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Values, Objectivity, and Explanation in Historiography

Tor Egil Førland

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Bringing sophisticated philosophy to bear on real-life historiography, *Values, Objectivity, and Explanation in Historiography* rekindles and invigorates the debate on two perennials in the theory and methodology of history. One is the tension between historians' values and the ideal—or illusion—of objective historiography. The other is historical explanation.

The point of departure for the treatment of values and objectivity is an exceptionally heated debate on Cold War historiography in Denmark, involving not only historians but also the political parties, the national newspapers, and the courts. The in-depth analysis that follows concludes that historians can produce accounts that deserve the label "objective," even though their descriptions are tinged by ineluctable epistemic instability. A separate chapter dissects the postmodern notion of situated truths.

The second part of the book proffers a new take on historical explanation. It is based on the notion of the ideal explanatory text, which allows for not only causal—including intentional—but also nomological, structural, and functional explanations. The approach, which can accommodate narrative explanations driven by causal plots, is ecumenical but not all-encompassing. Emergent social properties and supernatural entities are excluded from the ideal explanatory text, making scientific historiography methodologically individualistic—albeit with room for explanations at higher levels when pragmatically justified, as well as for plural subjects—and atheist.

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To Erik Stoveland: teacher, philosopher, friend

Contents

Preface

This book stems from a longtime interest in the philosophy of history, which since the turn of the century has resulted in the publication of various articles: some in English, others in Norwegian. Three of the Norwegian articles (Chapters 1, 3, and 8), published in, respectively, *Historisk tidsskrift* and *Tidsskrift for samfunnsforskning*, have been translated and are made available to an international audience in this book, along with three articles from *History and Theory* (Chapters 5, 6, and 7). The Norwegian articles have been thoroughly revised to make them accessible to international readers. In these and the other previously published articles I have corrected mistakes and included references to newer literature when pertinent. Two chapters (2 and 4), as well as the Introduction, have been written specifically for the present volume.

My main themes are classics in the theory of history: objectivity, values, truth, explanation, causality, narrativity, and methodological individualism—issues that do not go away, although several may have gone out of fashion for some time. This book is an attempt to show their continued relevance and to provide fresh answers to questions to which fully satisfactory responses have been lacking. Although written over a span of sixteen years, I believe the essays that form the chapters of this book exhibit a certain coherence: thematically as well as with regard to points of view. Still the book is composed with an eye to the possibility that not everybody will read the whole text. Each chapter (except Chapter 2, which is a sequel to Chapter 1) is therefore formatted in a fashion that enables it to stand on its own. A consequence is some repetition; for example, scholars whose views are discussed in several chapters, such as C. G. Hempel or Thomas Kuhn, are introduced several times.

Over the years I have piled up debts to colleagues and friends who have aided my efforts to master theoretical topics far removed from the Cold War history in which I was educated. I want to thank my old supervisors Nils Petter Gleditsch and Helge Pharo, who have watched and assisted my journey from the study of economic warfare to the philosophy of history. So, too, has my first university teacher, Erik Stoveland. Being a philosopher and a teacher of God's grace, as well as a longtime friend, he is probably the one person who, unwittingly, has done the most to inspire the present work, and I dedicate this book to him.

One person has been by my side through all the years I have been working on theoretical matters and has been a constant source of intellectual sparring as well as love: Hanne Monclair. Her reading being wide, her mind agile, and her enthusiasm considerable, her suggestions would have been deserving of more forthcoming responses than I have been able to give. So the least I can do is take this opportunity to give her my heartfelt thanks.

I also wish to thank advisors, friends, and colleagues who over the years have provided comments, ideas, or aid of various kinds. They include Pål Andreassen, Brian Fay, Karen Gammelgaard, Olav Gjelsvik, Ståle Gundersen, Finn Erhard Johannessen, Pål Kolstø, Thomas Lystfell, C. Behan McCullagh, Raino Malnes, Klaus Nathaus, Kim Priemel, Peter Railton, Paul Roth, Hilde Sandvik, Erling Sandmo, Kristian Steinnes, and Kai Østberg.

Stig Oppedal has translated the articles that form the basis of Chapters 1, 3, and 8. I am grateful to him and to Louise D. Hylin and Mari Salberg for research assistance with the articles that make up Chapter 1. For the purpose of the present book I have had the great fortune of being assisted by Henrik Olav Mathiesen, who in his knowledgeable, hardworking and meticulous way has been essential in preparing the manuscript. The list of his many tasks includes helping me with literature, preparing the index, correcting errors, and trying (with less success than he deserved) to keep my number of metaphors down. The contributions by him and the others mentioned previously—as well as unnamed colleagues at the University of Oslo and anonymous peer reviewers—have made this book much better than I could have done on my own. For the shortcomings and mistakes that remain, I alone am responsible.

University of Oslo,
August 2016

Acknowledgments

Chapter 1 is a translated and revised version of the article "Deltaker og medløper: Venstresiden, Sovjetunionen og objektivismens fall," published in *Historisk tidsskrift* (Oslo) 89, no. 4 (2010): 521–546. The chapter has been augmented with parts of my reply, "Vitenskap, verdier og venstrehistorikere: Tilsvar til Narve Fulsås og Hallvard Tjelmeland," published in *Historisk tidsskrift* (Oslo) 90, no. 2 (2011): 235–246.

Chapter 3 is a translated and revised version of the article "Det er ikke sant alt som glimrer: En kritikk av forestillingen om situerte sannheter og moralens primat," published in *Historisk tidsskrift* (Oslo) 79, no. 1 (2000): 100–117. The chapter has been augmented with parts of my reply, "Ekte poststrukturalister bryr seg ikke om virkeligheten: Gjensvar til Erling Sandmo om sannhet og kulturhistorie," published in *Historisk tidsskrift* (Oslo) 79, no. 4 (2000): 509–521.

Chapter 5 has been previously published as "The Ideal Explanatory Text in History: A Plea for Ecumenism," in *History and Theory* 43, no. 3 (2004): 321–340.

Chapter 6 has been previously published as "Mentality as a Social Emergent: Can the *Zeitgeist* Have Explanatory Power?," in *History and Theory* 47, no. 1 (2008): 44–56.

Chapter 7 is a revised version of the article "Acts of God? Miracles and Scientific Explanation," published in *History and Theory* 47, no. 4 (2008): 483–494. It is augmented with parts of my reply, "Historiography without God: A Reply to Gregory," published in *History and Theory* 47, no. 4 (2008): 520–532.

Chapter 8 is a translated and revised version of the article "Årsaksproblemer i historisk forskning," published in *Tidsskrift for samfunnsforskning* 54, no. 3 (2013): 355–370.

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Introduction

Values, Objectivity, and Explanation for a Postfoundational Time

When histories of historiography and of the philosophy of history are written in the future, chances are that the early years of the twenty-first century will be seen as a time when the tide turned and postmodernist or poststructuralist influences ebbed. Wishful thinking or not, the present work is intended to speed up such currents and take a fresh look at critical aspects of historiography that have been shipwrecked by the inflow of linguistic and narrative approaches to a discipline that despite its literary manifestations is also social science. I want to restore objectivity to historiography, if admittedly less than total objectivity. I want to show that political and ideological values can be kept in check by cognitive values—and why they ought to. I want to combat the notion of situated truths, which I consider misguided, although well intended. I want to propagate an understanding of explanation that has room for the way historians explain but also discerns between scientifically legitimate and unscientific explanations. By means of this take on explanation, I want to deny scientific legitimacy to emergent social properties and supernatural agents by showing why they are excluded from the ideal explanatory text of historians.

Having gone so much on the offensive, let me cover my flanks by reassuring that this book is not a nostalgic plea for returning to a historiography and a philosophy of history as conducted in the decades before the linguistic turn and the 1970s revival of narrative combined with political radicalization to overturn the old regime of seemingly self-complacent belief in neutrality, replication of the past, and deductive–nomological explanation. (Admittedly the belief in the latter called for more self-flagellation by historians than self-complacency.) What Jouni-Matti Kuukkanen in his recent book calls the postnarrativist philosophy of historiography has to take into account insights from writers such as Arthur Danto on narrative sentences, Donald Davidson on interpretive indetermination, and Hayden White on the power of literary tropes to bestow meaning upon narratives.[1] In the endeavor to establish philosophically solid ground for a historiography for our postfoundational time, I see myself as joining forces with Kuukkanen, Mark Bevir, and Herman Paul: scholars young enough to have escaped the shadows of C. G. Hempel, logical empiricism, and the

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tremendous prestige of social-scientific models and quantitative analyses of the 1950s and 1960s (and therefore also with no need of going to the opposite extreme by taking a linguistic turn to liberate themselves) and able to pay heed to narrativist insights while at the same time appreciating the value of more or less conventional historiography.[2] Acknowledging that I am a historian and not a philosopher of history, I do not aspire to propose new theories within the latter. Precisely because I know what it takes to work as a historian, however, I may have something unique to add to the philosophers' analysis, namely, knowledge of how historiography is produced. When studying only historians' texts, what you see is only the finished product: what Leon Goldstein calls the "superstructure." The "infrastructure," the work that is leading up to that product, is left in the dark, although it is a critical part of the historian's métier.[3]

Bridge Building

This book is an attempt at bridge building. At a time when the literary and narrative tide may have turned, I want to build (or rebuild) a bridge between historiography and philosophy. As just mentioned, I believe that philosophy of history would benefit from increased knowledge of what historians actually do: *wie Geschichte eigentlich geschrieben wird*. As a practicing historian with a long-standing interest in philosophy I also believe that historiography can benefit from insights gained by philosophers—and not only by philosophers of history.

On a substantive level I endeavor to establish lines of communication between three islands: the historian, the actors he or she studies, and readers. The relationships between them are not reciprocal. The historian is the mediator and the active part. Except in some instances of contemporary history, the objects of study are gone and unable to respond to the historian's account. Even the ontological status of the objects of study—past agents—is moot, since they have left with no returning or possible replication and have left behind only the traces that remain for the historian to study and on which to build constructs of the past, that is, history.[4] Readers for their part are actively choosing whether to study the image of the past as crafted by the historian. They are also active in processing the historian's message, interpreting—if not rejecting—it and sometimes even responding to the historian. And still the relationship is uneven, with the historian painting the picture of the past for readers to study.

The historian is a relay, then, seeking, teasing out, amplifying and (we have to admit) twisting signals from a sometimes long gone past as historical accounts are created and presented to prospective readers on the basis of sources available in the present. But I do not want to get lost in metaphors—or to lose in the relay metaphor the constructive role of the historian. My point here is that these three metaphorical islands—the past actors (for all the epistemological challenges involved in their study), the

historian, and readers—all potentially have different values and worldviews; perhaps we could stick to (and strain) the metaphor and say they work on different circuits. The successful historian manages to connect these circuits, making readers understand the past. Hermeneuticists would term this the fusing of horizons. For purposes of history the interest has primarily been with the relationship between historians and past agents. I want to press for the inclusion of the third group, that of readers. The temporal distance between historians and readers is normally smaller than that between historians and their objects of study. And still the former two may be worlds apart—or at least different worldviews may isolate them from each other and prevent, hamper, or seriously distort communication.

This is where values enter the picture. Different political, moral, or religious values can block communication between historians and readers even though their worldviews in other respects may be similar. That such values influence not only historians' choice of subject matter but also their interpretations and theories is accepted by all reflective scholars today and not just by the philosophically adept. What seems less appreciated is the risk this entails for readers to reject, a priori or at least without according serious consideration, the accounts presented by historians of other valuations, or seen as belonging to a different group than themselves. The effect is a breakdown in communication and, possibly, a fragmentation of society into segments with separate value systems and their own historians.

The divide along the Iron Curtain between communist and Western historians during the Cold War is an example of what happens when different scholars do not acknowledge each other as *bona fide* scientists. In the 1970s there were similar tendencies within the West, with mutual distrust between Marxist and "bourgeois" scholars within social science and the humanities. The first two chapters of this book delve into a recent instance of what amounts to an epistemological divide, namely the fight in the first fifteen years of the twenty-first century between mainstream, overwhelmingly left-leaning Danish Cold War historians and their allies in the press and parliament versus their strongly anticommunist colleague Bent Jensen and his allies in other parts of the press as well as in the Liberal–Conservative government of Anders Fogh Rasmussen and its parliamentary supporting People's Party. Jensen willingly became part of the prime minister's culture war against those whom Fogh Rasmussen, prime minister from 2001 to 2009, considered soft on Nazism during World War II, soft on communism during the Cold War, and soft on Islamist terrorism during the War on Terror proclaimed by his U.S. allies. Jensen fought his foes not only in the press and in books but also in the courts when in 2007 a longtime adversary, the influential leftist journalist and nuclear disarmament activist Jørgen Dragsdahl, sued for defamation after Jensen wrote that the KGB and the Danish intelligence service, PET, had considered Dragsdahl an agent of influence. The court case is discussed in Chapter 2, "Court Historian: Matters of Fact," whereas Chapter 1, "Participants and Fellow Travelers: The Left, the Soviet

Union, and the Fall of Objectivism," presents the political battles that preceded it. At a more abstract level this first chapter also brings politics and philosophy together, describing how the tables have turned since the attacks on positivism in the 1960s and 1970s. The insistence that all scholarship is value laden and thus in the last instance a function of ideology, once championed by leftist antipositivists, has been embraced by the Right in the different political climate following the demise of communism. Leftist historians, under attack for letting their (former) radical views taint their research, seek shelter in a notion of objective scholarship shattered by their antipositivist comrades a generation ago and since abandoned.

Scholars unable to acknowledge the research of colleagues with a different worldview are a well-known source of academic conflict, with the opponents of Galileo the prime example. What complicates matters in history is that the divergence of worldviews can be found not only between scholars but also between the historian and the agents who are his or her objects of study: indeed this gorge is one over which the historian must build a bridge in order to understand and successfully interpret the situation of the protagonists and the choices facing them. Further complicating matters is a second potential divergence: between the historian and readers, who may not share the former's worldviews and values (or those of the historical agents). If the difference is too wide, readers might devalue and disregard the historian's research no matter how meticulously and creatively it is conducted because they do not think a scholar with views so different from their own can produce research that they will appreciate as true and relevant.

This is exactly what happened in Denmark in the case mentioned earlier, when dominant Liberal–Conservative–Populist politicians discounted the mainstream, left-of-center consensus of Danish Cold War historians and decided to finance an alternative Cold War history. This history would be more appreciative of their favored interpretation, namely, that the United States and its allies, including Denmark, had been right and the Soviet Union and its communist henchmen and fellow travelers wrong, and that the timid Danish politicians who had wanted to appease the Soviets instead of standing up to them and be counted with Denmark's North Atlantic Treaty Organization (NATO) allies had been wrong, too—just as those who had wanted to appease Hitler in the thirties had been wrong, and just as those who hesitated to stand up with the United States against radical islamism were wrong. Critics—including mainstream historians—would lament that the prime minister debased historiography by lifting it out of its historical contexts to use it for present-day political purposes in his culture war against less gung-ho domestic opponents. Fogh Rasmussen and his allies in parliament and the press would retort that history had shown who had made the right choices in the past and that by using history to show the consequences of, respectively, courage and cowardice, historiography was made relevant. Therefore, they had no time for historiography that disregarded or downplayed who had been right and wrong by exhibiting

excessive empathy with historical agents or by explaining away their faults. Kierkegaard would have applauded his later prime minister. Ranke would have sighed with grief. The antipositivists of the 1960s and 1970s would have sided with Kierkegaard until realizing that their philosophical position implied that they cheered people who were thumping political views that stood in opposition to everything they believed in. The result would have been the same cognitive dissonance crisis between their philosophical position and their political sympathies as Danish left-leaning historians found themselves in but refused to acknowledge.

Another factor besides political ideology that can create differences in worldviews and values so wide that communication is prevented or at least seriously distorted is religion. We need not go to violent extremes to see how religious differences can make a worldview divide into an insuperable abyss. Science—humanistic and social science, as well as natural science—has evolved into an atheist effort, meaning not that every scientist must be an atheist but that notions of an active God are proscribed entrance into scientific explanations. The reason is methodological: modern science is naturalistic or materialist in a way that leaves no room for such a God or similar supernatural agents. This is discussed in Chapter 7, "Acts of God? Miracles and Scientific Explanation," taking a pithy miracle story from seventeenth-century Norway as a point of departure. Two brothers stranded on a tiny island were reportedly saved by God's repeated provision of edible plants on soil plucked barren the night before. How does a Christian historian of today, who not only believes in God but also goes to church and says evening prayers, explain this putative miracle? Not as the objects of study did, by referring to God's grace by means of his active hand in worldly matters but by explaining it away, declaring at the outset that it cannot have happened (as a miracle) and then seeking for naturalistic explanations. The result is a severance of communication with the historical actors, but since they are long gone this imperialism on behalf of the modern, atheistic worldview has no direct consequences for them. Choosing an atheistic explanation—or rather, dismissing out of hand the possibility of an active God manifesting in the historical sources—has the consequence, however, of laying bare the divide between the historian (or the community of academic historians) and the believer (or the community of believers). The believing reader is presented with an account of events in which the heart of the story as reported in the sources is taken out and rejected. So why would the believer put any faith in this rendering of the past? There is a further complication to this conundrum when—as in this specific instance of the seventeenth-century miracle story—the historian himself is a believer, rejecting a critical part of his own beliefs when narrating the story. It would seem we have another instance of schizophrenia: the historian qua scientist lives in one world—or at least does research according to one worldview—and qua believer lives in another, with widely divergent notions of how the world works.

The cognitive dissonance troubles of left-leaning Danish historians seeking refuge in a scientific objectivity long abandoned in order to escape the pursuers from the Right, as well as the split personality disorder haunting historians who believe in an active God but dismiss that belief when donning their historian mantle, are real dilemmas. They represent the challenge inherent in trying to communicate with readers who entertain worldviews that diverge widely from that of the historian.

Chapter 3, "Witches Cannot Fly: A Critique of the Notion of Situated Truths," presents a critique of an attempt to solve the dilemma by means of hyper-tolerance, namely by admitting different agents a different truth—and not just different, compatible parts of the truth, but different and at least potentially incompatible truths. This is the postmodern notion of situated truths, a well-intended but, in my opinion, misguided effort to keep the peace among factions with widely different worldviews by admitting each their own take on reality and refraining from pronouncing on whose take is veracious and whose are not. On this view, the seventeenth-century miracle report of God providing for the stranded brothers was true for them, since they were living in a culture permeated by notions of an active God. For twenty-first-century historians, situated within a culture with a naturalistic worldview, the truth is different and does not involve God. Acknowledging that humans are fallible and that part of what we regard as true accounts of the world probably will be superseded by other accounts in the future, why should we prioritize our notions by declaring our accounts of the world true and those of our objects of study untrue, even if they seem to be in contradiction? (If there is no contradiction there is no problem and consequently no need for a new truth concept.) And if we insist that we are right and they were wrong, how can we ever hope of understanding them?

To postmodernists—as in a sense we all are—imbued with tolerance as a supreme value, there is something inherently attractive in the notion of situated truths. With a twist of concepts—from one truth to many—conflict is lifted and resolved by the acceptance of our own epistemic shortcomings and a concomitantly increased respect for others whose ideas and lifestyle seem quaint to us. It is reminiscent of the difference between monotheistic religions and Hinduism with its plethora of deities living in what comes across as peaceful confusion. It seems particularly promising for the study of other cultures, whether distant in time or place, whose different worldviews and ways of life we no longer have to judge inferior to our own. No wonder new cultural history has been especially attracted to the notion of situated truths. Ideas of one's own superiority easily prevent appreciation of the way other people conduct their lives.

And yet I contend that the notion of situated truths is unfortunate: it is a bridge too far. In Chapter 3 I sustain this assertion by critically assessing the leading Norwegian postmodernist historian Erling Sandmo's writings on the subject. I think situated truths is a category mistake, since inherent in the notion of truth is the idea that it is precisely *not* situated but is

the same for all people at all times. Even if postmodernists should succeed in changing the meaning of truth to denote something situated, language would need a concept for what we, gilding the lily, might call absolute truth. It is better to retain the meaning of truth as absolute and to employ the ample resources language provides to express the idea that situated truth is intended to convey, namely, that people hold different things true at different times and places. Moreover, we cannot unring the bell by closing our eyes to knowledge accumulated since the time of study or fool ourselves into disbelieving what we hold true of how the world works.[5] What we can do is try empathetically to understand how our objects of study interpreted their world and to convey this understanding to readers. We do not have to embrace the views of other people in order to understand them. Attempting to fuse our horizon with those of our study objects need not entail abandoning our own beliefs. Furthermore, the notion of an all-encompassing culture so basic to the idea of situated truths is unable to accommodate intracultural divergences of opinions and worldviews such as those between Galileo and his adversaries or between people accused of witchcraft and their accusers. Truth cannot reside in culture or be determined by it. It must be super-cultural. If our objects of study believed things that contradict our best knowledge, we must stick to our own convictions and discard theirs, even if it would be more tolerant to refuse to choose between their views of what is true and ours. We cannot have the truth and deny it too.

Acknowledging the dilemmas following from possible worldview gorges between the historian and readers—or even within the historian—and accepting that the anything-goes notion of situated truth is no way out of the dilemma is not the same as resigning to it. Such resignation would mean that readers sharing (more or less) one worldview and one set of political, moral, ideological, or religious values would subscribe to some historians, while readers sharing other worldviews and values would subscribe to others. The work of historians conforming to other worldviews would be dismissed out of hand due to "wrong theory," as Marxists put it in the 1970s. To some extent this is the situation today; we read with more skepticism authors whom we know do not share our values than scholars known to stand close to us. Heeding E.H. Carr's advice, we study the historian before buying into his or her account.[6] Demands that scholars profess their views and values follow naturally on this line of thinking and may reinforce it: why listen to a professor whose views and values are widely different from our own? Segmentation is not complete, however, and Chapter 4, "In Defense of Objectivity: Facts and Theory Choice in Historiography," shows why it need not and should not be. The cognitive values of science give us a way to escape the dilemma of competing worldviews by acting as a bridge between scholars—as well as between scholars and readers—of different persuasions. This is done by going in the opposite direction of the extreme tolerance inherent in the notion of situated truth promoted by poststructuralists. Instead of accepting a multitude of contradicting truth claims and

refusing to choose among them, cognitive values allow for theory choice on the basis of values shared by the scientific community.

The exact denomination of cognitive, scientific values such as internal consistency, simplicity, scope, and accuracy is an ongoing project, as is the bordering work on which such epistemic values are to count as must-have and which as nice-to-have. The values informing theory choice in historiography conform to the values in science. In addition, historians since Ranke have developed a set of criteria for assessing evidence to tease out facts, an impressively large body of which remains uncontested to a degree that allows them being regarded as true. Such facts, having survived tests of extensive source criticism, are independent of the worldviews and values of the scholars presenting them. In and by themselves they can have huge significance for our valuations and our comprehension of history, as exemplified by the libel case discussed in Chapter 2. Moreover, facts are the foundation stones for second-order historical theories: whether of narrative or other forms. As such, facts and theories founded on them and chosen in accordance with the epistemic values of science can function as bridges between groups with diverging worldviews and extra-cognitive values, providing a basis on which society can debate and form decisions that are legitimized by resting on this common ground. Without consensus on a body of historical knowledge, consisting of both facts and theories, debate may degenerate into a shouting match between groups unwilling or unable to acknowledge each other's points of view, as exemplified by the Danish Cold War history debacle. The sad consequence is an *ad hominem* discussion in which adversaries are discounted a priori as unable to present valid arguments.

Insulation

Bridge building—between historiography and philosophy and between historians and readers with different worldviews and values—is the first goal of this book. The second goal is the flip side of this coin, namely the insulation of scientific historiography from extra-scientific influences by walling it off from values other than cognitive. In opposition to the so-called strong program in science studies that "reject[s] the distinction between epistemic and social factors,"[7] I want to eject social factors from historiographical arguments and theory choice, since their intrusion will prevent the establishment of a common ground of historical knowledge on which debate and decisions can build.

One way to wall off the historiographical undertaking from the influence of social factors is to describe what makes a scientifically legitimate explanation. This was C.G. Hempel's ambition with his 1942 essay "The Function of General Laws in Historical Explanation," which shaped the debate in philosophy of history for more than two decades until explanation as the dominant theme was superseded by narrative.[8] The explanation

debate, shaped by Hempel's insistence that a valid scientific explanation requires the formulation of a general law, angered many historians (and philosophers of history) who were hurt by his dismissal of much of what had counted for explanations in historiography as mere explanation sketches for their lack of such laws. When it finally receded, the debate was not so much won by one side as exhausted. While the debate raged, historians, unperturbed, went on explaining much as they had done before, until saved by the 1970s' revival of narrative that relegitimized their favorite form of exposition and let them hide causal and other explanations within their narratives without questions asked about the nature of these explanations. I ask such questions in Chapter 8, "Problems of Causation in Historiography," which is an attempt to assess the current status in the discipline of this most critical of explanatory concepts.

My analysis of historians' take on causality employs the conceptual tools developed in the earlier chapters in Part II of this book, in particular, Chapter 5, "The Ideal Explanatory Text in History: A Plea for Ecumenism." The notion of the ideal explanatory text is taken from Peter Railton's analysis of scientific explanation and transferred to historiography, where I think it has great relevance. I try to show this relevance by means of something sadly missing in much philosophy of history, namely, concrete analysis of contemporary historiography.

A key feature of Railton's analysis of explanation is that explaining means providing information on what the explanandum is due to. In this apparently unobtrusive definition lies an understanding of explanation that is particularly well suited to historiography. First, although encompassing covering law(like) explanations of Hempel's deductive–nomological kind—and historians should not underestimate the prevalence of (often implicit and taken for granted) covering laws in their narratives—this approach allows for the fact that historians' explanations, in general, take the form of accounts in which various types of explanatory information are assembled. The information can be variations of covering laws, but valid explanations can also be causal/mechanistic, intentional, structural, or functional. Practicing historians use all these kinds. Another advantage of seeing explanation as the act of providing explanatory information is that it enables us to see how historians explain not by presenting fully fledged explanations but by providing bits and pieces of explanatory information, leaving the reader to fill in the rest. Often readers need only a name or a smoking gun to figure out what had happened and why. They know the rest already. This is why historical narratives can be explanatorily satisfactory despite being meager on explicit analysis: they provide the reader with as much explanatory information as he or she needs to figure out the rest. Since there can be little doubt that readers accept historical narratives as explanatory, making room for such narratives is a *sine qua non* of an effective analysis of explanation in historiography.

Another distinctive feature of the view of explanation that I propose is that it admits explanations of totally different kinds of one and the same phenomenon under one and the same description. A complete and fully satisfactory covering-law explanation of an event (let us for the sake of argument accept that such explanations exist) does not exclude other, equally complete and satisfactory intentional or structural explanations of the same event—and *vice versa*. All provide information that lets us know more of what the event was due to. Such a plurality of explanatory accounts seems to run counter to Occam's razor and sound methodological principles, which tell us to prefer the simplest explanation and leave the rest. And yet who would deny that each layer of explanation leaves us wiser? Perhaps the key is that these explanations are of so different kinds as to be like different idioms: complementary, not competing ways of describing the same thing. This would be akin to us describing solid matter as solid despite knowing that in the idiom of nuclear physics, which we all accept as an accurate description of the material world, such matter is made up of atoms in which electrons circle the nucleus in ways that leave a lot of empty space within each atom. Thus, there will probably be room for intentional or rationalizing action explanations of the belief–desire kind no matter how far brain research on consciousness evolves.

A significant feature of Railton's analysis of explanation is his concept of the ideal explanatory text: an unrealizable textual entity encompassing, in its encyclopedic version, every valid due-to relation that is true of the explanandum. The historian's explanatory task can be described as illuminating parts of the ideal explanatory text. It is a never-ending task, since there is no end to the relevant explanatory information than can be had. From a philosophical point of view this need not bother us. Two other aspects are more challenging to the notion of the ideal explanatory text and its usefulness to the philosophy of history.

First, we should notice the similarity between Railton's ideal explanatory text and Laplace's demon. The latter, introduced in the history of science by Pierre-Simon Laplace in 1814, is an ideal intelligence cognizant of every force in nature, knowing the position of every entity in the physical world, and with recourse to unlimited data processing powers. Hence—since in Laplace's mechanistic worldview there is no causal uncertainty—the demon would know not only everything about the present but also all about the past and the future. Laplace's demon would know the entire ideal explanatory text—at least the causal/mechanistic version of it. The challenge—for Laplace's demon as well as for the ideal explanatory text—is that textual renditions of actions always come under a description, and there is no way, even for an intelligence like Laplace's demon, to determine which of several alternative descriptions to prefer. In the world of actions, this kind of indeterminism seems a nonredundant feature. Such indeterminism is perhaps less of a problem for the ideal explanatory text than for Laplace's demon, however, since it means only that the ideal explanatory text would

be dependent upon the description under which the explanandum action would fall.

Laplace's demon has a cousin in the humanities: Arthur Danto's ideal chronicler.[9] The ideal chronicler—or rather its limitations—represents the second challenge to the usefulness of the ideal explanatory text. The ideal chronicler knows everything that is going on. The chronicler can take note of every event and describe in perfect detail all that happens. The book of the ideal chronicler contains it all. A marriage of the demon and the chronicler would make a couple that would know all there is to know about the state of the world at any time past, present, and future. And yet this omnicognizant pair would be unable to write history. The reason is that the chronicler, not even aided by the demon's formidable knowledge of the workings of the physical world, is unable to write what Danto calls narrative sentences: sentences that make use of later events to describe what went on at some earlier point in time. The chronicler records what is happening but for all his or her knowledge and powers of cognition is insulated from knowing how the description of the events is dependent on—and is changed by—posteriority. The chronicler can record that a Great War is raging, but not that it is World War I. Every comma of the Treaty of Versailles that marked the end of that war, the treaty's etiology and the reactions caused, the chronicler painstakingly records in their totality. But the chronicler is bereft of future descriptions of the events to which he or she is witness, descriptions that distinguish a historian writing with the benefit of hindsight from the chronicler. Thus, the latter, locked in perennial present tense, is barred from reporting the signing of the Treaty (of Versailles) that left the Germans with such grievance that it would pave the way for the dictatorship that led them straight into an even more destructive war—and an even more complete defeat. Writing narrative sentences such as the latter part of the last sentence (from the Treaty on) is the privilege of the historian, and is out of reach for the ideal chronicler. Since there is no end to the amount of new narrative sentences that can be created by posteriority, the ideal explanatory text can never be penned, not even in the ideal world. This is why every generation must write history anew and why not even an ideal historian would ever be able to illuminate the entire ideal explanatory text. It is an ever-moving target, always one step behind (!) the historian.

The inherent limitations of the ideal explanatory text in historiography should not make us shy away from exploiting its usefulness, which I claim is considerable. First, as already mentioned, it shows how even bits and pieces of information can be explanatory: by elucidating parts of the ideal explanatory text that had been in the dark before this particular source was utilized or that particular theory was employed. In an account of why coins with high silver content went out of circulation when Britain's master of the mint Sir Isaac Newton in 1717 diluted silver coins without reducing their nominal value, effecting the hoarding of old coins as the British dumped new coins of lower silver value on tax authorities and sellers of goods and

services, Gresham's law (bad money drives out good) is explanatory. And so is a detailed causal analysis or narrative.

Another use of the ideal explanatory text is to check what kind of explanations are not legitimate. This is how the concept can wall off scientific historiography from unwanted intrusion. Chapters 6, "Mentality As a Social Emergent: Can the *Zeitgeist* Have Explanatory Power?," and 7, "Acts of God? Miracles and Scientific Explanation," investigate two types of explanations that I conclude are not within the ideal explanatory text, respectively, collective mentality as a social emergent—and not just as shorthand for the beliefs, desires, and worldviews of a large collectivity—and supernatural agents such as God. The problem with the former is that emergent social properties are vulnerable to the same kind of criticism that drove the notion of emergent mental properties underground, namely, that the notion of emergent properties seems untenable other than as mereology: a kind of supervenience that deprives them of causal power and leaves them doomed to a shadowy epiphenomenal existence. Superindividual entities have a role in social science, but only as institutions or as what Margaret Gilbert terms *plural subjects*, in which "participant agents" act jointly or have a jointly accepted view.[10] Since the ideal explanatory text allows for explanations of different kinds of the same phenomenon, plural subject explanations can augment rationalizing action explanations based on individual agents: we do not have to choose one or the other.

As opposed to plural subjects, there is no room for God in the ideal explanatory text. The reason is not that God does not exist. As noticed earlier, however, the notion of God is not part of scientific descriptions of how the world works. Contemporary science is atheistic because contemporary scientists *qua scientists* never make recourse to supernatural entities—irrespective of whether they believe in God in their other roles, private or public. We need not enter the debate on whether God exists in order to see why not even fervent believers in their capacity as historians never invoke God's will or actions in their explanations. We need only observe that supernaturals are not part of the ideal explanatory text—by convention. This means there is nothing that prevents God from entering the ideal explanatory text in historiography if scientists should include God in their scientific descriptions of how the world works. But unless this happens, historians, being participant agents in the plural subject of the scientific community, must refrain from invoking God in their explanations. God is barred from scientific historiography.

Explanations in historiography are often seen as synonymous with causes. David Lewis, whose view of explanation as providing information concurs with the analysis by Railton that is the philosophical foundation for my account of historiographical explanation, sees no room for other than causal explanations (i.e., to provide information on the "causal history" of the explanandum). Whereas I show in Chapter 5 that historians do not restrict themselves to causal explanations, this is clearly the main type of

explanation in historiography. In Chapter 8 I look closer at historians and causality. The first part of the chapter is an analysis of what, judging by their practice, historians understand by causality. In historiography, a cause is seen as something that makes something else happen or come about. I move on to ask three questions about causality. The first two are social scientific classics: whether reasons can be causes and whether methodological individualism can be transcended. On the definition just cited, the first question is answered in the affirmative. The second question is tougher. Building on the discussion in Chapter 6, my answer is a guarded yes, but only by recourse to plural subjects, a concept that many methodological individualists would accept and which at least is a far cry from conventional notions of methodological collectivism. The more radical transcendence offered by Foucauldian discursivism is rejected, since discourse supervenes on the utterings and writings that constitute the discourse, making it an in-virtue-of relationship in which the supervening level is void of independent causal powers.

The third question is more specifically tied to historiography and to many philosophers of history's favorite topic of narrativity, namely whether there are elements other than causal conjunctions that can emplot and thus constitute historians' narratives. One alternative is deeper meaning or significance relations, like in fairy tales or religious stories or morality tales. Once we enter the waters of deeper meaning, however, we leave the safe—if dry—ground of science and academic historiography: such relations are not found in the ideal explanatory text. Another alternative is David Carr's suggestion that narratives are explanatory because we comprehend life narratively: we live narratively.[11] Whereas I do not want to deny the centrality of narrativity in our mental processing of events along the time line, I find Carr's claim, and his concomitant refusal to analyze further the explanatory character of narratives, unsatisfactory. A third alternative to causality as emplotting constituent in narratives is, of course, Hayden White's tropology, in which the emplotment work is done by linguistic tropes that infuse different narratives with meaning as either romance (metaphors dominate), tragedy (metonymy is preponderant), comedy (dominated by synecdoches), or satire (characterized by irony).[12] Whereas White's perspicaciousness is widely acknowledged and few would doubt the importance of linguistic elements in providing meaning to historians' narratives, tropes merely add another layer of meaning to the narrative. Like significance relations they do not render causality irrelevant or put it out of work as an agent necessary for the emplotment of narratives claiming scientific status.

Facts and Interpretation

The benefits of holding fast to standards for evaluating data, for choosing among alternative theories, for deciding what belongs within an explanation and what does not, and for assessing claims of causality, should

not be underestimated. It gives different communities hope of agreeing on some basic facts *and interpretations* of the past: no mean feat once we think how much blood has been spilled between groups and nations that have insisted on a history in which the other was the source of evil. We should not forget the political import of facts. It is certainly no coincidence that the Truth and Reconciliation Commission set up in South Africa after the fall of apartheid insisted on establishing what was called the "forensic truth" about past events.[13] Nor should we be shy about the gains to be had by insisting that some interpretations are better than others. By holding historical accounts up to scientific standards, historians can reduce political and ideological division. But then they have to realize and insist that by taking heed of scientific standards (which, of course, are based on scientific values), political, religious, and ideological values and worldviews become less influential in shaping historians' accounts of the past. *Pace* E. H. Carr, the historian does not always get the facts that he or she wants. History is not just interpretation. Moreover, the interpretive part, important though it is, is far from wholly subjective or hostage to the historian's political, ideological, or religious values or aesthetic preferences. Interpretation in scientific historiography takes place within epistemically determined bounds. This is what separates historiography from fiction and propaganda.

Notes

1 Kuukkanen, *Postnarrativist Philosophy of Historiography*; Danto, *Narration and Knowledge*; Davidson, "Psychology as Philosophy"; White, *Metahistory*.
2 Kuukkanen, *Postnarrativist Philosophy of Historiography*; Bevir, *Logic of the History of Ideas*; Bevir, "*Logic*—Then and Now"; Paul, *Key Issues*.
3 Goldstein, *Historical Knowing*, 140–142. For a recent argument along these lines, see Paul, "Performing History."
4 Goldstein, *Historical Knowing*.
5 Collingwood, *Idea of History*, 215; Lowenthal, *The Past Is a Foreign Country*, 216 and 218n189.
6 Carr, *What Is History?*, 23.
7 Bloor, "Sociology of Reasons," 306.
8 Hempel, "Function of General Laws in History." On narrative, see Stone, "Revival of Narrative"; Roberts, *History and Narrative Reader*; Kuukkanen, *Postnarrativist Philosophy of Historiography*.
9 Danto, *Narration and Knowledge*, 143–181. Danto's ideal chronicler in turn has a twin brother, namely the ideal observer described in Novick, *That Noble Dream*, 478.
10 Gilbert, *On Social Facts*.
11 Carr, "Narrative Explanation and Its Malcontents."
12 White, *Metahistory*.
13 See *Truth and Reconciliation Commission of South Africa Report*'s definition of "truth" at 110–114. For the South African and other truth commissions and their conceptualization of truth, see Chapman and Ball, "Truth of Truth Commissions," esp. 9–12.

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Part I

Objectivity, Values, and Theory Choice

1 Participants and Fellow Travelers

The Left, the Soviet Union, and the Fall of Objectivism

Should historians make value judgments about the people and societies they study? In all likelihood, the vast majority of academics would instinctively say no: for modern, and indeed postmodern, historians, the task is rather to understand the various actors' (speech) acts, and potentially also explain them. (The latter ambition is perhaps reserved for the modernists among us.) But condemning or extolling these actors is something we abstain from—or, rather, we relinquish the right to make such judgments to the reader. And if historians are to make value judgments, we must do so by using the actors' values, not our own. For the proper historicist, every era is equally close to God, and our mission is not to pass judgment on the actors or hold them up as either role models or cautionary examples. History as a morality tale is both premodern and unscientific: Ranke killed off such ambitions for good.

In Denmark there is a historian named Bent Jensen who completely disagrees with this point of view. Jensen is a professor emeritus at the University of Southern Denmark in Odense, and he reserves the right to rebuke the Danish politicians and cultural figures he studies for their spinelessness toward, or willful ignorance of, the communist regime of terror in the Soviet Union. Among Danish contemporary historians he is a lone wolf—and his viewpoints are about as popular as his four-legged cousins are among sheep farmers. But during the first decade of the twenty-first century the center-right government and the majority in the Folketing, the Danish parliament, considered him useful in their cultural war against the enemies of liberalism, that is, those who fail to stand up to pressure from antiliberal movements, whether these movements are Nazi (during the Second World War), communist (during the Cold War), or Islamist (during the so-called War on Terror). Hence, the Liberals (*Venstre*), the Conservatives (*Det Konservative Folkeparti*), and the Danish People's Party (*Dansk Folkeparti*) kept a protecting hand over him, going so far as to establish a Centre for Cold War Studies that he could lead.

In one sense, Bent Jensen is doing precisely what the Norwegian philosopher Hans Skjervheim and other 1960s and 1970s critics of positivism insisted that scholars should do, namely, involve themselves in the society of which they are part. But the positivism critics of that era found

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themselves predominantly on the Left; Jensen is on the Right. Even though Jensen, as far as I am aware, never took part in the debate on positivism, his engaged—many would say politicized—historiography allows us to study what happens when the positivism critics' philosophical demands to involvement are turned against the political standpoints of the positivism critics on the Left. Jensen is the unexorcisable poltergeist in the critique of positivism, the Mr. Hyde to Dr. Skjervheim's Jekyll.

This chapter consists of four parts. The first part deals with the fall of objectivism in the discipline of history, with Norway serving to exemplify developments throughout the Western world. The second part comprises a brief comparative treatment of Norwegian and Danish historical research on foreign policy, in particular, during the Cold War. The third part discusses the culture war in Danish politics in the first decade of the twenty-first century and Bent Jensen's role therein. In the fourth and final part, I return to the criticism of objectivism and ask what Jensen's example can tell us about the relationship between historiography, morality, and politics. I conclude by elaborating on two points made previously in the chapter, concerning problems inherent in the critique of positivism and attempts to overcome them.

I. The Fall of Objectivism in Historiography

Sometime between 1965 and 1975, Western historiography lost its innocence. A fairly positivist or at least empiricist outlook on the production and science of historical knowledge, with objective and neutral (in the sense of "impartial" or "detached") historiography as an ideal, was supplanted by an explicitly antipositivist approach that rejected such more or less critical and empirical objectivism.[1] Though trying to provide a satisfactory definition of *objectivity* and *objectivism* is beyond the scope of this chapter, we should nonetheless take a brief look at how the discussion of these concepts should be understood. In *That Noble Dream: The "Objectivity Question" and the American Historical Profession*—a work that was instrumental in putting the final nail in the coffin of the belief in objective historiography in the United States—Peter Novick uses half a page to provide a "radically compressed . . . summary of the original and continuing objectivist creed," taking as its starting point the objective historian's role as "neutral, or disinterested." Novick goes on to use a further half page to account for the aspects of objectivity that were "reworked or reinterpreted over the last hundred years" as the belief in the historians' capacity to disregard all their own values and let the facts speak for themselves was undermined.[2] This emphasis on neutrality has been criticized by Thomas Haskell, who denies that objectivity requires neutrality. Instead, Haskell emphasizes "detachment," that is, the ability and willingness to consider other viewpoints than one's own. Haskell sees an objective historian as one who "makes a serious effort to bracket his own perspective long enough to enter sympathetically into the thinking of others," adding that "political commitment need not detract

from the writing of history—not even from its objectivity—as long as honesty, detachment, and intelligence are also at work."[3] Ottar Dahl, the late doyen of Norwegian philosophy of history, discusses objectivity and value freedom in his 1986 work *Problemer i historiens teori* (Problems of historical theory), and in line with later thinking he highlights objectivity as an ideal for intersubjectively valid descriptions and explanations.[4] Jörn Rüsen also underlines that within a Rankean paradigm of historiography—and no alternative paradigms were ever subsequently developed—objectivity entails "*intersubjective validity* in two respects: it can be verified by a recourse to experience, and it follows generally accepted patterns of interpretation or historical explanation."[5]

As I see it, there is hardly any doubt that the notion of objectivity that Ranke and his nineteenth-century successors promoted includes neutrality in the sense of "impartiality" as a core component. Gradually, however, only the most theoretically obtuse historians have clung to the belief that historians can "dissolve themselves" and let the sources speak unmediated; likewise, it is generally accepted that things can appear differently when seen from different angles. An ambition of objectivity would today have to be understood as a desire to approach what Novick calls an "ideal observer" who sees the phenomenon from all possible viewpoints.[6] To the degree that a historian's own sympathies or antipathies limit his or her field of vision, objectivity is reduced.

The fall of objectivity entailed that a neutral position was regarded as methodologically unattainable, and any ambitions or illusions of such neutrality were deemed anathema as they served as a form of moral and political (self-)delusion. This applied not least to the notion that historians incarnated such neutrality themselves, or at least that it was conceivable that they could. The new ideal, which virtually became the hegemonic norm in disciplines such as labor history and women's history, was that of engaged historiography, one that still adhered to general requirements to scholarly accountability and verifiability but where historians were aware of their political role. Instead of trying to conceal their political sympathies, historians should admit them, both to themselves and to their readers. In a short while, from the mid-1960s to the mid-1970s, the situation was turned on its head: whereas historians were previously seen as dubious if they articulated a clear, political stance, it was now the *lack* of such a stance that aroused suspicion. It is symptomatic that Ottar Dahl in a retrospective essay he wrote for the 1976 edition of his seminal work *Grunntrekk i historieforskningens metodelære* (Basic features of historical methodology) from the previous decade, felt the need to address the critics of positivism by referring their attention to antipositivist elements in his book. He also took a further cautious step: "If I, in regard to these points, were to make any concessions today to a more wide-ranging 'anti-positivism,' I would perhaps place greater emphasis on, for example, a *positive* connection between contemporarily determined value engagement and topical selection."[7]

This methodological somersault was not particularly endemic to the discipline of history or to Norway. On the contrary, the new ideals within the arts and humanities were only the disciplinary manifestations of a wider turnaround within the philosophy of science, where positivist-inspired notions of objectivity were attacked so vehemently that they fell, at least in the interpretive disciplines within the social sciences and the arts and humanities. But explaining such changes within the philosophy of science itself is an extremely difficult undertaking: How much is due to discipline-specific or wider academic trends, and how much is connected to changes in politics or society more in general? How much is the result of domestic thoughts and actions, and how much can be explained by diffusion or the importing of cultural impulses from abroad? I will hold off on the political angle and first content myself with pointing out two challenges within the philosophy of science that, linked to political and social changes, became more than postwar objectivism could withstand. The first assault came from the American philosopher of science Thomas Kuhn, whose *The Structure of Scientific Revolutions* from 1962 replaced the notion of science as a cumulative, objective activity with a notion of competing and incommensurable paradigms where the same set of facts could be used to support theories that contradicted one another.[8] In hindsight, the impact of Kuhn's work is comparable with a famous scene from the Viking Age Christianization of Norway, when Kolbein the Strong, upon the king's orders, struck open an ancient, hollow wooden idol representing the Norse god Thor, and out poured mice, reptiles, and snakes: likewise, Kuhn's single, mighty stroke obliterated the idol of science and laid bare the low creatures lurking therein, in the guise of all-too-human scientists engaging in at times truly petty quarrels and squabbles with one another.[9] Extending the metaphor, we can say that Kuhn cut science down to size. What is paradoxical here is that Kuhn's new paradigm was largely inconsequential for those natural sciences that his work, in fact, addressed. But in a wider context, especially within the social sciences and the humanities, Kuhn's work played an enormous role given its implicit destabilization of the objectivity that the empirical ideal of science—including within the social sciences and the humanities—rested on.

The second primary assault took place on German soil, with the philosopher Hans-Georg Gadamer playing the lead. With his monumental *Wahrheit und Methode* (Truth and Method) from 1960, Gadamer laid the foundation for modern hermeneutics, at the same time torpedoing the notion of objectivity that characterized positivist-inspired empiricism.[10] An essential point for Gadamer—one that is almost universally accepted by philosophers, not least within the so-called postpositivist philosophy of history of recent years—is that any description contains a subjective element, particularly so that any interpretation of a cultural manifestation (broadly understood) will depend on and vary with the interpreter.[11] We only have to carry this viewpoint a bit to the extreme for it to entail that events do not exist independently of the given viewer/describer. To be sure, things do in fact,

happen, and that is in one sense an objective fact, but how such an event is understood and shaped into a comprehensible *event*, and which context this event is situated within, depends on the viewer/describer and is thus subjective.[12] This leads to certain consequential questions: How can an event be defined independently of the person describing the phenomenon? How can it be contextualized independently of the person doing the contextualizing? If objective descriptions (in the sense of descriptions that are independent of the describer) are impossible, so that phenomena can only be regarded *under a description*—implying that there are several valid descriptions of the exact same phenomenon (with a caveat concerning the problematic nature of the concept of "the exact same phenomenon")—the foundation of objectivity is torn asunder.

The Norwegian spin-off of what has come to be known as the "positivism dispute," which was primarily a disagreement among German postwar sociologists, is above all associated with the philosopher Hans Skjervheim.[13] His most notable work, the essay "Deltakar og tilskodar" (Participant and spectator), was published in 1957 and has attained an iconic standing in the history of social sciences and the arts and humanities in Norway.[14] The primary argument of the essay—and indeed of much of what Skjervheim wrote and spoke about, especially during the 1950s and 1960s—is precisely a rejection of the objective position. For objectivity is impossible, according to Skjervheim, as "involvement is foundational in human existence": subjectivity is a necessity because we cannot opt to refrain from choosing a side. If we try to refrain by insisting on the neutral, removed position of an objective observer, we deceive both ourselves and others. To choose a side—to be participant—is an intrinsic part of being human.[15] We cannot avoid the subjective, and trying to do so is reprehensible—and, presumably, it is even worse to mislead others into thinking that we have succeeded in doing so.

This process, which in many disciplines evolved into something akin to a witch hunt of positivists,[16] represented a sort of farewell to objectivity. It is nonetheless important to emphasize that this did not entail a farewell to scholarly standards for data analysis (in the social sciences) or source criticism (in the historical sciences). When we read past the lofty statements of intent and get down to the nitty-gritty of source criticism and other academic methods, there is little difference between the politically active socialist historian Knut Kjeldstadli's textbook on methodology from 1992 and the avowedly "intellectualist" and objectivity-oriented Ottar Dahl's primer on the subject from 1964.[17] It should be noted, however, that Dahl's text was easier to bring into line with antipositivism than many other works on methodology, something Dahl emphasized in his retrospective essay for the 1976 edition.[18]

The transformation taking place during the 1965–1975 period can be seen in the historians' use of personal pronouns, as the impersonal *we* became supplanted by the highly personal *I*. *We* was the term the nonpresent authorial voice used on those occasions he (or once in a

while she) had to make his presence visible in the text. Otherwise, the author was to be as inconspicuous as possible: history should tell its own story, on the basis of the sources. When the ideal of objectivity disappeared, the author became visible in the first person—the reader had to know the historian in order to assess the history. This process has been taken even further by postmodernist historians, who often involve the reader directly in their personal encounter with the object of study.[19] If the description depends on the describer and the interpretation on the interpreter, history depends on the historian. This does not mean that just anyone can write just anything, for there are still methodological standards in force. But if the interpretation depends on the interpreter, the interpreter's worldview becomes, of course, far more crucial than if the interpreter's (the historian's) role is merely to present a history that is already present in the sources. The stronger the constructive element in historiography, the more significant the historian him- or herself and his or her values and perspectives. Most Norwegians today would be skeptical of a Nazi-influenced historian—and probably more skeptical the closer to the Nazi era the person's field of research was—even were this historian to follow all the rules of proper historiography. It is such a skepticism that comes to the fore when Erling Sandmo writes that the Holocaust revisionists' "political situating [is] such that their factual information is not part of something that would be truth for me."[20] Even though Sandmo's phrasing is idiosyncratic and his concept of truth is contested, his morally and politically justified dismissal of radically dissenting historians' potential to say something worthy of scholarly consideration can be said to represent an antipositivist consensus. We do not listen to geocentrists—at least not when the conversation is about astronomy. Likewise, we do not listen to adherents or sympathizers of totalitarian ideologies when the discussion is about the conflict between democracy and dictatorship. (Analogies can sometimes lead us astray, however; today, a geocentric astronomer would be incapable of adhering to the rules of astronomy, but no corresponding rules prevent totalitarian historians from sticking to a political conviction that would cause them to be ostracized from professional circles.)

The antipositivists of the 1950s and early 1960s formed what in Norway has been dubbed "the Second Front." From the vantage point of cultural conservatism, usually of a Christian-metaphysical bent, they often criticized the positivists' enlightenment philosophy, one they perceived as being overly rationalist and materialist. As such, these antipositivists were not politically radical, quite the contrary.[21] Skjervheim himself, despite a certain positive interest in Marx during the early 1960s (especially his juvenile writings), gradually renounced Marxism and, in particular, Marxism–Leninism.[22] But throughout the 1960s and especially the 1970s, the critique of positivism, in Norway as in other countries, came ever more strongly from the political Left and came to be identified as such. Not least, the Left itself self-identified

with this critique.[23] Though this was not necessarily the inevitable outcome of antipositivism, as a general fact it is indisputable. And although there were exceptions, such as the Conservative historian Francis Sejersted (who continued to use *we* as his personal pronoun of choice), the radical dominance among Norwegian humanists and social scientists as well became both overwhelming and deafening. Historians not only could but *should* be part of the Left, or at worst of the left-leaning faction of the center-right Liberal Party (*Venstre*).

Frode Lindgjerdet's survey of the political sympathies of the authors of various Norwegian textbooks in postwar world history for high school seniors is illustrative, even though its data for the period between 1983 and 1998 only include four textbooks written by a total of six people (including the present author). Three of the authors stated that they were members, and in part highly active members, of the Socialist Left Party, with a fourth author also voting for the party. Everyone who stated their party preference supported the Socialist Left, and three of the four textbooks had at least one Socialist Left author.[24] The Socialist Left Party had originally been founded (under the name of the Socialist People's Party) to oppose a pillar of Norwegian foreign policy during the Cold War—namely, to participate in NATO alongside the democratic, capitalist powers of the West—that all other political parties save the Communist Party agreed on. Despite the low number of authors, I think it gives food for thought that at least half of those who during these years wrote the high school textbooks dealing with the Cold War belonged to the political wing—the decile farthest to the left on the political spectrum—that opposed the foreign policy consensus in Norway. And when those students who had read these history textbooks by Socialist Left authors enrolled at the University of Oslo, the standard textbook they read on twentieth-century Norwegian history was written by none other than Berge Furre, a co-founder of the Socialist People's Party in 1961, a representative to the Storting for the Socialist Left from 1973 to 1977, and chair of the party from 1976 to 1983. The number of Socialist Left historians does not increase considerably with Furre, but their importance does.

Another example of the Left's dominance in recent Norwegian historiography concerns the history of the Norwegian Labour Party. The Labour Party went through a turbulent period during the early 1920s, with majority adherence to the Moscow Theses and a subsequent split, before the party sloughed its revolutionary agenda, merged with the Norwegian Social-Democratic Labour Party, and became a reformist, parliamentarian party, becoming the dominant force in Norwegian politics from 1935 on. What then, is the political stance of the historians who have studied this part of the Labour Party's past? Of the seventeen Norwegian historians included in the syllabus for a course at the University of Oslo on the Labour Party from 1918 to 1940, set up by the young historian David Redvaldsen in 2011,[25] all but one (whose affiliation is uncertain) are avowedly on the

Left. If Redvaldsen's syllabus is roughly representative—as there is good reason to believe, even though several high-profile Marxist or socialist historians of the Labour Party are missing—that means that this turbulent and decisive phase in Labour history has been almost exclusively studied, and then presented, by historians on the Left. Of course, the fact that socialist historians study socialist parties is not something for which they can be faulted.[26] That there are considerable differences in opinion between historians on the Left—not least those on the Far Left—is beyond doubt. But to the degree that values influence not only the *selection* of research topics, which is patently obvious in this case, but also their findings and interpretations—which must be said to be one of the main antipositivist arguments—we must believe that our picture of the Labour Party in the 1920s would have been different had historians from the other side of the political spectrum been involved in drawing it. And unless we assume that the Left enjoys a monopoly on the truth, it follows that this would have enhanced the picture.

It is with a certain reluctance that I have used these head counts to illustrate the overwhelming dominance of leftist historians in Norway.[27] In one sense, the numbers themselves are not all that important; what is essential is that the *discourse* among Norwegian historians is shot through with Marxist and antirightist notions and modes of thinking to such a degree that I would contend—for by nature it is a claim that is hard to substantiate, and impossible in this format—that truly pro-business or rightist historians would struggle to be listened to. In the discourse of leftist Norwegian historians, workers who wanted to work even though the trade union wanted to strike are implicitly disparaged as scabs, regardless of whether these workers were themselves members of the union. One of the primary activities for stimulating growth in modern societies, namely, moving capital to where it is most profitable, is deplored rather than commended. What in a rightist discourse would likely be referred to as vandalism or wanton destruction—for example, hurling objects at a foreign embassy and breaking its windows—is characterized as demonstrations and political activism. Military resistance to communist dictatorships in countries where one does not have economic interests is labeled as imperialism. I am not saying here that one discourse is more correct than the other. Rather, what I am saying is that one discourse belongs to the Left and the other belongs to the Right, and among Norwegian historians it is the leftist discourse that is predominant. The explanation of this discursive dominance-verging-on-hegemony is an intriguing topic but must be left for another occasion. Suffice it here to note that most Danish historians share the left-leaning political sympathies of their Norwegian colleagues, as do American professors.[28] In countries such as Sweden or Germany, by contrast, conservative historians are both more numerous and more vociferous than in Norway and Denmark.

II. Diplomatic History in Norway and Denmark

As mentioned above, the rejection of objectivity as an ideal entails that the researcher's worldview becomes more important when assessing his or her findings. The researcher's increased personal importance has a potentially explosive effect, not least politically. The Left's near-monopoly within the critique of positivism in its dominant speaking positions within the social sciences and the humanities—and within cultural life and the media—has led to this explosive force being left undetonated in Norway. In Denmark, by contrast, we see full well what may happen when the objectivity ideal falls by the wayside, and not least do we see the potential ramifications of what may happen when the Left loses its interpretive hegemony. The arena is foreign and security policy, specifically Cold War history.

Norwegian foreign and security policy is typified by a broad consensus on most issues, from membership in NATO to a limited military engagement in Afghanistan. There are nuances from one party to the next, but there is no schism. This political harmony is mirrored by the tranquility among Norwegian diplomatic historians, who exhibit "extreme coherence."[29] The disciplinary consensus includes both the methodology, which can be characterized as a sort of modernized Ranke with a heavy emphasis on archival material related to foreign policy, and the overall approach, which can be described as a sort of realism in which power relations dominate the analysis (as opposed to research on domestic policy, the economy, or social conditions, or to discourse analysis for that matter). Another aspect of the consensus is the near-total absence of discussions and disputes between differing viewpoints, an absence that I have explained by the diplomatic historians' pronounced tendency to leave the fields and archives of their colleagues in peace.[30]

Likewise, most of the Danish studies on the Cold War emphasize archival material on foreign policy, analyze the sources thoroughly, and use a so-called realistic approach that emphasizes power relations. To a certain extent—though by no means as much as in Norway—there has been agreement on the broad outline. But in one regard the situation in Denmark stands considerably apart from that in Norway: Danish Cold War history has figured prominently in the wide-ranging culture war between the Right and the Left in Danish politics. As a result, a schism has arisen between the center–left mainstream majority of Danish Cold War historians and what we may call a rightist–revisionist minority following the aforementioned Bent Jensen at the University of Southern Denmark in Odense. Using his proficiency in Russian, Jensen has published a number of major and minor works about Russia/the Soviet Union and about the relationship between Denmark and the Soviet Union both before and during the Cold War.[31] In particular, he has documented—and criticized—how Danish intellectuals and politicians on the Left let their enthusiasm for socialist ideals and the Russian Revolution blind them to the grim aspects of Soviet communism

and how others, who politically were farther removed from the communists, allowed their fear of Soviet military power to paralyze them so that they did not dare ally themselves as closely with the United States as the situation required. Mainstream historians such as Nikolaj Petersen and Thorsten Borring Olesen have praised Jensen for pointing out what in hindsight (and for many observers at the time) turned out to be the self-deception of the pro-Soviet brigade and the exaggerated fear of the bridge builders, that is, those who sought to reassure rather than deter Moscow. At the same time, however, these historians have reproved Jensen for his lackadaisical use of the sources, slapdash citations, and unwillingness to try to see things from the point of view of those he denounces.[32]

Both numerically and in virtually all other respects, the mainstream majority is far superior to Jensen. Nonetheless, the dispute between them remains interesting, both politically and historiographically.

III. The Culture War in Danish Historiography

When Bent Jensen's tussle with the Danish guild of diplomatic historians is more exciting, politically speaking, than could be assumed from the latter's numerical supremacy, it is because Jensen had a formidable backer in the newspaper *Jyllands-Posten* as well as powerful political allies in both the Liberal Party, at the time a member of Denmark's coalition government, and the right-wing populist Danish People's Party. The liberal-conservative *Jyllands-Posten*, Denmark's largest newspaper and the one that in 2005 published the cartoons of the prophet Muhammad, gave Jensen—political editor 1989–91 and a member of the board of the foundation that owns the paper—a seemingly unlimited amount of column space. The right-of-center Liberals governed Denmark from 2001 to 2011 in a coalition with the Conservatives, with parliamentary support from the Danish People's Party. Jensen's newspaper articles and criticism of Danish mainstream Cold War historiography were an important part of the lengthy cultural battle that Anders Fogh Rasmussen, Liberal prime minister from 2001 to 2009 and subsequently secretary general of NATO, led against the Left in Danish politics. In an article in *Historisk Tidsskrift*, Denmark's main history journal, Nikolaj Petersen writes that Fogh Rasmussen, from the time he assumed leadership of the Liberal Party in 1998, aimed to establish a center–right hegemony based on a platform that was centrist in regard to the redistribution of wealth and markedly conservative in regard to social values, which meant promoting such liberal values as democracy, freedom, and human rights. According to Petersen, "the success of this project rested on permanently transforming the public's beliefs by means of the culture war that the Fogh Rasmussen cabinet declared early on against cultural radicalism, relativism, and multiculturalism."[33] Fogh Rasmussen's platform entailed an activist foreign policy, working closely with the United States, the foremost exponent of such liberal values on the global stage, in a

common struggle against antiliberal forces. This explains Denmark's exceptional willingness to support American warfare in not only Afghanistan but also Iraq. In regard to foreign policy, the roots of this new Liberal platform extended back to the 1980s, when then-foreign minister and Liberal Party leader Uffe Elleman-Jensen positioned the party to the right of the Conservatives by supporting Reagan's foreign policy and resenting the so-called footnote policy—whereby Danish dissent to NATO strategy was duly expressed in footnotes in the alliance's documents—which the parliamentary majority dictated to the government. Against the foreign minister's and the government's wishes, the majority of the Folketing showed interest in making the Nordic countries a nuclear weapon–free zone; they were opposed to allowing (American) naval ships with nuclear weapons on board to dock in Danish ports; and forced the government to make, as the only alliance member, a formal reservation against NATO's Double-Track Decision of deploying medium-range ballistic missiles unless the Soviet Union withdrew similar missiles that targeted Western Europe. Even though the enemy twenty years later was no longer totalitarian communism but radical Islamism, the chief ally remained the same. And the strategy was still to actively stand up, with U.S. assistance—and with assistance to the United States—against those who threatened liberal values.

It was during this culture war against those who threaten liberal values—or, more precisely, against those who either deliberately or as the unintended consequence of originally well-meant actions help those who threaten liberal values—that Bent Jensen and his coterie found common ground with Fogh Rasmussen's Liberal Party (and the Danish People's Party) and provided mutual, albeit dissimilar, support. The support from the majority of the Folketing to Jensen was entirely concrete, coming as it did in the guise of allocations for establishing a Centre for Cold War Studies (Center for Koldkrigsforskning, CFKF). The center received a three-year appropriation of 10 million Danish kroner in the government budget for 2006, but it was first in the autumn of 2007 that it was up and running. The center's mandate was to "carry out free and wide-ranging research and dissemination in regard to Denmark during the Cold War," which would ultimately result in a "clear and informative publication." The latter comment was an implicit dig at a 2005 research report from the Danish Institute for International Studies (DIIS) titled "Danmark under den kolde krig" (Denmark during the Cold War), whose four volumes and 2,500 pages of, at times, jargon-heavy text were a tough slog for laypeople and parliamentarians alike and to which we shall return in a moment.[34] Explicitly taking the DIIS report "into consideration"—though perhaps rather being spurred into action by dissatisfaction with it—the center was to "further bring to light both Eastern Bloc influence on Danish decision-makers and the military threats aimed at Denmark and the Baltic Sea region." "Of particular interest" was "the era's clash of ideas in politics, in the media, and in the public debate, including the culture debate

in its widest sense—in short, the shaping of opinion and its domestic and foreign sources of inspiration."[35] The Right in Danish politics, as led by Fogh Rasmussen and the Danish People's Party's Jesper Langballe, did not believe that mainstream historians would provide them with such a historical analysis. By founding a separate Centre for Cold War Studies, one that hopefully would be led by Bent Jensen—and by locating this center under the Danish Institute of Military Studies, so as to prevent it from becoming part of the university system—they believed it was a sure bet that they would get what they were looking for, given Jensen's previous books and not least his newspaper articles. Even though Fogh Rasmussen and his allies were not allowed to go all the way and appoint Jensen to head the new center, no one who followed the debate leading up to the center's founding was in doubt that he was the man both the prime minister and the Danish People's Party wanted. Since no academic heavyweights would apply to become the director of a center with such a rightist genesis and a mandate that so clearly was aimed at hunting down fellow travelers, it was impossible for the center's board—appointed by the minister of defense and consisting of scholars led by Dr. Knud Larsen, the former secretary general of the Ministry of Research—to overlook Jensen.

Jensen's Centre for Cold War Studies was the culmination of a process that began as far back as in 1995, when Poul Nyrup Rasmussen's Social Democratic government asked the Danish Institute of International Affairs (DUPI, which later merged to become part of DIIS) for a report on the role Thule Air Base played in conjunction with U.S. nuclear bombers flying over Greenland from 1945 to 1968.[36] The call for such a report was highly controversial, but all parties seemed to find the report itself acceptable when it was finalized two years later. Similarly, the decision in 1999 to appoint the so-called PET Commission, whose mandate was to investigate the Danish Security and Intelligence Service (PET) for its monitoring of the Left in particular during the Cold War—including the leftist activities that had prompted the surveillance efforts in the first place—as well as the legality of the PET's intelligence gathering after 1968, was a highly contentious one.[37] The same applies with even greater force to the decision in 2000 (with a second go-around and subsequent widening of the mandate in 2002, after the new Liberal–Conservative government had assumed power) to request DUPI, and subsequently DIIS, for a report on Denmark during the Cold War.[38] The point of contention concerned how much the researchers were to investigate the connections between the Soviet bloc and what Fogh Rasmussen and others on the Right considered to be Danish fellow travelers: in other words, what eventually became the very mandate of the Centre for Cold War Studies.

The DIIS report was published in 2005 and was vehemently criticized by the Right.[39] One thing was its unwieldy scope and abstruse language, derided by Langballe as "a babble of pseudo-scientific terms."[40] Bent Jensen's main objection concerned another point entirely:

> The fundamental weakness of the DIIS report is the lack of a realistic description of the Soviet Union as a system and of the Soviet party–military leadership as what it in fact was, namely, a gathering of gloomy, claustrophobic men whose outlandish beliefs and worldviews were the very motor of the Cold War. This serious deficiency turns all two thousand pages of the report into a highly academic writing exercise that is alarmingly out of touch with reality.
>
> The Cold War was a deadly earnest conflict, sparked off by totalitarian police-states led by the Soviet Union. These were systems that lived on and could not exist without lying to and oppressing their peoples and without hatred and aggression toward the world at large. They would have collapsed had the populace been able to live a life in freedom and had the borders been opened, so people could see with their own eyes what conditions were like in democratic–capitalist societies. As long as free and open societies existed, such societies represented—as seen through the eyes of the police-states—a fatal threat.
>
> The Cold War ended because these totalitarian systems crumpled under the weight of their own absurdity and impotence. The Soviet command economy was a war economy, where the needs of the military always came first. . . .
>
> These inhuman and intimidating systems ultimately collapsed because the democratic societies did not let themselves be overrun, but stood firm in their defense against this threat.[41]

Bent Jensen was given his own research center. In return, the Liberal Party (and the Danish People's Party) received legitimacy for their culture war, in the form of ammunition for the attack by Fogh Rasmussen and his like-minded associates on those who in their view had not stood up to the totalitarian threat during the Cold War, or who had done so insufficiently, and who had instead advocated or at least been open to the idea of adapting to the enemy's wishes and demands. Fogh Rasmussen and his culture war allies saw a parallel between those who sought to accommodate communism during the Cold War and those who were accommodating during the war on Islamist terror: this was in a sense not entirely unreasonable, especially since their criticism concerned some of the same people and organizations.[42] The Danish Right therefore wanted historical research that reviewed the Left's Cold War "accommodation," what Langballe during the Folketing debate in March 2006 on founding a center for Cold War research called "the intellectual and political fascination for a totalitarian ideology that, next to Nazi-Germany, had established the most heinous terror-state in world history." For Langballe, the relevance of such a review was obvious:

> Gaining an overview of the era's accommodation can, among things, put today's accommodation in perspective. Accommodating the terrifying totalitarian ideology of the present age, Islamism, causes quite

> a few of today's intellectuals, artists, and politicians to fail to defend freedom of speech and the free criticism of religion. We experienced accommodation to totalitarianism during the 1930s, we experienced it during the footnote era, and we experience it today—and this is a history that shall be told. For the choice between freedom and totalitarianism always turns up in new guises.[43]

As Fogh Rasmussen put it during the same debate, "the Cold War was a battle of values, an ideological conflict between different systems, between dictatorship and democracy, between totalitarian ideologies and free thought, between market economy and plan economy—and in this conflict between democracy and dictatorship one cannot remain neutral."[44] By alluding to how extensive the accommodating tendency was during the Cold War—how well-intentioned, naïve, and frightened Danish socialists, social democrats, and radical politicians, cultural figures, and media representatives served the interests of the Soviet dictatorship by arguing in favor of compromising with Moscow rather than, with help from the United States, standing firm against the demands from the enemies of freedom—Bent Jensen implicitly supported the Liberal-led government's arguments against accommodating the demands from Islamists, who, for example, stated that they were offended by the Muhammad cartoons. To be sure, such arguing by way of analogy is problematic from a logical standpoint.[45] But it can certainly be politically effective.

IV. Historiography without Judgments?

Enter the mainstream historians, as represented by three of Denmark's leading experts on the Cold War. First, there is Thorsten Borring Olesen, who, in reference to commissioned Cold War history such as the aforementioned DIIS report, wrote the following in an English-language article for the 2006 DIIS yearbook:

> What this research has not clarified, nor should it try to, is which security positions and policies were morally right. Historians should be encouraged to engage in such debates, but . . . the debate over which were the "right" or "correct" Cold War policies cannot be solved unequivocally or conclusively by historians. They can merely qualify the debate by marking out the playing field and analysing . . . the rules and choreography of the game.[46]

Second, there is Nikolaj Petersen, in a 2009 article on the debate surrounding the Centre for Cold War Studies:

> The yardstick for a program within the discipline of history is different [from one for a political ideology], namely, its ability to provide an

> intersubjectively valid interpretation of history, that is, an interpretation that, by adhering to the commonly acknowledged rules and norms of scholarly inquiry, is also meaningful for peers with different values and worldviews.[47]

The third and final historian presented here is Poul Villaume, who in a 2009 article opposing a recent dissertation on Stalin also weighed in on the historian's task:

> All serious historical research—whether consciously or unconsciously, explicitly or implicitly—is governed by a certain moral framework. . . . But if *moralizing* historiography should begin to overshadow or completely usurp and supplant the search for complex causalities and explanations, then we have gone astray. Historiography must not only be a courtroom where the defendant is found guilty or acquitted, and where one-dimensional verdicts are rendered on what is right and wrong, good and evil. This is not to say that the historian must be "neutral" or "value-neutral." The historian's attempt to *understand* the past and its actors and their actions must not be confused with attempts from his or her side to *defend* the past's actors and their actions. It is a matter, albeit in different ways, of striking a balance between analyzing, understanding, and judging. Such a mode of writing history *opens* the past and an understanding for the open choices and uncertainties about the future that the actors of the past confronted. They did not have the luxury that we have today of knowing how things actually turned out as the outcome of the decisions that were made.
>
> The approach to writing history outlined here must presumably also apply to phenomena such as Stalinism, the Stalinist system, and Stalin himself.[48]

It is not entirely clear what these three historians are contending. I am uncertain whether I understand Borring Olesen's sports metaphor: What does it actually mean to mark out the playing field, to analyze the rules of the game and its choreography? And if we interpret it literally, Petersen's stipulation that your interpretations must be meaningful to peers whose values differ from your own is not particularly stringent—the question is rather what is otherwise meant by "meaningful." Both Borring Olesen and Petersen are primarily concerned with warning against a type of historical research where the conclusions have been predetermined by the politicians who commissioned the research. As Petersen writes, "the problem for Cold War research occurs in particular when the expectation is that the research will proclaim what was politically right or wrong during the Cold War and that it will stick to a certain political interpretation of this era."[49] Villaume seems to be open to the idea that the historian can make value judgments ("Historiography must not *only* be a courtroom"), but not if it is

at the expense of those being studied—including Stalin. In total, the preceding statements seem to lean more toward an objective historiography than toward the type of engaged historiography on which the critics of positivism insisted. The principal view seems to be that the historians' mission is to create presentations that, because they have adhered to scholarly norms and rules, can be regarded as true by (and hence be meaningful for?) readers with highly diverse political and moral viewpoints.

Opposed to these views stands Bent Jensen. Not only does he assert that there is "not necessarily a state of tension or contradiction between morality and historiography," he even claims that "it is impossible as a historian to engage in topics such as Bolshevism, Nazism, and the Second World War without taking a moral stance in regard to these phenomena. In essence, what divided researchers in these fields was not differences in methodology, but differences in moral outlook—in other words, fundamental differences in how they viewed these phenomena."[50]

Appeals to a higher authority—that is, historians who are even more well known and respected than the Danish ones—do not conclusively answer what place values should have within the discipline of history. In an article published a few years prior to his death in 2004, Wolfgang Mommsen discusses the very question of "Moral Commitment and Scholarly Detachment." Horrific events, such as the Armenian genocide of the First World War, the Stalinist terror of the 1930s, or the Holocaust during the Second World War, must not only be described and understood, he contends; the historian must also "demarcate the moral responsibility of the actors involved—including those who share in the guilt because of their passivity." At the same time, Mommsen warns against sliding into "the bottomless swamp of subjectivism." His reservation against historians acting as moral judges is not unlike Villaume's and is thus similarly ambiguous:

> The historian is not a judge, who pronounces verdicts on the basis of fixed, existing rules, regardless of his own feelings in the matter, but a scholar, guided by ethical principles of responsibility when approaching his topics. This imposes on him a duty to come to terms with the ethical dimension of the events he represents. It is not his task to offer a moral evaluation of the case he describes, but rather to make sure that the case is presented to the public, and brought into public discourse, without neglecting the aspect of moral responsibility involved.[51]

Most historians will likely feel a stronger affinity with Petersen's requirement of intersubjectivity—and hence a disinclination to make political and moral assessments—than with Fogh Rasmussen's insistence that they shall "stand on the side of democracy" and thereby renounce for example the footnote policy of the 1980s, as such a policy "tended to be in line with the Soviet Union's wishes, and the footnotes represented a lack of solidarity with our NATO partners and allies."[52] Such a preference for Petersen's

ideals over Fogh Rasmussen's would not be surprising, given that most people on the Left (which is where, as mentioned earlier, most historians are to be found) have a fundamental ideological sympathy for socialism, despite recognizing the many mistakes and deficiencies of socialist regimes. But would the same historians be as steadfast in maintaining the requirement of intersubjectivity across differing values and outlooks if the topic of study is not Cold War policies vis-à-vis the Soviet Union, but Western policies vis-à-vis Nazi-Germany before and during the Second World War? Is it an acceptable requirement that both Nazis and anti-Nazis would have to hold the presentation to be true and meaningful? Would a history of the Second World War written by avowed Nazis or Nazi sympathizers, or by historians with a Nazi past they have not categorically abjured, be acceptable? If the answer is no, does not that then mean that there is a double standard at work here, with one for (former) communists and communist sympathizers and one for (former) Nazis and Nazi sympathizers?[53]

Two lines of defense against such an accusation spring to mind. The first is to say that Hitler's Nazi regime was so monstrous that comparing the Soviet Union with the Third Reich is unreasonable. There is, however, no doubt that at least Fogh Rasmussen and the Right in Denmark believe that the Nazis and the communists are comparable. Bent Jensen shares this view, as when he writes, in a book on the relationship between Denmark and the Soviet Union: "The Soviet regime was an illegitimate regime and responsible for appalling crimes perpetrated throughout the vast territory it controlled. Under Stalin it became an outright kleptocracy, one that in no way bowed to Hitler's regime in that respect."[54] Concerning this particular point, Jensen seems to be expressing something close to a consensus opinion among historians.[55] With a few exceptions, historical works on the Stalin era in particular, not least after the communist regime fell in the 1990s and the archives were opened up, seem fairly concordant in their descriptions of the regime's atrocities and Stalin's brutality, even though opinions are divided on whether the regime can be characterized as totalitarian and on how much the excesses were systemic or due to Stalin himself.[56] In any event, claiming that that Hitler/Nazism was worse than Stalin/communism entails making a political–moral judgment. Hence, this first line of defense is based on the very type of judgment that, it is argued, historians should not engage in, and is thus dispelled by what Skjervheim and his fellow philosophers call self-referential inconsistency.

The second line of defense is to say that the historian's task is not to make judgments based on today's retrospective knowledge and norms, but to demonstrate how the historical actors used their own knowledge and norms to assess the situation at hand. This ultimately implies that historians must write just as empathetically about Hitler and Stalin as about all other actors—otherwise, we are using our own norms to judge them—and can only criticize these actors for a lack of instrumental rationality, and not for their values. In real life, however, any such neutral descriptions of Hitler's

death camps or Stalin's mass murder would presumably strike most people as conspicuously odd (although, of course, such subjective impressions cannot be a decisive methodological benchmark). To take a less extreme example, this second line of defense implies that Neville Chamberlain's policy of appeasement must be treated with as much understanding and as little condemnation as Churchill's opposition to trying to reach a peaceful agreement with Hitler. Likewise, Denmark's foreign minister from 1945 to 1950, Gustav Rasmussen—one of the principal scapegoats in Bent Jensen's criticism of Danish politicians' exaggerated fear of Soviet clout—would have to be described with as much good will as the more Western-oriented and, in Jensen's estimation, more prescient prime minister from 1945 to 1947, the Liberal leader Knud Kristensen.[57] It seems likely that many observers would believe this is possible, and perhaps even desirable. But as a historiographical principle it is fairly limited if it can only encompass actors that the historian fundamentally agrees with, that is, actors who have the same political outlook as oneself (liberal democrats, in this case).

In Denmark, the Right has explicitly denied such a historicist limitation of which value judgments are legitimate. At a speech commemorating the sixtieth anniversary of the liberation of Denmark from Nazi Germany, Fogh Rasmussen rejected accusations that he was judging people in 20/20 hindsight at a safe distance from the events—in this case, the question of whether to cooperate with or resist the Nazis during the occupation—by referring to those who had in fact made the right choices at the time, concluding that "thus, I can naturally enough also permit myself today to support their choices and their mindset on the terms of the present."[58] Petersen comments that "it is, among other things, such judgments on the terms of the present that separate political ideology from historiography."[59] Many, and perhaps indeed most, historians would concur. Erland Kolding Nielsen, director general of the Royal Library in Copenhagen, seems to represent the mainstream line when he writes that what he perceives as a demand from Bent Jensen's ally, the historian and writer Bent Blüdnikow, namely, "that those who write articles in a historical lexicon must condemn the past on the values of the present as projected back onto the past, is clearly in conflict with all of the discipline's norms of epistemology and attempted objectivity, or merely of reasonableness."[60] Among those who disagree are R. G. Collingwood, who insists that the historian "not only re-enacts past thought, he re-enacts it in the context of his own knowledge and therefore, in re-enacting it, criticizes it, forms his own judgement of its value."[61] I would also contend that Petersen's restrictive approach is somewhat at odds with modern hermeneutics, which precisely emphasizes how you can never detach yourself from your own horizon of understanding—including your norms—when trying to understand other people. That you must start an inquiry with your own prejudices, in Gadamer's hermeneutical sense, can, of course, become a convenient excuse for not expanding this horizon when encountering historical actors with another horizon. But to avoid becoming a mere accumulation of

curiosities, all inquiries, including historical ones, must take as their starting point their utility for the present day. Or as Langballe put it during a parliamentary debate, "history shall above all teach us to understand the present in which we live."[62] Those who want a more complex formulation of this idea can consult Heidegger.[63]

Incidentally, one of Heidegger's (and Gadamer's) fundamental insights is that we cannot have a disinterested, neutral relationship to what we are studying. This is what Skjervheim also emphasized so strongly in a normative way in his seminal 1957 essay, when he wrote, alluding to Søren Kierkegaard, that "we do not exist outside of time. . . . Existentially, it is precisely the questions of good and bad, of right and wrong, that are central, and to eliminate them by way of abstraction is to cheat in existence. One shirks out of making a choice." Seen from the outside, existence can seem to be a necessary or natural development, "but seen from the inside it is a continuous series of choices. Therefore, if one sees it from the outside—that is, objectively—one muddles the entire situation, for then one eliminates all the either–or choices. It is precisely therefore the subject's mission to 'become subjective', to make a choice oneself." It is this choice between right and wrong that Fogh Rasmussen insisted that we must make—and that the Danish mainstream historians waive qua historians. But in that case they become what Skjervheim, following Kierkegaard, condemns as "the abstract thinkers who have forgotten what it means to exist, and who therefore exist 'phantasmally,'" who "are on the verge of ceasing to be Human."[64]

In view of the Left's virtual hegemony among critics of positivism, it is undeniably paradoxical and ironic that it is center–left historians in Denmark who sought refuge in what Kierkegaard and Skjervheim call a phantasmal existence, while the Right, as represented by Fogh Rasmussen, followed the philosophers' exhortations to be whole human beings. It seems likely that this was less the result of a sea change in the philosophy of history than that the political winds have changed direction. Since the Left has been put on the defensive in the Danish debate on foreign policy, where Cold War history is used in the culture war as an argument for liberal–democratic values, center–left historians sought refuge in a phantasmal world governed by objective norms of methodology, where they would not be required to take political or moral stands. They were not left in peace there, however, but were pursued by Jensen and other like-minded individuals such as Blüdnikow, who in a comment to Nikolaj Petersen's earlier-quoted 2009 article claims that "it is a massive intellectual problem in Denmark that so many historians believe that their own research is value-neutral, while that of their opponents is politically or morally infested." Historians such as Jensen and Blüdnikow, on the other hand, "recognize that history is not value-neutral."[65]

In an op-ed piece attacking Poul Villaume, his favorite foe among university historians, Blüdnikow writes that he considers it "decisively important that history students at our universities know that historiography is a

subjective business."[66] During the 1970s, Villaume—award-winning professor in Cold War history at the University of Copenhagen, doctor of history on the back of his monumental book *Allieret med forbehold* (Allied with reservations), coauthor with Borring Olesen of the volume on 1945 to 1972 in a seven-volume history of Danish foreign policy, and coeditor of a lexicon on the Cold War and Denmark—was active in the Communist Workers League (Marxists–Leninists).[67] In the 1980s he was among the founders of the Danish chapter of the European Nuclear Disarmament movement. The readers of *Ulve, får og vogtere* (Wolves, sheep, and shepherds), the work that came out of Jensen's center, are reminded of Villaume's Stalinist past.[68] Moreover, Villaume's research, in particular, his criticism of Jensen's own research, is criticized on a website set up in conjunction with the publication, presenting sources and discussing literature.[69] For our purposes, it is the philosophy of history in play here that is interesting. Blüdnikow encourages his readers to "of course critically assess what Bent Jensen or I myself write." His wider point, however, is to deny Villaume and other old radicals legitimacy in the eyes of those readers whose worldview has not been informed by seventies Marxism: "And it is crucial that readers know that the books that their radical baby boomer teachers write are very much shot through with ideology."[70]

Villaume responds by ensuring the newspaper's readers that "neither I myself nor other serious historians I know have ever said or believed that objective historiography or research exists. Knowing that the findings a historian manages to unearth are colored by his or her own views and background, and so forth, is History 101."[71] How, then, does Villaume intend to prevent the exposition of Danish Cold War history from becoming a shouting match between various political wings, where the readers only listen to people who agree with them politically or to those who shout the loudest, that is, those who have the best access to the op-ed pages or media coverage? His answer is to appeal to academic methodology: "The daily challenge is to keep this subjective background in check, and in this area a stringent source analysis and well-balanced methodology are the most important tools."[72] The problem is that source analysis and "well-balanced methodology" (however that is to be understood) are primarily suited to determine facts and interpretations of a limited range, and are ill suited to decide which general expositions provide the most accurate picture of, for example, Denmark during the Cold War.

The fate of Søren Hein Rasmussen, nominated by Aarhus University as one of the members of the PET Commission in 1999, is an extreme example of the consequence of erasing the line between history and political values. Hein Rasmussen had been a member of the Danish Communist Party from 1977 to 1989 and had attended a "Communist Party school" in Moscow in 1983–1984. When several politicians expressed their opposition to a person with such a background sitting on the commission, the Ministry of Justice declared him unfit. Hein Rasmussen and his fellow historians

referred to his academic production, which the politicians had neither read nor saw a need to read: since the historian had been disqualified because of his political past, it meant less that no one doubted that he lived up to academic standards.[73] The question I ask here is whether Hein Rasmussen's colleagues would have clung as strongly to the notion that political background should be irrelevant had he been a neo-Nazi.

V. Concluding Remarks

Two concluding remarks are called for. The first is in reference to the statement above where I refer to the Soviet regime's "atrocities" and to Stalin's "brutality" and "excesses." The sentence, which is undeniably value-laden, illustrates just how difficult it is to write neutrally about phenomena that arouse strong moral feelings in us. Try to reformulate the sentence by using language free from inherent value judgments: I, for my part, find myself unable to do so without using a plethora of additional words or articulating myself in a way that seems artificial and awkward. Perhaps this is what Mommsen is thinking of when he says that historians are obliged to relate to the ethical dimension of the events they cite and therefore cannot neglect the element of moral responsibility involved. Actions that are far beyond the pale of what is considered morally acceptable behavior more or less force us to use morally charged language in describing them—and in doing so, we must base ourselves on our own standards of morally acceptable behavior and not on those of the people we are studying. The moment that we as historians want to communicate with readers whose moral compass differs from ours, however, such a morally charged language can be a hinder to "cross-ethical" communication. We are then relegated to being historians for those whose moral compass coincides with our own. I see no way out of this dilemma. By striving to describe phenomena as "clinically" as possible—that is, so factually and neutrally that all who have seen the sources can accept our description as accurate and not misleading—we increase our potential to communicate with readers whose moral compass differs from our own. But there are limits to how much can be covered by such a clinical description. In particular, I believe that more extensive interpretations can hardly be written in such a clinical fashion.

This brings me to my second remark. Part of the critique of positivism was based on, and led to, a notion that pure facts do not exist or, if you prefer, that all data are contingent upon theories and hypotheses and that, consequently, one cannot use data or facts in order to establish hypotheses, much less to test them (given that the data one uses to test a hypothesis are, in a sense, themselves the product of the hypothesis). This notion, which is certainly different from and more wide-ranging than Skjervheim's existentialist critique of positivism, is usually supported by referring to Kuhn and—for those with an above-average interest in philosophy—W. V. Quine.[74] It entails an epistemological holism that insists that descriptions (which build

directly on data or facts) are not more firmly grounded than more extensive interpretations or hypotheses (which build on simpler descriptions). For our purposes, the crucial point is the relationship between descriptions and interpretations. If we agree that data are contingent upon theories and hypotheses—in the sense that the truth of descriptions cannot be detached from the interpretive framework encompassing the description—this entails that every description, and indeed every fact, is contingent upon a corresponding interpretation or hypothesis. In our context the consequence is that the stronger one insists on this holism, the more important becomes the interpreter's—in our case the historian's—political standpoint, since this assumedly must have informed the scholar's interpretive framework, and thereby his or her descriptions. The critique of positivism has thereby brought us to E.H. Carr's advice to "study the historian before you begin to study the facts," because all in all, "the historian will get the kind of facts he wants. History means interpretation."[75] What is paradoxical here is that with such an attitude, nothing can stop the Right in Denmark from having a respected, but (formerly) communist historian such as Søren Hein Rasmussen disqualified from the PET Commission. If we instead take a more empirical starting point, one where data and descriptions are in a sense seen as more primary than hypotheses and interpretations, it will be historiographical craftsmanship rather than political outlook that is relevant for assessing who is to curate the country's past. The breakdown in communication that was seen in the Danish Cold War debate illustrates how easily things go astray—in the sense of how helpless we become when dealing with people with dissenting views who deny us legitimacy as interlocutors—when the free exchange of professional opinions is reserved for those who agree with us.

Acknowledgments

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Notes

1 The fact that positivism criticism supplanted the ideal of objectivity does not mean that vestiges of objectivism do not live on. In a recent Norwegian doctoral dissertation, the introduction ends as follows:

> It is the clear contention of this author that the Soviet authorities, during the period covered by the present exposition, committed criminal and morally repugnant atrocities against segments of its own population as well as against foreigners residing in the Soviet Union, including many communists.

> Having said that, it is a goal for this exposition to avoid a censorious historiography—or an apologetic one, for that matter—and instead try to provide an account and analysis that is as historically objective as possible and based on an empirical reconstruction of the contemporary context.

Rønning, "Stalins elever," 33. All translations from the Scandinavian languages are my own.

2 Novick, *That Noble Dream*, 2.
3 Haskell, "Objectivity Is Not Neutrality," 160 and 167.
4 Dahl, *Problemer i historiens teori*, 61–96, esp. 68.
5 Rüsen, "Historical Objectivity," 58, italics in the original.
6 Novick, *That Noble Dream*, 478.
7 Dahl, *Grunntrekk*, 4–5.
8 Kuhn, *Structure of Scientific Revolutions*. How competing paradigms can be in conflict with one another when they are incommensurable was a question that preoccupied Kuhn for the rest of his life. See Kuhn, *The Road since "Structure."*
9 See, for example, Friedman, *Politics of Excellence*.
10 Gadamer, *Truth and Method*.
11 Ankersmit et al., "Philosophy of History."
12 Roth, "Narrative Explanations." Roth's views are presented in greater detail, and critiqued, in Chapter 4.
13 For Skjervheim's standing in what we somewhat imprecisely may call Norwegian critical philosophy, see Slaattelid, *Regime under kritikk*, and Sørbø, *Hans Skjervheim*.
14 Skjervheim, "Deltakar og tilskodar." The main views had been anticipated in Skjervheim's 1956 thesis for the degree of *magister artium* at the University of Oslo, subsequently published as *Objectivism and the Study of Man*.
15 Quotation from Skjervheim, "Deltakar og tilskodar," 64. See also Skjervheim, *Ideologianalyse, dialektikk, sosiologi*, 176–177.
16 For an example, see Helsvig, "Pedagogikkens grenser," esp. 213–287.
17 Kjeldstadli, *Fortida er ikke hva den en gang var*, 302–303; Dahl, *Grunntrekk*, 131–132.
18 See Fure, "Kritisk empirisme."
19 See for example Sandmo, *Voldssamfunnets undergang*, 17; Sandmo, "Michel Foucault som maktteoretiker," esp. 79.
20 Sandmo, "Mer og mindre sannhet," 397; for an explication, see Sandmo, "Historie som fasit," esp. 418.
21 Slagstad, *De nasjonale strateger*, 392–396; Sørbø, *Hans Skjervheim*, 70–71 and 99.
22 For Skjervheim's cautious openness to Marxism, see his 1963 lecture "Sosialismen: Filosofi eller samfunnsanalyse?" For his gradually ever more categorical opposition, especially against what he perceived as dogmatic Marxism(–Leninism) and Marx's theory of value, see the two articles he wrote especially for a 1976 collection of his essays: "Utgangspunktet for den politiske økonomi hjå Karl Marx," and "Tillegg: Karl Marx i den aktuelle debatt"; and Sørbø, *Hans Skjervheim*, 201–246.
23 For a couple of examples from the discipline of history, see Berntsen, "Objektivitet og historie"; Egge, "Empirisme eller marxisme."
24 Lindgjerdet, "Den kalde krigen," ch. 4, cf. ch. 5, appendix, 246–273. My page numbers refer to the PDF version and diverge from the differently formatted, printed version.
25 Redvaldsen is the author of the comparative *Labour Party in Britain and Norway*.
26 It must be noted, however, that of the four authors who wrote the official history of the Conservative Party from 1884 to 2005, only Francis Sejersted was party

member. Perhaps the party was unable to find any other merited historians in its ranks.

27 It should not have been necessary, but Tjelmeland, "Objektivitet, vitskap og politikk," shows how great the potential for self-delusion is in this group.

28 Gross, *Why Are Professors Liberal*.

29 Førland, "International History in Norway," 167.

30 Førland, "International History in Norway," 167. There has been a single dispute since 1995, albeit about a relatively tangential part of the historiography, and without involving the broader spectrum of diplomatic historians: Agøy, "Skal Mars tvinge Clio?"; Berg, "Svar til Agøy"; Riste, "Agøys forsvarshistoriske konspirasjonsteoriar"; Agøy, "Om brutte eder"; Agøy, "Bergtatt og filleristet." Recently, Hallvard Tjelmeland has critically scrutinized the "realism"-dominated Cold War historiography in Norway and suggested that Norwegian prime minister Einar Gerhardsen's somewhat more Soviet-friendly line should be brought more to the fore. See Tjelmeland, "Ein norsk Sonderweg"; Tjelmeland, "En utenrikspolitisk amatør?"

31 See, for example, Jensen, *Købmænd og kommissærer*; Jensen, *Sovjetunionen*; Jensen, *Tryk og tilpasning*; Jensen, *Bjørnen og haren*; Jensen, *Den ny Ruslandshistorie*; Jensen, *Zarmoder blandt zarmordere*; Jensen, *Stalinismens fascination*; Jensen, *Gulag og glemsel*; Jensen, *Stalin*.

32 Petersen, review of *Stalinismens fascination*, by Jensen; Scocozza, review of *Stalinismens fascination*, by Jensen; Villaume, review of *Tryk og tilpasning*, by Jensen; Petersen, review of *Tryk og tilpasning*, by Jensen; Olesen and Sørensen, "Spiser bjørne harer?," review of *Bjørnen og haren*, by Jensen; Thing, "Det absolut onde," review of *Stalin*, by Jensen. In a 2014 coda, the Norwegian mainstream diplomatic historian Helge Pharo, in his review of Jensen, *Ulve, får og vogtere*—a work that was published by the center set up by the government to correct the picture that Danish mainstream historians had painted of Danish foreign policy (see the following)—criticized Jensen harshly for just such a shortcoming. In return, Pharo was ferociously attacked in Jensen and Jensen, "Replik til Helge Pharo."

33 Petersen, "Kampen om Den Kolde Krig," 157.

34 DIIS, *Danmark under den kolde krig*.

35 CFKF's website, accessed January 5, 2010, http://www.cfkf.dk/?id=9. The center closed in 2010 and the website is no longer functional. For more on the DIIS report (and the genesis of CFKF), see Olesen, "Truth on Demand." Jensen's CFKF book appeared in two volumes in 2014 as Jensen, *Ulve, får og vogtere*, with an Internet appendix on literature and methodology. The book split readers (or at least reviewers) into two camps—enthused fans and scathing critics—as effectively as anything Moses could have done. "Anmeldere er tordnende uenige om nyt Bent Jensen-værk," *Politiken*, February 25, 2014, accessed March 18, 2014, http://www.pol.dk/2217725.

36 Christensen and Nehring, *Grønland under den kolde krig*. For overviews and interpretations of the process leading up to Jensen's Centre for Cold War Studies, see Petersen, "Kampen om Den Kolde Krig"; Olesen, "Truth on Demand"; Farbøl, "Koldkrigere, medløbere og røde lejesvende"; and Pedersen, "Past, Present, and Future."

37 PET-kommissionen, *PET-kommissionens beretning*.

38 See Petersen, "Kampen om Den Kolde Krig," 178–186; Olesen, "Truth on Demand," 82–89.

39 For academic criticism of the DIIS report, see, for example, Andersson, "Danmark under den kolde krig"; Eriksen, "Danmark under den kolde krig"; Villaume, "Danmark under den kolde krig"; Nielsen, "Danmark under den kolde krig"; Mariager, "Danmark under den kolde krig"; Nielsen, "Replik til Rasmus

Mariager"; Ole Hasselbalch, "Kronik: Mangelfuld koldkrigsrapport med akademisk fernis," *Jyllands-Posten*, August 10, 2005, accessed June 13, 2016, http://jyllands-posten.dk/debat/ECE3802830/Kronik-Mangelfuld-koldkrigsrapport-med-akademisk-fernis/; Bent Jensen, "Tungen lige i munden—eller ud ad vinduet?," *Jyllands-Posten*, September 16, 2005; Bent Jensen, "Kronik: Fornemmelsen for frihed," *Jyllands-Posten*, September 29, 2005, accessed June 13, 2016, http://jyllands-posten.dk/debat/ECE3810990/Kronik-Fornemmelsen-for-frihed/. The sixteen-volume PET report seems to have fared somewhat better when it was finally published in summer 2009. For criticism of the PET Commission's report, see, for example, Lars Villemoes, "Debat: Intetsigende, kritisabelt og anonymt," *Berlingske Tidende*, August 14, 2009, accessed June 13, 2016, http://www.b.dk/debat/debat-intetsigende-kritisabelt-og-anonymt; Claus Bryld, "PETs sort/hvide verdensbillede," *Jyllands-Posten*, November 23, 2009, accessed June 13, 2016, http://jyllands-posten.dk/protected/debat/kronik/article4268903.ece.

40 Jesper Langballe, speech no. 61, addressing proposal 77, "Forslag til folketingsbeslutning om etablering af et nordisk institut til forskning i den kolde krig" [Proposal for a resolution by the Folketing to establish a Nordic institute for studying the Cold War], *Folketingets parlamentariske dokumenter*, session 2005–2006, March 3, 2006, accessed August 2, 2016, http://www.ft.dk/samling/20051/beslutningsforslag/b77/beh1/63/forhandling.htm?startItem=.

41 Jensen, "Tungen lige i munden—eller ud ad vinduet?," *Jyllands-Posten*, September 16, 2005.

42 Bent Jensen, "Kronik: Frihedens forsvarer har intet at beklage," *Jyllands-Posten*, February 14, 2006; Langballe, Speech no. 61, see note 40.

43 Langballe, Speech no. 61, see note 40.

44 Fogh Rasmussen, quoted in Petersen, "Kampen om Den Kolde Krig," 195.

45 See McCullagh, *Justifying Historical Descriptions*, 85–90.

46 Olesen, "Truth on Demand," 111.

47 Petersen, "Kampen om Den Kolde Krig," 159.

48 Villaume, "Stalins særligt hemmelige verden," 214–215, italics in the original.

49 Petersen, "Kampen om Den Kolde Krig," 203.

50 Jensen, *Gulag og glemsel*, 16 and 442.

51 Mommsen, "Moral Commitment and Scholarly Detachment," 51–52. See also Bédarida, "Historian's Craft, Historicity, and Ethics," and Haskell's review article based on Leerssen and Rigney's book, "Perspective as Problem and Solution."

52 Petersen, "Kampen om Den Kolde Krig," 188.

53 So as to put the issue in sharp relief, it can be pointed out that no one seemed to think it was odd when, after a meticulous review of the sources, Åsmund Egge—in his youth a leading Norwegian Stalinist—determined that Joseph Stalin had not been complicit in the 1934 murder of Sergei Kirov, the head of the Leningrad chapter of the Communist Party; see Egge, *Kirov-gåten*. Would it have been as unproblematic if a former member of the Hitler Youth, after having thoroughly sifted through archival material, had acquitted the NSDAP leadership of being complicit in the Reichstag fire in Berlin in 1933?

54 Jensen, *Bjørnen og haren*, 651; see also Jensen, "Totalitarismeteorier."

55 For comparisons between Stalin's Soviet Union and Hitler's Germany, see Overy, *Dictators*; Geyer and Fitzpatrick, *Beyond Totalitarianism*.

56 See, for example, "the revisionists" who seek to emphasize that Stalin's terror during the 1930s should not primarily be seen as Stalin's personal doing, but must be seen in a much wider context; for example, Thurston, *Life and Terror*; Getty and Naumov, *Road to Terror*.

57 Jensen, *Bjørnen og haren*; for a criticism of Jensen's attack on Rasmussen and defense of Kristensen, see Olesen and Sørensen, "Spiser bjørne harer?"

58 Anders Fogh Rasmussen, "Statsminister Anders Fogh Rasmussens tale i anledning af 60-året for Danmarks befrielse Københavns Rådhus 5. maj 2005," the Prime Minister's Office, accessed July 1, 2016, http://www.stm.dk/_p_7501.html; quoted in Petersen, "Kampen om Den Kolde Krig," 161.
59 Petersen, "Kampen om Den Kolde Krig," 161.
60 Erland Kolding Nielsen, "Blüdnikow anmelder ikke—han debatterer," *Berlingske*, April 18, 2011, accessed November 7, 2011, http://www.b.dk/node/13578466.
61 Collingwood, *Idea of History*, 215.
62 Langballe, Speech no. 61, see note 40.
63 Heidegger, *Being and Time*.
64 Skjervheim, "Deltakar og tilskodar," 70–71.
65 Blüdnikow, "Kampen om Den Kolde Krig," 561. See also Petersen, "Endnu en hvid kanin."
66 Bent Blüdnikow, "Poul Villaumes 'objektivitet,'" Bøger, *Weekendavisen*, March 14, 2014, 10–11.
67 Villaume, *Allieret med forbehold*; Olesen and Villaume, *I blokopdelingens tegn*; Lauridsen et al., *Den Kolde Krig og Danmark*.
68 Jensen, *Ulve, får og vogtere*, 424.
69 Jensen, "Kilder, forskning og litteratur," Gyldendal, accessed March 28, 2014, http://www.gyldendal.dk/~/media/Files/bent_jensen_kilder_og_forskning.ashx.
70 Blüdnikow, "Poul Villaumes 'objektivitet.'"
71 Poul Villaume, "Metafysik og kilderespekt," Bøger, *Weekendavisen*, March 12, 2014, 10.
72 Villaume, "Metafysik og kilderespekt," 10.
73 Sandvej, "Justitsministeriet afviser tidligere medlem,"; Ministry of Justice, Retsudvalget (REU), almindelig del. Appendix [bilag] 1025, "Kopi af indstillingerne om udpegning af formand og medlemmer til undersøgelseskommissionen vedrørende PET" [Copy of the recommendations to appointing a chairman and members of the fact-finding commission concerning PET]. *Folketingets parlamentariske dokumenter*, session 1998–1999, July 12, 1999, accessed June 13, 2016, http://webarkiv.ft.dk/?/samling/19981/menu/00000002.htm; Niels Rohleder, "Historiker vraget ubeset," *Information*, July 29, 1999, accessed June 13, 2016, https://www.information.dk/1999/07/historiker-vraget-ubeset; Niels Rohleder, "Uklar besked til historikere om PET-habilitet," *Information*, July 28, 1999, accessed June 13, 2016, https://www.information.dk/1999/07/uklar-besked-historikere-pet-habilitet.
74 Quine, "Two Dogmas of Empiricism"; see also Zammito, *Nice Derangement of Epistemes*.
75 Carr, *What Is History?*, 23.

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2 Court Historian
Matters of Fact

The previous chapter depicted Bent Jensen, firmly anticommunist, anti-fellow-traveling, and anti-appeasement Danish Cold War historian, in his lonely fight against the mainstream, center–left Danish international historians whom he thought were soft on communism. Whereas Jensen had no comrades in arms among other senior scholars, he had mighty allies in the press, in parliament, and in government. In the first decade of the twenty-first century Jensen was locked in a mutual embrace with the Liberal–Conservative government of Anders Fogh Rasmussen and its populist People's Party parliamentary allies in the prime minister's "culture war" against socialists, radicals, and other groups hesitant to accept his apparently unquestioning support of the foreign policy of the United States, Denmark's protection superpower. As the decade turned, Jensen got embroiled with yet another societal power, finding himself as the accused in a libel suit brought about by his writings in *Jyllands-Posten*, the major liberal, center–right newspaper where Jensen had been editor in the early 1990s. Jensen's defamation case has gone all the way through the Danish court system and is at the time of writing before the European Court of Human Rights in Strasbourg. In the process it has turned out three different verdicts and countless newspaper articles. Moreover, it has brought in sharp relief the demands on historians who want to determine what are the facts of politically charged matters. As such, the present chapter stands as a coda to the previous chapter as well as a prelude to Chapter 4, illuminating the challenges and significance of historians' labor to establish facts.

Jyllands-Posten is a paper that does not shy from provocation. In the winter of 2005–2006, its publication of the so-called Muhammad cartoons inflamed Muslim groups first in Denmark and then in the Middle East, and brought havoc on Danish embassies and interests in Arab countries.[1] In January 2007, Bent Jensen set peace groups and the left on fire by claiming, in a two-page article he wrote in the same paper titled "They Called Him No. 1," that the journalist Jørgen Dragsdahl had been considered a KGB agent of influence not only by Danish counterintelligence but also by the KGB itself. Jensen reiterated the claim in an interview with *Jyllands-Posten* the same day and in several other writings that followed. To Dragsdahl,

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famed left-wing foreign policy reporter and former lodestar for the nuclear disarmament movement of the 1980s, which he had been instrumental in setting up in Denmark, the charge was ruinous. It left him out of work and with a reputation in tatters.[2] So he repeated what he had done when the newspaper *Ekstra Bladet* had published similar accusations fifteen years earlier: he filed a libel suit. *Ekstra Bladet*, having based its reporting on classified police intelligence documents that its journalists had seen but could not be produced in court and on oral sources who would not speak, had been forced to withdraw the charges and settle for an out-of-court compensation to Dragsdahl.[3] But Jensen, who had had privileged access to archives of the Danish Security and Intelligence Service (PET), would not be cowed. So he and Dragsdahl met in court.

The town court found Jensen guilty of defamation, fined him 40,000 DKK and ordered him to pay Dragsdahl an unprecedented 200,000 DKK in damages and costs of more than 300,000 DKK.[4] Jensen appealed to the high court, which acquitted him. The payments were reversed: now Dragsdahl was ordered to cover Jensen's costs totaling 600,000 DKK.[5] Dragsdahl's subsequent appeal to the Supreme Court produced a split and somewhat Solomonic verdict in which Jensen was acquitted for the majority of the statements but convicted for others. The fine was cut to 10,000 DKK and the damages to 100,000 DKK, but since Jensen also had to pay Dragsdahl's costs, which had risen to 500,000 DKK, the total price of the libel had gone up.[6] Supporters of both Jensen and Dragsdahl had raised more than these sums to pay for the court case. What was at stake was therefore not money but pride, honor, vengeance, politics, and freedom of speech and of the press. And history: not only the empirical questions of whether Dragsdahl had been employed by the KGB as an agent of influence and whether the PET believed that he had been a KGB agent but also the methodological question of what are the requirements for the evidence on the basis of which historians draw their conclusions.

The Dragsdahl–Jensen case lends itself to the study of historians' methodology not because of the drama but in spite of it. The dust produced by rival bulls stamping their claws before locking horns may distract and disrupt the view instead of keeping pertinent matters in focus. The public display of arguments in court, however, forces the proponents to present the justification for their conclusions to the lay public (or, strictly speaking, to the judges but the point is that these are not historians and cannot be expected to be familiar with historical methods). So let us see what, judging from what was presented in court, are the requirements on historians wanting to pass judgment on matters of fact.

Before proceeding, let me emphasize how significant even simple, first-order descriptions of this kind can be. In the present case, the outcome of the libel case in part depended on whether the claim inherent in the description could be substantiated: if yes, Dragsdahl would lose. (As we shall see, he might lose even though the veracity of the libelous assertions was not

proved, but that is another matter.) But there was more than Dragsdahl's reputation at stake. Not least there was the political fallout, since the credibility of what is left of the peace movement of the 1980s—admittedly not much—would be hurt if it turned out that one of its central sources of information, namely Dragsdahl, had been furnished with part of that information from the KGB. A corollary is that the historiography of this movement, and of its role in the Cold War of the 1980s, would need mending—which is probably one reason why Bent Jensen wrote his piece in *Jyllands-Posten*.

Also worth noticing is the nonnarrative nature of the historian's task in this case. There is only a hypothesis to be checked: was Jørgen Dragsdahl a KGB agent? The job of the historian is to access the sources—no mean feat in this instance, since they are classified police intelligence documents—evaluate their reliability, and place them in their proper context. There is no story to be composed, no colligation to be made, no theory to be constructed: all such activities belong to other parts of the historian's métier. The question before the historian in this case is quite another, and yet such questions are a major part of being a historian: was Dragsdahl a token of the type KGB agent? In the effort to answer such questions, which take up so much of what historians manifestly do, narrativists are of no help. This chapter therefore attempts to de-narrativize the study of historiography, or rather, to join the ranks of those recent historiographers who are pulling the study of the historical discipline away from the writing of text and over to often more time-consuming, and no less important, parts of historians' *praxis*.[7]

I. Defamation in First Degree

The accusations for which Jensen was taken to court were of four kinds. First, he had claimed that the KGB considered Dragsdahl one of their agents. Second, he had claimed that the PET shared this opinion. Third, although Jensen insisted he had only reported the views of the KGB and the PET, Dragsdahl charged he had claimed that Dragsdahl had in fact been an agent. The distinction between this third and the former two claims is that Dragsdahl might not have been an agent despite the KGB and the PET believing so. Fourth, Jensen had accused Dragsdahl of presenting disinformation.

There are nuances to all these accusations: nuances significant enough to decide who would win the court case. Of prime importance is the definition of an agent: what it means as well as who is to decide. Is there some authoritative meaning of agent, and if so, is this definition what is juridically relevant or is the salient point how readers of *Jyllands-Posten* interpret agent—and if so, who is to tell? Then there is the question what kind of agent (if any) Dragsdahl had been. Jensen had written that he had been an agent of influence (*påvirkningsagent*), meaning someone who tries to influence the public opinion in a direction that suits the foreign power for whom the agent works. Can one be an agent of influence without being paid for

such work, perhaps even without being aware of one's function as such—for instance, by relaying views one has been presented for orally or in documents or articles furnished by official representatives of a foreign power, without specifying the provenance of these views? Are agents of influence a subgroup of agents, or are the former so much less blameworthy that they do not deserve the agent label? Finally, in the same vein, what is meant by disinformation, and who is to decide? Is it just information suited to mislead, or does it require a misleading intention, some sort of ill will?

A conscientious historian must know what meaning he or she wants to convey when using notions such as agent (of influence or not) and disinformation. The historian must also be aware of the fact, so dear to poststructuralist writers, that no author is master of the meaning of his or her words once they are in the domain of readers—although giving precise definitions, of course, helps maintaining some element of control or at least influence. The historian also knows that he or she must be able to sustain every assertion, which is the function of the evidence referenced in the footnotes. In practice, of course, the majority of assertions in a work of history are not backed up by footnotes; only new or uncommon or disputed claims are given such support. In order for the evidence to support the claims, the sources must be publicly accessible: methodological solipsism, whether in the form of personal revelations or documents to which the author has privileged access, is deemed unscientific and the purported evidence discounted.

No one could deny that Jensen's articles in *Jyllands-Posten* were defaming of Dragsdahl. It may have been an important motive in writing them. But whatever might have been Jensen's inner motivation for publishing the defaming articles, as professor of history he could argue that he had a duty to make his findings known to the public, since their relevance to the evaluation of the Cold War of the 1980s was clear for all to see. Since Dragsdahl was a public figure and a well-known Cold War activist, he could demand less protection of his privacy than could citizens with a lower profile in the media. This meant that the court case boiled down to the question of what grounds Jensen had had for his claims. In order to free himself of the libel charge, Jensen's first option was to prove to the court that his assertions were true. Failing this, his second line of defense would be to argue that he nonetheless had had good enough ground for publishing the accusations for him to be acquitted. If he could convince the judges that he had had reason to believe that his claims were true, he might go free.

Based on the interpretation in the preceding paragraph, the libel case became a showcase in the justification of historical descriptions. The firmer evidential ground Jensen could establish for his defamation of Dragsdahl, the greater would be his chances for acquittal. The standard required of him was not to establish the truth of the claims but something less demanding. Exactly how much less demanding would be for the courts to decide, and was what the lawyers would fight over.

II. What Is an Agent?

At the center of the Dragsdahl–Jensen case stood the latter's assertion—in his article in *Jyllands-Posten* on January 14, 2007, in the interview with the same paper the same day and in articles in *Jyllands-Posten* and other newspapers in the debate that followed—that both the KGB and the PET considered Dragsdahl a KGB agent. Jensen's main evidence for this claim was classified PET documents. Several of these contained information by Oleg Gordievsky, a highly placed KGB officer and British double agent from 1975 who defected to the United Kingdom in 1985.[8] Gordievsky was placed at the embassy in Copenhagen from 1966 to 1970 and from 1972 to 1978 and had a central position at the KGB headquarters in Moscow from 1978 to 1982. Then he became head of the KGB staff at the embassy in London, where he stayed until his defection. He had identified Dragsdahl as a Soviet agent of influence and had told that he had seen reports of clandestine meetings between Dragsdahl and KGB officers. Western intelligence services regarded Gordievsky as very reliable, but he had never himself handled Danish agents; his information on Dragsdahl was based on reports from the Danish KGB office, which he had seen at the headquarters in Moscow. Gordievsky's memory might be trusted, but was the KGB in Copenhagen telling the truth when they reported that Dragsdahl was their agent of influence, or was the local office merely seeking to impress Moscow and to inflate its importance, as such offices were known to be prone to?[9]

Another part of the PET documents was composed of information on the PET's own intelligence on Dragsdahl from 1983 to 1985, which had included telephone surveillance and observations of meetings with KGB officers in Denmark. The PET considered some of these meetings conspirative, complete with attempts—many successful, others not—by the local KGB officer to "shake off" the PET surveillance team by changing trains and other evasive maneuvers. Although many of the PET documents remain classified, no one disputed that in many of them Dragsdahl was identified as a KGB agent. In addition to PET documents declassified for the court case, several former PET leaders testified to the effect that Jensen's representation of the material had been correct on this point. Still this meant neither that Dragsdahl had been an agent—police intelligence is not always accurate—nor that the PET regarded him as one. Dragsdahl's lawyers argued that what was found in the PET documents was only the views of individual PET employees. For Jensen to substantiate his assertion that the PET considered Dragsdahl an agent, the historian would have to produce either documentary evidence or testimonies showing this to be the view of the PET as an institution.

What the professor could produce was documents penned by lower-ranking PET employees branding Dragsdahl an agent of influence apparently without anyone at higher levels in the organization taking exception by means of a dissenting note or comment in the margin, or in other ways indicating reservations with the use of the term. Jensen argued that this

silence showed that the categorization of Dragsdahl as an agent had been (tacitly) accepted at all levels of the PET. *Argumentum e silentio* is a risky exercise, however, since there might be more than one reason for the silence. High-ranking PET officer Per Larsen and deputy chief Mikael Lyngbo explained to the high court that lower-ranking employees had quite wide latitude as to formulations in internal documents, and superiors might not have felt the need to correct or comment points of view with which they might disagree. PET chief between 1984 and 1987 Henning Fode also underlined that one could not take the views expressed by individual employees as representing the views of the PET. Specifically, Fode in his testimony took exception with the view that the PET had regarded Dragsdahl as an agent, although he admitted that individual PET employees had seen him as such. It would have been more accurate to say that the Russians considered him an agent, Fode said, explaining that the use of the notion agent for Dragsdahl expressed eager PET employees, but this was not necessarily views that had the support of the leadership.[10]

Fode's testimony to the high court threatened to pull the rug out from under Jensen's case. In the eyes of the former PET chief, an agent was a person who had breached §107 or §108 of the penal law, forbidding, respectively, espionage and assistance to foreign intelligence services. No one had accused Dragsdahl of espionage. Whether he had assisted the KGB, Fode could not say. The PET with him as chief had decided, however, not to press charges against Dragsdahl for being an agent. Confronted with evidence showing that he had used the word agent for Dragsdahl when presenting to the government's Security Council (*Regjeringens sikkerhedsudvalg*) a proposal to expel a Soviet KGB diplomat, Fode explained that the notion referred to attempts by the KGB officer to hire agents and should not be taken as proof that he thought Dragsdahl was an agent. On the appropriateness of the term agent of influence for Dragsdahl, the former chief was evasive, saying only that opinions differed among experts and within the PET whether agents of influence were agents as described in the penal law. If Fode was evasive, however, the logic of his testimony was clear and detrimental to Jensen. For the PET to regard Dragsdahl as an agent, in their view he must have broken the law forbidding assistance to foreign intelligence. If the PET had thought he had broken the law, the Service ought to have pressed charges against him. Since it did not, it follows that the PET had not considered Dragsdahl an agent.

Fode's predecessor Ole Stig Andersen, PET chief from 1975 to 1984, gave a contrasting picture, providing Jensen with much-needed support. In the high court as well as in the Supreme Court, Andersen testified that in the eyes of the PET—and not just individual employees—Dragsdahl had clearly been an agent of influence. He explained that this notion meant conspirative cooperation with KGB officers, leading to newspaper articles in consonance with Soviet foreign policy. Whether such activity was covered by §108 of the penal code mattered less to Andersen, since an attempt to

press charges against Dragsdahl would only have been detrimental to the PET.[11] Andersen referred to the debacle concerning the PET's failed attempt to have leftist foreign policy activist and writer Arne Herløv Petersen convicted. Petersen had been arrested in the fall of 1981 and charged with cooperation with Soviet intelligence, namely, by acting, since the early 1970s, as an agent of influence for Moscow. The evidence for Petersen's collaboration with the Soviet embassy was overwhelming, but it was unclear whether his behavior—publishing pamphlets and articles drafted by the Soviets and initiating various antinuclear and anti-American activities—was illegal even if it could be proved that he had been directed and paid by the embassy. The affair ended with the tacit expulsion of Petersen's KGB case officer and with the charges against Petersen being lifted through a juridical *coup de main* by Minister of Justice Ole Espersen (with the backing of the prime minister and other central ministers). Espersen used a paragraph that normally was employed when culprits had admitted guilt, to put off the charges but also to declare that Petersen's activities had been illegal.[12] The conclusion drawn by the PET—who had arrested Petersen against the express wish of Espersen—was that when such an obvious agent of influence as Petersen was not tried, no Danish agent of influence could be convicted.[13]

So the PET chiefs apparently disagreed among themselves whether the service had considered Dragsdahl an agent. The fact that he had not been charged might be due either to a lack of certainty that he had broken the law or to a realization of the futility of trying to get the politicians to take an agent of influence to court when Minister of Justice Espersen had let Herløv Petersen go free (albeit tarred by the minister's public declaration of Petersen's guilt). The authors of the 2,500 pages commissioned report *Danmark under den kolde krig* (Denmark during the Cold War) published in 2005 by the Danish Institute for International Studies (DIIS), who had seen several but not all the PET documents on Dragsdahl and had conducted an interview with him, described his activities briefly and euphemistically, merely mentioning that he had been in the PET's searchlight due to his contacts with East European journalists and his role as opinion maker.[14] Morten Heiberg of the PET Commission, with access to the PET archive including the Gordievsky files, devoted more than forty pages of the volume he wrote on Herløw Petersen and Dragsdahl to the latter but concluded carefully, referring to his listing as agent by the KGB but emphasizing the tendency of local KGB offices to inflate their achievements and the importance of their contacts. Heiberg took care to state that the PET surveillance had not produced conclusive evidence of Dragsdahl cooperating with the KGB or taking money from the Soviets.[15]

Contrast the DIIS report's gloss on Dragsdahl's KGB involvement and the PET Commission's cautious description of what the PET chief at the time according to his testimony in court thought were the activities of a KGB agent of influence, with Bent Jensen's unhedged—evil spirits would suggest unhinged—references to the KGB and PET descriptions of Dragsdahl as an

agent. With reference to the claims in *Ekstra Bladet* in 1992 that Dragsdahl had had conspirative meetings with KGB agents and had received money from them, charges which the paper had been forced to retract, Jensen stated that "today it can be documented that a great deal" of what the newspaper wrote "was true."[16] Jensen quoted from an internal PET memo:

> "In the eyes of the KGB D[ragsdahl]'s position has . . . been extraordinarily useful. It is not without ground that at one time in the Center [the KGB headquarters in Moscow] he was denoted 'no. 1' in Denmark, but how much substance one got from this agent, one can only guess." So Dragsdahl was KGB agent according to both the KGB and the PET,

Jensen wrote. For the DIIS report he had nothing but contempt, describing its picture of Dragdahl's activities as misleading:

> There is nothing on his conspirative interactions with KGB case officers in Denmark and abroad. And it is concealed that the PET characterized Dragsdahl as agent. Although Danish intelligence services via Gordievsky got "extraordinary insight" into the Soviet Union's operations in Denmark, his information is not employed, nor has he been interviewed, as Dragsdahl was, in connection with the report. The DIIS researchers have seen the same material in the PET archive as the present article is based on, and must therefore know that the PET considered Jørgen Dragsdahl a KGB agent. Still one has chosen to be silent on this relationship.

Jensen returned to the shortcomings of the DIIS report in another article in *Jyllands-Posten* five days later, leaving no doubt of Dragsdahl's complicity: "Dragsdahl surely is mentioned several times in the report, but the reader is kept ignorant of the kind of his relationship with the KGB, and it is concealed completely that both the KGB and the PET denoted him agent."[17] Readers of the weekly *Weekendavisen* were also reminded of the gist of Jensen's criticism of the DIIS report: "It conceals that the PET characterized Dragsdahl as 'agent,' although the DIIS researchers knew about the PET's assessment."[18]

Jensen likewise minced no words when describing what he considered Dragsdahl's disinformation on the foreign policy of the United States and its allies. A major article in *Jyllands-Posten* on January 26, 2007, lambasting select pieces of Dragsdahl's writings, was titled "Disinformation" and commenced as follows: "Jørgen Dragsdahl during the Cold War conducted campaign journalism on the basis of false documents produced by the KGB, and the daily *Information*, where he was employed, brought false spy accusations against Soviet critics of the system. It was DISINFORMATION [*sic*]."[19] In May the year after, Jensen took the battle to Dragsdahl's old home turf *Information*, homing in on five examples—"not the worst"—of

Dragsdahl's "incredibly impudent relationship to the truth—what in KBG jargon is called desinformatsija." The conclusion to one of the examples, an article on a purported U.S. attempt to recruit Soviet dissidents as spies published in *Information* in 1988 under Dragsdahl's foreign policy editorship, is illustrative of the tone: "JD's hypocrisy is nauseating." The article "was a disgraceful mess, which the KGB could not have made better."[20]

III. Who Is the Better Historian?

The quotations in the previous section are cited for a specific reason: they were among the thirty-five statements that the town court ordered Jensen to retract, and also among those eighteen statements that were taken up by the Supreme Court after Jensen's appeal to the high court succeeded and Dragsdahl appealed further. The issue before the courts was to decide whether Jensen had sufficient support in his sources to allow him to publish his defaming descriptions. In effect, this made the courtroom an arena for a seminar on historical source criticism. Who was the better historian: the straightforward Jensen branding Dragsdahl with the labels of agent (of influence) and conveyor of disinformation, or the euphemistic DIIS researchers who seemed to place more faith in Dragsdahl than in PET employees—except PET chief Henning Fode—and the circumspect Morten Heiberg of the PET Commission who refused to pass judgment on the bone of contention, namely, whether Dragsdahl had been an agent of influence?

To reduce Jensen's credibility, Dragsdahl's lawyers presented several academic witnesses who explained the need for extra critical care when studying intelligence records. Svend Aage Christensen, Slavonic scholar of Russian history and head of the research team producing the DIIS report, underlined the pitfalls of placing too much trust in internal documents from intelligence services. The information available to intelligence employees is often extremely patchy, and their worldviews are often influenced by their long career in the service and by the notion that they must prevent objects of surveillance from conducting illegal activities. As a result, alternative interpretations may be lost. Information is recorded in the belief that no outsiders will have access and often without knowing what will become important and not. All this presents major interpretive challenges to a researcher working on intelligence documents, Christensen explained, challenges making such work a litmus test of his or her critical abilities.[21] The implication was that Jensen had failed the test. Professor of international politics at Copenhagen University, peace researcher of renown Ole Wæver declared to the town court that Jensen's newspaper articles fell short on source criticism. To the high court he elaborated, faulting Jensen's January 14 *Jyllands-Posten* article for jumping, on the basis of individual employees' opinions, to conclusions regarding the head of the institution and for a lack of focus on essential parts of the documents. Instead, Wæver thought Jensen piled up quotations and documents.[22] Neither Wæver nor

his Copenhagen colleague professor of history Poul Villaume, who in 1979 had joined forces with Dragsdahl to establish the Danish organization No to Nuclear Weapons, had worked with intelligence material. Still Villaume's testimony echoed that of Christensen, warning on a general basis against placing trust in such documents. In particular records of the civilian intelligence services (such as the PET), focusing on possible illegal activities, may include many rumors and uncollaborated information, Villaume said.[23]

Apparently Dragsdahl's expert witnesses impressed the town court. Referring to the testimonies of Christensen and Wæver, the judges stated that Jensen had been insufficiently critical of his PET sources. They concluded that Jensen had not convinced the court that his accusations against Dragsdahl for being KGB agent and for spreading disinformation on behalf of the Soviets were true. Nor did Jensen manage to have the town court think that he had had good reason to believe in the truth of his claims. Had he presented his accusations within the circle of scholars, Jensen might have been able to argue that his freedom of speech as a professor should include the otherwise defaming statements, the court said, but having published them in a newspaper meant that this was a forgone possibility. Also, had he been journalist, the court reasoned that the European Convention on Human Rights would have given him a wider freedom of speech, but his writing as professor had forfeited this opportunity. So by presenting his charges against Dragsdahl as professor in a newspaper article, Jensen, in the eyes of the town court, had closed the door on both his legal loopholes.[24]

The high court disagreed, and opened both doors for Jensen to escape. In the eyes of the regional judges, Jensen's professorship gave him a right to inform the public on topics of great interest. Since his charges had been published in newspapers with a nationwide circulation, moreover, the freedom of the press meant that Jensen's freedom of speech should be wide. Hence, "a lot" was needed for his statements to be considered a crime.[25] Or put the other way around: not much would be needed for him to go free of punishment. The high court, having heard more or less the same witnesses as the town court, agreed that Jensen's claims were defaming of Dragsdahl and that he had not been able to convince the court of their truth. And yet the court acquitted him, concluding that his accusations were legal. The reason was that the regional judges applied a lower standard, requiring of Jensen neither truth nor good faith—the application of which it did not discuss—but only "sufficient factual basis" for his claims that Dragsdahl had been a KGB agent and that he had spread disinformation. The legal basis for the high court's decision was verdicts by the European Court of Human Rights, which also had application for Denmark. So Jensen won, and Dragsdahl was ordered to pay his adversary's bills, not because the court found the accusations true or even probable but because they had a factual basis and Jensen was a professor who had published his defaming articles in major newspapers.[26]

IV. A *Bonus Pater* Historian?

Dragsdahl's supporters honed in on the critical point in the high court's verdict: the weak requirement of a sufficient factual basis for Jensen's defaming accusations. What factual basis was a sufficient factual basis? The court had offered no criteria, lending itself to a charge of circular reasoning: a sufficient factual basis was what the judges in camera decided was a sufficient factual basis. Dragsdahl's lawyers (he switched attorney after losing in the high court) repeatedly insisted that Danish law requires that someone who issues defaming statements and fails to prove their truth, in order to escape punishment must show that he or she had acted in good faith.[27] Shortly after the verdict, legal scholar Trine Baumbach of Copenhagen University criticized the same aspect in an article in the scientific journal *Juristen*, published by the union of Danish lawyers. Baumbach regretted that the high court had not discussed what would have been the implications of a requirement of good faith.[28]

In the high court, as well as before the Supreme Court, Dragsdahl's lawyers insisted that Jensen's treatment of the PET documents showed that he had not acted in good faith, that is, like a *bonus pater* historian would have done. The *bonus pater* notion is a requirement formerly used in compensation law, indicating how a normal, conscientious person or professional would have acted in the circumstances. This standard is lower than the demands of *optimus vir*—the conduct performed by the best people within a profession—and the idea is that one cannot ask compensation for accidents or ill fortune caused by someone acting like a *bonus pater*. Since Jensen had not conducted himself like a *bonus pater* historian, he had not acted in good faith and therefore ought to pay damages to Dragsdahl for the defaming statements. The issue was not whether there had been a factual basis in the archives for Jensen's claims but whether his descriptions would hold when measured against the methodological standard held up by his peers. A *bonus pater* historian would have been much more critical of the opinions of the lower-ranking PET employees, of the often-inflated reports of local KGB officers, and of intelligence sources in general—Western or Soviet alike. He would have hedged his judgments, showing the room for doubt and for alternative interpretations, and not blasting out condemnations like Jensen did in his newspaper articles. "A *bonus pater* historian would not have reached Bent Jensen's conclusions," as Dragsdahl's lawyer René Offersen stated in the Supreme Court.[29] He would have acted like Christensen, Wæver, and Villaume cautioned in their testimonies, and like the DIIS researchers and the PET Commission—who had had access to the documents on which Jensen had based his diatribes—had done when writing their reports. According to this line of reasoning, Jensen's lack of reservations showed his lack of good faith. Since his methodological naïveté could not be due to a lack of training as a historian, it must have been due to the professor's ill faith, namely, his wish to harm Dragsdahl.[30]

The town court sided with Dragsdahl since Jensen had not been able to convince the court that he had acted in good faith. The high court sided with Jensen since it thought rulings of the European Court of Human Rights gave the professor more freedom of expression and considered that he had had sufficient factual basis for his claims. The Supreme Court sided with both, concluding that Jensen had had the right to accuse Dragsdahl of conspirative meetings with KGB officers and to claim that the journalist had spread disinformation but that he had not had the right to assert that Dragsdahl had been a KGB agent or that the PET had considered he was one. The verdict was close in more than one respect. The Supreme Court conceded that Jensen lawfully could write that Dragsdahl had been an agent of influence but judged that normal readers would interpret the charges of having been KGB agent *tout court* as meaning that Dragsdahl had broken the law forbidding assistance to foreign intelligence services (§108 of the penal code). Jensen's unspecified reference to the truth of the reports that *Ekstra Bladet* had had to retract was particularly salient in this respect, since they expressly had referred to illegal activity for the KGB, including taking money from Soviet intelligence. Likewise, when claiming that the PET considered Dragsdahl a KGB agent, Jensen should have qualified these claims by stating that the PET eventually decided not to press charges against him.

The decision was close in another respect, too, since two of the seven judges thought that Jensen should have been acquitted on all points. They argued that the context made sufficiently clear to readers that Jensen only accused Dragsdahl of having been an agent of influence.[31] Since the majority convicted Jensen for seven statements, fined him, and ordered him to pay Dragsdahl compensation and also to pay his costs for the lower courts, Jensen was widely conceived as having lost (and Dragsdahl as having won) despite the fact that he was acquitted for eleven statements. This impression was strengthened by Jensen appealing to the European Court of Human Rights.

By insisting that normal readers would interpret Jensen's labeling of Dragsdahl as KGB agent as an accusation of having committed a crime by breaking the law against assisting foreign intelligence services, a charge Jensen never tried to substantiate in court, the Supreme Court side-stepped the question of whether he had acted in good faith as a *bonus pater* historian. It convicted him for charges that he claimed he had never asserted but which the Supreme Court majority reasoned were entailed by his reference to *Ekstra Bladet* and his unqualified use of the term *agent*. In the process it added another criterion to the list of alternative requirements for lawful defamation, namely, "due diligence." With reference to verdicts by the European Court of Human Rights, the Supreme Court accepted that one could not demand of someone issuing defaming statements in a public debate on a topic of significant social interest that the truth ("the veracity") of the libelous assertions must be proved. But it

added that the author of the contested statements, whether a journalist or not, must meet the standard required of journalists, that is, "act in good faith and on an accurate factual basis and provide 'reliable and precise' information in accordance with the ethics of journalism ('due diligence')."[32]

So out went the *bonus pater* historian of Dragsdahl's lawyers, and in came the duly diligent journalist of the European Court of Human Rights. The implication is that when a historian takes part in a public debate in mass media, the methodological standard by which he or she is tried is not that of history but of journalism. What consequences this might have for historians is unclear. Perhaps the Strasbourg judges will clarify if and when they decide to hear Jensen's appeal. In the Dragsdahl–Jensen case the Supreme Court, "after a full assessment" of the relevant circumstances, especially the fact that Jensen on the court majority's interpretation had accused Dragsdahl of a criminal offense after the PET had decided not to charge him, found that Jensen had not shown the due diligence required by the European Court of Human Rights. It is hard to escape the conclusion that Jensen's fault was to leave the definition of agent open to readers instead of explaining that in his view it included not only criminal activities (breaching §108) but also such conspirative meetings with KGB agents as were described in the PET documents and such cases of disinformation in the interest of the Soviet Union as the Supreme Court deemed lawful to accuse Dragsdahl of having conveyed. Historians wanting to issue libelous statements ought to be careful not only with the factual basis of the claims but also with their definitions. The price of sloppy or lacking definitions may be high: in Jensen's case 10,000 DKK in fines, 100,000 in damages, more than 500,000 to cover costs incurred by the adversary, plus probably more than 1,000,000 DKK in legal costs of his own.[33]

Few scholars are quite as eager to grind their axes for hatchet jobs on political adversaries as Bent Jensen seems to have been. The methodological demands for careful definitions and source criticism apply to all historians, however. Adherence to such standards bears the promise of achieving what Jensen thought he had achieved: the establishment of facts. But purported facts based on definitions that are wanting or sources that do not stand up to historical criticism or public scrutiny fail to be adorned with the status of facts. To borrow a phrase from a political ally of Jensen, assistant secretary of defense under Ronald Reagan, Richard Perle, such factual claims may be rich in assertion but poor in evidence. The Dragsdahl–Jensen case probably left both parties poorer, at least in standing. But the discipline of history may gain from the court's display of the rules of source criticism and the need to define concepts. Only by adhering to such requirements, scholars may succeed in the fundamental part of the historical métier, namely, the establishment of facts on which both theories and moral judgments should rest.

Notes

1 For an account, see Klausen, *Cartoons That Shook the World*, which includes a chronology of the events. See also the 2009 special issue of the journal *Ethnicities*: Lindekilde et al., "Muhammad Cartoons Controversy."
2 Eastern High Court's decision, 37–38.
3 Jensen, *Ulve, får og vogtere*, 1:574–575.
4 See the Town Court of Svendborg's decision, 77–86. *Jyllands-Posten*'s editor in chief and the journalists who had relayed Jensen's description of Dragsdahl as KGB agent were acquitted and Dragsdahl was ordered to pay their costs of 50,000 DKK.
5 Eastern High Court's decision, 101.
6 Supreme Court's decision, 59–61.
7 Goldstein, Historical Knowing; Tucker, Our Knowledge of the Past; Paul, "Performing History"; Kuukkanen, *Postnarrativist Philosophy of Historiography*.
8 Heiberg, *KGB's kontakt- og agentnet i Danmark*, 10.
9 See the Town Court of Svendborg's decision, 65.
10 Eastern High Court's decision, 55–60 (Larsen), 64–71 (Lyngbo), 44–49 (Fode).
11 Eastern High Court's decision, 50–52; Supreme Court's decision, 35–39.
12 Heiberg, *KGB's kontakt- og agentnet i Danmark*, 44–136.
13 Eastern High Court's decision, 50; Supreme Court's decision, 37–38.
14 DIIS, *Danmark under den kolde krig*, 3:363–364, cited in the Supreme Court's decision, 32–33.
15 Heiberg, *KGB's kontakt- og agentnet i Danmark*, 137–180. The conclusion, 179–180, is cited in the Supreme Court's decision, 33–34.
16 Bent Jensen in *Jyllands-Posten*, January 14, 2007, cited in the Supreme Court's decision, 10–16, translation mine.
17 Bent Jensen in *Jyllands-Posten*, January 19, 2007, cited in the Supreme Court's decision, 16–19, translation mine.
18 Bent Jensen in *Weekendavisen*, February 9, 2007, cited in the Supreme Court's decision, 27–29, translation mine.
19 Bent Jensen in *Jyllands-Posten*, January 26, 2007, cited in the Supreme Court's decision, 19–23, translation mine.
20 Bent Jensen in *Information*, May 3, 2008, cited in the Supreme Court's decision, 29–32, translation mine.
21 Town Court of Svendborg's decision, 56–58.
22 Town Court of Svendborg's decision, 60–61; Eastern High Court's decision, 60–61.
23 Town Court of Svendborg's decision, 63–65.
24 Town Court of Svendborg's decision, 77–79.
25 Eastern High Court's decision, 91, 100.
26 Eastern High Court's decision, 90–100.
27 Eastern High Court's decision, 73; René Offersen to the Appeals Permission Board, November 8, 2013, on Jørgen Dragsdahl's website, accessed July 18, 2016, http://www.dragsdahl.dk/A20131108A.pdf, 4; René Offersen, Appeal to the Supreme Court, February 20, 2014, on Jørgen Dragsdahl's website, accessed July 18, 2016, http://www.dragsdahl.dk/Hoejesteretsstaevning.pdf, esp. allegation nos. 18 and 28; Supreme Court's decision, 5.
28 Baumbach, "Historieforskning, injurielovgivning og god tro."
29 Supreme Court's decision, 5.
30 See note 27, plus Henrik Holm-Nielsen's written procedure for the Eastern High Court, on the Association for the Support of Jørgen Dragsdahl's website, accessed July 1, 2016, http://www.jorgendragsdahlforeningen.dk/111819/.
31 Supreme Court's decision, 51–61.

32 Supreme Court's decision, 53.
33 Jensen's exact costs are unknown. According to Dragsdahl's supporters, the latter's costs totaled 1,341,408.42 DKK. See http://www.jorgendragsdahlforeningen.dk/109584/, accessed July 1, 2016.

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3 Witches Cannot Fly

A Critique of the Notion of Situated Truths

One day in 1999, while in the midst of writing the article that this chapter is based on, I found in my mailbox an anthology on theory and criticism of the notion of power, published by a government-sponsored Power and Democracy Study (*Maktutredningen*). The book included an essay by the historian Erling Sandmo on Michel Foucault as a theorist of power. The article summarizes Foucault's argumentation for regarding truth as a social entity bound to power and thus to discourse.[1] In the mailbox was also the journal *Prosa*, containing another article by Sandmo, on the history of truth. There he concludes that the theory of "situated truths," that is, "that truth is relative to the position from which it is spoken, is a critical premiss for much of the most radical poststructuralist thinking right now."[2] Seventeen years later I find this description as unsettling as when I first wrote the original article to defend the conventional notion of truth against the charges filling my mailbox, signaling that the science war had come to historiography. The radical poststructuralist assault on the idea of truth has not been driven off. In Norway it is still led by Sandmo, who in 2015, in his erudite, eloquent, and best-selling primer *Tid for historie* (Time for history) reiterates most of his views from the turn of the century on situated truths.[3] So here I go again, spurred by the same desire to fight back against the postmodern attack on scientific historiography. Whereas Sandmo is the target of my counterattack, this is only because he is leading the charge of the poststructuralist brigade in Norwegian historiography. Thus, he stands proxy for a host of postmodern writers and historians (including philosophers or theorists of history) all over the Western world. As such, he bears the brunt of my critique. My aim is to take part in the defense of history as a scientific undertaking against those trying to undermine a central tenet of that work, namely, the pursuit of objective truth.

Erling Sandmo has been regarded—and justifiably so, I would presume—as the spokesperson and standard-bearer for so-called postmodern approaches among Norwegian historians.[4] His elegant and engaging discussion of complicated ideas that may seem foreign and impenetrable, at least as formulated by their original authors, has led many to follow him with interest, but also with astonishment and at times perhaps even bewilderment. Sandmo's

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doctoral dissertation from 1997 has been read by many, myself included, whose area of expertise is far removed from its subject matter, namely, Norwegian sixteenth- and seventeenth-century social history.[5] His role as the very personification of postmodernism in Norwegian historiography is undoubtedly a boon to his fellow postmodernists, who have gained far more respect and goodwill than they otherwise would have. That is precisely why I have my doubts about whether Sandmo's influence concerning matters of theory is beneficial to historians in general. This concern is what has motivated me to spend so much time and energy analyzing Sandmo's view on truth and the writing of history, as articulated in his dissertation and in the subsequent debate with mainstream theorists Ottar Dahl and Knut Kjeldstadli in *Historisk tidsskrift*, the leading Norwegian journal of history.[6] In addition to the notion of situated truths I discuss a few related issues, namely, the notion of culture and Sandmo's ideas of how history should be studied and how cultural history should be written. Although his motives are the best, I believe that Sandmo's view of truth is fundamentally at odds with the tenets of modern scientific historiography, as his conceptual toolbox fails to differentiate between belief and truth. Mine does.

I. Situated Truths

Attacking Erling Sandmo's view of truth is no easy matter. "It is easy to agree with the principle of non-contradiction," he replies to Ottar Dahl. "Two mutually contradictory statements about the same thing cannot both be true." Only to declare on the next page that the ways in which French peasants sought to remedy drought in the fifteenth century, which Sandmo views as being an uncommonly straightforward matter, are nonetheless "complex enough to warrant putting the principle of noncontradiction to the side and say that 'the same thing' is seldom to be found."[7] For whereas drought in the pre-modern era was a supernatural occurrence that required magic, drought today is a natural phenomenon that requires other, decidedly nonmagical solutions. Awareness of "the local validity, broadly understood, of truths" does not mean "that a historiography influenced by postmodernism would be incapable of differentiating between lies and truth," he contends in his doctoral dissertation. "If your linguistic skepticism runs so deeply that that you don't believe that language reflects any form of 'reality' at all, then I believe you won't become a historian. For if you are a historian, you believe in the value of using sources. And in that case there is no form of situating that makes it possible to claim that you are telling the truth if you deny that the Holocaust took place. For example."[8] At the same time, however, Sandmo underlines that it is impossible to find the reality that underlies the narratives in these sources: "The 'reality' behind the story becomes a mirage. . . . We will never be able to grasp or articulate what really happened. But we can read different narratives about it, juxtapose them with one another, and study how they form patterns of historical narrator positions."[9]

When faced with the leaps and bounds in Sandmo's language, combined with what I perceive to be a typically and almost programmatically postmodernist reluctance to define concepts, it is easy to become resigned. Instead, I shall try to interpret him as benignly and well-willingly as possible. In brief, this is how I perceive him and the postmodernist position he epitomizes: What people at any given time believe to be true is, broadly speaking, culturally contingent, as determined by their concepts and worldviews—for example, concerning whether or not the principles that govern the world are supernatural. This overall context shapes what people perceive and how they order and explain what they perceive. This is how it was for previous generations, and we must presume that this is how it is for us as well. What we hold to be true is as much culturally contingent as what people in previous eras held to be true (or what people in other cultures today hold to be true). Our truth is not superior to their truth, because both are determined by the respective cultures. There is no extra-cultural or extra-historical vantage point from where we can objectively observe and evaluate existence. We are all tinged by and hence captives of our own culture: in short, we are situated. Since we therefore in principle cannot have better access to the "truth" about phenomena—understood as a form of objective description of these phenomena—than people in other cultures, our understanding is, in principle, not better or truer than theirs. We should therefore speak of local, or situated, truths, in the plural form. What fifteenth-century French farmers held to be true was true for them; what French farmers today hold to be true is true for them. Making our own "truths" the yardstick for others is showing these other people a lack of respect.

II. Truth and Contradiction

A red house cannot be non-red. This is the very principle of noncontradiction, a prerequisite for meaningful communication. It received its classical formulation by Aristotle: "For the same thing to hold good and not to hold good simultaneously of the same thing and in the same respect is impossible."[10] The principle of noncontradiction prohibits logical contradictions. A red house, in the sense of the same red house at the exact same point in time, can be many different things: large, wooden, residential, romantic, expensive, dilapidated, hellish—it can even have a conspicuous greenish hue in certain lighting. The only thing it cannot be is non-red. It cannot have any properties that per definition exclude it from being red, for example by being purely blue. (It can, however, be bluish or contain segments that are blue. If the phrase "a red house" denotes the house's front, its back can even be blue.) All the other properties mentioned above can be true descriptions of a red house.

The principle of noncontradiction is a liberal principle. This entails that several highly dissimilar descriptions of the same phenomenon—even descriptions that are in opposition to one another—can be true simultaneously, as long as they do not logically contradict one another.[11] As the

Norwegian author Aksel Sandemose described Halfway Mountain in Newfoundland: "I have been round it while on a hunting trip and it was curious to observe how entirely different such a mountain could be each time I had walked on a little way and turned to look at it again. A thousand different descriptions of Halfway Mountain may be presented to you, and all will be equally accurate."[12] A house can be a paradise for a domestic tyrant and a hell for his family. The women's liberation of the 1970s and the dissolution of the postwar nuclear family are true descriptions of the same phenomenon. A traitor to some can be a hero to others. This is not especially postmodern. Much of what Sandmo writes about situated truths is entirely unproblematic in a conventional understanding of truth, that is, as correspondence with reality. That Africans and women describe parts of world history differently from the traditional world history written by white men, so that people now see, in Sandmo's words, "oppression where they previously had seen civilizing, and disempowerment where previously it was democracy that was in focus," does not need to have any consequences at all for how we understand and use the concept of truth.[13] Our new, more multifaceted interpretation of world history does not entail that the old descriptions were not true. But they were not the whole truth, and to the degree they falsely claim to describe the main outlines of world history, they were *not fair*, in the sense that they implied something untrue.[14] The domestic tyrant's description of his house as a paradise is true but not fair if you are left with the impression that the house is a paradise for his family as well.

Let us take another example: Even with a conventional concept of truth, there are infinitely many true descriptions of, say, John F. Kennedy. People know him, or know of him, from different vantage points: as politician, commander in chief, head of state, family husband, womanizer, friend, foe. Every description of Kennedy will be situated. Some of these descriptions may seem to stand in opposition to others but can be true nonetheless—for example, that he was seeking peace and taking the world to the brink of nuclear war. But there are also descriptions of Kennedy that are not true. Because he was a Democrat, he cannot have been a Republican (at least not at the same time). We can find out which description—Democrat or Republican—is true by studying sources that refer to his political affiliation. Kennedy can also be described in a way that is true but not fair, for example, if we characterize his political views by saying that as a young man, he worked as an aide to Joseph McCarthy. Some descriptions are more difficult to verify than others. Opinions will be divided on whether Kennedy kept his country out of war in Indochina or took the first steps down the road to the Vietnam War. Perhaps both of these descriptions are true, or perhaps neither is. But in any event, whether we are describing Kennedy or a wider event or phenomenon, it is the descriptions' congruence with reality that determines whether they are true.

Note that the latter sentence contains two key claims that are open to debate. First of all, there is the claim that there is, in fact, a reality, in the

sense of something outside of our own consciousness, something that exists independently of us.[15] We can never know this—the claim must always have the character of a working hypothesis. In our context this is less important, for as we have seen, Sandmo shares this hypothesis, with a reservation for what I consider to be his somewhat unfortunate metaphor where "reality" is a mirage and hence nonexistent. On the other hand, I am uncertain about whether Sandmo agrees with my second claim, namely, that the truth of a description is determined by the description's congruence with reality. With "congruence" I do not mean correspondence in the sense that reality is as we describe it; we can never know how reality is in itself, only how it appears to each of us individually. We see a red house, but we cannot know whether the house is actually red or whether the red we see is created by our own perception.

In the debate with Sandmo, Knut Kjeldstadli concurs with C. Behan McCullagh's understanding of truth as a "correlation" between description and reality.[16] This entails that a description is regarded as true if it describes reality more or less as reality normally appears (or appeared or would have appeared) for persons with the same senses within a given culture.[17] McCullagh's definition of truth can be seen as some kind of advanced understanding of correspondence. Its critical point is, of course, the final addendum: "within a given culture." This may seem to open up for the type of cultural relativism that Sandmo advocates. So before we continue with the analysis of truth, we must first clarify the concept of culture.

III. Truth in Culture

McCullagh mentions two reasons for including culture in his definition of truth. He does so in part because people from different cultures perceive the world differently and describe it therefore differently.[18] I have retained vestiges of a premodern understanding of motors and valves and such things—my first car was a Citroën 2CV called Rudolph, and it had soul—so that I view the motor in my father's old fishing boat differently than a mechanic does. We do not see and hear the same thing. The second reason why McCullagh includes culture is that different cultures explain the world differently, even when they perceive the same thing. The result can be that a culture's description of a phenomenon contradicts another culture's description of the same phenomenon.[19] I say that the motor is entirely fine but that it is possessed by an evil spirit, whereas the mechanic explains that the carburetor needs to be fixed.

In one sense, McCullagh's correlational definition of truth is culturally relative. In one passage he refers to descriptions as true "relative to the culture which offers them."[20] But his definition of the truth about the past is anything but culturally relative: "Historical descriptions are true if the world really was as they describe it, or more strictly, if it would have been experienced appropriately had *the historian* been there to perceive it."[21] An

attempt to unify these seemingly contradictory ideas is the following statement, which is likely McCullagh's most accurate summary of his point of view: "Instead of saying that their well-supported conclusions about the world are simply true, historians should say that they are probably true, relative to the available evidence and to their culture at the time."[22] Here the truth is absolute but difficult (or impossible?) to find, and the articulations of it must hence be culturally relative. This is a notion I find attractive. The reason I nonetheless do not share it is because I believe it is based on an uncritical approach to the concept of culture. For if we look more closely at this concept, it turns out that there are major problems associated with using it as a basis for analyzing situated truths. Culture can neither explain nor justify differences in perceptions of the truth.

"Culture" is at the forefront of postmodern historical writing. This mystical, collective concept has been accorded the same privileged status in the social sciences since the 1980s as was the case during the 1960s and 1970s with the not quite as mystical concept of "class." Sandmo's concept of truth entails that what any given culture regards as true represents truth for them, and that the truths of our own culture are no truer or better than anyone else's. I consider this to be a logical fallacy and a categorical mistake, one that McCullagh commits as well when he describes how different cultures perceive each other's truths as erroneous. Truth is a property that individuals, or institutions with suitable decision-making mechanisms, allocate to descriptions. A person can regard something as true, as can a religious community or a political party. A culture cannot do so, however, unless we define culture so narrowly that each institution or person becomes a distinct culture.[23] But the mechanic and I are not each our own culture, even though the above scenario can be interpreted that way; we only use two different cultures' understanding of motors, or rather, we understand motors in ways that are influenced by two different cultures. In other contexts, for example, when the same mechanic claims that God can cure polio if the cripple believes in Christ, he is the one being influenced by premodern modes of understanding.

In the debate on truth in *Historisk tidsskrift*, Kjeldstadli addresses the challenge of defining and delimiting cultures.[24] Sandmo admits the need to address such issues, although he never relates to them.[25] But closing your eyes to tough questions does not make them go away. Obviously, distances in time and space are insufficient criteria for distinguishing one culture from another. If we try to use language as a criterion, we encounter problems both with people who speak or write the same language but who clearly belong to different cultures, such as our great-great-grandfathers and ourselves, and with people who speak or write different languages but who can be regarded as belonging to the same culture, as today's Swedes and Norwegians, or perhaps even Americans and Norwegians. Differences in gender, living standard, profession, and education can also create so dissimilar horizons of understanding (in a Gadamerian sense) that we can speak of

different cultures. In general, I would contend that it is impossible to define cultures precisely and unambiguously, though there will of course be cultural differences that do represent insurmountable obstacles to understanding. It must precisely be the potential to understand another's point of view that is decisive here. If we understand how people in another culture perceive and explain the world, cultural differences are no longer a reason for culturally relative truths. It is only if we fail to understand how other people perceive and explain the world—or rather, what it is they are perceiving and explaining—that we must accept that they have their truths and we have ours, because we cannot know in such cases whether we are in fact describing the same phenomenon. Sandmo touches on this point when he explains that his reason for shunting the principle of noncontradiction to one side is that it is "so seldom that we encounter 'the same thing' as historians."[26] However, his example, of drought in the fifteenth century compared with drought today, suggests a rather uncanny ability to see the same phenomenon as not being the same after all.

IV. More Truth: We Know Better

In the fifteenth century, French peasants viewed drought as a supernatural phenomenon; we do not. The explanations contradict each other, and drought is but one of an infinite number of such examples. Why should we have a monopoly on the truth? Cannot each culture have its own truth(s)? Our understanding of the world—including theories that most educated Westerners are incapable of seriously doubting, such as evolution and the laws of physics—is the result of a complex historical process owing in many ways to coincidences and power, in both an everyday and a Foucauldian sense, rather than to a purely rational development of science. Theorists, philosophers, and scientists developed their understandings of how nature works by making analogical deductions from the societies in which they lived. Our notion of there being laws of nature can be traced back to pre-Socratic Greek philosophers of the sixth century BC. Previously, people had believed that capricious, uncontrollable deities ruled over nature. A vestige of this idea has survived into the present day in the (more joking) notion of "the weather gods." The Greeks' idea of there being laws of nature mirrored the relatively predictable lives they led in their city-states, sheltered from the whims of nature and governed by the rule of law.[27] Evolution's breakthrough as a scientific theory in the United Kingdom in the 1860s can hardly be explained by the intellectual force of Darwin's idea of natural selection. True Darwinism, devoid of any notion that socially learned traits can be inherited, only became prevalent with the breakthrough of modern genetics many years after Darwin's death. Evolutionism's supplanting of the belief in God as an active creator was primarily due to the sustained advocacy of Darwin's followers, combined with the theory's appeal as an analogy to those segments of the British middle class who wanted to break the

aristocracy's stranglehold on power: the notion that the various species had not been created by God in a single act of creation, but had evolved to ever higher levels through an internal struggle where the most well adapted won out, could be used both to legitimize laissez-faire capitalism and to undermine the political legitimacy of the British lords and bishops. It is likely that Darwin and other evolutionists were inspired in their theories by the daily struggles for existence they observed in the capitalist-industrialist societies of the nineteenth century.[28] Aristotelian physics, after many a fierce political and theological battle, was forced to give way to Newtonian physics, which in turn had to yield to Einstein and his theory of relativity (though certainly without the same amount of kicking and screaming). What makes us think that our worldview will not be rejected by future generations? The answer, it would seem, is nothing other than a knee-jerk belief in our own superiority. Nevertheless, I insist that we must uphold a conventional rather than a situated concept of truth, for two reasons: first, and most important, because we know more than our predecessors and, second, because language obliges us to do so.

The first point first: the mechanic, with his mechanical explanation, and I, with my animation of objects, cannot both be speaking the truth about the motor in my father's fishing boat. He knows more than me about motors, which makes him see and hear things I do not see and hear, or in any event take what we both see and hear (a sputtering motor, a clunking noise) and explain it differently and more correctly than I do. His description corresponds to reality; mine does not (other than in a purely metaphorical sense, but in that case the two descriptions do not contradict each other). His description is true, and mine is untrue. I would contend that this scenario is entirely parallel with the French peasants and drought. We know more than they do: we know that nature is not more animated than a fishing boat motor, and that supplications to supernatural forces do not prevent drought. Therefore, our description is true, and theirs is untrue. To claim, as Sandmo does, that we are thereby dabbling in science, rather than writing history, seems unfair to historians and scientists alike.[29] Nor can I see that our objects of study, in this case, French fifteenth-century peasants, are being disrespected in any way by our pointing out that humankind, during the five hundred years that have elapsed since that time, has acquired a type of knowledge to which they did not have access. Science has increased our understanding of the world and thereby given us a truer picture of it than the fifteenth-century peasants had. A part of this picture is the knowledge that science, despite its mistakes, setbacks, and morally debatable aspects, will in the future further increase humanity's understanding of the world and thereby give our descendants a truer picture of it than the one we ourselves currently have. When radical poststructuralists refuse to consider our understanding—and hence our truths—as qualitatively superior to that of our predecessors, I believe the reason is they have not entirely grasped a core trait of modern science, namely, that in order to replace an established

theory, the new theory must explain not only those phenomena that the established theory was incapable of explaining but also phenomena that the established theory was, in fact, able to explain. Our theory of drought explains drought both today and in the fifteenth century better than the theory of the fifteenth-century French peasants.

Two examples will shed light on what is untenable in the radical poststructuralist understanding of truth, such as it is presented by Sandmo. First, it was commonly believed in seventeenth-century Europe that people could be in league with Satan and thereby acquire magical powers. Most of those who were accused of such practices were women, that is, witches. With clerical backing, such witches were identified, tortured, and often executed, and there was no lack of people who stepped forward to bear strong testimony to the witches' misdeeds. In short, people held the existence of witches to be true. We, however, do not. If we are to follow Sandmo's understanding of truth, we must nonetheless state that witches existed *for them*, that is, for those people who claimed to have seen witches and witchcraft. But what about those who claimed to have been unjustly branded a witch? For those people, it was not true that they themselves were witches. They may nonetheless have believed that witches did in fact exist, only that the witches were other people than themselves. But what if no witches turned up? Was it nevertheless true that they existed? In our secularized culture we can hardly imagine Satan conferring magical abilities upon people; most of us hold neither Satan nor magic to be true. Moreover, we do not hold confessions extracted during torture—and in any case, confessions of things we do not believe exist—to be true. But people did so during the seventeenth century—that is how witches confessed. Does that mean that witches were indeed for real—not in the sense that that people *believed* witches were for real, for it is beyond question that people did so, but in the sense that what people believed (i.e., that witches were for real) was *true*? Even though no witches existed in actual reality? Is it enough that a phenomenon is to be found in someone's imagination for it to be true (for them)? As far as I can understand, Sandmo must make this claim, for otherwise, he is in danger of dabbling in science, to use his own terminology.

At the beginning of the seventeenth century, people believed that the Earth was the center of the universe, and hence that the sun revolved around the Earth. It was what was considered true at the time. To be sure, Copernicus believed that it was the other way around, but his view did not gain any support. Subsequently, Galileo believed he could prove, with the aid of controlled experiments and accurate observations, that Copernicus was, in fact, right. But under the force of power in a conventional, nondiscursive sense, Galileo was coerced into abjuring his theories, theories that we today hold to be true. I am certain that Sandmo also believes that the Earth orbits the sun. According to his understanding of situated truths, however, we cannot write that Galileo's theories were true and Ptolemy's (i.e., that the Earth is the center of the universe) were untrue. For we can only make that claim

if we use the criteria of our own culture to decide what is true, namely, the criteria found in the empirical sciences—criteria that precisely were not recognized by the culture that Galileo was a part of. (Or perhaps Galileo was not a part of that culture? Perhaps he constituted a culture all by himself?) To the best of my knowledge, the culture of that era regarded dogma as having greater authority than experience. Therefore, so as not to become amateur scientists, we must, in a postmodernist understanding, write that until a certain point in time during the seventeenth century, the sun revolved around the Earth. And then the sun and the Earth switched roles. But if it was true for Galileo's contemporary age and for his adversaries that the sun revolved around the Earth, even as it was true for Galileo that the Earth revolved around the sun, it should not surprise Sandmo that this notion of truth sets quite a few heads spinning as well.

V. Truth in Language

Now to my second reason why the idea of situated truths is untenable. For the conceptual usage that will result should we accept Sandmo's analysis, breaks not only with common, everyday usage but also with the usage Sandmo himself must employ in order to express his views on reality, namely, that it exists. This is a central point in Ottar Dahl's article in the truth debate: when we say that something is true, we say how (or at least that) the phenomena actually *exist*, in reality, independent of the person making the statement. The sentence "It is true that witches existed during the seventeenth century" means the same as the sentence "Witches existed during the seventeenth century."[30] In common usage, the meaning of the statement "It is true that witches exist"—which thus means "Witches exist"—is different from the meaning of the statement "I believe that witches exist" or "I hold it to be true that witches exist." The first two statements are descriptions of how something is; the latter two are descriptions of how a certain person believes that something is. In common usage, the use of words like *truth* and *true* presupposes a (more or less sophisticated) conventional understanding of the concept of truth.

There is no one now who, in a pre-Foucauldian sense, is forcing Sandmo or other postmodernists to follow common usage. The meanings of words change over time, and someone must be the first one out. On the other hand, there is a considerable structural coercion in language; I imagine that this is precisely one of the chief insights of the structuralists. It is quite hard to swim against the tide. The consistent position for those who contend that all truths are situated, and that truth in a conventional sense is therefore impossible, would be to refrain from using the word *truth* at all, as certain postmodernists have, in fact, called for.[31] A consistent advocate of nonviolence who uses weapons is not in actuality a consistent advocate of nonviolence. When Sandmo, in his debate with Ottar Dahl, admits that "my own usage of words does not always meet the demands to precision I

would like to require of it," I would argue that he has not understood how deeply the problem, in fact, runs.[32] Each time he uses the word *truth*, *true*, or a similar term that connotes a nonsituated correspondence with reality, he shoots himself in the foot. Furthermore, if Sandmo really believes that reality exists—in the sense that it exists not only for him and his culture, but as a sort of absolute entity that is not culturally contingent—how will he express this linguistically? One way could be to introduce a new set of concepts, namely, truth with an asterisk ("true*"), to denote that he is using the concept of truth in a conventional sense.[33] But each such asterisk only hammers in another nail in the coffin of the claim that all truth is situated.

Of course, neither Sandmo nor anyone else needs to resort to ploys à la "true*" to describe other cultures with the respect they deserve. Language already contains words we can use to say that people hold something to be true, without forcing us to adjudicate whether what they believe to be true is, in fact, true. The sentence I just wrote is one such example. We can write that people believed this or that—for example, that witches were for real—or that they held or considered it to be true. Indeed, historians have done this for a long while. In that sense I believe that Sandmo is right, that is, that he speaks the truth, when he writes that his form of cultural relativism "is neither new nor particularly radical."[34] But if we are to accept Sandmo's notion of situated truths, with the consequences that entails for our usage, it becomes impossible to express what the aforementioned sentence expressed, because the difference between belief and truth evaporates. Instead of accepting the logical consequences of radical poststructuralism, we should, in my opinion, use the language we have, wholly and fully. In order to explain the French peasants' use of magic, for example, we can say that they *felt* they were facing problems that were supernatural. And if someone finds this stance patronizing, I will say in my defense that that is how Sandmo himself expresses it, for the previous sentence, in fact, is lifted straight from his article on truth, even though the emphasis is mine.[35] When we distill Sandmo's discussion of the French peasants' magical view of drought down to its essence, the force of his radical postmodernism dissolves.

Let me conclude this discussion by clarifying a particular point. Strictly speaking, we can never be completely certain about anything, including our own existence (although I find myself unable to doubt it). Hence, we can never be sure that empirical statements such as "Witches do not exist" or "The Earth revolves around the sun" are true. Unlike analytical statements whose truth is determined by predefined rules of language, synthetic claims of empirical circumstances can only have the status of hypotheses whose probability ranges upward toward certainty. But language becomes unnecessarily awkward if we are to replace concepts such as "true" with expressions such as "probability ranging upward toward certainty." Moreover, were we to consistently use such awkward phrasings, we would need a word to denote circumstances we consider to be absolutely or definitely

true, given certain prerequisites such as that we exist as phenomena bound by space and time and that we possess consciousness and usually are, in fact, conscious. For example, I hold it to be true—in the sense "absolutely true"—that I am the only person in this office as I am writing this sentence. (I checked under the desk. There was no one there, or at least I was unable to see, hear, smell, or feel anyone.)

VI. Historiography and Truth about Social Behavior

Erling Sandmo's appeal to respect people in other cultures and in other times is something on which we should all be able to agree. But I cannot see how respecting the people of the past means that we must set aside our own knowledge and, so to speak, limit our horizon of understanding to *their* horizon of understanding. Surely, the goal must rather be to expand our horizon of understanding to include or at least touch upon theirs. Hitler had certain beliefs that we do not think were true, for example, his racial ideas and his idea that the Jews were the root cause of many of the ills that had befallen Germany. Is it impossible to understand these beliefs without considering them to be true for Hitler? If Sandmo's answer is yes, were they then true also for the Jews in Nazi Germany? Sandmo claims that "if drought had not been a supernatural problem in the fifteenth century, it is impossible to explain that magic was attempted as the solution."[36] I totally disagree with Sandmo on this point. When Sandmo contends in the same passage that "the positive explanation of the peasants' magic must be that they felt they were facing problems that were supernatural," it suggests that he does not even agree with himself.[37] In order to explain that people in the fifteenth century tried to end drought through magic, it is sufficient to understand that people *believed* it was a supernatural problem. In that manner, historians, entirely independent of postmodern trends, have explained premodern actions or, more generally, actions that from the historian's or the reader's point of view seem bizarre and irrational. Likewise, I would contend that to explain the Nazi persecution of the Jews and the Holocaust, it suffices to understand that Hitler and his followers *believed* that the Jews were subhuman and that they were to blame for many of Germany's problems. We need not agree with the Nazis. But in that case, nor do we have to agree with the French peasants of the fifteenth century. What we should do, however, is try to understand how they thought—both the Nazis and the French peasants.

My disagreement with Sandmo on this point pertains not only to whether we should, as he proposes, state that other people's descriptions are "true for them" even when we ourselves believe these descriptions to be wrong. The issue at hand also determines what sort of historiography we aim to promote. As historians we can be humanists as well as scientists. Indeed, if we fail to use the scientific knowledge we possess, we become feeble historians. For how shall we otherwise explain how peasants from later eras were

able to manage drought, or at least adapt to it? Reducing all historiography to the study of the attitudes and opinions of people from the past would be meaningless. Of course, part of the explanation lies in demonstrating how the people we are studying understood and explained their era and their actions themselves. As I read Sandmo, he contends that we should limit ourselves to this aspect, or at least he wants to do so for himself. But in order for us to achieve an adequate understanding of the topic we are studying—an understanding that is more profound and comprehensive than that of the historical people we are studying, who were limited by frameworks that no longer constrain us, such as the notion of witches and other types of supernatural phenomena, or who were less familiar with the laws of nature than we are—we must not declare *non grata* all knowledge that our objects of study lacked. A study of the plague in the Middle Ages that does not avail itself of modern-day knowledge of infectious diseases—obviously without limiting the study to outright epidemiology, for in *that* instance we are, in fact, dabbling in science—would be a highly lacking study in and for an era when the plague is, in fact, understood as an epidemic disease.

One thing is whether we should use our scientific knowledge when we explain the past. An entirely different matter is whether we should also use our culture's nonscientific concepts to explain social behavior in other—for example, historical—cultures. (Social behavior is, of course, not detached from how nature is perceived. We must nonetheless be allowed to set up an analytical divide here.) A point in Sandmo's doctoral dissertation on the seventeenth-century social coining or construction of the category of "violence," in our modern-day sense of the word, is that by using our own era's concepts to analyze a past that did not use this concept in our sense, we commit a sort of conceptual violence against that era. This is a viewpoint with which it is easy to sympathize, but which I nonetheless find problematic. Let me emphasize that studies on the coining of social concepts—whether "violence" or, for example, "housewife"—and studies of how such concepts in turn shape how people understand society and relate to it are, in my opinion, both legitimate and academically commendable, even though they are intellectually highly demanding, and even though such studies of the relationship between concepts and behavior, in my opinion, should not be the sole or the most important form of historiography. If we for the sake of argument accept Sandmo's (contested) claim that the modern sense of violence originated during the seventeenth century, must we then also accept a claim that violence in the modern sense of the word originated during the seventeenth century? I can hardly imagine that Sandmo believes that to be the case. On the other hand, in several passages he does formulate himself in a way that suggests that that is, in fact, what he means.[38]

And if Sandmo really means what he says, in my opinion, he makes things uncommonly and unnecessarily difficult for himself—and for other historians as well, to the extent his stance is intended to be generally applicable. As in the case with science, I fail to understand why we must put aside

the conceptual apparatus that the humanities and social sciences so strenuously have established. The discipline of history will be severely curtailed if we must abstain from using analytical terms coined in our own time, such as structure, function, and class, or discourse and habitus for that matter. We will be curtailed even more if we must also forsake a good many of the terms we surround ourselves with in everyday language. What I perceive as Sandmo's methodological credo will turn the conceptual universe of historical and foreign societies into a straitjacket that constrains our understanding of these societies, since we will be unable to analyze other actions and other thoughts than what our objects of study would be able to express in words. There must be a better way to try to mind our p's and q's and avoid the linguistic social imperialism that lies in projecting our own, modern-day concepts into the minds of people who lacked such concepts. That does not mean that we must abandon using the concepts of our own era as analytical tools. We must for example be permitted to use a concept such as "rape," understood as "coerced sexual activity," and study the incidence of such activity both inside and outside of marriage, including during eras—and it is not so long ago—when rape within marriage was in a sense a logical impossibility, given that coerced sexual activity in regard to spouses was not encompassed by the legal (and common) understanding of rape.

VII. Truth in Cultural History

I begin this final section with a general observation on the relationship between cultural history and other fields of history in connection with the debate on truth. Perhaps this can explain why many react so strongly against the postmodern relativizing of the concept of truth that Sandmo represents. In the type of cultural history that Sandmo advocates, reality does not seem to play any role. As a cultural historian, he is interested in what people believed and why they believed it. Whether these beliefs were accurate is irrelevant. This view of reality as irrelevant, as articulated in what is often referred to as new cultural history, is shared by many historians of ideas, historians of religion, and social anthropologists. It is also shared by, and is inspired by, the New Historicism movement within literary studies. I assume that the scant interest in reality is the reason why new cultural historians have shown such a keen interest in the poststructuralist conviction that texts should only (or can only?) be read as texts and not as expressions of reality. Reality lies outside of their field of research. For them, it does not matter one iota who started the Reichstag fire in 1933—rather, what interests them are the various takes on and stories about this fire. In other fields of history, however, questions that strongly refer to reality, such as who set fire to the Reichstag, are indeed highly relevant, to put it mildly. This must be the reason why political historians, economic historians, and social historians have largely refrained from following the new cultural historians over to the side of the poststructuralists. I assume that Sandmo would not insist that

the historians of reality must adopt his concepts, as long as he can use them himself as he so pleases. But it is not as easy as that, unfortunately. The moment that two (or more) mutually incompatible patterns of usage begin circulating with the same discipline—history, in our case—collisions and conflicts are inevitable. This is what has happened to Sandmo's attempt to introduce a new concept of truth, which entails that statements that are false within other fields of history can be true within the new cultural history.

As should be clear by now, Sandmo and I disagree on the concept of truth in cultural history. More precisely, we disagree on whether his concept of truth—one where what our objects of study consider to be true is deemed to be true, even though both Sandmo and I know better—leads to better cultural history than research based on a conventional concept of truth. Most of what I have read of cultural history—admittedly, a rather narrow sample—does not use Sandmo's concepts the way he does. I cannot see how Emmanuel Le Roy Ladurie's *Montaillou* becomes less valuable as a work of cultural history simply because he declares that his objects of study were wrong when they believed that leprosy was hereditary, or because he characterizes as superstition the idea that owls were "devils flying over the roof to carry away the soul" of the recently dead.[39] Nor can I see that Sandmo has explained exactly why that would be the case. It is in this context worth noticing Sandmo's comment that not even Stuart Clark, his lodestar in the debate on French peasants' understanding of drought in the fifteenth century, uses Sandmo's concept of truth.[40] Clark has in other words joined the growing ranks of deserters from the phalanx that Sandmo mobilized to defend his concept of truth.

What Sandmo is saying with his poststructuralist idea of truth can be said far less ambiguously with a conventional understanding of truth. Instead of saying that "for Norwegian villagers in the seventeenth century, it was honor that determined whether what a person said was true," one could say that "for Norwegian villagers during the seventeenth century, it was honor that determined whether what a person said could be considered to be true." The latter is, in fact, what Sandmo writes in his dissertation.[41] Thus, in this passage he uses an entirely conventional concept of truth in order to make the same point he otherwise introduces a new concept of truth to make. In so doing he demonstrates that one does not need a new concept of truth in order to analyze how a statement receives the status of truth—which is, of course, something entirely other than whether the statement is true in the sense of corresponding to reality.

We disagree about more than concepts, however. In my view, it seems as though Sandmo does not take his own situating of the truth seriously, when he insists on ignoring that he knows better than his objects of study. Does he really mean that he can study the mental universe of people from the early modern period without considering that large parts of this universe consisted of superstitions and mistaken ideas of how the world works? Does he actually believe that drought in the fifteenth century was magical, that

those who performed magic rituals were in league with Satan, that the sun revolved around the Earth in Galileo's era? No, clearly, he does not believe that. I am also sure that when Sandmo gets the flu, he does not explain it as being God's punishment for incorrectly having claimed that the word *vold* ("violence") was not used in Norwegian court protocols or legal codices before 1687.[42] Rather, he explains it scientifically, as an infectious viral disease. Why should we then pretend that we do not know better than people who lived before us? As historians of today we are informed by the modes of understanding that are prevalent in the rationalist West, and we write for people who share our horizon of understanding. If our research is to be relevant to our readers, we cannot deny our conviction that we understand more about how the world works than people before us did, though this, of course, does not mean that our understanding is final.

Sandmo shares with me the certainty that we know more than the people of the past did; he is only more modest about his superior knowledge than I am. I would contend, however, that it is precisely our superior knowledge that is a decisive heuristic tool when studying alien cultures. It enables us to decide what we want to study, namely, why they are not like us—including, not least, why their notions of reality differ so much from ours. If we had not been sure that those who were convicted of witchcraft were not in league with Satan, we would not have spent so much time and energy on understanding and explaining the witch trials. It is not least because we *are* sure that the witches had not done what they were accused of, namely, being in league with Satan and obtaining magical powers from him, that we study this part of cultural history so intensely—the notion that you can have children through sexual intercourse is subject to far less research.

This latter example has not been chosen simply because sex sells. There is a wider point about methodology here. Bronislaw Malinowski notes that Trobriand Islanders forbid sexual intercourse with women who are regarded as particularly repulsive because they are albino, feeble-minded, or suffering from serious skin diseases. When such women nonetheless have children, the Trobrianders take this as proof that virgin births can and do occur.[43] In the Sandmonian sense, virgin births are true for the Trobrianders. For Malinowski—and for Sandmo himself, I presume—virgin births are not true. Such a usage, where two logically contradictory statements are both true, could have spared the church plenty of trouble and turmoil, both in Galileo's era and today. In actual practice, however, another conceptual understanding and another heuristics are in play—also among postmodernist cultural theorists. When someone, such as Malinowski's Trobrianders, believes that you can have children without intercourse (or without modern artificial insemination), cultural theorists attempt to understand and explain what we cannot but help see as misguided ideas.

A key premiss in Sandmo's thinking is what he calls "the fundamental dissimilarity of the past." He views this dissimilarity "as something holistic: the cultures of the past are precisely cultures" and claims that "cultures

constitute contexts, systems where it is not possible to separate individual parts that are independent of the others."[44] This latter point is apparently "nothing less than the main premiss for cultural research." A related premiss is that "societies are cultures . . . their cosmologies are intertwined."[45] One problem with this understanding of culture is how we should differentiate cultures. Another matter is what it means that historical cultures are to be regarded as fundamentally dissimilar and as holistic systems. How dissimilar is *fundamentally* dissimilar? Are all the various parts so interconnected that *none* may be separated, and what should such a separation consist in? It is not easy to see how such a holistic concept of culture could be used to analyze conflicts between the church and heretics who held in some respects highly divergent worldviews. It must be even more difficult to explain changes in core beliefs. Before such changes take place, they often exist side by side with the notions they are on the verge of supplanting. Often they exist among close neighbors, or among members of the same family, or even as contradictory thoughts in the same mind. As I see it, we must have concepts that provide far more leeway for disagreements and conflicts and contradictions in people's worldviews than is permitted by Sandmo's totalizing concept of culture. If societies are cultures, and cultures are holistic systems, all conflicts of a certain scope become a struggle between different cultures. I cannot see how such a premiss would lead to fruitful historiography.

Let us take the question of whether witches existed, which can be articulated as follows: Shall we believe that witchcraft has taken place in a given situation because the witches say so? Sandmo replies in the affirmative, that is, he replies (a) that the belief in witches was widespread during the seventeenth century, (b) that many people had in fact performed the magic rituals they were accused of, and (c) that anyone who requires proof that the witches *actually* were in league with Satan will never be able to understand anything at all about "seventeenth-century culture or about the network of power and culture that surrounded the witch trials."[46] In contrast, my answer is no, we do not have to believe witchcraft has taken place just because the witches say so. My arguments have been written for me by the lawyer Alonso de Salazar Frías, who claims that one clearly cannot believe in witches "unless the case can be proven by external and objective evidence." However, he adds,

> who can accept the following: that a person can frequently fly through the air and travel a hundred leagues in an hour; that a woman can get out through a space not big enough for a fly; that a person can make himself invisible; that he can be in a river or in the sea and not get wet, or that he can be in bed and at the *aquelarre* at the same time . . . and that a witch can turn herself into any shape she fancies, be it housefly or raven? Indeed, these claims go beyond all human reason and many even pass the limits permitted by the Devil.[47]

What makes this so fascinating—and also so problematic for insisting on holistic cultures and the fundamental dissimilarity of the past—is that Salazar was one of three inquisitors at work in Logroño in Spain and that his rationalist apology stems from 1612. His two colleagues in the local Inquisition disagreed with him entirely. All three of them were priests, but whereas the other two were theologians, Salazar was a specialist in canon law. The two theologians based their evaluations of the defendants' testimony on demonological theory. Salazar, on the other hand, "carried out an investigation . . . in such a rationalistic way and with such purely empirical methods that it can be considered as the first systematic study in the history of witchcraft belief, revealing its social and psychological foundations and, needless to say, demonstrating the non-existence of witches," Gustav Henningsen states in his doctoral dissertation about this witch trial.[48] Salazar was far from alone in his views; he was, in fact, supported by the Spanish Grand Inquisitor, who was far less eager to pile up wood for the stake than, for example, his French colleague was.

The witch trial in Logroño illustrates the problems with Sandmo's notion of the holistic cultures and fundamental dissimilarity of the past. Henningsen's work also illustrates how our—in this case, Salazar's—superior knowledge can serve as a heuristic tool, allowing us to search for other ways of explaining the vast number of confessions and accusations of witchcraft than that they were true. Salazar found a "dream epidemic" among children and youth in particular, featuring "indoctrination, stereotyped dreams, forced confessions."[49] Postmodernists following Sandmo would have found witches.

Acknowledgments

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Notes

1 Sandmo, "Michel Foucault som maktteoretiker." All translations from Norwegian are my own unless otherwise stated.
2 Sandmo, "Sannhetens historie," 25.
3 Sandmo, *Tid for historie*, ch. [6], "Den uoppnåelige sannheten," 177–208. At the time of writing, the book is translated into Danish, Swedish, and Serbian.
4 Gundersen, *Da postmodernismen kom til Norge*, 305–310.
5 Sandmo, "Slagsbrødre" (published as Sandmo, *Voldssamfunnets undergang*).
6 Dahl, "Om 'sannhet' i historien"; Kjeldstadli, "I hvilken forstand kan vi snakke om sannhet i historie?"; Sandmo, "Mer og mindre sannhet."
7 Sandmo, "Mer og mindre sannhet," 390, 391.

8 Sandmo, *Voldssamfunnets undergang*, 88.
9 Sandmo, *Voldssamfunnets undergang*, 262.
10 Aristotle, *Metaphysics*, 7.
11 "Descriptions" should here be understood as descriptive statements. I thus use this term in contrast to normative statements and not in contrast to explanations, which in my terminology here is a type of description.
12 Sandemose, *A Fugitive Crosses His Tracks*, 413–414. According to Eriksen and Hessen, *Egoisme*, 15, Rudolf Steiner used the same example, only in a more moderate version, as a reply to the question, "[W]hat is truth? . . . He answered by asking the counter-question of what the truth was about a mountain. One can approach the mountain from the north, south, east, west, or above, if one wants to see five different things. All are true, even though they are different." In other words, both literary fiction and anthroposophy, albeit to different degrees, understood the liberality of the principle of noncontradiction.
13 Sandmo, "Mer og mindre sannhet," 395.
14 On descriptions that are not fair, see McCullagh, *Truth of History*, 33, 57–61.
15 Reality would, of course, be slightly different if we had not been part of it; in that regard, it is not entirely independent of us. The point is that it would have existed also without us, even though it would have been marginally different.
16 Kjeldstadli, "I hvilken forstand kan vi snakke om sannhet i historie?," 379. Incidentally, McCullagh, *Truth of History*, 26–27, bases his arguments on Hilary Putnam's understanding of truth, which Ottar Dahl also follows; see Dahl, "Om 'sannhet' i historien," 367.
17 "A true description asserts the existence of things in the world which will regularly produce perceptions within an appropriate range of kinds, under normal conditions, within a specified culture. . . . When it comes to historical descriptions, they are true if there *were* things in the world which *would have* produced perceptions of the appropriate kind under normal conditions for the members of the culture which made those descriptions." McCullagh, *Truth of History*, 30, italics in the original.
18 McCullagh, *Truth of History*, 26–27.
19 McCullagh, *Truth of History*, 30.
20 McCullagh, *Truth of History*, 30.
21 McCullagh, *Truth of History*, 42, cf. 39, were he writes in explicit opposition to Barthes and Derrida: "Sentences about the past mean, among other things, that there was something such that, if *you* had been there to perceive it, *you* would have had experiences of certain kinds. . . . Historical descriptions are true if it is the case that, had *anyone* been present, then things in the world would have produced the perceptions which the sentences imply." Cf. also p. 56: "To call a statement of the world true means that if *someone* were in the relevant position, they could perceive the events it describes, or perceive evidence from which the events could be inferred." My italics.
22 McCullagh, *Truth of History*, 42–43.
23 Of course, culture, in the sense of conceptual context, limits both what we perceive and how we explain what we have perceived. My aim is not to eradicate the concept of culture from the vocabulary of social scientists but, rather, to curb its overuse. The more a concept is meant to encompass, the less substantive it becomes.
24 Kjeldstadli, "I hvilken forstand kan vi snakke om sannhet i historie?," 386.
25 Sandmo, "Mer og mindre sannhet," 393.
26 Sandmo, "Mer og mindre sannhet," 390.
27 McNeill, *Rise of the West*, 213–215.
28 Bowler, *Charles Darwin*.
29 Sandmo, "Mer og mindre sannhet," 391. Radical postmodernists are not known for their scientific insight. It is tempting to poke fun at Sandmo's reference to

science by drawing a parallel to Alan Sokal's two-staged attack on French poststructuralists' veneer of scientific knowledge, where he followed up his landmine of a hoax article "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity" in *Social Text*, 46/47 (Spring–Summer 1996), 217–252, with the full-frontal assault of his book *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* (New York: Picador, 1998). Sandmo ("Mer og mindre sannhet," 388) and I agree, however, that it would hardly benefit historical research if the debate on the postmodernist challenge to the discipline were to be about French literary theorists and their scientific understanding (or lack thereof). I would nonetheless contend that scientific understanding should not *a priori* be deemed irrelevant for evaluating what is solid linguistics, philosophy, or historiography.

30 With one reservation, namely, that "It is true that . . ." sentences make explicit what is only implicit in sentences without this prefix, that is, that one regards the rest of the sentence as being true. See Tallis, *Not Saussure*, 249.
31 See McCullagh, *Truth of History*, 50.
32 Sandmo, "Mer og mindre sannhet," 390.
33 Karl R. Popper anticipates this argument in *Logic of Scientific Discovery*, 276n3.
34 Sandmo, "Mer og mindre sannhet," 391.
35 Sandmo, "Mer og mindre sannhet," 391.
36 Sandmo, "Mer og mindre sannhet," 391.
37 Sandmo, "Mer og mindre sannhet," 391.
38 Sandmo, *Voldssamfunnets undergang*, *passim*, esp. 169–171, 211–212, 246–247. I have come to appreciate the fundamental poststructuralist insight that the conceptual content of words can never be determined with complete precision. In a sense, words acquire a new meaning every time they are used. No author can control the readers' connotations. But that does not relieve authors of their responsibility to try to explain as precisely as possible what they mean by the words they use. If we are interested in studying changes in real-life entities, for example, the occurrence of a phenomenon such as drought, the concepts that we use must be kept fixed and constant. A corresponding point applies if we are interested in studying changes in the use of concepts, in which case we study the same phenomenon over time.
39 Ladurie, *Montaillou*, 49, 42.
40 Sandmo, "Historie som fasit," 407.
41 "In those cultures I have studied, it was honor that determined whether something a person said would be understood and considered as being true." Sandmo, *Voldssamfunnets undergang*, 259.
42 Imsen, "Kunsten å konstruere," 483–486.
43 Malinowski, *Sexual Life of Savages*, 292–293.
44 Sandmo, "Historie som fasit," 409, 406.
45 Sandmo, "Historie som fasit," 406.
46 Sandmo, "Historie som fasit," 413.
47 Henningsen, *The Witches' Advocate*, 350, italics and ellipsis in the original.
48 Henningsen, *The Witches' Advocate*, xxvii.
49 Henningsen, *The Witches' Advocate*, 140; Henningsen, "The Greatest Witch-Trial of All," 39.

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4 In Defense of Objectivity

Facts and Theory Choice in Historiography

In a 1993 review article Raymond Martin laments that "after a full century of critical philosophy of history, we still do not know how historians do, or should, decide among competing historical interpretations." Martin finds it "rather surprising, perhaps even an intellectual scandal, that we philosophers of history have never even made it one of our central preoccupations to find out."[1] This chapter is an attempt to remedy this sad state of affairs or, rather, to show that, in fact, there exists considerable agreement on criteria for theory choice. This agreement, although to some extent tacit and unappreciated, is critical in the effort to keep extra-cognitive values in check and make historiography an objective science—albeit with certain limitations that cannot be overcome and ought to be acknowledged.

Beginning with the limitations, my point of departure is the assertion that strict objectivity is unattainable for historians. Having substantiated and nuanced this hardly controversial claim by discussing four putative impediments to objectivity in the first part of the chapter, I proceed in part II to an assertion that is perhaps less in vogue, namely, that facts nevertheless are of utmost importance to historians since they enable us to distinguish historiography from propaganda. Thus, facts are essential to establishing history's position as science and, consequently, to much of its social value. Part III contains a description of the rules for scrutinizing and assessing evidence that scholars follow to determine what are historical facts: rules that have shown themselves so effective that no conscientious historian would question them, and not even avowed poststructuralists divert from them in their practical research. This is followed in part four by an analysis of an element of historians' practice that is often overlooked by avid empiricists and narrativists alike, namely, the standards scholars employ in order to assess competing theories: whether in the form of explanatory interpretations, narratives, or other kinds of syntheses. These criteria carve out a place for historiography insulated from political, moral, religious, and aesthetic values, giving scholars an important social role to play by establishing and expanding the room for societal consensus on contested matters of more than facts. This room is not stable. The chapter concludes with a brief discussion of why, and to what extent, it is smaller in contemporary history

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than in those parts of the discipline that more easily can keep (temporal) distance from current political affairs.

I. The Unattainability of Objectivity in Historiography

It is widely accepted that strict objectivity is epistemologically unattainable for historians, no matter how conscientious they are. I shall not contest this basic philosophical tenet, the implication of which is that all historiography is to some extent political in a wide sense of the term, taken to include (moral) values and worldviews. To ascertain to what degree the ineluctably political dimension of history influences the discipline's possibilities to create uncontested descriptions and interpretations, however, we must analyze what prevents history from being an objective science such as geology or meteorology. Of course, the objectivity of sciences such as geology or meteorology is not uncontested, to say the least. Quine and Kuhn shook the objectivist pretentions of science beyond repair.[2] The views of these and other critics of the belief in objective science have met their share of criticism but we need not enter into that discussion,[3] since all I claim is that there are additional, and severe, impediments to objectivity in history—if perhaps less severe than many think.

I discuss four reasons why historiography is epistemologically barred from strict or total objectivity: what I term *global objectivity*, where the perspective is irrelevant. Two have to do with historians' preferred format of exposition, namely, narrative; three have to do with the way historical phenomena are described (and two plus three equals four since the second reason has to do with both format and way of description). Whereas none of the challenges are exclusive to historiography, historians, if not alone in being up against all four, at least are more heavily challenged by them than most scholars within other disciplines.

Challenge 1: The Independence of Narrative (H. White)

The first, and today probably the most well-known, challenge to objectivity in historiography is that facts never determine the narrative. With facts I mean singular descriptive statements that everybody with knowledge of the subject matter and the idiom used to describe it accepts as true descriptions.[4] It is a fact that King James's Bible contains four gospels, whereas it is not a fact that the Bible is the word of God. (The latter may be true, but it is not a fact.) Historians make use of facts when composing narratives. Although facts delimit the range of acceptable interpretations by putting a cap on the liberty of historians to narrate events or processes, very few academic historians would argue today that the facts of their subject matter entail a specific narrative. Since the publication of Hayden White's *Metahistory* in 1973, the significance of literary emplotment for the meaning imputed on events by the narrative has become part of historians' theoretical *doxa*.[5] White and other narrativists have reminded historians that the room for varying, and

in some ways also conflicting, narratives is considerable, making the establishment of facts less important to historians' work—and certainly to their literary products—than convention had it prior to the narrative turn.

White was particularly interested in the role literary tropes played in providing meaning to historians' narratives. According to his analysis, whether an account was dominated by metaphors, metonymies, synecdoches, or irony would determine its character as, respectively, romance, tragedy, comedy, or satire, and hence its meaning to readers. How conscious is an author's choice of dominant trope was never clear from *Metahistory* (or from White's later writings), but there is no doubt that the choice depends upon the historian as much as upon the facts the historian is narrating.

It may be debatable whether White managed to prove a constitutive relationship from tropes to modes of emplotment (romance, etc.), and his views have perhaps been more influential than their epistemological status warrants.[6] And yet his major achievement has been to decouple the narrative from the facts, and to show how important the mode of writing is to the meaning. A simple example of how the choice of words produces different meanings, less sophisticated than White's analysis but with great illustrative power, is the different tags for the American Civil War—a.k.a. the War between the States, the War of the Great Rebellion, the War for Southern Independence, the War of Southern Aggression, the Second American Revolution, and so on.[7] The upshot of White's insight is a greater role for the historian as the person who chooses not only which facts to emphasize but also the theoretical lense through which to view them, and how to colligate, synthesize, and narrate the facts. That the persona behind the historian—and hence his or her worldviews, values, and opinions—is important to historiography had, of course, been argued long before the publication of *Metahistory*. E.H. Carr's famous dictum, "Study the historian before you begin to study the facts," is a succinct formulation of the view that history is not an objective science.[8] But the continuation of the quotation is important: "the historian will get the kind of facts he wants."[9] This shows that despite his renouncement of objectivism, Carr and his contemporary relativist historians had yet to take the linguistic turn that White took away from the belief in the determining role of facts. Carr thought that facts determine the account; he just thought that the historian would select the facts that suited his (or her) purpose.

What White's narrativism leaves intact is the potential for making non-narrative, factual descriptions of limited scope. We can produce descriptive sentences without recourse to metaphors or other tropes. Such descriptions abound in historiography—which is not to say that historical accounts are free of tropological elements, only that the metaphors are not all-invasive. The import is that, even granted the power of tropes in imbuing a narrative with meaning, White's analysis reduces but does not eliminate the room for objectivity in historiography.

Challenge 2: Narrative Sentences (Danto)

The second challenge to historiographical objectivity has to do with a different kind of narrativism from that of White. The point of departure is Arthur Danto's analysis of narrative sentences: descriptions that depend on and presuppose information that is posterior to the event or process that is the subject of the sentence. Danto's example is "the Thirty Years War began in 1618," since no one in 1618 could know that what broke out would be known as the Thirty Years' War.[10] Another example would be "Columbus discovered America in 1492," since the continent upon which he landed, the existence of which was unknown to Europeans at the time of his journey, was not yet named after the Italian navigator Amerigo. The point is not that narrative sentences of this kind are not true, but they cannot even be formulated (unless by a wild guess) at the time of the events depicted.[11] Not even Danto's Ideal Chronicler, the omniscient observer recounting every detail of every event unfolding at every moment, would be able to formulate such narrative sentences to describe the events witnessed, since their formulation depends upon information that comes about at a later point in time.

The consequence of Danto's seemingly innocuous observation is far-reaching, since it means that the description of events in history is always provisional, always deferred, never stable. If the way in which events can be described cannot be fixed but is subject to the possibility of perennial change as subsequent events unfold and change the description of earlier events, then in one sense the notion of objective descriptions must be discarded, since new developments can always imply new descriptions of the original event. Also, the description of an event will depend upon which of a potential plenitude of subsequent events an author will pick to describe the original event, and there is no way to determine which description is to be preferred.

Danto's analysis shows that descriptive closure is not to be had by narrative sentences or, rather, that their potential for creating ever-new descriptions of events precludes closure. We must not forget, however, that for all their constructive potential narrative sentences do not have the same destructive power. What new narrative sentences cannot do is invalidate all previous narrative sentences. (It is, of course, conceivable that subsequent events may show that some earlier descriptions were false or at least severely misleading.) It is the completeness, not the truth, of descriptions that the creative potential of new narrative sentences makes illusory. This makes them less disastrous for historiographical objectivity than they first seem. It is only the notion of a final, inaugmentable description that must be discarded. Global objectivity is a chimera. But the possibility of local objectivity exists despite the creative potential of narrative sentences.

Challenge 3: Interpretative Indeterminacy (Davidson)

The third challenge to historiographical objectivity concerns actions: intentional, meaningful behavior. Donald Davidson has pointed out that accounts of actions must always be under a description and that there is always room for more than one description of one and the same action.[12] Since actions are intentional behavior, they have an inner or internal aspect. Since there is no way to ascertain the content of the internal aspect, an action can only be approached through its outer aspect, leaving its determination uncertain and hence leaving room for alternative descriptions. Out goes objectivity, since it is impossible to determine objectively what action a certain behavior represents. Under one description, U.S. president George W. Bush's efforts to get his country to go to war against Saddam Hussein's Iraq were an earnest (if, as it turned out, unnecessary and misguided) attempt to eliminate weapons of mass destruction that Bush feared Saddam might use—at least as a threat—against Israel. Under another description Bush grasped an opportunity to fulfill a long-standing ambition to topple Saddam and achieve what his father had been unable to when he was in office.

When every action is under a description and we know there are alternative (though not necessarily contrasting) descriptions available, global objectivity is lost for this part of historians' accounts. This leaves what we again might term *local objectivity*: within the realm of the specific description under which the action is described. And whereas there can be other descriptions of an action (which strictly speaking makes it another action, since the intention is part of the determination or definition of the action), taking behavior under a certain description as a point of departure at least provides the ground for analyzing or explaining this action. The possibility of objective historiography of specific actions is thus retained, although the objectivity pretensions are hedged by our knowledge that its validity is restricted to a particular description among several alternatives.

Challenge 4: Events as Constructs (Roth)

The last challenge to historiographical objectivity that I want to discuss shares some traits with the obstacle raised by Danto's narrative sentences as well as by Davidson's interpretative indetermination but poses an even more radical challenge to objectivity. In one sense it is the most far-reaching of all four, but as such it is overstretched. Paul Roth's analysis of the description of events claims that every such description has an indelibly subjective element.[13] The idea is that the linguistic constitution of a phenomenon as a specific event is never objectively given but depends on the person describing it. Take Gavrilo Princip's assassination of Franz Ferdinand in Sarajevo on June 28, 1914.[14] Is the young Bosnian Serb's firing of the shots that killed the archduke and his wife one assassination event or several minor events—Princip's taking up position along the route, his surprise when the

car carrying the Habsburg heir pulled up in front of him, his bringing out his gun, his pointing it at Franz Ferdinand and Sophie, his pulling the trigger, the bullets' journey along their trajectories, their entering the victims' bodies, and so on—or should it be described as part of a wider process that includes the formation of the Black Hand group, Princip's recruitment into it, its place within Serbian nationalism, and even wider ramifications? How are events and processes to be demarcated? How fine-grained should our accounts of what goes on in the world be? Are what we call events nothing but linguistic or narrative constructions? If so, their description loses all claim to objectivity, since the way in which events are described—and hence what counts as or constitutes an event—is determined by the narrator. (This need not entail antirealism, since the claim pertains not to the existence of things in the world, only to the description of phenomena as events.)

"Events exist only by proxy," says Roth, "knowledge of events is restricted to happenings isolated under descriptions provided by interested parties."[15] He is certainly right in his insistence that there is no given level at which a description of what goes on counts as an event. But accepting that the workings of the world can be described adequately on several levels from the subatomic via the molecular to the level of everyday parlor and beyond at ever-greater syntheses, and that there is no way of determining the "correct" or best level, need not entail an embrace of subjectivism other than in the choice of descriptive level. And I think historiographical objectivity can live with this kind of subjectivity. Once the level is (subjectively) set, Roth's radical challenge to objectivity loses its force. As with Danto's narrative sentences, the subjectivity involved in descriptively carving up in events the stream of goings-on in the world invalidates any claim to descriptive closure but does not affect the possibility of providing objective descriptions of the event thus selected. After the descriptive unit—the event—is chosen, the scene is set for local objectivity to come into play.

It is telling that Roth illustrates his assertion of the indelible subjectivity of events by means of Akira Kurosawa's 1950 movie *Rashomon*, in which a story of rape and killing is told by three parties: the victimized wife, the bandit who rapes her, and her samurai husband who is killed. That Roth has ventured into the movie theater to find an example of his views on events should make us wary, since it may indicate that historiography does not provide instances to serve his purpose. Not even the realm of fiction is supportive of his views, however. Roth observes that "none tells the same story; indeed, their stories are inconsistent."[16] But they are not inconsistent in virtue of the agents recounting the same behavior under different descriptions and thus describing different actions. Nor do the event-units selected by the three protagonists differ much in size or shape, only in substance. The parties recount different *behavior*, thus making it a story of different—and incompatible—events. Roth's contention that "each person's story in *Rashomon* involves, *ex hypothesi*, no alteration of the facts" is false.[17] The example shows the opposite of what Roth asserts,

since Kurosawa in order to make the stories inconsistent had to change the behavior of the agents—even to the extent of the samurai being killed by a different person in each version: the bandit claims he killed the husband, the wife claims that she killed her husband, and the husband's spirit through a medium claims that he killed himself.[18] It is this alteration of the facts as the story is told and retold, not the agents presenting the same actions under three different descriptions or the body of happenings being cut at different joints, that creates three different events. Moreover, and unmentioned by Roth, the movie includes the account of a fourth person: a woodcutter who says he witnessed the drama and who may have been implicated in it. The complexities of the movie need not bother us too much here; suffice it to say that it is an example of contradictory testimonies, not of the logical inattainability of objective description.

Instability—and So What?

The four challenges discussed here make strict objectivity in historiography an illusion. Each represents an impediment to presenting globally objective theories or narratives of past phenomena and processes. Local objectivity is salvaged, but global objectivity must be discarded. There is a basic epistemological instability to historical descriptions. The question is what this instability entails.

Several writers draw far-reaching conclusions from the futility of attempting global objectivity, typified recently by Anton Froeyman. He claims that the narrativist exposition of the illusionary character of historiographical objectivity entails that "we arrive at a view of historians as engaged intellectuals, partisans of a political cause." According to Froeyman, narrativists such as Hayden White and Frank Ankersmit, inspired by poststructuralist thinking, have shown that "for every given set of facts, there are always several possible narratives that are equally true. Hence, the choice of one narrative over another . . . is always determined by personal, ideological preferences."[19] But the "hence" is unwarranted. Froeyman's wide-ranging conclusions do not follow from their premisses, since theory choice—including the choice of one narrative over others—can be informed by criteria other than political or ideological preferences. As we shall see, historians have access to tools that can prevent historiographical analyses from falling prey to subjective worldviews and values, be they religious, political, or otherwise ideological. What distinguishes historiography from propaganda is a disciplinary code and a set of cognitive values that, while unable to save global objectivity, make historiography a truth-tracking science. This provides a ground that enables historians of different religions, political bents, or ideologies to decide which among competing accounts to prefer. But before I get to theory choice, I want to upgrade the importance of facts.

II. Facts Matter

It may seem trivial but it needs to be restated: facts are basic to historiography. Despite notions of the theory-dependence of data—notions that I do not intend to dispute on a philosophical level but want to cut down to the small size that I think appropriate on a pragmatic level—historiography is littered with facts, large and small, which no one ever disputes (which is what makes them facts) and which have enormous consequences for the evaluation of the accounts based on them, and sometimes for our ideas of how the world works. A few cases where we do not know the facts serve to illustrate this point. Think of the potential ramifications for our views of U.S. society and politics if one could ascertain that John F. Kennedy was murdered on the request of the Central Intelligence Agency—or the KGB, or the Mafia, or Lyndon B. Johnson, etc.[20] Or to use the case discussed in Chapter 2, whether officers within the Danish intelligence police were correct when they suspected that journalist and nuclear disarmament activist Jørgen Dragsdahl was a KGB agent of influence has significant consequences for our views on the establishment and workings of the 1980s peace movement in Scandinavia.

In practice, of course, the epistemological status of historical facts is taken for granted. No one disputes facts such as the involvement of Brute in the assassination of Caesar or Gavrilo Princip's role as the man who fired the shots that killed Franz Ferdinand in Sarajevo. The significance of these facts for the fate of the Republic or the outbreak of World War I is a related but different matter. Likewise, to stay with the downfall of empires but stray from murders, very few would dispute the purported fact that Mikhail Gorbachev was central to the policy of *glasnost* and *perestroika* that led to the demise of the Soviet Union. One can, of course, argue that the downfall would have occurred anyway. My point is not to claim that the exact role and significance of facts like these are beyond doubt but that the facts as such are never questioned and probably never will be. Nor will anyone dispute that they had causal import, although there may exist different opinions as to their exact significance. History is full of facts that matter. It is worth noticing that even such an avowed historical constructivist as Leon Goldstein, although reserving the notion of historical facts for other purposes, states that "for all the rivalry and jealousies with which historians are affected, for all the spite and pettiness which sometimes mar their disputes, a large and growing body of established historical truth, while always subject to correction, is nonetheless agreed to."[21]

Another example can serve to underline the importance of historical facts, as well as their potential political sensitivity. In the spring of 1940 the Soviet secret police NKVD executed almost twenty-two thousand Polish prisoners of war, mostly officers, policemen and other elites, who had been interned in the three Russian concentration camps at Ostashkov, Starobelsk, and Kozelsk and in several NKVD prisons in

Belarus and Ukraine when the Soviet Union and Germany invaded Poland in 1939. The killings have become known as the Katyn Massacre after the forest where mass graves of the executed internees from the Kozelsk camp were buried. The Germans discovered the Katyn mass graves in 1943 and tried to exploit the massacre to drive a wedge between the Soviets and Stalin's Polish and Western allies, but Moscow retorted with the claim that the killings had been at the hands of the German Army. Despite prodding by the Polish anticommunist government in exile, the U.S. and British leadership during the war and immediately afterward refused to look into the matter or to accept the mounting (but not public) evidence that the perpetrators were the Soviets. Instead, Western authorities were adamant on putting a lid on the affair. The Soviets for their part kept blaming the Nazi Germans until eventually, as a product of Gorbachev's *glasnost* policy and the subsequent end of the Cold War and Soviet communist power, the Kremlin leadership in 1990 accepted Soviet responsibility and professed remorse, although they have refused to concede to Polish demands that the massacres be deemed an act of genocide.[22]

What the Katyn Massacre story shows—apart from the ruthlessness of Stalin and his associates—is not only how politically sensitive historical facts can fall prey to great power politics and propaganda but also how significant (and difficult) the uncovering of such facts can be. To establish beyond reasonable doubt the blame for the elimination of a large part of the Polish military and civilian elite has been a work that has called for tenacity in the search for sources and critical scrutiny of the evidence, including the evaluation of conflicting testimony and forged documentation. And the significance of the findings for the valuation of the Soviet political system is clear to all, and is underlined by the communists' attempts to hide the truth. In instances such as this, facts matter a lot. And facts such as the Katyn Massacre constitute the basis for more general or overarching theories on Soviet communism: the theory supervenes on the facts, whereas the facts are independent of the theory in question. There is no symmetry of dependence: the facts are basic.[23] (This is not to say that historians never get the facts wrong. But when they do and someone finds out, they are not let off easily: anyone who has read academic book reviews or had an article undergone peer review knows how scholarly nitpicking may act correctively.) Differing valuations or interpretations of the Communist rule during Stalin's reign do not change the facts of the Katyn Massacre. At most, different worldviews can make researchers unwilling to consider the facts, or make them want to see the facts in a different light and value them differently, for example, as acts of political expediency or necessity instead of as a lack of respect for human life. Differences of terminology may remain, such as whether genocide is an accurate term for the actions of the Soviets at Katyn. But conscientious historians can agree on the facts.

III. The Establishment of Facts: Conscientious Historians' Code of Conduct

Normally historians' travails to establish facts large and small are hidden in the confines of their offices and footnotes and in methodology textbooks. But at times their code of conduct is brought out in the open. This is when issues of great impact and public interest are at play, typically court matters such as the libel suit Jørgen Dragsdahl filed against Bent Jensen in Denmark in 2007, treated in Chapter 2, or the even more notorious libel case Irving vs. Penguin Books and Deborah Lipstadt in Britain in 2000. In the latter, high-profile Holocaust denier historian David Irving sued his U.S. colleague Lipstadt and her publisher for defamation because in her book *Denying the Holocaust* she had accused him of deliberately distorting the evidence.[24] The judge acquitted Lipstadt in a verdict that smashed Irving's credentials as a historian. Of relevance here is not the outcome but rather Justice Charles Gray's grounds for deeming Irving an unfit historian despite his indubitable command of vast amounts of evidence relating to the Nazi concentration camps of World War II. In concluding that Irving had refused to accept the facts of the Holocaust, the justice based his ruling on the exposition by defense witness professor Richard Evans of what it meant to be a conscientious historian—and how Irving had failed to meet this standard. Better than any textbook on historical methodology, Evans's statement, as rendered by the justice and summarized by Wendie Ellen Schneider in her Case Note in *The Yale Law Journal*, sums up the code of conduct that historians must abide by when evaluating evidence in order to establish historical facts.

First, the conscientious historian must "treat sources with appropriate reservations," that is, be critical of all purported evidence. Second, counterevidence must not be dismissed "without scholarly consideration": criticism must not turn into hypercriticism of evidence that goes against the theory favored by the historian. Third, the conscientious historian "must be even-handed in her treatment of evidence, and eschew 'cherry-picking'" by selecting or inflating the significance of evidence that points in the preferred direction. Fourth, historians must clearly indicate if and when they engage in speculation. Fifth, and fairly obvious, the historian "must not mistranslate documents or mislead by omitting parts of documents." Sixth—and this seems like a restatement, by inversion, of the second and third rule—the historian "must weigh the authenticity of all accounts, not merely those that contradict her favored view." And seventh, the conscientious historian "must take the motives of historical actors into consideration."[25]

These rules are variations of conventional source criticism in the tradition of Ranke. One standard element seems curiously to be missing, namely, the duty to scrutinize and weigh the plausibility of what the evidence says. Perhaps this was thought to go without saying, or maybe it was left out of the courtroom because what was at case was not whether Irving was a bad historian but whether he was an *unfit* historian because he deliberately

misrepresented the evidence. When we move from the courtroom to the conscientious historian at work with establishing facts, the evaluation of plausibility plays an important role. So too does an evaluation of the representativity of the evidence which goes beyond even-handedness. It includes active attempts to assess whether the evidence available is representative; if it is not, to try and remedy this deficiency; and, if despite our efforts we are left with a skewed body of evidence, to take such deficiencies into account.

Two additional elements must be included to make complete our enumeration of the duties of a historian seeking to establish facts. Both of these involve more imagination and considered judgment than those mentioned so far, but I shall nevertheless claim that their inclusion in the toolbox of the *bonus pater* historian is beyond dispute. The first has to do with optics, or rather viewpoints, and might be anchored philosophically in Gadamerian hermeneutics though it has been part of historians' methodology long before Hans-Georg Gadamer presented his analysis of the hermeneutic circle.[26] The gist of it is that the conscientious historian has to endeavor to view situations, threats, and options as seen by the historical actors. Of course, historians cannot erase completely from their minds their superior knowledge of the historical situation and its consequences. "No perceiver, however immersed in the past, can divest himself of his own knowledge and assumptions," observes David Lowenthal.[27] One need not read many first-class historical biographies, however, before noticing that the best historians come impressively close to seeing the world through the eyes of their actors. One part of historical understanding hinges on this kind of reenactment, to use a notion that points in another philosophical direction from hermeneutics—namely, that of R.G. Collingwood—but whose insistence upon the necessity of taking the actors' point of view is similar in its consequences for the historian.[28]

At first glance the other element of the pair of demands on the conscientious historian that transcend source criticism looks like a contradiction to the hermeneuticist insistence on attempting to fuse the historian's horizon with that of the historical actors. I am referring to the need to move beyond the horizon of our study objects by including as much context as adds to our understanding of the situation—which normally is much more than the actors could ever possibly be cognizant of—and by using concepts that are meaningful and useful today but did not exist (or were understood differently) at the time we are studying. In one way this is making a virtue out of necessity, since every time we write a narrative sentence, as defined by Danto, we make use of privileged knowledge vis-à-vis our objects of study. Employment of such insights permits understanding that was beyond the reach of our objects of study. To quote Lowenthal again: "Modern interpretations of past events are both more intelligible to moderns and psychologically 'truer': 'charisma' better explains the rise of a dynasty than the relics it possessed, although

people at the time believed in relics and would have found charisma incomprehensible."[29]

So, on one hand, the historian must try to see the world as the actors saw it in order that we understand their plight and points of view; on the other, he or she must employ his or her superior knowledge in order to show readers the true situation of the actors, which tends to differ from the situation as seen by the latter. This surely is a challenge, but historiography is a demanding métier. This ability to move seamlessly in and out of the actors' worldviews and interpretations is one of the properties that distinguish the master historian from the mass. And yet if historians are crafty enough, they can establish facts—descriptive statements that everybody with knowledge of the subject matter and the idiom used to describe it accepts as true descriptions—not only by scrutiny of sources but also by zooming in and out of the actors' minds and mind-sets.

IV. Criteria for Assessing Historical Interpretations

The code of conduct outlined above can only bring historians so far: namely to first-order factual statements. Not that this is anything to be shy about: as we have seen, the establishment of historical facts is of great import. But historians tend to have bigger ambitions. Normally they aspire to produce accounts of higher order: colligations, interpretive explanations, theories, narratives, syntheses. The list indicates an epistemological hierarchy in which the latter accounts are further removed from facts than the former, although the exact position of each type of account is up for discussion and may be hard to pin down. In recent decades the notion that all truly historical accounts are narratives has dominated philosophy of history. While admitting that narratives are important, I agree with Jouni-Matti Kuukkanen that the significance of this format has been overstated lately.[30] Historiography can take other forms than narratives and still be historiography; that is, narrativity may be a dominant trait of much historiography but is not constitutive of it.[31] Not only the establishment of singular facts but also colligation, explanation, the presentation and discussion of a theory, and a broad synthesis need not be fitted into a narrative straitjacket—indeed some such accounts can hardly fit a narrative at all.

My ambition here is not to work out a taxonomy of formats of historiography but to examine the criteria historians use for comparing and assessing alternative, sometimes conflicting interpretations, whether these interpretations have narrative form (or perhaps an underlying narrative structure) or not. My contention is that standards exist and are in constant use and that their application, while admittedly insufficient to establish positively true and globally objective accounts, prevents historians from falling into the abyss of epistemological relativism.

Criteria for Theory Choice in Science

It is one of the ironies of history that one of the first to present the standards by which interpretations are gauged was the scholar who arguably has been the one most responsible for the surge of relativism in science studies in the last half-century, namely, Thomas Kuhn. In his 1973 paper "Objectivity, Value Judgment and Theory Choice" Kuhn discusses criteria that scientists employ to choose between rival theories. Kuhn wants to show that scientists do not decide which theory to prefer over a competitor by means of some algorithm but by value judgment: "the criteria of choice . . . function not as rules, which determine choice, but as values, which influence it."[32] From the vantage point of historiography or some other social science after the onslaughts of antipositivism followed by that even more amorphous movement termed postmodernism, the idea of choosing among various theories on the basis of some algorithm sounds quaint. My point in invoking Kuhn is to remind readers that even he, who did so much to tear down science from the pedestal of progressive accumulation of knowledge, was aware of—even insisted upon—that scholars' preference for one theory over others need not be based on or explained by reference to different religious, moral, political, or aesthetic values, since scientists have a common set of cognitive values which they use to evaluate theories. These cognitive values—and now I am moving beyond Kuhn—the adherence to which is part of the scientific ethos, function as a bulwark against the influence on scholars of other, extra-scientific values that otherwise would rip the scientific community apart and lay it open to endless warfare among adherents of competing religious, political, and other groups. In absence of scientists' cognitive values, history as well as other disciplines would be just that kind of sociological and political battlefield that adherents of the so-called strong program in science and technology studies seem to think they are, with their belief in what David Bloor calls "the priority of the social" as underlying explanation of scientific change.[33] Bloor and others who embrace the strong program "reject the distinction between epistemic and social factors."[34] This would deprive the social sciences—including history—of one of their major functions to society, namely the provision of a field for the pursuit of knowledge at which differences of (extra-scientific) values are irrelevant or at least kept at bay. Most historians probably still see this function as important, although narrativists and other postmodern philosophers of history have been undermining it in recent decades.[35]

Before moving on to a discussion of the criteria for theory choice as seen by Kuhn and others, a few words on terminology. Kuhn and others see these criteria as values held by scientists.[36] Alternatively they could be regarded as virtues inherent in the theories.[37] There seems to be little more than a semantic difference between the two approaches, as illustrated by Kuukkanen who uses them interchangeably.[38] Both approaches list criteria for theory choice: standards against which a proffered theory is measured. Perhaps we could say that successful theories exhibit virtues valued by the scientist.

There is no agreement on the exact specification of cognitive values for theory choice. Lest this be too much of a disappointment, I hasten to add that there is also little disagreement, even though each philosopher tends to formulate the criteria in his or her own way. Kuhn lists five: accuracy, consistency, scope, simplicity, and fruitfulness. With accuracy he means that the consequences deducible from the theory should accord with existing data ("experiments and observations"). Consistency is both internal to the theory and external: with other "currently accepted theories applicable to related aspects of nature." Scope means that the theory's consequences "extend far beyond" the data or theories of lower order or laws for which it was designed. This is closely related to simplicity, which means that the theory should bring order to phenomena that without it would be left "isolated individually and, as a set, confused." Fruitfulness demands that the theory "disclose new phenomena or previously unnoticed relationships among those already known."[39]

Ernan McMullin, in basic agreement with Kuhn on the role of what he terms epistemic (as distinguished from pragmatic) values "implicit in contemporary scientific practice," refines Kuhn's list somewhat, specifying that accuracy must imply accordance with data not yet available when the theory was designed (predictive accuracy); splitting consistency into internal coherence and external consistency; substituting "unifying power, the ability to bring together hitherto disparate areas of inquiry" for Kuhn's scope; and relabeling fruitfulness "fertility." Moreover, he is somewhat reserved with regard to simplicity and admits that "one could easily find other desiderata."[40]

Others have indeed found not so much other desiderata as slightly different versions of the criteria suggested by Kuhn and McMullin. W.V. Quine, arguably the one scholar who can rival Kuhn for the position as the major scourge of positivist science, presents five virtues scientists value when assessing competing hypotheses.[41] The first seems akin to external consistency but is labeled conservatism and calls for compatibility with previous beliefs. The second, modesty, is closely related to conservatism and apparently has no counterpart among Kuhn's criteria. There might even be some tension between it and fruitfulness/fertility (which matches none of Quine's criteria), since Quine explains that modesty means that "events that it assumes to have happened are of a more usual and familiar sort, hence more to be expected."[42] Perhaps the tension partly has to do with Kuhn describing theories that often are very comprehensive, whereas Quine's virtues are primarily (but not exclusively) for assessing more limited hypotheses. His next two criteria dovetail with Kuhn's, however, namely simplicity and generality. The latter seems to be just another word for scope, since "the wider the range of application of a hypothesis, the more general it is."[43]

Quine's fifth virtue, refutability, as well as a sixth, precision, which he mentions as a possible supplement to the other five,[44] should probably be consigned to the category of pragmatic values/virtues. Like other cognitive

values/virtues they are desiderata, but they are not epistemic since they are not truth-indicative or, to use McMullin's idiom, they are not "likely to improve the . . . conformity between theory and world."[45] Cognitive pragmatic values are important but of a different kind. Their importance seems to be accepted across the board, and they need not be discussed further here.

Probably by coincidence, almost every philosopher who lists cognitive values ends up with five. An exception is Paul Thagard, who in 1978 proposed a mere triplet when discussing criteria for theory choice in the mode of inference to the best explanation.[46] This is all the more noticeable since Thagard's third criterion seems to have been left by the wayside by everybody else. The neglected child—so neglected it seems no one has cared about it—is analogy: "the explanations afforded by a theory are better explanations if the theory is familiar, that is, introduces mechanisms, entities, or concepts that are used in established explanations."[47] Thagard's other two criteria cover familiar terrain, although the first one, consilience, is a neologism. In substance, consilience turns out to be scope in disguise: "how much a theory explains, so that we can use it to tell whether one theory explains more of the evidence than another theory."[48] The remaining criterion is simplicity, defined as the admonition to shun the intriguing company of the ad hoc hypothesis "that serves to explain no more phenomena than the narrow range it was introduced to explain."[49] For practical purposes this ban on ad hoc hypotheses might well be the gist of simplicity, although Quine strives to delimit simplicity and McMullin throws a curse at it, explaining that "it was a favorite among the logical positivists because it could be construed pragmatically as a matter of convenience or of aesthetic taste, and seemed like an optional extra which the scientist could decide to set aside, without affecting the properly epistemic character of the theory under evaluation." He adds that "efforts to express a criterion of 'simplicity' in purely formal terms continue to be made, but have not been especially successful."[50] David Bloor in discussion with Larry Laudan over the strengths of the strong program is even less gracious, claiming that "simplicity as a formal criterion has been a disaster for rationalist philosophers."[51]

Criteria for Theory Choice in Historiography

The philosophers above focus on science. Behan McCullagh, Mark Bevir, and Jouni-Matti Kuukkanen argue that historians judge the virtues of alternative theories by means of similar values. McCullagh presents five (of course!) criteria historians employ when deciding which of two or more competing explanations is the best. These are denoted plausibility—the data available must imply the explanatory hypothesis; scope—the wider range of data explained, the better; power—the more probable the data are made by the hypothesis, the better; less disconfirmation by other accepted beliefs, which seems akin to external consistency; and the essence of simplicity, namely, less ad hoc-ness.[52]

Bevir lists as many as six criteria for comparing "webs of interpretation": three arising out of respect for "established standards of evidence and reason" and three "because we should favor positive speculative theories to those merely blocking criticism." All six are familiar albeit sometimes under other labels. To the first group belong accuracy ("a close fit to the facts"), comprehensiveness (fit to "a wide range of facts with few outstanding exceptions, and especially . . . facts from different areas, or from areas that previously seemed unrelated"), and consistency (not "contravening the principles of logic"). The criteria in the second group are progressiveness ("postulating new predictions not previously connected with that web of interpretations"); fruitfulness (which entails that "the new predictions made by associated speculative theories characteristically receive support from the facts"); and openness (consisting of "clearly defined propositions thereby facilitating criticism").[53]

Kuukkanen takes a somewhat different route, presenting values that historians use when evaluating colligatory concepts. Still he ends up with five criteria. The first, exemplification, has a likeness to accuracy but with a twist due to the theories being colligations: "the descriptive content of a colligatory expression has to exemplify the historical data it subsumes." The second, coherence, rings familiar, but it turns out Kuukkanen is aiming not at the theory (the concept) but at "a maximally coherent set" of material to be subsumed under the colligatory concept. The third and fourth criteria, comprehensiveness and scope, are close kin, giving preference to colligations that apply to as large amount (for comprehensiveness) and area (for scope) of data as possible. Kuukkanen's fifth and final criterion is reminiscent of Bevir's progressiveness, namely, originality: "a more innovative and original concept should be preferred to a more customary one."[54]

Consensus on Theory Choice

McMullin observes that scientists may differ not only over the evaluation of how a particular theory fares on each criterion but also over the value to be attached to the different criteria.[55] The latter especially can explain why scientists disagree on some theories. Heather Douglas, observing that "the value of cognitive values has been underdeveloped in philosophy of science," attempts to bring order to what appears as a clash or perhaps a confusion of cognitive values.[56] She claims that there is general agreement among philosophers on the necessity of a theory's internal consistency as well as (external) empirical adequacy, that is, its ability to account for existing evidence. These two "minimal criteria" are distinguished from "desiderata" regarding either the theory's internal qualities—scope, simplicity, and potential explanatory power: in short, "fruitfulness"—or its qualities in relation to evidence. Scope, simplicity, and explanatory power are again critical in the latter category, in addition to consistency with other theories,

the ability to produce novel empirical predictions, and, finally, a more pragmatic value, precision.

Although it may be debatable whether consistency with other theories should be categorized as a value concerning the relation between theory and evidence, the internal/external dichotomy seems useful. Empiricists inclined to think that the relation to the evidence is all that matters would do well to remember the tremendous appeal of Marxist theories in history or the social sciences, or the attraction of Freudian psychoanalysis, both of which to a large extent must be explained by the theories' internal qualities that its adherents allowed to trump empirical challenges for several decades. These examples also point to a problematic part of Douglas's analysis, however, namely, her placing empirical adequacy on a par with internal consistency as an absolute demand on theories. This seems to underestimate the ability of ad hoc-ery—which in history includes conspiracy theories—to save theories that are wanting in support from available data. A similar conclusion can be drawn from Imre Lakatos's observation that the outer shell of a theory will be adjusted to accommodate and thus absorb the blow from evidence that would seem to run counter to the theory, leaving the inner core more or less intact.[57] Of course, ad hoc-ery can also save much internal inconsistency (although not quite as much as empirical inadequacy), which is another reason why the minimal criteria are less absolute than claimed by Douglas.

The degree of consensus emerging from the discussion here is remarkable. Philosophers of science and philosophers of history of various persuasions who have studied theory choice all agree that such choice is grounded in deeply held values about the scientific virtues of theories. Moreover, they agree on the vast majority of these criteria. Internal consistency is a minimal criterion: failure on this score makes the theory a nonstarter. Simplicity, which in historiography basically means an abhorrence of ad hoc hypotheses, is another staple criterion, as are scope and accuracy: the latter entails adequacy as a must-have and predictive accuracy (of data not yet known or analyzed) as a nice-to-have. Power is mentioned by some and ignored by others, perhaps because it is hard to gauge. There is little doubt that all five (!) of these virtues would be valued by philosophers from Kuhn to Kuukkanen.

Between Quine and McCullagh's conservatism/coherence with existing beliefs on the one hand and Kuhn/McMullin's fruitfulness/fertility and Bevir/Kuukkanen's progressiveness/originality on the other there might be some tension. At least Quine and McCullagh seem to give greater weight to the need to cohere with the prevailing "web of belief" (Quine) whereas Kuhn, McMullin, Bevir, and Kuukkanen seem to value innovativeness higher. (In Kuhnian "normal science," however, concordance with the overarching theory is *de rigueur*.) This difference is reminiscent of Peter Lipton's distinction between "the likeliest" and "the loveliest" explanation, where the former is the one most warranted and the second is the one that provides most understanding.[58]

No one would claim that these values are objective; indeed, the notion of an objective value appears contradictory. What the preceding examination has shown, however, is that most cognitive values are shared by philosophers across the board. My contention is that they are shared by historians as well, although to prove it would demand a broad and in-depth analysis of historians' assessment of alternative theories.[59] Of course, the evaluation of the degree to which a specific theory meets the criteria will vary among historians, as will the valuation of each criterion. But these differences are variations within the scientific community and are contained by it; they are not—at least they need not be—the result of extra-scientific influences or values emanating from political convictions, ideology, and so on.

V. The Virtue of Cognitive Values

This analysis has shown that historians have acquired not only tools for establishing and critically examining historical facts but also tools for assessing and choosing between theories: be they colligations, explanations, narratives, or syntheses. The societal import and benefits of historians having these means at their disposal should not be underestimated. They give historians the ability to pronounce verdicts on competing factual claims such as who were responsible for the Katyn Massacre. Getting the facts straight is important if the goal is understanding how the world works—or, strictly speaking when it comes to historians: how the world worked. It provides a basis for allotting praise and blame accurately—which is important for those who want to pass moral judgment on people responsible for past events. Such judgment ought to be based on facts.

Historians have also the wherewithals to create an understanding of the past by means of theories. Scientific historiography is not the only candidate for the provision of such understanding: political, ideological, or religious convictions are alternative sources. Within its realm, however, historiography is a more reliable guide to true understanding of the past than the alternatives. Moreover, scientific historiography has a unique potential in that it provides a basis for creating a common understanding of its object of study—the past—despite and irrespective of differences of political, ideological, or religious persuasion. The benefits of this function of historiography are perhaps best understood if put negatively: without scientific historiography, society is bereft of a critical means to create common and consensual ground among groupings that have different political preferences, different ideologies, and different religious convictions. The outcome is increased strife as a product of a dialogue of the deaf between adherents to differing political, ideological, or religious creeds.

The ambition to increase the consensual space and thereby reduce the danger of clashes over different interpretations of the past is one reason why we should try to expand the room where historiography applies. The attempt, described in Chapter 1, by Anders Fogh Rasmussen's Liberal–Conservative

Danish government to use Bent Jensen's *revanche*-traditionalist Cold War interpretation as a bludgeon in their culture war against socialists and radicals illustrates that the insulation of historiography against political influence cannot be taken for granted. With the sole exception of Jensen, the academic establishment—professors across the whole specter of mainstream history—were united in their disavowal of the prime minister and his associates' efforts to substitute presentist moral/political judgment for historicist understanding of the Cold War in Denmark. Historians from the far left to the center rallied behind scientific values to fight what they saw as an illegitimate invasion by politicians into academic quarters. The effect of Fogh Rasmussen's politicization (with Jensen as an accomplice) was the dissolution of the Cold War history consensus, which had so far provided the common ground for Danish foreign policy making.

VI. Postscript: An Ineluctable Political Project? Contemporary History as Politics by Other Means

In conclusion, let me reflect for a moment on the implications of the analysis in this chapter for my own historiographical period, namely, contemporary history. It is probably no coincidence that both my Danish Cold War example and the Katyn Massacre example lie within this period. Topics treated in contemporary history are often contested, "hot" topics, as opposed to topics in the more remote past, where the temporal distance often (but not always) has cooled things down. The more or less direct political relevance of much contemporary history serves as an impediment to a more detached, scientific treatment of the topics at hand. For this reason contemporary historians, who often have a more direct political motivation for their research than historians who study more distant periods, can have trouble letting cognitive values prevail over their own political values. Accordingly they must fight harder to let cognitive values reign supreme than historians for whom the object of their study has a less clear political relevance.

The political sensitivity of much contemporary history can be described, and perhaps explained, in the terms minted in Danto's analysis of narrative sentences. Danto noted that narrative sentences are inherently unstable, since developments after the depicted event—including what happens in the present—can change the description of past events. In the mid-1980s, Mikhail Gorbachev's introduction of *glasnost* and *perestroika* could be described as bold efforts to rescue Soviet power by founding the Communist Party's rule on a more solid basis with regard both to legitimacy and to the economy. Many western observers unacquainted with Soviet society would subscribe to this interpretation. After the collapse of Soviet communism, Gorbachev's initiatives were redescribed as foolhardy or naïve attempts to shake up a system so brittle that when touched, it would fall to pieces. The instability of narrative sentences makes contemporary history particularly prone to revision because effects and significances often proliferate at high

speed at close range, decelerating after a while. When events are at close temporal range, their description changes quicker than the description of more distant events, the trail of which is not shifting at the same pace.

The fluidity of the description of events in contemporary history may be one reason why many historians—including myself to an increasing degree—have reservations against such history, at least when it becomes so contemporary that it borders on journalism. Contemporary history descriptions are too influenced by current political and other trends for the discipline to be truly scientific and not merely politics by other means. For example, the description, and hence the valuation, of the 1993 Oslo Middle East Peace Accord have changed as subsequent events showed that the accord did not lay the foundation for peace between Israel and the Palestinians as it might seem at first. Instead of providing for Israeli safety and a sovereign Palestinian homeland, it merely relieved the Israelis of their responsibilities as occupation power, leaving law and order as well as the provision of food and other necessities to the Palestinian authorities in the Gaza and the West Bank. Or, moving to another part of that unfortunate region, who can tell how the Iranian revolution of 1979 will be described some years from now, or the Arab Spring of 2011, or the toppling of Muammar Gaddafi following NATO's bombing in Libya, or Western nonintervention in the country which at the time of writing is the worst tormented of them all, Syria. Surely there will be produced narrative sentences on these developments that neither the ideal chronicler nor the committed historian can foresee today. The train of events is still rolling, at such speed and with so uncertain direction that no one knows where the journey will end. Until it does, narrative sentences will proliferate. And without approaching some kind of (admittedly never fully attainable) descriptive closure, political values will tend to influence descriptions to such a degree that objectivity in contemporary history is further out of reach than in periods further removed from current events.

Notes

1 Martin, "Objectivity and Meaning," 29.
2 Quine, "Two Dogmas of Empiricism"; Kuhn, *Structure of Scientific Revolutions*.
3 Zammito, *Nice Derangement of Epistemes*.
4 Bevir, "Objectivity in History," 333, defines a fact as "a piece of evidence which nearly everyone in a given community would accept as true." The difference between my facts-as-statements and Bevir's facts-as-evidence evaporates further down on the same page: "A fact is a proposition members of a community accept as true."
5 White, *Metahistory*.
6 See, for example, Carroll, "Interpretation, History and Narrative," esp. 158 for a critique of the putative link from tropes to modes of emplotment.
7 See Tulloch, *Debate*, esp. 104 and 12.
8 Carr, *What Is History?*, 23.
9 Carr, *What Is History*?, 23. Matters are a bit more complicated since Carr in his next sentence says that "history means interpretation." Still it seems that Carr, a practicing historian more than a theorizing one, never severed the bind from

facts to narrative to the degree done by White and other proponents of a linguistic turn.

10 Danto, *Narration and Knowledge*, 143–181, quotation from 152.

11 A frustrated early modern history teacher at the University of Oslo, conscious of himself and his period, in the late 1970s declared that he was starting a Thirty Years' War against the history department. A few years later he resigned his position and, by implication, the war.

12 Davidson, "Psychology as Philosophy."

13 Roth, "Narrative Explanations."

14 For the assassination and its context, see Clark, *Sleepwalkers*, esp. 367–376.

15 Roth, "Narrative Explanations," 9.

16 Roth, "Narrative Explanations," 9.

17 Roth, "Narrative Explanations," 13.

18 See Richie, *Rashomon*.

19 Froeyman, "Ideal of Objectivity," 3.

20 "Who Killed Kennedy? The Kennedy Conspiracy Theories Explained," accessed February 10, 2016, http://www.theweek.co.uk/55933/who-killed-jfk-52-years-of-enduring-conspiracy-theories. The thought of what such facts would reveal about the American political system has probably been instrumental in generating and sustaining the torrent of interest in conspiracy theories relating to the assassination. See Knight, "Kennedy Assassination and Postmodern Paranoia."

21 Goldstein, *Historical Knowing*, 200.

22 The most authoritative English-language overview on Katyn so far remains Cienciala et al., *Katyn*, which also provides translated versions of 122 documents relating to the massacre. See also Sanford, *Katyn*, which discusses not only the events themselves but also provides a lengthy discussion of Britain's, the United States', and the Soviet Union's role in the postwar obfuscation or outright denial of Soviet guilt. Etkind et al., *Remembering Katyn* is a recent study of the memory cultures of Katyn. Mikhail Gorbachev admitted Soviet guilt in 1990; in 1992, documents that included the Politburo's decision to murder the Polish prisoners of war were released. The Soviet investigation, tasked to establish the names of the victims and the accused, was closed in 2004. After the plane crash of 2010 that killed the Polish president and other officials on the way to Katyn, Russia released a further 67 case volumes out of a total of 183 volumes from the investigation, but 35 volumes still (in 2015) remain classified. See Petrov, "Katyń: Kremlin's Double Game." A group of relatives of victims of Katyn was dissatisfied with the closing of the Russian investigation and, with the Polish government as a third party, sought justice at the European Court of Human Rights at Strasbourg, but the court in 2013 ruled against their favor. See Kamiński, "Katyń Massacres."

23 McCullagh, *Logic of History*, 139, makes a similar point.

24 Lipstadt, *Denying the Holocaust*.

25 Schneider, "Past Imperfect," 1535.

26 Gadamer, *Truth and Method*.

27 Lowenthal, *The Past Is a Foreign Country*, 216.

28 Collingwood, *Idea of History*.

29 Lowenthal, *The Past Is a Foreign Country*, 218n189.

30 Kuukkanen, *Postnarrativist Philosophy of Historiography*, 87–88, 91–92, 96.

31 For influential proponents of the view that narrative is constitutive of historiography, see Mink, *Historical Understanding*; Ankersmit, *Narrative Logic*; Rüsen, *Rekonstruktion der Vergangenheit*. Hayden White acknowledges in passing the existence of a "dissertative" mode of historiography in "Narrative in Contemporary Historical Theory," in *Content of the Form*, 28 and 217–218n4.

32 Kuhn, "Objectivity," 331. See also Kuhn's remarks in the postscript of the second edition of *Structure of Scientific Revolutions*, 184–186, on his neglect of values in the first edition of the book.
33 Bloor, "Sociology of Reasons," 310. Bloor's paper is a response to McMullin, "Rational and the Social." For a classical formulation of the strong program, see Bloor, *Knowledge and Social Imagery*.
34 Bloor, "Sociology of Reasons," 306.
35 For a presentation and critique of the mainstream view, see Froeyman, "Ideal of Objectivity," 2–3.
36 For example Kuhn, "Objectivity"; McMullin, "Values in Science"; Douglas, "Value of Cognitive Values."
37 Quine and Ullian, *Web of Belief*, 66–81; Lipton, *Inference*, 122–123; Kuukkanen, *Postnarrativist Philosophy of Historiography*, 214n9.
38 Kuukkanen, *Postnarrativist Philosophy of Historiography*, 127 and supra.
39 Kuhn, "Objectivity," 322.
40 McMullin, "Values in Science," 16–17.
41 Quine and Ullian, *Web of Belief*, 66–81.
42 Quine and Ullian, *Web of Belief*, quotation from p. 68.
43 Quine and Ullian, *Web of Belief*, 73.
44 Quine and Ullian, *Web of Belief*, 98.
45 McMullin, "Values in Science," 19. Truth-indicative is used by Douglas, "Value of Cognitive Values," 798, whereas McMullin, "Values in Science," 20, uses "truth-bearing." Douglas, "Value of Cognitive Values," 801, classifies precision as an epistemic value. She admits, however, that its function is as a modifying, additional layer on what I would suggest are genuine epistemic values such as scope, simplicity, and consistency.
46 Thagard, "Best Explanation."
47 Thagard, "Best Explanation," 91.
48 Thagard, "Best Explanation," 79.
49 Thagard, "Best Explanation," 87.
50 McMullin, "Values in Science," 16.
51 Bloor, "Strengths of the Strong Programme," 77.
52 This enumeration and order is from McCullagh, *Logic of History*, 51–52; see also McCullagh, *Justifying Historical Descriptions*, 19–29.
53 Bevir, "Objectivity in History," 336. Bevir, *Logic of the History of Ideas*, 102–103, has a similar list.
54 Kuukkanen, *Postnarrativist Philosophy of Historiography*, 123–128, quotations from 123, 126, and 128.
55 McMullin, "Values in Science," 16; see also Kuhn, *Structure of Scientific Revolutions*, 185–186.
56 Douglas, "Value of Cognitive Values," 796.
57 Lakatos, *Methodology*, 47–52.
58 Lipton, *Inference*, 59.
59 Bevir, "Objectivity in History," 337–339 provides one example. See also McCullagh, *Justifying Historical Descriptions*, and McCullagh, *Logic of History*.

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Part II

Explanation and Causality

5 The Ideal Explanatory Text in History

A Plea for Ecumenism

In his authoritative overview "Four Decades of Scientific Explanation" Wesley Salmon in 1989 called Peter Railton's PhD dissertation "Explaining Explanation" from 1980 "quite possibly the best thing written on scientific explanation" since C. G. Hempel's "Aspects of Scientific Explanation."[1] Hempel's magisterial essay had presented and refined his so-called covering-law model of scientific explanation, a model that held a sway verging on hegemony over the empirical sciences from the mid-1950s to the early 1970s. Hempel's influence on historians, historiographers, and philosophers of history started even earlier, with the publication of "The Function of General Laws in History" in 1942. Always debated and today out of fashion, his views have had a vast impact on metahistorians, despite (or perhaps partly because of) the fact that most explanatory accounts in historiography he labeled mere "explanation sketches."[2] Scholars from Robert Fogel to Hayden White got their ideas of historiographical explanation from Hempel.[3]

Whereas Hempel is known to every graduate student in history, Railton is unknown even to philosophers of history. One reason could be that Salmon was wrong in his laudatory evaluation. Another reason for historians' ignorance of Railton may be that the subject of his analysis is science, not social science or historiography. Most important is probably bad timing: Railton published his work on explanation at the very moment when historiographers and philosophers of history lost interest in the topic. The linguistic turn, the fascination with narrativity, and the focus on what has become known as the new cultural history of meaning and symbols drove explanation if not off the agenda then at least to the bottom of it. These trends ensured that many philosophers and historiographers put aside the whole idea of objective explanation, considering it a quest for a less than a holy grail.

While certainly influenced by the linguistic turn, most historians have in their practice been much less affected by the philosophical slump in the interest in explanation. Most historians, it seems, think it is their job to explain, that is, to tell why what happened, happened. The vast majority of historians still explain in basically the same way as historians did a generation

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ago. They ask for no more than a guideline to help them decide what kind of explanations are methodologically acceptable and which are not. In this chapter I try to show how Railton's analysis can be such a guideline—and one that accepts a wider range of explanations than most other guidelines do. This ecumenism is the main reason why I think Railton deserves to be known to historians.

I first present Railton's general analysis, comparing it with Hempel's in order to appreciate better how it builds on and improves upon the latter. This is followed by a brief look at three other important analyses of explanation—by David Lewis, C. Behan McCullagh, and R. G. Collingwood—which I believe are compatible with Railton's analysis. This part also helps to highlight the specifics of Railton's approach. In the next section I show the advantages of Railton's account for the analysis of historians' explanatory practice, using an example from everyday historiography. By incorporating pragmatic aspects, Railton's analysis brings out the logic behind the economy of historiographical explanations; still it discriminates between objective, legitimate explanations and unscientific pseudo-explanations. Its central concept of an *ideal explanatory text* provides a framework for historiographical explanation that is dynamic in that it invites the historian to search for ever-more explanatory information. It is also pluralistic, encompassing both causal (including intentional), structural, and functional explanations.

Practicing historians may sometimes need more than they want. Even if Railton's account seems to represent a useful tool for sorting out bona fide explanations from pseudo-explanations, it must still prove its worth when confronted with the criticisms that Hans-Georg Gadamer and Donald Davidson have put forward against the idea of objective explanation in the humanities, and which combined with the linguistic turn and the focus on narrativity to push the topic of explanation off center stage of the philosophy of history. I argue at the end of this chapter that neither the revival of narrative, nor Gadamer's hermeneutics, nor the indeterminacy of interpretation pondered by Davidson eliminates the need for guidelines on explanations in history and that Railton's analysis, properly augmented, can provide such a guideline.

I. Railton on Explanation: Laws, Causes, and Structures

In Railton's terminology to explain is to provide *explanatory information* about a subject.[4] In the case of so-called explanation-seeking why-questions, this means to *reduce insecurity* about what the explanandum—the thing to be explained—is *due to*. All information that reduces the questioner's uncertainty about what the explanandum is due to is explanatory.[5] These seemingly innocuous sentences not only take Railton's analysis away from Hempel's but also separate it from a model of explanation in which to explain means to identify the causes or the etiology of an event.

Notice that Railton regards explanations as "*accounts*, *analyses* of what happens and why."[6] This is contrary to Hempel, whose deductive–nomological explanation model has the structure of an argument where the premisses (the explanans) consist of specified, empirically verifiable initial conditions and universal laws, and where the conclusion (the explanandum) is deduced from these premisses. The inductive-statistical covering-law model that Hempel developed in response to criticism of his original deterministic model also has the form of an argument where the explanandum is "to be expected."[7] From Railton's point of view, even when covering-law arguments provide explanatory information, they nevertheless may and often must "contain much that is unnecessary for demonstrating the nomic expectability of the explanandum."[8]

Indeed, Railton does not accept Hempel's insistence that to be valid an explanation must show the logical necessity or high probability of the explanandum. On the contrary, Railton devotes considerable energy to developing a "deductive–nomological–probabilistic" model that accommodates indeterministic phenomena. This model, the technical points of which I leave out here, does not show that such phenomena *were expected to* occur—in instances without high probability it would indeed not be expected—but shows, by means of statistical laws, that the occurrence of the explanandum had a specific probability and observes that the explanandum did occur. This is how statistical phenomena are explained. To ask for more in an indeterministic world would be to ask for too much.[9]

Most important, Railton's model of valid explanations is not restricted to causal or etiological explanations but admits several kinds of due-to relations.[10] Railton argues that valid explanations need not be only causal but can also be structural or functional. I shall return to both structural and functional explanations below. Here the important point is the inclusiveness of Railton's due-to concept, which I believe makes it useful to historians.

Since the (imagined) questioner's knowledge of the subject matter may vary considerably, what is effective explanatory information will vary too. One bit of correct information couched in technical language may be explanatory to someone in command of the language and the knowledge but may only confuse those lacking such language and knowledge. There is a context-dependent, pragmatic aspect to explanation having to do with the *salience* of the information provided.[11]

Yet to classify Railton's account as pragmatic would be to ignore that to be explanatory information must pass not only a context-dependent salience test but also an objective *relevance* test: it must be part of what Railton calls the *ideal explanatory text*. As its name indicates, this text is never produced in reality but is a theoretical concept that includes *all accurate information about every due-to relation relevant for the explanandum*.[12]

To say that the ideal explanatory text is vast is a euphemism: potentially it is infinite, since we can imagine causal relations going back to the beginning of a universe with no beginning. This text nevertheless is restricting in

two important respects. First, a lot of things that are true of the explanandum are not part of its due-to relations. Second, many things that, if true, would have been part of the explanandum's due-to relations are, in fact, false and therefore not part of the ideal explanatory text. This is what gives Railton grounds for calling the ideal explanatory text objective: it potentially rules out much of what he calls pseudo-explanations. In this way the ideal explanatory text allows for discrimination between scientifically legitimate and illegitimate explanations. A valid explanation consists of information that composes part of the ideal explanatory text. The more of this text the information reveals, the more complete the explanation.

One of the main features of Railton's account is that it accommodates many kinds of explanation that often have been regarded as incompatible and combines them in one comprehensive model that still, as just seen, is not totally pragmatic. Thus, Railton introduces the concept *encyclopedic ideal explanatory text* that combines causal and structural explanations, which are seen not as mutually exclusive but, rather, as compatible explanations that from different starting points converge in the explanandum.[13] I would make special room for an intentional explanation as a subcategory of causal and would add a functional explanation as a third category (see the following discussion). One and the same fact may have valid explanations of three kinds: causal (whether deterministic or probabilistic and in social science including intentional), structural, and functional. In practice, explanations often combine elements from each, which means they elucidate different parts of the encyclopedic ideal explanatory text.[14]

Since explanatory ecumenism is an important feature of Railton's analysis, I shall dwell on it. We should notice that the pluralism is methodological and does not imply ontological pluralism. One can be a "causal fundamentalist" like Philip Pettit, insisting that the only real causes are subatomic, and yet allow for structural explanations. Indeed, Pettit does.[15] Seeing higher-level entities (such as institutions) as supervening on more basic entities (in this case, individuals) need not preclude high-level explanations. (I discuss this question in greater depth in the next chapter.) Jon Elster thinks that "to explain is to provide a mechanism, to open up the black box and show the nuts and bolts, the cogs and wheels of the internal machinery," and claims that "the scientific practice is to seek an explanation at a lower level than the explanandum," which in social science means "individual actions and motivations."[16] But methodological individualism may be an unduly restrictive explanatory straitjacket. Salmon, a mechanist like Elster, argues that "top-down" causal/mechanistic explanation can be reconciled with what he calls "bottom-up" or "explanation by unification," which means to explain by referring to general principles or laws. He also accepts functional explanation in cases where this is relevant. Which kind of explanation should be preferred—in Railton's terms, which part of the ideal explanatory text should be elucidated—is a pragmatic question, depending on the interest and knowledge of the person for whom the explanation

is meant.[17] And, of course, we may not always know more than one kind of explanation: the causal mechanisms or the unifying principles may be unknown to us.

Indeed, explanation can occur without providing laws. "Because Scriven knocked it over" is a valid answer to the question, "Why is the ink bottle on its side?" since it conveys hitherto unknown information about what the ink bottle's being on its side is due to—there are many possible etiologies for a turned-over ink bottle, and the quoted explanation even identifies the philosopher who did the deed—although the proposition provides no law. In Railton's opinion such a lawless explanation—a mere sketch in Hempel's judgment and terminology—can always be improved by the incorporation of relevant laws. Nevertheless, even with no laws attached it provides explanatory information, which we have seen is Railton's definition of an explanation.[18]

By accepting explanatory accounts without laws Railton demands less for an explanation to be valid than does Hempel. On the other hand, he also demands *more* of explanations than Hempel does, since the ideal explanatory text contains all due-to relations relevant to the explanandum. This may include a lot of information left out of covering-law arguments, such as the causal history leading up to the explanandum. Railton has a more ambitious goal for explanations than the nomic expectability that is Hempel's requirement and with which Hempel is satisfied. In Railton's view, scientific explanation seeks to establish comprehensive *theories* that provide us with some conception "of what the *organizing principles* of the world are (even if these principles embody randomness) which we must grasp in order to know the *how* or *why* of things." In a sentence that indicates why he regards his account as nomothetic Railton declares that "the main characteristic of a developed and mature science is the presence of a theory that enables us to fit a variety of phenomena (even intuitively unconnected or dissimilar) under general principles."[19] This nomothetic character of the ideal explanatory text makes Railton's analysis of explanation dynamic. In his depiction, the search for ever-more comprehensive explanations—for more elucidation of the ideal explanatory text—is never-ending.

II. Compatible Alternatives

We may get a better appreciation of Railton's analysis by a brief look at some other alternatives to Hempel. The one with the most similarities is probably David Lewis's analysis of causal explanation—maybe no great surprise since Lewis was Railton's advisor for the latter's PhD dissertation.[20] More surprising is perhaps that Lewis acknowledges being heavily influenced by his student.[21] Lewis shares Railton's view of explanation as information and argues forcefully against Hempel that explanations are not "things we may or may not have one of; rather, explanation is something we have more or less of." Thus, when Elster (referring to biology) says,

"[F]unctional explanation does explain, although incompletely," Lewis would reply, "Your explanatory information is only partial. Yes. *And so is any serving of explanatory information we will ever get* . . . There is always more to know."[22] Railton would concur. Lewis goes further than Railton does, however, in discarding the requirement that all explanations consist of or contain a deductive–nomological argument. Lewis also rejects the ecumenism of Railton, declaring himself as "committed . . . to the thesis that all explaining of particular events gives some or other information about how they are *caused*."[23] For this reason Lewis prefers the concept "causal history" to Railton's "ideal explanatory text," and sees no need for Railton's due-to concept.[24] While I agree that information about the causal history of the explanandum event is central in historiography, I consider Lewis's insistence that particular events have only causal explanations too restrictive. As will be seen shortly, I think the due-to concept can be of use to historians.

A philosopher who has devoted much time to the study of how historians explain is C. Behan McCullagh. In his recent analysis of causal explanation in historiography, he observes that such explanation normally consists in describing events that made the occurrence of an event of the explanandum-type neither necessary nor highly probable, but more probable than if the causal event had not occurred. (Railton, Salmon, and Lewis all share McCullagh's view on necessity and probability.) McCullagh defines a cause as an event which "sets in train a tendency" for a certain kind of effect to occur. Such tendencies represent regularities rather than laws and can be offset by other tendencies at work.[25] McCullagh sees the main type of explanations in history as genetic and argues that "an adequate explanation of an action will go back to the first event which made an action of the kind described significantly more probable in the circumstances than it had previously been, and then describe all the contingently necessary events which altered the probability of such an action up until it occurred."[26] Although, in my opinion, explanations should list only events that *increased* the probability of the explanandum,[27] I believe McCullagh's causal analysis covers major parts of historiographical practice. His analysis of contrastive explanations, which corresponds to Lewis's, captures an important reason why historians' explanations are often much too brief to explain why the explanandum event happened but still are accepted as bona fide explanations because they explain why the explanandum event happened instead of another event that might have been thought of as equally probable.[28] Both contrastive and comprehensive causal explanation can be seen as ways of elucidating an ideal explanatory text, but such texts can also comprise other kinds of explanations as well; this is why I consider McCullagh's analysis complementary to Railton's rather than alternative to it.

A different strand of explanatory analysis is the idealistic tradition represented by R. G. Collingwood's observation that historians explain by reenacting in their own minds "the thought in the mind of the person by whose agency the event came about."[29] Such intentional or rational explanations

can certainly convey explanatory information, and Karsten Stueber has recently argued convincingly that reenactment must be central in historiography.[30] Reenactment is easily fit into Railton's model. Putting it there highlights that the search for explanations should not come to a stop with intentional explanations, however, since they can be supplemented by explanations of other kinds that will improve our understanding of the explanandum by providing information about what else it was due to. Actions are always due to more than the agents' thoughts, even though for pragmatic reasons the agents' intentions often may be what interest historians most, and certainly were what interested Collingwood most. In the same idealistic tradition, William Dray has claimed that historians explain by showing that for the agent "what was done would have been the thing to have done."[31] The by-now well-known problem with this assertion is easily seen when placed within Railton's analytic framework. Even when what was done would have been the thing to have done, such correspondence may be coincidental and therefore only pseudo-explanatory and no part of the ideal explanatory text.

III. Railton Applied: Explanatory Practice in Historiography

Historians seem not to have worried much about the scientific status of their explanations lately. A discipline that conflates practice and methodology by letting often unreflective explanatory practice be the sole judge of what is acceptable, however, risks granting legitimacy to pseudo-explanations. This is what Hempel warns against in his 1942 article, which is directed against and may have been partly motivated by his desire to counter, pseudo-explanations based on "empirically meaningless terms" such as "the historical destination of a certain race."[32] An advantage of using Railton's concepts to analyze historiographical explanatory practice is that his analysis restores to historians' explanations the scientific legitimacy denied them by Hempel. Substituting for Hempel's *idée fixe* of explanations as nomological arguments the concept of explanatory information whose validity depends on the elucidation of an ideal explanatory text, releases the bulk of explanations in historiography from the purgatory of Hempel's "explanation sketch" label and enables us to see them as bona fide scientific explanations. This boost to historians' scientific self-respect should not be underestimated. Historians may not rest too self-satisfied, however, as they have to relate to Railton's claim that lawless explanations can always be improved—that is, more of the ideal explanatory text can be revealed—by the inclusion of more and more explanatory information, including relevant covering laws, since the aim of science is an ever more comprehensive account of how the world works.[33] This may be a useful antidote to, or at least a warning against, excessive particularism or smugness in given historical accounts.

Although scientific legitimacy is the most obvious advantage Railton's analysis gives historians, it may also be the most dubious. In the 1950s and 1960s the great majority of historians and social scientists wanted such legitimacy.[34] Today the concept "scientific" has lost much of its attraction—indeed, in some quarters it has attained derogative connotations, signaling a lack of self-reflectiveness and a naïve belief in the possibility of objectivity. An ironic effect of what I called Railton's bad timing is that what would have been a major feature of his analysis had it been presented in 1960 had become greatly devalued when his work was published twenty years later. Leaving consideration of the philosophical issues aside for the moment, I shall describe what I consider the major benefits of Railton's model to historians who still have explanative ambitions.

The Economy of Historical Explanations

Admitting explanation without laws into a model of scientific explanation is not the only way in which Railton restores scientific respectability to historians' explanations. His model, while useful in helping to discriminate against pseudo-explanations that are not part of the ideal explanatory text, is also much more sensitive to context than Hempel's nomological models.[35] Explanations proffered by historians are often extremely economical: the mere mention of (or just allusion to) a well-known fact, or pointing to some tiny bit of new information—a long-standing grievance, a desperate financial situation, jealousy—is normally considered sufficient. The implication is that this is all readers need in order to answer the explanation-seeking why-question in the same way as the author, "since they can fill in the remainder [of the ideal explanatory text] (or rather, as much of the remainder as interests them) by themselves."[36] As this quotation from Railton indicates, historians' practice fits nicely into his account of scientific explanation.

An example from everyday historiographical practice will illustrate the merits of this part of Railton's analysis. From 1951 to 1952 Norwegian tanning output fell by more than 40 percent. In a study of the tanning industry's history Finn Erhard Johannessen submits the following explanation (which to my knowledge has never been disputed, the fate of the tanneries being little debated among Norwegian historians): "The crisis had many causes. There was an international slump, but it is also probable that there had been an over-expansion in the Norwegian tanning industry during the first five postwar years. In addition came changes in national economic policy: liberalization of foreign trade and deregulation of price controls."[37]

Johannessen goes on to describe how the elimination of import restrictions and the dismantling of price controls—measures that took away the barriers that had provided the Norwegian companies with a protected home market—led to domestic demand being satisfied increasingly by imports instead of domestic production. When import restrictions on shoes were lifted, many domestic shoe producers were unable to compete with

cheaper imported shoes, and in the wave of bankruptcies that followed, Norwegian tanneries lost both customers and investments. The removal of import restrictions on leather added to the crisis as competition increased. The deregulation of price controls made the situation for the tanneries even worse because it meant the elimination of subsidies on the import of overseas hides that were used as raw material. When input prices rose as a result of this and the increase in demand and transport costs following the outbreak of war in Korea, Norwegian tanneries were exposed to competition not only from low-cost foreign tanneries but also from rubber boots and especially rubber soles.[38]

Johannessen's explanation contains four causal factors: the international slump, domestic overexpansion, liberalization of foreign trade, and deregulation of price controls. The first two factors are mentioned only in passing; readers are left to fill in the rest. Whether this part of the explanation is explanatory—that is, whether it effectively conveys explanatory information—depends on readers' acquaintance with the workings of market economies. Johannessen's book is written for the public (or a part of it), and the author has probably taken for granted that his readers have the basic knowledge needed to understand the explanations that are only hinted at in the sentence quoted earlier. This requires no more than an appreciation of the laws of supply and demand corroborated with appropriate *ceteris paribus* clauses. Since the author himself probably would be able to provide these laws on request, this part of the total explanation would fall within Hempel's category *elliptically formulated explanations*.[39] Indeed, the explanation would be incomprehensible without readers' tacit awareness of these laws, which connect both the international slump (a fall in demand) and the domestic overexpansion (a rise in supply over and above the level which would have cleared the market) with the fall in output.

The treatment of the last two factors is different. The contribution of these two to the crisis is described in some detail (although the explanation still leaves a lot to be filled in by readers); we might see this as a causal/mechanistic explanation revealing the ideal explanatory text in increasing detail as the reader moves from the quoted sentence to the fuller description paraphrased earlier. Hempel would probably argue that behind these descriptions—and thus implicit in the explanation—lie the laws of supply and demand once again, or at least some derivation of them. While not denying that a market economy—even a modern one with heavy state involvement—is based on the requirement that demand and supply meet at any given price level, I doubt whether cognizance of such laws is needed to understand the explanation presented by Johannessen, as he shows readers in detail how liberalization and deregulation exposed Norwegian tanneries to competitors offering goods at prices that the domestic industry could not match. It can hardly be disputed that the information provided by Johannessen in this part of his account gives readers a better understanding of what the crisis was due to, irrespective of their knowledge of the relevant

laws. Johannessen shows how the various elements work together to produce the observed outcome, and thereby explains how this outcome came about.

Structural Explanations

Another advantage of Railton's analysis is that it accommodates structural explanations, which make no use of causal information about either individual or collective agents. Many will stall at this point. Causalists will argue that there is no such thing as a valid noncausal explanation. We have seen that this is Lewis's position on the explanation of particular events. Methodological individualists will argue that all valid explanations must be reducible to the level of the individual agent. This is Elster's position. I claim not that phenomena can occur without something having caused their occurrence, nor do I wish to deny that the basic unit of society is individuals. My assertion is only that the encyclopedic ideal explanatory text for some phenomena under some descriptions contains elements that are not causal on either the individual or the collective level, and that these should properly be called structural since they show what the explanandum is due to by pointing to features of the structure of which the phenomenon is a part or which constitutes the phenomenon. I further assert, and try to show in the following, that historians use such structural or constitutive explanations as complementary to causal explanations.

One type of structural explanation is *delimiting* explanation, in which the explanatory information refers to laws that limit the possible variation of a phenomenon.[40] The maximum speed of a deplacement boat is a function of the length of its waterline; the gravitational pull that one body exerts upon another is a function of the former's mass and the distance that separates them. These examples, which are Railton's, are perhaps of limited use in historiography (although a loose version of Newton's gravitation law is often employed metaphorically). Several laws, however, postulate covariance relations of a similar, if often looser, kind among social phenomena without positing causal relations. The explanation of the postwar crisis in the Norwegian tanning industry is again illustrative. Johannessen's explanation of the fall in output from 1951 to 1952 is based on a specific kind of delimiting laws, namely, the market-clearing equations of neoclassical economics that say that in a market economy supply and demand have to meet.

Now it may be argued that although the laws of market equilibrium are noncausal the *explanation* is causal since it explains one phenomenon—the reduced output by Norwegian tanneries—by pointing to preceding events to which the explanandum is due, such as the increase in production capacity during the war. To a certain extent this is true: just as we might explain a change in the gravitational pull of one body upon another by pointing to some event that increased the distance between the two bodies. The gravitational pull at any given moment could be deduced from the distance

between the bodies, however, so that the causal part of the explanation would refer to (i.e., would explain) the increase in distance, not the decrease in gravitational pull. In the same way the causal part of this explanation of the fall in Norwegian tannery output refers to (i.e., explains) the increase in production capacity over demand in peacetime. From aggregate supply and demand functions, the sales could be deduced. At least the first two elements of Johannessen's explanation use no information concerning either individual or collective agents. The delimiting, noncausal laws of supply and demand explain the fall in production—or to put it more precisely, they explain how much of the production would be sold—whether the tanneries understood the workings of the market mechanisms or not and irrespective of their attempts to adjust to the fall in demand, the increased competition from abroad, and the price rise on hides. The outcome is explained by nothing but the structure of the market economy, which forced the output to fall given the reduced demand internationally and the excess supply resulting from domestic overexpansion during the war. If the explanandum had been not the reduced output, but the fact that a disproportionate part of the reduction was achieved by the closing down of small, less than fully industrialized tanneries, supply and demand functions would not do and a causal/mechanistic explanation would be called for.

Despite efforts by political scientists, sociologists, and psychologists, economics is the only social science that has developed laws that are refined and precise enough for sophisticated mathematical deductions. Delimiting structural explanations might still be useful in history, however, to define the range of possible outcomes. Here is an example. In 1989, Hungary reneged on its obligation in a treaty with Czechoslovakia jointly to canalize and dam the border river Danube and build and run a huge electricity plant together at Gabcikovo. The Slovaks responded by diverting the Danube into Slovak territory and building the plant there, putting it in operation in 1992. The quarrel could have been a perfect pretext for war, but instead of taking up arms Hungary and Slovakia in 1993 submitted their dispute to the International Court of Justice in The Hague.[41] The proposition "democracies don't go to war against each other" might be offered as an explanation for why the two countries chose arbitration instead of war and, if so, would have provided a delimiting structural explanation.

The argument goes like this. The proposition about democracies is explanatory if it reduces our uncertainty as to what the peaceful outcome of the conflict was due to. It may have been due to other things: we can imagine that the two countries just acted in accordance with an arbitration treaty or were forced by great powers to settle for arbitration. Hungary and Slovakia had no arbitration treaty, however, and although their desire for membership in the European Union probably pulled them toward The Hague, they were not forced by the great powers. By pointing to the two countries' democratic (if imperfect) regimes, which as opposed to these imaginary possibilities is a fact, our proposition is a candidate for status as explanatory

information. The truth of the proposition can also be tested, and has, in fact, survived repeated attempts at falsification.[42] Since this explanation carries no information about either causality or agency, it cannot be causal. Instead, it is structural: the contention is that refraining from war against a fellow democracy follows constitutively from being a democratic state.

It may be wise at this point to repeat that structural explanations are complementary to causal explanations; they are not in competition with them. A major point in Railton's analysis is that one and the same phenomenon can have valid explanations of various kinds: opting for one kind need not imply the exclusion of other kinds.[43] Thus the two phenomena explained here by structural information have valid causal/mechanistic explanations as well. We may see how the fall in demand and the increased competition from abroad put pressure on Norwegian tanneries, some of which adjusted by cutting production, others by cutting prices, while others continued as if nothing had happened while sales figures fell, perhaps hoping the troubles would be only temporary. I know less about how the political machinery of Hungary and Slovakia worked to choose arbitration instead of war. My ignorance serves to illustrate a general methodological point in favor of structural explanations, however, namely, that covariance relations that reflect structural facts are often the only explanatory information we have, since we do not (yet?) know the mechanisms that bring about the effect.[44] Knowledge of the relevant mechanisms does not make structural explanations void of explanatory power, however, just like knowledge of the molecular mechanisms effecting gravitational pull has not invalidated Newton's structural law of gravity.

One way of seeing the relationship between structural relations and causal/mechanistic accounts that show how the connection in question is effected is to see the former as supervening on the latter. Under this description the structural explanation reduces to the causal/mechanistic account. I suppose this is how methodological individualists view the relationship between the laws of supply and demand and the workings of a market economy or, indeed, the relationship between so-called social facts and individuals in general. Whether we subscribe to methodological individualism or not we may safely assert that many a phenomenon is made up of other phenomena, as matter is made up of molecules and groups are made up of individuals. When we explain (the workings or properties of) a higher-level phenomenon by reducing it to the parts on which it supervenes—as we would do were we to explain why democracies never fight each other—we engage in another type of structural explanation analyzed by Railton, namely, explanation by *reduction*.[45] Such activity provides explanatory information, yet it cannot be causal since cause and effect are not distinct: the supervening phenomenon is made up of the phenomena on which it supervenes. To the extent that historians explain by reduction, then—and who would deny that at times we do—our scheme of legitimate explanations must include structural ones.

Functional Explanations

A third advantage of Railton's analysis is that it accommodates another kind of noncausal explanation, namely, *functional explanations*. By functional explanations I mean those that state that the explanandum is due to its positive effects for (the reproduction, maintenance, strength, growth, or well-being of) something else of which the explanandum is a part. In social science the relevant explanandum is often an institution or practice and the something else for which the explanandum has a positive effect are the persons or other social entities such as organizations and companies who individually or collectively are conducting the explanandum activity. Whereas functional explanations figure prominently in psychoanalysis—the scientific status of which is at best unclear—and in evolutionary biology, their legitimacy in social science and historiography is disputed. Railton clearly considers functional explanation valid in science but treats it only in passing.[46] Salmon accepts it in social science, too.[47] McCullagh, while allowing functional explanations in historiography, concludes that they "are only special cases of rational explanations or explanations in terms of objective interests."[48] Elster thinks they have no place in social science.[49]

The analysis of functional explanation is an area where the nuances of Railton's due-to concept come to light. We should notice that the claim in functional explanation as it is defined here is not that the explanandum trait is *caused* by its effects, only that it is *due to* these effects. Hence Elster's warnings against the "general functionalist fallacy" do not apply,[50] since the explanation claims only that if the explanandum had not had its beneficial function, it would not have existed—or at least would have been less numerous or prominent. This limits the scope of functional explanations, since they cannot explain why an institution or practice came into existence for the first time, only why it is upheld. On the other hand, we are often more interested in the reasons for the continued existence or spread of a phenomenon than in the reasons for its first appearance.

We should accept functional explanations in historiography in cases where a lasting institution or practice can be shown to have been beneficial to someone occupying a niche—be it territorial or functional—for which the competition was such that the entities in question would have been unable to keep that position without this institution or practice, that is, their survival or prominence was due to this trait, which hence explains why they thrived while competitors ceased to exist or lost their prominence. An example is the organization of joint stock companies in the nineteenth century, which by reducing investors' risk of losing money made it easier for such companies to attract capital than for personally owned companies with no limitation on the owners' financial responsibility, with the result that companies that needed to attract large amounts of capital had to be organized as joint stock companies. Another example is the practice of negative campaigning by U.S. political parties and candidates, which by smearing competitors

were so effective (at least at one time in the 1980s) that contenders who preferred to present their own policies instead of attacking opponents soon found themselves out of the race.

In the two examples just presented the explanations are the clauses starting with "which," and what is of concern here is that it does not matter for the validity of the explanation whether the agents were aware of the advantages of the institution or the practice in which they took part. Elster's insistence that agents must be unaware of the benefits of the activity for the explanation to qualify as functional seems unduly restrictive, especially since in many cases we simply do not know whether people who engage or engaged in such activities are or were conscious of their effects.[51] All we have to show in order to provide a valid functional explanation of a lasting institution or practice is that without this trait the agents would be unable to hold onto their niche. In this sense the continued existence—or at least the favorable position—of the agents is due to the functionally advantageous activity. My conclusion is therefore on a par with my conclusion regarding structural explanations, namely that an explanation of this kind that makes no use of information about the intentions of the agents—whether such information is available or not and whether, if available, it is confirmative or not—is a bona fide functional explanation. Following Railton and Salmon we need not choose between a functional and an intentional (or a causal/mechanistic) explanation of a phenomenon. The two types complement each other instead of being in competition: they simply elucidate different parts of the encyclopedic ideal explanatory text. One and the same phenomenon, under one and the same description, can be explained in more than one way, just as one and the same phenomenon under different descriptions can also be explained in more than one way.

I have confined functional explanations in historiography to situations in which there is competition. Competition being a universal human trait, the potential scope for functional explanations is wide. It includes war, courtship, market relations, and all kinds of jockeying for positions of power and influence. For example, Norbert Elias's *History of Manners* explains functionally how a "civilized" code of conduct was established at European medieval courts when the level of everyday violence ceased, as such refined ways came to be regarded as superior to the earlier more blunt conduct as a means to get the king's ear.[52] Thorstein Veblen's analysis of conspicuous consumption is another classical example, showing how such extravagance functioned as a tool to manifest and consolidate one's superiority. Conspicuous waste of time, which had served the same function well at a time when chains of dependence were short enough for lower-ranking people to observe big men demonstratively idling away their time, was less effective when society became more complicated and the commoners lost sight of their masters.[53] Whether other men who emulated the behavior of the civilized or conspicuously consuming magnates did so because they thought it would increase their power or wealth or just tried to please or

impress their women, is in this context irrelevant for explaining why such behavior came to dominate.

The endemic state of war between European states in the fourteenth through eighteenth centuries provides further examples of functional explanation. William McNeill has shown how the financial requirements of this armed struggle spurred rulers to expand their revenue to ever-increasing levels by a mixture of taxes and loans, creating as an unintended effect the modern revenue state—and perhaps modern democracy, too, as the idea of no taxation without representation led to new institutions and demands. Rulers unable or unwilling—or unaware of the imperative—to transform their states by making use of the new financial devices soon found their armies less well equipped than those of their adversaries, and as a consequence had to see their territory reduced or incorporated into neighboring states. The choice facing rulers was to find the means to finance an increasingly costly war effort or to perish. Many chose the latter by default.[54] The hegemony of the modern revenue state can thus be explained functionally.

Perhaps the same goes for the nation-state as well, if we are to accept an argument put forward by Charles Tilly. In *Coercion, Capital, and European States* Tilly explains the prevalence of what he calls the European national state, which is his preferred term for states that are dominated by but not exclusively composed of one nationality. In such states concentration of capital in commercial cities combined with territories large enough to sustain an indigenous population from which national armies could be drawn. This combination gave national states the upper hand in conflicts with neighbors lacking either the merchant capital or the indigenous population base necessary to fight long-lasting wars that acted as a drain on both the financial and the human pool of resources. Tilly argues that today's (or yesterday's?) hegemony of the national state is due to the advantageous combination of these two factors.[55] This seems to be another example of a functional explanation.

IV. Radical Challenges

I have argued that historians can use Railton's analysis as a guideline in their quest for better explanations. I end by briefly discussing three challenges to the idea that explanation is central to the historian's craft: the return of narrative, Gadamerian hermeneutics, and Davidsonian indeterminacy. The latter two are the most radical, since they question the idea that there is an objective ideal explanatory text that historians can elucidate.

The Revival of Narrative

In November 1979 Lawrence Stone observed what he called the revival of narrative.[56] Since then narrative force has overwhelmed historiography and left nomological explanations by the wayside. It seems that philosophers of

history and historiographers have more or less lost interest in how historians explain, and instead focus on how they narrate.[57] Paul Ricoeur and others have argued that narrativity is the defining trait of all historiography.[58] Although I would maintain that there are quite a few analytic, nonnarrative works of historiography, there is little doubt that narrative is central to the craft.

Few would dispute that narratives contain much information that is irrelevant to explanation. But most would also agree that narratives are explanatory. They explain by presenting information of a causal (including intentional), structural, or functional nature and organizing it in such a way that it reduces our insecurity as to what the explanandum was due to. In this way narratives are themselves part of the ideal explanatory text. Margaret Somers and Gloria Gibson think that narrative explanations consist in the causality they detail: "narratives are *constellations of relationships* . . . embedded in *time and space*, constituted by *causal emplotment*."[59] If they are right, narratives are explanatory to the degree and because they are causal, and their explanatory claims should be judged accordingly.

The Fusion of Horizons

The 1980s and 1990s saw an increased interest in and influence of Gadamer's analysis of hermeneutics. Gadamer insists that the attempt to rediscover the intentions of authors rests on a positivistic misunderstanding of the relationship among author, reader, and meaning. Instead of reenacting the thoughts of the author, the reader should attempt a "fusion of horizons" with the text or event under consideration. Such a fusion involves translating the meaning of a text or action into the language of the interpreter—the meaning of something is always its meaning *for someone*. On this construal of interpretation it might appear that there can be no "ideal explanatory text," even supposing that interpreting the meaning of a text or act is itself a kind of explanation or part of an explanation. The reason for this is that interpretation is always confined to a particular interpretive context, such that as these contexts change the interpretations themselves also change.[60] If Gadamer's rejection of the attempt to seek authorial intent is applied to the intention of agents in general—and it is hard to see how we could avoid this—it seems impossible to reconcile his hermeneutics with the professed objectivity of Railton's ideal explanatory text.

But there are two responses to this apparent difficulty. The first is that, as Stueber has explained, "interpreting behavior as rational action conceptually requires taking into account the intentions of individual agents as the causes of their actions." This is opposed to the interpretation of literary texts or works of art where "authorial intentions are not the primary focus of interpretation" and "which cannot be easily modeled on the basis of well-understood speech acts like assertions." The reason why historians try to grasp the intentions of the agents they study is the belief that

these intentions moved the agents to act.[61] So no matter whether one adopts a Gadamerian understanding of interpretation or not, historians will still explain actions in terms of their agents' intentions—a form of explanation that Railton's approach appealingly illuminates.

The second response begins by admitting that Railton's idea of an objective ideal explanatory text sits somewhat uneasily with the notion of historians fusing their individual horizons with those of distant authors, acts, and texts. Such a fusion seems to entail an element of subjectivity, since different historians have different horizons. But this does not, in fact, render the notion of an ideal explanatory text incoherent, as it might appear. Recall that an ideal explanatory text is infinite since there is an infinite amount of information as to what a phenomenon is due to. There is thus room in this ideal for all the various interpretive perspectives that might be brought to bear in ascertaining the meaning of an act or text. Indeed, far from being a challenge to Railton's view, Gadamerian hermeneutics actually attests to the power of this view precisely because Railton's account can accommodate Gadamer's insights.

The Indeterminacy of Translation

In 1997 Railton observed that Donald Davidson seemed to have "convinced a notable number of analytic philosophers that the social sciences are essentially interpretive."[62] One critical element of Davidson's argument stems from W. V. Quine's observations on the indeterminacy of translation. Davidson argues that since we have no way to find out what a person means by what he says unless we know what he believes, and since we can only find out what he believes from what he says, there is no way in which belief and meaning can be separated, and hence meaning can never be fixed. This indeterminacy "lies behind our inability to discover deterministic psychophysical laws," and this "nomological irreducibility of the psychological" means that we can never expect "to be able to explain and predict human behaviour with the kind of precision that is in principle possible for physical phenomena." Davidson admits that "psychological events are describable, taken one by one, in physical terms, that is, they are physical events." Nevertheless, they "do not fall under strict laws *when described in psychological terms*."[63] This is not the only reason why Davidson thinks there is an unbridgeable gulf between what he calls the psychological and the physical sciences, but it is indicative of the kind of challenges that confront Railton's notion of an objective ideal explanatory text when applied to a social (psychological) science such as history.

Although the physical sciences are perhaps less deterministic than Davidson seems to think, the basic indeterminacy of the interpretive effort of the social sciences might seem to be a problem for historians trying to elucidate objective ideal explanatory texts. Depending on the interpretation of the agent's intention, one and the same action can be described in

several ways, no one superior to the others, making it appear impossible to explain the action by reference to an objective ideal explanatory text. But appearances deceive here. An ideal explanatory text can allow for different descriptions of the same phenomenon, and it can allow—indeed, insists on—different explanations of the same phenomenon. That the same physical motion might be described as different actions depending on how the meaning of its causal intentions is interpreted and might be explained in different terms depending on how it is described is precisely what Railton's account allows for. As with the case of Gadamer so with Davidson: what appears to be a problem for Railton's account turns out to be a strength of it. Even if we accept both Davidson's and Gadamer's criticism of the belief that objective descriptions of actions (including speech acts) are possible, and hence concede that any description of an action must to a certain degree be subjective, the explanatory quest could still be regarded as the work to reveal, for each description, an ideal explanatory text of local objectivity, limited to the description under which the action is seen. The awareness that this description is not a privileged one should not prevent historians from trying to explain the action so described.

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Notes

1 Salmon, "Four Decades," 120. Railton's dissertation, "Explaining Explanation," runs to more than 800 pages. More accessible versions of Railton's main points are presented in Railton, "Probability, Explanation, and Information," and Salmon, "Four Decades," 154–166. Hempel's massive essay constitutes a major part of his *Aspects of Scientific Explanation*.

2 Hempel, "Function of General Laws in History," 238. Clayton Roberts's recent attempt to resuscitate Hempel's analysis in *Logic of Historical Explanation* illustrates that Hempel's major influence is a thing of the past: Roberts, born in 1923, published his main work, *The Growth of Responsible Government in Stuart England* (Cambridge: Cambridge University Press), in 1966.

3 Fogel begins his acclaimed (and debated) *Railroads and American Economic Growth* by approvingly quoting Hempel's insistence in "The Function of General Laws in History" that "in history as anywhere in empirical science, the explanation of a phenomenon consists in subsuming it under general empirical laws" (Hempel, "Function of General Laws in History," 240). White's dependence on Hempel's analysis of historiographical explanation is evident, if unacknowledged, in White's discussion of what he calls "explanation by formal argument" in *Metahistory*, 11–12. Another example of Hempel's authority in the 1960s is Graham T. Allison's influential *Essence of Decision*, 278–279n5.

4 Railton, "Explaining Explanation," 133–135.

5 Railton, "Explaining Explanation," 406–409, 414–416.

6 Railton, "Explaining Explanation," 168, Railton's emphasis.
7 Hempel, "Aspects of Scientific Explanation," 376–412.
8 Railton, "Explaining Explanation," 117–128, quotation from 168.
9 Railton, "Explaining Explanation," 256–346.
10 In the rest of this chapter, unless otherwise specified, the word causal includes both deterministic and dispositional phenomena. The alternative would have been to employ Railton's terminology and reserve "causal" for deterministic phenomena, and use "etiological" to include both deterministic and indeterministic causes (*sic*).
11 Railton, "Explaining Explanation," 154, 419. The concept "salience" is from Salmon, "Four Decades," 162.
12 Railton, "Explaining Explanation," 128–131, 147–148.
13 Causal and dispositional explanations would be alternative explanations of one and the same phenomenon. The encyclopedic ideal explanatory text for a specific explanandum may nonetheless include elements of both in the history leading up to the explanandum.
14 Railton, "Explaining Explanation," 413, 419–420.
15 Pettit, *Common Mind*, 151–152, 218.
16 Elster, *Explaining Technical Change*, 23–24.
17 Salmon, "Scientific Explanation," 69–78; Salmon, "Four Decades," 180–185.
18 Railton, "Explaining Explanation," 139–143, 172.
19 Railton, "Explaining Explanation," 117–128, quotations from 118, cf. 157, Railton's emphasis.
20 Railton, "Explaining Explanation," vi–vii.
21 Lewis, "Causal Explanation," esp. 221n5 and 238–239.
22 Lewis, "Causal Explanation," 217–221 and quotations from 237–238; Elster, *Explaining Technical Change*, 21.
23 Lewis, "Causal Explanation," 221–224, quotation from 239, emphasis mine. Lewis (239) admits, however, that his definition of causal information is very wide and that the difference between himself and Railton is correspondingly small.
24 Lewis, "Causal Explanation," 214–217, 237–239.
25 McCullagh, *Truth of History*, 172–188, quotation from 173.
26 McCullagh, *Truth of History*, 190–194, quotation from 211. Hempel also thinks genetic explanations are "widely used in history," but his model of genetic explanation is based on covering-law arguments and is lacking the teleological element that is an important feature of McCullagh's model. See Hempel, "Aspects of Scientific Explanation," 447–453, quotation from 447.
27 In his latest book *The Logic of History*, McCullagh seems no longer to insist that events that decrease the probability of the explanandum are part of its causal history.
28 McCullagh, *Truth of History*, 188–190; compare Lewis, "Causal Explanation," 229–231. I think the fact that most explanations in historiography are contrastive explains why few causal histories (Lewis) or genetic explanations (McCullagh) need go very far back in time, since distant events seldom explain why one outcome was realized instead of another outcome that differs only in one or a few respects.
29 Collingwood, *Idea of History*, 214–215.
30 Stueber, "Psychological Basis of Historical Explanation."
31 Dray, *Laws and Explanation in History*, 126.
32 Hempel, "Function of General Laws in History," 238, cf. 234 and 240.
33 Lewis, while less insistent than Railton on the inclusion of laws in explanations, still thinks that historians "may generalize modestly, without laying claim to

universality, and say just that quite often an event of such-and-such kind has a causal history with so-and-so features" ("Causal Explanation," 225).

34 Iggers, *Historiography in the Twentieth Century*.

35 Hempel tried to avoid pragmatics by concentrating on the logical structure behind (or underneath) the actual explanations presented (see "Aspects of Scientific Explanation," 425–428). By ignoring pragmatic aspects, however, his account of scientific explanation becomes very strained.

36 Railton, "Explaining Explanation," 154.

37 Johannessen, *Lær og skinn i tykt og tynt*, 153, translation mine.

38 Johannessen, *Lær og skinn i tykt og tynt*, 153–154.

39 Hempel discusses three types of "incomplete" explanations in "Aspects of Scientific Explanation" (415–424). Elliptically formulated explanations "forego mention of certain laws or particular facts that are tacitly taken for granted, and whose explicit inclusion in the explanans would yield a complete D–N [deductive–nomological] argument." The idea is that the missing law or fact could easily be produced on request, this form of incompleteness is therefore "rather harmless" (415). More serious is the kind of incompleteness in "partial" explanations, often "offered in the literature of psychoanalysis and of historiography" (416), where the explanans does not logically imply the explanandum but only indicates a range of outcomes within which the realized explanandum phenomenon would fall. Even more deficient is the "explanation sketch," which is introduced in the following way: "A proposed explanation, for example, which is not explicit and specific enough to be reasonably qualified as an elliptically formulated explanation or as a partial one, can often be viewed as an *explanation sketch*, i.e., as presenting the general outlines of what might well be developed, by gradual elaboration and supplementation, into a more closely reasoned explanatory argument, based on hypotheses which are stated more fully and which permit of a critical appraisal by reference to empirical evidence" (424, Hempel's italics). To judge whether a proffered explanation is one or the other of these three kinds is a *pragmatic* question, depending on the needs and abilities and knowledge of the persons involved in the specific act of explaining.

40 Railton, "Explaining Explanation," 350–361.

41 Fitzmaurice, *Damming the Danube*. In 1997 the court found both parties guilty and ordered them to return to the original joint venture (the court's decision may be found at http://www.icj-cij.org/docket/files/92/7375.pdf, accessed August 3, 2016).

42 Russett and Starr, "From Democratic Peace to Kantian Peace."

43 Railton, "Explaining Explanation," 381–384.

44 This point is also made in McCullagh, *Truth of History*, 295.

45 Railton, "Explaining Explanation," 369–380.

46 Railton, "Explaining Explanation," 416–419.

47 Salmon, "Comets, Pollen, and Dreams," esp. 60–63; and Salmon, "Four Decades," 111–116.

48 McCullagh, *Truth of History*, 203.

49 Elster, *Explaining Technical Change*, 20.

50 Elster, *Explaining Technical Change*, 60.

51 Elster, *Explaining Technical Change*, 57–61. If people knew (or thought) the effect was positive Elster would classify the explanation as intentional. He acknowledges the explanatory value of that what he terms "filter-explanations, in which the beneficiary is able to perceive and reinforce (or adopt) the pattern benefiting him," but insists that they are intentional, not functional explanations. See also McCullagh, *Truth of History*, 202–203.

52 Elias, *History of Manners*.

53 Veblen, *Theory of the Leisure Class*.
54 McNeill, *Pursuit of Power*.
55 Tilly, *Coercion, Capital, and European States*.
56 Stone, "Revival of Narrative."
57 Roberts, *History and Narrative Reader*, testifies to this development.
58 Ricoeur, *Time and Narrative*.
59 Quoted in Kane, "Reconstructing Culture in Historical Explanation," 315.
60 Gadamer, *Truth and Method*.
61 Stueber, "Psychological Basis of Historical Explanation," 37–38.
62 Peter Railton, "Explanations Involving Rationality," 530.
63 Davidson, "Psychology as Philosophy," 230–231, emphasis mine.

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6 Mentality as a Social Emergent

Can the *Zeitgeist* Have Explanatory Power?

In the winter of 2006 crowds of angry Muslims attacked Danish and Norwegian diplomatic missions in the Middle East, breaking windows and setting fire to the buildings. The anger was provoked by the publication first in a Danish newspaper and subsequently in a Norwegian weekly of offending cartoons of the prophet Muhammad.[1] Although the background and the setting were different, the events were reminiscent of politically induced crowd violence in the years around 1968, when youthful left-wing demonstrators clashed with police and broke office windows. After a Vietnam War protest march in May 1970 in peaceful Oslo, twenty-four windows in the U.S. Embassy were destroyed. How should the action of such crowds be explained? The claim of this chapter is that strict adherence to the tenets of methodological individualism can impoverish our explanations of collective behavior by blinding us to its social aspects. Even behavior that at first sight looks like typical individual actions may on closer investigation acquire added explanatory value when seen in a social context.

However, going beyond individualistic rationalizing action explanations means entering a path that may turn into a slippery slope at whose end lie such monsters as conscious classes and nations with needs and emotions. We should therefore tread carefully. My first step is a discussion of whether emergent social properties exist. Accepting an ultimately individualist ontology yet unwilling to resign myself to all-out methodological individualism, I proceed to discuss, in the second part, whether social emergents can serve an explanatory function. In the third part, I suggest how a social emergent such as the mentality of a group can provide added explanatory value. Probing the limits of this approach, I ask whether the cultural climate of an era such as the radical or anti-authoritarian "spirit of the sixties" can explain the spread of the so-called New Left protest movement.

I. Emergent Social Properties

There seems to be agreement that social facts (or social phenomena) exist, and that they are emergent in the sense that they are the result of configurations of individual agents on which they supervene. Indeed, social

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facts are established as the configurations form.[2] The critical question is whether, when social facts emerge, there can also emerge properties that are not reducible to properties of the components that form the configurations. This amounts to asking whether social facts have nonreducible emergent properties.

The classic proponent of the nonreducibility of emergent social properties is, of course, Émile Durkheim. In *The Rules of Sociological Method* he observes that social facts are created when individuals "associate," and he insists that the properties of social facts cannot be reduced to those of its component parts.[3] What is easily overlooked is that Durkheim wrote his *Rules* at a time when emergentism was a promising way of explaining why compounds exhibit properties not found in their component parts, such as the liquidity of water or the hardness of bronze, which are Durkheim's own examples.[4] However, today's nuclear physics has made this idea obsolete.[5]

It seems, however, that emergentism, slightly disguised, has a last refuge in the so-called brain–mind dilemma. The disguise is the currently influential notion of supervenience, which seems to allow the supervening entity (the mental) a more or less shady existence emerging out of its physical base (the material brain).[6] The reluctance to see humans as completely determined by, and thus victim to, nerve cells and synapses, with but an ornamental role for desires and beliefs that make up the folk-psychological model of decision making, has produced what seems to be an amorphous majority view known as "nonreductive physicalism." This view accepts that "everything concrete is exhausted by basic physical objects" and yet insists that mental phenomena "are neither identical with, nor reducible to, physical properties and relations."[7] For our purpose, perhaps the most relevant variation of this view is the mereological, in which the lower-order level realizes the higher-order level, making theirs a noncausal, in-virtue-of, relationship.[8] This would seem an apt description of the relationship between individuals and social groups (and social phenomena in general).[9]

Keith Sawyer has recently argued that wildly disjunctive complex mechanisms can lead to the emergence of multiply realizable and therefore nonreducible properties.[10] He thinks society exhibits such traits. However, Sawyer and others such as Roger Sperry, who want to use the brain–mind analogy to plead for nonreducible, emergent properties,[11] need to defend this view from the challenge that emergentism entails the acceptance of downward causation. Jaegwon Kim has put this challenge succinctly, arguing that mental emergentism (or strong supervenience) implies that "*the causal powers of mental properties are novel, and irreducible to the causal powers of physical properties.*"[12] Kim's reason for this is that if a property is devoid of causal powers, it cannot be said to be real but is only an epiphenomenon, and if the powers of a property are reducible to the properties that realize it by associating in a specific way, the higher-level property is not emergent. To be real implies having causal powers, and genuine emergence implies nonreducibility. Kim observes that this means that mental

properties, if real, must be able to cause changes in physical properties, since the materialism or physicalism accepted by emergentists and reductive physicalists alike insists that physical particulars are all-exhaustive, and this means that for a mental supervenient property to change, its material supervenience base must also change. So if a mental property is to have causal powers of its own, it must be able to effect changes in the material world. Thus, mental emergentism commits materialists to downward causation.[13] It should be noted, however, that Kim's argument rests on his double assumption that to be real means to have causal powers and that if a property is reducible it is not emergent.

My reason for going into the brain–mind dilemma at such length is that Kim's challenge is relevant to social emergentism as well. One can substitute "social" for "mental" and "individualist" for "physical" in the preceding sentences, and the argument translates into a charge that the notion of emergent social properties is logically inconsistent since it entails downward causation from the social level to individuals on whom the social properties supervene and in virtue of whom they exist. Indeed, Raimo Tuomela in his analysis of social groups concludes that social groups, supervenient on their individual members, "are not ontologically real entities." Since "'groupness' is a relation between persons," groups have no independent existence.[14]

The emergentism discussed—and dismissed—so far can be called strong emergentism. There is the possibility of a weaker version, in which the condition of strict nonreducibility is relaxed. In this version an emergent property, while not reducible to properties of its components—that is, not additive or "aggregative"—can still be reducible to properties of its parts *and their internal relations*.[15] Weak social emergentism is ontologically individualist in that its social entities are reducible to individuals and their interactions. The question arises whether the ontological individualism of weak emergentism entails a methodological individualism at the level of explanation. In other words, does weak emergentism entail methodological individualism? To answer this, the critical question is whether there are social facts that cannot (or only in a very artificial way) be explained within the confines of methodological individualism. To that question I now turn.

II. Explaining Social Change

Methodological individualism has been defined in a variety of ways. For the present purposes I shall stick to the description offered by one of its leading proponents, Jon Elster, who defines the doctrine's central claim as follows: "to explain social institutions and social change is to show how they arise as the result of the action and interaction of individuals."[16] Elster declares himself committed to the reduction of sociology to psychology, although he adds that "in many cases it would be impracticable to try to carry it out."[17] It is clear from the context that he is referring to individual psychology, not social psychology.

The urge of methodological individualists to explain in terms of individual agents should not be construed as a disavowal of social facts, only as an insistence that such facts are in principle reducible to facts about individual agents, including relational properties and individuals' beliefs about irreducible supra-individual entities.[18] It is when explanations invoke collective agents, such as the consciousness of a class or the spirit of a nation or an era that cannot be reduced to its constitutive parts and their relations, that methodological individualism rules out the explanation. Typical functional explanations are also hit by the ban on explanations void of individual agents since they invoke notions such as structures and systems as causal factors.

The question I pursue here is whether, within the confines of ontological individualism, there can be legitimate explanations of social facts that transcend the borders of methodological individualism in that they are not reducible to the actions and interactions of individuals. Put another way, does historical reality contain elements that disappear or are seriously distorted when we confine our explanations to individuals only? I consider two affirmative answers to this question. The first is the most radical and the most problematic, namely, Rajeev Bhargava's suggestion that the meaning of terms and the content of beliefs are socially determined. This leads easily to the idea that we should study not individual thoughts but the discourse of a field. The second answer, less drastic but perhaps easier to understand and put to use, is Margaret Gilbert's plea for accepting the agency of plural subjects. I link this notion to "situationist" social-psychological research that shows the influence of peer pressure and authorities.

The Social Dimension of Meaning and Discourse

It is commonly accepted that people act on the basis of beliefs based on meaningful concepts. Since intentionality, beliefs, and desires are thought of as attributes of individuals, reason-explanations of action are often seen as a device of, and an argument for, methodological individualism. However, building on Hilary Putnam's analysis of meaning, Rajeev Bhargava has argued that meaning has an inescapably social dimension due to its linguistic character. Bhargava says that according to Putnam, individual agents cannot hold the full meaning of their beliefs because "the complete meaning of a term (its extension and intension) is socially possessed. The entire linguistic community possesses the full meaning of a term."[19] Invoking the authority of Émile Durkheim, Ludwig Wittgenstein, Peter Winch, Alasdair MacIntyre, Charles Taylor, and Ernest Gellner, all of whom apparently agree that individuals cannot use concepts in ways that completely break with the practice in the society in which they are used, Bhargava, moreover, maintains that beliefs are not individual–psychological entities but are embedded in "irreducible social practices." He pleads for a "contextualist" approach in which "the content of beliefs does not exist internally, merely in the heads of individuals, but . . . is also determined by social context."[20]

Bhargava's "contextualism" seems but a short step to the suggestion that one should substitute the discourse of a field for the methodological individualist's attempt to isolate the beliefs of individual agents. To the degree that beliefs are discursively defined, such isolation would be an ecological fallacy, since no matter exactly how discourse is defined it is a social or collective concept.

But to focus on discourse is not without problems. There is the challenge of deciding how the field should be distinguished: which (and whose) discourse is the relevant one? Saussureans think that in the last instance the meaning of every word depends on the meaning of all other words, making perfect delimitation impossible. If we let go of perfection, however, it should be possible at least to define some borders beyond which the relevance of the discourse is so small that we can disregard it.

Then there is the tricky problem that every discourse supervenes on words uttered and written by individuals populating the field. This takes us back to the relation between individuals and society, and to the pitfall of downward causation. I return to this later. There is an added complexity, namely, that people may speak and write not as individual persons but as members of a group. This brings us to Margaret Gilbert's analysis of social facts.

Plural Subjects and Group Pressure

According to a common definition, only individuals can think, act, and so on. In this definition the notion of plural subjects would seem to be an oxymoron. Yet not only Margaret Gilbert but also John Searle and David Carr have defended the idea.[21] In Gilbert's terminology a plural subject is a social group defined as two or more individuals who consider themselves part of a "we" that is "jointly ready for an action" or who have a "jointly accepted view" (a group belief).[22] As part of a social group, individuals believe and act differently from what they do individually, to the extent that, in extreme cases, none of the members of the group may individually share the view that is jointly accepted as the group belief, or desire the action that they seek to satisfy as a group.[23] What Gilbert calls "participant agency" or "action as part of a plural subject occurs when someone rightly sees his act of will as an expression of the 'cause' of a plural subject of which he is a member."[24] Unlike "singular agents," participant agents pool or suspend their will with the group, like players on a football team or hoplites in a phalanx. Indeed, the drill of both football players and soldiers is aimed partly at achieving and facilitating such pooling or suspension of individual wills.[25]

On Gilbert's wide definition, the majority of our actions may be conducted as parts of social groups, from pairs of lovers to nations, including such fleeting entities as two bridge players joining forces for a single game. As long as we keep in mind that there is no such thing as a group mind, Gilbert's analysis means that we can—and should—endow groups with beliefs, attitudes, and the capacity to act in accordance with the practical

syllogism that is historians' folk-psychological way of analyzing action.[26] There is no need always to go down to the micro level of individuals, as indicated by the following example of how the attacks on the Danish and Norwegian diplomatic missions might be explained: the angry crowd had a wish to avenge the offense against the Prophet, and believed the offense would be avenged by destroying buildings housing the representatives of the offending nations; hence the crowd threw stones and Molotov cocktails against the buildings. (That this is not the whole story of the violence prompted by the Muhammad cartoons does not invalidate the explanation; it means only that other bits of explanatory information can shed additional light on the event.)

Reduction to individuals may still be possible *in principle*, bearing in mind that methodological individualism has room for relational aspects and for the beliefs of individual agents about collectivities.[27] Explaining the actions of the angry Muslim crowd in individualist terms, however, requires either much space (and patient readers) or the readiness to make extensive use of group vocabulary. The latter course, which is the one taken in practice, means losing part of the doctrine's virtues, as the border between methodological individualism and collectivism gets blurred when descriptions are loaded with collectivist terms. Still, insisting on the ontological primacy of individuals as the anthropological equivalent to physicalism, methodological individualists might argue that the concepts of social psychology are only shorthand for the thoughts and actions of what is after all (a collection of) individual agents.[28]

The significance of taking account of plural subjects is indicated when we look at so-called situationist explanations of social action. Social psychologists have shown how people act contrary to predictions, and often contrary to their moral values or even their own senses, when they find themselves in situations in which they the feel pressure to conform to the judgment or expectations of peers, authorities, or a role.[29] Paul Roth has argued that situationism is a mode of explanation that can provide an alternative to both structural and intentional explanations.[30] In such an explanation, in a given type of situation a high proportion of agents will act in a specific way—for example, by throwing stones—irrespective of their personal character traits. By showing how participant agents act *qua* members of social groups, Gilbert's analysis goes some way toward providing a mechanism behind actions that situationism leaves unaccounted for.

Explanatory Ecumenism

I see three reasons for refusing to accept the explanatory imperialism of methodological individualism. The first is epistemological: as historians we often have information about a collectivity or a structure while we lack evidence about individuals that make up the group or the structure. For example, we may have access to a party platform but not to the individual "platforms" of party members or even party leaders.

The second reason is methodological: we may choose a supra-individual level of description of (parts of) society just as we choose a supra-molecular level of description when we account for the movements of a cougar. Accepting the ontological primacy of individuals or atoms does not imply that we are compelled to reserve the notion of causality for events at the lowest or most primary level of description.[31]

The third reason for rejecting methodological individualism has to do with the nature of explanation. As shown in Chapter 5, Peter Railton and Wesley Salmon have both argued that causality is not the only notion worthy of explanatory status. Whether facts are explanatory depends on their being part of what Railton calls "the encyclopedic ideal explanatory text" that contains all information that reduces insecurity about what the explanandum is due to.

This approach accepts both lawlike statements and causal mechanisms—including reasons—as explanatory and where appropriate also allows for structural and functional explanations.[32] The almost-counterintuitive thrust of this ecumenical view of explanation is that there can be more than one legitimate (kind of) explanation of one and the same phenomenon, even under one and the same description. On this account there is no reason to accept Roth's assertion that structural, situationist, and intentional approaches cannot be combined when attempting to explain, for example, the Holocaust.[33] Different approaches yield additional explanatory information that illuminates different parts of the ideal explanatory text. Although agents act according to the situation—and they act differently depending on how they construe it[34]—we should still see them as acting on the basis of the practical syllogisms of belief and desire that are basic in rationalizing action explanations and within structures that constrain the scope of possible actions.

Moreover, it seems to me that there is room in the ideal explanatory text for practical syllogisms with plural subjects that have beliefs and pro-attitudes. Indeed, in the next section I suggest that the ideal explanatory text may perhaps even accommodate linguistic–communal possession of meaning and contextualist understanding of beliefs as partly determined by social practices. This is possible if the community in question is identified as a social group in Gilbert's terms, that is, a plural subject.[35] There are limits to how wide such plural subjects can be construed, however. Drawing an example from my own field of research, I proceed now to test these limits by asking whether the "*Zeitgeist*" or the mentality of an era (in this case, the "spirit of the sixties") can serve an explanatory function.

III. The "Spirit of the Sixties" as Explanans

In an analysis of new social movements, sociologist Karl-Werner Brand writes,

> [T]he *Zeitgeist*, "social mood," or "cultural climate" of a given period means the specific configuration of world-views, thoughts and emotions,

> fears and hopes, beliefs and utopias, feelings of crisis or security, of pessimism or optimism, which prevail in this period. This *Zeitgeist* creates a specific sensitivity for problems; it narrows or broadens the horizon of what seems socially and politically feasible; it directs patterns of political behavior and life-styles; it channels psycho-social energies outward into the public or inward into the private sphere.[36]

Used in this way, an incarnation of the *Zeitgeist* would be the radical or anti-authoritarian mentality of the 1960s. As shorthand it carries with it a host of images and connotations: student revolt, protests against imperialism and particularly the Vietnam War, Marxism, Maoism, hippies, and sex, drugs, and rock 'n' roll. But can it carry the explanatory burden that Brand seems to load upon it—as a creator of sensitivity, a regulator of the width of horizons, a director of patterns of political behavior and lifestyles, and a switch of energy outward or inward? Although Brand's metaphors go far toward giving the *Zeitgeist* agent status, analysts generally shy away from treating it as such. What is the explanatory status of the cultural climate in general and of the "spirit of the sixties" in particular?

We should first notice the close resemblance between the *Zeitgeist* as Brand defines it and the notion of discourse. The latter focuses on the linguistic or literary expressions of the thoughts that make up the *Zeitgeist*: its textual embodiment. (In addition any *Zeitgeist* worthy of the name finds expression in distinct styles of music, visual art, manners, and so on.) Indeed, it seems that a study of the discourse of the time is an indispensable and in practice often the only way to approach the *Zeitgeist* of a period. The two concepts also share some methodological problems indicated earlier. How to define the borders of a discourse, to decide who and what are comprised by the *Zeitgeist*? John F. Kennedy might perhaps have been part of the spirit of the sixties, but hardly Lyndon B. Johnson, and definitely not Richard Nixon or Spiro Agnew. If Bob Dylan was the voice of the sixties generation, must the Beach Boys be turned to silence? Can several competing *Zeitgeister* coexist, showing different horizons to different people at the same time? There is the added complexity of how to define starting and cutoff points for the *Zeitgeist*, illustrated by the subtitle of Arthur Marwick's massive *The Sixties: Cultural Revolution in Britain, France, Italy, and the United States, c. 1958–c. 1974*, or the chapter "Finding the 1960s in the 1950s" in *The Sixties Papers: Documents of a Rebellious Decade*.[37]

I believe the only feasible solution to these dilemmas is to try as best one can to cut the *Zeitgeist* down to size. The "spirit of the sixties" should be regarded as but one cultural factor influencing people in that period (and later). Other worldviews and values made their (competing) influence felt, too. Almost everybody had to relate to the "sixties spirit," but far from everybody was swept up in it, just as everybody had to relate to the presence of Catholicism, but only some converted. Neither Rudi Dutschke nor Paul VI was omnipotent in 1968, though both were hard to ignore completely.

Then there is the question of the relation between the *Zeitgeist* and its discursive expressions, on one hand, and the individual thoughts and texts on which it supervenes, on the other. The *Zeitgeist* as (part of) the cultural climate of an era is best considered as a social emergent that is realized by a multitude of thoughts embodied in texts, music, pictures, and so on. As shorthand, to suggest the direction of prevailing intellectual winds in a period, it can be useful and explanatorily economical. However, once we endow it with cognition or will or other faculties associated with agents, and suggest (by words such as *influence* in the preceding paragraph) that the cultural climate has the power to move individuals who realize it and in virtue of whom it exists, the red flag of downward causation appears. We seem compelled to conclude that the *Zeitgeist*—as well as its textual embodiments—is causally parasitic on its supervenience base.

The issue is complicated by Bhargava's insistence that meaning is socially embedded and by the (post)structuralist view that individual agents can never define or control the meaning of what they say and write. The implication is that the supervenience base of the "sixties spirit" is not individuals of that era but the discourse of (the radical part of) the sixties society as a collectivity. If the full meaning of a text is possessed not by its author but by the linguistic community, and if beliefs are not individual-psychological states of mind but are inseparably tied to social practices and that thus they can be properly understood only in the light of such practices, the cultural climate resists reduction and must be deemed an emergent social property.

Such a radical contextualist or poststructuralist interpretation of the "spirit of the sixties" seems to render explanation as normally practiced by historians a conceptual misunderstanding, since individual agents with beliefs and desires disappear into the linguistic community or the discourse. A somewhat more conventional interpretation would be to see the "spirit of the sixties" as representing the worldview and values of the radical "sixties community" or "sixties generation" as a plural subject. That group, of course, did not comprise everyone coming of age in the 1960s but consisted of the radical or anti-authoritarian part of the cohorts, those who saw themselves as participant agents in what became known as the "New Left." That the New Left had no clearly defined membership does not mean it should not be regarded as a plural subject; the question is whether its participants saw themselves as being part of a "we" with a jointly accepted view or jointly ready for action. If the answer is yes, Gilbert's analysis of social facts suggests that the "sixties spirit" may have an explanatory function and not only as shorthand.

In the United States, and probably also in the rest of the Western world, the group belief of the "sixties generation" was articulated in the Port Huron Statement by Students for a Democratic Society (SDS) in 1963. The twenty-page document, which argued that "a new left must consist of younger people who matured in the post-war world" and must "include

liberals and socialists," is a suitable yardstick for measuring the group of radicals, defined as those who jointly accepted the statement's views:

> We are the people of this generation, bred in at least modest comfort, housed now in universities, looking uncomfortably to the world we inherit. . . . We regard *men* as infinitely precious and possessed of unfulfilled capacities for reason, freedom, and love. . . .We would replace power rooted in possession, privilege, or circumstance by power and uniqueness rooted in love, reflectiveness, reason, and creativity. As a *social system* we seek the establishment of a democracy of individual participation.[38]

Let us allow that the sixties radicals were a social group whose worldview and values were represented by the "spirit of the sixties." This would make that spirit causally relevant to the degree that participant agents of the New Left *qua* members of that plural subject suspended their personal beliefs on some matters to make their views accord with the group belief. In that respect the *Zeitgeist* consolidated and harmonized the beliefs of the members of the group not by any action of its own—thus, we escape the charge of downward causation—but by being regarded by participant agents as containing the proper worldview and values of sixties radicals, who pooled their wills in order to be part of the "sixties generation" or "the movement."[39] Such suspension of judgment or pooling of wills by members of the anti-authoritarian "sixties community"—the irony of which should not be lost on us—may account for not only *prima facie* group actions such as protest marches or group beliefs such as blaming most of what was wrong in the ("third") world on U.S. imperialism but also for apparently more private actions and beliefs such as young men's decision to let their hair grow, for radicals' acceptance of illegal nonviolent (and sometimes a bit violent) protest actions, for students' massive denunciation of liberal university teachers and the demand for equal voting rights for students and teachers, for socialists' uncritical endorsement of Marxism, for leftists' idolization of Mao Zedong and Ho Chi Minh (and even Lenin), and for other relevant phenomena.

The potential value of the notion of plural subjects for the explanation of the events often somewhat imprecisely grouped under the term *1968* becomes clearer when we keep in mind the flexibility of social groups instead of focusing solely on the unwieldy concept of a "sixties generation." Most people felt the influence not of the "spirit of the sixties" in its totality but of their peers and tried to relate, adjust, or conform to what they conceived were the views and values of friends and of organizations of which they were members. For example, the readiness of many Marxist–Leninists to accept often without question dramatic shifts in the political directives emanating from the party leadership can be explained in this light.

Diehard methodological individualists may argue that all the above actions and beliefs can be explained within the confines of that doctrine. I would reply that sometimes we cannot (because we lack evidence of

individual beliefs), and often we need not (because an explanation at a higher level of supervenience satisfies); we should not always attempt to meet the demands of methodological individualism. Besides, by insisting on treating and explaining all action on a methodologically individualist basis we run the risk of depriving ourselves of the social or collective element of what went on at the time. The building of barricades in Paris or the siege of the Pentagon can hardly be fully understood outside of a social group context. To employ Gilbert's terms, it was not singular agents that occupied university buildings and so on; it was a plural subject—a configuration composed of individuals but acting as a group. Perhaps it was not even singular agents who dropped out of college and moved into hippie communes, but members of the imagined community of "the movement." The proper explanation for the destruction of twenty-four windows at the U.S. Embassy in Oslo after a protest march against the Vietnam War in May 1970 is not twenty-four practical syllogisms each starting with an individual agent's desire to throw stones in order to break windows in the belief that this would compel Nixon to make peace; it is a syllogism starting with *we* (and continuing on the same [peculiar] logic as the individual one).[40] That the police, backed by the law, insisted on holding six protesters personally responsible for the destruction should not blind us to the collective nature of the action. The police do their job—trying to prevent damage to private property—using whatever legal tools they have at their disposal. As historians we have another job to do, namely, to understand and explain the action. Instead of restricting ourselves to methodological individualism, we should use all the tools available to us.

Acknowledgments

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Notes

1 For the events, see Jerichow and Rode, *Profet-affæren*; for the debate, see Modood et al., "Danish Cartoon Affair"; Klausen, *Cartoons That Shook the World*; Lindekilde et al., "Muhammad Cartoons Controversy."
2 Mandelbaum, "Societal Facts"; Brodbeck, "Methodological Individualisms."
3 Durkheim, *Rules of Sociological Method*, *passim*, esp. 39–40 and 128–129. Greenwood, "Social Facts," presents a Durkheimian account of social facts that does not presuppose the idea of social emergent properties.
4 Durkheim, *Rules of Sociological Method*, 39.
5 MacLaughlin, "Rise and Fall of British Emergentism."
6 Beckermann, "Reductive and Non-Reductive Physicalism," esp. 11.
7 Kim, "Downward Causation," esp. 128–130.
8 Beckermann, "Reductive and Non-Reductive Physicalism," 14.
9 For an analysis of social groups along these lines, see Tuomela, *Importance of Us*, esp. 256–269, 301–305, 356–375.

10 Sawyer, "Mechanisms of Emergence."
11 Sperry, "Macro- versus Micro-determinism."
12 Kim, "Downward Causation," 134–135, italics in the original.
13 Kim, "Downward Causation," 134–135. Pettit, *Common Mind*, 149, has a similar line of argument.
14 Tuomela, *Importance of Us*, 194.
15 Bunge, *Treatise on Basic Philosophy*, 40–42. On aggregative wholes, see Sawyer, "Mechanisms of Emergence," 271–272.
16 Elster, *Nuts and Bolts*, 13; cf. Elster, *Making Sense of Marx*, 5.
17 Elster, *Nuts and Bolts*, 74.
18 Elster, *Making Sense of Marx*, 6.
19 Bhargava, *Individualism in Social Science*, 194, cf. 221–222: "Not an individual on his own but only a society can know the full meanings of a term."
20 Bhargava, *Individualism in Social Science*, 198–227, quotations from 212 and 199, respectively.
21 Gilbert, *On Social Facts*; Searle, *Construction of Social Reality*, esp. 25–26; Carr, *Time, Narrative, and History*, 130–135 and 147–163; cf. Carr, "Narrative and the Real World," esp. 151–154. Tuomela's analysis of social groups is in many respects similar to Gilbert's, though he is critical of the vagueness of such notions as "joint readiness" and "joint acceptance." The idea of the pooling of individual wills into a shared "group will" is central to both Gilbert and Tuomela. Their main difference is that on Tuomela's account every social group must have an authority system, a notion that Gilbert disregards (see Tuomela, *Importance of Us*, 217–220 and 310–312). Although Tuomela (174) insists that such an authority system that "represents the group members' transference of their wills to the group for group disposal" need not be formal, his account appears more suited for analyzing formal organizations. These fit less well within Gilbert's notion of plural subjects, which seems tailored to the analysis of informal groups such as a family or a crowd. For a recent version of Gilbert's views, as well as answers to critics, see Gilbert, *Joint Commitment*.
22 Gilbert, *On Social Facts*, quotations from 204–205 and 306–307. "Jointly" indicates that the acceptance must be what Gilbert terms "common knowledge" (186–197). While Carr focuses on larger communities, his definition resembles Gilbert's: "we are not here making a simple, straightforward claim that group-subjects objectively exist. We are saying that individuals, in their sense and use of 'we,' certainly *take* them to exist and that their taking them to exist in a sense makes it so. In saying 'we,' the individual identifies himself with the group and thus . . . *constitutes* the group as comprising those who similarly, in the relevant context, say 'we'" (Carr, *Time, Narrative, and History*, 133).
23 Tuomela, *Importance of Us*, 320–322, agrees, but labels this a "spurious group belief."
24 Gilbert, *On Social Facts*, 422.
25 See McNeill, *Keeping Together in Time*.
26 McNeill, *Keeping Together in Time*, 422–427.
27 See also Pettit, *Common Mind*, 138.
28 There is, of course, the dilemma that individual agents, too, are supervenient on their microphysical base. Accepting Donald Davidson's anomalous nomism, we might still argue that the concepts of folk psychology are nomologically irreducible to microphysics and hence indispensable in the social sciences. Cf. Davidson, "Psychology as Philosophy."
29 Haney et al., "Interpersonal Dynamics"; Ross and Nisbett, *The Person and the Situation*.
30 Roth, "Hearts of Darkness."

31 This is C. Behan McCullagh's argument against Pettit's "causal fundamentalism," see McCullagh, *Truth of History*, 295–297.
32 Salmon, "Scientific Explanation"; Railton, "Probability, Explanation, and Information." Pettit exhibits another variation of explanatory ecumenism in *Common Mind*, 218–220.
33 Roth, "Hearts of Darkness," 215.
34 Ross and Nisbett, *Person and the Situation*, 11–13 and 59–89.
35 Bhargava, *Individualism in Social Science*, 204 and 224–225, indicates that this is a possibility.
36 Brand, "Cyclical Aspects," 28.
37 Marwick, *The Sixties*; Albert and Albert, *Sixties Papers*. For a discussion of what I have termed the calendric fallacy, see Førland, "Cutting the Sixties Down to Size," esp. 126–127.
38 "The Port Huron Statement," in Albert and Albert, *Sixties Papers*, 176–196, italics in the original.
39 Cf. the title of Terry H. Anderson's impressive and kaleidoscopic *Movement and the Sixties*, still perhaps the best overview of the movement in the United States.
40 On this event, see Godbolt, *Den norske vietnambevegelsen*, 274.

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7 Acts of God?

Miracles and Scientific Explanation

At the conference "Secularism and Beyond" that I attended in Copenhagen in May 2007, professor of theology at the University of Birmingham Werner Ustorf expressed dismay that participants treated religion "just like any other social phenomenon." Apparently he thought too little focus was given to the transcendental qualities of religion: to the idea—believers might say to the conviction—that religion is the ordering of the relation between humans and the supernatural (God).

The theology professor's intervention points to a major methodological issue, namely, how social scientists of the twenty-first century, including scholars in historical disciplines, should approach religion and religious beliefs and practices. This is the question I discuss in this chapter, taking a report about a miracle from the seventeenth century as my point of departure. While the case is historical, the issue is of current relevance since supernatural or religious explanations, and acts founded in such beliefs, abound.

Before presenting my historical case I sketch what I think is the dilemma of modern scholars of religion. The scientific worldview has no room for God or other supernatural entities. I claim not that all scientists must be atheists or that science has proved that God does not exist. (I think it has disproved some notions of the concept and the theology accompanying these.) Scientific explanations, however, are void of supernatural entities: they explain the world naturalistically. At best, God is left as a spiritual possibility but one that is not allowed to enter into or influence scientific explanations of how the world works.

Most religious worldviews, on the other hand, are based on, or at least include, a contrasting notion, namely, that God or some other supernatural entity or force not only exists in a sphere excluded from this world but is active in the world, having the ability to affect matter and matters. Accordingly, the supernatural at least potentially has a role in religious explanations of how the world works.

The result of these opposing views is what David Gary Shaw in *History and Theory*'s 2006 theme issue on Religion and History called the "deep ontological dissonance" that is created "when historians disagree with their subjects as to the possibilities of supernatural beings or supernatural

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actions."[1] The problem is not limited to historians or historians of religion; rather, as Shaw observes, it is "the classical anthropological problem: confronting the seriously other and trying to find the best way to learn from that encounter."[2]

I am aware of no successful attempts to unite, in a logically consistent language, the competing worldviews of science and religion—religion defined in a substantive way, as entailing belief in supernatural beings or powers that can influence the course of events in the material world.[3] They appear mutually exclusive. Does this mean that not only science but also social science and the empirical humanities—including history and the study of religion—are unable to speak to believers in an idiom they accept? And vice versa: is the implication that believers cannot convey their experiences to (social) scientists in a way acceptable to the latter? Is there an unbridgeable gap of communication? I think yes. This chapter is an attempt to show why.

I. A Miracle on Fox Lakes

In 1652 God reportedly saved two brothers from starvation. The two, Anders and Ola Engebretsen, had been fishing for some days in some lakes called *Revsjøene* (the Fox Lakes) in *Gudbrandsdalen*. On the morning of August 6, they rose early, left their cabin, and went out in their boat to take in their fishing nets. They left their outer garments by the shore so they would be able to work more freely. They rowed over to an islet called *Studenten* (the Student), where they had put out their nets the previous day, and went ashore.

Suddenly a gust of wind took their boat and left the brothers stranded on the tiny island, the area of which was less than ten by twenty meters. The distance to the shore was only 150 meters, and the water was not very deep. That was of little comfort, however, since it was too deep to wade, and neither of the two could swim. They could hope that a hunter or fisherman might accidentally come by and save them, but chances were small. Their only additional hope was God, "who by no ordinary means and contrary to all expectancy can help his own."[4] So they prayed fervently.

They had few clothes, the wind was blowing, and the first night was freezing cold, so on the second day, the brothers gathered stones to build a small, rectangular, roof-less shack to get shelter from the wind. They had another reason for doing this as well: the stone walls might stop their dead bodies from being taken by the waves that washed over the islet in the fall. The prospect of their corpses being left to decay in the wilderness—the devil's domain—was an ugly one. If the shack could function as a temporary tomb there was hope eventually their bodies would be found and receive a Christian burial.

The cold and the wind were only the first problem. Worse in the long run—which would not be very long unless their luck turned—was the hunger they soon felt. Eventually they found a kind of edible plant known

as *viola canina*. Rations were very small: no matter how thoroughly they searched, they never found more than one spoonful for each brother every morning and every evening. Stranger still: even in places where they had picked every blade the night before and had carried the remaining soil inside their shack to make a floor, they would find fresh plants the next morning. They ate the plants, praised God, and sang hymns to the Lord who thus provided for them—albeit meagerly.

On the twelfth day the youngest brother, Anders, who by the way had studied (theology, of course) in Copenhagen, was so exhausted he could no longer walk but could only crawl around on his knees. His older brother was even more worn out, and when that night they found no edible plants, they understood this was the end. They made their wills and used their knives to write on some wooden sticks what had happened to them. For a funeral homily, they chose a text from Psalms and crept into their shack to die.

But death did not come. Instead, there came a search team from the village, alarmed by the lonely return of the brothers' dog. The dog had accompanied Ola and Anders on their fishing trip but had stayed on land when they went out in the boat. For eight days it had waited and had wisely—and fortunately for its masters—resisted their attempts to lure it over to the island (where they had planned to eat it). Instead, it had finally run the 40 kilometers back to the village, where its peculiar behavior made people suspect something was wrong with the brothers. One neighbor went looking for the two, found their outer garments by the shore, decided they must have drowned, and went home.

Two days later more neighbors went to search for the brothers, perhaps to find them in the lake. On the night before the thirteenth day, they came to the lake, and when the brothers heard men and horses, they managed to shout so the rescue mission noticed their cries and saved them. It was in the nick of time: for eight days Ola was suspended between life and death. Both he and his younger brother survived, however, and in 1691, almost forty years later, Anders (the former student) wrote down the story of their being saved by the grace of God.

Finn Erhard Johannessen, who has written the local history of the Fox Lakes on which I have based my rendering of the story, has established that Anders Engebretsen probably wrote at least two slightly different versions of the dramatic event. Although lost, the two versions found their way into books written by local priests in, respectively, 1732 and 1743. Johannessen notes that "the story is told in a very reliable way" but dismisses the report of "the grass that came up in small portions every day, also where it had been plucked bare the day before," stating that "it can hardly have happened."[5] He goes on to explain away the miracle as the result of the Engebretsen brothers being so weakened that they had lost track of where they had found the plants the day before, and suggests that Anders had modeled his report of the miracle on the Bible's miracle stories of the

Israelites in the desert who were fed with manna from heaven (Exodus 16) and the widow in Zarephath whose bin of flour was not used up and whose jar of oil was not dry (1 Kings 17:8–16).

Interestingly, Johannessen is no atheist; he may well say his evening prayer. The idea of God's hand (or in this case, his little finger) should not be anathema to him. Still he rejects out of hand the possibility of a miracle so puny that even the theologian Erik Pontoppidan, who included the story in his *Natural History of Norway* published in 1752–1753, found its most remarkable feature its economy.[6] Why is God's providence written off by Johannessen in his discussion of the Fox Lakes incident when in another capacity he believes in his existence?[7] More generally, why are historians unable to accept God as agent?

The answer to this question contains the crux of the issue I raise in this chapter, namely, the reconciliation of scientific and religious worldviews. Before presenting my answer, however, I must resist no less than four temptations to take the easy way out of what I insist is a real dilemma, and where consequently what may look like easy ways out are dead-end streets.

II. Four Temptations

Temptation 1: Explaining Evidence not Events

The first temptation may seem a technicality but has a real presence. Normally when we speak of explanations we think of an explanandum *event*: some incident or process that is to be explained, causally or otherwise. Philosophically inclined historians would perhaps specify that the explanandum is an event *under a description*, to ensure that we do not create a multitude of seemingly contradictory explanations that, in fact, are compatible because they explain different features or descriptions of the same phenomenon. To ensure that all explain the same thing (that is, the event under one and the same description) it is often wise to keep the description of the explanandum clinical: as neutral as possible, as I have attempted in my description of the Engebretsen brothers' ordeal at the Fox Lakes.

The alternative to the explanandum-event perspective is the view that historians really explain not events but *evidence*: remains that carry information of a purported incident or process in the past.[8] In my example of the Engebretsen brothers, the explanandum would then be not what happened on the islet—that would be the event—but the evidence left behind in the form of the priests' versions of Anders's story. (Since his reports are lost they are no longer available as evidence.)

This distinction between explanandum events and evidence could be of major importance, since if we are to explain the evidence—the reports in the books by the local priests—it seems we can escape the question of what really happened on the islet. It would be sufficient to note that both the

Engebretsen brothers and Anders's clerical ghostwriters lived in a cultural climate in which the notion of God's providence was commonplace and would seem the natural way to explain the daily provision of edible plants. We could trace the evolution of this cultural climate; we might even speculate that the Norwegian priests were aware of the fierce debate on miracles that was raging in Britain at the time and suggest that they saw their rendering of the *viola canina* miracle as their contribution. This idea is less far-fetched than it might seem, since according to R. M. Burns the "Great Debate on Miracles" had "at its height—around the 1720s— . . . received enormous attention; so much that almost every English theologian, philosopher, or even simply man of letters of the period made some contribution to it."[9] There would be no need for us, however, to enter into speculation as to *wie es eigentlich gewesen* that the brothers found the plants.

This is tempting but succeeds only to a point. It is of no use when scholars are interested in what really happens or happened—how the world works—and not just in what actors think happens or thought happened. Put differently, we might take an interest in the event of which the evidence reports, whether current acts of miraculous healing or historical incidents such as the angelic communication of the Book of Mormon to Joseph Smith, Jr., in the late 1820s, or the resurrection of Jesus of Nazareth. The accuracy of such evidence matters. To the degree that we want to explain the event, we are back at the problem of providing an explanation that is acceptable to the religious actors as well as to the scholarly community. We cannot take the easy way out.

Temptation 2: Sophistry on Hume and Miracles

The great debate on miracles in Britain was capped by the publication in 1748 of David Hume's essay "Of Miracles." Here Hume argues against the possibility of miracles, which he defines as "a violation of the laws of nature."[10] The exact nature of Hume's argument is still debated, especially whether he intends to provide, in the first part of his essay, an *a priori* argument against miracles, or mainly intends this part to show the extreme evidential difficulty of asserting miracle claims.[11] There is little disagreement that in the second part his intention is to demolish the credibility of testimony about specifically *religious* miracles: violations of laws of nature—by God or some other supernatural being—that serve as a foundation for religion. Fortunately for our purpose it suffices to be aware of the debate and of the eventual influence of Hume's essay.

Aviezer Tucker has faulted Hume for being ahistorical, since neither the ancient Hebrews who introduced the concept of miracles nor the Greeks, who provided the other main source of Christianity, entertained the idea that nature was governed by inviolable laws. He also states that, strictly speaking, none of the main forms of biblical miracles constitute absolute violations of the laws of nature. Resurrections of the dead, transmutations

of essential fluids, curing of sicknesses (including prolonged infertility), and military victories—all by divine intervention—and prophecy only violate laws of nature relative to specified initial conditions, and such conditions are not specified in the Bible.[12]

Again we can avoid going into the essence of this debate. The concept "miracle" is not used in the stories of the Engebretsen brothers' narrow escape from death on the Student islet, and no one has argued that the provision of fresh plants on ground that had been barren the day before constituted a violation of the laws of nature. Tucker's sophistry is beside the point, which can be put as follows. First, there is no reason to doubt that the Engebretsen brothers and their contemporaries believed Providence had acted to save the two from what otherwise would have been a certain loss of life. Second, there is nothing in Anders's description that enables us to give a convincing naturalistic explanation of the daily provision of fresh—if admittedly meager—*viola canina* on barren ground. That we need not imagine God breaking any laws of nature to see his active hand in catering to the lost brothers only makes it clearer that we are facing a real dilemma: either we accept the religious explanation provided by Anders and his clerical ghostwriters, or we substitute a secular explanation that accounts for the same events without invoking God. While the latter path is fairly easy—which again serves my methodological purpose—it is immensely more challenging to provide a naturalistic, that is, scientifically acceptable, explanation of the events that would also be acceptable to Anders and his brother.

Temptation 3: Becoming Resigned to the Epistemological Gap

The event I have taken as my point of departure happened more than 350 years ago. The central actors probably believed the Earth was the center of the universe; Newton had yet to develop his physics. With Foucault we could speak of an epistemic shift—or several—since the days of the Engebretsen brothers' ordeal on the Fox Lakes. To put it simply and in an un-Foucauldian way, today we know a lot more about how the world works than did Anders and Ola Engebretsen. Though their clerical ghostwriters—and, to a lesser degree, Anders—were educated men, the progress of science has given us immensely more knowledge than they had about the workings of the world. It is even a question whether a meaningful conversation on ontology would be possible between them and us, so wide and deep is the knowledge gap. In one respect this gorge renders meaningless the ambition to provide an explanation that would be acceptable across this 350-year epistemological divide.

But concluding that the epistemological gap is too wide to cross is just another apparently easy way out, and I am not looking for escapes. The problem I am discussing is whether we can provide mutually acceptable explanations, on the admittedly hypothetical and in many instances even counterfactual premiss that the epistemological gap is not so wide that it

cannot be bridged to the degree that meaningful conversation is made possible. The epistemological distance between the Engebretsen brothers and us is much greater than the 150 meters from the Student islet to the shore. And yet we must imagine them walking on the water to cross over to current epistemological shores. (The suggestion that we come to them is temptation 4.)

One reason why we should not leave the brothers stranded on their epistemological seventeenth-century islet is that the situation described—someone claiming God acted to save them or, more generally, influence matters—is not at all restricted to historical actors in long-gone periods. In the West today events are often explained by reference to the active presence of God or other supernatural forces. According to a Gallup poll, in 1989 four in five Americans agreed with the proposition that "even today, miracles are still performed by the power of God."[13] President George W. Bush reportedly sought God's advice before striking back on terrorism. Norwegian Princess Märtha Louise, who is trained as a physiotherapist and has made a career as a storyteller, in 2007 claimed she could communicate with angels and had healing powers.[14] A course in which students, for 3,000 euros a year for three years, would learn reading, healing, and touching from the princess and her associate was fully subscribed after only two days of media reporting.[15]

Presidents and princesses aside, highly educated people pray in earnest. Forty percent of top U.S. scientists "believe in a God to whom one may pray in the expectation of receiving an answer," meaning "more than the subjective, psychological effect of prayer."[16] Here there is no epistemological time gap: not only Galileo and Newton (a devout Christian) but also Einstein and Bohr and so on—even Darwin—are part of the common knowledge ground for both believers and atheists. We can converse meaningfully with Mormons or Baptists. But can we provide mutually acceptable explanations of how the world works in instances where believers claim God has acted to influence events?

Temptation 4: Asking What It Meant to Them

In the 2006 *History and Theory* theme issue Brad Gregory recommends that historians who study religious actors have as their "guiding question" the simple "What did it mean to them?" This will help historians "to 'get' religion on the terms of religious people," which is achieved when the actors would "recognize themselves in what we say about them."[17] While tempting, on closer scrutiny this route leads nowhere.

Gregory's guiding question is a variation of what historians have been doing since Ranke, namely to ask how their actors construed the situation. Anthropologists excel in this task. This is but a first step in the historian's (or anthropologist's) research, however, for we cannot rest content with merely retelling or repackaging the conceptions of our study objects. Our job is not only to understand and explicate what actors thought but to

explain why they thought so. In this task there is no escaping our own ontology. We cannot satisfy ourselves with explicating that believers in the seventeenth century thought a certain woman was possessed by the devil—or that some people have such notions today. In the winter of 2007 Pastor Leif Sommerseth of the Norwegian Lutheran State Church was called to exorcise an evil spirit from a kindergarten in Lakselv, in northern Norway. After eliminating the possibility that the problem was due to psychological factors, the pastor was able to locate the spirit in a small part of the area of the kindergarten.[18] Asking what this meant to the pastor is perhaps necessary but surely insufficient and unsatisfactory. We must explain the events, and this often entails, as Gregory admits, judging the truth value of the beliefs of our study objects.[19]

There is another, fundamental, reason why Gregory's empathetic approach is no solution to our dilemma of reconciliation between science and religion. The reason is hermeneutical. R.G. Collingwood has argued forcefully that the whole idea of asking "what did it mean to them" by bracketing one's own worldview and values—in Gregory's words, "not to impose *any* metaphysical beliefs or moral judgments on religious people"—is mistaken.[20] The historian, according to Collingwood, "not only re-enacts past thought, he re-enacts it in the context of his own knowledge and therefore, in re-enacting it, criticizes it, forms his own judgment of its value, corrects whatever errors he can discern in it." Such criticism, moreover, is an integral part of reenactment:

> The criticism of the thought whose history he traces is not something secondary to tracing the history of it. It is an indispensable condition of the historical knowledge itself. Nothing could be a completer error concerning the history of thought than to suppose that the historian as such merely ascertains "what so-and-so thought", leaving it to some one else to decide "whether it was true". All thinking is critical thinking; the thought which re-enacts past thoughts, therefore, criticizes them in re-enacting them.[21]

Although I reserve judgment on Collingwood's assertions, his remarks point to the basic question of what is the purpose of attempts at historical reenactment or understanding the thoughts of religious actors. We do not study past (or contemporary) religious actors for the sake of the past or the actors themselves; we study history or religion for our own sake. The fundamental question is not "What did it mean to them?" but "What does it mean for us?" What it meant to them is just a step on the road to what is in it for us, and at some point along this road our own ontology must substitute for the ontology of our study objects which, after all, has only historical (or anthropological) interest in a limited sense.

In support of my view that when explaining purported miracles, we should substitute our scientifically based ontology for the supernaturally

infused religious ontology of our objects of study, I invoke yet another authority, namely, that of Marc Bloch. In his study of the reputed ability of medieval and early modern English and French monarchs to cure subjects of scrofula by their "royal touch," Bloch states flatly that "hardly anyone nowadays would dream of invoking [supernatural causes] in the matter under discussion." Continuing that "it is obviously not good enough simply to reject the ancient interpretation [that the kings were blessed with healing powers] out of hand because it is repugnant to reason," he asserts that "we must try to discover and substitute a new interpretation acceptable to reason. This is a delicate task; yet to avoid it would be a kind of intellectual cowardice."[22] Discussing the evidence in light of modern medical knowledge, he concludes that the physician-princes, while not impostors, never healed anyone. "The real problem, then, will be to understand how people believed in their wonder-working power when they did not in fact heal."[23] Bloch dismisses the testimony of contemporary witnesses and the effectiveness of the royal touch because he applies his superior, scientifically based knowledge. Describing how the notion of miracles abounded in medieval and early modern Europe—"there were no saints who did not have their miraculous exploits; no sacred things or persons without their supernatural powers"—and pointing to the political advantages kings could get from having healing powers bestowed upon them, Bloch moves from understanding the actors he studies to explaining them:

> It was noticed that a much-feared disease would sometime yield—or appear to yield—after [the kings] had laid hands, which were almost unanimously considered as sacred, upon it. They could scarcely avoid seeing it in terms of cause and effect and concluding that the looked-for miracle had indeed occurred. What created faith in the miracle was the idea that there was bound to be a miracle. And this was what kept the belief alive, as well as the accumulating witness of the generations down the ages, all those whose testimony, apparently based upon experience, seemed impossible to doubt. As for the probably fairly numerous cases where the disease resisted the touch of the august hands, they were soon forgotten. Such is the happy optimism of believing souls.
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> Thus it is difficult to see faith in the royal miracle as anything but the result of a collective error.[24]

III. The Best Explanation of the Fox Lakes Miracle

Having resisted the temptation to seek easy ways out of the dilemma of reconciling science and religion, it is time to return to the puny Fox Lakes miracle. Why does the historian Johannessen, a believer in God, dismiss the actors' religious explanation of the event?

Philosophers seem to converge on the view that historians choose or at least justify singular descriptions of past events (or evidence of these events) by a form of inference known as arguments to the best explanation. This is a method for choosing one among two or more incompatible hypotheses from which can be inferred statements describing the available evidence. The critical question is which explanatory hypothesis to prefer or, rather, which criteria to employ in order to select the hypothesis that best explains the evidence (and, at one remove, the event about which the evidence reports).

The exact enumeration and formulation of the criteria for arguments to the best explanation are matters of philosophical debate, as seen in chapter 4 on values guiding theory choice. Still most scholars would regard the following criteria as relevant to the discussion here.[25] One is *scope*: the greater variety of data or evidence that is implied by the explanatory hypothesis, the better. A second criterion is *power*: the more probable the hypothesis renders the data, the better. If the hypothesis is that God acted to save the Engebretsen brothers, this renders the existence of the reports of the miracle more probable than the alternative hypothesis that Anders made up the story. *Simplicity* is another agreed-upon criterion, meaning that the fewer ad hoc suppositions a hypothesis includes, the more reason there is to prefer it over more ad hoc alternatives.

The final and perhaps most important criterion is *external consistency*. This has both a positive aspect—the degree to which accepted beliefs (or "truths") imply the hypothesis—and a negative aspect: the degree to which the hypothesis is disconfirmed by existing accepted beliefs. Norway's grand old man of philosophy of history, Ottar Dahl, puts it thus in the methodology textbook that was used by students at the University of Oslo for forty years after its appearance in 1964: "the decisive justification criterion is whether a hypothesis *fits* 'what else we know,' i.e. the totality of our theories."[26] People's evaluation of external consistency, however, varies with their ontology. Who are Dahl's "we" that "know" things? Believers and atheists have different ideas of which beliefs are accepted—by whom?—or "reasonable"[27] about the setup of the world. By glossing over or ignoring this difference, which amounts to a divide with respect to the existence of an active God, philosophers make things too easy. When it comes to God, there is no consensus on "what else we know" or on what are accepted or reasonable beliefs, let alone truths.

Johannessen, perhaps unaware of the formalities of inference to the best explanation but surely cognizant of Dahl's methodological advice, observes that the Fox Lakes story "contains nothing that is contrary to usual notions of how the world works *when we except the little grass miracle.*"[28] This, as we have seen, he thinks "can hardly have happened." Why not?

My take is that Johannessen rejects the miracle hypothesis because as a historian—that is, *when doing historical research* (including writing)—he sees himself as part of what Margaret Gilbert terms a "plural subject" that is constituted by the scientific community.[29] Thus, he is part of a "we"

whose ontology is provided by, or at a minimum must conform to, the findings of nuclear physicists, chemists, evolutionary biologists, and others. Their explanations of how the world works have no room for supernatural beings: the scientific quest is *natural* science. "The physical world," claims Nobel Laureate in medicine Gerald Edelman, "is causally closed to anything but the interactions of matter–energy."[30] As Jaegwon Kim puts it, "the totality of physical particulars 'exhausts' . . . all of concrete existents."[31] Natural scientists as well as social scientists may believe in God—but not *qua* scientists. The plural subject of science—"we scientists"—are atheists.[32]

To scientists *qua scientists*—natural and social alike—the consistency criterion of arguments to the best explanation should therefore be understood to relate to the degree to which accepted *scientific* beliefs (truths) imply or disconfirm the hypothesis. This is fatal to the miracle hypothesis in the Fox Lakes story, since no accepted scientific truths imply provision miracles and it can well be argued that quite a few scientific truths disconfirm them, irrespective of whether the miracles in question entail a violation of the laws of nature.

To be accepted as science—to be part of the scientific community—even theology must conform to scientific atheism. This means, of course, not that theologians must be atheists, only that when doing theological research they do not have recourse to the idea of a God that takes an active part in the world. Admittedly this amounts to a somewhat anemic theology, which is exactly the criticism leveled at university theologians from lay colleagues. Indeed, theologians have only reluctantly accepted the exclusion of the supernatural from their explanatory toolbox *qua* scientists.[33] The alternative, however, is the exclusion of theology from the scientific community.

IV. Metaphysical Naturalism as Secular Confessional History?

Believers and radical postmodernists sometimes attack what they consider unwarranted claims to a privileged epistemic position on behalf of science. This position is exemplified by Gregory's assertion that the "metaphysical naturalism" of current science is parallel to and a functional equivalent of confessional religion, since it is based on assumptions that are "undemonstrable," namely the nonexistence of the supernatural. He thinks that "to adopt a theory or theoretical hybrid purporting to explain religion in terms dictated by metaphysical naturalism is to work in a manner analogous to that of a traditional, religious confessional historian, insofar as one's analysis relies substantively on one's own beliefs."[34] Not only an unashamed and self-declared social *scientist* such as Émile Durkheim but also cultural-relativist doyens Clifford Geertz and Michel Foucault are seen as having built their analyses of religion "ultimately on a dogmatic metaphysical naturalism, or on its functional equivalent, a thoroughgoing skepticism about all religious claims—that *no* religion is, indeed *cannot be*, what its believer-practitioners claim that it is."[35] In Gregory's opinion,

"to assume metaphysical naturalism in order to analyze religion's cultural expressions and meanings . . . precludes understanding those meanings on practitioners' terms."[36]

I think Gregory misrepresents science and misunderstands social scientists. First, metaphysical naturalism is not a dogma but an admittedly basic working hypothesis of modern science. Naturalism has attained this position because modern science is an empirical quest, and the realm of the supernatural has so far been inaccessible empirically. Science refuses to include the supernatural in its description of the world because claims about the existence of the supernatural have been impossible to support, or even evaluate, empirically,[37] hence the naturalistic assumption of science, which is a whole different matter from the unempirical and unscientific—though not necessarily false—beliefs of religious people about God, salvation, the afterlife, and so on.

Second, Gregory seems not to have grasped the consequences for social science of the unempirical foundation of religious beliefs, which has led to their exclusion from the scientific realm. He quotes Richard White's comment "I am a historian. I don't believe in transcendence" and observes that "the assumptions behind such remarks are theologically atheistic, metaphysically materialist, and culturally relativist, framed by the postulates of the natural sciences."[38] *Qua* historian White does not have much choice, however, but to renounce belief in transcendence. If we amend his comment to "*When* I am a historian, I don't believe in transcendence," the point becomes clear. When discussing Geertz, Gregory gets close to the gist of this matter. In a footnote he quotes Geertz's references to "the business of the scientist" and "the self-imposed limitations of the scientific perspective" that bar him from considering the questions of, respectively, divine intervention and the truth value of religious claims.[39] With Collingwood I think Geertz cannot escape taking a stand on such matters although he need not "pronounce upon" them,[40] but that is not what is at issue here. Gregory fails to see the implication of these remarks, namely, the admittance that *qua* anthropologist (or any other empirical scientist), one cannot believe in transcendence or divine intervention and so on, since such claims lack empirical substance and cannot be tested scientifically. Personally one may believe in God or even miracles—lots of scientists do—but not as a historian or social scientist.

V. Living with an Unbridgeable Gap

The gap between, on the one hand, atheistic science and, on the other hand, believers in supernatural beings that take an active part in the world, seems impossible to bridge. Scientific descriptions of the world exclude the realm of the supernatural, on which religious worldviews are based. This means that religious descriptions *ipso facto* are unscientific. But being unscientific is not equivalent to being false. Perhaps the awareness

of this is what explains why so many scientists are religious, that is, they believe in supernatural beings with powers to influence events in the material world. The explanation can hardly be that scientists are schizophrenics but, rather, that people have a great capacity for what we might call multiple partaking: being part of several plural subjects at the same time, as well as being a private subject, without feeling torn to pieces even when the different subjects oppose each other. Still it remains a paradox that historians who believe in real, supernatural forces (God) and who practice their beliefs by partaking in various activities (such as praying, attending mass, baptizing) in various arenas (church, charities, religious meetings, and so on) refrain from referring to God's influence in their historiographical works.

To solve the paradox I have taken recourse to Margaret Gilbert's notion of a plural subject, elaborated in Chapter 6. In Gilbert's terminology, a plural subject is a social group in which participant agents are ready to act jointly or have a jointly accepted view (a group belief). Examples range from two dancing partners to huge organizations. People can be part of different plural subjects at the same time, and thus possibly also identify themselves with "we" groups with different aims and with activities informed and circumscribed by different worldviews. This makes it less puzzling why someone can engage in one kind of activity as part of one plural subject—for example, as an active member of the local church, worshipping a God who responds to prayers, guides actions, and perhaps even intervenes personally and, if need be, miraculously in the world if it suits his purpose—concomitant with being part of a scientific community in whose descriptions of the world God is conspicuously absent.

The suggestion that the notion of plural subjects can explain how religious scientists tackle the contradictions between, on the one hand, their religious beliefs in supernatural intervention and, on the other, their inability to refer to supernatural entities in their work *qua* scientists, has similarities with, and may be compatible with, Peter Lipton's views on the tension between religion and science.[41] Lipton, a progressive Jew, recoils from adjusting the content of his religious beliefs to the findings of science by reading the biblical text metaphorically ("the metaphor view"), since large parts of it obviously were intended to be interpreted literally. Nor is he willing to read it as a purely normative text ("the value view"), since that also would mean discarding too much of its obviously factual claims. The same argument militates against the third alternative for adjusting the content of his religious beliefs, what he calls "the selection view," in which those parts of the Bible are thrown out that on a literal interpretation go against the findings of our best science. Lipton thinks this leaves too much of the text by the wayside.

Instead of changing the contents of his religious beliefs to accommodate science, Lipton suggests he might change the *attitudes* he holds as a believer: not believing different things but believing in a different

manner. He discusses two options in this direction. The first is inspired by Thomas Kuhn's analysis of competing scientific paradigms, the incommensurability of which Kuhn in his later work interprets as a kind of untranslatability.[42] Kuhn's idea is that different theories describe the world so differently that beyond a certain point they are no longer comparable because the claims of the one cannot be translated into the language of the other. Although bilingualism is possible, translation is not. On one—Kantian—interpretation of Kuhn, the different concepts the theories use mean they no longer refer to the same phenomenal world, although their noumenal referents are identical. This would seem like a solution that would appeal to believers who think God cannot be described in an idiom commensurable with science:

> God might exist, have created the world in a certain way, and then informed us about that creation. At the same time, it may be that, our intellects being what they are, we are unable to take information about the noumena straight, so God ladens the descriptions with a conceptual structure that makes them comprehensible to us and generates a phenomenal world that is their subject.[43]

Whereas Lipton admits that this is possible, he hesitates to place religious texts so much on a par with scientific discourse as this approach implies. Instead, he describes his preferred solution to the conflicting truth claims of science and religion as "immersion," building on Bas van Fraassen's "constructive empiricist" interpretation of science. Here the idea is that scientists need not believe that the theories with which they work are true. They need only accept them as empirically adequate, meaning that what the theories say about observables—both past and future—is accurate; they need not believe as such their theories' claims about (the existence of) inobservable entities and powers.[44] For Lipton, adapting this approach to religious beliefs means a willingness "to enter imaginatively into" the "world" of the religion without having to believe that all its claims are true.[45] For his part he "cannot believe that the miracles in the Bible occurred," since "the factual claims about some of the miracles contradict what our best science tells us about how the world has behaved. Thus acceptance of religion in this sense and belief or even just acceptance of science would still leave us with contradictory beliefs."[46]

Lipton's immersion solution comes dangerously close to the selection view that he discarded because too many of the factual claims of the Bible fail when confronted with scientific findings. The main difference, and the advantage that makes Lipton prefer the immersion solution, is that by allowing a kind of doublethink, immersion "preserves the integrity and hence the useful power of the religious text . . . we have the text to use in its full, unexpurgated form, the form in which I believe it can do us the most good as a tool for thinking and for living."[47] Allowing practitioners to accept

religious texts while withholding belief in them provides Lipton with a way to live with the tension between science and religion: "Some of the claims of religion may conflict with the claims of science. The immersion solution does not aim to remove that inconsistency, but by distinguishing acceptance from belief it finds a way to achieve consistency of belief without effacing incompatibility of content. On this approach, we preserve content by adjusting our attitude towards it."[48]

VI. Can the Ideal Explanatory Text Accommodate God?

One way of expressing the dilemma of how to reconcile religious and scientific worldviews is to ask whether there is room for the notion of God or other supernatural beings or powers in any scientifically legitimate ideal explanatory text. As I have explained in Chapter 5, the ideal explanatory text is a theoretical construct that includes accurate information about all due-to relations—causal or of other kinds—that converge in the explanandum. My own reply to the question of whether the ideal explanatory text can accommodate God is in the negative, since God, like other supernatural entities, is scientifically inaccessible.[49]

Let us see how the purported miracle on Fox Lakes fares when analyzed against the ideal explanatory text. The presupposition that God did not miraculously provide for the brothers need not entail the exclusion of the divine from the ideal explanatory text for this event. Without working miracles, God might have heard the brothers' prayers and responded by guiding them in his mysterious ways to the right places to look for edible plants, and by seeing to it that the village neighbors arrived in time to save them. Surely something along these lines would be part of the brothers' narrative, even if they were made to doubt the provision miracle. Why shouldn't we, as twenty-first-century historians, think that this element of their explanation is part of the ideal explanatory text for this event? I submit that we dismiss it because the moment God becomes an integral part of the narrative, it falls outside of what science considers accurate information about due-to relations, and therefore outside any ideal explanatory text. In order not to be regarded as irrelevant by the scientific community—including other social scientists—historians must abide by its limitations on what there is room for in the ideal explanatory text. And in this text there is no room for God.

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Notes

1 Shaw, "Religion within History," 3.
2 Shaw, "Religion within History," 7.
3 For such definitions, see Butler, "Theory and God in Gotham," esp. 52; and Stark, "Reconceptualizing Religion, Magic, and Science," esp. 108–111.
4 Quoted in Johannessen, *To vann og ett under*, 16, translation mine.
5 Johannessen, *To vann og ett under*, 30–31, translation mine.
6 Johannessen, *To vann og ett under*, 23.
7 Personal communication from Johannessen.
8 Tucker, *Our Knowledge of the Past*, 185–191.
9 Burns, *Great Debate on Miracles*, 10.
10 Hume, "Of Miracles," quotation from 72; cf. Earman, *Hume's Abject Failure*, 8.
11 Burns, *Great Debate on Miracles*; Earman, *Hume's Abject Failure*; Fogelin, *Defense of Hume on Miracles*.
12 Tucker, "Miracles, Historical Testimonies, and Probabilities."
13 Gregory, "The Other Confessional History," 138n12.
14 Front-page articles in the newspapers *Aftenposten* and *Dagbladet*, July 25, 2007.
15 In mid-September 2007 the course had a weekend gathering during which "we seek contact with angels and learn how we can attempt to create heavenly miracles in our own existence." This quotation is from the course's website, http://www.astarte-education.com/, accessed August 2, 2007 (translation mine). Originally the website had stated with no qualifications that contact with angels would be established and miracles made, but the text was watered down after a high-profile journal editor and finance mogul accused the princess of fraud. "Misleading advertising" that promises gains that cannot be achieved is illegal in Norway. See *Aftenposten*, August 2, 2007.
16 Stark, "Secularization, R.I.P.," esp. 265.
17 Gregory, "The Other Confessional History," 146, 148.
18 Reported in *Aftenposten*, March 1, 2007, 6.
19 Gregory, "The Other Confessional History," 147n36.
20 Gregory, "The Other Confessional History," 146–147, italics in the original.
21 Collingwood, *Idea of History*, 215–216.
22 Bloch, *Royal Touch*, 231.
23 Bloch, *Royal Touch*, 238.
24 Bloch, *Royal Touch*, 243.
25 McCullagh, *Justifying Historical Descriptions*, 19–20; Lipton, *Inference*.
26 Dahl, *Grunntrekk*, 100, cf. 79 and 111, italics in the original, translation mine.
27 McCullagh, *Logic of History*, 52.
28 Johannessen, *To vann og ett under*, 30, translation and italics mine.
29 Gilbert, *On Social Facts*, 163–164, 199–214.
30 Edelman, "The Embodiment of Mind," 30–31.
31 Kim, "Downward Causation," 128–129.
32 For a sophisticated discussion of the methodological naturalism of modern science, see Dawes, "In Defense of Naturalism." I think Dawes's distinction between the agnostic procedural requirements and the—a posteriori and provisional—atheist metaphysical commitments of science makes perfect sense.
33 For an example of theologians' resistance to let go of God in their explanatory vocabulary, as well as of historians' insistence that he should be excluded, see Seip, "Kirkelig historieteori."
34 Gregory, "The Other Confessional History," 137, 138.
35 Gregory, "The Other Confessional History," 136–137, italics in the original.
36 Gregory, "The Other Confessional History," 144.
37 Stark, "Reconceptualizing Religion, Magic, and Science."

38 Gregory, "The Other Confessional History," 136.
39 Gregory, "The Other Confessional History," 142n25.
40 Geertz, "Religion as a Cultural System," 112.
41 Lipton, "Science and Religion."
42 Lipton, "Science and Religion," 37–40. See Kuhn, *Structure of Scientific Revolutions*; Kuhn, *The Road since "Structure*," esp. chapters. 2, 4, and 11.
43 Lipton, "Science and Religion," 41.
44 Van Fraassen, *Scientific Image*, esp. 80–83 and 88.
45 Lipton, "Science and Religion," 41–42.
46 Lipton, "Science and Religion," 43.
47 Lipton, "Science and Religion," 45.
48 Lipton, "Science and Religion," 46.
49 It should be noted that our difficulties in observing God is no reason to exclude him from the ideal explanatory text, since this text abounds with unobservable, theoretical entities from atoms and genes on one extreme of the empiricist–realist continuum to causal powers on the other. Rather, the reason for keeping God out is that no scientifically accepted or even respected theories today refer to God or other supernatural entities in their description of the world. See also Dawes, "In Defense of Naturalism."

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8 Problems of Causation in Historiography

"It cannot be denied that many historians in recent years have not had a clear conscience when they, operating with notions of causality, have been obliged to articulate an actual chain of cause and effect."[1] This is not the heartfelt sigh of a frustrated modern-day empiricist, lamenting how the poststructuralist influence and the literary turn of recent decades have led some historians to view causal explanations as an unsavory practice best conducted on the sly. Rather, this status report was delivered by the leading Norwegian historian Jens Arup Seip in 1957, a time when it seemed as though logical empiricists, fronted by C.G. Hempel, were on the cusp of doing the same to the notion of causality as to the notion of God, that is, banishing it from the realm of reputable scholarship. For causality is not unlike God, in that it cannot be observed other that in its alleged effects. What, then, should its scientific status be? In reply, historians have largely turned a deaf ear to the reservations of theorists who contend that causality may not even exist, and instead continued to search for causes: some somewhat sheepishly, as Seip suggests, others unperturbed or indeed unaware of the entire discussion.

Even though historians could hardly make do without a concept of causality, the discipline's empirical nature has led to a relative lack of interest in how this concept is to be understood. Such issues have been consigned to philosophers, or at least to other historians (few in number) with an interest in philosophical questions. In Norway in the latter half of the twentieth century, this meant primarily—one might even say exclusively—Ottar Dahl, a noted professor of history at the University of Oslo who wrote several key works on theory and methodology.[2] Dahl launched his career with a doctoral dissertation on issues of causality in historical research, which he defended in 1957 at the subsequent disputation.[3] Dahl's dissertation implicitly challenged the skepticism of philosophy that prevailed among his fellow historians, as articulated by Seip as one of the appointed opponents at the disputation: "It is not often that history and philosophy meet—and it is rarer still that the meeting proves auspicious."[4] But even though specialization and the division of labor are tried and true methods for increasing productivity, it is not always a good idea for historians to focus exclusively

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on empirical matters, since important insights made in other disciplines may thereby escape attention. Glancing every once in a while across disciplinary boundaries—including toward philosophy—can therefore be recommended.

In the following I show how historians think about causality and explore what characterizes causal explanations in historiography. I then discuss certain questions related to causality that have interested both historians and not least philosophers of history and that are also relevant to other social sciences. One such question is whether reasons can be causes: this is important because so many of a historian's objects of research are people who act, that is, individuals who perform certain actions on the basis of certain intentions. Another question, approached from the angle of explanation in Chapter 6, is whether we must search for causes at the individual level alone, or whether causes can also be found at more aggregated levels. This is a question of methodological individualism: are there truly social causes? Finally, I discuss a set of questions pertaining to historians' modes of presentation, and more specifically narrativity, a concept that in recent decades has been much debated by philosophers of history. A crucial point here concerns the relationship between narrativity and causality: do all narrative accounts contain more or less hidden causal links that help constitute the narrative, or can a narrative be held together by something other than cause and effect?

I. The Causality of Historians

At the outset historians are fairly open-minded about what can be regarded as a cause. Most historians would probably state, somewhat vaguely, that a cause is something that makes something else happen. Both causes and effects can be of many different kinds, such as an event, an action, an utterance, an idea, or a phenomenon of virtually any type. An *event*, put simply, is something that happens: the birth of Jesus, the death of Stalin. In themselves they are commonplace events—birth and death—but with major causal consequences. *Actions* involve something more than events, namely, the intentional behavior of actors who believe they can achieve a desired objective by acting as they do. Gavrilo Princip's assassination of Franz Ferdinand, the heir presumptive to the Austro–Hungarian throne, in Sarajevo on June 28, 1914, is an example of an action with wide-ranging effects—it was literally the starting shot of the First World War. *Utterances* in the form of speech acts are usually not as dramatic, but can be causally active all the same, whether in the form of saying "I do" at the wedding altar, or in the form of signing the divorce papers when that "do" has become a "don't." The causal force of *ideas*, for their part, can be seen in religion or other ideologies. Finally, *phenomena* that lie outside of human agency can also be causally active: consider drought, for example, or the migration patterns of pelagic fish. Historians include all of the above and more among their causes, even though they probably have a preference for speech, writing,

and (other) actions. As R. G. Collingwood put it, "all history is the history of thought"—though admittedly, this is what Collingwood himself studied.[5]

Historians who theorize about causes often place them along two axes: a horizontal timeline, and a vertical axis that we may call "depth." As in all history, the timeline is crucial. It is used to arrange causes according to *proximity*, that is, how close or how distant in time they are to the phenomenon being described, with the closest causes often called triggering causes. By contrast, determining where something belongs on the vertical axis is no easy matter. A rule of thumb is that the shorter the duration, the higher the placement. Extremely slow changes, which in essence are changes of structure, such as mentality and geography (e.g., continental drift and sea-level changes), are regarded as the most profound.

Historians thus seek both backward and downward for causes. Moreover, they will rarely state that a phenomenon has but a single cause, tending rather to be skeptical of claims of monocausality. Phrases such as "contributory cause" are often used. There are many elements that may have helped bring about an outcome (an effect), and historians would like to capture them all and weigh their causal importance. In this pursuit, however, scholars are constrained by the fact that the past is indeed past, which negates the possibility of controlled experiments and makes statistically satisfactory population studies a rare occurrence.[6] Historians are by and large relegated to carrying out counterfactual thought experiments: a given factor is imagined not to have existed, and the harder it is to envisage the same outcome without this factor, the greater its causal weight. This is primarily a matter of so-called negative counterfactual hypotheses, which make claims about what would *not* have happened if factor X had not been present. Such arguments can be said to be intrinsically part of every claim that one or more causal factors were necessary for a given effect, and they are therefore hard to avoid when writing about history.[7] So-called positive counterfactual hypotheses—concerning what would or could have happened in the continuation of an event, if only something else had happened than what actually did happen—are different and much bolder, and should be treated with caution. Every single link in a concatenation of actions or events usually includes a number of plausible or at least possible continuations, and we need not move many links down an alternative timeline before such counterfactual historiography leads us down a false trail, or indeed entirely astray, and we end up with speculation that is entirely unsubstantiated.[8]

In order to illustrate how tricky it can be to weigh the significance of a causal factor by imagining it out of existence, let us look at the shots in Sarajevo that killed Archduke Franz Ferdinand.[9] How much causal weight shall we accord these shots? Princip's assassination plot was far from the only one carried out against the Habsburgs in the first years of the early twentieth century. Moreover, Princip was part of an organized conspiracy, one that had been set up by the Pan-Serbian secret society known as the Black Hand, whose official name, tellingly enough, was Unification or

Death; had Princip not succeeded, another of the assassins along Franz Ferdinand's scheduled route might well have. (Indeed, one of Princip's co-conspirators had thrown a bomb at Franz Ferdinand's car earlier the same day, but the archduke had swatted it away with his arm—or else it hit the folded cabriolet top behind him—so that it instead exploded right behind his car.) And if the shots in Sarajevo had never been fired, there would presumably have been other occasions where Serbian nationalists could have killed the heir to the throne. The assassination itself is furthermore only one part of the counterfactual analysis that aims to weigh the importance of the shots, as there is no chain of causal necessity from the assassination to the outbreak of war. Austria–Hungary could have issued a less strident ultimatum to Serbia, or reacted less belligerently to the reply they received. Germany could have given less wholehearted support to the hawkish rulers in Vienna. Russia could have gone back on its promises to Serbia. And so on and so forth.

I have yet to see a satisfactory explanation of exactly which criteria should determine how a factor is weighed. Clayton Roberts presents a list of no less than ten things that, according to historians and philosophers of history, may allow a factor to rightly be called the most important cause.[10] Most of these are purely numerical, for example, that a factor that occurs more often in a chain of events, or that is shared by more of the actors in that series, is of greater importance than factors that occur more sporadically. Furthermore, the uncommon is often considered more important in a chain of events than the common, but this seems to be for pragmatic reasons rather than because uncommonness lends causal weight in itself.[11] In addition, what I have called depth often seems to be privileged as a causal factor. This predilection for deeper structural traits is most easily seen among French Annales historians, especially in Fernand Braudel's favoring of *la longue durée*, that is, history that "functions along the border between the moving and the immobile."[12] This penchant for causal profundity typifies the entire field.[13] The Marxist view that the so-called base (i.e., the forces and relations of production) determines the political and ideological superstructure has led to a similar stance as the Annalistes, so that, for example, factors such as imperialism are considered to be more important than the shots in Sarajevo when explaining the causes and effects that led to the First World War. Similarly, others who also emphasize more deeply lying causal factors contend that the war was nigh inevitable given such phenomena as nationalism, imperialism, the alliance system, and the arms race. In this view, Gavrilo Princip—the man with the smoking gun—and his co-conspirators were merely the spark that ignited the inferno, one that would in any case have been ignited one way or the other. This a priori emphasis on more deeply lying causes, however, is hard to defend logically.[14] For what is the argument for regarding phenomena near the surface as less significant, while more deeply lying phenomena are accorded greater weight? If the preference for depth is due to a notion of causal importance, the argument becomes

tautological. Why are more deeply lying factors causally more important than ones closer to the surface?

Another way to distinguish between causes is to define some as *(initial) conditions*, as opposed to a "cause" in the first-person singular. Per definition, such conditions exist prior to the cause and constitute in a sense the very ground upon which such a cause stands. As yet, however, I have only seen the factor of chronology used to differentiate such causal conditions from the cause, or, if you prefer, to differentiate the events or actions allegedly underlying the causal conditions from the event or action designated as the cause. Moreover, the labels we apply to one thing or the other say nothing of their actual causal significance. The "conditions" (or whatever created them) may well be more important than the "cause," which, in turn, becomes neither more nor less important from being labeled as the "triggering cause." Why should the straw that broke the camel's back have a different causal status than all the other, larger bits of straw that weighed the camel down before that final, fateful straw was added?

What remains is that the weight of each causal factor does not depend on the placement of the factors along the horizontal or vertical axis but must be determined from an analysis of what the effect would have been if the factor in question had not existed.[15] Such an analysis comprises two dimensions: an estimate of how much the factor in question contributed to the effect, and a counterfactual assessment of the likelihood that another event or action would have taken its place in the causal chain. The less likely such a replacement is, the more causal weight we must assign to the factor in question. The more realistic it is to imagine this factor being replaced by another, the less decisive it becomes. Along the first dimension, the question becomes how much closer the assassination of Franz Ferdinand brought Austria–Hungary and Serbia and their respective allies to war with one another. Along the second dimension, the question becomes how probable it is that the archduke would have been killed even had Princip not succeeded. If the shots in Sarajevo are to be deemed a preponderant cause of the First World War, the assassination must have edged the parties significantly closer to war, and it must be hard to envision that Franz Ferdinand would have been killed by someone other than Princip—or perhaps, more to the point, by another group than the Black Hand.

II. Causal Explanation in History

There may be purely pragmatic reasons for why we view the final straw that breaks the camel's back as the most significant one. For example, it might be that we are interested in finding out why the camel's back broke at time t_1 while it did not break at time t_0—that is, we may be interested in finding the cause. In that case the answer is the final straw, or, in our example, Gavrilo Princip: even though the load of nationalism, militarism, and so forth had gradually piled up, it was only upon his fatal shots that the

European camel broke its back and the wheels of war were set in motion. Historical writing is indeed full of such *contrastive* explanations.[16] But not all exploratory "why"-questions are contrastive. Sometimes we are interested in understanding why a certain outcome did, in fact, occur, without wanting to know (at least not initially) why it was precisely that outcome that occurred instead of another one. In that case, we are not seeking a contrastive explanation: we only want to know more, perhaps to fill in a particular knowledge gap.

Both C. G. Hempel, famous and indeed perhaps infamous for his deductive–nomological model, which he contended also should be the norm for the discipline of history, and C. Behan McCullagh, relying on his own analysis of causal explanations in historical research, agree that what historians are providing in such instances is a *genetic* explanation.[17] I follow here McCullagh's analysis, since it best fits the historian's practice. Genetic explanations are characterized by the historian following history back in time—how far back is open to debate—and showing how various factors along the timeline changed the probability of the outcome ending up as it did. All these factors have causal force for the outcome: some positive, others negative. Such an explanation of the outbreak of the First World War should factor in nationalism, imperialism, the alliance system, and the arms race, in addition to more temporally contingent events: Austria–Hungary's annexation of Bosnia and Herzegovina in 1908; the assassination of Franz Ferdinand in Sarajevo on June 28, 1914; Germany's assurances to the authorities in Vienna that Germany would stand with them no matter what happened; the Habsburgs' ultimatum to Serbia, which was held accountable for the assassination; Russia's pledge to Serbia that the country would not stand alone should it come to war against Austria–Hungary; and the mechanisms whereby a country that chose not to mobilize risked standing undefended if and when war broke out, even as its adversaries saw any mobilization as a preparation for war and responded in kind.

I believe Hempel and McCullagh are on to something when they highlight the genetic aspect of causal explanations in historical writing. In actual practice, however, it is often difficult to make the accounts of historians fit this pattern. This may partly be because historians write narratives, a point I return to later. But it may also be because the most accurate descriptions of historiographical explanations stem not from Hempel or McCullagh, but from two other philosophers: David Lewis and Peter Railton.[18] Lewis and Railton contend that to provide a causal explanation entails *providing information about the causal history* of the phenomenon being explained. This entails that all information that sheds light on what Railton calls the "ideal explanatory text" is indeed explanatory. As explicated in Chapter 5, such a text does not exist but is an ideal entity that contains all the explanatory information about the phenomenon being explained, both general laws of the type described by Hempel as well as the detailed mechanisms that scholars such as Jon Elster believe are the hallmark of a valid explanation.[19]

What historians do is to write (or, rather, to shed light on) fragments of the ideal explanatory text. Though such fragments are often miniscule, they nonetheless represent explanatory information and are often adequate in the context, in the sense that they present readers with a key piece of the ideal text that that they did not know beforehand. To explain is to provide such information, Lewis and Railton maintain. I believe this is a good description of how historians operate.

I follow McCullagh on another point, one where he differs from Hempel, namely, in his view that causes are *predisposed to* their effects; they do not *determine* them.[20] This way of looking at causes is predicated on a certain idea of causal forces, wherein things have the power to create certain outcomes if they are not offset by other forces.[21] Whether, outside the laboratory setting, something produces the effects it is predisposed to depends on what other causal factors are active: power, for example, tends to corrupt, but scrutiny by the media or the courts can hold it in check. Hence, power does not always corrupt. Or more precisely, power always predisposes to corruption, but this tendency or predisposition does not always manifest itself.

Most historians would not necessarily articulate their view on causality in quite the same way that I have outlined here, where entities (understood broadly) have causal forces that act in certain directions, but where this tendency can be counteracted by other forces. I would nevertheless contend that this description tallies fairly well with how a great many historians relate to causality in actual practice. One advantage of such an understanding of causality is that it provides space for an element that historians, if perhaps not enamored of, are at least interested in and appreciate much more than most social scientists. This element is *contingency*: unforeseen, unplanned (but not stochastic) elements or factors that influence a chain of events and that lead to a different outcome than otherwise, and "normally," would have occurred. The German philosopher of history Jörn Rüsen is among those who have studied contingency as a distinctive trait of historical processes.[22]

History is rife with contingent processes. Because the chauffeurs in Franz Ferdinand's motorcade were unaware of the archduke's spontaneous decision to change the route because of the failed bomb plot, the cars followed the originally scheduled itinerary. When the head of security saw the mistake and ordered the cars to turn around, the motorcade came to a halt—at the very spot where Gavrilo Princip was standing ready with a loaded revolver.[23] For social scientists trying to model reality, contingency is a kind of pollutant: it is white noise that disrupts the pure, unalloyed processes. For historians, contingency is what makes reality impossible to predict, and hence exciting. That a certain historical outcome cannot be predicted does not mean, however, that its causality cannot be explained. Contingency is not a supernatural intervention in processes that otherwise follow the laws of nature. Contingency encompasses unforeseen factors that, because of their causal force, change a course of events that would otherwise have had a certain—or at least a different—outcome. When we know the outcome, we

can rewind the tape and, in a genetic way, provide a causal explanation. No one could say for certain that the Great War would break out in 1914 or how it would break out. In hindsight, however, we can follow the various developments and provide a causal explanation for the outbreak of war, from the Franco–Prussian War in 1870–1871 (or other events even further back in time) to the shots in Sarajevo, the subsequent Habsburg ultimatum to Serbia, and the pan-European mobilization that followed.

I shall use the rest of this chapter to discuss three questions that lack simple, clear-cut answers—at least, I have yet to see such answers. The first question is a kind of classic in the field, namely, whether what we often call reasons—that is, states of mind or mental events—can be regarded as causes on par with physical states and events. This is not a question that exclusively or primarily concerns historians, but it is certainly one that concerns us as well, interested as we are in human agents and their actions and ideas.

The second question—again one that is not specific to history but important for how the discipline is practiced—has also been much debated. It concerns whether causes can be found elsewhere than on an individual level, in other words, whether we can speak of social causes. Since we in a certain sense can, of course, use such a concept, the real question is whether social causes have their own existence, above or at least not reducible to the individuals who constitute the collective in question. This is in other words a question of methodological individualism.

Hempel maintained that all valid explanations are essentially deductions predicated on a general law. Conversely, Railton and Wesley Salmon have both argued that explanations are *accounts* or analyses where ever more explanatory layers are revealed: some of these explanations are nomothetic, and hence formulated on the basis of laws, while others are what Salmon calls "causal/mechanical," where the explanation is "an effort to lay bare the mechanisms that underlie the phenomena we observe and wish to explain."[24] Historians will typically tend to explain things in such a causal/mechanical manner, with the intent of laying bare the causal mechanism that led to the event or phenomenon being explained. In recent decades, philosophers of history, and gradually also practicing historians, have emphasized that historical writing—including scholarly, footnote-heavy historiography—has a *narrative* mode of presentation. This gives rise to a critical question—the third question to be addressed here—concerning the role of causal explanations in such narratives. Is causality a necessary but implicit part of the narration? Or is it possible for historical narratives to include other unifying elements that make causality less necessary?

III. Reasons as Causes

Historians are not solely interested in human actions, but the study of such actions is undoubtedly a major part of the field. Action is often understood as intentional behavior. A common—but not unchallenged—explanatory

model consists in describing the action as the result of a desire, or pro-attitude, combined with a belief. The actor wants to achieve a goal: in our example, ending Austria–Hungary's hegemony over Bosnian Serbs. The assassin, Princip, believes that a certain type of behavior will help him achieve this goal, in that killing the heir to the Habsburg throne will destabilize the regime in Vienna. As a result, he carries out an action with the intent of taking the life of Archduke Franz Ferdinand: the assassination in Sarajevo.

As far as I am aware, it is uncontroversial to read the shot fired by Gavrilo Princip on June 28, 1914, in such a way. What is debatable, however, is whether this means that we have, in fact, explained the actions causally.[25] We are in the mental domain, inside Princip's mind, but what killed Franz Ferdinand was rather the physical bullet and gunpowder, the finger that pulled the trigger and sent the projectile out from the muzzle, and the electrical impulses in Princip's brain that were transmitted through nerve fibers and made the fateful finger bend. Is presenting (an interpretation of) the reasons behind an action synonymous with explaining the action? We have not referred to a general law of any kind. We have not identified any mechanism. We have merely stated the assassin's possible desires and thoughts: in other words, the intention behind the behavior. Can such an intention—such reasons—be said to be causes? We have not referred to any causal forces predisposing to the assassination. Or can we say that intentions or reasons have causal force?

At least three possible answers to this question spring to mind. The first is that the manner in which I have defined causality does not preclude what Donald Davidson calls *mental events*—that is, reasons of the type that have been outlined here—from being causes, from being something that makes something else happen.[26] This does not solve the problem, however, but merely postpones it, to the question of *how* reasons trigger behavior—and that remains one of the unsolved riddles of philosophy.[27]

My second possible answer is to take the easiest way out of the dilemma, by simply stating that regardless of the philosophical problems that arise when viewing reasons as causes, this is how historians do, in fact, explain actions. We simply state what we believe are the plausible reasons behind the behavior, and usually do not worry about explaining the leap—that great, unresolved leap—from the mental events or states of mind (desires and beliefs) to the hand lifting the pistol, the finger pulling the trigger, and so on, that is, to the physical processes that can be traced back along the nervous system to neural activity in the hippocampus, or some other part of the brain, but never, or at least not yet, over into the mental sphere, which is not even a sphere per se but perhaps just a manner of speech we use to describe cerebral activity.

I have thereby suggested a third possible answer to the dilemma, namely, that it might be that we are talking about two fundamentally different modes of describing the processes: one anchored in *science*, and the other

in *folk psychology*, in our everyday way of explaining things. Within the ecumenical view of historical explanations that I, following Railton and Salmon, have argued in favor of in Chapter 5, there is room for both modes of thought. This type of peaceful coexistence between two fundamentally different explanatory models seems almost counterintuitive. But until science colonizes the mental world entirely, we should rather try to live both in and with two parallel and, following Kuhn, incommensurable worlds.[28] As noted earlier, most historians seem to lead this double life without any doubt or regret, similar to how religiously inclined scholars—seemingly without causing any major damage to their psyche—manage to lay their belief in an active God aside when carrying out scholarly research, as discussed in Chapter 7.

IV. Social Causal Forces?

With regard to my second question, concerning whether causes must always be localized on the individual level or whether we can also operate with what we can call social causal forces, I can reveal at the outset that I once again end up by seeking refuge in explanatory ecumenism. Leading Norwegian historians and social scientists from Jens Arup Seip to Jon Elster have insisted on methodological individualism, that is, that a valid explanation of social phenomena must be traceable back to descriptions of individuals.[29] This entails a rejection of social causal forces other than as aggregates of individual causal forces, in which case they are not really social. Nor is this a standpoint that is particularly Norwegian; both Elster himself and his outlook on methodological individualism are fairly predominant in the social sciences internationally.

There are those who disagree, however. Francis Sejersted declares that instead of taking freestanding individuals as the starting point, the analysis should begin with a supra-individual, a priori set of norms that bind or at least strongly constrain the actions of individuals.[30] But even though Sejersted wants to begin the analysis at a different end, he also ends up on the individual level. To be sure, Sejersted's article includes the subtitle "A Showdown with Methodological Individualism"—that is, Seip's methodological individualism—and he describes his own approach as "a sober methodological collectivism."[31] But this is an oversell. Sejersted never promotes methodological collectivism or a notion of real, existent social causes that cannot be traced back to individuals. And when historians do, in fact, use methodological collectivism to explain a given issue, this seems to be more a case of inattentively worded sentences than any deliberate or principled support for the notion of social causes per se.[32]

There are at least two approaches that diverge radically from such methodological individualism. These are what we may call, from the names of their respective founders, Foucauldian discursivism and Durkheimian collectivism. To be sure, it is only the latter that operates with social causes, since the

followers of Foucault often prefer to avoid anything that smacks of causality. There may well be good reasons for this, since Foucault's understanding of the relationship between words and things makes it quite a challenge for him to explain change.[33] This means that Foucault, in a certain sense, becomes less interesting in our context, though no less of a trendsetter among historians of a poststructuralist bent, who seem largely uninterested in cause and effect. Their primary interest is rather how discourse constitutes (the understanding of) practice. Since discursive analysis replaces agents with an impersonal or supra-individual discourse, causality as well disappears. From a methodological viewpoint, making the agents disappear in this vanishing act is not unproblematic, since discourse is in one sense the sum of individual acts of speaking and writing, and must thus be said to supervene on them. The followers of Foucault, however, seem to not care or worry about this point. We are thus at an impasse, and can only conclude that the Foucauldian approach represents a potential challenge to methodological individualism's requirement that valid causal explanations must ultimately be traceable back to acting, thinking individuals.

The followers of Durkheim are a different matter. Durkheim took causality seriously and believed that every social phenomenon has a social cause—precisely in the sense that the cause *cannot* be rediscovered in the individuals who constitute the collective. In other words, the collective contains causal forces beyond the sum of the actors it comprises: these are forces that surface when, and because, individuals congregate and thus bring the collective into existence.[34] We are here talking about genuinely emergent forces. Whether in the family, in the gang, in the crowd, the trade union, the class, the nation, and so forth, the followers of Durkheim contend that these collective entities have traits that erode and crumble the moment that Elster or other methodological individualists come and split them up to analyze them in their constituent parts. The group is more than the sum of the parts.

This notion of emergent social traits—that is, causal forces in our context—is a fascinating one. As seen in Chapter 6, Durkheim culled it from the natural sciences of his day, where ideas about emergence were prevalent in the late nineteenth century. In the twentieth century, however, nuclear physics expelled the notion of emergent traits from the natural sciences, though the impetus was not strong enough to expel the analogous way of thinking from sociology. But on a purely logical basis, the notion of collective traits (causal forces) that cannot be found in the individuals who constitute the collective is problematical. The relationship of supervenience is asymmetrical. A change in the supervening level—that is, the collective—presupposes a corresponding change in the supervenience base. A forest cannot change without the trees that constitute it, and that it supervenes on, also change. It therefore seems wholly unfeasible to maintain that the collective (the forest) can cause a change—that is, have causal force—without conceding that it is the individuals constituting the collective (the trees) who cause this change and that it is thus these individuals who are causally active. As a result, Durkheimian socialism seems mystical.

But even if we reject social emergence, we do not need to abandon the idea of social causes. If we take supervenient relationships seriously—which we must—it is strictly speaking only at a subatomic level that we find true causal forces. This means we also cannot speak of causality at the individual level, except when we talk about reasons—in which case, as noted above, we use idioms that are not taken from the natural sciences. From a purely terminological standpoint, it may therefore be argued that it is unreasonable to demand that causality can only be ascribed to the most basic level and the most minute entities. In that case, sciences such as biology and geology would not be able to talk about causality in their objects of study either.

In one particular area, however, the social sciences and the humanities are in a unique position that makes it possible to argue consistently that causality should be applied to individuals only. It is common to assert that only individuals can be said to have intentions and reasons. However, several philosophers have argued in favor of what Margaret Gilbert calls a "plural subject": a group of individuals who have a joint understanding that they constitute a community and who think and act *qua* this community—and who therefore think and act differently than they otherwise would have done.[35] This intuition—that we think and act differently as part of a community than as individuals—can thus be combined with the fact that thinking is something that transpires inside individual minds. But since we think as part of a community, it is in one sense the community that thinks and acts.

The advantage of operating with concepts that allow for this intuition—that is, the knowledge that being part of a community does something to our preferences—is that we can thereby explain thoughts and actions that otherwise would seem inexplicable or at least difficult to comprehend. Gavrilo Princip and his co-conspirators did not act *qua* individuals, but were rather jointly committed to carry out their actions together precisely as part of a conspiracy. And they had joined this conspiracy because they were ardent (pan-)Serbian nationalists who believed that Austria–Hungary, through its annexation of Bosnia and Hercegovina in 1908, hindered the unification and development of the Serbian nation, since the annexation led to Bosnian Serbs being ruled by the Habsburgs. It is, for example, but by no means exclusively, in such a sense that we can claim nationalism was a cause of the First World War: the ideology made people identify themselves as belonging to an ethnic or national community with a goal of national sovereignty. And this ideology, along with other ideas that were prevalent at the time, made young idealists such as Princip and his co-conspirators commit acts of violence it would be hard to imagine they would have committed entirely on their own, that is, had they not felt they belonged to this community.

Most historians would probably have few qualms about identifying nationalism as a (contributory) cause to the First World War. At the same time, hardly any would contend that ideology as such can be an agent. Like other isms and beliefs, nationalism only manifests itself through

people—either as individual human beings or as groups, which, in turn, consist of and supervene on individuals. But even though actors who take action are in one sense always individuals, most actions are perhaps carried out as part of a plural subject. Gavrilo Princip and the other assassins believed they were acting jointly as part of the Serbian nation. As such, they did not act qua individuals: it was the conspiracy that acted. The causal force behind the assassination of Franz Ferdinand was of an entirely different magnitude than a young man with a pistol: it was a young man whose ideas of the Serbians' demands for national unification and autonomy had impelled him and his companions to seek out the nationalist movement the Black Hand, which gave them arms, training, and a conviction that they were acting jointly on behalf of an entire nation. In the bargain they also received potassium cyanide to commit suicide in case they failed—which most of them did before also failing in their attempts to take their own life.

V. Narrative Causality

The final question I intend to discuss in regard to causality is unique to the discipline of history—or more precisely, to history and related disciplines. In recent years it has even become common to assert that this aspect is *constitutive* for historical writing. I refer here to the concept of narrativity, that is, that which turns an exposition into a story.

Narrativity became part of the philosophy of history in the 1960s through the work of W. B. Gallie, Morton White, and Arthur Danto, and in the succeeding decades it became popularized (to the extent that one can speak of the philosophy of history as being popular) and radicalized through scholars such as Hayden White, Louis Mink, and Frank Ankersmit.[36] Hardly any other concept has been as frequently cited and more or less embraced in recent decades, by both philosophers of history and theoretically inclined historians. One reason is probably that it has provided long-awaited academic respectability to a mode of exposition that historians have felt a bit ashamed about when confronted by self-assured social scientists and their more analytical texts. Especially in the sixties and seventies, "narrative history" often had a slightly disparaging tenor, as compared with the more sought-after "analytical history."[37] But now the tides have turned. Emboldened by the literary turn, one can today be a narrative historian and proud of it. Indeed, some even claim that it is hardly possible for a historian to be anything else. The idea is that what makes an exposition narrative, is also what turns an exposition into historiography. We speak in this regard of emplotment, that is, of connecting the various elements into a meaningful whole with a beginning, middle, and end, all placed along a timeline.

As recently shown by Jouni-Matti Kuukkanen, the claim that historical writing can hardly be anything other than narrative is certainly open to questions.[38] I shall not pose such questions here, however, but instead ask what happens to causes in such narrative historiography.

There is a fairly simple—and, I believe, basically correct—answer to this question, namely, that causality is the very element that emplots the exposition, connecting the temporal elements together and turning the exposition into something more than an out-and-out chronicle of the type "and then this happened, and then that happened, and then this happened." According to Margaret Somers and Gloria Gibson, "narratives are constellations of relationships . . . embedded in time and space, constituted by causal emplotment."[39] The classical example here is a sentence such as "First the king died, and then the queen died *of sorrow*," with the causal emplotment inherent in the final two words. Narrative historians excel in such implicit expressions of causality, which undoubtedly make the exposition more polished than overuse of the phrase "the cause of" would have, as in "First the king died, and then the queen died; the cause of her death was sorrow over the passing of the king." But incorporating causality in this manner also serves to conceal it. It becomes harder to discern, harder to pin down—Did the queen stop eating? Did she lose her will to live and become more susceptible to disease? What actually took place?—and much harder to assess. Nevertheless, it is causal hypotheses of this obscure nature that philosophers of history believe turn historians' accounts of the past into history as we know it. At the same time, this makes the discipline of history into something fairly different from (other) social sciences, where accurately formulated hypotheses of cause and effect are a key part of the methodology and a prerequisite for testing whether the formulated connections (the causal hypotheses) are substantiated by the data. We can nevertheless say that if causality is what emplots a historiographical narrative, then a causal explanation is inherent in this narrative. This seems to be the understanding of narrative history entertained by its foremost German champion Rüsen.[40] Its implicit nature, however, entails that both the question that the narrative is meant to answer—what are we trying to find the cause of?—and the causes themselves can be difficult to grasp. If precision is a goal in academia—and I believe it should be—the narrative form of exposition makes the discipline of history less scholarly, less scientific. At the same time, there is reason to believe that precisely the narrative form is an element that gives history its mass appeal, in contrast to other social sciences.

There is an alternative answer to the question what becomes of causes in narrative historiography, namely, that it is not, or in any case that it is not exclusively or necessarily, causality that is the element that connects the various elements in a narrative and emplots it. In a narrative, events do not merely come "*after* one another, but *because of* one another," writes Narve Fulsås, adding that "a plot links the events together by establishing relationships of causality, meaning, or significance between them."[41] Likewise, Peter Goldie, like Fulsås concurring with Paul Ricoeur that emplotment "extracts a configuration from a succession" and thus lends a narrative its defining coherence, claims that "it is certainly true that narratives need not involve causal relations between the events that are related in the narrative."[42]

Apparently other elements may render coherence by emplotting the narrative, although neither Fulsås nor Goldie is clear in explicating how such noncausal emplotment works. Probably they think of stories that are bound together by a sort of literary ploys that make them appear meaningful, in the way narratives do when they show the religious or predestined significance or necessity in things unfolding as they do, thus revealing the deeper meaning behind the events. The Christian narrative of the redemptive crucifixion of Jesus is perhaps one example, with fairy tales being another. David Velleman has suggested that narratives essentially complete "emotional cadence" in readers or the audience.[43] The reason is that the stories resonate with deeply held, visceral, or "biologically programmed" (if also liable to cultural modification) patterns of "arousal and resolution of affect."[44] His illustration, which I believe catches the noncausal emplotment that Fulsås and Goldie also have in mind, is a classical example of a story in which the significance lies in noncausal elements of the narrative, namely Aristotle's tale of how Mitys's murderer was killed by the statue of Mitys "falling down on him when a looker-on at a public spectacle," that is, an event wholly unconnected to the murder.[45] Velleman argues that the plot-producing significance that inheres in narratives by means of emotional cadence provides a "subjective understanding of how to feel about them" that should not be mistaken for "an objective understanding of how historical events came about."[46] Likewise, I find it difficult to see how giving meaning to—that is, emplotting—texts by spinning webs of significance that complete emotional cadence is applicable in academic historiography. It must in any event be recognized that this form of emplotment, which emphasizes the noncausal meaning of events and actions, makes it less relevant to establish a chain of cause and effect.

Closely related to Velleman's analysis of narrative as providing emotional understanding is an interpretation of noncausal emplotment that can be linked to David Carr, who in his analysis of historiographical narrativity underlines that narratives are explanatory in themselves, and not because they contain causal elements.[47] When we are told a story, Carr says, we understand how things are connected. Carr maintains that searching for an intrinsic causality in the narrative misses the point, because narratives represent an alternative mode of explanation and are not implicitly causal. Personally I find it difficult to follow Carr here; rather, I contend that the reason we perceive narratives as being explanatory is precisely their implicit causality. Velleman's analysis is to the point when he criticizes the notion that the noncausal, significance-producing aspect of a narrative can provide what he terms *objective understanding* (which is akin to what I would call explanation): "The story makes sense to us in emotional terms. We don't know why it happened, but we know how it feels. Not knowing why it happened, we ought to question whether it really did happen that way."[48]

Yet another noncausal interpretation of narrativity is the one described by Hayden White, namely, that emplotment occurs linguistically by way of

images that imbue the history with the traits of romance, tragedy, comedy, or satire, depending on whether the historian's primary trope is metaphor, metonym, synecdoche, or irony, respectively.[49] This also shunts the question of causal explanation to the side, because it is the tropes, and not causality, that gives the narrative meaning. White's analysis is, however, largely unsatisfactory for anyone who is trying to find out why things unfolded as they did, that is, for anyone who is inquiring into the causes of the events. That an exposition acquires a romantic, comic, tragic, or ironic nature, and thereby a certain type of meaning, does not make the question of causality superfluous or misplaced. For someone whose errand is to find out why things happened as they happened, White's analysis of the emplotting function of literary tools is by and large irrelevant. We do not understand more of the world through this type of textual analysis.

After this review of three noncausal ways of understanding narrativity, we are back where we started: with causality serving as that glue that binds the various parts of a historiographical narrative together into a coherent whole. A relevant question, then, concerns what it is that distinguishes such narratives from the sort of genetic causal explanations that McCullagh and Hempel (who admittedly strove to avoid the concept of causality) regard as characteristic of the discipline of history. I believe the answer is that genetic causal explanations, wherein a chain of cause and effect is shown to have led to the outcome in question, have the uncovering of precisely such causal connections as their objective. Genetic explanations are thus both more explicit in their discussion of causality and more exacting in their demands to what should be included in the account, namely, that which indicates the causal connections. In historical narratives, by contrast, it is the narrative itself that is the objective, while causal connections feature less prominently in the exposition and are not discussed as explicitly as in genetic explanations. To be sure, causality does help constitute the narrative, but even though the narrative builds on causal connections, it does not focus on such connections in the way that genetic causal explanations do. Causal connections are necessary for the narrative, but they are not its objective.

VI. Conclusion: Living with Unresolved Questions

Scholarly attention to issues of causality in historical research was at its apex around the 1960s, when the influence from the social sciences was most pervasive. Subsequently, the linguistic turn and the revival of narrative have made historians and not least philosophers of history less interested in causality. Nevertheless, these trends as well have their own inherent struggles with causality. Narrativity has an unresolved relationship both to causality—is it causal connections that bind the elements together in a narrative?—and to causal explanations, which are incorporated or hidden (or forgotten) in the narrative. Foucault-inspired discursive analysis struggles even more with causality, partly because of an unresolved relationship

between language and the world outside of language and partly because the concept of discourse itself is destabilized when one seeks to find what—or rather who—might have caused changes in the discourse, since discourse supervenes on individual acts of speaking and writing. Also historians who are less influenced by recent trends have their issues with causality. This concerns the question of whether social causes can be given a logical foundation. It concerns the question of whether reasons—mental events—can be included in a causal explanation, whether nomothetic or mechanical. And it concerns the questions of which criteria are to assign causal weight and how claims of such weight can be corroborated without positive counterfactual speculation. These are problems no one should take lightly. In their tried and true manner, however, historians will probably continue to provide causal explanations for the phenomena they describe, in the form of narratives that verge on popular literature, and leave it to the philosophers to wrestle with the issues of causality.

Acknowledgments

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Notes

1 Seip, "Årsaksproblemer," 175, translation mine.
2 E.g. Dahl, *Grunntrekk*; Dahl, *Problemer i historiens teori*; cf. Kjeldstadli, "Minnetale over Ottar Dahl."
3 Dahl, *Om årsaksproblemer i historisk forskning*.
4 Seip, "Årsaksproblemer," 175, translation mine.
5 Collingwood, *Idea of History*, 317.
6 For the wide-ranging epistemological effects of the fact that historians are never able to observe the past, only its artifacts, see Goldstein, *Historical Knowing*.
7 Nagel, *Structure of Science*, 588–589; Christensen, "Approaching Counterfactuals in History," 61–62.
8 Sørensen, *Historien om det som ikke skjedde*; Christensen, "Approaching Counterfactuals in History."
9 Clark, *Sleepwalkers*, 367–376.
10 Roberts, *Logic of Historical Explanation*, 252.
11 White, *Foundations of Historical Knowledge*, 115–133; Roberts, *Logic of Historical Explanation*, 96–99.
12 Braudel, *On History*, 74; cf. Burke, *French Historical Revolution*; Iggers, *Historiography in the Twentieth Century*, chap. 5.
13 Roberts, *Logic of Historical Explanation*, 99.
14 White, *Foundations of Historical Knowledge*, 134–141; Roberts, *Logic of Historical Explanation*, 99; Northcott, "Weighted Explanations in History."
15 Martin, "Causes, Conditions, and Causal Importance," 69–71; Northcott, "Weighted Explanations in History."
16 Van Fraassen, *Scientific Image*.
17 Hempel, "Aspects of Scientific Explanation," 447–453; McCullagh, *Truth of History*, 172–194.

18 Lewis, "Causal Explanation"; Railton, "Probability, Explanation, and Information."
19 Elster, *Explaining Technical Change*, 23–24.
20 McCullagh, *Truth of History*, 178–187.
21 Harré and Madden, *Causal Powers*; Mumford and Anjum, *Getting Causes from Powers*.
22 Rüsen, *Rekonstruktion der Vergangenheit*, 22–47.
23 Clark, *Sleepwalkers*, 374.
24 Salmon, "Scientific Explanation," 71.
25 Roth, "Three Dogmas."
26 Davidson, "Mental Events."
27 Galaaen, "Disturbing Matter of Downwards Causation"; Strand, "Metaphysics of Mental Causation."
28 Kuhn, *Structure of Scientific Revolutions*.
29 Seip, "Årsaksproblemer," 178–183; Elster, *Nuts and Bolts*, 13.
30 Sejersted, "Norsk historisk forskning."
31 Sejersted, "Norsk historisk forskning," 322, translation mine.
32 For examples, see Qviller, "Keiserinnens gamle klær."
33 Fitzhugh and Leckie, "Agency, Postmodernism and the Causes of Change."
34 Durkheim, *Rules of Sociological Method*, 39–40 and 128–129; Greenwood, "Social Facts."
35 Gilbert, *On Social Facts*. See also Carr, *Time, Narrative, and History*, 130–135 and 147–163; Searle, *Construction of Social Reality*, 25–26; Tuomela, *Importance of Us*; Schmid, "Plural Action."
36 Gallie, *Philosophy and the Historical Understanding*; White, *Foundations of Historical Knowledge*; Danto, *Narration and Knowledge*; White, *Metahistory*; White, *Tropics of Discourse*; White, *Content of the Form*; Mink, *Historical Understanding*; Ankersmit, *Narrative Logic*.
37 Stone, "Revival of Narrative."
38 Kuukkanen, *Postnarrativist Philosophy of Historiography*.
39 Quoted in Kane, "Reconstructing Culture in Historical Explanation," 315.
40 Rüsen, *Rekonstruktion der Vergangenheit*.
41 Fulsås, "Forteljing og historie," 50, translation mine, italics in the original.
42 Goldie, *Mess Inside*, quotations from 14 and 16.
43 Velleman, "Narrative Explanation," 6.
44 Velleman, "Narrative Explanation," 12–13.
45 Velleman, "Narrative Explanation," 5.
46 Velleman, "Narrative Explanation," 20.
47 Carr, "Narrative Explanation and Its Malcontents."
48 Velleman, "Narrative Explanation," 21.
49 White, *Metahistory*, 31–38.

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