to be appointed full professor was fifty-one for female emigrants and fifty-seven for females who remained in their home countries; and female home-guards had to wait more than seven years longer, on average, than male home-guards to reach that level, and thus considerably longer than female emigrants whose waiting time was only five years more, on the average, than that of their male co-emigrants.

University chairs are positional assets: a position held by one individual cannot at the same time be held by any of his or her competitors. Since it can be reasonably assumed that in spite of all adversities, war-related and otherwise, no position at German or Austrian universities would have remained

vacant for long, the reality is that in the 1940s and 1950s, 150 chairs were available for and occupied by (also-) sociologists in the German-language area. Assuming, as a thought experiment, that no eviction of Jews and others had taken place, it is evident that the 126 sociologists who rose to be 'full professors' during emigration would have had to either compete with those others for the rare vacancies in Germany and Austria or, for some of them, to fall back on the sour-grapes strategy of renouncement.

### GAINING, AND MEASURING, REPUTATION

In addition to his or her professional trajectory, a scholar's curriculum vitae usually includes information on academic productivity which, in the case of social scientists, mainly means publications. Teaching activities, consulting, participation in scientific organizations and the like played a minor role in the past and are therefore not taken into account in the present analysis. Besides the manifest function of publicizing one's own contributions to the advancement of knowledge – or, on a more modest scale, demonstrating one's own scientific productivity – publications also serve to build prestige. The tacit rules of good academic conduct stipulate that an author's esteem for the work of others is expressed by references to it in their own writing. An author who is not referred to in the work of others is either subject to dishonest exploitation or – arguably the more frequent case – has failed to contribute anything to the body of knowledge that others have deemed worth considering. The bad feelings of those whose achievements are ignored provide the breeding-ground for the suspicions that accompany the whole business of citing and being cited.

While the question of how to measure an author's productivity is basically uncontroversial, there is much less consensus on how to solve the issue of reputation. Things are further complicated when the aim is to come to a comparative rating of previous generations of sociologists. If the work and the influence of a single scholar are considered case by case, identifying their position in the status structure is easy, and one is free to turn to the more or less passionate task of declaring them under- or overrated, corresponding arguments supplied. If, however, the object of one's scrutiny is comparison within a group larger than the size of a local school, one first needs to identify the status and prestige order that prevails within this population before tackling the task of establishing comparative ratings for its individual members. Below the level of those who are the undisputed eminences of a discipline, establishing a distribution of prestige is far from easy. The prestige hierarchy among those of 'third-rate' and 'last-rate' renown is arguably much more controversial than the academic squabble about whom to count among its most eminent figures. The bulk of the population is as grey and monotonous among scholars as elsewhere.

In short, if the aim is to come to any statements concerning the scientific productivity of more than 800 sociologists, and to rank them in terms of who has gained more or less recognition for it, relying on relatively rough measures

is inevitable. One may well deplore the coarse-grained nature of the analysis, but for lack of better tools, so long as certain groups are not systematically underrated, we will have to make do with what is feasible.

To this end, the following questions have to be answered. Who belongs to the population to be studied? Which data should be used to establish the (primary and secondary) publications? And what is the best way to cope with the awkward problem of measuring the recognition gained by an individual author? In the present case, the group to be studied is circumscribed by the five samples that are supposed to represent to a satisfactory degree, as has been argued above, the entire population of German-speaking sociologists in the mid-twentieth century.

If the unit chosen for measuring reputation is the academic journal, one is at once confronted with all the familiar objections raised against citation analyses: citation cartels, short-lived preferences, the Matthew effect of self-reinforcing feedback, language barriers and much more. And who would deny that these biases do exist? The only thing that can, in turn, be held against these views and misgivings is that citation analyses might well be a way of verifying the very presumptions concerning any such biases. If preconceived ideas could be disproved, this would be an interesting result in itself. A second point could be that in the present case, the authors concerned started to publish and to be cited many decades ago, so what is attempted here is a kind of historical citation analysis. In former decades, the practice of citing and being cited was no doubt less biased by the idea that one's individual 'impact factor' has an influence on one's career opportunities. Regardless, establishing the productivity shown, and recognition gained, by a large number of authors is only feasible – or meaningful – if the procedure used can be expected to ensure approximate equality of opportunity for all those being considered in the analysis.

Another source to draw on for measuring reputation is provided by the institutionalized forms of recognition that an author may hope to gain. But since there is no Nobel Prize for sociologists, and considering that symbolic honours that are similarly recognized, such as being elected a member of an academy or awarded an honorary doctorate by some prestigious university, do not exist in any internationally comparable form, one must make do with some provisional measure. For this kind of second-best strategy, recognized standard works for the social sciences can be used to see how prominent a place certain authors are accorded and whether others are at least worth naming. The most widespread reference work for those who have sought to inform themselves of the state of social science research in the last three decades has been the *International Encyclopedia of the Social Sciences* (Sills 1968) and its supplemental volumes (Sills 1979; Sills and Merton 1991). The recently published *International Encyclopedia of the Social & and Behavioural Sciences* (Smelser and Baltes 2001) provides an update that will fulfil the same function for the decades to come.

Since both encyclopedias use very different criteria to determine whether an author is included, a comparison is not easy. Still, what can be said is that



sixty-one sociologists from our five samples were granted a biographical article in Sills (Sills 1968; Sills 1979) and that fifteen of these even crossed the much higher hurdle of being included in the new encyclopedia as well (Smelser and Baltes limited the number of biographical articles to one hundred and fifty, while Sills's volumes each comprise more than eight hundred articles).<sup>29</sup> The only additional author to be included as late as 2002 is Norbert Elias, whose star was not yet shining brightly enough in the 1960s and 1970s. Using the even broader criterion of whether an author is referred to at all in one or both multivolumed works, the correspondence is even stronger (Pearson's correlation coefficient, based on a comparison of the number of references in the index of names, is 0.792).<sup>30</sup>

In addition to the two encyclopedias, two further reference works concerned with sociologists were used: the *Internationales Soziologenlexikon* (Bernsdorf and Knospe 1980–84), already variously referred above, and the *Who's Who in Science: A Biographical Dictionary of Notable Scientists from Antiquity to the Present* (Debus 1968), whose selection of about 30,000 important persons bears the mark of the place where it was published, Chicago. The correspondence between these two publications is almost mutually exclusive (-.291), which makes both works exceedingly well suited for simultaneous use.

Information from all the four sources used for measuring the *recognition* gained by individual sociologists is summarized in an index<sup>31</sup> whose top ranks are not much of a surprise: the top ten are Weber, Freud, Lewin, Simmel, Mannheim, Schumpeter, Lazarsfeld, Tönnies, Michels and Popper. The bottom ranks are taken up by those 65 per cent of sociologists from all our samples whose names do not turn up in any of the four sources. The entry rate is relatively higher in the ISL (33 per cent of the authors in the samples are referred to while the proportion in Debus's *Who's Who* is only 8 per cent), while eight out of ten sociologists were not deemed worth considering in either encyclopedia.

Since there is no citation index that reaches back far enough in time, and since I needed to minimize the risk that my assessment of scientific productivity and recognition by peers was biased while maximizing comparability across findings, I used the electronic full text journal database JSTOR.<sup>32</sup> For the period surveyed, the JSTOR archive includes more than 100 so-called 'core journals' of various disciplines<sup>33</sup> from their first year of publication onwards, with selection criteria described as follows: '(1) the number of institutional subscribers a journal has; (2) citation analysis; (3) recommendations from experts in the field; (4) the length of time that the journal has been published'.<sup>34</sup> I used this service to find out how many articles had been published in these journals by any of the authors in the five samples, and how often these authors had been cited by others. JSTOR search routines allow the user to search for authors' names in the archived journals while limiting the search to certain types of texts (articles, abstracts, reviews, other texts such as professional communications, letters, etc.). Moreover, users can conduct full text searches for strings of any kind and, thus, find the names of persons and the frequencies of their appearance. I used this automatic search routine to do a kind of

citation analysis that slightly differs from its narrower SCI (Science Citation Index) and SSCI (Social Sciences Citation Index) models.<sup>35</sup>

The following three measurements were conducted with the JSTOR electronic archive:

- *Visibility*, i.e. how often the name in question appears in one of the fields used for searching articles, reviews, reports, etc. Since the JSTOR search routine does not allow for a differentiation to be made between reviewer and the reviewed, this measure only shows how visible someone was for the scientific community.
- *Productivity*, measured by the total number of scientific articles published in any of the 117 journals.
- *Recognition*, i.e. the perception of the name and, thus, the works of an author by all other authors. This comes rather close to measuring an author's reception. It should be noted that 'citation' may just mean a book title, or even just an author's name, without any bibliographical reference.

In Table 4.5, first names are spelled out for the top ranks and are abbreviated for the lower ranks. The results of this analysis cannot be reported in a few sentences. Still, some explanations and interpretations seem warranted. First, there is the risk that names are being confused. For example, Austrian mathematician and philosopher Karl Menger may not actually be mistaken for his father, economist Carl Menger, but since the Karl/Carl spelling invites confusion, he cannot be clearly distinguished from him either. The same may have happened with frequent names like Kohn, Adler or Kaufmann.<sup>36</sup> The first approximately forty names in this table represent only 5 per cent of those whose appearance in JSTOR was checked. Among those who were counted but do not belong to the first forty names in either of the three columns are such well-known names as Hannah Arendt, Max Horkheimer, Karl Mannheim, Herbert Marcuse, Ludwig von Mises and Franz Oppenheimer. Given the database, it would be less surprising if authors who never stayed in the United States failed to appear. But this is true only for the younger ones, such as Helmut Plessner, Norbert Elias and René König; it is not true for the older ones who were included in one of our samples. Among the forty names with the highest recognition rates, there are eight German social scientists, namely Michels, Scheler, Simmel, Sombart, Tönnies, Troeltsch, Alfred Weber and Max Weber. In 1933, at the beginning of the period we are concerned with here, only four of them were still alive. Non-emigrants who enjoyed at least a certain degree of visibility are Carl Schmitt (rank 41 on the prestige scale), Otto Hintze (47), Alfred Verdross (67), Otto Brunner (73), Marianne Weber (74), Othmar Spann (76), Elisabeth Noelle-Neumann (80) and Konrad Lorenz (80).<sup>37</sup>

The two 'leaders' in terms of visibility obtained their positions because they were among the busiest reviewers: 165 out of 239 hits for jurist and international organizations expert Josef L. Kunz are reviews; for Leo Gross, the proportion is even higher, with 169 out of 198. Both, however, also published numerous articles, which earned them ranks three and eight, respectively, in

**Table 4.5** Visibility, Productivity and Recognition of Austrian and German Social Scientists

Rank	Visibility	Productivity	Recognition
1	Kunz, Josef L. (239)	Tietze, Christopher (67)	Weber, Max (1938)
2	Gross, Leo (198)	Moreno, Jacob L. (64)	Lazarsfeld, Paul F. (959)
3	Coser, Lewis A. (193)	Kunz, Josef L. (61)	Freud, Sigmund (818)
4	Kohn, Hans (179)	Machlup, Fritz (48)	Bendix, Reinhard (479)
5	Machlup, Fritz (135)	Haberler, Gottfried (40)	Deutsch, Karl W. (469)
6	Bendix, Reinhard (123)	Tintner, Gerhard (30)	Hayek, Friedrich A. (413)
7	Hoselitz, Bert F. (102)	Hirschman, Albert (28)	Machlup, Fritz (389)
8	Carnap, Rudolf (97)	Gross, Leo (27)	Simmel, Georg (385)
9	Lazarsfeld, Paul F. (90)	Bergmann, Gustav (26)	Hirschman, Albert (374)
10	Tintner, Gerhard (90)	Blau, Peter M. (26)	Blau, Peter M. (369)
11	Haberler, Gottfried (88)	Kohn, Robert (25)	Haberler, Gottfried (316)
12	Moreno, Jacob L. (88)	Lazarsfeld, Paul F. (25)	Morgenstern, Oskar (313)
13	Blau, Peter M. (85)	Gumbel, Emil J. (24)	Cassirer, Ernst (302)
14	Fürer-Haimendorf, Christoph (85)	Vagts, Alfred (22)	Carnap, Rudolf (289)
15	Hayek, Friedrich A. (84)	Schumpeter, Joseph A. (21)	Schumpeter, Joseph A. (257)
16	Bergmann, Gustav (82)	Marschak, Jacob (19)	Tietze, Christopher (254)
17	Gerschenkron, Alexander (80)	Simmel, Georg (19)	Strauss, Leo (249)
18	Vagts, Alfred (78)	Deutsch, Karl W. (18)	Gerschenkron, Alexander (239)
19	Deutsch, Karl W. (74)	Gerschenkron, Alexander (18)	Lewin, Kurt (236)
20	Manheim, Ernest (73)	Back, Kurt W. (17)	Coser, Lewis A. (215)
21	Tietze, Christopher (70)	Menger, Karl (17)	Adorno, Theodor W. (214)
22	Wolff, Kurt H. (69)	Bendix, Reinhard (15)	Popper, Karl (207)
23	Hirschman, Albert (68)	Colm, Gerhard (15)	Moreno, Jacob L. (194)
24	Streeten, Paul P. (65)	Ichheiser, Gustav (14)	Kelsen, Hans (189)
25	Back, Kurt W. (64)	Kaufmann, Felix (13)	Michels, Robert (184)
26	Schumpeter, Joseph A. (64)	Kelsen, Hans (13)	Sombart, Werner (179)
27	Colm, Gerhard (63)	Stolper, Wolfgang (13)	Back, Kurt W. (164)
28	Morgenstern, Oskar (63)	Kohn, Hans (12)	Scheler, Max (161)
29	Heberle, Rudolf (60)	Rierner, Svend (12)	Gumbel, Emil J. (160)
30	Landauer, Carl (60)	Schütz, Alfred (12)	Wolff, Kurt H. (146)
31	Cahnmann, Werner J. (57)	Staehle, Hans (12)	<b>Jahoda, Marie</b> (134)
32	Cassirer, Ernst (55)	Carnap, Rudolf (11)	Troeltsch, Ernst (133)
33	Speier, Hans (55)	Hayek, Friedrich A. (11)	Drucker, Peter F. (127)
34	Sturmthal, Adolf F. (55)	Lauterbach, Albert (11)	Vagts, Alfred (112)
35	Simmel, Georg (53)	Morgenstern, Oskar (11)	Hoselitz, Bert F. (110)
36	Ehrmann, Henry W. (50)	Pribram, Karl E. (11)	Schütz, Alfred (107)
37	Kelsen, Hans (50)	Hoselitz, Bert F. (10)	Weber, Alfred (107)
38	Redlich, Fritz (50)	Speier, Hans (10)	Tönnies, Ferdinand (103)
39	Stolper, Wolfgang (50)		<b>Freud, Anna</b> (103)

Notes: Women in bold; hits in parentheses; full name given only at the first place mentioned.

Source: JSTOR. Author's calculations.

the dimension of productivity. Hans Kohn also was a busy reviewer. Because of its multiple dimensions, visibility is not really helpful with respect to issues of productivity and recognition, although its correlation with the first of these dimensions is very high (.749). Its correlation with recognition is, in contrast, comparatively modest (.432).

Apart from limitations due to JSTOR's emphasis on American journals, the rank order of productivity reflects two characteristics of the database: First, the 117 journals that were in the JSTOR archive at the time of the survey clearly do not provide the same coverage for all the different social science disciplines. As a result, authors who published in demography, statistics and economics journals are more likely to make it to the top ranks.<sup>38</sup> Second, JSTOR seems to have privileged those disciplines which in the period analysed had already adopted journal publications as their primary means of communication. This is obviously a reason for demographer Christopher Tietze's premier rank, as well as for the squad of economists.

The most remarkable finding in the recognition rankings is the large proportion of Austrians among the highest scoring authors. While nobody will object to Sigmund Freud coming behind Max Weber among the most recognized authors, the fact that he is also clearly outdone by Paul F. Lazarsfeld is something of a surprise, as is the long list of other Austrians that follows. Disregarding cases of doubtful or mixed nationality such as Rudolf Carnap, Karl Deutsch, Kelsen, Moreno and Schumpeter, the first forty ranks are occupied by a majority of Austrians, even if one excludes authors such as Kurt Back, Peter Blau, Bert F. Hoselitz and Christopher Tietze who had emigrated at a very early age. Lewis A. Coser, Reinhard Bendix, Albert O. Hirschman and Kurt Wolff might be similarly excluded on account of their youthful age. The result is a more balanced distribution of Austrians and Germans, all of whom – with the exception of Theodor W. Adorno and Leo Strauss, who were also relatively young – had by the Nazi period reached an age where there was little left for them to do to further enhance their reputations during emigration. Thus, both Central European science cultures exported approximately the same number of younger scholars, but only Austria also contributed scholars who were slightly older when they went abroad, while German high-reputation emigrants had acquired their reputations before the Nazi era. In the next twenty ranks, there are the names of three German non-emigrants – Carl Schmitt (rank 41), Otto Hintze (47) and Werner Conze (64) – and nine German emigrants – Fritz Redlich (43), Karl Mannheim (45), Jakob Marschak (46), Hans Staehle (49), Hannah Arendt (52), Karl Loewenstein (58), Fritz Morstein Marx (60), Rose Laub Coser (63) and Herbert Marcuse (70). When Emil Lederer (68) and Martin Buber (69) are excluded because of their dual nationality, there again is a majority of Austrians in ranks 41 to 70.

Lazarsfeld's position in second place on the recognition scale can be explained by the fact that he published in a variety of domains and, as an authority on methodology, was probably more cited than read between his first citation in 1938 and the 1950s and 1960s. The same cannot be said of his student and

follower – and very much his junior – Peter M. Blau, who never established a school or published textbooks and manuals on methodology such as Lazarsfeld's much-referred-to *Language of Social Research* (Lazarsfeld and Rosenberg 1955). Blau's prominent position, just like that of Bendix, Deutsch, Coser and Wolff, is most likely due to a change in citation habits. Kurt H. Wolff's rank may further be attributed to his editorial activities, another factor that tends to boost reputation. Adorno, on the other hand, is a special case since his high rank is in all likelihood due to the fact that his name comes first among the four authors of *The Authoritarian Personality* (Adorno *et al.* 1950). No other author among the top seventy in this recognition ranking gives rise to comparable doubts as to his personal contribution to the reputation gained.

The high number of Vienna School economists – Hayek, Machlup, Haberler, Morgenstern and others who, such as Schumpeter or Gerschenkron, were not completely alien to this school of thought, is due to the pre-eminence of neo-classical economics, which also included authors such as Jakob Marschak. It should also be noted that especially those junior members of the Vienna School who emigrated to the United States were key figures in, and contributors to, the economic debate in the United States. The presence of Drucker, Hoselitz or Polanyi, who each had his own way of grappling with Vienna School thought, is indicative of the highly coherent culture of debate in the field of economics, arguably the first social science discipline to frame itself as a profession (Shils 1982). The pre-eminence of economics is again highlighted by the high ranks of German economists Marschak, Gerhard Colm, Wolfgang Stolper and Paul Steeten from the Kiel and Heidelberg schools. The comparatively lower ranks of other German economists must be seen in connection with the fact that *Social Research*, the journal of the New School for Social Research, was not included in JSTOR during the period covered by this analysis. Moreover, more emigrant German than emigrant Austrian economists were teaching at the New School (Krohn 1987). The relatively prominent position of Jacob L. Moreno can be similarly attributed to JSTOR's selection of journals, since his high values for productivity as well as visibility are arguably due to *Sociometry*, the journal he had founded and which was not 'officialized' until later.

Setting aside these interpretations of individual cases, the data can also be systematically analysed in view of the links between input in terms of publications and output in terms of recognition by peers. This is relatively easy in cases like that of Tietze, who not only published a lot but was obviously active in fields beyond demography as well, making him highly visible. Statistician Emil J. Gumbel is a similar case, if on a lower level. Both also arguably owe their prominence to the fact that almost all the journals dedicated to their specialities are included in JSTOR, which is not necessarily true for other disciplines.

Among the authors who published a considerable number of articles, which helped them to join the top forty ranks of productivity but earned them less recognition, there are methodologists Karl Menger (recognition rank 44) and Gustav Bergmann (182), social psychologist Gustav Ichheiser (96), econometrician Gerhard Tintner (108), economists Marschak (46), Staehle (49), Pribram (66),

Stolper (112), Lauterbach (119) and Cohn (170), and sociologists Hans Kohn (169), Hans Speier (211) and Svend Riemer (350). Whether this means that a certain number of contributions is a prerequisite for scoring on recognition is an open question.

The other perspective with respect to JSTOR content is no less instructive: among those authors who ranked highly on the dimension of recognition, there are more than a dozen whose visibility was obviously not based on articles published in JSTOR 'core journals'. Anna Freud, Sigmund Freud, Max Scheler, Ernst Troeltsch, Alfred Weber and Max Weber did not publish a single article in one of the journals covered by JSTOR, while only one text was found for each of Adorno, Marie Jahoda, Robert Michels and Werner Sombart. Similarly, Peter F. Drucker (four articles) and Ferdinand Tönnies (six) seem to owe their recognition primarily to the books they published.

In order to enhance the database and, thus, the validity of results, two further sources were tapped using two separate indices. For the *International Encyclopedia of the Social Sciences* (IESS), editor David Sills had assembled an illustrious company of authors in the late 1960s to supply authoritative accounts of the fields they were experts in. It can be assumed that those who were deemed worthy of contributing to the encyclopedia had, of course, previously also published in the field. Therefore, the articles in the encyclopedia can be used to counterbalance the JSTOR journal bias. The combined index of publications consists of the logarithmic value of JSTOR publications (maximum value 4.2) and of an index + 1 for those forty authors who contributed at least one article to the IESS (among them eight non-emigrant German sociologists).

A second source was similarly consulted to balance JSTOR-based recognition scores: in 1991, Sills and Merton published another supplement to the IESS in which they compiled the most important quotations of social science world literature (Sills and Merton 1991). These standard quotations were selected with the help of an advisory body of more than two hundred social scientists. Forty-five authors that figure in one of our samples had the honour of being represented by one or more quotations (Freud comes first with forty-three quotations, Max Weber second with twenty-nine). Together, JSTOR and IESS results make up the citation index.

All three sub-indices were then added up to form an *overall reputation index*. The values of the publications index were weighted to boost the importance of personal contributions and to level out the otherwise overwhelming predominance of status.<sup>39</sup> Since both the citation and recognition indices are based on third-party judgements, it can be assumed that the decision to attribute prestige by including an author in a biographical or encyclopaedic reference work was not reached without consultation with advisors. Correlations between the three basic indices are 0.615 for the citation index and 0.27 for the recognition index, with the publication index as the point of reference; thus, compiling an overall index seems justified. Table 4.6 shows the top forty ranks in this reputation index.

**Table 4.6** Index of Reputation: Twentieth-century German-Speaking Social Scientists (weighted)

Rank	Last Name	First Name	score
1	Lazarsfeld	Paul Felix	24.84
2	Simmel	Georg	23.22
3	Weber	Max	22.94
4	Schumpeter	Joseph Alois	22.33
5	Freud	Sigmund	20.99
6	Popper	Karl	20.17
7	Hayek	Friedrich A.	19.71
8	Blau	Peter M.	19.32
9	Lewin	Kurt	18.72
10	Bendix	Reinhard	18.63
11	Machlup	Fritz	18.19
12	Morgenstern	Oskar	18.04
13	Tietze	Christopher	16.95
14	Moreno	Jacob Levy	16.89
15	Tönnies	Ferdinand	16.77
16	Deutsch	Karl W.	16.11
17	Kelsen	Hans	16.05
18	Gerschenkron	Alexander	15.66
19	Marschak	Jacob	15.47
20	Schütz	Alfred	15.43
21	Carnap	Rudolf	15.35
22	Adler	Alfred	15.23
23	Haberler	Gottfried	15.13
24	Mannheim	Karl	15.04
25	Cassirer	Ernst	14.98
26	Coser	Lewis A.	14.84
27	Gumbel	Emil Julius	14.43
28	Michels	Robert	14.01
29	Hirschman	Albert	13.97
30	Mises	Ludwig von	13.19
31	Back	Kurt W.	13.15
32	Tintner	Gerhard	12.99
33	Kunz	Josef L.	12.78
34	Münsterberg	Hugo	12.54
35	Adorno	Theodor W.	12.25
36	Strauss	Leo	12.18
37	Sombart	Werner	12.10
38	Nadel	S.F.	11.91
39	Zeisel	Hans	11.90
40	Redlich	Fritz	11.89

Source: Author's calculations.

Since, obviously, the rank occupied as a result of this measurement depends on the data and definitions used, no attempt whatsoever will be made in the present context to discuss the correct ranking of individual sociologists. A comparison with the recognition measurement based on the analysis of JSTOR citations reveals certain changes in ranking that are due to the overall index. Younger scholars such as Bendix and Deutsch have somewhat dropped in rank, while Peter Blau is the only one to have risen. The strong presence of economists in the JSTOR-based recognition ranking did not really diminish since they still occupy about a quarter of the top ranks, but due to the fact that citations from journals contribute less to the overall value, an internal regrouping has taken place. As a result, Schumpeter and Hayek are now the most important economists, both of whom also had the strongest impact beyond the boundaries of their original discipline. The only one who is comparable in this respect is much younger Albert O. Hirschman who, like all those who are still alive or recently deceased, was not included in the analysis of recognition in reference works. The pre-eminence of neoclassical economists, however, remains. Quantitatively, the sociologists are more strongly represented in the list and in the top ranks, followed by younger scholars, such as Blau and Bendix. A new name in the overall index is Karl Mannheim whose non-appearance among the JSTOR top forty is due to the archive's American bias. Representation of psychologists has improved, with Freud, Lewin, Moreno and Alfred Adler. Gestalt psychologists are largely absent from the five samples, and the Böhlers remained confined to the lower ranks, one reason no doubt being that they largely failed to establish themselves after emigration. Among the philosophers, Karl Popper's now considerably higher rank seems justified given his broad reception in the social sciences. Rudolf Carnap and Ernst Cassirer represent, as it were, philosophy of science and the classical school of philosophical thought. Representation of political theory and political science is relatively weak: Karl W. Deutsch and Hans Kelsen are found in the midfield and, lower still, Leo Strauss. Economic history is represented by Alexander Gerschenkron and Fritz Redlich, statistics once more by Emil Gumbel, demography by Christopher Tietze, and anthropology by S.F. Nadel.

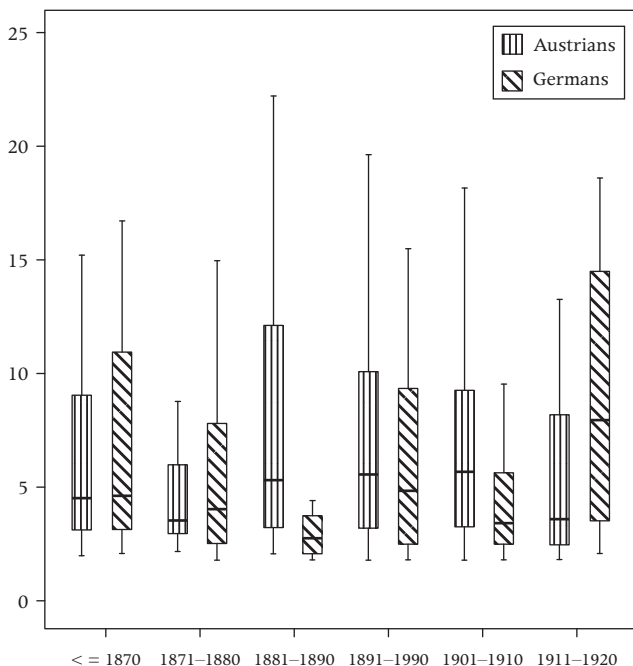
What I feel to be more informative than this analysis geared to individuals, however, are the group properties brought to light by this table and, even more so, by a cross tabulation of index values with other variables that have already been used. Thus, one look at Table 4.6 reveals a striking absence of female scholars. Hannah Arendt, the first woman on the list, turns up at rank 42 and, thus, fails to make the top 40 by just two ranks; the next one is Elisabeth Noelle-Neumann at rank 58, followed by Anna Freud (64), Rose Laub Coser (92), Marie Jahoda (103) and Charlotte Bühler (147). What is evident from the low number of female names is also reflected by a comparison of means: 1.48 for female sociologists and 2.68 for males.

The comparison of reputation by birth cohorts and between Germany and Austria shows that the arguments put forward so far for individual cases also



apply to generations and to those who came from, or stayed in, the two countries. Again, there are an equal number of Austrians and Germans among the social scientists with top reputation; moreover, the aggregated index value of the Austrians (3.18) is higher, for all cohorts, than that of the Germans (2.1); and both are clearly topped by the aggregated index value of individuals with double nationality (3.89). Germans score higher than Austrians only in the oldest cohort. Here again, individuals with double nationality (aggregated index value 5.3) outdo their generational peers who worked only in one of the countries. Thus, the founder generation can be said to have also founded the reputation of German-language sociology, and this is not only due to the two outliers Weber and Freud.

The diagram (Figure 4.5) shows the median and the second and third quartile below and above it, respectively, as well as the lowest and the highest observed values that are no outliers (thin line). Because of the large number of cases without an index value, the presentation was limited to cases with at least a minimum of reputation (index value > 1.1). In the two oldest cohorts, the medians of the Germans are clearly above those of the Austrians. In contrast,



**Figure 4.5** Index of Reputation, according to Birth Cohorts and Country

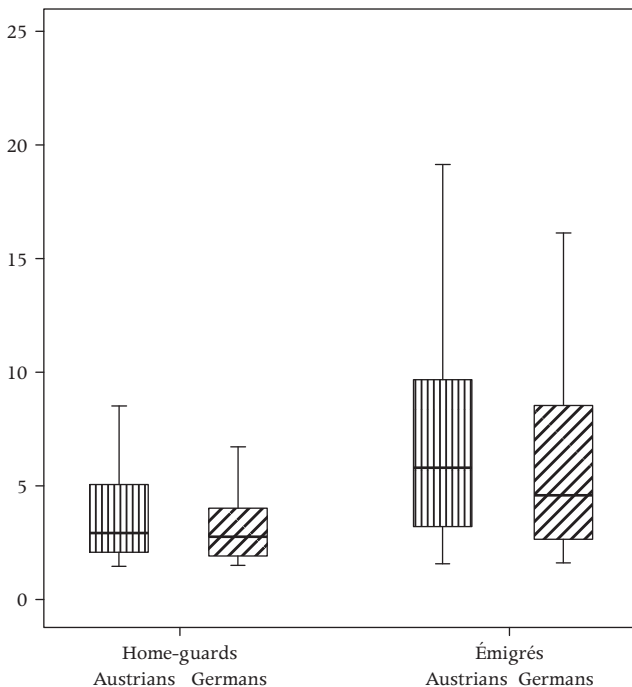
Notes: n = 343; bold bar: mean, the rectangle below the mean stands for the 2nd quartile, the rectangle above the mean for the 3rd quartile; only includes scholars with a calculated reputation >1,1.

Source: Author's calculations.

Austrians clearly come first in the three following cohorts (born between 1881 and 1910). Only in the youngest cohort is the picture again inverted. The Austrian 'home-guard' cannot keep up with the Germans.

If the population of German-speaking sociologists is organized by country and emigration status, with the comparison focused on the two generation units of emigrants and non-emigrants (excluding those who had died before 1933 and 1938, respectively), the groups are quite clearly outlined: emigrants from both countries of origin show a distinctly higher mean reputation value than 'home-guards'. Among the emigrants, former Austrians outdo former Germans (Figure 4.6).

Results of bivariate computations were somewhat surprising since they give grounds to the well-founded assumption that pre-Nazi discrimination against Jews was less strong than has so far been suggested in the literature. Of course, on the level of individuals, it is quite conceivable that people were discriminated against because they were Jewish. But on the level of group comparisons, no such pattern can be observed and, what is more, the findings almost suggest the contrary: there is no doubt that German and Austrian universities were



**Figure 4.6** Index of Reputation, according to Migration Status and Country

Notes: n = 320; bold bar: mean, the rectangle below the mean stands for the 2nd quartile, the rectangle above the mean for the 3rd quartile; only includes scholars with a calculated reputation >1,1.

Source: Author's calculations.

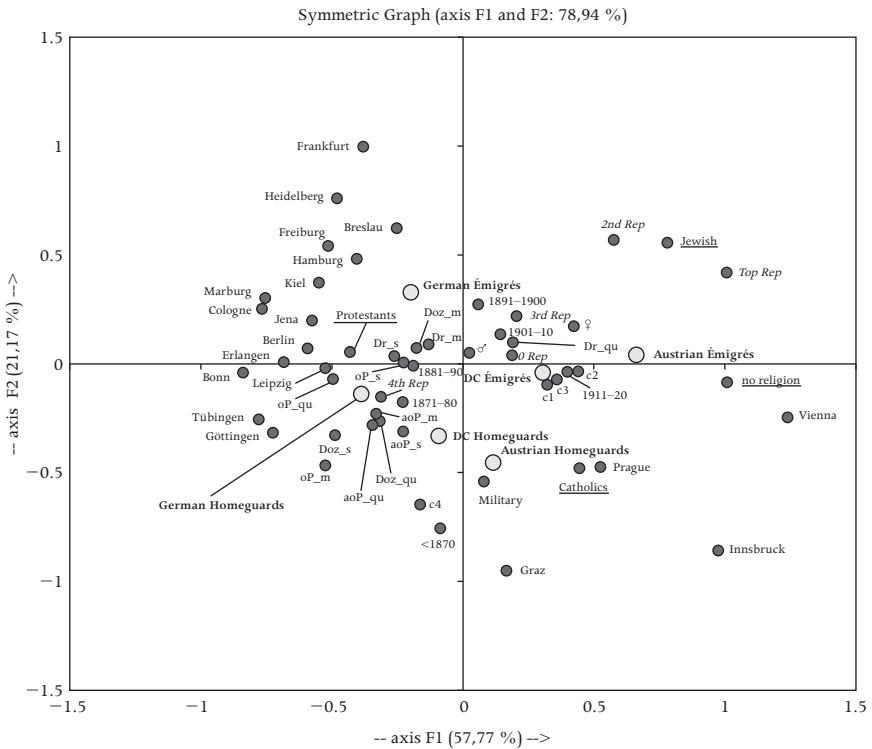
restrictive with respect to junior scholars and imposed long waiting times, but among those who succeeded in crossing these hurdles, Jews were no less successful. The other remarkable result that contradicts the conventional view is even less challengeable: in the United States, those who had been forced to emigrate met with an extraordinarily open university system that was not only quite ready to admit them but also offered them career openings that were better by far than those available to the 'home-guards'.

## CORRESPONDENCE ANALYSIS

To consolidate the analysis and, at the same time, to find a way to present the results more clearly, the data on the approximately 800 German-language social scientists of the early twentieth century who have been dealt with in this chapter were submitted to a multivariate analysis. To understand the results of the latter, no professional skills in statistical analysis are needed.<sup>40</sup> The so-called correspondence analysis can provide a graphical representation of the commonalities and the differences between groups. One has to keep in mind, however, that this procedure can show the combined effects of several variables, which is why it is called multivariate, and that the result is independent of the number of cases that are included in the analysis. The representation is done in a multidimensional space allowing for a four-step interpretation of the data. First, one can try to interpret the distribution on the horizontal and on the vertical axes with respect to content. Second, one can study the quadrants. Third, one can project the points that belong together in terms of content (the values of a variable) on one of the axes (or look at ordered pairs on an imaginary line that traverses the intersection) and, again, consider the points that are more close to each other as indicative of similarity and those that are more remote as indicative of dissimilarity. Fourth, one can 'spread' the angles that depart from the intersection of the two axes, with the points situated within the angle indicating similarity (the smaller the angle the higher the similarity between the traits included in it).

For better readability, not all of the variables taken into account in the analysis are shown in the following diagram.<sup>41</sup> Figure 4.7 shows the comparison of all German and Austrian social scientists as well as those working in both countries, with an additional differentiation between the subgroups of emigrants and non-emigrants.

The horizontal line results from the contrast of Austrians versus Germans; social scientists who worked in both countries are in-between. Since the vertical line can be interpreted as the contrast of emigrants versus 'home-guards', the combination of these two traits (country and emigration status) results in the positioning of the two groups of emigrants and non-emigrants above and below, respectively, an axis, i.e. the diagonal running from the upper left-hand side to the lower right-hand side. The places where doctoral degrees were taken correspond to the difference between the two principal countries in our analysis: Austrian universities are on the left-hand side, German universities on the right-hand side. A projection of the six groups formed on the basis of



**Figure 4.7** Austrian and German Social Scientists in Comparison

Correspondence Analysis Symmetric Graph: explained variance: Axis F1 and F2: 78.94%; horizontal axis F1: 57.77%, vertical axis: 21.17%. n = 676.

Notes: ♀ Female; ♂ Male; DC Émigrés: Dual Citizenship Émigrés; DC Homeguards: Dual Citizenship Non-Émigrés; Father's occupation (according to Ringer 1993): c1: educated upper middle class; c2: economic upper middle class; c3: economic lower middle class; c4: non-educated lower middle class; (added): Military; Denomination (Religion): Protestants, Catholics, Jews, no religion; Birth Cohorts: In decades, starting with '<1870' born before 1870; Occupational career (four levels): Dr: Doctorate; Doz: *Habilitation*; aoP: *außerordentlicher/extraordinarius professor* or associate professor; oP: *ordentlicher/ordinarius Professor* or Full Professor; Career Pace: quick (x\_qu), medium (x\_m), slow (x\_s); Reputation (in quintiles): 0 Rep: none at all; 2nd Rep; 3rd Rep: 2nd and 3rd quintile; 4th Rep: Rank 11 to 30; Top Rep: Top 10; Universities, where doctorate degrees have been acquired: Name of town (Graz, Innsbruck, etc.)

Source: Author's calculations.

the traits of country and emigration status on the horizontal axis reveals that the major contrast is between Austrian emigrants and German non-emigrants. The vertical axis is an adequate representation of religion: A projection of the data on religion on the vertical axis results in the Jewish being in the highest position, followed by the agnostics and the Protestants, with Catholics in the lowest position.

It is obvious from the way the three points of the reputation measure are organized that they cannot be represented on one of the two dimensions, and

that other factors intervene. Social scientists with a 'lower midfield' reputation can be easily characterized by the neighbouring point of German non-emigrants and by all the points that indicate career pace, while those with 'no' reputation as well as those with 'upper midfield' reputation are close to the centroid, which shows that these traits contribute little to an explanation of social space. The two top-reputation groups are close to the trait of 'Jewish' in the upper right-hand quadrant.

An analysis of the four quadrants reveals the similarities between the six groups. The upper right-hand quadrant includes two groups: Austrian emigrants and emigrants who had worked in both countries, both of which can be further characterized by two other traits – Jewish or non-denominational. Austrian non-emigrants are grouped in the lower right-hand quadrant and are further characterized by being Catholics. German non-emigrants and non-emigrants working in both countries include the highest number of social scientists who were 'habilitated' for an academic career and had even taken further career steps, while the factor of career pace does not particularly contribute to this.

The most remarkable finding brought to light by the correspondence analysis is a lack of correlations: neither social background nor gender nor age contribute to an explanation of the space in which the roughly 800 German-language social scientists can be located. In contrast, the traits that do contribute to an explanation are religion, the country where someone worked, and the contrast of home-guards and emigrants.

If the analysis is limited to the population of emigrated social scientists (n=408), the diagram brings to light some interesting connections. While the trait of Jewish/non-Jewish does not unequivocally differentiate between emigrants, the trait of home country does. Figure 4.8 shows that except for the contrast of Austrians versus Germans, the two main axes do not unequivocally correspond to specific variables. On the other hand, the four quadrants show some interesting combinations of traits. For the German Jewish emigrants in the upper-left quadrant, these traits are: above average number of remigrants (those who returned to Germany), support from refugee aid committees, pre-emigration positions as at most outside lecturers or associate professors, and membership primarily in the cohorts born between 1880 and 1899. For the German non-Jewish emigrants in the lower left quadrant, there are only two traits that further characterize them: the above average number who were full professors before emigration, and that (therefore) they did not engage in a second or third course of studies afterward.

Among the Austrian non-Jewish scholars in the upper-right quadrant, the number of those who took a second degree after emigration was above average, and their careers during emigration stopped at the level of an assistant professor (in the period analysed), though this may be due to the fact that they belonged to the youngest cohort (there are no career data for the post-1955 period). The Austrian Jewish emigrants in the lower right quadrant are characterized by the following traits: the number of female scholars was above average, they did not remigrate, they had at most taken a doctoral degree before emigration, and the



clearly. The degree of pre-Nazi discrimination against Jews differed in the two German-language countries, but those who stayed at home not only gained less reputation, which can be explained by their being rooted in a single science culture, but were also inferior, all else being equal, when competing for career positions with the mostly Jewish future emigrants whenever their success was not due solely to discrimination against their rivals.

# 5

## THE RADIO, ADORNO AND THE PANEL

As the previous chapter has shown, Paul F. Lazarsfeld (hereafter PFL for short) was a high-reputation social science emigrant. Although his departure from Vienna in the autumn of 1933 was not directly related to the handing-over of power to the Nazis in Germany or the elimination of democracy in Austria, he has always been considered a member of the group of political refugees, and rightly so since sticking to his plan to return to Vienna would have been highly unreasonable. This chapter will describe the beginnings of PFL's academic career in the United States. However, rather than re-narrate his first years of scraping along in New York after his RF fellowship had run out (cf. Fleck and Stehr 2007), I will focus on his entry into the research project that smoothed the way to the top for him: the Princeton Radio Research Project (PRRP) which, from 1937 onward, gave him the opportunity to move from the margin to the centre of the American science system.

PFL joined a research project of which he had had no part in either the design or the acquisition. Initially, his position was that of the 'second (or third) in line', but when negotiations for continuation began after the first two years, he already acted as the one and only negotiator for the project, making his own proposals and having them complied with. In the third year, he succeeded in having the project transferred to Columbia University, where at age thirty-nine he was appointed to the Faculty of Political Science, first as a visiting lecturer and, as of September 1941, as an associate professor. Becoming established like this is a highly contingent undertaking that requires aspirants to show consideration in all areas, avoid antagonizing the sponsors, keep any promises that are made and accept whatever setbacks are imposed; in short, prove that they are reliable and agreeable partners in the complex interplay of academic forces. In the following, PFL's trajectory will be reconstructed from two closely interrelated points of view, one of them organizational, i.e. PFL's interaction with the various other actors, and the other one oriented to content, i.e. the development of techniques of radio research and their results. A special concern will be paid to the analysis of the cooperation between 'American



empiricist' PFL and 'European theorist' Theodor Wiesengrund Adorno. Unlike previous literature dealing with this cooperation, the following will show that Adorno did not fail because of PFL, but because of himself.

### WANTED: RESEARCH DIRECTOR!

In the summer of 1937, Hadley Cantril and Frank Stanton, the designated co-directors of the research project that was due to start on 1 September, were in a tight spot: unable and unwilling to undertake the project themselves, they were looking for a replacement. Robert Lynd and George Gallup, independently from each other, recommended asking PFL, who was now running his own research centre at the University of Newark. For personal reasons but also as an emissary of Max Horkheimer's Institute of Social Research, PFL was back in Europe at the time. As a collaborator with the institute, he was charged with leading the European part of the study dealing with the erosion of authority as a result of unemployment and to represent the institute in Paris at a congress of neo-positivists (Dahms 1994: 227). It was there, arguably, that PFL first met Theodor Wiesengrund, who at the time did not yet call himself Adorno.<sup>1</sup>

In August of the same year, PFL in Newark, was vacationing in the Austrian Alps when he received a telegram from Cantril: 'Would you accept full time position beginning September directing Rockefeller Radio Research. Salary seven thousand another thousand Herta. Assistantship two years sure possible four. Headquarters Princeton. Lynd advises acceptance. Cable answer or questions ..., Hadley.' PFL replied: 'Much interested wondering if some connection of project with Research Center possible suggest decision end of August or telephone call Vienna Stop please prepay return cables. Letter follows, thanks, Paul.'<sup>2</sup>

In a subsequent letter, Cantril gave more details of the planned research project. As Stanton had in the very last moment declined to act as a director, Cantril had turned to Lynd to ask him if he could recommend someone 'like Lazarsfeld'. Cantril had not thought of Lazarsfeld himself as a candidate since he had assumed that PFL wanted to stay in Newark. As to the substance of the research project, Cantril was not very explicit. It remains unclear what had caused the Rockefeller Foundation – and more specifically John Marshall<sup>3</sup> as the officer in charge – to be 'quite excited' about this research project, but it may have to do with the fact that, given that the major educational value of the radio had become a special concern of John D. Rockefeller Jr, they were pleased to have found anyone at all who was ready to address the issue.

Rather than be more explicit about matters of content – 'you know my heart is not in this type of research as much as yours' – Cantril elaborated on the details of organizational issues. He and Stanton, as agreed upon with Marshall, would remain associate directors of the PRRP, providing the occasional idea but certainly not wishing 'to "boss" the job, especially if we could get someone of your caliber to run things'. As for PFL's wish to bring in his Newark Research Center, Cantril felt that the odds were rather against it. The PRRP

was one of those uniquely American grants, absurd in a way, that were awarded to a specific institution – in this case, Princeton University’s School of Public and International Affairs. Nominally at least, project headquarters would have to be at Princeton, a requirement certainly backed by the university authorities. Could PFL not take a leave of absence from Newark, or reduce his teaching activities to a *pro forma* lecture? Cantril declared himself ready to shield this, assuring that nobody at Princeton or in the RF needed to know about it.

After the two years of directorship, which as Cantril saw it were very likely to become four years, it would be easy for PFL to secure another job or just return to Newark. Until then he could easily ‘produce a couple of studies that would put your name at the top of the list’. Any publications from PRRP ‘would be entirely yours’. Cantril was evidently not thinking of himself as an honorary author. But the recently appointed associate professor of psychology at Princeton University speculated on catching the European through his uncertainty of status: ‘Your title throughout the job would be Research Associate in the School of Public and International Affairs at Princeton University. Doesn’t that appeal to your bourgeois soul!’ Finally, Cantril proposed to double the salary that Herta Herzog, PFL’s second wife, was to receive as an assistant, and specified further financial and organizational benefits which included the fact that the Lazarsfelds and the Cantrils would be able to see more of each other.

However, in the summer of 1937, PFL no longer was a ‘penniless immigrant’ who had to be grateful for any job he was offered; he was already in a position to choose between several options. In a detailed letter he expedited from the Austrian mountains – ‘it is a queer experience to sit in a mountain village and to discuss American research problems’ – immediately after his telegram to Cantril (whose letter had not yet reached him at the time) he wrote that while he was certainly pleased at the offer, he nevertheless had to balance it against his own plans. Information on what was being offered him was much too sparse and, what was more, he was unwilling to abandon what he had set up in the course of the past year. By founding the Newark Research Center, he had pursued two aims: he had wanted to conduct the largest possible number of different studies in order to enhance his own methodological know-how, and he had tried to set up groups of junior scholars whom he wished to train in the kind of research he was interested in. While it seemed unproblematic to pursue the first issue within the PRRP – ‘radio is a topic around which actually any kind of research methods can be tried out and can be applied satisfactorily’ – he was unsure of whether, and how, a satisfactory solution could be found for the second issue. He wondered whether Cantril was aware that by that time students from other universities were coming to Newark on a regular basis to work with him on specific projects. The question, therefore, was whether Cantril felt that it would be possible to conduct at least part of the PRRP in a way ‘that it could be used as a sort of training institution’.

This led to a third question that pertained to a strategy he had adopted for himself: while the Newark Research Center had indeed reached a level of consolidation that allowed it to function without him, his personal perspective was to link up all his research activities and publications with the centre's name. Founding a research centre was not something one did every day, and even if the University of Newark might not be a top address, at least his research centre might still become just this within another decade. It was an institutional investment he had no intention of writing off:

You see all comes back to an European attitude which might be not so easy to understand from your point of view. I feel strongly that I don't want to go ahead alone, that I want to stay for an institution and I try to build up an institution which is able and willing to stand for me. Of course, I will have to do very different things, less glorious but about the same way as you are a Professor in Harvard, then in Columbia, then in Princeton. But as my poise and my past and my name cannot compare with yours, I try to identify whatever I do with an institution which might after some time acquire the dignity which I myself for reason of destiny and maybe of personality can hardly aspire at.

After some further exchanges with Cantril, PFL accepted the offer of taking charge of the Princeton Radio Research Project. Cantril immediately reported on this welcome news to the Rockefeller Foundation as well as to the authorities of his university, who followed suit by appointing PFL a research associate as agreed.<sup>4</sup>

In one major point, PFL refused to give in to Cantril: he did not transfer to Princeton but continued to live in New York's Upper West Side, with his professional headquarters located on the other side of the Hudson River in the shabby industrial town of Newark. That said, for some time his postal address was 'Office of Radio Research, 203 Eno Hall, Princeton University, Princeton, New Jersey', and only the initiated addressed their letters to the 'Research Center, University of Newark, New Jersey'.<sup>5</sup> The old brewery building that housed the Research Center suited PFL far better than any room in one of the red-brick buildings at Princeton's campus could have done. During his whole life in the United States, PFL, whose mother was a specialist in individual psychology, believed or at least tried to make others believe that he was the social odd-man-out, suffering from an inferiority complex. At Newark, he was the university President's chosen innovator while at Princeton he would have been reduced to being the bottom dog for one or the other recently appointed young professors and held to respect the academic pecking order. Another reason for PFL to not really transfer to the famous Ivy League university may have been that at the time, as part of a complex organization, he preferred having a protector of the highest possible rank. He may have felt that Cantril, being five years his junior, and notwithstanding his upper-class background, was not quite up to this role. The veiled hints dropped in the letter quoted above – 'my poise and my past and my name' – may also be read as saying that at Princeton, he feared coming up against anti-Semitic and anti-socialist prejudice.

In complying with PFL's demands, Cantril had gone much farther than he had initially planned, which probably had also to do with his own situation: the PRRP was located at Princeton's School of Public and International Affairs, and Cantril himself was affiliated with Princeton's Psychology Department. Any failure to get the project started as agreed would have resulted, only some months after his arrival, in a severe loss of face for the ambitious young professor and would have given rise to second thoughts not only in his own department. As for Rockefeller, to give up a project that had already been approved would jeopardize Cantril's chances of receiving RF support for future endeavours. By acting as he did, Cantril showed the sponsors that he was not prepared to slip out of a previous commitment as soon as something better presented itself yet made it his duty to ensure that the handing-over and transition were carried out in due form. After all, Cantril could be sure that in PFL he had found someone whose intellectual and organizational capacities were up to the task. And, finally, being an associate director of such a well-endowed project would boost his own reputation not only at the local level.

Ironically enough, the ex-Viennese succeeded in having Cantril sell to the sponsor one of those little departures from the truth that Cantril himself had suggested to PFL to pry him away from Newark. In a letter dealing with the start of the PRRP, Cantril informed John Marshall that after lengthy discussions an agreement had been reached with PFL about the administration of the project. The 'central office' would thus be located in the room provided by Princeton, housing a typist, Cantril and 'the complete record and data of the study'.

Lazarsfeld, as you know, has been doing Marketing Research in Newark for the past year or two, and has a very complete knowledge of Newark and its environment. It occurred to us that for the first few months or the first year, it might be well to use Newark and vicinity as a testing center, where Lazarsfeld has so much information regarding educational, vocational and economic status. Would the Foundation have any objections if Lazarsfeld physically spends the majority of his time, and had an office, at the University of Newark?<sup>6</sup>

A few weeks later, Stanton – who apparently had not taken part in the summer's negotiations – and PFL paid their first official visit to the RF, where PFL managed to live up to his life-long reputation for being spectacularly late.

At the time, there was no consensus on the duties bound up with being a project director. Cantril obviously felt that the task, being as it was a regular job, excluded any other activities, while Marshall took the line that would later be the rule, namely that the receiving party had to meet the objectives agreed upon with the sponsors, no matter how. Given that there was no day-by-day contact between the RF and its grant recipients, this contractual interpretation of 'management by objectives' obviously met its expectations for control (and the practice soon became the binding norm).

In terms of content, however, the PRRP was still as vague two months after its official start as it had been in Cantril's first letter to PFL. One of its objectives

was to develop methods for determining the 'actual rôle of radio in the lives of listeners'. PFL declared that, for the time being, rather than proceed with staff recruitment he would engage in more elaborate studies whenever the opportunity presented itself. His concern was with the motivation of radio listeners, since the main question was, 'why people listen. When he [PFL] has laid out the groundwork for a methodological approach to this question, he intends proceeding to such other questions as Who listens, When, How, and What is listen[ed] to.'

To this end, Marshall goes on to say in his memorandum that PFL had organized a conference with other social psychologists, among them Robert Lynd, and general psychologists, such as John Dollard,<sup>7</sup> with a view to benefiting from their approaches. PFL himself had been interested in radio research for a couple of years and, together with his wife, had worked and published on the issue in Austria.

PFL used his first months as the new project director to proceed with his ongoing studies at the Newark Research Center and to pass them off as contributions to the elaboration of a working programme for the PRRP. In this, he acted on a maxim that would become the trademark of Lazarsfeldian institutions: receipts from new contracts were used to cover the debts run up by previous projects.<sup>8</sup> PFL did not act on Cantril's initial proposition to hire four assistants or let Herta Herzog be one of them. The latter decision may also be due to his wish to avoid being accused of nepotism<sup>9</sup> and to the fact that his lavish salary seemed more than sufficient to serve as a family income.

PFL used his contact with Stanton to familiarize himself with the market research that was being done at CBS (Columbia Broadcasting System) or was otherwise accessible via Stanton, from time to time suggesting improvements as well. From this, and rather by coincidence, a new research strategy seems to have emerged: the collection and reanalysis, from a new perspective, of the data of past studies. Thus, 'secondary analysis' was born,<sup>10</sup> a solution that was in more than one way ideal given PFL's needs and dispositions at the time. He could rapidly gain access to a wealth of data, and he could be lavish in collecting them by relying on the numerous members of his staff. As an individual worker, given the costs of mechanical (and absence of electronic) computing devices at the time, not even PFL would have hit on the idea of collecting the data of others. But as the director of a large-scale project, he had collaborators whom he could train in the use of statistical procedures and methods of data analysis with which he was either already familiar or developed. His collaborators-cum-apprentices, in turn, were likely to find work on real problems more rewarding than exercising with dull textbook examples. Finally, the whole procedure was bound to be relatively cost-effective since it can be assumed that the Newark Research Center did not need to buy the data but got them for free, as they were of little value to their owners once their analyses were done.

As a sideline, PFL organized the above-mentioned conferences with psychologists and sociologists with the aim of profiting by their expertise.

For this, he recruited psychoanalysts such as Erich Fromm and Fritz Redl<sup>11</sup> in New York and went to Chicago to meet the local brand of social scientists, among them Ernest W. Burgess, Louis Wirth, Herbert Blumer and his own former collaborators Arthur Kornhauser, Samuel Stouffer and L.L. Thurstone.<sup>12</sup> PFL also invited John Marshall; his attendance at both of them demonstrated the RF Humanities Division's strong interest in the progress of the PRRP.

The very fact that such meetings took place is remarkable and substantially differs from habitual European practices then and later. At the time, social research conceived of as a cooperative undertaking was already an established form of communication in the United States, where colleagues were not generally seen as rivals from whom one seeks to hide as much as possible for as long as possible. Rivalry among social scientists was limited to priority rights for publications but did not extend to discussions about research projects. It can be assumed that even in Vienna, PFL, well aware that transforming bright ideas into serious results usually requires a good deal of hard work, would not have been haunted by the fear of somebody stealing his ideas. But in Vienna, and even disposing of the same means, he would not have dreamed of offering Sigmund Freud (or anybody) \$20 in remuneration for participating in a discussion on the design of a planned study.<sup>13</sup>

## A NEW TESTAMENT

On New Year of 1938, PFL drew up a memorandum of fifteen pages containing a detailed research plan for the PRRP.<sup>14</sup> During the first radio years, researchers studying it in the United States had primarily focused on technical issues of frequency allocation, operating stations, and the relation of private and public broadcasting:

As broadcasting and listening rapidly increased, it became evident that it was not the technical aspect alone which needed systematic organization and planning based on actual knowledge. Who should broadcast, when, and how, has been the main concern in the past. To whom one should broadcast, what, and why, has now come to the foreground of general interest.<sup>15</sup>

The PRRP did not need to start out again from scratch, but could draw on the wealth of practical experience and systematic research findings accumulated by commercial radio stations in the previous years. Non-commercial operators in particular needed detailed knowledge about their public, since their programmes were targeting specific audiences. While the commercial stations could be satisfied when their isolated message was effectively broadcast, educational programmes needed to know how their programme compared to other activities in terms of relevance. Therefore, the first thing to find out was if there were any patterns of interest in radio listening: 'Do the people who listen to the spoken word neglect music over the radio? Does the interest in modern dance music compete with that in the classics?' The point was to find out what part radio listening played for each individual, whether

it brought about changes in their behaviour and attitudes and if so, which kind of changes. The insights of psychological personality research – an allusion to Cantril's teacher, Gordon Allport, albeit without naming him – could be made fruitful for radio research.

In order to fully appreciate this research perspective it should be noted that the new medium had to break up conventional behaviours and make listeners modify their time budgets. Wanting to know which other activities might be superseded by radio listening was therefore a legitimate concern. In later decades, television and, most recently, internet use would give rise to the same question. Concerns about the new medium's potential to eliminate other well-established and well-regarded activities were as widespread then as in later years. PFL shared neither this cultural pessimism nor the fears that arose from it, pointing out that the new medium could act both ways, as likely to balance existing social disparities as to be a source of new forms of experiencing insecurity:

Here is the farm agent in a southern area who feels that the sharecroppers will take a more independent stand toward their landlords when they can learn the price of cotton directly over the radio. Here is a journalist on a local newspaper who fears that he will lose his job because the radio news services and the news commentators will do away with the local newspaper all over the country. Here is a musician who hopes that the radio will contribute much to the advancement of modern music because the classical forms will be more quickly worn out due to the increasing frequency of their presentation over the air.

Besides investigating the personality (types) of the radio listener, another aim was to identify successful programmes (i.e. those which appealed to a great variety of listeners). For this, the procedures used in experimental psychology could be drawn upon. Stimulus variations could be implemented with the aim of systematizing the technique of programme analysis. Due to their limited financial scope, however, non-commercial radio stations could not run tests on every single programme. But if the experiments planned in the PRRP turned out to be successful, this might result in:

an abbreviated panel method by which non-commercial broadcasters can stay in contact with a representative group of the listeners and get leads as to the extent of and reasons for acceptance of programs ... We shall try to develop a body of directions as to how listener panels should be selected and contacted, what questions they can and should be asked, and how far that information can be used for the guidance in program making.<sup>16</sup>

This is the first time in PFL's *œuvre* that the term 'panel' is used to describe a specific method. It is still far from what it would later be understood to mean, but the PRRP was to become the site where this method, so closely bound up with the name of PFL, made its first steps into the cache of social research techniques.

While according to PFL, the commercial radio stations were only interested in the 'sales effect', the educational programmes were committed to more long-term interests and more sustained effects. Therefore, one of the core objectives of the PRRP would be to investigate the effects of the radio in three ways. A first would be to use selected programmes, such as the round-table discussions broadcasted by the University of Chicago, to study the immediate effects of this kind of stimuli, using control groups to consolidate results. But since it was impossible to know beforehand which programmes might or might not have an impact, a second approach would be to collect data on 'incidental situations' that had demonstrably had an impact on the life of the listeners.

In a third step, since one could not reasonably assume that any impact the radio might have would be an isolated incident, 'radio biographies' would be collected to establish the chain of influences that had acted on the individual, while allowing for the possibility that these effects might well be different at different times. In this respect, comparisons between adults and younger people were the obvious method, since only the former could look back on periods in their life where there was no radio effect. For those who had grown up with radio, changes in their relationship with the medium that occurred over the course of their lives could be observed in detail.

The impact of the radio on social life cannot, however, be studied solely on the level of individuals, but must take into account the larger social trends that either alone or in a covariance yet to be established determined social development. Expectations concerning the manifestations of such trends in the population were powerful determinants of individual action. It had occasionally been argued that the radio contributed to the urbanization of smaller and rural communities: 'In a special situation, how should we test the extent to which this urbanization has taken place? How can we separate the influence of radio from other factors working in the same direction, such as the car and the movies? Does this urbanization come about with different speeds in different areas? What accounts for the differences?'

However, if the PRRP were to limit the range of its studies to the topics described, the results might quickly become obsolete because of the rapid evolution of the whole field. The only remedy for this was to seek results that were universally valid and, thus, applicable to changing conditions:

The kind of questions pertaining here are usually given such forms as: What are the basic motivations of people, why do they act or feel the way they do? We shall try to attack this most difficult sector of any social research proposition; but so little common understanding exists here that a theoretical background must first be gained. One of our publications will consist of a scrutiny of all the theories and techniques available for studying motivation in social research, paying particular attention to radio.

Certain technical aspects of radio programmes should not be ignored either. In the movies, 'personal glamour and rich settings' played a major role because



they could be shown on the screen, while on the radio these elements were irrelevant:

Could it, therefore, be that the radio play will become more sober and more adapted to the habitual middle-class setting of the listeners than is the movie? The microphone permits acoustical effects which a concert hall does not have. Will that affect the future trends in music? The political speech over the radio commands attention in a different way than does the printed page. Will that finally lead to a division between the content that is conveyed over the radio and that which remains preserved for newspapers and magazines?

Since all these questions were highly dependent on the respective fields, the investigation had to be narrowed down to just a few of them. The following four had been provisionally selected: music, book-reading, news, and politics.

The research plan elaborated by PFL after a few months as a project director does not strike the reader as overly precise with respect to what was to be done. It is, rather, a loose collection of potential sub-studies concerned with the role of the radio in one way or another, tied together by a vague conceptual framework. PFL sought to adapt the studies initiated in Newark before the start of PRRP to the new task, and said so quite openly when talking to Marshall, as the latter noted:

At the same time, a number of tentative surveys are being undertaken to test the feasibility of certain mediums of inquiry; e.g. the recent New Jersey gubernatorial campaign provided an opportunity for gauging the extent to which radio is influencing political opinion. A random sampling was made in a number of New Jersey cities which has produced rather interesting results.<sup>17</sup>

Cantril's original exposé, 'The Essential Value of Radio to All Types of Listeners', (disparagingly dubbed the 'old testament' at Newark in contrast to PFL's 'new testament' memorandum; see Rogers 1994: 270), contained a list of questions that needed to be cleared up before starting. It resonates with Harold Lasswell's classical formula for the objectives of content analysis, even though Lasswell focused more explicitly on the nexus of radio station, medium and listeners.<sup>18</sup>

The orientation to educational radio programmes that looms large in the 'new testament' of the new year of 1937/8 is another feature that PFL seems to have inherited from the original exponent of the Princeton Radio Research Project. PFL permitted himself to introduce some of his own hobby horses into the programme. Listener motivation certainly was one of them, as well as the consistent emphasis on the role of musical programmes which, given PFL's endowment with cultural capital as a Viennese and lifelong violinist, is not surprising. The listener survey conducted for the Austrian RAVAG had already brought to light the high importance of musical broadcasts (Mark 1996). What is more surprising is that some of PFL's more recent experiences, which he had

found important enough to mention in his preliminary talks with Marshall, were not mentioned in the research programme. Herta Herzog's Vienna doctoral thesis on the patterns of attribution of social traits to given voices and other psychological experiments seem to have been of less interest to PFL by this point.

If PFL was not too busy giving lectures to meetings of potential clients, who meanwhile included all those who were interested in the PRRP or might eventually become important for its success, his primary concern was with the improvement of the techniques used for analysing surveys done by questionnaires. In the context of local elections in New Jersey, he had in the autumn of 1937 started his first experiments with early forms of the panel technique, which he described in an enthusiastic report not only to Marshall, as mentioned above, but also to his two associate directors in regular updates.

As to the political survey, we are still very busy developing the questionnaire. The interviewers are making a number of interviews every day and then I change the questionnaire on the grounds of the returns ... our main interest is to develop the right kind of questionnaire which will next time be used immediately after the election in a pre-organized way. We will now get a lot of important material which will permit us to make an inventory of all the very different ways in which radio speech might influence people with different kinds of political interest.<sup>19</sup>

Cooperation with Cantril and Stanton developed along rather different lines. Stanton, whom PFL hardly knew, soon turned out to be a loyal partner who provided PFL with everything that could be helpful for the PRRP and the Newark Research Center. A friendship soon developed that far outlasted the project. Dealing with Cantril was more complicated, since he was not only suspicious of the repositioning of the content of his project but also seemed unable to meet deadlines for the tasks he was supposed to supervise or carry out himself. Moreover, he thought of himself as the real boss – intentionally or not, his August 1937 declaration of not wanting 'to boss' the PRRP soon turned out to be inaccurate. To a certain extent, however, Cantril had no choice: since the financial part of the project was handled via Princeton, he acted as the local contact and was probably given a piece of the administration's mind concerning PFL's chaotic bookkeeping more often than he had bargained for. The mutual friendship of former years and the first months of the project cooled and later broke up completely.

PFL had decided early on to give considerable room in the project to the study of music on the radio. At the time, the radio was the only medium that offered musical performances to a broad range of listeners. The record had already been invented but played a very minor role, leaving those who wanted to listen to music to go to a concert hall or another more or less exclusive place dedicated to musical performances, or to perform themselves. More than half of the broadcasting time of all American radio stations was dedicated to musical programmes. This almost inevitably raised a number of questions. Would

the radio contribute to the elimination of the distinction between elite serious music and popular music? What kind of music people would choose to listen to on the radio, and why? Did listeners give their undivided attention to the music they heard or was it rather a constant background stream? For these and other questions, PFL was searching for adequate collaborators.

### RECRUITING A MUSIC EXPERT

Although PFL had met Theodor Wiesengrund in the summer of 1937 in Paris, shortly after the launch of the PRRP he chose to contact him via Horkheimer. At the time Adorno was struggling to complete his Ph.D. in England. PFL was familiar with his article 'Zur gesellschaftlichen Lage der Musik' (On the social position of music) published in the *Zeitschrift für Sozialforschung*, but perhaps did not know that Hektor Rottweiler's 'Über Jazz' (On Jazz) actually came from the same author. What he could not know by any means was that Adorno and Horkheimer had repeatedly discussed him in their letters, reaching a consensus that was clearly unfavourable.

In October 1936 Horkheimer had informed Adorno that an acquaintance of the latter, Paul Oppenheim, would be 'delighted' to contribute to the forthcoming issue of *Zeitschrift für Sozialforschung*:

Lazarsfeld, who has long been called upon to write an English article on how to employ a number of American empirical sociologists who are interested in the Institute, has now availed himself of the opportunity to take on Oppenheim and speedily write a little article on the concept of type. Since I wrote a note making it quite clear that Lazarsfeld's criticism of O. as expressed in this study is not based on dialectical logic but remains inscribed in the field of statistical logic, our theory-minded readers will at once see that his contribution is by no means about philosophical issues as such but about special problems of purely empirical social research. On the other hand, Paul [Lazarsfeld] and a number of others who are interested in this kind of discussions will see a scientific finesse in this study.<sup>20</sup>

On 20 October 1937, Adorno received a telegram from Horkheimer offering to relocate him to the United States: 'Part-time job with new Radio Project Princeton University. Two-year income to be provided \$400 per month guaranteed.'<sup>21</sup> In a letter written on the same day, Horkheimer explained the arrangement in more detail. After his arrival, Adorno would have to take in hand the relationship to PFL. On no account should he work as PFL's assistant, 'but participate in the implementation of the research project for which considerable funds have been allotted to Lazarsfeld by Princeton University'. For both correspondents, the European model of the assistant quite naturally came to mind.

Leaving aside the question of how Adorno's transfer to New York would impact the balance among the planetoids orbiting around Horkheimer (cf. Wiggershaus 1986: 293–364), Horkheimer made fairly clear in his letters what he was offering Adorno with respect to life outside the universe of the

institute: an opportunity to establish himself in the New York world of social science or music. To a certain degree, Adorno seems to have not only understood this but subscribed to it. Thus, in December 1937 PFL reports to his two co-directors in terms that are otherwise rare in his memoranda:

In the meantime I have had more correspondence with Dr. Wiesengrund whom I suggested for the musical study and came to some tentative arrangements in case he can be here by the end of January. His record is really marvelous and splendidly suited to our purpose ... I had a letter from him yesterday telling me that during the time he was in charge of music in the Frankfurt radio he got good contacts with American people which he has maintained since he is working in Oxford. That will make work with him easier. I think he is another case where a foreigner can be gotten at half the price we would have to pay an American of equal competence.<sup>22</sup>

In this context, it should be noted that unlike Horkheimer, who used to spend hours each day on his large correspondence, calculating it tactically to the last detail,<sup>23</sup> PFL was a lousy letter writer who wrote only when something really important was at stake. PFL's letters to Adorno clearly show that he did not just want to do Horkheimer a favour.<sup>24</sup> He obviously expected a major contribution to radio research from Adorno as a collaborator. In the first letter of 29 November 1937, he asks Adorno to discuss ideas concerning his future work by letter even before coming to New York: 'I intend to make the music section, so to speak, the hunting-ground for the "European approach." By that I mean two things: A more theoretical attitude toward the research problem, and a more pessimistic attitude toward an instrument of technical progress.'<sup>25</sup>

It is the first point that is primarily brought to Adorno's attention. PFL argues that even though research in the PRRP was to be done from an empirical perspective, it could only benefit from previous theoretical reflections. Thus, Adorno's article in the *Zeitschrift für Sozialforschung* was exactly what he and his associates expected Adorno to contribute. However, the theoretical reflections would need to be taken two steps further: (1) toward an empirical research problem; (2) toward an actual execution of the field work.

For inspiration, PFL adjoined a copy of the letter that he had addressed to the New York psychoanalysts explaining the research questions he had sent them, and asked Adorno to provide a list of the problems he considered to be of special importance. In all this, PFL was fully aware of the difficulties involved with coming to a fruitful cooperation with Adorno: 'I understand completely that it will take much writing and talking back and forth before we would really understand one another.'

Adorno took his time answering, going to Italy on a vacation in the meantime, and did not send an answer to 'dear Mr. Lazarsfeld' until the end of January 1938. This seven-page letter is arguably the first document in which Adorno comments on questions of empirical research, and for this alone merits

further attention. He starts out by saying that his theoretical stance does not at all imply an aversion to empirical research:

On the contrary: the concept of 'experience,' in a very specific sense, is more and more in the core of my thought. However, it is my conviction – and on this, I believe, we fully agreed in the talks we had in Paris: that 'theory-free' research is a fiction and that there is interplay between theory and empirical research: the very interplay we usually describe by the term of dialectical method. I am particularly delighted to see that you, too, share this point of view in your letter, and I think that I can in return promise that my part of the work will not be confined to the realm of free reflection either.<sup>26</sup>

This passage makes use of some highly familiar techniques of persuasion, such as claiming that anyway there is complete agreement, which had been recently stated by both parties, and that the other had said so himself in his previous statement. Adorno at least pretends to believe that there was no problem in coming to an understanding. He ignores PFL's allusion to the need for 'much writing and talking back and forth before we would really understand one another', implicitly shrugging it off as exaggerated misgivings on the part of the other. The 'very specific sense' of the concept of experience is not further elaborated on, and the truism that there is no research without theory is used to postulate a consensus even before the positions from which such a consensus might be reached have been specified.

For Adorno, the conclusion to be drawn from the 'theory-based approach' is that it seems unreasonable to set in motion the whole clumsy research apparatus in order 'to come to results that are clear as daylight even before one started'. Those who have never dealt with any such 'apparatus' tend to cultivate prejudices against the unknown, which always boil down to saying that one might as well do without it. This argument has been raised against procedures of participation as well as against instruction sheets and, in this case, empirical research. Adorno's favourite truism is: 'to employ the mental and economic power of extensive research works ... where it is really *worth* employing, i.e. where findings are obtained that would be impossible to obtain without this effort' [*italics in the original*]. Whether or not the findings obtained as a result of an empirical study are worth the effort is something one never knows beforehand – otherwise one could indeed do without the whole effort. In the PRRP, despite assertions to the contrary (Wiggershaus 1986: 276), there was no client to define specific targets, and sponsors and researchers in those days were quite undaunted in their belief that empirical research just made sense.<sup>27</sup>

For Adorno, the 'clumsy research apparatus' he repeatedly alludes to is reducible, or reduced, to the mechanism of questioning. At any rate, he feels that it should primarily be brought into play, 'where the theory has proposed theses of an exposed or problematic nature, thus using the research apparatus as an agency of control,' and ironically adds: 'I believe that this is not the

opinion of a hopeless Hegelian highbrow but also the wisdom of the provident paterfamilias.' The example he then gives illustrates his level of familiarity with research issues:

Thus, in order to find out that the acoustic type, in terms of experimental psychology, will respond more strongly to the radio than the optical type, one need not set in motion a large research apparatus; not only can the result be prophesized, but the discrimination between acoustic and optical individuals is such a neutral one, even on the social level, that even the most reliable statistical data cannot be expected to contribute to a social theory of the radio.

It is something of a mystery where Adorno unearthed this typology, which is to be found neither in PFL's memorandum nor in the psychological literature of the time on radio listeners. Allport and Cantril considered a broad range of aspects to understand radio listeners – age, income, sex, intelligence, cultural level, attitudes, occupation, etc. – but it never occurred to them to use an approach based on a distinction of optical versus acoustic types (Cantril and Allport 1935). Actually, what Adorno wanted in the first place was not to propose a concrete psychological hypothesis but to reject psychological studies in general as superfluous or at least of secondary importance:

Generally, I would like to advise against a 'psychological' method that conceives of individual consumers as test subjects and hopes to obtain valid results by testing them. This kind of procedure presupposes nothing less than a liberal view of society where people act according to their psychological constitution and where the societal side of their action, their social character, becomes visible only as a variable in the framework of psychological constants.

Adorno rejects the idea of humanity that underlies this kind of research, but it remains unclear whether he believed that the presupposition he describes could be replaced by a different view of the societal determination of individual action. His remark concerning the presupposed liberal views of society is ambiguous since it can be read as saying both that the psychologists thus criticized were presupposing one of several views and that they took the existing American society as their unquestioned starting point. Referring to social character as a variable in contrast to psychological constants results in a mix-up of the concepts of social research with those of philosophical anthropology where the search for constants (i.e. for universals of social action) is a longstanding favourite.

In his general rejection of psychological methods, Adorno completely failed to see that PFL's understanding of psychology was, at the time, pitted against two rival views. PFL tried to democratize, as it were, the older European tradition of introspection by postulating that trained psychologists were not the only ones who could produce valid data on individuals' mental processes. Rather, he felt that by refining questioning techniques he could enable ordinary people to give valid information on why they had acted as they had.

In this, he was opposed to the American behaviourists, then entering their golden age, for whom this was just mumbo-jumbo.

The only thing to be gathered from Adorno's remarks on issues of questionnaire design is that he had no real knowledge of the field but thought himself wise enough to lecture others. He was generous enough, however, to concede that it would be 'in everyone's best interest ... to put [my] theses on verification to the test of adequate survey methods'. As to who had the power to define what an adequate survey method was, and under which conditions it was adequate, nothing is said.

For the rest, most of Adorno's remarks are dedicated to an exposé of his ideas on music on the radio. He apodictically states that, 'for the radio, as for political economics as a whole, the decisive information is to be gained from the relations of production on which consumption depends'. His understanding of this analogy between the relations of capitalists and proletarians (i.e. the relations of production in a Marxist perspective) and the relations of radio production soon becomes clear when he writes that 'the analysis of the musical radio needs to start out from its technique, as its mode of production'. However, the analogy with the criticism of political economy, arbitrarily introduced by Adorno, suddenly gets muddled. Just as the mode of production it is not exclusively determined by technology in the realm of material production, it seems similarly unreasonable to assume that the 'societal relevance' of the radio could be exclusively explained by an analysis of its 'technical method of production'. This, however, was his key thesis – underlined for emphasis – which 'needed to be verified and the verification of which I see as the essential task of our cooperation'.

Adorno later proposed a rather consistent explanation of what he had in mind, relying on the ideas published by Ernst Krenek in an article entitled 'Bemerkungen zur Rundfunkmusik' (Remarks on radio music) in the *Zeitschrift für Sozialforschung*: presentation of music on the radio fundamentally differed from presentation in a concert hall; radio listeners were not only confronted with the lesser quality of the broadcast but were induced, by factors that are not described in detail, to attribute something in the nature of a picture to what they heard. Moreover, while in the concert hall, the real apperception of time resulted in an 'Aufhebung' (suspension) of chronological time, radio listeners were continuously made to feel this real time as a result of the 'Hörstreifen' ('hear stripes'; e.g. the rolling background or 'white noise' sound of radio, a constant reminder of its reproduction). But rather than elaborate in detail on the possible inter-connections this suggested and confront the experts of social research with the demand to think up, or at least try out, an adequate survey technique, Adorno felt sure from the start that this could not be done, couching his judgement in the rhetorical question: 'But how could this mode of apperception be captured by surveying the listeners?'<sup>28</sup>

However, not carrying his obvious aversion to empirical research to extremes, Adorno conceded that after a detailed analysis of radio production,

'it might be possible for us to develop methods for analysing their "correlates" in listeners'. Thus, once his exploratory work for a theory of the production of radio music was accomplished, he wanted 'to conduct interviews ... at key points of radio production, that is, with operators, technicians and artists'.

Adorno then proposed – 'since I am already making propositions in empirical terms' – to dedicate one of the listener studies to an analysis of listeners' letters, since he was familiar with this kind of material from his time with the radio in Frankfurt:<sup>29</sup>

As a rule, interpreting [these letters] should be done with a good deal of caution since the authors, just like those who write letters to newspaper editors, generally belong to the type of the know-it-all, the grumbler, the stuffed shirt while those who are positively impressed are much more likely to be quiet. On the other hand, these letters are more informative than the responses given to questionnaires since they are written spontaneously and without the inhibitions produced by questionnaires. Most notably, the grumblers here are much less likely to pretend to a higher intellectual level than what is actually theirs, and will much more often give vent to their hatred of superior intellectual demands, which I assume, once more drawing on my Frankfurt experience, to be rather wide-spread among today's passive radio listeners.

Thus, where Adorno can draw on primary experience in the field, he brims over with suggestions but at the same time already knows all about the grumblers and their motives.

PFL's hopes that he had scored a hit by inviting Adorno were indeed justified. He could reasonably expect that they would be able to talk the latter out of his prejudices against empirical survey techniques, and any lack of knowledge just spurred on the pedagogue in PFL. Even before Adorno left for New York, he got an answer. PFL wrote that he had been much impressed by Adorno's remarks on the changes imposed on music through broadcasting, as well as by the paper by Krenek that he had read in the meantime. He also agreed that 'such an approach needs a theoretical analysis first'. On the other hand, he never tired of stressing what Adorno would have to do: 'We shall have to understand that you have to end up finally with actual research among listeners, although in many cases we might have to stop with the formulation of the theoretical problem and discussion of techniques to answer them, simply for reasons of time.'<sup>30</sup> All of Adorno's most valuable remarks needed to be tested to see whether they were true:

Of course, there is no reason to expect that the answer could be found just by a questionnaire method, and I certainly do not think that empirical research and interviews are identical. It might be that experiments are necessary, or a very detailed introspective analysis with a few subjects especially trained for introspection, as the Wurzburg school used them. At any rate, I feel very strongly that the experimental beginning taken in the German psychology of thought has never been properly exploited for more realistic problems. Opportunities for experimental broadcasts will be easily given here.



PFL also agreed with Adorno that the 'dulling character of entertainment music is of much greater social importance than the direct propaganda which might be exercised over the radio'. This thesis was at odds with the views of 'liberal educators ... in this country' and would have to be further elaborated. However, the PRRP was subjected to two restrictions. First, it was limited to the 'American scene', which precluded comparisons with Europe; still, the European experience could be drawn upon for analysing the American situation. Second, the working conditions of musicians and similar issues were not part of the research project: 'I am enclosing the original statement of the project on the basis of which the money was granted. Although we have, as you know, much more precise ideas as to what we should do, the basic trends of this draft are still binding for us.'

In mid-February, Adorno crossed the Atlantic, and on 26 February 1938, he first set foot on the premises of the Research Center at Newark (Wiggershaus 1986: 268). What was going on there must have made a lasting impression on him, since even three decades later his memories were still so vivid that he used them as an introduction to his report on his activities in the PRRP:

At the time, the center of the Princeton Radio Project was neither at Princeton nor in New York but in Newark, New Jersey, and was set up in a rather improvised way in a disused brewery. When I went there by the tunnel beneath the Hudson, it felt a bit like Kafka's Nature Theatre of Oklahoma. I admittedly was rather attracted by this absence of prejudice in the choice of locality, which is hardly imaginable by European academic standards. My first impression of the ongoing studies, however, was not marked by much comprehension. On Lazarsfeld's suggestion, I went from room to room, talking to the staff, listening to words like 'Likes and Dislikes Study', 'Success or Failure of a Programme' and the like, which at the time meant next to nothing to me. Still, what I did gather is that this was about collecting data that were supposed to be of benefit for the planning agencies in the field of mass media, either directly in the industry or by way of cultural advisory boards and similar bodies. (Adorno 1981: 303–4)

What Adorno believed he understood was not quite what the PRRP was doing, since their goal was precisely not to let anyone directly benefit from their data. Nevertheless, on a half-day basis, Adorno started to direct the 'so-called Music Study' (Adorno 1981: 302) which in the first half-year, however, meant that he was at the same time the conductor and the orchestra. In a letter to Walter Benjamin written a few days after his first workday at the PRRP, Adorno even went one step further and claimed to have taken over the overall theoretical direction of the project (Adorno 1994: 313). There is nothing to suggest that Adorno was joking.

## A PROJECT SEEKS ITS OBJECT AND FACES ADVERSITY

In the spring of 1938, two of the four fields that the PRRP wanted to cover in its studies – 'music, book-reading, news, and politics' – had actually started

operating. Besides Adorno's work, there was a study directed by Cantril, described as the Commentator Study in the memorandum, which had been given priority over the Success and Failure Study by PFL. For this study, James Rorty had already been recruited as a collaborator at the end of 1937. Not unlike Adorno, he had been assigned the task of a month's collection of material followed by a presentation of his ideas on the subjects he had singled out for further research.<sup>31</sup> In May, 1938, PFL complained to his two co-directors that Rorty was still conducting 'interviews with commentators and other people on the production side' and failed to realize that 'we are running a listener project and that [h]is material is valuable as background material and as a lead to listener research but not as an independent unit.' He goes on to say: 'The Commentator Study is definitely a strategic unit. It really could show the interlocking of general information as Rorty collects it and of special experiments as they are now summarized in Had's [Cantril's] final outline. I did not feel that the two Rortys [Rorty's wife was also working for the project] are aware of the importance of those twelve special experiments.'<sup>32</sup>

In November 1938, PFL and Cantril agreed that a showdown with the Rorty couple was inevitable. PFL once more wrote a memorandum of several pages summing up his experience with both of them and comparing it to the Adorno case. For understanding the relationship between PFL and Adorno, the following lines are significant insofar as they were written after their first 'showdown':<sup>33</sup>

The best way for me to describe my feelings is to put them on a comparative basis between the Commentator and the Music Study ...

1) W[iesengrund] contributed a great number of original ideas and in many discussions with the Rockefeller people and with broadcasting specialists, I realized how stimulating some of his observations and theories are once they are translated into understandable language. R[orty], as far as I know, has hardly contributed any ideas to the whole study. The original plan was worked out from an incidental remark by Gallup and I had all the details formulated and explained to R at my apartment when I laid the whole plan before all of you – pretty much the same as it is now. It is true that I still have some difficulty in getting W down to earth but there can be no doubt of his originality and the fruitfulness of his approach. With R, I do not even know whether he has produced a new aspect although Had might correct me on this point.

2) W. took his assignment very earnestly from the beginning and he is certainly spending the greater part of his day on the project. R. might do some work now but he certainly was not honest about his time when I had the opportunity to observe him.

3) W. considers himself definitely a part of the project. He attends the seminar, keeps himself informed of the other studies, and is helpful wherever he can be. R. might have to be a lone wolf insofar as his intellectual production is concerned, but there is certainly not the slightest justification for his keeping himself absolutely isolated from the project in spite of my efforts to draw him in. I just do not believe that someone could work with us who does not participate, even to some small degree, in our own development and who does not catch some of the atmosphere in which we all work.<sup>34</sup>

On the same day on which this memorandum was sent to his co-directors, PFL also sent a letter to Marshall presenting the PRRP publications plan. He wrote that they were planning 'a series of monographs' and gave a detailed account of the first four titles: Stanton was to write a volume on 'Measurement Techniques', PFL himself would produce an article on 'The Art of Asking Why', James Rorty and Hadley Cantril would contribute a study on 'Radio Commentators', and the fourth volume would be 'Music on the Air', written by Adorno.<sup>35</sup> None of these monographs ever saw the light of day in the world of publications. The first project PFL had to write off was the Commentator Study because cooperation with Rorty was past saving even by Cantril (though he was not ultimately given notice until autumn 1939).<sup>36</sup> As a consequence, PFL must have all the more felt the need for the Music Study to be a success even if Adorno had not been the always helpful and interested collaborator PFL had made him out to be to his co-directors.

Rorty and Adorno were not the only ones to become a problem. After only half a year, a heated argument arose between Cantril and PFL about the latter's easy-going way with the budget. At the end of March 1938, faced with PFL's statement of expenses for the PRRP's second quarter and having by chance heard that a union had been formed by the project collaborators, Cantril's patience gave out. The result was an ultimatum to PFL, summoning him to present the following information at the next directors' meeting: 'If you could bring down not only your latest budget, but an outline of the persons working in Newark, the salaries they are receiving, their assignments, and the tenures they have been promised.'<sup>37</sup> Once this information was produced for all to see, he said, they could decide on a budget that was strictly binding for all of them and to which all three directors would commit themselves by signing a statement set up by Cantril. The argument went on for a certain time, but in the end Cantril failed to impose himself. Within just six months, the balance of power had clearly shifted in favour of PFL, who as a good tactician knew how to rally Stanton for his cause.

However, the differences did not only concern PFL's notorious easy-going way in budgetary matters as well as with promises made to collaborators, but also the choice of projects, the selection of collaborators and relations with the International Institute of Social Research where PFL was still due to deliver the study on the impact of unemployment on American families. Auxiliary work for this study was partly paid for by PRRP funds, and Cantril insisted that the money be claimed back from Horkheimer's institute. Given his delicate position with the institute – the study that he, that is, Mirra Komarowsky, was conducting for it was already two years overdue and would take another two years to be terminated – this was the last thing PFL wanted to do.

Another difference that kept turning up in the three directors' internal correspondence concerned the conductor of the Music Study. A few months after his arrival in the United States, Adorno had submitted a 160-page typescript which, it is true, drove PFL to despair but – contrary to the accounts given in Jay (Jay 1976: 227–8 and 265), Morrison (Morrison 1998; Morrison 1978a),

Wiggershaus (Wiggershaus 1986: 272–3) and Dahms (Dahms 1994: 247–53) – did not prompt him to terminate their cooperation. In a long letter PFL accused Adorno of using an unnecessarily difficult language, blithely ignoring empirical reality in favour of unproven ideas, indulging in polemics against an allegedly American method that actually was PFL's preference for introspection (and was thus strictly opposed to American psychology) and of ignoring the most elementary rules of argumentation at the height of his polemics. At the same time, he assured Adorno of his high esteem and desperately tried to make him see that unless he was bent on jeopardizing the continuation of the PRRP, his pamphlet could not be made public.<sup>38</sup>

Adorno remained a collaborator of the PRRP for two-and-a-half years, and the RF's refusal to further employ him only came when negotiations about transferring the project to Columbia University had started. This refusal was preceded by talks between PFL and Friedrich Pollock as well as Max Horkheimer, who both spoke in favour of Adorno. In the letters accompanying these talks, all parties sought to find a way of keeping Adorno in the music project.<sup>39</sup> PFL's later attempt to obtain separate RF support for Adorno failed due to the reservations expressed about him by various people inside and outside the RF as well as Adorno's own failure to come up with a final report after the two-and-a-half years working on the project. The reason ultimately given for the refusal was that Adorno's working plans did not fit into the programme structure of RF sponsoring measures. Even then, PFL tried to keep Adorno in the project but had to give up in the end.

Since both the Commentator Study and the Music Study were more or less stuck, PFL was reduced to hoping for success in the other sub-studies. One of these was the Success and Failure Study mentioned above, concerned with the relation between the intensity of radio listening and the reasons for discontinuation, on the one hand, and specific traits of radio listeners, on the other. Conducting this study was easier thanks to the comprehensive material that was made available for secondary analysis by the commercial radio stations. The procedures used, as described by PFL, sharply contrasted with conventional textbook wisdom:

I think that we should go out quite naively first to get any available information. In this way, we will see what all the factors are which enter into the situation. Then we can work out a sort of list containing all the accounting relationships which should be considered in all cases so that our results may become somewhat comparable. There is little to say about that at this moment, however, because all the methodological problems will come up only as we proceed.<sup>40</sup>

As a matter of fact, these reanalyses and supplementary surveys were the part of the PRRP that advanced most rapidly. They served as a basis for the first articles that can be attributed to the PRRP. PFL's enthusiastic comment, which suggests that some results of the primary surveys 'are about as important to social psychology as the first learning curves were in experimental psychology',<sup>41</sup>

must, however, be attributed to the near-desperate situation of the PRRP at the end of its first phase. To compensate, the director emphatically took to whistling in the dark.

The first major publication of PRRP findings was an omnibus volume of short articles in the February 1939 issue of the *Journal of Applied Psychology*, which was devoted wholly to radio research. Besides the introductory remarks signed by PFL as a guest editor, there were so many articles he had written himself that he fell back on a trick to cover up for it: he published two under the pseudonym Elias Smith. Topics that should have been dealt with in the monographs he had so often promised were presented in a highly condensed fashion in the articles of this issue, for instance an eight-page article by Stanton modestly entitled 'Notes on the Validity of Mail Questionnaire Returns', or one of PFL's own entitled 'The Change of Opinion During a Political Discussion' (Lazarsfeld 1939b). Actually, however, this first issue devoted to radio research – there would be another one in the following year – mainly presented reflections on the problem of indices. As a topic, this had been announced in neither the old nor the new 'testaments', but it clearly reflects what PFL had frankly admitted in his very first letter to Cantril after having been offered the position as director: 'Radio is a topic around which actually any kind of research methods can be tried out and can be applied to satisfactorily.'<sup>42</sup> One-and-a-half years later, PFL might have further extended this sentence by adding that the radio also was a topic around which new survey techniques and, above all, new techniques for analysis could be elaborated. Shorter still, radio research allowed for serendipity – in this case, the finding of new methods one had not looked for or even been charged to look for.

One event that comes close to the serendipity pattern (Merton and Barber 2004) opened up an unanticipated opportunity for the PRRP to conduct a special study. On Halloween of 1938, several radio stations broadcast Orson Welles's adaptation of H.G. Wells's *War of the Worlds* from New York's Mercury Theater. Thanks to Welles's impressive stage technique and the use of fictitious radio news, about a million listeners responded to the broadcast of Wells's Martian invasion with panic. On the very evening this happened, PFL and Herta Herzog decided to do an in-depth investigation of this phenomenon and wrote a detailed project proposal. PFL contacted Stanton the next morning to ask him for money from the CBS research fund, and then both of them approached the RF General Education Board for additional funding. Three decades later, PFL observed with resignation that today this kind of 'improvising is made so much more difficult by the bureaucratization to which the granting of funds for research is subjected' (Lazarsfeld 1969: 313). In 1938, this blitz project came to be realized.

Herta Herzog rapidly conducted a large number of exploratory interviews with listeners. Many of the initial ideas could not be realized, but in the end, the project team had gathered a total of 135 in-depth interviews. They also drew on survey data from Gallup's American Institute of Public Opinion (a representative national survey conducted six weeks after the broadcast),

presented an analysis of more than 10,000 newspaper reports, and conducted further surveys (with school principals, radio stations, telephone companies, etc.) and statistical analyses to assess the scope and intensity of the panic.

Cantril then took on the task of distilling a book from this material, presumably moved, among other things, by the prospect of being able to pass it off as a substitute for the failed Commentator Study. The published version essentially consists of a qualitative analysis of the in-depth interviews. While Cantril was still busy compiling the book, a heated argument arose between him and PFL, the latter accusing him of monopolizing the authorship at Herta Herzog's expense.<sup>43</sup> In the preceding weeks and months PFL seems to have harboured an ever-growing grudge against Cantril that now exploded on the first occasion that presented itself. Cantril retaliated with all the harshness of the upper-class American: he accused PFL of being infantile and proposed that director's uniforms with badges of rank be introduced in the PRRP to make sure that each and everyone was aware of PFL's outstanding role. Cantril once again stressed that he would rather do his own work instead of having a part defined for him by others in the radio project.<sup>44</sup> Even though relations between both men were beyond patching, Cantril did not abandon his unloved role as a co-director who was outshone by the others until he could finally withdraw without doing harm to anyone.

The second issue of the *Journal of Applied Psychology* devoted to radio research did not appear until December 1940 because the editors were unwilling to dedicate two consecutive issues to the same topic (Lazarsfeld 1969: 326, n. 73). The number of articles dealing with the radio and its audience had actually increased, but the articles on technical and methodological issues still outnumbered those dealing with other aspects of the new medium. One of the mediating articles presented the reader with PFL's first invention: 'The "Program Analyzer": A New Technique in Studying Liked and Disliked Items in Radio Programs.' The Stanton-Lazarsfeld Program Analyzer, as it became known in the history of communications research, provided a device for an experimental design where up to ten subjects listening to music could push a button each time they did not like what they heard. The device recorded each subject's 'dislikes' on a moving paper tape. Both of its creators were inordinately proud of their device, and this pride was shared by all those who were socialized in the atmosphere of the PRRP and its successor organizations, the Office of Radio Research and the Bureau of Applied Social Research. Those less involved would probably consider it a rather curious kind of toy and tend to agree with Adorno: 'A little machine, the so-called program analyzer, which allowed one to signal what one liked or disliked etc. by pushing a button while a piece of music was presented appeared to me, in spite of the apparent objectivity of the data it provided, to be sadly inadequate given the complexity of what was to be explored' (Adorno 1981: 306).

'Little Annie', as insiders called the machine (Rogers 1994: 275-7), was later primarily employed as a source for clues to be used in the in-depth interviews

that were conducted after the presentation. In this form, it helped to elaborate the technique of the focused interview (Merton 1987b: Merton *et al.* 1990); in its original form, it was long used in the advertising business.

## EVALUATION AND ITS CONSEQUENCES

In the spring of 1939, after eighteen months of work, extensive negotiations began for the continuation of the Princeton Radio Research Project beyond the initial two-year period. By that time, the project was no longer located in Newark since that local university had gone bankrupt in the autumn of 1938. The PRRP had moved, without having to change its name, to a new location on Union Square in Manhattan, where during the next months it operated under the name of 'Princeton University, Office of Radio Research, Field Headquarters, 22 East 17 Street, New York'.

The RF had set up a reviewer committee of nine members charged with evaluating the PRRP's output. Members of the committee included representatives of the two big radio broadcasters, the National Broadcasting Company (NBC) and Columbia Broadcasting Company (CBS), social scientists such as Robert Lynd and Harold D. Lasswell, and other representatives of universities and educational institutions. Three decades later, PFL described the atmosphere of these months as follows: 'The outside impression one got of the Research Center [left] something to be desired. One could not identify a central topic, and rumors were beginning to reach us that key people started to ask themselves if we really knew what we were up to' (Lazarsfeld 1969: 317).

All in all, evaluation by the reviewer committee was positive, and the RF decided on a continuation of another six months and a further funding of \$17,500 (\$267,000 in 2010).<sup>45</sup> These six months, as seen by Marshall, were to be dedicated to the elaboration of a programme for a subsequent three-year project phase. There were, however, two conditions bound up with the additional half-year and the promise of further continuation. The first was that PFL had to present an overview of the results obtained so far; the second was that continuation would, then, only be approved if no new projects were initiated. Instead, the data collected thus far were to be analysed in more detail. Stressing the urgency for PRRP activities to get more focused, Marshall informed PFL by telegram of the demand for an interim report:

Discussions in office indicate reluctance to invest in new research pending formulation of present findings Stop feeling here that need is for breathing spell to save project from being victim of its own success Stop result decision to make no recommendation to trustees now Stop ready to review situation in June if formulation is sufficiently advanced by then to provide basis Stop Marshall<sup>46</sup>

By their criticism, the members of the reviewer committee and the RF Humanities Division, who were indeed favourably inclined towards PFL, had hit on the PRRP's sore spot. Overjoyed at having so much money at his disposal, PFL – besides using part of the new funds to complete other projects

that were overdue – had initiated so many sub-projects and started so many trial balloons that the coherence of the whole enterprise was somewhat lost to view. This constellation – foundation officers interested in the issue, favourable external reviewers and a project director brimming with ideas – is also interesting for evaluating the development of empirical social research from a sociology of knowledge perspective, since it illustrates the felicitous interaction of forces that prevent free academic research from dissociating itself from reality, which is in turn likely to result in a loss of purpose and a failure to reach its goals. In the case of PFL, it brings to mind the influence exercised by Otto Bauer on the young social psychologist in Vienna in the early 1930s, which prevented him from engaging in a study on leisure time behaviour and directed his attention to the problem of unemployment. Free research, conceived of as an opportunity for scholars to pursue their ideas, fancies and inclinations to the last ramifications of an issue, is only too likely to end up in idiosyncratic constellations of incessant activity while even the most discerning outsider is at a loss to identify its output.

In the spring of 1939, PFL was facing two problems: he had to find some future institutional affiliation for the PRRP, and he had to put himself to the task of bringing its previous work into a presentable form. With respect to the former, support by Lynd enabled him to manage a smooth transfer of the Office of Radio Research to Columbia University after two-and-a-half years of merely formal affiliation with Princeton. The field headquarters on Union Square were closed down and transferred to a Columbia building on Amsterdam Avenue. Yet this would not have been possible if PFL, in the spring of 1939, had not focused all his efforts on the requested report on the work done so far for the radio project.

The typescript submitted by PFL at the beginning of June 1939 was in the following year published by a commercial publisher;<sup>47</sup> the title then was *Radio and the Printed Page: An Introduction to the Study of Radio and Its Role in the Communication of Ideas*. PFL had worked virtually night and day for weeks on end to complete it. The only other authors to contribute reports of their own were Herta Herzog and Samuel Stouffer, whom PFL had roped in for support from Chicago.

The book largely consists of a presentation of the results of reanalyses that had been done for all the studies that the PRRP had collected during its first months of operation.<sup>48</sup> For these reanalyses, the socio-cultural level of the various study participants was consistently used as the independent variable. In order to differentiate between cultural levels, PFL relied on a discovery he had published in the first issue of the *Journal of Applied Psychology* devoted to radio research that he called ‘the rule of the interchangeability of indices’ (Lazarsfeld 1939a). Differences in literacy, he argued, were strongly correlated with socio-cultural position. Since socio-cultural position was a multidimensional phenomenon, different cultural strata could be delimited by drawing on data that were more easily accessible, such as income, housing quality and housing district.<sup>49</sup>



It is not hard to see the problems bound up with this procedure: as long as no clearly defined indicators are used, the validity of such intuitive classifications is impaired by prejudices, stereotyped and erroneous perceptions, etc., in short, by the whole bunch of what has meanwhile become known as the bias effects of social classifications. In PFL's case, this bias arguably was rather small since he was content, or had to be content, with a very rough division of strata. Occasionally, listeners were just dichotomized into those with a (somewhat) higher and those with a (somewhat) lower educational level.<sup>50</sup> The four-level classification of social strata was adopted from market research where the first four letters of the alphabet were used to identify four strata, from A for the highest to D for the lowest level. In PFL's analyses, the latter level was rare because the surveys had been conducted by telephone – there were not many telephones in this stratum so it could only be incompletely covered (cf. Beville 1985). These 'ratings' from information on what subjects listened to and when (or, more precisely, on when their radio was turned on) were consistently used as the dependent variable in PFL's analyses. Age and sex were introduced as the intervening variable whenever this was feasible. No factor analysis, as announced in one of PFL's memos, had been realized, nor had any of the ideas that had filled the PRRP papers in the first months: listener biographies, psychoanalytically informed in-depth interviews, experiments under hypnosis, etc.

Still, the results are not completely trivial. Of course, it is difficult to say, after more than sixty years, which of the findings reported is most likely to have impressed the readers at the time or which of them had at least been felt to be new, but some results and interpretations are still rather interesting in a historical perspective, at least when they are considered in terms of social history and the history of mentalities. The result most likely to interest prospective readers was that the members of the higher strata listened to the radio less frequently but preferred the more serious programmes. In contrast, members of the lower social strata were more interested in light entertainment and spent more overall hours close to a turned-on radio. The programme type that was most popular with audiences of a lower cultural stratum was the quiz show, in which ordinary people had to answer questions.

The most impressive part of the book is thus Herta Herzog's report on a case study conducted with a small number of listeners to a programme called 'Professor Quiz'. This programme was less popular among members of the top socio-economic group but ranked significantly higher among those of the B stratum than among those of the next lower stratum. The programme offered listeners various gratifications: competition, instruction, self-assessment and sportsmanship. (One may remember that at the very beginning of the PRRP, PFL had wanted to consult some psychoanalysts about the gratifications that people were likely to get from radio listening.) As a listener, one could compete with the contestants in the quiz; by trying to answer the questions oneself, one learned something about the knowledge one actually possessed; and even if one was more or less passive, one could get a lot of casual fun from just

listening to a contestant trying to find the correct answer. Herzog brings to light a number of instructive details concerning the 'love triangle' (Lazarsfeld 1940: 66) of listeners, candidates and questions. The secret opponent of all listeners was the group of those with a higher education level to whom they could demonstrate, in this competitive setting, that they were not as dumb as the latter tended to make them believe. If the contestant's education level was too high, virtual competition with them ceased to be fun. Sitting at home, however, one could just skip some 'irrelevant' questions, which was an easy way to improve one's hit ratio, as Herzog dryly notes. If, on the contrary, one could find the correct answers, one could be proud of oneself and of one's knowledge. Finally, listening was a means of gaining knowledge that could come in useful next time one met someone who was talking about things learned in the quiz. Herzog's small and simple study plainly shows that measuring programme preferences was not the only way to gain insight into the effects of radio listening; in-depth interviews with listeners on how they actually got involved with what they heard resulted in interpretations that were much more instructive as well as more generalizable.<sup>51</sup>

Stouffer's contribution, filling the void left by the failed Commentator Study, provides a very detailed analysis of a large representative survey that had been co-designed by the PRRP and conducted by Gallup's American Institute of Public Opinion. This survey was about the reading of and listening to political news or commentaries. While listening ratios to radio news were approximately the same in all strata, only the more well-to-do also regularly read newspapers. Stouffer produced a finely honed statistical analysis, though the results are hardly interesting today (in 1940, some readers may have been impressed by the statistical competence of its author, and others by the wealth of individual findings).

PFL concluded with some reflections on future developments. The impact of the radio would be visible in its influence on people's attitudes and behaviours. These, however, must not be seen independently of the more general trends in social development:

Radio can, for better or for worse, facilitate many tendencies toward centralization, standardization, and mass formation which seem prevalent now in our society. But of the many alternative developments that can be visualized now, very few will occur 'by a tip of the balance'. They will be the result of powerful social forces which during coming decades will affect radio broadcasting much more than they will be influenced by it ... We can be fairly sure only that radio will not itself mold the future. (Lazarsfeld 1940: 331-3)

In a different perspective, the prospect thus outlined by PFL was to become, as is well known, the trademark of the Lazarsfeld School, i.e. the so-called 'Columbia tradition' of communications research: the social 'effects of radio' were not as important as the conservatives and the critical theorists feared. In this, Lazarsfeld was anticipating findings that would only later emerge from

further research – and one may well ask to which degree these later findings were influenced by the researcher's prior assumptions.

The research design of most of the sub-studies was of the early positivist type: there was no systematic elaboration of the problem, let alone any prior formulation of the hypotheses to be tested. The frame of reference for the presentation of results was provided by a largely uncritical adoption of the perspective of the group of readers that was primarily targeted. In some cases, the findings were at odds with the prior assumptions of the 'educator', which arguably was decisive for the resonance of the book, but the key idea of the well-meaning representatives of educational institutions was only rudimentarily challenged in the concluding remarks quoted above. In this respect, however, *Radio and the Printed Page* is not an example of 'administrative research' discussed below (a derogatory label created by Adorno and later adopted by Lazarsfeld in order to turn it, with little success, against its author). The most adequate response to the book is no doubt to regard it as a report on the first explorations in the domain of radio use, disproving some of the more popular assumptions on the impact of radio listening. As for the alleged cumulation of the variant of mass communications research that would later be associated with PFL's name, in which critics refused to see more than an effort at measuring short-term 'effects', while there are certainly some signs of it, more prevalent here is a pre-exploration done by simple surveys and rudimentary statistical analyses.

### ADORNO'S TEMPORARY CONVERSION TO CRITICAL EMPIRICISM

Given that the interim report had been submitted in due time and had been favourably evaluated by an external reviewer charged by John Marshall, the latter could have started to approach the RF boards on the issue of how to continue the radio project in a different institutional setting. However, world politics did not play along as the outbreak of war in Europe brought the well-practised RF routines to a temporary standstill.

This did not keep PFL from maintaining his relationships with the foundation officers. Additional guests were invited to attend one of the regular PRRP 'staff seminars' on 26 October 1939. The referent of the day was Adorno, who was scheduled to talk about the 'principles by which his research has been guided'.<sup>52</sup> There is no report on who exactly attended this seminar, but John Marshall certainly was there, and other visitors no doubt included a good number of those regularly attending the monthly meetings initiated by Marshall in September 1939 to discuss the direction of mass communications research.<sup>53</sup> Inviting them to Adorno's presentation was thus a kind of counter-initiative with the aim of giving the established radio researchers some idea of the divergent position of a European theorist who had made himself conspicuous in previous months by repeatedly demonstrating his social incompetence.

Adorno's presentation at the seminary is available as a manuscript, 'On a Social Critique of Radio Music'.<sup>54</sup> It was written with the assistance of George Simpson, who had been assigned to Adorno when an attempt at cooperation with another staff member had ended in disaster. Adorno's junior by five years, Simpson had translated Émile Durkheim (*La division du travail* and *Le Suicide*) and, having graduated from Columbia University, had worked as an instructor at Queens College since 1940. Notwithstanding this rather obvious status difference, Adorno, a beginner in sociology, felt the need to belittle his co-author as an 'adjoined assistant' in his memoir (Adorno 1981: 313), generously crediting him with 'rather a theoretical frame of mind'.

In his presentation, Adorno pursued a double purpose: he wanted to explain what social critique of the radio with respect to music might be, and he wanted to show the methodological implications of this perspective. The conventional approach, as seen by Adorno, was to study the radio by registering its impact on the audience, exposing certain groups of subjects to a specific stimulus, observing their reaction to it and subsequently selecting the settings that had produced the desired effect. The whole procedure was based on an 'administrative technique': how can a more or less 'central agency' successfully influence given groups with the aim of making them behave as this central agency wanted them to behave? 'The logical form of such investigations is moulded [!] according to the ideal of a skilled manipulation of masses.' In this, the patterns followed were those of market research, even though nothing was sold. This 'administrative research', as Adorno called it, came in two variants: an 'exploitive' and a 'benevolent' one.

Adorno describes his own approach as the exact opposite of the exploitive variant of 'administrative research' and as a complement to the benevolent variant. The question of how good music could be made more accessible to a larger audience could be studied using both forms of 'administrative research'. In contrast, Adorno goes on to write, his own concern was with the question of what good music was, in the first place. Even the best music, such as for instance music by Beethoven, had degenerated, through constant reiteration, from a vital force to a museum piece. Even if Beethoven might not yet be worn down in a young country, musically speaking, such as the United States, it still was far from certain whether or not broadcasting this music had a detrimental effect on its quality.

Adorno's concern was not with the individual effects of the radio on its audience but 'to discover the social position and the social function of radio and particularly of radio music'. To this end, one had to start out from four 'axioms': first, people lived in a society of commodities; second, in this society of commodities, there was a general trend towards a concentration of capital, constraining the free market and leading to an ever increasing standardization of goods; third, the more strongly contemporary society struggled to continue in its present form, the more imminent the prospect of ideological levelling

would become; fourth, contemporary society was increasingly characterized by antagonisms.

In the second part of his presentation, Adorno illustrates the method underlying the critical approach in music research by describing a study he had conducted himself: a very detailed study of listeners' mail sent to a rural radio station dedicated to the promotion of 'serious music'.<sup>55</sup> This mail had consistently taken on the form of 'standardized enthusiasm', and the reason for this was that, 'the listeners were strongly under the spell of the announcer as the personified voice of radio as a social institution and his call to prove one's cultural level and education by appreciating this good music'.

Adorno goes on to argue that while he was well aware of the dangers of arbitrariness and prejudice, he believed that they could be avoided by what he called 'musico-technical control of sociological interpretation'. Music was a rigorous discipline that did not allow for arbitrary turns but made clear statements on what was right or wrong, true or false. Thus, if correctly employed, a musico-technical interpretation could easily show that distinct light popular music varieties of the time, such as 'sweet' and 'swing', were essentially the same.

Adorno concluded his presentation by pointing out the relationship between a social critique of music and empirical research. In an almost conciliatory vein, he argued that while his approach and that of 'administrative research' were indeed antagonistic, they proved to be mutually supplementary whenever they could directly deal with the material. His own contribution to listener research could consist in the elaboration of a provisional typology of music listening (a continuum extending from the 'expert listener – the musically trained and fully conscious listener' to the 'musically ignorant' and the 'music hater'). The interviews that had so far been conducted showed that the extreme types were rarely found. Both the theoretical and the empirical typology were compatible with 'administrative research'. Types in terms of a psychology of music were sociologically not invariant. This had nothing to do with the fact that the more affluent preferred more serious music or that rural listeners were more inclined towards 'old-fashioned popular music'.

With respect to mass audiences, it had been shown that there were mainly two types: the emotional listener who could also be characterized, in musical concepts, as the 'harmonic-melodic' listener, and the rhythmic type. The former was similar to the 'moving-picture spectator'. Music was experienced as 'release', and the happiness music induced was due only to its power to allow the listener to be momentarily *unhappy*: 'Music for them, is a remedy against the "keep smiling" attitude.' The rhythmic type was also adapted, in relation to the monopolistic mode of production, to given social conditions: examples were Nazi stormtroopers on the march, or people who admired authoritarian discipline in sports.

Adorno's presentation of 'social critique' reveals that he was willing to modify his position in light of the criticism he faced. At the same time, the text highlights the cornerstones he was by no means willing to give up. One of

these was his conception of a theory of music, which is nothing short of scientific. What is disconcerting about it is not so much his use of the term of 'musico-technique', although given the Frankfurt theorists' relentless insistence on the importance of word usage as part of their criticism of ideologies and their more or less endemic phobia of technology, one might well ask why 'musico-technique', of all things, should be an exception. But it is the content of Adorno's remarks that strikes one as quite antagonistic to the conception of science he subscribes to in his other writings. Since he seems to conceive of 'musico-technique' as a set of general rules for the correct way of producing musical effects, one may well ask why such general regularities should exist in the field of artistic skills while they are definitely ruled out for other domains, particularly for the social universe. Second, one might ask how people should come to know these production rules: why is the production of music, which presupposes highly sophisticated intellectual competencies, something that can apparently be learned, which in turn presupposes that knowledge of the underlying rules is possible, while knowledge of any such rules in the field of social regularities is not?

This musical scientism is all too obviously part and parcel of the normative stance of the well-educated bourgeois who knows exactly what is good and what is bad in cultural matters. Undaunted by any relativism whatsoever, the admirer of the modern conception of music delivers judgements on those who incurred his displeasure, such as Toscanini, Tchaikovsky or Sibelius, among others, and passes off these judgements of value as objective. However, since the whole PRRP was driven by an unquestioned preference for serious programmes serving an educational purpose, it would be unfair to accuse only Adorno of normativism. Rather, it could be argued that Adorno was subject to a normative fallacy, while what PFL stood for could be described as an empirical fallacy. The former is sure of the real facts prior to any empirical research, the latter tends to believe that only what can be empirically found actually exists. PFL was at least conscious of this, as when, with a maximum of goodwill, he summed up Adorno's position as follows:

Omitting a number of details and specifications, the 'operation' basic in this approach consists of four steps:

- (a) A theory about the prevailing trends toward a 'promotional culture' is introduced on the basis of general observations ...
- (b) A special study of any phenomenon consists in determining how it expresses these prevailing trends (introduced in (a)) and in turn contributes to reinforcing them.
- (c) The consequences of (b) in stamping human personalities in a modern, industrial society are brought to the foreground and scrutinized from the viewpoint of more or less explicit ideas of what endangers and what preserves the dignity, freedom and cultural values of human beings.
- (d) Remedial possibilities, if any, are considered. (Lazarsfeld 1941: 12–13)

As for defending the opposite view, for which the label of 'administrative' is accepted without protest, PFL confines himself to saying that 'fact-finding'

is indispensable, and to refuting its alleged 'futility' by a figure of thought that he would use again a few years later in an extended review of the *American Soldier* (cf. Lazarsfeld 1949):

One might ... tell such an opponent [e.g. of empirical social research] that according to studies which have been done people who make up their minds during a political campaign as to how to vote are influenced by very different factors than those who have more permanent political affiliations. The opponent will find that immediately understandable and will say that he could have come to this conclusion by using good common sense. It so happens that the opposite is true and that it is possible to predict to a high degree the vote of originally undecided people by means of the same characteristics which describe people with actual party affiliations. There are many other examples by which common sense first can be led to conclusions which then are proved by actual data to be incorrect. (Lazarsfeld 1941: 8, n. 1)

Although PFL does not explicitly say so in his text, it is clear that for him the common-sense point of view of the opponents of empirical social research is the one subsumed under point (a) of his summary of the critical approach. Nevertheless, he insists that critical analysis must be an integral part of the everyday workings of social research. Yet one cannot help feeling that he chose the wrong place for his message, since the task here would rather seem to be to convince the readers – not to speak of the makers – of the *Studies in Philosophy and Social Science* (as the *Zeitschrift für Sozialforschung* was now called) that empirical social research, vilified as 'administrative research', was quite justified in laying claim to a place in the universe of critical theory:

It might very well be, however, that we are all so busy finding our place in society according to established standards of success that nothing is more important at this moment than to remind ourselves of basic cultural values which are violated, just as it was of decisive historic importance a hundred years ago to remind the English middle classes that they were overlooking the sacrifices which the new strata of industrial laborers underwent when the modern industrial world was built.

There is a touch of heroic futility in this attempt to submit this utterly social democratic credo to the Frankfurt parlour Bolsheviks.

But let us get back to Adorno's 'social critique'. Four aspects should be noted. First, Adorno was willing, at least in principle, to submit his 'theory' to the empirical test. Second, he tried to advance empirical work by proposing a draft typology, even though it is unclear how he constructed his types or where he got the material he relied upon. Adorno seems to have taken no account of PFL's text on the issue that was published in the critical theorists' house magazine, since in a letter to Walter Benjamin he described it as something one could safely ignore.<sup>56</sup> Third, Adorno's typescript contains remarks on the social function of the radio, which are described, in a somewhat unclear use of the concept, as axioms.<sup>57</sup> Adorno's axioms of commodification,

standardization, ideologicalization and social antagonism were indeed reconcilable with the terminology of social functions.

The most momentous aspect of Adorno's 'social critique', however, was the fourth – his creation of the term 'administrative research'. While in his presentation, he still differentiated between its exploitative and its benevolent aspects in order to prevent his opposition to what PFL stood for from becoming too obvious, he – and, with a vengeance, his followers – later dispensed with this differentiation by disposing of all that did not conform to the commandments of critical theory in the garden dump of 'administrative research':

Lazarsfeld later explained ... the difference between administrative research and critical social research as pursued by our Institute. In the context of the Princeton Project, however, there was little room for critical social research. Its charter, laid down by the Rockefeller Foundation, explicitly stipulated that the studies be conducted within the context of the established US commercial radio system. This included that this system itself must not be studied, but only its consequences in terms of a sociology of education. I cannot say that I strictly abided by this charter. (Adorno 1981: 304)

To represent so many things in so distorted a way in less than ten lines is a rare feat indeed. PFL more than once made an attempt to reconcile critical and empirical social research, but to no avail. Citing all the cases in which Adorno's polemically distorted description became an arcanum would be nothing but tiresome (one that is representative of all the others is Bonß 1982: 194–201).

## WORTH READING, NOT WORTH SPONSORING

Adorno's presentation in October 1938 resulted in a series of intensive efforts to maintain the Music Study as part of the continuation of the PRRP. A few days after the presentation, PFL sent copies of Adorno's typescript to the RF where it was circulated as usual. The head of the Humanities Division, David H. Stevens, jotted 'well worth reading' on its title page. In December, the following note is found in PFL's 'Proposal for Continuation of Radio Research Project 2', as part of the 'Summary of Project 2': 'Completion and preparation of a book on the Music Project: By Dr. Adorno, assisted by Dr. George Simpson'. The duration of this project is given as 'March 1, 1940 to October 1, 1940'.<sup>58</sup>

In order to secure continuation for Adorno's work, PFL sent a three-page, single-spaced letter to Marshall on 27 December 1939, attaching an exposé of a 'Study of Likes and Dislikes in Light Popular Music' (apparently the study was published in 1941 as 'On Popular Music', see Table 5.1) and another two essays by Adorno: a revised version of 'social critique', from which the above quotations were taken, and an empirical study of the 'Damrosch Music Appreciation Hour'. This radio programme, named after its creator Walter Damrosch and produced and broadcast from Manhattan, sought to give listeners an understanding of serious contemporary music, and Adorno had analysed the printed 'booklets' that were used to advertise the programme. In the



second part, Adorno had interpreted the social implications of the programme. The programme was, PFL goes on to write, 'an example of what Dr. Adorno calls a social critique, and although it will probably be of great interest to radio educators, its position in his whole work is to exemplify principles and concepts. I suppose you will agree with me that such an analysis of an actual broadcast is a good bridge between mere theory and empirical research.'<sup>59</sup>

In order to fully convince Marshall and all those in the RF or among its advisors who might harbour doubts about the importance of continuing Adorno's work in the Radio Research Project, PFL informed him that two further studies were nearing completion, and gave a detailed description of their content. A manuscript entitled 'The Radio Voice' corresponded to that part of the presentation where Adorno discussed the question of whether good music broadcasted on the radio would remain what it was before the broadcast. To answer this question as precisely as possible, Adorno had chosen music of quality beyond doubt: Beethoven's symphonies. In the following, PFL wholly adopts Adorno's description, including those parts that he could hardly have been convinced were valid: 'The result is that a symphony transmitted over the air is in danger of becoming a piece of entertainment. Some basic categories of the social critique – particularly those of atomistic listening, of musical fetishism (quotation listening) and of the objective conditions for a general retrogression of listening – are *deduced* from this technical analysis.'<sup>60</sup>

The third study, PFL wrote, which Adorno had written on PFL's request, showed that the previous 'communication studies' had failed to sufficiently conceptualize the problem. All in all, using his manuscript on 'social critique' as a basis, Adorno was planning six essays where empirical material would be used.

A typology of music listening existed as a draft, and a certain number of experiments dealing with the 'likes and dislikes in popular music, and atomistic listening' had already been conducted by the PRRP. A study on the structure of the material of light popular music had been launched. According to Adorno, the 'vested interests' of the four big music producers and the three radio networks was conducive to standardization. Their gigantic advertising efforts resulted in the eviction of all those rivals who were not integrated in this network, and in the factual elimination of the market. The obvious hypothesis was that listener reactions, too, were confined to this frozen pattern, but tendencies to the contrary should be studied in more detail. Since a large part of the popular music that was broadcasted consisted of jazz arrangements, a detailed socio-psychological theory of jazz was indispensable. In a second part, the manuscript was going to study the automatism of 'plugging'.<sup>61</sup> In a step-by-step investigation, the trajectory of a hit would be reconstructed from the moment it was being formed in the composer's mind until it reached fame and, finally, became 'obsolete and dead music'. The theoretical thesis concerning the monopolistic and manipulative character of today's 'mass fads' would be grounded in detailed empirical material. The third part would be dedicated to the psychology of listener behaviour. Listeners' 'likes and dislikes' were not only socially preconditioned but, rather, 'socially "substituted"' – free choice

of what one liked to hear had been superseded by institutionalized and monopolistic 'tunes'. A theory of 'substitutional listening' would be elaborated, addressing the silent substitution of divergences in musical taste by schematic patterns. It would be shown that what had been demonstrated for the narrow field of music also applied to all the other branches of the entertainment industry. There were a number of 'analytic studies of current American literature on music' that would be dealt with in a separate study, including essays by 'some practical musicians'. Also, a comprehensive report would provide a résumé of all the existing material on musical programmes, and finally one of Adorno's essays would 'contain detailed, positive suggestions for program-policy in the field of radio music'.

PFL was perfectly aware, he wrote, that the approach proposed by Adorno was unorthodox and daring, but the more the latter's work was advancing, the more optimistic PFL felt. And, apparently feeling that all this was still not enough, PFL concluded by referring to 'moral responsibility' as another reason for the Music Study 'not to be dropped now'. He felt that a device of such major importance as the radio should not be studied without situating it in the frame of reference of our entire culture. In doing so, one was sure to obtain rather controversial results, which was the main reason for PFL to recommend grounding them in a field that was least subject to public distrust. Music, luckily, was such a field.

The whole letter echoes the concern of a project director seeking to convince the sponsor and his advisors of the palatability of a sub-project that was dear to him: 'Besides the great intellectual expectation I attach to Dr. Adorno's work, I should feel that the Project had failed its major task if nowhere in its work were a social critique attempted.'

PFL's letter was not the only statement on Adorno and the Music Study that John Marshall would dismiss. Anthropologist Geoffrey Gorer and Charles Siepmann (who became a leading proponent of responsibility in broadcasting), notwithstanding a number of critical points, pronounced themselves in favour of the continuation of the Music Study, whereas the representatives of the radio stations and the world of music more or less clearly opposed granting further support to Adorno.<sup>62</sup> Ultimately, Marshall decided against the Music Study and further financial support for Adorno in the context of the Radio Research Project, soon to be accommodated at Columbia. In a long memo, he noted the reasons for his decision. He did not doubt Adorno's originality but felt that unless he was made to cooperate with a competent co-author he could not be trusted to commit his ideas to paper. He doubted that Adorno's exposition was of any use since he 'seems psychologically engaged ... by his ability to recognize deficiencies in the broadcasting of music to an extent that makes questionable his own drive to find ways of remedying them'. Marshall informed PFL orally of his decision and the reasons for it and asked him to inform Adorno in the way he thought best. PFL pronounced himself against the decision to discontinue the project and suggested giving a personal 'grant-in-aid' to Adorno to enable him to complete his work outside the Radio Research Project. Marshall

refused to comply with this as well, pointing out that by now Adorno had a full-time job at the International Institute of Social Research anyway, and that a break in which he could think things over could only do him good.

Adorno's manuscripts were committed to the RF archives to keep them 'available for possible future development'.<sup>63</sup> Marshall did not even unearth them when, in March 1940, he got PFL's jubilant message:

You will be interested, I am sure, in hearing that on the basis of Dr. Adorno's analysis of the Damrosch Hour, WNYC has offered him the opportunity to give a Music Appreciation Hour of his own every Sunday from 1:00 to 1:30. He will start in April. I suppose that this is the kind of direct application of our work you are looking for.<sup>64</sup>

Adorno's papers indeed include two texts from 1940 entitled 'Zum Rundfunkkonzert' ('On Radio Concerts'), which remain unpublished according to the current collections of his works. Adorno seems to have overcome his aversion to musical announcers and commended, as was to be expected, Schönberg, Zemlinsky, Eisler, Krenek, Berg and Mahler to his audience. The *Hörstreifen* (white noise) problems played no part in the musical announcement, nor did any other of his musico-technological subtleties (Adorno 1984). Further indications of Adorno's activities as a radio announcer are found in his collected letters to his parents (Adorno 2003b), in which he repeatedly informed them about his study of the Damrosch Hour as well as of his own appearances. In December 1939, he still confined himself to telling his parents that he had, 'completed a major critical study on the musico-pedagogical hour presented by Walter Damrosch for NBC, in English, for the Radio Project, complementing each point of my criticism of this execrable enterprise by positive propositions that show how one should go about acquainting children and laypersons with music' (Adorno 2003b: 49). On 20 February 1940, he wrote: 'The concert, then, will be on Thursday, 1 p.m., on WNYC, wave length 801 kilocycles. But be sure to start listening 10 minutes earlier, we may start before that' (Adorno 2003b: 68). A few days later, he proudly wrote: 'New York City Radio has charged me with their musical education program ... Every Sunday noon, with half an hour entirely at my disposal, the best time there is, with an audience of literally hundreds of thousands of young people.'<sup>65</sup> The letters to his parents prove beyond doubt that Adorno was very much interested in being a musical educator on the radio, and was proud to report it. The sour grapes suddenly, and temporarily, had turned sweet.

On reading the RF documents one gets the impression that something else, besides what was committed to paper, must have weighed in on Marshall's decision. While none of the other RF documents I studied impressed me with anything like 'vested interests', these seem quite palpable here. The mystery was solved some time ago by David Morrison who, in the course of the research he did for his doctoral thesis on PFL, had conducted an interview with John Marshall himself in 1973. Here, Marshall conceded that he had suffered

a personal insult from Adorno. Adorno had remarked, in the presence of third parties, that the RF Radio Research Project was 'at the whim of a young ignominus' (Morrison 1978a: 342). Although Marshall felt neither young nor an ignominus he abstained from retaliating with an outright rejection, which is evident in his offer to intervene at the Carnegie Foundation in favour of Adorno.<sup>66</sup>

There is another point of view from which the whole procedure seems rather strange, since Marshall himself had repeatedly assured PFL, when the project had started, that he had no intention of intervening in its internal affairs – something both sides agreed included the recruitment of collaborators. In contrast, one might argue that deciding which sub-projects of a proposal submitted to the Foundation should be approved – and, thus, made possible in the first place – was well within the RF officers' responsibilities. The peculiar position of the Radio Research Project in the world of sponsored research at the time is again highlighted by Marshall's decision against support for Adorno. No other RF-supported projects I know of show such an amalgamation of researchers and foundation officers, on the one hand, and third parties belonging to the radio, advertising and, arguably, music industries, on the other. The decision to discontinue financial support for Adorno had been reached – as Marshall recalled in his interview with Morrison – after talks with representatives of the music business.

It would be erroneous, however, to attribute Marshall's attitude towards Adorno to the latter's non-conformism or to suggest that the RF had objected to his critical theory, let alone its hidden Marxism. The RF documents abound in cases where support, often essential for somebody's survival, was granted to scientific outsiders provided the latter could at least rely on the backing of some people in the scientific community in question. Thus, W.I. Thomas, having been sacked by the University of Chicago, had only been able to pursue his sociological studies because he was granted RF project funding. More specifically, the case of Hanns Eisler shows that the RF was indeed quite willing to support the activities of outsiders among the refugees.<sup>67</sup> Thus, there is every reason to believe that Adorno lost his RF-funded salary because of his arrogant attitude towards the foundation officers and other New York locals. Allowances were surely made for neo-immigrants, but arrogance will have been the very last among them.

Three decades later, two of the people involved in these events were offered the opportunity to have the memories of their past encounters published in one and the same book. On the invitation of the two Harvard historians Donald Fleming and Bernard Bailyn, Adorno and PFL both contributed exclusive essays to *The Intellectual Migration: Europe to America 1930–1960*. Given that PFL's daughter was married to Bernard Bailyn, Adorno would surely not have been invited to contribute to the book if PFL had objected to it. Thus, PFL once again let Adorno enter the American scene where, in contrast to PFL himself in the late 1960s, he was a completely unknown entity. Adorno abundantly profited by the opportunity and without delay republished his contribution – which

he had written in German and sent to Harvard – in his new old homeland where the worldview he propagated was taken up much earlier and much more effectively. Thus, the approach to memory that prevailed in German-language sociology (and beyond) was determined by Adorno rather than by former Vienna resident PFL, who was much less known in Germany. Actually, PFL's text was translated into German one year before his death, but failed to attain a similar standing in the collective memory of German-speaking sociologists, let alone in the spheres of the semi-educated with respect to the history of science.<sup>68</sup>

Besides the differences in the prominence and specific appeal of the two authors, there may have been another reason for their different reception, which may have to do with the texts themselves. PFL was bent on writing a thoroughly sociological text. When he started working on it, he wrote a letter to the co-editor of the planned book, his son-in-law Bernard Bailyn, telling him that he planned to write a '*biografie structurale*'. This choice of words not only superficially reflects the fact that the letter was sent from Paris, where PFL was a guest professor at the Sorbonne at the time, but probably also his proximity to Fernand Braudel with whom he had, with support from the Ford Foundation, set up the Maison des Sciences de l'Homme and whose writings he was well familiar with:

How to treat the personality of the biographee has a parallel in literary criticism. The Romantic School centred on his person. The 'new criticism' centers only on the work. But, for a sociologist, the biographee is a transmission channel, the characteristics of which have to enter in a characteristic way. (I think Adorno's piece is so dull because he just makes propaganda for his philosophical ideas, without relating them to anything.)

The second point is one to which I want specially to draw your attention. What difference does it make if a structural biography is written by the biographee himself? Take your work on Jefferson and the Enlightenment. Can you speculate on what you would have wanted to interview Jefferson about, in order to add his introspection? I will try to think about this problem further.<sup>69</sup>

PFL's solution was to see himself as an 'expert witness'. This term has an unequivocal meaning in US law. An expert witness reports to a court of justice or a House investigations committee on his scientific findings rather than as a subjective witness. Thus, the text is organized along systematic rather than chronological lines and, according to its author, aims to describe how his European background had affected the way he had structured the two activities in the United States that he considered to be the most important ones: the founding of research institutions and the elaboration of a research style (Lazarsfeld 1969: 270). In doing so, PFL much more strictly abided by the editors' guidelines than his Frankfurt colleague did, given that Bailyn and Fleming's concern was 'to isolate the impact of this group [of émigrés] on American intellectual life' (Fleming and Bailyn 1969: 4). A few years later, on the occasion of the (first) reprint of his autobiographical text, PFL once more explained its underlying structure. As often as not, the social researcher was

facing the problem of how to come to a meaningful integration of a large number of individual data in a 'conceptual "formula"' that would allow him to connect with a 'more general imagery'. This was also true for one's own memories. In his case, these were organized around two such 'integrating formulas': 'anticipatory clues' and 'latent strategies' (Lazarsfeld 1972: xii–xiii).

Adorno's text, 'Scientific Experiences of a European Scholar in America', differs in more than one way from this. Adorno starts out by saying that he had been called upon to write this contribution, and that he considered this to be a licence for him to engage in an autobiographical narration since he had entered the scene not on his own initiative but because he was asked to do so. The text sticks to a chronological order and fails to name any sources that would allow the reader to proceed to his or her own verification of what is said. Bibliographical references are omitted even for Adorno's own writings. Reporting anecdotes is a key structural element of the text, with the author seeking to stress his role as a subjective witness by introducing the occasional English term or even sentence into the German text to prove that he had actually been there.<sup>70</sup> On the other hand, he is more than reluctant to provide any names, writing about more than one of his interlocutors, colleagues and contacts without naming them, and fishes for compliments from his readers by telling them that 'only the so-called secretarial assistants at once strongly responded to my suggestions. Even today, I gratefully remember the ladies Rose Kohn and Eunice Cooper.'<sup>71</sup>

Behavioural demands inherent to the new surroundings – before February 1938, Adorno had been to the United States only once, for a short time – are reported in the form of impersonal imperatives: 'Thanks to my affiliation with the Institute of Social Research I was not as strongly exposed to direct competition and external pressure as this would otherwise have been the case' (Adorno 1981: 302). The framework for Adorno's text is provided by the pressure to adapt, which he mentions at the beginning of the text as something he was immediately confronted with – "'adjustment"' still was the magic word' – but felt himself 'not capable of ... by my nature as well as my background' (Adorno 1981: 299). At the end of the text, the reader is told that the testifying hero succeeded in resisting coercion and temptation and, in doing so, had even learned something.

In the late 1960s, PFL probably thought that his status in the history of social science was proof against any damage coming from Adorno, who had been back living in Frankfurt since 1950. Moreover, in the previous years, he had repeatedly shown his interest in sociological historiography and had contributed to it himself. Finally, as for cultivating his own reputation, he tended to a mild form of self-destruction which is manifest in the fact that he repeatedly granted his most violent critics more social space than it would seem advisable for someone interested in maintaining and enhancing his own fame. The 'atrophy of his political instincts' (Stehr and Lazarsfeld 1976: 788; Stehr 1982: 151) may well have affected his memory policies as well. He not only did refrain from vetoing Adorno's invitation to contribute to *Intellectual Migration* but,

soon after, included three texts by 'expert witnesses', as he once more called them, in a volume of selected articles by himself. Two of these 'witnesses' were loyal followers, but the third was the one person who during and after his stay in the United States had managed to cause near-worldwide sustained damage to PFL's reputation by creating another defamatory label. This was C. Wright Mills who, having been a collaborator of the Bureau of Applied Social Research in the 1940s, had caricatured PFL's line of research as 'abstracted empiricism' in *The Sociological Imagination* in 1959. PFL published this text in 1972 in *Qualitative Analysis*, declaring it to be 'a good antidote in a book where I might have overplayed the humanistic strain in my work' (Lazarsfeld 1972: xvi).<sup>72</sup>

While Mills's accusation of 'abstracted empiricism' would have gained notoriety even without PFL's support, the same cannot be said for the label of 'administrative research', which had only been reanimated by Adorno's contribution to Bailyn and Fleming's anthology.<sup>73</sup> In this 1968 contribution to *Intellectual Migration*, Adorno's verdict on PFL was given the form it would have from then on – and it should be noted that in the meantime, Adorno had apparently forgotten the differentiation that was so important to him in October 1939, namely, the distinction between an exploitative and a benevolent version of 'administrative research'.

In the 1960s, selective memory, which always plays so great a part in commemorative writing, played tricks on both Adorno and PFL, since they both attributed the discontinuation of Adorno's work with the Radio Research Project to their methodological differences. These differences of course existed, but it is no less true that in 1940 Adorno would have been quite willing to dispense with his reservations had he been invited to continue his work there.

By way of corroboration, one could cite all the articles, manuscripts and reports that Adorno had either written himself, or had been a co-author of, or had overseen as a 'conductor' (Table 5.1). Significantly, those of Adorno's writings that were largely in line with the demands of the methodological paradigm of the PRRP have not been integrated in Adorno's *Gesammelte Schriften* (Collected Writings).

After Adorno's collaboration in the Radio Research Project had ended, two of his publications resulted in a surprising success for the project outcast. Winthrop Sargeant, who was working as an editor for *Time* and had obviously been sent an offprint of one of his reviews by Adorno himself, responded with a friendly letter and invited Adorno to further talks. The composer and musical critic Virgil Thomson used the publication of the essay 'On Popular Music' as an opportunity to publish excerpts from it in his column in the *New York Herald Tribune* and to recommend reading it to whoever was interested in music.<sup>74</sup>

PFL, whom Adorno kept informed of these successes, profited by them to once more approach Marshall on the subject of granting Adorno individual support. In his letters to Marshall, he enclosed copies of the letters of approval that Adorno had received as well as a copy of the article from the

**Table 5.1** Theodor W. Adorno's Participation in the Princeton Radio Research Project

Title (Year)	Collaboration, Context	Original Publication	Pages	Reprint
Plugging study (1939)	By T.W. Adorno and M. MacDougald	60 pages, unpublished research report B-0070 of PRRP and BASR respectively	60	
Cuthbert Daniel, three types of like reactions in judging popular songs (1940)	The experimental work reported in this study was done by Mr. Gerhart Wiebe with the assistance and supervision of Dr. T.W. Adorno	Journal of Applied Psychology: 746-8	2	
On popular music (1941)	With the assistance of George Simpson'	Studies in Philosophy and Social Science, 9: 17-48	31	
The radio symphony. An experiment in theory (1941)	The author wishes to express his indebtedness for editorial assistance to Josef Maier and George Simpson (note to title)	Lazarsfeld, Paul F. and Stanton, Frank (eds) (1941), <i>Radio Research 1941</i> , 110-39	29	
Analytic study of the NBC Music Appreciation Hour (1938-41)			52	<i>Musical Quarterly</i> (1994), 78 (2): 325-77
Review of Wilder Hobson, American Jazz Music and Winthrop Sargeant, Jazz Hot and Hybrid (1941)	With the assistance of Eunice Cooper	Studies in Philosophy and Social Science, 9: 167-78	11	<i>Gesammelte Schriften</i> (2003c), vol. 19, 375-99 (no remark on Eunice Cooper)
A social critique of radio music (1941)	With the assistance of George Simpson	<i>Kenyon Review</i> (1945) 11 (2): 208-17	9	
Review of W van de Wall, The Music of the People (1941)	Probably written for <i>Studies in Philosophy and Social Science</i> , not published because of closing of the periodical		2	<i>Gesammelte Schriften</i> (2003c), vol. 19, 373f.
Zum Rundfunkkonzert vom 22. Februar 1940			3	<i>Gesammelte Schriften</i> (1984), vol. 18, 577-80.
Zum Rundfunkkonzert vom 11. Juni 1940			2	<i>Gesammelte Schriften</i> (1984), vol. 18, 581-3.



*New York Herald Tribune* and a copy of another letter from the *Dictionary of the Arts* inviting Adorno to collaborate.<sup>75</sup> Thereupon, at the end of June 1941, Marshall and Adorno had a talk. Marshall repeated the reasons that, he felt, made it impossible for the RF to award Adorno a 'grant-in-aid'. Since PFL, however, had informed Marshall that what Adorno really needed was 'recognition and hearing of his work', he suggested that Adorno should try to publish his studies with one of the big publishing houses. In order to help him, Marshall called several music experts and collaborators of publishing houses he knew, all of whom however were unable to discuss the matter with Adorno before autumn. One-and-a-half years after his brusque refusal to give further aid to Adorno, Marshall seems to have softened a bit in his judgement of him. After his talk with Adorno, he wrote a memorandum specifying his current position:

The grounds given Adorno undoubtedly justify declining to consider further aid, but this talk left JM rather unhappy about that decision. He is convinced now that Adorno's work has substantial value ... Adorno clearly has something to say about the social position of music in this country ... He can earn his living if he undertakes other research which is within the program of the International Institute of Social Research at Columbia, to which he is attached. But these studies which he began with Lazarsfeld and which he is at present most keenly interested in do not come within the Institute's program. Hence he cannot complete them unless he finds some further support.

JM has suggested informally to Lazarsfeld that he discuss Adorno's work with Dollard of the Carnegie Corporation. If DHS [David H. Stevens, director of the RF Humanities Division] agrees, JM would like to leave the possibility open of considering a smaller grant-in-aid than Lazarsfeld asked (\$1,000 to \$1,500) toward Adorno's further work with Lazarsfeld, if funds for it cannot be found elsewhere.<sup>76</sup>

This last attempt to secure an income for Adorno as a researcher in the field of radio music also failed. One of the reasons arguably was that it seems Adorno's interest in this kind of work had diminished considerably since he had seen the opportunity to boost his position in Horkheimer's circle. In April 1941, Horkheimer had relocated to California, at first allowing only his friend Friedrich Pollock and Herbert Marcuse to accompany him. Adorno was left to stay in the New York branch of the institute, a task easily imposed on him since at the time not all seemed lost for his further work with the Radio Research Project. But when Horkheimer's scheming resulted in Marcuse's capitulation and transfer to a job in the Office of War Information, the way was finally open for Adorno. The perspective of being able to collaborate on a book on dialectical logic that Horkheimer had long been planning obviously had a stronger appeal for Adorno than staying in New York to grapple with people he clearly felt to be intellectually second rate. In November 1941, Adorno moved to California, thus putting an end to his collaboration at the Radio Research Project. There, his next encounter with empirical social research was only a few years away (see Chapter Six).

## TRANSFER TO COLUMBIA UNIVERSITY

In early 1940, it became evident that the RF would go on funding the Radio Research Project. Negotiations between Marshall and the president of Princeton had resulted in the project being set free, so to speak, by Princeton. Robert Lynd once more acted in the role of a mentor in favour of PFL. He succeeded in convincing the authorities at his university that Columbia would profit from taking in the PRRP, now called the Office of Radio Research (hereafter ORR for short), and giving it the former premises of the Medical School. On top of it, he obtained a lectureship for PFL in the Department of Sociology.

Knowing for sure that he would be the director of a well-endowed research project for another three years, PFL considered reorganizing his research agenda. But this reorientation was also motivated and made possible by the clear downfall of the Music Study. To put it bluntly, with Adorno gone, the way was open for pursuing the development and sophistication of the panel method. As for matters of content, recalibration was facilitated by the fact that a new supervisory board had been set up where Frank Stanton was the only member to represent the radio stations while all the others were professors from Columbia University, with no psychologist among them. Thus, the row with Cantril had also had an impact on the content of future work. There was no need any more to consider using experimental designs, never a favourite with PFL. On the PRRP's last day in February 1940, PFL wrote a detailed letter to Marshall, using the research plans formulated in November as an occasion for self-evaluation.<sup>77</sup>

The promise he made in this letter, that all the studies that had not been terminated in the Princeton phase of the Radio Research Project would be completed at Columbia, was, however, never fulfilled. This was due to three developments: a new research assignment that turned out to be ideal for linking up several of the research interests pursued by PFL during the previous years; his appointment to the Department of Sociology at Columbia, which enabled him to shake off his dependency on RF financial support; and his acquaintance with Robert K. Merton, which helped him to embed his own work in a more concise theoretical paradigm.

The research project that was to give a new turn to PFL's work originated in the very part of the Radio Research Project that PFL, in the above-mentioned letter to Marshall, had described as being terminated. PFL had succeeded in finding other sponsors to supplement the funds he could dispose of from the RF sub-project. The new sponsors, however, had demands of their own on the researchers. The first of these new sources was the United States Department of Agriculture, which wanted them to explore the possibilities of using the radio for propagating its agricultural policies. As a consequence, the field selected for this research was a rural area: Erie County in Ohio. In later years, PFL himself was unable to recall why he had transformed a study of the use of the radio into a study of voters' choice (Lazarsfeld 1969: 330). The presidential elections scheduled for the autumn of 1940 probably motivated him to take up again his early Newark attempts of 1937 to use election campaigns to study

how voters made up their minds and what influence the radio might have on this. Anyway, he started to look for sponsors. He found one of them by selling the rights to the first publication of the planned study to *Life* magazine. This also implied that he had to come up with results before the autumn of 1940. Whatever he might say about voting preferences before or after the elections would also be a taking of sides in the inveterate struggle over election forecasts. During the election of 1936, George Gallup had conducted a study on a small but well-drawn random sample and succeeded in routing the *Literary Digest* (the previous champion in the field of election forecasts) and its so-called 'straw-polls' because the readers of the latter journal, who had been called upon to send back their ballot papers, had not been representative of the electorate (Keller 2001).

But the funds PFL could rake in were far from sufficient for the panel studies he planned, and so he transformed a practice that had proven to be successful during the first months of the PRRP into a new means of funding empirical research. At the time, he and his collaborators had managed to persuade a number of commercial market researchers to include one or another question that was of interest to the academic radio researchers in their questionnaires. So why not resort to this practice to aid his own projects? PFL found a producer of radio sets, and another enterprise that wanted to sell refrigerators, and proposed to do market research for them.<sup>78</sup> This was the invention of the omnibus survey which since has become the common technique, racking the nerves of even the most willing respondents with its reiteration of the phrase 'and now for something completely different', and which arguably worked much better in the beginning than it did later. Thanks to this invention, PFL was able to raise the handsome sum of \$100,000 (\$1.5 million in 2010), much more than the PRRP had disposed of for the entire first two-and-a-half years of its existence.

Erie County had a population of about 40,000, half of them living in urban and rural agglomerations, respectively. The industry consisted of various firms, and the county was interesting mainly because, since 1900, elections there had always rather faithfully mirrored overall results. A random sample of 3,300 inhabitants was questioned on socio-demographic traits, radio listening and reading habits, and political attitudes. From this sample, ORR collaborators selected sub-samples of 600 respondents each. Between May and November 1940 these sub-samples were canvassed (by Elmo Roper's firm) in seven waves of interviews addressing their opinion about the coming elections. In addition, three same-size control groups were included in the study. Its results were published in 1944 in a book entitled *The People's Choice: How the Voter Makes up His Mind in a Presidential Campaign*, which attained the status of a classic of election research as soon as it was published. The belated publication date was, at least in part, deliberately agreed upon in the hope that a book on voters' choice would draw more public attention if published on the eve of the next election.<sup>79</sup>

While previous surveys had sought to detect the roots of decisions that had been reached in the past, and surveys conducted with radio listeners had asked for preferences more or less at the time of the survey, this kind of survey investigated decisions pertaining to an event that was sure to come in the near future and obliged voters to come to a choice – or a non-choice in case they decided to abstain from the vote. On their way to this choice, opinion building could be studied, as it were, as an ongoing process. Moreover, the date up to which this process had to come to a conclusion was sure to set a limit to the activities of the social researchers, as well, since they, too, had to be content to collect data during the election campaign. At any rate, this type of research precluded extending data collection into an open future (or going into ever more details in reconstructing the past history of a decision). After the day of the election, all the researchers could do was to analyse the data since conducting additional surveys would no longer make sense.

In May 1941, PFL was made an associate professor at Columbia University.<sup>80</sup> The appointment was preceded by a heated controversy between Robert MacIver and Robert Lynd, accounts of which have not been entirely mutually corroborative.<sup>81</sup> There is agreement on the fact that there had long been a vacancy at the Department of Sociology: Robert E. Chaddock had voluntarily ended his own life and the committee that had to decide on his successor could not come to an agreement. Thereupon, the university's patriarchal president, Nicholas M. Butler, appointed another committee that did not include any Columbia professors. These external advisors, while equally unable to come to a unanimous recommendation, at least suggested that Chaddock's salary of \$8,000 (\$115,000 in 2015) be split up between two younger candidates. This suggestion enabled the opposing sides in the Sociology Department to each propose the candidate that suited them, but also required them to agree to the other's choice. Agreement was quickly reached on MacIver's candidate, Robert K. Merton, but not so for the other half. Williard Waller, who taught at Barnard College, the part of Columbia still at the time reserved for women, was the favourite for a certain time. Then Lynd brought up Louis Wirth and Samuel Stouffer, although nobody expected that either of them would accept the offer. Finally, PFL's name came up as a potential candidate. Theodore Abel, who was an associate professor in the department, took it upon himself to present PFL to MacIver by arranging evening games of bridge during which the Scotsman could closely examine the Central European (Abel 2001: 305–6). PFL passed the test, and MacIver agreed to his being appointed.<sup>82</sup> Since it had previously been agreed that the older of the two candidates would be given the position of associate professor and the younger that of assistant professor, the remarkable situation presented itself in which PFL, who as everybody knew had held no regular university position in either Vienna or Newark, skipped the first rung of the academic career ladder. His initial salary as a professor was \$5,500 (\$79,000 in 2010), plus \$2,500 (\$36,000 in 2010) from ORR earnings.<sup>83</sup> Still, PFL's skipping of a career step was less unusual than the formal downgrading

of Merton, who abandoned his position as a full professor as well as his function as a chairman at Tulane University to become an 'assistant' in New York.

It is part of the myth about the appointments of Merton and PFL that both opposing camps of the department hoped that a personal conflict, passed off as a rivalry between theorist and empiricist, would continue. But things turned out differently. When the two newcomers, who had previously neither met nor known each other's work, were informed that they would be future colleagues in the same department, they exchanged polite letters and recent publications (Merton 1998: 166–7). PFL, who was nine years older, felt called upon to invite the younger one and his wife for dinner soon after they had taken up their duties. On their arrival at the Lazarsfelds' house, their host gave them an unusual welcome: 'What a pleasure to have you here. But keep on your coat, dear Merton. I have got a sociological surprise for you. We will have to let the ladies dine alone here and will try to be back as fast as possible' (Lazarsfeld 1981: 338).

PFL had received a telephone call that very afternoon that required him to conduct a listeners' survey on a new radio programme. This programme was broadcast by the Office of Facts and Figures, the predecessor of the Office of War Information, as part of its moral rearmament effort directed at the population in view of the impending American involvement in the Second World War. The two men took a taxi to the radio studio where a group of listeners was assembled who, after the programme 'This is War' had been broadcast, were interviewed on their 'likes and dislikes' as recorded by the Program Analyzer. Merton, taken by surprise, had initially been sitting nearby, but after a while had started to slip little notes with suggestions for improving the interview to PFL, who was conducting the first group interview. Whether Merton's rhapsodic experience as an interviewer (Merton 1998: 202, n. 13) was sufficient for him to suggest improvements or whether it was his self-assurance that prompted him to do so cannot be decided here. At any rate, when the second round of listeners was invited to come in, PFL asked Merton if he wanted to conduct the interview himself. Merton did so to the satisfaction of the director of the ORR, and when they were finished, PFL decided that they should further discuss the matter. He phoned the waiting wives at home and asked for further leave. The two men went to the Russian Tea Room where, in the following hours over caviar and champagne, they came to know each other better. That very night, PFL invited Merton to join him in completing the report.

With the report due in a week or so, the weekend and the days following found us hard at work on our first, and impromptu, collaboration. The man who had come to dinner stayed on for almost a third of a century, first at the ORR (Office of Radio Research) and then, at its successor, the BASR (Bureau of Applied Social Research). (Merton 1998: 168)

This episode was the birth of the focused interview, which was improved by Merton during the following years and later said to be the model for the

technique of using focus groups. In contrast to the way participants' preferences were measured by the Program Analyzer, the most momentous innovation of the focused interview was that a random, or even natural, group of participants could be confronted with a specific stimulus of the so-called 'particular situation'. Subsequently, participants were questioned in individual interviews that built on the prior analysis of the material – which could be a film, a radio broadcast, a text to be read, an experiment, or participation in 'a political rally, a ritual or a riot' (Merton *et al.* 1990: 3). In the course of this 'content or situational analysis', hypotheses concerning the conditions and the consequences of the stimulus for the participants would have to be formulated and, then, used as a guideline for the subsequent interview. The interview itself was centred on the subjective experience of the initial situation so that the researches could work out, based on the Thomas theorem,<sup>84</sup> how the situation was being defined. At first, the new method primarily served to find valid interpretations of statistical correlations that had already been established. Later, the emphasis was on the non-directive nature of the interview because this was the only way to obtain 'unanticipated responses'<sup>85</sup> from the participants. The compendium later written by Merton, with Marjorie Fiske and Patricia Kendall (Merton *et al.* 1990), is a model piece of evidence for the successful mutual stimulation between theorist Merton and empiricist PFL.

Within less than two years, the situation for PFL and the Radio Research Project had changed as fundamentally as possible, and for the best. With the panel method, PFL had found a new way of investigating decision-making that allowed him to overcome the limitations of the Program Analyzer, where only isolated 'likes and dislikes' could be recorded. Social change could now be investigated with empirical accompaniment, so to speak. Given the security of his position as a professor at Columbia, PFL could now leave behind the well-founded feelings of insecurity that had marked his initial years as an immigrant – even though it remained one of his inveterate habits to recall his status insecurity to others<sup>86</sup> – and with cooperation soon turning into friendship with Merton he had found someone who knew how to reign in his intellectual flights and could put up with his propensity to improvisation in organizational matters.

But for PFL, entering the new world of a university professor was not only bound up with the security he had longed for but also with the challenge to live up to different normative expectations. His old mentor-cum-friend Lynd, while acting as the chairman of the ORR advisors' board, refused to get involved with daily routines and, unlike Cantril, was much less given to treating PFL as his inferior. What he did want, however, was to make sure that his protégé became acclimatized to the different mores of academic research – an undertaking in which he was supported by John Marshall who, as before, was in charge of the Radio Research Project at the RF Humanities Division.<sup>87</sup> They both felt that the ORR, with PFL in a regular university position, could be expected to operate with a reduced global budget and that PFL himself, given his secure position, should be content with a reduced salary. Primarily though,

they agreed that the activities of the ORR needed to be reoriented in a more general way. PFL's consulting activities for private enterprise, which had lately seemed to get a bit out of hand, were to be reduced and restricted to such tasks as had a topical link to the research programme. PFL needed to be confronted with a clear alternative:

In short, it [is] necessary for Lazarsfeld to decide which of the two alternatives confronting him he is to take. If he chooses the academic course, he must do so with a clear recognition of the fact that he is giving up the other alternative where, with the present interest in polling and marketing research, he could undoubtedly earn a considerably larger salary. Lazarsfeld has always said he preferred academic research, but he must now decide.<sup>88</sup>

Knowing what we do about PFL so far, it comes as no surprise that he did not comply with the recommended restrictions on his entrepreneurial ambitions. In doing so, however, his aim was not to maximize his personal income but to keep his research institute set on an expansionist course since he was always full of ideas he thought worth investigating. Support came, as it were, from world politics. After the start of the war, the United States was in a hurry to establish a large network of organizations that engaged in intelligence activities and collected data on the morale of the enemy as well as their own fellow citizens, and soon also began to plan for postwar times. Social scientists were ranking high in this, and soon universities and research-only institutions began to suffer from a lack of personnel since they were unable to keep up with government salaries. Thus, ORR collaborators, if they did not go to Washington, DC in the first place, also multiplied their consulting activities. The ORR became a provider of social science human capital for the war effort.

In the summer of 1943, PFL asked the RF for permission to suspend the operations of the Radio Research Project until the end of the war, since he lacked the staff and time to continue radio research. He informed Marshall that if the RF chose to freeze support until after the end of the war, when redundancies were to be expected among those who now worked for the government but would then no longer be needed, and to resume it when those who returned would need to be paid, this would be all right by him.

## ATTEMPTING AN EVALUATION

PFL's trajectory and eventual establishment between 1937 and 1941, as described on the preceding pages, was accompanied by the emergence of a number of products that now need to be reviewed in a more systematic fashion. When speaking of 'products' in the sciences, publications are, of course, what first come to mind. Other achievements that could legitimately be seen as products might be patents and the like in the case of technicians, and, in the case of social scientists, methodological and other research innovations that could be evaluated on the same level as things written. A third dimension that is well worth considering is the successful formation of new generations of

academics. The difficulty in evaluating all these products is that there is little agreement on the measurement tools, indices and benchmarking procedures to be employed. Furthermore, there are hardly any such evaluations of past social science achievements so far, which means that there is no way to even begin with a comparative evaluation. It is obvious that the only competitor in a one-man race cannot lose, but judgement on the success or failure of a single actor can be formed if his initial goals are known. Where these have been handed down to us – and for the PRRP this was indeed the case, given PFL's self-evaluation for the RF – the problem is probably only transferred to another level, i.e. having to judge whether the goals in question were realistic. Where programmes never existed or failed to be preserved, one has to rely on a more or less intuitive evaluation of what was actually achieved. This should be kept in mind in the following. Wherever it is possible, and necessary, the question of evaluation standards will be reconsidered.

In the case of the PRRP and its successor organizations, the existing data are indeed sufficient to allow for a rough analysis and evaluation. The following account is based on three sources: the reports and, more particularly, the financial accounts that were submitted to the RF for the first phase of the Radio Research Project, allowing for a number of simple quantitative analyses of these two-and-a-half years; an internal RF evaluation made after four years of project operations, providing material for qualitative considerations; and Judith S. Barton's bibliographical Bureau of Applied Social Research (BASR) documentation (Barton 1977). In this bibliography – as well as in all the other reports on the BASR – the founding date is given as 1937 even though the PRRP/ORR did not take on this name until 1944. Whatever it was that motivated this backward extension of the BASR lifespan – Converse (Converse 1987: 149–50) contrasts PFL's approach with that of Cantril, whose autobiography is centred on his foundation years of the later Princeton Office of Public Opinion Research and ignores the years 1937 to 1940 – it is thanks to this bibliography, with its record of both published and unpublished (hectographed) works, that an analysis of the written output of the PRRP is possible at all. As a supplement to these sources, the analysis will also include the three journal issues devoted to the work of the Radio Research Project and the three monographs and two omnibus volumes published between 1937 and 1945, which suggest a number of additions and corrections to the official bibliography. The period covered is somewhat longer than the duration of the projects that were financed by the RF, because some works were only completed or published later. For an analysis of the staff structure I will limit myself to the first phase of the project, i.e. the years from 1937 to the end of 1941, the period that ended with the Office of Radio Research clearly transferred to Columbia University.

Marshall's final report<sup>89</sup> of September 1941, which was submitted to the RF officers and the members of the 'board of trustees', lists the 'substantial accomplishments' of the Radio Research Project: its research, based on the idea that the radio was a public service, had met with approval. In its initial stage,



the radio listener had only been seen as a future buyer of the products that were advertised on the radio. At that time, the idea that the radio listener might turn on his set in order to be informed or to seek cultural satisfaction was little if at all understood. This new conception of the listener was due to the PRRP and manifested itself also in the fact that the radio and advertising industry, initially rather reserved towards the project, was now consulting it for advice on their own initiative. Here, of course, the Program Analyzer had to be referred to since, according to Marshall, it gave the industry an idea of how listeners would respond to a new programme even before it was broadcast.

Marshall's report states that during the first two years 'some ninety studies' had been conducted. Given the unexpectedly large number of single findings which would not have been possible without secondary analysis, it had seemed plausible to analyse the large data set in more detail and to use the half-year extension and first months after the transfer to Columbia to do so. Marshall roughly classified the studies of the Princeton phase as follows:

- 1) study of the characteristics of radio stations and programs on which listener interest seems to depend;
- 2) studies of the characteristics of listeners attracted by programs of different types; and more specific studies of;
- 3) news broadcasting, including its relation to news and print;
- 4) of broadcast music;
- 5) of the varied stimuli radio offers listeners, for example, in rural areas, or during political campaign.

Half of the studies had been of a methodological nature. In general, the studies had shown that techniques of social psychology were meaningful and reliable in view of obtaining information on the value attributed to the radio by listeners. One of the most remarkable results was the development of the panel method. A series of individual interviews conducted with the same group of persons over time had allowed the social scientists to study change in listeners' attitudes, conduct and way of taking in information.

Besides the methodological studies there had been studies on the following four substantial fields: '1) Educational radio programs; 2) Reading and listening; 3) Radio news and commentators; and 4) Music and radio.' Thus the four fields initially designated by PFL as promising in terms of substantial results – 'music, book-reading, news, and politics' – resurface in the final evaluation, but in an inverted order. The study on music on the radio, the initial number one, was now relegated to the last place, and politics had mutated into educational programmes, to which Marshall had always given more importance and therefore were now promoted.

In the areas of educational radio programs ... the project made a study of listener's reactions to educational broadcasts. The following are general conclusions formulated from the data collected by this study: 1) educational broadcasting is more effective when done on a regional, than a national basis; 2) educational radio succeeds best when embedded in an institutional or psychological set-up; and 3) people will come to like good programs if they are exposed to them long enough.

Reports on the studies of reading behaviour can be found in the book *Radio and the Printed Page*. A summary of its findings was published in the *Trustees' Bulletin* of the RF. Marshall's report concludes by giving a list of PRRP publications that had appeared so far.

Reading Marshall's final report, one must of course keep in mind that these evaluations invariably were also self-evaluations, since the foundation officer had to convince the members of the board who decided how to invest the Foundation's money that he had successfully fulfilled his own role as an intermediary. Later analyses, which can dispense with the consideration that a person in Marshall's position had to stand in his own best interest, might well come to different conclusions. Given that the publication of research findings may have been delayed due to factors over which the researchers had no control, it stands to reason that the period chosen for our evaluation should exceed the formal termination of project support. Therefore, the following account will include all PRRP, ORR and BASR publications that appeared between 1937 and 1945, with the caution that there is no way of telling which of these publications were actually produced in the context of the Radio Research Project and which of them were either funded from other sources or conducted for other clients.<sup>90</sup>

It can be reasonably assumed that at least in the second half of the period covered, the number of ORR, then BASR, collaborators who did not work for the Radio Research Project increased considerably. At any rate, in September 1942, PFL is obviously proud to report the achievements of the first year to his new supervisory board, to the secretary of Columbia University, and to John Marshall:

September 4, 1942

To Members of the Advisory Council (Copies to John Marshall and Philip Hayden)

From: PFL

RE: Financial Operation of the Office of Radio Research during the past financial year

...

First year of our present three-year grant

RF grant \$ 25,000 and the income from the Consulting Division was \$ 25,581.09

We thus were able to double our grant this year. It should be perfectly clear, however, that this rather high consulting income is really contingent on the existence of the Rockefeller grant. It enables us to develop procedures and ideas which then become 'marketable.'<sup>91</sup>

The names of the clients of the Consulting Division are not mentioned in the bibliography. It stands to reason that a certain number of publications were produced for these third parties.

According to Barton's bibliography, 112 articles were published in these 9 years, with a total of more than 100 different author's names (and about 100 hectographed reports). For half of these articles, of course including the three

**Table 5.2** Authors and No. of Articles, Mimeographed Manuscripts and Chapters in Books, including Co-authorship and Pages Written between 1937 and 1945

Author	No. of articles	Pages
Lazarsfeld, Paul F.	55	1247
Herzog, Herta	19	652
Fiske, Marjorie	17	455
Merton, Robert K.	10	318
Berelson, Bernard	9	271
Suchman, Edward	9	168
Green, Janet (Jeanette)	7	187
Gaudet, Hazel	7	183
Curtis, Alberta	6	157
Bayne, Martha Collins	5	122
Watson, Goodwin	4	268
Robinson, William	4	94
Franzen, Ray(mond)	4	47
Kendall, Patricia	4	44
Daniel, Cuthbert	4	36
Sayre (Smith), Jeanette	3	126
Dichter, Ernest	3	102
Adorno, Theodor W.	3	90
Peterman, Jack	3	65
Wiebe, Gustav D.	3	45
Stanton, Frank	3	32

'Elias Smith' articles (Lazarfeld's pseudonym, as previously explained), PFL was either the author or co-author. All in all, article output totalled 1,169 printed pages, 416 (or 36 per cent) of them being authored or co-authored by PFL. In addition, there were the three monographs published during this period: *Radio and the Printed Page* (Lazarsfeld 1940), *The Invasion from Mars* (Cantril *et al.* 1940), and *The People's Choice* (Lazarsfeld *et al.* 1944). These comprise 1,427 pages, 357 (or 25 per cent) of them by PFL, if one discounts the contributions signed Stouffer and Herzog in *Radio and the Printed Page* and attributes one-third of the authorship of *The People's Choice* to PFL (but none for *The Invasion from Mars*).

Among the other 70 authors who produced articles or contributions to omnibus volumes are Herzog (with 19 contributions totalling 652 pages), Fiske (17 contributions for 455 pages), Merton (10 contributions for 318 pages) and Bernard Berelson (9 contributions totalling 271 pages) (see Table 5.2 for further details). The 35 collaborators whose non-published manuscripts are listed in the BASR bibliography include, again in order following PFL (with 472 pages in 14 texts), Herzog (457 pages in 13 texts), Fiske (423 pages in 11 texts),

**Table 5.3** Ranking of Authors, Total Pages and Periods (1937-41 and 1942-5)

1937-41	Pages 1937-41	1942-45	Pages 1942-45	1937-45	Pages 1937-45
Lazarsfeld, Paul F.	750,0	Lazarsfeld, Paul F.	496,5	Lazarsfeld, Paul F.	1247
Herzog, Herta	252,5	Fiske, Marjorie	424,5	Herzog, Herta	652
Suchman, Edward	133,0	Herzog, Herta	399,5	Fiske, Marjorie	455
Gaudet, Hazel	124,0	Merton, Robert K.	317,5	Merton, Robert K.	318
Bayne, Martha Collins	121,5	Berelson, Bernard	271,0	Berelson, Bernard	271
Robinson, William	93,5	Watson, Goodwin	268,0	Watson, Goodwin	268
Adorno, Theodor W.	90,0	Green, Janet (Jeanette)	161,0	Green, Janet (Jeanette)	187
Cantril, Hadley	80,5	Schneider, Helen	113,5	Gaudet, Hazel	183
Curtis, Alberta	75,0	Sayre (Smith), Jeanette	110,0	Suchman, Edward	168
Arnheim, Rudolf	71,5	Curtis, Alberta	81,5	Curtis, Alberta	157

Goodwin Watson (268 pages in 4 texts), Merton (223 pages in 3 texts) and Bernard Berelson (192 pages in 6 texts) (see Table 5.3 for further details).

Thus, there can be no doubt about PFL's outstanding role for the volume as well as the intellectual content of the publications of the Radio Research Project and of the other studies that were terminated under his guidance in those years. He was not only the director, but also the author who outshined all the others in terms of scientific findings. If one is to believe the account he gives of himself, his role went even farther. His internal correspondence with his co-directors in the early days of the project, as well as his memoir, suggest that he also had to rewrite the texts of others.<sup>92</sup>

While in the first years, the bulk of the publishing contributions after PFL's were provided by his wife Herta Herzog and a number of junior collaborators, the second half was marked by Merton in fourth place while Adorno and other collaborators contributed distinctly less. It therefore does not seem unfounded to say that PFL's expectations concerning Adorno and Merton were basically the same, namely to find a partner who was productive and full of ideas and who could bear part of the burden of producing an immense number of research reports and articles. While Adorno was perfectly capable of fulfilling the writer's role expected of him, he failed to establish himself as PFL's partner in theoretical matters due to his social incompetence which, in retrospective, he glorified by framing himself as an outsider. The success of the Lazarsfeld-Merton duo, however, was something that others soon tried to emulate and in later years the two Columbia professors were themselves actively involved in an attempt to clone their model of cooperation: when Stouffer, PFL's long-term partner in statistical matters, was appointed to a chair at Harvard, Merton took to commuting between New York and Cambridge

for one semester with the aim of launching cooperation, based on the New York model, between Stouffer and Merton's teacher and friend, Talcott Parsons. (The attempt failed even faster than PFL's effort to find a basis for cooperation with Adorno.<sup>93</sup> Intellectual cooperation obviously is a delicate matter.)

In the present chapter, reference was repeatedly made to the methodological innovations achieved in the years of the Radio Research Project that were financed by the RF. Some of these, which were adopted by others – 'inventions' that saw successful diffusion in the social sciences – can truly be described as outstanding. This is true beyond doubt for the panel design and for the focused interview, even if the former turned out to be a technique that attained textbook honours but actually was less and less often used (let alone in the excessive manner that characterized its early use) because the funds it required ceased to flow as abundantly in later years. In contrast, by the end of the twentieth century, the focused interview (in its vulgarized form as the 'focus group interview') had become a serious rival of the personal interview, long assumed as the ideal way to do social science research. The 'focused interview' is singled out with a name of its own due to Merton's predilection for semantics, but one also must not overlook the various improvements of interview techniques, among them notably the detailed interview, all further milestones of the Radio Research Project.

While these are the 'mega-hits', as one is tempted to say, the other innovations that date back to the Radio Research Project should not be underestimated, such as secondary analysis and improvements in the formation of indices as well as some minor achievements in methodology. Among these, the analysis of divergent cases is worth mentioning even though it has, quite unjustly, been rarely emulated. Insiders and BASR worshippers are more likely to give priority to the Program Analyzer.

The third aspect that remains to be examined is the output of the Radio Research Project in terms of training for social science research. As one may recall, PFL's primary concern in his initial negotiations with Cantril was to make sure that he could maintain his Newark Center as a training facility. During all the phases of the work supported by the RF, this concern remained very important to PFL. He succeeded in convincing the RF officers to award grants to American students and graduates to have them participate in the PRRP. The project's appeal for junior scientists was such that, for instance, Bernard Berelson saw to it that the grant he had been awarded, in view of his cooperation with Ernst Kris and Hans Speier on studies on propaganda at the New School, was transferred to Columbia. About half of the almost fifty collaborators whose names appear in the PRRP accounts (because they received individual payments of more than \$100) have no publications to their name for the period covered. Some of these paid collaborators had evidently not been hired for this kind of work – they were charged with computations, copy-editing or typing assignments. Those who were hired for doing research work that included the production of pages obtained very different incomes from the activity. A calculation of the 'fee' per page for the initial years of the PRRP, however, shows that wage levels were not correlated with either status

**Table 5.4** Salaries and Productivity

Collaborator	\$ received from PRRP	\$ received per page
Wagner, Isabella	1,200	120.00
Sayre (Smith), Jeanette	1,679	104.94
Stanton, Frank	3,154	98.56
Fiske, Marjorie	2,333	77.77
Wiebe, Gus D.	580	58.00
Ollry, Francis	221	44.20
Cantril, Hadley	3,400	42.24
Adorno, Theodor W.	3,800	42.22
Stouffer, Samuel	899	33.92
Gaudet, Hazel	3,555	28.67
Lazarsfeld, Paul F.	18,750	25.00
Fleiss, Majorie	493	21.43
Suchman, Edward	2,817	21.18
Curtis, Alberta	1,082	14.43
Peterman, Jack	722	11.11
Daniel, Cuthbert	375	10.56
McCandless, Boyd	50	9.09
Robinson, William	750	8.02
Beville Jr, H.M.	50	2.94
Green, Janet (Jeanette)	65	2.50
Herzog, Herta	350	1.39

or personal proximity to the project supervisor; on the contrary, PFL's wife was plainly exploited while Adorno's salary more or less equalled that of Cantril (Table 5.4). But even those who had been hired for administrative tasks fell in love with social science research. PFL's long-time secretary, Rose Kohn, decided to give up her job to major in sociology and was so brilliant a graduate that she was immediately – and as the first woman ever – appointed to a position in the Department of Sociology at Cornell University where, under her married name Rose Golden, she was active for many years.

PFL had initially succeeded in placing former collaborators with radio stations, but after the outbreak of war, this also changed. More and more collaborators left the PRRP to work in one of the new government agencies. As an unwelcome side-effect of this migration, PFL found it ever more difficult to recruit collaborators at all. Government salaries were much higher, and more than once a potential collaborator chose the more remunerative government position.

On examining the list of collaborators of the Radio Research Project, two things stand out: the uncommonly high proportion of female collaborators, and the fact that while most of the collaborators produced no, or very few, publications during their time with the project or immediately after it, a considerable

proportion of them made a name for themselves with publications in later years.

There certainly is something coquettish about the way PFL, in an interview shortly before his death, claimed that his achievements in the social sciences consisted primarily in the foundation and establishment of the BASR (Stehr and Lazarsfeld 1976), but certainly there is no doubt that he was very successful in this field.

After the end of the war, PFL did not resume his collaboration with the RF Humanities Division. The last document to be found in the Rockefeller Foundation files on radio research is a paper entitled 'Columbia University's Bureau of Applied Social Research. Its Objectives and Purposes', one half of it a condensed statement of accounts, the other half a plan for future activities. What ranks first in the memorandum is the necessity of providing training in research techniques for students. In contrast to statistics classes and book-based research, the teaching of social research competences should be about coming to know the procedures used for collecting and analysing data: 'If the training of the student is to be fully rounded, he must receive parallel schooling in sociological findings and in the specific techniques for the methodological analysis and appraisal of researches.'<sup>94</sup> In the following two decades the BASR, co-directed by PFL and Merton, played a major part in the realization of this principle, with dozens of scholars who received their primary formation in this setting.

The detailed reconstruction of the workings of the Princeton Radio Research Project and its successors shows that European social scientists who emigrated to the United States due to the handing-over of power to the Nazis in Germany and Austria encountered an institutional environment that fundamentally differed from its Central European counterpart. Both the funding modalities and the work style were very different from European ways. In the United States, relatively flat hierarchies combined with the welfare-state work-creation schemes of the New Deal to offer promising openings for those who were capable of taking the opportunity, and willing to adapt to the new environment. In addition, there was a readiness among the larger public to give support to the new empirical social sciences, and to take their results seriously. Whoever seized the chance that was offered him, as PFL had done, could succeed rather quickly and could provide younger and more marginal new immigrants with jobs. And due to the openness of the social science field, new procedures for collecting and analysing sociological data could be created.

The following chapter will be dedicated to another case of European migrants trying to obtain funding for, and make advances in, their research in the United States.

# 6

## THE HISTORY OF AN APPROPRIATION

The cooperation between Paul F. Lazarsfeld and Theodor W. Adorno in the Radio Research Project was just one of those cases where émigré German-speaking social scientists got involved in some sort of cooperation after they had come to stay in the United States. In Central Europe, co-authorship of publications by German-speaking social scientists was rare in the first third of the twentieth century, and the project as a specific form of research organization was no less unusual, if not unknown. The predominant form of publications was the single-author monograph, with the same pattern holding true for journal articles. The only existing forms of cooperative production were the *festschrift* and the proceedings of academic conferences, but even in these cases the common grounds did not extend in most instances beyond the book's hardcover. Textbooks written by more than one author appeared only when the original author had died and the textbook was updated by one of his followers.

In the United States, change had set in as early as right after the First World War, and with time the differences with the conditions of scholarship in Central Europe became all the more marked. The first sociological book that was written by two authors who became famous was published in 1918: William I. Thomas and Florian Znaniecki's *The Polish Peasant* (Thomas and Znaniecki 1918–20). However, this cooperation did not yet result from a 'project' as a novel form of research organization, but came about because Thomas needed a Polish-speaking partner for his study of Polish immigrants' documents. Other publications written by two authors soon followed: W.I. Thomas and Dorothy Swaine Thomas, authors of *The Child in America* (Thomas and Thomas, 1928), and Robert Lynd and Helen Lynd, authors of *Middletown* (Lynd and Lynd 1929), were married couples, with husbands sovereignly ignoring the alphabetic order of (first) names. Double authorship gradually gained right of place in American publications. There were textbooks such as Park and Burgess (Park and Burgess 1921); first monographs resulting from teacher-student cooperation – such as Sorokin's study on social



mobility (Sorokin 1959) or the migration study by Thomas, Park and Miller (Thomas *et al.* 1921); and large-scale cooperative efforts such as *Recent Social Trends* (President's Research Committee on Social Trends and Mitchell 1934), in which a special board was appointed to take care of the design and general management while responsibility for individual contributions remained with the authors. At the same time, author duos also began to contribute to journals.

With the rising number of cooperative publications that differed from the customary omnibus volumes by the facts that several authors were involved in the overall product and individual contributions were hard to identify, there was also a rising need to solve the problem of which order to adopt for citing the collaborators' names. Where the authors were married couples, decisions were probably reached without further discussion, and where both partners were fully aware of their status difference, discussions will have been similarly unlikely. But the less obvious the status difference, the more urgent the question of who was to precede whom in the list of authors. In projects with more than two collaborators, this was often further complicated by the fact that it was impossible to identify each person's specific contributions to the final product.

This chapter is about one of the first social science publications that was produced and published by a group of authors with hardly any status differences: *The Authoritarian Personality*, cited most often simply as 'Adorno *et al.* 1950' (hereafter TAP for short). The aim of the analysis is not only to identify the contributions of the four, or rather six, authors, but to go into some further questions, as well. What was the institutional context of this study, how was the project managed, what are its outstanding innovations with respect to methodology and content, and, of course, what was the reception of this widely appreciated cooperative effort, including its selective translation into German?

According to Lepsius (Lepsius 1981b), the specific contribution of the social scientists evicted by the Nazi regime consists in their contributions to a theory of totalitarianism. Table 6.1 shows the works referred to by Lepsius and their reception in the social sciences since 1981 (due to the absence of citation indexes, there was no way of establishing reception before this date). A small number of the works referred to by Lepsius is still frequently cited. Considering the unusual fact that even forty years after their publication, these works are still appreciated (which is amazing in itself given the alleged shortening of the 'half-life period' of scientific publications – 'half-life periods' of more than ten years are considered highly unusual), the impact of the social science analyses of totalitarianism proposed by emigrants can indeed be said to be uninterrupted. However, for the entire period in question, only two hundred and twelve articles referring to 'totalitarianism' in their titles have been registered in the journals covered by the SSCI (see last line in Table 6.1), which suggests that there was more to these works than their analysis of totalitarianism. The three books with the highest citation rates – Schumpeter 1942, Adorno *et al.*

**Table 6.1** Citations of Selected Studies on Totalitarianism by German Émigrés, Social Science Citation Index 1981–2003

Author(s)	Book	First edition	No. of quotations in SSCI
Schumpeter, Joseph A.	<i>Capitalism, Socialism and Democracy</i>	1942	2018
Adorno, Theodor W. et al.	<i>The Authoritarian Personality</i>	1950	1421
Popper, Karl R.	<i>Open Society and its Enemies</i>	1945	504
Horkheimer, Max and Adorno, Theodor W.	<i>Dialektik der Aufklärung</i>	1944	482
Arendt, Hannah	<i>The Origins of Totalitarianism</i>	1951	471
Fromm, Erich	<i>Escape from Freedom</i>	1941	469
Hayek, Friedrich A.	<i>Road to Serfdom</i>	1944	440
Horney, Karen	<i>Neurotic Personality of Our Time</i>	1937	221
Mannheim, Karl	<i>Man and Society in an Age of Reconstruction</i>	1941	110
Neumann, Franz	<i>Behemoth</i>	1943	83
Voegelin, Eric	<i>Order and History</i>	1956	44
Mannheim, Karl	<i>Diagnosis of our Time</i>	1943	33
Fraenkel, Ernst	<i>The Dual State</i>	1941	27
Mannheim, Karl	<i>Mensch und Gesellschaft im Zeitalter des Umbaus</i>	1935	15
Rauschnig, Hermann	<i>Die Revolution des Nihilismus</i>	1938	14
Lederer, Emil	<i>State of the Masses</i>	1940	13
Plessner, Helmut	<i>Die verspätete Nation</i>	1959	13
Drucker, Peter F.	<i>The End of Economic Man</i>	1939	11
Neumann, Sigmund	<i>Permanent Revolution</i>	1942	10
Röpke, Wilhelm	<i>Civitas humana</i>	1944	5
Borkenau, Franz	<i>The Totalitarian Enemy</i>	1939	4
Loewe, Adolf	<i>The Price of Liberty</i>	1937	3
Mises, Ludwig von	<i>Omnipotent Government</i>	1944	2
Heimann, Eduard	<i>Communism, Fascism, and Democracy</i>	1938	1
Loewenstein, Karl	<i>Hitler's Germany</i>	1940	1
Rüstow, Alexander	<i>Ortsbestimmung der Gegenwart</i>	1951–7	1
Röpke, Wilhelm	<i>Die Gesellschaftskrisis der Gegenwart</i>	1942	1
Articles with 'totalitarianism' in title			304

1950 and Popper 1945 – are prototypical examples of works whose role can be described as seminal for the forming of schools or discourses. But of these only TAP is a publication that builds on empirical research.

## AN ANNOUNCEMENT

The first issue of the last volume of the *Zeitschrift für Sozialforschung* included a number of articles from the Radio Research Project that were expected, by Horkheimer, to impress American readers and, by Lazarsfeld in his function as a kind of guest editor, to help reconcile empiricists such as himself with critical theorists.<sup>1</sup> After the last article that deals with the subject of communication, there is a new section, 'Notes on Institute Activities', containing, besides some introductory remarks, a synopsis of a planned study on anti-Semitism. Horkheimer, who signed the note, informed the reader that a research project on anti-Semitism had been elaborated by his Institut für Sozialforschung a year previously but had been suspended since the United States, due to the international situation, was facing problems of an otherwise urgent nature. Anti-Semitism was considered by most people to be a relapse into the Dark Ages. The institute, in contrast, meant to show that anti-Semitism constituted a danger that was inherent to contemporary culture. In these introductory remarks on the 'special nature of the project', there is more than one statement that strikes one as rather peculiar, as illustrated by the following quotation:

Several new hypotheses will be presented which are the result of former studies of the Institute, such as that progressive modern thought has an ambivalent attitude toward the concept of human rights, that the persecution of the aristocrats in the French Revolution bears a resemblance to anti-Semitism in modern Germany, that the foreign rather than the German masses are the spectators for whom German pogroms are arranged. (Horkheimer 1941: 124)

Horkheimer further wrote that in their effort to identify the deeper mechanisms of anti-Semitism, they were bound to insist on facts that would not meet with approval by Jewish people. For the sake of scientific truth, however, these had to be accepted; nor would they refrain from maintaining that the ideas of freedom, progress and enlightenment were nothing but catchphrases in modern society since the fascists were saying the same things. It would be easy to criticize these and other statements of the synopsis, but what is really remarkable is that, at the time, these invectives and speculations were not only jotted down on paper but were published with the aim of garnering support, which suggests that this type of reasoning could well be expected to meet with approval.

Besides studies in the history of ideas, two empirical projects were planned: a typology of anti-Semites and an experimental part. Horkheimer argued that success in the fight against anti-Semitism essentially depended on the ability to distinguish between the various types of anti-Semites, which was hard to do in daily life. Experimental settings that were as close as possible to real-life situations would be used to develop a typology. Furthermore, the project would try to provide insights into regional and social differences in the occurrence of anti-Semitism. What Horkheimer had in mind at the time was not the kind of experiments that were later conducted by Stanley Milgram and, as is

well known, were explicitly conceived as a sequel to TAP, but the production of films that were to be shown to various audiences.<sup>2</sup> First, participants' responses to the film would be observed during the presentation, then interviews would be conducted and, finally, participants would be asked to write down their impressions. The following example may give an idea of the plan:

A film will be made, showing boys of 12 to 15 at play. An argument and a fight ensue. The relation of guilt and innocence is difficult to untangle. The scene ends, however, with one boy thrashed by the others. Two versions of the film will be made. In one, the thrashed boy will be played by a Gentile, in the other by a Jew. Another variation will be introduced by showing each of these versions with two different dramatis personae. In one version, the thrashed boy will bear a Jewish name, and in the other a Christian name. Thus the film will be shown in four different combinations:

- 1) The thrashed boy is a Gentile with a Gentile name.
- 2) " " " " " " " " " Jewish "
- 3) " " " " " Jew " " " Gentile "
- 4) " " " " " " " " " Jewish ". (Horkheimer 1941: 142)

The almost naive pleasure with which standard procedures of empirical social research are thus written down in so many words shows that Adorno had after all learned a number of things during his participation in the Radio Research Project: the need to specify the stimulus, the formation of control groups, the hidden observation of audience responses and the techniques of the indirect interview are presented with the typical pride of the new convert.

The publication of the synopsis primarily served the purpose of having something that could be presented to potential sponsors. However, Horkheimer himself was handling the matter rather nonchalantly, a fact which historians concerned with the institute attribute to his intention to reduce the number of its collaborators and to wholly dedicate himself, with the remaining staff, to his philosophical concerns (Wiggershaus 1986: 307–13). By the intermediary of Paul Oppenheim, who was a personal acquaintance of his and enthusiastic about the synopsis, he tried to win over the latter's cousin, Max Warburg, as a sponsor for the Anti-Semitism Project.<sup>3</sup>

Those members of staff who risked being sacked, and primarily Franz Neumann, saw a chance to secure their position at the institute by establishing contacts with some representatives of the American Jewish Committee (AJC). During these talks, it soon became evident that the sum of \$50,000 (\$720,000 in 2010) they had initially hoped for was unrealistic. The project proposal was reduced in substance as well as scope, and in the autumn of 1942 expectations for funding were downsized to a \$10,000 subsidy (\$144,000 in 2010), to be matched by money from the institute's own funds.<sup>4</sup> Rather than focus on the ever-expanding parts that were conceived of in terms of a history of ideas, there would now be only four sub-projects:

- (1) A section on the political role of anti-Semitism
- (2) A section on current socio-psychological mechanisms utilizing anti-Semitism as their spear-head

- (3) A section on the position of labor as regards anti-Semitism
- (4) A crucial concluding section, drawing on all of the above and looking ahead to the post-war world.<sup>5</sup>

The outline is clearly marked by the hand of Neumann and is rather instructive with respect to the degree to which the state of social science methodology was taken into account, since there is nothing to suggest that the planned study of the 'attitude of different groups' of labourers would involve attitude measurement on the individual level. Neumann intended to analyze the programmes of various groups in the labour movement as if it was possible to extrapolate from the programmatic declarations of the elites to the common members' attitudes:

Both elements [lack of a well organized political labor movement and the presence of a considerable Jewish element within the working class] together make for a prevalence of a variety of attitudes and for the lack of a well defined general policy. Analysis is needed of how the organizational structure and the racial and social composition of the different American unions influence their stand in the Jewish question.

As for the study in social psychology, the synopsis explains that its aim was to analyse anti-Semitic literature and other sources such as jokes, 'moving pictures, cartoons and other iconographical material', in order to identify types of anti-Semites, as illustrated by a 'preliminary classification' that was also part of the published synopsis. Since preliminary talks with the AJC had shown that the latter was primarily interested in material that could be used for educational purposes and was unwilling to support the studies unless they would contribute to the fight against anti-Semitism, the types of anti-Semites were also presented as 'Types of popular responses to anti-Semitic propaganda'. The success of counter-measures was said to depend on how clearly the differences between the types were perceived.

On 21 October 1942, notwithstanding some reservations, the Administrative Committee of the AJC approved a 'grant' of \$10,000 (\$144,000 in 2010). Only after this decision had been made did a detailed critical review of the Anti-Semitism Project, written by historian Abraham G. Duker, reach the AJC. If the Committee had been able to take note of this statement before reaching its decision, the Project would not have stood much chance of being considered worth supporting.

The long interval between the date of approval and the signing of the contract thus seems to suggest that those in charge at the AJC, while holding themselves bound by their decision, had begun to have doubts. A phrase in the AJC's letter to the institute in which the agreements reached between the two parties were laid down suggests as much, since it stipulated that the institute had to prepare, within the first month, a 'general statement of the scope of the project, the fields to be investigated'.<sup>6</sup> Requesting researchers to find out what they consider worth investigating in the first place was a rather obvious

divergence from the customary procedure, at the time, for awarding a research contract or approving a request for funding.

The objections raised by Duker, however, were such that an a posteriori concretization of the research project seemed inevitable. In his statement, the American historian noted nine 'major shortcomings' of the memorandum. First, the project was too strongly focused on Central and Western Europe, and particularly on Germany. However, in Eastern Europe the Jewish problem was somewhat different since in Poland, for instance, Jews were a national minority. Second, while the authors might well have a 'very good knowledge of general history', they were definitely out of their depth with Jewish history. Third, their 'predominantly' economic perspective led them to ignore certain important factors in the fight against anti-Semitism. Fourth, the authors' knowledge of the working class seemed deficient, leading them to overrate the role of the labour movement in the fight against anti-Semitism. No systematic distinction was drawn between ideologies and the 'individuals who profess them' – the American historian, unlike the authors who belonged to the sphere of the institute, was obviously familiar with recent social science literature that advocated attitude measurement on the individual level. Admitting only the Communists as a revolutionary party amounted to the 'willful ignoring of other revolutionary left-wing parties'. The observations on 'labor and Jews in the U.S.' were of too general a nature to be criticized at all. Fifth, although Duker's own approach as a historian was 'largely economic', he felt that the 'almost exclusively economic orientation' of the memorandum was an error. Due to this orientation, the authors failed to see the particular characteristics of anti-Semitism in cases where Jews constituted 'sizeable minorities', as well as where they had to be considered the 'spearhead of the cultural imperialism of the ruling groups'. Sixth, the part dealing with postwar anti-Semitism was 'nebulous', as well. No reference at all was made to Zionism and the mass emigration of Jews that was to be expected. The authors seemed to be disinterested in a 'specific solution of the Jewish problem'. One need not be a Jewish nationalist or a Zionist to realize that such a solution could well be realized within a democratic framework. Seventh, the relations between 'scholarly approach' and 'practical results' were particularly unclear with respect to postwar strategy. Eighth, 'many of the suppositions, particularly in the first part' were so clear and self-evident that it would be a waste of time to invest any more research efforts in this respect. Ninth, the authors 'have bitten more than they can chew'. Duker had – very roughly – counted no less than eighty-six individual research issues, and 'this in spite of the reservation given on page 2 that "the above examples are only a few of the situations that should be studied".' Duker felt sure that in 1943 and, presumably, again in 1944, the AJC would be faced with 'new requests for a new subvention', a prediction that turned out to be true.<sup>7</sup>

## THE ANTI-SEMITISM PROJECT, PART I

In the beginning of March 1943, the detailed negotiations were at last concluded, the delay being arguably due to internal AJC problems and a turnover

of AJC personnel. The agreement that was sent to the institute provided for a one-year duration and two locations for the Project.<sup>8</sup> The New York group would be co-directed by Friedrich (now Frederick) Pollock and Robert MacIver, each dedicating part of their working time to the task. Originally, co-directorship had been proposed to Robert Lynd, MacIver's rival at the Columbia department of sociology who, however, had in the end declined for reasons of overwork. Lynd had been approached by Neumann, much to Horkheimer's annoyance, who had not been informed of this move. At the time, both Columbia professors were members of the institute's Advisory Committee and their names appeared, among many of others, on its official letterhead. The New York group was to have Leo Löwenthal as Pollock's assistant and full-time collaborator, as well as Arkadij Gurland and Paul Massing, also on a full-time basis. MacIver himself was to have two part-time assistants, one of whom was Isaque Graeber, the man who in the summer of 1941 had proposed to act as a fundraiser for the Anti-Semitism Project and to approach the relevant foundations and Jewish organizations.<sup>9</sup>

The California-based second group was to be directed by Horkheimer, with Adorno as his full-time assistant and two further assistants yet to be recruited. The list of prospective collaborators reveals that, at the time, social-psychological competence was not represented at the institute. After falling-out with Erich Fromm, no effort had been made to fill the void. Rather than hire new collaborators, the institute made do with a number of assistants who were confined to the institute's periphery and were recruited for tasks that were considered of secondary importance. Given that a general orientation to a social-psychological reinterpretation of some fundamental Freudian insights was one of the core competences of the institute, this is remarkable. The conclusion that suggests itself is that someone belonging to the institute's core group felt that Fromm's contribution could be substituted. It seems that Adorno thought himself up to the task, and was supported in this by Horkheimer.<sup>10</sup>

The contract specified that the institute was held to match AJC funds by its own means and that it was given one month in which to draw up a more detailed work programme, as mentioned above. From the very beginning, a one-year deadline was fixed after which six to ten reports had to be submitted, their publication being subject to an agreement between the contracting parties: 'No empirical data developed through the project are to be published without our specific consent.'

The terms of the contract were much more detailed and specific than those of, for instance, the Rockefeller Foundation, and this constellation corresponded to the rules that Adorno was later to denounce as characteristic of 'administrative research': 'The collecting of data ... for the benefit of ... planning agencies' (Adorno 1981: 303–4). That is, the contract unequivocally stated that the project was expected to be a 'useful undertaking' with the aim of being 'better prepared to forstall and to deal with the possible developments and manifestations in the area of anti-Semitism'. The only difference from the Radio Research Project that prompted Adorno to develop the concept of 'administrative

research' was that the planning agency was the 'Committee on Public Relations' of the AJC rather than the radio stations and some well-meaning *educators*.

Given the AJC's lack of experience with respect to the planning and conducting of social research, it was probably unable to foresee that it was completely unrealistic to plan for six to ten reports to be completed within just one year; and those among the institute's collaborators who might have been able to foresee this did not bother about realistic planning. In July 1943, Horkheimer admitted in a letter to Marcuse that 'the problem of Antisemitism is more complicated than I thought in the beginning'.<sup>11</sup> At any rate, meeting this arrangement would have meant that each of the five full-time and three part-time collaborators, backed up by their three directors, would have to come up with a report within just one year. Not surprisingly, after this year, only a fraction of the announced reports was completed.

After only half a year into the work of the Anti-Semitism Project, and just as Duker had predicted, Pollock approached the AJC to demand additional funds to extend the three studies. Due to the collaboration recently begun with R. Nevitt Sanford, enhanced support was mainly required for the interviews and the psychological experiments, and Pollock therefore asked for an additional \$5,500 (\$72,000 in 2010) to be dedicated to the project.<sup>12</sup> It seems that the request either failed to gain approval by, or was lost in, the AJC. At any rate, no trace of further insistence by the institute, on the one hand, or of AJC documents referring to the demand, on the other hand, could be found.

Meanwhile, Horkheimer had either become more interested in the Anti-Semitism Project or felt that the odds of being granted additional funds by the AJC were by now more in favour of the institute. At any rate, beginning in the autumn of 1943, he sought out social relations with people who could exercise a certain influence on AJC policies. A meeting brokered by psychologist Maurice J. Karpf with the Vice-President of the AJC, Morris D. Waldman, was particularly important. During a dinner given at his home by Karpf, who was in a leading position in a Jewish welfare organization in Los Angeles, Horkheimer and Adorno explained their previous work. Sometime later both wrote down their reflections in an aide-memoire, which they sent to Waldmann at the end of December 1943. What had been said during that dinner was, thus, transformed into a kind of interim report on the work completed by the institute after three-quarters of the project's duration. The New York group was investigating the following questions:

To what extent did the distribution of Jews in the different branches of German economic life affect the rise of National Socialism? What can we learn from the attitudes of the various strata of the German population towards the National Socialist measures against the Jews? What were the various concrete causes in domestic and foreign politics for the anti-Semitic steps taken by the Nazis in each year since 1933? Why were the policies of the German Centralverein ineffective, and how could they possibly have been improved? In addition, our friends in New York are studying the ways in which anti-Semitism might be used as a political instrument in post-war America. Attention is also devoted to the attitudes of the various Churches, and the possibilities and means of winning some of their forces over to our cause.<sup>13</sup>



In spite of the succinct wording, it can be said that the New York group was still proceeding along the lines of the traditional humanities and political science analyses that aimed to capture the attitudes of the various social strata in Germany; the same procedure seems to have been used to identify American religious groups. The above-mentioned tendency to view anti-Semitism in the Weimar Republic and Nazi Germany as a response to Jewish presence in various societal spheres has been maintained. The arrogant attitude towards the 'Centralverein' – Centralverein deutscher Staatsbürger jüdischen Glaubens (Central Association of German Citizens of Jewish Faith) – which had been founded in 1893 and was the only Jewish association tolerated by the Nazis after 1933, may be something of a surprise (it is not quite clear whether the planned study was to cover the period after 1933, as well). The double allocation of blame – presuming that the emergence of anti-Semitism in Germany was solely due to the intense Jewish presence in certain social milieus, and stating that the Jewish agencies had been inefficient in fighting anti-Semitism – echoes previous statements by Horkheimer on Jewish issues, now made into a dogma. Thus, in Horkheimer's 'Die Juden und Europa' (The Jews and Europe), there is the maxim – later often quoted in an abbreviated form – that whoever refused to talk about capitalism should abstain from talking about fascism. Those who liked to chant this sentence in the 1960s surely overlooked the one that follows it, with Horkheimer arguing in a crudely Marxist manner against the Jews as the forerunners of capitalism (Horkheimer 1939).

In the written version of his remarks of that evening, Horkheimer then turned to the work of the West Coast group where, in spite of the 'tremendous importance of economic and social tendencies', anti-Semitism was considered to be 'fundamentally a psychological phenomenon'. This was one of the 'destructive human attitudes' that were closely linked to other 'expressions of hatred' and was part of a neurotic syndrome that could with good reason be labelled as an 'anti-democratism'. The Jews and all the others for whom this was a threat were not the only ones to ignore the true nature of the destructive forces they were obeying, since this was true even for the anti-Semites and anti-democrats themselves. The totalitarian agitator knew instinctively how to utilize these forces. Those who wanted to fight anti-Semitism had to study these hidden psychological forces under the microscope, so to speak: 'Only when we know them as thoroughly as medical science knows the biological infectors of mankind, can we hope to discover effective remedies, to be translated into devices of education and other sectors of our cultural life.'

This certainly was a powerful way of putting things, but as an argument it was highly ambivalent. Horkheimer's initial statement that they did not want to downplay the social and economic role that was so pivotal to the work of the New York group is counterbalanced by a notion that refers to deeply rooted psychological traits, on the one hand, and chooses a name for them, on the other hand, which strongly suggests – no doubt even more so at the time than today – that this had something to do with each individual's free choice. Of course the 'ism' suffix has since then been so frequently misused that one

tends to forget that 'isms' originally described ideologies, or worldviews, to which one adhered, or refused to adhere, by one's own free will, a semantic change perfectly illustrated by the term 'anti-Semitism' itself. After all, the term started its career as a self-attribution of people who, while they probably did not want to be actually viewed as haters of Jews, still made no secret of their antagonism to them, as shown by organizations and parties that called themselves 'association of anti-Semites' or 'alliance of anti-Semites'. In the course of the twentieth century, the meaning was actually inverted, with the result that if today people say that they are not anti-Semites, this can indeed be taken as a clue to the existence of a distinct aversion to Jews. Today, almost anything to which one wants to attribute great importance, or of whose universal and widely received nature one is convinced, is dubbed an 'ism'. Thus, anti-Semitism was at least verbally, for the time being, identified as a current threat to American society. By adopting the rhetoric of containment and precautionary action that was natural to the AJC and other organizations of self-defence, Horkheimer was led to propose a theory to his clients that managed to present their fight against this threat as a task of universal historical scope, with only one thing remaining unclear, namely whether the discovery of the infectious disease would go along with the delivery of its remedy. The analogy with medical research with which Horkheimer let himself get carried away might have told him – if his only purpose had not been to find some catchy images – that the detection (of the causes) of an infectious disease by no means implies that the vaccine that would guarantee immunity against it is discovered in the same process. But the critical theorists were far from being daunted by this Promethean task.

Before giving the floor to his assistant Adorno, Horkheimer indulged for a certain time on his hobby horse, the experimental film, and then declared *en passant* that in Europe, and even in Germany, anti-Semitism was currently quite insignificant. The German population, terrorized by fascism, was craving democracy. However, after the end of the Second World War, anti-Semitism might easily be resurrected by 'a few seemingly unimportant steps', which could be prevented by preparing for them early enough: 'We are confident that the responsible military agencies can be made aware of the importance of these problems and directed to do the right things without putting an undue weight on the antisemitic aspect.'

Whatever this last half sentence is supposed to mean, it is clear at any rate that Horkheimer offered to act as an advisor to the future US military government in Germany. However, this self-commitment of critical theory to the new rulers – which, in spite of all the verbal distance taken from administrative research, did indeed come to pass – was still several years away.

Adorno's contribution was only half as long as that of his boss, and limited to a report on what the California group had so far accomplished. According to this report, radio addresses by a certain Christian fundamentalist – as one would probably call him today – preacher had been subjected to a detailed study by means of content analysis.<sup>14</sup> The stimuli regularly applied by this

preacher were 'mostly of a psychological nature'. What the preacher, named Martin Luther Thomas, did was not so much address the objective concerns of his audience or try to rope them in for a concrete political programme, but manipulate 'their emotions and ... their largely unconscious desires which he attempts to lead into the channels of his organization'. Modern anti-Semitism was 'to a very large extent, a matter of conditioned reflexes rather than of aboriginal and spontaneous behaviours' (whether this was not already true for, say, the followers of the Wilhelminian court chaplain Adolf Stoecker at the end of the nineteenth century is an open question). Therefore it was particularly important to study both these 'conditions' and the "'reflexes'" which very often are but the automatic imitations of those stimuli'. A detailed study of this stimuli-reflex relation was necessary before any 'large scale field work' could be conducted. These future field studies, however, should not be limited to an investigation of the beliefs of the masses. This was not only difficult on the technical level, since respondents tended to follow a 'mechanism of "psychological censorship"', but in a deeper sense, as well, since fascism was not just an expression of what the masses were thinking:

Though the opinion of the majority certainly cannot be discounted, it is the ideology of the self-styled Fascist elite that is decisive. It moulds public opinion to a much larger degree than is the case in democratic movements. Hence the weight of the 'stimuli' produced by the 'leaders', and the importance of our knowledge of those stimuli, is disproportionately greater than we are led to believe by our democratic persuasions.

That the people who conducted public opinion polls were primarily interested in what was expressed by the respective majorities was one of the constants of empirical social research, as seen by Adorno. In 1943, however, he felt that the role of the 'opinion leaders' – he did not explicitly use this term from Lazarsfeld's study, but this is what he meant – was more important. The idea which, for the Lazarsfeld school, was closely associated with this, i.e. that the mass media did not directly influence their audience – not even in the case of fascist agitators – had not been adopted by Adorno before he left for the West Coast. His specific line of thought, i.e. that fascist agitators such as Thomas were able to influence their audience more deeply, is not enlarged upon in the aide-memoire, which says nothing of either the stimulus or the responses. The claim that this preacher was 'highly successful' in Southern California is all there is, allowing Adorno to speculate on the higher degree of anti-Semitism in Los Angeles.<sup>15</sup>

In a letter to his life-long friend Pollock, Horkheimer again referred to the reasoning he had relied on to convince the AJC's Vice-President: 'there should not be one Thomas study, but a series of similar analyses. To prepare the right antidote for a definite social illness, one needs even greater and better equipped socio-chemical laboratories, and at least as well-trained staffs as for the fight against physical illness or for the invention of new weapons in real warfare.'<sup>16</sup>

Coming from the very authors who never tired in their life-long criticism of positivist sociology for its naturalistic tendency, i.e. for dealing in the same way with the social and the natural world, the idea of socio-chemical laboratories for critical sociology seems rather incongruous. Horkheimer's desire to be able to direct a research organization like the one at Los Alamos, where at that time the atomic bomb was in the making, can hardly be taken seriously, and even less so considering his permanent alternation between megalomania and asceticism.

At least on one point, Adorno's worldview was more stable than that of the institute's Director, his senior by eight years, since even a quarter of a century later he seriously advocated putting all anti-Semites on the proverbial psychoanalyst's couch.<sup>17</sup> Even in middle age, he still adhered to this scheme, albeit in a more modest form: it would suffice to provide them with a textbook.

At the end of February 1944, the institute submitted a report stating that it employed a total of twenty-two researchers, which was hardly consistent with the accounts presented in the course of the year, where only fourteen names had appeared. The grandiose claim of thirty-eight existing individual studies, the titles of which were listed in an appendix, does not stand up to closer inspection. In the said appendix, the 'studies' are classified according to whether they were 'completed', 'in the process of research' or even merely 'in the state of preliminary study'. Seven studies were included in the first category, seventeen were classified as works-in-progress and for the remaining twelve studies, a title was all there was. In trying to identify the seven studies that were said to have been completed by February 1944, one finds that none of them has ever appeared in print. However, given the inconsistencies between the list of collaborators and the list of studies, verification is all but easy (Table 6.2). Horkheimer's correspondence with the members of the institute abounds with clues suggesting that the studies that were said to have been completed consisted of no more than a bibliography or, worse, were in a state that made it seem advisable for Horkheimer to refrain from putting them forward at all. In June, then, the institute submitted a thousand-page brick to the AJC and considered this to be the 'report on the first year's work'.<sup>18</sup>

Among the full-time employees in the project, one does not find Graeber's name on the list of collaborators, while the names of some of those who had obviously done some kind of work for the project fail to turn up in the account sheets submitted to the AJC. Manuscripts that actually existed at the time, such as the one written by Else Frenkel-Brunswik on a preliminary study with a number of female Berkeley students, are not mentioned in the report although payment of wages to Frenkel-Brunswik had been made.<sup>19</sup> All these inconsistencies are not addressed in the report, let alone explained. Similarly, there is no indication of what had become of the two sub-projects on anti-Semitism among American blue-collar workers that had initially been announced, or of the outlook on the 'post-war world' that had originally been advertised as a 'crucial concluding section'. In contrast, the tables were reversed, so to speak, and the institute pointed out to the AJC that the institute

**Table 6.2** Collaborators of the First Anti-Semitism Project of the Institute of Social Research, made possible by a grant of the American Jewish Committee (AJC)

Collaborator	Location	Total Salary
Financed by AJC:		
Gurland, A.R., L., Ph.D.	NY	2,853
Graeber, Isaque	NY	2,145
Peck, George, Ph.D.	LA	1,575
Massing, Paul, Pol.Sc.D.	NY	1,226
MacIver, Robert, Ph.D.	NY	1,000
Dupont, Grete	LA	750
Simmel, Alice	LA	450
Total AJC costs		10,000
Financed by the Institute of Social Research:		
Löwenthal, Leo, Pol.Sc.D.	NY	4,800
Adorno, Theodor W., Ph.D.	LA	4,600
Horkheimer, Max Ph.D.	LA	2,400
Pollock, Frederick, Pol.Sc.D.	NY	2,400
Massing, Paul, Pol.Sc.D.	NY	1,560
Reinheimer, Jane, B.A.	LA	675
Brunswick, Else F., Ph.D.	Berkeley	600
Sanford, R. Nevitt, Ph.D.	Berkeley	350
Psych Dept UC Berkeley	Berkeley	500
Other costs		554
Total Institutes costs		18,439
Other Collaborators of the Institute:		
Edelheim, Margaret T., J.D.		
Freeman, Joseph, B.A.		
Kirchheimer, Otto, J.D.	NY	
Langerhans, Heinz, Ph.D.	NY	
Levinson, Daniel, Ph.D.	Berkeley	
Neumann, Franz, J.D.	NY	
Paechter, Henry M., Ph.D.	NY	
Porter, John, B.A.		
Roberts, Fred		
Tillich, Paul J., Ph.D.	NY	
Weil, Felix J., Pol.Sc.D.	LA	

had raised almost double the sum provided by the AJC. (This claim is incorrect since about half of the payments allegedly made by the institute were made to people who apparently were not working for the project, cf. Table 6.3). Moreover, the AJC was accused of having failed to fulfil its part of the cooperation with the institute:

The Committee did not find it possible to provide the kind of guidance, so vitally required, on specific aspects of the Project and its focusing on practical considerations. Requests of the Institute for intensive work-sessions with members of the Committee did not bear fruit; and we cannot be sure that any careful consideration has been given to either the suggestions or the actual studies made by the Institute.

**Table 6.3** Studies of the Institute of Social Research during the First Anti-Semitism Project

Studies	Title (according to report 1944)	Presumed Author	Joint Meetings (Speaker)
I. The Danger	'The unique character of antisemitism as an instrument in domestic and foreign politics'  'Joseph E. McWilliams: An American disciple of Adolf Hitler'		
II. The European Experience	'A defense that failed: The policy of the Jewish Central-Verein'  'Attitudes of civilians toward Nazi antisemitic policies, based on personal observations in a penitentiary, in a concentration camp, in a contractor's shop and in Berlin, discussion from 1933 to 1939' <sup>a</sup>	Paul Massing  Paul Massing, Heinz Langerhans, Heinz Paechter	'Analysis of the policies of the Centralverein deutscher Staatsbürger jüdischen Glaubens' (Gurland)
III. American Antisemitic Agitators and their Followers	'X from the West Coast'  'George Allison Phelps'	Theodor W. Adorno  Leo Löwenthal	
IV. Potential Allies	'Catholicism and Anti-Judaism'	Otto Kirchheimer	'The Catholic Church and her Jewish Policy' (Kirchheimer)

Note: <sup>a</sup> In October 1943 *Der Aufbau* reported on a writing competition at the Institute on the theme 'Experiences with Nazi-Antisemitism'. Among the judges were: Manfred George, Max Horkheimer, Thomas Mann and Paul Tillich.

Still, for all their accusations of having been deprived of guidance by their sponsor, the institute seems to have had no doubts as to the continuation of their cooperation when this 'Note' was submitted to the AJC. Again, a number of rather vague projects were proposed for the next phase: an in-depth study of the 'nature of modern antisemitism, especially in its relation to the mind and emotions of modern man' was to be conducted, as well as an 'investigation into the roots of antisemitism among one or more specific social groups'. The AJC was invited to select those of the ongoing studies on the list in which they were most interested; a compendium on anti-democratic propaganda was to be published; and, last but not least, a research laboratory should be established 'for developing methods to measure the scope and depth of anti-semitism in the U.S.A. and to test in a continuous and practical way the efficacy of counter measures'. One outcome of this report was a seminar for both the AJC and the institute, offering sixteen lectures, between May and December 1943, by members of the institute as well as by close associates such as MacIver and Paul Tillich.

How was all this possible? Three factors suggest themselves as the most likely reasons: the turnover of personnel at the AJC; the negotiating skills of Horkheimer and his colleagues, who did their thoroughgoing best to exploit their cultural capital for maximum effect; and, finally, the AJC's lack of experience in the administration of research projects, combined with the strong appeal of social science research.

As a result of the high turnover rates among the AJC staff and the frequent reassignment of responsibilities among those AJC officers who were supposed to ensure the support, control and administration of the project, members of the institute presently found themselves in the role of having to fill in the new AJC officers on what the project was about in the first place.<sup>20</sup> This, of course, strengthened the institute's bargaining position, while the differences between this situation and the routines for controlling support measures that had been elaborated over the years by the RF, for instance, became all the more obvious. At the RF, a small group of officers was exclusively charged with the close control of ongoing projects, relying, among other things, on a system of written notes that had been elaborated for this purpose and that even after so many years allows readers who confront this material for the first time to get their bearings in a very short time. In contrast, such AJC documents as still exist remind one of those state bureaucracies that have long ago dispensed with the ideal of readability. What is laid down in writing is comprehensible only if one can draw on a considerable amount of prior knowledge.

In a world as confusing as this, poise, belief in one's own importance and a diffuse display of symbols of reputation – in short, impression management – tends to prevail over any other factor. The impression a person is able to convey in a specific situation outweighs his or her proven skills or achievements. When it came to acting on the stage of impression management, the institute indeed proved to be an expert player. Allusions to the prominent role the

scholars had had in pre-Hitlerian Germany and to the impressive, if somewhat nebulous, list of the institute's previous achievements in the field of social research were garnished with the names of prominent Americans who had supported their cause and now acted either out of solidarity with academics driven into exile or had themselves fallen prey to the critical theorists' impression management. All this was accompanied by emphatic demonstrations of economic independence which, due to the severe losses suffered by the various foundations on the stock market, was far from what it had been during the institute's first years of exile, it is true, but was nevertheless presented with an aplomb that would have done honour to a con man (Conwell and Sutherland 1937). (The resemblance of the manner adopted by the institute's negotiators with that of con men, who inveigle their victims into believing that they should be grateful even to have the opportunity to help the con men out of a momentary financial fix, is evoked quite deliberately in this context – a financial history of the institute would provide evidence for this down to the very details of book-keeping.) Of course, acting in this manner posed no difficulties for these sons of the German bourgeoisie. After all, Horkheimer and Pollock expertly knew how to maintain the impression, for their American interlocutors, that their sole desire was a truly altruistic one – namely, to ensure the continuation of fruitful work for their collaborators – rather than for their own private luxury, although moderation in their luxury was the last thing they would consider. While the institute complained to everyone who cared to listen that they would soon have to close up shop, Horkheimer was having a house built for himself in Pacific Palisades that well-befitted his rank and at the same time saw to it that Herbert Marcuse's and other collaborators' wages were cut.

Another factor that may have weighed in on their success with the AJC officers was that the great majority of the latter had either come to the United States as the young children of parents who had emigrated from Eastern Europe to the United States, or had been born in the United States shortly after their parent's immigration. Denigrated – also by members of the institute – as 'Eastern Jews' (i.e. Jews from Russia and Russian Poland), they were no match in terms of endowment with cultural capital for these sons of long-established German-Jewish families, notwithstanding their social advancement which in most cases was a result of their having graduated from a state college such as the well-known City College in New York.<sup>21</sup>

Finally, another reason for the AJC – no different in this regard from other Jewish organizations – to be set on establishing a research department of its own and on putting its trust in social research may have been that during the years right before and after the entry of the United States into the Second World War virtually every self-respecting public and private organization was doing just this. The prestige of the social sciences arguably has never been higher than in those years. The successful election forecasts of the opinion research institutes contributed to this as much as the research done by the Chicago School of Sociology, the incipient industrial and work sociology of the



Human Relations School, and – more to the point for Jewish organizations – the orchestrated success of the study directed by Gunnar Myrdal on racial discrimination in the United States.<sup>22</sup>

In mid-June, 1944, the new AJC Executive Vice-President, John Slawson, unwilling to reject Pollock's and Horkheimer's argument that in the first year of their 'collaboration' the institute had spent more money on the Anti-Semitism Project than the AJC, granted a preliminary \$2,500 (\$30,000 in 2010) to enable them to go on with their work.

### A NEW RESEARCH DIRECTOR AT THE AJC

John Slawson, Horkheimer's junior by one year, was six years old when he came from the Ukraine to the United States and, like many other new immigrants, had climbed the social steps that enabled him to graduate from Columbia University as a psychologist in 1927. He then started working for various Jewish organizations, and in 1943 became the AJC's Executive Vice-President. The AJC was not the only Jewish self-defence organization, and it was reputed to be not only the most conservative of them, but also the one where German Jews were in charge. The ascent of 'Eastern Jew' Slawson, however, shows that there were exceptions. In the spring of 1944, at his suggestion, a group of scholars met with officers of Jewish organizations in New York to discuss the AJC's future activities in the field of research on, and in the fight against, anti-Semitism. The Institute of Social Research was represented by Horkheimer and Adorno. Other participants in the two-day meeting were Gordon Allport, who in the next decade became the uncontested authority for social-psychological research on prejudice (Allport 1997); Talcott Parsons, who at the time was deeply engaged in projects doing research on Nazism (Gerhardt 1993; Gerhardt 2002); as well as John Dollard, Edward Bribing, Kurt Lewin and Rensis Likert, among others.<sup>23</sup> A similar conference, although not initiated by the AJC, was held soon afterwards on the West Coast, with most of its participants being exiled psychologists and psychoanalysts (Simmel 1993).

Horkheimer had the opportunity to quickly establish a relationship with Slawson that was favourable for his own interests as well as those of the institute. In June 1944, Horkheimer addressed several letters to Slawson that abounded in flattering allusions to their elective intellectual affinities. He tried to talk Slawson out of his desire to see the final report on the work of the first year and, at the same time, to persuade him to found a new institute:

If science is really to be mobilized against the menace we face, a research organization should be created with a minimum annual budget of at least \$ 100,000.-. For this purpose we could use either an already established institute like ours, which is recognized as a tax exempt educational organization, or a new set-up like the Institute for Research in Social Prejudice, as conceived by one of the friends of the Committee.<sup>24</sup>

A few days later Horkheimer was invited to join the Scientific Department of the AJC as a 'research consultant in domestic defense'. This, however, was too fast a move for Horkheimer, and he once more tried to gain time before making his choice. In the end, he accepted the offer and on 1 November 1944 took up his new function as an employee of the AJC, eleven-and-a-half years after he had lost his position as a university professor at the University of Frankfurt. The document that records Horkheimer's appointment to the AJC states that 'Doctor Horkheimer and his staff of scientists will conduct their experiments, tests and surveys under the supervision of Dr John Slawson.'<sup>25</sup>

In a letter to Adorno, Horkheimer described his working day as follows:

Most of the time, I am sitting in a rather nice office of the Committee reading some of the myriads of brochures that have been scattered all over the world in the course of the last year. I share the room with a secretary who is charged with taking my dictations and in her free time works for Dr. S[lawson]. This free time takes up most of her day since I very rarely dictate anything. My main activity is to attend meetings and have talks with individual members of the staff to whom I politely point out my opinion on the myriads. My negotiations in view of having an assistant have, for the time being, led to the part-time employment of one Dr. Kornhauser whom I got to know via Lazarsfeld. He has an excellent reputation as a public opinion expert. He is to work with me as of December 1st. During the first months we plan to elaborate a maximum research program and already initiate a number of studies on the effects of the Committee production. Furthermore, Hertha Lazarsfeld [i.e. Herta Herzog] has agreed to devote her evenings and her Saturdays to the testing of radio programs.<sup>26</sup>

Arthur Kornhauser was the Chicago professor of psychology who had been Paul Lazarsfeld's co-author for the latter's first article on psychological market research after his transfer to the United States, a cooperation that continued in subsequent years. That Herta Herzog had only the evenings and the weekends to dispose of had to do with her job at the advertising and marketing firm of McCann-Erickson. The recruitment of these two collaborators strongly suggests that Horkheimer intended to make use of the 'like-dislike studies' so abhorred by Adorno in order to investigate the effects of the brochures and the radio programmes designed by the AJC.

Horkheimer's AJC activities predominantly consisted of the tasks that are usually assigned to the director of the social science research department of an organization worried about the effectiveness of its products. That the outfit in question was a not-for-profit organization such as the AJC did not mean that there was no need to optimize the resources involved and no need to have a research department of their own to do just this by testing the effectiveness of their 'products' before they were released. Horkheimer's own account to the Committee on Scientific Research of the AJC, which was something like a sub-committee of the board that was supervising the new department, says so quite unequivocally:

(1) *The testing of the Committee's projects:* Pamphlets, speeches, broadcasts etc. are to be tested in such a way that we shall endeavour to ascertain whether they are good or bad in their practical effect, and how they can be improved.

(2) *Group research*: Various strata of the population, such as labor or war veterans, or the Irish Catholic group, are to be studied by trained members of the group to interview their colleagues and neighbours, to discuss the results with our field workers and then to test out certain counter-measures.

(3) *Memoranda on practical questions*: The department should prepare theoretical analyses on important subjects with which the Committee is concerned ... for example, of ... a possible Jewish state in Palestine ... The foregoing projects should produce results within a year or less.<sup>27</sup>

More time would be needed to push on with the two other tasks: sociological and psychological studies designed to identify prejudiced and non-prejudiced individuals and to submit them to follow-up interviews and detailed tests to find out how propaganda could be improved. This also included preparing and testing 'new weapons' for this purpose. The self-surrender of the critical theorist to the practices of administrative research went even further than this, since Horkheimer informed Adorno right after having entered into office that he considered, 'rounding up a group of psychologists and sociologists, to meet once a month and discuss our ongoing work ... I am thinking of four or five analysts, plus Lazarsfeld, Merton and one or two others from the faculty' of Columbia University.<sup>28</sup>

He repeatedly presented this proposition to his superiors together with complaints about his lack of collaborators and the need to change this situation straight away. As a matter of fact, the department rose to a respectable size, and its collaborators included not only social scientists who would later become quite renowned, such as Marie Jahoda and Alvin Gouldner, but also, and primarily, junior American researchers such as Eunice Cooper, Genevieve Knupfer, Helen Schneider and Marion Schneider, who had undertaken their practical social science training with Lazarsfeld at the Office of Radio Research.<sup>29</sup> Besides Horkheimer, or his later placeholder, the associate director Samuel H. Flowerman, at least three other social scientists were working at the department at any given time. Their work consisted mainly in supervising the major projects approved by the AJC and in organizing the operations of minor research assignments which, however, were usually conducted 'outside'. In the first years, Lazarsfeld's BASR served as the department's test station. In a detailed memorandum submitted to Slawson in 1947 to justify his demands for pay raises, Horkheimer also gave a retrospective account of the beginnings of his activities in New York:

It was the Department's task to integrate the outside studies into a concerted effort of various teams. This required the exchange of staff members between New York and the projects, as well as trips of some of our experts. I was the more inclined to advocate this policy as it would have enabled us to gradually transfer certain parts of the projects to the Bureau of Applied Social Research which is the leading agency in some important methods of our research. Even now I consider the intensive personal cooperation between the Department, the various projects, Dr. Lazarsfeld and myself one of the most pertinent presuppositions for the success of our work.<sup>30</sup>

Horkheimer's role was more similar to that of a department head of, for instance, the Rockefeller Foundation than to Lazarsfeld's role in the BASR. Unlike the latter, there was no need for him to bother with fundraising and with acquiring commercial commissions in order to be able to employ a troop of collaborators. As a department head, Horkheimer had to act within a battlefield consisting of the AJC boards, which were his superiors (he was expected to report to them on proposals that were submitted for approval, and to receive instructions by them); his collaborators in the department; and the external projects in Berkeley, Chicago and New York City. Unlike Lazarsfeld, Horkheimer was not allowed to make strategy decisions. He was a senior executive who could try to impose certain preferences of his own within his organization. While Lazarsfeld's role was more like that of an entrepreneur, Horkheimer's status resembled that of a member of a bureaucratic structure with its familiar command lines and office intrigues. Both, however, had a material safety net. For the former, this was a position, albeit poorly paid, as an associate professor at Columbia University; for the latter it was his access to foundation funds (dependent on the capital market) on which he could draw to have himself rather liberally supported.

Now, nothing would be more misleading than to conclude from all this that Horkheimer had changed sides, so to speak, and had agreed to be a science bureaucrat. His letters to Adorno at the time clearly show that he was far from enjoying his new task. But why, then, had he agreed to return to New York, sharing an office with a secretary and doing work he detested? Two reasons suggest themselves: negotiating with the AJC, he had failed to win them over for his plans, i.e. to have them fund the institute as an independent contractor, but he had, at the same time, so strongly insisted on his commitment to seeing this work through that he could not very well refuse the offer to take up the position of a research consultant without forfeiting his credibility. Here again, it is instructive to compare his situation with that of Lazarsfeld who, in 1937, as the head of the fragile Newark Center, was offered a transfer to the well-endowed Princeton Radio Research Project. Lazarsfeld, as shown in the preceding chapter, had from the very start of the negotiations sought to maintain his institutional independence whereas Horkheimer chose to try to temporarily secure financial support for his institute by accepting the position of an AJC employee.

Second, during the one year of the institute's more or less autonomous work on its own Anti-Semitism Project, Horkheimer must have realized that, all in all, the result would not really be up to scratch. Most of the collaborators of the years 1943–4 were either no longer available, including former collaborators Otto Kirchheimer, Neumann and Marcuse, who had been relegated to Washington, DC or had turned out to be incapable of going beyond the conventional analyses conducted in the humanities and in history, such as Gurland, and Massing and Heinz Langerhans. The strategy of reducing the New York branch to a mere organizational shell, which Horkheimer had pursued independently from his relations to the AJC, had now produced an

unwanted side-effect: there were no collaborators whom Horkheimer, Pollock and Adorno, who was steadily drawing closer to the hub of the circle, thought capable of conducting the kind of research that would help the institute to be appreciated by the Americans.<sup>31</sup> Even though they more or less looked down on the American social scientists, they were well aware that for reputation's sake they had to produce (or have others produce) something that met the prevailing expectations and standards.

As a result of this constellation, Horkheimer found himself in a dual role that was novel and unusual for him. Formally, he was a medium-level employee of the AJC with superiors and inferiors and a more or less clearly stated work assignment that included treating the Institute of Social Research as a contractor. His heart, however, was still almost exclusively in the institute whose director he remained, which provided him with an additional income, and whose positioning and material safety he tried to promote. He and his colleagues of the institute's inner circle were no doubt much more aware of this position as a broker than his superiors and collaborators at the department.

The institute's grandiose promises of the preceding years could only be kept if it reverted to its previous policy, namely, hiring external experts to conduct and analyse empirical studies, as it had done with Lazarsfeld for the empirical part of the *Studien über Autorität und Familie* (Authority and the Family) (Horkheimer 1936) and with Mirra Komarovsky for the institute's only extensive American publication thus far, *The Unemployed Man and his Family* (Komarovsky 1940). But in 1944, hiring low-cost high-competence staff was not that easy anymore, as the experiences that had led Lazarsfeld to suggest that the Rockefeller Foundation suspend the Radio Research Project until the end of the war had clearly shown. The institute's own resources no longer sufficed to support the only asset they still had in the United States market: the Berkeley Public Opinion Study, the group around Nevitt Sanford, Daniel Levinson and Else Frenkel-Brunswik, who had written a voluminous 1,000-page text that had aroused greatest interest at the AJC. The institute had obtained this contribution for very little money (less than 10 per cent of the money paid by the institute was dedicated to Sanford's group, see Table 6.3). Unless Horkheimer was prepared to let go of this chance to succeed on the American academic market, he had to swallow the pill and try to use, at least temporarily, his own position at the AJC headquarters to make sure that things went the way towards the consolidation of the institute's position between the AJC and the Berkeley group.<sup>32</sup>

The Berkeley group which, like Lazarsfeld, adhered to an epistemological orientation that was contrary to the anti-positivism of critical theory, seems to have been as inexperienced as they were disinterested in anything that had to do with research organization. Anti-Semitism as a research concern had not come to Sanford of his own initiative. In the spring of 1943, one Mr Blumenthau, a New York-based Jewish proprietor of a theatre, had donated the sum of \$500 (\$6,100 in 2010) to the University of Berkeley to enable it to conduct a study

on anti-Semitism (Levinson 1992). The university's administration approached the psychology department, which charged Sanford – still an associate professor – with the project, who in turn hired graduate student Daniel Levinson as his collaborator. Since the donation alone did not cover the costs incurred, the psychology department added to it from its own funds. This tiny project, the goal of which (as defined by the donor and remembered by Levinson) was to do away with anti-Semitism once and for all ('a rather grandiose idea'), resulted in the publication of the first anti-Semitism scale (Levinson and Sanford 1944). Both authors as well as other Berkeley psychologists felt that it would be interesting to go on working on the issue and, thus, came into contact with Horkheimer. However, the Berkeley group was so far away from the New York centre with its foundation headquarters and generous Jewish organizations that Horkheimer found it possible to interpose himself and the institute as an intermediary and to prevent Sanford and Slawson from communicating directly. The Berkeley group wanted to continue the psychological studies they felt to be promising. They needed some money for this, and the institute held it out to them. As long as the funds were coming in, the Berkeley psychologists could not care less about who acted as an intermediary.

Finally, Horkheimer used his stay in New York, which he kept considering to be temporary (in a letter to Adorno he refers to the 'nine months that we expect this to last'), to reactivate his contacts with Lazarsfeld and other social scientists, and exploit their expertise. Given that Lazarsfeld and his colleagues at the university, as well as the psychoanalysts Horkheimer intended to consult, were more than occupied with their own work, neither of them would interfere with the institute's plans: 'When I get back [to California], however, everything and not only the Committee issues should be ordered to the point of not having to bother about anything but our most immediate concerns. Thinking of this time gives me the courage in this horrible bustle and all the false talk and false actions to which it compels me.'

In the end, the nine months became many years of work for the AJC. Although after more than a year Horkheimer retreated to California, allegedly for reasons of health, he remained on the AJC pay roll until his return to Frankfurt in 1949. At first he retained a still princely, if reduced, yearly income of \$6,000 (\$70,000 in 2010)<sup>33</sup> and, after that, as a 'West Coast Consultant' with a meagre support of \$1,000 (\$11,600 in 2010).<sup>34</sup> And, what was most important, he remained in the AJC Scientific Department's corridors of power. His collaborators and superiors in New York were made to believe that his absence was merely temporary and that, once his health was restored, he would return to New York. Actually, he returned to New York only as a visitor, since there was always some doctor ready to certify that for health reasons it would be highly unwise for him to return to the East Coast metropolis.<sup>35</sup> During his absence, the associate director, Samuel H. Flowerman, and the permanent secretary of the department, Marie Jahoda, both of whom Horkheimer had selected before his departure, organized the work to the satisfaction of all

concerned, and regularly asked about his health as well as for his suggestions in research matters. They continued to pass off his absence as a temporary vacation. After a move by a number of AJC officers to have him dismissed had failed, his position was formally downsized to that of a Chief Research Consultant for the AJC: 'finally ... it (should) be made clear to Dr. Horkheimer that he does not have the authority to initiate new projects without the approval of the New York staff and the Committee on Scientific Research'.<sup>36</sup> What Horkheimer was deprived of was a competence which, after one year, he was not much interested in anymore. Initiating further projects probably was the very last thing Horkheimer wanted to do. Securing those that existed was something he could do from Pacific Palisades.

By the turn of the year 1944–5, the first Anti-Semitism Project had been definitely buried. In the following months and years, only four of the various studies that had initially been envisaged were continued, often in a thoroughly revised form. Paul Massing, drawing on the various manuscripts on the history of anti-Semitism in Germany, took on the task of writing at least one volume on anti-Semitism in Wilhelmine Germany for the *Studies in Prejudice*. And at the end of 1940 Leo Löwenthal, who in the meantime had worked for Lazarsfeld's Bureau of Applied Social Research and the Office of Strategic Service,<sup>37</sup> began to compile, in cooperation with Norbert Guterman, some of the content analyses of the speeches given by anti-Semitic agitators, which made up another volume of this series. Both books were published in 1949 as the first two volumes of the *Studies in Prejudice*.

The third study to be continued was an investigation of anti-Semitism among American blue-collar workers for which the institute, thanks to Gurland's commitment and Daniel Bell's help, had meanwhile found a different sponsor, the Jewish Labor Committee.<sup>38</sup> For Gurland, this was a way to secure his job at the institute, while Bell, who in the years before had been one of the young Americans who copy-edited the texts of the members of the institute, was interested in helping the institute. Lazarsfeld was soon enlisted as an expert to help conduct the study. Although a final report was – after many delays for various reasons – completed, the typescript of the report has never been published.

The fourth study was TAP (Adorno *et al.* 1950). Here, the original typology eventually found its home. The two other volumes of the *Studies in Prejudice* had not even been conceived before Horkheimer took up his work at the AJC. Bruno Bettelheim and Morris Janowitz's study on anti-Semitism among war veterans, initially intended as a parallel to the Berkeley study, soon evolved to be a study in its own right (Bettelheim and Janowitz 1950) – a development that may have benefited from the fact that Horkheimer had quickly lost interest in exercising any influence on its content. A key actor in this transformation of the initial Anti-Semitism Project into the five-volume series of the *Studies in Prejudice* was the Advisory Council referred to in the very letter by Horkheimer to Adorno that has already been quoted above.

## AN ADVISORY COUNCIL – FROM CONSULTATION TO RESIGNATION

In the first half of the year 1945, the Advisory Council met once every month on a Saturday afternoon. It initially consisted of eight members, but was later enlarged to include a number of additional members and, in certain cases, special guests. Advisors were paid \$50 (\$590 in 2010) for each meeting they attended. The core group, that is, all those who attended every meeting, consisted of Horkheimer as the chairman; Nathan W. Ackerman, a New York psychoanalyst; Vienna-born psychoanalyst Ernst Kris, who at the time was participating in a large-scale project on German propaganda that was financed by the Rockefeller Foundation; the two Columbia sociologists Lazarsfeld and Merton; psychologist Gardner Murphy, also from Columbia; and anthropologist Margaret Mead. The second meeting already included Herta Herzog and social psychologist Solomon Asch, who at the time was affiliated with the New School for Social Research in New York. Otto Klineberg and Rudolphe Lowenstein were among those members who attended irregularly. Among the guests, Leo Löwenthal was the most regular one, but Frederick Pollock also frequently attended the meetings, whereas Adorno, Siegfried Kracauer, Gregory Bateson, Bruno Bettelheim, Bernard Berelson, C.W. Mills and a number of others appeared only once.

At the first meeting of the 'Advisory Council to Dr Horkheimer', as it is officially called in the minutes, Horkheimer explained the state of affairs and what the advisors were expected to do. The success of his activity, he said, would depend on whether he succeeded in getting the necessary support and whether the projects conducted by the social scientists of the AJC would meet with approval by their peers. Kurt Lewin had accepted a similar function at the American Jewish Congress and intended to commit himself to the study of group dynamics. Horkheimer, in contrast, declared himself to be more interested in the study of 'individual attitudes'.<sup>39</sup> The work plans of the new department would be submitted for discussion to the Advisory Council.

Surprisingly, the first project to be discussed was Horkheimer's hobby horse of many years, the experimental film. He once again explained the design of the study and the film which, as a result of discussions with Siegfried Kracauer and Hans Richter, had been modified. It now featured a white Christian American, a Jew, a Black and a Briton, who were involved in an accident that an unprejudiced spectator might construe as the wilful wounding of a handicapped person. The film was to be shown to different groups of spectators. Follow-up interviews would be conducted to find out whether the four types were judged differently. Horkheimer then wanted to immediately discuss the questions to be asked after the film had been shown, but was stopped by Merton and Murphy who felt that they should first come to a clear definition of what exactly was to be measured. Was it to be the intensity of the prejudices held towards various ethnic groups or the occurrence of prejudice in specific situations? Merton felt the second option to be more promising since it allowed them to observe which persons responded by latent anti-Semitism in which of the situations represented. Kris pointed out that another of



Horkheimer's ideas was no less problematic, since he wanted to interview the spectators under the pretext that he was interested in the reliability of their testimony. It should be admitted, however, that people's responses tended to be different according to whether they were in the role of a witness or in the role of a spectator.

Horkheimer then wanted to discuss the design of the questionnaire that was to be used. Its first question would be, 'How did it really happen?' followed by, 'Is the Jew really guilty?' The others, however, rather than deal with these technicalities, raised further fundamental questions. Thus, Mead worried that the film experiment itself might contribute to the deepening of prejudice. No Jew should be shown attacking a one-legged person. Murphy doubted whether it made sense to lay open the actors' moral misconduct in order to find out something about prejudice in general. With each new contribution to the discussion, the number of influences to be considered increased. While Horkheimer again tried to get advice on what this film could measure, Lazarsfeld, who had definitely taken to the idea, suggested that the film should be used for exploratory purposes and not for obtaining easy results such as whether anti-Semitism was stronger among blue-collar workers than among the middle classes, or on the West Coast than on the East Coast. Finally, Merton advocated pre-testing the film to gain more insight into all these questions. At the time, the BASR was conducting studies for the United States Department of Defense to investigate the impact of films, using both the Program Analyzer and focused interviews.

The second part of the meeting was devoted to a discussion of the current state, and further outlook, of the Berkeley group studies. Sanford and Levinson had so far used questionnaires to interview students on ethnocentrism, chauvinism and other kinds of prejudice, and had then done case studies for an in-depth investigation of the extreme groups. For this, the Rorschach test and the Thematic Apperception Test (TAT) had been used, and Horkheimer wanted his advisors to tell him if these were really the best tests available. Murphy suggested the additional use of story-completion tests such as those employed by Frederic Charles Bartlett.

Other ongoing projects were a survey of young children who could not yet have any prejudices at all, and a study on the stereotypes of fascist agitators. For the latter, Horkheimer raised the question of whether it made sense to create a stereotype of an agitator that would make people laugh. Thereupon, Mead firmly objected to anything that could create enmities, while Lazarsfeld found the idea quite appealing, which earned him a rebuke by Mead. Merton argued that what mattered for the impact of propaganda and for people's adherence to prejudice was never just conformity with people's beliefs, but also group affiliation and the adherence to, or refusal of, beliefs by their reference groups. If the leader of a group that propagated crass forms of prejudice was accidentally or intentionally eliminated, the prejudices held by the group often enough disappeared with him. Merton knew what he was talking about since at the time he was also acting as an advisor to Stouffer's study,

*The American Soldier*, which later induced him to write his well-known works on reference groups.

At subsequent meetings, some members of the Advisory Council who misinterpreted the role they had been assigned also presented research projects they thought worth pursuing. This was not quite what Horkheimer had hoped to get from his advisors, but in the end, the *Studies in Prejudice* came to benefit from this, since at least one of its volumes originated in a proposal that had been submitted to the Advisory Council. This was one of two projects proposed by Nathan Ackerman, i.e. a study on anti-Semitic prejudice among the clients of psychoanalysts. With Jahoda as a co-author, Ackerman published this study in 1950 under the title of *Anti-Semitism and Emotional Disorder*.

The Advisory Council indeed discussed the film experiment at another of their meetings, but Horkheimer pushed for it mainly within the AJC. Screenwriters were recruited and potential directors discussed, but in the end it was never realized.<sup>40</sup> It passed away as peacefully as the 'Handbook of Fascist Agitators' and a number of other institute projects that had at some point simply been forgotten by their protagonists.<sup>41</sup> By contrast, cooperation with third parties made for continuity in the institute's research projects. Besides the Chicago Study by Janowitz, Bettelheim and Edward Shils, this was especially true for the work of the Berkeley group, which managed to remain unfazed by Horkheimer's changeable moods. The documents kept in the AJC Archive include many project proposals that, while they almost always had Horkheimer's support, ultimately came to nothing. What Horkheimer actually had in mind, and consistently discussed in the Advisory Council over a considerable period, came down to the idea of being able to publish a definitive work that 'might become as influential as Gunnar Myrdal's work "The American Dilemma"'.<sup>42</sup> What this was to be in terms of content, however, changed with each meeting, as did its estimated costs.<sup>43</sup> For a certain time, Horkheimer considered writing, with Robert MacIver and Gordon Allport, 'a standard book based on our present knowledge of the nature, motivation and extent of prejudice in general and antisemitism in particular',<sup>44</sup> but he soon lost interest in this, as well. Allport, who proved to be more persistent, published just this definitive work on prejudice, i.e. *The Nature of Prejudice*, in 1954, for which, however, he had not needed Horkheimer's help (Allport 1997).

In early June 1945, the Advisory Council held its last meeting before its summer recess, and was not called back until after Horkheimer's relocation to Los Angeles. The members of the Scientific Advisory Council, as it was now called, unanimously declared 'that their service had not been sufficiently utilized and (they) would welcome a reorganization that gave them a more active part in the work of the Department'.<sup>45</sup> An agreement was reached, stating that it was unreasonable to go on operating in the same style as before and that members should be better informed on what the department intended to do in order to enable them to be more specific in their suggestions

for improvement. Until Jahoda's resignation, the Advisory Council functioned in an individualized form, i.e. its former members were called upon as consultants.<sup>46</sup>

After Horkheimer's departure from New York, some of the collaborators he had left behind suddenly seem to have seen his work style in a different light. After some hesitation, Marie Jahoda, who was his assistant at the time and felt bound to loyalty to him for more than one reason, since Horkheimer's efforts had helped to free her from detention in Vienna in 1937, finally gathered the courage to write a letter voicing her discontent with the man who had so hastily retreated back West. Due to Flowerman's administrative skills, she wrote, the department now was a 'proper and orderly working outfit', but Horkheimer's actions of the previous week had more than once roused her feelings to the point of 'committing an illoyalty to you'. Since his departure, these feelings had even intensified. Her impression was that Horkheimer did not employ her in a way that corresponded to her capabilities. Her admiration for him as a philosopher was unbroken, but his competences as a methodologist had raised certain doubts in her. Before going into the details of her criticism, taking the experimental film as an example, she wrote a sentence that made Horkheimer explode: 'The movie was a brilliant idea that took everybody in when you explained it, using all the nice tricks of your personality that ... could make you get away with murder'.<sup>47</sup>

Jahoda's message was clear: Horkheimer's skills in convincing others of an idea largely exceeded his skills in implementing this idea. This, however, was something he refused to listen to, and even less so when it came from a woman who was his subordinate. His response, seven days later, was a proper dressing-down:

Our difference in scientific matters is as evident as the difference between Hegelian logic and an orderly working outfit cleansed of philosophical sloppiness ... It is downright inadmissible, however, for you to lecture me and, in doing so, not only to deliberately conform to the situation, long since denounced by more far-sighted persons, of the modern sociological employee who has to limit himself to pseudo-exact paraphernalia if he wants to avoid being dismissed by his client, but also to pass it off, for yourself as well as for me, as intellectual integrity, responsibility, and incorruptibility. You will not expect me to engage in a controversy over this (my inability of realizing my own research intentions). I refrain from humiliating you by drawing your attention to a number of reflections which, if you would take the time for it, you will undoubtedly come upon by yourself.<sup>48</sup>

One might be content to point out that it was Jahoda who, in the end, was borne out by history, since it is well known that the experimental film never saw the light of day. But aside from the mere facts, the actions that followed upon this correspondence are highly significant in a quite different respect. Horkheimer responded to the well-intentioned criticism not only with all the arrogance he commanded but also instructed others how to act in this matter. Before sending his letter to Jahoda, he conferred with his intimate friend

Pollock and, having sent it, he instructed Pollock via Löwenthal how further to deal with Jahoda, namely, to prevent her from resigning. The whole affair not only shows that Horkheimer devoted a sizeable part of his life time to the art of high-level office intrigue but also that he was simply incapable of assessing his own competencies. Pollock and Löwenthal, who probably did not stop one second to reflect on what Jahoda may have meant, presently agreed with Horkheimer that Jahoda was a case of 'masochist rigidity'.<sup>49</sup>

There is more than one case of an author whose fame would have remained unblemished but for the papers he left behind, papers that so harshly disavow him and the image one expects to find, or constructs, on reading his works. Adorno and Horkheimer doubtlessly belong to this category. While the former seems to have anticipated this and, therefore, stipulated that access to his legacy be blocked until fifty years after his widow's death, Horkheimer seems not to have harboured any such doubts. Access to the documents in the Max Horkheimer Archive has been free for many a long year, and with the publication of his *Gesammelte Schriften* (Collected Writings), all the embarrassing details are there for everyone to read. Here, the art of networking in terms of arrangements and intrigues, exercised on an almost daily basis, and the contemptuous judgements of others that verge on social denunciation, are bound up with a specific attitude of grandeur and an almost paranoid suspicion of everyone and everything. After the Holocaust, all this coalesced into a habitus of persecution that made Horkheimer all but unfit to cope with reality, as he was no longer able to assess the true dimensions of the danger. When, around 1960, anti-Semitic graffiti began to spring up in Germany, Horkheimer panicked. For several months he desperately tried to re-obtain the United States citizenship he had lost (there was a ruling at the time that deprived anybody of his or her US citizenship who was not US-born and had spent too long a time living outside the United States). In order to be re-granted US citizenship, he wrote letter after letter describing his activities after his return to Frankfurt as if he had been a member of the United States occupation army.

While Horkheimer's reaction in 1960 may after all be interpreted in terms of a post-traumatic syndrome, the same cannot be said of a similar reaction shown by Adorno some time before. In 1954, Richard Christie and Marie Jahoda were co-editing a volume dedicated to a critical review of TAP, to be published as part of the series *Continuities in Social Research*. Jahoda sent the manuscripts of the contributions to Adorno and Horkheimer, asking for their opinion. One of these contributions was a shock for Adorno. Edward Shils, a long-term collaborator of the *Studies in Prejudice* that in the end were published solely under the names of Bettelheim and Janowitz (Bettelheim and Janowitz 1950), attacked TAP because of its failure to include leftwing authoritarianism (Shils 1981). This criticism was as justified as it was unfair, since when TAP had been in the making, i.e. between 1944 and 1947, focusing on rightwing anti-Semitic anti-democrats indeed made sense. In 1953, denouncing the failure of TAP to discuss leftwing authoritarianism could easily be construed as a gesture of submissiveness on the part of the critic to the

zeitgeist of the so-called McCarthy era – and was arguably seen as such by a majority of readers. Adorno responded to this criticism by sending a number of drafts to Jahoda in her capacity of an editor, proposing changes to be made in her introduction and in Shils's text. Jahoda integrated these changes in an attenuated form, while Shils refused to have his text interfered with in any way.<sup>50</sup> Adorno, who was staying in California at the time, wrote to Horkheimer that he had the 'definite feeling that I should be out [of the United States] before the book edited by charming Mizzi is published' (quoted in Wiggershaus 1986: 518) – as if the book edited by Christie and Jahoda was a testimony given before the House Un-American Activities Committee rather than a contribution to academic social science discourse which, of course, was no field of interest for the McCarthyites.

## THE ANTI-SEMITISM PROJECT, PART II

At the end of 1945, the large-scale anti-Semitism project that the institute had pursued since 1940 had definitely collapsed. Thanks to Horkheimer's one-year presence in New York and Adorno's insistent letter writing, prospects for the Berkeley project were not bad at all. In the spring of 1945, it had been granted \$17,500 (\$206,000 in 2010) to be spread over two years, by the AJC, and thus disposed of as much money as the initial Anti-Semitism Project with its eighty-seven sub-studies. And this was only the beginning of their financial support. By the end of the project, the AJC had invested almost \$170,000 (\$1.5 million in 2010) in the *Studies on Prejudice*, \$52,000 (\$458,000 in 2010) of which had been spent on TAP.<sup>51</sup>

R. Nevitt Sanford, born in 1908, and Daniel Levinson, born in 1920, were affiliated with the psychology department of the University of California. So was Galicia-born Else Frenkel-Brunswik, born in 1909 and a student of the Böhlers. However, due to the so-called Anti-Nepotism Regulations that were in force at California universities at the time, and to the fact that her husband, Egon Brunswik, had obtained a regular appointment at the department when his Rockefeller fellowship was terminated, her position was only that of a research assistant. These three were not only interested in psychoanalysis but had undergone psychoanalysis themselves. Back in Vienna, Else Frenkel-Brunswik's analyst had been Ernst Kris, but the negative attitude of her teachers, the Bühler couple, to psychoanalysis had at that time prevented her from pursuing this line of research. Berkeley, by contrast, was much more favourably disposed towards psychoanalysis, as can be deduced from the very fact that Erik Erikson was at the time affiliated with the Psychology Department (Friedman 1999). It was no doubt their common concern with psychoanalysis that brought the three of them together as well as into contact with professional psychoanalysts, among them many emigrants such as Siegfried Bernfeld and Otto Fenichel. Via German emigrant psychoanalyst Ernst Simmel they came into contact with Horkheimer who was probably initially more impressed by the evidence of their affinities to psychoanalysis than by any specific

psychological competences the three of them might possess. For Frenkel, due to her Vienna studies, these competences were in the field of Karl Bühler's 'Denkpsychologie' (cognitive psychology/psychology of thinking) and, primarily, in child psychology and psychological life course research, to which Frenkel had contributed in a leading role under the direction of Charlotte Bühler. Also due to her Vienna activities, Frenkel-Brunswik was profoundly interested in the philosophy of the Vienna Circle which she felt to be superior to psychoanalysis on a theoretical level and which she later relied upon to reconstruct Freudian theory in terms of the philosophy of science (Paier 1996). During the first years of her stay in California, Frenkel-Brunswik worked on a large study on *Motivation and Behaviour* (Frenkel-Brunswik 1942)<sup>52</sup> with the aim of elucidating the link between drive structure and behavioural manifestations. In the academic year of 1942/3, a fellowship of the Social Science Research Council allowed her to benefit from an extensive study stay in Chicago, where she worked with Chicago-based anthropologist Robert Redfield and sociologist Herbert Blumer, as well as in Harvard where she was in contact with psychologists such as Henry A. Murray, the inventor of the Thematic Apperception Tests (TAT), and anthropologists such as Clyde Kluckhohn (Paier 1996: 36–7). During her Harvard stay, Frenkel-Brunswik came into contact with the emerging 'Culture and Personality Approach' which she subsequently enhanced in her discussions with anthropologist Alfred Kroeber in Berkeley. The small anti-Semitism study thus easily fitted in the Berkeley research programme, and after Frenkel's return from her fellowship, the Sanford, Levinson and Frenkel-Brunswik decided to go on working on this issue as the Berkeley Public Opinion Study group.

Sanford's works pertained to the field of the psychology of personality. Together with his student Levinson he worked on a scale for assessing anti-Semitic attitudes, giving precedence to his junior partner when it came to the names under which the scale was published (Levinson and Sanford 1944).

While this scale was indeed the first of those that would be used in TAP, it was by no means a scientific innovation. Since Louis L. Thurstone's article 'Attitudes can be measured' (Thurstone 1928), published in the *American Journal of Sociology*, and a monograph with a similar title published the following year (Thurstone and Chave 1929), a technique of attitude measurement existed that was based on scaling. Rensis Likert (Likert 1932; cf. Allport 1968–9) then contributed the variation that has been named after him, differing from Thurstone's in that respondents were required to indicate grades of approval or refusal, which proved to be primarily advantageous for statistical analysis. The theoretical concept of attitude that was a precondition for this type of procedure had been formulated most stringently by Thomas and Znaniecki, who defined it as the 'state of mind of the individual toward a value' (Thomas and Znaniecki 1918–20: 21). In the decade between Thomas and Znaniecki's and Thurstone's publications, the empirical recording of attitudes evolved from a technique that was applied more or less intuitively to an elaborate research routine. The two sociologists had addressed each of these

'attitudes' as an isolated factor, but soon scales were developed where the researchers themselves defined the intervals between items. The most famous of these scales was Bogardus's 'social distance scale' (Bogardus 1925) where respondents had to report on the degree of proximity they would tolerate for the members of an alien social group: from refusing them entry into the country to working at the same establishment or having the same occupation, to being a member of the same association, to letting them marry a close relative. These *a priori* scales – called thus because the researchers were the only ones to define the intervals between items – were followed by Thurstone's scales where items were ordered according to the judgements of a maximum number of 'judges'. These 'judges' were asked to order a very large number of opinions on a specific issue that had been collected from newspapers, books, opinion polls, etc. Respondents were then presented with the resulting scales and were asked to indicate the statements with which they agreed or not. In the 1940s, these scales were further adapted for various social groups, mostly by way of a pre-test conducted with a small sample of the group to be studied. In contrast to their predecessors, the Thurstone scales provided an adequate basis for the more sophisticated statistical methods that were to be used for analysing measurement results.<sup>53</sup> The Guttman scale was a later development and had no part in TAP.

At the same time, Lazarsfeld was concerned with problems of index construction. Therefore, a short outline of the differences and the similarities between these two procedures seems necessary. For constructing an index, a number of traits – measuring behaviour, in most cases – are identified, and respondents are given one point for each hit. The overall number of points is the index value. Lazarsfeld used a simple example, i.e. socio-economic status, which can be determined by possession of a home, possession of a car, presence of a telephone at the home and college education. Respondents can score from 0 to *n* points, and no connection between individual variables needs to be assumed (Horwitz and Smith 1955: 74). At the time, Lazarsfeld was interested in detecting the connections between various indices that had been constructed independently from each other, leading him to the concept of the 'interchangeability of indices'. Researchers working with scales, by contrast, assume that the individual items of a scale are degrees on a dimension. This goes to show that the choice of the survey tool also has an effect on the questions to be solved (or considered interesting). Scales require detailed work on the intervals between items while Lazarsfeld's indices of that time resulted in a rough division, fourfold in most cases, and required more attention to be paid to the correlations between the various indices.

With regard to the content of the attitudes to be measured, Sanford and Levinson were no pioneers, either. Attempts to measure political attitudes and prejudice and, in several cases, even to identify fascist attitudes, had been made before them. As early as in 1925, Henry Moore, drawing on a questionnaire of twenty items, had published a paper on radical and conservative attitudes among undergraduates at Dartmouth College and Yale and Columbia

Universities. He had subsequently tried to link the results to intelligence, neuro-muscular constellations and other factors (Moore 1925), but in the present context this is of minor importance. What is more to the point is the history of attempts to assess specifically fascist attitudes. The first publication dealing with this matter is by Ross Stagner (Stagner 1936a), who used Thurstone scales to assess the affinity of American college students in the Midwest to German or Italian fascism. His item list already included a number of neutral items, and his analysis also considered sex differences and the social context, which was measured by indicating the college the respondent was attending. What is more, Stagner did something that Adorno and Horkheimer later thought was their very own invention: he grouped the extreme cases of those who had obtained particularly high or particularly low scores, respectively. Stagner's results suggest that 'high scorers' tend to refuse workers' unions, feel superior to blue-collar workers, show no sympathy for the disadvantaged and the unemployed, and largely identify with middle-class values. With respect to an improved version for adults, however, the author pointed out that a distinction should be made between emotionally charged stereotypes and 'true attitudes' (Stagner 1936b).

Raskin and Cook (Raskin and Cook 1938), directly building on Stagner, tried to eliminate the problem of stereotypes from their assessment of basal attitudes. Katz and Cantril (Katz and Cantril 1940) and Edwards (Edwards 1941), based on their finding that strong signal words provided a 'frame of reference' (Allport 1940) and affected response behaviour, addressed the question of how to measure 'unlabeled fascist attitudes'. Finally, Abraham H. Maslow published a treatise on 'The authoritarian character structure' (Maslow 1943) in which he tried to break down the concept of the authoritarian character structure that was pivotal for Erich Fromm, a former member of the Institute of Social Research, as well as for the Frankfurt School, into its psychological elements while steering clear of the risk of over-psychologization. Maslow discussed worldview, tendency to hierarchy, general tendency to classify people as superior or inferior, striving for power, hostility, hatred, prejudice and many more traits as evidence for the authoritarian character (drawing these distinctions from Arthur Koestler's *Darkness at Noon*, the impressive description of the mindset of the accused in the Moscow show trials). If the authors of TAP had taken into account this latter aspect, they would have been spared the accusations of one-sidedness – i.e. of having ignored left-wing authoritarianism (cf. Shils 1981).

These few indications must suffice here to point out something that Sanford and Levinson would no doubt have acknowledged, that their work certainly was up to the state of the art of contemporary psychological research, but it was by no means more than that.<sup>54</sup> The work they had done before they were invited by Horkheimer to join TAP project was a prototypical contribution to an ongoing cumulative psychological exploration of prejudice. Their original achievement was the construction of a scale for assessing anti-Semitism which up to then had not been given a great deal of attention. The few



socio-psychological or sociological interpretations of the phenomenon of anti-Semitism that were not embedded in the larger context of comparative studies on attitudes towards ethnic minorities had been proposed by authors who, for the most part, were either psychoanalysts themselves (Fenichel, Freud, Fromm, Reich, Simmel) or were influenced by Freud's teachings (Harold Lasswell). Frenkel-Brunswik's case studies enhanced the psychology of personality, which was nearing its peak at the time, by a number of new facets, and perfectly fitted into this psychoanalytic frame of reasoning.

From the very beginning, the encounter between the group around Horkheimer and the one around Sanford must have taken place in a friendly atmosphere, for no derogatory comments on the Berkeley group are found in the letters of the members of the Institute of Social Research at the time, which is highly unusual, and something of a surprise. After all, the members of the Berkeley group, in spite of their interest in psychoanalysis and their political affinities with the concerns of the institute, were primarily academic psychologists, which should have triggered some adverse reflexes in Adorno if, in 1944, he still adhered to the anti-psychological views he had expressed when arriving in New York in 1939. But in the meantime, Horkheimer and Adorno had discovered psychology as an effective conceptual framework that assured a certain congruency with traditional thought even though 'psychology' for them still was something rather different from academic psychology, as shown by a letter which Horkheimer wrote to Marcuse at the beginning of the alignment with the Berkeley group: 'I don't believe in psychology as in a means to solve a problem of such seriousness. I did not change a bit my skepticism towards that discipline. Also, the term psychology as I use it in the project stands for anthropology and anthropology for the theory of man as he has developed under the conditions of antagonistic society.'<sup>55</sup>

What Horkheimer and Adorno wanted, as noted by Wiggershaus, was 'to be able to promote, under the cover of a traditional terminology, a maximum of their own thought' (Wiggershaus 1986: 398). Their own thought, in this case, was the attempt to embed the studies in a larger framework of social theory and in the typology that had been published as early as in 1941 but had apparently more or less fallen into oblivion in the meantime. Both concerns were in line with those of the group around Sanford, and they ultimately resigned themselves to having Adorno's collaboration forced upon them.<sup>56</sup> One may assume that in matters of social theory, they felt inferior to the German exiles, if only for the latter's permanent arrogation of competence in this respect, while in the field of their own competence they were actually facing an unsolved problem for which they needed all the help they could get.

The help that the critical theorists could give on the theoretical level did not consist of what at that time was gradually emerging as the mainstream of sociological theory, i.e. the attempt to attain a higher degree of generalization by the systematic conceptual reconstruction of empirically verifiable connections – for which Merton, with his 'middle range theories', provided the programmatic catchword (Merton 1949). Horkheimer and Adorno adhered

to Hegelian-Marxist thought and produced interpretations of the contemporary era, albeit with a stronger orientation to the history of ideas than to the Marxian model of class conflict and capitalist exploitation, as is evident from their best-known text of those years, the *Dialectic of Enlightenment* (*Dialektik der Aufklärung*) (Horkheimer and Adorno 1969). Referring to anthropology, as the two authors of the *Dialectic of Enlightenment* did, arguably provided another bridge to the Berkeley group, although it is obvious that their understanding of anthropology was something quite different. Sometimes, even misunderstandings may serve to establish a shared vision of the world and of the problems to be addressed. Levinson remembered that Adorno 'represented sociology much more fully than any of us could' (Levinson 1992). It can be supposed that not even Adorno thought of himself as the embodiment of the sociology of his time, but making others believe just this was something else altogether.

Attitude measurement by scales regularly faces the problem of what to do with the data so painstakingly collected. After all, all they collected were the frequencies of respondents' agreement to each of the items. How to come to a content-based characterization of the empirical distribution of the 'scores' obtained by a group under investigation was the key problem of academic psychologists at the time – and the issue of how to empirically identify the types of anti-Semites that had been constructed in 1941, and to establish their actual occurrence, was a problem Adorno could not help but be interested in if the final product of their concerted efforts was to succeed on the academic market. This opportunity to benefit from each other's contributions to their respective problems seems to have been the basis for the fruitful collaboration between these partners who, on the face of it, were not really meant for each other.

An additional incitement to intensive cooperation was provided by the idea, probably coming from Horkheimer and Adorno, that anti-Semitism should not be treated as an isolated phenomenon but should be embedded in the larger context of 'anti-democratism'. Exploring the latter implied that the dimensions that characterized it had to be identified, and that adequate scales had to be constructed. This challenge was enthusiastically taken up by Sanford & Co. since it offered them the welcome opportunity to apply a technique they had successfully developed for a different case to other phenomena as well. Thus Levinson, who was primarily interested in working at the scales, spent a great deal of energy on their improvement and sophistication.

Another congruence between the two sides of this unequal joint venture was that both of them wanted to do more than just establish distributions of certain attitudes. Horkheimer & Co. were not inclined to attribute much importance to what they disparagingly called measuring the opinions of the majority. They were not interested in what was fascinating Lazarsfeld, their partner of so many years, namely looking for causal factors that could be isolated and would allow him to break down the apparently uniform groups of

the population into distinct sub-groups, and engaging in the follow-up of divergent cases in order to find factors of influence that had so far been overlooked. This was something which Horkheimer and Adorno, provided they took notice of it at all, considered to be too atomistic an approach. Their own key concern was with holistic models such as those that, due to Fromm's contributions, could already be found in *Autorität und Familie*. Fromm and, following him, the institute's other collaborators were bent on identifying the specific social character of their epoch, an undertaking that was in line with their veneration of psychoanalysis as a critique of contemporary civilization. Here, Frenkel-Brunswik's line of research fitted in especially well since in her Vienna time she had participated in the local psychological life-course research and after her emigration to California had increasingly turned to the psychology of personality. Both research concerns required a concept positing that personality traits remained more or less robust over time. Thus, the psychology of personality might have provided some substantiation of Adorno's typology. At the same time, it helped them to get rid of the problem that Merton had pointed out in Horkheimer's Advisory Council: the definition of the situation. That is, the same person might show different attitudes and beliefs in different situations or, at any rate, give different answers in an attitude assessment to questions that had no consequences on the social level. With the reference group theory, Merton provided a tool for this which, however, was ignored not only by the authors of TAP but also by most of the others who were conducting studies on prejudice.

The extension of the scales that were used to measure anti-democratic attitudes soon led to complications. The original scale consisted of nothing but negative comments on Jews, because it was assumed that this would help to bypass the mental resistance that caused respondents to abide by the norm of not speaking disparagingly of an ethnic group. In the next step, a sophistication of the tool for assessing anti-Semitism was attempted. Sanford and Levinson came up with the idea to subdivide their original anti-Semitism scale into sub-scales. This, they reasoned, would allow them to identify different types of anti-Semites. This line was later abandoned but is found in Levinson's contributions to TAP. On reading these passages, one cannot help thinking that all this was in reality just some method-crazy psychologists tinkering around.

Another modification, namely the attempt to assess anti-Semitism without using a single phrase in the questionnaire that said anything at all about Jews, resulted in a nearly aporetic situation for the Berkeley group. In their discussions and statements, this approach was called the 'indirect tool'.<sup>57</sup> The idea behind this is tempting indeed: if genuine anti-Semites tended, for reasons of social conformity, politeness, fear or whatever, to keep quiet about their true feelings in a survey situation, all one needed to do was to construct a tool that allowed the researchers to penetrate to their true values. This could be achieved, or so they felt, by conceiving of anti-Semitism as embedded in a set of more general attitudes, a syndrome of something more deeply rooted in an

individual's personality. In other words, what they tried to do was to identify latent structures, an undertaking radically different, it is true, from Lazarsfeld's cross-table efforts. The F- (or Fascism-) scale that was subsequently constructed, however, was validated on the basis of its correspondence with other scales measuring anti-Semitism, ethnocentrism, and political and economic conservatism, as well as with the clinical interviews, which suggests some circularity. The indirect F-scale instrument was enhanced by statements which could be expected to meet with strong approval by those respondents who would not voice their anti-Semitic attitudes in direct measurements and in-depth interviews.

### FURTHER ATTEMPTS AT A TYPOLOGY

During the long time it took to construct these instruments, all those involved were perfectly aware that it would be advantageous if they could find a way to identify different types of anti-Semites. This was the field in which Adorno felt at home and, therefore, he made every effort to contribute to it. In the summer of 1945, he presented two drafts. An undated thirteen-page memorandum states that in order to identify different types of anti-Semites, he had worked his way through 'about 40 of the detailed case interviews', i.e. those in-depth interviews that constitute the top and the bottom quarters.<sup>58</sup> In terms of content, Adorno sought to reconcile his typological ambitions with the previously established dichotomy of 'high scorers' and 'low scorers'.<sup>59</sup>

The drawbacks of this typology are evident. First, the types are not clearly delimited from one another. Second, in an effort to distinguish between the analytical levels of psychodynamic roots, criteria for identifying the type and reflections on therapy, Adorno inserted subheadings, but was inconsistent in doing so. Speculations about the psychodynamic roots, i.e. what might be described as theory, keep alternating with listings of empirical indicators. Third, the dimensions of the typology are not clearly labelled, and the possible variants of any of these dimensions are not exhaustively treated. Adorno seems to have worked through the interviews in a way that was loosely connected to psychoanalysis, trying to link them up with the typology he had constructed in 1945. His abstractions, rather than resulting from his examination of the material, actually preceded it. He neither worked out genuine ideal types in the rigorous Weberian way nor used Lazarsfeld's techniques to construct a typology.

On the whole, Horkheimer was enthusiastic about Adorno's summer writing efforts, dubbing the manuscript as 'one of the most important steps in the Berkeley study' and advocating its distribution to all the other AJC projects.<sup>60</sup> But in spite of this praise for the man who was waiting in Los Angeles for his co-author of the big theory book to return, Horkheimer did not hold back with his criticism in the rest of his letter. What it ultimately comes down to is a completely different perspective on anti-Semitism, which makes it worth

dealing with in the present context. Horkheimer started with the authoritative statement that, 'if, in this current period, one is born into an average Gentile family one does not have to be a "type" in order to be an antisemite. One simply learns to speak disrespectfully of Jews as one would learn to curse, tell dirty jokes, drink heavily, or to rage about taxes and strikes.'

Anti-Semitism, he went on to argue, was a 'cultural pattern of modern society'. If this was true, it would cut the ground from under any approach to anti-Semitism that relied on the concept of an anti-Semitic personality as an identifiable part of the population, one different from other personality types that are less, or not at all, anti-Semitic. In the final analysis, his perspective implied that in a world in which 'the average human being, who lives in reaching distance of the ideological machinery of mass domination' everyone could without exception turn into an anti-Semite at certain times and under circumstances yet to be specified. Thus, the difference was not to be found in individual personalities but in different degrees of an anti-Semitic culture that could unleash an anti-Semitic mob whenever the forces of mass domination felt like it. Horkheimer did not further elaborate on what would, then, be the decisive point, i.e. whether in a situation like a pogrom all would join in or whether specific groups of individuals would abstain from or even protest against it. If the latter was conceivable, research on individual differences would again be useful – provided, however, that it was capable of identifying pro-democratic or anti-anti-Semitic types.

Horkheimer then actually suggested that Adorno should abandon his typology for another one that had been constructed by Horkheimer himself. In the present context, however, this is of minor importance since nothing came of it, although it does tell us something about the relational dynamics between the two critical theorists. Two further suggestions of Horkheimer's were, in contrast, acted on by Adorno. He eliminated the parts dealing with therapy, and he wrote the introduction to TAP Horkheimer had insisted on, specifying the status of typologies in their 'general theory'.

In his introductory pages to Chapter Nineteen of TAP, Adorno sought to defend the typological approach. He explicitly referred to Gordon Allport and David Boder (both in Harriman *et al.* 1946) who had pointed out that psychologists such as Erich Jaensch became instrumental to Nazi extinction policies on account of the concept of 'Gegentypus' (anti-type). Adorno's answer to this was that psychologists such as Jaensch were expressing the 'stereopathic mentality' that was one of the 'basic constituents of the potentially fascist character' (Adorno *et al.* 1950: 746). With regard to the refusal of American psychologists to consider types as an adequate research tool, Adorno stressed that typologies had been constructed by such irreproachable authors as Ernst Kretschmer, C.G. Jung and Sigmund Freud, and that those were the models to follow, rather than Jaensch, whose perspective was 'quasi-biological'.<sup>61</sup> Using typologies was justified if three conditions were met.

First, classifying people according to types was nothing that could be done 'neatly statistically', or in terms of ideal types, but had to rely on 'mixtures': 'Our types are justified only if we succeed in organizing, under the name of each type, a number of traits and dispositions, in bringing them into a context which shows unity of meaning in those traits' (Adorno *et al.* 1950: 749).

Second, a typology had to be critical, that is, the standardization of people itself had to be seen as a social function. This obscure formulation can be supposed to mean that a valid typology was tantamount to a conceptualized form of respondents' self-standardizations: 'Here lies the ultimate principle of our whole typology. Its major dichotomy lies in the question of whether a person is standardized himself and thinks in a standardized way, or whether he is truly individualized and opposes standardization in the sphere of human experience' (Adorno *et al.* 1950: 749). Adorno went on to say that the tendency to self-standardization was also found in 'low scorers', and that the stronger this tendency, the more clearly the latter 'unwittingly' expressed 'the fascist potential within themselves'. In a footnote, Adorno specified that his types were real types rather than logical classes. While the three other main authors of TAP stuck to the rules of attitude measurement in their construal of the opposition of 'low scorers' and 'high scorers', i.e. took low scores of agreement to the statements of a scale as evidence for a low degree of adherence to the respective attitude (anything else being just absurd and disavowing the whole effort), Adorno insisted until the end, i.e. until the publication of TAP, that he had a deeper insight into the personality of the potential fascist than the three experts in psychological measurement with whom he had cooperated for years.

The third criterion to be met by a typology, Adorno wrote, is a pragmatic one, i.e. the need to make simple distinctions such as, for example, the distinction between the Romanic and the Gothic styles in art history. In order to make any sense at all of these deliberations, it should be kept in mind that in the initial synopsis of 1941 the typological classification always referred to social positions, as well. My impression is that Adorno came up with the third criterion because it allowed him to reintroduce this aspect of his original beliefs. An orientation to social group membership is 'easier' than the multidimensional psychological reflections that characterized his previous attempts.

The typology that was published in TAP differs from its predecessors in that the two extreme groups of 'low scorers' and 'high scorers' are used after all. Further changes are documented in Table 6.4, in an effort of mine to document the development of the typology across its four stages. It is not hard to see that the project came up against a number of problems. Some of the initial types just disappeared at later stages, others were split and, most notably, new ones kept popping up.

**Table 6.4** Development of the Typology of the Anti-Semite

ZfS 1941	Memo Types	Memo Counter-types	Typescript Sep.45	TAP
A1 Born			D1 Idiosyncratic	
A2 Religious-philosophical				
A3 Back-woods or sectarian				
A4 Vanquished competitor			D2 Surface resentment	H1 Surface resentment (≠ L1)
A5 Well-bred			D3 Conventional	H2 Conventional
A6 Condottiere			D4 Rebel (≠ D5)	H4 Rebel and psychopath
A7 Jew-baiter				
A8 Fascist-political			D5 Manipulative (≠ D4)	H6 Manipulative (≠ L4)
A9 Jew-lover ▶ B1-B6	B1 Genuine liberal ▶ L5	C1- Leader ▶ D8		
	B2 Ticket progressive ▶ L1	C2- Manipulative ▶ D5		L1 Rigid Low Scorer (≠ H1) <sup>a</sup>
	B3 Devout Christian	C3- Conventional ▶ D3		
	B4 Conscientious ▶ L2	C4- Oedipal ▶ D7		L2 Protesting Low Scorer (≠ H3)
	B5 Easy going type ▶ L4	C5- Follower ▶ D9		L4 Easy Going Low Scorer (≠ H6)
	B6 Impulse ridden ▶ L3	C6- Tough guy ▶ D6		L3 Impulsive Low Scorer (≠ psychopathic high scorer)
	B7 Paranoid non-Antisemite	C7- Paranoid ▶ D10	D10 Paranoid	H5 Crank <sup>b</sup>
			D6 Tough Guy ▶ H4	
			D7 Oedipal	H3 Authoritarian (≠ L2)
			D8 Leader	
			D9 Follower	
				L5 Genuine Liberal (≠ A9)

Note: The columns represent the four stages of the development of the typology (column 3 is related to column 2 and gives the opposite type [*Gegentypen*]). The letters should be read as follows: Letters A to D stand for the three drafts, the numbers represent the order in which Adorno mentions them in the different drafts. In TAP (*The Authoritarian Personality*, Adorno et al. 1950) (right column), Adorno made a new differentiation and relates the types to the two extreme groups (H for high scorer and L for low scorer). In each line one finds from left to right the fitting types. No data to the right indicate that there were no follow-ups to find. Follow-ups in different lines are indicated with ▶. In TAP one find hints towards *Gegentypen*, here represented in parentheses (≠ Xn).

<sup>a</sup> In a draft of the table of contents of TAP from July 1947 this type was named 'Ticket "Low"'. AJC records, b. 19: Studies in Prejudice, YIVO.

<sup>b</sup> In a draft of the table of content of TAP from July 1947 this type was named 'frustrated'. AJC records, b. 19: Studies in Prejudice, YIVO.

It seems justified to say that Adorno failed in his attempt at a typological identification of anti-Semitism. First, it remains unclear at each of these stages which dimensions were taken into account in the typology; and in those cases where these dimensions can after all be identified they are rarely exploited exhaustively. The initial ordering along a quasi-stratified dimension was abandoned for a complex structure of types and counter-types that are mainly described in psychoanalytical terms. And in the end, Adorno seems to have been converted to the social-psychological conception that takes into account the fact that the attitudes measured differ in intensity. Four of the five types of 'low scorers', however, differ from each other only in the way they were labelled while the fifth one, the 'genuine liberal', is an ad hoc addition. Similarly, the central, i.e. 'authoritarian', type is a last-minute addition, as it were, to the typology and, with regard to content, does not go beyond what Fromm had written about the authoritarian-masochistic character as early as in 1936 (Fromm 1936: 110ff.) Considering that almost from the start the aim had been to speak of a fascist rather than an 'authoritarian' personality (Wiggershaus 1986: 458), the belated appearance of the authoritarian type is not really surprising. If, however, the authoritarian type originally was the fascist one, the question arises how the results of the F-scale could be reconciled with Adorno's typology. It is obvious that 'high scorers' on the F-scale are 'fascists', but what, then, are all the other types of 'high scorers' in Adorno's typology? Adorno himself added to the confusion by proposing, in a long memorandum on the 'book plan' that was found among the papers of the AJC Department, to bridge a major gap by introducing a section on anti-Semitism among Jewish people:

This chapter could be linked organically with the total book plan. By discussing AS among Jews and the importance of the Fascist character among them, we could demonstrate concretely to what extent AS is irrational, that is to say, largely independent of actual interest and also that it is psychologically conditioned, not due to belonging *objectively* to this or that national or racial group.<sup>62</sup>

Second, Adorno's types and syndromes do not match the description of the dimensions of the authoritarian personality, which appear to be primarily based on Frenkel-Brunswik's contributions. Frenkel's aim was to construct a personality model by using variables that would allow researchers to empirically identify individual traits and, in a next step, to construct relatively stable personality types based on combinations of these traits. While she was very resourceful in her isolation of personality traits, her construction of personality types was less satisfactory. As a result of the lack of alternatives, her types do not go beyond the opposition of the prejudiced and authoritarian (or fascist) personality versus the non-prejudiced personality, i.e. they are just another way of labelling the contrast of 'low scorers' and 'high scorers'. The large variety of variables, however, would no doubt have allowed her to construct other types, as well. Table 6.5 shows the variables



**Table 6.5** Variables of The Authoritarian Personality and Dimensions

F-Scale		Frenkel-Brunswik		
Variable	Definition	Personality Trait		Definition of the prejudiced
		of the prejudiced	of those with no prejudice	
Conventionalism	Rigid adherence to conventional, middle-class values	Conventionalism	Genuineness	Adoption of conventional values and rules
Authoritarian submission	Submissive, uncritical attitude toward idealized moral authorities of the in-group	Rigidity	Flexibility	The avoidance of ambiguities, lack of individuation, superficial adjustment
Authoritarian aggression	Tendency to be on the lookout for, and to condemn, reject, and punish people who violate conventional values			
Anti-intraception	Opposition to the subjective, the imaginative, the tender-minded	Repression	Awareness	Repression or awareness of unacceptable tendencies and impulses in himself
Superstition and stereotype	The belief in mystical determinants of the individual's fate; the disposition to think in rigid categories			
Power and 'toughness'	Preoccupation with the dominant-submission, strong-weak, leader-follower dimension; identification with power figures; overemphasis upon conventional attributes of ego; exaggerated assertion of strength and toughness	Power	Love-orientation	Admiration of, and search for, power
Destructiveness and cynicism	Generalized hostility, vilification of the human			
Projectivity	The disposition to believe that wild and dangerous things go on in the world; the projection outwards of unconscious emotional impulses	Externalization	Internalization	The mechanism of projection, by which much of what cannot be accepted as part of one's ego is externalized
Sex	Exaggerated concern with sexual 'goings-on'			

Source: Adorno *et al.* 1950: 228 and 474-6.

that were used in the 'F-scale,' with the four co-authors of this chapter claiming that, 'these variables were thought of as going together to form a single syndrome, a more or less enduring structure in the person that renders him receptive to antidemocratic propaganda. One might say, therefore, that the F scale attempts to measure the potentially antidemocratic personality' (Adorno *et al.* 1950: 228).

The variables of the authoritarian personality are opposed to the personality traits, as shown in the three columns on the right-hand side of Table 6.5, that Frenkel-Brunswik considered worth mentioning in the chapter of which she was the only author. There is little correspondence between these two sets of variables. The attempt to obtain a more differentiated scale of personality types had failed to produce any workable results and was deferred in favour of a multifaceted description of the traits of the two basic types.

Third, over the course of the years, Adorno's methodological justification of his typology kept changing in ways that remain incomprehensible, and where arguments were presented, they do not hold up under examination. The typology was said to be based on ideal types, or to capture real types, or – a frequent reference – to consist of transitions and mixed types.

Fourth, there is a technical objection, as well. Starting with the second typology, Adorno based his work on the clinical interviews conducted by the Berkeley project. That he himself, as far as we know, never conducted a single interview would not be objectionable as such. But considering how the clinical interviews came into being, it is obvious that the material, as Hyman and Sheatsly have convincingly shown, is subject to a strong interviewer effect (Hyman and Sheatsly 1981). The interviewers were aware of the results respondents had obtained in attitude measurements and had been instructed to systematically draw on this prior knowledge in conducting their interviews. Given the lack of technical recording devices, the content of these clinical interviews had to be noted from memory, and there is every indication that there was a large margin for interpretation – too large a margin to allow for so finely tuned an interpretation.

Fifth, one might add that using a typology that consisted of eleven types to analyse eighty interviews is a misconception, since it is too detailed to allow for a grouping of cases. Significantly, each of Adorno's types is illustrated by just one case. While it may just be possible to construct one type relying on a single case, doing so for eleven types seems highly improbable. Adorno's types, thus, are no more than eleven cases that have been given pompous names.

#### (NO) DISPUTE ABOUT 'CREDIT'?

Since Adorno lived in Los Angeles where he could direct a number of collaborators and was close to Horkheimer, he did not participate in the work of the Berkeley group. Levinson estimated that during the two-and-a-half years of

concentrated work on the project, there were at most eight to ten meetings a year with Adorno. In an interview conducted in 1992, Marie Jahoda, who attended one of these meetings as a delegate of the New York headquarters, described Adorno's performance:

I remember, at the time of the 'Authoritarian Personality' in America – I was in New York working at the American Jewish Committee, and Horkheimer was the boss, also in New York, (and) he said: 'I think you should go to California, have a look at what they are doing down there.' So he sent me there as a kind of spy or something. I remember that we had a meeting at Else Frenkel-Brunswik's house, and that Adorno kept lecturing; there was Sanford and Levinson and Else and me, and Adorno paced up and down in this large elegant salon – there was an armchair in the center, and when he passed there, he always bumped against it. Then he made a little bow saying: 'Excuse me, please!' to the armchair (laughs), went on, and on his way back, this happened all over again. He, he was really crazy.<sup>63</sup>

Given that all her life Jahoda was anything but a gossip (this disposition being poorly developed in her, or deliberately kept at bay, so much so that her autobiography suffers from it), her judgement surely has a certain weight. Nevertheless, in an interview, Levinson insisted that 'we always liked Teddy [i.e. Adorno]'. In California, for the year 1945 and following, the unlikely case of successful cooperation between people who, on the face of it, do not fit in well with one another, seems to have come true.

There were frictions and, later, a conflict of the kind that are likely to occur in almost any cooperation that lasts for a number of years. But with regard to the institute, these did not result in the kind of defamation that Lazarsfeld was subjected to in the context of the Radio Research Project. The reason is that in this cooperative work the institute could not withdraw back to anything that could be said to be irreconcilable with 'administrative research', as had been the case with Adorno's music theory. For the institute, the only way to solve the conflict with its cooperative partners was to try to claim as its own as much as possible of the Berkeley group's achievements.

The first conflict flared up when Sanford & Co. discovered that, during most of his AJC time, Horkheimer had made the local officers believe that they were just a group of subaltern assistants. After Horkheimer had retreated to his new house in Pacific Palisades, his deputy, Samuel Flowerman, considered it his duty to visit the various projects that were now supposed to flourish under his direction in order to gain a clear picture of what they were doing. In the course of these visits he came to Berkeley in January 1943. What the psychologists gathered from their visitor prompted Sanford to write two letters to the Director of the AJC, Slawson, in which he tried to get things straight. In these letters, Sanford described how the cooperation between him and his colleagues, on the one hand, and Horkheimer and the institute, on the other hand, had come about, a story with which the reader is already familiar. In 1943, Sanford and Levinson had started to do research on anti-Semitism,

quite independently from the institute. They had then, via Simmel, come into contact with Horkheimer who had opened up prospects of cooperation and, more importantly, of funding for their work. This, however, had at first failed to materialize. Thereupon Sanford had continued working with the modest means of his department. In November 1944, that is, about the time when Horkheimer started to work at the AJC, 'all techniques and instruments which have since been used' had been ready:

Our greatest indebtedness to Dr. Horkheimer is for undertaking to convince people to whom we were unknown that our approach was a sound one. In this circumstance it seemed to us natural that there should be some lack of clarity concerning the origin of the ideas, methods and results. Since Dr. Horkheimer was acting as the representative of our researches we never felt called upon to claim credit for our role – the role of the Berkeley Public Opinion Study – in the overall undertaking.

There is more than one point that is remarkable in this. The contact Horkheimer established between Berkeley and the AJC was only an indirect one, as is well known. In the process, he seems to have described the Berkeley group as some kind of rear vassals of the institute, and he also seems to have passed off, at least to Flowerman, certain achievements that actually were those of the Berkeley group as those of the institute. Furthermore, it becomes evident – and later passages of this letter unequivocally say so – that Adorno was not a member of the Berkeley Public Opinion Study but only a cooperating partner, about whom Sanford went on to write:

We have particularly valued Dr. Adorno's deep understanding of Fascism and of the broader contexts within which anti-Semitism has to be considered, his grasp of important concepts in contemporary social science, and his belief – so much like our own – in the integration of psychoanalysis, sociology and the best in academic social psychology ... We would like the continued collaboration of Dr. Adorno.<sup>64</sup>

There is no need to argue here about how deep Adorno's understanding of fascism really was. The previous remarks should have enabled the reader to come to his own judgement in this matter.

Horkheimer was immediately informed of Flowerman's visit to Berkeley and Sanford's letter, and his reaction shows that Sanford had indeed hit on a sore point. To Leo Löwenthal, whom Horkheimer had sent a copy of Sanford's letter instructing him at the same time to destroy the copy and not to let Flowerman know that he knew about it, he wrote a wordy complaint about the badness of the world. Sanford, he said, seemed to think that the institute and he himself 'must have received tremendous amounts of money' (which for the first phase in 1943/4 can hardly be denied, see Table 6.2) and that in the previous year, when Horkheimer was sitting in his AJC office, too little funding had been channelled to 'his [i.e. Sanford's] racket'. Horkheimer, in his typical manner,

exploited this episode in order to distil some general insights on the United States:

In this country you can get along and make headway if you develop great activity. Even in the intellectual branches, there are plenty of opportunities and the money lies so to speak in the street. There is only one condition: that you subscribe to action for action's sake without any reservation. It is not so much the content of your work which counts, but the quantity and even more than that: human relations.<sup>65</sup>

However, in writing this, he was not thinking of himself but of Sanford! Flowerman, who had gotten wind of all this and largely shared Horkheimer's view of the matter, confidentially approached Frenkel-Brunswik and implored her to make sure that the existing ruling according to which the institute was the AJC's 'agent' in California was accepted by Berkeley. As soon as the 'operational part of the project ceases,' Adorno's usefulness would become evident.<sup>66</sup>

Although Horkheimer and Adorno must have been aware from the winter of 1946, at the latest, that there was an impending conflict about the question of priority, this conflict strangely enough never really broke out. The reason is to be found in the rivalry between Frenkel-Brunswik and Sanford. Concerning the publication of TAP there surely was a debate about the order in which to present the authors' names, but in the end the three authors accepted the alphabetical order, being listed after Adorno. They even conceded priority to Adorno for the chapter on the F-scale, which was signed by all four of them. Adorno most likely had contributed little more than the F-word to this chapter but nevertheless set great store by coming first (Wiggershaus 1986: 457).

In the case of publications by more than one author, there are multiple solutions for the issue of how to order the names, none of which is binding in a normative sense. The easiest one, but by no means the one most often used, is the alphabetical order. Another one is an ordering which reflects how much each of the authors contributed to the overall product, and a third one is an ordering according to the age, status or prestige each author has so far acquired, i.e. reliance on the Matthew effect (Merton 1973: 439–59; Merton 1988). Other authors decide by lot or opt for a reverse alphabetical order. As yet, hardly an author will have felt entirely indifferent about the issue, not even those who choose to turn it into ridicule, such as the trio of 'Alpha, Beta, Gamow' that was in reality just one author (Watson 2002).

Horkheimer and Adorno were anything but disinterested in priority issues, as shown by the ordering of names for the *Dialectic of Enlightenment*, where the one that was superior by status and age came first and the one who presumably contributed more to the success of the fragmentary work had to be content with the second place. Author duos, however, have the great

advantage that, with only two names, both of them will be cited as a rule, just as in the case of the *Dialectic of Enlightenment*. The critical number is three or more, since in these cases references to sources, etc., are mostly in the form of 'XY *et al.*' or 'XY and collaborators'.

The issue of how to order authors' names for a collective work is only the most visible manifestation of the struggle for recognition among scholars. There is more than ample evidence that the four main TAP authors were very much aware of the issue of 'credits', and that a certain amount of infighting was going on about it. Levinson recalled that Frenkel-Brunswik and Sanford quarrelled about priority, i.e. the question of who had contributed what, and how much of it. Sanford himself had given priority to Levinson, who at the time had not even graduated, for the first publication that resulted from their cooperative work. But he could at the same time be sure that his name would always be cited, as well, and that, to the initiated, this would suggest that the senior researcher was also likely to have had a greater share in the intellectual output. When the Sanford-Levinson duo became a trio, debates soon set in between those two who were more or less equal in terms of age and status. Levinson, retrospectively, was very open about this:

As soon as we [three] were seen as a project and we were in a larger world, then there were questions of credit. Each of them [i.e. Frenkel and Sanford] felt that they have made an important contribution, and at times felt that the other one minimized or neglected it. But I would say there's a sociology of this, which is that the world around us wanted to know who was the real hero of this, who was the intellectual God of this, the source, and there were those who said it was Nevitt [Sanford], and those it was Else [Frenkel-Brunswik]. Nobody said it was Teddy [Adorno] or me ... (Levinson 1992)

In the late 1940s, Else Frenkel-Brunswik had more publications than Sanford who, however, topped Frenkel-Brunswik with respect to status, since he held a regular university position while Frenkel-Brunswik had to settle for the position of a research associate. The activities of the Berkeley group were communicated among psychologists long before they published their book, and the project team was invited to contribute to more than one publication. In most cases, a single author wrote about some aspects of the results. But when they were supposed to produce a summary contribution to a collected volume of recent works in social psychology, the three were sure that Adorno would have no part in it, but Frenkel-Brunswik and Sanford locked horns with one another about the ordering of names. According to Levinson, who was much younger and had no voice in this, the decision in favour of an alphabetical order was not reached for reasons of conventionality, but because it expressed the order of intellectual achievements. Sanford could put forward neither his role as a founder nor that as a director against Frenkel-Brunswik's disproportionately larger contribution to the content.

**Table 6.6** Authors' Contributions to TAP by Chapters and Pages

Author	Contribution		Contribution	
	%	no. pages	%	no. chapters
Adorno, Theodor W.	19	178	17	4
Frenkel-Brunswik, Else	21	195	22	5
Levinson, Daniel J.	22	213	17	4
Sanford, R. Nevitt	7	67	13	3
Aron, Betty	6	55	4	1
Morrow, William	8	73	4	1
Levinson, Maria Hertz	8	79	4	1
Sanford, Adorno, Frenkel, Levinson	9	88	4	1
Unlabelled	3	32	13	3
	100%	948	100%	23

Table 6.6 shows that, when TAP was published, the authors who were further down, alphabetically speaking, would have had more than one reason to object to Adorno's prime place in the citation. Given the number of chapters and the number of pages, Frenkel-Brunswik and Levinson certainly topped, or were even with, Adorno. Since Co-Director Sanford had contributed relatively little to the book as such, an ordering according to the quantity of writing, and the quantity of intellectual input that went along with it, i.e. Frenkel-Brunswik, Levinson, Adorno, Sanford, would have more or less reflected the facts but would have made Sanford's position appear less important than he was ready to accept. To be the last one in the alphabet is something quite different from being the last one on an authors' list that was ordered according to other criteria. Any attempt to relegate Adorno to the end of this achievement list would have failed because of his resistance as well as the resistance of those whom he would have been able to mobilize. If Theodor Wiesengrund (Adorno) had not changed his name even before his naturalization in the United States in 1943 and thereby catapulted himself to the top of the alphabet, the order would have corresponded to the actual contributions, and Adorno would have stood hardly a chance to protest against this. That the alphabetic ordering was ultimately chosen was only due to the fact that Frenkel-Brunswik and Sanford could not agree on which of them had made the larger contribution to the study – the rejoicing third who became the first was Adorno.

In the spring of 1950, TAP was published as the fifth and last volume of the series *Studies in Prejudice*, and for the institute Adorno's top position on the list of authors must have been something like the crowning victory in a long battle for priority. In the preceding years, there had in fact been a number of conflicts about the issue of what part the institute had had in the work compiled in the *Studies in Prejudice*. The key question here was who was to be

presented as their editor. Finally, a compromise was reached that was satisfactory for both sides: editorship was credited to Max Horkheimer and Samuel H. Flowerman. The short-term director and long-term adviser of the Department of Scientific Research, and his successor, who had been in office five times longer than he but was inferior to him in terms of age and status, both represented the AJC. But to third parties, Horkheimer could also present himself as the Director of the Institute of Social Research who was cooperating with a minor AJC officer. When the first reviews of the first two volumes appeared, the AJC became aware of Horkheimer's double strategy. In a long letter, Flowerman, Horkheimer's successor in the AJC Department of Scientific Research, confronted him about this:

These items [reviews and reports] were discussed yesterday at a Staff Policy Committee meeting where I found myself on the defensive against allegations that a common element was present – and another common element absent – in each of these items: the Institute of Social Research is invariably mentioned; the Department of Scientific Research of the American Jewish Committee is invariably omitted ... it was pointed out that these studies were launched while you were on the staff of the American Jewish Committee ... that the books by Massing and Lowenthal were completely financed by the American Jewish Committee; that Massing was not even a regular member of the Institute but was hired to do this special job ... the [American Jewish] Committee likewise paid for editorial work on the books ... and completely subsidized the publication ... The inference was made that in both book reviews, in the sociology journal, and in the McWilliams book, Institute personnel were in a position of influence and that therefore the omission of reference to the Department of Scientific Research ... could not be regarded as accidental. The further inference was made that the chief aim of the Institute has been to seek its own self-aggrandizement even at the expense of other groups.<sup>67</sup>

Horkheimer was asked to immediately clear up the accusations and to make sure that in the future due reference was made to the role of the AJC. Contrary to his usual conduct, the addressee took his time before responding. In a letter to Adorno, who at the time was in Germany, the reason for this delay is explained, as were Horkheimer's countermeasures:

There was a number of reasons for me (to go to San Francisco). Given the fact that the Slawsons and the Flowermen keep foaming at the mouth because in the first reviews of the Series in the New York Times the Institute was highlighted rather than the Committee, I felt that the thing to do was to renew relations with Sanford and Brunswick by a personal visit – without, however, mentioning the affair itself. If the Committee wants to conspire, it would probably not help anyway, but ... I did it so as not to feel I neglected something.<sup>68</sup>

Horkheimer's response to Flowerman was a strange medley of servility and arrogance. He started out by denying that he or anybody else from the Institute had influenced the reviewers. But, then, he launched a counterattack by listing those publications and those articles in the press that mentioned the AJC



but not the Institute of Social Research. And, finally, he proposed to Flowerman that, in the beginning, the AJC had been rather fearful that there might be too much publicity. This letter, as well as others, also tells us that the AJC refused to abide by Horkheimer's request that the institute be cited on the jacket or the cover of TAP. In a letter to Slawson, Horkheimer rather brazenly argued that since the group no longer existed, there was no need any more to cite the Berkeley Public Opinion Study.<sup>69</sup>

As a result of the high visibility that the Frankfurt School temporarily enjoyed among the German-language audience, the history of the institute was not only written down but was also, and no less so, informed by self-portrayals, speeches, lectures, conferences, interviews and other forms of oral records. Among the published self-portrayals, Adorno's 'Wissenschaftliche Erfahrungen in Amerika' (Experiencing Research in America) (Adorno 1981) is a key contribution. The study of the first historian of the Frankfurt School, Martin Jay, which was published in English in 1973 and in German three years later, had a lasting impact on the American and, after the German version was published, on the German perceptions of the Frankfurt School (Jay 1973). Jay built on information by those members of the institute who were still living at the time, as well as on the selective exploitation of the writings of those who were already dead. Rolf Wiggershaus's even more detailed history of the Frankfurt School, it is true, addressed some of the more problematic aspects of the history of the institute as seen by insiders; but while he provided many more historical details than Jay, this did not do much to modify the institute's by now well established image (Wiggershaus 1986). All the historical accounts of the institute are almost exclusively based on the papers left behind by former members of the institute, and the prime source for this is the Max Horkheimer Archive, which not only houses the papers of its namesake but also a lot of material by others who had temporarily been affiliated with the institute.

By the late 1970s, the legend of the years of exile of the Institute of Social Research was firmly established, and was told more or less as follows:<sup>70</sup> the Frankfurt Institut für Sozialforschung, thanks to the foresight of its directors, had emigrated in good time, its independent funding allowing its members to settle first in New York, then in California, and to go on with their theoretical work as well as to support other emigrants. Its financial independency enabled the institute to keep its distance from the corrupting business of American social research. After some time, on their own initiative, they decided to address American issues. When reports on the almost complete extermination of European Jewish life began to register with the more general American public, they were charged by an honourable Jewish defence association to do a research project that investigated the fascist potential of an outwardly democratic society and, five years after the end of the war, delivered a five-volume report that provided evidence that anti-Semitism was a widespread phenomenon and a danger to democracy in the United States, as well. Having accomplished this task of enlightening the people, and having published a

philosophical manifesto, the *Dialectic of Enlightenment*, the key figures of the institute turned to a new challenge: re-establishing the institute in Frankfurt and participating in the democratic reconstruction of German universities and German society. Their transatlantic luggage included a suitcase with brand-new methods in social research, which they made it their job to use and to teach. Only when they could no longer deny that German social scientists were more interested in stabilizing capitalist exploitation than in an emancipatory overcoming of it, did they turn away from sociology and re-turn to philosophy.

Since there was no complete translation of TAP, this legend was all the easier to propagate. Horkheimer was initially interested in having a German translation and, in 1952, had Frankfurt students do a rough translation of all five volumes.<sup>71</sup> (Many years later, this version was distributed in a pirate edition.) Since the AJC was unwilling to subsidize the German edition, Horkheimer lost interest. Later, he and Adorno seem to have actually done their best to prevent the publication of a complete German edition of TAP. Rather, German readers were provided merely with a partial edition of less than one-fifth of the text. And after what has been described in the present chapter, it will come as no surprise that this translation included only those chapters where Adorno was either the author or had successfully laid claim to being included in the authors' quartet.

# 7

## RECONNAISSANCE EXPEDITIONS, RECONSTRUCTION SUPPORT AND THE RARE RETURN

Social scientists in the United States who wanted to contribute to the fight against the Nazi regime could do so in a broad variety of institutional contexts. After the attack on Pearl Harbor by the Japanese and the entry of the United States into the Second World War, the United States government rapidly created new institutions and substantially extended existing ones. Scientific expertise, it is true, had also been mobilized for the war effort in the First World War, but now, in contrast to the previous three decades, a large number of former citizens of enemy nations joined in the fight against the Nazis and their allies. Many of these new immigrants had been awarded US citizenship before the usual waiting period had expired, or came to benefit from special provisions that were set up to enable them to cooperate. The social scientists who actively participated in the current 'war effort' included not only psychologists, who once more deployed their testing tools. Rather the whole range of the social sciences that in the decades before had undergone so rapid a differentiation was represented. Even philosophers such as Herbert Marcuse, art historians such as Ernst Kris and cultural anthropologists such as Margaret Mead joined federal agencies such as the Office of Strategic Service, the Office of War Information or the Schools for Overseas Administration. Economists conducted studies exploring the situation of the population in Europe; communications researchers studied the effect of Joseph Goebbels' propaganda on the German population and the inhabitants of the occupied territories, and explored the possibilities of counter-propaganda; historians and political scientists were directly involved in the warfare as part of 'strategic bombing units', or participated in the planning of the postwar order in the countries to be occupied, and in the training of soldiers.

The Second World War arguably was the first war in which psychologists and other empirical social scientists were doing applied research in combat units or immediately behind the frontline, studying the morale of their own troops and interrogating prisoners to elicit information not only on military issues but also on the current morale of the Germans. Such top Nazi representatives as could be apprehended after the unconditional surrender of Germany were not actually laid on the psychoanalyst's couch, it is true, but were at least examined by professional psychologists. Given the incomparably more massive deployment of the United States government's agencies and staff of advisers, the private institutions, which, as funders of research, had in previous years been so important for the development of the social sciences, were pushed into the background. Even before the end of the war, philanthropic foundations such as the Rockefeller Foundation, which could boast long-term experience and contacts in Europe, started to discuss whether or not they should resume the support activities that had been brought to a standstill by the Nazis, and how to proceed under the changed conditions. Very soon after the surrender of Nazi Germany, experts who had been specifically recruited for this task went on reconnaissance missions to Germany and Austria where, unlike in the years before 1933–8, they primarily engaged in talks with representatives of the United States and British occupation authorities rather than with German and Austrian government officers or politicians. In addition to the reconnaissance missions charged with exploring the situation at the German and Austrian universities, émigré scholars submitted their own propositions on how to reconstruct the Teutonic universities. The first American guest professors came to Europe as early as in 1947/8, teaching in summer schools and at universities. Many of them wrote down their impressions. What these emissaries saw and deemed worth reporting may help us to understand the situation of social science research, its institutional structure and its human resources, after the Nazi purges. In the present chapter, we will analyse some of these travel reports before discussing the support the RF considered giving to sociological research in Germany and Austria, as well as the remarkable similarities that characterize the measures proposed for reorganizing the universities. As a conclusion, we will give an overview of those emigrants who decided to return to Germany and Austria.

#### THE ROCKEFELLER FOUNDATION'S EXPLORATION OF GERMANY AND AUSTRIA

At the end of the Second World War, unlike at the end of the First World War when philanthropic foundations in the United States first had to familiarize themselves with the situation in Europe, the RF could have simply resumed its support of German and Austrian scholars and research institutions that had come to a halt in the 1930s. The only barriers they would now have to overcome were the more than justified animosities with respect to the atrocities that had been committed under the Nazi dictatorship, news of which had already reached them before the end of the war but were now established beyond doubt. RF officers were indeed far from unanimous in how to deal

with the Germans and the Austrians, and controversial positions were openly discussed at the RF's New York headquarters. These discussions were based on two reports on the situation in Europe, and particularly in Germany and in Austria, written by Robert J. Havighurst after the two journeys that, as an RF emissary, had taken him to these two countries in the summer months of 1947 and again in 1948.<sup>1</sup>

Havighurst was an almost ideal choice for this mission. Born in 1900 in Wisconsin as the grandson of an immigrant from Germany, he had obtained his Ph.D. in chemistry and had then, benefiting from a grant by the National Research Council, proceeded to Harvard as a post-doctoral fellow. He had worked as an assistant professor of chemistry in Miami, but had then transferred to the University of Wisconsin, his home state, where he was appointed associate professor of physics. There, he became interested in issues of science education in secondary schools, whereupon he joined the General Education Board of the Rockefeller Foundation in 1934, first as an assistant director and, as of 1937, as a director. At the time, he was also in contact with the Princeton Radio Research Project and was among the audience of Adorno's lecture on 'A social critique of music' (see Chapter Five). In 1940, Havighurst was appointed professor for educational theory at the University of Chicago by its new president, Robert M. Hutchins, and chairman of the Interdepartmental Committee of Child Development. His interest in international comparative studies on educational issues and educational institutions, his German language proficiency and his competence as an interlocutor for scholars from a wide range of disciplines made for the particular quality of his reports on the situation in Germany and Austria. Nevertheless, his recommendations were subject to controversy among the RF officers. This was at least partially due to the fact that Havighurst was not the only expert the RF had sent on a reconnaissance mission.

Before Havighurst, Philip E. Mosely<sup>2</sup> and Norman S. Buchanan<sup>3</sup> had gone to Europe in July 1947.<sup>4</sup> Mosely, who like Havighurst had been specifically recruited as a consultant for this mission, and Buchanan, who was an associate director of the RF Social Sciences Division, visited not only Germany and Austria but also a number of other countries, among them Poland and Czechoslovakia, i.e. two countries that soon after would become part of Soviet Europe. One can be sure that the other RF officers read the diary they co-authored – unlike Havighurst, they seem to have written no final report – and that their opinion was of some weight in the discussions about future RF policies.

Buchanan, who wrote about his stay in Vienna in July 1947, started out by characterizing the most evident differences. In a country that was occupied by four powers, the Austrian government succeeded in playing off these four 'elements' against each other, and the Allied occupation forces were regularly blocking one another's action.<sup>5</sup> In economic terms, Austria was facing the problem, on the one hand, of the massive integration carried through after 1938 of the Austrian economy into the economy of the Third Reich, which no longer existed after the war, and, on the other hand, that Austria's economic

reconstruction was severely hampered by the country's division into occupied zones. The result, Buchanan said, was an attitude among the population that he described as follows:

For these and other reasons too the Austrians give the impression of being balked and frustrated at every turn. In fact, there is more than a suggestion of mass frustration in a psychological sense. Certainly the drive and force and determination so noticeable in Poland is lacking in Austria. People seem to stress the difficulties in any proposed line of action and nearly to imply that the situation is hopeless. At least NSB [Buchanan] was impressed with the bland way in which people looked forward to a month of summer-long vacation under present circumstances and with the generally leisurely pace of work. This may be unfair. But with so much to be done on every hand one wonders if a little higher tempo of work is not to be expected. (The usual answer here is that people have not enough to eat or that the custom is of long standing.)<sup>6</sup>

Although these were the years of a near-endemic production of studies on national character, no such study was written on Austria, as far as I know.<sup>7</sup> The lines quoted above as well as the rest of Buchanan's description, however, indeed qualify as a draft for a study on Austrian national character. Grumbling and lack of initiative, and sticking to traditional institutions – such as going on one's summer holiday even under conditions that seemed prohibitive – were paired with excuses when it came to explanations why something could not be done. What is remarkable is Buchanan's reference to Poland and the Polish readiness, in the same year, to show initiative and engage in action, given the well-known fact that it would take four decades of waiting for the Polish people to be allowed to make use of these skills.

These difficulties were further enhanced by a 'dearth of competent people' due to forced displacement by the Nazis, German occupation and denazification. At the universities, faculties were a sad sight, aggravated by the fact that junior and middle-aged scientists 'are just not there'. As an illustration of this general impression of the situation at Austrian universities, Buchanan described his personal impression of the people with whom he had talks during this visit (in the diary, as shown below, he not only characterized their personalities but also indicated their age, disciplinary affiliation and other institutional characteristics).

Prof. [Alfred] Verdross-Drossberg ... Vigorous personality, well spoken of by certain students ... Might be worth trip to USA.

Prof. R[oland] Grassberger, former fellow ... Besides books and journals he specifically asked for some films on the development of crime situation. Seems earnest and intelligent.

Prof. [August M.] Knoll ... dismissed by Nazis, appears beaten although only about 50.

Prof. Hans Mayer, once quite a figure perhaps but at 70 seems dignified but uninteresting.

Prof. [Ferdinand] Degenfeld-Schonburg ... elderly and quite ineffectual. Probably never was much good according to [Oskar] Morgenstern.

Dr. [Alexander] Mahr, former RF fellow who has not yet been reinstated because he applied for membership in Nazi party. Although not a professor he was presented by the Rector along with the others. Not a strong character on first acquaintance. Morgenstern gives him a bad billing.

Prof. [Ferdinand] Westphalen ... is perhaps the best of the academic economists, about 48, would like to visit US in early 1948. Interested in social problems and also in theory.

These brief characterizations clearly show the traditional approach adopted by RF officers: to search for persons who seemed worth supporting and to rely on competent peers to evaluate their scientific quality, as in the case of Morgenstern, a longstanding RF partner. (At the time, Morgenstern was in Vienna teaching at a summer school.) When urged by Buchanan to name some junior scientists who might be granted a fellowship, none of these professors was able to suggest any names (see Chapter Two).

Buchanan got a somewhat more favourable impression on visiting the Institut für Konjunkturforschung (Institute of Business Cycle Research), re-baptized Institut für Wirtschaftsforschung (Institute of Economic Research), which had received RF support until 1938 (see Chapter Three). Mosely went on to visit Graz, Salzburg and Innsbruck. With respect to Graz, he noted that 'new blood' was particularly important, because this town and its university had much more strongly succumbed to Nazism than the other Austrian towns.

After his first visit to Germany in the summer of 1947, Havighurst described the situation of the social sciences in Germany by saying that 'research and teaching in the social sciences in Germany are now at an unbelievably low level of quality and quantity'.<sup>8</sup> While at American universities the social sciences were 'a well-organized and integrated set of departments of economics, political science, sociology, social anthropology, and psychology, with certain aspects of history and geography', nothing like this could be found at German universities. Frankfurt and Munich were the only two universities in the Western zones that had 'a faculty or division of social sciences'. At all the other West German universities, the social sciences were still divided between the law and the arts faculties. Sociology, which before 1933 had been represented by 'substantial centers' at Frankfurt, Cologne, Leipzig and Berlin, had 'suffered more than any other individual disciplines' from Nazi rule. Psychology, parts of which had shown such a remarkable progress before 1933, had been annexed by the Wehrmacht and, having lost the leading representatives of gestalt psychology to the United States, had almost completely disappeared from the universities. 'Modern history' was represented by a very small number of names, because most of the historians had been dismissed for being Nazis. Political science had been perverted by the Nazis, and 'social anthropology' 'worthy of the name' was currently non-existent in Germany. In the field of economics, the Nazis had been somewhat less destructive, and

at least the Institut für Weltwirtschaft (Institute for World Economics) in Kiel had more or less survived. However, while it was well provided in terms of personnel, economic research itself was 'at a relatively low level compared with American standards'. Applied and pure statistics were taught only in Munich while the Russian zone could boast no less than four chairs dedicated to this discipline.

Havighurst's not-so-surprising diagnosis was followed by some reflections on the potential role of the social sciences in the reconstruction of Germany. In all modern societies and, thus, in Germany as well, the social sciences had to fulfil a double task: '(1) to provide data and analyses of economic, political, and social processes, (2) to produce informed, active, and moral citizens.' It was obvious that in Germany, the social sciences were currently conducting no adequate 'fact-finding and factual analysis'. This opinion was shared by German social scientists, as well. In the domain that would later be institutionalized under the heading of '*Sozialkunde*' (social studies) or '*Staatsbürgerkunde*' (civics), the shortcomings were even more appalling. As an illustration, Havighurst related an episode he had witnessed at a place he called 'University of A' during a discussion with students (and teachers-to-be) of English and American Studies. During a debate in which he had participated, a female student who had just returned from a one-month study stay in England had told her fellow students that, to her – still obvious – amazement, there were factories in England where all the workers had to be union members. 'This struck her as being quite undemocratic and she said: "I thought that sort of thing occurred only under Hitler." I asked her whether she knew what was meant by the "closed shop" and it turned out that neither she nor any of the others were acquainted with this concept.'

Since university students, due to the German system of higher education, needed to specialize at a very early stage, they were lacking in a more general education that would include the social sciences. What these students knew was what they had been, in the best of cases, taught in their history classes in secondary school, but now:

they were all going to teach German boys and girls about English and American institutions through the medium of the language and literature of these countries. It seems to me that they were quite unprepared for this responsibility, not that they should take anything they find in Britain or America as ipso facto democratic but rather that they should have sufficient general knowledge about economic, social, and political life in the modern world.

Havighurst's friendly attitude towards Germany and the Germans that characterized his propositions did not meet with undivided approval among the RF officers. In their discussions and written documents, they were primarily concerned with the moral side of any support that might be given to Germany. Robert S. Morison, who had worked for the RF in both the Medical Sciences



and the Natural Sciences Divisions, used the metaphors of his discipline, medicine, to drive home his point of view:

My first impulse last year was to regard the question of RF aid to Germany as in no way different from that of aid to any European country. Such a course would have been in line with AG's [Allen Gregg, Director of the RF Medical Sciences Division] proposition that a physician does not censure or punish his patients. A physician is, however, under obligation not only to his patient but to society at large. No matter how much sympathy he has for the individual nor how fully he understands his wishes to injure or kill those about him, he must discourage these wishes and, if they prove intractable, isolate the individual from society. It is this second consideration that has slowly convinced me that the giving of aid to Germany must be regarded as a special problem. We ordinarily proceed on several assumptions but the one which is most important for our present purpose reads as follows: New knowledge of whatever kind, so long as it is 'true', will, through the ordinary processes of society, be put to use for the welfare of mankind. The history of the last half century raises doubts as to whether this proposition can be accepted in regard to Germany without qualifications so serious as to make it inoperative at least for the next few years.<sup>9</sup>

While in the years before the Second World War the RF had indeed shied away from doing anything that could have been considered as directly opposed to US foreign policy, the policies adopted by the respective governments had nevertheless only played a minor role in the Foundation's decision-making. This changed in the years after 1945, when the RF like other foundations sought to align their approach with that of US policies even at its planning stage, which did not prevent RF officers from committing to paper some rather harsh comments on their compatriots in the occupation authorities.<sup>10</sup> RF participation in the United States policies of 'inculcation of "democratic principles"', however, would now imply – or so Morison felt – that the RF had to use quite different methods. While in democratic societies increased scientific productivity would 'in all probability' serve to consolidate the democratic layout of the society in question, it would only fortify the 'citadels of reaction and special privilege' in a 'stratified dictatorial society'. Its effects, which were obvious when they led to a gain in power for the dictator, could also be brought about in a more subtle way:

by increasing the prestige of the Wissenschaftler [scientist] so that they became in effect a priestly class separated from the people and ministering to the needs of an impersonal and, if I may say so, amoral state. The fact that some of these scientists and scholars presented to world at large with discoveries of considerable advantage to the race as a whole admittedly arouses our gratitude and commands our sympathy. This should not blind us to the fact that in Germany the primary motivation of the scientist was the personal one of increasing his own status in a pathological and dangerous system. Even today the majority of pleas for support which come to us from Germany are for aid for MY laboratory and MY library.

Therefore, Morison said, while he supported two of Havighurst's propositions that served the democratic education of German youth, he was opposed

to any aid for German researchers, if only because 'the gap between geheimrat [privy councillor] and ordinary citizen is still much too wide'.<sup>11</sup> Once a 'sound self-supporting democratic Germany' had been achieved, support for German research could be resumed.

The directors of the Natural Sciences and the Social Sciences Divisions, Warren Weaver<sup>12</sup> and Joseph Willits,<sup>13</sup> expressed their views in two memoranda that followed shortly one after another, with Willits proceeding first. In his résumé of previous discussions within the RF, he attacked Weaver in an uncommonly sharp way, hoping that the latter would feel prompted to clarify his position. Just as Morison had done before him he called to mind the guiding principle of promoting 'human welfare throughout the world'. He accused Weaver of having on several occasions expressed opinions that amounted to seeing the German nation as a monolithic entity and, thus, indiscriminately holding all Germans responsible for the Nazis. Weaver, he wrote, seemed to abide by an Old Testament view in proposing that, for the Germans, continued suffering was only right and just. The consequence for RF policies would be to limit itself to small-scale aid, at most. Willits then proposed a counter-position to this – admittedly 'exaggerated' – representation, substantiating it by arguments on three levels. In terms of morals, he rejected any reasoning in monolithic blocks and advocated centring on individuals, since only individuals could be subjected to judgements of guilt or innocence. Germans under twenty should not be treated on the same terms as older Germans who had incurred guilt. It was the task of 'scholarship (and religion)' to always differentiate and be oriented to the future. The RF, rather than modelling its policies on the Old Testament and the War Department, should be oriented to the New Testament and to the spirit of the American Quakers. In terms of self-interest, it should be kept in mind that it was in America's own interest to have Europe prosper and that, as General Marshall had said, a productive Germany was an essential prerequisite for 'a prosperous Europe and a world in peace'. Somewhat incongruously at this point, Willits referred to the findings of modern criminology; while punishment had its place, the priority was on reform. Finally, Willits pointed out that all policies also had repercussions for their authors. One of the subtle effects of wars was that they had dire consequences not only for those who were defeated but also for those who had won and, thus, one had to ask what these consequences were in the case of the United States: reliance on power politics, 'top-dog mindedness', too narrow a proximity of the sciences to politics and the military.<sup>14</sup>

Within a week, Weaver responded by a memorandum of his own. He started out by denying that he subscribed to the three opinions attributed to him by Willits and excused himself for not having made himself sufficiently clear in the discussions. After all, he neither thought that Germany was a monolithic block nor advocated relying on the morals of the Old Testament: 'I think that, by New Testament standards, the Nazis sinned foully, and that all German bear some share of the responsibility. I think that for this they will inevitably suffer, have suffered, and should suffer. I think that all the world must to some extent share this suffering.'<sup>15</sup>

The differences, for Weaver, concerned the first steps to be taken. Given famine, houses without heating, school children without pencils and libraries without books, it was irresponsible to funnel aid to 'projects in the upper reaches of advanced scholarship', and all the more so since 'advanced knowledge' could be used for good as well as for evil purposes. Therefore, he felt that the RF should support activities that would benefit young Germans and familiarize them with democracy. After one year, the Foundation's policies could be reoriented, if need be.

Comparing the RF officers' evaluations of the situation in Germany after the end of the First World War (see Chapter One) with their perspective on the same country and, partly, the same professors in the first years after the end of Nazi rule, it becomes evident that German science and the German university were no longer considered as high-ranking, let alone exemplary. Since all German scholars were discredited due to their self-surrender to Nazi rule, it no longer seemed appropriate to stick to the neat separation that conceived of science and politics as two distinct spheres. During the early 1920s US aid programmes were oriented to providing famished scholars with a livelihood, and since their scientific reputations were beyond doubt, to allow them to resume or continue their work. But after the Second World War they shied away from any such step. As a result of the excessive politicization of the sciences during the Nazi period, RF officers were unwilling to abide by the traditional separation into two worlds: the world of pure science and the – accidentally contaminated – world of politics. The idea of freedom from value judgements – and its corollary, namely that individuals, provided they were willing to stick to a clear distinction between the two worlds, could compromise themselves in one of them without compromising their reputation in the other one – no longer worked as a defence strategy.

Furthermore, it is evident from the post-1945 discussions that the social structure of German science was seen in a more critical light than after 1918. In those days, a transatlantic visitor was surprised to see that such a thing as a 'high table' had to be institutionalized to get German professors to engage in a dialogue with one another at all. But neither he nor his American colleagues would, at the time, have ventured to describe the German mandarins' obsession with status as something particularly pathological. Since it is rather unlikely that German professors after 1945 actually differed from their colleagues (or from themselves) in the Weimar Republic in this respect, this change in perspective is no doubt due to the experiences the observers had had in the meantime. The massive expansion of post-secondary education in the United States and the growing number of people engaged in scientific work, as well as American scholars' growing tendency to avoid social closure, had resulted in a more democratic and more egalitarian habitus that made the traditional habitus of the German mandarins seem even stranger than in 1920.

After 1945, this motive now linked up with another one to which US politics had subscribed after some hesitation. In the discussions about the politics to adopt toward a defeated Germany, the Morgenthau Plan, which advocated

the de-industrialization of Germany and the downsizing of its economy to the pre-modern level of a Central European agrarian state, was given priority only for a very short time (while its weight in anti-American propaganda in Germany was greater by far).<sup>16</sup> The alternative of 'reorientation', soon re-baptized 're-education', was no doubt also strengthened through the reception of social science studies analysing the ascent of Nazism, which was no longer seen as a dictatorship over the Germans but as a dictatorship relying on the participation and approval of the German masses. A case in point is the early study by Emil Lederer, which his friend and follower, Hans Speier, edited from his posthumous papers under the title of *The State of the Masses*. With respect to the formulation of US policies towards Germany, Speier's influence was probably more important than that of the studies of former collaborators of the Frankfurt Institut für Sozialforschung.<sup>17</sup> As soon as the perspective on Nazi Germany was no longer constrained by the double concept of an 'evil Führer versus the seduced (or even oppressed) masses', the question necessarily arose of what to do with the surviving population and how to undo the Gordian knot of their moral involvement with Nazism.

Those subsequent authors who insisted that, after 1945, the Americans dedicated themselves to missionary work in Germany, deliberately refrained from even suggesting that there might have been a viable alternative to 're-education'. And those latter-day Alexanders who thought they possessed the sword that would allow them to smash capitalism and, in doing so, to eliminate fascism as well, have been sufficiently disavowed by world history. Viewed in this light, it is not surprising that in the years after 1945 American foundations and government agencies in occupied postwar Germany acted in close cooperation. They were united by more than their citizenship, and the frequent exchange of personnel between these two worlds is proof enough of the existence of a shared frame of reference (cf. Berghahn 2001; Gemelli and MacLeod 2003).

As a matter of fact, RF efforts in Germany and Austria were initially confined to those activities that were considered compatible with 're-education' policies.<sup>18</sup> In the end, however, the Program of European Rehabilitation largely corresponded to what Havighurst had suggested. To begin with, American social scientists, and among them primarily European emigrants, were sent as guest professors to Germany and, less frequently, Austria: Karl Brandt went to Heidelberg for a whole year, Arnold Brecht, Curt Bondy, Waldemar Gurian, Eduard Heimann and others went to various German universities for shorter periods.<sup>19</sup>

In the summer of 1948, Havighurst spent several weeks in Central Europe and, this time, also explored the needs of the scholars he contacted. From Zurich, he went first to Austria, where he met former RF fellows, in keeping with RF traditions, but he also attended the meetings of the leftist youth organizations Rote Falken and Freie Österreichische Jugend, which impressed him more strongly than his talks with the state officials. A scholar

whom neither he nor other RF officers had met before was young Vienna psychologist Walter Toman who, having benefited from a Quaker Fellowship to study at Haverford College, Pennsylvania, in the previous year, had returned to Vienna with a reference library worth \$150 (\$1,300 in 2010), which he felt to be an 'adequate library to begin with'. Havighurst's diary entry on his meeting with Toman notes that he was impressed to see 'how much a fellowship can do to influence the teaching in a University'.<sup>20</sup> In contrast, conditions for those who had not been offered any such opportunity were appalling (Fleck 2005).

The malaise of Austrian universities in the Second Republic and the waste of human capital due to the protectionism of conservative associations have been deplored more than once. It was associated both with a craving for status, which the American delegate's diary highlights along with the widespread indifference towards scientific research. When, at the request of the local Director of Education of the US Forces in Austria (USFA), Havighurst went to see the professors of economics, only two of the three of them were present (Hans Mayer was on vacation), but 'they seemed to have nothing special to say'. The diary comments on the meeting with Ferdinand Degenfeld-Schonburg and Alexander Mahr as follows:

Count Degenfeld is a man of about 55, and Mahr must be about 45. M. had an RF fellowship about 1930 for work at Columbia and Wisconsin.

D asked RJH whether he was interested in research matters or teaching matters. When RJH said 'research', D started to talk about the great amount of teaching they have to do, and steered away from talk of research. He said that Mayr [Mayer] has 100 students in his seminar and he (Degenfeld-Schonburg) also has 100. Many of these are now writing doctoral dissertations, and they take a good deal of time of professors. A very few will go ahead to do further scientific work, and they are good people ...

American books have been received with thanks by the Institute [für Wirtschaftswissenschaften], but they need journals, especially the *American Economic Review*. When RJH said that this journal is coming currently to the University Library (*in the same building*), they displayed little interest, and said they never used the University Library. They asked whether there was a chance of their getting American journals for the Institute, and RJH said he thought it was a matter for them as University teachers to work out a way of using the journals which go to the University Library.<sup>21</sup>

That same evening, in a 'beautiful villa in the northern part of the city, which belonged to a leading Nazi' and was now used by the United States liaison officer charged with educational issues, Havighurst also met the head of the Department of Higher Education of the Vienna Ministry of Education, who had been ill and, thus, unable to attend Havighurst's official visit to the Ministry. Baron Otto Skrbensky made ten minutes time, after dinner, for the American guest to tell him that:

there were many small needs in Austria with which he thought the RF should not be bothered. As the economic basis becomes stronger, these will be met automatically. But there are certain substantial projects that he would like to commend to RF attention. First would be a great Library, perhaps to be called the 'Rockefeller Library', to house the present National Bibliothek and the Universitaets Bibliothek.

Disregarding the fact, bewildering though it is, that even after 1945 Austrians obviously thought it a matter of importance, notwithstanding legal prohibitions, to impress American interlocutors with their titles of nobility, it is at any rate noteworthy that the top official in charge of Austrian higher education tried to win the American emissary over for a major project that would be of little use to research (and which, with or without the name of the American philanthropist family, has not been realized to the present day). He did this rather than present Havighurst with concrete issues that could be solved and the existence of which he could not possibly ignore, since all the other Austrian interlocutors actually bombarded Havighurst with just this. One will fail to understand this attitude unless one considers that in the 1940s and 1950s, conservative Austrian politicians and officials felt that Europe was being endangered by the West as well as by the East. For this, Skrbensky's successor Heinrich Drimmel, who had been his closest associate and would later become Minister of Education himself, coined the inimitable expression of 'negative good will'. The low interest, to put it mildly, in good relations with the RF emissary shown by the Austrian Minister of Education was also noted by the Assistant Director of the Humanities Division, Edward F. D'Arms.<sup>22</sup> During his stay in Vienna in November 1947, he also had a talk with Skrbensky (Minister Felix Hurdes was tied up with other matters) which, however, was 'quite unsatisfactory', because Skrbensky was on the phone most of the time – 'The general impression was one of good will, but uncertainty and confusion.'<sup>23</sup>

Actually, it would have been much easier for the Austrians than for the Germans to obtain support by the RF because, at the time, official US policies tended to treat 'liberated' Austria significantly better than Germany, with which the United States was, *de jure*, still at war. This fact did indeed have a negative impact in the early years on the willingness of the RF to grant financial support to German scholars or institutions. In his final report, Havighurst noted that the economic situation in Austria was better than in Germany. Bombing damage on buildings was less severe and would be repaired in a few months, the Austrian economy was functioning quite satisfactorily since support by the Marshall plan had set in, inflation had been brought to a standstill, prices and wages were under control, and the food supply was sufficient. On the political level, the situation was stable and the morale of the population was higher than in Germany, since the Austrians had come to terms with their status of having been 'liberated', 'Austria is more free, in the psychological sense, since most Austrians regard themselves as "liberated" and as not

responsible for the coming of the Nazis into power. Accordingly they deal with foreigners with more freedom and frankness than do the Germans.<sup>24</sup>

Only the situation at the universities, and especially in Vienna, was a matter of concern, since there old men 'who are looking to the past' were in charge. There were few middle-aged scholars, and 'few of them outstanding', while those younger ones who were 'of first rate quality did not yet have a chance to exert leadership'. In this context, Havighurst once more referred to psychologist Toman who had obviously impressed him very much:

Although only an Assistant in the University, he has started seminars or workgroups in three areas – clinical psychology, social psychology, and statistics. In these work groups he has university students and members of the city vocational guidance staff ... He expects to learn along with his seminar members. Toman is also getting a didactic psychoanalysis, and several of his seminar members are doing likewise. It is quite possible that Toman will be able to found a modern department of psychology within a few years, in Vienna, if given a little assistance.

A few years later, Toman accepted a position in the United States, after which he returned to Germany in the late 1950s. For a short time in the early 1960s he acted as director of the Institut für Höhere Studien (Institute for Advanced Studies) in Vienna, but soon left his home town again (cf. Fleck 2000).

As for the senior professors, Havighurst wrote, no initiative was to be expected from them; the only thing they showed any interest in was obtaining some funds to allow them to publish the works they had previously written. What was needed most for Austria was to establish ties with the rest of the world, which included stays abroad for Austrian students as well as improving their access to scientific publications. Finally, future RF activities should focus on the support of the younger scholars. Havighurst reported an American general as saying that, 'we and the Austrians often feel that we are "at the end of the line" and that nobody pays any attention to us as compared with Germany and Greece'.

However, in the years that followed, in spite of these more favourable material and psychological conditions, Austria failed to attract the support from abroad (i.e. the occupying powers) that might have allowed it to overcome the sentiment described by the American general. The interest shown by the Western powers was consistently low, with negative effects especially in the fields of science and education. Left to their own devices, the Austrian universities were primarily dominated by those forces that were mainly interested in restoring their supposed past glory. As a consequence, there was a return of the Nazis and an almost complete severance from the scientific development in Western countries, accompanied by a new wave of migration by young intellectuals.

In the late summer of 1948 Havighurst left Austria to visit West Germany. There, his diary tells us, he met returning migrants at various places who were

trying to reconstruct the universities and university education, partly in cooperation with American guest professors, and energetically supported by the social science experts in the occupation authorities. In Frankfurt, Havighurst met colleagues from Chicago who participated in the exchange programme set up for the professors of these two universities; in Heidelberg, he had talks with agrarian economist Karl Brandt; and in Freiburg he met Friedrich Lutz, who had returned from Princeton as a guest professor and later acted as an unofficial RF advisor for German matters.<sup>25</sup> American social scientists, some of whom, such as Nels Anderson, Howard Becker and Edward Y. Hartshorne Jr, worked in the military government while others, such as Everett C. Hughes and Conrad Arensberg, were guest professors, confirmed Havighurst in his impression that there were at least some institutions worth supporting.

### EVERETT HUGHES'S ATTEMPT TO EXPORT CHICAGO SOCIOLOGY TO GERMANY

The most interesting case of individual reconstruction support in postwar Germany is that of Everett C. Hughes, the first American sociologist to come to Germany as a guest professor in the summer semester of 1948. He had already been to Germany in 1932, studying the Catholic workers' movement in the Ruhr area as a post-doctoral fellow of the Social Science Research Council. Now, at age fifty, he was back once more in Germany. Along with him, six other colleagues, among them psychologist Louis L. Thurstone, participated in the exchange programme set up by the University of Chicago for the University of Frankfurt, its costs being borne, in the first years, by the RF.

Hughes was a man for whom everything he did was at the same time an opportunity for field research, an attitude which, as a student at the Chicago department, he had adopted from his teacher and role model Robert Park, who was said to do research even on a taxi ride (Lindner 1990). Hughes, however, was anything but the stereotypical American. He admired Georg Simmel's sociological style. He had been in personal contact with German sociologists during his first stay in Germany and read their works. His style of research and his texts resembled essays rather than the conventional sociological research reports. Thus, he strongly contrasted with the type of American empirical social research the emergence of which had been actively promoted by emigrants such as Lazarsfeld and to which also the group of the authors of *The Authoritarian Personality* was committed. Hughes later wanted to rework the notes written during his stay in Germany and to publish them as a book, but abandoned the project when he had it firmly pointed out to him, by an editor of the University of Chicago Press, that there was no public for this kind of report. The diary notes and the thematic memoranda written for the book illustrate what the situation in postwar Germany was like in the eyes of a detached observer.<sup>26</sup> In addition, they provide a number of surprising insights into the history of sociology, in general, and into the controversial question of whether, how and to what degree autochthonous German sociology – i.e. conceived of in the tradition of the so-called '*Geisteswissenschaften*' – was being



'Americanized', in particular. We must here limit ourselves to those parts of Hughes's notes that are relevant for the present context:

Twenty-four hours after leaving Chicago, I was being driven from the Frankfurt airport to a hotel with a German student as a companion. He immediately asked ... what Americans think of Germans. Soon we passed some nice suburban villas which had been damaged by bombs, and I made some banal remarks about it, as I imagine anyone who hasn't seen bomb-damage is likely to do. The young man said that Mainz was bombed much worse than Frankfurt. I supposed it was because there was more industry in Mainz. 'Oh, no', he replied, 'that wasn't the reason. It was because Frankfurt was a Jewish city and the international Jews would not allow it to be bombed.'<sup>27</sup>

No initiation into the field could have been more perfectly planned. A few days later Hughes, quite unruffled, started his teaching programme in German: a lecture that had been announced as 'Problems and methods of American sociology' and a seminar intended to allow for a 'Closer review of some American investigations, with field work'. On 12 April 1948, Hughes noted in his diary:

The first lecture is over. It was about like my first lectures to French students in Quebec: a little stiff at first, but at the end of fifteen or twenty minute I got the courage to walk away from my Mss. And start practicing my mistakes harder and with gestures. I jumped at once into the forbidden subject by saying that the leading theme of American sociology grew out of the leading theme of American life – the relations of a variety of ethnic and racial groups to each other. I am convinced it is the right way.<sup>28</sup>

In alluding to Quebec, Hughes referred to the fact that in 1942/3, he had been a guest professor at the Université Laval and had apparently lectured in French. With very few exceptions, Hughes had no difficulty in speaking German with the Germans, seeking every occasion to engage them in talks with him, and it seems that some of his interlocutors even failed to notice that this lean, tall man was a foreigner. Only when certain specialist terms came up was he momentarily at a loss to find the German equivalent ('Boy, did I have a time getting "frame of reference" over in German').<sup>29</sup> In the seminar, too, Hughes overturned his plans in the very first session when he noticed that the majority of his audience was not majoring in sociology but in economics and business management.

I decided to turn the seminar toward problems of occupations and work organization. That seems to please the students, and after all, sociology is sociology whether you do it on one institution or another. The students here must ... have a 'praxis' – a job in industry or business. So I will put them to work on reports about the organizations they have worked in. I have already mentioned race questions, Jews, military occupation as sociological phenomena, just as they come up in discussion. The formula of frankness, avoiding no subjects whatsoever, is popular with the students.<sup>30</sup>

This attitude of 'frankness' was due, rather than to an effort on the part of Hughes to comply with the young Germans' struggle for orientation and moral absolution, to his methodological persuasion, the 'sweeping comparison'. This frankness made it easier for him to access his students who, however, were rather reluctant to follow his unorthodox teaching method. The invitation to analyse their own experiences as soldiers, working students and displaced persons in sociological terms was acted upon in oral discussions rather than in seminar papers. The help offered by Hughes, i.e. to use the studies he had brought along as a basis for comparison, was only reluctantly accepted, and it again required Hughes to direct his students' attention to the similarities between black job entrants in the United States industry and the problems accompanying the integration of displaced persons in the West German industrial working process, or to the structural similarity between the classification of the negroes in the United States and that of Jewish people in Nazi racial legislation. In the debates launched by Hughes, however, the students ventured to talk about their experiences in the Nazi youth organization ('Hitler-Jugend'), in the armed forces, with their parents' educational styles, on the black market and with '*Schiebern*' (black-marketeers). Being the sons and daughters of businessmen and white-collar workers, the idea of being forced to violate rules and regulations very much frightened them, while at the same time, they no less strongly insisted that it was easier for the lower classes to do so. When Hughes asked the students to compare their difficulties with the centuries-old tribulations of the Jewish population and their status as pariahs, however, they fell silent. 'I said that perhaps the Germans were now in a position to understand and sympathize with the Jews, who have long been in a position like the one they are describing, where one has to be very inventive to survive. They got the point, and talked of it very briefly – but were not too comfortable.'<sup>31</sup>

As a teacher, Hughes was most effective in more informal contacts. He had set up his workplace in a corner of one of the corridors at the university so that students could come to him at all times, but this invitation had to be repeated several times before they dared to trust the guest professor's spatial arrangement.<sup>32</sup> Rather than simply address him, they asked an assistant whether they were permitted to do so. In addition to informal seminars in the corridor, Hughes organized extra-curricular meetings in some students' lodgings, where he contributed the coffee, and he accepted invitations by his German colleagues to attend their seminars. When in one of these seminars the professor had to leave early and let his assistant take over, Hughes was appalled by the assistant's strange behaviour.

Dr. C., the assistant, was obviously taking his opportunity to be free, and punish, in the absence of his master. And the whole class was being more free than they would have dared. The main point, to me, was the revelation of so much suppressed heat, an almost hysterical desire to discuss. I finally broke in with two observations: 1) that it was very interesting to be accidentally present in a seminar when the professor wasn't there, and 2) that the question whether a student should introduce his own

ideas in the course of a report on a book could be a matter of such heated discussion, struck me also as rather odd. This made hell break loose. The assistant, Dr. C., then oddly changed his ground and lambasted the group for not speaking their mind when the professor was there – which was a contradiction of his earlier criticism of the report of the day.<sup>33</sup>

Hughes more than once reflected on the problematic position of the assistants, their almost complete subordination to the professor's regime and their submissiveness in adapting to this situation. When Leopold von Wiese, whom he had come to know personally in his years as a visiting student in the 1930s, described how difficult it was for him to find a successor, Hughes, who had come with Talcott Parsons to see von Wiese, asked him why the assistant whom he had so highly praised should not be considered for this position. The doyen of German sociology shrugged and replied: of course he could fill this position, but due to his military service, the 32-year-old had not yet obtained his doctorate and that, therefore, it was quite out of the question for him to be appointed. Dismissing well-worn routines in order to promote a good man without a *Habilitation*, let alone a doctorate, was simply unthinkable, he argued, and it would take the assistant at least three or four more years to clear these two hurdles.

The precision Hughes had learned to bring to his observation is also evident in his descriptions of the 'eternal assistants'. What struck him most was their habitual adaptation to their positions as subordinates. At a party he met a German colleague whom he had already come to know in 1932 in a sociological seminar. At the time, the young man had been a promising candidate for an academic career, which after the take-over of power by the Nazis had turned out to be impossible for him because he was Jewish. He had survived the Nazi years in Swiss exile, struggling along at the margins of the academic universe:

He is now back in Germany, and – although bald and forty – still an Assistant. He has not the right to lecture, – i.e. has not been appointed dozent [university lecturer]. Because of his maturity, he is often delegated to give lectures by the professor for whom he works. He was one of a party of four at lunch a few days ago; I was the host. Although I was the host, and had invited him as a colleague, he continued to play the role of Assistant. He grabbed my lighter a couple of time while I was holding it for him to light a cigarette; having grabbed it, he held it for me and then the others present to light up. When I passed sugar for his coffee, – purposely offering it to him first as an additional gesture of not wanting him to play the Assistant role of deference – he could not bring himself to take it first, but half stood up at table to pass it first to the others present. He is nearly as old as the professor whose Assistant he is, and is as good a scholar.<sup>34</sup>

The description brings to mind a drawing by Paul Klee, *Two Men Meet, Each Believing the Other to Be of Higher Rank*, which shows two men of equal rank who offer one another precedence in entering an imaginary room by out-bowing each other, doubling over to knee height. For the book he planned,

Hughes later reworked the plight of junior scholars, on which he more than once commented in his diary, into a portrait of the eternal assistant:

I found that most seminars still have an Assistant, usually a very bookish person who ought to have become an instructor years ago. He carries the professor's papers, bows, hands the professor things, speaks in a very formal, over-academic way. His hands usually are moist. Some things haven't changed. My assistant, Fräulein Becker, was indispensable; she is eager to do empirical work on the consumption habits of various classes of people.<sup>35</sup>

While there obviously was no way for Hughes to persuade his students to go out and do empirical research, it seemed no less obvious to him that no one endowed with the habitual frame of mind of the eternal assistant would be able to do so, in the first place. And this in spite of the fact that the situation in postwar Germany seemed to be almost prototypically calling for empirical social research:

After all, American sociology grew up around the study of crisis, personal and social, – the immigrant, race relations, social movements, etc. Thomas and Park both were interested in what happened when the 'cake of custom' was broken. So why shouldn't our concepts – if there are any good – be useful in understanding present-moment Germany. Those who object so much to the use of the word 'caste' to American race relations, should take a look at a military occupation, and see how many insights American race relations theory gives one into what is going on here. It would be perfectly silly to try to account for the relations between Americans and German here just on the basis of one group trying to get an economic or political advantage over the other.<sup>36</sup>

Similarly, Hughes told his students that they were in the favourable situation to make a significant practical as well as scientific contribution by exploring the realities of life in Germany, because 'the cake of custom is so broken that human behaviour can be seen in flux'.<sup>37</sup> But while the assistants dedicated themselves to the study of old books, and the older the better, the students complained that they did not even know how to construct a questionnaire, eliciting a deep sigh – '(God save us)' – from Hughes in his 'field note'.

In the four months Hughes spent in Germany – he returned there two more times during the tenure of the exchange programme, in 1953 and in 1958 – he also visited other university towns and met acquaintances from the past who, having lost their homes in Berlin or elsewhere due to the bombings, had sought refuge in the Frankfurt environs. He admired von Wiese, albeit more as a great old teacher than as a sociologist, given that his teaching programme was no less strongly marked by the appalling separation of theory and empirical work. Ludwig Neundörfer and his private Soziographisches Institut (Sociographical Institute) in Frankfurt impressed him with his attempt to do urban research on a statistical basis (at the institute, Hughes saw boards visualizing intra-urban migration between the home and the workplace, and

analysing the religious intermix brought about by the presence of displaced persons among the population in Hessen). In Marburg, he met Max Graf zu Solms, who is mentioned very cordially in the diary. In Frankfurt and Göttingen he was much more at home with the students of ethnology than with their teachers, with whom it was impossible to discuss social anthropology. To the teachers he had to spell out the difference between the Nazi concept of '*Lebensraum*' ('vital space') and related anthropological models, and had to firmly keep them from addressing him as a representative of a superior race. Visits to Münster and Dortmund allowed him to familiarize himself with the work of the Sozialforschungsstelle (Centre for Social Research), which he felt to be the most promising German centre for social science research. He was especially interested in their studies on displaced persons and on the problems of industrial work, which prompted him to promise them to act as a consultant at a future visit.

One semester of teaching is unlikely to have much of a sustainable effect, and no doubt Hughes knew this as well as anybody. Back in the United States, therefore, his interest was more in his own role in occupied Germany. At the same time, he made an attempt to grasp the German population's attitude towards their recent past in sociological terms. Concerning his German experience, his intention was to rework it into a paper that would be 'a record of what an American saw, thought and felt, not merely about Germany, but about being an American in an occupied country'.<sup>38</sup> The former issue was addressed in a lecture at McGill University in Montreal in 1948, which however was not published until 1962 (Hughes 1994).<sup>39</sup>

The twelve years of Nazi dictatorship had prevented the Germans from keeping abreast with the development of the social sciences that had occurred elsewhere, and more particularly in the United States. The eviction of Jewish professors and the subsequent draining of the teaching staff (further aggravated by the fact that the number of young academics who were dead or missing due to the war was disproportionately high), combined with the inertia of the traditional academic status system, had resulted in a climate that was averse to innovation. In spite of the experimental nature of the anomic post-war society with its near-complete uprooting of the social structure, no empirically minded sociological imagination had sprung up. By their own efforts, the social sciences in the successor states of the Third Reich were unable to reconstitute themselves.

The reports on Germany and Austria written by RF officers and by guest professors such as Everett Hughes highlight the differences between the two successor states of the Third Reich. The Americans treated the Germans with a mixture of a strict orientation towards re-education and a restraint in view of former Nazi institutes, on the one hand, and a certain indulgence, as it were, for 'persecuted' individuals, on the other hand. The occupation forces in Austria, in contrast, remained largely inactive and left the Austrians to their own devices. The RF tended to comply with the Austrians' wishes and idiosyncrasies, overlooking their inadequate ways of conducting research projects

as well as the fact that funds were draining away or were privatized. The obvious inability of German and Austrian social scientists to conduct research projects successfully and within a given time suggests that, during the Nazi period, this cultural technique of scientific work had not been developed. Only where former emigrants endowed with the relevant experience returned, at least for a certain time, to their former home countries, did projects function in a way that met American standards. This asynchronous development of a modern culture of project research all the more clearly highlights how much the Germans and the Austrians lagged behind.

When talking about the Americanization of postwar sociology, one should keep in mind that it was primarily brought to bear on the institutional level by targeting pent-up demand, since this was the main focus of US social science exports, rather than in the domain of empirical social research, which was seen as epitomized by questionnaire technology. The activities of sociologists such as Hughes, Anderson and Arensberg in Germany clearly show that what was at stake was not methodological preference but the more basic skills of social research. While there were young people in both countries who seemed worth supporting, the RF failed to spot talents in Austria as they had done in the interwar period. As a result, there were significantly fewer fellows from Austria than from Germany. The main difference between both countries, however, seems to be that in the case of Austria, next to no emigrants had returned from exile, thus allowing for the moral degeneracy deplored by Friedrich August von Hayek, and confirmed by other observers of the scene, to become rampant.<sup>40</sup> This will be examined and analysed in the following section.

## WHO RETURNED?

Few of the sociologists who had been driven into exile seriously considered returning to their former home country. Why should they return to an environment where they would be confronted with the very people who had behaved in so deplorable a way a few years earlier? Strong motives were needed for them to decide to return, motives which, again, may be seen as push or pull factors. Among the reasons that could pull emigrants back to Germany or Austria, the most immediate one, arguably, was the existence of strong ties to their original home countries. Whoever is suffering from homesickness will spare no effort to quiet the pain. However, sociological tools are inadequate for detecting the subterranean sources on which homesickness feeds, and as for a psychological interpretation, we not only lack the necessary data but also, as far as I am concerned, the competence. A somewhat milder form of attachment that, in most cases, is also better documented can be found in terms of a more general cultural bond with the country of origin. This may range from the overwhelming experience of being unable to express oneself as well in the acquired second language as in one's mother tongue (Robert Lowie who emigrated to the United States when he was a child reported this in Lowie 1959) to preferences for cultural forms and styles for which there is no

correspondence in the new surroundings, whether it is poems, rhetorical styles or the distinctive German way of doing science. It is easier by far to record a political commitment that makes it imperative for someone to go and join the reconstruction effort.

Among the factors that pushed people back to the former home country, there are not only all the variations of disappointment with life in a foreign country, but also assignments to official roles by the occupation forces. In the latter case, their return – at least in the beginning – could be experienced as not quite real since, in travelling in one's old home country, one was fulfilling a mission for the new one (cf. Fleck and Berger 2000).

About a hundred members of the group of social scientists referred to in Chapter Four returned to their old home country at one time or another. Among them, there were a few whom the Nazis had caught when they were on the run: Alfred Missonig was captured by the Gestapo in Yugoslavia where he had escaped after a short stay in Switzerland; Gustav Eduard Kafka, who had lived in Germany until 1938 and then escaped to elude arrest, was caught by the Gestapo in 1940 in the Netherlands. While Missonig was taken to his hometown, Vienna, where he remained in prison until the end of the war, Kafka was taken to the prison of Karlau in Graz where, after the end of the war, he settled down and later became a professor of law and political science. For Walter Beck, on the other hand, who went back to Germany in 1937, the return to the Nazi Reich was largely voluntary. After the end of the war, ninety-six sociologists returned to Germany or Austria (among them three sociologists who did not go abroad until the end of the war and returned later).<sup>41</sup> About one quarter of them did not return to Germany or Austria before the 1960s; at that time, these homecomers were already of an age that made it unlikely for them to start afresh. They were confronted with a country where economic conditions as well as scientific institutions were already in a post-reconstruction stage, and they can hardly be expected to have had a decisive influence on the outcome.<sup>42</sup> The following analysis, therefore, focuses on those homecomers who returned between 1946 and the early 1960s to one of the successor states of the Third Reich.

Thirty-seven pioneers of reconstruction returned before 1950. These were fifteen Austrians and twenty-two Germans, who did not always return to the country from which they had escaped, such as the Vienna autodidact Leo Kofler, who had fled to Switzerland after the *Anschluss* and went to Leipzig in 1945. His return, as well as that of eight others (Kurt Blaukopf, Julius Deutsch, Josef Dobretsberger, Bruno Frei, Jürgen Kuczynski, Oscar Pollak, Ludwig Renn and Leo Stern) was primarily due to political considerations. Most of them were members or sympathizers of anti-fascist parties and went at their request. A second and rather clearly identifiable group of pioneers returning from exile were Catholic advocates of the authoritarian state who had gone into exile because of their commitment to this state before 1938. Among them were Dobretsberger, already referred to above, as well as Johannes Messner, Johann Mokre and Wilhelm Koppers. Except for social-democratic politician

Julius Deutsch and sociologist Johann Mokra, all the other left-wing Germans and Austrians, as well as the former advocates of the authoritarian state, had spent their years of exile not in the United States, but in Mexico, Great Britain, Switzerland, the Soviet Union, Palestine, Turkey or Egypt.

Before 1950, only ten social scientists returned from the United States, while eleven came from Great Britain, seven from Switzerland, three from Mexico and six from other countries. This suggests that the countries other than the United States had indeed only served as a country of exile for the emigrants and that integration into the local cultural and scientific life was more difficult than in the United States. The small proportion of returning migrants from the United States at these early times presumably also had to do with the fact that exit conditions for these migrants were difficult in two respects. Besides the scarcity of transport facilities in the years right after the war, the United States citizenship that most emigrants had acquired proved to be a high barrier because, at the time, dual citizenship was excluded by law, and new-Americans leaving the United States for long periods were sanctioned by being deprived of their nationality.

Among the ten early homecomers from the United States, Martin Fuchs was the only one charged with a mission by the United States government. Among the eight former Germans, there were four members of the Institute of Social Research: Horkheimer, Pollock, Adorno and Henryk Grossmann. Horkheimer, at least, thought of himself as a semi-official representative of the United States, while the same can definitely not be said of Grossmann, the institute's factotum, who moved on to the Soviet occupation zone. Anthropologist Julius Lips also returned to the Eastern zone (Lips 1950), while Sigmund Neumann and Fritz Sternberg settled down in West Germany.

In the following decade (1950–60), another thirty-four emigrants returned: twenty-seven Germans and only seven Austrians. Most of them came from the United States, only two came from Great Britain, two from the Netherlands, three from Turkey, and one each from Switzerland, Sweden, Bolivia, Palestine and Mexico.

Taking these two cohorts of homecomers as the group of those who returned at an age when they were still active in their professions, i.e. when they were true returning migrants in terms of their working life, and attempting to compare and contrast these migrants, the first thing one notices is the ratio of former Germans to former Austrians. Although the overall number of emigrants from both countries had been almost equal, more than twice as many Germans than Austrians returned to their former home country. This reversal of proportions is remarkable, at any rate. While almost one out of three German emigrants came back, at some point or other, to stay in one of the German-language countries, only one out of six Austrians returned.

Remarkably, those Germans and Austrians who returned during the first one-and-a-half years after the end of Nazi rule were nearly the same age when they came back. These pioneers of reconstruction returned at an age at which one used to reach the top level of one's career, i.e. appointment to a professorship,



in the Teutonic science system and at which successful emigrants in the United States system similarly crowned their academic advancement by gaining the position of a 'full professor'.<sup>43</sup> German returning migrants were somewhat younger than their Austrian counterparts, with a median age of forty-six years as compared to forty-nine years for Austrians. In the following decade, this relation was reversed. For those who returned in the 1950s, the median age was fifty-five for Germans and fifty-two for Austrians. On closer examination, these small differences are quite remarkable. In both countries, the median age for emigrating was almost the same (thirty-five for Austrians, thirty-six for Germans). But since the great majority of Germans emigrated four years earlier than the Austrians, their average age at the end of Nazi dictatorship was higher (at the end of the war, German emigrants were forty-seven years old, Austrian only forty-two years). Thus, for the Germans, there was hardly any difference between the median age of the returning migrants and that of the emigrant population as a whole, while for the Austrians there was a significant upward variation.

This somewhat confusing age pattern of returning migrants can be accounted for by the fact that, because of Austrian restitution regulations, the great majority of early Austrian returners could reaccess the positions they had been forced to abandon in 1938. This primarily benefited those who already had civil servant status at the university before 1938, and these in turn were primarily the more or less avowed representatives of the authoritarian state (Dobretsberger, Koppers, Messner and Mokre). In contrast, the younger Austrians among the early returning migrants were barred from access to the universities in their home country and had to find other openings for themselves. Martin Fuchs initially worked as a member of the American occupation force before he joined the diplomatic service and finally returned to the United States as the Austrian ambassador; Leo Stern and Leo Kofler returned to East Germany; and Franz Borckenau went straight to Germany from his British exile to teach as a supernumerary professor at Marburg/Lahn. The two journalists Oscar Pollak and Bruno Frei continued to work in their profession. Only Kurt Blaukopf, having worked as a journalist in Vienna for some time, succeeded in engaging in an academic career in 1962 (Blaukopf 1998).

In the 1950s, the picture for those who returned to Austria, or for those Austrians who returned, is much the same. German philosopher Günther Anders settled down as a freelance author in Vienna. Adolf Kozlik worked in adult education, also in Vienna, before a short interlude at the newly founded Institut für Höhere Studien (Institute for Advanced Studies) (Fleck 2000). Ex-Viennese Eric (formerly Erich) Voegelin accepted a professorship in Munich. Similarly, Emerich K. Francis taught sociology in Munich before he was made an honorary professor in Innsbruck in 1967, where he had been to *Gymnasium* and had started his law studies in the 1920s. The two others who returned were 'returning migrants' only in a restricted sense, since during the Nazi dictatorship they had acted as a psychologist for the armed forces and as a member of the diplomatic service in a state that was an ally of the

Third Reich, respectively, and had gone to the United States only after 1945. Peter R. Hofstätter transferred to the United States for a certain time after the *Habilitation* he had obtained in the Nazi period was invalidated, and Slatko Zagoroff left Bulgaria for California. Hofstätter later returned to a professorship in Germany, and Zagoroff, having ended up out of work in the United States, pulled off the feat of being appointed to a statistics professorship in Vienna (Fleck 2000).

The situation faced by returning migrants in both parts of Germany was clearly different from that in neighbouring Austria, and this difference is due in no small part to the fact that the three successor states of the Third Reich each had to find their own ways of coping with their Nazi past and were treated differently by the occupation forces (Lepsius 1989). In East Germany (German Democratic Republic), lecture rooms and university positions were open to non-communists only in the beginning, and then only when they qualified as at least 'anti-fascists'. In the long run, survival in the GDR was possible only for those social scientists who submitted to party dictatorship. These included Jürgen Kuczynski, Alfred Meusel, Ludwig Renn and Leo Stern. For Henryk Grossmann, the question of whether or not to submit to the party line did not arise. He returned to a professorship at the University of Leipzig in the year of the proclamation of the anti-fascist republic, but died only one year after. Leo Kofler and Hans Mayer, somewhat more given to non-conformism, left the GDR, the former soon after its foundation, the latter only after the Berlin Wall was built. Kofler was suspended in 1951 and cleared to go to the West in the following year, while Mayer returned from his Swiss exile first to Hessen and then, in 1948, to Leipzig, where he held out until 1963. Lips, Meusel, Renn and Stern served the workers' and peasants' state until their deaths; and Kuczynski was reserved the privilege of honouring the state even after its sudden collapse (Kuczynski 1994).

In the Western part of Germany, conditions were significantly different from those in the two smaller successor states, each of which had its own way of ducking the responsibility for the 'Thousand-Year Reich'. Although the literature is, for once, unanimous in its lament that remigration was a small-scale affair, a closer look reveals that this was not true, at least not with respect to access to strategically important university posts. Almost all German universities saw the return of at least a few emigrated social scientists who initiated a remarkable reorientation of German social sciences in their new domain, often with the material as well as non-material support of the British or US occupation forces. They thus did the very thing that F.A. Hayek had recommended, as early as in 1948, as a means for Austria to solve her moral plight: 'I have come to the conclusion that even the presence of a single distinguished scholar with a strong personality and a spirit of independence in each faculty might completely change the moral and intellectual climate and that consequently this is the point at which any outside assistance might be most effective.'<sup>44</sup>

In later years, when the prevailing tendency was to denounce supposed Americanization, the United States was severely criticized for its reconstruction

work. This criticism originated in the anti-imperialist sentiment of the student movement, but was later also shared by a number of advocates for the better tradition of the German brand of social sciences, i.e. as a humanistic discipline (Tenbruck 1984).

In Chapter Four, the centres of social science research in the early 1930s were identified on the basis of their number of graduates: Berlin, Heidelberg, Leipzig, Frankfurt, Munich and Cologne were the German towns where most of the social scientists of the five samples obtained their doctor's degree, 34 to 81 per cent of whom later emigrated. Were these, also, the universities to which the migrants returned? Did they really import, when returning from emigration, an Americanized version of the social sciences? Could all the holes from after 1933 be refilled? I will try to answer these three questions by examining the places of German social science remigration.

The university with the largest number of returning migrants was Frankfurt. These were Horkheimer, Adorno and Pollock, three members of the Institute of Social Research who returned quite early; Gottfried Salomon-Delatour, who came to Frankfurt as a guest professor in 1954 and, from 1958, as a full professor; Julius Kraft, as a full professor from 1957; Fritz Neumark, returning from Turkey, as a public finances specialist; and Walter Sulzbach, as an honorary professor at Frankfurt University from 1956. Although the Institut für Sozialforschung (Institute of Social Research) presented itself as an institution that took up its pre-1933 activities again, its scientific profile had changed, which was probably due, among other things, to the experiences gained in the United States. At least in the 1950s, Horkheimer, Adorno and Pollock indeed thought of themselves as the German representatives of the new American-style empirical social research. In the initial years, Horkheimer recruited collaborators of Lazarfeld's Bureau of Applied Social Research at Columbia University as consultants of the Frankfurt institute, and in terms of methodology, the studies conducted by the institute itself were oriented to models that its members had come to know during their American exile. This is particularly true for the study on how people were coping with the past, published as *Gruppenexperiment* (Group experiment) (Pollock 1955), a title that rather downplays the issue. Its model was the 'focused interview', i.e. a method first developed by Robert K. Merton in the context of research on mass communication.

But even if Salomon-Delatour and Kraft, with the leniency that is indispensable in these matters, may be seen as successors to Oppenheimer and Mannheim, respectively, and Neumark as the one who continued the work of Adolph Löwe and Karl Pribram, there still were voids that remained unfilled. In the reconstruction phase, there was no successor for Max Wertheimer, Martin Buber or Paul Tillich.

The second university town that accommodated a remarkable number of returning migrants was Berlin, with Kuczynski, Meusel and public health specialist Kurt Winter returning to the East sector, and pedagogue Fritz Borinski as the first returning migrant to settle down in the West sector (where, however, he had to make do with working in adult education before being

appointed to a professorship in 1956). The returners who came to West Berlin were mainly political scientists who set out to take up the tradition of the Deutsche Hochschule für Politik. Thus, there was a nucleus of scholars doing empirical research in political science, which included returning migrants. These included Arkadij Gurland (from 1950) and Ossip K. Flechtheim (from 1952), both of whom had for different lengths of time been affiliated with the Institute of Social Research during their exile; Ernst Fraenkel (from 1951); and, later, Richard Löwenthal (from 1959, first as a guest professor) and Otto Stammer who, having plunged into the private business sector during the Nazi period, had obtained his *Habilitation* in 1949 and had in 1951 been appointed to a professorship at the newly founded Free University. Given this setup, the political sciences can be truly said to owe their specific profile uniquely to the experiences encountered during emigration. Both Flechtheim and Fraenkel had served the American government for a certain time before returning to German science, Fraenkel as a constitutional expert for Korea and Flechtheim as a collaborator of the chief prosecutor in the German war crime trials.

The changed situation in, and division of, Berlin had an impact on the non-university research centres, as well, only a small number of which still existed after 1945–9. The former Kaiser Wilhelm Institutes were transferred to West Germany,<sup>45</sup> and Bonn, the new capital of the Federal Republic, attracted further centres. It was not until the foundation, in 1963, of the Max-Planck-Institut für Bildungsforschung (Max Planck Institute for Human Development) that things began to change for Berlin. However, since in the interwar period the profile of Berlin as a social science hub had been less distinctive than one might expect given its position as a metropolis at the time, one can hardly say that there was a substantial local deficit to make up for. Berlin's reputation in the 1920s and 1930s was not due to the social sciences. The mandarins were old (Werner Sombart), or failed to make their mark (Alfred Vierkandt), or positioned themselves in sharp contrast to the social sciences (Eduard Spranger), while newcomers such as Emil Lederer were in place for too short a time to have a strong impact. In economics and law, the situation was a little more favourable. Ernst Wagemann and Carl Schmitt, having compromised themselves during the Nazi period, left a void after their retirement that no emigrant came to fill after 1945.

The two other German social science strongholds in the interwar period, Heidelberg and Leipzig, also had their returning migrants, albeit a smaller number of them. Among those who went to Leipzig – Germanist Hans Mayer, philosopher Leo Kofler, anthropologist Julius Lips and economist Henryk Grossmann – there was no pronounced representative of the key social science disciplines, and the political conditions did their bit to prevent the social sciences from flourishing in the first place. More or less the same can be said, albeit for very different reasons, for Heidelberg where Karl Löwith, Herbert Sultan and Alexander Rüstow returned to hold the professorships of philosophy, finance and political economics, respectively. Löwith and Rüstow were no doubt influential, each in his own way, but certainly not with respect to

what might qualify as a contribution to the development of the empirical social sciences. Heidelberg sociology was entirely informed, after the death of Alfred Weber, by Wilhelm Mühlmann. In the late 1950s, he was joined by Ernst Topitsch, who indeed supported an empirical orientation for sociology but made no contribution to it himself. None of these three homecomers can be said to have exercised a specific intellectual influence through social science competencies gained during emigration. They continued their work from the point where they had been forced to abandon it in 1933.

In Munich, sociology and the other social sciences, in spite of the well-known names that marked their history (Wilhelm Heinrich Riehl, Lujo Brentano, Max Weber), had failed to develop a distinct profile in the interwar period – the two dozen Munich graduates in our samples seem to have chosen this university primarily because it was in their home town – and for a long time failed to do so in the Federal Republic of Germany, as well. Hans Nawiasky returned from his Swiss exile to his professorship of public law and political science at the University of Munich, from which he had resigned in 1933. Apart from his contributions to the Bavarian constitution of 1946, he left no scientific trace, which may also have to do with the fact that he had kept his professorship in St Gallen, Switzerland.

The two other Munich returning migrants, Francis and Voegelin, seem to have been influential in different ways. Voegelin, founding professor of political science, not only succeeded in establishing his normative idea of political science, but succeeded even in making sure that it prevailed until long after his death. Francis, who was born in the Austrian Catholic milieu and had obtained his doctorate at the German University of Prague, did not turn to the social sciences until his stay in Canada and the United States, which was due to some twist of fate rather than to the necessity of emigrating. During his emigration, he turned to empirical research, investigating inter-ethnic relations and ethnic minorities. During his entire stay in the United States, Voegelin remained resistant to any attempt to change his style of thought. Although both of these two refugees from the Nazis (both of them Austria-born, Catholic and politically right of centre) had no links whatsoever to Munich before they were appointed founding professors of the two twin social science disciplines, they seem to have had no problem in getting their bearings. Francis no doubt taught the skills and insights he had acquired during emigration, but for all that, these two can hardly be considered as the representatives of American social sciences.

Other German universities that had returning migrants among their faculty members were Cologne, Göttingen, Hamburg and Kiel. In Cologne, René König succeeded Leopold von Wiese and established a version of sociology that significantly differed from that of his predecessor. In the years that followed he came to be perceived as the embodiment of American sociology. The Cologne-based UNESCO Institut für Sozialwissenschaften (UNESCO Institute for Social Sciences) headed by Nels Anderson, a Chicago sociologist who had done research on hobos, was another influence that likely determined the

orientation of Cologne-based social sciences. König, who made a living by writing book reviews for newspapers during his exile in Zurich, is likely to have benefited from this opportunity to familiarize himself with recent developments in sociology and neighbouring disciplines. In Cologne, the reception of recent social science research methods did not take root until after 1945 and relied much more on König's newly established international networks within the International Sociological Association and UNESCO than on his Zurich studies. The second returning migrant, Alphons Silbermann, had survived the Nazi period in Australia, which made him an unlikely 'principal agent' of the 'American way of doing social research' (Silbermann 1989). Given that another returning migrant, historian Dietrich Gernhard, came to Cologne in 1951 and set up an Institute for American Studies before transferring to the Max-Planck-Institut für Geschichte (Max Planck Institute for History) in Göttingen in 1961, Cologne can be seen as a university that was open to the new times.

Hamburg in the interwar period had provided a home for three very different professors – Albrecht Mendelssohn-Bartholdy, Andreas Walther and Ernst Cassirer – among whom only Nazi-partisan Walther showed a marked affinity for the social sciences. After the war, Siegfried Landshut, who was appointed to a professorship of political science at the University of Hamburg in 1951, was no more able to influence the social sciences than was pedagogue Anna Siemsen, who for a short time taught literature in Hamburg before she retired. Göttingen, where Walther taught for a short time, was a social science desert before the war but afterwards, thanks to the appointment of Helmut Plessner, who returned from exile in Groningen, the Netherlands, in 1951, a new high-profile sociological site was able to be established. Two other returning migrants were appointed professors. Jurist Gerhard Leibholz, who had been a professor at Göttingen University until 1936, returned from exile in Britain at first temporarily and from 1951 for good. Having come to know the Anglo-Saxon way of studying comparative government in Oxford, he dedicated himself to this field, in addition to his function as a judge at the Federal Constitutional Court. The second returning migrant was social democrat Gerhard Kessler who had held a professorship of economics and social policy in Leipzig before he emigrated to Turkey, and then taught as an honorary professor in Göttingen.

Before the handing-over of power to the Nazis, Kiel had had a prominent place in the chorus of German social sciences due to the Institut für Weltwirtschaft (Institute for World Economics) and the workings of Ferdinand Tönnies. After the war, its reputation was soon re-established through the appointment of Erich Schneider to a professorship of economics and as head of the Institut für Weltwirtschaft. For Gerhard Mackenroth, in contrast, the sun was shining much less brightly than in his time under Tönnies.

The post-1945 reconstitution of the social science strongholds of the Weimar Republic was achieved in different ways, but to a similar degree. For the further development of the social sciences in the Federal Republic, however, new sites alongside Frankfurt, Göttingen and Berlin became increasingly important,

and at these new sites the influence of returning migrants was much weaker than at the traditional universities, or non-existent, as for example at the Sozialforschungsstelle (Centre for Social Research) at the University of Dortmund.

However, this review of the sites where returning migrants came to work in significant numbers in postwar Germany does not provide an answer to the question of what may have prompted them to return to their former home country in the first place. Only a few of them returned to the positions they had held before their eviction by the Nazis. In contrast to early Austrian remigration, their returns were not the immediate results of restitution policies. Only a small number of those who returned to the three Western occupation zones, and then to the Federal Republic, had an explicitly political agenda. When looking beyond a psychological exploration for systematic patterns of the motives for return, one is more likely to find them among those factors that made return seem the preferable alternative. Thus, an obvious push factor for remigration came from the country where emigrants had survived in the years of Nazi dictatorship. More than half of both cohorts of returning migrants from both countries of origin returned from countries that, at the time, were obviously not very immigrant-friendly or where academic life was comparatively poorly developed. This is true for Great Britain where the social sciences were at the time still rather poorly institutionalized and where there were few attractive employment openings beyond the LSE and 'Oxbridge' (cf. Hasley 2004). But this is also true for Switzerland, which was glad to get rid of the unwanted refugees from Hitler. Return from countries outside Europe, including Turkey and Palestine, was an alternative that was favoured for obvious reasons (in the case of Palestine, this was likely to be true for those who kept their distance from Zionism). In this sense, one may argue that one out of two returning migrants really came home from an exile where, from the very start, there seems to have been little opportunity for them to feel at home.

A prime indication that the opportunity structures of the countries of exile were paramount for emigrants' decisions to return is the proportion of female returning migrants. In the whole group of about one hundred returning migrants, only three were female, and for one of these it is not even sure whether she should be counted among the returning migrants, at all. Clara Maria Liepmann, had obtained her doctorate with a thesis on penal law in Hamburg in 1927<sup>46</sup> and, in the same year, had gone to the United States on a Rockefeller Foundation fellowship which enabled her to study criminology and do 'practical case work' in penal institutions. When her fellowship ended, she seems to have temporarily returned to Berlin, where she worked with the Prussian penal authorities. But by 1932, she was already back in the United States, first working in Muncy, Pennsylvania, at a State Industrial Home for Women and, later, as a research assistant at the Russell Sage Foundation. There she seems to have met her future husband, Willem Van De Wall, a harpist at the Metropolitan Opera in New York, who acted as a musical therapist at the Russell Sage Foundation and had published several books on the use of music

in total institutions. For one of these books, *Music in Institutions*, Liepmann was a co-author. In 1950, the couple is said to have been in Germany. Liepmann's further trajectory, however, is unknown.<sup>47</sup>

On the two other female returning migrants, information is somewhat more precise. Anna Siemsen's return to Hamburg at the age of sixty-four has already been reported. The third female returner was Charlotte Lützens who, having graduated from Heidelberg, had worked as an international secretary for the executive committee of the Social Democratic Party at the beginning of the Weimar Republic. After that she seems to have made a living as a freelance sociologist. At any rate, she published a sociological study on the German youth movement in 1925 and another book, *Staat und Gesellschaft in Amerika* (State and Society in America), in 1929. During the Nazi period, she and her husband, a social democratic politician and diplomat, were in exile in London, where she worked as a senior research assistant at the LSE and as a lecturer at the University of London. Right after the end of the war she published a work on *Women and a New Society*, the pictograms being contributed by Otto and Marie Neurath's Isotype Institute in Oxford. Lützens is said to have returned to Germany after 1949, taking up her political work and later acting as a member of the Deutsche Gesellschaft für Soziologie (German Sociological Association). Not much is known about her postwar activities. While still in London, she wrote a German summary of the so-called Beveridge Plan, published in Hamburg in 1946.

From the biographies of the three female returning migrants, they can rightly be said to have been part-time social scientists. Even Lützens, whose phases of social science work were most frequent, only temporarily engaged in this occupation, and even then only among other activities. This, however, would strongly suggest that for women exclusively doing scientific work returning to the German-language countries was not an attractive option. The virtually complete absence of women among the returning migrants after 1945 was not due to any lack of adequate female candidates for positions to be filled. Although female scholars in the United States, the main country of destination for social science emigrants, were facing discrimination in finding positions, as well, they seem to have found nevertheless that American conditions were more likely to allow them access to high-level academic positions. Between 1934 and 1951, seven female emigrants were appointed associate professors or full professors. What is remarkable is that they all belonged to the younger groups of emigrants, while older female emigrants such as Charlotte Leubuscher (born in 1888) and Charlotte Bühler (born in 1883) were not granted the privilege of obtaining a position in exile that corresponded to their previous associate professor status. Käthe Bauer, who was born in 1894 and, thus, only slightly younger, and who had done odd jobs in academia to stay afloat in the time between her eviction from her position as a private lecturer at the Handelshochschule Mannheim (Mannheim Commercial College) and her emigration to the United States in 1939, succeeded in getting a first post-escape job, supported by the Emergency Committee, at a public college in the Midwest



United States. She later transferred to a regular position at a minor college in New Jersey.

The three female social science emigrants who were born just before the turn of the century, i.e. Wally Reichenberg-Hackett (1895), Hertha Kraus (1897) and Julie Meyer-Frank (1897), were rather quick to access positions that in Germany or Austria would most likely have been inaccessible to them. Reichenberg-Hackett, however, is an exception in that even before her Vienna graduation in psychology in 1935, she had spent a year as an exchange student in the United States, where she emigrated in 1936. After a first job at the Menninger Clinic in Topeka, Kansas, she worked as an instructor at Fordham University and at the two women's colleges Sarah Lawrence and Hunter, and finally came to Duke University in 1951. Hertha Kraus's career in the United States was probably facilitated by the fact that, as a Quaker, she could rely on social contacts, which made it easier for her to get a first job after her escape to New York in 1933. But by the next year, she had already entered the world of university education, first at the Carnegie Institute of Technology in Pittsburgh (today Carnegie Mellon University) and then, from 1936, as a full professor at the renowned women's college Bryn Mawr, near Philadelphia. Julie Meyer-Frank, who had taught at Bavarian adult education centres before emigrating, started her American career as an assistant at the New School for Social Research, New York, where she was appointed lecturer in 1943 and associate professor in 1948.

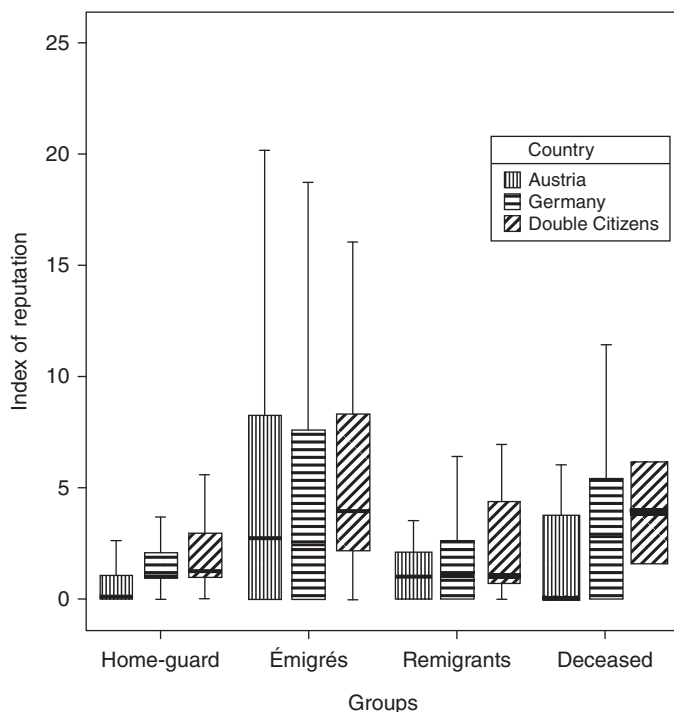
The female emigrants of the next cohort, i.e. those born after 1900, were not as quick to establish themselves in the United States as their male emigrant colleagues but still did so significantly faster and more permanently than their female non-emigrant counterparts. Else Frenkel-Brunswik, it is true, was denied a formal affiliation with the department of psychology in Berkeley because of the anti-nepotism regulations that were in force at the time, but in this case her position as a research associate can be more or less equated to a regular job. Edith Weisskopf, who had been one of Frenkel's Vienna collaborators, was appointed associate professor at Purdue University in Indiana in 1949. Weisskopf's private life, however, was far from following a straight line, and certainly not without problems. Having escaping, by a hair's breadth and with break-neck daring, from Nazi-Austria, this younger sister of the well-known physicist Viktor Weisskopf was twenty-eight when she came to the United States, where she soon obtained a position as an instructor at a college near New York. Two years later, she married Gustav Ichheiser, her senior by thirteen years, who had also managed to escape from Vienna. After two years, the marriage ended, and both went to some trouble to erase the episode from their lives. Ichheiser was later institutionalized for more than ten years and was released only because the Rockefeller Foundation, in response to a reader's letter he had written (which for some unknown reason he had also sent to the Foundation), offered him a grant to have him explore in more detail the questions raised in this short text. Embittered, he spent the last years of his life under the tutelage of Hans Morgenthau, trying to have his forced institutionalization recognized as

unjustified and arbitrary. At the same time, he managed to collect his various studies for an anthology that was published posthumously (Ichheiser 1970). Only a few years later, his former wife Edith Weisskopf met with the same fate: she was also institutionalized for a number of years. And she also succeeded, after her release, to regain a foothold in her professional life (Weisskopf-Joelson 1988).

In 1948, Marie Jahoda, who had spent the war years in England, followed by a spell at the American Jewish Committee, was appointed associate professor at New York University. Two of Charlotte Bühler's other students, Hedda Bolgar and Katherine Wolf, obtained university positions in the early 1950s. Economist Steffie Browne of Vienna did so in 1954, while Ilse Mintz, who was younger, first obtained a second degree in the United States and spent some time on a job at the National Bureau of Economic Research before she transferred to a university position in 1962.

Two patterns emerge from this examination of the careers of those female social science emigrants who were successful. Establishing oneself was comparatively easier and prospects were significantly more favourable for them than for those female scholars who stayed at home. Among the younger female German emigrants, only Hannah Arendt and Rose Laub Coser were able to pursue a university career. Philosopher Elizabeth Hirsch (née Feist), who like Arendt had been a student under Heidegger, had received her doctorate in Marburg in 1928 and had in the following years continued her studies with a Rockefeller grant in Paris. Being Jewish, she lost her job as an assistant in Berlin after 1933 and emigrated to the United States in 1938 where she married another German emigrant, Felix E. Hirsch. For many years, she worked at the renowned Bard College in upstate New York as an instructor of political science, and she later transferred to the public college in Trenton, New Jersey, where she was finally appointed professor. Her study on the Portuguese liberal humanist Damião de Gois, which she had pursued since the 1940s, was not published until 1967. Arendt, who seems not to have been overly interested in a permanent university position after having come to the United States, finally accepted a position at Brooklyn College in 1953. Her prestige, however, was much more due to her guest professorships in Princeton, Chicago and Harvard. Rose Laub Coser, in contrast, had the picture-perfect career, which, in any case, was a surprise to herself. After taking her (second) degree at Columbia University she had not expected to go beyond a teacher's job, as her husband Lewis A. Coser recalled in a portrait of his wife (Coser 1999). Actually, after some intermediate stages and interruptions due to the birth of her two children, she achieved a respectable position at the new State University of New York in Stony Brook.

Finally, there is another factor that may have had an influence on emigrants' readiness to return, namely the response in the world of science that they met with during their emigrant years. It is true that the data underlying our reputation measurement do not allow for any isolated assessment of a scholar's pre-remigration reputation, but the differences between those who decided to



**Figure 7.1** Boxplot of Reputation, Comparison of Four Groups, by Countries

Note: n = 666. The bold bar indicates the median; the rectangle below stands for the 2nd quartile; the rectangle above stands for the 3rd quartile; the line goes up to the largest value which is neither an extreme nor an outlier value.

Source: Author's calculations.

return and those who did not, which are revealed by a tentative approach based on the reputation acquired over the whole period of observation, are quite impressive after all. Figure 7.1 shows that for social scientists from both countries of origin, the median reputation was significantly lower for returning migrants than for non-returning migrants. The conclusion thus seems to be that at least on a group level of comparison, there is a relation between the problems of establishing oneself encountered abroad and the readiness to return to Germany. For the returning migrants, the return option was attractive because the option of staying in their guest countries was less so.

# 8

## RED THREADS

In the preceding chapters, some of the stages and developments of empirical social research, initially labelled 'realistic' or 'inductive' by their proponents, have been described and analysed. In the course of the turbulent middle years of the twentieth century social life changed on various levels, faster in some parts than in others, more or less profoundly as well, and things hitherto quite unknown appeared on the scene. Most of the changes followed from the logic of their respective social sub-systems, or fields, but at certain times and for certain periods, these otherwise autonomous fields interacted to produce altogether novel configurations. As a result of these knottings, as one may call them, extraneous influences acted on these autonomous trajectories, initiating a change of direction. Two examples may serve as an illustration. Without the handing-over of power to the Nazis in Germany in 1933 and in Austria in 1938, the development of the science systems in both countries would no doubt have taken a different turn – which is true even assuming that all those professors who inched in to the Nazi fold would have done so even if the Third Reich had never been proclaimed. A university-based Nazi camarilla would no doubt have been in a position to deny their adversaries access to their institution, but it would have lacked the power to have them killed and might not even have commanded enough of a menace to make them emigrate. Also, if, in the 1920s, decision-makers at the foundations of the American 'robber barons' had come to the conclusion that it was better for the image of their founders to spend all that money at home, many of the scholars evicted by or fleeing from the Nazis would have had fewer contacts in Western countries to reactivate after their escape. Unravelling these knots and working out the changes that followed from the internal logic of the respective social sub-systems, and the interactions that would result from these changes, is a challenge which, while daunting, is sensible and worth taking up. In the present conclusion, I will try to work out the red threads of fate in this melange, such as it was described in the preceding chapters.

A sociological analysis of the development of the science system necessarily views the internal dynamics of what is called scientific progress – the supposedly autonomous unfolding of the cognitive gestalt of each of the scientific specialties – in its interaction with the social gestalt of the scientists involved. In this perspective of a sociology of knowledge, the changes that occur within institutional structures are generally not sufficiently taken into account, and the interactions between the science system and higher education, national politics, international relations and economic development in general are rarely explored in detail. It is these interconnections, however, and their impact on the processes of change that make up the distinctive character of the development of the sciences in the mid-twentieth century.

The rise of the United States to the position of a world power was due to its economic momentum, but also the weakening of the traditional powers of the nineteenth century and their involvement in two world wars that closely succeeded each other, while the United States remained for the most part on the sidelines in the first one but massively influenced the outcome of the second one. This shift of the centre of global economy from the British islands to the other side of the Atlantic, and the subsequent shift of the focus of world politics (probably strongly co-determined by the first one) are quite obvious when one examines the economic performance of the more developed parts of Europe<sup>1</sup> as compared to that of the United States. In 1820, the per capita national product in Europe was \$1,270 (\$19,000 in 2010), slightly higher than that of the United States (\$1,257); half a century later, Europe was already slightly outperformed by the United States; in 1913, the American per capita national product was 50 per cent higher than that of the most developed European states; and in 1950, the United States per capita national product was almost double that of the European states. By the end of the twentieth century, the distance was back to the level of 1913. Other indicators go in the same direction.<sup>2</sup>

These facts inevitably also affected the world of science. Even in classical economics, the mercantilist view had been abandoned in favour of a view that postulated the dependence of a country's economic success on inventions and, thus, on research. In the course of the nineteenth century, this view prevailed to the point of becoming a truism that governed the reasoning even of kings and emperors. But as for the question of how innovations take effect on the social level, how new knowledge and inventions are turned into products, and which factors allow for markets to be opened up to these products, the answer is something quite different from inventions and economic success. Constructing a steerable vehicle driven by a gasoline-powered engine is one thing, but to hit on the idea of pressing this automobile on a mass public as a leisure time entertainment is another. The discovery of the masses as consumers with money to spend is a social innovation quite distinct from the technical one. Mass production for a mass public made its first appearance in the United States and has since been described as Fordism.

Similarly, it is far from trivial to conceive and propagate the idea that what started out as a series of discoveries about mechanical forces and the living world, and had subsequently been generalized as the idea of 'scientific progress', represented a cognitive pattern that was equally adequate for reaching a deeper understanding of the social life of man. This extension of the application for principles of scientific knowledge to the social domain may be prompted by a number of problematic analogies (as was the case with social Darwinism), but the two processes involved should nevertheless be distinguished: the social acceptance of the idea that a science of social life was possible *more geometrico*, and the promise that the insights of this branch of science were adequate tools for the purposive improvement of social life. A science of social life and the idea of social technology or, more explicitly, social engineering, imply two quite distinct programmes. It was only with the joint appearance of these two possibilities – for in the beginning, this was all there was – that they triggered a dynamic that resulted in the rise in the number of the personnel needed to actualize them.

A culture that is satisfied with believing in the statements of its mandarin class and their claims that only an extended vision of this culture's past or of the nature of man is of any use to the present, produces a different social organization of those who are permitted to direct this vision than does a culture that subscribes to the model of the sciences and social engineering. But while the secularization of the power to construe meaning indeed resulted in privileging a new social group, namely the philosophers, historians and poets who replaced the shamans, priests and prophets, the capacity to construe meaning remained restricted to those predestined for this art. The 'genius religion' (*Geniereligion*) of the nineteenth century, ironically commented on by Edgar Zilsel (1990), is bound up with an elitist social organization.

The adaptation of the model of causal knowledge taken from the natural sciences, in contrast, tends to suspend social closure. If new knowledge can, in principle, be gained when and because one relies on the algorithms that have already proved to be the basis for new insights, this means that a substantially larger number of people is able to do so than under conditions where this capacity is transmitted from master genius to junior disciple by way of instruction. If, then, the rich source that produced the insights of the natural sciences and the inventions based on them is adopted as a highly efficacious model for the social sciences as well, the extension of the personnel that are allowed to contribute to the realization of a deeper understanding of social life through research is only one small step away. Suddenly, just as in nature, there is so much to discover in social life, and the more dynamically the change in society in which this attitude gains ground, the more this is true. The 'breaking the cake of custom' of Chicago and the sociologists there is the best-known case in point. If this attitude combines with a climate marked by the willingness to engage in social reform, and if social science knowledge rallies to the idea of improvement through social engineering, this virtually marks the dawn of the age of social science. The turbulent middle years of the short twentieth

century were the Golden Age of the social sciences as they enjoyed unchallenged authority, which is also why they had the means needed to continue their activity virtually forced upon them. Only the sociologists of those days could tell stories like Everett Hughes, who one day had a representative of a foundation standing in his room and asking him to name whatever subject he might want a grant for.

For this undertaking to be successful, however, a number of institutional innovations had to be realized on a low level of aggregation. The social organization of social science in a given society cannot be established unless a certain number of these preparatory steps have been realized – once this has been done in one place, it can be imitated by, or applied to, other societies (cf. Drori *et al.* 2003). Something like the institutionalization of social science research is the result of the interplay of several factors. Even given the changed worldview, the growing prevalence of a scientific perspective on social life, the opening-up of access to acquiring this view (through the instructional routines of education that was, in principle, open to everyone) and the intention to actually utilize this scientific knowledge of social life as a tool for good, there still have to be workable middle-sized institutions before the green light can be given to the new train of social research. What is required is a constellation where access to the means of learning about this new perspective is democratized, combined with a new orientation to practise that sees improvement as the result of a succession of small steps.

Those who collaborate in the new undertaking of gaining knowledge of social forces and their interaction need an organization – their organization – on more than one level. The functioning of the new science of social life depends not least on the fact that those who are involved in it learn to see each other as equals in a certain respect, as participants in the same undertaking. Whether someone is doing the same thing as oneself, or something different, can be established either by asking, ‘Who are you?’, or by enquiring about what the other is doing. While the answer to the latter question can be very succinct, but also very grandiose, the answer to the first can be easily standardized even without having to rely on pre-standardized options. In modern times, the non-specified question of what one does or who one is can be satisfactorily answered, without the risk of boring the interlocutor by too many details, by referring to one’s profession. Similarly, among scholars, this is achieved by designating a discipline.

The social gestalt of a scientific discipline fulfils a number of other functions as well. It allows for a reduction of complexity by demarcation (Gieryn 1999). A demarcation that has been hotly debated for a long time, although perhaps less so today, is the one between the sciences and all those other things – religion, ideology, metaphysics, astrology, superstition, esotericism, acupuncture, natural medicine, etc. – that are perceived as being different from them. The consequence of any demarcation based on this difference, regardless of how the line is drawn, is that the members of the insider group of ‘scientists’ no longer need to bother with the ‘nonsense’ produced by those others. Similarly,

the internal differentiation that results from the creation of disciplines relieves its members of the strain of having to take account of the products of other disciplines, however closely related. This helps them to focus on the work in their own field at least for a certain period – i.e. until this field becomes so vast, or the personnel engaged in it so numerous, that one loses track. Then comes the emergence, first, of new specialties within a discipline and, second and soon enough, of new disciplines altogether. To use a figure of dialectic thought, all is well until the quantity in terms of cell division into ever more disciplines begins to affect quality, because in the end each new discipline contributes little to knowledge in general. This, in turn, leads to a demand for interdisciplinary and transdisciplinary arrangements, though this is a different story.

The first differentiation of the sciences of social life into disciplines took place in the United States and gave rise to the canon of social science disciplines that UNESCO, in a fit of normative madness, tried to codify in 1945, and that have persisted more or less unchanged to the present day. In Europe, by contrast, the decisive issue remained what someone did rather than who someone was, until the UNESCO nomenclature slowly came to prevail. Apart from Émile Durkheim – and maybe Ludwig Gumpłowicz – those who congregated in the emerging circles and scientific associations as well as on the international platforms provided by sociological conferences were mainly individuals who intended to contribute to the matter at hand but were not interested in where they came from or to which discipline they belonged. One of the consequences of this absence of established demarcations along the lines of scientific disciplines, however, is that everybody is free to select what he or she deems worthy of reception from what the others did.

A look at those disciplines in Europe that were among the first to differentiate into autonomous undertakings may illustrate the consequences of creating disciplines. Psychology, never quite ready to decide whether or not it was a social science anyway, was the first discipline to become autonomous. Similarly, economics gained autonomy in some, but by no means in all countries; in Germany it remained embedded in *Staatswissenschaften*, where no doubt it thought of itself as queen, while those parts of it that strongly advocated a historical orientation even situated themselves in the humanities. Outside the natural sciences, it was the psychologists who first ceased to be interested in what the members of the neighbouring disciplines were doing. Among the economists, the radical caesura of complexity reduction was achieved by those who attained an independent reality on the level of discourse by indulging in language-games that others no longer understood.

Disciplines and their demarcations, like all institutions, only operate when they have the power to impose sanctions, so to speak. As long as people cannot be prevented from referring to themselves as a professional of a certain science, a scientific discipline is unable to fend off its dilettantes. The organizational levels capable of administering inclusion and exclusion are professional organizations, on the one hand, and university sub-structures bearing the names of their respective disciplines, on the other. In the field of the social



sciences, both of these institutions were again established for the first time in the United States. To be a 'professor of economics with a political science affiliation and a teaching appointment in sociology' in a humanities faculty is something quite different from being a professor in a department of sociology, given that this structure is the same at all other (American) universities to boot. The American department model took decades to prevail over the European faculty-chair model – an incomplete victory even nowadays, as shown by the symbols of the 'ordinary' and the 'chair' that still persist as mental relicts in some places – but wherever productive research has been done in the last eight decades, this victory has been achieved.

The department as the basic structure of day-to-day university-based academic life has consequences for the way both its members and all those who would like to become members act and interact. A department, as a rule, includes several representatives of the discipline, some of them on an equal footing at least in terms of formal status (on three levels: assistant, associate, full professor). Initially, the vertical stratification of department personnel in the United States was even more pronounced than in the Teutonic science culture where the prevailing structure was the simple dichotomy of professor and private lecturer, and complexity was only gradually enhanced by first including 'extraordinary' professors, then private lecturers with civil servant status and, finally and much later still, assistants, teaching assistants and the like. In contrast, department personnel has from the very start included individuals who are employed, some of them having tenure, and staff such as instructors or lecturers, whose formal affiliation is somewhat looser. The really important thing, however, is that there is something like a corporate responsibility and an obligation for each member of a discipline and department to cooperate. Given the little – or non-existent – influence exercised by state agencies, and the fact that university sub-units have from early on been held responsible for their finances and subjected to financial sanctions, the recruitment of personnel is not handled in so idiosyncratic a way as in the Teutonic model of the 'ordinary', where the almighty professor was free to choose whomever he wanted as his assistant or as a protégé to be guided towards *Habilitation*.

Since the department model is quite clear in that, as a rule, its scientific staff is recruited only from members of the discipline that is represented by the department in question, it can at the same time be more magnanimous in making exceptions with regard to appointing individuals who do not have the required degree, or have a degree in a different discipline. In the Teutonic model, while disciplinary affiliation was not important, personal aptitude and adjustment were all the more so. In both respects, decisions depended on a single person, and although the faculty had veto power, it rarely made use of it. In the United States, given the size of the American home market and the traditionally higher degree of regional mobility, there was always some opportunity somewhere for outsiders to enter the science system. In later years, given that departments have to be oriented to the market, students' preferences were being accounted for by recruiting outsiders.

While the Teutonic model implies, as a rule, a symmetrical discretion among individuals who see themselves as men of genius and, as such, as equal, the department model requires its members to bring themselves to cooperate as individualists on an egalitarian footing. Against this background, the emergence of 'teamwork' comes as no surprise. For the lower ranks, teamwork means that their chances of advancement double. They can rely on the reputation they have gained in the lower levels at one place when transferring to another, and their prospects of promotion are more favourable due to the higher degree of vertical mobility that is a side-effect of the extension of tertiary education establishments. Furthermore, the United States system has no equivalent of the bad Teutonic habit of obliging its subordinate junior scientists to have their own scientific achievements published under the name of their protectors. In Germany and Austria, junior researchers are trapped in a double dependency: their very admission to the realm of research is subject to a personalized selection, and their further fate as scholars depends on their protectors who, by customary right, are entitled to appropriate the results of the work of their subordinates.

For empirical social research, teamwork is especially adequate because the work to be done can be easily subdivided. This division of labour operates in the context of another momentous and successful invention that, again, first emerged in American research: the 'project'. Nowadays, project work is so familiar as a form of scientific work that the fact that this invention is only several decades old actually needs pointing out. Creating a project means that collaborators have to engage, in various ways, in strict planning and coordination. Since projects are subject to separate funding, the dimensions of what one feels can be done within a given period have to be translated into a reasonable agenda. Besides, one has to make sure that at the end of a project there is a result that can be presented as a basis for fundraising in view of further projects. Due to the restrictions in terms of deadlines and external funding, the recruitment of project collaborators is free of the intimate burden which, when recruitment means admission to a school, tends to damage the faculty of individual thought and to jeopardize a person's life planning. An individual who has become affiliated with a master and his school in the Teutonic model can escape this social bond only as a traitor to the common cause or by a quasi-parricidal break-up with the master. Therefore, repeated changes of school affiliation are highly improbable (and examples hard to find) in German intellectual history. But since one may reasonably assume that the first choice is, biographically speaking, highly contingent, precautions that aim to ensure immunity against temptations have to be all the more strict under these conditions. Even today, one of the favourite ways for someone socialized in the Teutonic model to initiate a talk with a colleague is to ask him whose student he is or to which school he belongs. Not to have a master and not to belong to any cognitive sect is dubious, and anyone referring to a different affiliation each time he is addressed would soon find himself heading for a social dead end. In contrast, with projects, transferring from one to another is something

quite normal and is not regarded as defamatory. It stands to reason that the recombination of perspectives and skills that goes along with these transfers is, all in all, more fruitful for scientific research than the jealous degradation ceremonies that are part of the hacking order within schools, where the priority is on whether or not someone belongs to Us and has learned to reason like Us, rather than on the potential contribution he or she might make to a clearly outlined task.

Cooperation within projects minimizes another risk that is a corollary of intellectual work and becomes obvious when part of a supposed plan for life is pursued independently and, in most cases, in isolation: the risk of forming too high an opinion of oneself. An individual cooperating with others is exposed to a healthy social control that can prevent him from crediting himself with a touch of genius. In a group of worshippers assembling around the head of a school, one tends to measure one's own contribution against the (usually supposed) genius of the master whom one strives to resemble as closely as possible. As a consequence, one learns to interpret the slightest hints of brilliance in oneself as signs of predestination, even though in the end they turn out to be nothing but a manifestation of self-conceit.

In the early twentieth century, independency from the heads of schools or those professors who deemed themselves to be such was strengthened, in the emerging American world of research, by some further innovations. The implementation of post-doc and other fellowships for junior researchers implied that candidates were not only examined by a single senior scientist to ascertain whether or not they were worthy of being supported as future inhabitants of the halls of science. To apply or be nominated for a fellowship is possible only for candidates who can invoke other qualities besides being recommended by a teacher. Students, but also teachers who want to provide for their charges, are thus obliged to be oriented to the market of their own discipline which, in turn, is supervised by their disciplinary colleagues. The result is early independent publications. The committees charged with the awarding of fellowships operated according to the older pattern of the cooperative university examination committee where the supervisor of a thesis was not included or, if he was, had only one vote among others. Committee-based selection procedures make life difficult for the 'outliers', which is likely to be less hard on those who are actually brilliant than on those whose achievements are deemed sufficient by only one teacher.

Research fellowships for one year or more for scholars whose reputation was established, fellowships like those awarded by the Guggenheim Foundation where candidates were nominated by a select group of nominators, and the implementation of sabbaticals which initially were only partly paid, all homogenized the standards of excellence to which the members of a scientific discipline subscribe, sometimes nearing conformity but at least always supervised and answered for by peers. This seemingly democratic selection procedure is not error-proof, but it prevents the emergence of a science culture that is the structural equivalent of German small-state conditions, where a multitude of

rulers had the sovereignty of small-scale territories and peace was achieved only because there happened to be an armistice.

When philanthropic foundations began to act as the financiers of social science research, this already highly efficacious system was enhanced by a number of further elements that arguably were decisive for the rise of the American science empire. Before going into this in more detail, however, one must draw attention to the quasi-religious trust in science that resulted in the establishment of these foundations themselves. They did not want to fund research for research's sake. On the contrary, their founders were convinced that all sciences, the natural as well as the social, medicine as well as the humanities, were capable of helping to eradicate certain social wrongs. Moreover, the founders and their initial advisors believed that the sciences themselves could supply the tools that would allow them to put the activities of their foundations on more solid ground. The birth of 'scientific philanthropy' was one of the rare cases where social science insights were successfully applied, or at least promised to be so. Because of the huge sums provided by Carnegie, Rockefeller and Ford – and it needs to be emphasized that they would have been free to spend this money on sports, mass entertainment or the arts, which they only started to do when the star of the social sciences was beginning to fade – they felt the need to charge a group of experts specifically recruited for this purpose with the distribution and administration of these funds, rather than do so themselves. In this new world of foundation funding, the decision on which projects would be funded and which applicants would be denied support was reached in a triangle of power. There were, first of all, the foundation officers themselves, mainly well-trained junior scientists with research experience of their own who, when major research projects were at stake had to convince the foundation trustees that their propositions were well-founded and, in the case of small-scale support, had to come to an agreement with their colleagues. Second, applicants had to learn to come to terms with this novel state of affairs and, third, there was the role of the advisors, filled by scholars not involved in the concrete research project. Individuals could repeatedly change between these three roles. Quite often, this change occurred between the roles of applicant and advisor, but former foundation officers having returned to the world of science might also turn up as applicants.

The frequency of moves in these tripartite cooperation games resulted – naturally, as it were, and as no surprise to those familiar with the logic of cooperative games (the theory of which was worked out, by the way, thanks to foundation support) – in something like shared morals that were essentially distinct from the mores of sinecures and clientelism of the European state bureaucracies. The foundation officers fulfilled the neutral role of the mediator, the applicant was allowed to act in his own best interest, while the advisors could become active in their role only when they arrived at their judgement behind the veil of non-involvement, so to speak, and evaluated projects only on the strength of whether or not they promised to yield some scientific surplus value. The vested interest of the mediators was that the greater the

number of projects they were able to successfully administrate, the more unchallengeable their status and the greater their prestige.

In Europe, the Rockefeller Foundation gave financial support to a number of social science institutions that could easily be kept track of, from the London School of Economics to the Geneva-based Institut universitaire des hautes études internationales to the Institute of the Social Sciences in Stockholm. As for the support granted to German and Austrian institutions, it was quite obviously linked to the grants programme that had started earlier. In Austria, the two major recipients were the non-university Institut für Konjunkturforschung (Institute of Business Cycle Research) and the Psychological Institute of the University of Vienna, the very institutions from whence the first Austrian fellows had come, impressing the RF officers with their seriousness during their stay in the United States and, thus, enhancing the latter's readiness to grant support to their 'home institutes'. A similar link exists in the case of the Institut für Weltwirtschaft (Institute for the World Economics) in Kiel, although the first fellow to come from it, Andreas Predöhl, did little to impress the RF.

What is more important, however, than the close connection between the granting of fellowships and the support given to institutes is that the two fellows who went to the United States from Vienna obviously returned with the practical experience of dealing with funding institutions and with the administration of research based on third-party funding – i.e. with the very project culture that is being discussed here. In the following years, research in project form was practised at both institutes, where Bühler and Morgenstern very actively assumed their leadership roles, and the institutes, in turn, benefited from this by having their grants more than once renewed by the RF.<sup>3</sup>

A controversial issue in this context is whether the foundations were biased in their commitment. They were criticized for acting in the best interests of their founders, or of capital, or of American imperialism, or for pursuing a secret mission to export the American way of life and bring the rest of the world under US hegemony; they were said to discriminate against minorities and outsiders while leading a worldwide search for talents to import to the United States or, at least, to cement the ideological hegemony of all that was implied by one or the other – or all – of these catchwords. In the attempt to formulate an adequate response, the first thing that has to be pointed out is that responses have to be different for different phases.

For the pre-First World War period, at least for the Rockefeller foundations that were the main actors at the time, it can truly be said that their work was not directly aiming to promote the concerns of the founder's family and their capitalist enterprises. Apart from a short episode at the very beginning, which ended in disaster, none of the support measures was in favour of the concerns of the Rockefellers or their associates. The same is true for the other foundations at that time. Similarly, no support for American foreign policy and no concerted action between foundations and the State Department can be observed. Rather the contrary seems to be true, given that even while President

Wilson's move to have the United States become a member of the League of Nations was rejected by both houses of Congress, the Rockefeller Foundation was giving massive support to institutions associated with the League of Nations, as well as to the League itself. In the 1920s and 1930s, the RF acted quite autonomously, with little regard for government policies. Its commitment to the promotion of scientific progress in view of the wellbeing of all mankind went so far that the RF, while not showing any sympathy for the communist regime, established contacts even with the Soviet Union in order to support local scholars. Similarly, RF officers detested the Nazis but did not immediately discontinue support after 1933, not least at the massive insistence of German scholars. In the interwar period, political orientations were not decisive for their selection of the individuals, institutes or countries to be supported, even though foundation officers who were politically left of centre preferred individuals who shared their convictions and tended to avoid anti-Semites, Nazis and communists.

During the Second World War, the RF did not remain politically neutral, doing a great deal to help save threatened scholars in Europe and supporting projects that could be described as contributing to the 'war effort' in the broadest sense. While not involved in actual weapons research (most notably the development of the atomic bomb), the RF did participate in studies on the effects of propaganda and counter-propaganda, on the possibility of influencing the morale of the American population, and on the development of new management methods that may even have become effective in the conduct of the war.

After the end of the Second World War, the picture changed. There were more and more formal arrangements between the foundation and the occupation authorities, initially aiming at the 're-education' of the Germans, and after the beginning of the Cold War, more offensively directed against communism as well. Along with support for the sciences, they were now committed to a democratic mission as part of the fight against totalitarianism. The Ford Foundation, which started operating only after 1945, took a much more prominent stance in this regard than the RF, which did not depart much from its previous attitude.

An examination of the research programmes that were actually supported reveals, however, a partiality for empirical research which, after 1945, was specified to the effect that research that limited itself to a recording of facts was now considered deficient and was no longer supported. For an analysis of foundation policies, not even the very vague concept of cultural hegemony, which goes back to Antonio Gramsci, seems adequate. If one wants to avoid the fallacy, widespread since Hegel, of taking what is real for what is true and to maintain that everything that happens to be the case is a manifestation of the dominant hegemony, then one has to come to the conclusion that the RF had by no means elevated the consensus to which a majority of contemporaries would have agreed to the level of a maxim governing their support decisions. Armchair research, philosophical speculation and 'library research'

are the research programmes that failed to find favour in the eyes of the foundation officers; but they were quite ready to make exceptions, particularly when granting fellowships, and one cannot help feeling that in doing so they may have hoped to bring about some kind of conversion on the part of the fellow in question, which in some cases actually occurred. No such conciliatory attitude is found with respect to the grants awarded to institutions; at any rate, no renewal was approved when it turned out that only traditional research was conducted.

There was no political screening of scholars by the foundations, and members of extreme right-wing or left-wing parties are to be found among those who received RF support (not out of any intention to promote their political agenda, but simply because there was no enquiry into their political beliefs).

The institutional conditions and changes outlined here were primarily created in and for the United States. The fact that the RF was prepared to channel a portion of its financial aid – albeit a small one – to foreign institutions and to scholars from abroad and, in doing so, chose not to discriminate against the former enemy states of both world wars, resulting in transatlantic enrichments. The allotment of funds to Europe and the invitation to European scholars to come and do research in the United States allowed for the crossing of elements of the European research culture with elements of the American research culture that would otherwise not have gained ground. Without the money from overseas, many a European research project of the interwar period would never have come about, and many a scholar's outlook would no doubt have been quite different.

Apart from these immediate effects, the support given to Europeans by American foundations had two side-effects that made themselves felt in the time before the Second World War. One of these was that the beneficiaries were being socialized in all the new routines and tools of the American system, the most important result of which seems to me to be the transplantation of the 'project' to Europe. Designing short-term research projects revolutionized the social sciences by endowing them with an (albeit tacit rather than explicit) affinity and open-mindedness for new techniques and procedures such as the sample, the case study or cooperative research, as revealed by a comparative review of three of the best-known German-language research projects of the 1930s: the *Studien über Autorität und Familie* (translated as *Studies of Authority and the Family*; Fromm 1936) and the survey of German workers conducted on the eve of the rise National Socialism, on the one hand, and *Die Arbeitslosen von Marienthal* (*Marienthal: The Sociography of an Unemployed Community*; Lazarsfeld-Jahoda and Zeisel 1933), on the other. While the early studies of the Frankfurt Institute of Social Research were conceived and ultimately brought to a conclusion without any influence from the United States, the Vienna study was already conducted in the context of support given to the Vienna Institute of Psychology in terms of a RF grant. The major part of Erich Fromm's *Studien* is the result of prototypical 'library research'. The findings from empirical surveys that were also reported were

regarded, by the inner circle, as mere details to be published for tactical reasons as proof of their competence. The study on the attitudes of German workers, conceived by Fromm and published as late as 1980, is no match on the methodological level for American studies that were conducted at the same time; it was innovative only against the German background. Both Frankfurt undertakings lack the formative experience of project research. In contrast, the *Wirtschaftspsychologische Forschungsstelle* (Research Unit for Economic Psychology) in Vienna, having to abide by market conditions, was obliged to complete its studies in a short time and was perfectly up to the task. Here, the indirect effect of American models was stronger than any personal exposure to the new research culture. The really decisive factor, however, was that in Vienna empirical research was seen as much more important than in Frankfurt, where an orientation to theory prevailed to the point of preventing research from ever being completed. In both cases, publications were cooperative works, with the respective institutes acting as the editors. But in the Frankfurt case, the contribution of the head of the school has the most prominent place while in the Vienna case no authors' names are given for individual parts.

For individual scholars, the social relations established as a corollary of RF aid were even more important than the import of American research routines. For many of those driven into exile by the Nazis, the very existence of such ties was of vital importance. Those who emigrated on their own initiative were initially accommodated by colleagues they had come to know at previous visits – and contrary to what is commonly believed, these were neither their own students nor students with roots in the Teutonic science system. With organized aid for refugees, differences in culture once more became obvious. In the spring of 1933, right after the handing-over of power to the Nazis, aid committees were set up, or at least strongly advocated, in various countries. The two most important were the British Academy Assistance Council, later renamed Society for the Protection of Science and Learning, and the Emergency Committee in Aid of Displaced German (later: Foreign) Scholars, set up in New York. An analysis of the origin of and actions taken by these two aid committees shows that scholars of both countries were prepared to come to the aid of those among their German colleagues who found themselves in serious difficulties. In providing this aid, however, they had to take into account conditions in the national labour market where many scholars, and primarily the younger ones, were out of work due to the world economic crisis. For the whole period where assistance was provided, this was a fact and the maxim that guided both organizations, ruling out any attempt to interpret the oft-quoted remark of an American academic administrator – ‘Hitler shook the tree and I picked up the apples’ – as saying that there was any active recruitment going on. There is a little evidence for this claim, for instance the transfer of the Warburg Library from Hamburg to London, which the Americans would have liked to have had instead, but this was a library and not a refugee in search of a job.

While the conviction that they had to help their evicted German colleagues was shared by almost all scholars in the United States and in Western and



Northern Europe (the few anti-Semitic scholars outside of Germany may well be ignored), the means at their disposal strongly varied across countries: in Austria, people met in the coffee house, agreed that something had to be done and never got beyond the planning stage (cf. Fleck 2003); in Britain, helpers could appeal to their colleagues to participate in a self-taxation action, and when the number of those in need of help kept increasing, they could turn to His Majesty's government, which was receptive to their demand. In all these years, however, the financial aid granted in Britain was essentially what in social policy is called a 'one-time grant'. Hardly anyone received more than this interim aid, often enough granted with the aim of enabling the recipient to buy a steamer ticket and go on to the United States.

The New York Emergency Committee, in contrast, based its policies on the by now established routines of the American science empire: financial aid was allocated by a selection commission, its amount corresponded to the salary of a scholar in a regular job, it was granted for one year at a time (with the possibility of renewal), and it was given not directly to the person in need but to an institution which, in turn, was held responsible for the co-financing and subsequent integration of the recipient into their regular staff. This largely corresponds to the model of the research project and is a far cry from British state support. Beneficiaries were selected according to the modalities observed for the awarding of fellowships: peers were approached for a confidential report, previous research achievements were taken into account, and chances for careers and establishment were considered. Those who, during their first grant, could demonstrate that they were able to carry on with their scientific work in the new surroundings improved their chances for further support. Rather than follow the bureaucratic model of a strict set of regulations and impersonal decisions, however, the Emergency Committee proceeded as a rule on a case-by-case basis.

Reviewing the work and the results of these emergency committees in the context of the science systems in Britain and the United States, it becomes evident that only the American system actually had the capacity to absorb. While those supported by the British committee could, in the best of cases, find some niche to exist in, many of those who had emigrated to the United States managed to get a regular job in a comparatively short time. Those supported by the Emergency Committee were only a tiny minority among all those immigrants who went ashore in New York Harbor endowed with large amounts of European educational capital. To say so, however, by no means diminishes the importance of what this organization achieved.

The comprehensive literature dealing with these transatlantic migrations of scholars and intellectuals unfortunately tends to adopt a somewhat limited perspective. Since most of these studies are dedicated to narrations of individual biographies or to the description of smaller groups of emigrants, too little account is taken of the context, and where the context does appear as the background of a biography, it is no more than a cluster of conditions the hero had to cope with. It is one of the rules of biographical narratives that

adversities are (and have to be) presented in a dramatic way to have the protagonist stand out against this background all the more brilliantly in the case of success, or all the more tragically in the case of failure. Overviews that list all the great and the successful nevertheless fail to give an adequate picture of the collective aspect of this migration. And finally, comparisons with the countries the emigrants came from have been conducted very superficially. Balances of losses, instructive as they may be in the individual case, have favoured the idea that 'at home' there was no one left. This situation, in turn, spurred other authors to take a closer look at those who worked as social scientists during the Nazi period. But the finding that there was still a substantial number of them conducting research in the service of the race, the '*Volk*', and the extermination machinery, still remained an isolated fact since it failed to relate to those who had been driven into exile and to the work they had done after emigration.

A systematic comparison of the two generational units who lived through the Nazi period in so different a way – those who benefited from it or, much less frequently, were driven into internal migration, and those others who were traumatized and displaced and had to burn all bridges behind them to start out anew – has as yet not been attempted. Any such undertaking would come up against a great number of serious difficulties. In the first half of the twentieth century, it was quite unclear who was a sociologist in the German-language countries, since there were neither grades nor professional associations that could be relied on for demarcation. Thus, for want of a well-defined population consisting of the members of an emerging discipline, all statements based on arbitrary samples have to be read with caution. One has to make do, in this field, with all sorts of second-best solutions. The line taken in the present work was to include so many individuals and careers that biases ceased to matter in any systematic way. The more than 800 cases included in the analysis can be assumed to represent the quasi-totality of the population.

In analysing this generation, an additional differentiation was used besides the obvious one between emigrants and the 'home-guard', i.e. the differentiation between Germans and Austrians, since prior studies had shown that despite many commonalities the two German-language countries differed in many respects. The Teutonic science culture existed in two variants geared to the two nation states; in the present study, individuals were identified as Germans or Austrians, respectively, according to their place of residence, or to the length of their stay in either of these national cultures, in the period under investigation. The most significant difference between Austria and Germany was the stagnation of positions in Austria that, as the heiress of a larger empire, was obliged to continue the science system of the latter, which was perceived as oversized. The academic heritage of the monarchy was assumed, but not taken care of. The university system of the First Republic persisted at the pre-First World War level, while in the Weimar Republic, a moderate expansion took place. Second, Austria's First Republic also inherited the monarchy's German-speaking elites. Furthermore, in its centre, Vienna, the population

included a comparatively high proportion of Jewish people who were ready to assimilate and motivated their children to dedicate themselves to the acquisition of educational capital, a wise line to pursue given the desolate economic situation. This resulted at once in the emergence of a nationalist radicalism of the non-Jewish petty bourgeoisie, who saw their educational capital jeopardized by inflation, and a large number of young university graduates who were denied access to a university career because there were no vacancies. A side-effect one cannot help but call ironic was that no prejudice was needed to keep the Jews out of the universities. The underemployed, but well-educated young people somehow struggled along in interwar Vienna; some of them imitated the peregrinations of the journeyman and took to the road, others administered the diminishing fortune they had inherited from their parents, still others buried the hope for a university career and worked as bank directors or sports reporters, and many filled the coffee houses and other meeting places that favoured the uncommitted exchange of ideas. This surplus production of talents in Vienna was conspicuous even for the RF emissaries who were hard-pressed to cope with the mass of those who were worth supporting. In comparison, but in comparison only, the situation in the Weimar Republic was somewhat better. New universities and other institutions had been established and offered a substantially greater number of regular jobs, which even prompted some immigration from Austria. In the Weimar Republic, Jews were still able to obtain their *Habilitation*, and some of them even a job, while in Vienna the '*Ungeraden*' ('odd ones'; the code word for Jews and leftists) were denied access to private lectureships.

When the Nazis marched into the world of research and imprisoned their political adversaries, and when immediately afterwards the victims of their racist mythology had to vacate their jobs, the ensuing wave of emigrations took a different course in each country and had distinctly different consequences for the refugees. While in 1933, many could still cherish the hope that the ghastly goings-on would soon be over, nobody thought so in 1938, after the *Anschluss*. It is therefore quite possible that initially the German emigrants actually planned to only temporarily take refuge abroad, and that they quite literally went into exile: in exile, one does not intend to feel at home and, therefore, is likely not to try too hard to adapt quickly and completely to the new circumstances. In addition, the mental bond to their country of origin may have been stronger for German refugees than for Austrian refugees who at the time did not yet really conceive of themselves as a nation and whose Jewish members were therefore significantly less patriotic than the German Jews. With respect to the Jews in Austria, the fact that they or their parents had only recently come to Vienna may also have contributed to their weaker national self-perception as Austrians. Finally, it has to be kept in mind that in Vienna during the *Anschluss*, the Nazis managed to repeat in a few days what had taken them years to accomplish in the Old Reich. In addition, their dealings with their adversaries and victims at the three Austrian universities were much more brutal than they had been even in the worst German Nazi

strongholds. In the spring of 1938, even non-political, non-Jewish professors and hardly known junior social scientists suffered incarceration by the Gestapo and internment in concentration camps. Karl Bühler, Bruno Bettelheim and Paul Neurath are cases in point.

With respect to the cultural orientations and the existence or non-existence of a specific Austrian habitus in the collective of expatriates, all that can be said is that this must always be subject to the caveat that we dispose of too few self-reports. Concerning some of the harder variables, by contrast, the difference between Germans and Austrians can be established with greater reliability. Emigrating Austrian social scientists were younger on average, they left precarious jobs in their profession and, therefore, were still on a lower level of their university careers than the Germans. Those who managed to escape came to host countries that were better prepared, in two respects, for receiving the new immigrants than they had been five years earlier. The most devastating consequences of the world economic crisis were over, or at least wearing off, the war that loomed on the horizon led to a recomposition of the labour market for university graduates (due to the transfer of staff to government posts, there were vacancies in colleges and universities), and meanwhile for the transition period there were the committees for aid for refugees that did their work quite routinely. Having said all this, it should come as no surprise that the social science emigrants from Austria were more successful than their German colleagues.

Measuring success in the world of science is not a simple task, and attempts to this end have always been accused of being arbitrary or relying on external features or merely reflecting an ephemeral vogue. Moreover, there is no agreement at all as to what should be taken as evidence for scientific success. All the scepticism about this kind of measurement strongly contrasts with the day-to-day experience of mutual competition and the jealous efforts of all those involved to get a larger share of the cake of attention, as well as with individual efforts that can again and again be observed to be more dedicated to the management of attention than to the research that ought to be its basis. The attempt to differentially establish the success of past generations of scientists meets with even more difficulties. Here, the flaws of such data as are available play as important a part as the necessity to resist the temptation to apply latter-day standards to earlier times.

Today, the most common tool for establishing success is the citation analysis. It shows how often and for how long an author or a publication was cited by others. However, the relevant information does not date back very far in any systematic way (and current data are subject to the caution that the independence of the measurement cannot be vouched for – success can be actively influenced by citation cartels, selective choice of publication organs, patching up of research and multiple publications). Electronic storage, such as full text database systems, and highly sophisticated search technologies make up for the lack of historical citation indexes (and one may reasonably assume that in past decades, there were fewer, if any, attempts to strategically influence one's

own position in this ranking). Thus, the systematic search in full text database systems, such as JSTOR, dedicated to the archiving of older scientific journals, allows for something like a historical citation analysis.

Against this procedure, an objection of some weight can be raised: the limits of the analysis are set by the journals archived in the database; authors who published or were cited in other journals are put at a disadvantage. To make up for this selectivity and to complete the present reputation measurement, some further sources were used. The importance of a scholar also comes to be expressed in, for instance, biographical articles in dictionaries and encyclopedias, or in Who's Who-like registers. For sociology or the social sciences a number of such reference works have been published in the last four decades and, in principle, include most of the individuals belonging to the generation of concern in the present study.

The different measurements were aggregated in a weighted index that gave more weight to the authorial productivity of the author in question. As for the interpretation of rankings, comparing individual authors seems fairly irrelevant – after all, the fact that Sigmund Freud rates slightly higher than Karl Popper, or Albert O. Hirschman higher than Theodor W. Adorno, should not invite any far-reaching conclusions. Comparing groups, in contrast, indeed makes sense. When all those included in this analysis are grouped by age cohorts, the distribution of social scientists with the highest reputation is u-shaped. While the reputation gained by the members of the oldest cohort, which included Freud, Max Weber and Georg Simmel, among others, is higher than that of the subsequent three cohorts, each covering one decade, the cohort of those born in the first decade of the twentieth century, which included Adorno, Lazarsfeld, Morgenstern and Popper, reached similar reputational heights as the oldest cohort. Another between-group difference is suggested by the names cited in this context. A comparison of all 823 social scientists with respect to their reputation and the country with which they were associated shows as many Germans as Austrians with top reputation and approximately the same mean value, but more Austrians with above-average rankings.

While among the social scientists who emigrated during the Nazi years, those coming from Austria clearly rank higher, on average, than the Germans, the reverse is true for those who stayed at home during those years. Among the social science emigrants with the highest reputation, Peter Blau, Alexander Geschenkron, F.A. Hayek, Paul F. Lazarsfeld and Alfred Schütz are Austrians, and Reinhard Bendix, Lewis A. Coser, Kurt Lewin, Jakob Marschak and Karl Mannheim are Germans; but viewed as a group, the Austrians accumulated more attention from their contemporaries and from later generations than their German counterparts. By contrast, German non-emigrants acquired much more reputation than those Austrians who had not left their country during Nazi dictatorship. Werner Conze, Elisabeth Noelle-Neumann, Carl Schmitt and Dolf Sternberger are some of the German non-emigrants whose reputation was not equalled by the Austrians, even with Othmar Spann and Ernst Topitsch among the latter.

The strengths of the American science system are also evident in that it managed to grant access to a remarkably large number of emigrants. In the mid-1950s, almost one hundred immigrants from Austria held professorships in the United States, while at the same time there were only two professors who were teaching sociology full-time in the Austrian homeland and perhaps a dozen more who did so sporadically. The numbers clearly show that it was forced migration which, for many of them, opened up social science and university career options that without Nazi dictatorship they would never have had in their home countries, if only for the fact that there were no such positions.

Those who were granted access to the university field in the United States – and it can be supposed that almost nobody was confronted with insurmountable barriers in those years (which remains true even if one considers the few exceptions who did not succeed in gaining access during their American exile) – were able to climb the university career so fast that those who made it to the very top did so at the same age as their generational peers who had preferred to stay at home. These spectacular aspects of the American science empire – high absorption and quick advancement of immigrants – substantially contributed to its becoming dominant in the postwar years and a model for countries worldwide.

A collective biography can only ever be as good as the existing data permit. Regrettably, in the present case, these are heartbreakingly meagre. Virtually no biographical reference work includes information that is as simple to collect as father's profession or religious affiliation, and archival material is often incomplete and very selective as well. This is no doubt the reason why collective biographies have as yet only been written of groups of individuals for whom these data are available as a matter of routine, such as members of the Royal Society, people registered in the *Dictionary of Scientific Biography*, Austrian 'Sektionschefs' (department heads), members of German parliament, or individuals who could be given a questionnaire. But even if all the socio-demographic standard data were available, there are still a number of important aspects about which nothing could be said. Individual action strategies and ephemeral but consequential chance interactions cannot be analysed in terms of a collective biography, nor is it possible to reconstruct the internal perspective of several hundreds of actors. The only way to make up for this is to supplement the perspective of the collective biography with a more traditional one, i.e. the in-depth analysis of a number of carefully selected individual cases.

In order to do so in the present study, individuals and research projects were selected whose milieu of origin were the two institutes that have been assigned a paradigmatic role in the literature on the development of the social sciences in the German-language countries: the Österreichische Wirtschaftspsychologische Forschungstelle (Austrian Research Unit for Economic Psychology) and the Frankfurt Institut für Sozialforschung (Institute of Social Research). For the latter, there are a number of comprehensive descriptions of its history (Jay 1973; Wiggershaus 1986), but these are one-sided since they mainly rely on

self-reports by members of the institute and on the many documents that make up the Horkheimer Archive, which is virtually the legacy of the institute. The literature on the Forschungsstelle and its *spiritus rector*, Paul F. Lazarsfeld, while less voluminous in comparison, is also primarily based on the documents left behind by Lazarsfeld which, not unlike the Horkheimer papers, can in part be seen as the archives of the research institutes of which he was the founder or the director. Before the present study, no use had been made of American sources such as the archives of the Rockefeller Foundation, the American Jewish Committee and the Ford Foundation, or of the papers of American social scientists such as Gordon W. Allport, Else Frenkel-Brunswik, Everett C. Hughes and David Riesman. And no interviews had been conducted with witnesses of the period such as Daniel Bell, Lewis Coser, Marie Jahoda and Robert K. Merton. Drawing on these sources enabled me to add some facets to the familiar pictures and to reveal some things in a somewhat different light, and at some points even to find a new way of telling the story.

In the Radio Research Project, Lazarsfeld tried to win over Theodor W. Adorno, the proponent of critical theory, to empirical research. Lazarsfeld's persistence in this was remarkable, as was the risk he ran with the sponsor. Adorno was quite ready to play along, much more so at any rate than he was willing to concede retrospectively in an autobiographical text written shortly before his death intended for publication in the United States. The end of Adorno's first career as an empirical social researcher was not brought about by Lazarsfeld, but by the RF officers who refused to be convinced of Adorno's usefulness. By then, Adorno himself was no longer interested in continuing this kind of work anyway, having seen a possibility to replace Marcuse as Horkheimer's closest collaborator and to cooperate with the head of the institute on the great book which became *Dialectics of Enlightenment*.

During the work on this manuscript, a second incursion of critical theorists in the realm of empirical social research occurred. The Institute of Social Research had been unlucky in placing its endowments in Wall Street and was badly in need of third-party funds to continue with its activities. Due to their lack of experience with project design and funding acquisition, several attempts to do so failed until, finally, with the American Jewish Committee, an organization was found that was ready to employ Horkheimer as a research director and to fund some of his collaborators. However, of the five tomes of *Studies in Prejudice* completed on this basis and which, back in Germany, the Frankfurt Institute was so eager to refer to as proof of its competence in the field of empirical social research, only one, *The Authoritarian Personality*, was actually the result of this kind of research. Since this is a study by four authors, it is mostly cited as 'Adorno *et al.*', suggesting that Adorno was the senior author of the study. Studying the documents submitted to the American Jewish Committee and accounts of the other three authors and those who witnessed the genesis of the book, however, one gets quite a different impression from the one which Horkheimer, Adorno and Co. succeeded in laying down as the standard narrative for German histories of sociology. Adorno's contribution

to the actual investigation of the authoritarian personality syndrome was extremely marginal, and the chapters he contributed were based on the types of anti-Semitic personality that had been established even before any empirical work had been done. Empirical research, then, contributed a certain amount of illustrative material, but the types in question neither resulted from it nor were verified by it.

Nevertheless, after its return to Germany, the Frankfurt Institute was for a certain time quite successful in garnering social acceptance for its competence to do empirical social research. This was only possible because during the twelve years of Nazi dictatorship, German sociology had reverted from a promising, colourful, early spring meadow to a brown steppe. When the Rockefeller Foundation was again dispatching its officers to Germany and Austria to explore who or what it might support and what had become of the institutes that had once been supported, they returned with reports that emphasized this very image: the image of a sociological steppe populated by a few fellahin who had served the regime that had just been brought down. Thus, the Frankfurters' claim to competence hardly met with resistance. Other remigrants, as well as the American occupation forces and American guest professors, attempted a recultivation which, after many an effort, they more or less accomplished. But the deficit as compared with the United States was huge, and sociology only began to recover when imports from the United States started again, and junior scientists were sent overseas for training.

In Austria, where virtually nobody with any new social science proficiency remigrated and where the occupation forces left the liberated country to its own devices, no such reanimation was achieved. Austria's contribution to the development of sociology in the twentieth century consists in having driven a large number of promising young candidates for social science expertise out of the country. Those who accommodated them largely profited by it, and the less traumatized among them went to their former home country for their summer holidays.

In the three decades between the mid-1920s and the mid-1950s, the development of empirical social research was so dramatically accelerated, and the general institutional framework so favourable for this kind of research, that only those scholars who were actively involved could keep up with it. To be sure, the conjuncture of several developmental threads and their successful knotting played a decisive role in this, but it would be quite erroneous to credit any one of these red threads with sole authorship.



# APPENDIX

## Comparative Income

In the present book, information on individual salaries and fellowships as well as on the financial support given to institutions plays an important part. Whenever it was possible, this information, rather than just report some isolated data, was contextualized in one way or the other and made understandable by confronting it with other data, contemporary to it or subsequent. The present Appendix proposes yet another attempt at contextualization by comparing the personal income of scholars at the time. Comparisons of salaries across countries are problematic in more than one way. The following presentation hopes to steer clear of any such pitfalls. Valid data on the income of academics are rarely found in the literature. Comprehensive biographies are the most likely source, but they hardly ever bother with a comparative perspective (for an exception, see the biography of Sombart by Lenger 1994). Besides the question of who earned how much, there is the even more difficult question of how to go about doing an international comparison of incomes. In order to get a rough idea of income differentials on a national as well as international level, two methods will be used. First, the range of incomes earned by academics will be presented for the countries that were the main object of the present study, i.e. Austria, Germany, Great Britain and the United States. Second, these incomes will be related to the respective gross national product per capita. Both types of data should at least allow for a rudimentary international comparison.

Data on the income of Austrian university teaching staff can be distilled from various sources. For three categories of teaching staff – university assistants, extraordinary professors and ordinary professors – one can consult the ‘pay scales’ that were in effect at the time. In the nineteenth and twentieth centuries, Austrian university teachers were civil servants and, thus, benefited from the system of pay raise every two years. For all three groups, initial salaries were about 56 per cent of the maximum reachable final salary, but not all three groups started their careers at the lowest level. In the case of an appointment as full professor, the pay level was agreed upon in individual contract negotiations; for young university assistants, the pay level was invariably established according to the model of previous service, which as a rule meant one of the initial salary levels. Income differences between the three status groups were considerable. Pay for university assistants was lower by one-third, on all levels, than that of an extraordinary professor whose pay, in turn, was lower by

one-third than that of a full professor. An assistant – in the improbable case of this being a life-long status – would, at the end of his working life, not be able to earn more than 44 per cent of the income of a full professor.

However, due to additional allowances and special payments, the real income of Austrian university teaching staff was considerably higher than what can be read off from the pay scales. These additional allowances, which used to be considerable, are less easy to establish. In the case of professors, sources that allow us to come to some conclusions concerning the dimensions of this additional income are the nomination decrees which are included in some of the archived personnel records. For instance, when Hans Kelsen was appointed full professor of public and administrative law at the University of Vienna in 1919, he was paid, in addition to his annual salary of öS (Austrian Schilling) 8,960, an '*Aktivitätszulage*' (activity allowance) of öS 2,576 and a '*Wiener Ortszulage*' (Vienna residential allowance) divided between a so-called '*Mehrbezugszulage*' (supplementary expenses allowance) of öS 560, and a '*Teuerungszulage*' (cost-of-living allowance) of öS 6,160. For Kelsen, all these allowances added up to more than his base pay of öS 8,960. Still, Kelsen's base pay was much lower than that of Othmar Spann, who was appointed full professor in the same year and was granted a 'base pay' of öS 10,080. Furthermore, Spann was not only granted a higher salary, but also a total of allowances that, in turn, was considerably higher than this salary. He was able, only one year after his appointment, to negotiate another substantial raise in his allowances in response to an attempt to lure him away from Vienna by an offer of better pay. The real income of professors was further increased by the fees they collected for lectures and examinations, the amount of which is not specified in their personnel records, and by honoraria for publications and lectures, which in the interwar period were far more important than they are today. Hans Kelsen, for instance, drew the handsome sum of almost öS 11,000 from the latter as an additional income in 1932, when he was already teaching in Cologne.

In the years after the onset of the world economic crisis, the level of the Gross National Product (GNP) was considerably reduced whereas the income of university teachers with civil servant status remained virtually unchanged (in Germany civil servants salaries were reduced between 1932 and 1938 whereas in Austria civil servants were laid off). In order not to use a GNP level that reflects these cyclical fluctuations, the data chosen as a basis for comparison are those of the year 1929. For each of the three groups of Austrian university teachers, the top nominal income was, for university assistants, 2.9 times the GNP per capita, for extraordinary professors 4.4 times the GNP per capita, and for full professors 6.6 times the GNP per capita.

The real income of Austrian full professors, which, as the above examples show, could easily be twice as high as their nominal pay, was about twelve times the GNP per capita. Since it can be assumed that the lowest status groups were not in a position to realize comparable sums from allowances and additional income, a conservative estimate would be that incomes of university

teachers in Austria's First Republic ranged from three times to twelve times the GNP per capita.

Concerning the real incomes of German-speaking scholars, there are some interesting records in the archives of the Society for the Protection of Science and Learning (SPSL) at the Bodleian Library, Oxford. The Academic Assistance Council (AAC), the precursor organization of the SPSL, required German and Austrian scholars who had been evicted and were asking for support to supply information on their last income, among other things. Even though not all of them completed this section of the questionnaire, the data that do exist are sufficient to serve as a basis for calculating the incomes of German and Austrian scholars immediately before emigration. Among those who supplied the data on their income are former full professors, i.e. high-level civil servants, but also junior scientists whose income was very low. The top earners who supplied this information in the questionnaire included Kelsen, already mentioned above, who was a full professor in Cologne before being evicted. He reported RM (Reichsmark) 45,000 as his last annual income and noted that he had realized an additional SF (Swiss Francs) 31,000, or RM 17,500 (at the rate of exchange at the time), per year from honoraria and lectures. However, Kelsen was by no means the top earner among the 'refugee scholars'. Philosopher Fritz Heinemann, who had been appointed extraordinary professor in Frankfurt in 1930, reported an annual income of RM 65,000, without further specification. Others reported considerably lower annual incomes that, however, still ranged from RM 21,000 to RM 26,500 for full professors. The lowest incomes were reported by Günther Stern, who later called himself Günther Anders, aged thirty-one at the time, and Franz Borkenau, aged thirty-two. While the former had no regular job but reported an annual income of RM 5,100, Borkenau had a regular income of RM 3,000 as a member of the Institut für Sozialforschung (Institute of Social Research). For all that, their annual incomes still were about 2.5 times and 4.3 times, respectively, the GNP per capita for Germany in 1929. Top earners Kelsen and Heinemann had annual incomes of fifty-two times and fifty-five times, respectively, the GNP per capita, while professors with a more moderate income had to be content with a factor of eighteen to twenty-two. The average income, as established by an admittedly somewhat arbitrary computation, of German academics who turned to the London organization for relief to refugees was seventeen times the German GNP per capita.

Austrian scholars who turned to the Society for the Protection of Science and Learning (the AAC successor organization) after the *Anschluss* reported much lower incomes in the questionnaire, which was unchanged and still in use. Among them there were Emil Goldmann and Heinrich Gomperz, two former full professors, who reported more or less the same annual income of about öS 14,000. They were topped by Friedrich Engel-Jánosi who, while not holding a paid university position, was the owner of a manufacturing plant and, thus, had an income of öS 16,000 before he was forced to go abroad. The lowest incomes were reported by Adolf Kozlik, whose monthly salary at the

Institut für Konjunkturforschung (Institute for Economic and Social Research) was öS 100, and Rockefeller fellow Gerhard Ladner who, with öS 125, topped him by a mere öS 25 per month. Else Pappenheim, a young physician at the Neurological Clinic of the University of Vienna, earned about three times as much as those reporting the lowest incomes. Those with middle-bracket incomes were lawyers, collaborators of the Arbeiterkammer (Workers' Chamber) and university teachers, who earned between öS 5,500 (Erich Voegelin) and öS 12,000 per year. While the lowest income reported in this context was about equal to the GNP per capita, the highest was eight times as high (the arbitrary average being four times the GNP per capita).

The considerably larger amount, and broader range, of German scholars' incomes as compared to the Austrians is supported by other sources. Peter-Christian Witt reported comparative data on the income of various German civil servants and scholars, confirming the income distribution outlined above (Witt 1990). According to Witt, the German Chancellor of the Reich had a salary of RM 48,870 in 1930, the Prussian Prime Minister's salary was RM 43,720 and that of the ministers was RM 39,600. In comparison, the incomes of professors as reported above were considerably higher, which might cast some doubt on the validity of the AAC data. But Witt also reported the incomes of leading officers of the Kaiser-Wilhelm-Gesellschaft (Kaiser Wilhelm Society, today Max-Planck-Gesellschaft). Thus, the president of this organization had an annual salary of RM 52,540; the director of the Kaiser-Wilhelm-Institut für Anthropologie, Eugen Fischer, already referred to in Chapter Three, had a salary of RM 34,466; and Michael Polanyi, department head at another Kaiser Wilhelm Institute and also private lecturer at the University of Berlin, still had an annual salary of RM 19,900. Since Fischer's and Polanyi's salaries do not include their income from their teaching activities, one may reasonably conclude that the incomes of Heinemann and Kelsen, as reported above, were high but not incredibly so. In Wehler, the annual salary of State Secretaries, reported as RM 26,500, is described as 'fabulous', but Wehler failed to see that his colleagues at the time, i.e. the professors, had similarly astonishing incomes (Wehler 2003, 725).

For Great Britain, only rhapsodic data can be included in this international comparison of academic incomes, which however give a quite good overall idea. From Skidelsky's biography of John Maynard Keynes (Skidelsky 1992) we know that the latter had, in 1930, an annual income of £2,000 from his professorship, and William Beveridge is known to have had, as director of the LSE, an annual salary of £2,500. Initial salaries in those years were £310 for assistants and about £660 for professors. The annual salary of Walter Adams, General Secretary of the AAC/SPSL, was £500. Halsey reported an average annual income of all British university teachers of £584 in 1928/9 and £612 in 1938/9, which is 3.7 and 3.3 times, respectively, the average income of industrial workers (Halsey 1995: 131). Halsey estimated the range of incomes for various groups of university teachers as 1:4 in the 1920s, but does not cite any sources (Halsey 1995: 129).

For the United States, there is a very precise analysis by Beardsley Ruml, former director of the Laura Spelman Rockefeller Memorial. In 1955, he published a little study on the income development of American teaching staff in the fifty years previous, with the aim of demonstrating their relative deprivation as compared to other professional groups. The incomes, reported on an annual basis, of various categories of university teaching staff primarily show that university teachers in the United States were directly hit by the world economic crisis. Their nominal incomes declined between 1929 and 1933 and did not recover until the late 1930s. The various status groups were differentially hit, with university presidents having to cope with the strongest losses, while low-status instructors got off relatively lightly provided they were not dismissed and, thus, not included in these data. Ruml's data bring to light a further historical detail which should not go unmentioned. In 1933, when the universities were most strongly hit by the fallout of the Depression, the range of incomes considerably narrowed due to the loss of income suffered by the university presidents. The \$2,000 fellowships awarded to post-docs by the Rockefeller Foundation corresponded to the income of an instructor, and the \$4,000 which the Emergency Committee in Aid of Displaced German Scholars considered to be an adequate salary for professors exactly corresponded to the annual income of a full professor at a major state university. This was considerably more than the salary of an associate professor (\$3,100) or an assistant professor (\$2,600).

The data on scholars' incomes in the four countries presented in Table A.1 clearly reveal that the Weimar Republic showed the largest income disparities. It paid its mandarins exorbitantly well. Income disparities in Austria, as well,

**Table A.1** Comparison of Annual Income of Scholars in Austria, Germany, Great Britain and USA, c.1933

	Austria	Germany	Great Britain	USA
Range of incomes	ÖS 1,200–20,000	RM 3,000–65,000	£310–2,500	\$1,900–12,000
Ratio of incomes	1:16	1:22	1:8	1:6
National GDP per capita	ÖS 1.337	RM 860	£81	\$450
Income to GDP per capita	3–12	2.5–55	4–25	2–20
GDP (in international \$) 1930	3,586	3,973	5,441	6,213

Note: Data for the range of income include in the cases of Germany and Austria also people employed outside the universities; data for GDP per capita for 1933 in current prices; data for GDP purchase parity according to International Geary-Khamis Dollars for 1930.

Sources: Maddison 2001; Ruml 1955; Witt 1999; *Bundesgesetzblatt für die Republik Österreich* 1928, Issue 6, No. 22; *Bundesgesetzblatt für die Republik Österreich* 1931, Issue 19, No. 75.

are still considerably above those of both Anglo-American countries, but Austrian professors had only moderate incomes as factors of GNP per capita. While in all four countries scholars at the lowest status level had to be content with incomes that were only two to four times the GNP per capita, the two more prosperous countries, the United States and Great Britain, did not yet show any astronomic income differentials. That in the interwar period it was the United States, of all countries, that showed comparatively modest academic incomes as well as the narrowest range of incomes, is rather more of a surprise than the generosity shown by the German Republic towards its intellectuals with civil servant status.

The incomparably high incomes of German professors surely did not prevent anybody from fleeing the Nazis when this was necessary. However, for the displaced German mandarins, the experience of their relative deprivation very likely made for some additional stress on top of all the well-known problems that made it hard for refugee scholars to adapt.

# NOTES

## Introduction

1. Lepenies named the social sciences the third culture, referring to the well-known two-culture thesis where the sciences are seen as standing in opposition to the humanities and literature (Lepenies 1985). The third culture established the social sciences as a distinct mode of knowledge and practice occupying a place in between the two traditional fields.

## Chapter 1 The Building of an American Empire

1. Initially in a 1935 lecture, Walz still subscribes to the German model (Walz 1936), whereas Hartshorne explicitly argues that, with respect to the institutions of American higher education, 'descendants by far outgrew their ancestors' after the First World War (Hartshorne 1946: 9).
2. The second edition of *Webster's New International Dictionary* of 1934 contains the following passage: 'The science and art dealing with homemaking and the relation of the home to the community; theory and practice concerning the selection and preparation of food and clothing, conditions of living, the use of income, the care and training of children, etc. also the study or teaching of home economics, or an academic department concerned with this.'
3. If one sets the value for the United States as 1, in 1910 the values are 0.96 for Germany, 0.93 for France and 0.82 for Great Britain. See Goldin 2001.
4. Data from the *Handbuch der österreichischen Statistik*, various years.
5. There is extensive literature that takes the opposite view, referring to the barred entry opportunities for later immigrants; a representative example is Coser (Coser 1984); by contrast, a quite early study based on an empirical survey of European immigrants can be found in Davie (Davie 1947). Of all German emigrants in these data, 62 per cent left their country in 1933 and 1934, while 54 per cent of Austrian refugees left in 1938 and 1939.
6. This subjective barrier most likely was a question of status rather than age. Former Vienna City Counsellor for Finances Hugo Breitner may serve as an illustration: having come to the United States at the age of sixty-six without out a university degree, he seriously considered engaging in a course of studies, as shown by his correspondence with Joseph A. Schumpeter. Schumpeter advised against it. See Joseph A. Schumpeter Papers, Harvard University Archives (cf. Fleck 1997).
7. That only the Anglo-American world (if even that) allows for 'lateral entry' may be in both cases due to the respective cultural maxims (in Britain, it is the idea of fair play that dominates, while American culture rewards the successful outsider). This can, of course, be taken as further evidence for the competitive superiority of these two scientific cultures.
8. One has to bear in mind that the sociologists accounted for in the *Kürschners* are only part of the German-speaking sociologists of the time.

9. Alfred Weber used the term of 'pensioner intellectual' in this context (Weber 1923: 173).
10. No German-language university, for instance, would have accepted someone like Thorstein Veblen as a professor. The most notorious eviction case from an American university, that of W.I. Thomas, illustrates the pattern, as does the non-appointment of Bertrand Russell at the City College in New York – on the grounds of sexual liberalism in the former case, atheism in the latter, both of which would seem to be individual personality traits of a more arbitrary nature than those that would qualify as a reason for eviction from German and Austrian universities. A comparative study of eviction cases could be helpful here, but as far as I know there is no such study.
11. Cf. Sorokin 1933; Holcombe 1933; Biegelow 1933; among others.
12. For applied research in the science and technical domain in the nineteenth century, see Fox and Guagnini 1998; for industrial research, see Serres 1994.
13. For the debate concerning the issue of introducing sociological courses as part of law studies at the 31st Deutscher Juristentag (Annual Convention of German Jurists) in 1912, see Fleck 1990: 60–64.
14. Moreover, rectors did not yield much power, as illustrated by the failed attempt of Eugen Ehrlich, temporary rector of the University of Czernowitz, to introduce research on ethnological jurisprudence at his university; see Fleck 1990: 57–60.
15. In this, I follow the terminology of Anton Blok (Blok 1981: 220–6) who expanded Eric Wolf's ideas on patron-client relations; see Wolf 1999 and Wolf and Silverman 2001.
16. C.H. Becker, *Vom Wesen der deutschen Universität*, 23, as cited in Düwell (Düwell 1971: 55); cf. Wende on how professors were imposed (Wende 1959: 110) and on how sociology was attributed with the role of helping us 'to see the whole picture' (Wende 1959: 126).
17. In addition, Worms headed the Société de Sociologie in Paris and was the editor of the *Bibliothèque Sociologique Internationale*, with its more than 50 volumes of French translations of the works of IIS members, among others (cf. Clark 1973).
18. By 1913, 32 ordinary and 14 associated members had already died.
19. As an illustration of the records that provide the basis for the following analysis, see the following examples: 'GUMFLOWICZ (Louis), professeur de sciences politiques à L'Université de Gratz. Vice-Président en 1895'; for an associated member: 'WEBER (Max), professeur honoraire d'économie politique à l'Université d'Heidelberg, membre du bureau de la Société allemande de sociologie' (*Annales de l'institut international de sociologie* 1913).
20. On the concept of 'also-sociologist', cf. Fleck 1990.
21. Among them, the familiar names of the founding generation (see Ross 1991) of American sociology: Charles A. Ellwood, Franklin H. Giddings, Edward A. Ross, Albion W. Small, Lester F. Ward and William G. Sumner, all of whom had also acted, in the early days, as Presidents of the American Sociological Society (today Association) founded in 1905. In addition, there were two other outstanding personalities, namely social psychologist James Baldwin and economist Thorstein Veblen, as well as two former ministers and the longtime President of the Rockefeller Foundation, George E. Vincent, and three professors of sociology, history and political science, respectively, who were less prominent in later times. The first two generations of American sociology are well documented, which allows for a verification of IIS representativity with regard to the United States: Charles H. Cooley was the only one who was not an IIS member.
22. Belgian IIS members, who also showed a very large proportion of members who were established at a university, were much more frequently affiliated with the neighbouring disciplines of economy and law.
23. 'In democratic ages men rarely sacrifice themselves for one another, but they display general compassion for the members of the human race. They inflict no useless ills,



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- and they are happy to relieve the griefs of others when they can do so without much hurting themselves; they are not disinterested, but they are humane.' De Tocqueville, A., 'Influence of Democracy on Manners Properly So Called', *Democracy in America*, vol. II, section 3; as quoted in Bremner 1988: 54.
24. Cf. Bremner 1988; Fosdick 1952; Kohler 1991; Lagemann 1983; Lagemann 1999.
  25. The popular American socialist Eugene V. Debbis, for instance, was strongly opposed to Carnegie's library donation. See Bremner 1988 and Lagemann 1989.
  26. There are hardly any studies dealing with the programme officers; cf. the short references to their gate-keeper functions in Coser (Coser 1965: 337–348) and, more recently, some case studies in a special issue of *Minerva* on this theme (Gemelli and MacLeod 2003).
  27. Cf. Coser 1971; Kaesler 1999; Käsler 1976; Ritzer 2000; '50 Klassiker der Soziologie' <http://agso.uni-graz.at/lexikon/index.htm> [accessed 18 October 2010].

## Chapter 2 Fellowships and What They Entailed

1. Shortly afterwards, the Carnegie Foundation set up an international fellowship programme (Duggan 1943).
2. As quoted in Bulmer and Bulmer 1981: 362–3.
3. 'Laura Spelman Rockefeller Memorial (LSRM) 1924 Annual Report', Rockefeller Foundation (RF), Record Group (RG) 1.2, series (s.) II, folder (f.) 2: 11, Rockefeller Archive Center (RAC), Sleepy Hollow, New York.
4. For a retrospective summary, see 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, box (b.) 50, folder (f.) 380, RAC.
5. 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC.
6. See the list of the organizations that were members of the Social Science Research Council (SSRC). Besides representatives of the three core disciplines of later years, economics, sociology and political science, members also included historians, statisticians, psychologists and anthropologists.
7. The internal analysis of the fellows of the first five years shows a significant distribution of disciplines: besides the core disciplines of economics, sociology, political science and history there are 'social work and social technology' and, after psychology, law, business administration, anthropology and education, 'General Social Science – Modern' as well as 'General Social Science – Ancient', 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC: 19.
8. Memorandum, as quoted in Bulmer and Bulmer 1981: 362–3.
9. This last information (*Reichshandbuch der deutschen Gesellschaft* 1931: 1731) seems somewhat inexact, to say the least, since Schumacher was only one of eleven foreign advisors, a function also fulfilled by two other advisors of the LSRM and the RF, Charles Rist and Luigi Einaudi, respectively.
10. For the German-American exchange of professors, see Brocke 1981.
11. The *Biographisches Wörterbuch zur deutschen Geschichte* refers to this as 'forced retirement' which seems hardly compatible with Oncken's age of seventy-four (*Biographisches Wörterbuch zur deutschen Geschichte* 1974: col. 2074–5).
12. Butler reports that in his first year he used to coordinate selections with three colleagues, among them William Beveridge, which later proved to be unnecessary. 'Memorandum for the Committee on Reorganization by the Memorial's Representative for Great Britain and Ireland, 31 December 1926', LSRM, s. III, Subseries 6, b. 50, f. 529, RAC.

13. 'Social Science European Fellowship Committee 1934–35', 15 April 1931, RF, RG 1.1, s. 717, b. 16, f. 151, RAC.
14. Requirements remained the same in later years, see 'Announcement of fellowships in the social sciences awarded by the Rockefeller Foundation', 3 January 1929, RF, RG 1.2, b. 49, f. 375, RAC.
15. Courses were discontinued after a few years. While all Italians attended language courses, this was deemed necessary by only one out of five Czech, French or Dutch fellows, but by one out of three Austrians and three out of four Germans. 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC: 18.
16. 'Memorandum', 29 April 1932, RF, RG 1.2, s. 100 international, f. 376, RAC.
17. Sources differ with respect to quantitative data, for three different reasons: the difference between the date when the fellowship was granted and the date when it actually started (including some withdrawals); the inclusion of other fellows from other Rockefeller foundations, primarily the International Education Board (or the Memorial's special fellowships); and the fact that directories that were published later did not always include individuals who had died or whose whereabouts were unknown.
18. The mean duration of a fellowship in social sciences dropped from almost twenty-two months for the first cohort to only seventeen months for the cohort of 1929/30. 'Memorandum on the administration of the fellowship program on social Science (Analysis)', RF, RG 1.2, b. 50, f. 383, RAC.
19. Among them Voegelin 1928, Myrdal and Myrdal 1944, Huizinga 1930 and Huizinga 1972, where he dealt with the experiences of his stay in the United States, a fact which John V. Van Sickle, an RF officer, found worth mentioning. See his letter to E.E. Day of 30 September 1930, RF, RG 2 General Correspondence (GC), b. 47, f. 387, RAC.
20. This is also suggested by the fact that on the occasion of her second RF fellowship in the United States in 1935, Lawrence Frank, already in charge of research on children at the Memorial, recommended her to researchers in her field. Fellowship Card Bühler, RAC. For support given to research in child psychology, see Lomax 1977; Bryson 2002.
21. The first printed list of fellows in 1932 notes, for instance, with respect to Harold K. Salvesen's position after his fellowship: 'on sub-Antarctic whaling grounds 1928–29, 1929–30, 1930–31. In business.'
22. In the first five years, the proportion of women dropped to 18 per cent; 14 of the 32 women came from Great Britain, 4 from France and the Netherlands, respectively, 3 from Germany, 2 from Austria; all the other countries (Poland, Czechoslovakia, Romania, Switzerland and the United States) nominated one woman, at the most.
23. The age distribution of the fellows in the first five years shows that 43 per cent were younger than 26 years, and another 38 per cent were younger than 32 years. See 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC: 23.
24. See Kuznets's autobiographical text <http://nobelprize.org/economics/laureates/1971/kuznets-autobio.html> [accessed 13 October 2010]. Thomas's ASA Presidential Address (Thomas 1952) also includes some autobiographical parts. See also the obituary by Read Bain (Bain 1951) for an idea of the reception at the time.
25. 'Social Science Fellowship Program in Europe Rockefeller Foundation 1924–38', RG 1.2, b. 50, f. 384, RAC.
26. This figure included fellows from Canada (2), South Africa (1), Ireland (6), Wales (5) and Scotland (7), 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC: 11.

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27. 'Report of the European Fellowship Program in the Social Sciences of the Laura Spelman Rockefeller Memorial 1923–1928', LSRM, RG 1.2, b. 50, f. 380, RAC: 23.
28. 'Staff meeting, 24–7 August 1927, Introductory Remarks by Beardsley Ruml', LSRM s. II, b. 3, f. 39, RAC.
29. This and all the following comparisons are based on: Fellowship Cards, RAC and Rockefeller Foundation (1951, 1955, 1972).
30. The Rockefeller Foundation, 'Social Science Fellowships of the Rockefeller Foundation 1924–1932, New York 1933', RF, RG 1.2, b. 50, f. 382, RAC.
31. In addition, assessments are based on reputation scores (from JSTOR analysis), which will be discussed in detail in Chapter Four.
32. Although Huizinga (Huizinga 1930: 369–70) extensively refers to an unpublished article of hers about her experiences in the United States, as reported by Van Sickle to Day, 30 September 1930, RF, RG 2, GC, b. 47, f. 387. Sir Henry de Waal, Ephrussi-Waal's son, informed me that his mother did not continue her scholarly work, letter to the author, 10 December 2001.
33. John V. Van Sickle, 'Diary', 19 January 1930, RAC.
34. Van Sickle, 'Diary', 19 January 1930, RAC.
35. Her very low reputation score (9) is due to the under-representation of psychological publications in this measurement.
36. John V. Van Sickle, 'Interview with E. Voegelin', 17 October 1933, RF, RG 1.1, s. 705 Austria, b. 5, f. 46, RAC.
37. 'I have tried to make a Sociological survey of Vienna and I have the people. I have tried to interest the Rockefellers but political events in Austria make them hold off' (Kaesler 1991: 129).
38. Tracy Kittredge to Syndor H. Walker, 12 September 1934, RF, RG 1.1, s. 705 Austria, b. 5, f. 46, RAC. According to his Fellowship Card, Voegelin had received \$400 in 1931 and \$500 in 1932 from the RF.
39. 'Fellowship Card Voegelin, note 11/1/58 EWM [Erskine W. McKinley]', RAC.
40. Heberle refers to his SA membership in his notes on his career (Institut für Angewandte Wirtschaftswissenschaft 1938: 83).
41. Oskar Morgenstern, Tagebuch, Morgenstern Papers, Rare Book Division, Duke University Library. See Morgenstern 1976 and Leonard 1995, who agree in saying that their close proximity in Princeton, where Morgenstern taught at the University while von Neumann did research work at the Institute for Advanced Study, was a condition for the ideas that made up the book to be elaborated.
42. Van Sickle, 'Diary', 20 October 1931, RAC. Schumpeter did not change his mind, as can be seen in his correspondence with European fellowship candidates and American officers (Schumpeter 1999).
43. Van Sickle–Kittredge, 'Diary', 30 November 1931, RAC. Morgenstern referred to Predöhl and Vossler as examples of those who had been appointed to a professorship after their fellowship.
44. Distribution across the various scientific disciplines was never balanced. In the pre-Second World War period, 40 per cent of all the fellowships worldwide were dedicated to public health and the nurses programme, while the medical and the natural sciences were each allotted one-fifth, and the social sciences and the humanities had to make do with 13 per cent and 7 per cent, respectively (Rockefeller Foundation 1951: Appendix, author's estimation).
45. Selskar M. Gunn (1883–1944) studied at the MIT and Harvard Public Health School, and worked as a bacteriologist and as an expert of infectious disease control in the United States, Europe and, later, in China. He was a RF officer from 1922, head of its Paris office until 1927, Associate Director in Europe of the RF

- Social Sciences Division between October 1930 and the end of 1932, then RF Vice-President in New York.
46. Max Mason (1877–1961) studied mathematics in Göttingen under David Hilbert, received his doctoral thesis in 1903, then taught at MIT, at Yale and at the University of Wisconsin. In the years 1917 to 1919 he was a member of the National Research Council, where he participated in the development of submarine detection devices. After that he taught again in Wisconsin, was President of the University of Chicago from 1925 and head of the Division of Natural Sciences of the RF from 1928. He was the successor of George E. Vincent as President of the RF between 1930 and 1936, and after that served on a supervisory board at the California Institute of Technology, where he retired in 1949.
  47. Letter to Max Mason, no date (c. August 1931), Mason's answer, 11 November 1931, both in RF, RG 12, s. 100 ES international, b. 49, f. 376, RAC.
  48. Their success in establishing themselves seems to have been only temporary. For the Directory of 1950, the RF was unable to establish the whereabouts of the three Czechoslovakians, and Suranyi-Unger had ceased teaching in Szeged and was now at Syracuse University, NY.
  49. 'Social Science Fellowship Program in Europe – Rockefeller Foundation 1924–38', RF, RG 1.2, b. 50, f. 384, RAC.
  50. 'Correspondence, Van Sickle and Mason', 8 and 27 July 1933, RF, RG 1.1, s. 717, b. 16, f. 151, RAC.
  51. In 1929, the 'preferred' maximum age of applicants as stated in the application form was thirty-five years. RF, RG 1.2, s. 100 international, b. 49, f. 375, RAC.
  52. Marschak, Machlup, Schneider and Lutz were included in the *Who's Who in Economics*. Cf. also the biographical articles in *International Encyclopedia of the Social Sciences* (Sills 1968) and the *Times Literary Supplement's* '100 Books of the Century, 6 October 1995.
  53. Term used by E.C. Hughes in a review of Lazarsfeld and Thielens's *Academic Mind*, 'to distinguish the people of small, particular orbits from those of larger identifications and connections'; he calls the latter 'itinerants' (Hughes 1959: 572).
  54. On Willits, see Stapleton 2003.
  55. Van Sickle was aware that the way Strauss was supported was contrary to RF rules. He justifies the procedure by citing all those who supported Strauss. Van Sickle, 'Diary', 19 October 1936, RAC.
  56. 'TBK [Tracy B. Kittredge] Interview with Fürer-Haimendorf, 10 December 1934', RF, RG 2-1934, GC, b. 108, f. 837, RAC.
  57. Paul F. Lazarsfeld, letter to Shepard Stone, 29 June 1959, 'Lazarsfeld Papers', Rare Book and Manuscript Library, Columbia University, New York; Fleck 2002.

### Chapter 3 Institutional Support in Europe

1. 'Memorandum, Executive Committee and Director to Board of Trustees for the year October 1925 to 30 September 1926', LSRM, s. II, b. 2, f. 16, RAC.
2. 'Laura Spelman Rockefeller Memorial, Final Report Summary of Work 1919–1928 Inclusive', LSRM, s. 2, b. 1, RAC: 10.
3. Edmund E. Day (1883–1951): Ph.D. Harvard 1909; Professor at the Department of Economics, Harvard 1910–23; University of Michigan since 1923; Dean of the School of Business Administration of this university since 1924; Laura Spelman Rockefeller Memorial since 1927; Director of Social Sciences Division between 1929 and 1937; then President of Cornell University, Ithaca, New York.
4. Staff Meeting, October 24, 1927, LSRM, s. 2, b. 3, f. 42, RAC.

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5. Harold Laski, 'Foundations, Universities and Research', first published in 1928 in *Harper's Magazine*, reprint in Laski, H. (1930), *The Danger of Obedience and Other Essays*, New York: Harper, as quoted in Bulmer 1986: 217; see also officers' reaction, as quoted in Bulmer 1986: 217 and Bulmer and Bulmer 1981: 400.
6. Estimations based on the sums detailed in the 'Memorandum on the situation of research in social sciences in Austria, 1931', RF, RG 1.1, s. 705, b. 4, f. 35, RAC. Erich Voegelin, at the time working as a lecturer with civil service status at the University of Vienna, reported a monthly income of ATS 200, corresponding to \$40. RF, RG 1.1, s. 705, b. 5, f. 46, RAC. Fellowships for RF fellows were \$2,000 per year.
7. Fisher reports that owing to Memorial funds, a chair for political science was established (Fisher 1980: 290–91).
8. Rockefeller Foundation 1932: 282. Support for the International Institute of African Languages and Culture was not taken into account in the present context.
9. Rockefeller Foundation 1930: 224. In 1931, the officers of the Social Sciences Division refused to support this institution on anything but a 'tapering basis': 'the strictly scientific work of the Institute had not come up to expectations', RF, RG 1.1, s. 700, b. 22 A, f. 164, RAC.
10. Beveridge 1939. Similar compilations were published for Austria, the Netherlands and the United States.
11. Due to a lack of sound information on its research programme, the Geneva Research Institute, founded in 1930 and also associated with the League of Nations, has not been taken into account here.
12. Van Sickle, 'Diary', 29 September 1930, RAC. The minutes of a meeting of the officers of the Social Sciences Division of 3 October 1932 note: 'Institute for Economic Research, Paris – the Rist plan. EED [Edmund E. Day] stated that he was still prepared to support an acceptable plan if presented before June 30, 1933. Otherwise, our commitment should be taken off the books.' 'Memorandum', Van Sickle, RF, RG 1.1, s. 700, b. 22 A, f. 164, RAC.
13. Rockefeller Foundation 1933: 243; Rockefeller Foundation 1934: 177; Rockefeller Foundation 1936: 260–1; cf. Lyon 2001.
14. 'Minutes of Office Conference, 6 December 1928', RF, RG 3, s. 904, b. 2, f. 13, RAC.
15. See the detailed overview in 'Social Science Projects in Europe 1935', also including grants-in-aid, RF, RG 1.1, s. 700, b. 22 A, f. 165, RAC.
16. 'Memorandum, Executive Committee and Director to Board of Trustees for the year October 1925 to 30 September 1926', LSRM, s. 2, b. 2, f. 16, RAC.
17. In a letter to Raymond Fosdick, James T. Shotwell reports on his visit to this table: the Professorentisch 'offered them the first chance they had ever had to get together and became a sort of Faculty Club where they learned to know each other and to discuss university problems together to the advantage of the whole university. That is symbolic of the whole situation. We should support the effort to enlarge the point of view of German scholars; keep them busy on their own tasks and not simply spending their time in self-pity as some do, or turning to tasks, commercial, or even of manual labor as in the case of the younger men.' LSRM, s. 3, b. 52, f. 558, RAC.
18. LSRM, s. 3, b. 52, f. 558, RAC.
19. The following account is based on: RF, RG 1.1., s. 717 S Germany, b. 20, f. 187, RAC.
20. The report also reveals that the research had obviously been sponsored for quite a while (Notgemeinschaft der Deutschen Wissenschaft 1930: 52, 58–60).
21. The annual report of the Notgemeinschaft (Notgemeinschaft der Deutschen Wissenschaft 1930: 185) states that for five years, the RF had supplied 100,000 RM (Reichsmark) per year, 'the Notgemeinschaft being obliged to continue supplying the same sums as spent to date'.

22. For budgetary data, see Notgemeinschaft der Deutschen Wissenschaft 1930: 198–9. In 1929/30, 10 per cent of Notgemeinschaft expenses were allotted to ‘experimental research’, about double the amount of its administrative costs. About one-third of its funds were spent on ‘individual scientific projects’. On their visits to Berlin, Gunn, Van Sickle and Kittredge were at least twice confronted, among other projects, with the research conducted on twins. Van Sickle notes that Otmar Freiherr von Verschuer, ‘is making an interesting study of twins’ but comments on the Institute in general: ‘one did not gain the impression of scientific activity commensurate with the equipment’, 13 January 1931; half a year later, Kittredge reports on the twin study, but refrains from commenting it, Van Sickle–Kittredge, ‘Diary’, 11 November 1931, RAC.
23. In his introduction, Schmidt-Ott dedicates fifteen pages, as usual, to the national sentiments that had of course never been abjured in the preceding years, expressing his ‘high admiration for the Führer’, expatiating on the ‘sacred love of the fatherland’, and promising to persevere in his ‘service to the German people’ (Notgemeinschaft der Deutschen Wissenschaft 1933: 12, 15). Nevertheless, several references are made, in the same Annual Report, to RF sponsorship; apparently, these passages had already been set up before.
24. While no RF support for ethnogeny is referred to in the otherwise very detailed chronicle of the Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften (Kaiser Wilhelm Society for the Advancement of Science) by Eckart Henning and Marion Kazemi (Henning and Kazemi 1988), support is explicitly reported in the fields of natural sciences and medical research. Hammerstein 1999 does not mention RF support. In Vierhaus and Brocke 1990, RF support is mentioned in passing, but no reference is made to the ‘*Gemeinschaftswerk*’ on German ethnogeny, and Fischer’s Kaiser Wilhelm Institute is likewise neglected. Another voluminous anthology on the history of the Kaiser Wilhelm Society and its Institutes does not refer to either RF support or racial research (Brocke and Laitko 1996). A tabular overview of the receipts of the Notgemeinschaft is given by Zierold 1968: 38–9; RF subsidies, however, are hidden under the category ‘private donations’.
25. Tracy Kittredge, ‘Social Sciences in Germany’, August 1932, RF, RG 1.1, s. 717, b. 20, f. 186, RAC.
26. Van Sickle, ‘Diary’, 27 January 1930, RAC.
27. Van Sickle, ‘Diary’, 17 February 1930, RAC.
28. Van Sickle, ‘Diary’, 15 April 1930, RAC.
29. This was Schmidt-Ott’s title, regularly referred to by the RF officers, Van Sickle, ‘Diary’, 8 January 1931, RAC.
30. Van Sickle, ‘Diary’, 4 July 1930, RAC.
31. This aid is also referred to in the 1932 annual report of the Notgemeinschaft, where the respective activities are for the first time subsumed under the heading of ‘Gemeinschaftsarbeiten auf sozial- und geisteswissenschaftlichem Gebiet’ (collective works in the social science and humanities field): ‘recently, more specifically, more large-scale studies on vital economic and social science issues have been launched, and partly conducted, also with the help of foreign funds’ (Notgemeinschaft der Deutschen Wissenschaft 1932: 19f).
32. ‘Social Science Projects in Europe’, 30 September 1932, RF, RG 1.1, s. 700, b. 22A, f. 164, RAC: 3.
33. Note of 5 November 1932 in: ‘General Memorandum, Tracy B. Kittredge’s Visit to Central Europe, October 29–November 8, 1932’, RF, RG 2- 1932 GC, s. 700, b. 76, f. 611, RAC. Some of the individuals mentioned also figure in the Annual Report of the *Notgemeinschaft*: owing to RF support, ‘two large-scale research projects on the international economic situation of our time’ could be launched. ‘Under the direction

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- of Professor H[erbert] von Beckerath, Bonn, and in cooperation with Professor E[rich] Kaufmann, Berlin, Geheimrat A[lfred] Weber, Heidelberg, Geheimrat [Kurt] Wiedefeld, Leipzig, and Priv. Doz. [Arnold] Wolfers, Berlin, the conditions for, and forms of, international economic relations are to be subjected to fundamental scientific investigation.' The 'second study is concerned with the springs, effects, and attempts at solution of the critical price development of a number of important world trade commodities. It will be conducted, under the direction of Geheimrat Schumacher, Berlin, in cooperation with Geheimrat Mendelssohn-Bartholdy, Hamburg, and others, and with the concurrence of a number of junior researchers' (Notgemeinschaft der Deutschen Wissenschaft 1932: 21).
34. It would be misleading, however, to take these differences in language use as a manifestation of differences in some vaguely defined national character. Charlotte Bühler, who was born and educated in Germany, overdid her cosmopolitanism to the other extreme. During a meeting with Van Sickle and some Vienna co-professors, she insisted on speaking English although Vienna co-participant Hans Mayer did not sufficiently master this language: 'Mrs. Buhler ... managed to rile me considerably. In the first place, despite Pribram's request that the conversation be in German on account of Mayer, she insisted on speaking English.' Van Sickle to Edmund E. Day, 17 June 1931, RF, RG 1.1, s. 705S, b. 8, f. 75, University of Vienna, Psychological Institute, RAC.
  35. Tracy Kittredge, 'Social Sciences in Germany', RF, RG 1.1, s. 717, b. 20, f. 186, RAC: 8. Quotations that follow are from the same source.
  36. RF, RG 2- 1933, GC, b. 91, f. 724, RAC. As early as in May of 1932, Van Sickle suggested that Gunn should visit Cologne. There is also a note saying that the RF had rejected an application for financial support for the Yearbook. Van Sickle to Gunn, 'Memorandum suggestions for a German trip', RF, RG 2- 1933, GC, s. 717, b. 77, f. 617, RAC.
  37. Similarly already mentioned in Van Sickle, 'Diary', 8 January 1931, RAC.
  38. This is not Edward Y. Hartshorne, Jr, known for his 1937 study *The German Universities and National Socialism*, for which he visited Germany as a SSRC Fellow in 1935/6, but Richard Hartshorne, a geographer and SSRC Fellow in 1931/2 who, during his fellowship term, visited German and Polish geographical institutions and also studied the drawing up of a frontier in Upper Silesia as well as problems of the Polish Corridor. Papers on both subjects were published in *Mitteilungen des Vereins der Geographen* at the University of Leipzig, among others (Social Science Research Council 1951: 158f ).
  39. Van Sickle, 'Memo, May 16, 1931, re: Institut für Soziologie, Leipzig, Director Dr. Hans Freyer', RF, RG 2-1931, GC, b. 64, f. 520, RAC.
  40. In a diary note of 8 January 1931, Van Sickle already referred to some Berlin researchers in European ethnology who used this survey technique. The Annual Report of the Notgemeinschaft (Notgemeinschaft der Deutschen Wissenschaft 1930: 192-93) refers to an 'Atlas of German Ethnology' for which 'almost 40,000 copies' of a questionnaire had been sent to 'representatives of the Church and the educational system, the government and self-administration, the sciences and professional organizations' in 'the whole of the Reich as well as Austria, Luxemburg, Gdansk, and the German-language territories of Czechoslovakia and Transylvania'.
  41. Van Sickle to Gunn, 'Memorandum suggestions for a German trip', RF, RG 2- 1933, GC, s. 717, b. 77, f. 617, RAC.
  42. Both Institutes had been established in 1924 with KWG (Kaiser-Wilhelm-Gesellschaft) participation, but were not promoted to be full Kaiser Wilhelm Institutes until 1935.
  43. Fehling to Van Sickle, 19 May 1932, RF, RG 1.1, s. 717, b. 21, f. 194, RAC.
  44. In his diary, Van Sickle alludes to brief contacts with psychologists on 8 January 1931: 'Short visit to the Psychologisches Institut of Köhler. Almost desert as most of

- the work goes on in the afternoon.' It could be, however, that the Medical Division of the RF was in charge of German psychologists.
45. Van Sickle, 'Diary', 29 December 1930, RAC.
  46. RF, RG 2-1933, GC, b. 90, f. 724, RAC.
  47. Van Sickle to Day, 29 April 1933, RF, RG 2-1933, GC, b. 91, f. 725, RAC; same source for the quotations that follow.
  48. As is known, the Natural Science Division continued to support German scientists until 1937 (Macrakis 1989; Macrakis 1993).
  49. Alva and Gunnar Myrdal to Van Sickle, 20 July 1933, RF, RG 2-1933, GC, s. 717, b. 92, f. 728, RAC; same source for the quotations that follow.
  50. Myrdal refers here to the German romanticism youth movement of the beginning of the twentieth century, one of the ideological predecessors of Nazism.
  51. For Jessen, see Heiber 1991: 199; Janssen 2000: 153f.
  52. Fellowship Card, Andreas Predöhl, RAC. There is another Kiel scholar whose political activities are not referred to until after 1945: In May 1953, Hans Gerth informed Leland DeVinney that Gerhard Mackenroth had joined the SS in 1933, and soon after, Frederic C. Lane noted in his diary that he had heard that Mackenroth had been in the United States in the 1930s: 'sent over ... (by the Nazis?)'.
  53. Kittredge to Walker, personal letter, 21 September 1937, RF, RG 1.1, s. 700, b. 22 A, f. 165, RAC.
  54. Van Sickle, 'Diary', 24–8 September 1930, RAC.
  55. Van Sickle to Day, 12 August 1930, RF, RG 2-1930, s. 650 GC, b. 47, f. 387, RAC.
  56. Mentioned in Van Sickle's letter to Day, 30 September 1930, RF, RG 2-1930, s. 650 GC, b. 47, f. 387, RAC.
  57. Day to Van Sickle, 9 December 1930, RF, RG 2-1930, s. 650 GC, b. 47, f. 387, RAC.
  58. Day to Van Sickle, 9 December 1930, RF, RG 2-1930, s. 650 GC, b. 47, f. 387, RAC; Gunn suggests hearing Josef Redlich on this project, who was teaching at Harvard at the time.
  59. Day and Van Sickle, telegram, 20 May 1931, RF, RG 2-1931, s. 650 GC, b. 63, f. 515, RAC.
  60. Van Sickle, 'Diary', 31 July 1931, RAC.
  61. RF, RG 1.1, s. 705, b. 4, f. 35, RAC; same source for the quotations that follow.
  62. See list of the members of the committee in Fleck 1990: 161.
  63. Gunn to Mason, 10 February 1932, RF, RG 2-1932, GC, b. 76, f. 611, RAC.
  64. Kittredge, 'Memo', 1 November 1932, RF, RG 1.1, s. 705, b. 8, f. 75, RAC.
  65. Cf. Paier 1996: 7–70; Benetka 1995.
  66. Gunn to Day, 3 May 1931, RF, RG 1.1, s. 705, b. 8, f. 75, RAC.
  67. Kittredge, 'General Memorandum Visit to Central Europe, October 29–November 8, 1932', RF, RG 2-1932, GC, b. 76, f. 611, RAC.
  68. Kittredge, 'Conversation with Prof. Mises, Paris, March 23, 1933', RF, RG 1.1, s. 705, b. 4, Format 35, RAC.
  69. Van Sickle to Day, 10 October 1933, RF, RG 1.1, s. 705, b. 4, f. 35, RAC; same source for the quotations that follow.
  70. Van Sickle to Day, 28 October 1933, RF, RG 1.1, s. 705, b. 4, f. 35, RAC; same source for the quotations that follow.
  71. Pribram had very early tried to include Friedrich Engel-Jánosi in the circle of RF beneficiaries, but had failed in his attempt to have him go to Harvard to study some Medici papers. Engel-Jánosi does not mention this episode in his autobiography (Engel-Jánosi 1974) but gives a friendly portrait of Pribram and some descriptions of his students.
  72. Van Sickle to Day, 20 November 1933, RF, RG 1.1, s. 705, b. 4, f. 35, RAC.



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73. Van Sickle to Walker, 1 December 1933, RF, RG 1.1, s. 705, b. 4, f. 35, RAC.
74. Day to Van Sickle, 20 January 1934, RF, RG 1.1, s. 705, b. 4, f. 35, RAC.
75. Engelbert Dollfuss, Austrian chancellor in 1932–4, established an authoritarian government in 1933 and was assassinated during a Nazi putsch in July 1934.
76. Van Sickle to Day, 24 January 1934, RF, RG 1.1, s. 705, b.4, f. 35, RAC.
77. Van Sickle to Day, 10 March 1934, RF, RG 1.1, s. 705, b.4, f. 35, RAC.
78. Walker to Van Sickle, 26 March 1934, RF, RG 1.1, s. 705, b.4, f. 35, RAC.
79. Van Sickle, 'Memo: The Status of the S[ocial] S[ciences] in Vienna, April 12, 1934', RF, RG 1.1, s. 705, b.4, f. 35, RAC.
80. In October 1937, \$18,000 granted for the years 1938–40 were revoked 'since it is apparent that the conditions under which the grant was made no longer exist' (Rockefeller Foundation 1937: 256f).
81. Kittredge to Walker, 10 March 1938, RF, RG 1.1, s. 705, b.4, f. 35, RAC.

## Chapter 4 In the Shadow of Nazi Rule: Two Generation Units of Social Scientists

1. These were: Jakob Baxa, Hermann Roeder and Oskar Paul Hausmann.
2. The group who reported sociology as their only affiliation also included some eminent Nazis such as Franz Jerusalem, Hans L. Stoltenberg and Andreas Walther; sociology as the first of several disciplinary affiliations was reported by Willy Gierlichs, Leopold von Wiese, Johann Plenge, Max Rumpf and Karl Heinz Pfeffer.
3. The following journals were analyzed: *Archiv für angewandte Soziologie*, *Archiv für Rechts- und Wirtschafts- oder Sozialphilosophie*, *Archiv für Sozialwissenschaft und Sozialpolitik*, *Ethos*, *Jahrbuch für Soziologie*, *Kölner Vierteljahrhefte für Soziologie* (later: *Kölner Zeitschrift für Soziologie und Sozialpsychologie*), *Soziale Welt*, *Jahrbuch für Sozialwissenschaft*, *Volksspiegel*, *Weltwirtschaftliches Archiv*, *Zeitschrift für Nationalökonomie*, *Zeitschrift für öffentliches Recht*, *Zeitschrift für Sozialforschung* (later: *Studies in Philosophy and Social Science*), *Zeitschrift für Völkerpsychologie und Soziologie* (later: *Sociologus*), *American Journal of Sociology*, *American Sociological Review*, *Annales: Économies, sociétés, civilisations*, *British Journal of Sociology*, *Economic Development and Cultural Change*, *International Journal of Opinion and Attitude Research*, *International Postwar Problems*, *Human Relations*, *Journal of Economic History*, *Journal of Social Issue*, *Journal of Social Philosophy*, *Science and Society*, *Social Compass*, *Social Forces*, *Social Problems*, *Social Research*, *Sociometry*, *Sociological Analysis* (formerly *American Catholic Sociological Review*), *Sociological Review*, *Sociology and Social Research* (formerly the *Journal of Applied Sociology*) and the *Sociological Review*.
4. In their analysis of Nobel laureates in physics and chemistry, Crawford, Heilbron and Ullrich (Crawford *et al.* 1987) also used a place-of-residence criterion, but opted for a much longer period of eight years. Due to the political discontinuities, a period of this length was ruled out in the present context.
5. One might argue that the mere fact of having been to Gymnasium (upper secondary school) would influence future intellectual development, but since at the time the upper secondary school curriculum surely did not show any particular openness towards the social sciences, there is no reason to pursue this hypothesis.
6. For instance, in 1931, Ludwig von Mises and Erich Voegelin chose sociology as their disciplinary affiliation whereas Othmar Spann, Adolf Günther and Hans Kelsen opted for other disciplines (*Kürschners Deutscher Gelehrten-Kalender* 1931).
7. It is not always possible, in an individual case, to be unambiguous about who is to be considered an Austrian or a German. The Wittebur sample, for instance, includes the names of eight sociologists (Franz Borkenau, Martin Buber, Emerich Francis,

- Friedrich O. Hertz, Hans Kelsen, Emil Lederer, Ernest Manheim and Karl Pribram) who are labelled dual nationality in the journal sample. Forty-six of the Germans included in the Wittebur sample are also to be found in the ISL sample below.
8. There are four groups: first, those who are seen by some as having made some literary contributions to the discipline but who would never have seen themselves as professional sociologists – among them Otto Bauer, Hermann Broch and Elias Canetti; second, those who were only for a short time engaged in the field and left sociology either of their own free will, or of necessity – for instance Gisèle Freund, Otto Leichter, Nina Rubinstein and Elisabeth Zerner; third, those who taught sociology at minor colleges but published next to nothing and were, therefore, not found by either the journal search or the other samples, their names rather turning up quite by accident; fourth, the group of non-affiliated scholars is, at any rate, not clearly delimited and, therefore, invites extension.
  9. My thanks are to Professor Fritz Ringer for letting me have a copy of this data set.
  10. This is not without a piquancy for the history of sociology, considering that Max Weber, who in his writings advised German bourgeois to give up this relict of feudalism, was not only a member of a duelling fraternity but tried to live up to the duelling code of honour even at a rather advanced age.
  11. Wehler (Wehler 2003: 500) reports the proportions of Jews, as defined by their religious affiliation, as 11 per cent for doctors and 16 per cent for lawyers, and emphasizes that these numbers, published in June 1933, may already have been reduced by the first wave of emigrations.
  12. Of course, the *Encyclopaedia Judaica* 1972 also reports this kind of data, but due to its age and to the small number of references to sociologists, it was not very helpful.
  13. The seven female emigrants whose names appear in the ISL are Hannah Arendt, Ida Berger, Rose Laub Coser, Marie Jahoda, Viola Klein, Anna Siemsen and Mathilda Vaerting. The eleven non-emigrant females, with the exception of Elisabeth Noelle-Neumann, are of comparatively little renown.
  14. Two other 'marginals' were Götz Briefs and his Institut für Betriebssoziologie (Institute for the Sociology of Industrial Relations), established in 1928 at the Technical University of Berlin, and Carl Dunkmann who, while only acting as a lecturer at the Pädagogische Hochschule (Teacher Training College) was head of a private institute after 1918 and excelled as an author of textbooks in the late 1920s (cf. König 1987: 258 ff; Stölting 1986).
  15. The title of one of Peter Gay's books, *Freud, Jews, and Other Germans: Masters and Victims in Modernist Culture* (Gay 1978), is misleading in this respect.
  16. The proportion of those with a 'non-educated lower middle-class' background in the small group of individuals with dual nationality is in line with Ringer's findings, suggesting a positive correlation between a certain degree of economic security and the readiness to migrate.
  17. See Schmeiser (Schmeiser 1994: 378, Appendix II, Table 2) who regrouped and recomputed the data of the Göttingen Survey. Ringer (Ringer 1993), based on the same data set, reports slightly higher values, probably because he uses the data collection dates of the Göttingen Study as a reference while Schmeiser's analyses are based on the *Habilitation* dates.
  18. It could, on the other hand, be argued that there is no problem at all: since the twentieth century saw a considerable increase in life expectancy, a recalculation of the phases of people's lives according to the average life expectancy around 1900 would very likely show that, at the time, the mid-life years of the average Central European were not much above the average age of young men reaching the status of assistant professor. However, since there are no data that show the life expectancies for

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- different occupational groups, this view cannot be further exploited. An approach that uses age data that are referred to the life expectancies of the respective social strata and occupational groups (rather than basing its analysis on units of calendar time) seems nevertheless worth considering.
19. Ferber categorizes the following disciplines as social sciences: sociology, social sciences and social policy, sociology and economics, industrial research and occupational safety, political science and journalism (Ferber 1956: 192).
  20. Only first doctoral degrees taken in Germany and Austria were included in this analysis; degrees taken by emigrants after emigration were not included.
  21. Interview with Machlup, as quoted in Craver 1986a: 24.
  22. For Neurath's activity in the Munich Soviet Republic, see Cartwright *et al.* 1996.
  23. Many contemporary observers mention in passing the youthful age of the brownshirts who dominated the German universities after 1933 (Hartshorne 1946: 11).
  24. Germanist Hans Schwerte who, under his real name Hans Ernst Schneider, had been one of the commanders in the SS organization 'Ahnenerbe' during the Nazi period, is perhaps the most illustrious example.
  25. Siegert 1971. The fate of Edgar Zilsel and his efforts to obtain his *Habilitation* may have kept others from trying.
  26. Albert Müller points out that according to Austrian *Habilitation* regulations, the first step to be taken was an examination of the candidate's personal aptitude, which implied that one could be rejected for any number of bizarre reasons (Müller 2000).
  27. One of the rare exceptions is the autobiography of Austrian historian Friedrich Engel-Jánosi (Engel-Jánosi 1974). Letters provide another rich source of information, of course.
  28. Letter to Walter C. Langsam, Department of History, Columbia University, 10 May 1938, Emergency Committee in Aid of Displaced Foreign Scholars, Engel-Jánosi File, New York Public Library, Rare Book and Manuscript Division, New York.
  29. These are Adorno, Arendt, Freud, Hayek, Heider, Hintze, Lazarsfeld, Lorenz, Lewin, Mannheim, Polanyi, Schumpeter, Schütz, Simmel and Weber.
  30. Due to the availability of an electronic version of the *International Encyclopedia of the Social & and Behavioural Sciences*, searching was easier here than in the *International Encyclopedia of the Social Sciences*, where the index for the seventeen main volumes was used (excluding the two supplements); references in a biographical context or in articles dealing with specific schools or currents were ignored, as well as references to titles of works. This approach somewhat discriminates against schools and against individuals who were part of a close-knit personal network or had published outstanding monographs.
  31. This index consists of combining the nominal information provided in the two biographical reference works with the logarithmized values for the number of index notations in the two encyclopedias, the range of which was between 0 and 88 for the *International Encyclopedia of the Social Sciences* and between 0 and 256 for the *International Encyclopedia of the Social & and Behavioural Sciences*.
  32. For details, see <http://www.jstor.org/> [accessed 16 October 2010].
  33. Between four and twelve journals are dedicated to each of the following disciplines: African American studies, anthropology, Asian studies, ecology, economics, education, finance, history, literature, mathematics, philosophy, political science, population studies, sociology and statistics. The biggest systematic gap is the lack of psychological journals.
  34. <http://fsearch-sandbox.jstor.org/about/selected.html> [accessed 26 April, 2007].
  35. Typos and spelling errors in names are not decipherable, just as in the SCI and SSCI, but this can to a certain degree be compensated for by using alternative inputs,

- which was done when there was a known change of name or when specific typos could be expected; the JSTOR search routine has an advantage over the SCI/SSCI routines of automatic data caption.
36. Only after completing my data collection did I become aware that there are two scholars by the name of Karl Pribram and, what is more, both of them are Vienna-born social scientists: psychologist Karl Harry Pribram and economist Karl Emanuel Pribram. Only the latter is included in our sample.
  37. A comparative analysis of the Periodical Content Index (PCI), which covers not only English-language journals, but also many German-language journals, results in more names of non-emigrants, or of emigrants who did not go to the United States. In this context, Franz Ronneberger, Walter Ullmann, Richard Thurnwald and Kurt Blaukopf are nearer to the top forty. The PCI search, however, does not allow for distinctions to be made between different kinds of texts. As a result, busy reviewers such as Eduard Reut-Nicolussi, Otto Weinberger, Emanuel Hugo Vogel and Wladimir Eliasberg can boast rather advanced rankings.
  38. Eight demographic and eleven statistic journals were included in JSTOR, but only nine sociological, ten philosophical and thirteen economic journals. Sociological journals that were not included at the time of the research are, for example, *Social Forces*, *Sociology and Social Research*, *Social Research* and a number of more recent journals such as *Social Problems*.
  39. The means of the three indices are 1.1 for the citation index, 0.76 for the recognition index and only 0.34 for the publications index. Due to the weighting, the mean of the publications index is now 0.69. The standard deviation is 1.7 for the citation and the weighted publication indices and 1.4 for the recognition index.
  40. The most famous user of this procedure is probably Bourdieu (cf. Bourdieu 1982; Bourdieu 1988). Current controversies among the proponents of correspondence analysis cannot be dealt with in the present context.
  41. Technically speaking, variables with an inertia of  $< 0.15$  were not taken into account. Although the data points are not shown in the diagrams, the variables were included in the computation of that space.

## Chapter 5 The Radio, Adorno and the Panel

1. Cf. Dahms 1994: 150–53. With Benjamin as a co-author, Adorno wrote a report on the discussions between the logical positivists and the representatives of critical theory, reproduced in Adorno 2003a: 560ff.).
2. Handwritten copies of both telegrams in Lazarsfeld Microfilm 1, f. Princeton 1, AGSÖ; same source for the letters quoted in the following.
3. Buxton gives a short outline of Marshall's life and career (Buxton 2003).
4. Cantril to Marshall, 27 August 1937, and DeWitt C. Poole, Director School of Public and International Affairs, Princeton, to Marshall, both in RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
5. On some of PFL's letters bearing the letterhead of Office of Radio Research, Princeton University, there is a stamp at the bottom reading 'Please address your replies to Dr. Paul F. Lazarsfeld, Research Center at the University of Newark, Newark, New Jersey', RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
6. Cantril to Marshall, September 29, 1937, RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
7. At the beginning of December 1937, PFL met with John Dollard to discuss the 'application of his life-history work to our project', PFL to John Marshall, 7 December 1937, RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.

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8. PFL acknowledges it in his memoirs (Lazarsfeld 1969: 310), and others frankly said as much; see Bernard Berelson, as quoted in Morrison (Morrison 1998: 66).
9. The first letter to John Marshall, 21 October 1937, unambiguously states: 'The author [of the Vienna study on Voice and Personality], by the way, is now in this country, partly as assistant to Professor Lynd, and partly as Mrs. Lazarsfeld.' RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
10. I could not find any previous reference to this term. Should this turn out to be another example of multiple discovery, one can at least say that PFL himself forgot his discovery, since he never again mentioned it.
11. In the memoirs, PFL notes that he had not been concerned with psychoanalysis before, which seems rather unlikely (see Fleck 1990: 226, n. 8), and that further participants were Karen Horney, Harry Stack Sullivan, John Dollard and Erich Fromm (Lazarsfeld 1969: 319–20).
12. PFL to Louis Wirth, 15 November 1937, and PFL to Fritz Redl, 'List of those to be invited to meeting Sunday, 28 November', 18 November 1937, both RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
13. The \$20 remuneration was offered to the Chicago social scientists and to the psychoanalysts; the latter, however, were unwilling to sacrifice more than one evening for this amount (Lazarsfeld 1969: 320).
14. In November 1937, he had informed Marshall that he was preparing a 'sixty-page memorandum on the motivation of radio listening', to be completed by the end of the year, which was to serve as the 'theoretical framework for the Princeton Study'.
15. PFL Memo to Cantril and Stanton, 1 January 1938, Lazarsfeld Microfilm 1, f. Princeton Program, AGSÖ; a version in a somewhat different style, sent to Horkheimer in January 1938, is kept in the Max-Horkheimer-Archiv, Korrespondenz mit Lazarsfeld, Archivzentrum der Universität Frankfurt: 153–66; subsequent quotations from the latter.
16. In the final version of the memorandum, the first sentence reads as follows: 'But if our experiment lead to some commonly accepted standards, it may be possible to develop a panel method by which non-commercial broadcasters can maintain contact with a representative group of listeners. They can then get some ideas of the extent of and reasons for the acceptance of programs in spite of the difficulty which the average person has in expressing himself with any degree of reliability in this field' (Horkheimer Archiv, Lazarsfeld Korrespondenz: 156).
17. Marshall talk with PFL, 17 November 1937, RF, RG 1.1, s. 200, b. 271, f. 3234, RAC.
18. In a letter to PFL, John Marshall argues that, as far as he can remember, Lasswell had used this formula in public for the first time in the Communication Seminar he had organized in 1939/40, letter to PFL, 12 January 1969, RF, RG 2, s. 200 GC, f. Columbia University, RAC. For the Communication Seminar, see Morrison 1978b.
19. PFL to Stanton, 10 November 10 1937, Lazarsfeld Microfilm 1, f. Princeton V: Stanton (1936 and 1937), AGSÖ.
20. Max Horkheimer to Theodor W. Adorno, 22 October 1936 (Horkheimer 1995a: 686). In print, the dissociation is rather toned down: 'Differences in theoretical outlook come second here (in studies by specialists pertaining to issues of social research) to the elucidation of certain facts' (Horkheimer 1937).
21. Adorno 2003a: 440; cf. Wiggershaus 1986: 266. In 2010, this would be a monthly salary of \$6,000.
22. PFL Memo to Cantril and Stanton, 15 December 1937, Lazarsfeld Microfilm 1, f. Princeton, AGSÖ.
23. Alice Maier, Horkheimer's secretary of many years, remembers that he 'used to reflect very thoroughly on each word, sometimes not dictating a single word within two hours' (quoted in Wiggershaus 1986: 297).

24. In both Wiggershaus 1968 and Dahms 1994, his offer to Adorno is only seen from this point of view.
25. PFL to Adorno, 29 November 1937, Horkheimer Archiv, Lazarsfeld Korrespondenz, Blatt 181f. Same source for the following quotation.
26. Adorno to PFL, 24 January 1938, copy in: Horkheimer Archiv, Korrespondenz Adorno, f. 351. Same source for the quotations that follow.
27. Thus, the President's Review of the RF for 1939 says: 'The publication of the Princeton reports ... will test the expectation which led the Foundation to contribute toward this project, i.e., that knowledge of what radio is doing for its audience should be basic in any effort to increase still further public service' (Rockefeller Foundation 1939: 66).
28. In the course of his participation in the PRRP, Adorno sought to charge other collaborators with the empirical verification of his ideas; however, in his autobiographical text on his experiences in the United States, he reports that he had failed to win them over: 'But rather than helping me to translate my problem in research tools, of however limited a nature, he [Gerhard Wiebe, who had been assigned to him as an American musician and expert in social research] wrote a kind of protest memorandum in which he, not without a certain pathos, opposed his scientific world view to what he felt to be wild speculation on my part' (Adorno 1981: 310).
29. See Adorno 1986b: 793ff.
30. PFL to Adorno, 2 February 1938, Horkheimer Archiv, Korrespondenz Lazarsfeld: 168–9. Same source for the quotations that follow.
31. Memo PFL to Cantril and Stanton, 15 December 1937, Lazarsfeld Microfilm 1, f. Princeton, AGSÖ.
32. Memo PFL to Cantril and Stanton, 16 May 1938, Lazarsfeld Microfilm 1, f. Princeton Cantril and Stanton (1938), AGSÖ.
33. Contrary to what Wiggershaus suggests (Wiggershaus 1986: 272f.) and Dahms affirms (Dahms 1994: 240ff.), this first conflict did not put an end to their collaboration.
34. Memo from PFL to Cantril and Stanton, 7 November 1938, Lazarsfeld Microfilm 1, f. Princeton Cantril and Stanton (1938), AGSÖ.
35. PFL to Marshall, 7 November 1938, RF, RG 1.1, s. 200, b. 271, f. 3236, RAC.
36. Memo PFL to Cantril and Stanton, 8 September 1939, which included a draft of a dismissal notice for Rorty (in quite friendly wording), which PFL wanted both co-directors to read before dispatching, Lazarsfeld Microfilm 1, f. Princeton Cantril and Stanton (1939), AGSÖ.
37. Cantril to PFL, 28 March 1938, Lazarsfeld Microfilm 1, f. Princeton VI, AGSÖ.
38. Undated letter, Lazarsfeld Microfilm 1, f. Princeton I: Cantril and Stanton (1938), AGSÖ, and Horkheimer Archive, Adorno Korrespondenz: 288–95. A copy of Adorno's 161-page typescript entitled 'Memorandum. Music in Radio', of 23 June 1938, is to be found in the Paul F. Lazarsfeld Archive in Vienna, including PFL's marginal notes, quoted in detail by Dahms (Dahms 1994: 241–5).
39. Horkheimer to PFL, 12 July 1939, PFL to Horkheimer, 18 July 1939, Horkheimer Archive, Lazarsfeld Korrespondenz: 145–6.
40. Memo PFL to Cantril and Stanton, 5 February 1938, Lazarsfeld Microfilm 1, f. Princeton I: Cantril and Stanton (1938), AGSÖ.
41. Memo PFL to Cantril and Stanton, 9 June 1938, Lazarsfeld Microfilm 1, f. Princeton I: Cantril and Stanton (1938), AGSÖ.
42. PFL to Cantril, 8 August 1937, Lazarsfeld Microfilm 1, f. Princeton, AGSÖ.
43. When the book was published in 1940 by Princeton University Press, the authorship was indeed given as 'Hadley Cantril with the assistance of Herta Herzog and Hazel Gaudet'. Except for a footnote, PFL does not refer to the dissent on authorship in his memoir (Lazarsfeld 1969: 313, n. 54).

## Notes

44. Cantril's letter of 26 January 1939 is extensively quoted without a further source in Morrison (Morrison 1976: 217–18) and, more succinctly, in Rogers (Rogers 1994: 279), who gives Columbia University's Butler Library as the source. Morrison also mentions that the only question PFL refused to answer in his interviews with him was the one pertaining to the reason for his split-up with Cantril. The FBI file on PFL includes several statements: Hadley Cantril and other colleagues were interviewed on PFL by the FBI and were very outspoken on his behaviour towards women. Although as a rule the FBI obliterated the names of its informants or respondents, there is at least one document (dated 18 June 1951) where Cantril is explicitly cited: 'Dr. Hadley Cantril stated that in 1938 [redacted] and he had a dinner engagement with the applicant and [redacted] and were to meet at the applicant's apartment a little before Mr. Cantril. Applicant put his arm around [redacted] and "his hands started to roam."' And so on. What this has to do with PFL's loyalty to the United States, explicitly confirmed by Cantril, is anybody's guess. Cantril's characterization of PFL as a 'Viennese Romeo', however, which is also recorded, is interesting at least for the biographically minded. I thank Professor Mike Keen of Indiana University, South Bend, for letting me have a copy of PFL's FBI file.
45. 'Final Report' in RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
46. Marshall to PFL, telegram, 16 March 1939, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC, as quoted by Morrison 1998: 79.
47. The choice of a commercial publisher, which promised to reach a larger public, is likely to have also given some satisfaction to PFL, since Cantril's *Invasion From Mars* had been published by his own university press (Lazarsfeld 1969: 329, n. 79).
48. PFL felt obliged to include a footnote on the problem of reanalysis in his introduction: 'It should be stressed that the re-analysis of a study made by another agency is not at all equivalent to quoting a published result. Such an analysis may be as laborious as the original, and may be more difficult because the material has been collected for other purposes. Refined means of analysis, which would have been unnecessary if the field work had been done for the immediate problem in question, must sometimes be used. The difference is merely financial, inasmuch as the field work does not have to be paid for again. Wherever such a re-analysis is reported here, therefore, the reader will have to dissociate the responsibility for the field work, which lies with the co-operating agency, from the responsibility for the analysis and interpretation, which rests with the Office of Radio Research' (Lazarsfeld 1940: xvii, n. 3).
49. In a way, this brings to mind Pierre Bourdieu's theory of differential endowment with capital, where the underlying idea similarly postulates relations of exchange among different types of capital (Bourdieu 1982).
50. Only the sub-studies that had been conducted in collaboration with a book club included a four-step classification based on an index that aggregated several variables.
51. Given the format of 'Who Wants to be a Millionaire?', which is part of television programming in many countries today, the topicality of this study seem unbroken.
52. Adorno told his parents that on 1 November 1939, he was scheduled to report 'on my Music study, in the project, before a number of representatives of the Rockefeller Foundation' and asks them to 'keep fingers crossed' for him (Adorno 2003b: 41).
53. Besides Robert Havighurst and the director of the Humanities Branch of the RF, David H. Stevens, PFL had considered inviting Lynd, Lasswell and the English Anthropologist Geoffrey Gorer (who had been charged by Marshall to read, and comment on, the radio book) as potential guests, RF, RG 1.1, s. 200, b. 223, f. 2672, RAC. On the Communication Research Seminar cf. Morrison 1978b.
54. There seem to be different versions since in Lazarsfeld (Lazarsfeld 1940: 181–2, n. 31) there is a quotation from a manuscript by Adorno similarly entitled 'Elements of a

- Social Critique of Radio Music', which is not included in the version that I used, found in the RF archives. A later publication is Adorno 1945.
55. This study is mentioned by Lazarsfeld (Lazarsfeld 1940: 113).
  56. Adorno to Benjamin, 13 September 1937: 'This also ... spares you certain things such as ... reading articles by Neurath and Lazarsfeld' (Adorno 1994: 273).
  57. This perspective was not alien to the one adopted by PFL; in a 1948 text that has since become canonical (Katz 2003: 12–38), PFL and Merton proposed a phrasing that was rather close to Adorno's axioms, namely that, 'mass media undoubtedly serve many social functions which might become the object of sustained research'.
  58. PFL to RF, 22 December 1939, RF, RG 1.1, s. 200, b. 272, f. 3242, RAC.
  59. PFL to Marshall, 27 December 1939, RF, RG 1.1, s. 200, b. 272, f. 3242, RAC.
  60. Emphasis added.
  61. 'Plugging' was the term used for the selection of the music to be broadcasted and for the influence the record companies tried to bring to bear on this selection. See Adorno's version (Adorno 1941b: 27).
  62. Gorer memorandum to Marshall, 2 January 1940; Siepmann to Marshall, 12 December 1940, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
  63. Marshall memorandum, 5 January 1940, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
  64. PFL to Marshall, 13 March 1940, RF, RG 1.1, s. 200, b. 222, f. 2661, RAC.
  65. PFL to his parents, 3 March 1940 (Adorno 2003b: 69). The musical education programme seems actually not to have materialized as soon as that, but in the spring of 1940 Adorno apparently gave 'introductory lectures to the modern concerts of this station' (Adorno 2003b: 81). The manuscripts underlying these lectures can be assumed to be those published in the collection of his complete works, Adorno 1984.
  66. Marshall, 'Memo Dr. Theodore Adorno', 19 June 1941, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC; handwritten approval of this procedure by Marshall's boss Stevens, cf. Morrison 1976: 308.
  67. Eisler benefited from long-term support by Marshall's Division, Marshall, 'Diary', RAC.
  68. Adorno's three biographers all belong to the latter type, since they published voluminous studies for his centennial, though these were exclusively based on material provided by people more or less close to Adorno: Claussen 2003; Jäger 2003; and Müller-Doohm 2003. For a 'Theorie der Halbbildung' (Theory of Semi-Education), see Adorno's 1959 essay under this title (Adorno 1972b: 96–7).
  69. PFL to Bernard Bailyn, 7 February 1968, Lazarsfeld Papers, b. 1A, Columbia University.
  70. Cf. Geertz 1988. In doing so, Adorno repeatedly loses his geographical bearings, for instance when he alternately refers to the United States with 'here' and 'over there'.
  71. Both were PRRP collaborators, and both were secretaries only for a certain time. Rose Kohn Goldsen later became a professor at Cornell University, and Eunice Cooper, who assisted Adorno with more than just secretarial work (see Table 5.1), later collaborated on the 'Studies in Prejudice' (see Chapter Six).
  72. 'The Frankfurt school has played a considerable role in recent student protests. Perhaps it is worthwhile to listen to the way the debate sounded in a more detached context' (Lazarsfeld 1972: x).
  73. Adorno had used this term previously in *The Authoritarian Personality*, in the slightly modified form of 'administrative ideology', for characterizing Nazi scientists such as Erich Jaensch (Adorno *et al.* 1950: 748, n. 1), and, later, with a positive connotation, in an article on the state of German sociology in 1959 ('Zum gegenwärtigen Stand der deutschen Soziologie', in Adorno 1972c: 506) and, ironically, in Adorno 1976: 182.
  74. See Adorno's letters to his parents of 22 January 1942 and 12 February 1942 (Adorno 2003b: 126–9).



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75. PFL to Marshall, 9 June 1941 and 17 June 1941, containing the enclosures, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC. In a letter to Horkheimer, Adorno tells him of this invitation, calls the editor an 'East European Jew and busybody who has the advantage of not pretending to any expert knowledge whatsoever', and reports that he would be made an associate director, which 'would be rather favorable for a number of things, especially in California'. The greater work he shared with Horkheimer, however, would not suffer from this.
76. 'Marshall Interview with Dr. Theodore [!] Adorno', 19 June 1941. PFL had suggested granting Adorno \$3,000, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC. Later, Marshall could not recall ever having objected to Adorno, but gave 1938 as the date of the refusal. John Marshall to PFL, 12 January 1969, RF, RG 2, s. 200 GC, f. Columbia University, RAC. \$1,000 is about \$15,200 in 2010.
77. PFL to Marshall, 29 February 1940, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
78. The proposal was also submitted to the RF, see PFL to Marshall, 18 June 1940, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
79. Marshall interview with PFL, 26 June 1944, RF, RG 1.1, s. 200, b. 222, f. 2666, RAC.
80. In his final report on the Princeton phase and the first one-and-a-half years of the Columbia phase of the Radio Research Project, Marshall erroneously gives June 1940 as the date of the appointment, RF, RG 1.1, s. 200, b. 272, f. 3243, RAC.
81. The first account was given by journalist Morton Hunt, who published it in the same year as part of a portrait of Merton (Hunt 1961). Morrison (Morrison 1976: 229–34) could also rely on interviews with PFL (identically in Morrison 1998: 87–8; see also Merton 1998). MacIver briefly refers to the affair in his autobiography (MacIver 1968: 141); on his role in the department, see Halas 2001.
82. Lazarsfeld thought that a letter of recommendation from Stouffer had been of great importance (Lazarsfeld 1969: 301). It seems doubtful, however, that the latter's word would have been able to convince the sceptics, since it was known that Stouffer and PFL were friends. MacIver apparently had temporarily entertained the idea of giving the position to a member of the Frankfurt School (Wiggershaus 1986: 312).
83. In the interview that Morrison conducted with PFL, which he quotes at length in his doctoral thesis, PFL claims to have received only \$4,500 (Morrison 1976: 229).
84. 'If men define situations as real, they are real in their consequences' (Thomas and Thomas 1928: 572). For the context in which this theorem was formulated, and for elucidating why this theorem is attributed only to W.I. Thomas and not also to Dorothy Thomas, see Merton 1995a.
85. See the classical article by Merton (Merton 1936).
86. Virtually all the manuscripts of his lectures that have been preserved begin with an anecdote aimed at disarming the public, expounding on his accent and his role as an outsider. In most cases, PFL would say that he had not arrived here with the *Mayflower*, as everybody could hear.
87. In the spring of 1941, the two of them had many talks to this effect, see notes in RF, RG 1.1, s. 200, f. 2662 and 266, RAC.
88. Marshall, interview with Robert Lynd, 21 April 1941, RF, RG 1.1, s. 200, b. 222, f. 2663, RAC.
89. RF, RG 1.1, s. 200, b. 272, f. 3243, RAC. Same source for the quotations that follow.
90. The BASR bibliography (Barton 1977) does not specify the titles that were published with PFL as an editor but had not been written within the PRRP, ORR or BASR.
91. File 1. 1. 230 Lazarsfeld, University Archives, Columbiana Library, Columbia University, Low Memorial Library. \$25,000 is about \$370,000 in 2010.
92. PFL to Cantril and Stanton, 5 December 1938, Lazarsfeld Microfilm, f. Princeton II: Stanton, AGSÓ; Lazarsfeld 1969: 328. Similarly, the correspondence accompanying

- the protracted genesis of the study conducted by Komarovsky contains indications that PFL's role was not only that of a project supervisor: PFL to Pollock, 26 January 1938, Horkheimer Archiv, Lazarsfeld Korrespondenz: 172.
93. Personal communication by Robert K. Merton, New York.
  94. RF, RG 1.1, s. 200, b. 222, f. 2666, RAC.

## Chapter 6 The History of an Appropriation

1. Lazarsfeld could not know that, at the same time, Adorno suggested to Horkheimer 'to let Lazarsfeld have the Journal, for trash, fillers, and success' as long as they could be sure that the contributions of the members of the Institute were published. Adorno to Horkheimer, 30 July 1941 (Horkheimer 1996a: 112).
2. For a detailed description of Horkheimer's film projects, see Koch 1992.
3. Horkheimer to Franz Bischofswerder, 25 October 1939 (Horkheimer 1995b: 652); Horkheimer to Franz Neumann, 13 August 1941 (Horkheimer 1996a: 125ff). For the Warburg dynasty, cf. Chernow 1994.
4. Horkheimer's notorious ambivalence is expressed in a letter to Löwenthal: 'If the Committee would give us \$50,000, it could expect us to expand our activities and an extended stay of mine in New York. But for \$10,000 ... it can expect nothing but a careful work [in contrast to Neumann's outline] and a religious adherence to the budget.' Horkheimer to Leo Löwenthal, 31 October 1942 (Horkheimer 1996a: 370).
5. 'Memorandum on a Research Project on Anti-Semitism prepared for the American Jewish Committee by the Institute of Social Research', RG 347.17.12, AJC Records, Gen – 12, b. 58: Institute of Social Research, YIVO. Same source for following quotation, cf. Horkheimer 1941.
6. David Rosenblum to Frederick Pollock, 3 March 1943, AJC Records, b. 58: Institute of Social Research, YIVO.
7. Memorandum A.G. Duker to Mr Trager, 18 November 1942, AJC Records, b. 17: Studies in Prejudice, YIVO.
8. David Rosenblum to Frederick Pollock, 3 March 1943, AJC Records, b. 58: Institute of Social Research, YIVO.
9. Horkheimer to Neumann, 13 August 1941 (Horkheimer 1996a: 125–7).
10. At any rate, Adorno more than once reported with enthusiasm on the 'Jewish project', or 'my project', to his parents, as well as on the fact that 'we take great care to be on good terms with our Jewish bigwigs' (Adorno 2003b: 186–9).
11. Horkheimer to Herbert Marcuse, 14 July 1943 (Horkheimer 1996a: 463). In this letter, Horkheimer also mentions that he had been reading up on the 'silly psychological literature'; he did not believe in psychology – this term, for him, was an equivalent 'for anthropology and anthropology for the theory of man'.
12. Frederick Pollock to Richard C. Rothschild, 9 August 1943, AJC Records, b. 58: Institute of Social Research, YIVO. Rothschild had succeeded to David Rosenblum, deceased in the summer of 1943 as Director of Research of the AJC.
13. Horkheimer to Morris D. Waldman, 30 December 1943, Supplement, AJC Records, b. 58: 2: Institute of Social Research, YIVO. Reprinted in Horkheimer 1996a: 520–22. Same source for the quotations that follow.
14. In a letter to his parents, Adorno reports that while in the 'some 150 pages of the typescript on anti-Semitism, very little is said ... on anti-Semitism in the stricter sense, I have as systematically as possible described all the typical tricks and feints of fascist propaganda, and categorized them by keywords, so they can be ... easily identified and be done with' (Adorno 2003b: 225).

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15. The results of the survey as reported in Cantril (Cantril 1978: 384–5) suggest, at any rate, that in the United States, anti-Semitism was less strong in the Western regions than at the East Coast, but more common in the bigger cities than in rural communities (cf. Dinnerstein 1994).
16. Horkheimer to Pollock, 19 November 1943 (Horkheimer 1996a: 499).
17. As late as 1966, Adorno still wanted 'to study those who were guilty of Auschwitz, using all the methods that science can provide, specifically subjecting them to many years of psychoanalysis ... in order to bring to light, if possible, how a human being comes to be like this ... it may then be possible to come to a number of practical conclusions to make sure that this will never happen again', 'Erziehung nach Auschwitz' (1966), in Adorno (1971).
18. Pollock to AJC, 14 June 1944, AJC Records, b. 58: Institute of Social Research, YIVO; clues to the deficiencies of the working papers are in Horkheimer 1996a.
19. See the thirty-nine-page typescript by Else Frenkel-Brunswik, in: AJC Records, b. 18: Studies in Prejudice, YIVO. This was, most likely, 'Some personality factors in anti-Semitism' (later published, under the same title, in *Journal of Psychology*, 1945, 20: 271–91); see Else Frenkel-Brunswik Papers, Signatur 25/4, AGSÖ.
20. On 30 November 1943, the new Executive Director of the AJC, John Slawson, thanked Pollock for his letter explaining the issues at stake in the Anti-Semitism Project, AJC records, b. 58: Institute of Social Research, YIVO.
21. Sonnert and Holton report that the second generation of Jewish immigrants from Central Europe excelled its counterparts from Eastern Europe. They explain these differences in occupational and related successes with regard to differences in their furnishings with cultural capital (Sonnert and Holton 2006).
22. Myrdal's study that had been commissioned by the Carnegie Corporation and was first published in 1944 under the title *An American Dilemma*, immediately meeting with a lively response (Myrdal 1962; cf. Turner and Turner 1990; Turner 2001).
23. AJC records, b. 17: Scapegoats and Stereotypes, YIVO.
24. Horkheimer to Slawson, 10 June 1944, AJC records, b. 58: Institute of Social Research, YIVO. This sum is the equivalent of \$1.2 million in 2010.
25. AJC records, General 12, alphabetical files 1933–162, Horkheimer, Dr. Max, YIVO.
26. Horkheimer to Adorno, 13 November 1944 (Horkheimer 1996a: 604).
27. Horkheimer, as recorded in the 'Minutes of the Meeting of the Committee on Scientific Research, 24 January 1945, AJC records, b. 23: Advisory Committee, YIVO.
28. Horkheimer to Adorno, 13 November 1944 (Horkheimer 1996a: 604).
29. 'Report of the Department of Scientific Research and Program Evaluation' (probably written in the summer of 1947), AJC, AJC records, b. 24: Reports 45–8, YIVO.
30. Memorandum, Max Horkheimer to John Slawson, 22 May 1947, AJC Gen – 12, b. 57: Horkheimer, Max, YIVO.
31. This is also quite manifest in the list of the institute's collaborators as presented in its publication *Ten Years on Morningside Heights: A Report on the Institute's History 1934 to 1944*: the only remaining members now were Adorno, Horkheimer, Löwenthal, Pollock, Felix Weil and Karl A. Wittfogel, as well as Research Associates Else Frenkel-Brunswik, Arkadij Gurland, K. Wilhelm Kapp, Daniel Levinson, Paul Massing and Josef Soudek, and Consultants Herta Herzog and R. Nevitt Sanford. Kirchheimer, Marcuse and Neumann were 'in Government Service'. Collaborators of the first Anti-Semitism Project, such as Langerhans, are not included in the list (Institute of Social Research 1945).
32. In a letter to Flowerman, Horkheimer instructs him that, 'in order to avoid confusion, you address any correspondence concerning our West Coast projects either to him [i.e. Adorno] or myself', 17 November 1945, AJC records, b. 23: Horkheimer, Dr. Max AD 45–48, YIVO.

33. Horkheimer to Löwenthal, 6 February 1946 (Horkheimer 1996a: 695).
34. Memorandum, Flowerman to Horkheimer: 'Relationship between Dr. Horkheimer and Dept. of Scientific Research', 20 January 1948, AJC records, b. 23: Horkheimer, Dr. Max, YIVO.
35. He much later claimed that his wife had been unable, for health reasons, to tolerate the New York climate and that it was for her sake that they had moved to California, Horkheimer, 'Additional Remarks on my Stay in Germany in the years 1950 to 1962', AJC records, Gen – 12, b. 57: Horkheimer, Max, YIVO.
36. 'Meeting of the Committee on Scientific Research', 4 February 1946, AJC records, b. 23: Advisory Committee, YIVO.
37. In a letter to Löwenthal, 24 July 1944, Horkheimer expressed the hope that the institute would later profit from what Löwenthal would learn in the BASR (Horkheimer 1996a: 565).
38. Personal communication by Daniel Bell, Cambridge, MA, September 1993.
39. In his letters, Horkheimer speaks disparagingly of Lewin. This also shows that he saw Lewin as a rival. Horkheimer to Adorno, 24 November 1944 (Horkheimer 1996a: 607).
40. For detailed information on the film project, see Koch 1992.
41. As late as in 1949, Lazarsfeld argued that this project had only been postponed (Lazarsfeld and Stanton 1949: xviii).
42. Minutes of the Meeting of the Committee on Scientific Research, 22 February 1945, AJC records, b. 23: Advisory Committee, YIVO.
43. In October 1942, Horkheimer once again estimated the costs at 'no less than \$100,000', 'Minutes of the Meeting of the Committee on Scientific Research', 24 October 1945, AJC records, b. 23: Advisory Committee, YIVO.
44. 'Minutes of the Meeting of the Committee on Scientific Research', 22 February 1945, AJC records, b. 23: Advisory Committee, YIVO.
45. 'Minutes AJC Meeting of the Scientific Advisory Council', 3 November 1945, AJC records, b. 23: Committee on Scientific Research, YIVO.
46. Jahoda to Lazarsfeld, 12 December 1945; same wording in the letters to all the other six members of the original Advisory Committee, AJC records, b. 23: Committee on Scientific Research, YIVO.
47. Jahoda to Horkheimer, 21 November 1945 (Horkheimer 1996a: 668).
48. Horkheimer to Jahoda, 28 November 1945 (Horkheimer 1996a: 675–6).
49. Löwenthal to Horkheimer, 3 December 1945 (Horkheimer 1996a: 682).
50. See letters to and by Jahoda in Max Horkheimer Archiv, Korrespondenz Jahoda: 166–79. In his autobiographical text that was published posthumously, Shils is even more severe, accusing the authors of TAP of having been 'favorably disposed to totalitarian ideology' (Shils 2006: 91).
51. AJC records, Gen 10, b. 20: f. Finances, YIVO.
52. Else Frenkel-Brunswik Papers, AGSÖ Signature 25/5.
53. For a contemporary overview, see Allport 1935.
54. In an interview in 1992, Levinson is unequivocal about this: 'See, what Nevitt and I did was more standard academic psychology' (Levinson 1992).
55. Horkheimer to Marcuse, 17 July 1943 (Horkheimer 1996a: 463–4).
56. 'He [Horkheimer] said, if you wanna do a project we'll pay for it, if Adorno was in it' (Levinson 1992).
57. Horkheimer's idea of the methodological utility of these indirect instruments, however, was rather strange: 'If any indirect questionnaire is used with any social group, we must assure ourselves that the indirect questionnaire indeed measures anti-Semitism in this group. Therefore, there must always be a sufficient

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- number of intensive or at least abbreviated interviews with members of the group. This will enable us to compare the correlations between these interviews and interviews in other social groups with the correlations between the results obtained by the indirect questionnaire in the first group with the results obtained by the indirect questionnaire with any second group. If we do not constantly control our findings through the indirect questionnaire by this or other methods, the results will certainly not be recognized as scientific.’ Horkheimer to Adorno, 11 October 1945 (Horkheimer 1996a: 661–2).
58. Memorandum from Adorno ‘re: case interviews and typology’, AJC records, b. 18: Berkeley study: interviews 44–5, YIVO.
  59. To his parents, Adorno explained his work as follows: ‘Typology literally means the theory of types, and it is understood to be the division of a field or a group of individuals according to prevalent types. Mine, thus, is a listing of the basic psychological types of anti-Semites’ (Adorno 2003b: 334).
  60. Horkheimer to Adorno, 11 October 1945, AJC records, b. 18: Berkeley study: interviews 44–5, YIVO, reprinted in Horkheimer 1996a: 656–8.
  61. In a long-winded footnote, Adorno then criticized Jaensch from yet another point of view, arguing that the latter had defined the anti-type by its tendency to synesthetics. Apprehending sounds when looking at pictures and vice versa was a characteristic sign of decadence, in particular in Baudelaire. Wasn’t it typical for the Nazis to choose someone as their arch-enemy whose entire attitude was characterized by ‘rebellion against stereotypy’? Reversing an *ad hominem* argument into a *pro hominem* argument, however, is not yet an objective justification.
  62. ‘Memorandum to Berkeley group from Adorno re: book plan’, 2 October 1946, AJC records, b. 18: Studies in Prejudice, YIVO, emphasis in the original.
  63. Interview with Marie Jahoda, 11 November 1992, interviewers: Hans-Joachim Dahms and Christian Fleck, AGSÖ; parts of this interview were published in Dahms 1996.
  64. Sanford to Slawson, 31 January 1946 and 8 February 1946. The second letter, stressing that Slawson should by no means take his first letter to say that there was any opposition between the Berkeley group and the institute, was presumably written due to an intervention by Horkheimer, AJC records, b. 58: Institute of Social Research, YIVO.
  65. Horkheimer to Löwenthal, 6 February 1946 (Horkheimer 1996a: 694).
  66. Horkheimer to Else Frenkel-Brunswik, 4 April 1946, AJC records, b. 18: Studies in Prejudice, YIVO.
  67. Flowerman to Horkheimer, 14 December 1949, AJC records, b. 20: Studies in Prejudice, YIVO. The reviews referred to in the letter are: a review by Carey McWilliams of Löwenthal and Guterman’s *Prophets of Deceit*, and a review by Thomas Mann, at Horkheimers’ request, of Massing’s *Rehearsal of Destruction*, both published in the *New York Times Literary Supplement*. In the same year, McWilliams published *A Mask for Privilege: Anti-Semitism in America*, and a note in the *American Sociological Review* announced the reopening of the Institute of Social Research in Frankfurt, with reference to the work done in exile.
  68. Horkheimer to Adorno, 29 December 1949 (Horkheimer 1996b: 82).
  69. Horkheimer to Flowerman, 9 January 1950, AJC records, b. 20: Studies in Prejudice, YIVO. Cf. Horkheimer to Slawson, 2 February 1950 (Horkheimer 1996b: 95).
  70. The roots of this legend can be traced back to Max Horkheimer’s writings.
  71. Horkheimer to Marc Vosk, 7 April 1953, AJC records, Gen – 10, b. 20, f. German version, YIVO.

## Chapter 7 Reconnaissance Expeditions, Reconstruction Support and the Rare Return

1. Havighurst wrote two reports on this: 'Report, November 1947', and 'Recommendations for Program in Germany and Austria, November 1948', both in RF, RG 1.2, s. 700, b. 11, f. 95 and 96, RAC.
2. Philip E. Mosley (1905–72), Ph.D. Harvard, 1933, professor of history at Cornell University between 1936 and 1942, worked at the State Department until 1942; after that, professor of international relations and director of the Russian Institute at Columbia University; worked for the Rockefeller Foundation between 1947 and 1951; and, later, had an important role in the Council on Foreign Relations.
3. Norman S. Buchanan (1905–58), Ph.D. Cornell, 1931, Canada-born economist, was a professor at Cornell, Colgate University, University of California, and a guest professor at Columbia University before being appointed, in 1947, associate director of the Social Sciences Division of the Rockefeller Foundation. From 1950 onward, he was again a professor at the University of California.
4. Even earlier, John D. Rockefeller III had visited Europe, as had, in the beginning of 1947, another RF emissary. Both wrote reports on their visits, which however dealt with educational institutions rather than the social sciences.
5. Buchanan obviously referred to the consequences of the so-called Second Control Agreement enacted by the four powers, which significantly facilitated Austrian governmental action.
6. Buchanan, 'Diary', 11–16 July 1947, RAC.
7. Lowie 1954 contained some remarks on the particularities of Austrian national character.
8. Robert J. Havighurst, 'Report 1947', RF, RG 1.1, s. 700, b. 11, f. 96, RAC: 62. Same source for the quotations that follow.
9. Robert S. Morison, 'Memorandum re: Germany', 20 November 1947, RF, RG 1.2., s. 700, b. 10, f. 83, RAC. Same source for the quotations that follow.
10. For instance in Buchanan, 'Diary', 11–15 July 1947, where he described the assistant of the head of the USFA Education Department as having 'moderate capacity but probably hard-working'.
11. Lowie was similarly disconcerted by this obsession with titles (Lowie 1954: 77–9).
12. Warren Weaver (1894–1978), Ph.D. University of Wisconsin, 1922, professor of mathematics, was offered the post of a director of the RF Division of Natural Sciences in 1932, which he held until 1955 when he became Vice-President of the Rockefeller Foundation. During his time at the RF, he rendered great service to the advancement of molecular engineering. See his autobiography (Weaver 1970) and the article by Kohler (Kohler 1976).
13. Joseph H. Willits (1889–1979), Ph.D. University of Pennsylvania, 1916, from 1921 professor at the Department of Industrial Relations of this university, later Dean at Wharton School of Finance and Commerce; director of the Division of Social Sciences at the Rockefeller Foundation between 1939 and 1954 (cf. Stapleton 2003).
14. Willits, 'Memorandum RF and Germany', 24 November 1947, RF, RG 1.2., s. 700, b. 10, f. 85, RAC.
15. Weaver, 'RF, Germany, and WW', 1 December 1947, RF, RG 1.2., s. 700, b. 10, f. 85, RAC. Same source for the quotations that follow.
16. Lowie reported 'constantly encountered bitterly references' which he had already been confronted with during his field studies in Germany in 1950/51 (Lowie 1954: 348; cf. Gerhardt 2005).

## Notes

17. See Lederer 1995; Speier 2007; cf. Horkheimer *et al.* 1981.
18. Staley pointed out that, in the first years, only a marginal part was dedicated to support for research projects, and argued that until 1950, the RF acted as an extension of the occupation authorities, not (re-) engaging in the promotion of science until later (Staley 1995).
19. 'Rehabilitation Program', RF, RG 1.2, s. 700, b. 10, RAC.
20. Havighurst, 'Diary', 20 September 1948, RF, RG 1.2, s. 700, b. 11, f. 92, RAC.
21. Havighurst, 'Diary', 23 September 1948, RF, RG 1.2, s. 700, b. 11, f. 92, RAC.
22. Edward Francis D'Arms (1904–91), Ph.D. Princeton University, 1936, Rhodes Scholar in England, from 1932 onwards, chairs in ancient philology at Vassar College, NY, the University of Minnesota, the University of Colorado, head of the Education and Religious Policy Division of the US War Department in 1946, between 1947 and 1957 assistant director of the Division of Humanities at the Rockefeller Foundation, associate director as of 1950, from 1957 onwards, at the Ford Foundation, charged with the Humanities and Arts Programme.
23. D'Arms 'Diary', 17 November 1947, RAC.
24. Havighurst, 'Recommendations IX', 3, RF, RG 1.2, s. 700, b. 11, f. 95, RAC.
25. Willits, 'Diary', 1 March 1951, RAC.
26. It may have contributed to the failure of the book project that Hughes, as an illustration of his approach, provided the editor with a chapter entitled 'Innocent abroad, 1948 or How to behave in occupied Germany'. The editor misconstrued the text as a moral treatise on guilt and forgiveness. Everett C. Hughes Collection, b. 100, f. 6 and 7, Joseph Regenstein Library, University of Chicago, Department of Special Collections. The trial chapter was published in 1948 in a journal by the name of *Christian Century*. I owe this reference – as well as a number of others – to the masters thesis of Dirk Raith (Raith 1999).
27. Hughes, 'Germany Memorandum 1948', Hughes Collection, b. 100, f. 1.
28. Hughes, 'Frankfurt Diary', 12 April 1948, Hughes Collection.
29. Hughes, 'Memorandum on university, students', Hughes Collection, b. 100, f. 5.
30. Hughes, 'Diary', 14 April 1948, Hughes Collection.
31. Hughes, 'Diary', 4 May 1948, Hughes Collection.
32. Lowie reported similar experiences with shy Germans (Lowie 1959).
33. Hughes, 'Diary', 25 May 1948, Hughes Collection.
34. Hughes, 'Diary', 18 July 1948, Hughes Collection.
35. Hughes, 'Memorandum on university', Hughes Collection, b. 100, f. 4.
36. Hughes, 'Diary', 2 April 1948, Hughes Collection.
37. Hughes, 'Diary', 25 May 1948, Hughes Collection.
38. Hughes, 'Memorandum on proposed Mss. on Germany', Hughes Collection b. 100, f. 6.
39. That the paper was never published may also have to do with a characteristic of Hughes's which he later explained in a letter to his friend David Riesman: writing field notes was so important for him that he later tended to forget, as often as not, to rework them into a regular academic publication. David Riesman Papers, correspondence with Everett C. Hughes, Harvard Archives.
40. 'Memorandum on Conditions and Needs of the University of Vienna', F.A. Hayek to Henry Ford II, 12 February 1955, copy under Grant number 63–193, Microfilm reel 2574, Archive of the Ford Foundation (FF) (cf. Fleck 2000).
41. Peter R. Hofstätter, Walter Toman and Slatko Zagoroff, a Bulgarian. Two other postwar emigrants, economist Louise Sommer, who worked in Switzerland, and former concentration camp prisoner Roman Rosdolsky, left Europe.

42. Since even late homecomers could have a decisive impact, as was the case for Paul Neurath with respect to statistics in sociology, the local backwardness of the home countries is only further emphasized.
43. If one restricts the analysis to those who were still alive during the Nazi period, the median age was forty-six years for a regular (German or Austrian) professorship and forty-seven years for the position of a (US) full professor.
44. Hayek, 'Report on visits to Austria and Switzerland, July and August 1948', RF 1.1, series 700, b. 2, f. 15, RAC.
45. The Kaiser-Wilhelm- (later: Max-Planck-) Institut für ausländisches öffentliches Recht und Völkerrecht (Institute for International Law) was transferred to Heidelberg; the Institute for German History was refounded in Göttingen, in 1956, as Max-Planck-Institut für Geschichte (Max Planck Institute for History); and the Institut für ausländisches und internationales Privatrecht (Institute for Foreign and International Private Law), having been evacuated to Tübingen in 1944, was re-established in Hamburg in 1956.
46. Her doctoral thesis was published soon afterwards as 'Die Selbstverwaltung der Gefangenen' (Auto-administration among prisoners) (1927), *Hamburgische Schriften zur gesamten Strafrechtswissenschaft*, vol. 12, Mannheim: Bensheimer.
47. Biographical information based on Liepmann's Fellowship Card, RF, RAC.

## Chapter 8 Red Threads

1. This includes the following countries: Austria, Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Sweden, Switzerland and Great Britain (Maddison 2001: 185).
2. Between 1880 and 1913, the US part of worldwide industrial production doubled, rising from 15 per cent to 32 per cent. Index figures for the total industrial power of the states in question highlight the fast rise of the United States. In 1900, the figure was 100 for Great Britain and, even then, 128 for the United States, while the figures for the other powers were: 71 for Germany, 47 for Russia, 37 for Austria, 26 for Austro-Hungary, 14 for Italy and 13 for Japan. Hardly one decade later, at the beginning of the First World War, Germany had reached 138 and had thus outperformed Great Britain (127), but the United States figure was 298 even then (57 for France, 77 for Russia, 41 for Austro-Hungary, 22 for Italy, 25 for Japan (Kennedy 1989: 311).
3. Those concerned about national pride might of course insist that the two persons identified as 'Austrians' in the present context, Charlotte Bühler and Oskar Morgenstern, were Germans by birth and, in the case of Bühler, had even done the major part of her training in Germany; against this view, I can only point out that the attribution to the two German-language cultures has in the present context been based on their residence at the time or, as the case may be, on the time they stayed in one or the other of these countries.



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