DOWN TO EARTH ARCHAEOLOGY

WILLIAM Y. ADAMS

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WILLIAM Y. ADAMS

CONTENTS

DATING

CERAMICS

INTERPRETATION

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LIST OF PLATES

LIST OF FIGURES

pottery (Family D), Nubian wheel-made pottery (Family N), Aswan pottery (Family A), and certain other Egyptian wares, between AD 200 and 1600.

Figure 10.5. Cross-site comparison of the chronology of the ceramic data clusters.

Chapter 11

Figure 11.1. Systems of time reckoning used by archaeologists and historians. The different systems are not cross-correlated on the chart; that is, entries in any given column do not correspond in time to entries in other columns. Earliest dates are at the bottom of each column.

Figure 11.2. The 'index fossil' method of periodization, in which culture periods (right column) are defined a priori by the presence or absence of particular pottery types (left column).

Figure 11.3. The 'cladistic' method of periodization, in which pottery periods (far right column) are defined by the presence or absence of groups of co-occurring pottery types. The types shown (R20, W18, U10, etc.) are medieval Nubian wares. Absolute dates are shown at the far left (from Adams 1986a, 477).

Figure 11.4. Bar graph showing the variable distribution in time of different medieval Nubian pottery wares. Time scale is shown at the top of the chart, ceramic periods at the bottom of the chart. (The figure actually shows a form used for recording sherd tallies from medieval Nubian archaeological sites; see Adams 1986a, 623-626).

Figure 11.5. Chronological diagram showing variable rates and times of change in the three main medieval Nubian pottery families (three right-hand columns), in relation to culture periods. Time scale is at the far left (from Adams 1986a, 408).

Figure 11.6. 'Battleship graph' showing relative frequencies of different ware groups (N. I, N. II, etc.) in the principal medieval Nubian pottery family (Family N), from AD 200 to 1600. Time scale is at the far left. 'Index clusters' (second column from left) are equivalent to ceramic periods (from Adams 1986a, 608).

Figure 11.7. An example of the form used for recording potsherd tallies from medieval Nubian excavation units. Raw tallies are entered in the blanks to the right of the ware numbers (H1, H9, H11, etc.); equivalent percentage figures are entered to the left of the ware numbers. This figure shows the obverse of the form, used for recording indigenous Nubian wares. The reverse side is shown in Figure 11.8.

Figure 11.8. The reverse of the form shown in Figure 11.7, used for recording tallies of imported pottery wares found in Nubian sites. See Figure 11.7 caption.

Figure 11.9. Historically recorded dates that have helped to date the medieval Nubian pottery wares, and the wares or groups that they have helped to date.

Figure 11.10. Chronological chart showing that the developmental periods in the three main medieval Nubian pottery families (three right-hand columns) do not closely reflect concurrent changes in political organization or ideology. Time scale is at the far left (from Adams 1979, 728; see also chapter 13, this volume [ed.]).

Figure 11.11. Hypothetical 'battleship graph' of an individual pottery type. The sharp lower end represents the time when the type was first coming into use, but had not yet become abundant. The narrowing section near the top, labelled 'persistence of vessels', represents the time when the type was no longer being made, but many individual vessels were still in use. The very attenuated section at the top, labelled 'persistence of sherds', represents the longer period when types were neither made nor used but when their sherds still found their way into occupation deposits in predictable quantities.

Figure 11.12. Percentage distribution of medieval Nubian pottery wares and ware groups in successive ceramic periods (shown in the scale along the top). Each figure represents the prevalence of a particular ware of group measured as a percentage of the total sherd population at that period. A few exotic wares are omitted.

Figure 11.13. Chronology of ware groups in the three main medieval Nubian pottery families (three left-hand columns) in relation to stratigraphic levels in various sites. Pottery factory sites (identified by numbers and names in the second row from the top) are 24-R-23, 24-V-13, 24-N-3, and 24-E-21 (from Adams 1986a, 602).

Figure 11.14. Chronology of pottery wares found in the Faras pottery factory, in relation to successive ceramic periods. Periods 4-7 correspond to the actual period of production at the Faras factory (from Adams 1962b, 283).

Figure 11.15. Dates calculated for medieval Nubian pottery wares and ware groups. Dates shown under 'persistence of vessels' are the last dates at which some vessels of the ware were still in use, after production had ceased. Dates shown under 'persistence of sherds' are the last dates at which sherds of the ware continued to find their way into refuse deposits (from Adams 1986a, 614).

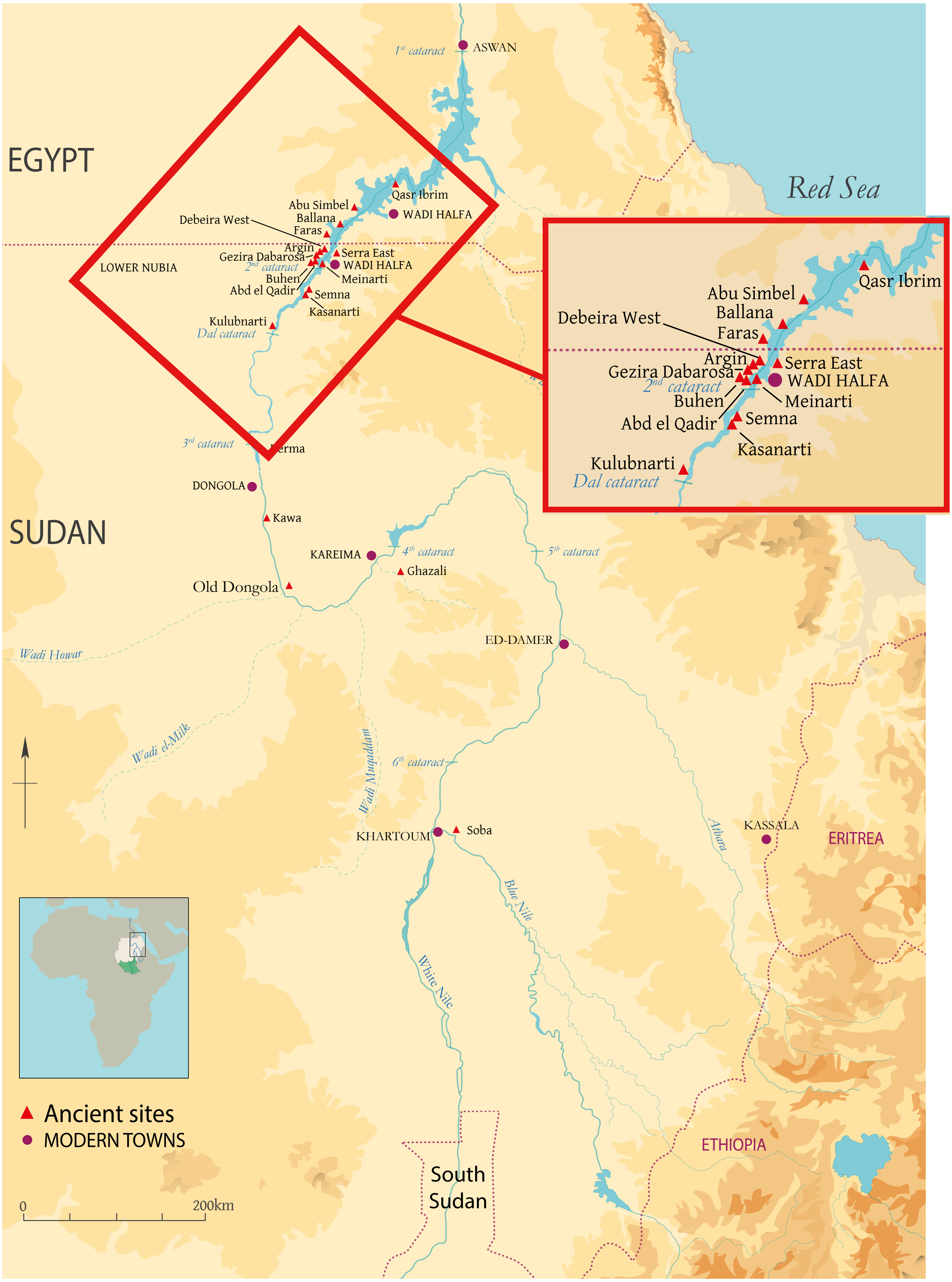
Chapter 13

Figure 13.1. Political and ideological changes in ancient and medieval Nubia and the principal pottery groups in use. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.

Figure 13.2. Empirical evidence of change in Nubian wheel-made wares (Family N) between AD 200 and 1550. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.

Figure 13.3. Typical vessel forms and painted designs at each developmental stage in the Nubian wheel-made wares.

Figure 13.4. Empirical evidence of change in Nubian hand-made wares (Family D) and imported pottery made at Aswan (Family A) between AD 200 and 1550. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.



Map with places in Sudan mentioned in the volume.

Editor's Preface

Over the course of 2018 and 2019, Professor William Yewdale Adams (known as Bill to his friends and colleagues) compiled a select collection of his archaeological papers and added an introductory commentary to each one. These articles had been written at various times during Bill's lengthy and productive academic career for different purposes and for different audiences. Most of those selected had been previously published only in a limited way, either as conference proceedings or contributions to various *Festschriften*, and as such he wanted to enable them to reach a wider readership than they had originally. He described this collection as his '*dernières pensées*'.

The essays encompass a wide range of topics, from reflections upon the successes, failures and lessons learned from the UNESCO International Campaign to Save the Monuments of Nubia in the 1960s, in which Bill was very much a leading figure and which he was uniquely positioned to critique, to discussions and criticisms of the theoretical framework of 'New' or 'Processual Archaeology' and its application of 'scientific' methods. Bill published 26 books through his career. In 1977 an impressive synthesis of the history and archaeology of Nubia from the Palaeolithic through to the 1960s entitled *Nubia: Corridor to Africa* appeared. This was later translated into Arabic, and in 2005 the Order of the Two Niles was conferred upon him by the Sudanese government in honour of his contribution to knowledge of Sudan and Nubia. Other papers included here are seminal works discussing the ideological concepts of typology and classification and their practical application to archaeological excavations, notably his own major excavations conducted at the large Nubian cityscapes of Meinarti, Kulubnarti and Qasr Ibrim, and the ceramic kilns at Faras.

In April 2019, Bill approached the Sudan Archaeological Research Society, of which he was Honorary President, to see if they might be interested in publishing this compendium of essays. They agreed and this project was undertaken. Sadly, he passed away in August 2019 and was unable to see this book come to fruition.

It was a pleasure to re-read many of Bill's articles again and to be introduced to others for the first time. The first publication and original pagination of each article is provided within the relevant chapter. The articles themselves have been reformatted and this volume is paginated sequentially. The language of the articles is as it appeared originally in print. Bill had intended to modify and update these papers slightly to make them clearer for a present-day audience, but much of this did not come to pass. I have added footnoted comments and additional references for the benefit of readers less familiar with Bill's work and with the personalities present at the time. The photographs and figures included are from the original articles unless otherwise credited. I am grateful to Loretta Kilroe for her assistance in bringing this volume to fruition, and to both her and Julian E. Reade for their comments and suggestions. Any errors or omissions remain my own.

I am deeply honoured to have known Bill, and for him and his wife Nettie, with whom he worked and collaborated throughout his life, to have shared their passion for Nubia with me.

Julie R. Anderson
December 2021

GENESIS OF A MAVERICK

Plate i. William Y. Adams and Nettie K. Adams in Khartoum in 2005 after he received the Order of the Two Niles, Sudan's highest civilian decoration (photo courtesy D. A. Welsby).

I was born in California, in 1927. However, I spent some of my formative early years in the Indian country of Arizona and New Mexico, and that experience determined me early on to be an anthropologist. Like many anthropology students in those years, I was drawn initially to the sub-discipline of archaeology. The glamour of digging interesting and sometimes valuable things out of the ground was well-nigh irresistible for the youthful imagination. In addition, in the Southwest there were those ruined cliff-dwellings and pueblos all around—most of them still unexcavated.

That interest carried over into my earlier college years at Berkeley (1946-52). During that time, I participated in three student digs in northern California, all directed by Robert F. Heizer. However, they were all burial sites (there seemed to be little else in California archaeology), and I learned very little of consequence except how to uncover bones. Almost no formal instruction was involved.

In 1949, my family moved back to the Navajo Indian Reservation, were I had lived in my early youth, and I found my interest drawn increasingly to the Indians, their culture and their language. By 1952, I had pretty definitely decided to study living rather than dead peoples, and that interest was cemented by three years of operating a trading post in the remote Navajo community of Shonto (1954-56). This was in time to provide the basis for my doctoral dissertation at the University of Arizona (Adams 1963).

As I was completing my doctorate, I looked forward to securing a university teaching position, and to continuing ethnographic fieldwork in any part of the world where there were interesting peoples to be studied. But jobs for anthropologists were few and far between, at a time when the majority of universities did not yet offer anthropology, and only about a dozen had PhD programs. There was no such thing as an organized job market, and also no such thing as a C.V. You learned about job openings through the grapevine—usually from your professors—and it was then up to you to sit down at your typewriter and compose a letter of self-advertisement. I was never good at it, and it never landed me a job. I either got no reply at all, or a 'glad to keep your application on file' letter.

Meanwhile, the Bureau of Reclamation had decided to build the Glen Canyon Dam, which flooded substantial parts of both the Colorado and the San Juan River canyons. Contracts for preliminary archaeological salvage were divided between the University of Utah, for areas north of the San Juan, and the Museum of Northern Arizona for the San Juan canyon and the area south of it. A decision was made, apparently in Washington, that I should be hired to direct the Museum of Northern Arizona work—not because of any archaeological expertise but because I knew the remote area in question (which was entirely within the Navajo Reservation) and could speak Navajo.

When the position was offered to me I turned it down at least once, and I think possibly twice. But the summer sped by and nothing else turned up, and it became a matter of accepting anything that would put food on the table. Consequently, the time came when I had to phone the Museum of Northern Arizona (MNA) to say that I would take the job if still available.

I spent the next two years on the Glen Canyon job—the first simply in locating sites (my wife and I found 88 of them) and the second in excavating a few of them (see W. and N. Adams 1959).[1] None of the sites were of any great consequence; they were obviously summer farming sites occupied seasonally by peoples who had lived most of the year in much more substantial pueblos on the plateaus above. None of their walls, of very rough stone, stood more than 60cm high, and two or three days were generally sufficient to clear out all of the fill in them. Artifact finds were mostly chipped stone tools, metates and manos.[2] Excavating and recording these sites involved no more than 'doing the obvious', and my lack of archaeological training was no great impediment.

In 1959, through a complicated and unlikely set of circumstances, I was offered a four-month contract to assist the government of the Republic of Sudan (as it was then called) in preparing a program of archaeological salvage for the area of the Nile Valley that would soon be flooded by the Aswan High Dam. To make a very long story very short, I went for four months and stayed for seven years, during which time I developed a complete salvage program almost from scratch.

So far as Nile Valley archaeology was concerned, my mind was an absolute *tabula rasa*. Here were remains going back 5000 years to the dawn of civilization, and far beyond that into the stone ages; all kinds of sites, residential, administrative, religious, industrial, military, and mortuary; and of every size from minuscule to monumental. As my job developed over time, it had four basic components. The first was to conduct a preliminary survey of the whole area to be flooded; the second was to try and attract foreign expeditions to come and dig the most important of the sites that we found; the third was to dig as many of the sites not claimed by foreigners as we could, with the time and resources at our disposal; the fourth was to create a central archive containing information about all the sites excavated by all of the excavations.

None of my previous experiences had any relevance to this, nor by and large had the work of others in the Sudan. If my mind was a *tabula rasa*, so also was Sudan archaeology, with a very few exceptions. The fairly extensive archaeology undertaken earlier, mostly between 1907 and 1931, was entirely in sites of three types: monumental fortress-towns from the age of Pharaonic Egyptian occupation (1800-1000 BC),

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[1] See Plate i [ed.].
[2] *Metate*, a stone quern upon which grains are ground, usually with a concave surface or depression. It is used together with a *manos*, a handheld stone grinding tool [ed.].

monumental temples and tombs from the Empire of Kush (800 BC–AD 300), and cemeteries of all ages up to AD 1500. Apart from cemeteries, no attention had been paid to non-monumental sites.[3] In my time, I was to excavate almost nothing else, for these were the sites uninteresting to foreign expeditions.

During the period between 1960 and 1966, I personally excavated about 50 sites, and oversaw the excavation of about the same number by assistants. These included habitation sites, churches, temples, watchtowers, fortifications, pottery workshops, a quarry, rock pictures, and cemeteries. They dated to all periods from about 2000 BC to AD 1800, and varied in size from minute to monumental. The most outstanding of my excavations were of a stratified mound containing 18 layers of village remains (Meinarti), which I dug for a solid year with a huge crew, and an administrative, religious, and commercial center containing remains, some of them monumental, dating from at least 750 BC to AD 1812 (Qasr Ibrim).[4]

The challenges

None of my previous experience prepared me for these digs. To begin with I had to learn Arabic—fast, for none of my foremen and only a handful of my laborers spoke English, and none could read or write it. Then, I had to learn to work with masses of unskilled laborers (up to 250 at Meinarti), whereas in America, a few assistants and I had always 'thrown our own dirt'; we never had hired laborers. In addition to the local laborers, I was also provided with a small cadre of supposedly skilled Egyptian laborers (*Quftis*),[5] and I had to find out what they could and could not be used for, and deploy them accordingly. At the head of the *qufti* group was an Egyptian foreman (*reis*), whose main job was just to keep the men working. Over the years I had about six different foremen (they were hired for me by the Sudan Antiquities Service), and I found that they varied considerably in their abilities not only as disciplinarians, but even more in their knowledge of archaeology. A few understood what I would call archaeological tactics, but only one very unusual individual understood strategy (the big picture); at least a couple understood neither.

A job on nearly every dig, that had to be thought through, and periodically rethought, was that of deployment. With such large crews, it was necessary to select digging areas, dumping areas, and routes between them so that the men didn't get in each other's way, and so they weren't tramping back and forth over excavated, but as yet unrecorded, remains. Such challenges were common to all Sudanese and Nubian digs; in contrast to anything I had known in the States. And there was no instructional literature relevant to these conditions, or sites of these types.

In sum, my entire seven years in Sudan was a vast learning experience—not without its mistakes and false starts. Quite simply, the number one requirement on every site was to *think.*

Do I want to dig this site, and if so why?

What can I hope to learn that I don't already know?

What is the best way to go about it?

Do I have sufficient resources, and if not is there a chance to get them? Who else besides me may be interested in the results?

A few days on the ground were enough to make it plain that I couldn't possibly do it all—sites on the rich Nile floodplain were beyond number. Therefore, the basic procedural issue from the start became

[3] For major survey and excavations conducted prior to the Nubian campaign see for example, Arkell 1949; 1953; Dunham 1950; 1955; 1957; 1963; 1967; 1970; 1982; Dunham and Chapman 1952; Dunham and Janssen 1960; Emery 1965; Emery and Kirwan 1935; 1938; Firth 1909; 1912; 1915; 1927; Garstang 1910; 1912; 1913; 1914-1916; Garstang and George 1914; Garstang *et al.* 1911; Monneret de Villard 1935a; 1935b; 1957a; 1957b; Randall-MacIver and Mace 1902; Randall-MacIver and Woolley 1911; Randall-MacIver *et al.* 1909; Reisner 1910; 1923; Smith and Jones 1910 [ed.].

[4] For a detailed autobiography see Adams 2009 [ed.].

[5] Originally employed and trained by W. M. F. Petrie for work on his excavations at Koptos in 1893-1894. See further Roland 2014; Stevenson 2015, 6-7 [ed.].

one of *triage*, or in other words prioritization. It was a term unfamiliar to me at the time, but it's what I was doing. For each site we encountered, there were four possibilities:
1) If it looks big and rich, leave it and try to attract a foreign expedition,
2) If it looks good but probably wouldn't attract foreigners, reserve it for later excavation after the preliminary survey was completed,
3) Dig it now, while you have the necessary men and resources on hand,
4) Write it off as not worth digging, when there are still so many more promising sites.

A factor affecting site selection in all salvage programs, and quintessentially in the Aswan High Dam project, was that of available time. Inundation from the dam was scheduled to proceed in stages. The first stage was scheduled to back up water in the most northernmost 62km of the Sudan, between 1964 and 1966. What this meant in practice was that there wasn't time to complete a preliminary survey before selecting sites to dig, as I had hoped in the beginning. As in the case of medical triage, we had to make on-the-spot decisions, to dig or not to dig, without knowing what lay ahead that might be more important. Needless to say, we sometimes guessed wrong, though our expertise increased with each passing season.

Further complicating the decision process was the fact that nearly all sites on the west bank of the Nile, where the great bulk of our work was done, were deeply buried in drifted sand (Almost the whole of the east bank was taken by foreign expeditions). You had to spend a day or two just throwing off overburden before any decision could be made about the value of further digging—and by then the dig was already started!

Another consideration, almost unique to my situation, was the knowledge that I was working for a foreign nation and a foreign people, who paid for every dime of my excavation expenses (UNESCO paid my salary, but that was all). I was for all practical purposes a member of the Sudan Antiquities Service, as it was then called. While they never gave me any direction or guidance, I had to be conscious that my results should be interesting to them and to the Sudanese people. This affected to some extent my choice of sites, but even more the way I wrote them up.

In the triage process, a final consideration of importance to me as an anthropologist was the state of existing knowledge. I have always felt strongly that the most important goal of science was to diminish the unknown, not to replicate the known. I had already noted, many years before, that a great deal of southwestern archaeology was replicative because so little had been properly published (cf. Adams 1960, 19). Applied in Nubia, this meant that medieval sites were more important than earlier ones; churches were more important than temples; small sites were more important than larger ones. Cemeteries were of virtually no importance, because scores of them, of all periods, had already been dug by museum-based expeditions in search of attractive objects for display.

In sum and in simple, peasant sites were more important than elite sites, because the vast majority of Nubians had always been peasants, and still were. The enduring popularity of my book, *Nubia, Corridor to Africa* (Adams 1977),[6] stems not from the fact that it is more accurate than others, but from the fact that it gives so much attention to everyday life. My greatest satisfaction, arising from the Nubia experience, lies in the fact that the Nubian people have adopted my book as their national epic, and have translated it into Arabic.

The essays

As a teacher I have always felt a compulsion to pass along what I have learned, and this was especially true in Nubia. The approaches I had devised, the things I had learned, and the explanations I had come up with were however, much at variance with prevailing thought among archaeologists. While espousing my

[6] See Plate ii [ed.].

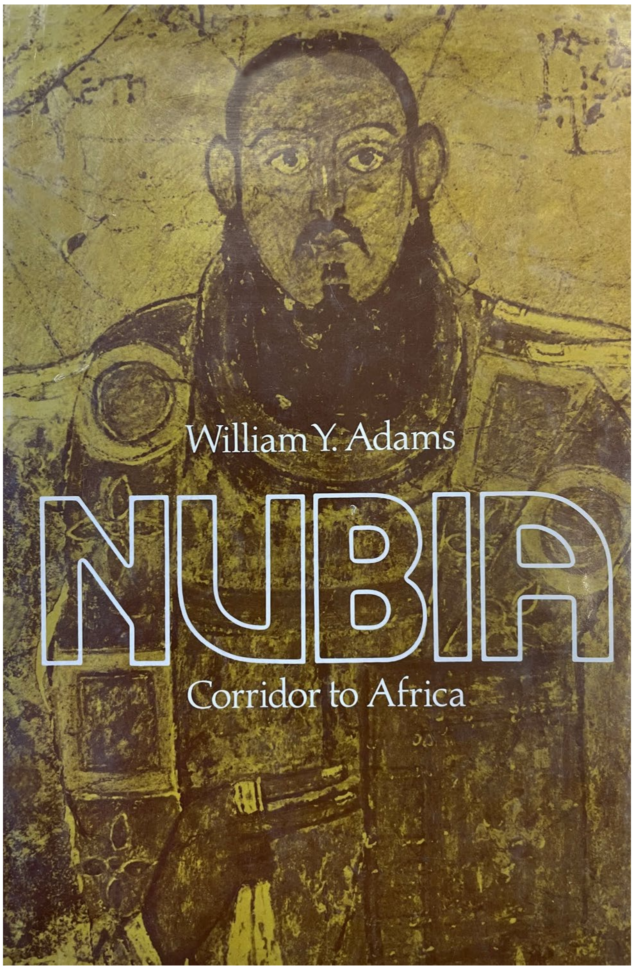

Plate ii. Cover, W. Y. Adams. *Nubia Corridor to Africa.* Princeton (1977).

own views, I often found that I had at the same time to dispute other, and more popular, ones. However, publication was always a problem. Archaeologists, or at least prehistorians, tend to be doctrinaire, and their professional journals are exceptionally so. I felt that I had little hope of getting my ideas into print in *American Antiquity* or the *Journal of Field Archaeology* (I have been rejected by both); I had to wait for circumstances where I knew that I could not be rejected.

Such opportunities were of two types: invited contributions to *festschriften,* and invited contributions to conferences. Readers will find in fact that ten of the fifteen papers in this volume fall into one or another of those categories. But while it has been a satisfaction to see my words on record, I have paid an inevitable price in terms of readership. Who, apart from the dedicatee, reads *festschriften*? Who, apart from the contributors, reads conference proceedings? It is precisely those considerations that have prompted me to put together the present volume, in hopes of rescuing at least some of my ideas from oblivion.

The papers were written or delivered over a period of 25 years, and in a wide variety of circumstances. They were published variously in the United States, Canada, Britain, France, Germany, Sweden, and Italy. Most importantly, they were written for a very wide variety of audiences, differing in the extent of their archaeological background and interests. Some should be intelligible to just about everyone; some mainly to persons with an archaeological background; one or two mainly to persons knowledgeable in Nile Valley archaeology.

Inevitably, given these circumstances, readers will find a lot of repetition from essay to essay. They will also recognize certain ‘pet peeves’, which I have been prone to air; for example, the so-called ‘New Archaeology’, and computerized classification. If there is a single theme that runs through most of the essays, it is the disconnect between what archaeologists profess and what they do in the field.

Here then is a summary of what I learned.

William Y. Adams, July 2019

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PERSPECTIVES

Plate 1.1. Air photograph of the Semna cataract and Semna West fortress, facing north (SARS Adams Archive, ADA S002_02).

Plate 1.2. Fortress at Semna with the New Kingdom temple dedicated to Senwosret III and the god Dedwen visible in its interior (SARS Adams Archive, ADA S002_04).

Three Questions for the Archaeologist[1]
(1992)

This is considerably the most recent of the essays included in the present volume. It was an invited contribution to a memorial volume honoring Michael Hoffman, an American archaeologist who in his later years directed a major excavation in Egypt.[2] I did not know Mike personally, and was not familiar in any detail with his work, so I was originally disinclined to accept the invitation. However, the two editors (both close friends) insisted they wanted me in anyway, so I simply used the opportunity to give vent to some of my dernières pensées *after a lifetime in archaeology. I assumed that the vast majority of readers would be Egyptologists, some of them focused on language studies rather than excavation, so I kept the discussion at a highly generalized level.*

An ethnologist colleague of mine once remarked, 'if you want to find the Goldwater Republicans (i.e. political conservatives) in any Anthropology faculty, look among the archaeologists'. I'm not sure if that is really valid statistically, but it is certainly the case that archaeology has tended to appeal to law-and-order types, who seek to impose a rigid and somewhat arbitrary order on the data of prehistory. Often enough too they seek to impose order on the methods and objectives, not only of themselves but of all their colleagues. The theoretical literature abounds in doctrinaire, programmatic declarations telling us not only how we ought to dig, but even what we should and should not be interested in, and why. To my knowledge there is nothing comparable in the literatures of ethnology and linguistics – the other two major subdivisions of Cultural Anthropology. Archaeologists, I conclude, have a low tolerance for ambiguity, which at times may mean a low tolerance for reality.

Rule-book archaeology has two undeniable advantages. First, it insures a certain comparability of results between different digs, leading usually to a reaffirmation of prevailing orthodoxy. Second, it saves the archaeologists from having to think. This strikes me as the major appeal of most formalistic procedures, and it puts me in mind of a remark by the statistician R. M. Cormack (1971, 353): 'How can statistics be tailored to the real needs of the user, when the real need of the user is to be forced to sit and think?'

One finds, however, that the rules and the rulebooks are not the same in all parts of the world, and it becomes increasingly difficult to avoid thinking as one moves from one area of operations to another. The archaeologist who jumps, as Michael Hoffman and I did, from Native American prehistory to Nile Valley archaeology, finds himself confronted with new doctrines and new orthodoxies that are at variance with his previously received wisdom. Under those circumstances he may undergo a certain identity crisis, and begin asking himself some rather fundamental questions about the nature and meaning of the archaeological enterprise — questions that are too easily ignored when rulebook procedures are followed. I know that this happened in Mike Hoffman's case, as it did also in mine. In honor of Mike's memory, I want here to consider three basic questions about the nature and purposes of archaeology, that I know were of continuing concern to him.

[1] Originally published in: R. Friedman and B. Adams (eds) 1992. *The Followers of Horus: Studies dedicated to Michael Allen Hoffman 1944-1990.* Egyptian Studies Association Publication No. 2, Oxbow Monograph 20. Oxford, 1-6. The paper was accompanied by two photographs of Michael Hoffman which are not included here. They were germane to the overall volume, being placed in honour of Hoffman's memory, rather than with reference to this specific paper and are not mentioned within. The first, taken by J. R. Geller showed him digging at Hierakopolis while the second taken by R. Friedman, was entitled 'Foundation Ritual Revisited: Michael Hoffman scattering bonbons in the foundation trench of his dig house at Hierakonpolis before laying the first stone' [ed.].

[2] Michael Hoffman (1944-1990) co-directed excavations and headed the Predynastic Research Team at Hierakonpolis, Egypt between 1978-1990. See further http://www.hierakonpolis-online.org/index.php/history-of-exploration/michael-hoffman [accessed 22.04.2020] and Bierbrier 2012, 259 [ed.].

What are you digging for?

A. V. Kidder, a great raconteur as well as a great archaeologist, liked to tell a story about his early excavations at Pecos Pueblo. The dig was located only a couple of miles from one of New Mexico's major highways, and was plagued with a steady stream of uninvited visitors. According to Kidder, the visitors fell pretty consistently into two categories: the admirers and the scoffers. 'To the admirers,' said Kidder, 'we used to say that we were digging to gain new understanding about human culture that would ultimately benefit mankind. To the scoffers we used to say that we were digging because of the fabulous pay'. Concluded Kidder: 'It would be hard to say which was the bigger fabrication.'[3]

The story, I am sure, has a familiar ring to all field archaeologists. We have all encountered our share of the admirers and the scoffers, and we have all formulated relatively flip and facetious answers to their questions, if only to be rid of them as quickly as possible. How many of us have really tried to explain what we are doing, and why, to casual visitors? 'It would take too long', is our usual excuse, but could it be that we really don't have a better answer, even for ourselves? In truth, the question, 'what are you digging for?' is frequently an awkward and uncomfortable one. We make up one kind of pat answer for the casual visitors, another for the funding agencies, and perhaps another still for our academic employers, but they may all involve a certain element of mythology that we don't honestly believe in.

A generation ago, it was a favorite accusation of the New Archaeologists that their predecessors had no clear sense of purpose. That problem was supposedly resolved by the introduction of scientific principles and scientific rigor into archaeology. But in reality science defines means, not ends, and the answers that the New Archaeologists gave to 'what are you digging for?' were no more convincing and no less mythological than were those of their predecessors, including Kidder. My own view is that the lack of clearly defined objectives in archaeology has become more conspicuous, not less so, with the passage of time.

Whatever we may think of their motivations, the pioneer archaeologists of the 17th and 18th centuries were clear and unequivocal as to why they were digging: it was to recover treasures. Initially these were merely for the benefit of private patrons. Later, when museums came to the forefront as archaeological sponsors, the treasures were to be for public exhibition, which gave a somewhat broadened scope to the work of the archaeologist. Then, around the middle of the last century, came the recognition of two new possibilities. One was that archaeology could confirm and enlarge upon the information contained in ancient texts, such as the *Bible* and the *Iliad*. The other was that systematic archaeology could fill in the huge historical blank preceding the earliest written records. For about a century, the great majority of archaeologists were avowedly pursuing one or another of those two goals, both of which were essentially historical. That is, they were seeking either to put together a systematic and chronologically ordered record of the prehistoric past, or to fill in lacunae in the record of written history.

Around the middle of the 20th century, archaeologists, at least in North America, decided that history wasn't enough. Influenced by contemporary developments in ethnology, they proclaimed that we should in effect be trying to write ethnographies of the prehistoric cultures we studied, and to see the ancient worlds as they would have appeared to those who lived in them. History was at best a means to that end, rather than an end in itself.

Not very much later, the self-proclaimed New Archaeologists turned their backs on history altogether, declaring it 'unscientific' and announcing that the goal of archaeology must be to test general, causal propositions about human culture and its development. But this enterprise was epistemologically flawed from the beginning. Although they pretended to a high degree of intellectual sophistication, the New

[3] I have paraphrased the story as I heard it from Kidder's lips, around 1950. I'm not sure if he ever committed it to paper.

Archaeologists somehow never grasped the fact that all social science theory is necessarily probabilistic, and cannot be either verified or falsified by single-case field tests. The result was that New Archaeology produced a great proliferation of sententious programmatic literature, but almost nothing in the way of field results that could be taken seriously. Perhaps the most telling verdict on this enterprise can be found in a recent article on *What the New Archaeology has accomplished* (Watson 1991) – which never once mentions anything accomplished in the field!

In a widely read and much-cited article, Kent Flannery (1967) drew a contrast between Culture History and Culture Process, insisting that the proper concern of archaeologists should be the latter and not the former. He and fellow 'processualists' failed to notice that you cannot excavate 'process'. Process is an inference you make from the things you find in the ground, just as in ethnology, culture is an inference you make from the behaviors you actually observe. The ethnologist cannot get to culture without going through the evidence of behavior, and the archaeologist cannot get to process without going through the evidence of history. The dichotomy between history and process is, in other words, a spurious one.

Although the absurdity of its pretentions made New Archaeology an inviting target for criticism, it was surprisingly long in coming. Once begun, however, it has swelled to something like a cacophony in the recent past. But archaeologists would seem now to have thrown out whatever baby there was along with the bath water. In rejecting the excessive formalism of New Archaeology, the so-called post-processualists have apparently decided that there is no inherent order to be discovered in nature or in culture – it's all in our heads. We can therefore place whatever construction we like on the remains we uncover, with no fear of refutation. Under these circumstances a dig becomes nothing more than an exhibition of technical and interpretive virtuosity for its own sake; an end rather than a means. What, then, are we digging for – just to show off our skills?

Why are you digging here?

My old friend and colleague Jean Vercoutter tells of a joke that is common among French archaeologists: 'This is a beautiful spot – let's dig here'.[4] This too should have a familiar ring. Long ago I observed how promptly the archaeologists in the American Southwest shifted their interest from the desert-dwelling Anasazi to the forest-dwelling Mogollon, once they discovered that there were sites to be dug among the tall, cool pine trees.[5]

The question, 'Why dig here?' has to be answered at three levels of specificity: in the choice of an area in which to work, in the choice of a site or sites to dig within that area, and in the choice of excavation areas within each site. If our actions were entirely consistent with our purposes, the determining factor should be the same in each case. In fact, it is likely to be somewhat different at each level.

Archaeologists in my youth were inclined to be highly parochial. They worked all their lives within a single area for no better (or worse) reason than that they were strongly and particularly interested in that area, and often enough were little interested in any other area. Subsequently the profession has become much more cosmopolitan, and it is rare today to find one-area archaeologists, except in such avowedly particularistic fields as Egyptology and Sinology. As nearly as I can determine, the choice of fields (at least among colleagues of my acquaintance) has been determined almost entirely by opportunity, rather than by any previous interest. This of course was true in my own case, when I made the jump from the American Southwest to Nubia in 1959. A specific interest in the area and its problems came subsequently to the

[4] Jean Vercoutter (1911-2000) was a French archaeologist and Egyptologist who began work in Sudan in 1953. He was Director General of Antiquities in Sudan (1956-1960) and later Director the Institut Français d'Archéologie Orientale, Cairo (1977-1981). He excavated, for example, Mirgissa and Sai in Sudan and at Thebes, Egypt. See further, https://www.egypt.edu/actualite/2000/000729/29juillet04.htm [accessed 22.04.2020] [ed.].

[5] In an unpublished paper, 'Kidder up to Date; A Reintroduction to Southwestern Archaeology'.

beginning of my work there.

Several factors may be involved in the choice of a site or sites to dig within a chosen region. For a long time the hope of reward, in the form of material finds, was the overriding consideration, and archaeological 'sampling' really consisted of pulling the plums out of the cake. But logistics too could (and can) seldom be disregarded. For obvious, mainly financial, reasons we go for the most accessible sites in preference to the more remote ones. Neither of these factors is a legitimate consideration from the standpoint of sampling; with the result that our reconstruction of culture history in many parts of the globe is a somewhat distorted one, based on skewed 'samples'.

Salvage (rescue) archaeology in the recent past has gone a considerable way to correct that deficiency. Reservoir, highway, and pipeline construction projects have led to the more or less enforced excavation of all kinds of sites that would probably otherwise have gone untouched. The results have provided a major corrective to the somewhat elitist perspective that comes from digging only the biggest and the richest sites. But these projects are usually too big to allow the excavation of every threatened site, and the problem of selection still remains. Various area sampling procedures have been developed, such as the use of transects and randomly selected grid squares, but the development of genuinely effective sampling procedures in archaeology is still in its infancy. There is an unfortunate tendency to regard totally randomized procedures (eliminating any need for judgment) as the most 'scientific' – an assumption that has no legitimate foundation in the fields of social science. No economist, political scientist, or sociologist makes generalizations or predictions on the basis of purely random population samples.

The same *caveat* applies to the selection of excavation areas within any given site. Here too the accepted procedure used to be to go after the plums – that is, start with the most rewarding-looking feature visible at the surface, and work outward from there. More recently there have been attempts to eliminate subjective decision-making by excavating randomly selected squares. But in my rather long experience of total-area surveys and total-site excavations, I have found that the results obtained in this fashion almost never correspond very closely to the expectations derived from sampling. The truth is that we understand, so far, very little about patterning in archaeological data. Most of what we profess to understand is based on samples that at best yield hypotheses to be tested by full-site excavation.

As a townsite archaeologist, I object strongly to any sampling procedure that results in cutting houses in half, as often happens when grid squares are randomly excavated. I don't know of any archaeologist who would treat a burial in such disrespectful fashion – excavating only the pelvis, leg bones and feet while leaving the upper torso and the cranium in the ground. A house is as much an integrated, organic unit as is a skeleton, and is potentially far more culturally informative, yet many archaeologists habitually excavate part of the building and leave the rest in the ground. What is interesting and informative about a house is the plan, and it can rarely be extrapolated with confidence from any accidentally uncovered 'sample'.

A sampling problem that commonly arises in townsite excavation is 'down or out?' Having opened an initial excavation area or areas, shall we enlarge it downward, or outward? The much-admired Kenyon-Wheeler method of excavation[6] seems to prescribe 'downward, ever downward'. Squares or trenches are dug from the surface to the level of sterile subsoil, and the so-called 'archaeological record' is then read in a series of vertical soil profiles. My own view is that this technique of excavation reveals only one particular kind of archaeological record, and often not the most informative one. First of all, the

[6] Also referred to as the Wheeler-Kenyon method. This system was first used between 1930-1935 during excavations at Verulamium by R. E. Mortimer Wheeler (1890-1976), and then was modified by Kathleen Kenyon (1906-1978) during her work at Jericho conducted between 1952-1958. See further Callaway 1979; Davis 2008 [ed.].

succession of ash, refuse, building rubble, and sterile lenses that is revealed in any particular profile comprises a highly particularized record of events at that one spot only. It can never be generalized even to adjoining excavation units, let alone to the whole site. A dozen such profiles taken from a dozen excavation units yield a dozen different stories, none necessarily informative about the site as a whole. Second, the Kenyon-Wheeler method establishes the association between artifacts and deposits, but ignores the association between artifacts and buildings. When you cut a trench or pit down arbitrarily through walls and floors, you can usually form little idea about the relationship between artifacts and the nearby walls that have been cut in half.

Obviously, the decision between 'down' and 'out' must depend on what you're trying to learn. The vertical record is largely a record of the unintentional: fires, destructions, refuse depositions, over-buildings. The horizontal record is more often a record of the intentional: house plans, spatial layouts, the siting of temples and cemeteries. One could say therefore that the vertical record is purely a historical record, and the horizontal a cultural record. We have a choice, then, as to whether we are more interested in the handiwork of nature or in the handiwork of man. As a cultural anthropologist my interest has always been in the latter – hence my preference for stripping a townsite layer by layer rather than digging it full of holes.

Who are you digging for?

Once, in the early days of the Nubian Monuments Campaign,[7] I was traveling along the Nile in company with a carload of journalists enroute to visit the great fortress site of Semna.[8] In the back seat, I was discussing with one of my companions the new archaeological museum that the Sudan was to build, and I remember saying something to the effect that 'a museum is an educational institution'. Abruptly, the *London Times* reporter turned from the front seat and thundered, 'NO! A museum is for enjoyment! Dammit!!'

That quintessentially English remark set me to thinking, really for the first time, about the different constituencies that archaeology and its discoveries may serve. On the one hand there are those, like myself, whose enjoyment of a museum can be measured in direct proportion to the length of the explanatory labels. On the other hand there are those, like the *Times* man, whose enjoyment was evidently in inverse proportion to the labels.

The issue became more practically relevant for me within the next months, when I had to make strategic decisions about the directions that the Nubian salvage campaign would take in the Sudan. This was in the heyday of New Archaeology, and we were being told in the most insistent terms that we should be testing general hypotheses and not wasting our time on anything so ephemeral as cultural history. But I was funded directly by the Sudan Government rather than by the National Science Foundation or any faraway academic institution, and that gave me a certain sense of obligation – conspicuously lacking among most New Archaeologists – toward the people for whom and among whom I was working. Whatever my own inclinations and those of my fellow archaeologists might be, I felt compelled to produce results that would be maximally interesting and meaningful to the Nubian people whose land and heritage were being destroyed.

It seems to me now that at least four archaeological constituencies deserve to be considered. There is, first of all, the general public (represented by the *Times* man) that simply enjoys the contemplation of beautiful or interesting things and doesn't particularly care about learning anything in the process. There is another general public (represented by myself) that likes to learn about culture and history. There

[7] UNESCO International Campaign to Save the Monuments of Nubia (1960-1980) https://whc.unesco.org/en/activities/172/ [ed.].
[8] See Plates 1.1-1.2 [ed.].

are, thirdly, the people whose remains are being uncovered, and who very probably have a historical self-image that they would like to see protected and enhanced. Finally, there are the archaeologists, who would like to show off what they have done and what they have found.

It could be argued that only a minute proportion of what we uncover will ever find its way behind museum glass, and that only in these special cases need we bother about public constituencies. But our published reports are also displays, and in this broader sense a very large part of what we uncover will, or should ultimately, go on exhibition. I hold firmly to the belief that our sense of public obligation must extend to our published reports no less than to our museum displays; they must do something more than just show off our skills for the benefit of fellow archaeologists.

Excavation reports from the past were often highly speculative, drawing rather large conclusions from a small and imprecisely recorded body of data. But whatever their shortcomings, they were interesting. There is a tendency today to publish excruciatingly detailed, descriptive reports with a minimum of interpretation, and these are rarely of much interest to anyone but their authors. We are left to admire the technical virtuosity of the diggers, which is often conspicuously displayed, without gaining much understanding of what they actually learned. The commonly expressed hope is that such reports are creating an archive that will serve as a basis for interpretations and reconstructions in the future, but that (like plundering the environment) merely puts a burden of responsibility on later generations that properly belongs on us.

W. B. Emery, the excavator of Buhen,[9] was notorious for the brief and uninformative progress reports that he published at the conclusion of each season. When I questioned him about this, he advised me, 'Never publish detailed progress reports. They leave you with no audience for your final report'. The logic was flawed, but the advice was not without foundation. He might better have said, 'Never publish detailed progress reports. They leave you with no incentive to write a final report'. All too often, the publishers of detailed, season-by-season reports have failed to produce a final synthesis, on the grounds that they have already put everything on record piecemeal. But of course they haven't. Piecemeal reports, without interpretation, are difficult enough to make sense of even for the professional archaeologist, and for the general public they are in a foreign and undecipherable language. You end up with so many partially assembled parts of a jigsaw puzzle, from which you can rarely make out the big picture.

All too often, it seems to me, the archaeologist has a curiously truncated sense of responsibility to his profession and to some kind of abstract scientific standard, but not to any larger public, and certainly not to the people whose remains he is so methodically disturbing. He condemns the work of earlier generations for its elitism, its imprecise methods and its lack of documentation, yet he may be pursuing interests and publishing reports that are of no interest to anyone except fellow archaeologists. But the remains that he disturbs and carries away are, properly speaking, a public trust and not a private hunting preserve for archaeologists. In a narrow sense they are the heritage of a particular people, whose descendants may still be living nearby; in a broader sense they are the heritage of all mankind. I am convinced that the current antagonism toward archaeology that is so conspicuous among Native Americans was provoked in part by the cavalier attitude of the New Archaeologists, who disdained any interest in the history of the very Indians whose remains they were disturbing.

Almost everyone would agree that archaeology began as a purely humanistic enterprise, but over time has become an increasingly scientific one. There are enormous advantages in this transformation,

[9] See further, Bierbrier 2012, 176-178; http://egyptartefacts.griffith.ox.ac.uk/people/walter-bryan-emery [accessed 23.04.2020] [ed.].

from the standpoint of recovered information, but it is not without cost. Whether we like it or not, the interest of the general public in our work is overwhelmingly humanistic. They really don't care about the precision of our methods; what they care about is our contribution to history and to cultural knowledge and to aesthetic enjoyment. From that perspective they find the work of Schliemann and Evans more interesting than that of Petrie and Kidder, just as they find the work of Petrie and Kidder more interesting than that of Binford and Hodder.[10]

In short and in sum, our public constituency has been shrinking with each generation, as we pursued the elusive goal of 'pure science'. The museums are full of treasures excavated in the bad old days when we were only out for treasures, and of culture-historical charts from the prescientific days when we were only out for history. Museum visitors avidly follow the sequence of development from Basketmaker to Pueblo I, II, III, IV and V, because it makes intelligible and interesting history.[11] How much of the result of recent, problem-focused archaeology will you find on display in the same museums?

Another measure of the same trend can be observed in the pages of the popular journal *Archaeology*. The magazine's wide circulation attests to the public's continuing appetite for archaeology; the distressing small amount of actual field archaeology that is reported in each issue is a reflection of how little of the current work is really of much general interest. Who are we digging for? Mainly, it seems, for ourselves.

Whither archaeology?

The late Glyn Daniel[12] was fond of asking this question, in the days when he edited *Antiquity*, and from time to time he invited different contributors to answer it. I have always regarded it as a non-question, implying as it does that archaeology is an autonomous discipline capable of setting its own goals. To me this is tantamount to saying that means can dictate ends – as though a shovel could decide where and what it wanted to dig.

I always looked at the matter from just the opposite perspective, and assumed that archaeology would have to go wherever Anthropology, European Prehistory, Classics, Egyptology, Sinology, and half a dozen other disciplines directed it. Now, however, archaeology threatens to become an end in itself: an exercise in technical virtuosity carried out for the delight of other archaeologists and for no one else. Under these circumstances one might conclude that Daniel's question has more relevance that it formerly had. But then, an end can't really go anywhere, can it?

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[10] For example, Heinrich Schliemann (1822-1890) began the first of several excavations at Troy in 1871 and at Mycenae in 1876, publishing *Troja und seine Ruinen* (1875) and *Mykenai* (1878). Sir Arthur John Evans (1851-1941) began excavations at Knossos in 1900, discovering the Minoan civilisation and published numerous works among them, but not limited to *The Palace of Minos*, volumes 1-4 (1921-1935). Sir William Matthew Flinders Petrie (1843-1952) excavated numerous sites in Egypt and Palestine. See further Bierbrier 2012, 428-430; Petrie 1931. Lewis Roberts Binford (1931-2011) was an archaeologist and archaeological theorist and a leading proponent of the New Archaeology and one of the pioneers of processual archaeology. In contrast, Ian Richard Hodder (1948-present) is an archaeologist and founder and proponent of post-processual (interpretive) archaeological theory and symbolism. He currently leads the Çatalhöyük Research Project. See further Binford 1962; 1972; Hodder and Orton 1976; Hodder 1982; and http://www.ian-hodder.com/ [accessed 23.04.2020] [ed.].

[11] Basketmaker to Pueblo I, II, III, IV and V are cultural periods of the Ancestral Puebloans, a Native American culture situated in Utah, Arizona, New Mexico and Colorado. The periods date from 8000 BC to the present. These divisions are known as the Pecos classification as created by Alfred Kidder [ed.].

[12] See further, Hammond 1989; Pretty 1987 [ed.].

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Plate 2.1. Interior of the laboratory at Wadi Halfa, used during the UNESCO Campaign to Save the Monuments of Nubia. Note the air photographs of Lower Nubia mounted on the wall (SARS Adams Archive, ADA Di026).

Plate 2.2. Sudan National Museum, Khartoum, inaugurated in 1971 (photo J. R. Anderson).

Science and Ethics in Rescue Archaeology[1]
(1984)

This was my invited contribution to a festschrift *in honor of the eminent Swedish Egyptologist Torgny Säve-Söderbergh, with whom I had been in close collaboration during the Nubian archaeological salvage campaign.[2] Of my numerous Egyptologist colleagues at that time, he was very much the most intellectual, with a wide-ranging mind and an interest in ideas for their own sake. As a consultant at UNESCO he had been involved from the beginning in the formulation of policy for the Nubian campaign, so I knew that my ideas on the subject of archaeological ethics would find a ready reception. This was the first of many writings in which I expressed my profound opposition to 'New Archaeology'.*

I welcome the invitation to contribute to this volume, for it will allow me to do three things that I have been wanting to do. The first and most important is to pay tribute to the work of Professor Säve-Söderbergh, and more particularly to an aspect of his work that I am in a special position to appreciate. I refer to his role in developing archaeological policy both for the International Campaign to Save the Monuments of Nubia in general and for the Scandinavian Joint Expedition to Sudanese Nubia[3] in particular.[4] At the same time and in the same context, I want to answer those critics who have argued that rescue archaeology is of little value because it is unscientific. Finally I want to make a point that is well underscored by the Nubian campaign and its results: that in all archaeology there are ethical constraints that must be balanced against scientific and scholarly ones.

For many readers of these pages, the decade of the 1960s will be forever memorable because of the Nubian rescue campaign. It was an international effort without parallel before or since, and for those who participated it brought a sense of excitement and of fulfillment that has persisted down to the present day. In spite of omissions and imperfections, the amount of field archaeology accomplished in Nubia was prodigious, and we may perhaps be forgiven if we think of this as one of archaeology's finest hours.

A special virtue of the Nubian campaign, as many of us saw it, was the extent to which archaeologists set aside personal and selfish interests in order to save whatever needed saving. Indeed, this was required under the concession policy adopted by both Egypt and the Sudan, on the advice of UNESCO. The effects are apparent if we compare the results of the High Dam campaign with those obtained by the earlier Nubian rescue programs, from 1907 to 1911 and from 1929 to 1934.[5] In the earlier campaigns the archaeologists largely followed their preferences, with the result that temples and tombs were dug but townsites generally were not, and the pharaonic and earlier periods were extensively investigated while the latter periods were slighted. This neglect was especially marked in the case of medieval remains. In the High Dam campaign, however, Egyptologists found themselves for the first time digging in prehistoric sites, prehistorians worked in medieval towns, and the load of salvage obligation was unselfishly shared by many. Collectively, they gave us a much fuller and more balanced picture of the cultures and the history of Nubia than we had previously held.

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[1] Originally published in: R. Holthoer, R. and T. Linders (eds) 1984. *Sundries in Honour of Torgny Säve-Söderbergh.* Stockholm, 9-15.
[2] UNESCO International Campaign to Save the Monuments of Nubia (1960-1980) https://whc.unesco.org/en/activities/172/ [accessed 22.04.2020]. Initially, W. Y. Adams was employed by UNESCO earlier in 1959. See further 'Genesis', this volume and Plate 2.1. Regarding Säve-Söderbergh, see Bierbrier 2012, 488-487 [ed.].
[3] Regarding the Scandinavian Joint Expedition, see further Säve-Söderbergh 1984 [ed.].
[4] I make this remark because I was also involved to some extent in planning for the rescue campaign in the Sudan, and before that for the Glen Canyon archaeological salvage campaign in the U.S.A. See my 'Strategy of Archaeological Salvage', *American Geophysical Union, Geophysical Monograph Series*, Vol. 17 (1973), 826-35 [W. Y. Adams].
[5] For details and references concerning these campaigns see Adams 1977, 74-77 [ed.].

For archaeologists who were not involved in Nubia, the 1960s are memorable for quite another reason. This was the decade when the 'new archaeology' launched itself upon us as a radical new approach, ostensibly rendering obsolete most of what had gone before. Lewis Binford, the leading prophet of the new dispensation, unhesitatingly proclaimed that '...advances in achieving the aims of archeology *necessitate* the enforced obsolescence of much of traditional theory and method...' [emphasis mine] (Binford and Binford 1968, 27). The 'new archaeologists' asserted that archaeology must be scientific or it must be nothing. They had, moreover, a very specific view of what was and was not scientific. Scientific archaeology must somehow involve the testing of causal principles about human behavior or cultural evolution. 'The 'new archaeologists' have...asserted that the criterion for correctness of argumentation... should be the notion of *scientific explanation*. Further, they have asserted that all scientific explanation can be...cast in what is called the covering-law model, in either its deterministic or its statistical form' [emphasis in the original] (Read *et al.* 1978, 307).

Rescue archaeology came in for special criticism because of its supposedly unscientific character. As perceived by Hole and Heizer, '...salvage archeology runs counter to practices that will be advocated here. First, salvage archeologists must be indiscriminate and take the sites as they come. That is, it is not possible to be very selective when the atmosphere is one of a rescue operation. Second, salvage archeologists must operate under the pressure of time and thus may be forced to ignore many of the refined techniques of modern archeology. The resultant loss in information is rationalized by saying that, after all, something was saved' (Hole and Heizer 1966, 32). Thus, a feature of the Nubian campaign that was seen by us as especially laudable was seen by others as blameworthy.

The 'new archaeology' can be, and has been, challenged on its own ground of scientific epistemology.[6] The notion that all scientific endeavor must be related to 'covering laws' was taken over from the philosopher C. G. Hempel,[7] whose ideas never gained full currency among his fellow philosophers of science. Indeed, they were already going out of vogue at the time when the 'new archaeologists' seized upon them. The definition of science, which they proclaim is so narrowly restrictive that it would exclude, for example, much of the current work in biology and astronomy. A great deal of the research in these disciplines, as in archaeology, is basically exploratory. It involves highly systematized discovery procedures that are unconnected with causal theory.

It is worth noting also that in much exploratory and descriptive research, random sampling is a recognized and a necessary scientific procedure. Its purpose is to eliminate subjective bias in the selection of units for study. From this perspective we might argue that, precisely because '...it is not possible to be very selective when the atmosphere is that of a rescue operation' (Hole and Heizer 1966, 32), the approach of the rescue archaeologist is more nearly scientific than is that of his more selective colleagues. I think that the results of the Nubian campaign will hear this out, inasmuch as they have given us a more accurate picture of Nubian culture and history than did 50 years of selective archaeology. I have elsewhere made the same point with regard to rescue archaeology in the American Southwest (Adams 1973, 828).

In the view of Binford and some of his followers, archaeology will become more 'scientific' by becoming more 'anthropological', and vice versa.[8] There is a certain irony in this position, since archaeology is already the most scientific of the various sub-branches of cultural anthropology. Ethnologists, social anthropologists, and linguists are much more humanistic in their approach to culture and behavior, because they have to deal with the complexity of living man instead of with the simplicity of inert things. Moreover, and for the same reasons, their field methods are less rigorous. One would suppose, therefore,

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[6] See especially Johnson 1972, 366-377; Tuggle *et al.* 1972, 3-12; and Flannery 1973, 47-53.
[7] See especially Hempel 1965; 1966.
[8] See Binford 1962, 217-25; Longacre 1970, 1-3.

that to bring archaeology more into the mainstream of anthropology would be to make it less scientific, not more so.

The rather presumptuous claim that archaeology must serve the purposes of anthropology, and that both must be scientific, ignores the fact that most of the world's archaeologists are not, and do not claim to be, either anthropologists or scientists. They are Medievalists, Classicists, Egyptologists, Assyriologists, Sinologists, European prehistorians, art historians, and humanists of other sorts. A rigid enforcement of 'new archaeology' structures would put all of these scholars out of business, and would leave the entire buried record of history to the attentions of a small group of zealots who disavow any interest in history.[9] Colin Renfrew has put the matter clearly in recognizing that '...archaeology may in a sense be the past tense of anthropology alone' (Renfrew 1982, 4).

My purpose in this essay, however, is not to pursue further the issues I have touched on above. They have already been debated at very considerable length and with considerable heat.[10] Instead, I want to look at another issue that lies between the rescue archaeologist and the 'new archaeologist', but has not received enough attention from either. This is the issue of ethics in archaeology.

Binford and his followers are explicit about the archaeologist's responsibility to science, but they do not seem aware that he has any other responsibilities. In this they are exhibiting, I think, a moral myopia not much different from that of the 19th century treasure-seeker, who felt no responsibility except to recover objets d'art for their patron or museum. Both engaged in excavation which is to say destruction of archaeological sites for narrowly defined objectives of their own, disregarding any interest which other scholars, or the lay public, may have had in the same sites.

In discussing the question of ethics, let us first consider simply the legal context of our work. In much of the world, including all of Latin America, the Near East, and continental Europe, archaeological sites are public property by legal definition. The same is not always true under British and American law. At least in America, however, the great majority of sites that are actually excavated are on public land, and these sites too have the status of public property. The laws that protect them were not passed in the interest of archaeologists; they were enacted to protect the interest of the public at large.

The archaeologist, then, is rarely the 'freehold' owner of the site he digs; he is the trustee for public owners whose interest may be substantially different from theirs.[11] A further expression of public interest may be found in the fact that most archaeological work is supported by public funds.

During the Nubian rescue campaign, there were certain Egyptologists who argued against the great expenditure of money and effort devoted to the reconstruction of Abu Simbel and other temples. They asserted that there is nothing more to be learned from Abu Simbel, whose inscriptions and reliefs have long since been recorded, and that in any case it is an aesthetic mediocrity. Why not let it go under water, and spend the $63 million on legitimate archaeology? I heard this argument more than once, as I am sure Professor Säve-Söderbergh did. Whatever his personal views on Abu Simbel may be, I am sure that his reaction was the same as mine 'Who are we, the scholarly community, to decide such matters? Abu Simbel does not belong to us, and we may be poorly qualified to judge its worth'. The decision to save Abu Simbel was ultimately made by Egyptian politicians acting on behalf of the Egyptian public, and rightly so. Their judgment, and that of the scholars who agreed with them, has been more than justified by events. Over 1,000 tourists a day are now paying to see the restored monument, which is very much greater than the number who used to see it in a month.

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[9] Cf. Flannery 1967, 119-122.
[10] For a resume on the debate see Dumond 1977, 330-349; Read and LeBlanc 1978, 307.
[11] These issues are more fully discussed in McGimsey 1972, 5-19.

The thinking of the Egyptological purists who wanted to abandon Abu Simbel seems absurd in retrospect. It is no different on principle, however, from the thinking of the scientific purists of the 'new archaeology'. The Egyptologists were, and the 'new archaeologists' still are, asserting that we should engage in nothing that does not serve the interests of our own scholarly disciplines, however narrowly those may be defined.

Some 'new archaeologists' have acknowledged that rescue archaeology is a special case, which must pursue goals different from those of 'scientific' archaeology. Most of the time, however, this is a specious distinction. All archaeology is rescue archaeology, insofar as it involves the rescue of objects and information, which if left in the ground, will sooner or later be threatened with destruction (Hole and Heizer 1966, 31-32). At the same time, paradoxically, all archaeology is destruction, in that it destroys much of the archaeological record in the process of investigating it. Whether or not a site is subsequently threatened with inundation or with overbuilding, no one else will ever be able to go back to it and recover the same information, once the archaeologist has finished with it. As a public trustee, therefore, the 'scientific' archaeologist has no more right to disregard the public interest than has the rescue archaeologist. In both cases their sites are subject to destruction by their own activities, regardless of subsequent developments. An excavation permit, then, is not a license to indulge our personal or our professional interests exclusively, however laudable they may be. It is a privilege granted us by the public, to help conserve a public heritage.[12]

How shall we define the public interest, which we are bound to consider? This is not any easy question, and perhaps for that reason it is not sufficiently often asked. In truth, archaeology has many publics with many interests, and most of them are as legitimate as are ours.[13]

We might begin by considering the various other scholars who could be interested in our sites and our data. At the very least they will probably include historians and art historians, in addition to anthropologists and prehistorians. They might also include ceramologists and other technicians with specialized interests. Then there are various lay publics. There may be local people with special knowledge and a special pride in local antiquities, who want to see them displayed as prominently as possible. There is probably a national intelligentsia, not knowledgeable about local remains but interested in all aspects of the country's past. There are assuredly the idly curious, who love to look at anything old just because it is old. Finally, there are national, state, and local governments that have their own interests in archaeology: to promote tourism, to provide museum materials, perhaps even (as in Mexico and Israel) to enhance the national self-consciousness. None of these are considerations that we can disregard in our pursuit of scholarly purity.

Another approach to the same issue is to consider the various scales by which the value of archaeological remains can be measured. I can think of at least six of these:

Informational value. Because the uninformed public tends to think of us as treasure-seekers, we may have to protest from time to time that we dig only for information and not for objects. For most of us, most of the time, this will indeed be the primary consideration in choosing which sites to dig and which not to dig. Once we have started upon a site, however, it cannot entirely determine our excavation strategy, for we have now also started on a process of destruction. From it we have to salvage what we can for all interested parties.

Aesthetic value. If Chartres Cathedral or the Taj Mahal were to be buried in volcanic ash, scholars and the lay public would surely unite in support of an effort to clear them. Even the most zealous of 'new

[12] Cf. McGimsey 1972, 5-6.
[13] For further discussion, see McGimsey 1972, 12-19.

archaeologists' would probably agree in principle that man's most beautiful handiworks ought to be seen by man, not hidden away in the ground. Anyone who acknowledges such a feeling must also acknowledge the legitimacy of digging for the recovery of art treasures, whether or not they contribute anything to the advancement of science.

Display value. Art museums may display objects largely on the strength of their aesthetic value. There are many other kinds of museums, however, whose function is more to educate than to entertain. For them, display value may be a matter of how well an artifact exemplifies its type, or gives an insight into the culture of its makers. Digging for the recovery of good display material, like digging for objets d'art, can be a legitimate archaeological objective.

Historical value. Some things are valued not for any inherent properties they possess, but because of known historical associations. The Holy Grail, if such a thing existed, would be the prime example. On a less exalted level, we are often asked to contemplate such mundane objects as a pair of eyeglasses or a rosary, because they once belonged to Benjamin Franklin or to Martin Luther. An archaeological site where Alexander was known to have camped would surely be of more than ordinary interest, just because of the chance to find objects that Alexander had used. They would be prized displays in many a local museum.

Market value. Most of us deplore the trade in antiquities, legal as well as illegal, and wish that it could be suppressed. Yet in some sense we contribute to it, for our work creates a public awareness, which whets the appetite of collectors. In our excavations we cannot afford to ignore the question of market value, not because we want to sell artifacts but because we want to make sure that others do not. We assume, at least in the Nile valley, that our excavations are probably being watched by local antiquity hunters, who are ready to move in as soon as we move out. Unless we can afford to put guards on our sites, we have to make an extra effort to find and remove anything, which might subsequently attract the activities of treasure hunters.

Symbolic value. Finally, archaeological sites and antiquities can have a meaning for others that is outside the scope of aesthetics, science, or history. They may be associated with powerful religious feelings, with magical powers, with tales of heroes, or even with a national mystique. These factors can offer a strong reason for digging some sites, and for not digging others. They can also legitimately affect our goals and our strategies in digging a particular site.

These various scales of value are obviously not wholly distinct; there is a good deal of overlap among all of them. The important point, however, is that we, the scholarly community, are not necessarily the best judges of any of them except the first. That is why the archaeological policy, which affects the public interest, should never be made by archaeologists alone. It should also involve other kinds of scholars, as well as politicians, who can represent the different facets of the public interest.[14]

Not all of these considerations were made explicit in the planning for the Nubian rescue campaign. Consciously or unconsciously, however, I think that most of them were taken into account, if only because so many people were involved in the planning process. As a result, the campaign succeeded on many levels. It gave us a newly enhanced picture of Nubian culture history; it filled the National Museum in Khartoum with beautiful objects;[15] it gave to the world of art the unique Faras murals; it attracted thousands of tourists to see the reconstructed temples at Abu Simbel and Khartoum; and it gave to the Nubian people a new historical self-awareness. It failed only as science, if we are to accept the judgment of the 'new archaeologists'.

What price science, then? It is the world's good fortune that planning for the Nubian rescue campaign

[14] I have argued this previously in Adams 1973, 832. See also McGimsey 1972, 6-7.
[15] See Plate 2.2 [ed.].

was left to the humanistic vision of Torgny Säve-Söderbergh and his colleagues, and not to the scientific purists of the 'new archaeology'.

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Plate 3.1. Wall Painting *in situ* in the Faras Cathedral during excavations. A Nubian Eparch is shown under the protection of Christ (SARS Adams Archive, ADA S001.08). The painting is now in the Muzeum Narodowe in Warsaw, Poland (no. 234033).

Three Perspectives on the Past: The Historian, The Art Historian, and The Prehistorian[1]
(1987)

The original, much shorter version of this article was delivered orally, and ad hoc, *at the Sixth International Conference for Nubian Studies in Uppsala, Sweden. It was originally the traditional closing commentary ('Schlusswort') for a session of papers devoted to Nubian Christianity. The several eminent scholars mentioned in my text were the authors of previous papers delivered during the session, and it was the differences in perspective among them that suggested the focus of my discussion. At a considerably later date the editors of the published proceedings asked me to prepare a version for publication, and I took the opportunity to expand considerably on the original discussion, to the extent that I could remember it.*

It is obviously not possible, in the compass of a few pages, to comment on the many important points of detail that are raised in the papers of [László] Török and of our Polish colleagues.[2] There are, however, certain general issues that are brought into focus in these papers, and that are appropriate subjects for comment. All of them relate in one way or another to cultural differences—in time, in space, and 'in the eye of the beholder'.

To a certain extent the last of these issues holds the key to understanding the other two. Students of Nubian history, particularly in the Meroitic and Christian periods, will have been aware for some time that there are significant differences of view between Török, the Polish scholars, and myself as to how we should interpret the evidence of the Nubian past that is presented to us by history, art, and archaeology. Our different perspectives may be likened to those of three cartographers who set out to map a largely unknown terrain from three different sets of fixed points. Before proceeding to other issues I would like to consider briefly what is involved, and what is a stake, in our differences of perspective in regard to the past. They are, I would argue, the perspectives, which are characteristic respectively of the fields of history, of art history, and of prehistory.

Perspectives

Historical particularism. Török's perspective, as I conceive it, is essentially that of the historian. For him the fixed datum points that must be used in mapping the past are known events, personages, and dates, as these are preserved for us in textual records. The use of such data is of course not uncritical; the historian has developed many evaluative measures by which to judge the relative accuracy of particular authors and particular texts. Nevertheless, it may be said as a general rule that for the historian the archaeological evidence, no matter how numerically compelling, must always be brought into accord with the human testimony that is preserved in the textual record.

In the historian's view, 'culture' in the abstract is generally less important than are the human individuals who create and express it. History is seen as a concatenation of individual lives, deeds, and events, from which one may derive a certain abstraction called culture. But it is the people, deeds, and events that are the ultimate data. Moreover, the precise timing of events and personages is of critical importance.

Elitist diffusionism. The perspective which is presented by our Polish colleagues, in their numerous

[1] Originally published as: 'Three perspectives on the past: The Historian, the Art Historian, and the Prehistorian (Comments on Session II)', in T. Hägg (ed.), *Nubian Culture: Past and Present.* Stockholm, 285-292.

[2] See further, Török 1987; Jakobielski 1987; Gartkiewicz 1987; Ryl-Preibisz 1987; Martens-Czarnecka 1987; Żurawski 1987 [ed.].

writings on Faras and Old Dongola, is essentially that of the art historian. The fixed points in their map of the past are places: centers of artistic and cultural creativity, which set the pattern for surrounding areas and peoples. They see culture as flowing, in a series of impulses, from Byzantium and other external centers to Faras and Dongola, and from these in turn to the 'hinterlands' of Lower and Upper Nubia.

The art historian's perspective is one in which human individuals are only occasionally important, as when cultural impulses are set in motion by a great ruler or artist. The focus is nevertheless on the overall dynamic of artistic growth rather than on the activities of individual artists. Sequences of development are important, but actual recorded dates usually are not.

Normative periodization. My own perspective, reflected in *Nubia, Corridor to Africa*[3] and many other works, is essentially that of the prehistorian. Our way of mapping the past—almost unavoidably in the absence of recorded events and dates—is that of normative periodization. That is, we divide the continuum of prehistory into a series of discrete phases—often of unknown duration—which are distinguished from each other by different typical forms of architecture, pottery, graves, and other cultural manifestations.

Periodization is not, of course, a device that is unique to the prehistorian. It has also been widely used by historians as a convenient way of making generalizations about particular eras in history. Long before there was any concept of prehistory, scholars were talking in general terms about the characteristics of the Antonine and the Merovingian and the Tudor eras, for example.

The prehistorian's view of the past differs from those of the historian and the art historian in two important particulars. First, it involves what the biologists would call a cladistic view of cultural evolution, in contrast to the more even-flowing conception held by historians and art historians. That is, culture history is conceived as a sequence of 'steady states' (to borrow a term this time from physics), which are separated from each other by very rapid cultural transitions. There is usually little effort to understand why and how the transitions came about, although in an earlier age they were almost uniformly attributed to the migrations of new peoples.

Second and still more importantly, the cultural norms, which the prehistorian establishes for each of their periods are statistical norms; that is, they are based on the weight of numbers. The art historian, on the other hand, will often equate the normative with the ideal. Thus, for example, the Faras wall paintings are seen by our Polish colleagues as the standard which all Nubian church decorators attempted, with varying degrees of success, to imitate. The prehistorian and the normatively-minded historian, however, will consider that the somewhat less elaborate paintings which are found in many provincial Nubian churches represent the true norm, from which the complex Faras paintings and the very simple Abd el Qadir paintings are equally deviations.[4]

So important is the weight of statistical evidence to the prehistorian that it might be said that, for him, numbers are the most important datum points to be used in mapping the past. Cultures and culture periods are defined neither by documentary records nor by the influence of particular artistic centers, but simply by the frequency with which certain traits recur at certain times and places.

[3] Adams 1977; Plate ii, this volume [ed.].

[4] The wall paintings conserved and excavated at Faras are now in the Sudan National Museum (see Plates 2.2 and 3.1) and in the Faras Gallery in the Muzeum Nardowe, Warsaw Poland and much literature has been devoted to them. The paintings from the small late church at Abd el Qadir were conserved by a team from Yugoslavia in 1965, and are now housed in the Sudan National Museum. See further for example, Adams 1977, 466-467; Griffith 1928; Jakobielski *et al.* 2017; Michałowski 1966; 1967; Monneret de Villard 1935, 214-217 and pls. 174-80; Zielińska 2019; National Museum in Warsaw, https://www.mnw.art.pl/en/collections/permanent-galleries/faras-gallery/ [accessed 23.04.2020]; and Palaić and Frelih n.d. https://takingcareproject.eu/article/the-nubia-campaign-international-salvation-of-cultural-heritage-during-the-1960s [accessed 20.09.2021] [ed.].

Chronology

Let us consider now how the aforementioned differences of perspective affect our conceptions and our measurement of time. We must observe first that both the archaeologist and the historian are obliged to keep several different kinds of time, involving different combinations of measurement, continuity, and sequence.

Linear or calendric time involves the continuous measurement of the passage of time from some known, fixed point. It is the only conception of time which combines the three qualities of measurement, sequence, and continuity. This is the scheme by which all of us order our own lives in the present day, and we would like if we could to impose the same kind of order on the past. Unfortunately, the historical and archaeological evidence is rarely sufficient so that we can do so.

Historical time. The record of written history generally provides us with a more or less incomplete series of dated events, personages, and dynasties. These can be arranged in an absolutely reliable chronological succession, and the time between one event and another can be measured, but there is often no continuity in the historical record. It gives us, so to speak, occasional flashes of illumination in an otherwise dark sky. Historical time gives us measurement and sequence, but not necessarily continuity.

Periodic time, as I have already suggested, divides prehistory and history into a continuous series of phases. It provides sequence and continuity, but no measurement.

Sequential time. There are many techniques by which artistic works and archaeological remains can be arranged in chronological order, without necessarily representing a continuum. For example, the archaeological sites of Kerma, Kawa, and Dongola can easily be arranged in a chronological sequence, but they do not collectively span the whole of Nubian history. Sequential time gives us only sequence, without either measurement or continuity.

Returning once again to the differences in perspective between the historian, the art historian, and the prehistorian, I would suggest as a generality that the historian would like to keep calendric time, but is usually obliged by lack of evidence to keep one of the different sorts of historic time. That is, his reconstructions of history and culture history are based on a limited series of fixed and dated points. This means that, in dealing with archaeological evidence, he will often propose rather sweeping cultural reconstructions on the basis of a few datable finds, sometimes ignoring a much larger body of undated material which may seem to tell a rather different story. This is a characteristic that I have occasionally noted in the work of Török, including the paper that is included in the present volume.[5]

The art historian is proportionately more interested in sequence and in continuity than in absolute dates or in testamentary evidence. His thinking, like that of the prehistorian, is to some extent periodic, but he differs from the prehistorian in conceiving of cultural influences as flowing continually outward from particular centers of creativity. To this extent he thinks of time more in continuous or linear terms and less in incremental terms than does the prehistorian.

Unfortunately, the art historian often has a poor empirical control of time, for he may be obliged to work with museum specimens for which there is no secure provenience or dating. In the absence of external bases for dating, he has had to develop techniques of logical seriation; that is, arranging objects in sequence on the basis of what appears to be a logical trajectory of artistic or technical development. It may be said therefore that the art historian often does not find chronological order in his data, but imposes order on it.

The prehistorian's view of past time, as I have already suggested, is essentially a periodic or incremental one. Very often it is based on stratigraphy; that is, the prehistorian's culture periods correspond to the

[5] See Török 1987 [ed.].

actual levels that he has found in their sites. In this respect the prehistorian's reconstruction of cultural sequences often has a much sounder empirical basis than has that of the art historian; it is based on external evidence rather than on internal logic.

At the same time the prehistorian's view, far more than that of the historian or art historian, reduces history to a succession of 'steady states'. This approach has been criticized by many historians for overgeneralization and for ignoring historically recorded dates and events, and it has been criticized by art historians for ignoring the ebb and flow of artistic and cultural influences.

Geographical variation

The intended theme of Session II was *Nubian Christianity: North and South*, but this actually became a secondary issue in many of the papers, and even more in the discussion that they engendered. The most lively and controversial issues which emerged were those of chronology and of perspective, which I have discussed in the previous paragraphs. It is however possible to offer a general comment about the 'north vs. south' issue, again with reference to the different perspectives of historians, art historians, and prehistorians. I will confine my remarks to the Christian period, ignoring the much more controversial pre-Christian periods which are actually the main foci of Török's paper (For these periods it would require a paper as long as his to deal effectively with the many points that are raised).

From the historian's perspective there is a fairly rich documentary record in regard to both Nobatia and Makouria, which makes plain the significant economic and political differences between the two areas in spite of their union under a single king. This evidence is well known and is not controversial. On the other hand, we have far too little reliable evidence about the Kingdom of Alwa to permit any kind of meaningful comparison, based on documentary evidence, between this southern kingdom and either of its northern neighbors.

From the art historical perspective, the Polish investigators have regarded Faras and Old Dongola as more or less equally important centers of influence, respectively in Nobatia and in Makouria. While pointing out certain specific regional differences, they have generally stressed the fact that both Faras and Dongola were subject to the same external influences, which in turn were transmitted to the regions subject to them. Consequently, they perceive relatively little cultural difference between the northern and middle Nubian kingdoms. Once again, there is insufficient evidence to permit the comparison of either Nobatia or Makouria with Alwa.

The prehistorian, with his insistence on statistical norms, would have to argue that there is insufficient evidence to permit any kind of normative comparison between Nobatia, on the other hand, and either Makouria or Alwa on the other. The medieval territory of Nobatia, inundated by the successive Aswan dams, has been investigated with a thoroughness that cannot be approached in the other two areas. We have detailed descriptions of more than 50 medieval settlements of all kinds and periods, as well as churches, monasteries, cemeteries, pottery workshops, and other kinds of sites. Against these we can set only the Polish excavations in the churches at Old Dongola, and the still more limited trial excavations of [Peter] Shinnie and of the British Institute [in Eastern Africa] at Soba.[6] From the prehistorian's standpoint,

[6] See Shinnie 1955. Academic knowledge is still weighted towards Nobatia, although this is gradually changing. In the years that followed the publication of this paper, extensive archaeological work was conducted by the British Institute in Eastern Africa at Soba East (see Welsby 1998; Welsby and Daniels 1991), and at several medieval sites in Upper Nubia, notably Hambukol and Ghazali (Grzymski and Anderson 2001; http://nubianmonasteries.uw.edu.pl [accessed 27.04.2020], as well as those discovered in the 4th cataract during the Merowe Dam Archaeological Salvage Project completed in 2008, though the majority of the latter remain as yet unpublished (see for example: https://research.britishmuseum.org/research/research_projects/all_current_projects/merowe_dam_project.aspx and http://www.sudarchrs.org.uk/fieldwork/merowe-dam-salvage/). Archaeological work remains ongoing at Old Dongola, with numerous publications appearing in recent years (https://pcma.uw.edu.pl/dongola-ancient-tungul/) and a new project entitled 'Urban Metamorphosis of the community of a Medieval African capital city' led by A. Obłuski from the Polish Centre of Mediterranean Archaeology (PCMA), University of Warsaw was initiated there in 2017.

therefore, the question of cultural differences between the Christian north and south is one that must still be 'argued with a shovel'.

Conclusion

To emphasize my points in the foregoing discussion, I have deliberately overstressed and oversimplified the differences between the three perspectives I have presented. My historian, art historian, and prehistorian are of course 'ideal types', in the Weberian sense.[7] In real life, all of us probably partake to some extent of all three of their perspectives in different circumstances. There are, nevertheless, significant and legitimate differences of emphasis among us, which must surely proceed from the sources I have identified. Török will always put the historical evidence ahead of the archaeological in cases where the two seem to be in conflict; the Poles will always view Nubian culture history from the heights of Faras and Dongola; I will always insist that numbers speak loudest of all in determining the importance of evidence. The point on which I wish to conclude is that none of these perspectives is inherently right or wrong, good or bad. They simply represent different ways of mapping the unknown past for different purposes. A topographic map is not inaccurate because it is not a road map, and a road map is not inaccurate because it is not a political map. The utility of these and other kinds of maps depends entirely on what one wishes to learn. The historian wants to know, first and foremost, who did what, when. The strengths of this approach are that it relates to the actualities of human behavior, rather than to the classificatory abstraction of culture; that it deals with known points in time, and that it is based on human testimony. Weakness lies in the fact that the volume of testamentary evidence (at least in relation to Nubia) is small, with the result that a great deal of territory must be mapped from a very limited number of datum points.

The art historian wants to know how, when and where artistic and cultural impulses originated, and how they were subsequently transmitted and transformed from area to area and from age to age. The strength of this approach lies in the fact that it views history as an ongoing continuum of development, and it allows for the historical reality of cultural diffusion. The weaknesses are that it often lacks a firm empirical basis, and it is essentially an elitist view which attributes creativity always to a few elite centers.

The prehistorian wants to know as much as possible about what ordinary people ate, made, did, and thought at any given moment of time. In other words, he wants to formulate something akin to an ethnographic description for each of his successive culture periods, based on a maximum accumulation of evidence. The strengths of this approach are that it has a statistically sound empirical basis, it is comprehensive in taking into account all different kinds of archaeological and historical evidence, and it avoids the elitism of the art historian. The weaknesses are that it forces culture history into a set of chronological pigeonholes, and it takes no account of the influence of particular individuals and events.

What each of us would like to know about the past is probably a reflection, in the end, of what we would like the future to know about us. This is something that is bound to vary from person to person. As long as it does so, we should not expect that we can ever achieve any one common perspective on the past.

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[7] See further, the theories of Maximilian Karl Emil Weber (1864-1920) [ed.].

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STRATEGY

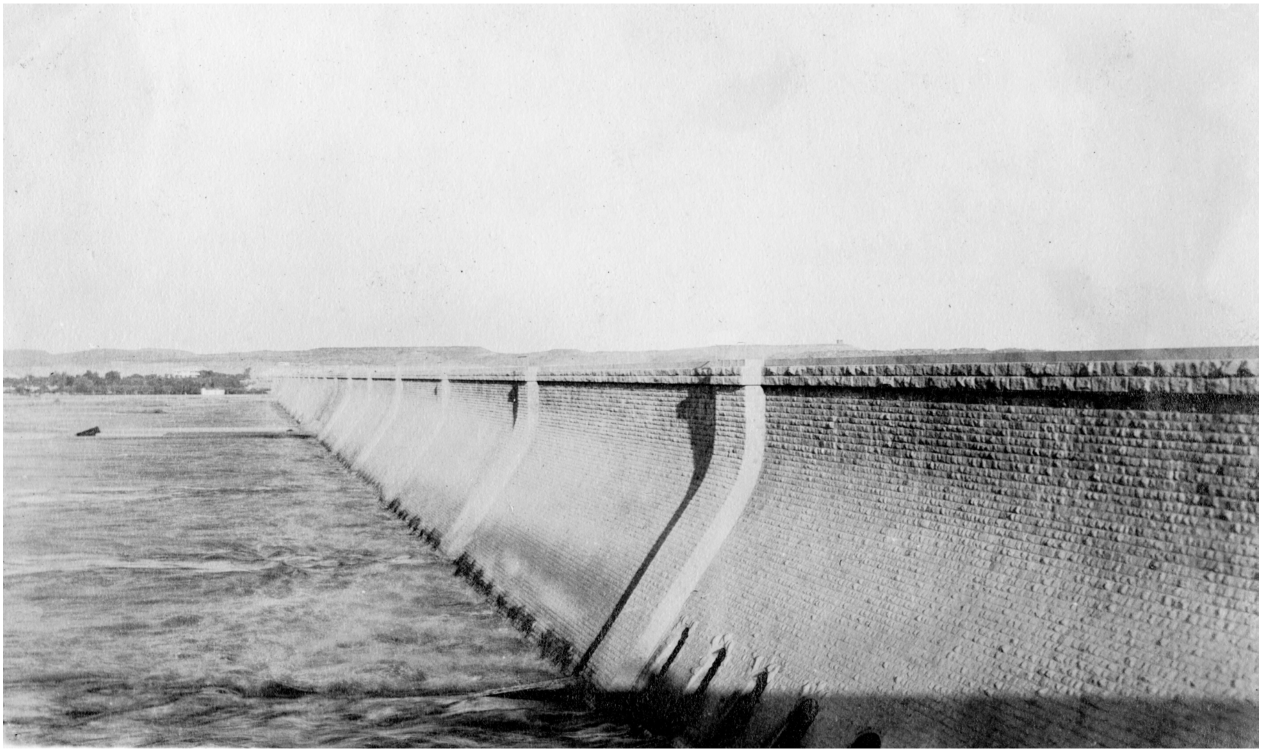

Plate 4.1. Aswan Dam, photo taken *c.* 1916/1917 (from photo album, J. R. Anderson collection).

Strategy of Archaeological Salvage[1]
(1973)

This was an invited paper submitted to a conference on Man-Made Lakes: Their Problems and Environmental Effects*, organized by the Tennessee Valley Authority (TVA) and held at Knoxville, TN. Since I knew that nearly all the attendees would be hydraulic engineers, agricultural extension experts, and others with no background in archaeology, I chose to follow the principle of 'keep it simple'.*

Archaeology can be narrowly defined as the recovery from the earth of the material remains of man and his works. From the study of those remains we hope to reconstruct something of the pattern of the past to learn things about our ancestors and predecessors which historians have not been able to tell us.

It might be argued that all archaeology is a work of salvage. Yet usually it is salvage only from temporary oblivion, not from permanent destruction. The earth, properly considered, is more often a preserver than a destroyer of the record of the past. Over the centuries, far more damage to property has been wrought by man than by nature, and it has long been acknowledged that the safest place to preserve nearly anything for posterity is in the ground. Consciousness of this fact has been a great comfort to archaeologists, for it has freed them from the strictures of moral obligation. They could and did justify any choice of objectives on the ground that whatever they left uninvestigated not only was safely preserved but would provide research opportunities for future generations of archaeologists.

Only when the earth itself is threatened with destruction does it cease to be a safe repository for the remains of the past. That possibility was hardly considered by archaeologists before the twentieth century. Man's technical capacity for destruction was limited, and the supply of antiquities, as of so many natural resources, seemed inexhaustible. Today we know better. In half a century we have witnessed more manmade disruption and alteration of the face of the globe than was wrought by 10,000 generations of our ancestors.

Gradually but inevitably a sense of our responsibility for the consequences of these actions, both to man and to nature, has dawned. It has found expression in all kinds of conservationist movements, including a belated but growing concern for the preservation of archaeological remains. The heritage of the past is now regarded in most nations as a public trust, and the salvage excavation of remains which are threatened with destruction is legally sanctioned and publicly supported as a necessary measure for the preservation of that trust.

The first archaeological salvage campaign in history was carried out in Egypt between 1907 and 1911.[2] It was made necessary by the enlargement of the original Aswan Dam, which inundated nearly 160km of the Nile Valley, including several of its most spectacular monuments. The proposed inundation of the famous temples of Philae, in particular, caused an outcry in scholarly communities of Europe and North America, and aroused the world's attention for the first time to the threat of destruction to cultural remains which is inherent in all development projects (cf. Emery 1965, 35-45).

Since the original Aswan Reservoir was a seasonal rather than a permanent lake, it was ultimately decided that no effort need be made for the conservation of those monuments which could withstand periodic inundation. At the same time, however, it was decided to organize an archaeological survey devoted to the investigation and, where it was possible, the preservation of remains which would be

[1] Originally published in W. C. Ackermann, G. F. White, and E. B. Worthington (eds) 1973. *Man-Made Lakes: Their Problems and Environmental Effects.* Geophysical Monograph Series 17. Washington DC, 826-835.
[2] See Smith and Jones 1910; Firth 1909; 1912; 1915; 1927; Reisner 1910; and Plate 4.1 [ed.].

destroyed by inundation. The survey, originally directed by George A. Reisner of the Boston Fine Arts Museum,[3] was active in the field for four seasons and eventually excavated about 150 sites. It was by far the largest coordinated archaeological program undertaken up to that time.

After the conclusion of the First Archaeological Survey of Nubia, there was to my knowledge no further salvage activity anywhere in the world until a generation later, when a second enlargement of the Aswan Dam made necessary another salvage program in Nubia. The second archaeological survey, active between 1929 and 1934, covered an additional 145km of the Nile Valley and investigated about 75 sites.[4]

The 1930s witnessed also the first major efforts at archaeological salvage in Europe and the United States. In Holland the reclamation of large areas of agricultural land from the sea brought to light archaeological remains which had long been submerged, and the government assumed responsibility for the investigation of these before the reclaimed land was given over to cultivation. In North America the beginnings of salvage archaeology were associated with the Tennessee Valley Authority (TVA) project. Surveys and excavation, mostly directed by the late W. S. Webb,[5] were carried out in each of the major TVA reservoir areas (Brew 1962, 732).

Significant as were the beginnings made in the 1930s, it was not until after World War II that salvage archaeology really came into its own. The destruction wrought by the war undoubtedly created a heightened appreciation for the value of cultural remains, and at the same time the massive reconstruction of cities in Europe, the proliferation of economic development projects in Africa and Asia, and the long building boom in America all provided expanding challenges and opportunities. One of the largest postwar salvage programs was associated with the Missouri Valley Project,[6] which ultimately included more than 100 reservoirs, but there were scores of smaller reservoir projects as well. Some measure of the scope of salvage archaeology in the United States can be gained from the fact that between 1946 and 1957 archaeological, historical, and paleontological surveys were carried out in 310 reservoirs in 42 states. Over 9000 sites were located and recorded, and over four million objects were recovered from excavations (Brew 1962, 16). Meanwhile there were other major reservoir projects on every continent, and in the 1960s the largest of all archaeological salvage programs was made necessary once again by the building of a dam on the Nile. Under the sponsorship of UNESCO, more than 40 different expeditions from as many countries took part in the Campaign to Save the Monuments of Nubia (Emery 1965, 96-100).

In addition to reservoir projects the postwar years saw also the development of right-of-way salvage programs connected with the building of highways, canals, and pipelines. These operations have thus far been confined largely to the United States. In Europe, meanwhile, the stringent antiquities laws enacted by many postwar governments have required that any threatened archaeological remains must be excavated before new buildings can be erected on top of them. Since the great majority of European cities stand upon the remains of earlier settlements, the new laws have led to a multiplication of small-scale salvage projects all over the continent. It is a rare European city which does not play host to at least one or two building salvage excavations each summer.

The past few years have witnessed a tapering off in the volume of archaeological salvage, concurrent with the worldwide building recession. Yet it is still true that salvage programs provide more research opportunities than do all other sources of support combined, and in the aggregate they have probably accounted for three-fourths of all archaeology carried out since World War II. As a result of this development the parameters of the archaeological profession have been altered to an extent which is

[3] Concerning G. A. Reisner see further Bierbrier 2012, 459-460; https://arthistorians.info/reisnerg [accessed 27.04.2020] [ed.].
[4] See Emery and Kirwan 1935; 1938 [ed.].
[5] William Snyder Webb (1882-1964). See Haag 1965 [ed.].
[6] Initially referred to as the Missouri River Basin Project, then as the Pick-Sloan Missouri Basin Program. See https://web.archive.org/web/20090118035708/http://www.usbr.gov/dataweb/html/psmbp.html [accessed 27.04.2020] [ed.].

not yet fully appreciated even by many of its members. The salvage archaeologist has ceased to be a private practitioner and has become in effect a public official with a public responsibility. If his research opportunities are thereby greatly enlarged, it is at the price of surrendering his traditional discretion in the choice of research interests and objectives. To a large extent the decision not only as to how and when to dig, but even as to where to dig, is no longer his.

Criticisms of salvage archaeology

Some archaeologists, like some other social scientists, find the mantle of public responsibility inconsistent with scientific objectivity. Their objections to salvage archaeology echo the traditional attitude of `pure' science toward 'applied' science. They complain that the salvage archaeologist is often compelled to sacrifice methodological rigor, that he has insufficient choice in the matter of research objectives, and that as a consequence his investigations lack any problem focus or orientation (cf. Hole and Hazier 1966, 31-32).

The salvage archaeologist may reply that none of the supposed limitations under which he works is necessarily a disadvantage, at least when they are compared with traditional operating procedures in archaeology. Although he does indeed find it necessary at times to dispense with technical niceties, the quantitative increase in his results usually more than compensates for any qualitative shortcomings. It is also true, as Jennings (1966, 67) has pointed out, that non-salvage archaeology in recent years has developed an excessive meticulousness which often results in a very low return, either in material or in information, per man-hour of excavation. To this development, salvage archaeology has offered a useful countertrend, which will hopefully end in some sort of practical compromise between quantity and quality.

The salvage archaeologist's lack of discretion in where to dig may be viewed as a positive advantage when contrasted with the traditional selection of sites for excavation on the basis of largely nonscientific criteria. It has produced for the first time in history a certain involuntary randomness in archaeological sampling. The results are dramatically attested in such areas as the Colorado Plateau, which for two generations was regarded as the best-known archaeological province in the United States. Yet the Glen Canyon and Navajo Dam projects, along with a number of highway and pipeline salvage projects, led to the excavation of a great many inconspicuous and unprepossessing sites of the type which had been ignored by earlier generations of archaeologists. The result was a substantial enlargement and revision of our picture of prehistoric Indian life in the Southwest (Jennings 1966, 57; Wendorf 1962, 82). There are other areas, like the American High Plains (cf. Jennings 1966, 57) and Nubia, in which virtually all our knowledge of prehistoric cultures has come as a result of archaeological salvage programs.

It is undoubtedly true that most salvage archaeology, like most other archaeology, has been carried out without much regard for theoretical problems. Its objectives have been descriptive rather than explanatory. Yet this too is not always a practical disadvantage. Where a 100% sample is to be taken, as is usual in building and right-of-way projects, it is not necessary to have a sampling strategy. The salvage archaeologist on a highway or building project can always look for problem orientations after the fact, so to speak. His special challenge is to find problems appropriate to his data instead of following the more usual practice of seeking data appropriate to his problems. Either procedure is equally legitimate scientifically. It is only in the larger reservoir salvage programs, where the archaeologist is indeed faced with strategic choices, that problem orientation in advance of excavation becomes a practical necessity.

It must also be recognized that lack of problem orientation, even when it is relevant, is a de facto limitation rather than an inherent limitation of salvage archaeology. In salvage programs, as in most other programs, practical considerations have too often supervened over theoretical ones in the selection of sites for excavation. Yet insofar as archaeology has any potential for explanation as well as for description, it has the same potential in salvage operations as it does in any other context.

My purpose here is not to suggest that salvage archaeology is above criticism, but rather that it has usually been criticized for the wrong reasons. Some of its alleged deficiencies are in fact advantages, others are common to nearly all archaeology, and not one is specifically inherent in the context of salvage. If, nevertheless, many archaeological salvage programs have failed to achieve their full potential, I believe that their failure has been due to reasons nearly opposite to those usually cited. The most legitimate charge to which salvage archaeology is open, it seems to me, is that it has failed to develop distinctive strategies appropriate to its distinctive circumstances. In other words, salvage archaeology can be criticized not so much for its differences from conventional archaeology as for its lack of difference.

This criticism is relevant above all to reservoir salvage programs. For obvious reasons the potential contribution to knowledge of right-of-way and building projects is limited. Such as it is, however, it is usually fully achieved, thanks to a level of funding which normally permits the excavation of all remains encountered. Man-made lakes, on the other hand, present the archaeologist both with challenges and with opportunities on a far larger scale. Neither of these facts has, I believe, been fully appreciated by the archaeological profession, and therefore reservoir projects have not always lived up to their full promise.

Special characteristics of reservoir salvage

River basin salvage stands apart from other kinds of archaeology (both salvage and non-salvage) in a number of important respects. The sheer territorial extent of many reservoirs far exceeds the areas in which systematic survey and excavation are usually carried out. Moreover, since river valleys generally are or have been desirable habitats, there is virtual assurance that a large number of archaeological sites will be found. (By contrast, right-of-way projects often avoid productive areas for the sake of lower land acquisition costs, and as a result there is no a priori probability that important remains will be encountered) Finally, river basins are very often discrete ecosystems, within which patterns of settlement and land use can be understood without reference to larger environmental units. Thus the salvage archaeologist is presented at the outset with a meaningful unit of study; he is not sampling an undefined universe, as he often is in right-of-way and building projects.

Offsetting the scientific advantages inherent in reservoir salvage are certain obvious disadvantages. One, which may not be immediately apparent either to laymen or to archaeologists themselves, is the impossibility of preserving architectural remains *in situ*. When intrinsically valuable or important structural remains are encountered in the course of building or highway projects, it is usually sufficient reason for halting construction and, if necessary, relocating the building or highway on an alternate site. In the case of reservoirs, however, anything that is to be preserved must be either removed or protected with restraining dikes. The physical conservation of monuments for historical or esthetic reasons rather than for purely scientific ones is therefore a special challenge in reservoir archaeology. It is, properly speaking, a challenge to the engineer rather than to the archaeologist, since the expertise necessary to build levees or to dismantle, transport, and re-erect buildings is not part of the archaeologist's normal equipment. Nevertheless, the money for these enormously costly operations and the money for scientific excavations usually come from the same appropriation, and whatever is devoted to the conservation of monuments is therefore in a sense diverted from the conservation of knowledge. Monetary considerations have not been a serious consideration up to now in American reservoir projects, but their importance in the case of Nubia is readily apparent. At least 95% of the funds contributed to the Campaign to Save the Monuments of Nubia were allocated to the dismantling and removal of the Nubian temples, with the result that the amount of 'dirt archaeology' which could be accomplished was necessarily diminished (cf.

Adams 1967, 1).

Even when there are no monuments to be preserved, the level of funding in reservoir salvage projects is rarely sufficient to permit the excavation of all threatened remains. The reservoir archaeologist must therefore make strategic choices among alternate possibilities. In this respect his situation is no different from that of the archaeologist in non-salvage contexts; the latter however can make his choices with the comforting knowledge that future generations may supply his deficiencies and correct his mistakes. For the salvage archaeologist on the other hand there is no tomorrow. He alone among the members of his profession must make not only choices but final choices, in the knowledge that whatever he leaves is left forever. 'After us the deluge' must be his guiding consideration.

Because he must make hard choices, the salvage archaeologist in reservoir projects, unlike his colleagues in building and right-of-way projects, must have a coherent research strategy before he enters the field. Yet because his choices are final, he cannot operate with the kind of strategies which have been characteristic of non-salvage excavation programs. He has to consider the public interest as well as his own interests, and to ask himself not only what he would like to know but what others might like to know, now and in the future. The strategy of reservoir archaeology is therefore a matter not merely of program but of policy; it represents the only instance in which archaeology may perhaps be regarded as a policy science.

Archaeology in man-made lakes, then, demands both a coherent and a comprehensive research strategy, designed to answer as many questions as possible in the time available and with the resources at hand. This is the unique challenge which I believe has not been fully perceived by many salvage archaeologists. Salvage expeditions have taken to the field either with no sampling strategy at all (as is usual and legitimate in right-of-way and building projects) or with a narrowly specialized and non-comprehensive strategy of the kind which is appropriate to non-salvage projects. In these cases, a sense of the importance of what they are doing may blind the archaeologists to the possibly greater significance of what they are not doing. When several expeditions are involved in the same program, as has occurred in some of the very large reservoirs, the problem is further complicated, for each institution is apt to follow its individual preferences on the assumption that its deficiencies are being supplied by others, and that somehow all the various and disparate efforts of the different groups will in the end add up to a meaningful whole. Unless there is a comprehensive strategy for the entire project, however, that hope may be a vain one, as the recent salvage program in Nubia illustrates.

The Nubian campaign as a case study

The Nubian campaign of the 1960s exemplifies both the greatest triumphs and the greatest failures which salvage archaeology can achieve. Not without reason, the general public accounts it an unqualified success. The dramatic and ultimately victorious efforts to save the Nubian temples from inundation excited the world's admiration, and few but the most narrow-minded specialists will quarrel with the scale of priorities which allocated the bulk of available funds and resources to the conservation of monuments rather than to the conservation of knowledge. Even in the latter domain there were nevertheless substantial additions to the understanding of Nubian cultures and history (cf. Adams 1966a; 1967; 1970).

Moreover, when we compare the achievements of the 1960s with those of the 1900s we can hardly fail to be impressed by the progress which has been made in Nubian archaeology in 50 years. The work of the First Archaeological Survey was confined to the excavation of graves and the epigraphic recording of temples (Emery 1965, 35-45). Even the Second Archaeological Survey, a generation later, concentrated largely on cemeteries, although excavation was also carried out in a couple of village sites and two fortresses. There was a concurrent survey of churches and other medieval buildings, but it was not supported by excavation funds (Emery 1965, 46-55). The most recent Nubian campaign on the other hand involved the first comprehensive investigations of prehistoric remains and of rock drawings and inscriptions, as well as much more systematic attention to villages and industrial sites than had been given in earlier years.

None of the refinements in Nubian archaeology can, however, be recorded specifically to the credit of salvage archaeology as distinct from archaeology in general. Wherever excavations are carried out over a long period of time, each succeeding campaign will inevitably benefit by the experience of its predecessors in terms of more precise expectations and more refined methodologies. In addition, archaeology in Nubia and throughout the world has been characterized in the twentieth century by a continually broadening sense of what is worth investigating and what is worth preserving. No longer are we dominated by esthetic and museological considerations; in the words of Daniel (1964, 74) we have 'exorcised the demon of taste'. Concern for the investigation of humble as well as of monumental remains has led inevitably to the development of more precise excavation techniques and more comprehensive recording than were typical of the past. Yet these achievements belong to all archaeology, and are no more conspicuous in salvage programs than in any others.

Individually, the objectives and methods of the various expeditions that worked in Nubia in the 1960s were well up to the high standards of the times. Yet for most expeditions they would have been the same whether the region was threatened with inundation or not. What was conspicuously lacking was a set of special objectives and methods appropriate to the salvage context, and above all a master strategy to which the objectives of the individual expeditions should have been subordinate (cf. Adams 1966a, 161). Because of this strategic failure at the highest level, the purely scientific work of the Nubian salvage campaign must be accounted only a partial success. As Trigger (1970, 347) has recently written:

> Because of the vast amount of archaeological work that has been accomplished in Lower Nubia, it is easy to overestimate how much we know about the cultural history of the region and to underestimate the loss of historical data that has resulted from the building of the High Dam. The reasons for this are clear and mostly understandable. Few archaeologists who worked in Lower Nubia arrived there with specific objectives in mind; their aim was to salvage as much archaeological material as possible before the region was flooded. By the time their research had led them to formulate more detailed problems of Nubian culture history, field work was no longer possible. Because of this, much of the archaeological work done in Nubia has been repetitive rather than problem-oriented and the amount of material that has been collected greatly exceeds its significance.

A policy for the future

The failures as well as the successes of Nubia may yet serve as guideposts for the future. Simply because of its unprecedented magnitude, the Nubian campaign has underscored, far more than any earlier work, the need for a special approach in reservoir salvage archaeology which is commensurate with its special circumstances. While operating strategies must be based in part on the particular challenges and opportunities inherent in each field situation, they must also be based on considerations of public policy which are unique to the salvage context and which remain largely constant from one program to the next. It is because these considerations remain constant that the lessons of Nubia have relevance for the world of archaeology as a whole. In the remainder of this paper I shall discuss what I believe should be the broad strategy guidelines for future archaeology in man-made lake basins, based on my experience both in Nubia and in earlier programs in the United States.

Clearly the overriding need in reservoir salvage programs is for a coherent strategy in advance of fieldwork. Since in many cases not all sites, or even a majority of them, can be excavated, it is essential to have a consistent scale of priorities to determine what is to be done and what is not to be done. The archaeologist's problem may therefore be expressed in terms of classic economic theory: to allocate limited resources among alternate ends on some rational and consistent basis. Accordingly, three questions must be asked at the outset of any salvage program:

1. What are the possible ends?
2. What are the available resources?
3. What shall be the basis of allocation?

Possible objectives must be defined in terms of material available for investigation. How many sites are there, how big are they, how deep are they, how well preserved are they, how are they distributed spatially, and what are their physical characteristics? These questions can be answered only by field reconnaissance. As a basis for strategy development it therefore becomes essential to begin every salvage project with an exploratory survey. Its purpose is not merely to locate sites for later investigation, but to make an accurate descriptive record of every site encountered, so far as this is possible from surface examination. Surface collections should be made of pottery, artifacts, and any other potentially informative material. The information thus gathered will not only serve as a basis for the future selection of sites for excavation; for those sites which are not selected it will be the only surviving record. In areas of heavy overburden (as in Nubia) it may be necessary to carry out a certain amount of trial excavation in the course of survey in order to obtain the minimum necessary information about each site. However, the strategy at this stage of operation should always be to keep excavation at a minimum in order to complete the preliminary survey as rapidly as possible and get on to the next and most critical stage of the program. Obviously, a basis for sampling has not been established until the preliminary inventory of sites is complete.

In a number of recent salvage campaigns the work of field reconnaissance has been aided enormously by aerial photography. Air photos may reveal the existence of sites which are not apparent from the ground, even at close range. In no case, however, do photos provide all the information which the archaeologist will need to record; they are an aid and supplement to rather than a substitute for ground exploration. Their greatest value is often in providing an accurate medium for recording site locations where detailed maps are lacking (cf. Miller 1957; Adams and Allen 1961).[7]

The information recorded for each site must be categorized in terms of a set of characteristics which, taken together, will determine its suitability or unsuitability for further investigation. Gross features such as size, depth, and state of preservation will indicate whether excavation is likely to reveal anything not visible from the surface; chronological and morphological characteristics will suggest which sites show promise of answering important historical and cultural questions. Although recording in the field is often most conveniently done in the form of raw notes, the most important categories of information must eventually be transferred onto standardized recording forms which will serve as a basis for site comparison and selection. Punch cards were once considered ideal for this purpose, but they have of course been wholly superseded by computer programs.

Having defined the size and complexity of his problem in terms of sites to be excavated, the archaeologist must next survey what resources he can bring to bear on them. In salvage projects where he has been given little advance notice, the most critical resource may be time. Inundation schedules will obviously determine not only the final cutoff date for his operations, but in many cases also his sequence of operations. The lower the contour level, the earlier the deadline. Since most sites are located on or close to the banks of the river which is being flooded, those which are farthest downstream are obviously

[7] Recently supplemented by the use of satellite photos, Google Earth images, drone photos etc. [ed.].

the first to be threatened. Where the period of reservoir filling is to be a protracted one, as was the case in Nubia, the variable time factor may in itself serve as a basis for the selection of one site in preference to another (cf. Wendorf 1962, 33-47).

Aside from time, the necessary requisites for excavation are of course trained professional personnel, equipment, and money for the maintenance of field crews and for the employment of unskilled labor. These resources are normally to be found in learned institutions; either in universities or, less commonly, in museums. Under most present-day antiquities laws, specified institutions have automatic responsibility for salvage operations in specified areas. Yet there may be times when the normal resources of an institution are inadequate to the challenge of a salvage project; in these circumstances an effort must be made to increase the available resources either through a monetary subvention from the dam-building agency or some other source, or by enlisting the cooperation of additional institutions. Reservoir projects in the United States ordinarily include a substantial governmental appropriation for archaeological salvage, which will enable the participating institutions to purchase equipment and to employ temporary personnel above and beyond their normal operating levels. In reservoir projects abroad, and particularly in the developing nations, supplementary funds for archaeological salvage have much more often had to be raised through public or private appeals.

In sum, the salvage archaeologist must begin by measuring his available resources against the size of the job and the time allotted. If they seem inadequate to obtain even a minimum sample, he must make every effort to increase his resources through one or more of the channels suggested. When maximum possible resources have been secured, he is ready to estimate the overall sampling level which will govern his operating strategy. What percentage of the known sites in the region can he reasonably expect to excavate with the time and facilities at his disposal? His preliminary estimate will probably have to be modified continually in the light of experience, but, unless some operating figure is kept in mind, he will have no firm basis for the allocation of priorities.

Allocation of priorities, or in other words the choice of what to do and what not to do, is the critical step in strategy formulation. In the broadest terms, the goals of salvage archaeology are the conservation of material remains of the past and the conservation of historical and cultural knowledge of the past. Very often (particularly in the prehistoric periods) these go hand in hand, such that maximization of either result is more or less tantamount to maximization of the other. Yet they cannot always be reduced to a common denominator. In many parts of the Old World there are historical monuments which have long since been thoroughly examined and recorded. Their physical loss would not diminish from our scientific knowledge; yet their cultural, historical, and, if you will, symbolic value is enormous. These values cannot be weighed against scientific values on any rational basis, nor is the professional archaeologist especially competent to judge them. The choice between conservation of monuments and conservation of knowledge, where it must be made, is properly a matter of public policy rather than of archaeological strategy, and it should not be left exclusively to the archaeologist. Ideally, priorities should be established independently and on their own merits in each area, and conservation funds should be allocated accordingly.

Relieved of a policy burden which is not properly his, the salvage archaeologist can concentrate upon his familiar goal of maximizing knowledge of the past through the excavation and, when feasible, the preservation of its material remains. His main challenge is to translate that goal into a practical operating strategy. If he has decided that he can excavate, at a maximum, 40% of the remains discovered in his preliminary survey, on what basis will that 40% be selected? Obviously the archaeologist will not adopt a wholly random sampling procedure except in those rare instances where there are no indications of variability among sites. The caprices of time and nature usually assure a considerable amount of randomness in the archaeological record without the conscious intervention of man. The archaeologist will also not select automatically the largest and potentially richest sites in his inventory, though he will probably eliminate the poorest. He must not, however, run the risk of ignoring the remains of any time period or of any type of human activity about which he might reasonably hope to learn through excavation. In order to avoid that risk, his strategy must be one of categorical sampling.

An initial reduction of the 'sampling population' can usually be made on purely technical grounds. Many eroded and disturbed sites give no promise that excavation will reveal any more than is visible at the surface, and these can be eliminated from consideration at the outset. Such sites may make up as much as 50% of the total inventory; yet, because their aggregate area is usually small, their elimination may not significantly reduce the amount of work confronting the archaeologist. Usually hard choices must still be made after obviously unsuitable sites have been eliminated, and those choices must be made on other than technical grounds.

The two most common and useful bases for site classification are chronology and morphology. Since human cultures vary in time, the remains of any given era are seldom exactly the same as those of earlier and later eras. Even when houses and graves remain the same, the pottery and artifacts found in them are sure to undergo minor stylistic changes in the course of time. Periodically there are more radical changes as well: new house types or new technologies or new arts, resulting from the arrival of new peoples or the transmission of new ideas. As a result, the archaeological sites in most parts of the world can be arranged in sequences of more or less well-defined time horizons, which are distinguished by such things as pottery styles, artifact assemblages, and house forms. When the sequence of 'horizon markers' is known in advance, the job of the salvage archaeologist in ordering his sites chronologically is made easy. When the sequence is not known, but cultural variation in time is suspected, it can sometimes be established in a preliminary way by trial excavations in one or more deep, stratified sites (cf. Adams 1961, 1962). Even when there are no obvious horizon markers, chronological ordering may still often be achieved through the application of such absolute time measures as radiocarbon dating and tree ring dating. Wherever a long sequence of occupation can be established, either by direct or by indirect means, chronology will certainly become one of the important bases for the allocation of priorities in reservoir salvage programs.

Aside from the factor of chronological change, the most common source of variability in archaeological sites is the fact that different types of human activity are apt to be performed at different places. Almost anywhere in the world an immediate and obvious distinction can be made between living sites and burial places. Additional categories which can often be recognized include religious sites, military sites, various kinds of manufacturing sites, and roads and trails. In the investigation of complex societies, distinctions can sometimes be made between various kinds of community sites, as for example family homesteads, villages, and market centers.

Differential activity is not the only source of morphological variation among sites. Sometimes the same environment is shared by two or more peoples, each with distinctive cultural characteristics. In these cases, we may find, between contemporary sites, the same kinds of difference in house types, pottery styles, and so on which we usually find between sites of different periods. On the other hand, when the same people occupy a variety of environments, we are sure to find modifications in house form and settlement pattern in response to local resources and conditions. All of these differences will provide a basis for the classification of archaeological sites and for their categorical sampling.

Briefly, the strategy of the salvage archaeologist should be to excavate sites of all time periods, all peoples, all types of activity, and all ecological zones within the area of his operations. Other things being equal, he should attempt to sample the sites in each category at the same level, i.e., 20% or whatever his resources will allow. In practice, however, other things are rarely equal. Since his objective, properly defined, is to make a maximum addition to what is already known, the salvage archaeologist will begin from a base line of the known, which will vary from period to period and from type of site to type of site.

Failure to recognize this principle has been one of the most consistent shortcomings in large-scale salvage projects, and has resulted in a great deal of work which has reinforced the known without significantly diminishing the unknown (cf. Adams 1960, 19). This is not rational behavior in terms of the defined goal of maximizing knowledge. The archaeologist should allocate his resources in such a way as to insure not that sites of all types and all periods will receive equal attention, but rather that at the conclusion of his efforts the level of knowledge will be about equal in each category. If certain types of sites are already better known than other types will ever be, his efforts should be concentrated entirely on the less well-known sites. The archaeologist may have strong personal opinions as to the relatively greater importance of certain periods or certain types of sites, but those opinions must be overridden by the consideration of public responsibility. Posterity may approach the same material with a very different scale of values.

Some of the largest manmade lakes, like those in Nubia and in the northern Great Plains, have flooded what can be regarded as complete environmental zones. In these areas it has been possible to develop sampling strategies without reference to what may lie beyond the limits of inundation. In most reservoir projects, however, this procedure is not scientifically valid. It is meaningless to ask how much we know and how much we can expect to learn within an area which is artificially and arbitrarily delimited. As a basis for sampling in the smaller reservoir projects, therefore, it is necessary first to delimit some culturally or naturally meaningful unit of territory of which the river basin forms a part, and then to ask how much is known and how much can be learned with respect to the territory as a whole. In the small-area projects an additional variable may affect the allocation of priorities, for the sites within the reservoir area must be seen as parts of a larger universe of sites within the surrounding region. The archaeologist must ask himself, with respect to each time period and each type of site, 'how much can I still hope to learn beyond the limits of the reservoir area?' If certain types of sites are widely distributed both within and beyond the river basin, while others are largely confined to the river littoral, the danger of a permanent loss of knowledge is obviously much greater in the case of those which are confined to the littoral. In Glen Canyon, for example, there were small farming sites of a type which is found all over the Colorado Plateau, and there were also lithic workshops, which are unique to the gravel terraces of the major rivers. These workshops received far more attention than is usual in the course of the Glen Canyon salvage project simply because of the absence of comparable sites outside the threatened area.

I do not suggest that the theoretical guidelines proposed here can ever be fully operationalized. The salvage archaeologist in the field is confronted by many problems I have not touched on in this discussion: problems of logistics, of inter-institutional cooperation, and even of international relations (cf. Adams 1968). On the basis of firsthand experience both in Glen Canyon and in Nubia I can testify to the limiting influence which these factors may set upon the development of scientific research strategies. I feel safe in asserting, nevertheless, that any reservoir salvage project will come closest to realizing its full scientific potential by following the general policy guidelines I have suggested, as far as operating circumstances will permit. In the campaign which I directed in Sudanese Nubia on behalf of the Sudan Antiquities Service, for example, we were obliged by lack of time and resources to combine the survey and excavation stages of operation, and therefore to make strategic decisions on the basis of very incomplete information. Yet, merely by keeping in mind the overall objective of a maximum increase in existing knowledge, we were able to concentrate our efforts on sites of the least-known periods and types, and to revise our strategy on a day-to-day basis as knowledge increased in some areas faster than it did in others (cf. Adams 1966a, 162). Our reward has been, I believe, a much more balanced picture of Nubian culture and history than that which emerged from the earlier salvage campaigns in the same region (Adams 1966a; 1967; 1970).

Critical resource: Organisation

Lack of funds and lack of time will often be cited as sufficient reasons for the failure to develop systematic research strategies in many reservoir salvage projects. Yet it should be apparent that another resource is at least equally critical in large-scale salvage operations. That resource is organization. It too has often been lacking or insufficiently developed in river basin programs. Within the archaeological profession there is no standing organizational structure above the level of local institutions, nor is there any national or international organization which has overall responsibility for archaeological salvage. In the United States there has existed since 1945 the Committee for the Recovery of Archaeological Remains, which has acted as an advisory body to various governmental agencies involved in dam building and other construction activities (Brew 1962, 14-16). Rather similar committees, composed of archaeologists and other interested scholars, were set up to advise the Egyptian and Sudanese governments in the course of the recent Nubian salvage campaigns (Brew 1962, 21-22). These committees, however, have generally contented themselves with insuring that necessary salvage operations were actually carried out, without suggesting what their strategic objectives and procedures should be.

To the extent that overall strategic coordination has existed in large-scale reservoir projects, it has developed informally on an ad hoc, and too often on a post hoc, basis. In both the Glen Canyon and the Nubian campaigns, for example, there were no formal overall strategies because no individuals or institutions were specifically empowered to develop them (Jennings 1966, 34); moreover, both programs were well advanced before informal patterns of communication and coordination among the participating institutions began to emerge.

As a final policy guideline for salvage archaeology I would suggest that strategy without organization is an empty advantage, and that the essential first step in any reservoir salvage project should be to set up a 'high command' responsible for strategy development and resource allocation for the project as a whole. This suggestion runs counter to the cherished individualism of the professional scholar, but it is one of the many sacrifices which the salvage archaeologist should be prepared to make in the public interest. It is the only way to insure an efficient use of all available resources in preserving the heritage of the past.

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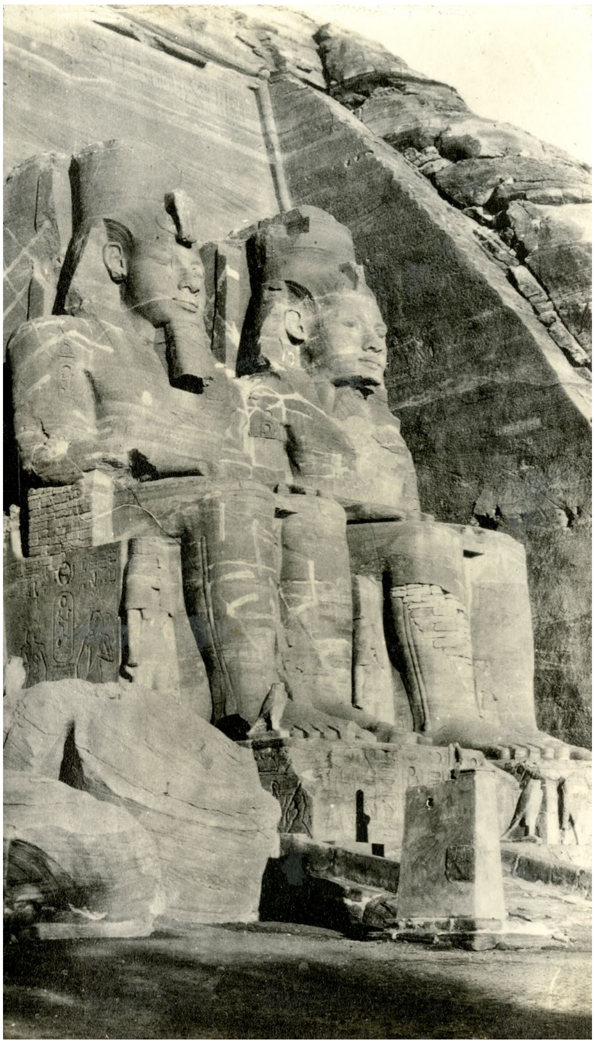

Plate 5.1. Seated colossi at Abu Simbel, photo taken *c.* 1927/1928 (from photo album, J. R. Anderson collection).

Plate 5.2. Friedrich Hinkel overseeing the reconstruction of Semna temple in the garden of the Sudan National Museum, Khartoum (photo W. Y. Adams).

Organizational Problems in International Salvage Archaeology[1]
(1968)

In this paper, originally published in the Anthropological Quarterly, *I discuss the problems of archaeological salvage with reference specifically to the UNESCO-organized International Campaign to Save the Monuments of Nubia. Because this project involved two separate nations, Egypt and Sudan, and because it involved the conservation of previously excavated sites as well as the excavation of new ones, it was very much the largest as well as the most complex archaeological salvage program ever undertaken up to that time.*

The recent and highly publicized International Campaign to Save the Monuments of Nubia is only the latest in a long series of large-scale salvage operations which have virtually revolutionized the field of archaeology since World War II. However, so far as I know this is the first campaign which has involved the coordinated efforts of many different nations, and certainly it is the first in which an international organization has played a major part of policymaking. I refer to the United Nations Educational, Scientific, and Cultural Organization, hereafter designated as UNESCO.

From first to last more than thirty-five expeditions, representing nearly as many countries, carried on excavations in Egyptian and Sudanese Nubia between 1959 and 1967.[2] Inevitably, an effort of this magnitude has produced substantial results in the field of knowledge, and for this UNESCO may claim a fair share of the credit. When the final reports of all the expeditions are in hand, our understanding of the culture history of ancient and medieval Nubia will certainly be enormously enhanced. The organizational problems involved in mounting and coordinating such a campaign were prodigious. My purpose in this paper is not to assess the very considerable advances in knowledge which have accrued from the Nubian campaign, but rather to review the organizational problems which were inherent in its international character, and the successes and failures of UNESCO in dealing with them.

Special characteristics of salvage archaeology

All salvage archaeology, whether on a domestic or an international scale, introduces a major problem of sampling. Sites or portions of sites cannot be selected or rejected for excavation on the basis of special interest or logistic facility; they must be selected in such a way as to insure a maximum contribution to knowledge of the cultures of the threatened area, in all of their manifestations and at all times in history, within the time available. A scale of priorities must be developed so as to minimize duplication of effort and to insure that maximum attention is given to those types of remains and those historical periods which are least understood.

Such a program involves no serious organizational difficulties where the salvage operation is carried out by a single institution, as is normally the case in domestic programs. Where not only many different institutions but a multiplicity of national governments is involved, however, the question of who is to make policy decisions and to develop an overall strategy becomes a critical one. Thus, UNESCO's involvement in the recent Nubian archaeological program became far more vital than was originally foreseen either by the institution itself or by its member states.

[1] Originally published in 1968 in *Anthropological Quarterly* 41, 110-121.

[2] UNESCO International Campaign to Save the Monuments of Nubia (1960-1980) https://whc.unesco.org/en/activities/172/ [accessed 22.04.2020] [ed.].

UNESCO and the Nubian campaign

The decision to sponsor the Nubian salvage campaign came as a surprise to many of UNESCO's friends both within and outside the organization. UNESCO had been chartered in 1946 as a specialized agency of the United Nations specifically to further international cooperation and understanding in the fields of education, science, and culture. Prior to 1959 it had never been directly involved in any archaeological activities, and none of its administrative divisions was equipped to deal with them.

The building of the Aswan High Dam, however, created salvage problems which were far beyond the limited resources of the Egyptian and Sudanese Antiquities Departments. One of the first persons to recognize this was Mme. Christiane Desroches Noblecourt, Director of the Egyptian Department at the Musée du Louvre.[3] Mme. Noblecourt carried on a spirited campaign to persuade UNESCO that the fate of the Nubian monuments was its proper concern, and her efforts eventually won over the then Director-General of the organization, M. Vittorino Veronese.[4]

Perhaps because it was not closely allied to any of UNESCO's established programs, the Nubian salvage campaign was undertaken from the beginning as an extra-departmental and extra-budgetary operation, and financed exclusively from funds contributed for that purpose by member states and other interested parties. Both Mme. Noblecourt and M. Veronese originally envisioned the role of UNESCO largely as an intermediary between the two affected states, Egypt and the Sudan, and other member states and institutions which were asked to contribute their efforts and resources toward saving the Nubian monuments. Money contributed to the Nubian Trust Fund was to be spent partly to publicize the need for action among member states and partly to finance individual salvage operations within the affected region. In practice, the enormously expensive project to relocate the great rock-cut temple of Abu Simbel soon became the chief focus of interest for the whole Nubian campaign,[5] and eventually absorbed a very high percentage of the total funds contributed for work in Nubia.

In view of the limited role which UNESCO itself expected to play in the Nubian campaign, it was decided to involve the organization administratively as little as possible. Rather than create a separate executive department within the secretariat, policy and program decisions were delegated to an Executive Committee of leading scholars from various nations, which met every three months. Day-to-day administrative responsibility for the Nubian operation was lodged in the Museums and Monuments Division of UNESCO's Department of Cultural Activities, because the Nubian operation was conceived almost entirely as a conservation rather than as a research program. The unforeseen demands of the campaign soon came to absorb virtually all of the time and efforts of this office, almost to the ruin of its other projects, but it was not until 1963 that a separate Nubian Operations Bureau was set up within the secretariat.

In the beginning, no field program of any kind was anticipated. UNESCO's participation was to be confined to the headquarters level, transmitting information from the national Antiquities Departments of Egypt and the Sudan to the world at large, and transmitting funds and offers of assistance in the opposite direction. There were, however, two UNESCO officers in the field on somewhat related missions: Louis Christophe in Egypt and myself in the Sudan.[6] The practical need for liaison at the national level between UNESCO and the national Antiquities Departments, and at the field level between the Antiquities Departments and individual archaeological expeditions, was such that Christophe and I, in addition to our

[3] Christiane Desroches Noblecourt (1913-2011) was Chief Curator of the Department of Egyptian Antiquities in the Louvre from 1974-1981. See further Bierbrier 2012, 151-152 [ed.].
[4] Vittorino Veronese (1910-1986) was Director-General of UNESCO from 1958-1961 [ed.].
[5] See further Säve-Söderbergh 1987 [ed.].
[6] See Christophe 1977; Bierbrier 2012, 121-122 [ed.].

normal duties, soon found ourselves devoting a great deal of time to liaison activity. This function was finally given official recognition in 1963 when we were both designated as Liaison Officers directly under the Nubian Office.

Successes and failures of the UNESCO's involvement

Before considering the effectiveness of UNESCO's participation, the archaeological program in Nubia must be seen in its proper perspective as part of a much larger whole. From the beginning the overall Nubian campaign involved two virtually unrelated operations: the removal and reconstruction of endangered temples, and the excavation of archaeological sites. The first of these was an engineering problem, the second a scientific one.

The Nubian Executive Committee quite correctly recognized that the temple removal operation was by far the larger of the two problems in terms of its requirements in organization, money, and material. Many archaeologists complained of this allocation of priorities, arguing that well-known and fully studied temples were of less importance than unknown and unexcavated sites. They ignored the fact that UNESCO's participation in the Nubian campaign from the start was founded on the principle of conservation of cultural treasures and not on the advancement of knowledge. From any point of view, however, criticism of the temple removal program was shortsighted. The continuation of all forms of archaeological research depends in the long run on public support, and in Nubia it was Abu Simbel which captured the public imagination. Had there been no Abu Simbel, the scope of the purely archaeological campaign in Nubia would assuredly have been smaller, not larger.[7]

In the final analysis the temple removal program has emerged as an outstanding success and has vindicated UNESCO's effort. Of the thirty-two temples originally threatened with destruction, every one has been safely dismantled and moved out of danger, and many have already been reconstructed.[8] After many trials and vicissitudes, funds for the complete restoration of Abu Simbel on higher ground were assured and the work itself is has been achieved. We have to realize, then, that when we turn a critical eye on the purely archaeological program in Nubia we are actually considering only a small portion of UNESCO's total involvement, most of which has been eminently successful.

The Nubian Executive Committee was a distinguished international body of scholars, including several experienced field archaeologists. Nevertheless, the nature of their responsibilities compelled them to devote the bulk of their time to engineering, financial, and administrative problems. Perhaps as a result of this overriding concern, no very clear picture of the special needs and problems of the archaeological campaign ever emerged from their deliberations. Instead, there was a tendency to regard the archaeological program merely as an extension of the conservation program: to define the problem in terms of excavation of a maximum number of sites and recovery of a maximum number of artifacts. The Executive Committee did draw up a list of priorities for excavation, based more on the promise of material reward than on the extension of historical knowledge. Above all, no clear-cut concept of sampling was ever developed in spite of the obvious fact that only a fraction of the several thousand threatened sites in Nubia could be systematically investigated.

Insofar as there was any explicit strategy in the Nubian archaeological program, it was formulated independently at the national level by the Egyptian and Sudanese authorities. Different approaches to the problem emerged in the two countries. In Egypt, the entire threatened area was divided into territorial segments of about equal size; participating expeditions were invited to select one or more segments

[7] As happened with the Merowe Dam Archaeological Salvage Project which accompanied the construction of the Merowe Dam in Sudan. See Plate 5.1 [ed.].

[8] See Plate 5.2 [ed.].

and then to investigate all of the important antiquities of every period and type within them. However, those expeditions concerned with prehistoric remains were allowed to range over very large areas and to exercise a selection of sites consistent with their normal operating strategy. In the Sudan, where the Antiquities Service was itself prepared to take an active part in field work, foreign expeditions were permitted to select individual sites or groups of sites to suit their special interests, while the Antiquities Service assumed responsibility for all those not chosen by others.

Since in practice the integration of the archaeological campaign was primarily at the national level, it might have been expected that UNESCO would make a major effort to establish liaison between the two governments involved. In fact, however, this must be accounted one of the signal failures of the Nubian campaign. Although the Directors of Antiquities of Egypt and of the Sudan were both ex officio members of the Nubian Executive Committee and met together with other members of the committee every three months, no serious effort was made to bring about a coordination of strategy or even to provide for regular consultation between the two countries. From first to last the archaeological programs in Egypt and the Sudan were carried out as if they were unrelated. This lack of liaison also affected such practical matters as the duty-free passage of goods and the free flow of personnel between the two countries, and was keenly felt by those in expeditions which attempted to work on both sides of the border.

UNESCO was considerably more successful in maintaining liaison between the individual governments of Egypt and the Sudan and the donor governments and institutions which contributed to the Nubian campaign. At first, the principal role of the organization was expected to be that of a formal intermediary, establishing initial contact between donor and recipient government. In practice this function was never fully developed because the eventual participants in the Nubian archaeological campaign were mostly those institutions already experienced in the field, and they therefore had long-established contacts with the respective Antiquities Departments. Consequently, their initial dealings with the recipient governments frequently bypassed UNESCO altogether.

Nevertheless, a need soon arose for continuing, practical liaison between individual expeditions and the host governments both at the field and at the national levels. This had not been clearly foreseen either by UNESCO or by the governments themselves. Louis Christophe and I, although originally posted to the field on quite different missions not supported from Nubian funds, soon found ourselves required to fill the liaison role simply because we were the only UNESCO officers on the scene. This development reflects not so much a lack of communication between expeditions and host governments, as the fact that expeditions as well as governments clearly expected UNESCO to provide a measure of leadership and guidance in matters of policy. Although Christophe and I had no official authority to provide such leadership we were eventually given recognition as Liaison Officers, which gave us a sort of low-level ambassadorial status.

In general, our dealings with the respective Antiquities Departments of Egypt and the Sudan were highly successful. A genuine concern for the proper investigation of the Nubian antiquities provided a common motivation for governments, expeditions, and liaison officers alike, so that practical negotiations were generally conducted in an atmosphere of good will and mutual trust. However, our dealings with government bureaus other than the Antiquities Departments were notably less successful. In the absence of a common ground of interest, we simply lacked the status or the backing to act effectively. Thus in Egypt important documents and maps which were controlled by the Survey Department and the Army could not be obtained for the use of archaeologists, although they had no military significance and would have facilitated the progress of field work enormously. Moreover, for long periods of time it was impossible to obtain from the Irrigation Department projected schedules of inundation which were of vital importance in allocating excavation priorities. In the Sudan, difficulties were encountered in persuading the Customs Department and the Government Railways to honor existing commitments in regard to the duty-free importation of excavation material and its transportation at special rates.

If, as its charter states, UNESCO's primary function is the furtherance of international understanding and cooperation, then one might plausibly argue that the organization's overriding concern in the Nubian archaeological campaign should have been the maintenance of communication and cooperation between individual missions working in the field. As a matter of fact, no effort was ever made in this direction. Probably, there was a feeling that professional organizations and scholarly journals provided adequate media for the exchange of information and ideas. A more practical knowledge of the professional structure of archaeology would have shown that this was far from the case. In fact, a great deal of unnecessary duplication of effort resulted from the lack of communication particularly between expeditions working on opposite sides of the Egyptian-Sudanese frontier. The only conference which was held specifically to discuss scientific results during the entire eight years of the Nubian campaign was held at Bellagio, Italy, in 1964, sponsored by the Society for American Archaeology.[9] All of those who participated will agree that it fulfilled an acute need, and was considerably overdue.

Factors limiting UNESCO's participation

In sum, the part played by UNESCO in organizing and coordinating the Nubian archaeological program was far more limited than many archaeologists could have wished. Several reasons for this are worth considering, in respect to UNESCO's possible future involvement in international salvage operations. Some of the same limitations would have applied equally to any other institution in the same situation; others are inherent in the constitution of UNESCO; still others resulted from a lack of perception which could and should be corrected.

Foremost among limiting factors is the question of money. The decision to make the Nubian program an extra-budgetary operation meant that from the start the entire cost had to be met from funds specifically contributed to the Nubian Trust Fund by member states. For most states these contributions were necessarily small since they were an extra burden added to their regular budgetary assessments for membership not only in UNESCO but in the parent UN, WHO, FAO, UNTAO, and other specialized organizations to which they might belong. Moreover, UNESCO early (perhaps injudiciously) staked so large a part of its reputation on the salvation of Abu Simbel that in the long run this single project swallowed up a very large part of the Nubian Trust Fund. Consequently, UNESCO's ability to contribute financially to other aspects of the campaign inevitably suffered.

Another limiting factor, not fully appreciated by many archaeologists, is the touchy question of national sovereignty. UNESCO is not an international government but only a loose voluntary association with no coercive powers over its members. Thus many simple policy decisions which can be established by executive fiat at the national level can only be brought about by delicate diplomacy at the international level. Under the UN and UNESCO charters there was no possible way in which Egypt and the Sudan could have been coerced into adopting a common excavation strategy, or even into accepting the Nubian campaign at all. Had they so desired, those countries could have allowed the Nubian antiquities to go under water without lifting a finger in response to protests from other nations. Under these circumstances whatever cooperation and coordination are established must clearly depend upon diplomatic persuasion and friendly discussion, maintaining always a careful respect for national interests and national sensibilities.

Such a consideration would affect any institution attempting to organize archaeological salvage on an international scale, but UNESCO is at least well aware of the problem and has developed a certain skill and

[9] See Spaulding 1965 [ed.].

cunning in the arts of diplomacy. At the same time, one could also argue that the organization's excessive regard for the sensibilities of its members--particularly its smaller members--sometimes results in a failure to exercise leadership where leadership is clearly required and would in fact be welcomed by all concerned.

There are additional peculiarities in the constitutional structure of UNESCO which limit its involvement in action programs such as the Nubian campaign. The overriding principle of nonintervention in the internal affairs of member states means that the secretariat can deal officially and directly only with national governments. Any relations with private institutions or individuals must be conducted through the intermediary of the national government. By the same token, UNESCO cannot take any initiative in purely national matters. If a need for action is perceived in one of the member states, the government of that state must be persuaded to propose a course of action and then to request UNESCO's assistance. Needless to say, this process may require a good deal of behind-the-scenes diplomacy.

In the case of archaeological programs, there exists a further limitation in UNESCO's table of organization, which is largely accidental. All regular UNESCO program activities are administered through the five so-called program departments: Education, Natural Sciences, Social Sciences, Cultural Activities, and Mass Communications. Over the years each of these departments has come to define its own rubric in fairly rigid terms, in such a way that certain fields of activity do not clearly fall within any one department. One of these is, quite explicitly, archaeology or at least salvage archaeology, which does not fully qualify either as social science or as a cultural activity according to UNESCO's operating definitions. This will go a long way to explain why the Nubian campaign began and remained an extra-departmental program with little initial support within the house, and why after eight years it has left almost no mark upon the organizational structure of UNESCO.

Unbelievable as it may seem, UNESCO has never employed a professional archaeologist as such at the headquarters level, either in the Nubian Office or in any other operation. Although as the Nubian campaign developed it became necessary to employ a professional engineer to ensure the maintenance of proper standards in the dismantling and reconstruction of temples, no consideration was given to the employment of an archaeologist in a parallel capacity. Thus, UNESCO as an organization has never truly understood scientific archaeology as distinct from conservation.[10]

Lack of organization in professional archaeology

The blame for this situation does not lie entirely at UNESCO's door. Archaeology, in the general sense in which we are now speaking, does not understand itself--nor has it, in fact, any sense of common identity at all. The Nubian campaign brought together in a common endeavor Americanists, European Prehistorians, Egyptologists, Classicists, Byzantinists, Arabists, Africanists, Physical Anthropologists, and a host of ancillary specialists--scholars whose intellectual backgrounds and interests are so diverse that they hardly speak the same language even when they attempt to. There has never existed a professional association to which even a majority of these specialists could belong, nor a journal which they could all read with profit.

UNESCO, lacking any previous experience in this field, is hardly to be blamed for not realizing that archaeology is not an organized discipline, but an adjunct to many different disciplines which normally have no common meeting ground. So far as UNESCO was concerned, the failure to provide for coordination among these different disciplines stemmed simply from a failure to recognize the need for it.

[10] This article was written prior to the 1972 UNESCO Convention concerning the Protection of World Culture and Natural Heritage, the World Heritage Convention (1975) and the creation of the World Heritage List (1978); however, this assertion remains largely true. See further Meskell 2013 [ed.].

The ultimate responsibility for any lack of communication, and to a large extent for a lack of coordination also, lies with the archaeologists themselves. As the principal interested parties, theirs was the responsibility for recognizing that involvement in Nubia provided them with a common interest transcending traditional boundaries of scholarship and organization, and created needs which were not satisfied by a conservation program alone. Rather than waiting for UNESCO to provide leadership and guidance they should have taken the initiative in organizing themselves to deal with the challenge of Nubia. They could have formed an interdisciplinary board of strategy, drawn up a plan of campaign, arranged for the prompt exchange of results, and then presented UNESCO with a plan and a program which would unquestionably have been welcomed.

Considerations for the future

In retrospect it is clear that (1) UNESCO's involvement in the Nubian archaeological program came about as much by accident as by design; (2) the decision to participate found the administration largely unprepared to provide effective guidance; and (3) the part played by UNESCO in purely archaeological operations was considerably larger than originally foreseen by the organization itself, but much less than was expected by many archaeologists. At this point we are entitled to ask: was UNESCO the proper organization to undertake responsibility for archaeology in Nubia? If so, can its effectiveness be increased in future international operations?

The answer to the first question would appear to be: yes and no. Yes, because UNESCO is the only organization in existence with the size, the prestige, and the diplomatic experience to organize archaeology on an intergovernmental scale. In this respect it performed services and accomplished results which no other institution could have achieved. No, because UNESCO is constitutionally prevented from taking the initiative in many policy matters, and because archaeology plays no part in its regular program.[11]

These two considerations should indicate the proper course for future international action in archaeology. On the one hand, the continued involvement of UNESCO in such programs is vital for success in intergovernmental liaison. On the other hand, professional archaeologists will have to take much more of the initiative in matters of strategic planning and scholarly collaboration. Future salvage campaigns should involve a partnership of UNESCO and an international and interdisciplinary organization of archaeologists representing all of the nationalities and all of the interest groups involved in the threatened area.

The archaeologists themselves should take the first step in forming such an organization. They should retain responsibility for communication and liaison among themselves, including regular scientific conferences and the publication of a journal or newsletter. In return, they might legitimately ask of UNESCO:

1. That it set up a permanent headquarters bureau, staffed in part by professional archaeologists, to handle matters of intergovernmental liaison in salvage archaeology programs.
2. That it set up temporary national liaison offices in each of the affected states for the duration of any particular campaign.
3. That it request the Antiquities Departments of each of the affected states to maintain field liaison offices, perhaps assisted by UNESCO personnel, in those parts of the country where extensive salvage work is in progress.
4. That it maintain a clear distinction throughout its program between problems of conservation and problems of research, and deal with each on its own merits.

Finally, if archaeology is to become a worldwide discipline, the archaeologists will have to learn to

[11] See footnote 9 [ed.].

accept their share of the delays, disappointments, frustrations, and misunderstandings which are inevitably a part of the contemporary international scene. These are, after all, only another aspect of the 'culture shock' of which we so often speak, and which we are apt to regard as a necessary crisis rite in the evolution of a finished anthropologist. Once this painful but necessary threshold is crossed, archaeologists are sure to find, as ethnologists long since have, that the rewards more than compensate for the ordeal.

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Plate 6.1. Kulubnarti, site 21-S-2. View of the 'Castle-house' from the south, taken 2014 (photo courtesy N. Spencer).

Plate 6.2. Qasr Ibrim, photo taken in 1986 (Qasr Ibrim archive, QI. 86, TB4/2).

Ends and Means in Large-Scale Excavations: Meinarti, Kulubnarti, and Qasr Ibrim[1]
(1995)

This was my paper delivered at the 8th International Congress of Nubian Studies, held at Lille in 1994. The quadrennial Nubian Congresses were meant first and foremost for the reporting of recent excavations, but at this point I had done no excavation for a decade, so I originally declined the invitation to contribute. As one of the founders of the Nubian Society, however, it was always expected that I would participate, and the organizers suggested that I compare my two earlier digs at Meinarti and Kulubnarti. My subsequent excavations at Qasr Ibrim had however presented entirely new problems, not previously reported, and this was something I was anxious to discuss also.

The invitation to speak about Meinarti and Kulubnarti is one that I would ordinarily have declined. These digs are more than a generation old, and they are already pretty well on record (Adams 1964; 1965; 1968; 1970; 1994b).[2] As it happens, however, the topic will allow me to discuss certain ideas of mine about archaeological methodology, and I intend here to approach it from that perspective. For good measure, I will throw in Qasr Ibrim as well.

The archaeologist of today can, if he or she wishes, choose from a considerable number of 'how-to-dig' manuals that have been published within the last two decades. These works however have been written mostly by and for prehistorians, working on a small scale in small sites. They have very little relevance to the kind of large-site excavation that most of us do in Nubia--operations that would simply be trivialized if we adopted all of the prehistorian's methods.

The worst feature of the excavation manuals from my perspective is that they ignore altogether the question of purpose, implying that there is only one right way to dig anything, regardless of the wishes of the excavator or of the sponsor or of the host country. Excavation undertaken in this spirit becomes an end in itself -- a mere exercise in technical virtuosity --rather than a means to the more intellectually legitimate end of enlarging knowledge or testing theory. Such an approach cannot be justified even on scientific grounds, and in large sites it is a practical impossibility as well.

There is not, and probably never can be, such a thing as a guidebook to large-site excavation. No two sites are the same in terms of their possibilities and limitations — not only scientific but also practical, political, and personal possibilities and limitations. For that reason, I believe that every large-site archaeologist is necessarily self-taught, and self-taught anew in each site that he or she undertakes.

In Nubia I have had the opportunity of working in a number of large sites and in three exceptionally large ones: Meinarti, Kulubnarti, and Qasr Ibrim (for the locations see Figure 6.1).[3] The circumstances and the challenges were quite different in the three cases, and each required different adaptations on my part. I propose here to review these three digs, in order to demonstrate how excavation methods may be

[1] Originally published in F. Geus (ed.), *Cahier de Recherches de l'Institut de Papyrologie et d'Egyptologie de Lille. Acts de la VIIIe conférence internationale des Études nubiennes*, *Lille 11-17* septembre 1994, 17 (1), 37-55.

[2] For books concerning these sites that appeared after this article was published see Adams 1996; 2000; 2001; 2002; 2003; 2009; 2013; Adams and Adams 1998; Adams *et al.* 1999; Aldsworth 2010; Alexander and Adams 2018; Connah and Pearson 2016; Crowfoot 2011; Hallof 2014; 2015a; 2015b; 2015c; 2016; Łajtar and van der Vielt 2010; Rose 1996; 2007; Ruffini 2014; van der Vielt and Hagen 2013; Veldmeijer 2012 [ed.].

[3] See also Plates 6.1 and 6.2. Figure and plate numbers used here have been modified from those used in the original publication through the addition of the chapter number from this volume; however, the figures and plates themselves remain the same as in the original article. In the original publication plates were numbered sequentially throughout the entire volume thus the first plate in this article was Plate 3a (not Plate 1), herein labelled Plate 6.3a, and Plate 8 was the final plate. The plate captions have been added and were not included in the original article. Plates 6.1 and 6.2 were added for this publication [ed.].

determined by circumstances and purposes: in short, how means are dictated by ends.

Initial considerations

The large-site archaeologist begins with the realization that, with the best of intentions, he or she cannot possibly do everything. Therefore, strategic choices are required at the outset. The considerations that may affect those choices fall into four broad categories:

Theoretical or historical considerations. What is it that you want to learn by excavating any particular site? This will be determined in considerable part by how much you already know, since there is usually no point in wasting scarce funds on an excavation that merely reconfirms the known. Legitimate theoretical or historical objectives may nevertheless be of several kinds: they may be historical, cultural, theoretical, or aesthetic. That is, you may dig to enlarge your understanding of the development of a particular culture over time, or to enlarge understanding of the culture at one particular moment in time, or to throw light on general theoretical questions, or simply to recover objects for study and display. Each of these is a legitimate goal; each may call for a somewhat different excavation strategy.

Practical considerations. How much digging, and what kinds of digging, will your funds, time, and personnel allow? Do you have qualified personnel to deal with burials, texts, and other remains requiring specialized expertise? What will the logistic situation allow: are there opportunities for on-site photographic developing,[4] laboratory processing, and the like? What will the site itself allow, in terms of its extent, conditions of preservation, nature of the deposits, etc.?

Political or institutional considerations. First, what do your institutional sponsors want; are they anxious to have objects for display, or monumental finds for publication, or are they content to sponsor work merely for the advancement of knowledge? Second, what does the host country want, and what will it allow? How much of the excavated material can you take out for study at home? Finally, what is the attitude of the local population, whose past you are exhuming? Are there certain areas or remains whose excavation would be offensive to them?

Personal considerations. Contrary to what the textbooks may suggest, personal preferences may quite legitimately affect your choice of sites to dig and of excavation strategy. The past does not speak to us all in the same way, and, within the limits of professional responsibility, all of us will prefer to dig what specially interests us. Since I was trained as an ethnologist, I have always been interested first and foremost in the lifeways of ordinary folk, both in the present and in the past. As a result, I prefer townsites to cemeteries, I am more interested in architecture than in artifacts, and I am more interested in domestic than in monumental architecture. Also and most importantly, I am more interested in discovering recurrent culture patterns than in tracing the course of historical events. But others have legitimately different interests, and will make their excavation choices accordingly.

Now let me turn to the three large sites of Meinarti, Kulubnarti, and Qasr Ibrim, and describe how my excavation strategy was determined by the combination of theoretical, practical, institutional, and personal considerations that were peculiar to each site.

Meinarti

Meinarti was, before its inundation, a small island just at the foot of the Second Nile Cataract, a few kilometers south of Wadi Haifa (Figure 6.1). Much of the island was occupied by a stratified *kom* approximately 150m long, 80m wide, and 12m high at its highest point. Before excavation it appeared as a mound of sand with bits of mud brick wall protruding through at various places along the top and sides (Plate 6.3a). The topmost elevation was occupied by a gun emplacement built during the years of British

[4] This being a consideration as the sites under discussion were excavated before digital photography [ed.].

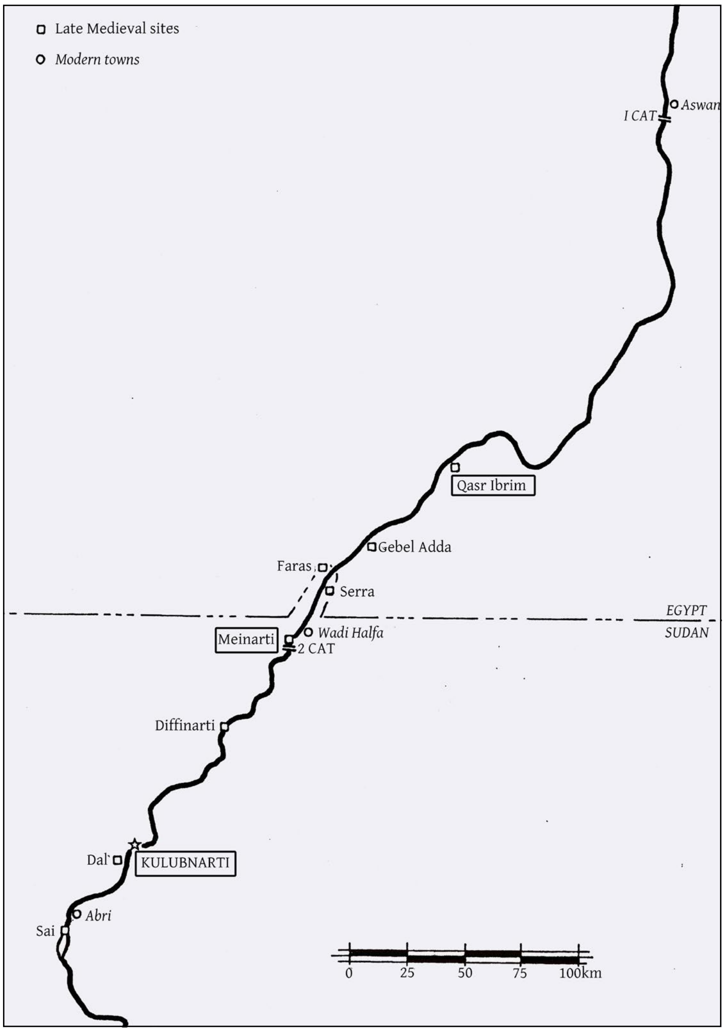


Figure 6.1. Map of Lower Nubia and the *Batn el-Hajjar* showing locations of Meinarti, Kulubnarti, and Qasr Ibrim.

military occupation in the 1890s; what lay below that was, at the outset, quite uncertain.

The circumstances under which I came to dig at Meinarti have been related elsewhere (Adams 1992, 17-18), and will not be repeated here. Suffice it to say that in February 1963, when I began excavations, I knew that I had only the remainder of that season and all of the next one to complete whatever could be done on that enormous site, since the impounded Aswan Dam waters were scheduled to reach this point in the summer of 1964.

Assuming that the deposits within the Meinarti mound were stratified, and that the stratigraphy could be revealed by careful excavation, I made two strategic decisions at the outset. The first was to limit my excavations entirely to the southern and higher half of the mound, having calculated that my resources would allow me to dig that much of the site all the way to the bottom. The second was to avoid trenching or pitting and to strip the site systematically downward by levels, like peeling an onion, following natural stratigraphy in all cases. This was dictated in part by the belief that, according to my excavation plan, everything would disclose itself in due course. There was however the further, practical consideration that nearly all the infill deposit at Meinarti consisted of fine windblown river sand, which would not hold any kind of a vertical profile.

Working with a crew of 150 to 250 men over a total period of eleven months, I did just manage to reach the bottom of the southern half of the mound — on June 11, 1964. As expected, the site proved to be very clearly stratified, with seven major building and rebuilding episodes extending from the later Meroitic to the early Post-Christian periods; that is, from perhaps 100 to 1600 AD. Within each of the major building episodes it was possible to recognize two or more stages of modification or growth, so that in all a total of 18 levels were separately designated and mapped. Throughout, the remains were very largely those of mud brick dwellings, though at certain levels there were also, a church, an extensive cemetery, and — at the very bottom — a Meroitic market compound and a wine press,[5] all of which were quite unexpected.

The remains

The major architectural features of the various levels are illustrated in Plates 6.3 and 6.4 and are planned in Figures 6.2 and 6.3; they need only be briefly summarized here: *British occupation* (Level 1; late 19th century) was represented by a level platform on the top of the mound, which had served as a gun emplacement. In

[5] See further Adams 1966 and *contra* Bishop-Wright 2019 [ed.].

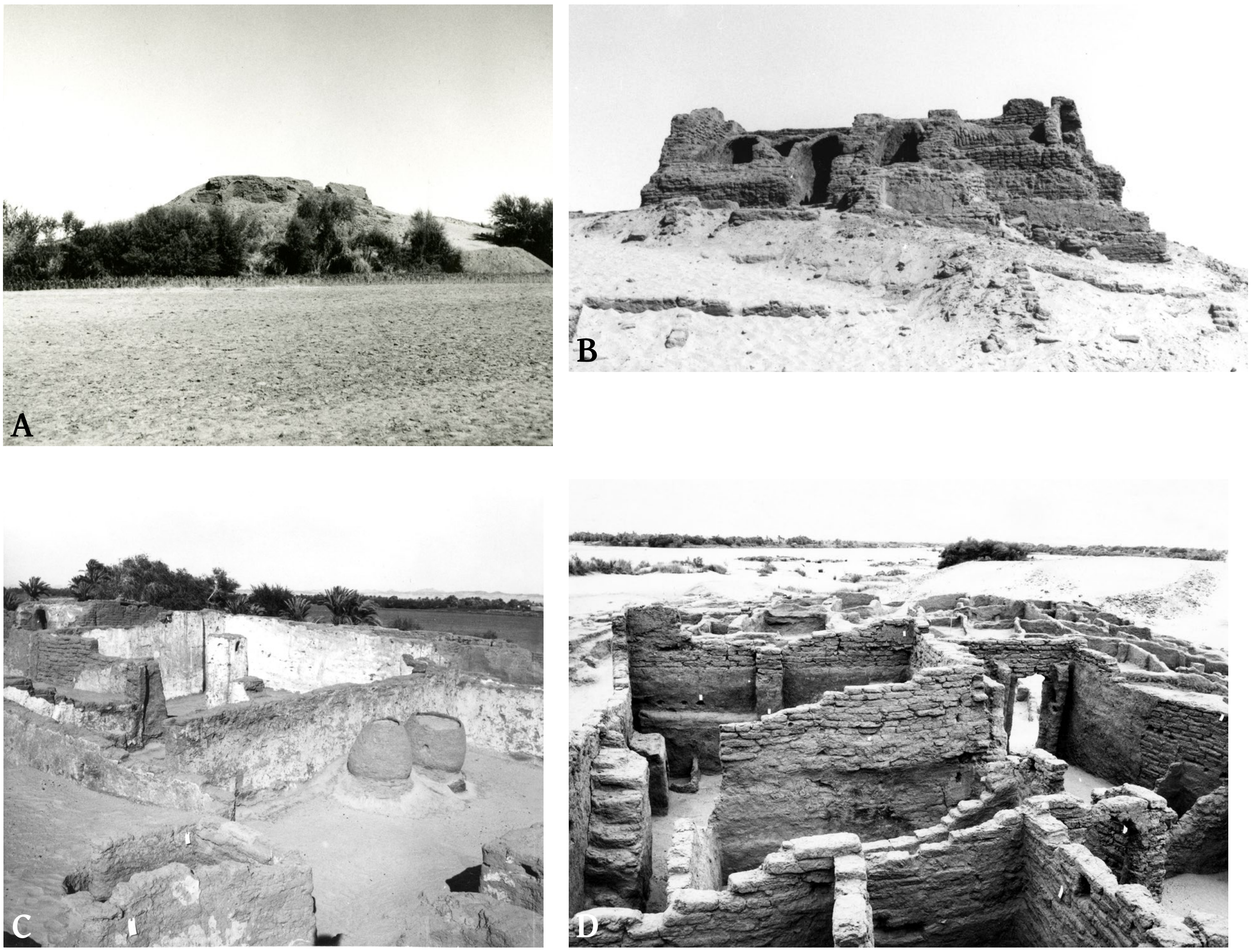


Plate 6.3. Meinarti. a. Mound before excavation, view from the south; b. Two-storey castle-house, from the west; c. Southern part of the site with contiguous cluster of rooms, looking northeast; d. Later Classic Christian period dwellings, view from the north.

the *Terminal Christian and Post-Christian* periods (Levels 2-3; *c.* 1350-1600 AD) the most conspicuous feature was a massive, two-storey castle-house (Plate 6.3b), though there were also a few smaller houses that had remained in use from the preceding period (Figure 6.2a). From the *Late Christian period* (Levels 4-6; *c.* 1150-1350 AD) there were several ordinary houses in the northern part of the excavated area, while the more southerly part was occupied by a contiguous cluster of rooms that may at times have served as an administrative center (Figure 6.2b, Plate 6.3c).

Remains from the *Later Classic Christian* period (Levels 7-9; *c.* 1000-1150 AD), comprising a tight cluster of dwellings, were the best preserved on the site (Plate 6.3d). The houses were more spacious than were those of any other period, but were rather flimsily built, with thin walls, many of which were party walls shared by two houses (Figure 6.2c). *The Earlier Classic Christian* remains (Levels 10-11b; *c.* 900-1000 AD) presented a marked contrast to those overlying. They were preserved only on the topmost part of the mound and were in very poor condition, the walls rarely standing to a height of more than 25cm (Plate 6.4a). This was evidently due to episodes of major flooding that had occurred during the tenth and eleventh centuries. The houses had been mostly very small and flimsily built (Figure 6.2d). Remains of the *Early Christian* period (Levels 12-13; *c.* 600-900 AD) were also poorly preserved, though not quite so much so as those of the overlying levels. They comprised a tight cluster of houses, some with curving and others

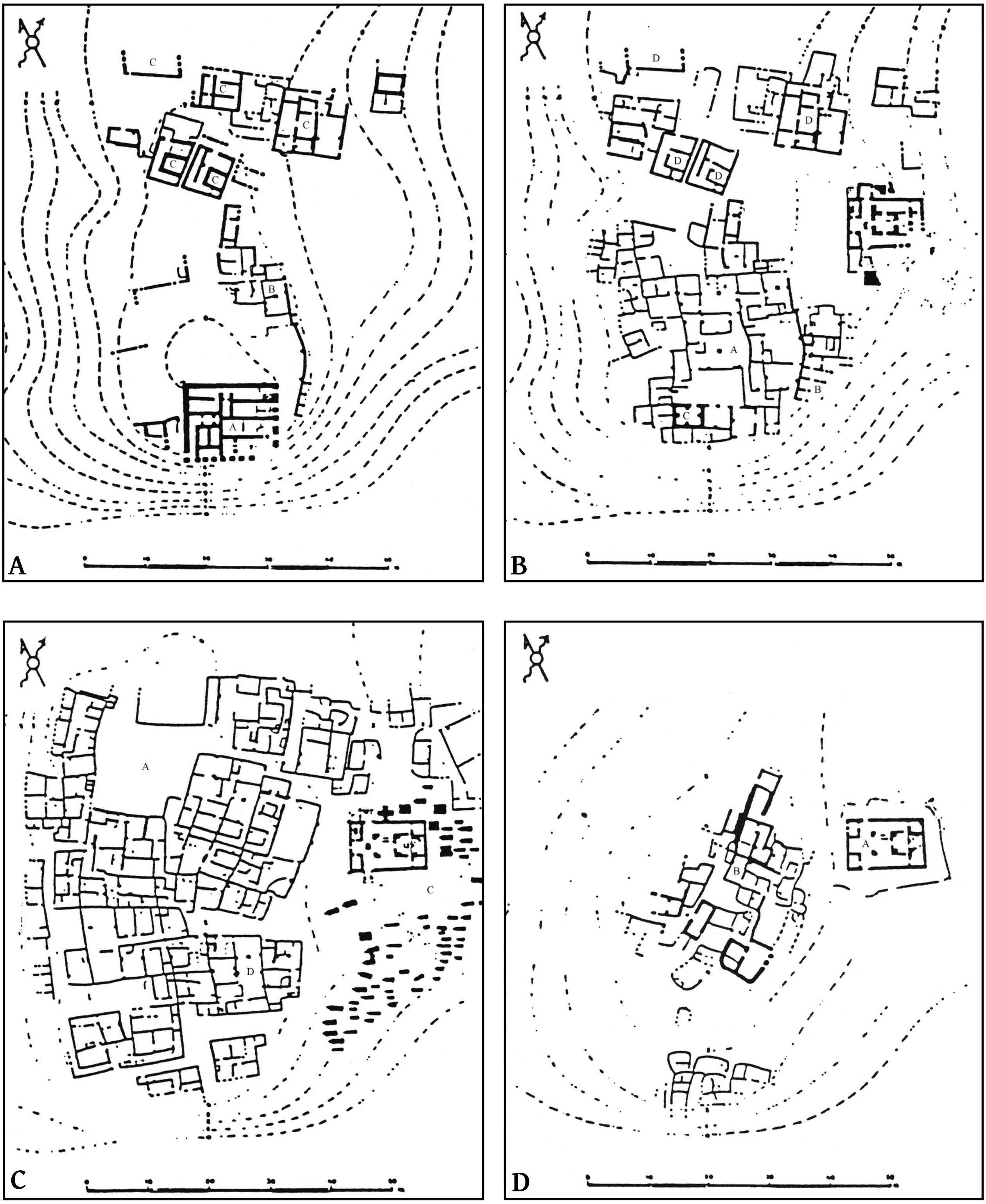
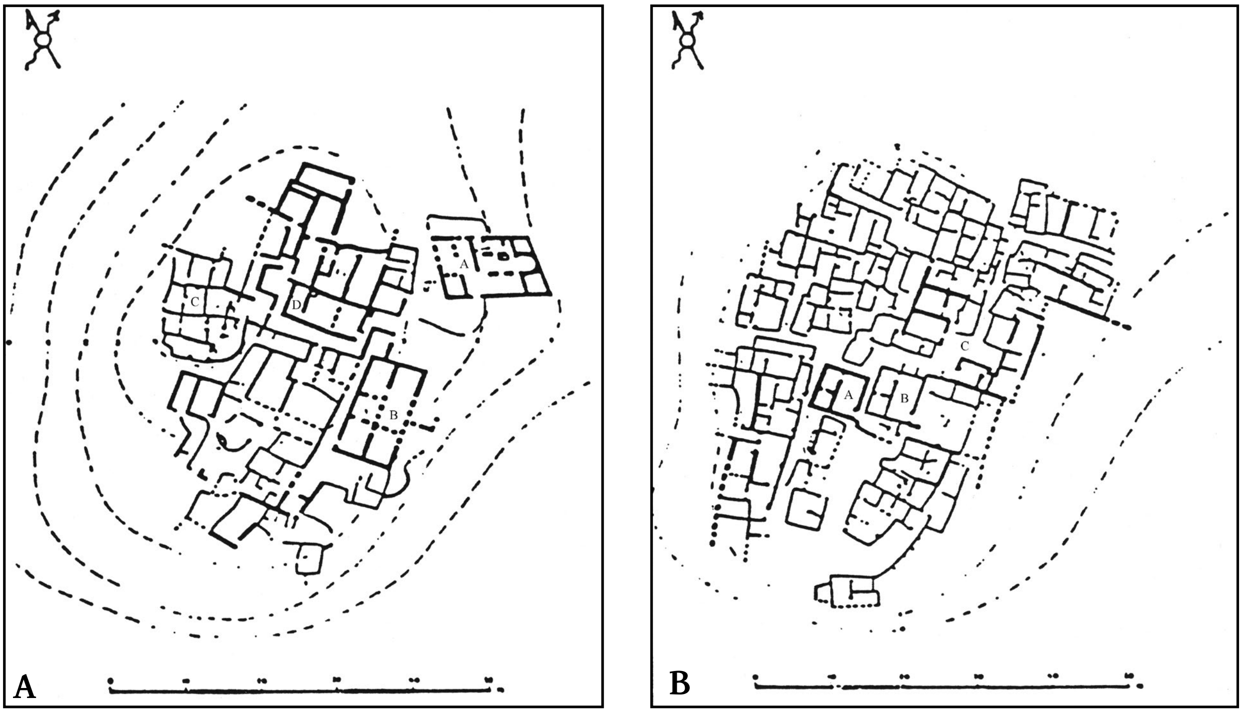


Figure 6.2. *Meinarti*: plans of upper excavation levels. a. Terminal Christian (Level 3). b. Late Christian (Level 5). c. Later Classic Christian (Level 8). d. Earlier Classic Christian (Level 11b).

with straight walls (Figure 6.3a).

Rather surprisingly, the *Ballaña* (X-Group) levels (Levels 14-16; *c.* 350-600 AD) were far better preserved than were those immediately overlying, with house walls in most cases standing to a height of at least a meter (Plate 6.4c). The plan at this period shows a tight cluster of very small houses with thin walls, leaning against each other somewhat like a house of cards (Figure 6.3b). Remains of the *Meroitic* period



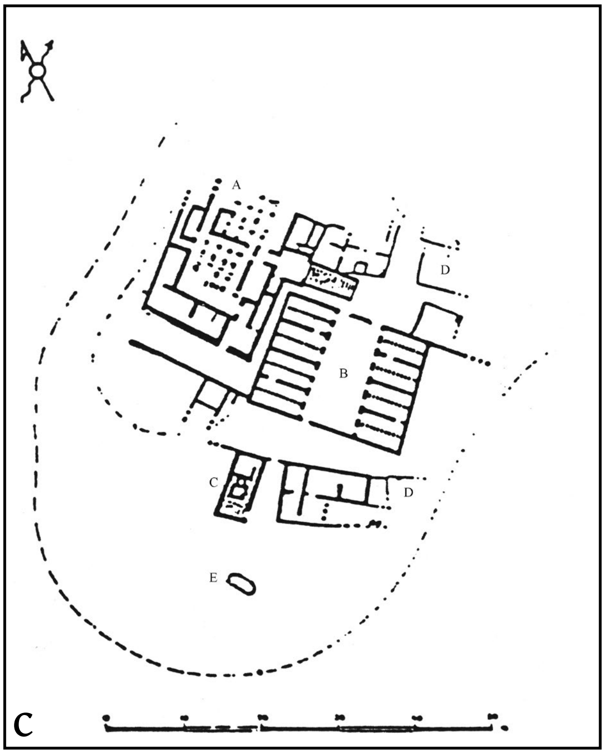


Figure 6.3. *Meinarti*: plans of lower excavation levels. a. Early Christian (Level 13). b. X-Group (Level 15b). c. Meroitic (Level 18).

(Levels 17-18; *c.* 100-250 AD?), unlike those of all later periods, were mostly not those of dwellings. They included the very denuded foundations of what may have been a large storehouse, a walled compound that was probably a collection of shops, and a wine press (Figure 6.3c; Plate 6.4d). These buildings rested directly on clean and sterile river sand.

A retrospective overview

Objectives. Given the fact that Meinarti was a stratified habitation site, the overriding considerations were, first, to work out the sequence of development as carefully as my resources would allow, and second, if possible, to get to the bottom of the mound in the season-and-a-half that were left to me. I had however a further specific objective: to work out a developmental sequence for the later Christian pottery wares. I was at this time still developing the comprehensive medieval pottery typology with which most readers are now familiar (Adams, 1967-68; 1986). As of 1962 I had not yet defined the later Christian wares or worked out their chronology (cf. Adams 1962, 276-285); fortunately, the Meinarti excavations enabled me to do both. That is, by carefully following the natural stratigraphy, and segregating the pottery groups accordingly, I was able to 'use the site to read the pottery'. This was a critically important step because it meant that, in later excavations at Kulubnarti and Qasr Ibrim, I could 'use the pottery to read the sites'.

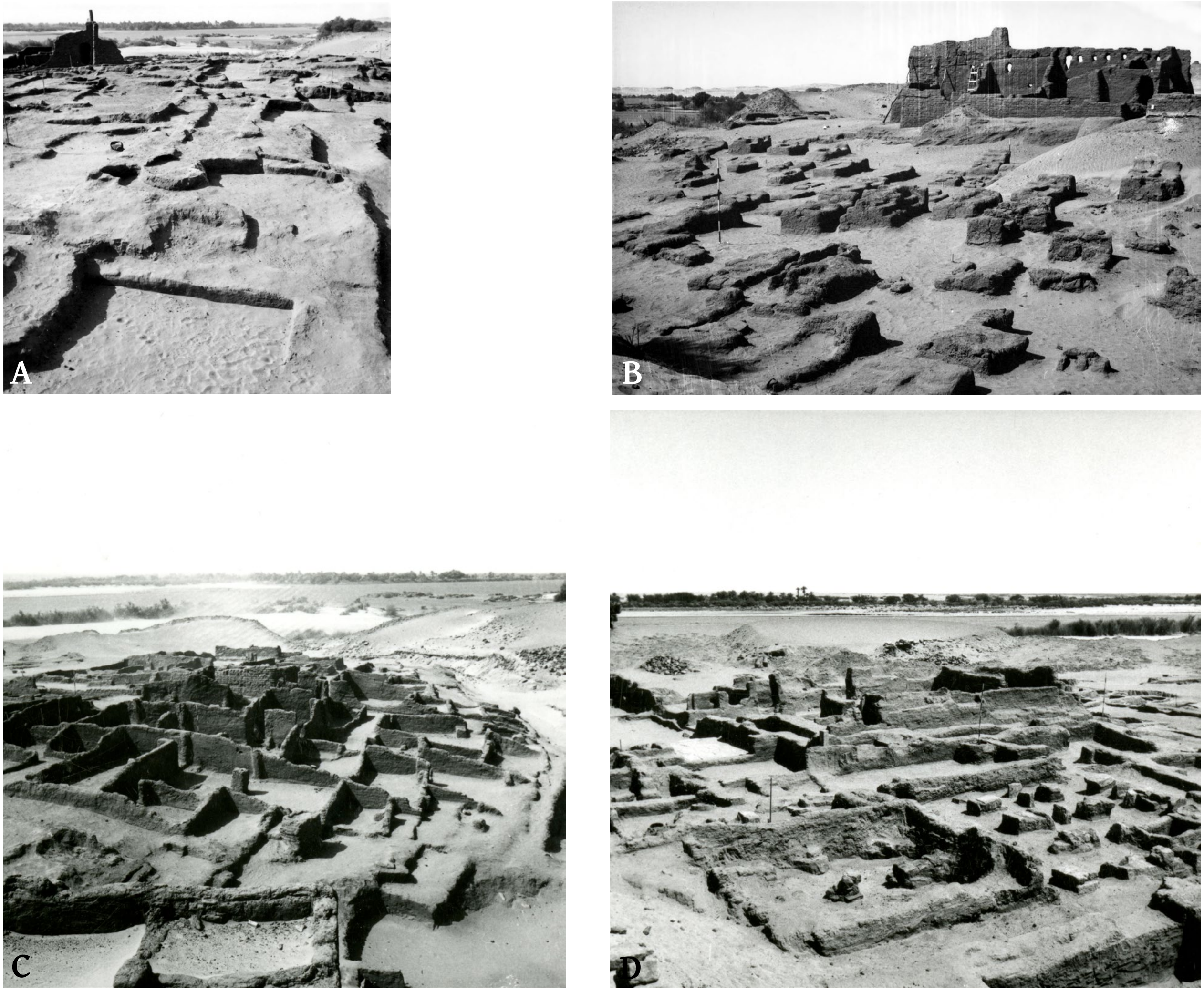


Plate 6.4. Meinarti. a. Top of the mound with walls in poor condition, view from the north; b. Cemetery with over 300 burials, view from the east; c. Ballaña level house walls, view from the north; d. Meroitic remains including a storehouse, walled compound and a wine press, view from the north.

Problems. In the Meinarti excavations I faced immediate and obvious problems in terms of the limitations of time and personnel, and the absence of on-site laboratory facilities. Added to this were the nature of the soft sand deposit, and the unexpected discoveries of a church and of another building with some preserved fragments of murals; also of a complete cemetery with more than 300 burials that had to be excavated and studied (Plate 6.4b). I was fortunate however in that the Polish team at Faras loaned me an expert[6] to copy and to conserve the murals, while the University of Colorado team[7] at Gezira Dabarosa volunteered to take on the complete study of the skeletal remains in the cemetery.

Advantages. If the Meinarti dig involved a number of specific problems, I had also certain advantages. First of all, I had the unlimited and unstinting support of my sponsors, the Sudan Antiquities Service. In addition, and thanks again to my sponsors, I was relieved of all logistic and administrative responsibilities: The Antiquities Service engaged, housed, and paid the laborers as well as providing my own housing and transport. The result was that I could devote my time 100% to archaeology – a luxury that I have not

[6] Józef Gazy (1910-1998) conserved the Meinarti wall paintings, and led the team who conserved, restored and removed the wall paintings in the field at Faras [ed.].
[7] The University of Colorado team was led by George W. Hewes [ed.].

enjoyed in any subsequent excavation.

Further advantages included the help I was able to receive from colleagues in other expeditions, as previously noted. The buildings at Meinarti were entirely of mud brick, which is generally much more regular and easier to map than is stone architecture, and the well-defined and continuous stratigraphic levels were easy to follow from building to building throughout the site. Finally, Meinarti – unlike any other dig of mine in the Sudan – was located within commuting distance of Wadi Halfa, where we had a home with electricity and running water. Certain documentary jobs could therefore be done in the evening under electric light, and I could also take advantage of the lab facilities and the photo darkroom that we maintained in the town.

Methods. As previously mentioned, my basic method was to limit myself to one half of the Meinarti mound, and to strip away the deposits from that area level by level, until the bottom of the mound was reached. I was fortunate in being able to dump the excavated spoil in any direction except to the north, and indeed by the end of the dig the Meroitic remains were surrounded by spoil dumps that were higher than the site itself. In excavating any particular level my basic procedure was to proceed from the center of the site outward while actually clearing the buildings, so that the laborers would not be passing back and forth over finished work, and would not have to step over or around high-standing walls. When dismantling the remains after a level had been completed, on the other hand, we worked always from the outside inward. Mapping, recording levels, and photography were of course continuous on a day-to-day basis, and each day's accumulation of potsherds was generally sorted and counted in the afternoon after the dig.

Successes. The most signal success of the Meinarti dig was surely the fact that I was able to work out the stratigraphic sequence clearly, and that I did reach the bottom of the mound. The cartographic and photographic records are generally excellent; there are detailed plans of each of the 18 stratigraphic levels, and several hundred photos. Unexpected successes included the discovery and complete excavation of the Meinarti cemetery, the study of the skeletal remains, the discovery, recording, and partial recovery of murals in the church, and the discovery of two large caches of complete pottery vessels of Late Christian types that had been buried under house floors.

Failures. In a sense, the greatest failure of the Meinarti dig was that I was able to deal with only half the mound. This might be considered an advantage if the unexcavated northern half the site remained available for investigation in the future; as it is, the world will never know what further treasures might have gone under the waters of Lake Nubia. I have to acknowledge also that the vertical cartographic record is not as complete as the horizontal. With my limited time resources I was able to record levels only along eight transect lines, rather than individually for every wall and floor.

Although animal bones were plentiful in the refuse deposits, I did not systematically collect them, since we had at the time no facilities for their study. Finally, and unfortunately, my available time allowed me to collect and to quantitatively record only potsherds of the decorated wares, and not of the much more common utility wares. As a result, my ceramic statistics from Meinarti are not fully comparable to those from Kulubnarti and Qasr Ibrim, where I collected and counted all sherds.

Leitmotiv. The single factor at Meinarti which played the largest part in determining my excavation strategy was clearly the stratigraphy. In that respect the dig remains unique down to the present day: no other Nubian site so far excavated has exhibited the same comprehensive and uninterrupted layering, offering as it does a conspectus of domestic architectural development during more than a millennium.

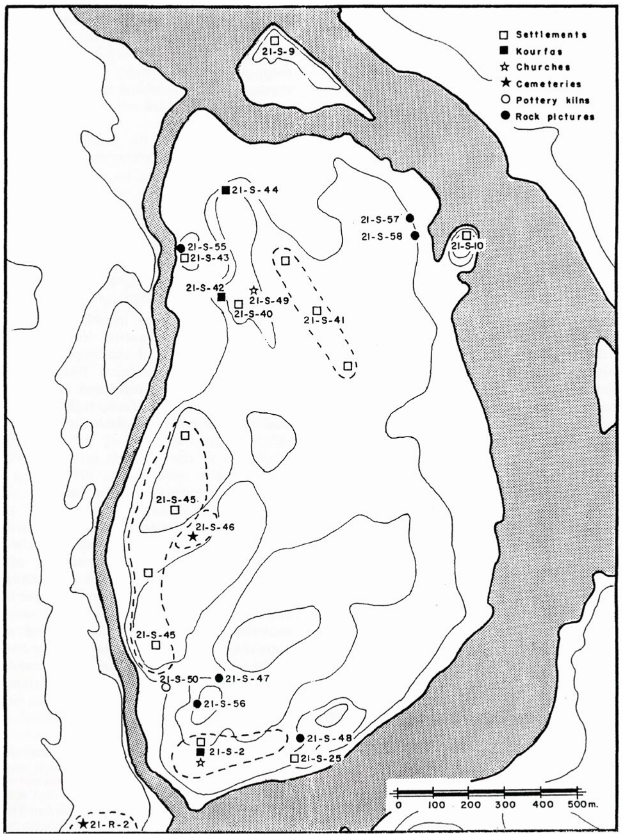


Figure 6.4. Map of Kulubnarti Island showing locations of sites excavated or recorded by the 1969 expedition.

Kulubnarti

Kulubnarti is the name of an island, about 2km long and 1km wide, situated in the heart of the Batn el-Hajjar region, 130km upriver from Wadi Halfa (Figure 6.1). At the island's southern extremity there was a cluster of ruined buildings of late medieval date (Site 21-S-2), which had obviously been occupied both in Late Christian and in Post-Christian times. I asked for a concession to dig this site in 1969 because of my feeling that the transition from Christianity to Islam was one of the least-understood episodes in Nubian cultural history (cf. Adams, 1977, 579-580). My concession however included the whole island of Kulubnarti, and we found after arrival that there were 19 other sites on the island in addition to 21-S-2 (Figure 6.4). Under terms of the concession, all of them required some attention on our part.

The project was sponsored by the University of Kentucky Department of Anthropology, under a grant from the U.S. National Science Foundation. Work at Kulubnarti was carried out between January and April, 1969, with a force of 118 laborers. In addition to myself and my wife and two sons, we were accompanied also by four graduate students from the University of Kentucky,[8] who did some of the excavation supervision and recording, and all of the object photography and registration. I had, in addition, the invaluable assistance of Fritz Hinkel[9] in mapping all the major buildings at the end of the season, and of Sergio Donadoni[10] in copying the numerous inscriptions in the Kulubnarti church and several of the houses. In the end, the four largest Kulubnarti townsites were excavated virtually in entirety, and some degree of excavation or recording was undertaken at the 16 other sites as well.[11]

The remains

Site 21-S-2 was the main focus and the *raison d'être* for the Kulubnarti excavations. It comprised altogether 71 houses, scattered somewhat haphazardly along a rocky terrace overlooking the Nile. The buildings were of many different types and clearly of different ages; five were fortified two-storey castle-houses which had evidently been occupied both in the Late Christian and Post-Christian periods (Plate 6.5a-b; cf. Adams, 1994a). One had been enlarged, in the Post-Christian period, with the addition of a walled courtyard and a watchtower, to become a giant *kourfa* (Plate 6.5a; see Vila 1979, 71-120). Also included was a very small church, situated on a terrace below the main site. It was evident however that there was virtually no depth to the archaeological deposits at 21-S-2, and no stratigraphy. The terrain at the site was extremely rocky and uneven, with many of the houses resting directly on boulders (Plate 6.5a-b). The pattern of

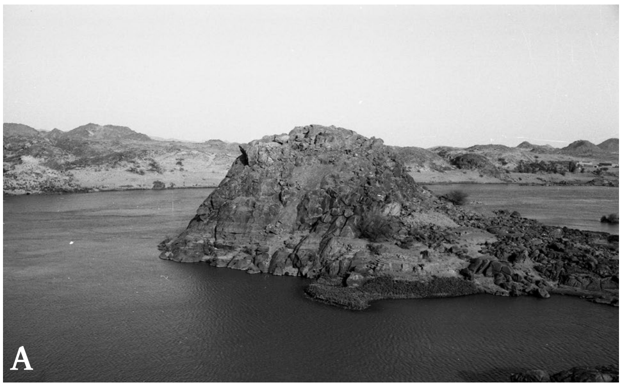
[8] Thomas E. Higel, Sandra T. Higel, Frank B. Fryman and Mildred L. Fryman, information courtesy Mrs N. K. Adams [ed.].
[9] Friedrich Wilhelm Hinkel (1925-2007), see Yellin and Adams 2007 [ed.].
[10] Fabrizio Sergio Donadoni (1914-2015), see Hölbl 2006 [ed.].
[11] Definitive reports have been published on all of the architectural remains (Adams 1994b), all of the artifacts (Adams and Adams 1998), and two large cemeteries (Adams *et al.* 1999) [W. Y. Adams].

Plate 6.5. Kulubnarti. a.-b. Late Christian and Post-Christian two-storey castle-houses at site 21-S-2; c. Wall painting from small church at site 21-S-2 (photos W. Y. Adams).

development had been basically horizontal, with newer houses built alongside older ones rather than on top of them. 'Using the pottery to read the site' therefore became a key consideration at Kulubnarti, and the ceramic sequences obtained from the Meinarti excavation were vital.

Site 21-S-10 comprised a scattering of mostly small and irregular houses, perched at the top and sides of a rocky *jebel* (Plate 6.5b). The site was of interest because it had witnessed two widely separated periods of occupation, respectively in the later Classic and in the Post-Christian periods, with a long hiatus in between.

Site 21-S-9 was similar to 21-S-10 in its topographic setting (Plate 6.6b),[12] but similar to 21-S-2 in its history. That is, it had been continuously occupied from the Late Christian period until perhaps a couple of centuries ago. The remains included one



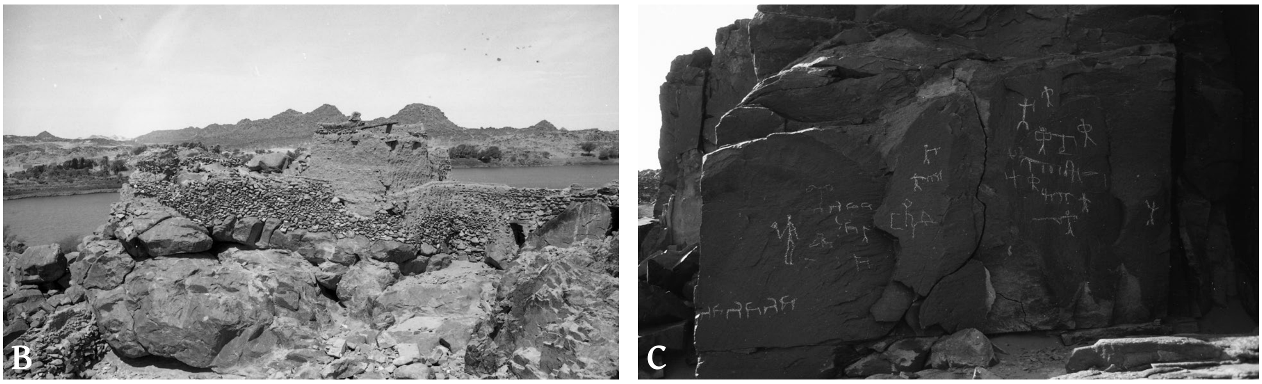


Plate 6.6. Kulubnarti. a. Site 21-S-9; b. Late Christian castle-house at site 21-S-9. c. Panel of rock art.

[12] And Plate 6.6a, included but not referred to in the original text [ed.].

two-storey castle-house (Plate 6.6b) and 19 other structures.

Site 21-S-25 was a walled settlement of seven houses, datable entirely to the post-Christian period. None of the other Kulubnarti sites were investigated with the same thoroughness as were those just named, but all were mapped, photographed, and described in notes, and at least some test excavation was done in all of them. Of special interest were three large *kourfas*, dating probably from the 17^{th} or 18^{th} century. There were also six rock picture sites, of which one panel is shown in Plate 6.6c.

A retrospective overview

Objectives. The overriding objective of the Kulubnarti excavations was to throw as much light as possible on the cultural transition from Christianity to Islam at the end of the Middle Ages. No specific or minimum amount of excavation was foreseen as necessary for this purpose; it was simply a matter of 'the more the better' — i.e. doing as much as possible within the constraints of time and funding. Under the terms of my concession however I was responsible for investigating all the remains on the island, so that a necessary minimum of excavation and recording in all the sites was a mandated objective.

Problems. The problems at Kulubnarti were fundamentally different from those at Meinarti. To begin with this was my first experience of 'running my own dig' in Nubia rather than working for the Antiquities Service, so that I had to accept all the usual responsibilities and headaches of the dig director: finding laborers, housing and paying them, dealing with disputes, and the like. There was, too, the sheer physical isolation of the site, 130km from the nearest town. Although we were able to rent a spacious village house close to site 21-S-2, we lacked the lab and photographic development facilities, not to mention the convenient shopping, that had been available throughout the Meinarti dig. There was in addition the unexpectedly large number of sites requiring our attention, although in the end we were able to cope with all of them satisfactorily.

Within the individual habitation sites, the most difficult problem, in terms of our overriding objective, was the lack of stratigraphy. Not only was there no architectural overbuilding, except at 21-S-10, but there was very little accumulated refuse deposit in most of the buildings and in the streets and plazas between then. A chronology of buildings therefore had to be worked out at each site on the basis of architectural typology and of ceramic deposits. While this has proved generally satisfactory, the sequence of the buildings can never be as secure as was the case at Meinarti, where it was confirmed by direct superposition.

A final problem was the unexpected discovery of mural paintings in the little church at 21-S-2, whose conservation automatically became my responsibility under the terms of my excavation license (Plate 6.5c). Since I had no facilities for this work during the 1969 season, it was necessary to mount a second whole expedition in the following year just for the conservation work.

Advantages. If the problems were different at Kulubnarti and at Meinarti, so also were the advantages. First and foremost was the fact that the none of the sites were threatened with actual inundation, for Kulubnarti lies at the very upriver end of Lake Nubia. Technically speaking, therefore, the Kulubnarti dig was not a salvage operation, and there was consequently no need for haste. Then too the absence of architectural superposition meant that no building had to be dismantled to get at something underneath. The absence of overburden and the very small amount of midden deposit in most of the sites was in its way a problem, as just noted, but it was also this circumstance which allowed us to dig so many houses and sites in their entirety.

The presence of student assistants, relieving me of some of the burdens of supervision and recording, was a very great advantage at Kulubnarti, as was the availability of my friend Fritz Hinkel to prepare my final site maps at the end of the season. It was this assistance that afforded me time enough to collect, sort, and count all the potsherds from each Kulubnarti site, and not just the decorated wares. Since I was here, unlike at Meinarti, 'using the pottery to read the sites', the calculation of maximally detailed ceramic statistics was far more important than in any previous dig. (For explanation of the methodology see especially Adams 1986, 617-633).

A further advantage at Kulubnarti, in contrast to Meinarti, was the considerable preservation of organic material. This has enabled my friend and colleague Ali Osman[13] to recognize various objects of wood, basketry, and leather whose counterparts are still in use in modern Nubian households.

Methods. The methodology employed at the major Kulubnarti settlements was straightforward; it involved simply the complete excavation of each site one house and one street at a time. First, in each case, all of the surface material was removed and discarded, then each house was dug out, and then the areas between the houses. Since there was in most cases no natural stratigraphy to be observed, sherds and artifacts were segregated by arbitrary excavation levels in each room. The major Kulubnarti townsites were tackled in a sequence which reflected our order of priorities, relative to the main objective of the dig. Since Site 21-S-2 was our primary target, all of our resources were concentrated here until the excavation was nearing completion. One of the student assistants was then detailed to undertake the excavation of 21-S-25, and subsequently of several of the smaller hut clusters. After 21-S-25 was completed, another student assistant did the excavation at 21-S-9. Meanwhile I remained at the main site until it was entirely completed, then went on to tackle 21-S-10, and subsequently also a small church and a pottery-making site.

Successes. The conditions we encountered at Kulubnarti enabled us to complete far more excavation than could initially have been anticipated. All four of the major settlements were dug virtually in entirety, and 16 other sites also received at least a minimally necessary amount of investigation and documentation. It should be added that the unexcavated portions of all the sites remain available for future investigation, since Kulubnarti will not be flooded. We have excellent map records and written notes of all the sites, although the photographic record is less satisfactory. We were able for the first time to undertake a complete quantitative study of all the sherd material, and we made comprehensive collections of all artifact collections as well as of animal bone and plant remains. Thanks to the preservation of organics, we have a good deal of artifactual material that is not normally preserved, and that allows comparison with the material used by Nubians today, or in the recent past. Much of this material, including masses of basketry and leather fragments, remains to be studied in detail.

Failures. The most signal failure of the Kulubnarti dig was simply that it did not achieve its basic historical goal. While it was possible to work out a fairly reliable developmental sequence for the architectural and artifactual remains, nothing was found which gave clear evidence either as to when Christianity finally disappeared or to when Islam made is appearance. Apart from a few specimens of Arabic writing, nothing found at Kulubnarti could be identified specifically as Islamic. The remains datable to the 16th century and later must simply be identified as Post-Christian, rather than as Islamic. Yet the fact that the coming of Islam was without material consequences is perhaps in itself an important discovery.

Leitmotiv. The dominating *motiv* of the Kulubnarti excavations was clearly the architecture, as represented not by stratigraphic levels but by individual buildings. We were able, far more clearly than at Meinarti, to develop a typology of late medieval houses, and undertake a special study of the two-storey castle-houses (cf. Adams 1994a).

Qasr Ibrim

Qasr Ibrim has become so well known, at least by reputation, to Nubiologists that it hardly needs

[13] Professor Ali Osman Mohammed Salih, University of Khartoum, Sudan.

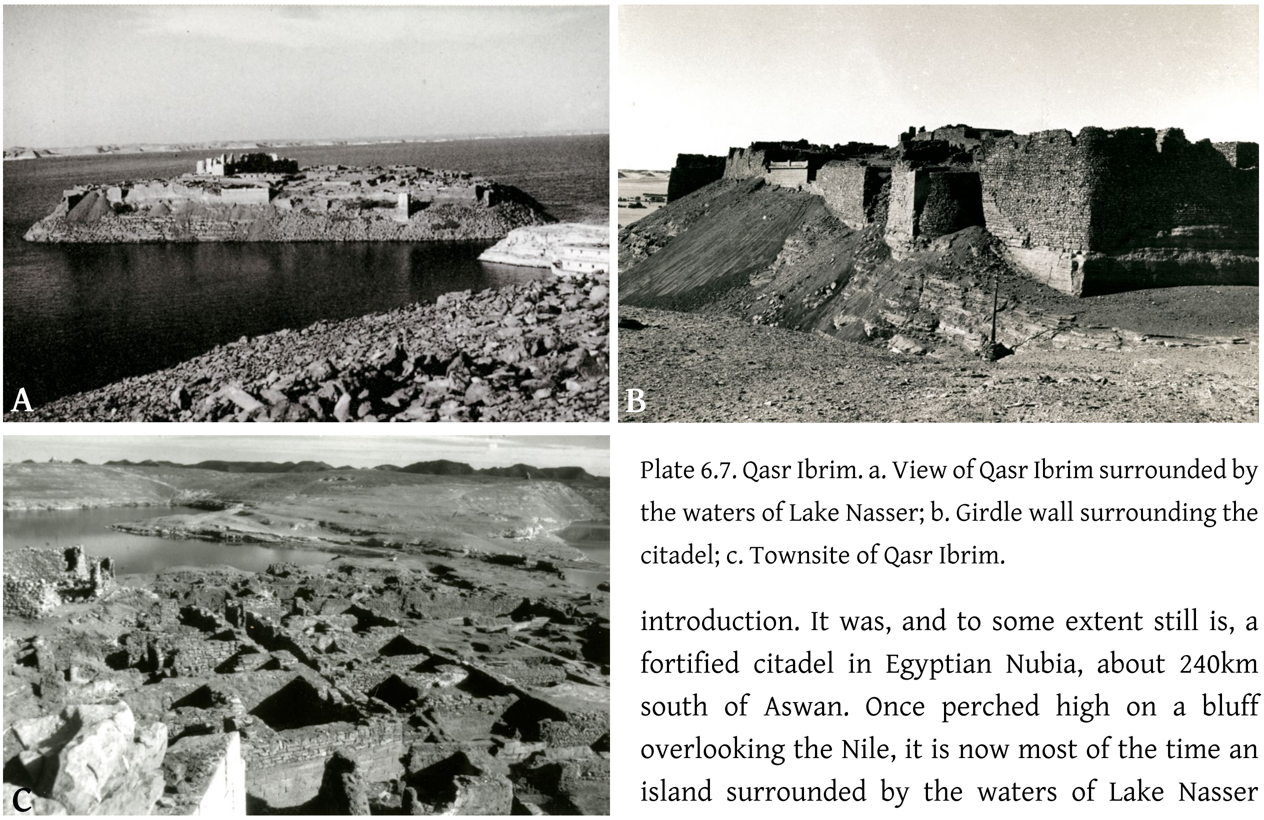


Plate 6.7. Qasr Ibrim. a. View of Qasr Ibrim surrounded by the waters of Lake Nasser; b. Girdle wall surrounding the citadel; c. Townsite of Qasr Ibrim.

introduction. It was, and to some extent still is, a fortified citadel in Egyptian Nubia, about 240km south of Aswan. Once perched high on a bluff overlooking the Nile, it is now most of the time an island surrounded by the waters of Lake Nasser (Plate 6.7a[14]). Excavations to date have revealed that the place was occupied at least from Napatan to Ottoman times, with considerable suggestions that there was New Kingdom occupation as well.

The excavation concession at Qasr Ibrim is held by the Egypt Exploration Society. Work was begun at the site in 1963, and has continued intermittently, usually at two-year intervals, from that time until the present.[15] I joined the dig in 1972, initially as kind of invited guest with no very specific function, and did not become officially the field director until 1978. I continued in that capacity, either officially or unofficially, until I reluctantly decided to give up field activities in 1984. I made this decision not in any spirit of disaffection, but simply because my backlog of unpublished excavation results was so great that I could not, in good conscience, go on adding to it.

For me, Qasr Ibrim was what in America we would call ‘a whole new ballgame' in nearly every respect. It was my first experience working in Egypt; my first experience at a site where excavation had already been underway for several seasons before I got there; my first experience on a dig where for a long time I was not the boss; and my first experience working with and for an organization — the E.E.S. — whose approach to archaeology is essentially an amateur one. The institutional problems were thus far more complex than on any previous dig of mine, but I want here to concentrate on the more purely professional and technical aspects of the dig.

The citadel at Qasr Ibrim occupies about two hectares, and is surrounded by an imposing girdle wall which itself had a complex history (Plate 6.7b). At most periods the area within the walls was mainly occupied by a densely congested townsite (Figure 6.5; Plate 6.7c), but there were always monumental buildings as well: at least three temples in Napatan, Meroitic, and X-Group times, and a cathedral and three other churches in the Christian period (Plate 6.8a). The remains of the different periods were unmistakably stratified, but with none of the regularity and predictability encountered at Meinarti.

[14] In the original text Plate 6.7a was reversed [ed.].
[15] Excavations conducted at Qasr Ibrim by the Egypt Exploration Society have since concluded. See further Rose 2011; footnote 2 above, passim; and Egypt Exploration Society (E.E.S.) https://www.ees.ac.uk/ [accessed 30.04.2020] [ed.].

Figure 6.5. Qasr Ibrim: Plan of uppermost (Ottoman) level.

Because Qasr Ibrim during most of its history was an elite governing or religious center, there had been occasional, massive ‘urban renewal’ when certain areas were cleared not only of surface buildings but of some of the underlying ones as well, as a preliminary to new construction. The result was a series of what in geology are called ‘unconformities’, or gaps in the stratigraphy. Consequently, architectural superposition does not provide a wholly sufficient basis for the dating of houses at Qasr Ibrim; the study of sherd deposits (‘using the pottery to read the site’) is here once again a critical necessity.

During most of its 3000-years history, Qasr Ibrim has been subjected to almost no destructive forces whatever, either natural or man-made. Its elevated situation has protected it altogether from flooding and capillary moisture, and it has almost never been rained on, nor have there been major fires, earthquakes, or military destruction, except on two brief occasions. As a result, the site offers almost total preservation of all kinds of organic remains: of basketry, textiles, leather, wood, and most importantly, written documents. To cite only one example, a typical season’s excavation yields over 20,000 fragments of textile alone.

The deposits in and around the buildings do not consist of windblown or waterborne sand, as at other Nubian sites. They are made up from top to bottom of densely compacted occupation midden, which contains not only a high density of sherds and artifacts but also sticks, straw, chaff, dung, and every kind of organic refuse. These deposits in places are enormously deep, reaching up to eight meters.

The corps of supervisory and technical personnel at Qasr Ibrim was far larger than on any previous dig of mine, numbering never less than a dozen individuals. Here, as nowhere else, a great many specialists were required simply to keep abreast of the enormous daily volume of finds. We had, at various times, specialists working exclusively on textiles, on basketry, on food remains, and on written texts in no fewer

Plate 6.8. Qasr Ibrim. a. Cathedral at Qasr Ibrim; b. Ottoman period stone houses; c. Storage pits and midden deposits.

than nine languages. On the other hand, the actual digging force was smaller than I was used to; in most seasons we employed no more than 60 laborers, and for a digging season of only two months. This too was a consequence of the tremendous volume of finds. We tried to work with 100 laborers in the 1976 season, and found that we simply could not keep up with the study and registration of finds on that scale.

The remains. The allocation of buildings and deposits at Qasr Ibrim to particular occupation phases is based partly on stratigraphy, and partly on architectural and artifactual typologies. I will here review only those phases in whose investigation I was myself involved. In its final, *Ottoman phase* (*c.* 1550-1811 AD), Qasr Ibrim was mainly a garrison point; however, the surviving remains were almost entirely those of rather roughly-built stone houses (Plate 6.8b). These structures covered the whole surface of the citadel except that occupied by the Cathedral, which had been turned into a mosque (Figure 6.5). The *Terminal Christian period* (*c.* 1350-1500 AD) saw the building of three massive 'castle-houses' of mud brick; other, smaller houses of stone remained in use from the preceding period. *The Late Christian period* (*c.* 1172-1350 AD) was inaugurated by the historically attested raid of Shams ed-Dawla Turan Shah in AD 1172. Following this event, the open spaces around the churches gradually filled up with ordinary dwellings, as the Qasr Ibrim mountaintop came to be regarded as a place of refuge. In the *Early and Classic Christian periods* (*c.* 700-1172 AD) Qasr Ibrim was primarily a religious center. An old Napatan and Meroitic temple was converted into a church, and the Qasr Ibrim Cathedral and another church were added, probably at the beginning of the eighth century (Plate 6.8b).[16] All or nearly all of the domestic dwellings of earlier times were abandoned and partially leveled, to create open spaces around the churches which may have been for the accommodation of pilgrims.

In the *X-Group and Earliest Christian periods* (*c.*(?) 300-700 AD) at least three Kushite temples were still in use; elsewhere the entire mountaintop was occupied by stoutly built stone houses, many of them two stories high (Plate 6.7c). Many of the buildings may have been shops as well as dwellings, with business carried on in the lower rooms and living arrangements on the upper floors. Many houses included covered storage crypts beneath the floors. It is apparent that many of the X-Group houses stand upon the

[16] Stone houses of the Ottoman period are shown in this plate, not the Cathedral or other Early or Classic Christian structures. The plate reference here seems to be an error [ed.].

stumps of Meroitic walls, dating from some time before 300 AD, but none of these dwellings has yet been systematically excavated. In the *Meroitic period* at least three temples were in use, though all of them may have been constructed earlier.

A retrospective overview

The site and the excavations at Qasr Ibrim have a kind of 'larger than life' quality, by comparison with other Nubian sites. Everything is on a more complex scale; not only the problems and the opportunities, but even the objectives themselves. I can here describe only my own perspectives, relative to those seasons when I was effectively involved.

Objectives. The Qasr Ibrim excavations had necessarily many objectives because the site offered so many unique opportunities. For me the overriding concern was to work out the correct sequence of the buildings and deposits, and on that basis to reconstruct the historical development of the community. It was especially important to gain a sense of the overall configuration of Qasr Ibrim as a community, because this place was not an ordinary village but an elite center: administrative, religious, at times commercial and at times military. To the limited extent that I could make choices, I gave particular attention to the remains of the Ottoman and of the X-Group phases, because these were architecturally the least studied and least understood phases in the later history of Nubia. For other members of the expedition, however, the study of various kinds of artifactual remains, and of written texts, were always primary objectives.

Problems. Problems in the excavation of Qasr Ibrim were of many kinds. The institutional problems in dealing both with the Egyptian Antiquities Organization and the Egypt Exploration Society were substantial, but I will not detail them further here. The logistic problems resulting from the isolated lakeshore location at Qasr Ibrim, where everything had to be brought in via a three-day boat journey from Aswan, meant that an inordinate amount of time had to spent on logistics. Then too there were the inevitable problems in dealing with a large and mostly amateur staff of specialists, many of whom were newcomers each season.

The most challenging and by far the most interesting problems at Qasr Ibrim were nevertheless the purely archaeological ones. First and foremost was the problem of 'reading the site', and working out the correct chronological sequence of the buildings and deposits, notwithstanding the complex and discontinuous stratigraphy. The densely compacted midden deposit in most of the buildings presented its own set of problems, for excavation in these circumstances had always to proceed very slowly if the total content not only of artifacts but of sherds, textiles, and food remains was to be recovered. Complicating the excavations was the fact that all the midden deposits had been dug through with grain storage pits — numbering several hundred in all--subsequent to the time when the refuse was originally laid down, and it was necessary to identify these and to clean them out in order to avoid mixing deposits (Plate 6.8c).

The largest problem at Qasr Ibrim was occasioned by the simple volume of the finds, plus the unusual circumstance that the Qasr Ibrim expedition had no institutional home. There was no dig house on the site; the whole team of supervisory personnel and laborers were housed on boats brought from Aswan for the duration of the season. There was, as a result, no officially sanctioned magazine where finds could be stored from season to season. Since nearly all finds went to the Egyptian Antiquities Organization at the end of each season, the inevitable consequence was that every single find had to be studied, drawn and photographed on-site in the course of the season itself. It was for this reason that actual excavations were limited to two months, while many of the supervisory team, necessarily including the director, had to remain at Qasr Ibrim for one to two months after that.

Advantages. The logistic situation at Qasr Ibrim, though beset with difficulties, did offer a few advantages. The houseboats we rented each season were large enough to accommodate a supervisory staff of more than a dozen persons, as well as providing some space for indoor laboratory and object-processing operations. The presence of a somewhat erratic electric generator on one of the boats also permitted evening work, on those occasions when it was not out of service. The presence of a large supervisory staff, though it created inevitable problems at times, was nevertheless not merely an advantage but an absolute necessity.

Methods. As already noted, I was not in actual control of the excavations in my earliest seasons at Qasr Ibrim.[17] As I increasingly assumed control, my first objective was to 'straighten up the site' where the excavations in the earlier years had been rather haphazard. A good many buildings had been cut in half, without a great deal of documentation, and my immediate concern was to investigate their remaining and unexcavated portions. Following this, a second objective was to connect up the various scattered test excavations that had been made in different parts of the site, so that the town plan could begin to disclose itself. My long-range objective, however, was eventually to bring all of the excavations throughout the site down to one common level (initially the Late Christian level), and then to proceed downward level by level as I had done at Meinarti.

I was never fully successful in this last endeavor, for a variety of reasons. One was the erratic and sometimes discontinuous stratigraphy previously mentioned. Another was the fact that test excavations before my time had already penetrated down to several different levels, some of them as deep as the Meroitic. Finally, there was the desirability of leaving various buildings, of different periods, standing *in situ*, since Qasr Ibrim will not be inundated and may someday be a tourist attraction.

For better or worse, I always approached the Qasr Ibrim dig, just as I did all my previous Nubian digs, as a salvage operation. As at Meinarti and Kulubnarti, I strove to get as much dirt moved as possible in each season. I was motivated partly by the consideration that capillary moisture from Lake Nasser is now affecting the deposits, and partly by the fact that the site was repeatedly ransacked by fishing crews, looking for burnable wood, during the off-seasons. At the same time and for the same reason, I confined my activities to the area of the walled citadel that was actually at risk, ignoring remains on the nearby desert terrace that were not under threat of destruction.[18]

An important methodological innovation during my time at Ibrim was to inaugurate the collection and study of all kinds of remains, like textiles, basketry fragments, and food remains, which had previously gone largely uninvestigated.

Successes. Since the Qasr Ibrim dig has not officially been discontinued, it is perhaps early to talk in final terms about either successes of failures.[19] So far as my time of involvement was concerned, I like to think that I brought a degree of professionalization to what had been almost entirely an amateur operation. I laid out a series of defined priorities and objectives, based in considerable part on my previous experience in Nubian archaeology, though these have in some cases been modified by my successors. I introduced a comprehensive system for the numbering and recording of both architectural and artifactual remains, and insisted on the collection and study of all kinds of materials that had previously been ignored. Finally, I played at least some part in persuading the Egypt Exploration Society to make a long-term commitment to the site, whereas in my earlier years I was always told that each season might be the last.

Failures. Whether or not it can be regarded as a failure, I never felt that I was in complete control of the Qasr Ibrim operation. Part of this was due to the institutional framework within which I worked, part to the logistic situation, and part to the unpredictable nature of the site itself, where the unexpected was

[17] William Hugh Clifford Frend (1963-1964) and John Martin 'Jack' Plumley (1963-1978) preceded the author as field directors of the excavations at Qasr Ibrim [ed.].
[18] For the hinterlands around the citadel see Rose 1996 [ed.].
[19] See note 13 above [ed.].

forever turning up. As a result, strategic decisions had continually to be modified, and *ad hoc* objectives adopted for each individual season. At Qasr Ibrim, far more than at any previous dig of mine, it always seemed as if one were reacting rather than acting. I do not know to this day if the long-range strategy and objectives that I had outlined could in practice have been realized; in any case my successors have chosen different methods and different objectives, which may well be more appropriate to present-day circumstances than my approach would have been.

Leitmotivs. With its multiplicity of opportunities and of challenges, the Qasr Ibrim dig has probably had as many *leitmotivs* as it has had diggers. Of the two field directors who preceded me, J. M. Plumley was most interested in the cathedral and the churches, and W. H. C. Frend in the remains of the monumental podium. The main interest for me was not in any particular building but in working out the sequence of the buildings and, more broadly, the developmental history of the community. Others at the site have been most interested in the artifactual record, and still others in the texts. But whatever our preferences may be, the simple fact is that the artifactual finds, by their sheer volume, are 'the tail that wags the dog' at Qasr Ibrim. Strategic and especially tactical decisions about when and where and how much to dig are determined more often by the volume and kinds of artifacts to be expected than by any other factor.

A personal philosophy

Though each of my large-site excavations in Nubia has presented its own problems and opportunities, there have been certain recurring choices in all of them. My responses have been determined partly by necessity, but often by a kind of over-arching personal philosophy. The choices can be phrased as a set of dyadic contrasts, or strategic alternatives:

1. History or culture? ('down or out'?) Excavation at any large site can be enlarged either by horizontal or by vertical extension, or some combination of the two. Downward excavation, by trenching or pitting, reveals the sequence of occupation, but only in a spatially limited sample. Each trench or pit yields a profile which is an individual record of taphonomic processes: building, dilapidation, refuse dumping, fires, wind deposition, and the like. To a very large extent these are unintended or random processes, not culturally patterned. In short, the vertical record revealed in profiles is a record of particular historical events, not of cultural developments.

Outward excavation, or opening up large contiguous areas at the same level, reveals the plans of houses and villages and spatially patterned activity; in other words, of culture and society at work. This is an anthropological rather than a strictly historical record. Since I am by training and preference a cultural anthropologist, more interested in culture patterns than in the accidents of history, I have always preferred horizontal to vertical excavation. When I go down, I always want to go by complete levels.

2. The forest or the trees? (big picture or small picture?) It is possible to busy oneself with the minutiae in any site, casting aside the *turiya*[20] and baskets for the trowel and whiskbroom. The danger in such cases is that one may come to see a whole series of individual trees without ever seeing the forest, so to speak. I am quite frankly more interested in the big picture than in the details, and for this reason in all my digs I strive to open as much area and to move as much dirt as my time and resources will permit, even if it means letting some of the detail go.

3. The whole or the part? Both the theory and the practice of sampling in archaeology remain problematical. Quite different principles and different theories are often applied in the choice of sites to dig, in the choice of areas to dig within a site, and in the choice of what to save. For example, a great many archaeologists are content to open up only small areas of any given site — a limited areal sample — but to save every scrap of material from the excavated areas. My inclination is to do just the opposite: to open

[20] A digging hoe (طورية) [ed.].

up the largest possible area, but not to try and recover every bit of artifactual content, if it means the use of sieving or other procedures that are not cost-effective. We experimented with sieving during one season at Qasr Ibrim, and found that we could increase the total take of sherds from each excavation unit by about 30%. However, the great majority of additional sherds recovered in this fashion were too small to be recognizable, as were also the papyrus fragments recovered. Even if this were not true, however, any statistician knows that a 70% sample is more than adequate for any kind of generalization. In short, the results of sieving simply do not justify the time spent on it, and this brings me to my next point:

4. For richer or poorer? There may be archaeologists with unlimited time and funds, but they are surely a small minority. Most of us have to be thinking all the time about how much we can accomplish in a limited time and with limited funds. We need, therefore, to consider every procedure from the standpoint of cost-effectiveness. With that in mind, I consistently dispense with procedures that do not yield results commensurate with the expenditure of time or money or man-hours. I do not usually screen deposit, and I do not precisely record the locations of discarded objects found in the refuse, since their locations do not reflect any kind of human intent. I also do not spend time on photographic aesthetics such as the preparation of straight profiles, right-angled corners, and neatly swept floors. Such activities make for an aesthetically appealing excavation report, but they contribute nothing to the informational value of the dig.

5. End or means? This brings me back to my starting point, that archaeology undertaken in the way recommended by the 'how-to' manuals is a mere exercise in technical virtuosity: an end in itself rather than a means to an end. Since I was trained as an ethnologist rather than as an archaeologist, however, I approach archaeology merely as a way of enhancing our knowledge of culture and of history. The value of all procedures and all strategies is judged by whether and how much they serve that purpose.

It goes without saying that all my excavations have been undertaken with a wider audience in mind than just my fellow excavators, and my excavation reports have been written accordingly. We archaeologists are sometimes prone to forget that we do not own the remains we investigate; we are the licensed trustees for a much wider public. Those persons may not have the technical expertise to conduct excavations, but their interest in the results is just as keen and just as legitimate as our own. The reports which are the final outcome of our work should therefore make our findings as accessible to them as to our professional colleagues. The recent backlash against archaeology and archaeologists on the part of Native Americans and some other groups shows that we have been ignoring that fact at our peril. In the case of my own Nubian excavations, the Nubian people themselves are the audience whom I have most in mind, and whose approval I most value.[21]

What I have sought to convey in these paragraphs is not a specifically a methodology or a theory, but rather a personal philosophy of archaeology. It is not the right way or the wrong way; it is simply the Adams way, for better or worse.

French resumé

Finalites et methods des fouilles a grande echelle: Meinarti, Kulubnarti and Qasr Ibrim

Les fouilles de Meinarti et de Kulubnarti, qui remontent déjà a plus d'une génération, ainsi que celles de Qasr Ibrim, permettent a l'auteur de presenter ses conceptions de l'archéologie et la méthodologie souhaitée. Les méthodes utilisées sur ces trois chantiers ont eté déterminées en fonction des objectifs et des circonstances et influencées parles considérations théoriques, pratiques, politiques et personnelles.

[21] For commentary regarding archaeological recording and accessibility for and engagement with Nubians with particular reference to the UNESCO campaign, see further Carruthers 2020 [ed.].

Les particularités de chacun, les objectifs, les problemes posés, ainsi que les méthodes employées a chaque fois, sont analysés ; le dégagement de Meinarti, fouille de sauvetage d'un site stratifié, a permis l'établissement de plans détaillés sur 18 niveaux et celui des séquences de la poterie chrétienne tardive ; le dégagement de Kulubnarti a contribué a l'étude du passage de la christianite a l'Islam ; les objectifs sont multiples A Qasr Ibrim, en rapport avec la complexité et la richesse du site occupé de l'époque napatéenne a l'époque ottomane, et peut-être des le Nouvel Empire égyptien. L'auteur conclut en exposant sa philosophic personnelle de l'archéologie et ses méthodes préférées, privilégiant les dégagements horizontaux aussi larges que possible.

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CLASSIFICATION

Plate 7.1. Baskets of washed pottery sherds at Kulubnarti awaiting processing, taken in 1969 (photo W. Y. Adams).

Principles and Pragmatics of Pottery Classification: Some Lessons from Nubia[1]
(1975)

This is the earliest of my several writings on the subject of classification, and the only one in which I have spelled out in detail how and why I came to be interested in the subject, when I was forced from practical necessity to classify the Nubian pottery wares. I have, along the way, identified some basic lessons about the classification process which I learned, and which I believe are applicable to all, or nearly all, classifications. These ideas were in incorporated in my full-length volume, Archaeological Typology and Practical Reality *(W. Adams and E. Adams 1991). The paper here was prepared for one of the archaeological conferences, latterly called Chacmool Conferences, that are held annually at the University of Calgary.[2] It was, consequently, prepared for an audience knowledgeable about archaeology, but with no necessary background in the Egyptian or Nubian fields.*

Sixteen years ago, through a series of unlikely historical accidents, I became director of archaeological salvage operations in Sudanese Nubia, or m§ore properly, in that portion of the Aswan Dam basin[3] which lay within the Republic of the Sudan. The region for which I assumed responsibility comprised a section of the Nile Valley about one hundred miles long, which proved to contain several thousand archaeological sites of all ages and types. Between 1959 and 1966 I was involved in the survey and recording of over 300 sites, and in the partial or complete excavation of more than 100 of them.

The majority of Nile Valley archaeological sites date from the Neolithic or later eras, and they regularly yield, among other things, prodigious quantities of ceramic refuse. At the time I began my field work there had not been, however, any serious attempt at a typological or chronological study of the later Nubian wares. Previous archaeologists had largely ignored the ceramic record except for particularly attractive specimens which were collected for illustration or museum display. (There are one or two honorable exceptions to this generalization; e.g. Reisner 1923, 41-46 and Steindorff 1935, 65-106). It was my feeling, backed by the conventional wisdom of prehistoric archaeology, that so potentially informative a material domain as pottery deserved more careful and systematic attention than it was receiving, and I made it a policy to collect and label for future study all of the sherds from my own excavations.

I soon found, however, that I had not the physical facilities to deal with the incredible volume of ceramic refuse from the Nubian sites. Before long most of my available excavation baskets – essential for simple earth removal in the Nile Valley – were in use as sherd containers, and my limited storage area was full to overflowing. It became a practical necessity to do something about the sherd situation, and the obvious solution seemed to be to develop at least a rough typology; to record the frequencies of sherd types from different sites and levels or localities, and then to discard the sherds and return the baskets to earth-carrying.[4]

It was these very simple but imperative constraints which initially forced me into the typological and chronological study of Nubian pottery – a subject which I had avoided in all my previous archaeological work (chiefly in the American Southwest; cf. Adams and Adams 1959; Adams *et al.* 1961). Due to an overwhelming lack of interest in the material I had, as a result, no formal training in the epistemology of

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[1] Originally published in J. S. Raymond, B. Loveseth, C. Arnold and G. Reardon (eds), *Primitive Art and Technology*. Calgary, 81-91 [ed.].
[2] Now called the ANARKY Conferences. See further https://www.gluseum.com/CA/Calgary/1239751216058345/Anarky-Conference [accessed 19.05.2020] [ed.].
[3] Aswan High Dam [ed.].
[4] See Plate 7.1 and Adams 2011, 6 and pl. 1.2 c-d [ed.].

taxonomy, no technical competence in pottery manufacture, and no aesthetic interest in the material itself. With such a combination of non-qualifications I was foredoomed to a good many false starts and missteps in my efforts to arrive at a workable taxonomic system. The experience was not without its value, however, for by beginning at the same zero point and by covering all the same ground as had the pioneer ceramic typologists of three generations ago, I was forced to rethink the whole business of pottery classification from the beginning. In the process I learned some lessons – mostly the hard way – which probably need to be re-learned from time to time. For whatever they are worth, I will endeavor in this paper to share those lessons with others.

Systems of classification must be developed *ad hoc*

In my first attempts to sort out the Nubian wares I naturally began by applying the typological approach which was familiar to me in the study of Southwestern American ceramics (for explanation of the system see especially Colton 1937). From the beginning I developed a classification primarily of and for sherds, and only incidentally for whole vessels. This approach places primary emphasis on fabric (paste and other internal characteristics), color and decoration. After some weeks of attempting to assign my material to various taxa which were conceptually equivalent to 'Flagstaff black-on-white', 'Tusayan polychrome' and the like,[5] I became convinced that my 'types' were neither very homogeneous nor very consistently distinct one from another. Most importantly, they did not seem to be taking into account some of the recognizable variability in the Nubian pottery. I therefore abandoned the effort at methodological transfer and returned my sherds to their original storage baskets.

On further reflection, it became apparent to me that the inappropriateness of Southwestern taxonomic methods, as applied to the Nubian material, lay in the fact that they involve presuppositions about what are and are not significant parameters of variability – presuppositions which are well founded in terms of what we know about Southwestern pottery, but which have no *a priori* probability value when applied to other areas. Thus, for example, Southwestern pottery typologies largely ignore vessel form and surface finish as significant variables, since the co-variance of these traits with other factors, either spatially or temporally, is difficult to demonstrate. But it is not safe to assume, in the absence of distributional data, that these same variables can also be ignored in other areas.

Unfortunately, I had not, at the outset of my studies, any distributional data to draw upon. Existing collections of Nubian pottery consisted almost entirely of whole vessels recovered from graves – a small corpus of material selected largely on esthetic grounds from a much broader range of pottery types. Even the collected vessels often lacked accurate provenience data. Under the circumstances I concluded, reluctantly, that I should have to treat all observable variability as significant – that is, to make a place for it in my taxonomic scheme unless and until I should be able to determine on distributional grounds that some variables were non-significant and could be eliminated from the scheme. My problem then was to find some way of ordering the complex mass of variability exhibited by the Nubian wares.

Classification necessarily proceeds from analysis to synthesis

In due time it became apparent to me that the total spectrum of variability exhibited by the Nubian pottery wares could be reduced to seven independent 'universes of variability': method of manufacture (hand-made, wheel-made or mold-made), fabric (internal properties), vessel form, surface treatment, color(s), painted decoration and relief decoration. Each of these universes itself encompasses a wide range of variables, but within any one category many of the variables are either functionally linked or

[5] For a description and illustration of the American Southwestern Ceramic Typology and Tusayan ceramic types see http://ceramics.nmarchaeology.org/index/organization-of-the-classification-system/organization-of-classification-system/image-gallery/tusayan [accessed 10.05.2020] [ed.].

functional alternatives, so that the presence of one trait makes predictable the presence or absence of others. On the other hand, there is no functional interdependence among the seven different universes of variability; no change within any one of them is automatically associated with changes in any of the others.

It was therefore necessary to develop, initially, a separate scheme of classification for each of the different universes of variability. To deal with painted decoration (the most complex of all the fields) I eventually devised a highly formal classification involving about a dozen major styles, two dozen minor styles and more than 2250 individual motifs. Vessel form was analyzed into 26 vessel classes and 640 individual forms. Other areas could be treated more informally; surface treatment (for example) resolved itself into a small number of basically alternative categories such as 'rough', 'matte', 'polished', 'burnished', 'glazed' and so on. In spite of a seemingly unlimited range of hues, I found in practice that all decorative colors could be glossed under five headings: white, yellow, orange, red and black.

The recognition and classification of variability within each separate universe constitutes the initial or analytical phase of the classificatory process. Analysis is however only one step in the development of a useful and usable typology. The definition of variability separately within each universe produces a set of overlapping taxa (i.e. every sherd belongs to seven taxa – one in each universe); moreover, it says nothing about the cultural or historical significance of any particular trait or taxon. Significance is, initially, a matter of co-variance between traits which are not functionally linked. If a particular group of vessel forms is always associated with a particular group of painted designs, even though there is no necessary reason why they should be, then we are justified in assuming that this juxtaposition of form and design reflects the intention of the potter – something which probably did not remain constant over a large area or over a long period of time. In short, we are moving closer to the extrasomatic and cultural considerations which are the ultimate goal of our classificatory studies; an issue which I shall return to presently.

The synthetic phase of classification, then, involves the study and plotting of co-variance between independent traits, and particularly between traits in different universes of variability, and finally the designation of taxonomic categories on the basis of recurring clusters of traits (this approach to classification was endorsed in principle by Anna Shepard in the last edition of *Ceramics for the Archaeologist*. (Shepard 1965, xiv)). The more independent are the variables involved in any given taxon, the greater is their presumed cultural significance. In applying this principle to the Nubian material I found in fact that my seven universes of variability could be arranged in a descending hierarchy of significance as follows: 1. method of manufacture; 2. fabric; 3. form, painted decoration and relief decoration; and 4. color(s) and surface finish. It is not my intent in this paper to give a detailed description of the taxonomic scheme which I devised on this basis, (for detailed explanation of the scheme see Adams 1970 and Adams 1973) but only to explain in a general way how the scheme exemplifies the basic principles of analysis and synthesis.

On the basis of method of manufacture, I found that I could regularly distinguish, on the one hand, wares which were locally hand-made throughout Nubia, probably by women potters, and on the other hand, wares which were wheel-made in a limited number of commercial factories, presumably by male specialists.[6] Not only did these two traditions involve fundamentally different manufacturing technologies (including pit firing in one case and kiln firing in the other), but they also existed side by side for 2000 years or more without significantly influencing one another stylistically. I thus found that, on the basis of method of manufacture alone, I could make predictions about fabric, surface finish, vessel

[6] Gender and archaeology has been a much discussed topic in recent years evoking both new theories and criticisms. See for example, Bacus *et al*. 1993 and Trigger 2007, 14 [ed.].

forms and painted and relief decoration which would hold consistently true in hand-made or wheel-made wares, but not both. No other variable would enable me to make an equal number of predictions about other variables. The distinction between hand-made and wheel-made traditions is therefore the most fundamental of all taxonomic distinctions in my scheme.

Among the wheel-made wares, I found on the basis of fabric that I could often distinguish the products of individual factories, some of which had a very long history of production. This was however true only for pottery made in Egypt (which at various times comprise from 15% to 50% of the total corpus of wares found in Nubia), for in Egypt factories did, and still do, tend to utilize fine residual clay deposits having distinctive properties of color and texture. (For recent discussion on this point see Butzer 1974). On the other hand, the factory or factories within Nubia all used a brown silt which occurs in pockets along the Nile floodplain, and which is uniform in color and texture from one end of the area to the other. On the basis of stylistic uniformity or variability it looks as if we are dealing with the products of only one Nubian factory at some periods in history, and with as many as half a dozen at others, but in terms of fabric we can speak only of one generalized Nubian 'family' as distinguished from a number of different Egyptian 'families', each representing in all probability the products of a single factory.

Although there was some tendency for factories to imitate one another's products, in general each pursued its own distinctive stylistic traditions, often over a very long period of time. There is, therefore, a high degree of co-variation between fabrics on the one hand and vessel forms, painted designs and relief designs which represent the stylistic traditions unique to a single factory (or sometimes, as in the case of Nubia proper, to a group of factories). 'Families', which are defined on the basis of fabric, thus represent the second hierarchical level in my classificatory scheme. It is however possible to make distinctions at this level only among the wheel-made wares. All of the hand-made wares (like the native Nubian wheel-made wares) were formed from Nile silt, and no consistent variability can be observed in the fabric.

Although each factory has its own stylistic 'trade marks', no factory continued to produce exactly the same product or products throughout its history. Painted decoration, relief decoration and vessel forms tended to remain fairly stable for periods ranging from 50 to 200 years, then to give way fairly rapidly to new stylistic preferences. It is therefore possible within the longer-lived families to recognize a succession of what I call 'ware groups', each corresponding to a period when certain specific canons of form and decoration were in vogue. A ware group by definition combines one tradition (wheel-made or hand-made), one fabric (that of the family of which it is a member), one or more distinctive decorative styles (which are usually painted in the case of the wheel-made wares and incised in the case of the hand-made wares, though there is some overlap) and a distinctive group of vessel forms. Ware groups essentially represent chronological variability within families, while families themselves represent spatial variability within the pottery industry as a whole. Ware groups can be recognized both in the hand-made wares and in some of the wheel-made families, but there is surprisingly little correspondence between them, for the rate and the course of stylistic evolution were different at each factory. Thus, during the same 1200-year interval of time, the hand-made wares exhibit three successive ware groups, wheel-made wares from Aswan (Egypt) exhibit four ware groups, and wheel-made wares made in Nubia exhibit seven ware groups.

Even within any given ware group the products are never wholly uniform; usually there are vessels with the same fabric, forms and decorative motifs, but differing in color and sometimes also in surface treatment. In one particular Nubian ware group, for example, we can recognize the existence simultaneously of a polished red ware (sometimes decorated in black and/or white), a matte red ware (never decorated), a polished white ware (sometimes decorated in black; never in red) and a matte white ware (usually decorated in red, or occasionally in black). These contemporary variants, which represent the minimal units in my taxonomic scheme, are designated as 'wares'. As already noted, their defining criteria are mainly color and surface treatment, though sometimes particular wares also exhibit some distinctive vessel forms. The fact that contemporary wares which are members of the same ware group do not always exhibit precisely the same vessel forms and designs gives rise to the suspicion that in some cases (particularly as regards the Nubian-made wares) we may be dealing with the products of different factories producing closely similar but not identical products. It is clear in other cases, however, that variations in color and surface treatment represent deliberate variability in the products of the same factory – as demonstrated by the finding of unfinished white vessels and red vessels in the ruins of one factory (Adams 1961, 40-41).

To return briefly to my initial point, my classificatory scheme has been developed specifically from and for the study of Nubian ceramics, and within that context has adequately demonstrated its utility. On the basis of potsherds alone most Nubian sites can now be dated within an interval of a century or less; these datings have been independently verified by textual finds in a number of cases. At the same time, it has no *a priori* applicability to material from elsewhere. Its utility elsewhere would have to depend upon the existence of the same set of variables and the same hierarchy of co-variance – conditions which can never be taken for granted.

Taxonomic distinctness is not the only measure of significance

On the basis of the criteria thus far discussed (i.e. internal co-variance of traits) I could in theory distinguish more than 200 wares in the Nubian ceramic complex. In actuality, my typology comprises 105 wares. This anomaly is due to the fact that, in the light of external evidence, some of my potential wares have no distributional significance, and can be omitted on that basis from the typology. This point requires a few words of clarification.

Classification is not, or should not be, an end in itself; it is a method of ordering data to achieve a specific purpose. Pottery studies can be made for various purposes: functional, technological, stylistic, historical, and others. Probably the largest number of studies (specifically including my own) have been undertaken for reasons which are external to the material itself: they are undertaken because, in the absence of language and literature, we have come to accept pottery as our most reliable surviving index of the age of archaeological remains and the identity of their makers. Our purpose in these studies is not to learn about pottery per se, but to learn to use pottery to measure the age and identity of other remains associated with it. Such studies properly fall into the same 'ancillary' category as do dendrochronology, radiocarbon dating and the like.

For my purposes, then, typological significance is not simply a matter of recognizability and co-variance; it is also a matter of utility for identifying and dating sites. To have such utility, any ware, ware group or other taxon must have a definable distribution in time and/or space which is different from that of other wares or ware groups. In short, the internal criteria of taxonomic definition must co-vary with external criteria of distribution.

Many wares, which I once attempted to differentiate as separate entities on the internal evidence of distinguishability, did not prove to have separate distributional significance. For example, I found that before AD 600, the distinction between polished and matte surface finish has no significance. Although the two categories are quite easy to distinguish empirically (that is, they do not significantly intergrade); wherever you find polished vessels you also find matte vessels with the same forms and designs, and vice versa. Thus, knowledge that any particular vessel or sherd is polished or matte does not lead to any conclusions that could not otherwise be drawn, and the distinction becomes irrelevant. After AD 600, on the other hand, polished and matte vessels even in precisely the same forms do not always have precisely the same distributions. Polished wares almost entirely disappear between about AD 750 and AD 850, while the matte wares continue. Thus, a site which exhibits only matte wares in the Early Christian style can be dated at approximately AD 750-850, while a site with both matte and polished wares in this style would fall in the interval between about AD 600 and 750. Similarly, it is meaningful after about AD 1100 to distinguish between white, yellow and orange wares, whereas before that date no distinction can be made among vessels with these three slip colors on the basis of their distribution.

By the application of these external criteria of significance (i.e. utility for identification and dating) it has been possible for me to lump together various wares to which I formerly gave separate designation, (cf. the numerous Meroitic wares differentiated in Adams 1964, 157-161, and the much smaller number of wares from this period in Adams 1973, 9-10) and to ignore a good many distinctions which on purely empirical grounds I could and should make.

It is necessary to 'split' before 'lumping'

The foregoing does not mean that my now-discarded taxonomic distinctions were wrong, or even misleading; they were merely superfluous in terms of my own purposes. But, as already noted, I had very little distributional data to go on at the time when I began my studies, and I could not afford to assume *a priori* that any given variable did or did not have distributional significance. It was precisely because of such assumptions that I was led astray in my first effort to classify the Nubian material, and I was therefore particularly on my guard against them. As a precaution I decided to give a separate designation to every empirically recognizable ware, on the ground that it might prove to have a distinctive distribution in time or space. Only when this was specifically proven to be untrue by distributional evidence would I eliminate a ware from the scheme, by combining it with another ware having the same distribution.

Distributional evidence usable for my purposes (i.e. masses of sherds systematically collected over a wide area and accurately recorded as to provenience) came to hand chiefly through my own excavations and those of others who collaborated in the Nubian archaeological campaign. As it accumulated I was able, from time to time, to eliminate the superfluous wares from my scheme. Ideally, then, an initial period of elaboration and proliferation should have been followed by a consistent reduction and simplification as more and more distributional evidence came to light. In practice, unfortunately, the elimination of old wares has tended to be offset by the discovery of new and unexpected ones – all of which, according to my procedure, must be separately designated and presumed significant until proven otherwise.

Beyond any doubt my scheme still includes a large number of superfluous distinctions, for the distributional evidence from some periods in Nubian history is still highly inadequate. That is, we have not enough excavated sites from enough of the area to speak with confidence about what pottery was and was not used at times and in places. Since the flooding of Lake Nasser has put an end to excavations in Lower Nubia, there is little hope that the needed evidence will now be forthcoming.

Anything with a distinctive distribution is significant

A criticism which has occasionally been leveled at my approach to pottery classification is that it indiscriminately combines 'emic' and 'etic' criteria; that is, qualities which reflect the deliberate intent of the potter, and others which may be accidental and perhaps even unsuspected by the maker (Lister 1967, v). From the standpoint of many approaches to pottery study (particularly those which treat pottery as a cultural expression) this is a legitimate criticism, but with reference to the utility of pottery for dating and identifying sites it is irrelevant. Any quality which is consistently recognizable and which has a distinctive distribution in time and space is equally valuable, whether it is the result of design or of accident.

To take an illustrative case, the potters within Nubia always made their vessels from a soft brown silt, as we have already observed, while the potters at Aswan employed a fine, hard pink clay from a lacustrine deposit. Apart from this difference, however, the Nubians at times tried to imitate as closely as possible the products of the Aswan factory, for the latter enjoyed a high popularity. (During some periods they comprise as much as 50% of the total Nubian ceramic complex). So successful were some of their efforts that certain Nubian bowls are virtually indistinguishable from their Aswan prototypes as long as they remain intact, though the differences are immediately seen when internal properties are exposed in a fracture surface.

Presumably the Nubian potters realized that they were not duplicating the technological quality of the Aswan product, but the ultimate consumers probably often did not. Many a Nubian may have paid a fancy price for a locally made bowl on the assurance that it was 'straight off the boat from Aswan'. Under the circumstances, it is logical to ask why a distinction should be made between the Nubian and the Aswan products – particularly when the difference can be verified only in broken specimens (The distinction between the Nubian and Aswan vessels was in fact never recognized prior to my own studies). The fact is, however, that the Aswan bowls had been in production and in circulation for a century before the Nubians began imitating them; on the other hand, the Nubians kept up their manufacture for a century after the Egyptians had given it up. (This phenomenon of 'marginal lag' is characteristic of Nubian culture at most periods in history). Thus, a site having only Aswan-made bowls can be confidently dated between AD 500 and AD 600, a site with both Aswan and Nubian bowls between AD 600 and AD 750 and a site with only Nubian bowls between AD 750 and AD 850, more or less.

No typology is useful for all purposes

Apart from considerations already discussed, utility is also in part a matter of simplicity and manageability. It is chiefly with that end in view that I have sought whenever possible to eliminate superfluous taxa from my scheme. My colleagues still complain of its cumbersomeness, and I have to agree with them; in the field I still have to carry four large MS binders with drawings and descriptions with me, because I cannot keep my own taxa in my head. Yet by the very processes of simplification and 'lumping' I have robbed the scheme of some of its potential utility for other purposes. I cannot, for example, make the kind of microstylistic distinctions which are essential for studies like those of Deetz (Deetz 1967, 51) and Whallon (1972), although my imagination is very much stimulated by their work. Reading of it makes me wish at times that I had back the million or so potsherds which I left behind in Nubia, and which now 'exist' only as numerical tabulations on my tally sheets. Yet I am sure that, human capacities being what they are, I would have had to choose at the outset between the approach taken by Deetz and Whallon and the approach I took, and that what was gained in one respect would have been lost in another.

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Archaeological Classification: Theory Versus Practice[1] (1988)

This article is in most respects a digest of my longer book on the subject, Archaeological Typology and Practical Reality *(W. Adams and E. Adams 1991) and was written at about the same time. This was at the time when desktop computers were new on the scene, and some of the younger, computer-literate archaeologists had exaggerated expectations as to what they could accomplish. My purpose both in the article and the book was to show that the task of classification was far more complex than was generally recognized.*

Classification is fundamental to all artifactual archaeology, and no one who works with artifacts can be unaware of the doubts that surround many classifications. How similar are the similar things that belong together? How different are the different things that belong apart? What do the classes of similar things actually amount to? This paper looks at some fundamental questions of classification, believing that classification is too important a practical matter to be left to the theoreticians.

While there is a clear commitment to a single general model of archaeological classification . . . in-use classification is entirely different and of a much earlier vintage. Nowhere . . . is the archaeological record organized and understood in terms supplied by the means debated in the contemporary programmatic literature. The 'theoretical' literature has diverged from practice to such a degree that the two are now unrelated (Dunnell 1986, 150).

As a salvage archaeologist working in Nubia for more than 25 years, I have had to make, modify, and sometimes unmake more than half a dozen classifications, simply because there were none in place when I began working there (W. Adams 1962; 1964a; 1965; 1973; 1986b; etc.). The origin of my interest is therefore strictly practical. Like Dunnell, I have found that the literature on archaeological classification was of little help to me in the field situation. For one thing, it almost completely ignores the cognitive and linguistic aspects of classification; for another, it gives little consideration to the role of purpose in the making and use of classifications; finally, it begs the questions of practicality and cost-effectiveness.

I should make it plain at the outset that the views set down here are not those of a typical archaeologist or prehistorian, especially in the present generation. First of all, I had almost no formal training in archaeological theory or method. I was originally trained as an ethnologist (see W. Adams 1963, 127), and I had to teach myself archaeology after I was hired to do it, in Glen Canyon in 1957 (see W. Adams and N. Adams 1959; Adams *et al.* 1961). Second, during 30 years of field work I have been exclusively a reservoir salvage archaeologist, conducting a series of very large excavations (cf. W. Adams 1968; 1970; Adams *et al.* 1983) under severe limitations both of time and of money. Third, most of my work has been done by choice in large and complex townsites, with their enormous volume and variety of artifactual content. Fourth, my excavations since 1960 have been entirely in Third World countries that do not permit the export of collections for later study, so that all of my artifactual analyses have necessarily been carried out in the field.[2]

I should add finally that my ideas have been influenced by a continuing dialogue with my brother, Ernest W. Adams, a philosopher of science. Although we approach the subject of classification from nearly opposite directions, respectively practical and theoretical, we share in common a perception that there is little relationship between the theory and the practice of classification in either of our two fields (see E.

[1] Originally published in *Antiquity* 62 (234), 40-59.
[2] For a discussion of background, education, fieldwork and locations see Adams' autobiography (Adams 2009) [ed.].

Adams and W. Adams 1988). Our views on this subject are set out much more fully in our book (W. Adams and E. Adams 1991).

Historical background

In the Old World, a concern for classification is nearly as old as prehistoric archaeology itself (cf. especially Chapman 1985). For the most part, New World archaeologists did not develop the same interest until well into the 20th century. Since that time, however, they have generated an enormous amount of both theoretical and practical discussion about the use and abuse of archaeological classifications (cf. Brew 1946, 44-66); a discussion that shows no sign of ending, and that has come to be known collectively as 'the typological debate'.

The North American literature on archaeological classification has gone through three rather distinct phases. The archaeologists between 1920 and 1940 were happily and somewhat uncritically engaged in making and publishing classifications, mostly for culture-historical purposes (Willey and Sabloff 1974, 42-130). Insofar as there was any debate among them, it was mostly at a practical level. Was this or that 'type' sufficiently consistent to be regarded as a type? Was it correctly and adequately described? Was it really different from such-and-such another type? The authors paid little attention to theoretical or epistemological issues; they were simply creating a basic vocabulary for the systematic description and comparison of cultural units.

Around 1940 came an awareness that classifications were not answering all of the questions that archaeologists were beginning to ask (cf. Kluckhohn 1939; 1940). This did not lead the archaeologists of the 1940s and 1950s to abandon their field-proven classifications; rather, they sought new and more sophisticated ways to justify their continued use. This was the heyday of Functionalism and Configurationism, when American anthropology was redirected toward the search for functional or emic understandings of native cultures (Willey and Sabloff 1974, 131-177).[3] Following what was then current fashion, archaeologists began to argue that artifact classifications should help them to 'get inside the heads' of the artifact makers (Rouse 1939; Krieger 1944; Taylor 1948, 113-151; Spaulding 1953), and that archaeological 'phases', `foci', and 'cultures' should correspond to social or ethnic units that the people themselves would have recognized.

Most of the literature on classification before 1960 was generated by active field practitioners, who both made and used classifications on a daily basis. However, the 'New Archaeology' revolution of the 1960s brought to the front a new group of theoreticians, proportionately more interested in the theoretical than in the practical aspects of archaeology. The question that was now asked of classifications was: do they satisfy the canons of science? (Binford 1965; Fritz and Plog 1970; Hill and Evans 1972). With these concerns, which have nothing to do with practical utility, the 'typological debate' passed from the hands of the practitioner into those of the armchair theoretician, where it largely remains today. We have subsequently been treated to a number of programmatic statements that are almost impossible to translate into field practice.

The theoretical revolution had not progressed very far before it was overtaken by the methodological revolution of computer technology. This quickly became, and remains today, the tail that wags the dog of archaeological classification. Laying aside considerations of specific purpose or of specific problem, archaeologists indiscriminately borrowed and applied algorithms from the natural sciences, such as Numerical Taxonomy and Factor Analysis.[4] A decade of frustrating and largely unrewarding

[3] For additional discussion concerning the history of anthropological theory see Erickson and Murphy 2016 [ed.].
[4] For statistical factor analysis see further Basilevsky 1994, and additional statistical resources found here https://www.statistics-solutions.com/directory-of-statistical-analyses-cluster-analysis/ [accessed 23.06.2020] [ed.].

experimentation has led many of them to the recognition that the carpenter cannot work very effectively with the plumber's borrowed tools (cf. W. Adams 1986a), yet archaeologists have been slow to develop algorithms adapted to their own purposes (but see Whallon 1972). Even today, some still cling to the utopian notion of 'automatic classification' (Doran and Hodson 1975, 158-186; Brown 1982, 18-34).

Where once we had successful but rather inarticulate field practitioners trying to find words to describe what they were doing, we now have sophisticated theoreticians trying to find ways to do what they are saying; that is, to find practical applications for their computer-generated classifications. As a result, Kluckhohn's admonition of 1939 is more nearly true today than it was 50 years ago: 'typologies are proliferated without apparent concern for what the concepts involved are likely to mean' (Kluckhohn 1939, 33-38).

The foregoing review is highly simplified, as any brief historical sketch must be. In every period there was of course active discussion and disagreement, and some voices were always raised against the predominant trends of the times. Nevertheless, I believe that I have fairly described the main currents of the 'typological debate' as it developed in North America. My perspective corresponds in large measure with that expressed in the much fuller historical review recently published by Dunnell (1986).

The Nubian experience

My own contributions to the 'typological debate', here and elsewhere, belong conceptually to its earlier rather than to its recent phase. That is, I am a theoretically unschooled field archaeologist trying to find words to describe what I think I have been successfully doing. Like Eric Wolf, 'I don't consider myself a theoretician. My primary interest is to explain something out there that impinges on me, and I would sell my soul to the devil if I thought it would help' (quoted in Friedman 1987, 114).

In my earliest field work in the North American Southwest, I had the advantage of a great many previously developed classifications and typologies: most particularly the Anasazi cultural classification originally developed by A. V. Kidder (1927), and the various pottery typologies of H. S. Colton (1955; 1956; 1958; Colton and Hargrave 1937). Like other field archaeologists I soon became aware of deficiencies in these schemes, but I also found them sufficient for any purposes of mine (cf. W. Adams and N. Adams 1959, 17-26). But when in 1959 I was asked to organize the archaeological salvage campaign in Sudanese Nubia, soon to be inundated by the Aswan High Dam, I found myself back where Griffin, Colton, and other pioneer typologists had been in America half a century earlier. I had within a matter of months to organize survey and excavation programs in an area containing literally thousands of sites, ranging in age from Paleolithic to late medieval, with only a 50-year-old typology of graves (Reisner 1909) as a starting point. It was somehow necessary for me not only to devise a strategy for sampling so large and diverse a universe, but also to create a system for cataloguing the results, and for presenting them to the public. Before I had finished I made, modified, and sometimes unmade several pottery typologies (W. Adams 1962; 1964a; 1973; 1986b; MS-1; MS-2), a classification of house types (1977, 357-360), a classification of church types (1965), and a classification of Nubian cultural periods (1964b, 241-247). Most of these schemes grew from hasty and sometimes rather awkward beginnings, through successive refinements, until today they are in general use in the Nile Valley. My various typologies are nevertheless still undergoing modification, and will continue to do so as long as fresh material continues to be excavated (see especially W. Adams 1986b, 9-11).

Although they were originally developed with unusual haste during the salvage campaign, I believe that the 'natural history' of my various Nubian typologies is not essentially different from that of many other classificatory schemes now in use. It is this background of experience that I draw on, here and in my larger book (W. Adams and E. Adams 1991), both in describing the practice and in criticizing the theory of archaeological classification. But while in the larger book I am mainly concerned with describing the practice, my main concern in this article will be to criticize the theory. My objective is not to achieve a final resolution of the many theoretical problems that have been discussed in the course of the `typological debate', for I consider that most of them are really insoluble, at least through discussion. Nearly everything that has been said about classifications is true in some contexts and untrue in others; nearly everything that has been proposed will work in some circumstances and will not work in others. I simply want to see the 'typological debate' returned to the arena where it rightly belongs: to the realm of the field archaeologist who has not only to make classifications but to use them, day in and day out, year in and year out.

An overview of the problems

What, then, is wrong with the theoretical literature? The shortest answer is simply that it is theoretical, and therefore sidesteps the main problems that beset the field archaeologist. They are above all problems of information and of judgment, which cannot be resolved at the level of theory. But within the domain of theory itself there are a number of recognizable deficiencies. I will first enumerate them briefly, and will then return to a fuller consideration of each.

First, there is the lack of an adequate conceptual vocabulary. The single word 'classification' is used to designate several different kinds of conceptual structures, which I will hereafter call classifications, typologies, and taxonomies. The same word is also used to designate two quite different processes, which I will call classifying and typing.

Second, the literature fails consistently to recognize the extreme complexity of type concepts. They are not simply pigeonholes, nor are they the pigeons in the holes, nor the labels attached to the holes, nor the positions of the holes in the pigeon roost. They are a combination of all those things, and other things besides.

Third, there is a failure to explore the interrelationship of types on the one hand and typologies on the other. A typology is not merely a collection or a congeries of types; it is a system of types, which are defined partly in relation to the other types in the system. Moreover, the system as a whole possesses structural features that are imparted, *mutatis mutandum*, to the member types within the system.

Fourth, there has been a failure to analyze systematically the relationship between variables and attributes, on the one hand, and types on the other. Variables are of several different kinds – intrinsic, contextual, and inferential – and these differentially affect the definition of types (but see Gardin 1980, 65-68, 84-89).

Fifth, the potential contribution of statistics and computers to the making of typologies, or at least of practical typologies, has been very much exaggerated. A statistically significant attribute cluster is usually a necessary but not a sufficient condition for the designation of a type; statistical significance is also a matter of degree rather than something absolute.

Sixth, there has been a general disregard for the issue of purpose. While many archaeologists have acknowledged in principle that typologies are made to serve specific purposes, and have to be judged in relation to those purposes (e.g. Brew 1946, 65; Rouse 1960; Shepard 1965, 316), there has been no systematic exploration of the different kinds of purposes that archaeological typologies may serve, or of how these affect the selection of variables and attributes to be considered and not to be considered.

Finally, and most important from the standpoint of the salvage archaeologist, there has been a total disregard for the issue of practicality. It is not just a question of whether or not classificatory procedures are absolutely 'doable', it is also a question of whether they are affordable, and whether they justify the time and money spent on them.

The problem of terminology

As I have suggested above, the word 'classification' has many meanings. I will try to clarify the confusion of terminology by distinguishing between classifications, typologies, and taxonomies, and also between the processes of classifying, typing, and sorting, all of which are sometimes called 'classification'. At the outset I will be concerned only with basic classifications and typologies; those that lack a hierarchic feature. These are partitioning systems in which all of the units ('classes' or 'types') are treated as being at the same level of abstraction. Hierarchy, insofar as it is present, is a secondary and not a basic feature of most archaeological classifications; it is a way of manipulating types after they have been created.

Classification is the most general and inclusive term to be applied to conceptual systems. A classification, in my usage, is any matched set of partially contrasting categories, which exist in a state of 'balanced opposition' to one another. A classification is a kind of formal and restricted language (or more properly a lexicon), which differs from natural language in that it is a bounded system (that is, every classification is a classification of some things but not of others), its units ('classes') are conceptually equivalent, and no unit is synonymous with or wholly subsumed under any other.

A *typology* is a particular kind of classification: one that is made specifically for the purpose of sorting entities into mutually exclusive categories. According to this usage most folk classifications and even many scientific classifications are not typologies: they are made for purposes of communication and not for sorting entities into categories. Even in scientific communication it usually does not matter that the category 'red' overlaps with the category 'red-orange', or that the terms may or may not be used at the same level of abstraction, since we are rarely called upon to distinguish between entities because they are either red or red-orange.

Typologies, unlike other kinds of classifications, are often used as a starting point for statistical generalizations and comparisons; for that reason, they possess features not necessarily found in other classifications. First of all, they must be quite rigidly bounded, so that it is clear what is and is not to be sorted within the system. Second, they must be comprehensive: there must be one and only one category for each entity to be sorted. Usually this requires the inclusion of a residual, or 'none of the above' category. Third, the categories must be mutually exclusive, so that each entity is assignable to only one category.

Typologies have other systemic features, not obviously related to statistical requirements. These include the equivalence of units (the assumption that each type is equally important with every other type), the equidistance of units (ignoring degrees of similarity or difference between types), and independence of units (the assumption that the presence or absence of any type is not determined by the presence or absence of any other type). These assumptions often run counter to common sense, for we may know in reality that some types are more important than others, that Type A is more similar to Type B than it is to Type C, and that Type B is not likely to be present unless Type A is present. However, the assumptions of equality, equidistance, and independence are characteristic of all segmentary systems (cf. Durkheim 1893). A typology, we may say, is a segmentary system of entities, which for archaeologists are usually artifacts.

Notice that nearly all classifications of artifacts are typologies, according to my usage, while most classifications of 'cultures', phases, foci, and the like are not. Danubian III or Early Helladic are not rigorous sorting categories, such that a site or site component must be assignable to one and no other.[5] It is common practice, and perfectly understood for descriptive purposes, to say that the occupation of a

[5] Danubian III and Early Helladic designate relative cultural periods transitioning between the Late Neolithic and the Bronze age in eastern and central Europe and in Greece respectively [ed.].

site extended from Danubian II to Danubian IV, or that a particular component falls on the borderline between Early and Middle Helladic, or that a site looks to be primarily Anasazi, but with a strong Mogollon admixture.[6] This important distinction between cultural classifications and artifact typologies has been overlooked by many commentators, who have treated the two as conceptually homologous (cf. Brew 1946, 44-66; Taylor 1948, 113-151; Clarke 1968, 187-191). The present article is concerned specifically with artifact typologies; for discussion of the quite different problems involved in cultural classification see especially Klejn 1982.

Taxonomy is a term that I restrict to classifications and typologies having a hierarchic feature; that is, systems in which the basic units are either grouped into larger and more inclusive units, or (much less commonly) split into smaller ones. This practice is so universal in biological classification that it may be regarded as an essential feature (Mayr 1942, 317; Simpson 1945, 14-17; Beckner 1959, 55-80); indeed, biologists often use the terms 'classification' and 'taxonomy' interchangeably. But in archaeology the great majority of artifact classifications do not have a hierarchic dimension. Where hierarchy is present, moreover, it is nearly always a secondary feature; a manipulation of the basic types after they have already been designated. Generally, an archaeological taxonomy is simply a 'classification of classes'. Most of the time it is a way of indicating relationships between types; something that cannot be done in a basic or one-level typology because of the principle of equidistance of types.

Seriation must also be mentioned as a way of manipulating basic types. The process of seriation is often associated with classification (cf. Brew 1946, 44-66; Rathje and Schiffer 1982, 208-9), and many artifact types have been created specifically for purposes of seriation (e.g. Brainerd 1951; Ford 1962). Nevertheless, seriation is not itself a classificatory process. It is a linear ordering of types that have previously been created, but unlike taxonomy it does not produce new groupings, or classes. Taxonomy and seriation are the two principal ways of ordering types, but the former is itself a classificatory process while the latter is not.

Turning now to the processes involved in classification, we find again a confusion of terminology. The word 'classification' has commonly been applied to two quite different activities: those of creating categories (which I call classifying), and of putting things into the categories once they have been created (which I call typing and sorting). Allocation of a single entity to a type category is an act of typing, while the systematic allocation of a collection of entities into type categories is a process of sorting.

These quite separate activities have very rarely been distinguished in the archaeological literature. There has been, as a result, a failure to recognize that the problems involved in classifying and in sorting are fundamentally different. The difference can be likened to the difference that exists between making up the rules for a game, on the one hand, and playing the game on the other. No matter how precisely the rules are specified on paper, there are always plenty of controversies over their interpretation on the field.

Because cultural phenomena have a marked tendency to intergrade, the great majority of artifact types have to be defined in terms of norms, or central tendencies, rather than of absolute boundaries (cf. Kroeber 1964, 23-24). This means that, in the sorting process, there will always be difficult borderline decisions requiring arbitrary judgment. Most field archaeologists have to develop 'rules of thumb' in sorting that cannot be translated into precise type definitions. But the important thing is not to achieve conformity to some abstract definition, but to achieve consistency of practice, by the same sorter and between one sorter and another (cf. Fish 1978).

[6] Two neighbouring prehistoric native American cultures situated in the southwestern United States and Northern Mexico [ed.].

The complexity of type concepts

The 'typological debate' has been bedeviled from the outset by false or misleading dichotomies: between 'natural' and 'artificial' classification, between essential and instrumental types, between intuitive and rational types, between induction and deduction (Jevons 1874, vol. 2, 34-36; Fritz and Plog 1970, 407-408; Read 1974), between lumping and splitting (Judd 1940, 430; Brew 1946, 55; Taylor 1948, 126-127), between object clustering and attribute clustering (Whallon and Brown 1982, xvi-xvii), between paradigmatic and taxonomic ordering (Dunnell 1971b, 69-86), between empiricist and positivist classification (Hill and Evans 1972), and others too numerous to mention. In reality the great majority of types and typologies partake of all these qualities and involve all these processes. All types are essential in the sense that they are objectively definable, but instrumental in that we would not retain them if they did not serve some purpose; most of them are based to some extent on initial gestalts that are subsequently objectified by rational analysis (which however may vary from type to type in the same system; see Rouse 1939, 11; Krieger 1944, 279-280; Gardin 1980, 11), and they have usually evolved through a continual dialectic or feedback, between induction and deduction, object clustering and attribute clustering, lumping and splitting.

False dichotomies are, I believe, symptomatic of our failure to acknowledge the true complexity of the type concept. Every recognizable and useful type is, in the fullest sense, an intricate combination of a number of separate but essential components, which are partly physical partly mental, and partly representational. That is, a type necessarily consists of things, plus our ideas about those things, plus the words and/or pictures in which we communicate our ideas. According to my analysis, the following are all essential components of 'typehood'.

Type concept

This is the purely mental aspect of 'typehood': a body of ideas about the nature and characteristics of a group of entities, which make it possible for us to think of them collectively, and under a collective label. In archaeology our type concepts will nearly always involve two elements: a mental picture of what the type members will look like (type identity), and ideas about where the members are likely to be found, what function they may have performed and so on (type meaning).

Type description

A type concept, to be shared between two or more users, must be communicated in the form of a type description: a verbal and/or pictorial representation of the concept. For maximum utility of recognition, a type description will usually set forth most or all of the known characteristics of the type, whether or not these characteristics are diagnostic in any formal sense.

Type definition

Every type has many characteristics that set it apart from some other types, but only a few characteristics that set it apart from all other types. While a type description sets forth all the known characteristics that may be useful for recognition, a type definition sets forth only those characteristics that are, in combination, diagnostic. It is an interesting paradox that while classifying is theoretically a process of definition, in practice most archaeological types are never given precise, formal definitions. Instead they are given exhaustive descriptions, and it is assumed that the definition is embodied within the description. It is nevertheless necessarily true that every type, if it is to be regularly distinguished from other types, must be capable of exclusive definition. We may therefore say that every type theoretically possesses a type definition, even if it is often unstated.

Type name

Both type descriptions and type definitions are normally too long for convenient communication. Consequently, every useful type usually possesses also a name, comprising one or a few words that may be partly descriptive, but are never comprehensively so. It is the type name that we most often use in talking and writing about our types; it stands alike for the description, for the definition, and for the type members themselves (in a few typological systems the types are given labels instead of, or in addition to, type names (e.g. W. Adams 1986b, 65-68). A label differs from a name in that it lacks any descriptive component; it is a purely arbitrary number, letter, or other symbol, or a combination of those things. The advantage of a label is simply that it is usually shorter than a name, and therefore more convenient for various kinds of data coding.

Type category

A type may theoretically have a description, a definition, and a name, without reference to any other types. But according to my usage every type is also a sorting category; a pigeonhole in a larger system of pigeonholes, into which entities are put to distinguish them from other entities in other pigeonholes. Note that in conventional speech we do not say that an entity is placed in a type concept, description, or definition; we say that it is placed in a type category. A type category expresses the systemic dimension of 'typehood'; the location of a type within a larger segmentary system.

Type members

Obviously, a type with no actual members has no practical utility. There is no point in having a sorting category if there is nothing to sort. Type members are, then, simply the physical dimension of 'typehood'; the entities that have been identified as agreeing with the description and/or definition of a particular type, and have therefore been put into that type category, to be called subsequently by that type name.

In my view, the foregoing are all essential features of useful types. When we think or speak of a type we may have in mind any or several of the different components of 'typehood'. When we use the term 'Tsegi polychrome' we may be thinking on some occasions primarily of the type concept ('Tsegi polychrome is a stylistic variant of Kayenta polychrome'); on some occasions of the type description ('You can recognize Tsegi polychrome by its red and black designs on an orange slip'); on some occasions of the type category ('That bowl is Tsegi polychrome, not Kayenta polychrome'); and on some occasions of the type members ('Put the Tsegi polychrome sherds in the third drawer').[7]

Moreover, it cannot be assumed that any of the different constituents of 'typehood' is subsumed under, or implicit in, any other. Unfortunately for all our attempts at rigor and simplicity, the relationship between the different constituents of 'typehood' is mutable. It is possible at any time to add new finds to our collection of type members, not precisely identical to any of the previous members, yet without altering the older type description, definition, or name. It is also possible to change either the description or the definition or the name of a type, with or without the addition of new type members (cf. Leach 1976, 19-22). The most useful types generally evolve through a continuous process of tinkering, in which we try to bring our concepts, our objects, and our descriptions of them into closer and closer congruence with one another (cf. Foucault 1973, 132).

In addition to the constituents of 'typehood' that have just been discussed, there are also two basic properties of all useful types: those of identity and of meaning. Obviously, every type that is to serve

[7] Pottery types from Ancestral Puebloan culture in the southwestern United States. For examples and further discussion see https://swvirtualmuseum.nau.edu/wp/index.php/artifacts/pottery/tsegi-orange-ware/kayenta-polychrome/ and https://swvirtualmuseum.nau.edu/wp/index.php/artifacts/pottery/tsegi-orange-ware/tsegi-polychrome/ [accessed 23.06.2020] [ed.].

any useful purpose must first of all be recognizable, but it must also have some meaning relative to some purpose. A type comprising all pottery made at Chaco Canyon between 1050 and 1100 AD would be a highly meaningful but (unfortunately) unrecognizable type, while a type comprising all potsherds exhibiting scratch marks would be readily recognizable but without meaning.

Both identity and meaning are relative, though in different ways. Types have identity relative to other types in the same system. A type may be distinguishable from all other types in its own typology, but at the same time may not be consistently distinguishable from one or more types in another typology. Meaning on the other hand is relative to the purpose of the typology. The same type may be meaningful in one typology, but useless in another. A pottery type consisting of all bowls is meaningless in a typology made for dating purposes, since bowls were made at all periods of history, but it may be important in a functional typology.

Note that entities have both individual and typological identity, and both individual and typological meaning. Every entity has a combination of characteristics that make it distinguishable from all other entities, and its individual identity is the sum of those characteristics. It also has a more limited range of characteristics that identify it as a member of a type, and its typological identity is the sum of those characteristics. Every entity likewise has (or in the case of artifacts had) a meaning for its makers and users, but this may be quite different from the meaning that it has for us by virtue of its membership in a type. We need always to keep in mind that artifacts were made and used by bygone peoples for purposes of theirs, which we may or may not be able to fathom, but typologies are made by us for purposes of ours.

Take for example the case of a red and orange bowl, made by a prehistoric Indian woman in Arizona in the 13th century AD. We believe that it was made primarily for holding and serving cornmeal mush, though it also and incidentally proclaimed the skill of the potter and showed her neighbors that she had nice household furnishings. All these were the individual meanings of the bowl. But the pottery type Tsegi polychrome, to which this vessel has been assigned, was created by the archaeologists Colton and Hargrave in 1937 (Colton and Hargrave 1937, 96), and the typological meaning of the vessel is 'pottery made in Tsegi Canyon between 1225 and 1300 AD'. Obviously, these are issues of overriding importance to the archaeologist, but about which the pottery makers and users could hardly have thought or cared.

A final point to be made about the complexity of type concepts is that most of them are polythetic. In the great majority of typologies there is no single criterion or uniform set of criteria specifying what constitutes a type. Some types are defined by some criteria, and some by others. On this point at least there has been significant progress in the last 20 years, for the polythetic nature of archaeological types has been generally if not universally acknowledged since it was first pointed out by David Clarke in 1968 (1968, 189-191).

The relationship of types and typologies

Just as there is no word except in relation to a language, so there is no type except in relation to some specific typology, which partly sets the rules by which the type is formulated. Moreover, since a typology is a system and not merely a collection of types, it must be coherent as a system, but at the same time the individual types must be coherent and meaningful as types. Neither of these two requirements necessarily implies the other.

In some cases, the systemic factor has been ignored, and we find that the 'validity' or identifiability of individual types is discussed without any reference to other types in the same system. As a result, there are typologies that include partially overlapping types. In Griffin's Fort Ancient pottery typology (Griffin 1943),[8] for example, it is possible to assign the same potsherd to any of two or three types.

[8] Fort Ancient is a designation for a Native American culture present in south and southwestern Ohio. See also Cook 2017.

In the case of computer-generated typologies, the shoe is on the other foot. Computers will not tolerate systemic ambiguity, and the erstwhile `typologies' that they turn out are perfectly logical and coherent as systems. But computers also cannot judge the issue of relevance, and, more often than not, the individual 'types' they generate are meaningless with reference to any specific purpose (cf. Dunnell 1971b, 97-98).

Variables, attributes, and types

In this area there has been significant conceptual progress since the advent of computers. All computerized classification programs require a systematic coding of attributes, and this has led to a more rigorous analysis of attributes, and their relationship to one another, than was commonly undertaken earlier. Indeed, in the older literature there is often a failure to distinguish between variables and attributes; the two terms are used interchangeably. Today we are all aware that variables are, in the broadest sense, dimensions, while attributes are in the broadest sense measurements on those dimensions. To give one of the most commonly cited examples, 'color' is a variable, while 'red' is an attribute of the variable 'color'.

Several further distinctions can be made. The difference between variables in any classification is essentially qualitative (as for example between color and shape), while the difference between attributes of the same variable is essentially quantitative. That is, differences in size, shape, color, etc. can usually be expressed in some form of measurement. Second, variables are omnipresent; they are manifest in one or another of their attribute values in every entity that is classified within the system. (Note however that a zero value, meaning absent, is one of the potential attribute values of many variables). Attributes on the other hand are only exclusively present; the presence of any one attribute of a particular variable automatically means that all of the other attributes are absent. (No object can be simultaneously red and green, 10 and 20 cm long, etc.). Third, variables are generally or at least potentially independent of one another; the form that one takes does not necessarily affect the form that another takes. On the other hand, attributes are interdependent, since the presence of one excludes all the others.

Finally, and most importantly, there are differences in the way in which variables and attributes are normally selected. Both are nearly always chosen by the classifier from a wider field of possibilities, but the basis of selection is somewhat different in the two cases. Variables are selected primarily with reference to meaning, so that, for example, the variable 'color' is usually included in pottery classifications but not in lithic classifications, because it is considered meaningful in the former but not in the latter. In the case of 'chipping' just the opposite is true; it is considered meaningful in the case of stone tools but not of clay pots. In both cases the selection or non-selection of the variables is dependent not on their recognizability, but on whether or not they can be expected to yield useful information for some specific purpose.

Attributes on the other hand are selected primarily on the basis of identity; whether or not they can be consistently distinguished from one another. We may choose 'color' as a variable in pottery classifications because we have found it useful, but whether we then subdivide 'color' into just 'red' and 'brown', or into 'red', 'brown', 'tan', and 'grey', or into still finer subdivisions, depends mainly on whether separate color norms can consistently be distinguished. In short and in sum, variables are selected mainly on the basis of criteria of meaning, and attributes on the basis of criteria of identity.

An important corollary is that variables can usually be selected without reference to any particular body of material, while attributes usually cannot. Before selecting attributes, we must first find which ones are present, and which ones can be regularly distinguished, in the material to be classified. Consequently, classifications cannot be made wholly a priori by a process of 'attribute clustering', as has sometimes been claimed (Dunnell 1971b, 70-76). A certain amount of 'object clustering' must first be done to see what attributes are available, and distinguishable.

If computer programming has clarified our understanding of variables and attributes, and their relation to one another, it has not necessarily enhanced our understanding of the relationship between either of those things and the types that they are meant to define. On the contrary, computer programs have tended to create the belief that attribute clusters are types, whether or not they have any physical representatives, whether or not they can conveniently be recognized, and whether or not they serve any useful purpose (Dunnell 1971b, 97-98). This rather arbitrary view arises from the practice of making types by attribute clustering, something that was rarely done before the advent of computers. Most of the earlier artifact types were made initially by object clustering, and the development of a typology commonly involved stepwise differentiation, as one type after another was removed from a diminishing residue of unclassified material.

Either attribute clustering or object clustering or simple intuition can serve as a starting point for a typology, but no one of them is sufficient unto itself. If we start with a theoretical definition (i.e. one made by attribute clustering) we will sooner or later have to determine whether there are actual objects corresponding to our definition, whether they can be consistently distinguished from the members of other types, and what is the full range of their characteristics. On the basis of those findings we may want to alter or expand our initial definition, then add new material to the original collection, further refine the definition – and so on *ad infinitum*. This is what I have called the dialectic of typology development (for much fuller discussion see W. Adams and E. Adams 1991).

What computers and attribute clustering have not done is to solve or eliminate the fundamental need for selection. A typology incorporating all possible variables and attributes is probably a physical impossibility (cf. Dunnell 1971a, 117) even for a computer, and in any case it would certainly yield tens of thousands of `types'. A computer program can help in the selection of attributes because it can show which ones do and do not co-occur or co-vary with something else. But it cannot help in the selection of variables because this depends on what we want to learn – something the machine cannot judge for us (Shepard 1965, xii; Thomas 1978).

In the selection of variables, it is helpful to consider the different kinds of variables (and their constituent attributes) that may be involved in archaeological classifications, and the different roles they may play. The variables and attributes that are regularly considered in artifact typologies, I suggest, fall into three categories: intrinsic, contextual, and inferential. *Intrinsic attributes*, like size and shape, can be determined by the direct examination of objects. *Contextual attributes* do not inhere in the object, but in the context of its finding, which may disclose the date and place of the object's manufacture and use, and its association with other kinds of remains. The time range and the geographical range of artifact types, so often included in type definitions, are contextual variables. *Inferential variables* cannot be discovered empirically; they are inferences that we ourselves make from the morphology or the context of the objects, or both. They include such things as function and emic significance.

These different kinds of variables and their attributes combine in different ways to make different kinds of classifications. A purely morphological classification is based on intrinsic attributes alone, while a historical classification combines intrinsic and contextual attributes, and a functional or an emic classification combines intrinsic and inferential attributes. (For additional discussion, employing somewhat different terminology, see Gardin 1980, 65-68, 84-89).

The role of statistics and computers

Albert Spaulding first introduced a statistical dimension into the 'typological debate' in 1953 (Spaulding 1953). Spaulding's position was a simple and fundamental one: in the analysis of any given assemblage of material, every valid type is defined by a statistically significant attribute cluster, and conversely (and much more controversially) every statistically significant attribute cluster corresponds to a valid type, since (a) it is empirically recognizable, and (b) it must mean something. Spaulding advocated his statistical approach only for the analysis of internal variability within assemblages, not for comparison between assemblages, but since the advent of computers it has been much more generally applied to the making of all kinds of archaeological typologies (see Whallon and Brown 1982 for some examples of these).

Largely overlooked in the early years of statistical discussion was the fact that statistical significance is a matter of degree. Attribute clusters were judged to be either 'random' or `nonrandom', ignoring the fact that 'nonrandom' associations can run the gamut from very strong to very weak (cf. Thomas 1978, 233; 1986, 46-54). Indeed, mathematicians are beginning to wonder if anything in the universe is totally 'random' in a statistical sense (cf. Kolata 1986). On this issue computer clustering programs have been genuinely helpful, since they can quickly discover the relative strength or weakness of different attribute clusters.

In most situations, however, a statistically significant attribute cluster is a necessary but not a sufficient condition for the designation of a type. It is moreover a condition that is met so easily that it can often be taken for granted (cf. Watson, LeBlanc and Redman 1971, 127; 1984, 203; Thomas 1978, 236). No matter what variables and what attributes are chosen for inclusion in a particular typology, they will almost certainly combine in dozens, and more probably in hundreds, of clusters that are statistically significant at some level of confidence, but that have no utility for the purposes of the typology.

Attribute clusters must satisfy at least two other conditions besides that of 'non-randomness' before they can be taken as definitive of useful types. First, the attributes that are non-randomly associated must be independent of one another, so that one does not automatically cause the other. The ceramic analyst will always find a strong correlation between high-iron clays and a red fabric color, because high-iron clays always turn red when fired in an oxidizing atmosphere. The constant association between iron clay and red color therefore does not provide a basis for the designation of a useful pottery type, despite its obvious statistical significance. As Gordon Childe (1956, 35) rather neatly put it, 'the significance of a type . . . is proportionate to its improbability'.

Still more important is the fact that an attribute cluster, no matter how consistent, must have some meaning or utility, relative to the purpose of the typology, before it can be equated with a type. If our typology is developed for culture-historical purposes, as were many of the earliest artifact typologies, we need to give type designation to those attribute clusters that have finite and discoverable distributions in time and space, but not to clusters that occur all over the world and all through history.

The question of utility, relative to a particular purpose, commonly affects our decision to split or not to split; a problem that continually confronts the typologist in practice (Kidder and Shepard 1936, xxv; Brew 1946, 55; Taylor 1948, 126-127). If two pottery types are alike in all but one characteristic, they nevertheless represent two statistically distinct attribute clusters. But if they always occur in the same contexts and with the same associations, giving them designation as separate types would probably serve no useful purpose.

A careful initial selection of variables and attributes, eliminating those that do not serve the purpose of our classification, in theory reduces the number of statistically significant but meaningless attribute clusters. The difficulty in practice is how to know in advance just which variables and attributes are relevant. This is particularly true in culture-historical classifications, where we are looking for types that have a finite and discoverable distribution in time and space. In the beginning we often do not know which are the variables that do and do not vary over time in our particular area; hence we must start by considering a great many. It follows that in the development of the typology we are likely to designate a good many provisional types that will later be discarded (or lumped with other types), when they prove to be meaningless for our purposes. The early archaeological literature is full of erstwhile 'types' that later disappeared for just this reason; something that is normal and inevitable in the dynamic of typological development. As Simpson (1945, 13) reminded us, all working typologies are in some sense experimental.

But if we ignore altogether the question of purpose, and try to code all of the recognizable variables and attributes, our reward will be a proliferation of significant but meaningless types (cf. Dunnell 1971b, 97-98; Voorrips 1982, 111). This has been the besetting sin of a great many computerized programs up to now, and above all of Numerical Taxonomy programs (see Sackett 1966; Clarke 1968, 512-634; Doran and Hodson 1975, 173-185). More than 15 years ago, R.M. Cormack remarked: 'The availability of computer packages of classification techniques has led to the waste of more valuable scientific time than any other "statistical" innovation (with the possible exception of multiple-regression techniques)'. The author went on to ask, 'how in practice does one tailor statistical methods to the real needs of the user, when the real need of the user is to be forced to sit and think?' (Cormack 1971, 321, 346).

In the great majority of archaeological typologies, the different types are the equivalent of apples and oranges. The differences between them are essentially qualitative, and they cannot be derived through any process of measurement. Thus, the primary value of statistics and computers lies not in the making of types, but in their subsequent manipulation, through seriation or taxonomic ordering. These processes, unlike that of classifying, are concerned with the relationships between types; something that is essentially quantifiable. Statistics and computers have played a genuine and important role both in seriation and in the development of taxonomies (Brainerd 1951; Ford 1962; Kendall 1969; Cowgill 1972; Marquardt 1978) – fields in which archaeologists have developed their own algorithms, rather than uncritically borrowing the tools of other disciplines as they have done when classifying (cf. W. Adams 1986a).

The question of purpose

We come now to the issue that should properly be the starting point for any discussion of classification: what good is it? The distinction must first be recalled between classifications in general and typologies as a special subclass of classifications. A great many classifications are intended for no other purpose than that of communication, but a typology is not. It is made for sorting entities into discrete and mutually exclusive categories; something that is unnecessary for communication. Why, then, do we do it? What do we hope to accomplish? These are the questions that must be asked at the outset in making any typology. If the task is properly understood, it will determine which variables and attributes will be considered, and which attribute combinations will be found meaningful, and designated as types. The measure of validity for any type will not be merely, 'does it communicate?' but 'does it work for our purpose?'

Archaeologists have often acknowledged, in principle, that typologies must serve a purpose (Brew 1946, 65; Rouse 1960; Dunnell 1971a, 115; Hill and Evans 1972, 235), yet they have seldom explored the practical implications of that fact. What I want to do here is very briefly to review some of the different purposes that archaeologists may have in mind when they make a typology, how these affect their choice of variables and attributes, and the different kinds of typologies that may result.

Lost in the recent discussion of scientific legitimacy in archaeology (e.g. Fritz and Plog 1970; Binford 1972, 187-326) has been the fact that classification is not intrinsically a scientific process. Like the development of any other kind of language, it is a prescientific process, necessary to create a basic vocabulary that can be used alike for scientific and for nonscientific purposes. Even within the field of archaeology, many of the commonly used and widely accepted typologies have not been created for strictly scientific purposes; some have been developed to meet such mundane but essential needs as that of filing artifacts in museum cabinets and drawers.

Archaeological typologies serve many purposes, but they fall into two broad categories, which I call basic and instrumental. Basic purposes involve learning or expressing something about the classified material itself; instrumental purposes involve using the classified material as a means to some other end (e.g. the dating of an archaeological deposit).

Basic purposes

Two of the most common basic purposes are descriptive and comparative. Descriptive typologies are most often developed simply for convenience or economy of reporting. It is a rare editor or publisher who will allow enough pages for the individual description of every artifact; we have to describe and illustrate them in batches, which usually means by type (Krieger 1944, 27-33; Taylor 1948, 176; Everitt 1974, 4). Comparative typologies permit the comparison of finds from one site or component to another, using a standard set of categories (Krieger 1944, 273; Deetz 1967, 51). Either case usually calls for a purely morphological, or phenetic, typology, involving only intrinsic attributes.

Typologies may also be developed for various analytical purposes, which in the broadest sense are also basic purposes. To discover how much variability exists within our material, a very detailed morphological typology may be needed. Exploring stylistic variability requires a stylistic typology, which may ignore many features of non-stylistic variation. The history or chronological development of our material calls for a historical typology, combining intrinsic and contextual attributes (i.e. evidence of distribution in time and space, combined with intrinsic attributes). Functional analysis requires a functional typology, which combines intrinsic and inferential attributes, from which, at least in theory, an emic typology can be developed (Chang 1967; Heider 1967; Read 1982), combining intrinsic and inferential features of another kind.

Instrumental purposes

Instrumental purposes can be designated as either ancillary or incidental. An ancillary typology is developed as an aid for some task not related to the classification itself. In archaeology, by far the most common ancillary typologies are pottery typologies that are used for dating sites or components. In these cases, accurate datability is one of the most important requisites of a good ancillary type, and the typology may exclude altogether types that cannot be dated. Nowadays, ancillary typologies made for dating purposes are usually used in conjunction with seriation techniques, to produce type-frequency seriations (e.g. W. Adams 1986c; Marks and Robertson 1986), which are normally much more sensitive time indicators than are types individually.

Incidental purposes, in my usage, relate strictly to practical convenience and not to any need for information. In archaeology these are most commonly reflected in systems for storing artifacts and for filing artifact records. My own museum experiences suggest that incidental purposes have sometimes played a larger role in the development of archaeological typologies than is generally recognized, because it is often necessary to develop a system for storing artifacts before it is necessary to develop a system for describing them. Systems developed for convenience of storage may in time acquire a life of their own, influencing the subsequent development of systems for description and analysis.

Multiple purposes

The problem of multiple purposes requires a word of further consideration. An ubiquitous error in archaeological classification is the assumption that there is a single 'right' classification, equally useful for all purposes (cf. Jevons 1874, vol. 2, 34-89; Brew 1946, 65; Hill and Evans 1972, 23-25); one might as well say that there is a single right scientific language, equally useful for physics, biology, and anthropology. Yet many typologies do work for more than one purpose, and typologies initially made for one purpose often acquire secondary purposes along the way. My own Nubian pottery typologies, originally developed strictly for ancillary purposes, were subsequently elaborated for historical and art-historical purposes (W. Adams 1986b, 3-11), partly by expanding the original type descriptions and by doing a certain amount of type-splitting, but mostly by introducing a hierarchic (taxonomic) feature into the system. In this way I was able to group together types (called wares in my system) that were developed for dating purposes into ware groups and families that were developed to indicate historical and stylistic relationships, without at the same time losing sight of my primary objective of dating. The taxonomy, in short, permitted the introduction of different purposes at different levels.

Open and closed typologies

Before leaving the subject of purpose, there is one further and critically important distinction to be made, between open and closed typologies. An open typology, though developed originally from a certain discrete body of material, is designed also to process additional material in the future. It must possess a degree of flexibility: a capacity for the addition, the deletion, and the modification of types as new material comes to light. From the standpoint of the field archaeologist, nearly all useful typologies fall into this category.

Closed typologies, also derived from a specific body of material, are intended to apply to no other material, usually because their categories are rigidly set and immutable. Systems of this kind are commonly developed in the laboratory, after the conclusion of fieldwork; they are par excellence the tools of the laboratory analyst. A good many typologies developed for descriptive convenience are of this type. For example, nearly every Egyptian cemetery excavation report has its own typology of beads (e.g. Reisner 1923, 1067; Steindorff 1935, 46-50; Emery 1938, pls. 43-44), but I have never found that any of these systems were adequate to describe the beads from my own excavations. It is also true that virtually all computerized typologies are of the closed variety, because of their lack of flexibility.

The bottom line: Practicality

American archaeology has recently emerged from an era of unprecedented (and very uncharacteristic) affluence, when there was usually little need to scrutinize the cost-effectiveness of fieldwork and its associated analytical procedures. Those happy days are over, and most archaeologists now have to consider very carefully whether their proposed field and laboratory activities are justified by the expected results. Typologies, like other archaeological procedures, are not ends in themselves but means to an end, and the ends must justify the means. It is not only a question of whether or not the procedures are 'doable', but whether they are affordable (considering what else could be done with the same time and money), and whether in any case the results justify the expenditure.

Issues of practicality and cost-effectiveness may arise in regard to both classifying and sorting. The first question is simply whether we need to classify at all. There is no rule that artifacts must always be classified, and in a body of really diverse material an attempted classification may actually hinder effective description. I would suggest a contrary rule: that classification should only be undertaken when the classifier has some purpose clearly in mind, and is reasonably confident that the classification will assist that purpose.

If classification is decided on, the next question must be, 'what kind of classification?' – the issue dealt with in the previous section. But there is still a third practical question, 'how much rigor or precision is required?' Many archaeological typologies strive for maximum precision in the definition and description of types, even though this may not be necessary for any demonstrable purpose. The result is often the designation of more types than are necessary, with a consequent encumbrance of the sorting process.

An example of misplaced effort, in my judgment, is the common practice of illustrating hundreds of pottery vessel rim profiles. It is frequently not clear whether this is typological data or not, since we are not told whether each drawing represents one vessel, or the norm of a group of vessels, or (in the latter case) how many vessels are represented. However, there seems usually to be an implication that a group of similar vessel drawings is meant to illustrate a 'type'. But of what practical value is such a cumbersome 'type description'? The drawings define neither the norm nor the boundaries of the type, and they certainly do not tell us everything we need to know about it. I have never known anyone to make practical use of this data, which consumes hundreds of hours in the compilation.

The question of affordability must be judged in relation to what else could be done with the same money. A high degree of precision in the definition of pottery types can be achieved by neutron activation analysis, and the results of such analysis are very often included in pottery type descriptions nowadays. They can be useful as incidental information, indicating in many cases where the pottery type was made, and they can sometimes help us to decide what we should be looking for in the analysis of fabric. But if the definition of the type and the recognition of its members depends on neutron activation analysis, then it is almost certainly going to cost far more than the results could justify, since the cost of analyzing every individual sherd will, in the aggregate, run to hundreds of thousands of dollars. In Egypt I can purchase 50 man-days of labor for the cost of analyzing just one sherd.

For the field archaeologist, practicality in sorting can be just as important as practicality in classifying. Where large amounts of material are to be dealt with (as in the case of potsherds and lithic debitage), any typology, no matter how rigorous, must translate into relatively simple and rapid sorting procedures. This usually means that the designated types must be readily recognizable without recourse to instruments.

An important but little-considered question is how much accuracy is required in the sorting process (cf. Fish 1978). In the Nubian excavations at Qasr Ibrim we normally have to sort between 3000 and 5000 sherds each afternoon. The task cannot be postponed to a later time, because in this very complex townsite we continually need the sherd distribution data to tell us where we are, chronologically, in the refuse deposits, and where, consequently, we want to dig and not to dig next. 'Today's sherds determine tomorrow's excavations' (see W. Adams 1984).

With so much material to contend with, we naturally tend to sort at a very high speed, taking usually about 15 minutes per 500-sherd basket. We do not pause to agonize over borderline cases, but make instantaneous, arbitrary decisions and keep going. Experimental results have shown that the errors resulting from very rapid sorting can sometimes affect as much as 10% of the total body of sherds. We have also found however that these errors almost never affect our frequency seriation calculations, because the errors show a high degree of randomness. More careful sorting would create a continually increasing backlog of unsorted material, without significantly aiding us toward our ultimate goal of dating the archaeological deposits.

Another example of misplaced effort is the relatively common practice of weighing potsherds instead of, or in addition to, counting them, which can add substantially to the time required for processing and recording. Where vessels of the same type vary substantially in size, neither weighing nor counting provides anything like an accurate indication of the numbers of vessels originally present, and the extra effort of weighing is only worthwhile if the weights provide a more reliable estimate of the original vessel numbers than do the numerical tallies. In Northern Arizona, King (1949, 109-114) and Colton (1953, 59-60) found that results obtained by counting and by weighing were so little different that they gave up weighing.

Some further issues

Space limitations have precluded the consideration of many additional issues in the present article. One of these concerns the mental processes through which type concepts are originally formulated and subsequently modified; a subject on which cognitive psychologists have had much to say that is relevant to our own endeavor. If we acknowledge that types involve any mental component whatever, then it is obvious that we ought to try and understand our own thought processes (cf. Ellen and Reason 1979, vii).

A related issue concerns the structural features of typologies, which have much in common with the segmentary systems that are basic to all human (and animal) social organization. On this subject the writings of Mary Douglas (e.g. 1973; 1975), Edmund Leach (1972; 1976) and others, with reference to categorical systems and the maintenance of categorical boundaries, are not without relevance to the study of archaeological typologies. They may at least help to explain the fervent and unyielding commitment that archaeologists often feel toward particular typologies to which they have become habituated.

Still another issue has to do with the dialectics of type development; something I have only hinted at in these pages. Types, like laws, are in the public domain; they do not remain exactly as they were first proclaimed, nor are they the exclusive property of their makers. If they have any utility they soon pass into the general discourse, and thereafter may acquire meanings and purposes that were neither intended nor desired by the makers. Very few of the original Linnaean types remain as Linnaeus described them; indeed, the very purpose of his classification has shifted, from a phenetic to a genetic taxonomy (Mayr 1942, 108-113; Simpson 1945, 4).

Finally, there is the much-discussed but generally misunderstood issue of the relationship between classification and explanation, prediction, and understanding. Archaeologists in the 20th century have continually shifted their expectations in regard to the classificatory endeavor, demanding in the classificatory era (1920-40) that it be predictive, in the functionalist era (1940-60) that it provide understanding (Krieger 1944; Taylor 1948, 113-151), and in the nomothetic era (1960-) that it provide explanation (Fritz and Plog 1970; Binford 1972, 187-326; Read 1974). In and of itself it can do none of those things. The best way to understand the classificatory process in this context is to think of it as qualitative measurement (cf. Levi-Strauss 1953, 528). Like quantitative measurement it can neither predict nor explain, but it is a tool that can lead to improved prediction, explanation, or understanding if properly used. And the value of any type and typology, like that of any other measure, can only be judged by its utility.

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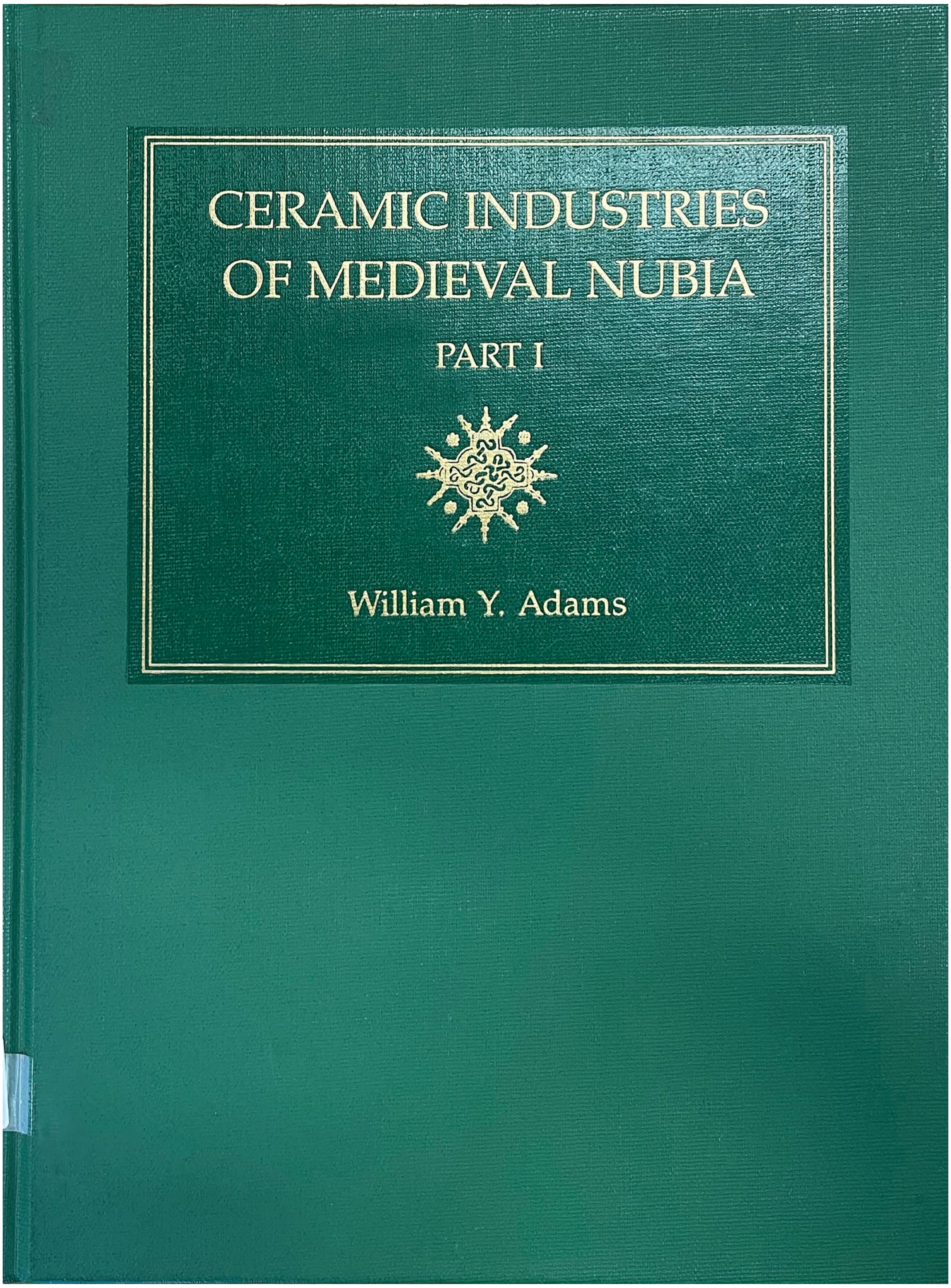

Plate 9.1. *Ceramic Industries of Medieval Nubia* published in 1986 by W. Y. Adams is a typology for classifying pot sherds based upon those recovered during the excavation of various medieval sites in Sudan and Egyptian Nubia.

Purpose and Scientific Concept Formation[1, 2]

(1987)

Ernest W. Adams and William Y. Adams

This article is another spinoff from the much larger volume that my brother and I co-authored (Adams and Adams 1991). In the preceding chapter I discussed the problems of classification from the perspective of the archaeologist. Here my brother, a philosopher of science, discusses them from the perspective of logical reasoning. This was published in the British Journal for the Philosophy of Science; *thus for an audience unfamiliar with the methods of archaeology. This will explain the didactic style, which is not mine.*

1 Introduction

The scientific concept formation with which we are concerned is that which occurs when technical terms or systems of terms are introduced or deliberately modified by scientists in the pursuit of their scientific objectives. We will advocate a 'philosophy' of this sort of concept formation in which the purposes for which the terms are introduced and employed are central and various features of their introduction and use are explained 'functionally' in terms of these purposes. We will also argue that many of the qualities that are often thought to be definitive of the scientific are 'accidental features' that are fairly well approximated in certain cases, but insistence that all scientific concepts should possess these qualities can also be counterproductive to the actual and legitimate purposes of many scientific activities. Among these stereotypes are that scientific concepts should be precise, objective, and subject to observational determination (the latter two have been extensively criticized in the Kuhnian tradition, but we will criticize them here from a different point of view). The failure to recognize that these qualities are desirable only to the extent that they serve scientific purposes, and they are not ends in themselves, stems from the failure to recognize the purposes for which concepts are employed and from mistaking properties that are frequently approximated for attributes that are essential to the scientific.

[1] Originally published in December 1987 in the *British Journal for the Philosophy of Science* 38 (4), 419-440. The British spelling used in the original publication has been maintained here [ed.]. The authors are greatly indebted to Professors Kent Holsinger and Paul Teller, and to an anonymous referee, for discussions and suggestions on earlier drafts of this paper [E. W. Adams and W. Y. Adams].

[2] Modifications or clarifications where added to this paper by W. Y. Adams in 2019, have been noted and included in square brackets [WYA 2019] [ed.].

Something very close to the foregoing was previously advocated by E. W. Adams (henceforth 'EWA') in the special case of systems of quantitative measurement (EWA (1966)). In the present paper we will concentrate on a different kind of concept formation, namely the construction of scientific *typologies*, and we will select a particular one of these as a case-study. This is an archeological typology developed by W. Y. Adams (*WYA's system*, WYA (1986a))[3] which involves the classification of pottery fragments (potsherds) recovered in the excavation of archeological sites in the Nubian region of the Nile valley; data which will be used for the purpose of estimating the ages of these sites. We will begin by describing this system in some detail and drawing attention to certain features of it that appear aberrant when judged by conventional stereotypes. Then we will attempt to explain how the seemingly aberrant features in fact serve the purposes for which the typology has been evolved. After that we will consider briefly the question of whether the conclusions we arrive at concerning WYA's system apply to other typologies, and to scientific concept systems in general. In particular, we will consider biological species classification, and one rather elementary quantitative concept system, namely the Mohs scale of hardness measurement. Except for occasional brief asides on the relevance of our findings to more general issues in the philosophy of science, we will eschew comment on these matters since we plan to discuss them in future publication.[4]

Before starting a word should be said in justification of our focusing on a case-study in one of the least exact and least formally sophisticated sciences, which therefore could seem to be too unrepresentative to draw general conclusions from. There are two things to be said in favour of this. One is that it is essential to study processes of scientific concept formation 'from the inside', in a way that allows the objectives of the scientists evolving and employing them to be ascertained with some assurance. That is something which we can do given that the author of WYA's system is one of the authors of this paper. The other point is that it is often less easy to discern the rationale for aspects of mathematically sophisticated concept systems than it is to do this in the case of simpler ones, simply because in the case of complex systems one's entire effort is often devoted just to mastering their technicalities. We will try to make it at least plausible that conclusions about a concept system in a very 'down to earth' science generalise to some extent to more sophisticated sciences, but it is important that the case-study that illustrates them should be understandable apart from technicalities.

2 WYA's typology of Nubian potsherds

2.1 General background

The system we are concerned with has been evolved in the course of research on archeological sites in the Nubian region of the Nile valley which extends over a period of 28 years, during which time WYA served as field director of several excavations (for reports see WYA (1961), (1962a), (1964), (1965) and (1970); Adams, Alexander and Allen (1983); Plumley and Adams (1974), Plumley, Adams and Crowfoot (1977)). A fundamentally important problem that arises in the course of such excavations is that of estimating the dates of so called *proveniences*, which are 'minimal units of excavation', typically measuring a square metre in area by 10 cm in thickness. It is the provenience rather than the site as a whole that it is important to date, given that a Nubian site will typically have been inhabited over a very long interval of time, say from *c*. 200 AD to *c*. I200 AD. Only the exceptional provenience is directly datable by reference to artifacts [it yields], such as dated writings, and dates for the others [proveniences] must be estimated indirectly. This estimation generally makes use of materials associated with the proveniences that exhibit systematic chronological variation. The materials best suited for this vary with the region of excavation, but one

[3] See Plate 9.1 [ed.].

[4] They are discussed in our larger book (Adams, W. Y. and E. W. Adams, 1991) [WYA, 2019].

widely used method makes use of the fact that ceramic fragments (sherds) [potsherds] tend to be both well preserved and very numerous (a typical Nubian provenience yields between 300 and 500 of them), and their types can be shown to vary in a way that is uniform for sites in a region and which is distinctive of the eras of the proveniences in which they are found (see WYA 1986a, 617-633; WYA n.d.).

The problem faced by the archeologist developing a sherd typology for the purpose of chronological estimation in a given region is that of determining the correlations which prevail in that region between the *attribute-variables* of the sherds, and the *eras* of the vessels of which the sherds are fragments. This is usually very difficult. WYA's present system, which began with an 'Urtypologie', WYA (1962), in which sherds were classified into 27 categories called *wares* ('ware' is the standard term for a category in a sherd typology) has evolved into the present system involving 105[5] wares, which is described in detail in WYA (1986a). This evolution required the analysis of data on more than one million sherds collected from independently datable proveniences (without any assistance from computers!). Independent dates were arrived at principally through the recovery of dated written materials from proveniences. The present 105-ware system is the one we will focus on in our case-study. We will shortly note seemingly aberrant features of the system, but first we will describe the procedure by which the era of a provenience is estimated from sherd statistics.

As noted, between 300 and 500 sherds are recovered from a provenience in a typical Nubian excavation. These are usually sorted on the day they are recovered (quick feedback is important because date estimates to some extent determine 'where to dig next'). Once sorted, the numbers of sherds in each ware-category are recorded, along with the number of sherds that are simply listed as 'unclassified', whose quantity, which can run as high as 10 per cent, may also be chronologically significant. This sorting is facilitated by the fact that any given provenience is apt to yield significant numbers of sherds in only about 15 categories, with 'trace proportions' of some 20 more (*cf.* WAY, n.d. and WYA and EWA *Archaeological typology and Practical Reality*, in preparation [published 1991]). These numbers are then converted to percentages, which are recorded on a *cluster index card* for the provenience, which exhibits the percentages in quasi-graphical form. The final stage is the comparison of the cluster-index card with a set of 11 'master profiles' of sherd proportions characteristic of given eras, to determine which of these the provenience's profile most nearly resembles. The era of the 'closest fitting' master profile is used as the estimated era of the given provenience (*cf.* WYA, 1986b). The durations of these eras give a rough measure of the accuracy of the method, which at the present stage of development vary between 50 years and 200 years in length (as compared with an average era-duration of 300 years for the estimates based on the Urtypologie). This level of accuracy might seem unacceptably crude to those used to the methods of the exact sciences. In fact the estimates are as good as those presently obtainable by any other means (neutron decay, etc., *cf.* Peacock 1970, 378) and they are far less costly in both time and money. But that is not to say that the method cannot be improved upon, and we will return to this possibility later.

But now we will comment on the ware-descriptions, and on the way in which the ware-concepts are acquired. This will bring to light some of the apparently aberrant features of the typology.

2.2 Ware-descriptions

The reader is now urged to glance at the description of a typical one of the 105 wares in WYA's system, the so called TERMINAL CHRISTIAN WHITE WARE, or more briefly W14, which is included as an APPENDIX to this paper. What is most immediately striking about the description is its complexity, which makes it evident that it has not been designed merely to facilitate sorting and statistical summary. It is also obvious

[5] This number was updated from 102 wares given in the original article to 105 and the latter number is used throughout this edition [ed.].

that the description is not a *definition* in the logician's sense that it formulates a system of independent conditions which are separately necessary and jointly sufficient to define the concept uniquely (*cf.* Suppes 1957, chapter 8). In point of fact there are no logical definitions of the wares, so we have a system of concepts without such definitions. This raises the question of how persons can acquire the concepts. That will be returned to below, but first we will comment on three features of the description that would be aberrant if it were taken for a logical definition.

W14, like the other 104[6] wares in WYA's system, is described in terms of seven groups of *attribute-variables*, or simply *variables*, under the headings CONSTRUCTION, FABRIC, SURFACES, etc., all but the first of which break down in turn into more specific variables such as *paste*, *density*, and so on in the FABRIC group. There is a total of 42 of these overall. We follow Hodson (1982) here in using the term *variable* essentially in the sense of the statistician's *random variable*. These can be regarded as functions whose values are specific 'attributes', which we will call *attribute-values*, which at least roughly partition the entities in their domains (here potsherds) into mutually exclusive and exhaustive classes.[7] For instance, the *density* variable roughly partitions the domain of potsherds into five classes described by the attribute-terms 'porous', 'medium', 'fairly dense', 'dense', and 'very dense'. These happen to be ordered, but that is inessential. In fact, many other variables, such as *principal style* in the PAINTED DECORATION class, are amorphous. What might seem aberrant is that many of these partitionings are *vague*, even where precise distinctions could be made. Moreover, they are *occult* in the sense that they are not subject to direct observational determination,[8] and their totality *overdetermines* the concept supposedly being defined. We will comment briefly on each of these.

Vagueness and overdetermination are the most obvious and they are the easiest to account for. Consider *density*. The density attributed to sherds of type W14 is vaguely described as 'medium', though obviously precise physical values could have been specified and should have been insisted upon if precision were desirable for its own sake. But to insist upon it in the present case would not have served the purpose for which the sherds are classified and it would have been practically counterproductive. There is no reason to think that more exact physical measures of density would lead to more exact chronological estimates than the vague ones do, and requiring more exact measurements would be counterproductive because it would delay potsherd sorting and estimation when quick feedback is important.

Next consider *overdetermination*. Even allowing for their vagueness, the 42 attribute-variables are neither logically nor factually independent, and moreover they are so overly specific that it is rare to find sherds exhibiting all of the attribute-values supposedly definitive of the ware in any one of the 105 categories. Of course this only underlines the point made earlier: that the ware description really is a description, and not a definition. It is more plausible to regard the basic description as being of something like a prototype whose adequacy is commented on in the APPRAISAL which terminates each ware description. This gives indications of range of variability and related matters (in some areas of the world there are officially designated 'type sherd'—i.e., prototype sherds—see Colton and Hargrave (1937) and Colton (1953, 29). This sort of 'prototypical characterization and appraisal' is very important functionally, though we cannot enter into details in analyzing it. But there is one observation to make

[6] 101 was used here in the original publication [ed.].

[7] It is customary in the typological literature to distinguish between the attribute-variable, e.g., density, and its value, e.g., medium, the former being called variables and the latter being called 'attributes'. Paul Teller has drawn our attention to the fact that our distinction between attribute-variables and their values corresponds closely to W. E. Johnson's distinction between determinables such as colour, and determinates, such as the particular colour red. See Johnson 1921, 174.

[8] This occultness is practical rather than in principle unobservable. For our purposes the distinction is unimportant, since the obstacle they present to sherd-identification and ultimately to chronological estimation is the same.

in passing, which relates overdetermination to something analogous that arises in the determination of physical quantities.

The analogy is to an apparent *redundancy* which can arise in the estimation of physical quantities. Two cases in point are that in which the same quantity is measured repeatedly in order to average and get a better estimate of it, and that in which an object is triangulated on from several directions to get a better fix on its location. There is apparent redundancy because only one measurement or two sightings would be sufficient if they were perfectly exact. But in reality the individual readings and sightings are not exact, and to obtain exactness one averages, either intuitively or mathematically. We suggest that something similar occurs in the process of sherd classification, where vague 'readings' on individual attribute-variables are averaged in some intuitive fashion to get a better fix on a sherd's 'position in the ware-space' than could be gotten from just one or a few readings, even though these few would suffice if they were perfectly exact.[9] We cannot go farther into this here, though we hope to analyze it in future publication from the information-theoretic point of view. But now we turn to the problem of accounting for the fact that in many cases it does not seem possible to determine the values of attribute-variables by direct observation.

There are two kinds of occultness that are noticeable in the attribute-values mentioned in the description of W14. One is simply due to the fact that while it is the entire ceramic vessel that has the attribute-value, what the sherd classifier observes and must classify are fragments of the vessel. For instance, strictly speaking when a sherd is described as having the 'form' attribute-value *cup*, what this means is that it is a *fragment of a cup.* But of course the sherd classifier cannot literally see that the fragment being examined came from a cup, and therefore if we were to accept the observation-inference distinction we would have to conclude that the principal form is not observed but inferred (our study can be viewed as calling traditional ways of drawing this distinction into question, though not the distinction itself; we expect to discuss this issue in a future publication). But even if entire vessels could be observed, there are certain attribute-values that could not be ascertained conclusively. One is that of being *wheel-made*, which falls under the heading of CONSTRUCTION. This means that the vessel was formed with the use of the potter's wheel. But given that vessels of type W14 are estimated to have been manufactured between 1300 and 1500 AD, and given the impossibility of time travel, there is no way that a sherd classifier in the 1980s can ascertain by direct inspection that the fragment being examined came from a vessel formed on a potter's wheel some 500 years ago. The problem that we are faced with is that of explaining 'functionally' why it is that descriptions of classifications that are designed for practical chronological estimation contain references to occult attribute-values. These points will be returned to, but first we will comment on 'practical' ware-concept acquisition.

2.3 Ware-concept acquisition

That the wares in WYA's system are not logically defined does not mean that the only things that the classifier has to go on in acquiring the concepts are the written ware descriptions. Hands-on study with specially compiled sherd collections under the supervision of experts trained in the method is also of great importance (*cf.* Shepard 1965, 306; Thomas 1972; WhalIon 1972, 5). In this process the typology manual, WYA (1986a), which contains all of the ware descriptions and a great deal else besides, serves in part as an *aide-mémoire*, which assists the student in assimilating the very complex skills that are involved in sherd recognition. Persons having had laboratory courses such as in mineral-identification will be familiar with

[9] This does not necessarily reify an objective *place* in an absolute space of wares. The question of whether systems of judgments (e.g., as concerns classifications of sherds) that are not perfectly consistent can be interpreted as though they were approximations of 'exact' judgments cannot be pursued here. A closely related issue concerning Mohs scale hardness judgments is discussed in the Appendix to EWA (1966).

this type of instruction (in this section we will confine ourselves to claims about concept acquisition in WYA's system, but some speculative remarks about possible generalizations are made in section 3.1, and Paul Teller has suggested in correspondence that something like this is also true of the acquisition of the concepts of physics, where practice under the guidance of a tutor is essential, *e.g.*, in learning to design and conduct experiments, and interpret their results). But, though the manual for WYA's system is unusually comprehensive, it is doubtful that one could become proficient in sherd recognition just from studying it. We next note certain specific features of laboratory instruction and its aims.

One thing that laboratory training teaches is familiarity with what might be called the *recognition-signs* of attribute-values, whether or not they are occult (Dunnell (1971, 456), distinguishes between *significata*, the attributes used to define classes, and *denotata*, the attributes used to assign objects to classes). For instance, the practical sign that a sherd is a fragment of a vessel formed on a potter's wheel is the evenness of its surface in comparison with those of sherds from handmade vessels, which tend to exhibit the sorts of uneven ridges that would be expected when a vessel is smoothed with the fingers while being held stationary. Given this 'operationalization' of the concept of being wheel-made we might be inclined to wonder why the occult attribute-value should be referred to at all in the ware description. In fact the same question could be asked about the recognition-signs themselves.

The aim of the laboratory instruction we are considering is the attainment of *proficiency in sherd-recognition*. The fundamental test of proficiency is simply 'getting the right answer', which in practice means getting the same answer as the expert. And of course that is what is wanted because what experience has shown is that it is the expert's answers that give the basis for reliable chronological estimation. But if that is all that counts then even proficiency in attribute-sign recognition is secondary. This leads to two points of fundamental importance, one theoretical and the other practical. The theoretical point is that the 'necessity' that connects the attribute-recognition signs with the ware-concepts is more plausibly regarded as psychological than as logical. It is indubitable that persons unable to determine the evenness of surfaces are less able to make ware classifications that agree with those of the experts than others can. But that is a fact of empirical psychology. If someone were miraculously able to obtain answers in agreement with the experts even though bereft of normal sensory capabilities (perhaps such a person would be described as having the gift of being able to 'look directly into the past') there would be no reason to deny that he or she had grasped the ware concepts, and there would be every reason to utilize his or her findings in arriving at chronological estimates.

The practical point about recognition-proficiency is even more important. In fact about 90 per cent agreement with the experts may at times be regarded as acceptable, and that is as it must be given that the experts themselves hardly attain higher levels of agreement in sherd classifications done under field conditions.[10]' Furthermore, this level of intersubjective agreement is normally all that is wanted in Nubian Archeology, because there is no reason to think that higher levels would significantly increase the accuracy of chronological estimates. *Objectivity in the sense of the attainability of intersubjective agreement is not an end in itself.* Furthermore, the fact that there are acceptable *levels* of it highlights as nothing else does the importance of taking purpose into account in the analysis of scientific concept formation, since

[10] This is discussed at length in WYA and EWA, *Archaeological Typology and Practical Reality*, in preparation (published 1991 [ed.]). Paul Teller has pointed out that this is also true of physics and astronomy. This is also connected with the metaphor of observers as measuring instruments ('A human being is, from the point of view of physics, a certain kind of measuring apparatus'—van Fraassen (1980, 17). This would fit in with the point made below that theorizing about the perceptual and interpretive processes involved when scientists make observations is dubious psychologizing given the present state of perception theory and cognitive psychology. The scientist's 'use' of observers whose psychology they don't understand is analogous to their use of instruments whose principles they don't understand. However, this is not to commit us to the aptness of the observer-measuring device metaphor.

what is acceptable depends on the purposes of the users of these concepts.

But now that we have stressed the importance of laboratory instruction in the acquisition of the ware concepts, we can make some observations on the role that the occult attribute-variables play in the ware descriptions.

2.4 The uses of descriptions in terms of attribute-variables

The sorting of sherds into ware-categories is to an extent a matter of educated guess-work in which the recognition of very complex and probably not wholly analyzable 'cues' plays an important part (Clarke 1968, 190; Dunnell 1971, 456). It is plausible to conceptualize a ware-categorization judgment as a *probable inference* whose premises are attribute-sign cues and whose conclusion is a ware-category judgment. This conceptualization suggests that the occult attribute-values that appear in the ware descriptions might fit into the picture in the following way. Consider the attribute-value of being a sherd of a wheel-made vessel. The judgment that a sherd has this attribute-value can also be regarded as a probable inference, but it is much more probable and much less complex than the judgment that it is of a particular ware-category, say W14. Furthermore we can say, counterfactually, that if it were possible to establish that the sherd was wheel-made and had the other attribute-values in terms of which W14 sherds are described, the sherd's ware-category could be established as a practical certainty. Hence for the purpose of sherd-recognition the classifier might be thought of as proceeding by stages, first by assuring the practical certainty that a given sherd is wheel-made (and it possesses other significant attribute-values), and then by making a 'secondary inference' that it is of a particular ware-category. There is much dubious psychologising in this, but there is a connection between it and another role that is played by the occult attribute-variables which is of great methodological importance.[11]

Though we have spoken of chronological estimation as *the* purpose for which WYA's typology has been constructed, it is more accurately said to be the *primary* purpose in that this is what guided the selection of the attribute-variables in terms of which wares are distinguished. Very roughly, we can describe the typology as being so constructed that sherd-categorization in terms of its types will be maximally informative about chronology. But that does not mean that other possible uses have been wholly ignored. In fact it is to be expected that technological advances will eventually permit more accurate chronological estimation, and this will render the present typology obsolete so far as its primary use is concerned. But the system will not thereby be rendered useless, because classification in its terms is potentially informative about other things (see WYA 1986a, 6-8). And, the attribute-variables entering into the ware descriptions give indications of what these other things might be. For instance, some research currently being planned will reconsider already gathered sherd statistics from the point of view of the information that might be gained from them about economic phenomena. The wheel-made *vs.* hand-made distinction is expected to yield information about trading patterns, since Nubian hand-made vessels were almost never traded while wheel-made ones tend to have been traded over long distances.

The point of the foregoing is that while a typology may have been designed with one or a few uses paramount, the typologist normally envisions a 'halo' of potential secondary uses, of which the attribute-variables are indicative. The typologist may even be unclear about his or her objectives, and think of the typology as 'universal' and informative for all purposes (*cf.* Sabloff and Smith 1969, 280-282; Schiffer 1976, 93). We believe that this must be an illusion (as do Brew 1946, 64; Dunnell 1971, 115; Hill and Evans 1972, 235), though this controversial point will be returned to in section 3.3. However, a final word must be said

[11] As with the observation-inference distinction previously commented on (footnote 9, above [ed.]), we hope that our functional-purposive approach can sidestep controversial issues having to do with cognitive processes involving sensations, mental images, inferences, meanings, gestalts, and so forth.

about the *general* ware concept in WYA's system, as against the *particular* ware-concepts that fall under it, like that of being a vessel of type W14.

2.5 The general concept of a ware in WYA's system

Looking ahead to section 3.3, let us note that it is at the level of the general type concept much more than at that of the individual type falling under it that methodological controversies tend to arise. Thus, in the current biological species controversy it is the general idea of a species, as against, say, that of *Eschscholtzia californica* (the California poppy), that is at issue (see Holsinger (1984) on this). It could appear strange, therefore, that in most of the sherd typologies in field use, including WYA's system, the general concept of a ware is specified in far less detail than are the concepts of the particular wares in these systems (for example, see Colton and Hargrave 1937, 2-3) In fact, this is easily explained. Ware-descriptions are designed to be used by persons applying typologies already in existence, while the function of giving a general definition of a system of ware categories, aside from helping users to 'get the general idea' of the system, is to guide researchers in evolving possible modifications or extensions of the system, or even in developing entirely new systems (see Colton and Hargrave 1937, 19-22). These are very different sorts of activities, and given that the typologist has usually already constructed his or her system and aims primarily to describe its application, he or she will naturally focus on the details of that and not on the general guidelines followed in constructing the system. Additional risks are run in attempting to formulate general rules of typology construction. This is something that is easier said than done, and it is easy to make mistakes in describing one's own methods (*cf.* Brew 1946, 44-46; Hill and Evans 1972). In Mach's famous phrase, one is to a large extent guided by the 'tact of the natural investigator' (Mach 1915, 285) whose years of experience and reflection in a particular field cannot easily be compressed into a few mechanical maxims to be handed over to neophytes. But there are a few observations that can be made on the general concept of a ware, and on the processes by which particular wares in WYA's system have come to be distinguished.

First, given the general goal of developing a typology that will be useful for the purpose of chronological estimation, it is to be expected that the actual development of the typology will be an evolutionary process involving successive approximations (this point is developed at length in WYA and EWA, in preparation (1991 [ed.])). One starts with guesses as to what attributes and attribute-combinations are likely to be chronologically significant, and then gathers statistics on sherds that have these attributes which are recovered from independently datable proveniences. Most emphatically, one does not attempt to be 'objective' in the manner recommended by the numerical taxonomists (*cf.* Sneath 1962) by indiscriminately seeking out all possible correlations between all possible attributes. Even if that were practically possible (whatever 'all possible attributes' might be) there is no *a priori* reason to suppose that the correlations that might be discovered in that WYA would be useful for the purpose of chronological estimation. As already suggested, we believe that it is vain to search for a typology that will be useful for all possible purposes, and the typologist must have one or a few purposes in view at the outset for guidance in the selection of attributes to be studied. That is not to say that there are no 'joints in nature' to be looked for, but what count as the *significant* natural divisions must still be dependent on the typologists' purposes (*cf.* Spaulding 1982, 11).

Given statistics on associations between attributes that appear chronologically significant *a priori,* two complementary processes are involved in refining the initial attribute selection. These are *splitting* and *lumping*.[12] In the splitting process sherds that were originally classified under a single heading are distinguished into two or more types, because it is found that these distinctions are chronologically significant. The reverse process occurs in lumping, when two or more previously distinguished types are found not to differ significantly in their chronological distributions, and so they are lumped into one. This happened in certain cases in evolving the present 105-ware system from the 27-ware Urtypologie, though obviously lumping was much less common than splitting. But the fact that it occurs at all is very important, because this runs counter to so called 'objective' typological methodologies such as those of the numerical taxonomists, according to which all associations of attributes are equally 'valid' and are equally important to distinguish (*cf*. Clarke 1968, 512-624; Doran and Hodson 1975, 158-186; Aldenderfer and Blashfield 1978). But that is because these approaches conceive of typology construction as an end in itself, without reference to the purposes for which such systems are constructed.

We will have more to say about the 'logic' of the relation between general type-concepts and the particular types that fall under them in section 3.3, but there are two further points to make about WYA's system in this connection. One is the obvious one that whatever general rules are formulated to guide the construction of a typology, the particular type distinctions arrived at in following these rules must depend on facts that are not 'given' *a priori*. In the case of WYA's system these are facts concerning the associations that actually obtain between sherd attribute-variables and chronology. It follows that knowing the *general* ware-concept in WYA's system would not permit one to deduce *a priori* that a particular ware with the description of W14 must fall under it. This is directly related to the second point.

Given that the formulation of a general methodology of typology construction is difficult and that it is something about which it is easy to make mistakes, and given that actual typology construction is a matter of successive approximations which depends as much on *a posteriori* associations as it does on general rules, it is to be expected that there will only be a loose association between general type definitions and the particular type concepts that fall under them. This suggests in turn that changes in general type-concept definitions should not be expected to result in radical changes in the particular type-concepts that fall under them. Where changes are found in the latter, as in the evolution from the 27-ware Urtypologie to the present 105-ware system, this is more apt to result from new factual discoveries, *e.g.*, as concerns chronological associations, than from changes in the rules defining the general concept of a ware. This broad methodological claim will be returned to in section 3.3, in remarks on the controversy concerning the biological species concept. But first we must comment on the degree to which our conclusions about WYA's system generalize to other scientific concept systems.

3 Generalizing to other scientific concept systems

3.1 Preliminary remarks

We shall not pretend that all of the features that we have noted in WYA's system are to be found in equal measure in other scientific typologies, much less in scientific concept systems in general. The extent to which they generalize is a matter of degree and depends on the feature in question. But in the following two sections we want at least to make it plausible that there is some general validity to lessons learned from WYA's system, by citing two more examples of systems which exhibit features similar to those observed in WYA's system. These examples are the Mohs Scale of hardness measurement, in which we will summarise points made in an earlier paper EWA (1966), and biological species classification. Hardness will

[12] See WYA 1975, 88. Paul Teller reminds us that phenomena akin to splitting and lumping occur in the evolution of classification systems in physics. Examples include 'splitting' elements into isotopes (e.g., C12 and C14), or 'lumping' hydrogen, deuterium, and tritium together simply as hydrogen.

be discussed in section 3.2 and biological species will be discussed in section 3.3. However, it is desirable first to say a bit more about the purposes and associated uses of the scientific concept systems with which we are concerned, not so much to clarify these problematic ideas as to note complexities.

Though it is tautological to say that deliberately developed scientific concepts are developed for purposes, nonetheless most well known scientific concept systems, unlike WYA's system, have been evolved through the efforts of many individuals, and they are applied by many more. We cannot claim identity of purpose on the part of all individuals developing and applying these concept systems, and therefore it is questionable whether `the' purposes or uses of these concept systems can be identified with those of their developers and users. Nonetheless we would still urge that useful insights into the nature of the scientific enterprise are to be gained by considering its concept systems from a functional point of view, though their purposes, uses, and functions now become autonomous (in somewhat the same way that 'use for sitting' is autonomous to the idea of being a chair, whatever uses individuals put chairs to). Thus we will shortly suggest that useful insights are to be gained into the Mohs Scale system of hardness measurement and into biological species classification if we regard these as technologies whose applications are designed to be useful for more or less well defined purposes, though particularly in the biological species case this begs important questions. These will ultimately be left unresolved, but for the present we will restrict our inquiry to concept systems that are like WYA's system in the following way. The intended applications of such a system involve the acquisition of data that are to be described in terms of the concepts of the system, and the data so described are intended to provide information of specifiable kinds about things that are extrinsic to the data.[13] WYA's system can be conceptualized in this way, since its application involves the examination of potsherds (the data), which are described in terms of its concepts (*e.g.*, as a W14), and the so described data are used to provide information about something extrinsic to the types: namely chronology. We will now argue that Mohs Scale hardness measurement can be similarly construed, and we will suggest more contentiously in the final subsection that it may be useful to regard biological species classification in the same WYA. The following remarks on the Mohs Scale are largely a summary of points made in EWA (1966).

3.2 The Mohs hardness scale

The original version of the Mohs Scale of hardness was described by F. Mohs in 1820, and through successive refinements it has evolved into the standard scale by which the hardnesses of minerals are measured today (though it has ancillary applications such as to ceramics, as its inclusion as one of the attribute-variables in the category of FABRIC shows). As this suggests, the primary purpose of Mohs Scale hardness measurement is to provide information about the properties of minerals, and in fact the most common use is to aid in their identification. With caution, we may say that mineral identification stands to Mohs Scale hardness as provenience dating stands to classification in WYA's system. For instance, mineral composition is extrinsic to the data from which Mohs Scale hardness measures are derived (essentially scratch comparisons with standard minerals) in some-what the same WYA that provenience dates are extrinsic to potsherd attributes.

Many features of the Mohs Scale are explainable by reference to its intended use in mineral identification,

[13] This is very broad and is not meant to be precise. The point is to contrast observations involving the determination of the applicability of a concept, such as measurements of distances, which are done for the purpose of testing theories of those concepts (e.g., geometrical theories), with observations which are done for 'extrinsic' purposes like calculating]-times. Of course, in the more sophisticated sciences one might say that all observations may be motivated by either intrinsic theoretical concerns or by extrinsic ones. We would argue, however, that philosophy of science has greatly overstressed the intrinsic-theoretical motivation.

of which the most obvious is the fact that only objects of hardnesses comparable to minerals have a place on it. Though in everyday speech we say that beds are hard or soft, it makes no sense to speak of a bed's hardness on the Mohs Scale. EWA (1966) also argues that the 10 minerals that have been selected as the *standards* on the Mohs Scale (*e.g.*, calcite near the low end with Mohs Scale hardness 3, and quartz near the upper end with Mohs Scale hardness 7) are well chosen in the sense that Mohs Scale scratch test comparisons with them will yield a maximum of useful information about the mineral content of specimens of unknown composition. Similarly, the fact that at present the finest divisions that are recognized on this scale are at intervals of 0.1 reflects the fact that while finer subdivisions could be made they would not yield useful information about mineral content because specimens of the same mineral generally vary in hardness by more than 0.1 on the Mohs Scale. Incidentally, it is significant that specimens of the standard minerals are less variable in their hardnesses than most other minerals are.

There are certain similarities between what could be called *Mohs Scale hardness descriptions* and the ware descriptions in WYA's system. While the following may not be the last word in Mohs Scale hardness characterisation, here is one such description, which is quoted from Ford (1947, 214):

'If the mineral under examination is scratched by a knife blade as easily as calcite its hardness is said to be 3; if less easily than calcite but more so than fluorite its hardness is 3.5.'

Obviously the description is incomparably simpler than that of W14 in WYA's system, which reflects the fact that Mohs Scale hardness is conceptually far simpler and easier to master than is ware-classification, but there are similarities both in vagueness and in occultness. 'Scratched by a knife blade as easily as calcite' is vague, but it is acceptable for most purposes because demanding greater precision (*e.g.*, by insisting in the use of sclerometers—scratch meters) would not yield substantially greater information about mineral content, and to insist on it would be counterproductive because of expense in time and money. The attributes of being calcite or fluorite may or may not be occult, since it is possible that they could be conclusively established with the use of sufficient laboratory equipment, but they are certainly occult under field conditions, where one takes for granted that specimens exhibiting the recognition signs of these standards are calcite and fluorite.

There is not space to enter into other aspects of the functional analysis of the Mohs Scale, and rather than going into further detail we will conclude this section with a few remarks on the contrast between this approach and the 'representational' analysis of this scale as developed in current theories of fundamental measurement (*cf.* Roberts 1979, 61). According to the representational conception, Mohs Scale hardness values must correspond to or 'represent' ordinal scratch comparison relations in the sense that one mineral specimen should have a higher value than another on the Mohs Scale if and only if the first scratches the second when it is rubbed over the second one's surface. While there is something to this, the thing to note is that it makes no mention of the use to which Mohs Scale measures are meant to be put: namely as indices of mineral content. It is made to seem that the primary use of Mohs Scale measurement is to provide information not about mineral composition but about scratch relations, as though knowledge of these relations were an end in itself. This in turn makes it appear essential that the scratch relation should satisfy certain empirical laws which are 'necessary conditions for representability' (cf. Manders 1977), such as that it should be *transitive*. Actually scratch relations, vague as they are, do not perfectly satisfy these laws, which, ironically, led N. R. Campbell to deny that the Mohs Scale really *measures* hardness saying: 'Accordingly the definition does not lead to a definite order of hardness and does not permit the measurement of hardness' (Campbell 1921, 128). We would say that given the intended use of Mohs Scale hardness determinations for mineral identification there is no reason why they should exactly satisfy the transitivity law, and Campbell's assertion is almost a paradigm case of a formal standard being applied inappropriately without taking account of the purposes for which measurements are made.[14]

As a concluding aside, let us comment briefly on the fact that in recent years controversy over fundamental measurement has shifted from what constitutes 'genuine' measurement to what may be done with the *results* of measurements, and what may be said 'meaningfully' about them (*cf.* Stevens 1946; Adams, Fagot and Robinson 1965). We cannot enter into the details of this controversy (see section 1 of EWA (1966) for a critique of these views), but again we would claim that both sides consider measurement as something that is done for its own sake. This ignores the actual extrinsic reasons for which measurements are made, and leads in consequence to inappropriate normative strictures. Incidentally, one of these is violated in the description of W14, which specifies the average hardness of specimens of W14 as 3 on the Mohs Scale. According to S. S. Stevens, averaging in the sense of taking an arithmetic mean is not a 'permissible' statistical operation on measurements of this kind. We believe that the question ought to be: does this serve the purposes for which these hardnesses are measured?

3.3 Biological species, and the species controversy

We will largely concentrate on the species controversy, but first we will make a few comments on the conception of biological species classification as a technology whose application involves the acquisition of data described in biological species terms, and which provides a specifiable kind of extrinsic information. It is plausible enough that sorting by species is a very complex procedure which involves 'recognition-signs' of attributes, many of which are vague and more or less occult. What is less evident is that this sorting is expected to yield specifiable kinds of information that are extrinsic to the attributes themselves.

What do we learn of a flower in establishing that it is of the genus *Eschscholtzia*, species *californica* (the California Poppy)? The answer 'everything about the flower that is true of it in virtue of its being of this species' is true but uninformative. It is more informative to be told that determining the species tells us about the flower's genetic makeup and thereby about all of the things that are determined by this makeup—morphology, probable life-history, reproductive behaviour, and so on. Probably information about nutritional and medicinal value also falls under this heading. The question is: is it reasonable to think of species distinctions as though they were made in such a way that determinations of species will yield maximal information about morphology, reproduction, and so on?[15] This is a very complicated question which we can hardly do more than raise here. However, we would suggest the following as a plausible claim about the Linnaean system of botanical classification in terms of the reproductive organs of plants. That is that this system is in fact more informative about the above-mentioned things than were the morphological classifications of the herbalists which it superseded. Why that should have been so is another matter. Conceivably this might be because the reproductive organs are related to reproduction, and reproduction is more than mere generation: it involves *re*producing the individual. If reproduction were mere generation, and all that could be said was that the seeds of one living organism would grow

[14] The work of Adams and Carlstrom (1979) suggests that such defects are not as devastating as critics like Campbell or opponents of the interbreeding population approach to biological species have thought (cf. comments on the latter in section 3.3). Adams and Carlstrom show that any relation that approximately satisfies the laws either of ordering or of equivalence relations (e.g., for *most* triples of individuals, *x, y,* and *z*, if *x* scratches *y* and *y* scratches *z* then *x* scratches *z*) must itself approximate an ideal ordering or equivalence relation. Hence, if the scratching relation satisfies the laws of ordering relations in most instances then necessarily a 'small change' in the relation must yield one which satisfies these laws in all instances. The general significance of ideas about 'small deviations' from ideal norms is something we hope to explore in future publication.

[15] An alternative possibility is that species theorists tend to be *evolutionary* biologists, who are tacitly agreed that the purpose of biological research is to clarify the evolutionary picture and who construct their concept systems accordingly. If true, there would be a certain irony in the fact that Linnaeus himself was not an evolutionist. As Larson says 'Widespread acceptance of the (Linnaean species) concept seems to indicate continued belief in the version of creation found in Genesis, . . .' (Larson 1970, 94).

into another living organism probably not resembling the first in either form or life-history, then indeed data concerning the parentage of individuals would give us little useful information. But that is not so, and the fact that within limits the individual reproduces the species is what gives reproductive behavior the *meaning* it has in biological classification. But these highly speculative considerations cannot be pursued further here, and we conclude with some sketchy comments on how the current controversy over the biological species concept appears from the point of view of our approach to scientific classification.

We suggested in section 2.5 that the primary function of definitions of general type concepts such as *species* or *ware* is that of laying down guidelines for extending or modifying systems of individual type concepts. Furthermore, given the fact that the evolution of type concept systems is an evolutionary and approximative process, one expects only a rather loose connection between general type definitions and the individual type concepts that fall under them. For instance, one would not expect a change in the definition of *biological* species to lead to a radical change in the description of *Eschscholtzia californica*, or in the laboratory and/or field techniques that are used to identify this plant. In fact one could argue that the shoe is on the other foot: it is the individual species concept that is primary and any changes that are proposed at the more abstract level of *species in general* must not radically tamper with the individual species distinctions that are now recognised. But though entrenched individual species concepts may be more or less impervious to changes in general species definitions, the latter may play a significant role in guiding extensions to include new species and even revisions in the descriptions of entrenched species. It is to be expected that this should be controversial, and our next comments pertain to that.

For the moment let us assume that the methodologists of biology are in agreement on the goals of biological classification, though this will be questioned below. Given this assumption there should also be agreement on what constitute good rules for describing new biological species. Roughly, these should be ones which, in combination with the facts, lead to characterisations of individual species concepts that best serve the purposes for which these concepts are designed (remember that the individual types arrived at in classification systems depend as much on facts established *a posteriori* as on general rules for type-characterization). On this view there could well be disagreements as to what general definitions and rules would lead to 'best' individual species concepts. Certain aspects of the current species controversy could be regarded as involving a particular kind of disagreement of this sort.

In an early version of his *interbreeding population* conception Ernst Mayr formulated this idea as follows:

'The word species is likewise such a relational term. It separates interbreeding populations from all others' (Mayr 1948, 371).

One of the controversial aspects of this approach has to do with the fact that the interbreeding population idea involves the factual assumption that interfertility is an *equivalence relation* (*i.e.*, it is transitive and symmetric). Recent criticism (*cf.* Hull 1970; Kitcher 1984) shows that this assumption is at best 'approximately true', and this can be viewed as a defect in Mayr's definition of species, regarded as implicitly giving rules for the characterization of individual species. Mayr's rules cannot be straight-forwardly applied in such a way as to lead to unambiguous characterizations of individual species. This type of 'structural defect' is common to many methodologies of concept formation, including theories of quantitative measurement. They have certain factual presuppositions, and the fact that these are only 'approximations' renders the rules either inapplicable or requires that further rules, often of an *ad hoc* character, should be formulated in order to 'deal with messy realities'. Criticising rules of concept formation because of structural defects like the one just noted is straight-forward, and if that were all that was involved in the current species controversy there would be little more to be said.[16] However, there is another aspect of the controversy that is much more difficult to deal with, which we will comment on in closing.

It is not uncommon to hold that certain classifications are 'objective' and reflect 'real divisions in nature', and to criticise others as 'artificial' and lacking in objective significance. Thus, Mayr says of his species characterisation that 'The gap between species is well defined and *has objective reality*' (Mayr 1949, 372, our italics). What is this 'objectivity' and why should it be thought desirable that scientific classification systems should possess it? Though it is an open question which we hope to discuss in future publication whether the idea of objectivity can be accommodated within our approach, we suggest that much discussion relating to it is confused, and that it might help to clarify matters if the purposes which are served by biological classification were taken more systematically into account.

Obviously any classification system will involve both artificial and 'objective factual' components. All man-made and deliberately developed concept systems are *ipso facto* artificial. On the other hand, to the extent that scientists follow rules in characterising individual concepts and the concepts to which they are led in following these rules depend on facts that are beyond their control, these concepts are not defined by wholly arbitrary `conventions'. Thus, whatever the facts are, Mayr's suggestion that the gap between interbreeding populations has objective reality at least makes sense. If it were true then to that extent the species concept that Mayr proposes to base on it would also have an element of objectivity.

The more controversial claim is that the rule that species correspond to interbreeding populations is 'natural' in contrast to possible alternative `artificial' rules for distinguishing species such as might rely on morphological features like the colours of flowers. If any sense is to be made of such a suggestion, it seems to us that it must be connected with the idea that there is one 'right' mode of biological classification which will serve all purposes. As to this, we agree with Holsinger that '. . . there is no reason to believe that the circumscription that is appropriate for one investigation will be appropriate for another' (Holsinger 1984, 301; much of what we say in the present section is in close agreement with Holsinger's views). But we take the widely held belief that there is just one 'right' way to classify the entities or phenomena of a field to be an indication that the methodologists of classification theory are not clear about the goals of biological classification. As W. S. Jevons put it: 'In approaching the question how a given group of objects may be best classified, let it be remarked that there must generally be an unlimited number of modes . . .' and '. . . we must not attribute exclusive excellence to any one mode of classification' (Jevons 1877, 677 and 722). We suspect that failure to realize this may as much as anything else be what is at the root of the species controversy.

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[16] See WYA and EWA *Archeological Typology and Practical Reality*, in preparation (1991 [ed.]) for further elaboration of this point. It is worth noting that the work of Adams and Carlstrom (1979) commented on in footnote 1, page 15 shows how the criticism that the relation of inter-breeding isn't a perfect equivalence relation could be met. If the relation approximately satisfies the laws of equivalence relations (i.e., those laws hold in most instances) then a small change in its extension will transform it into one which exactly satisfies these laws, and this in turn can he used as the basis for defining 'interbreeding species'.

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Appendix

A ware description in the Nubian pottery typology of W. Y. Adams

The following description of the TERMINAL CHRISTIAN DECORATED WHITE WARE, which is complete except for a final page with drawings of typical forms and decorations of vessels of this type, is drawn from WYA (1986a, 512-513).

Family N **Group N.VII**

Ware W14 Terminal Christian Decorated White Ware

A rather heavy matte white ware decorated in Style N.VII: the most distinctive ware of the Terminal Christian Period. It is presumably evolved from Wares W15 and W16, but is distinguished from them by its bolder and simpler decorative style and by a distinctive, rather heavy group of vessel forms.
CONSTRUCTION: Wheel-made.
FABRIC: *Paste*: Nile mud. *Density*: medium. *Texture*: medium. *Color*: tan, light brown, or red-brown shading to darker, often purplish core (typical Munsell signatures 2.5YR 4/5, 2.5YR 6/6). *Carbon streak*: occasional, seldom dark. *Hardness*: generally medium soft (Mohs' values 2.5 to 4.5, av. 3.0). *Fracture*: medium. *Solid temper*: fairly abundant fine sand, black and red fragments. *Organic temper*: none seen. *Variability*: apparently low. *Remarks*: same fabric as in Ware R28.
SURFACES: *Covering*: medium thick, soft slip. *Finish*: matte or sometimes lightly polished. *Texture*: usually rather chalky or gritty. *Configuration*: level, rotation marks not prominent on interiors. *Variability*: surfaces may be matte or lightly polished; never glossy.
FORMS: *Principal forms*: cups, plain bowls, vases (Fig. 284). *Other forms*: goblets, footed bowls, lids, jars (Fig. 284). *Forms not illustrated*: A9, A20, A23, D44, D47, F27a, Q6. *Doubtful forms*: C12, C34, C42, F6. *Vessel sizes*: mostly medium. *Rims*: rounded, frequently thickened. *Bases*: ring base in footed bowls. *Wall thickness*: generally notably thick, especially in larger vessels (7 to 13 mm av. 9.6). *Execution*: generally fairly precise. *Variability*: apparently low.
COLORS: *Natural color*: tan, light brown or red-brown (typical Munsell signatures 2.5YR 4/5, 2.5YR 6/6). *Slip*: shades from cream to pale pink, yellow, tan, or orange (typical Munsell signatures 7.5YR 7/8, 5YR 6/8). Interior usually cream or white. *Primary decoration*: very dark brown to dense black (typical Munsell signatures I0R 3/I, I0R 3/2). *Secondary decoration*: Medium to dark red (typical Munsell signatures 2.5YR 4/6, I0R 4/6). Common as rim bands or spacer bands in larger vessels. *Rim stripe*: usually broad red; occasionally narrow black. *Variability*: high variability in slip color, but fairly uniform in any given vessel.
PAINTED DECORATION: *Frequency*: usual. *Principal style*: N.VII. *Other styles*: II, V. *Most common elements*: rim stripes, borders, friezes. *Other elements*: plain body stripes, radials. *Exterior program*: most commonly a single broad frieze; less often a single narrow border; very occasionally a frieze with a narrow border above it. *Interior program:* not common. Most often a simple radial design extending to the vessel rim, without a surrounding border or frieze. *Execution*: fairly precise. *Delineation*: bold. *Variability*: apparently low.
RELIEF DECORATION: None.
APPRAISAL: *Material*: not common, but numerous whole vessels collected from Diffinarti.[17] *Adequacy description*: probably incomplete. *Variability*: apparently low except in regard to slip color. *Temporal variation*: not known. *Geographical variation*: probably none; all made at one place. *Intergradations*: possibly

[17] Diffinarti [21.33.N./31.03.E./Sudan Survey Department map 1:250,000 (NF-35-1) 11-P-1]: a large medieval settlement with *c.* 50 mudbrick structures and a small late-terminal Christian period church. See further Crowfoot 1927, 150; Mills 1965, 5; Monneret de Villard 1935, 230-232 [ed.].

with R28 and with predecessors W15 and W16; possibly also with companion white Ware W18. *Diagnostics*: matte white ware decorated in Style N.VII; peculiar group of vessel forms (shared with Ware R28). *Problems*: material insufficient; range and center of manufacture not known.

SIGNIFICANCE: *Earliest appearance*: 1250 AD. *Main period of manufacture*: not determined. *Continued use*: to 1600 AD. *Persistence of sherds*: not determined. *Archeological contexts*: domestic refuse. *Area of distribution*: identified from Qasr Ibrim through Batn el Hajar, wider distribution not determined. *Center of production*: not determined; presumably made in the same place as R28. *Frequency*: not common. *Relationships*: presumably an outgrowth of Wares W15 and W16 in Group N.IV. Companion Ware R28 is a red-slipped counterpart. There is no successor ware. *Associations*: see group description. *Index clusters*: LC2 (2%), TC (2%).

REFERENCES:[18] *Vessel photos*: Adams (1970), pl. 64; Monneret de Villard (1957), IV, pl. CCII, nos. B, D; pl. CCIII; Schneider (1970), pl. 35; Van Moorsel *et al.* (1975), pl. 35; Villa Hugel (1963), Kat. 486. *Sherd drawings*: Monneret de Villard (1957), IV, pl. CXCII, no. 75; pl. CXCI, no. 94.

[18] References included in the Appendix have been added to the bibliography although they do not appear in the original article [ed.].

DATING

From Pottery to History: The Dating of Archaeological Deposits by Ceramic Statistics[1]

(1989)

This paper was published originally in the German journal Meroitica, *whose readership was confined almost entirely to scholars working in the Nubian field. At that time few if any of those readers had any familiarity with the methodologies of ceramic classification and frequency seriation, long since familiar to prehistorians in both the Old and New Worlds. Since those methodologies were basic to my own studies of Nubian cultural chronology, by this time widely read by my colleagues, I felt that it was time to explain them to the uninitiated. The methodology is basically that of frequency seriation. I have omitted here the four data tables[2] that accompanied the original article, as these were enumerations of pottery wares unfamiliar to persons outside the Nubian field. They were illustrative of, but not essential to, the arguments made in the article.*

At previous Nubian conferences (Adams 1970; 1973b; 1978), and in a number of publications (Adams 1962; 1964; 1973a; 1975), I have discussed my methods of analysis and classifying Nubian pottery wares.[3] These discussions might suggest that analysis and classification are ends in themselves, as indeed they are for many students of pottery. As it happens, however, I am an excavator rather than a ceramologist, and I study pottery by necessity rather than by choice (for further discussion see Adams 1981). I am less interested in the material than in what it can potentially tell me about the contexts in which it is found, and particularly about their date. In this I am following well-established traditions in prehistoric archaeology (Colton and Hargrave 1937; Hawley 1936). The methods of analysis and classification that I have discussed previously are essentially qualitative. What I have not heretofore done is to describe the quantitative methods by which pottery wares, once they have been identified, are used to determine the date of archaeological deposits and sites. This will be my purpose in the present discussion.

The pottery typology

By way of background and introduction, let me briefly recapitulate the results of my taxonomic studies. I am now able to recognize and consistently to distinguish 106 pottery wares that are found in Meroitic, X-Group, and Christian archaeological sites. Some of these are handmade wares, products produced by Nubian women; others are the wheel-made wares from Nubian factories; still others were made at factories in Aswan and elsewhere in Egypt. The 106 wares are grouped into ware groups and subfamilies on the basis of shared technical properties. Families generally represent spatial variability; that is, different areas of manufacture, while ware groups are chronological subdivisions within families, reflecting the popularity of particular stylistic norms. Each ware, ware group and family has been given a separate identifying number and/or letter.

Dates of manufacture and use have been calculated, after a fashion, for each of the Meroitic, X-Group, and Christian wares. They are based in most cases on the observed association between particular pottery wares and dated objects such as coins and inscriptions. Qasr Ibrim, which has yielded scores of dated documents, has contributed greatly to this process (Adams 1979). Also useful is the evidence of dated

[1] Originally published in S. Donadoni and S. Wenig (eds), *Studia Meroitica 1984*. Meroitica 10, 423-450.

[2] See Adams 1989, 440-446. The tables were entitled as follows: Table 1, Outline classification of Nubian and imported pottery wares; Table 2, Ware percentages in Classic Christian units excavated in 1982 (from Qasr Ibrim); Table 3, Sherd material from Qasr Ibrim used as basis for defining index clusters; Table 4, Ceramic index clusters and their defining characteristics [ed.].

[3] A much more detailed explanation of my methodology and results will be found in *Ceramic Industries of Medieval Nubia* (Adams 1986).

historical events, like the Ayyubid capture of Qasr Ibrim in 1172 and the Mameluke attacks on Meinarti at the end of the 13th century.[4] Both these events left recognizable traces in the archaeological record, and they help to date the pottery wares in the immediately overlying and underlying deposits. Negative evidence is also considered in the dating of pottery wares. If a particular ware is not found in a datable deposit, then either it had already gone out of use or it had not yet been introduced at that date.

On the basis of all available evidence, I have attempted to calculate three separate sets of dates for each medieval pottery ware: the earliest date of appearance, beginning and ending dates for the main period of manufacture, and the final date for the continued use of individual vessels.

Methodological principles and assumptions

The dating of archaeological deposits by means of pottery involves three logical operations:

1. The transfer of dates from actually dated objects or other evidence to individual pottery vessels and sherds found in association with them;
2. the transfer of dates from individually dated vessels and sherds to all other vessels and sherds of the same wares;
3. the transfer of dates from pottery wares to the undated deposits in which they are found.

The first two of these procedures have been sufficiently considered already; the third will be my main concern here. It is not as simple and straightforward a matter as it might appear, for it involves the collection, sorting, and counting of potsherds from each excavation unit, the calculation of statistical relationships among the wares present, and finally the comparison of statistical relationships between one unit and another. Before detailing the actual procedures involved, it is necessary to consider briefly the underlying methodological principles and assumptions. Most of them are derived from the science of population statistics, which seeks to predict rather than to explain behavior in the aggregate.

Let us take, by way of illustration, the case of mortality statistics. No one can predict just when or how any human individual will die; all we know is that we will all die eventually. But by the collection of aggregated data – the recording of thousands of individual deaths – we can make quite accurate predictions about the probabilities of death from different causes, and at different ages. The profits of the insurance companies rest precisely on such calculations. Yet in many cases there is no satisfactory explanation for what is predicted. In the United States, for example, doctors and physiologists are still actively debating why it is that women as a group live longer than men, though nobody questions that they do. It is also observable that changes in death rates from different causes occur from one generation to the next, though again there is not always a satisfactory explanation for the change.

To return to our more immediate concern, every potsherd is in its own way a mortality statistic. No one knows in advance when or how a particular vessel will be broken and discarded, but sooner or later nearly all of them will be. Again, what is unpredictable in the individual becomes predictable in the aggregate. There are recurring proportions of different kinds of potsherds in deposits of the same age, but also observable changes from one age to the next. It is the observation of these patternings, both in terms of stability and of change, that provides a basis for ceramic dating of archaeological deposits.

Six specific methodological assumptions are involved in the ceramic dating procedures that will be described presently:

1. The type concept is fundamental.[5] The whole corpus of known pottery must be capable of division into recognizable types, such that each individual pottery specimen is assignable to one and only one type. A

[4] Concerning the Ayyubid raid of Shams ed-Dawla Turan Shah on Qasr Ibrim see further Abu el-Makarim (formerly Abu Saleh the Armenian) in Evetts and Butler 1895, 266-267; Adams 1996, 6 passim and Adams 2010, 5 passim, and for the Ayyubid and Mameluke raids and the decline of the medieval Nubian kingdoms see Welsby 2002, 75-77, 242-249 [ed.].

[5] For discussion see, among many other sources, Krieger 1944, and Rouse 1960.

further assumption of the type concept is that any two members of the same type could have had the same history; that is, they could have been made by the same person at the same time and place. Thus, the dates that have been calculated for any type as a whole are applicable, a priori, to each of its individual members. (In my typology I use the word 'ware' in place of 'type', because in Nile Valley archaeology a 'type' usually designates only a particular vessel form).

2. The basic unit of analysis and enumeration is the potsherd, not the whole pot. It is potsherds rather than complete vessels that are discarded into the rubbish deposits where nearly all of them are subsequently found, and it is potsherds that furnish the dates for those deposits. Nearly every potsherd still exhibits enough distinctive characteristics so that it can be assigned to one ware or another, without reference to the whole vessel to which it once belonged, and every ware has its calculated dates. Therefore, the fact that a potsherd was once part of a pot can be disregarded. Ceramic dating is the study of potsherd behavior, not of pot behavior.

3. All pottery wares have equal value statistically, because each has a calculated range of dates. No ware is considered to be more informative or less important than any other.

4. All individual potsherds have equal value statistically, regardless of size or other characteristics. It is assumed that the average number of sherds derived from any two identical vessels will be the same, and that for every exceptionally large sherd there is an exceptionally small one of the same ware. Moreover, it is not assumed that decorated sherds are more informative than undecorated ones, or that large sherds are more important than small ones.

It is hardly necessary to point out that counting potsherds in this fashion, and comparing the numbers in one ware with those in another, does not furnish a reliable measure of the number of vessels that were originally in use. Some wares were used primarily for large vessels, which break up into many sherds, while others were used for small vessels that break up into fewer sherds. Hardness or softness of the fabric, which varies from ware to ware, will also affect the number of sherds, as will thickness of the vessel walls. Because of these considerations, ceramologists who are primarily interested in the uses of pottery have sometimes resorted to the practice of weighing rather than of counting potsherds (for discussion see King 1949; Colton 1953). It is only necessary to repeat here that ceramic dating is based on the study of potsherds and not of pots, and for that purpose it is unnecessary to know how many whole vessels were originally involved.

5. A further assumption, and a controversial one, is that of uniform deposition: the assumption that deposits of the same age will yield about the same percentages of the same wares, regardless of their nature or location. This runs counter to a good deal of current thinking among archaeologists, some of whom look for evidence of different kinds of specialized activity in the spatial distribution of different pottery wares. In one or two cases – most notably at Teotihuacán (see Millon 1973, 40-41) – the evidence for this seems fairly persuasive. In Nubia, on the other hand, empirical evidence has demonstrated over and over again that the pattern of pottery deposition is relatively uniform over whole sites, and even from one site to another.

At Qasr Ibrim, I compared the totals of different wares that were found in 28 Classic Christian[6] units excavated in 1982. The most numerous group, the Nubian wheel-made wares of Family N, varied between a low of 43% and a high of 83% in individual units. However, in 20 of 28 units their frequency varied between 66% and 79%, and in 15 of 28 units between 73% and 79%. The mean frequency was 71%, and the

[6] The dates of the Nubian medieval periods according to Adams (1986) are as follows: X-Group (XC, including XC1 and XC2), AD 350-550; Early Christian 1 (EC1), AD 550-700; Early Christian 2 (EC2), AD 700-850; Classic Christian 1 (CC1), AD 850-1000; Classic Christian 2 (CC2), AD 1000-1100; Late Christian 1 (LC1), AD 1100-1300; Late Christian 2 (LC2), AD 1300-1400; Terminal Christian (TC), AD 1400-1500. See also Qasr Ibrim, Chapter 6, this volume [ed.].

standard deviation was around 10%. The hand-made wares of Family D varied from a low of 0 to a high of 14%, but in 22 of 28 units the figure was between 2% and 8%. The mean was 6.5%, and the standard deviation 6%. The Aswan wares of Family A were slightly more variable in Classic Christian contexts, ranging between 6% and 39%. Nevertheless, in 19 of 28 units they were between 13% and 24%, and in 13 of 28 units they were between 14% and 19%. The mean frequency was 18.7%, and the standard deviation 8%. Only slightly less consistency was found in 36 units of the Late Christian 2 period and in 35 units of the Late Christian 1 period. These deposits, moreover, were widely scattered in different parts of the site.

Not only do pottery deposits tend to be uniform at any given period of time, but they exhibit consistent patterns of change from one period to the next. This has been verified stratigraphically at more than 30 separate localities in the site of Qasr Ibrim. Finally, the overall pattern of change that has been recorded at Qasr Ibrim is very closely replicated at Meinarti, more than 100km away. The reasons for such uniformity are not entirely clear; for the time being it can be confidently predicted without being fully explained. Evidently we have much to learn about dumping behavior, which, so far as I know, has been very little studied by ethnologists (a partial exception is Watson 1979).

6. A final methodological principle, which is common to much archaeological thinking, involves the periodization of the data under study – in this case pottery. Again, this is somewhat controversial. We all know that culture evolves through processes of continual change, not by instantaneous leaps from one stage to the next. As archaeologists, however, our data do not permit us to observe the actual processes involved. What we are presented with, seemingly, is the evidence of a succession of 'steady states', represented by different stratigraphic layers or, just as often, by different sites. Change processes are inferred, and even measured, in terms of difference between one stratigraphic layer and the next, or between one site and the next.

Periodization is not merely a heuristic convenience for the archaeologist; it is a particular and a common mode of historical thinking. Given the nature of our data, there is an understandable tendency to think of the 'steady states' as fairly long in duration, and the intervening periods of change as fairly short. Cultural history is thus viewed as flowing in a series of pulses, or cladistically. This kind of thinking is basic to the formulation of archaeological chronologies such as Early, Middle, and Late Helladic, or Danubian I, II, III, IV, and V. It is just as common among historians who generalize about an Augustan age or an Elizabethan age.

Whether economic or political or social history really proceeds in a series of pulses is debatable. In matters of style and fashion, however, we are on safer ground. It is readily observable in many different areas of human expression that styles persist until we grow tired of them, and then give way rapidly to new styles. The period of their persistence may of course vary from a few weeks to a few years to a few generations, or even longer.

It is a basic assumption of my approach to ceramic dating that there were periods of relative stability in the production and use of particular pottery wares, alternating with periods of rapid change. In periods of stability, vessels that were accidentally broken would be replaced by others of new types that were coming into favor. The process of change would be complete when the last vessel of the old type had been broken or discarded. As later discussion will show, this is not merely a logical or a heuristic assumption on my part; it is well supported by stratigraphic evidence from Qasr Ibrim.

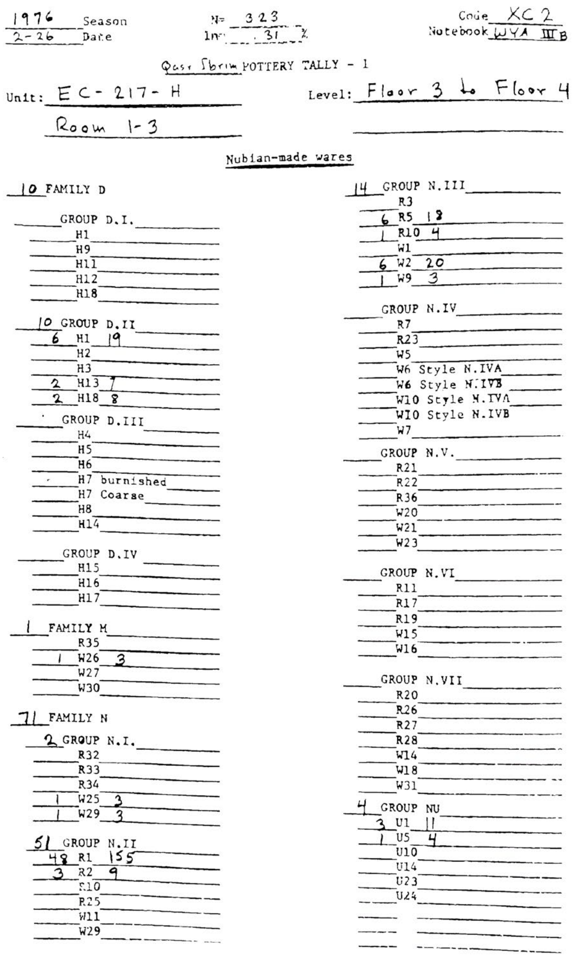
Field recording procedures

The methodological principles that have been discussed above make it necessary that every potsherd from every excavation unit should be collected in the course of excavation. Since I am primarily a townsite archaeologist, an excavation unit usually comprises a part of the deposit within a single room in a single house. Deposits of this kind are taken out in a series of layers, usually about 25cm thick, and each such layer is separately designated as a unit.

At Qasr Ibrim a typical excavation unit of the type just described will yield about 250 potsherds, although individual units vary widely in this respect. Since between 10 and 20 units are likely to be excavated in the course of a day, an average day's yield of sherds will be between 2500 and 5000. It would of course be possible to apply various scientific sampling procedures so as to reduce the volume of this material for examination. In practice, however, it has been found possible to sort and count up to 5000 sherds a day, and consequently sampling procedures have not been employed in my excavations.

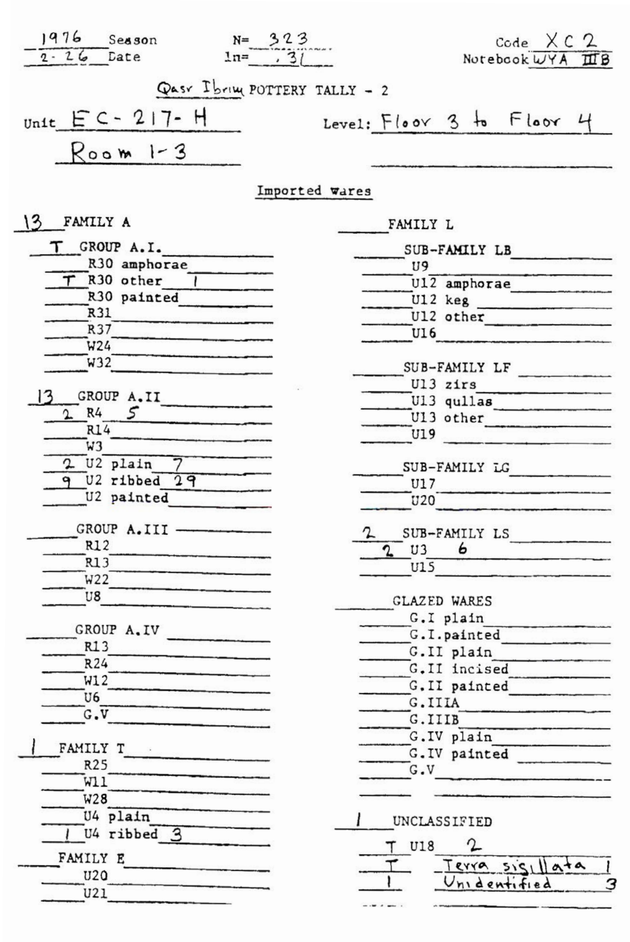
A certain involuntary sampling takes place because, without screening the deposit, it is impossible to recognize and to recover all of the potsherds in any deposit; some are inevitably discarded with the spoil. This process is not totally random in its effects, inasmuch as small sherds are easier to overlook than large ones, and dull-colored sherds are easier to overlook than bright-colored ones. This involuntary bias cannot be eliminated, but at least it is held constant in that it applies equally to all excavations in all deposits. The important consideration is that no sherd of any kind is eliminated by intent.

When an excavation unit has been completed, the collected sherds are then washed (necessary for accurate identification) and sorted by types (ware). For convenience I usually make a preliminary sort into five general and easily recognized categories: the hand-made wares, the Nubian wheel-made decorated wares, the Nubian wheel-made utility wares, the Aswan wares, and all other imported wares. Each pile is then sorted into its individual constituent wares.


1976 Season
2-26 Date
N= 323
In= .31 %
Code XC 2
Notebook WYA IIIB
Qasr Ibrim POTTERY TALLY - 1
Unit: EC-217-H
Room 1-3
Level: Floor 3 to Floor 4
Nubian-made wares

10 FAMILY D
GROUP D.I.
H1
H9
H11
H12
H18
10 GROUP D.II
6 H1 19
H2
H3
2 H13 7
2 H18 8
GROUP D.III
H4
H5
H6
H7 burnished
H7 Coarse
H8
H14
GROUP D.IV
H15
H16
H17
1 FAMILY M
R35
1 W26 3
W27
W30
71 FAMILY N
2 GROUP N.I.
R32
R33
R34
1 W25 3
1 W29 3
51 GROUP N.II
48 R1 155
3 R2 9
R10
R25
W11
W29
14 GROUP N.III
R3
6 R5 18
1 R10 4
W1
6 W2 20
1 W9 3
GROUP N.IV
R7
R23
W5
W6 Style N.IVA
W6 Style N.IVB
W10 Style N.IVA
W10 Style N.IVB
W7
GROUP N.V.
R21
R22
R36
W20
W21
W23
GROUP N.VI
R11
R17
R19
W15
W16
GROUP N.VII
R20
R26
R27
R28
W14
W18
W31
4 GROUP NU
3 U1 11
1 U5 4
U10
U14
U23
U24

Figure 10.1. Sherd tally sheet (obverse) listing all of the Nubian-made wares.


1976 Season
2-26 Date
N= 323
In= .31
Code XC 2
Notebook WYA IIIB
Qasr Ibrim POTTERY TALLY - 2
Unit EC-217-H
Room 1-3
Level: Floor 3 to Floor 4
Imported wares

13 FAMILY A
T GROUP A.I.
R30 amphorae
T R30 other 1
R30 painted
R31
R37
W24
W32
13 GROUP A.II
2 R4 5
R14
W3
2 U2 plain 7
9 U2 ribbed 29
U2 painted
GROUP A.III
R12
R13
W22
U8
GROUP A.IV
R13
R24
W12
U6
G.V
1 FAMILY T
R25
W11
W28
U4 plain
1 U4 ribbed 3
FAMILY E
U20
U21
FAMILY L
SUB-FAMILY LB
U9
U12 amphorae
U12 keg
U12 other
U16
SUB-FAMILY LF
U13 zirs
U13 qullas
U13 other
U19
SUB-FAMILY LG
U17
U20
2 SUB-FAMILY LS
2 U3 6
U15
GLAZED WARES
G.I plain
G.I.painted
G.II plain
G.II incised
G.II painted
G.IIIA
G.IIIB
G.IV plain
G.IV painted
G.V
1 UNCLASSIFIED
T U18 2
T Terra sigillata 1
1 Unidentified 3

Figure 10.2. Sherd tally sheet (reverse) listing all the Egyptian and other foreign wares.

When sorting is complete, the numerical tallies of each ware are entered on sherd tally sheets. One side of the sheet (Figure 10.1)[7] lists all of the Nubian-made wares, and the other side (Figure 10.2) all of the Egyptian and other foreign wares. As a final recording step, raw numerical tallies are converted into percentage data. That is, the number of sherds of each ware and family is expressed as a percentage of the total number of sherds collected from the same unit. In a collection of 250 sherds, for example, each individual sherd has a percentage value of 4/10 of 1%. Percentage figures are entered to the left of each ware number on the sherd tally sheets, while the raw numerical tallies are entered to the right of the ware numbers. Conversion of raw tallies to percentage figures is obviously necessary to permit comparison between one unit and another, since the actual numbers of sherds vary widely.

Comparison of units: The sherd index cards

Modern archaeologists will undoubtedly feel that, once pottery tallies have been recorded, comparisons and conclusions can be left to a computer. While this is undoubtedly true in principle, we have in practice had no access to a computer during most of the work at Qasr Ibrim, and certainly not during my previous excavations in the Sudan. We have therefore had to develop a means of comparing units by a visual inspection of the recorded data. For this purpose the sherd sheets are somewhat too cumbersome; particularly since data is recorded on both sides of them. To simplify comparison, percentage data (only) is transferred from the sherd tally sheets to 10 x 15cm summary cards, called sherd index cards (Figure 10.3).

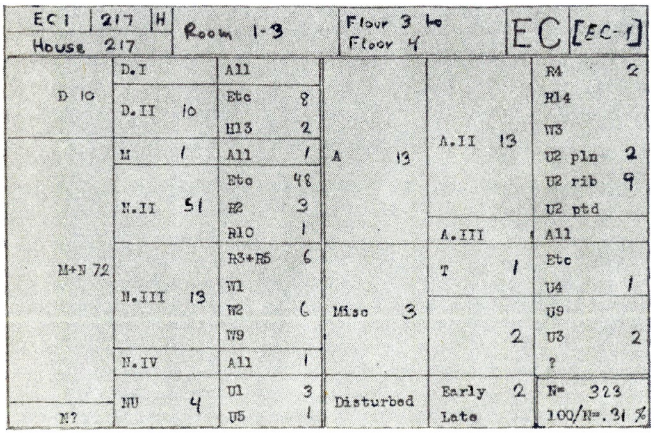


Figure 10.3. Sherd index card showing the data from the sherd tally sheet shown in Figures 1 and 2, entered on the appropriate index card for the Early Christian period.

Eight different index cards have been devised, each listing all of the wares that are known to have been in use during a particular period of about 200 years' duration. All wares from the preceding and from the following 200-year period are also listed, although in somewhat more summary form. The wares from still earlier and still later periods, which rarely comprise more than 5% of the total in any collection, are treated simply as 'noise'. That is, all of these wares are lumped together under the two headings 'earlier' and 'later'. For maximum economy of space, wares in the same family which are known to have the same distribution in time are grouped together in a single entry. In sum, the index cards are designed to highlight all of the percentage differences that are known to have chronological significance in a particular context, while lumping together or ignoring all other differences.

Figure 10.3 shows the data from the sherd tally sheet in Figures 10.1 and 10.2, entered on the appropriate index card for the Early Christian period. Nubian wares are entered on the left side of the card and Egyptian wares on the right. In each case they are broken down into three columns in which percentage totals are entered for whole families on the left, for ware groups in the middle, and for individual wares on the right. Totals of earlier and later wares that are not otherwise listed on the card are entered near

[7] Figure numbers used here have been slightly modified from those used in the original publication through the addition of the chapter number from this volume however, the figures themselves remain the same as in the original article. The forms shown in Figures 10.1-10.3 were typed on a typewriter, copied, then handwritten [ed.].

the bottom, right.

Choice of the appropriate index card for any particular unit must of course be made before the data can be entered, but this is very rarely a practical problem. A glance at the original tally sheet will be enough to indicate which of the successive ware groups in the Nubian Family N (by far the most numerous family in most collections) is predominant. This in turn will dictate the choice of index cards, since there is a different dominant Nubian ware group in each of the 200-year periods covered by the cards.

Dating of units: The index clusters

The procedures outlined thus far are different in detail but not in principle from those employed by many prehistorians. However, they constitute only half of the total process of ceramic dating. The other half, which remains to be described, is more novel and also more controversial. It involves a whole second classificatory procedure: the classification of percentage relationships themselves. Individual sherd index cards are compared with each other, and, on the basis of observed similarities, are arranged into groups called index clusters. The index clusters in turn are arranged in chronological order, and dates are calculated for each. Finally, the dating of any particular archaeological unit is a matter of assigning it to the index cluster which it seems most closely to remember in terms of its sherd content.

The underlying logic of this procedure can best be illustrated by means of a histogram: the familiar 'battleship graph' which archaeologists often use to represent the proportional frequencies of different kinds of excavated materials. Figure 10.4 is a histogram showing the proportional frequencies of the different groups of Nubian hand-made pottery (Family D), Nubian wheel-made pottery (Family N), Aswan

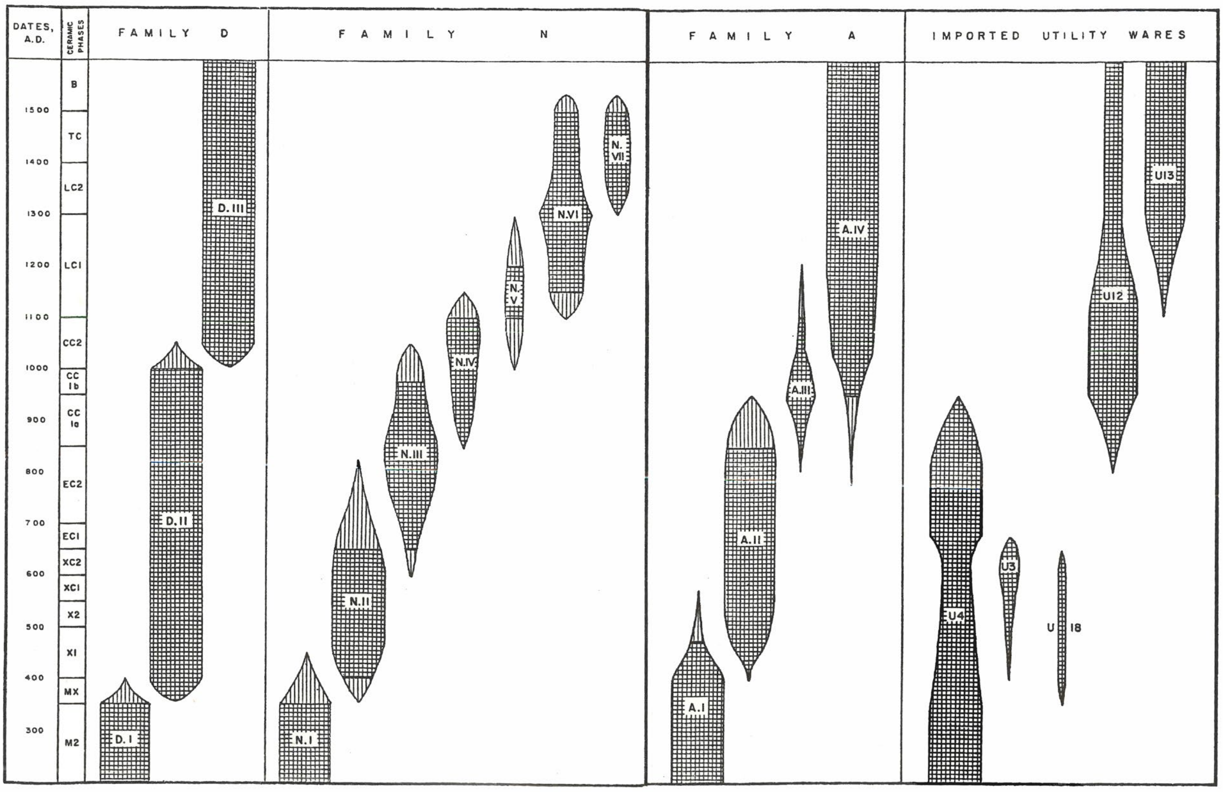


Figure 10.4. Histogram showing the proportional frequencies of the different groups of Nubian hand-made pottery (Family D), Nubian wheel-made pottery (Family N), Aswan pottery (Family A), and certain other Egyptian wares, between 200 and 1600 AD.

pottery (Family A), and certain other Egyptian wares, between 200 and 1600 AD.[8] Relative frequency, in proportion to other wares, is indicated by the width of each entry. Duration in time is indicated by the length from bottom to top. The rather pointed ends of each entry reflect the fact that most pottery wares came into use fairly gradually, continued through a period when they were predominant, and then were in turn gradually replaced by other wares. It is this characteristic shape which has caused the entries in histograms to be likened to battleships.

If we draw a horizontal line across the graph at any particular point in time, it will intersect at least 5 or 6 of the 'battleships', representing ware groups that were in use at that time. (Note that the entries in Figure 4 represent ware groups, not individual wares which are much more numerous). The proportional relationships between the different ware groups will be indicated by the width of the different segments that are intersected by our horizontal line. These proportions will not be quite the same as they would be if we drew the line higher or lower on the chart, because in the interval some ware groups would have become more prevalent and others less so. On this basis we can assume that any two archaeological units that exhibit about the same proportions of the same wares must belong to about the same period of time, and that any two units which exhibit substantially different proportions or different wares must belong to different periods of time. This is the basic assumption involved in the identification of index clusters, each of which involves a distinctive configuration of relationships among a specific group of ware groups and wares.

Notice that in many cases we could move our horizontal line upward or downward for a half century, and still intersect the same group of 'battleships'. In other words the ware groups present and absent would still be the same, but the proportional relationships between them would not. Identification of index clusters, then, is not simply a matter of enumerating the pottery wares present and absent. Just as importantly, it is a matter of recording differences in frequency among the wares present. It is this procedure which makes the recording of percentage data essential.

If we knew in advance the relative frequencies of different wares at different times in history, then the identification and dating of index clusters would be a simple matter. We have, of course, no such advance knowledge; the only way we can obtain it is by the comparative study of actual recorded sherd data from different units and sites. It is these comparisons which allow us eventually to identify index clusters, and it is the sequence of index clusters which in turn allows us to construct a histogram like that in Figure 4. This chart is the end product, not the beginning point, in the study of chronological relationships.

The beginning point is the individual sherd index card, which for purposes of argument we may liken to a potsherd. No two index cards are identical, just as no two potsherds are, but like potsherds they fall into readily recognizable clusters having essentially similar characteristics. As I have already suggested, the explanation for such similarities can only be that deposits containing about the same proportions of the same pottery wares were laid down at about the same point in time.

At Qasr Ibrim, about 2000 sherd index cards have been compiled since 1972.[9] Each represents a separate excavation unit; usually a specified stratigraphic level in a room or house. However, not all of these units are suitable for comparative study. Some units were badly disturbed between the time of deposition and the time of excavation; in other cases the excavation controls were inadequate for one reason or another. In addition, a great many units date from the 'Bosnian' or Islamic period, for which we do not yet have a fully satisfactory pottery typology.[10]

[8] For a description of these ceramic groups see Adams 1986 [ed.].
[9] Sherd data were not collected at Qasr Ibrim before 1972 [Author]. This is based on seven excavation seasons between 1972 and 1984, in which the author participated [ed.].
[10] See further Alexander and Adams 2018, with particular reference to Chapter 12 [ed.].

Altogether, 369 excavated units were considered to be suitable for comparative study, and they are the data base or 'study population' from which ceramic index clusters have been calculated. Two criteria were involved in the selection of these units for comparison. First, the deposits were reasonably undisturbed. Second, they belonged to definable stratigraphic sequences. That is, no unit was selected unless the immediately underlying and/or the immediately overlying unit was also selected. Because of this requirement, most though not all of the selected units were indoor deposits; that is, they had accumulated within house rooms rather than in the open spaces between houses. The total number of sherds contained in the 369 selected units was just over 200,000.

In the absence of a computer, the identification of recurring proportional relationships – that is, of index clusters – was achieved by physical manipulation of the sherd index cards. Four different steps were involved:

1. Cards from all of the units in any locality (usually a room in a house) were laid out vertically in their recorded stratigraphic order, with the earliest units at the bottom and the latest at the top. About 30 different localities, and hence 30 individual stratigraphic sequences, were represented in the 'study population'.

2. Within each stratigraphic sequence, any two or three adjacent cards which showed essentially the same percentage of the same wares were combined into a single stack. The assumption was that these deposits had all been laid down during a relatively brief period when the pottery wares in use, and their relationships, remained the same. When this process of combining had been completed, each card or stack of cards showed a configuration of wares and/or percentages significantly different from the card above it and from the card below it.

3. All of the stratigraphic sequences from the same general period of time (Meroitic, X-Group, Early Christian etc.) were laid out side by side in vertical rows, much as playing cards might be laid out in a game of patience (solitaire).

4. Finally, whole rows of cards were moved upward or downward until the best 'match' was obtained with the rows adjoining on either side; that is, until any particular card in any particular vertical row showed essentially the same wares and percentages as did the adjoining cards in the rows on either side. It could then be assumed that each horizontal row of side-by-side cards corresponded to a particular interval of time. The distinctive combination of wares and relationships which was exhibited in each row was taken as defining an index cluster.

I will not pretend that the results were always as neat as I have here suggested. While overall trends, involving the sequential appearance and disappearance of particular wares, were remarkably uniform, the rates of change were not always the same in different units. Sometimes adjoining units would show almost identical patterns of deposition at some points in time, but not at others. In spite of these inevitable discrepancies, recurrent patterns of ware distribution were far more conspicuous than I had expected them to be. This seemed clearly to verify two of my basic assumptions: that the pattern of refuse deposition was basically uniform throughout the site of Qasr Ibrim, and that in the use of pottery there had been periods of relative stability (represented by the index clusters) alternating with periods of rapid change.

From the comparative study of the 369 sample units at Qasr Ibrim, a succession of 18 different index clusters was identified. They span the period from the Ptolemaic, perhaps around 100 BC, to the Islamic, at around 1700 AD or later. Each index cluster was given a formal designation and definition. The wares present and absent were identified, and mean percentages and standard deviations were calculated for each ware present.

The next step was to try out the system on the remaining units at Qasr Ibrim, which were not included in

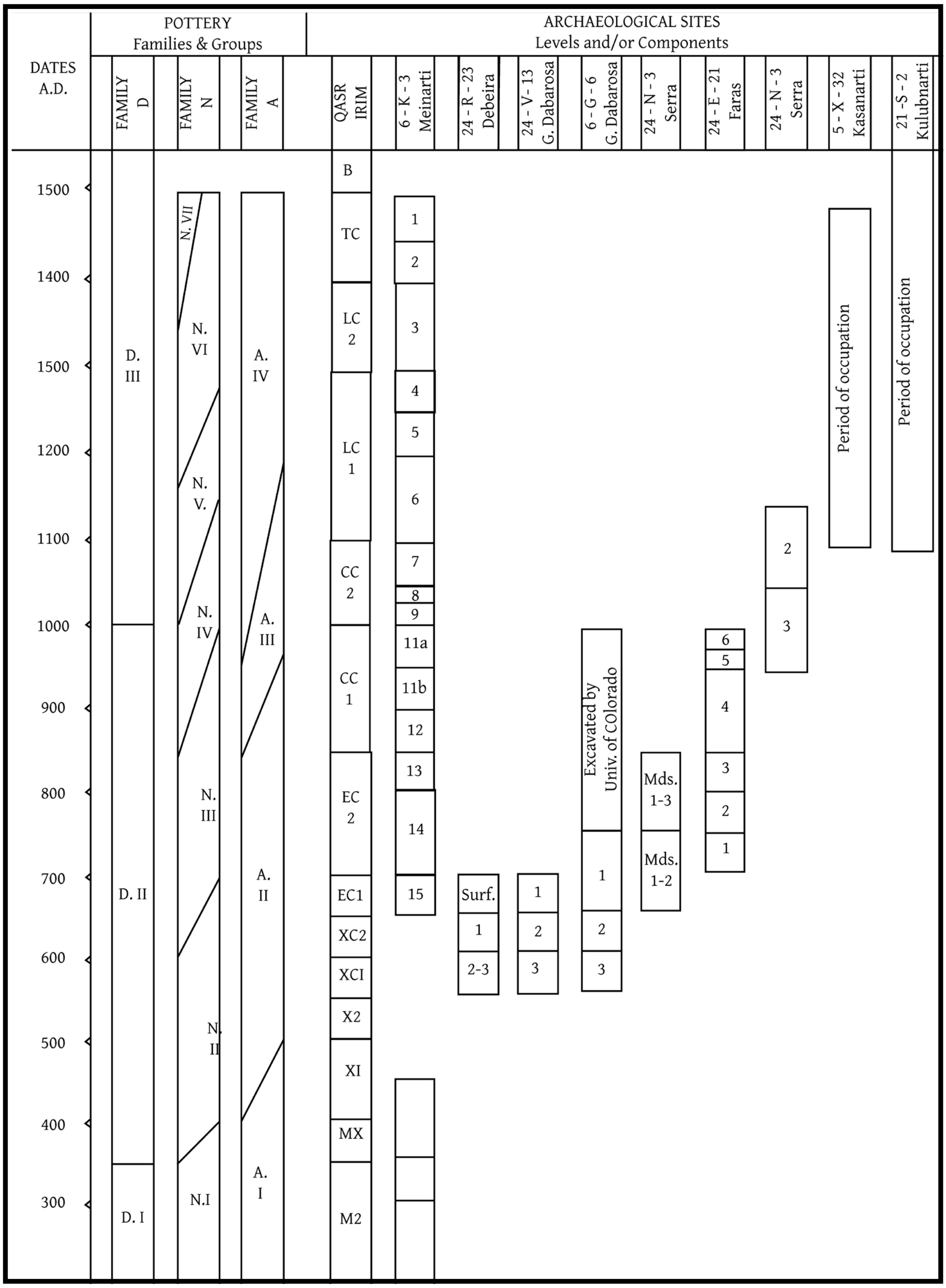


Figure 10.5. Cross-site comparison of the chronology of the ceramic data clusters.

the original study population. To my considerable surprise, these units fell as readily into the recognized index clusters as did those in the study population. Over 85% of the units belonged unmistakably to one group and no other, and most of the others fell quite clearly between two successive groups.

An unexpected discovery was that disturbance of the deposits did not affect the value or the reliability of index cluster calculations. This is because, at Qasr Ibrim, disturbance usually resulted from the digging of deep grain storage pits through older deposits. These were subsequently refilled with the same material, including the same sherds, that had been dug out, so that the number of 'intrusive' sherds of later date is seldom large. The most important point, however, is that the defining characteristics of most index clusters are not absolute percentages but relative proportions, or in other words ratios, between specific wares. If, for example, there are 200 sherds of Early Christian wares and 100 sherds of X-Group wares[11] in an Early Christian deposit, the ratio between them is not affected by the presence or absence of 20 intrusive sherds of Late Christian wares.

A final test of the system involved its applicability to the recorded data from other sites; particularly those which I had previously excavated in the Sudan. The data from Meinarti was found to conform almost as neatly as did that from Qasr Ibrim itself (Figure 10.5). Indeed, the Meinarti data allowed the further subdivision of the Classic Christian 1 index cluster into CC1a and CC1b phases, a distinction I could not recognize in the Qasr Ibrim collections.[12] The index cluster data revealed for the first time something that I had not previously suspected: that there was a hiatus in the occupation at Meinarti in the early part of the X-Group period (cf. Figure 10.5).[13] The clusters were also replicated in the data from half a dozen other sites in the Second Cataract area (Figure 10.5). The same was not quite so true at Kulubnarti, because the difficulties of overland transport beyond the Second Cataract resulted in a reduction in the volume of the Egyptian wares.[14] The proportional relationships among the Nubian wares, however, were essentially the same as at Meinarti and Qasr Ibrim.

The dating of the index clusters is a relatively simple matter, since dates have already been calculated individually for each of their constituent wares. They are of course approximate, and, like the individual ware dates, are calculated to the nearest 50-year interval. The chronology of the clusters, except for the four earliest, is shown in Figure 10.5. It will be seen that the estimated time for the different clusters is quite variable, because the pace of change was more rapid at some times than at others. The average time of duration for index clusters was about 100 years, but individual clusters varied in duration from 50 to 200 years.

The period between about 1100 and 1300 AD was a time of exceptional stability. There was no appreciable stylistic change either in the Nubian or in the Aswan wares, or in the volume of their production, and no new wares were introduced from abroad. As a result this 200-year interval, designated as Late Christian 1, has stubbornly resisted periodization into finer units. The index cluster data has not permitted a more accurate dating than could be made just by noting the wares present and absent.

By contrast, the results that have been achieved in the X-Group (Ballana) period are gratifying in the extreme. This 250-year interval, which previously was almost impossible to subdivide chronologically, can now be confidently broken down into four successive ceramic phases, of which the last three endured for only about 50 years each. This politically and economically disturbed era was, evidently, also a time of rapid cultural change.

To briefly recapitulate: the ceramic dating of any archaeological unit is a matter first of classifying and counting all of the potsherds found in it; then of calculating percentage figures for all of the wares present; then of classifying the percentage configurations themselves, and finally of assigning the unit to one of 18 dated index clusters. The dates for any cluster are the 'outside dates', or terminus post quem and terminus ante quem, for any unit assigned to it. This is by no means a precise dating, but it is more nearly so than can be achieved by any other means currently available to us.

[11] X-Group or Ballana period AD 350-550. See further Adams 1986 [ed.].
[12] This is probably because the excavation controls employed in digging the Classic Christian units at Qasr Ibrim were not very precise.
[13] In the original excavation report I spoke of '... a nearly unbroken continuum of occupation extending from the late Meroitic to the end of the Christian Nubian era' (Adams 1965, 149) [Author]. See further Adams 2000, and especially 45-53, 89-98 [ed.].
[14] See further Adams and Adams 1998, especially Chapter 4 [ed.].

Possibilities and limitations

I think it is fair to conclude, on the basis of the evidence I have presented, that the system of index clusters calculated at Qasr Ibrim would have been applicable to any other archaeological site between there and the Second Cataract. Unfortunately, there is no other surviving site in which this proposition can be tested. I can only hope that there may still be some unanalyzed sherd data or sherd collections from some of the sites that were excavated between 1960 and 1970. It is probably too much to hope, however, that any such collections will involve either a total or a legitimately random sample, and one or the other is necessary for the system to work.

How far the system would have been applicable to the north of Qasr Ibrim is anybody's guess. Again, there are no surviving sites where it can be tested. Presumably at some point, as one approached Aswan, the proportion of Aswan wares in relation to Nubian wares would have substantially increased. Going in the opposite direction, it has already been demonstrated that the system of index clusters identified at Qasr Ibrim is not totally applicable in the Batn el Hajar and beyond. The Second Cataract acted as a kind of filter which effectively screened out most of the Egyptian wares, though at Kulubnarti the relative proportions of Nubian wares were much the same as at Meinarti and Qasr Ibrim.

The system is obviously applicable to townsite deposits only, and offers no solution to the perennially vexed problem of dating Meroitic and X-Group graves. The wares that were buried in graves represent a deliberate section; they do not include all of the wares that were concurrently in everyday use. Moreover, the basic units for study are pots, not potsherds, and the 'rules of the game' are not the same in the two cases.

Ptolemaic, Roman, and Meroitic pottery

Up to now I have said little about the Meroitic and earlier wares, and their dating. This is not because I have no information; it is because the issues raised by the earlier wares are not primarily methodological. They are however important historical issues, and they will be of especial interest to members of this audience. I therefore feel justified in concluding with a brief discussion of the Ptolemaic, Roman, and Meroitic pottery found at Qasr Ibrim, and its significance.[15]

Let me first recapitulate the story told by the pottery, and confirmed by coins, inscriptions, and much other evidence. Qasr Ibrim was a Ptolemaic outpost from at least 100 BC until 30 BC, and thereafter it remained a Roman outpost until sometime around 100 AD. During that interval it had few if any native inhabitants. After 100 AD it was returned to Meroitic control, but there was evidently a substantial Roman garrison or colony on the site until much later, and possibly until the end of the Meroitic period.[16] Qasr Ibrim was therefore fundamentally different from any other site in Nubia at this time, and this is clearly reflected in the pottery.

Excavations in 1978 and 1980 yielded about 60,000 potsherds from what are unmistakably Ptolemaic and Roman refuse deposits. Both the identification and the dating is confirmed by coins and inscriptions. None of the wares are of familiar Meroitic or later types, and I have therefore not incorporated them in the typology of 106 wares which was described earlier. Instead I have devised a separate, highly provisional typology of Ptolemaic and Roman pottery, based on the material recovered at Qasr Ibrim in the two seasons. Copies of this typology have been distributed in MS form to interested scholars, and additional copies are available on request.

Of the 28 Ptolemaic and Roman wares that I have identified, only a very scarce hand-made ware and

[15] I have discussed the Ptolemaic and Roman archaeological remains at Qasr Ibrim in an article, 'Ptolemaic and Roman Occupation at Qasr Ibrim' (Adams 1985).

[16] This has been the subject of recent discussion and the date of Roman garrison's withdrawal from Ibrim (Primis) is not mentioned in textual sources. See further for example, Derda and Łajtar 2013; Edwards 2004, 161-163; Horton 1991 [ed.].

an even more scarce Aswan ware show much resemblance to the familiar pottery of Meroitic times. All of the other wares are of types not previously found in Nubia. The great majority of vessels (over 70% in the Roman period) are wine amphorae which were certainly made either in Egypt or further abroad. The establishment of Meroitic rule, around 100 AD, was marked by the wholesale introduction of familiar Meroitic pottery types, but there was a considerable persistence of Roman types as well.

Using the same methods that I have already described, I have tentatively identified six pottery index clusters for the Ptolemaic, Roman, and Meroitic periods at Qasr Ibrim.[17] The first of these is Ptolemaic, and the next two are Roman. The corpus of wares is virtually identical throughout the three periods, but there were substantial differences in proportion among the different wares. Most notably, there was a steady increase in the volume of wine amphorae in relation to other wares. Some exotic wares of trans-Mediterranean origin also made a first appearance in the Roman period.

There are three Meroitic index clusters, designated respectively as Ml, M2, and MX. Unfortunately, they are not marked by any perceptible stylistic changes in the Meroitic wares themselves. What sets the three groups apart is the heavy persistence of Roman wares in the first Meroitic period (M1), and the large-scale appearance of X-Group wares in the last of the periods (MX).

The six Ptolemaic, Roman, and Meroitic index clusters will be useful for future dating of archaeological units at Qasr Ibrim. However, I do not expect them to be of value anywhere else, because Qasr Ibrim at this period was clearly a unique site: a military outpost in an otherwise empty land.

This brings me, finally, to an aspect of quantitative evidence that I have not previously considered in detail. I am referring to negative evidence: something that seems to be very little understood by many of my humanist colleagues. Let me put the matter in the simplest of numerical terms:

1. The Ptolemaic and Roman levels at Qasr Ibrim, dated between 100 BC and 100 AD, have yielded over 60,000 sherds of 28 different pottery wares. Many of these are unmistakably of Egyptian manufacture.

2. Only 2 of the 28 wares, representing together less than 1% of the total corpus of sherds, have been reported from any other Nubian site.

3. The total of excavated and reported Nubian sites is in excess of 1000. While the original total was undoubtedly far larger, this represents an adequate sample by any statistical measure.

To my mind, the negative correlation between Meroitic sites and Roman-type pottery provides overwhelming confirmation for my previously expressed conviction that Northern Nubia was largely uninhabited between 100 BC and 100 AD, with the sole exception of Qasr Ibrim (see Adams 1976, 12-14). Had there been other inhabited sites, it is unthinkable that 26 different wares could have been imported in bulk to Qasr Ibrim without a single specimen reaching any other site. It is equally unthinkable that other sites containing Roman pottery could have gone undiscovered in three major archaeological salvage campaigns, and in a sample of more than 1000 sites. There, on the negative ceramic evidence, I will rest my case.

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[17] 'Pottery Wares of the Ptolemaic and Roman Periods at Qasr Ibrim', Adams n.d. (MS) [Author]. To the editor's knowledge, this manuscript was never published, however see Adams 1985 [ed.].

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Times, Types, and Sites: The Interrelationship of Ceramic Chronology and Typology[1]

(1987)

The original, somewhat shorter version of this paper was delivered at a small conference, held in conjunction with an exhibition of 'The Arts of Ancient Nubia and the Sudan' *at the Brooklyn Museum in 1978.*[2] *Because the audience would include a large number of practicing archaeologists in the Nubian field, this seemed an appropriate venue in which to discuss two issues in much greater depth than I had done in any previous publication. One was the very complex process of archeological chronology building; the other was the relationship between ceramic finds and their archaeological contexts. At the conclusion of the paper a very distinguished Israeli archaeologist was kind enough to say, 'Thank you for making us think'.*

In this article I discuss two of the most complex and most misunderstood concepts in the field of archeology: classification and chronology. Complexity and misunderstanding are evidenced by the fact that the theoretical literature on these subjects bears very little relationship to actual field practice (Dunnell 1986, 150; Adams 1988, 40-56). In classification we have on the one hand sophisticated computer-generated classificatory schemes that are incapable of application in the field,[3] and on the other hand successful field practitioners who have never been able to describe their largely intuitive procedures in appropriately scientific terms (Brew 1946, 44-66; Ford 1954, 42-54; 1962; Rouse 1960, 313-323). In chronology we have discussions of the many different ways by which archaeologists reckon time, but very little consideration of how the different ways relate (or do not relate) to one another.

The complexity of our time and type concepts derives partly from their complex interrelationship with one another. As I will suggest later, types partly determine archaeological time, and vice versa. To do proper justice to this subject I will have to give at least brief consideration to a whole network of interrelationships: between pots, sherds, and types; between all of those things and the deposits in which they are found; between the deposits and the structures or sites in which they, in turn, are found; and between all of the above and the inscriptions, coins, or historical records that furnish us with actual dates. Before getting down to practical issues, however, we must first consider briefly the nature of chronology and of typology.

Chronology

From choice or from necessity, the archaeologist keeps many kinds of time. He orders his own daily life and activities in terms of calendric time--the continuous progression of days, months, and years from a fixed point in the past. Ideally he would like to order past time in the same way, but the evidence is rarely sufficient to allow this. He is obliged instead to use various alternative systems of time reckoning, which I have designated under the general headings 'historical time' and 'archaeological time'.[4]

Figure 11.1[5] tries to show schematically what is involved in these different systems of time reckoning, with examples drawn from ancient Egyptian history and archaeology. Columns A and B illustrate two

[1] Originally published in *Bulletin of the Egyptology Seminar* 8, 7-46.
[2] The exhibition was entitled *Africa in Antiquity: The Arts of Ancient Nubia and the Sudan* and was open in the Brooklyn Museum between 30 September – 31 December 1978. See further, the Brooklyn Museum 1978; Wenig 1978; https://www.brooklynmuseum.org/opencollection/exhibitions/760 [accessed 19/01/2021] [ed.].
[3] For examples, see Whallon and Brown (eds), 1982.
[4] For an extended discussion of the many different types of time, see Thrift 1977a and 1977b.
[5] Figure numbers used here have been slightly modified from those used in the original publication through the addition of the chapter number from this volume however, the figures and themselves remain the same as in the original article [ed.].

Calendric time		Historical time			Archaeological time	
A	B	C	D	E	F	G
Linear	Cyclic	Biograph.	Dynastic	Episodic	Sequential	Periodic
1986	Year 2 of Rameses II		XXX			
1985	“ 1 “ “ “ “	Harkhuf	XXIX	Abandonment		
1984	“ 20 ” “ Seti I	third	XXVIII	of Nubia		Saite
1983	“ 19 “ “ “ “	journey	XXVII		Western	
1982	“ 18 “ “ “ “		XXVI		Thebes	
1981	“ 17 “ “ “ “		XXV			
1980	“ 16 “ “ “ “	Harkhuf	XXIV	Building of		Decline
1979	“ 15 “ “ “ “	second	XXIII	Abu Simbel		
1978	“ 14 “ “ “ “	Journey	XXII			
1977	“ 13 “ “ “ “		XXI	Reoccupation	Hawara	
1976	“ 12 “ “ “ “		XXI	of forts		
1975	“ 11 “ “ “ “	Harkhuf	XX			
1974	“ 10 “ “ “ “	first	XIX	Campaign of	Lahun	New Kingdom
1973	“ 9 “ “ “ “	journey	XVIII	Thutmose II		
1972	“ 8 “ “ “ “		XVII			
1971	“ 7 “ “ “ “		XVI	Abandonment	Lisht	Second intermediate
1970	“ 6 “ “ “ “		XV	of forts		
1969	“ 5 “ “ “ “		XIV			
1968	“ 4 “ “ “ “		XIII	Building of	Dashur	Middle Kingdom
1967	“ 3 “ “ “ “		XII	2nd Cataract		
1966	“ 2 “ “ “ “	Uni	XI	forts		
1965	“ 1 “ “ “ “	visits	X			
1964	“ 3 “ “ Rameses I	quarries	IX	Campaign of	Abu Sir	First Intermediate
1963	“ 2 “ “ “ “		VIII	Sesostris I		
1962	“ 1 “ “ “ “		VII			
1961	“ 20 “ Horemheb	Uni	VI	Harkhuf	Giza	Old Kingdom
1960	“ 19 “ “	governor	V	journeys		
1959	“ 18 “ “	of Nubia	IV			
1958	“ 17 “ “		III	Sneferu	Saqqara	Archaic
1957	“ 16 “ “		II	invasion		
1956	“ 15 “ “	Mernere	I			
1955	“ 14 “ “	visits		King Djer		
1954	“ 13 “ “	Nubia		inscription	HIerakonpolis	
1953	“ 12 “ “					
1952	“ 11 “ “					

Figure 11.1. Systems of time reckoning used by archaeologists and historians. The different systems are not cross-correlated on the chart; that is, entries in any given column do not correspond in time to entries in other columns. Earliest dates are at the bottom of each column.

different kinds of calendric time: on the one hand *linear time* (Col. A), which we keep today, and on the other hand *cyclical time* (Col. B), which the Egyptians themselves kept. The former measures time continuously from a single fixed point; the latter measures it from a recurring succession of fixed points. Both systems are, in their different ways, complete chronological records in that they provide sequence, continuity, and measurement of past time.

Columns C, D, and E illustrate different kinds of what is termed *historical time*--the kinds of time reckoning that can be achieved on the basis of historically attested, and sometimes dated, personages and events. Sometimes these records are essentially biographical, providing information about the existence and the activities of individuals who can be arranged in a chronological sequence (Col. C). Sometimes they are surviving dynastic annals (Col. D), and sometimes they are records of dated events (Col. E). All are, at least with regard to the distant past, incomplete systems of time reckoning. They provide us with sequence and sometimes with measurement (when personages and events can be dated according to a calendar), but they do not usually provide continuity. That is to say, our known personages and events are separated by wide gaps in time for which we have no information.

Columns F and G illustrate the two main types of *archaeological time*. Of these, *sequential time* (Col. F) is the most basic in that it is most commonly the archaeologist's starting point in constructing systems of time reckoning. It involves the arrangement of excavated materials (sites, site components, or individual objects) in a chronological sequence, without necessarily suggesting that the sequence represents a continuum in time (cf. the different Egyptian royal cemeteries listed in Column F). In and of itself sequential time involves only sequence, without either continuity or measurement. This is true, for example, in the case of sequences based on archaeological stratigraphy or on logical seriation. Conversely, sequences based on radiocarbon determinations involve a degree of measurement of the time intervals between one site or component and another. *Periodic time* (Col. G), finally, involves the division of cultural history into a continuous series of increments or periods. Periodization is the preferred method of time keeping employed by nearly all prehistorians. It is also widely used as a descriptive device in the historic periods, as in the case of the Egyptian historical periods listed in Column G. The cultural historian finds it convenient to categorize Egyptian history in terms of these increments of his own devising (Wilson 1951), even though he has also dynastic, biographical, and calendric records to fall back on. Periodic time involves sequence and continuity but no measurement.

Since sites and artifacts are the usual starting points of archaeological investigation, sequences of sites and artifacts are the usual starting points for archaeological chronologies. Until recently they were most commonly and most securely established on the basis of stratigraphy, somewhat less often and less securely on the basis of seriative techniques (Deetz 1967, 30-33; Rouse 1960, 313-323). Radiocarbon dating has provided an alternative, increasingly important basis for sequence determination in the past generation. With enough sequences of enough sites and objects, the archaeologist hopes to develop a cultural periodization that will then serve as a basis for ordering new sites and new artifacts as they come to light. Finally, the archaeologist would like to correlate his cultural periods with known historical events and personages and even, if possible, with calendric time records, to the extent that that can be achieved.

In sum the archaeologist's reckoning of time may involve four steps:

1) the determination of artifact or site sequences or both,

2) the construction of cultural periodizations, based on the cumulation of artifact and site sequences,

3) the correction of cultural periods with historical events and personages,

4) the dating of periods in terms of calendric time.

The latter two steps do not, strictly speaking, involve archaeological procedures and will not concern us in the present paper. But the first two steps are distinctly archaeological, and it is in these steps, and in the critically important connection between them, that pottery plays a key role.

There are four points to be stressed in summarizing the discussion of chronology. First, periodization is the basic scheme of time reckoning used by most archaeologists and cultural historians. Second, periodizations are the collective product of artifact and site sequences. Third, our sequences, in the case of artifacts, are not sequences of individual specimens; they are sequences of artifact types. That is, cultural periods are most commonly defined on the basis of recurring, stable combinations of house, tool, pottery, and grave types. Finally, among the different artifact types on which we base our periodizations, the role of pottery is usually paramount. This is partly because of the sheer volume of ceramic remains and partly because of their propensity to vary in a consistent way over time and space.

The use of pottery as a basis for archaeological time reckoning nevertheless involves its own set of complications, which must be mentioned at this point. Pottery yields at least three different kinds of archaeological time reckonings, which are referred to here as pottery dates, occupation dates and depositional dates. Pottery dates are the dates we calculate for the production and use of pottery types themselves. Occupation dates are the dates we attach to buildings, graves, or objects on the basis of the pottery found in association with them. Depositional dates are the dates of deposits in which we regularly find sherds of particular types. The interrelationships among these three kinds of time reckoning will occupy us in later sections

In concluding the discussion of chronology, it is necessary to reiterate simply that periodizations are basic, and that periodizations are the product of typologies, and above all of pottery typologies. Before proceeding further, therefore, we must now pause to consider briefly the nature of types and typologies.

Classification

Classification is one of the most universal and at the same time one of the most complex of all human mental processes. It lies at the root of all languages, or at least of all vocabularies, in that every word is an abstraction lumping together a number of things or ideas that are not identical in every detail but that are grouped under one label because of similarities that we consciously or unconsciously recognize. Part of the confusion in the literature on classification arises precisely from the fact that so much classificatory activity, even in the scientific field, takes place at a subconscious or unconscious level (Gilmour 1940, 461-474; Krieger 1944, 279-280; Gardin 1980, 11).

It would require a lengthy book to discuss all of the complexities and ambiguities of the classificatory process, and I have now published one (Adams and Adams 1991). Here it is necessary only to make a few points that are specifically relevant to pottery classification and more particularly to the use of pottery types for archaeological dating.

A major source of ambiguity is terminological. We use the word ‘classification’ to refer both to a mental process (here called ‘classifying’), and to the product of the process, which is a classification. Worse still, we use ‘classification’ in its process sense to refer to two quite separate activities: the act of creating categories, and the act of putting things into them once they have been created (here called ‘typing’ or ‘sorting’). Every practicing field archaeologist should be aware of the enormous difference between drawing typological boundaries, in the abstract, and deciding in a particular case whether a specimen belongs to Type A or to Type B. It is akin to the difference between defining what is a ball and a strike, in the baseball rule book, and calling actual balls and strikes in a game.

On this issue we can achieve a certain measure of terminological clarification that will simplify if not fully eliminate our problems. First, it is essential to distinguish between the acts of classifying (creating categories) and sorting (putting things into them). One must also recognize that these two processes are not just opposite sides of the same coin; they are not even consistently associated. There can be classifications without sorting; there can be sorting without classification.

A great many classifications (of color, size, or shape, for example) are made simply to facilitate verbal communication. They are not ordinarily used as a basis for segregating things into physically distinct groups, or for insisting on their distinctness. A classification that is to be used for sorting is of a special type and must have certain characteristics that are not merely necessary for verbal precision. The classification as a whole must be bounded, so that it is clear what is and is not to be sorted: it must be comprehensive, so that a category exists for every entity to be sorted; and the categories must be mutually exclusive, with each entity going into one and only one category. Such a classification is termed a typology.

In my usage, then, *a typology is simply a classification made for sorting*. As such it must somehow overcome two different sets of problems: the theoretical problems involved in defining categories (types) and the practical problems involved in recognizing them.

A typology, unlike other kinds of classifications, must have some specified purpose over and above that of mere communicative convenience. It is not necessary to sort potsherds into physically distinct red, white, and black piles in order to talk about each group in abstract terms. We can just as easily group them verbally without having to group them physically. Anyone who undertakes the arduous and monotonous task of sorting potsherds does so because there is something specific that he wants to know or to accomplish, and it is that purpose which determines the shape of the typology itself. What the classifier will look for, in the material under study, are those properties (variables and attributes) about which he wants to learn.

There are many legitimate reasons for classifying archaeological materials. The classifier may want to learn or to express something about technical or functional or morphological variability in the material itself. All of these are referred to here as *essential purposes*. On the other hand, the classifier may have no interest in the material as such but may want to use it as an aid to the functional or chronological understanding of ancient cultures. These are referred to as *instrumental purposes*. Finally classification can be undertaken merely for practical convenience, for example in deciding what material to store in what cabinets or drawers. These I call *incidental purposes*.

A word must necessarily be said here about the issue (or more properly non-issue) of 'natural' vs. 'artificial' classification. There are, unfortunately, archaeological classifiers who hold that purpose is irrelevant. Taking their cue from the biologists, they liken the archaeological type to the mammalian species (Krieger 1960, 141) and insist that classification is simply a matter of 'discovering nature's order' (Krieger 1944, 273; Spaulding 1982, 11). There is some doubt as to whether this is a legitimate procedure even in biology (Holsinger 1984, 293-307), but it should be evident in the case of archaeology that culture is not nature. Artifact types do not reproduce sexually and do not inherit their characteristics genetically; thus nature does not invest them with discoverable boundaries as it does in the case of mammalian species. The reality in the case of artifact types is that they are partly natural and partly artificial. We may say, in general, that nature (or more properly culture) creates modalities, or recurrences, but we, the typologists, select among them those that are useful for our particular purposes. Usually also it is we who must draw arbitrary boundaries between types, where neither nature nor culture has done so.

Ceramic typologies

What is true of archaeological typologies, in general, is true, with a vengeance, of pottery typologies in particular. Pottery can probably be legitimately classified in more ways and for more purposes than can any other kind of artifact because of its abundance, its complexity, and its potential to tell us about so many other aspects of culture and history. This very versatility has, at times, been a stumbling block in the development of effective typologies, not least in the study of Egyptian ceramics. There has been a tendency to try to develop a single classificatory scheme good for all purposes (Brew 1946, 44-66; Hill and Evans 1972, 236); something that in the majority of cases is likely to be self-defeating.

In classifying any kind of archaeological material, then, the first question must be 'classification for what?' (Rouse 1970, 9-12). Some of the possibilities in the case of ceramics include descriptive convenience, technological analysis, functional analysis, 'emic' analysis, (that is, attempting to recover the mind-sets of the makers), study of stylistic development, study of historical connections, and dating. Each of these purposes may cause us to examine one set of variables in making our classifications while ignoring a different set of variables.

It may be noted that some of these purposes are essential and other are instrumental, according to the distinction made earlier. Here a fundamental difference of perspective may arise between the field archaeologist, who digs up the pots and sherds, and the ceramologist, who is left to study them (Adams 1981, 44-45). The archaeologist, if he is interested in pottery at all, is likely to be interested primarily in what it can tell him/her that will aid in excavations; probably above all in its possibilities for dating individual deposits. He will, most likely, want to develop an instrumental typology with primary emphasis on variables and types that can be shown to have chronological significance. The ceramologist, on the other hand, is much more likely to be interested in the pottery for its own sake and will probably want to develop one or another of the several kinds of essential typologies mentioned earlier. If, however, the archaeologist has simply instructed the ceramologist to 'publish the pottery', but has allowed only 20 or 30 pages to do it in, the ceramologist will have little choice but to develop a typology of descriptive convenience. In Egypt, this latter circumstance seems to account for the great majority of published typologies and pseudo-typologies in the literature.

The archaeologist and the ceramologist may have different views not only on the question, 'classification for what?', but also on the question 'classification of what--sherds or pots?' Unless excavating undisturbed graves, the field archaeologist will find a great many sherds but not many pots. He will almost certainly want to develop a typology that will allow the processing of the bulk of the actual finds, that is, a typology based on what is actually found rather than on what was originally there. In such a scheme the types are not precisely types of sherds, but they must be consistently recognizable in sherd form. On the other hand the ceramologist may find that many and perhaps most sherds yield little information about the pots that they once were. He is likely to develop a typology of pots, in terms of which the great majority of sherds are unclassifiable. Hence the propensity among ceramologists to save only rim, handle, and base sherds, which are presumed to yield some information about the form of the original vessel. Obviously this not to suggest that either of these approaches is inherently good or bad, right or wrong. Each one is the right way to learn some things, and the wrong way to learn others. The important thing is for the archaeologist and the ceramologist to be clear about what they are trying to accomplish, to develop methods appropriate to their ends, and, if necessary, to come to terms with one another. My own concern, in this article and in all of my ceramic classification work, is specifically with chronology and dating (Adams 1962b, 245-288; 1964, 126-173; 1973, 1-50; 1986a, 6-8, 601-633), and for that purpose there can be no doubt that sherd classifications are of much more value than pot classifications. Be it noted, therefore, that hereafter I will be talking about sherd classifications made specifically for the purpose of dating sites and deposits.

Ceramic periodization

We are now back to our starting point and are ready to consider in practical terms the interrelationship between typology and chronology. How do we get from sites to sherds, from sherds to types, from types to periods, from periods to dates, and from dates back to types and to sites? In the present section we will consider the first four of these processes, deferring the fifth until the following section. Quite simply, five steps are required in getting from sites to dates: 1) excavation of the material; 2) classification of the excavated material into types; 3) seriation of the types; 4) periodization based on type sequences; and 5) dating of the periods.

Excavation: Here I address the process of excavation, rather than more generally of collection, because pottery that is to be used for the calculation of dates must normally be recovered from contexts below the surface of the ground. The contexts themselves must be carefully and accurately recorded; otherwise there is no basis for the critically important steps of seriation and periodization. Pottery collections with no recorded provenience, or with inadequate provenience data, can still be useful for various kinds of essential classifications (e.g. functional or morphological typologies), but they are obviously useless for most instrumental purposes, and in particular for dating.

The excavator of pottery must normally expect to deal with three conditions. First, ceramic finds will consist very largely of sherds, with few if any whole vessels. Since it is impossible to record the location of sherds on an individual basis, they must be grouped together according to clearly defined excavation units in which they occur--most commonly stratigraphic levels within a room or grid unit. Second, sherds will be found widely scattered through all of the excavated deposits, usually in tremendous numbers, and it is necessary to develop recording and sorting procedures that are practicable under those circumstances. Third, the sherds are very rarely found in their original *situ*; that is, in the actual place of vessel breakage. More often than not the archaeologist will not know exactly how the sherds came to be where they were found. The problems arising from this circumstance, when it comes to dating sites and deposits, will occupy us in later sections.

Classification: When a sufficient amount of material has been recovered from controlled proveniences, classifying can begin. Stress must be given, however, to the words 'sufficient amount'. One of the essential features of a pottery typology devised for dating purposes is that it must be an open system; that is, it is designed for the typing and sorting of future finds as well as for the material from which it is actually derived. Obviously such a typology cannot be based on a small corpus of material which may constitute an inadequate sample of a very large universe. Although there can be no general rule of thumb for sampling an unknown universe, there should at least be a substantial quantity and variety of material, preferably from many different proveniences, before any kind of classification is undertaken. Premature classifications based on inadequate samples have been a recurring problem in archaeology (Kaplan 1984, 31); once they are developed they can be extremely hard to change or to get rid of.

This is not the place to discuss in any detail the methodology of pottery classification.[6] Suffice it to say that in any classification intended for dating purposes, those attributes known or expected to have chronological significance will be given primary consideration in the differentiation of types. Among these, stylistic features are likely to be paramount, since we know *a priori* that style is historically fickle. However, the classifier cannot afford to disregard any feature, whether of fabric, surface finish, vessel form, color, or whatever, if it shows any kind of patterned variability.

In the beginning stages of classifying it is usually a good idea to designate a large number of provisional types. Some of these can later be discarded (i.e. in most cases combined with other types) if they are not found to occur with any consistency. Within reasonable limits, however, the more types included in a typology, the most useful it will be for calculating dates according to the procedure of frequency seriation, a process discussed in the next section.

Typology-making must necessarily involve practical as well as theoretical considerations. The types must not only be chronologically significant; they must also be consistently recognizable in sherd form and without recourse to such expensive and time-consuming procedures as microscopic or chemical analysis. Otherwise the cost, in time and effort, of calculating ceramic dates is likely to be greater than is justified by the results.

Seriation: Following the designation of individual pottery types, the critical next step is to determine their chronological relationships to one another: which types precede and follow one another, and which regularly occur together?

---

[6] See Adams 1986a, 63-259; Adams and Adams 1991.

POTTERY	PERIOD
Naqada II ware	NAQADA II
Naqada I ware	NAQADA I
Badarian ware	BADARIAN

Figure 11.2. The 'index fossil' method of periodization, in which culture periods (right column) are defined *a priori* by the presence or absence of particular pottery types (left column).

There are, obviously, several ways of discovering chronological relationships. The most common and most reliable is by using direct stratigraphic evidence: the observation of which pottery types are present and absent in each successive level in one or, preferably, more stratified sites. Those types that consistently occur together can safely be regarded as contemporaneous; those that always occur in levels below certain other types can safely be interpreted as earlier. This kind of evidence must normally be obtained from townsites, and in particular from rubbish deposits in townsites, since it is rarely possible to get very reliable stratigraphy from monumental buildings, such as temples or churches, or from cemeteries.

In the absence of stratigraphy, chronological seriation can be obtained when different pottery types are found in different deposits that can be independently dated by inscriptions, coins, or radiocarbon. The latter procedure is, of course, too expensive to be used on a very large scale, even when organic materials suitable for dating are preserved. Finally there is sometimes the possibility of logical seriation, that is, the arrangement of different pottery types in what appears to be a logical developmental sequence. This procedure is fairly common among art historians, who sometimes work with unprovenienced museum collections, but there is an obvious danger in imposing our own canons of logic on the products of another culture.

Periodization: If there must be considerable accumulation of sherds before classification can be undertaken, there must also be a considerable accumulation of dates or seriated types before periodization can begin, for this too is a form of classification. Once the chronological relationships among our types are known, we must identify clusters of co-occurring types that are more or less stable over time, and we have also to identify points or periods in time when there was rapid transition from one cluster to another.

There are three possible methods of ceramic periodization. The simplest might be called the 'index fossil' method, in which culture periods such as Badarian or Nagada I are defined *a priori* by the presence or absence of a single diagnostic pottery type (Figure 11.2),[7] just as geologic strata used to be identified by the presence of particular fossils (Daniel 1964, 34). This rather simplistic approach will suffice when only a few pottery types have been identified--sometimes that is usually true only in the earliest pottery-using horizons.

Somewhat more reliable is the 'cladistic' method, when periods are defined on the basis of clusters of types that regularly occur together, as shown in Figure 11.3. More often than not, the difficulty with this method is that pottery types do not all change at the same time; on the contrary, each has its own individual time range (Figure 11.4). In these cases it may be easy to identify recurring clusters of types, but it is much more difficult to say where one cluster stops and another begins. This is illustrated for medieval Nubian wares by Figure 11.5, in which it can be seen (in the three right-hand columns) that the three main pottery families (i.e. groups of types) evolved at different rates and underwent major transformations at different times.

[7] Egyptian Predynastic periods. Badarian 5500-4000 BC; Naqada I 4000-3500 BC [ed.].

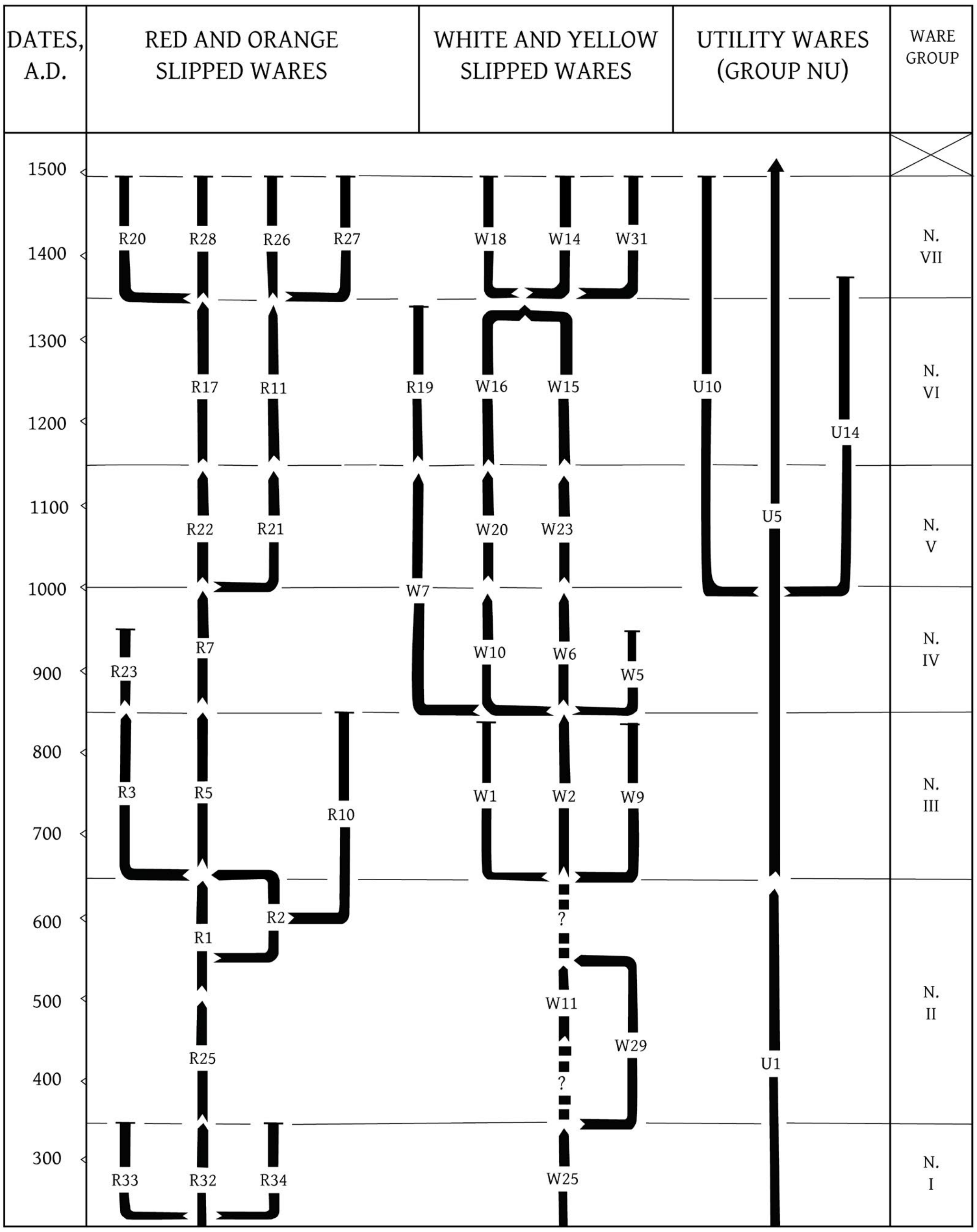

Figure 11.3. The 'cladistic' method of periodization, in which pottery periods (far right column) are defined by the presence or absence of groups of co-occurring pottery types. The types shown (R20, W18, U10, etc.) are medieval Nubian wares. Absolute dates are shown at the far left (from Adams 1986a, 477).

The aforementioned difficulties are overcome, to a considerable extent, by employing the method of frequency seriation. This procedure defines ceramic periods not only by the types present and absent, but also by their relative abundance or scarcity in relation to one another.[8] The relative frequencies of different types, or groups of types, are plotted in terms of the prehistorian's familiar 'battleship graphs' (Figure 11.6), in which the frequency of any given type, relative to the other types shown on the graph, is indicated by the width of its 'battleship' at any given point in time. It is important to notice that the plotted figures do not represent absolute numbers of sherds (raw tallies); they represent only the frequency of a given

[8] See especially Dunnell 1970, 305-319; Cowgill 1972, 381-424; Marquardt 1978, 257-314.

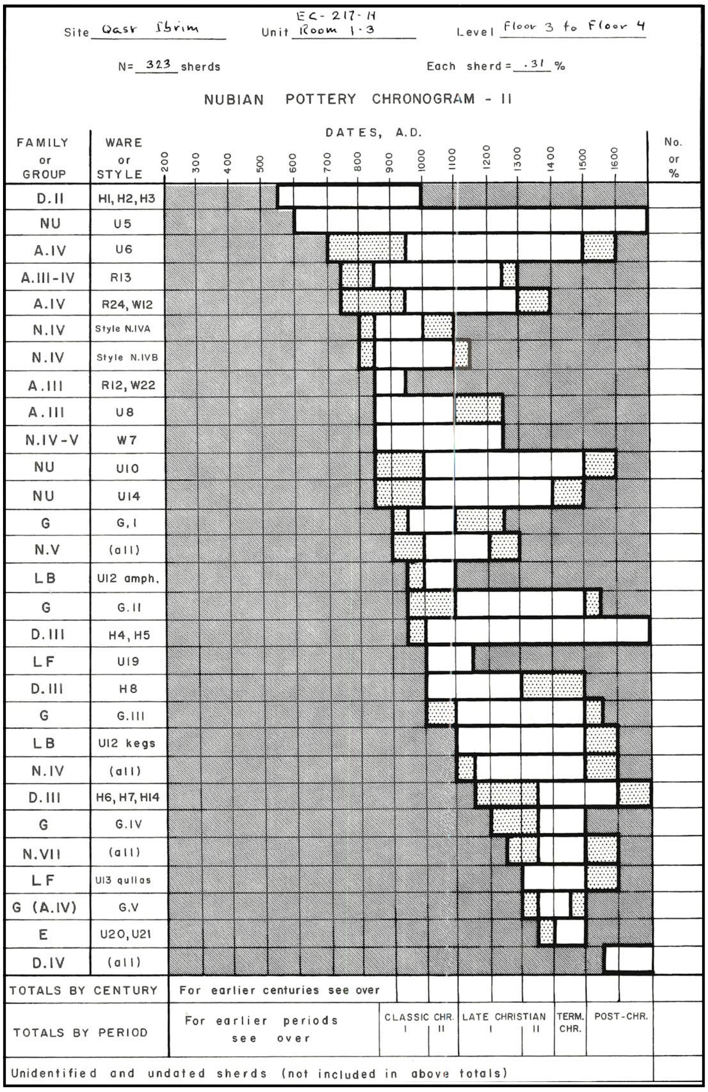

Figure 11.4. Bar graph showing the variable distribution in time of different medieval Nubian pottery wares. Time scale is shown at the top of the chart, ceramic periods at the bottom of the chart (the figure actually shows a form used for recording sherd tallies from medieval Nubian archaeological sites; see Adams 1986a, 623-626).

type or group as a percentage of the total sherd population at the same time. Thus a type can be absolutely more abundant in one period than in the preceding, yet still comprise a lesser percentage of the total sherd population if other types are more abundant still. This kind of proportional reckoning compensates for the variability in the size of total sherd populations between one period or level and another.

Figure 11.6 illustrates the periodization of medieval Nubian pottery on the basis of fluctuating percentages in the main ware groups (i.e. groups of types) in the family of indigenous wheel-made wares (Family N). Some of the designated periods (listed in the second column from the left) are differentiated from those that precede and follow by the presence or absence of certain ware groups. There are, however, certain pairs of horizons (XC2 and EC1; CC1a and CC1b)[9] in which all the same ware groups are present in each of the two successive periods; here it is the differences in percentage relationships that serve to distinguish one period from its successor.

It must be stressed that pottery development does not progress by instantaneous leaps, either in a quantitative or in a qualitative sense. When frequency seriation is employed, it is still necessary to make arbitrary decisions about where to draw the line between one period and another. Frequency seriation, however, does permit this to be done with a much higher degree of precision, or at least objectivity, than is possible with the system of 'cladistic' periodization. The more identified types are involved, moreover, the more finely tuned the periodization, provided of course that the various types are individually rather than collectively dated. Hence the importance of differentiating as many wares as can be consistently recognized.

Reference has already been made to the critical importance of provenience recording for studies of ceramic chronology. The use of frequency seriation requires, in addition, accurate quantitative methods

[9] See Adams (1986a, 440-441) for a fuller description of Family N. The dates of the Nubian medieval periods according to Adams (1986a) are as follows: X-Group (XC, including XC1 and XC2), AD 350-550; Early Christian 1 (EC1), AD 550-700; Early Christian 2 (EC2), AD 700-850; Classic Christian 1 (CC1 including CC1a, CC1b), AD 850-1000; Classic Christian 2 (CC2), AD 1000-1100; Late Christian 1 (LC1), AD 1100-1300; Late Christian 2 (LC2), AD 1300-1400; Terminal Christian (TC), AD 1400-1500. Also see further Figure 11.9 below [ed.].

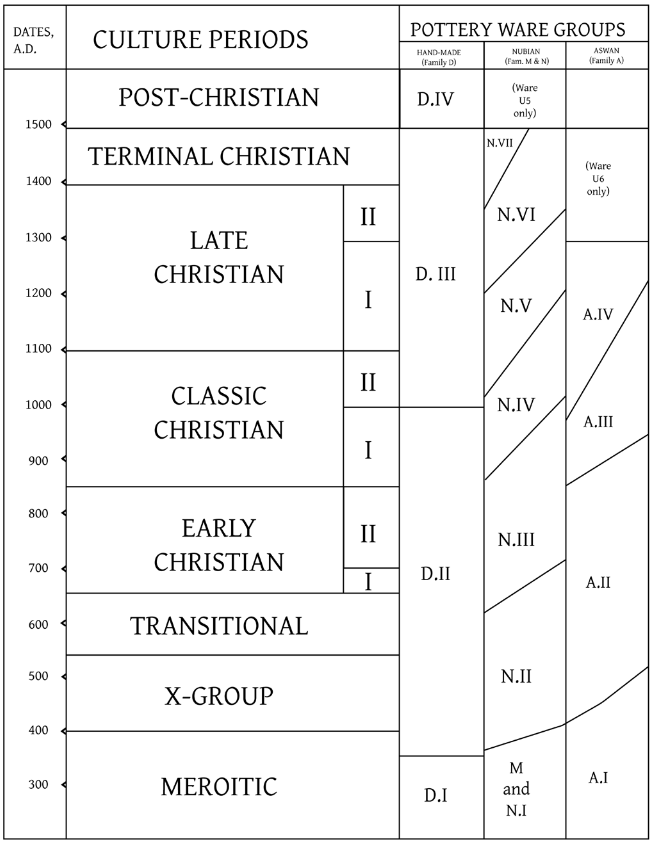

Figure 11.5. Chronological diagram showing variable rates and times of change in the three main medieval Nubian pottery families (three right-hand columns), in relation to culture periods. Time scale is at the far left (from Adams 1986a, 408).

and records. Sherds or types to be used in frequency seriation cannot be collected in an unsystematic or selective way. They must represent either the total sherd population for each unit, or a sample that has been drawn according to scientific sampling procedures. There are a number of different sampling methods that are scientifically acceptable,[10] but the methodology must be the same for every unit and every collection that is to be compared.

In my own excavations I have always worked with total sherd populations, a luxury that may not always be possible on Egyptian sites with their vastly larger numbers of sherds. Many of my excavation units yield only 100 to 200 sherds. Figures of this size are marginally significant statistically, but any reduction by means of sampling would reduce them below the level of significance. Figures 11.7 and 11.8 illustrate the recording forms on which I enter the number of sherds of each ware (type) found in each of my excavation units. Actual numbers of sherds (i.e. raw tallies) are entered in the blanks to the right of the ware designations. These are then converted into percentage figures that are entered in the blanks to the left of the ware designations.[11]

The calculation of incremental dates

The procedures described thus far have obviously yielded only archaeological time. They have allowed us to calculate the chronological relationships of pottery types and groups of types to each other, without indicating what intervals of calendric time were involved. How, then, do we go about correlating our ceramic types and periods with some kind of calendar?

No absolute dating is possible unless we have some means of calendric reckoning that is independent of the pottery itself. In earlier generations this usually meant coins or inscriptions, or some kind of historical records relating to the deposits under investigation. None of these were available to the prehistorian, who for a long time was resigned to the impossibility of absolute or calendric dating. The advent of radiocarbon has now largely changed this situation. There are nevertheless two practical limitations that still affect the use of radiocarbon for dating pottery wares. First, a great many determinations are required, and this can be ruinously expensive. Second, radiocarbon dates are still relatively imprecise, involving in most cases an error factor of 100 years or more. Often, particularly in the historic periods, we require determinations more precise than this.

[10] See Kendall 1969, 68-76; Keighley 1973, 131-136.
[11] See further Chapter 10, Footnote 7 [ed.].

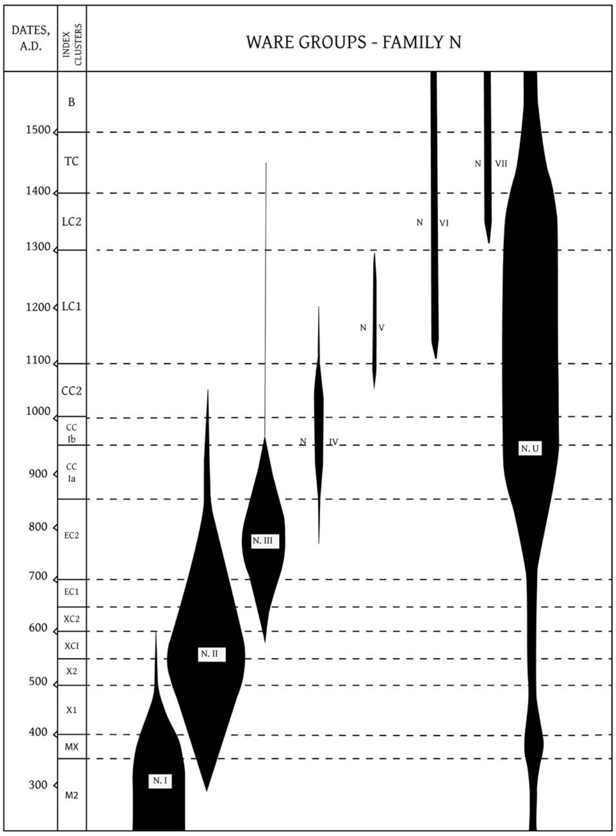

Figure 11.6. 'Battleship graph' showing relative frequencies of different ware groups (N. I, N. II, etc.) in the principal medieval Nubian pottery family (Family N), from AD 200 to 1600. Time scale is at the far left. 'Index clusters' (second column from left) are equivalent to ceramic periods (from Adams 1986a, 608).

It is important to notice that in the dating of pottery types we are talking about incremental dates, not about single fixed points in time. For each type we want to know, at least, when it began to be made and when it ceased to be made. We may also want to know when it reached its peak of popularity, how long individual vessels continued to be used after the type was no longer made, and possibly even how long sherds continued to 'kick around' on the ground surface after the last vessel was broken and discarded (cf. Figure 11.13). Since coins, inscriptions, and radiocarbon dating give us only individual fixed points, the determination of incremental dates depends on an accumulation of such individual datings.

Datings, whether from coins, inscriptions, or radiocarbon, apply to the deposits in which they are found, and, by extension, to everything found with them. In a negative way they may also apply to things not found in the deposits, if those things are sufficiently numerous elsewhere so that their absence is statistically significant. With regard to pottery, every dated deposit may be said to furnish:

1) a *datum ad quem* for all of the pottery types found in it;
2) a *terminus ante quem* for all of the types not found in it, but found in levels below it; and
3) a *terminus post quem* for all of the types not found in it, but found in levels above it.

A sufficient number of such individual datings provides a number of fixed points within the life span of each pottery type, and other fixed points which preceded and followed its life span. These points permit a rough calculation of the life span of each type. (In Nubia, we can usually calculate beginning and ending dates for types within about 50 years). Obviously the more individual dates we have, the more precisely we can calculate incremental dates for pottery types.

Dates for ceramic periods are, in turn, derived from the dates calculated for the individual types occurring during those periods. When periods have been defined 'cladistically' (see above), then the beginning date for any period is the earliest date of appearance for any type making its first appearance in that period, and the ending date is the date of disappearance for the last type to disappear (Figure 11.4). When periods are defined by frequency seriation, they are dated by points on a 'battleship graph' when new types appear and/or old types disappear, or when there is a significant change in the percentage relationship between types (Figure 11.6).

The extension of dates

Perhaps the single most important contribution that the study of pottery makes in archaeology is allowing us to 'extend', or transfer, dates from one deposit to another, once they have been calculated. Coins,

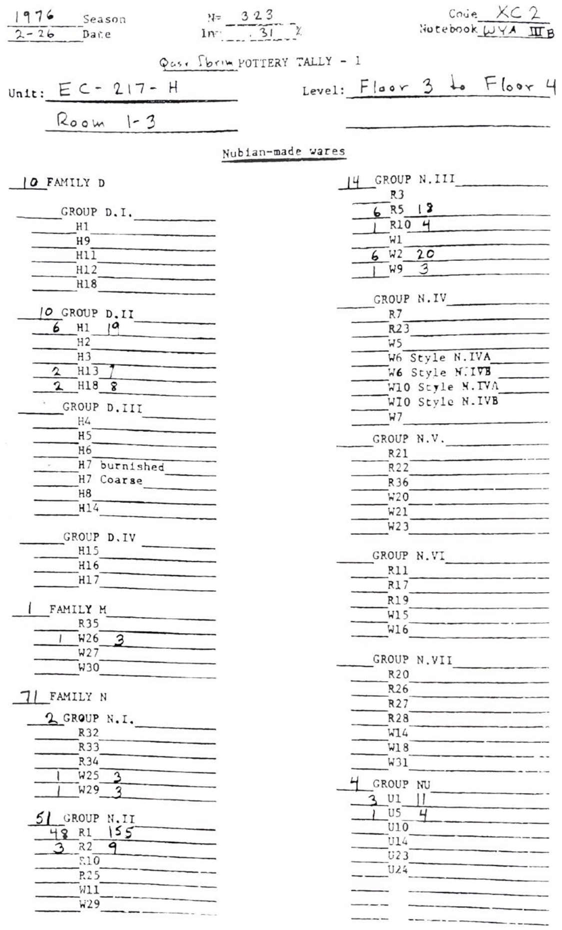
1976 Season N= 323 Code XC 2
2-26 Date In= 31 Notebook WYA IIIB
Qasr Ibrim POTTERY TALLY - 1
Unit: EC-217-H Level: Floor 3 to Floor 4
Room 1-3
Nubian-made wares
10 FAMILY D
GROUP D.I.
H1
H9
H11
H12
H18
10 GROUP D.II
6 H1 19
H2
H3
2 H13 7
2 H18 8
GROUP D.III
H4
H5
H6
H7 burnished
H7 Coarse
H8
H14
GROUP D.IV
H15
H16
H17
1 FAMILY M
R35
1 W26 3
W27
W30
71 FAMILY N
2 GROUP N.I.
R32
R33
R34
1 W25 3
1 W29 3
51 GROUP N.II
48 R1 155
3 R2 9
R10
R25
W11
W29
14 GROUP N.III
R3
6 R5 13
1 R10 4
W1
6 W2 20
1 W9 3
GROUP N.IV
R7
R23
W5
W6 Style N.IVA
W6 Style N.IVB
W10 Style N.IVA
W10 Style N.IVB
W7
GROUP N.V.
R21
R22
R36
W20
W21
W23
GROUP N.VI
R11
R17
R19
W15
W16
GROUP N.VII
R20
R26
R27
R28
W14
W18
W31
4 GROUP NU
3 U1 11
1 U5 4
U10
U14
U23
U24

Figure 11.7. An example of the form used for recording potsherd tallies from medieval Nubian excavation units. Raw tallies are entered in the blanks to the right of the ware numbers (H1, H9, H11, etc.); equivalent percentage figures are entered to the left of the ware numbers. This figure shows the obverse of the form, used for recording indigenous Nubian wares. The reverse side is shown in Figure 11.8.

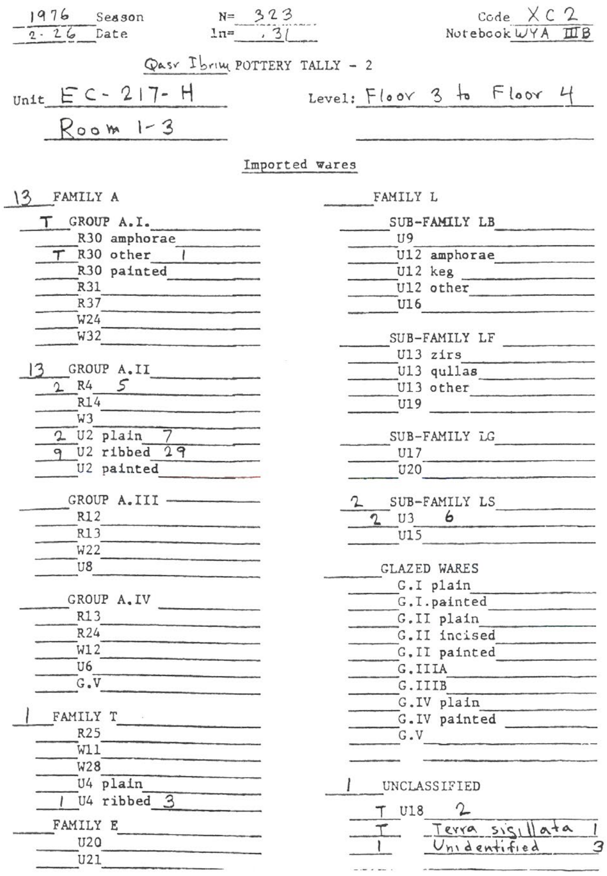
1976 Season N= 323 Code XC 2
2-26 Date In= 31 Notebook WYA IIIB
Qasr Ibrim POTTERY TALLY - 2
Unit EC-217-H Level: Floor 3 to Floor 4
Room 1-3
Imported wares
13 FAMILY A
T GROUP A.I.
R30 amphorae
T R30 other 1
R30 painted
R31
R37
W24
W32
13 GROUP A.II
2 R4 5
R14
W3
2 U2 plain 7
9 U2 ribbed 29
U2 painted
GROUP A.III
R12
R13
W22
U8
GROUP A.IV
R13
R24
W12
U6
G.V
1 FAMILY T
R25
W11
W28
U4 plain
1 U4 ribbed 3
FAMILY E
U20
U21
FAMILY L
SUB-FAMILY LB
U9
U12 amphorae
U12 keg
U12 other
U16
SUB-FAMILY LF
U13 zirs
U13 qullas
U13 other
U19
SUB-FAMILY LG
U17
U20
2 SUB-FAMILY LS
2 U3 6
U15
GLAZED WARES
G.I plain
G.I.painted
G.II plain
G.II incised
G.II painted
G.IIIA
G.IIIB
G.IV plain
G.IV painted
G.V
1 UNCLASSIFIED
T U18 2
T Terra sigillata 1
1 Unidentified 3

Figure 11.8. The reverse of the form shown in Figure 11.7, used for recording tallies of imported pottery wares found in Nubian sites. See Figure 11.7 caption.

			Associated Pottery		
Date A.D.	Dated Object or Event	Family D	Family N	Family A	Other Wares
1528	Ottoman annexation of Nubia	Groups D.IV, D.III			
1484	Last Christian document at Gebel Adda	Group D.III	Groups N.VII, N.VI	Ware U6	Ware U13
1464	Old Nubian documents at Qasr Ibrim		Ware R11		
1276-1323	Defacement of frescoes at Meinarti	D.III	Group N.VI	Group A.IV	
1199	Old Nubian documents at Qasr Ibrim	D.III	Group N.VI, N.V	Groups A.IV, A.III	Ware U12
1173	Rebuilding of fortress wall at Qasr Ibrim	D.III	Group N.VI, N.V	Groups A.IV, A.III	Ware U12
1045	Tombstone at Meinarti	D.III	Group N.VI, N.IV	Groups A.IV, A.III	
992	Document at Gebel Adda		Ware W10		
836-850	Reduction of wine importation to Nubia	D.II	Group N.IV, N.III	Groups A.III, A.II	Ware U4
738	Inscription at Faras pottery factory	D.II	Group N.III	Group A.II	Ware U4
543	Conversion of Nubia to Christianity	D.II	Ware R2	Group A.II	Ware U4, U3
450-550	Inscriptions on imported amphorae	D.II	Group N.II	Group A.II, A.I	Ware U4, U3
350	End of the Empire in Kush	D.II, D.I	Groups N.II, N.I	Group A.I	Ware U4
100	Main Meroitic reoccupation of Lower Nubia	D.I	Group N.I	Group I	

Figure 11.9. Historically recorded dates that have helped to date the medieval Nubian pottery wares, and the wares or groups that they have helped to date.

inscriptions, and radiocarbon samples do not simply date the deposits in which they are found; we can safely assume in most cases that they date all other deposits containing precisely the same combination of pottery wares, and all other things found in those deposits. The formula is, in principle, a simple one. Coins, inscriptions and C14 date the deposits in which they are found; the deposits in turn date the combinations of pottery types found in them. These same pottery combinations date other deposits in which they are found; and those deposits date the non-ceramic objects found in them. In this way an inscription found at Meinarti (Adams 1965, 172) can, through the intermediary of associated pottery wares, date a bronze chalice found a hundred miles away at Qasr Ibrim (Figure 11.9).

Figure 9 lists some of the historical dates that can be applied to medieval Nubian pottery wares (types), and the wares or ware groups to which they can be applied. It may be noted that these dates have been obtained in some cases by the finding of dated objects, and in others from historically recorded events that have left visible traces in the archaeological record. The fourteen fixed dates listed in Figure 9 have, either in a positive or in a negative sense, contributed to the dating of 102 different pottery types, and these in turn have contributed to the dating of more than 400 separate archaeological deposits at Qasr Ibrim alone (Adams 1986b, 27-45).

The significance of incremental dates

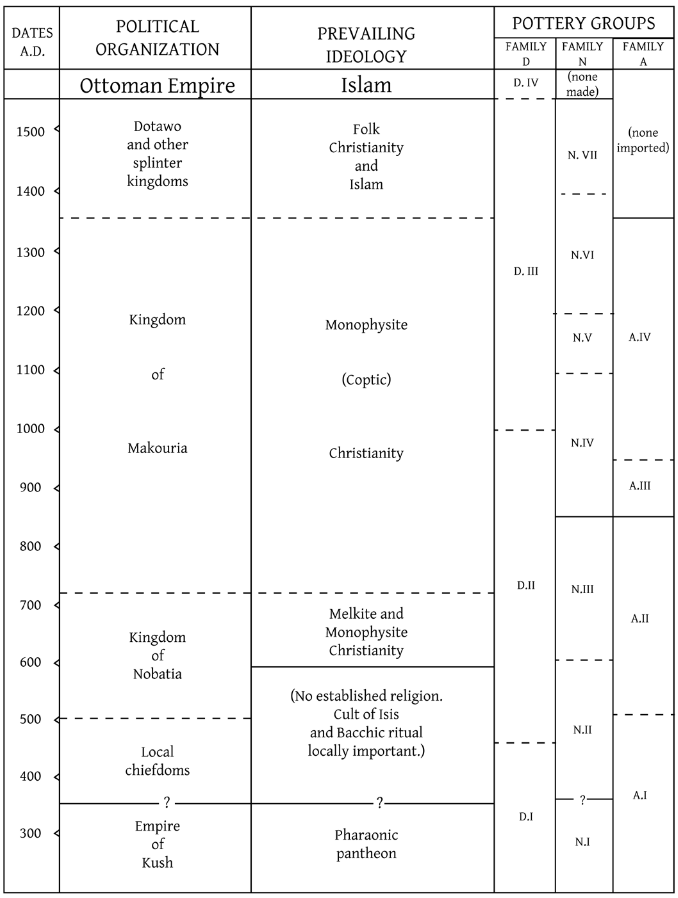

Figure 11.10. Chronological chart showing that the developmental periods in the three main medieval Nubian pottery families (three right-hand columns) do not closely reflect concurrent changes in political organization or ideology. Time scale is at the far left (from Adams 1979, 728; see also chapter 13, this volume [ed.]).

The foregoing discussion summarizes the methodology of ceramic dating – a methodology so commonplace among prehistorians that it is often taken for granted. To establish that a pottery bowl 'is' Badarian, and therefore dates from the fourth millennium BC, it is necessary only to mention that it was found in association with Badarian pottery, without bothering to specify the intermediate steps in the chain of reasoning. This might appear to be a considerable achievement, but we should, nevertheless, ask ourselves at this point what we have really accomplished and not accomplished by the procedures of pottery classification, seriation, periodization, and incremental dating.

Here we must recall the distinction made between pottery dates, occupation dates, and depositional dates. Pottery dates are the datings that we apply to pottery types themselves, that is, to the times of their production and use. These commonly involve archaeological time, but we hope to be able to calculate them in terms of calendric time as well. Occupation dates are the dates that we apply to houses and other structures on the basis of the pottery found in them.

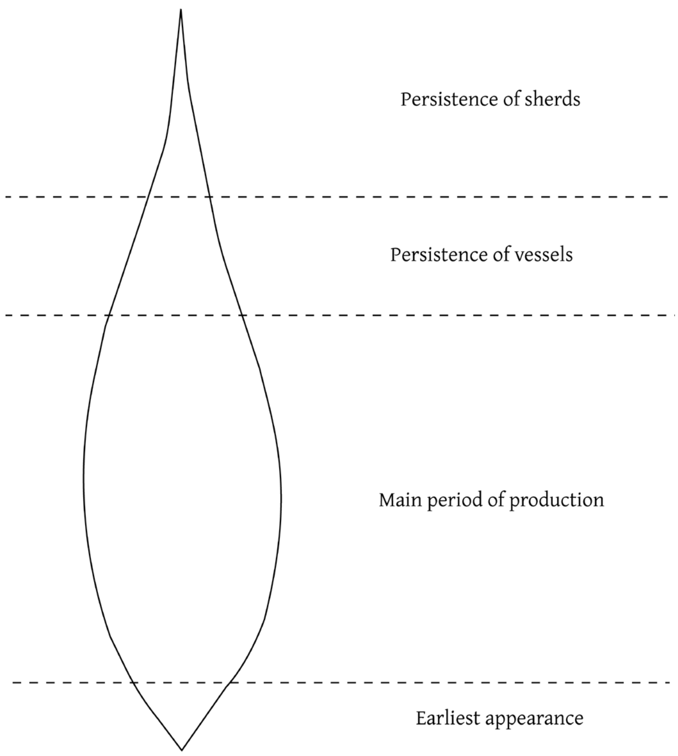

Figure 11.11. Hypothetical 'battleship graph' of an individual pottery type. The sharp lower end represents the time when the type was first coming into use, but had not yet become abundant. The narrowing section near the top, labelled 'persistence of vessels', represents the time when the type was no longer being made, but many individual vessels were still in use. The very attenuated section at the top, labelled 'persistence of sherds', represents the longer period when types were neither made nor used but when their sherds still found their way into occupation deposits in predictable quantities.

Depositional dates are the dates of deposits in which particular pottery types are consistently found. Throughout much of the remaining discussion we will be concerned with the problem of relating these three different kinds of calculations to each other.

In calculating dates according to the procedures discussed thus far, we have not necessarily identified and dated cultural periods, but only pottery periods. Elsewhere I have argued that pottery cannot always be used as a barometer to measure development and change in other areas of culture (Adams 1979, 727-744). Ceramic production frequently evolves according to its own internal dynamic, remaining stable when it might be expected to change, and changing when stability might be expected. This is readily illustrated in Figure 11.10, where we see that the three main medieval Nubian pottery families (three right-hand columns) all evolved at different rates and in different ways, and none of them reflected in any consistent way the important political and ideological changes that took place contemporaneously. Secondly, we have not necessarily dated any walls, floors, or buildings, that is, we have not calculated occupation dates. The chronological relationship between sherd deposits and the structures in which they are found is a complex and sometimes an uncertain one (see below). Finally, we have not really dated the pottery types themselves, if by 'dating' we mean ascertaining the dates of their actual production and use. In other words, we have not calculated what are called pottery dates.

If we study the frequency distribution of a typical pottery type, as shown in a 'battleship graph', we are likely to find that its 'battleship' is somewhat sharp at the lower (beginning) end but extremely thin and attenuated at the upper end. The first of these conditions simply reflects the fact that it takes some time for new types to displace older ones, even when the older ones are no longer actually being made. Whatever the actual numbers may be, we can say that the first date of production for any type also represents the first date of its use.

However, this is emphatically not true with regard to the last dates of production and use. After the cessation of production a large number of vessels will remain in use; indeed, for a time they may still be numerically preponderant. There comes a time, nevertheless, when the last whole vessel is lost or broken, and we can then say that the type has gone out of use. This does not mean, however, that we will no longer find sherds of the disused types in later deposits. At least in Nubia they will predictably turn

	M2	MX	X1	X2	XC1	XC2	EC1	EC2	CC1a	CC1b	CC2	LC1	LC2	TC
FAMILY D	2.0	6.0	4.5	5.0	9.0	8.0	7.5	5	7.5	7.0	9.0	7.5	6.0	40.0
Group D.I	1.5	3.0	0.5											
Group D.II	0.5	3.0	4.0	5.0	9.0	8.0	7.5	5	7.5	5.0	5.0	1.5		
HI, H2, H3	0.5	3.0	4.0	5.0	9.0_{b}	8.0_{b}	7.5	5	7.5	5.0	5.0	1.5		
H13														
Group D.III										2.0	4.0	6.0	6.0	40.0
H4										1.0	2.0	4.0	2.5	14.0
H5										1.0	2.0	1.0	1.5	16.0
H6, H7, H14												b	b	4.0
H8											b	1.0	2.0	6.0
Group D.IV														b
FAMILY M	1.0	2.0	b		0.5	b								
FAMILY N	55.0	62.0	69.0	74.0	75.0	63.0	70.0	72.0	79.0	80.0	74.0	63.0	65.0	37.0
Group N.I	45.0	34.0	6.0	0.5										
R32	38.0	29.0												
W25	6.0	4.0												
W29	1.0	1.0												
Group N.II	5.0	14.0	53.0	63.0	63.0	45.0	28.0	6.5	4.0	1.0	1.0			
R25	5.0	14.0	52.0	61.0	48.0	13.0								
R1		b	b	1.0	14.0	30.0	27.0	6.0	3.5					
R2					1.0	1.5	0.5	0.5	0.5					
W11			0.5											
W29			b	0.5	b	0.5								
Group N.III					1.5	9.0	20.0	38.0	28.0	13.0	2.0	2.0	2.5	1.5
R10					b	1.0	3.0	3.5	1.0	b				
R3, R5					1.5	5.0	11.0	24.0	20.0	9.0	2.0	2.0	2.5	1.5
W1						0.5	1.0	2.0	1.0	0.5				
W2						2.0	4.0	7.0	5.0	3.0	b		b	
W9						0.5	0.5	1.0	1.0	0.5				
Group N.IV								0.5	7.0	14.0	3.0	1.0	b	0.5
R7, R23									b	1.0				
W5									1.0	2.5	1.0			
W6, W10								0.5	5.0	9.5	1.5	1.0	b	0.5
W7									1.0	1.0	0.5	b		
Group N.V										1.0	1.0	2.5	b	b
Group N.VI												5.0	6.0	4.0
Group N.VII												b	6.5	3.5
R26, R27													4.0	1.0
R28, W14													2.0	2.0
Others													0.5	0.5
Group NU	5.0	14.0	9.0	10.0	12.0	9.0	15.0	15.0	47.0	51.0	50.0	44.0	49.0	25.0
U1	4.0	13.0	7.5	9.0	8.0	4.5	4.0	2.0						
U5	1.0	0.5	1.5	1.0	4.0	4.5	11.0	13.0	45.0	46.0	35.0	23.0	35.0	18.0
U10									2.0	4.5	15.0	20.0	13.0	7.0
U14									b	0.5	b	0.5	0.5	b
U23, U24											b	b		
FAMILY A	30.0	17.0	19.0	9.0	10.0	13.0	19.0	17.0	10.0	10.0	13.0	19.0	18.0	8.0
Group A.I	30.0	17.0	10.0	1.0										
R30	30.0	17.0	10.0	1.0										
R37		b	b											
W24	b	b												
Group A.II		b	9.0	7.5	10.0	13.0	19.0	14.0	7.5	2.0	2.5	2.0	1.5	1.5
R4				b	0.5	1.5	1.5	1.0	2.0	1.0				
W3							b	b	b					
U2 plain		b	8.0	7.0	7.5	7.5	8.5	4.0						
U2 ribbed			1.0	0.5	2.0	4.0	8.5	8.0	5.5	1.0	2.5	2.0	1.5	1.5
Group A.III								3.5	2.0	6.0	1.0	1.5	1.0	b
Group A.IV								0.5	0.5	2.0	9.5	16.0	16.0	6.0
R13								b	b	0.5	1.5	2.5	b	b
R24, W12								0.5	0.5	0.5	2.0	3.0	1.0	1.0
U6										1.0	6.0	11.0	14.0	4.5
FAMILY T	1.5	3.5	4.0	5.0	3.0	2.5	2.5	2.5	0.5					
W28		b	0.5	0.5	b	b	b							
U4	1.5	3.0	3.5	4.0	3.0	2.5	3.5	2.5	0.5					

Figure 11.12. Percentage distribution of medieval Nubian pottery wares and ware groups in successive ceramic periods (shown in the scale along the top). Each figure represents the prevalence of a particular ware of group measured as a percentage of the total sherd population at that period. A few exotic wares are omitted.

up, in significant though diminishing quantities, for two to three hundred years after the type itself has gone out of use.

The attenuated upper end of a 'battleship' therefore reflects two things (Figure 11.11). Firstly, there was a certain period when types were no longer being made but were still used (indicated in Figure 11.11 by the heading 'persistence of vessels'). Secondly, there was a usually longer period when types were neither made nor used but when their sherds still found their way into occupation deposits in predictable quantities (indicated in Figure 11.11 by the heading 'persistence of sherds').

How did this latter circumstance come about? As suggested earlier, we often do not know, for we still have a very limited understanding of depositional forces (i.e. human dumping behavior) in stratified sites (Lloyd 1963, 13-28). In Nubia, however, we have noted three factors that may contribute to the persistence of sherds. First, fill material was regularly taken from external refuse deposits for the renewal of house floors. Many sherds of older types might accidentally be included in this redeposited material. Second, enormous numbers of sherds were used as wall chinking in all stone buildings. These would in effect be 'out of circulation' as long as the buildings remained standing, but would be 'returned to circulation' once the walls were dismantled, typically two to three centuries after their erection. Finally, there was continual disturbance of outside ground surfaces and deposits by the passing feet of human and animal traffic, and this might mean that older, formerly buried sherds occasionally found their way back to the surface.

In sum, pottery dates in their 'raw' or uncorrected form do not necessarily date cultural periods, structures, or even the production and use of the pottery types themselves. They date only the deposits in which the types can expectably be found, for reasons that we may or may not understand. In other words they yield only depositional dates, from which we still have the problem of deriving occupation dates, and possibly also pottery dates themselves.

This does not necessarily diminish the value of pottery as a dating tool, providing we understand what it is that we are and are not dating. For example, Figure 11.12 shows the percentage distribution of the main Nubian and imported pottery wares (types) in successive periods. It can be seen here that the wares of Group A II[12] comprise, on the average, 14% of the total sherd population in Period EC2, 7.5% of the population in the succeeding Period CC1a, and 2% of the population in Period CC1b. We have good historical evidence to suggest that most and perhaps all of the wares in Group A II were not made after the EC2 Period, but we also know predictably that A II sherds will turn up in CC1a and CC1b deposits, and in a number of later periods as well. The differential frequencies--14%, 7.5%, and 2%--probably indicate that the wares were still used, but not made in the CC1a Period, and were neither made nor used in the subsequent periods. Whatever the explanation, however, these frequencies in and of themselves help us to differentiate deposits of the EC2, CCla, and CC1b Periods. That is, there is a strong *a priori* probability that any deposit in which 7.5% of the sherds are of the Group A II wares dates from the CC1a Period.

To reiterate: *sherds date deposits*. The questions of how these in turn date buildings, and of how they reflect on the production and use of the pottery types themselves, still require consideration.

From depositional dates to occupation dates

Buildings, graves, and other constructions result from the intentional and concerted activity of an individual or an organized group of individuals. Refuse deposits found in and around buildings reflect intentional but random and unsystematic activity carried on by many separate individuals over time. The presence of whole pots in deposits may be the result either of intent or of accident; in many cases we cannot be sure which. However, the presence of potsherds of any given type in any given deposit almost

[12] For a fuller description of Group A II, see further Adams 1986a, 542-545 [ed.].

certainly results from accidental occurrences, both the initial accident of unintended breakage and the subsequent accidents of deposition and redeposition. Only in the rarest cases can we say that sherds of any given type have ended up in any given place because they were meant to be there.

How, then, can pottery deposits be used to date the buildings in which they are found; in other words, how do we get from depositional dates to occupation dates? Conversely, how do we apply the dates of the buildings (if, for example, they have dated inscriptions on the walls) to the pottery wares found in association? The key word is of course 'association'. What is the nature of the association between pottery types and other cultural remains that we sometimes date with the aid of pottery types, and sometimes use to date the pottery itself? This is one of the most neglected issues in the field of archaeological chronology (Adams 1986c).

The problem is not simple. There are various ways in which pots and potsherds can get into refuse deposits and there are also various ways in which refuse deposits can get into buildings. In general, the origin and the significance of deposits vary in accordance with the kind of structure involved, and whether the deposits contain pots or sherds. As a result, pottery is likely to tell us different things about different kinds of buildings; conversely, different kinds of buildings provide different information about the pottery types found in them. The present section will cover the first of these issues--what the pottery tells us about the structures. We will consider the other side of the relationship in the following section.

Graves: Theoretically graves should be the most precisely datable of all archaeological structures, since the time between digging and filling can be measured in days, or less. Without historical records, however, we can seldom date graves any more precisely than we can other structures, usually not within intervals of less than 50 years. Like other structures, graves are more often dated on the basis of pottery found in them than by any other means.

Pottery contributes to the dating of graves in two quite separate ways. There are, first of all, the random sherds found in the grave fill. Unless the grave has been plundered, the latest pottery types represented in the fill will provide a *terminus post quem* for the digging and filling of the shaft. This may not be very precise, but it is often the only dating available for graves in which no offerings were included, as in virtually all Nubian graves after about AD 600.

Fortunately, most Nubian graves were dug into sterile soil or bedrock rather than through refuse deposits. Thus if any sherds found their way into the grave fill, it was probably because they were lying on the ground surface where the grave was dug, not because they were in the excavated material. More often than not such sherds would represent currently or recently used wares. Sherds found in the fill may therefore offer a more precise basis for the dating of graves than is usually supposed.

When possible, of course, graves are dated by the pottery offerings included in them. When several different types are interred in the same grave, the one of most recent origin (i.e. the one whose manufacture began at the latest date) provides the most precise basis for dating. The pre-Christian pottery found in Nubian graves seldom shows any signs of wear, suggesting that new vessels were preferred as funerary offerings over older or used ones. It is therefore unnecessary to allow for any significant passage of time between the moment when the vessels were made and the moment when they were interred.

Caches: By 'cache' is meant a group of pottery vessels that was deliberately buried, not in a grave but under a house floor. A cache presupposes the buriers' intention that these vessels would eventually be returned to use, but due to some accident it never happened. Pottery caches are relatively common in medieval Nubian sites. Most often they were deposited in holes dug in the house floors; occasionally they were placed directly on the floor, and the entire room was then filled sufficiently to cover them (Adams 1964, 237, pl. Ll, b. 27; 1986c). Caches differ from grave deposits in two respects. First, the fill used to cover them was usually dug from nearby rubbish deposits, and might contain sherds of any previous age. Second, the vessels had often been used for a long time before they were buried. In the two largest caches found at Meinarti, many of the vessels showed extensive signs of wear (Adams 1964, 237, pl. Ll, b. 27; 1986c). Therefore, the time of deposition of a cache does not necessarily reflect when the vessels were actually made, and the types themselves do not indicate the time of their deposition.

Floor Deposits: Pottery is commonly found on house floors and outdoor occupation surfaces, and is usually regarded as a reliable indicator of the age of the floor itself. We must nevertheless ask ourselves in the first instance how the pottery came to be on the floor. At least indoors, people do not ordinarily walk around over broken potsherds, especially not in bare feet. We have to assume therefore that most of the sherds found on house floors were not actually there during occupation. When only one floor level is present in a room, the sherds found on it most probably represent immediate post-abandonment deposition. When there are several successive floor levels, as is usual in Nubian houses, we assume that the sherds found on any floor were part of the fill material brought in from outside when the floor was renewed. The pottery types found in such a deposit may furnish a *terminus ante quem* for the use of the underlying floor, but they do not furnish a datum *ad quem* for the floor itself.

We also find, with surprising frequency, whole or restorable vessels resting directly on floors. It is difficult to believe that these were simply overlooked when new floor deposits were laid in; it is even harder to attribute them to accidental post-abandonment deposition. We are therefore left to conclude that they were deliberately buried, though to what end is far from clear. Perhaps we are seeing the equivalent of our own common practice of storing out-of-fashion clothing in attic trunks, with the vague feeling that there may still be some use for it in the future. In Nubia we find whole vessels buried on house floors rather consistently in two particular eras: around AD 500 and AD 850. It is noteworthy that each of these corresponds to a time when new and more elaborate pottery styles were coming into vogue, lending support to the idea that the older vessels were buried because they were considered out of date.

In any case we can make the reasonable assumption that pottery vessels found in contact with a house floor furnish a legitimate datum *ad quem* for the use of that floor.

When vessels of more than one type are present, the latest type is the governing one for dating purposes. As in the case of caches, there is also the possibility that all the vessels found on any floor had long been in use before they were buried, along with the floor itself.

Houses: Broadly speaking, house deposits fall into three categories which may be called overfill, infill, and underfill. All three are likely to be rich in sherds. Overfill means post-abandonment fill, which normally includes everything overlying the uppermost floor in each room. Much of this material is actually within the walls. By underfill is meant pre-construction deposits, normally everything underlying the earliest floor surface. Sherds found in the overfill and in the underfill provide *termini ante quem* and *post quem* for the building and occupation of the house. For a variety of reasons, however, they are often not very precise. Houses were sometimes built on recently cleared ground from which the uppermost deposits had been removed, so that a house might be several centuries later in date than the material found immediately beneath it. After abandonment, refuse of many different kinds and from many different sources was likely to find its way into the disused rooms. In addition the gradual collapse of the walls would 'release' sherds of earlier date that had been employed as chinking. In Nubia we have found that this is not much of a problem where brick construction was involved, but a very serious problem in the case of stone construction, where tens of thousands of sherds were often employed as chinking between the rough-hewn building blocks.

Infill comprises the material found on and between the different floor surfaces in a room or house or both, and it is this material that largely tells the story of occupation. The average medieval Nubian house has between three and seven recognizable floor surfaces in each room, and I presume that the situation is similar in Egyptian townsites. Whole or restorable vessels found directly on any of the floors are of course most reliable for dating. When no such vessels are found, as is true in the great majority of cases, then we have to fall back on the sherds found in the fill between floors. Close examination will usually show that these are of many different types and from many different vessels. Indeed, it is rare to find any two sherds in a floor-fill deposit that can be fitted together. This is fairly conclusive evidence that the sherds found between floor surfaces were part of the fill material brought in when the floors were renewed; they do not result from the breakage of vessels that were actually used in the rooms. Technically speaking, therefore, they furnish *termini ante quem* rather than data *ad quem* for the underlying floors. Infill sherds have, nevertheless, proven very reliable for dating house floors, at least in Nubia. Evidence suggests that the material used as floor filling was usually taken from the surfaces of nearby refuse deposits and therefore contained mainly sherds that had been recently dumped, representing pottery types still currently in use.

The complete occupation history of a house is generally indicated by the sum of the infill deposits between its various floors. If, for example, we have a five-room house with an average of five floors in each room, we then have four infill deposits that were laid down between the first building of the house and its final abandonment. If, after counting the sherds, we find that the three lower deposits exhibit the Late Christian I sherd complex, and the uppermost deposit exhibits the Late Christian II complex (Figure 11.12), we feel safe in assuming that the occupation of the house belongs to the Late Christian I and the beginning of the Late Christian II Period. If we find that the underfill also exhibits one or more Late Christian I occupation surfaces, then we can further conclude that occupation began in the later part of the Late Christian I Period. The combination of stratigraphic and ceramic evidence has enabled us at Qasr Ibrim to date most floor surfaces within an interval of 25 to 50 years (Adams 1986b, 27-45).

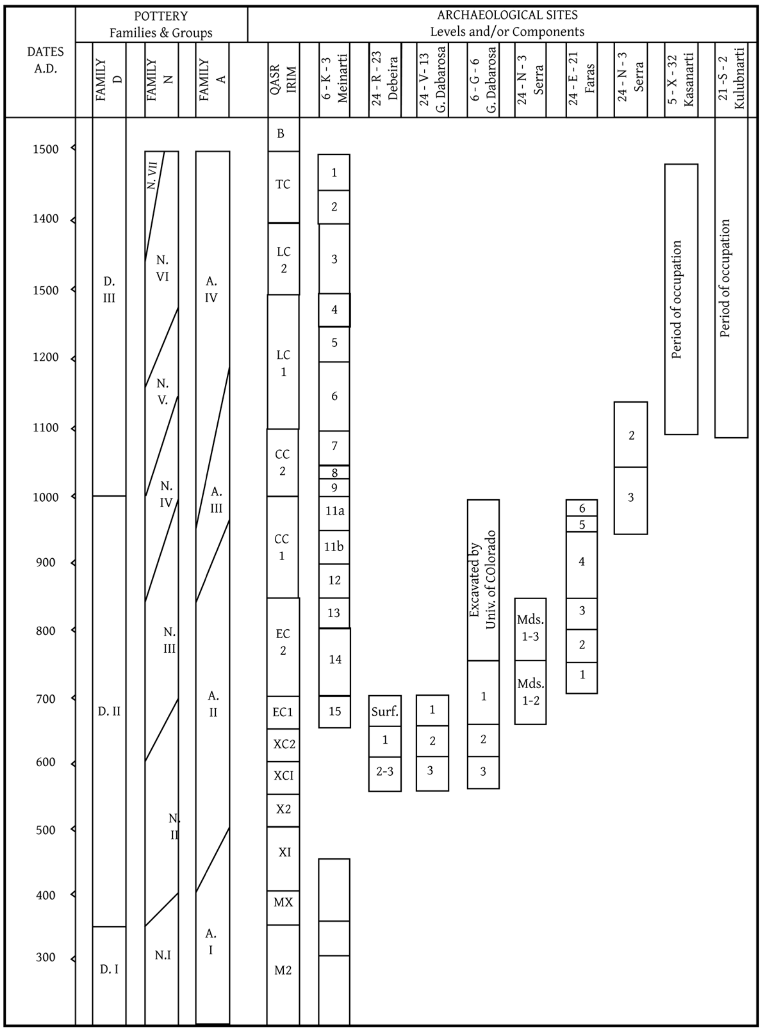

Figure 11.13. Chronology of ware groups in the three main medieval Nubian pottery families (three left-hand columns) in relation to stratigraphic levels in various sites. Pottery factory sites (identified by numbers and names in the second row from the top) are 24-R-23, 24-V-13, 24-N-3, and 24-E-21 (from Adams 1986a, 602).

The observation and recording of physical contacts is critical in this kind of dating. Pottery dates floors; floors date walls. Any excavation strategy that destroys or fails to record contact between pots or potsherds and floors, or between floors and the walls with which they are articulated, eliminates any reliable use of ceramics for dating. The relatively common practice of trenching down along walls until their foundations are exposed is, therefore, inimical to ceramic dating.

Monumental Buildings: Pottery is not of much use for dating temples or churches. These buildings typically had a very long history, during which they were kept largely clear of accumulated rubbish. When floors were renewed, they were not necessarily heightened at the same time. Any material found within temples or churches is therefore likely to represent post-abandonment deposition. However, Nubian churches often contain sherds of many different ages. In these circumstances the earlier types are interpreted as fallen wall chinking and later ones as part of the post-abandonment rubbish deposit, but there is no real way of knowing which is which.

It may be noted, by way of summary, that whole and restorable pots are more reliable for dating structures than are sherds. Pots often furnish data *ad quem*; sherds usually furnish only *termini anti quem*, and occasionally also *termini post quem*. On the other hand the great majority of archaeological deposits, excluding graves, contain no whole or restorable vessels. Sherds are, 99 times out of 100, our best clue to the age of archaeological deposits. They can be a fairly precise clue, providing we learn how to deal with them quantitatively, and understand what their dates do and do not tell us.

From occupation dates to pottery dates

After deciding how to date the refuse deposits in which particular pottery types are regularly found (i.e. how to calculate depositional dates), we now face another question: how do we determine the periods in time when pottery types were actually made and used, as distinguished from the time when their sherds continued to find their way into refuse deposits (Figure 11)? In other words, how do we get from depositional dates and/or occupation dates to pottery dates per se?

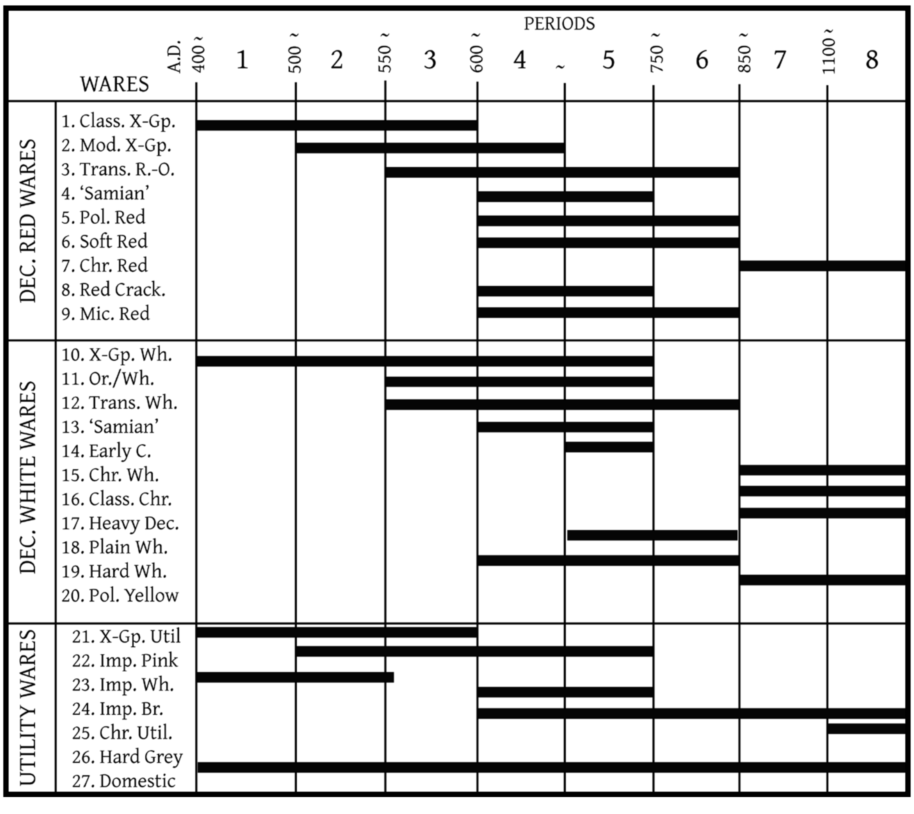

Figure 11.14. Chronology of pottery wares found in the Faras pottery factory, in relation to successive ceramic periods. Periods 4-7 correspond to the actual period of production at the Faras factory (from Adams 1962b, 283).

The answer, insofar as there is one, lies in the recovery and study of pottery from many different contexts. Pottery furnishes different kinds of information about different kinds of sites. The converse is also true: different kinds of sites provide different information about the making and use of pottery. Four different kinds of archaeological contexts have contributed in different ways to the dating of the medieval Nubian pottery types:

Factories: We have been fortunate in the discovery and excavation of three factories in which the medieval Nubian decorated wares were manufactured and several others that produced only utility vessels (Adams 1961, 30-43; 1962a, 62-75; 1986a; Adams and Nordström 1963, 14).[13] In the case of the former group, all three factories had a sufficiently long history so that they exhibited well defined stratification, with some change in the pottery from the earliest to the latest deposits (Figure 11.13).

The special virtue of factory sites is that they are not, by their nature, sites where people lived. The sherds and unfinished vessels we find in them consist entirely of wasters and other byproducts of the manufacturing process. We can be fairly certain, therefore, that all the sherds found at any given level represent types actually manufactured at that time; there are no 'heirlooms'. The abundance and variety of the material also gives us a good idea of all the different vessel forms and designs that were being produced at that time and place. Conversely, the absence of a type in pottery factory deposits constitutes reasonably good *a priori* evidence that is was not being produced.

The Faras pottery factory had six recognizable levels, each of which yielded a complex of types that was partly the same as, but partly different from, the complex exhibited by the overlying and underlying levels (Adams 1962b, 283). From this evidence it was possible to calculate the chronological relationships between no fewer than 27 different types, only a few of which corresponded exactly in time with any of the other types (Figure 11.14). Moreover, a dated inscription found in one of the rooms (Adams 1961, 42) provided a *datum ad quem* for nine types, a *terminus ante quem* for ten others that occurred only in earlier levels, and a *terminus post quem* for six types that occurred only in later levels.

Factories, in sum, give us our best evidence as to the types actually being made and not being made at any one place and at a particular moment or time period.

Graves: Pottery found in Nubian graves usually appears to have been new, or at least unused, at the time of its burial. The finding of two or more types in the same grave is, therefore, reasonable a priori evidence that they were being made at the same time. However, we cannot argue from negative evidence in the case of single graves. That is, the absence of a type does not constitute evidence that it was not being made. It is only when large numbers of graves have been excavated that we can begin to use the evidence of mortuary offerings quantitatively and statistically. When two or more types are never found together, we can then assume that it is because they were not made at the same time. When a very large number of excavated graves yields a number of positive and negative correlations between types, we may even be able to achieve a fairly sophisticated chronological seriation, both of the graves and of the pottery types found in them, as Petrie did in his famous seriation of Predynastic Egyptian graves (Petrie 1899, 295-301).

In Dynastic Egyptian graves, datable objects such as scarabs and seals may at times provide a calendric or historical dating of the associated pottery types. Such datings are most often provided by cartouches, which identify reigns rather than individual years. The dates in question are therefore incremental rather than precisely fixed. In the post-Dynastic periods, graves can sometimes be dated on the basis of dated tombstones. Unfortunately, we find these in association only with Roman, Christian, and Islamic graves, none of which contain any amount of pottery.

[13] Additional kiln sites have been discovered and excavated in Sudan since this article appeared which further illustrate the author's point, such as for example, Old Dongola, R1 (Pluskota 2001).

Group/ware	Earliest appearance	Main manufacture	Persistence: Vessels	Persistence: Sherds
Group D.I	100 B.C.	100-1000	-[b]	110
H1	100 B.C.	100-1000	-	110
H9	?	200-350	-	-
H11	100 B.C.	100-350	-	-
H12	?	200-350	-	-
Group D.II	350	350-1000	?[a]	1100
H1		(see Group D.I)		
H13	500	550-650	?	700
H18	?	350?-650?	?	-
H2	550	550-1000	?	110
H3	550	550-1000	?	110
Group D.III	950	1000-1600+	?	?
H4	950	1000-1600+	?	?
H5	950	1000-1600+	?	?
H6	1150	1350-1600	?	?
H7	1150	1350-1600	?	?
H8	?	1000-1300	1500	?
H14	1150	1350-1600	?	?
Group D.IV	?	1550-?	?	?
H4		(see Group D.III)		
H5		(see Group D.III)		
H15	?	1550-?	?	?
H16	?	1550-?	?	?
Family M	100	200-550	650	-
R35	100	200-350	?	?
W26	100	200-350	?	?
W27	?	?	?	?
W30	?	400-550	650	750
Group N.I	100	100-350	425	500
R32	100	100-350	425	500
R33	100	100?-350	425	?
R34	?	?-350	425	?
W25	100	100-350	425	500
Group N.II	300	350-650	750	800
R25	300	350-600	650	-
R1	400	450-650	750	800
R2	550	550-650	750	-
R10		(see Group N.III)		
W11	450?	450?-550	600	650
W29	400	450-550	600	650
Group N.III	550	650-975	1025	1100
R10	550	600-975	1025	1100
R3	?	650-975	1025	1100
R5	550	650-975	1025	1100
W1	550	650-975	1025	1100

Group/Ware	Earliest appearance	Main manufacture	Persistence: Vessels	Persistence: Sherds
W2	550	650-975	1025	1100
W9	550	650-975	1025	1100
Group N.IV	800	850-1100	1150	1250
R7	900?	850-1100	?	?
R23	900?	950-1100	?	?
W5	800	?	1100	1250
W6	800	850-1000	1150	1250
W10	800	850-1100	1150	1250
W7	850	850-1250	?	?
Group N.V	900	1000-1200	1300	-
R21	900	1000-1200	1300	-
R22	900	1000-1200	1300	-
R36	?	?	?	?
W20	900?	1000-1200	1300	-
W23	?	1000-1200	1300?	-
W21	?	?	?	?
W7		(see Group N.IV)		
Group N.VI	1100	1150-1500	1600	?
R11	1100	1150-1500	1600	?
R17	1100	1150-1500	1600	?
R19	1100	1150-1500	1600	?
W15	1100	1150-1500	1600	?
W16	1100	1150-1500	1600	?
Group N.VII	1250	1300-1500	1600	?
R20	?	?	?	?
R26	1250	1300-1500	1600	?
R27	?	1300-1500?	?	?
R28	?	1300-1500?	?	?
W14	1250	1300-1500	1600	?
W18	?	?	?	?
W31	?	?	?	?
Group NU	100	1000-1600+	?	?
U1	100	100-650	800	850
U5	100	1000-1600+	?	?
U10	850	1000-1500	1600	?
U14	850	1000-1400	1500	?
U23	?	1050-1200?	?	?
U24	?	1050-1200?	?	?
Group A.I	100 B.C.	100-475	550	650
R30	100 B.C.	100-475	550	650
R31	400	450-550	?	?
R37	?	350-475	550	?
W34	?	200?-400?	550	?
W32	?	400?-500?	?	?
Group A.II	400	400-850	950	1100
R4	400	550-850	950	1100
R14	?	650?-750?	?	-

Group/ware	Earliest appearance	Main manufacture	Persistence: Vessels	Persistence: Sherds
W3	650	650-850	-	-
U2 plain	400	400-750	850	1100
U2 ribbed	450	550-850	1000	1100
Group A.III	750	850-1100	1200	1250
R12	?	850?-950?	?	?
R13	750	850-1250	1300	1500
W22	?	850?-950?	?	?
U8	850	850-1100	1250	-
Group A.IV	700	950-1500	1600	?
R13		(see Group A.III)		
R24	?	950-1300?	?	?
W12	750	950-1300	1400	1500
U6	700	950-1500	1600	?
G.V	1300	1350-1450	1500	?
Family T	100	350-850	1000	1100
R25	300	350-600	650	-
W11	450?	450?-550?	600	?
W28	350	400-550	600?	750
U4	100	350-850	1000	1100
Family E				
U20	?	1400-1500?	?	?
U21	1350	1400-1500	?	?
Sub-family LB				
U16	300?	400?-500?	?	?
U9	?	700?-800?	?	?
U12	400	1100-1500	1600	?
Sub-family LF				
U13	350	1300-1500	1600	?
U19	?	1000-1150	?	?
Sub-family LG				
U17	?	1050-1300?	?	?
Sub-family LS				
U3	200	400-650	-	850
U15	?	1300-1400?	?	?
Unclassified				
U18	350	350-650	-	-
Family G				
Group G.I	900	950-1100	1250	-
Group G.II	950	1100-1500	1550	?
Group G.III				
	1000	1100-1500	1550	?
Group G.IV				
	1200	1300-1500	1550	?
Ware G.V	1300	1350-1450	1500	?

[a] Insufficient data for estimation
[b] Apparently no significant persistence

Figure 11.15. Dates calculated for medieval Nubian pottery wares and ware groups. Dates shown under 'persistence of vessels' are the last dates at which some vessels of the ware were still in use, after production had ceased. Dates shown under 'persistence of sherds' are the last dates at which sherds of the ware continued to find their way into refuse deposits (from Adams 1986a, 614).

Caches and Floor Deposits: In and of themselves, caches and floor deposits are useful only in telling us what pottery types were in use at the same time. Some of the vessels buried in any given cache, or on a floor, might be 'heirlooms', while others might be relatively new, so that we cannot deduce from their physical association that the types were all still being made at the time of their burial.

As noted earlier, only whole or restorable vessels can legitimately be interpreted as floor deposits. Sherds, even if resting directly on the floor, usually represent infilling at the time when floors were renewed, and they do not have a precise chronological association with the floor itself.

Houses: House floors occasionally yield whole or restorable vessels. In these cases it might be possible to date the pottery types in question on the basis of other datable objects found on the same floors, or even of inscriptions on the nearby walls (Adams 1961, 42). However, the great majority of pottery found in houses consists of sherds contained in the infilling between floors, which cannot be directly associated with the use of the floors either above or below. This material is, nevertheless, absolutely basic for the calculation of pottery dates.

The great virtue of house deposits, in contrast to other types of archaeological deposits, lies in their clearly demarcated stratigraphy. In the Nile Valley practically all habitation sites occupied for any length of time are stratified. The stratification of exterior deposits, however, is subject to all kinds of vicissitudes that may result in a discontinuous or disturbed sequence of deposits. Yet a series of deposits within any set of walls nearly always represents a continuous sequence of deposition episodes. It is sequence, rather than specific association with any particular floor or floors, that is critical. Since there are usually a number of different floor levels in each room, it is possible to observe, from level to level, which pottery types were coming into use, which were remaining in use, and which were going out of use. Even more importantly, we are able to plot quantitatively the relative frequency of types at each level and thus achieve the kinds of frequency seriations that were discussed above. In Nubia we have been able to record and to cross-correlate the pottery type sequences from level to level in hundreds of individual rooms in scores of separate houses, and in sites as far apart as Qasr Ibrim and Kulubnarti (Figure 11.12). It is these calculations, based largely on stratified house deposits, that have led to the fairly precise datings of the medieval wares that are shown in Figures 11.12 and 11.15.

Bases of calculation

Using all available evidence, I have attempted, for each of 102 medieval Nubian wares, to calculate the first dates of appearance, the main dates of production (or importation in the case of exotic wares), the periods when some vessels continued to be used after production or importation had ceased, and the periods following the end of both production and use when sherds would still continue to find their way into refuse deposits in predictable quantities (Figures 11.11 and 11.15). It may be worthwhile to say a word about the basis of each of these calculations, though I do not necessarily suggest that the same evidence can be interpreted in the same ways in other areas.

<u>Dates of first appearance</u> are calculated, whenever possible, from the first appearance of pottery types in the factories where they were subsequently made. Failing this evidence, they are calculated from those deposits in which the types first appear in numbers too large to be due to the chance of displacement of sherds (normally 1% or more of the total pottery count). In these cases we always find that the types are still very much outnumbered by other types in the same family which were their immediate predecessors. It is very rare indeed to find whole specimens of any pottery type in the earliest deposits in which that type appears.

<u>Main dates of manufacture</u>, like dates of first appearance, are most reliably calculated from the evidence of pottery factories. Otherwise, they are calculated from those deposits in which the type in question significantly outnumbers both the predecessor and the successor types in the same family. This is of course the necessary basis on which we calculate dates of importation for the exotic wares, since in these cases we do not have the evidence of the factories where they were actually made. It is chiefly in deposits corresponding to the main period of production or importation that we are likely to find whole specimens of the types in question.

<u>Persistence of vessels</u> after production had ceased is indicated by a combination of negative and positive evidence: we no longer find the types in the pottery factories, but we do still find sufficient amounts of sherds in refuse deposits to indicate that their presence cannot be due to the redeposition of sherds from other contexts. Typically these sherds constitute from 2.5% to 15% of the total pottery complex, but they are outnumbered by the successor types in their own families. More conclusive proof of the persistence of vessels is provided when, occasionally, we still find whole vessels in floor deposits or caches, even though sherds of the same types are no longer very prevalent.

<u>Persistence of sherds</u> after the cessation of both production and use is indicated by the regular appearance of small numbers of sherds (usually 1% to 2.5%) in deposits where we never find whole examples of the same type.

Underlying principles

All of the methodology discussed in this paper derives in one way or another from the science of population statistics, a discipline which in its turn is the foundation for most of the social sciences.

Recording: Obviously no scientific procedure is possible without accurate records. For the purposes of ceramic dating it is necessary to keep precise records of the numbers of sherds and vessels found, the types found, and where they were found. This latter consideration in turn requires the maintenance of accurate stratigraphic controls and records.

Sampling: The excavator may choose to collect all potsherds or to collect some fraction of the total population. In the latter case the fraction must be collected according to scientific sampling procedures that assure that what is studied is representative in every respect of what is not studied. This is not likely to happen if any judgmental factor is involved in the collection, a situation that will almost certainly result in the overrepresentation of certain types (e.g. decorated sherds or rim sherds) at the expense of others. Judgment is, however, permissible in deciding what total population to sample. For example, there may for legitimate reasons be a decision to collect only rim sherds. In that case what is collected must be treated as a sample of the total population of rim sherds, not of the total population of all sherds. It is necessary in any case that the sample procedure be uniform for all excavation units that are to be compared.

Quantification: It cannot be stressed too often that, for any kind of statistical procedure, numbers (and the accurate recording of numbers) are essential. As applied to the procedures of ceramic dating, this has several dimensions. First, a substantial number of sherds or vessels or both is necessary for the definition of each pottery type. Second, a considerable number and variety of types is necessary for effective frequency seriation. Third, a substantial number of sherds and types is necessary for the reliable dating of deposits. Finally, a substantial number of deposits from different levels, and preferably from different kinds of sites, is necessary for the dating of the pottery types themselves.

Classification: The role of pottery as a dating device is proportional to its classifiability. Precisely because pottery usually falls into readily recognizable types,[14] which have definable but limited distributions in time, we can transfer dates from one deposit to another. That is, we can say with confidence that a sherd of Type A in any deposit implies the same set of incremental dates as does any other sherd of Type A in any other deposit, even hundreds of miles away. In order to work effectively as a dating device, however, the typology must be developed for that purpose. First, the types must have discoverable but limited distributions in time, meaning that they must be defined at least partly on the basis of features (variables and attributes) that are chronologically sensitive. Second, as a basis for statistical procedures the types must not be cross-cutting; they must be defined in such a way that each sherd is assignable to one and only one type. (In most systems there must of course be a residual, 'none-of-the-above' category.) Finally, because they are involved in large-scale sorting and counting operations, the types must be readily recognizable to the naked eye.

Context and Association: The chronological ordering of pottery types can very rarely be done and can never be done with absolute reliability, on the basis of internal evidence alone. Thus we cannot take a group of pottery types and decide, by comparing their visible characteristics, which are earlier and which are later. We require evidence external to the pottery itself, and that evidence comes from the contexts in which the pottery is found. The most common contextual evidence is stratigraphic, but we may also have the evidence of association between pottery types and such things as datable objects, inscriptions, and buildings. In any case the observation and recording of context and association are critical, between pottery types and other pottery types, between pottery and other objects, between pottery and the surrounding deposits, between pottery and occupation surfaces, and between these and the surrounding structures.

[14] But see Wright 1967, 99-100.

Appropriate Caution: The theoretical basis of population statistics, and of all social science, is probabilistic, not absolute. This has two important implications. It means that our predictions are true most of the time, not all of the time. Also, it means that we often know that certain things will ordinarily happen without knowing why; in other words we can predict much more than we can explain. Translated into the specific terms of pottery dating, it should be evident that none of the procedures described here is foolproof. All will occasionally yield incorrect or misleading age determinations. Experience has simply shown that these procedures will work most of the time, though we may not always understand why they do. It is hard to explain, for example, why rates of deposition for dozens of different pottery types are uniform not only all over the five-acre site of Qasr Ibrim, but hundreds of miles away at Meinarti and Kulubnarti as well.[15] This simply underscores the fact that we are a long way from a systematic understanding of human dumping activity.

It should be apparent too that, in that large majority of cases in which ceramic dating really does produce useful results, those results are not obtained through simple, purely mechanical procedures. At the outset pottery gives us only depositional dates that may or may not apply to buildings, occupations, or even to the making and use of the pottery itself. To translate depositional dates into occupation and pottery dates requires further evidence and calculations. These vary according to the kind of site involved. There are many circumstances in which we do not have the necessary evidence and must be content with calculating depositional dates that do not tell us what we really want to know: when the buildings were occupied and when the pottery was made.

What we have been discussing all along is a very complex information feedback between pottery and its context, a two-way relationship that applies equally to the classification and the chronological ordering of pottery. There is much in both relationships that we still do not fully understand, and this leads to two final observations. First, the kind of dialectic that we have been discussing cannot be comprehended within the framework of simple linear models and flow charts such as are currently fashionable among prehistorians. Second, none of the problems discussed here can be eliminated, or even substantially alleviated, by recourse to computerized classification programs, neutron activation or thermoluminescence analysis, or any of the other scientific and technical aids recently introduced into the study of pottery. All of those procedures have their place, but they cannot substitute entirely for human judgment, which is the ultimate requirement in pottery studies. So far, it remains an exclusively human capacity.

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[15] For discussion, see Adams 1986b, 32-33.

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CERAMICS

The Archaeologist and The Ceramologist[1]

(1981)

This brief paper grew out of discussion at a small conference held at the FitzWilliam Museum at Cambridge University in 1981.[2] The participants, apart from myself, were all museum people having a special interest in ancient Egyptian ceramics, but with no experience as excavators. They complained that excavators too often did not share their appreciation for pottery for its own sake, and I took this opportunity to explain the difference in outlook between the digger and the student.

Because we are brought together by a common interest in ancient Egyptian pottery, we may at times lose sight of the fact that we are not all interested in it for the same reasons. Some of us are primarily archaeologists (i.e. excavators), and others primarily ceramologists. As such we are looking at the same basic corpus of material (primarily potsherds), but we are asking fundamentally different questions of it. The archaeologist is interested primarily in what the pottery can tell him about his site, while the ceramologist wants to know what the site can tell him about the pottery. Pottery is for the archaeologist a means, while for the ceramologist it is an end.

It is my contention that the distinction between archaeologist and ceramologist is a legitimate and necessary one, and that it is widely misunderstood. The two tasks may, and perhaps even should, be performed by the same individual, but he needs to keep the conceptual difference between them clear in his or her own mind, and to employ analytical procedures and conceptual tools appropriate to the questions he is asking at any given time.

In this paper I have identified and discussed five points of conceptual distinction between the archaeologist and the ceramologist:

1. The ceramologist is interested in pottery as a key to the behavior of people, while the archaeologist is only interested in the behavior of potsherds themselves. Thus, the ceramologist has a better claim to the title of Social Scientist or Historian than has the archaeologist.

2. The type concept is central to the analytical methodology of both ceramologists and archaeologists, but they use it in fundamentally different ways. To the ceramologist a type is ideally a type of whole vessel, while to the archaeologist it is merely a type of potsherd — a discrete data unit in its own right. The fact that a sherd was once part of a pot can be disregarded as irrelevant for the archaeologist's purpose.

3. The typologies of ceramologists are almost necessarily non-comprehensive, partly because they are usually given incomplete data to work with. A great many sherds and even some whole vessels are treated as having no type identity, because they do not conform to any established norms. On the other hand the quantitative methodology employed by the archaeologist requires that typologies be comprehensive; every sherd must be assignable to one and to only one type.

[1] Originally published in *Bulletin de Liaison du Groupe International d'Étude de la Céramique Égyptienne* 6 (3), 44-45. See further: https://www.ifao.egnet.net/publications/catalogue/?coll=BCE&page=3&total=29&nb=10&nv=0 [accessed 20 01 2021].

[2] For context, this was a two-day colloquium held 15-17 October, 1981 which coincided with a temporary exhibition in the Fitzwilliam Museum entitled *Umm el-Qa'ab: Pottery from the Nile Valley before the Arab Conquest* curated by J. Bourriau, and the fifth Glanville Memorial Lecture given by D. Arnold entitled *Ancient Egyptian Pottery: Seven Phases of Evolution*. The colloquium theme was Typology and Seriation. Short abstracts of the other papers presented at this conference may be found in 'Typology and Seriation: a discussion of recent work (Colloque tenu à Cambridge, 15-17 octobre 1981)', *Bulletin de Liaison du Groupe International d'Étude de la Céramique Égyptienne* 6 (3), 33-49 [ed.].

4. While the ceramologist measures change by the evolutionary development of particular types, and by their ultimate appearance and disappearance, the archaeologist can measure change more accurately by noting the fluctuating percentage relationships among types which individually may not be changing.

5. The ceramologist is ultimately concerned with explanation, while the archaeologist is ultimately concerned with prediction. Both are legitimate and necessary scientific ends.

On the Argument from Ceramics to History: A Challenge Based on Evidence from Medieval Nubia[1]

(1979)

This article was published in the highly regarded journal Current Anthropology, *in an issue devoted to the topic Material Culture. The journal employs a distinctive 'challenge-and-response' format, in which initial papers are submitted, before publication, to a series of qualified critics, whose comments are published following the article itself. Finally, the original author is allowed to reply to the critics.*

Here, the published comments following the article are omitted, for two reasons. First, the general tenor of the comments is well summarized in my Reply, which is included here. Second, many of the comments simply present or discuss details of data which do not bear on the validity of my argument.

My thesis, that pottery 'marches to its own drummer', has been presented a couple of times in popular articles, but not in any other learned journal.

Archaeological inference which transcends the narrow limits of the material domain must depend on the establishment of reliable linkages between the material and the non-material. Few archaeologists will dispute that proposition. While there has been debate as to whether we ought to discover the necessary linkages through ethnographic analogy or through the application of principles of abstract logic (cf. Klejn 1977, 8-9), the reconstruction of culture history or process is recognized to depend on inferences that are partly extrinsic to the material evidence.

Probably no aspect of material culture has been more subject to inferential interpretation than pottery. For example, Grieder (1975, 850, emphasis added) asserts:

> Social and environmental changes and external contacts . . . are evident in the archaeological records of ancient societies, especially in the form of changes in pottery, which *immediately reflects changes that affect the members of a society* . . . it is hard to find a material product in any period that provides more *immediate and exact* information about the state of a society than does pottery.

As it happens, this is not the author's main argument, but it nicely states a proposition that has wide acceptance among prehistorians. It is one of a number of assumed linkages between the material and the nonmaterial which are habitually made for purposes of cultural reconstruction but which, so far as I know, have never been independently tested against historical evidence or ethnographic observation. There have been a number of intriguing recent studies of the dynamics of ceramic change (e.g., Deetz 1965, Drost 1968, and especially Nicklin 1971), but their intent is to correlate changes in pottery with ongoing economic and social dynamics rather than with abrupt and major changes in the nonmaterial areas of culture. It is this latter correlation, frequently assumed but seldom empirically demonstrated, that I wish here to challenge. I will suggest, on the basis of dated pottery types from Nubia and independently dated historical developments, that a close connection between the two cannot safely be assumed and that reconstructions based on such an assumption may stand in need of reconsideration. In ancient and medieval Nubia I have found that some of the most significant political and ideological changes were not reflected in contemporary pottery, while at other times there were radical stylistic changes in the pottery for which no very immediate external cause can be identified.

[1] Originally published in *Current Anthropology* 20 (4), 727-744.

By 'Nubia' I mean the Nile Valley between the First and Third Cataracts, an area about 650km (400 miles) long in the far south of Egypt and the far north of the Republic of the Sudan. This region is, and for millennia has been, occupied by a culturally and ethnically homogeneous population of largely African ancestry (cf. Adams 1977, 5-6), and therefore large-scale migration or population change can usually be ruled out as an explanation for abrupt cultural change. The period I shall be considering extends from about AD 200 to 1550, those being the limits within which we have both a continuous and thoroughly investigated archaeological record (cf. Adams 1977, 71-88) and an externally dated historical record. The latter derives primarily from the evidence of a number of medieval Arab chroniclers, plus a few indigenous documents in the Old Nubian and Greek languages; the dating of pottery wares depends upon their finding in archaeological contexts in association with coins, inscriptions, and other datable materials. Such contextual dates are not, of course, absolutely precise, but their reliability within a generation or so has been independently confirmed by textual finds on a number of occasions. It should, however, be remarked that both historical events and pottery wares are much less accurately datable before AD 600 than afterward, since we have almost no external record for the earlier centuries.

To simplify discussion I have arranged the relevant historical and ceramic data in four chronological charts (Figures 13.1-13.4).[2] Figure 13.1 diagrams the succession of political and ideological changes in ancient and medieval Nubia and the principal pottery groups that were in use at the same time. More will be said about the ceramic evidence later; first it seems desirable to recapitulate briefly the political and ideological developments. (For more detailed treatment of this phase of Nubian history, see Adams 1977, 333-546).

Until the early 4th century AD, Nubia was a province of the far-flung Empire of Kush, whose later phase is sometimes called Meroitic. This was a Sudanese successor-state to the immemorial empire of the Pharaohs, and until its final breakup it remained faithful to the ideology and the iconography of ancient Egypt. Following the empire's collapse in the 4th century, however, Nubia was plunged into a dark age in which monumental architecture, glyptic art, established religion, writing, and other aspects of complex civilization were temporarily lost, while a number of local warlords struggled for control in different parts of the Nile Valley. In the north, between the First and Third Cataracts, the chiefdom of Nobadia gradually subdued its rivals and emerged paramount. After the conversion of both rulers and subjects to the Christian faith in the 6th century, the institutions of civilization and of statecraft shortly reappeared.

Early in the 8th century, Nobadia was absorbed by the larger neighboring state of Makouria. This extensive Sudanese kingdom remained stable and prosperous for more than five centuries and during the earlier caliphates maintained peaceful relations with the Islamic world. In the late Middle Ages, however, it was rocked by a combination of dynastic feuds, Mamluk incursions from Egypt, and massive immigration of Arab nomads from Egypt and from the Hejaz.

In 1323 Arab and Mamluk pressure succeeded in establishing a Moslem ruler on the throne of Makouria, and not long afterward the kingdom broke up into warring principalities. In the north, between the First and Second Cataracts, the splinter kingdom of Dotawo remained under the control of erstwhile Christian rulers until at least 1484, but this kingdom too had disappeared by the beginning of the 16th century. 'Not a trace of kingly authority remained in the country, and the people are now become bedouins', reported the great historian Ibn Khaldun (Crowfoot 1927, 148). Finally, around the middle of the 16th century, Nubia was annexed to the Ottoman Empire, though this hardly signaled a return of political stability.

As can be seen in Figure 13.1, ideological developments in ancient and medieval Nubia generally mirrored the changes in the political sphere. At the beginning, the Empire of Kush adhered closely to

[2] Figure numbers used here have been slightly modified from those used in the original publication through the addition of the chapter number from this volume however, the figures themselves remain the same as in the original article [ed.].

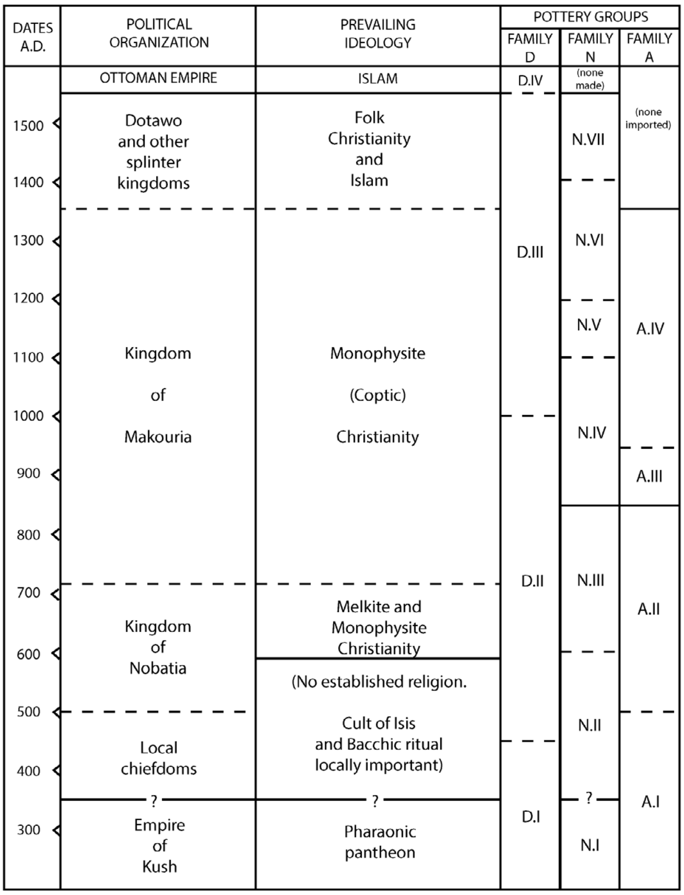

Figure 13.1. Political and ideological changes in ancient and medieval Nubia and the principal pottery groups in use. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.

the ideological traditions of Pharaonic Egypt, a fact which is attested by innumerable temples in Egyptian style decorated with the familiar reliefs of Amon, Isis, and other Egyptian deities. After the collapse of Kushite power, there is no archaeological evidence suggestive of established religion, but classical writers tell us that the cult of Isis was locally important in Nubia (see Adams 1977, 417), as it was also in contemporary Egypt.[3] Thousands of broken wine amphorae suggest also the importance of the Bacchic cult in post-Kushite Nubia.[4] Meanwhile, the rulers of Nobadia revived some very ancient funerary practices, including the large-scale practice of human sacrifice.[5]

Both the rulers and the subjects of Nobadia were rapidly converted to Christianity in the 6th century, and the visible symbols of paganism disappeared almost overnight. For a century or more there was active and sometimes hostile competition between the Melkite (Byzantine) and the Monophysite (Coptic) sects of Christianity, but the latter became wholly predominant after the subjugation of Nobadia by Makouria. The Nubian church then became an integral part of the Coptic Church of Egypt and so remained until the late Middle Ages. In the chaotic conditions of the 14th and 15th centuries, however, contact with the Egyptian patriarchy became difficult and finally impossible; the organizational structure of the Nubian church gradually disintegrated until Christianity survived only as a folk religion. Meanwhile individuals and families converted in increasing numbers to Islam, which became the principal and in time the sole faith of the Nubians under Ottoman rule.

The three right-hand columns in Figure 13.1 show the succession of ware groups (i.e., stylistic norms) in the three principal industries which supplied the Nubian ceramic market: the Nubian hand-made wares (Family D), the Nubian wheel-made wares (Family N), and imported wares made at Aswan (Family A). From time to time other wares were imported, but these three industries provided over 95% of the pottery used by the Nubians in the ancient and medieval periods. Before turning to a detailed consideration of the wares and their characteristics, a word must be said about the system of classification which I have employed. (For more detailed explanation, see Adams 1970; 1973b, 1973c; 1975).

In the analysis of Nubian pottery I have considered seven independent fields of variability: method of

[3] See further Ashby 2016; Dijkstra 2008; Yellin 2012 [ed.].

[4] Noted in Lower Nubia in particular at Qasr Ibrim (Plumley and Adams 1974; Edwards 1994) and possibly by the presence of features identified as wine presses (Adams 1966; Bishop-Wright 2019) [ed.].

[5] See further Emery 1938, for example Tomb B.95 at Ballana (Emery 1938, fig. 68) and Williams 1991, 27 and 33 passim. *Contra* Lenoble has suggested these practices were not newly introduced, but rather a development of extant late Meroitic traditions (1994; 1996) [ed.].

manufacture (hand-made or wheel-made), fabric (paste, temper, and other internal properties), surface treatment, vessel forms, decorative color(s), painted decoration, and relief decoration. The first two of these have tended to be relatively unvarying through time and serve to define what I call families: groups of pottery made by the same people in the same way, and using pretty much the same materials, over long periods. Within each family there have been periodic stylistic changes in the preferred vessel forms and in the canons of painted and relief decoration; these three clusters of variables therefore serve to define successive evolutionary stages within each family, which I call ware groups. Finally, within each ware group there is concurrent variability in regard to colors and surface treatment; that is, within the same group one may find red-slipped wares and white-slipped wares, or polished wares and matte wares, all in the same forms and with the same decoration. Colors and surface treatment therefore serve to define what I call individual wares within each ware group. It is, then, the ware groups, differentiated chiefly though not exclusively on the basis of forms and decoration, that are the chronologically significant units in the scheme. The succession of ware groups in each of the three main Nubian families is plotted in Figure 13.1, while the empirical changes which distinguished one group from another are schematized in Figures 13.2 and 13.4 and illustrated (in the case of the Nubian wheel-made wares) in Figure 13.3.

I will here be concerned primarily with the Nubian wheel-made wares (Family N), for these were the most abundant, the most varied, and the most stylistically sensitive of the ancient and medieval wares. At most times they comprised at least 60% of the total Nubian ceramic complex. They were, of course, commercially mass-produced at a few major factories and from them widely traded throughout the region and beyond it. Between AD 200 and 1550 at least half a dozen factories were involved in the production of the Nubian wheel-made wares; several of these were located and excavated in the course of the recent Nubian salvage campaign (cf. Adams 1961b, 1962c). The evidence suggests, however, that no more than two or, at most, three factories were in production at any one time, while the extreme uniformity of the wares after 1100 indicates fairly conclusively that they were all made at one place. Even when two or more factories were operating at once, however, their products varied only in minor details of fabric, form, or design. (Omitted from consideration here are unpainted utility wares, which always made up a considerable part of the Nubian ceramic inventory but which underwent little change from beginning to end of the historic period. They were made to some extent at the same factories which produced the decorated wares but also at a large number of local workshops which produced nothing else).

Reference to Figure 13.2 will show that between 200 and 1550 the Nubian wheel-made wares underwent some degree of change in six of the seven major fields of variability—that is, in all but method of manufacture (which is omitted from the figure). Change in fabric was, however, of a relatively minor nature. Before 850 most of the pottery was made basically from red-brown Nile mud, but until about 350 there were also some vessels (otherwise indistinguishable from their fellows) made from fine desert clays. After 850 a small proportion of fine clay was regularly added to the basic mud paste, and coarser and more abundant sand temper was also introduced.

Surface finish shows a gradual and more or less cyclic pattern of evolution between 200 and 1550. In the beginning there were some matte wares and some that were burnished, that is, rubbed with a pebble in such a way as to leave visible striations on the surface. The latter technique was abandoned sometime around 350, and for a time there were matte wares only. Polished wares (that is, with a uniform gloss) began appearing in small numbers in the late 6th century and gradually increased until they became predominant between about 1100 and 1400, after which they abruptly became much rarer.

Each of the seven successive ware groups in Family N exhibits a partly distinctive clustering of vessel forms; that is, there were always some forms unique to a particular time period, while other forms were shared with earlier or later periods or both. A truly radical change in forms, however, occurred at only

WARE GROUPS	FABRIC	SURFACE FINISH	PRINCIAL FORMS	SLIP COLOURS	PAINTED DECOR. % DEC.	PAINTED DECOR. PRINCIPAL DESIGNS	RELIEF DECORATION	DATES A.D.
N.VII	Nile mud with fine clay admix-ture	Mostly matte	Vases and footed bowls	50% orange 50% white & yellow	75%	Bold rectilinear geometric	None	1500
N.VI		Mostly polished		25% red 45% orange 30% white & yellow	95%	Frilly rectilinear geometric		1400 1300
N.V						Elaborate curvilinear geometric		1200
N.IV		Polished and matte		Nearly all white and yellow	85%	Elaborate curvilinear representational and geometric	Very rare individual center stamps	1100 1000 900
N.III			Plain and footed bowls	60% red 40% white	40%	Simple, formal geometric	Common collar grooves individual side stamps and mltiple side stamps	800 700
N.II		Matte only	Cups, plain bowls, bottles and jars	Nearly all red	15%	Very simple, informal geometric	Common waist grooves; Fine ribbing of lower bodies	600 500 400
? N.I	Nile mud, some fine clay wares	Burnished and matte	Cups, bottles and jars	40% red 60% white	70%	Elaborate curvilinear representational	Occasional multiple side stamps	300

Figure 13.2. Empirical evidence of change in Nubian wheel-made wares (Family N) between AD 200 and 1550. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.

one point in the evolutionary sequence, around the beginning of the 7th century. Before that time the great majority of Nubian vessels were liquid containers or servers of one sort or another (cups, bottles, jars, amphorae, etc.); afterward they were mostly more open forms. Plain and footed bowls predominated until about 850, when taller and more cylindrical vessels which I call vases abruptly made their appearance and remained thereafter the most characteristic Nubian vessel form (cf. Figure 13.3, left). The functional basis for these changing preferences is very poorly understood.

The Nubian potters had the same palette of decorative colors available to them throughout the historic period, but their color preferences fluctuated markedly and cyclically. In the beginning there was a fairly even mix of red-slipped and white-slipped wares. The latter might shade to cream, tan, or even light orange-brown in individual cases, but these were not developed as separate color norms. After 350 the light-slipped wares abruptly and almost completely disappeared, and for two centuries or more nearly all vessels were red. White-slipped wares began a modest comeback around 600 and gradually increased in number until by 850 they comprised perhaps 40% of the total complex of decorated pottery. At that point it was the turn of the red wares to disappear, and for the next two centuries virtually all pottery had either a white or a yellow slip. Orange and yellow emerged as separate color norms in the late Middle Ages, and there was also a reappearance of red wares, though they largely disappeared once more after 1400.

Painted decoration was usual on the white, yellow, and (except in the last period) orange wares and was nearly always in black or brown, sometimes with red as a secondary color. Decoration was much less common on the red wares; when present it might be in black or white or both. To a very considerable extent the drastic fluctuations in the frequency of painted decoration between 200 and 1550 reflect the shifting preference for red or for white-slipped pottery. There was, however, dramatic fluctuation in the form as well as in the frequency of painted decoration (cf. Figure 13.3). Of all the changes in Nubian pottery in the later historic period, these are the most conspicuous and also the most difficult to account for. The pottery of Group N.I, familiarly known as Meroitic, is justly famed for its elaborate representational decoration, which is frequently in two colors (black designs with red filling on a white background or black with white filling on red). The designs combine a variety of Hellenistic and ancient Egyptian motifs such as the ankh, the lotus flower, and various zoomorphs, and individual vessels may exhibit as many as five different decorative bands. Sometime in the 4th century this whole decorative tradition disappeared, along with the use of a white slip. For more than 200 years thereafter the great majority

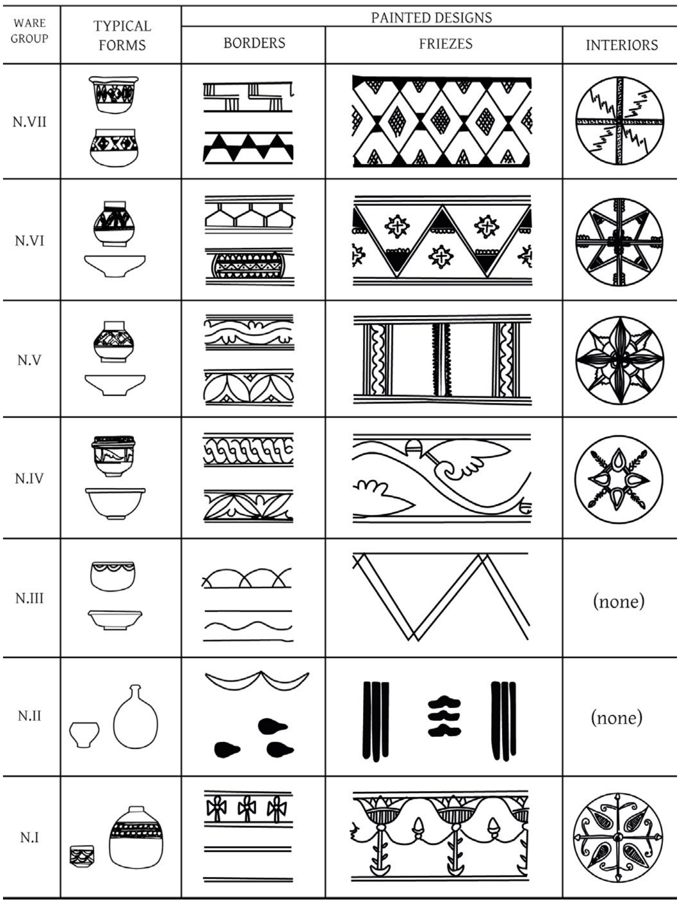

Figure 13.3. Typical vessel forms and painted designs at each developmental stage in the Nubian wheel-made wares.

of vessels were red and undecorated, though a few forms exhibited very simple and informal geometric designs bearing no resemblance to those of the preceding period. In the next stage (Group N.III), after the introduction of Christianity, geometric designs became slightly more formal and considerably more frequent but still consisted mostly of diagonal, cross-hatched, or wavy lines. Decoration was very rarely in more than one color, and there was only one design band per vessel.

In the middle of the 9th century occurred the second stylistic revolution in Nubian pottery. Along with the appearance of the vase as the principal vessel form (and with a minor change in fabric), geometric designs abruptly became much more ornate and frilly, with a marked preponderance of curvilinear over rectilinear motifs. Simultaneously there appeared a whole new range of representational designs, including animals, birds, fish, and numerous stylized floral motifs. Decoration was again often in two colors and involved more than one design band per vessel. Superficially the painted decoration of Group N.IV bears a much closer resemblance to that of Group N.I than to anything in the intervening periods, but its immediate inspiration came from Coptic manuscript illumination, in which the prototypes of most of the Classic Christian pottery designs can be found.

Decorative changes after the 9th century were mostly of a gradual and evolutionary sort. Zoomorphic forms and other representational designs in time disappeared, while geometric decoration became increasingly busy and frilly, and after 1200 rectilinear motifs once again became predominant over curvilinear. Finally, after 1400 there was a considerable simplification and the emergence of bolder and less embellished geometric figures.

Relief decoration was never a very significant feature of the Nubian wheel-made wares, but it was employed occasionally (usually as an alternative to, rather than in conjunction with, painted decoration) until the 10th century. Meroitic vessels (Group NJ) were sometimes encircled by rows of small, repeating stamp impressions. In the following period these had disappeared, but the undersides of goblets were often covered with fine ribbing. Later this was concentrated in a series of deep grooves encircling the widest part of the vessel only, while in Group N.III the grooves are found in pairs just below the rim rather than at the waist. In the same group, from about 600 to 850, there was also a significant revival of stamped decoration, sometimes involving elaborate combinations. The occasional use of individual center stamps continued for a time in Group N.IV, but after the 10th century all forms of relief decoration disappeared.

Although evolutionary change was gradual and more or less continuous in the Nubian wheel-made wares, a glance at Figure 13.2 is sufficient to indicate that revolutionary change occurred at only two points: between Groups N.I and N.II and between Groups N.III and N.IV. By any measure one wishes to apply, these were major turning points in Nubian ceramic history, for they involved abrupt and simultaneous change in several different fields of variability: in fabric, in vessel forms, in prevailing slip color, in relief decoration, in the frequency of painted decoration, and above all and most dramatically in the artistic canons of painted decoration. These, then, should correspond to the major transitions in Nubian political and ideological history.

Figure 13.1 seems to provide confirmation in regard to the first major ceramic change. Conventional wisdom has long associated the disappearance of Meroitic decorated pottery (Group N.I) with the collapse of the Kushite Empire. This is, however, an example of precisely the kind of a priori reasoning that I wish here to challenge. As it happens, neither the demise of Kushite authority nor the disappearance of the Meroitic pottery tradition is at all accurately dated in Nubia, and they could have occurred as much as 100 years apart. There is a growing body of contextual evidence from the excavation at Qasr Ibrim (cf. Plumley and Adams 1974; Plumley 1975; Plumley, Adams, and Crowfoot 1977) to suggest that the change from decorated white to undecorated red pottery took place well before the collapse of Kushite authority, but reliable dating is still lacking. In this instance, then, the contemporaneity of political/ideological developments and ceramic change must remain moot.

For the second major ceramic revolution, in the 9th century, the chronological evidence is clear and unambiguous. The pottery can be fairly closely dated from associated finds, including inscriptions in the factory where the wares of both Group N.III and Group N.IV were chiefly made (Adams 1961b, 42; 1962b, 282-84). Concurrent political and ideological developments are known from a number of medieval Arab sources. From them we learn that the 9th century, far from being a time of stress and change, represents very nearly the climax of stability and prosperity in the Kingdom of Makouria. It was, in fact, at just about this time (in 836) that the Nubian Crown Prince George paid a state visit to the Abbasid court in Baghdad, where he was received with honor and loaded with gifts (Vantini 1970, 47).[6] He was, so far as we know, the only Christian potentate ever so honored by the caliphs. Some art historians might be tempted to see the concurrent change in medieval Nubian pottery as a consequence of this visit, but the new motifs that were introduced in the 9th century were specifically Christian, not Moslem.

It seems, then, that of the two most abrupt changes in medieval Nubian pottery, at least one cannot be linked to immediate external causes. Turning back now to Figure 13.1, it is apparent that the reverse is also true. The introduction of Christianity in the 6th century, although it was immediately reflected in architectural, artistic, and literary canons, had no measurable impact on Nubian pottery until 250 years later (in Group N.IV). It is true that the Christianization of Nubia coincides fairly closely with the transition from Group N.II to Group N.III, but this was a gradual and orderly change without sharp dividing lines except in vessel form. Changes of this magnitude took place every two or three centuries throughout the historic period, with or without accompanying political developments. Similarly, the disintegration of Christianity and the gradual interpenetration of Islam, to say nothing of the cataclysmic political shocks of the late Middle Ages, are not at all reflected in the pottery of Groups N.VI and N.VII. It was only the Ottoman annexation of the 16th century that had an immediate and decisive impact in the ceramic realm, for at this time the wheel-made pottery industry came entirely to an end. Although the reason for this is far from clear, it seems probable (and wholly consistent with the general tenor of Ottoman policy) that a ruinous tax on potters' wheels or on the pottery trade was to blame. Thenceforth until modern times the Nubians made do entirely with the simplest of hand-made vessels, as in the prehistoric period millennia earlier.

[6] See further the records of Michael the Syrian, Severus, Abu Makarium (Abu Salih the Armenian), Barhebraeus and el-Maqrizi (Vantini 1975, 193, 330-331, 420, 644) [ed.].

It would be absurd to suggest that the major symbolic transformations in Nubian pottery were wholly unconnected with external developments. The first major change, in the 4th century, represented quite simply a total Romanization of the Nubian wares, that is, the abandonment of older traditions in favor of canons of form, color, and decoration (or lack of it) that had long been current in Roman Egypt and elsewhere throughout the Empire. What is striking, however, is that this took place almost 400 years after the Roman conquest of Egypt and at a time when Roman strength and cultural influence were declining. Nubia itself was, of course, never a Roman province.

In a sense, the major ceramic change of the 9th century can be seen as a reversal of the preceding or, in other words, as a 'de-Romanization' (though in the interval the Nubian wares had drifted rather far from the original Roman Egyptian norms). The remaining Roman influences were now consciously discarded, and specifically Christian motifs appeared for the first time. Again, it is the timing which confounds us. Egypt had ceased to be a Roman province for more than 200 years (since 642), and Nubia had been Christianized for an even longer period, when this second major change took place.

How, finally, shall we explain the stylistic revolution of the 9th century? At the very least we might suggest that a new factory came into production at this time, but in fact there is incontrovertible evidence that the wares both of Group N.III and of Group N.IV were made at a single factory near the important medieval community of Faras (Adams 1961b, 40-41). It seems inferable, however, that before 850, when decoration was relatively simple and unsophisticated, the potters (whose primary expertise was presumably in vessel throwing) did their own decorating. After that time the presence of a group of decorative specialists seems clearly indicated by the intricacy and refinement of the designs in Group N.IV. The newcomers, or at least some of them, were superbly skilled in the medieval genre of Coptic manuscript illumination and were therefore most probably Egyptian Copts.

How and why did they come to the Nubian factories? We can only note that there is evidence of some migration of Egyptian monks and other Christians to the safer confines of Nubia as a result of persecutions and political disturbances in the 8th and 9th centuries (Adams 1977, 446-447). There is also a good deal to suggest that the Faras pottery factory was part of a monastic establishment at this time (Adams n.d. a: chap. 4). Since little or no manuscript decoration was done in the Nubian monasteries, we may surmise that immigrant monks with artistic talents were perhaps put to work in the pottery factory instead. On such relatively inconsequential events, possibly involving no more than half a dozen individuals, the entire stylistic revolution in the Christian Nubian pottery industry may have depended.

The two other principal industries of ancient and medieval Nubia—the hand-made wares and the Aswan wares—can be more summarily dealt with. Although present during most if not all of the period between 200 and 1550, they were subject to less frequent and less dramatic change than were the Nubian wheel-made wares. Changes in both families are tabulated in Figure 13.4.

The Nubian hand-made wares (Family D) represent the survival of a very old tradition, tracing back to prehistoric times. So far as we know their manufacture was never industrialized; they were made locally, and chiefly for home consumption, by Nubian women in many different parts of the country, as to some extent they still are. Despite their widespread manufacture, however, the hand-made wares at any given time exhibited at least as much uniformity as the wheel-made wares.

Within the hand-made family the pace of stylistic change was glacially slow (at least by comparison with that for the wheel-made wares) and occurred chiefly in the domains of slip color, painted decoration, and relief decoration. In all these characteristics we can observe an almost perfect cycle of recurrence between 200 and 1550. Before about 450 there were red, white, and black-slipped wares, some with painted and some with incised decoration in simple geometric patterns. In the next period (Group D.II) virtually all decoration as well as white- and black-slipped wares disappeared, only to reappear again

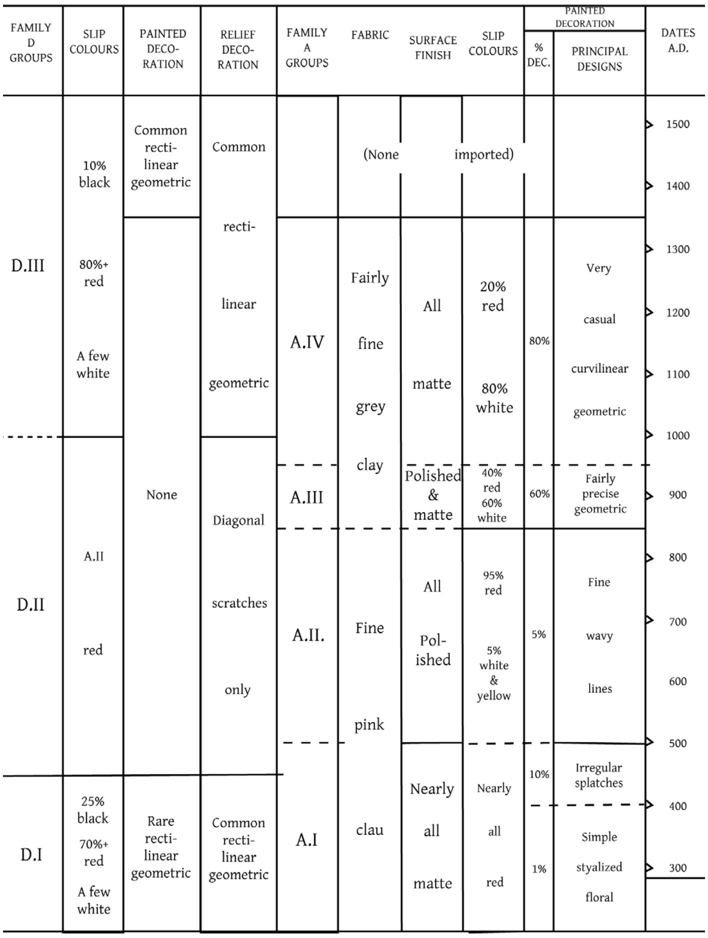

Figure 13.4. Empirical evidence of change in Nubian hand-made wares (Family D) and imported pottery made at Aswan (Family A) between AD 200 and 1550. Broken lines indicate gradual or evolutionary change, solid lines abrupt or revolutionary change.

after 1000 in Group D.III, many of whose designs are strikingly similar to those in Group D.I. (After 1650, in the period not covered by Figure 13.4, these decorative features disappeared once again.) It may be noted incidentally that the dates of stylistic change in the hand-made wares do not correspond in any meaningful way to those in the wheel-made wares (Figure 13.1), nor is there any comparability between individual designs and forms in the two industries. This same lack of correspondence between contemporary hand-made and wheel-made pottery has been noted by Balfet (1965, 161-169) in modern Tunisia, Algeria, and Morocco.

Finally, a word must be said about the Aswan wares (Family A). Technically they are not Nubian products, for they were made at a factory at or near Aswan, just to the north of Nubia. They seem always to have been destined in considerable part for the Nubian market, however, and between 200 and 1350 they comprised seldom less than 5% and occasionally up to 50% of the total Nubian ceramic complex. Basically they circulated in the same commercial market as did the native wheel-made wares, and they were evidently not always at a competitive disadvantage by virtue of their 'foreign' manufacture.

The Aswan wares exhibit the most nearly linear pattern of stylistic development among the three major industries considered here. From beginning to end there was a continual increase in the frequency and complexity of painted decoration, with a quantum increase between Groups A.II and A.III, around 850. Although this date happens to coincide with one of the most revolutionary changes in the Nubian wares, there is actually very little correspondence of individual designs in the two industries at this time or any other. As can be seen, other changes in the Aswan wares are not closely coeval with changes in the Nubian industry. After 1350 the importation of Aswan decorated pottery ceased altogether, and there is no information about the subsequent history of the factory except that it had disappeared before modern times.

From the foregoing data one conclusion is inescapable. Not only were the ancient and medieval Nubian pottery wares only very belatedly and indirectly influenced by external events; they also evolved with very little influence from one another. Each exhibits its own distinctive growth dynamic. If we were to allow pottery to define the major turning points in Nubian cultural history, as the prehistorian commonly does, each of the major families would tell us a different story, and none of these would be historically accurate.

At the very least, the Nubian material suggests that the argument from pottery to history must be treated with reserve when we are dealing with complex, partly industrialized economies (cf. Adams 1968a, 197-202 and 1973a, 24). In cases where pottery was made commercially for sale in a competitive market, stylistic changes may have been dictated either by the producers or by the consumers—who may or may not have belonged to the same ethnic group, culture, or social stratum as the producers. Markets are, moreover, subject to temporary vicissitudes which may be economically drastic but culturally inconsequential, a point which is too clearly demonstrated by events in our own recent past to need further elaboration here. Finally, under industrial conditions so large a part of the pottery used in a region may have been produced by so small a number of makers (sometimes working at a single factory) that purely local and even accidental events could have an immediate and significant effect on region-wide ceramic distributions.

In dealing with the non-industrialized ceramics of prehistoric peoples we may be safer in arguing from pottery to history, as we almost instinctively do. Even here, however, I wonder if we are not sometimes guilty of circular reasoning. In sorting out the cultural dynamics of any prehistoric people, we begin with an effort to periodize the pottery because we know from experience that it is, nine times out of ten, the most chronologically sensitive of surviving cultural remains. Too often, however, the ceramic stages which began as a heuristic convenience become reified as significant cultural stages (or as evidence of migration or invasion, if we are given to cataclysmic theories of history), and we proceed to invest them with nonceramic defining parameters which may exist primarily in our imaginations. To cite one example, I feel fairly certain that in the original 'Pecos Classification' of Anasazi cultural history (Kidder 1927) no distinction would have been made between the Pueblo I and Pueblo II stages had it not been for visible differences in the pottery. It may in fact be instructive to quote Kidder's complete statement on the subject of Pueblo II (Kidder 1927, 558): 'The stage marked by widespread geographical extension of life in small villages; corrugation, often of elaborate technique, extended over the whole surface of cooking vessels'. I am similarly dubious about some of the stages in the pre-Sumerian sequence in Mesopotamia, whose nonceramic diagnostics are not easy to recognize.[7] To avoid the pitfall of overreliance on pottery in the reconstruction of prehistory, I think we should be ready in all cases to answer a simple question: how would we interpret the same nonceramic evidence if the pottery were not present, or if there were no significant change in the pottery?

Reply

The various responses to my article[8] seem to call more for amplification than for argument. Nearly everyone agrees with my major thesis, and several respondents have furnished additional case examples in which ceramic change does not correlate with change in other areas of culture. Some have also cited instances in which ceramic change does correlate with other change, as indeed I myself did in the original article. I have to stress that these counter-examples do not refute my position, for of course I did not argue for a complete lack of congruence between ceramics and the rest of culture. My point was and is simply that such a congruence should not be assumed *a priori*.

If there is a common thread of criticism running through the responses, it is perhaps the suggestion that my warning is unneeded, because prehistorians are already well aware of the pitfalls of ceramic inference. I find this idea expressed or implied primarily by the Americanists (Abel, Arnold, Euler, Kolb, Myers, Simmons, Syms), while those who work with Old World materials (Chittick, Davis, de Maret, Fattovich, Franken) are more inclined to view my message as timely and appropriate. This suggests

[7] See further Cooper 2012, 295-297; Reade 1991, 15-16 [ed.].
[8] See responses from L. J. Abel, D. E. Arnold, N. Chittick, W. M. Davis, P. de Maret, R. Fattovich, H. J. Franken, C. C. Kolb, T. P. Myers, M. P. Simmons and E. L. Syms in *Current Anthropology* 20 (4), 735-740.

that the tendency to argue from pottery to history may be more prevalent in the Old World (where old-fashioned and nonanthropological paradigms of explanation still dominate in many areas and where art historians are influential) than in the New. In my particular areas of expertise (Nubia and Egypt) I can assure my Americanist colleagues that overgeneralizations based on pottery remain commonplace; some additional examples are furnished in the responses of Davis, Fattovich, and Franken.

New World archaeologists nevertheless cannot hold themselves entirely above reproach. It has not been so very long since Reed (1958, 7) cautioned that 'many proposed hypotheses of migration . . . have been based on pottery types alone'. My respondents show also that Americanists continue to disagree among themselves as to the reliability of cultural taxonomies that are keyed to pottery. Syms faults me for citing the half-century-old 'Pecos Classification' of Southwestern cultures (Kidder 1927) because it has been modified (which indeed was exactly my point), while Abel and Euler assert that the original classification continues to be validated by field research.

Several respondents have protested, not without justice, that despite its imperfections pottery is still more culturally sensitive than are most of the other material remains with which the prehistorian has to work. This is attested by a number of cited instances, including some of my own, in which major ceramic change can be correlated with change in other areas of culture. The important question then arises: what can be done to improve the reliability of cultural inferences based on pottery? In one way or another this issue is raised, and also to some extent answered, by several respondents, perhaps most cogently and explicitly by Davis, de Maret, Myers, Simmons, and Syms.

I can think of at least three ways in which we can hope to improve on the reliability of pottery as an index of cultural change. First, we need far more detailed, comprehensive, and objective studies of ceramic change, over long periods of time, than have thus far been undertaken in most parts of the world. This has been the main thrust of my own work with Nubian ceramics over the past 20 years. Rather than concentrate on a few of the fancier wares or on a few trait characteristics (form, decoration, etc.) or on change within a limited span of time, I have sought to define and measure the extent of change in every dimension of variability (method of manufacture, fabric, surface treatment, forms, colors, painted decoration, and relief decoration) in all of the 110 pottery wares found in the sites I have excavated, extending over a period of nearly 1,500 years. I gather that similarly detailed and exhaustive studies have been undertaken at least for Maya ceramics and perhaps elsewhere in Mesoamerica as well. On the other hand, I know of areas, such as the US Southwest and Egypt, in which archaeologists are still concentrating on selected aspects of variability or on selected wares while ignoring others. Wherever we approach the study of ceramic change selectively or intuitively or on the basis of haphazard samples (as is common in Egypt), I think our conclusions are bound to be unreliable.

Second, we can recognize and allow for the fact that pottery is produced and used under a wide variety of circumstances and these must be understood before we can be sure what the pottery has to tell us about the society of its makers. I have made a beginning in this direction by distinguishing between the Nubian hand-made, wheel-made, and imported wares, which I tend to interpret contextually in somewhat different ways. Syms touches on the same issue in talking about the need to develop separate models for pre-state and state societies, as does Myers with his distinction between folk and imperial wares. I think, however, that both of these dichotomies are too simple. According to Myers's scheme my Nubian Families D, N, and A are all 'folk' wares, yet I have shown fairly conclusively that a different dynamic of production, distribution, and consumption was operating in each of the three cases. Balfet (1965, 162-63) has observed the same kind of complexity ethnographically in the pottery of North Africa, where some wares are produced as a normal feminine domestic chore, some as an elementary specialization, and some as a handicraft. Yet none of the wares under discussion would, I believe, fit into Myers's 'imperial' category. In Nubia the wares which most nearly deserve the 'imperial' designation are the rare imported glazes from Lower Egypt (cf. Adams 1970, 120-21), which I deliberately omitted from my discussion.

Third, as several respondents have suggested, we must continue with ethno-archaeological studies which will clarify our understanding of the socioeconomic role of the potter, and of pottery itself, in the society of its makers and users. There has been a heartening proliferation of field studies of pottery making in the recent past, but not all of them have addressed very fully the problem of socioeconomic context. Three studies that I have found of outstanding value for my own work are those of Balfet (1965), David and Hennig (1972), and Drost (1968); several others are cited by Arnold.[9] I am happy to report that a number of particularly promising field studies are now under way in the Sudan under the aegis of the Department of Archaeology at the University of Khartoum.

I should perhaps say a word to dispel the methodological doubts raised by Syms, who questions the reliability both of my samples and of my dates. In retrospect I wish I had said more on these topics; I hoped that reference to the various earlier publications I cited would furnish a sufficient background on my work and methods. My 'sample', if that is the proper word, consists of all potsherds of all wares from the more than 20 medieval Nubian sites which I myself have excavated, plus all the pottery that was collected by half a dozen other expeditions that have employed me as a consultant to study their pottery. Total ceramic inventory has been and remains my main key to stratigraphic control, and consequently I make it a practice to examine personally and to tabulate by ware every potsherd from every excavation unit at the end of each day's dig. In the course of my current excavations at Qasr Ibrim I regularly handle between 2,000 and 5,000 sherds a day in this fashion, and the aggregate of sherd material that I have tabulated over the last 20 years runs well into the millions.

The Nubian sites which I have excavated have included villages, isolated farmhouses, refuse dumps, pottery factories, monasteries, churches, administrative centers, fortresses, and cemeteries. Two of them, Meinarti and Qasr Ibrim, were very large stratified mounds, representing in the former case over 1,000 years of continuous occupation and in the latter case over 3,000 years. Most of the smaller sites and even some large villages I have excavated in their entirety, while at the 'tell' sites of Meinarti and Qasr Ibrim I have excavated very large contiguous blocks of deposit. I should add that I excavate not by arbitrary levels but by following natural stratigraphy in all cases. This is made possible by my practice of 'stripping' large contiguous areas simultaneously, by my continual monitoring of all sherd material as it comes from the ground, and of course by the totally dry nature of Nubian midden deposits. The fact that the great majority of refuse in Nubian villages accumulates within walls and in streets between the houses, rather than in refuse dumps at the edge of town, insures that stratification is usually reasonably horizontal.

I regret that none of my Nubian excavations are as yet very fully published, though a series of memoirs will be brought out over the next decade by the University Press of Kentucky. Meanwhile, preliminary reports are available in vols. 9-15 of *Kush*, the journal of the Sudan Antiquities Service (Adams 1961a, 1961b, 1962a, 1962c, 1964, 1965, 1966, 1973c; Adams and Nordström 1963), and in vols. 60-65 of the *Journal of Egyptian Archaeology* (Plumley 1975; Plumley and Adams 1974; Plumley, Adams, and Crowfoot 1977; Adams *et al.* 1979). Perhaps the fullest account which is readily available to American readers is my description of the excavations at Meinarti in Chang's *Settlement Archaeology* (Adams 1968b).[10]

I attempted to deal with the question of dating in the latter part of my third paragraph, but Syms is perhaps justified in finding this inadequate. The important point I should make is that medieval Nubian sites fall within a well-documented historical era, and they provide considerable quantities of dated and/

[9] Response to the article by D. E. Arnold *Current Anthropology* 20 (4), 735-740 [ed.].

[10] Several final publications of these excavations have now appeared. See for example, Adams 1994; 1996; 2000; 2001; 2002; 2003; 2009; 2013; Adams and Adams 1998; Adams *et al.* 1999; Aldsworth 2010; Alexander and Adams 2018 [ed.].

or accurately datable material such as inscriptions, coins, weights, and the like. The excavations at Qasr Ibrim are yielding several hundred fragments of written material in each season. Some of these bear actual dates, while others can be accurately placed chronologically because of their reference to specific historical events or personages. My method of excavation allows me in every case to note the contextual association between any dated materials and any pottery types which may be found in conjunction with them.

As testimony to the reliability of my pottery dating, I cannot resist relating two anecdotes here. In the course of the 1974 excavations at Qasr Ibrim, we uncovered one morning a sealed jar which had been buried under a house floor. Previous experience led us to expect that this might contain documents, and there was naturally much speculation as to what they might be prior to the opening of the vessel. On the basis of the associated potsherds in the subfloor deposit I offered to bet that, if the contents should include any dated documents, they would fall within a generation of AD 1200 (because the associated potsherds were transitional between my Groups N.V and N.VI). The jar did indeed prove to contain a large number of dated legal documents, the latest of which (furnishing of course the *terminus post quern* for the sealing and burial of the vessel) bore a date equivalent to AD 1199 (see Plumley 1975, 7; Plumley 1978, 234). A year or two later, as I was conducting an informal seminar on the Nubian pottery collections in the Royal Ontario Museum, I singled out a particular vessel as exemplifying a decorative style that had come into fashion around AD 1000 (it was one of the later stylistic variants in my Group N.IV). I was then informed by the excavator, Nicholas Millet,[11] that the jar in question had actually contained a document bearing the date 992.

The matter of dating is of course more complex than I have suggested here. In pottery factory sites (particularly Faras) I find a fairly abrupt transition from one ware group to the next, suggesting that new styles immediately supplanted older ones. The same is of course not true in habitation sites, where there may be a transition period up to 50 years in length, while older vessels were still being used even though their production had ceased. Because of this discrepancy I am attempting to calculate separate dates of manufacture and dates of use for the Nubian wares, though my data do not allow me to make the distinction in all cases. The dates given in the article are specifically (as I should perhaps have made plain) dates of manufacture, since it is these which are germane to the issue of concurrent change in other areas of culture.

Like Davis I hesitate to enter very deeply into particularistic questions which may be of interest only to a handful of readers. I will, however, attempt to answer briefly a few essentially Nubiological questions that have been raised.

Davis suggests that the distinction I myself have made between the northern and southern parts of the Kushite Empire (Adams 1976) is based in part on ceramic evidence—a suggestion which I have heard before from Hintze (1976) and from Trigger (1976). There may be some truth in the assertion, but to my mind the really conspicuous and important differences between the Meroitic north and south are to be found in the political and ideological spheres. I have developed this theme previously in *World Archaeology* (Adams 1974; see also Adams 1976, 128).

Kolb observes that ‘it is unclear whether the painted decoration is applied before or after firing or whether an initial firing is followed by painting and the final firing’. The finding of large numbers of unfired but kiln-ready vessels, complete with painted decoration, shows conclusively that decoration was applied prior to any firing. Such pieces, incidentally, have been found at several different sites.

[11] Nicholas B. Millet (1934-2004), was Curator in the Egyptian Department at the Royal Ontario Museum and Director of the Gebel Adda Expedition in the 1960s during the UNESCO Campaign to Save the Monuments of Nubia. The vessel in question was excavated at Gebel Adda. See also Anderson 2004 [ed.].

Myers suggests that 'the oscillation between 5% and 50% of Aswan wares in the refuse deposits is probably a significant reflection of cultural events that were taking place in Nubia at the time'. This is true only in part. Of the earlier Aswan wares that were imported into Nubia (Group A.I and especially Group A. I) the overwhelming majority were wine amphorae. Their importation declined to some extent when the popular Bacchic cult of the 5th and 6th centuries was supplanted by Christianity and virtually ceased when the Abbasid rulers of Egypt attempted to terminate the wine trade with Nubia in the 9th century (see Forand 1971, 117). On the other hand, I can find no explanation at all for the meteoric increase in the middle of the 10th century, when the volume of Aswan-made pottery in Nubia increased from less than 5% to more than 50% as though overnight. This was in the midst of a period of general cultural and political stability both in Egypt and in Nubia. I can only suppose that the abrupt cessation of production at the Faras pottery factory (where most of the earlier wares of Group N.IV had been made) gave the potters of Aswan a competitive advantage over the surviving factories of Nubia, even though the rather haphazardly decorated product they were turning out (Group A.IV) differed markedly from the elaborate wares of Faras (Group N.IV). But it remains a complete mystery why production was halted at Faras so abruptly and so totally that scores of decorated but unfired vessels were left standing beside the kilns.

Syms asks, 'Are there similar patterns of change in elite residences, town dwellings, and peasant structures alike?' The answer for the medieval period is incontrovertibly yes. In earlier times (before about AD 200) there is some evidence that the pottery in use at the elite center of Qasr Ibrim was not the same as that in other, contemporary Nubian sites (Plumley, Adams, and Crowfoot 1977, 39-41), but the total complex of pottery exhibits surprising uniformity among all the different kinds of medieval Nubian sites that I have excavated. The one significant difference can be observed in the case of actual pottery-making sites, first because (as I have noted above) the transitions between successive ware groups tend to be more sharply differentiated than elsewhere and second because imported vessels are understandably scarce at these sites.

Syms also asks how much variability can be observed between the products of different factories which were in production simultaneously. The answer is that variability itself varies from one period to another. Groups N.II and N.III exhibit surprising uniformity despite the fact that they were demonstrably made in at least three different places. Group N.IV is a good deal more diversified. In this group I can in fact identify at least three local traditions or 'schools'. One of these is traceable specifically to the factory at Faras and another to a factory much farther south, at Ghazali, but there are also vessels with coarser fabric and less elaborate decoration which probably emanated from still a third center. A very high degree of variability is also manifest in the Nubian wares of Group N.I. Although for this period no centers of production have been specifically identified, I think that the diversity of the wares points clearly to multiple centers.[12]

At another point Syms asks about the origins of specific Nubian decorative motifs. 'Do they represent designs specific to a particular religious order in the host country, general designs found in the host country, or local variations of the host-country designs?' For another publication (Adams n.d. b) I have just completed a survey of the 11 most common medieval Nubian design motifs, the results of which may be briefly summarized here. Four motifs (the *guilloche*, interlace, *rinceau*, and bird figures) are virtually ubiquitous in medieval decorative art, both Christian and Islamic, but the Nubian potters seem to have drawn their inspiration specifically from Coptic manuscript decoration. Two other motifs (center medallions and detached, wing-like elements) are also derived from Coptic manuscript decoration; these, however, are uncommon elsewhere in medieval art. Connected-circle designs, on the other hand, seem to derive ultimately from Byzantine sources and have no analogs in Coptic art. Nubian animal figures show

[12] Other such centres have been subsequently identified see for example Pluskota 1991 [ed.].

a vague similarity to those found in Coptic and Byzantine manuscripts and on Islamic pottery, but the iconographic details are quite distinctive. Fish designs and running designs of connected leaves seem to be uniquely Nubian and cannot be traced to any external source.

Finally, elaborate center stamps in bowls (usually animal figures or crosses) represent a unique adaptation of stamped designs which elsewhere were applied only as identification marks on mud jar seals and the like. Finally, Syms suggests that I have understressed the correlation between Nubian pottery changes (notably between Groups N.II-III and Groups D.I-II) and concurrent historical events. I could perhaps dispute the evidence in each of these cases, but to do so would be beside the point. Notwithstanding any positive correlations between pottery and history which may be recognizable at other times, my main point was and is that the most dramatic ceramic transitions in Nubian history-between Groups N.III and N.IV and between Groups D.II and D.III cannot be linked to significant external events. So long as this is not refuted I think I have made my case that pottery is not always a safe guide to developments in the nonmaterial sphere.

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INTERPRETATION

On Migration and Diffusion as Rival Paradigms[1]

(1978)

In my student days at Berkeley (1946-52) I was thoroughly imbued with the diffusionist perspective of the Boasians, especially as invoked by Kroeber and Lowie.[2] It was their 'default explanation' for cultural distributions and most culture change, and for many years it was mine also. Then, when I arrived to work in Nubia (1959), I found that the field of Nubian studies was wholly dominated by a migrationist perspective. The early scholars, nearly all Egyptologists, had no anthropological background, and in particular no understanding of cultural dynamics. Their explanation for nearly all major culture changes was to suppose that the new culture traits had been introduced by the arrival of a new people. Their outlook derived support, for a time, by some wholly spurious craniological researches (cf. Adams 1977, 91-5).

My earliest excavations in remains of the post-pharaonic periods (Napatan, Meroitic, Christian, and Islamic) made it plain to me that the cultural continuities between these successive periods were much more obvious than the discontinuities, upon which my predecessors had placed so much stress. Their theories in fact were based almost wholly on drastic changes in pottery styles, and it was evident to me, from my knowledge of other areas, that these were not prima facie *evidence of population change.*

Before long I embarked on what became a series of articles, above all emphasizing the cultural continuities between the successive Nubian periods, and suggesting that a migration theory posed far more questions than it answered (Adams 1964; 1965; 1966). In time this perspective was much more fully developed in my book Nubia, Corridor to Africa *(1977). Eventually, and due in part to the abundance and variety of my own excavations, the anti-migrationist paradigm came to be generally accepted, as for the time being it is today.*

Moving abruptly from Berkeley and the American Southwest to Nubia brought home to me at once that diffusionism and migrationism are markedly distinct paradigms, involving different basic assumptions about the nature of culture. But when among prehistorians 'scientific evolutionism' became fashionable in the 1950s, all kinds of outside influences on the development of culture were dismissed as irrelevant, simply because they could not be controlled for. Every culture evolved as a logical adaptation to its environment, and outside influences were merely a distraction. And because diffusionism and migrationism were equally dismissed, there was a marked tendency to regard them as essentially the same thing. I wrote this article simply to point out the differences between the two doctrines, and in particular to illustrate their separate histories.

The readiness of materialist critics to dismiss migration and diffusion theories in the same breath obscures the fact that these are historically quite separate doctrines, with different intellectual pedigrees. As an explanation for culture change migration is a very old idea, diffusion a comparatively new one. In the explication of particular archaeological cases, moreover, the two have more often been in opposition than in alliance. Even those archaeologists who are ready to accept both principles are apt to apply them in widely different ad hoc circumstances.

Migration theory is as old and as widespread as tribal mythology; indeed, it is a rare body of mythology that does not include at least one migration episode. In this primeval aspect, migrationism is best understood as a necessary correlate of creationism. If we accept the idea, implicit in nearly all creation myths, that man and his culture went forth together from the hand of the creator, and that they are bound together by immutable bonds, then obviously we can imagine the movement of culture from one

[1] Originally published in P. G. Duke, G. Langemann and A. P. Buchner (eds), *Diffusion and Migration: their roles in cultural development.* Calgary, 1-5.

[2] Adams later wrote about Boasian anthropology discussing Boas, Kroeber, Lowrie and several other Boasians. See W. Y. Adams 2016. *The Boasians. Founding Fathers and Mothers of American Anthropology.* Lanham-London [ed.].

place to another only when its human carriers move from one place to another. This mode of thought has been remarkably enduring; it underlies not only the migration myths of antiquity but a great deal of purportedly scientific migration theory of the 19th and even of the 20th century. It was and remains true that migration is the only explanation for culture change that can be comfortably reconciled with a literal interpretation of the Old Testament. This alone was enough to insure its predominance down to the middle of the 19th century, and in some quarters down to the present day.

Throughout most of history, then, there was no concept of diffusion independent of migration; on the contrary diffusion was seen as *prima facie* evidence of migration. In order to have a concept of diffusion independent of migration it was first necessary to have a concept of culture independent of society, and that concept did not clearly emerge until the last quarter of the 19th century. In the English-speaking world its formulation is usually, and I think correctly, attributed to Tylor (cf. Kroeber and Kluckhohn 1952, 11). Although he was avowedly an evolutionist, Tylor deserves recognition at least as the godfather of diffusionism because he gave us the liberating concept of culture which made diffusion imaginable, and because he helped to turn anthropology from the study of kinship, toward the more expressive and stylistic areas of culture in which diffusion theory was subsequently nurtured. he was also responsible for the offhand remark that 'civilization is a plant much oftener propagated than developed' (Tylor 1958, 53) – by which he meant much oftener diffused than evolved.

It was, of course, the successors of Tylor who in the early 20th century elevated diffusion to the level of a general explanatory principle. This came about primarily as a reaction against the excesses of the 19th century evolutionists. The equally conspicuous excesses which were soon to be committed in the name of the new doctrine are attributable in large part to its very novelty and the exaggerated estimate of its explanatory potential, much as were the earlier excesses of the evolutionists, and the later excesses of psychological anthropologists and of cultural materialists. Whatever its limitations, the diffusion concept liberated historical anthropology from the straight-jacket of sterile rationalism in which it had been confined for more than forty years.

The battle between diffusionists and evolutionists was fought out primarily over the interpretation of ethnographic data. In the field of archaeology, on the other hand, the diffusionists found themselves much more consistently at odds with older migration theories, because at that time evolutionism had not yet been widely applied to the ordering of archaeological data except in the Western European Paleolithic sequence. As Daniel (1964, 9-49) and others have pointed out, evolutionism is a doctrine that only works properly within a reasonably extended time frame, and at the beginning of the 20th century no such frame existed except in the study of the European Paleolithic. The Neolithic and Bronze Age cultures, and even more the aboriginal cultures of the Americas, were seen as developments of the relatively recent past, and their interpretation was still dominated by the migrationist rather than by the evolutionist paradigm, at the time when the diffusionist challenge arose.

Because migrationism, for all its situational popularity, was never championed as an abstract principle of explanation, the battles between migrationists and diffusionists were necessarily fought on an ad hoc, case by case basis. They were also waged primarily in areas of the world where the investigation of prehistory became specifically the province of anthropologists, for diffusion, unlike migration, is a relatively esoteric anthropological concept. In areas beyond the investigative reach of anthropology, as, for example, in the prehistory of the Near East, of China and of parts of Europe, the migrationist paradigm has survived relatively unchallenged down to the present day.

The parameters of disagreement between migrationists and diffusionists were remarkably simple and clear-cut. The foreign origin of cultural innovations was generally taken for granted by both; in effect they accepted what we might call a 'law of fewest inventions'. The issue between them was whether a movement of people or only a movement of ideas should be envisioned. On the Northwest Coast, for example, there was one school of thought which attributed the distinctive culture complex of late prehistoric times mainly to a migration of people from the interior plateaus, while another school stressed cultural diffusion from Northeast Asia. In eastern North America nearly all of the most seminal cultural developments -- Adena, Hopewell, Mississippian, and Iroquoian – were attributed on the one hand to migration and on the other to diffusion by adherents of the rival theories. Much of the same debate was waged over the Neolithic and Bronze Age cultures of Europe, though here the influence of anthropological (i.e. diffusionist) thinking was considerably more muted. Perhaps because European prehistorians are still as often trained in the classical as in the anthropological tradition, migration theory continues to flourish on European soil as on no other. Even the avowed diffusionist Gordon Childe (cf. 1950, 9), who is credited by Daniel (1964, 84) with the creation of a whole new paradigm in European prehistory, was in practice primarily a migrationist. In specific case after case we find that his notion of diffusion involves the wholesale movement of peoples, as though he could not quite bring himself to believe in the dissemination of culture through communication alone.

One further arena of dispute between migrationists and diffusionists which deserves mention is that of Predynastic Egypt. By the time of Petrie (e.g. Petrie 1939) the Mesopotamian stimulus for Egypt's dynastic civilization was widely recognized, but there was, and is continuing, debate as to whether the idea of civilization was introduced by Asiatic immigrants (Petrie 1939; Baumgartel 1970) or merely borrowed by the native Egyptians (Kantor 1944, 135-6; Trigger 1968, 77-86). Here, as in a number of other cases, the migrationist position was originally buttressed by purported racial evidence which has since been discredited (Berry *et al.* 1967).

Although some of the disputes I have just mentioned are still flourishing, their number and intensity seems to have been declining throughout the last 50 years. Undoubtedly this is attributable to the skeptical attitude toward all generalizing explanations which developed among archaeologists between the 1930s and the 1960s. They were willing in those pragmatic years to envision either migration or diffusion in specific (and usually quite localized) cases, but not to espouse either as a general principle of explanation. There developed in consequence a kind of de facto accommodation between diffusionist and migrationist interpretations, which generally prevails down to the present day.

To begin with, it is readily observable that migration theory is most often invoked today as an explanation for site distributions, and diffusion as an explanation for trait distributions. This, however, is an oversimplification, for migration is also taken to account for trait distributions in cases of extreme discontinuity. The rule of thumb here seems to be that diffusion is the preferred explanation for relatively gradual culture change, especially when an external source is clearly specifiable, while migration is the preferred explanation for seemingly drastic change, whether or not an external source can be recognized. Both Rouse (1968, 65) and Trigger (1968, 40-41) have warned that this is empirically unacceptable; before we can convincingly suggest a migration we should point to its place of origin. A survey of currently accepted migration theories shows, however, that this test is met in only a minority of cases. Surprisingly often the finger of origin points only toward some vaguely defined human reservoir such as the 'gran chichimeca' of northern Mexico, the Arabian desert, or the central Asian steppes.

Among *ad hoc* theories of historical change we must at this point give recognition to still a third alternative which is not quite either diffusionism or migrationism, and which for want of a better term I shall call invasionism. This model envisions a temporary or permanent incursion of a new population whose primary effect is not cultural enrichment or replacement, but rather destruction. We have seen this idea proposed over and over again to account for the abandonment of individual sites, but it has also been proposed at a much more general and systemic level to account for the disappearance of whole civilizations such as the Classic Maya, the Aegean, and the Harappan.

Because migration theory has no ideological foundation and no overt champions, the extent of its influence in culture history is easily overlooked. There is no part of the world in which it has not been extensively applied as an *ad hoc* explanation, and the number of cases in which it is still the preferred paradigm is truly astonishing. It remains true today, as it was at the beginning of the century, that migration theory flourishes best in the later phases of prehistory and the earliest phases of recorded history. Early prehistory, both in the Old and New Worlds, is still dominated by evolutionary stage theory, which at least conceptually has made little advance since the beginning of the century, while in the later stages of recorded history diffusion is universally accepted as the more normal mode of cultural transmission.

It must at the same time be recognized that migrationism, for all its enduring strength, has been more or less continuously in retreat throughout the 20th century. Increasingly, its most dedicated adherents are eccentrics and crackpots who have done the doctrine more harm than good. In scientifically respectable circles, meanwhile, migration theories have been in retreat not only before the rival paradigm of diffusionism, but also because additions to the archaeological record have shown that earlier impressions of cultural discontinuity were frequently exaggerated. When the continuities between successive cultural stages are seen to outweigh the discontinuities, a theory of migration often ends by posing more problems than it solves. Reinterpretations forced by amplified archaeological evidence have taken place, among other areas, in the American Southwest (Kidder 1924, 122), in Neolithic Britain (Clark 1966), in Predynastic Egypt (Trigger 1968, 77-86), in Nubia (Adams 1977, 1-7), and in pre-literate Sumer (Frankfort 1932, 21). It seems probable that there are many other cases in which migrationist theory survives only because there has not yet been a systematic re-examination of the archaeological evidence – Jericho being one obvious candidate for such reconsideration (cf. Kenyon 1960).

On a very different plane from the positivist attacks of diffusionists and revisionists have been the sweeping, systemic denunciations of both diffusionism and migrationism launched by the newly ascendant school of cultural materialism. Only rarely do the materialists dispute the older theories at the level of empirical evidence; instead they loftily dismiss diffusion and migration theories at one stroke as unscientific and therefore unacceptable. Their argument essentially is that everything important in culture is caused by techno-environmental factors, that these factors can usually be identified in the local environment without looking abroad for them, and that since we need not look abroad we should not do so.

It is beyond the compass of this paper to dispute the logic of the materialist position, except as it reflects on the continued acceptability or unacceptability of diffusion and migration theories. In this latter connection I will, however, suggest two logical weaknesses in the materialist critique. First, although the materialists insistently claim that their doctrine is ‘scientific’ and that migration and diffusion are not, in fact they have only substituted a new ‘law of least moves’ for an older ‘law of fewest inventions,’ to which both diffusionists and migrationists consistently adhered. As an operation to be performed on the unresisting body of prehistoric data, either seems logically acceptable; as an explanation for the recorded facts of history, neither seems to carry us very far.

Second, while repeatedly assailing migration and diffusion theory in general terms, what the materialists have actually constructed is not an opposed paradigm but an alternative one, in which migrations are ignored rather than specifically refuted. Why look abroad for the sources of cultural innovation when you don’t have to? is the essence of their argument. It is, however, an argument that works only if stylistic factors are disregarded, for no amount of techno-environmental determinism will account for the stylistic resemblances between Sesklo and Asiatic pottery (Childe 1950, 45-6) or between

Northwest American and Northeast Asiatic art (Kroeber 1939, 31). By their impatient dismissal of stylistic phenomena as non-significant (cf. especially Harris 1975), the materialists threaten to carry us all the way back to the simplistic evolutionism of the 1860s. Indeed, a reading of the more doctrinaire formulations of Fladmark (1975), of Plog (Martin and Plog 1973) and of Moseley (1975) produces an uncomfortable feeling that they may have already done so. If so, we are quite literally back at 'square one', for it was precisely in reaction against such simplistic and selective manipulations of the prehistoric data that diffusionism arose in the first place.

The pendulum swing of anthropological theory from simplistic evolutionism to hyper-diffusionism and back to simplistic evolutionism calls to mind Edmund Leach's comment on the 'New Archaeology': 'Professor Binford's remark that 'behavior is the byproduct of the interaction of a cultural repertoire with the environment' may be proto-typical of the 'new' archaeology, but to a social anthropologist it reads like a quotation from Malinowski writing at a time when naive functionalism was at its peak – that is to say about 1935' (Leach 1973, 761-62). If nothing else, these cycles of intellectual fashion within our own discipline should be enough to destroy anyone's faith in linear evolution.

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Plate 15.1. Peter Shinnie and Bryan Haycock at Meroe, February 1968 (SARS Haycock Archive, HAY S014.23).

Plate 15.2. Ahmed Ali Hakim and Bryan Haycock at Meroe, February 1968 (SARS Haycock Archive, HAY S014.24).

Paradigms in Sudan archaeology[1]
(1981)

This article was written, by invitation, for a special issue of the journal Africa Today, *subtitled 'The Sudan: 25 Years of Independence'. It was, consequently, written for a wider audience than most of those in preceding pages—persons with a broad overview but mostly without a detailed knowledge of Sudan archaeology. I took the opportunity therefore to paint with a broader brush than in previous articles, and to consider Sudan history from the perspective not just of archaeology but of the conjunction of archaeology, ethnology, history, and art history. This approach had been first developed a few years earlier in my book* Nubia, Corridor to Africa.[2]

Ever since Thomas Kuhn (1962) wrote *The Structure of Scientific Revolutions*, it has become fashionable to speak of the growth of science in terms of a succession of paradigms. Each paradigm represents a distinctive combination of methodology, accumulated data, research interests, and a philosophical point of view. Paradigms are usually tantamount to developmental stages, but the concepts of paradigm and of stage are not precisely equivalent. Stages in scientific development are often inaugurated simply by technological breakthroughs or basic discoveries, but paradigm shifts always involve a new philosophical orientation. Moreover, paradigms do not follow one another in a rigid succession. Often they co-exist for substantial periods of time, when some researchers are working within the framework of an older paradigm while others have adopted a new one.

I shall argue in these pages that field archaeology in the Sudan has been governed by four successive paradigms during the period of 164 years since Giovanni Belzoni cleared out the temples of Abu Simbel in 1817.[3] These paradigms, as I hope to show, have in large measure reflected the changing political circumstances of the colonial and post-colonial worlds, but they have also been shaped by methodological advances, by the growing professionalization of archaeology, and by the Western world's changing philosophical vision of Africa and her peoples. My discussion will be concerned mainly with archaeology in the narrow sense of the word: that is, with investigations that begin with the physical unearthing of buried remains. Many scholars have contributed to the reconstruction of Sudanese history through the description of exposed ruins, the analysis of texts, and the study of previously recovered *objects d'art*, but I do not include them in the category of archaeologists because their work lacks the dimension of field methodology which is a critical part of every archaeological paradigm.

For the sake of terminological simplicity I shall temporarily annex to the Sudan that part of Egypt, formerly known as Lower Nubia, whose peoples, culture, and history are more integral with the Sudan than with Egypt proper. That is, I shall use the terms 'Sudan' and 'Sudanese' to refer to all of the peoples, cultures, and archaeological remains to the south of the First Nile Cataract, where ethnic Egypt ends and Nubia begins. The pursuit of archaeology developed hand-in-hand in Egyptian Nubia and in the Sudan, and from the beginning both followed a rather different course than did the investigations of Egyptologists in Egypt proper. Because of the scarcity of written texts, it was always recognized that field archaeology

[1] Originally published in *Africa Today* 28 (2), 15-24. Note, in the original publication the references were placed in footnotes. Here the citations have been incorporated into the text with the references following at the end of the paper for uniformity and clarity. The following biography accompanied the article:
'William Y. Adams is Professor of Anthropology at the University of Kentucky. and is concurrently Field Director of archaeological excavations at Qasr Ibrim. Egyptian Nubia. He was formerly Director of Excavations for the Sudan Government Antiquities Service during the period when a part of the Sudan was flooded by the Aswan High Dam. He is the author of *Nubia, Corridor to Africa*, which received the Melville J. Herskovitz Prize from the African Studies Association 1978.' [ed.].
[2] See further Adams 1977 [ed.].
3 See further Belzoni 1820, 206-214.

had a much more central and critical part to play in the reconstruction of Sudanese history than in the northern country, where archaeology has generally been treated as supplementary to textual history.

The four paradigms in Sudan archaeology I have designated as the extractive colonial, the enlightened colonial, the post-colonial, and the independent national. I shall be concerned here chiefly with the first three, leaving the fourth and most recent for consideration as it develops in future years.

The extractive colonial paradigm

What I have called the extractive colonial paradigm in Sudanese archaeology was dominant throughout the 19th century; it is exemplified by the field activities of the aforementioned Belzoni, of Joseph Ferlini,[4] and of E. A. Wallis Budge (1907, 66-504). The three men make a rather disparate group, for Belzoni and Ferlini had no pretensions to scholarship, while Budge was one of the most respected philologists of his time. From the standpoint of field archaeology, however, their work was very much of a piece; it is remarkable for its almost total irresponsibility. Like the colonial powers in general they were out to extract whatever they could, and they belonged to an age when the White Man was above the law in Africa. The lack of any kind of standards in their excavation is understandable in view of the total lack of professionalization in 19th century archaeology, and the lack of any sense of accountability toward the peoples and countries in which they worked was characteristic of the 19th century colonial mentality. But their almost wanton destruction of the monuments themselves bespeaks a contempt not only for the modern cultures but for the ancient cultures as well. This point of view is clearly implied in the words of Budge:

> 'Many archaeologists have imagined that we shall find in the Sudan the ruins of purely native buildings and monuments which will enable them to reconstruct a connected history of the country, but none of the surveys and explorations which have been made by ancient and modern travellers has resulted in the finding of any ruins which are not … in fact, the work of foreigners' (Budge 1907, 511-512).

Fortunately for posterity, the Sudan throughout the 19th century was remote and difficult of access, and the amount of 'extractive archaeology' undertaken was mercifully limited. The situation was much worse in Egypt, where wholesale looting by supposedly cultured gentleman and lady amateurs was an accepted practice. There was however one important philosophical difference between the two countries. The scholars and diggers in Egypt generally respected and admired the country's ancient civilization, while despising its modern-day inhabitants. In the Sudan the reverse was true: the travellers and field workers tended to like the modern Sudanese (albeit as 'happy savages'), while holding their ancient cultures in contempt.

No discussion of 19th century archaeology in the Sudan can omit all mention of Richard Lepsius. His two-year sojourn in Egypt and the Sudan (1842-44) resulted in the 12-volume *Denkmaler aus Agypten und Athiopien* (Lepsius 1849-1853), an encyclopedic work recording hundreds of hieroglyphic texts and reliefs which were then visible on the walls of temples and tombs. Lepsius was, for his time, a meticulous observer, respectful of the antiquities and the cultures he studied, and the volume of his published work certainly shows a well developed sense of responsibility toward the community of scholars. He stands as an example of the levels of responsible achievement that scholarship occasionally reached in 19th century Africa, but I have not included him in my discussion of paradigms because in the narrow sense he was not an archaeologist.

In sum, then, 19th century archaeology in the Sudan was very much a reflection of the regime that nurtured it: contemptuous, exploitative, and irresponsible. The field workers, primarily, were amusing

[4] See further Budge 1907, 285-320. See also Priese 1993, particularly 12-15; Wildung 1997, 302-303 [ed.].

themselves, with no sense that what they did could ever be of real consequence to anyone. Their only redeeming virtue was that they did so little.

The enlightened colonial paradigm

The Anglo-Egyptian colonial regime which was instituted after the Kitchener reconquest was like enough to its 19th century predecessor so that many Sudanese referred to it as the '*second Turkiya*' (Adams 1977, 641). Yet nearly all observers will agree that it took a more enlightened view of its responsibilities toward its subjects, as is evidenced especially in its development of educational institutions. This relatively enlightened paternalism is reflected also in the archaeological paradigm that became dominant in the Sudan during the first half of the 20th century.

The outstanding figure — indeed one might almost say the mythological father — of the enlightened colonial paradigm in Sudanese archaeology was George A. Reisner. Beginning with his inauguration of the First Archaeological Survey of Nubia (necessitated by the building of the original Aswan Dam) in 1907 (Reisner 1910), he went on to organize and direct the monumental Harvard-Boston Expedition, which was active in the northern Sudan in nearly every season until 1931 (Dunham 1955, 70-74). Many other scholars came in his footsteps, and most in one way or another were influenced by his example of responsible field work and imaginative scholarship. Other pioneers of the early 20th century included F. Ll. Griffith, John Garstang, Henry Wellcome, Leonard Woolley, and David Randall-MacIver; a generation later came H. W. Fairman, W. B. Emery, L. P. Kirwan, M. F. Laming Macadam, Ugo Monneret de Villard, Georg Steindorff, and a good many others. A little-recognized figure who contributed almost as much as did Reisner to the development of the enlightened colonial paradigm was J. W. Crowfoot. As Inspector (later Director) of Education in the Sudan he had little opportunity for direct involvement in field work, although he did take part in several surveys.[5] But it was he who, within the Education Department, created the administrative nucleus for what was later to become an independent Department of Antiquities, and it was through his influence and interest that the Anglo-Egyptian regime sought both to encourage responsible archaeology and to protect the surviving antiquities of the Sudan. If the work of Reisner, Griffith, and others shows a much greater sense of accountability toward the Sudan than did their 19th century predecessors, this is in part because Crowfoot, on behalf of the Sudan, demanded it.

The leading figures in early 20th century Sudanese archaeology were a fairly diverse lot, but they had a number of important characteristics in common. They were nearly all established scholars from recognized institutions, and their sense of scholarly responsibility is reflected in the volume of their published work. Yet their orientation was still primarily toward the recovery of objects rather than the reconstruction of history, and most of their publications are hardly more than illustrated catalogues. They had, inevitably for the time, no formal archaeological training, but they had at least an incipient sense of appropriate methodology. Reisner himself was in the forefront of this development, for it was he who pioneered the use of standardized forms for recording archaeological data (Rowe 1961). Yet Reisner and most of his colleagues continued the 19th century travellers' practice of recording most of his notes in the form of a diary — a charming anachronism that survives to the present day among many Egyptologists. Field work mostly took the form of sustained campaigns carried out over many seasons in the largest and most conspicuous archaeological sites in the northern Sudan; the massive financial investment in this work shows for the first time a clear appreciation that Sudanese archaeology had an important story to tell.

The archaeologists of the enlightened colonial period were nearly all Egyptologists, and their outlooks, their interests, and their deficiencies were those characteristic of the field of Egyptology in general. Field

[5] See further Budge 1907, 437-438; Crowfoot and Griffith 1911.

methods, though gradually improving, lagged far behind those that were developing in other parts of the archaeological world; there was not (and indeed still is not) a full appreciation for the importance of contextual evidence.[6] But the most significant deficiency of the enlightened colonial paradigm was philosophical: the persistence of 19th century racism and of the colonial mentality. In this respect Reisner himself was as bad as any, for he was to write:

> 'Wretched Nubia' was at first a part of Egypt. After the First Dynasty it was only an appendage of the greater country. and its history is hardly more than an account of its use or neglect by Egypt (Reisner 1910, 348).

Racism led Reisner and many of his colleagues to attribute the more advanced stages of Sudanese cultural history not merely to Egyptian influence but to actual Egyptian (or Libyan) immigration, while periods of cultural decline were blamed on the departure of the Egyptians or on migrations from the south. Sudanese history thus resolved itself into a series of disconnected episodes attributable to different actors. This rather myopic viewpoint shows conspicuously the lack of cross-cultural insights which might have been obtained from the study of cultural and of racial history in other parts of the world.

The enlightened colonial paradigm was to become increasingly enlightened with the passage of time, and then to be supplanted in part by other points of view. Yet it is by no means extinct even yet; it continues to flourish especially among the older generation of Egyptologists. The Nubian Salvage Campaign of the 1960s stimulated a renewed interest in the Sudan on the part of Egyptologists, and in so doing gave the old, Reisnerian vision a new lease on life. A striking demonstration of this can be seen in W. B. Emery's *Egypt in Nubia*, a work of general history published in 1965 which hardly departs from the theoretical position staked out by Reisner at the turn of the century. Except for the accumulation of some new data, it is the book that Reisner himself could and probably should have written. A less happy example of the persistence of the enlightened colonial paradigm can be found in the International Society for Nubian Studies, an organization which was founded by and which continues to be dominated by Egyptologists of the old school. The steadfast refusal of this group to admit a Sudanese or Nubian scholar to its governing council, despite the presence of eminently qualified candidates, is an indication that the colonial mentality dies hard.[7]

The post-colonial paradigm

There has been an interval in the history of nearly every African country, following the achievement of independence, when the upper levels of bureaucracy were carried along to a large extent by momentum from the colonial era. In the field of Sudanese archaeology this transitional phase has its counterpart in what I have called the post-colonial paradigm, which still reflects some of the thinking of the colonial era while showing a keen and necessary appreciation for altered political circumstances.

If George A. Reisner was the founding genius of the enlightened colonial paradigm, that honor must surely be accorded to A. J. Arkell with respect to the post-colonial paradigm. His field work in the Sudan preceded the formal granting of independence in 1955, but it was carried out at a time when the coming of independence was clearly foreseeable. Arkell prepared for that time by organizing the Sudan Antiquities

[6] See Trigger 1968, 61-90.

[7] Since the publication of this article in 1981, numerous Sudanese and Nubian scholars have become members of the Society and many have been elected and served on its board (i.e. Abdelgadir Mahmoud Abdallah 1990-1998, 2006-2014; Salah ed-Din Mohamed Ahmed 1998-2006; Abdel Rahman Ali Mohamed Rahma 2006-2014; Intisar Soghayroun el-Zein 2010-2018; alHassan Ahmed Mohamed 2014-2022; Mohamed Ahmed Abdelmagid 2018-2026). The Director General of the National Corporation for Antiquities and Museums, Sudan and the Head of the Supreme Council of Antiquities, Egypt are now permanent board members [ed.].

Service for the first time as a sub-ministerial government branch, and by training the first Sudanese antiquities inspectors. One of his protégés, Thabit Hassan Thabit, was in time to become the first Sudanese Commissioner for Archaeology (as the Director-General of Antiquities was then called).

Another pivotal figure in the development of the post-colonial paradigm is P. L. Shinnie, whose work in the Sudan began in the last years of the colonial era and has continued right down to the present day.[8] To the pioneer names of Arkell and Shinnie I would add those of three North Americans who came to the Sudan in the first decade following independence: Bruce Trigger, Fred Wendorf, and myself.

Arkell, Shinnie, and the three Americans come from backgrounds even more diverse than were those of their colonial predecessors, but again they have important characteristics in common. The most important is that none of them was or is an Egyptologist. All of them except Arkell had formal archaeological training in fields far removed from the Nile Valley, and all, including Arkell up to the time of his recent death, remained active in other and distant fields. Their first involvement in the Nile Valley was, in every case, in the Sudan rather than in Egypt; to the extent that they are also interested in the cultures of Egypt, it is as a spillover from their interest in the Sudan, rather than vice versa as in the case of Egyptologists.[9]

I cannot, obviously, speak of the virtues and defects of the post-colonial paradigm with total objectivity, but I think that nearly all observers will agree as to the methodological advances which it has introduced. Its practitioners have brought with them a variety of analytical techniques and an attention to precision which are the legacy of their earlier work in prehistoric archaeology. Their publications are clearly focused, for the first time, on the reconstruction of culture and of cultural history rather than on the description and illustration of objects per se.

An enhanced sense of accountability toward the Sudan, demanded by the Sudan Antiquities Service itself since the earliest years of independence, is another important component of the post-colonial paradigm. Expeditions are now required to demonstrate technical as well as financial capability before they are granted licenses; licenses are drawn up with much more attention to detail than was formerly the case; and excavators are held strictly to account both for the publication of their results and for a fair division of finds with the Antiquities Service.[10] Yet these requirements have not, as in Egypt, resulted in a confrontation relationship between archaeologists and the government, for the practical assistance and numerous facilities granted to all archaeological expeditions by the Sudan Antiquities Service are a source of worldwide admiration. The warm, collegial relationship between archaeologists on the one hand and antiquities officials on the other is, in my experience, without parallel among Third World countries.

The most significant advance of the post-colonial paradigm, from my point of view, is again to be found in the ideological sphere. The recent generation of archaeologists are free at last from the taint of 19th century racism, and also at least to some extent from the dominant shadow of Egypt. They are finally able to appreciate the cultures of the Sudan for their own sake, and to see the pattern of Sudanese history as a connected whole and not as a series of foreign-inspired episodes.[11] It is surely significant that all five of the leading figures of the post-colonial school (Arkell, Shinnie, Trigger, Wendorf, and Adams) have written wide-ranging books on Sudanese history (Adams 1977; Arkell 1949; Shinnie 1967; Trigger 1976;

[8] Peter Lewis Shinnie (1915-2007). His final archaeological field season in Sudan was conducted at the Royal City of Meroe, 1983-1984 after which much of the focus of his work switched to Ghana. See Plate 15.1 [ed.].

[9] For further information concerning B. Trigger (1937-2006) see Phillips 2007. For additional information regarding F. Wendorf (1924-2015) see Schild 2015 [ed.].

[10] The Ordinance for the Protection of Antiquities, originally conceived in 1905 was modified in 1952, and in 1999. For the most recent ordinance see National Corporation for Antiquities and National Museums 1998-2002, 15-24 [ed.].

[11] See further Adams 1977, 4-7.

Wendorf 1968), a task which was undertaken by none of their predecessors except W. B. Emery (1965).

While I have attributed the transformations of the post-colonial paradigm chiefly to the influence of non-Egyptologists, it would certainly be unfair to omit all mention of two leading Egyptologists, Torgny Säve-Söderbergh and Fritz Hintze, who have also been important contributors. Both have directed important field operations, but their main contribution has come through the study and interpretation of textual material (Säve-Söderbergh 1941; Hintze 1959). They are unique among Egyptologists in focusing their attention and interest on the Sudan rather than on Egypt.

Sudanese observers will insist, rightly, that the post-colonial paradigm is not wholly free from deficiencies and prejudices. Its adherents are far more Sudan-oriented than were their predecessors, but in the last analysis they were still reared in environments and climates far removed from the Nile Valley. With the best of intentions they cannot see the cultures and the history of the Sudan through the eyes of someone who was raised in their midst. There is, too, a continuing tendency to look almost automatically for the sources of Sudanese culture in the Mediterranean Basin, which was the cradle of so much of our own civilization. On the other hand I think none of us except Shinnie has as detailed a knowledge of sub-Saharan African archaeology and culture history as we appropriately should.

A further limitation of the post-colonial paradigm is an accident of economic history. Sudanese independence was followed almost at once by the signing of the Nile Waters Agreement and the subsequent inundation of Sudanese Nubia by the Aswan Reservoir, which for a decade forced the archaeologists to concentrate their attention almost exclusively on the most northerly part of the country. Many of us, like Trigger and myself, have never had the opportunity to work elsewhere than in Nubia, and mostly in Lower Nubia at that. If our point of view is no longer 'Egyptocentric,' it is still to a degree 'Nubiocentric,' which is perhaps another form of parochialism.

This latter deficiency is being corrected by the younger generation of European scholars who are now busily at work in many parts of the Sudan. In other respects I have the impression that most of them still adhere to the post-colonial viewpoint, but there may be some who have fully adopted the independent national outlook.

The independent national paradigm

For any newly independent people, a significant turning point is attained when they begin to rewrite their own history to suit the needs of emergent nationalism and of cultural integration. This development, which is already very conspicuous in many Third World countries, significantly affects the aims if not the methods of field archaeology. I can thus far see only the faintest glimmerings of such a development in the Sudan, if only because Sudanese archaeologists are still routinely sent abroad (mostly to England) to complete their graduate training, and here they are firmly indoctrinated in the colonial or post-colonial traditions. But I can see the gleam of nationalistic aspiration in the eyes of a few of my Sudanese colleagues,[12] and the number of able and energetic students who are now enrolled in the Archaeology program at Khartoum University foretells a time in the not too distant future when the study of Sudanese antiquity will be largely in Sudanese hands.

If Reisner was the pioneer of the enlightened colonial paradigm, and Arkell of the post-colonial, I think we must identify a pioneer figure for the independent national paradigm in the unlikely person of the late Bryan Haycock. Nurtured originally in conventional Egyptology, his was converted, during his tragically abbreviated career at Khartoum University, into a genuine Sudanese nationalist, with a keen appreciation for every phase of the country's history. Lack of proper training prevented his carrying out any actual

[12] A nationalist archaeological paradigm did begin appearing after this article was written as the author suggests. See further for example, Osman 1987; 1992 [ed.].

archaeological excavations, but his lectures and his numerous field surveys stimulated the interest of a whole generation of Sudanese students in the history and the antiquities of their country (Adams 1976, 174; Hintze 1976, 6).[13] The current, very active Archaeology program at Khartoum University is Haycock's legacy, and nearly all of the Sudanese scholars on its staff were originally his students.

My colleague Professor Shinnie, who has himself taught at Khartoum University in the recent past,[14] is better qualified than am I to talk about the most recent developments in Sudan archaeology. I do not expect him to use my paradigm model of analysis, since he likes to poke fun at my anthropological jargon, but I suspect that a lot of what he will have to say will be relevant to the emergency of an independent national paradigm in Sudanese archaeology.

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[13] See Plate 15.1 and also Edwards 2013, 82-85; Shinnie 1974, 2-3; SAD.804/15/37-38 [ed.].

[14] Peter Shinnie was Head of the Department of Archaeology, Faculty of Arts, University of Khartoum from 1965-1970. He was succeeded by Abdelgadir Mahmoud Abdallah, who was in turn succeeded by Ahmed Ali Hakim. See Plate 15.2 [ed.].

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Plate 16.1. William Y. Adams in the Bayuda desert, 2011
(photo courtesy D. A. Welsby).

The Archaeologist as Detective[1]
(1973)

When I began my teaching career, in 1966, one of the first courses assigned to me was 'Beginnings of Civilization'. It was not a course often taught in anthropology departments at that time, for anthropological thinking on the subject was wholly theory-driven, with very little empirical foundation. Civilization (i.e. complex, literate society) was seen as an inevitable stage in the process of social evolution, and no other explanation was necessary. Consequently, there was no such thing as a suitable textbook in the anthropological literature, and I was forced to turn outside anthropology if my course were to have any kind of empirical foundation.

Civilization very definitely meant, to me, complex, literate society, and that translated archaeologically into late Neolithic and early Bronze Age, with primary focus on the Near East and the eastern Mediterranean. I had no familiarity whatever with the literature in those areas, and had to 'bone up' from scratch—and in a hurry. I found a lot of reputable, anthropologically sound literature on the Neolithic, but nothing comparable for the Bronze Age. All I had available were works based on excavations before the Second World War, and mostly before the First World War, when Near Eastern archaeology was pretty much the province of what I can only call gentleman and lady amateurs, digging in monumental sites.

Whatever may have been the quality of their excavations, their explanations were naïve in the extreme. These people had no concept of cultural dynamics—hardly even a concept of culture in the full anthropological sense. Whenever they found evidence of major culture change over time, they could only attribute it to external forces. Migration of new peoples was the preferred explanation, but otherwise diffusion or invasion. Even the most advanced of peoples, like the Egyptians, were not given credit for inventing anything for themselves.

I had long been a devoted Sherlock Holmes fan, even while I recognized that a lot of his deductions wouldn't stand up to critical analysis. Before long it dawned on me that the same thing was true of a lot of older archaeological literature. I got the idea of rewriting, for fun, some archaeological interpretations in the form of detective cases. I didn't suppose that such an exercise would find acceptance in any of the standard journals, but the invitation to contribute to a festschrift opened a door for me.

Archaeology has often and justly been likened to detective work; undoubtedly this explains a good deal of its appeal to the professional as well as to the layman. The necessity of reconstructing large events from small clues provides a satisfying challenge alike to the intellect and to the imagination. It is probably no coincidence that detective novels form the principal light reading of many archaeologists, and more than one member of the profession has actually contributed to the detective *genre*.[2]

Scientific detective work, as Sherlock Holmes long ago pointed out, consists of observation and deduction (Conan Doyle 1953, 13).[3] Anyone familiar in detail with the work of this greatest of fictional detectives,[4] however, will recognize that his genius was much more apparent in the first of these directions than in the second. Again and again, our admiration for his skill and imagination in discovering evidence

[1] Originally published in D. W. Lathrap and J. Douglas (eds) 1973. *Variation in Anthropology. Essays in Honor of John C. McGregor*. Urbana, 17-29.

[2] The best known example is certainly Agatha Christie—not herself an archaeologist but the wife of one (M. E. L. Mallowan). Professional archaeologists who have written successful detective fiction include Stanley Casson, Glyn Daniel, and E. C. Vulliamy.

[3] Kenneth Rexroth (1968) points out that Holmes actually does not discriminate between deduction and induction, using the same term for both operations.

[4] Let outraged Holmes aficionados be assured that I am one of the most devoted of their number. My choice of Holmes to exemplify the foibles of the fictional detective was dictated by the convenience of familiarity, since I have read every one of the four novellas and 55 short stories at least a dozen times—eloquent testimony to my longstanding admiration for the Wizard of Baker Street. By way of further exculpation, I quote Christopher Morley (Conan Doyle 1953, xiv): 'Even in the less successful stories we remain untroubled by any naiveté of plot; it is the character of the immortal pair that we relish.' [Author]. See Plate 16.1 [ed.].

is tempered by a certain incredulity in regard to his conclusions. On the whole, one feels relieved in the thought that Holmes never had to take his cases to court (cf. Conan Doyle 1953, 1179: 'My friend has not yet stood in the dock.').

As a practical illustration of the Sherlockian method, let us consider the Adventure of the Abbey Grange (Conan Doyle 1953, 743-760), which incidentally includes some of Holmes' finest sleuthing. It also includes, in the denouement, the following chain of 'deduction':

> No one but an acrobat or a sailor could have got up to that bell-rope from the bracket, and no one but a sailor could have made the knots with which the cord was fastened to the chair. Only once had this lady been brought into contact with sailors, and that was on her voyage, and it was someone of her own class of life, since she was trying hard to shield him, and so showing that she loved him. You see how easy it was for me to lay my hands upon you when once I had started upon the right trail (Conan Doyle 1953, 760).

Confronted with this seemingly inexorable logic, Captain Crocker (the accused) readily confessed; however, as he proved to be well motivated, Holmes connived at his escape from justice. Just for fun, however, let us suppose (contrary to the most sacred canons of detective fiction) that he had protested his innocence and entered a plea of not guilty in court. Holmes, as the original discoverer of the incriminating evidence, must then necessarily have been the chief witness for the Prosecution. Following his testimony, we can imagine this exchange in cross-examination by the Defense:

'Mr. Holmes, you stated your opinion that no one but an acrobat or a sailor could have got up that bell-rope from the bracket. Is that correct?'

'I did not state it as my opinion; I stated it as a fact.'

'Do you still hold that statement to be true?'

'Yes.'

'Can you conceive of no other individual capable of such a feat—a chimney-sweep, for example, or an athlete, or a tree pruner?'

'Possibly, but there were no chimney-sweeps, athletes or tree pruners among the suspects.'

'Very well. You stated that the accused had cut the bell-rope with his knife, but had afterward frayed the cut end to make it appear that it had broken naturally. Why in your opinion would he have done such a thing?'

'To disguise the fact that he had had to climb up and cut it—which I repeat could only have been done by an acrobat or a sailor. He wanted to make it appear that anyone could have brought down the cord simply by pulling on it, because it was already frayed.'

'Frayed or not, would it not have taken a pull sufficient to ring the bell, and thus alarm the whole household, before the cord actually came down?'

'Yes, sir, I suppose it would. Evidently Captain Crocker had not thought of that.'

'Now, Mr. Holmes, your argument is essentially that the accused tried to give the impression that the cord broke naturally in order to disguise his own acrobatic prowess; in other words to leave no clue that a sailor had been involved in the crime. Why, then, did he tie up the lady with a series of knots which proclaimed his occupation unmistakably?'

'He must have been flustered and in a hurry.'

'His other actions do not suggest it. Let us turn now to the next point in your evidence. You say, 'Only once had this lady been brought into contact with sailors.' How did you know that she had only once been brought into contact with sailors—that she had never made a previous voyage?'

'I did not know it, sir. I must confess that when I made that statement, I only meant that I knew for certain that she had once been brought into contact with sailors.'

'That is a very different matter, Mr. Holmes. Let me turn now to your final point, in which you state: `... it was someone of her own class of life, since she was trying hard to shield him and so showing that she loved him.' Do you consider that love is the only motive strong enough to induce a lady to come to the defense of a gentleman? How about fear? Has not the threat of blackmail driven ladies to crime in some of your own most famous cases, for example those of Charles Augustus Milverton (Conan Doyle 1953, 667-679) and the Second Stain?' (Conan Doyle 1953, 761-782).

'I did not consider it a possibility in this case.'

'Very good. One final point: do you consider it impossible for a lady to fall in love with anyone but a member of her own class?'

'Of course.'

'Mr. Holmes, did you not once tell Dr. Watson that 'love is an emotional thing, and whatever is emotional is opposed to ... reason?'' (Conan Doyle 1953, 172).

'Yes sir, I did. The idiot had proclaimed his intention of marrying, and I felt it my duty to counsel him against such a step. However, my logic failed to move him.'

'Just so. And are you not, with your wide and varied knowledge of the ways of mankind, aware of such adages as 'love is blind,' `love conquers all,' love knows no season,' `love knows no frontier,' and the like?

'They do not apply to an English lady.'

'Have you forgotten that Lady Brackenstall was reared in Australia, and that she actually protested to you that she found English conventions uncongenial?'

'That is immaterial; her people were English.'

'The same could be said of everyone in Australia, from convicts to the Governor-General of Victoria. Have you ever been there?'

'No.'

'I ask the jury to make a note of that fact. Very well, Mr. Holmes you may stand down.'

* * * *

This somewhat frivolous exercise may seem far removed from the sphere of the professional archaeologist, but it is not without relevance, as I hope to show. The point of the demonstration is to suggest that observation and deduction are not the same thing, and that a brilliant series of observations may be betrayed and invalidated by an inept series of deductions.

Deductions may be defined as general propositions of causality applied to the explanation of specific facts. As such their validity has nothing to do with the facts they are intended to explain. It is one thing, obviously an important one, to be sure of the facts; it is altogether something else to determine their meaning once they are established. Successful deduction, while necessary to the detective, has nothing to do with sleuthing; it depends upon a thorough knowledge of human nature and human experience.[5] Indeed, Sherlock Holmes often proclaimed himself a dedicated student of man, and insisted that 'all knowledge comes useful to the detective,' (Conan Doyle 1953, 911) even if his deductions do not always support his reputation for sagacity.

[5] Virtually the same principle, applied to the study of history, was articulated by Ibn Khaldun (Mahdi 1964, 171-187) in the Middle Ages, by Giambattista Vico (Collingwood 1956, 63-71) in the 18th Century, and by Auguste Comte (Teggart 1962, 99-109) in the 19th Century.

Before proceeding to more serious matters, it must be acknowledged that the detective story is a species of fantasy which is not expected to conform closely to the realities of existence, but rather to its own formal canons. Ingenuity and novelty are the *sine qua non* of the fictional dénouement, rather than logical uniqueness or sociological probability. Thus it is one of the rules of the *genre* that the criminal either confesses or dies, or both—in any case eliminating the prospect of an awkward and probably inconclusive trial.

What about explanation in the kindred field of archaeology? It is my belief that the resemblance between the fictional detective and the real-life archaeologist is sometimes too close for comfort. The archaeologist too is likely to excel at painstaking and meticulous observation rather than in any broad understanding of society or history, and may often undo the value of his observations with naive and even preposterous deductions. He is at enormous pains to establish the admissibility of his evidence in court, and is ever on guard against a challenge to his facts. Once they are admitted, however, he may be careless and illogical in his use of them. He has his own shibboleths (pottery = people, layered stratigraphy = interrupted occupation, general destruction = warfare, etc.) which have no more inherent probability value than Sherlock Holmes' class snobberies.

One finds all too often in archaeological explanation, as in detective fiction, that ingenuity wins out over common sense. Above all, one finds that probability values are wildly inflated: the possible becomes the probable, and the probable becomes the certain. In this way both the archaeologist and his reader are led to overlook the fact that the proposed solutions are, in nearly every case, non-unique. As an exercise in elementary logic, one can take the data from which many of the classic historical theories in archaeology have been derived, and use the same data to construct any number of alternate theories.[6]

In short, archaeological explanation sometimes appears to be a *genre* of its own, with its own canons and rules of logic. Some of our most famous and popular historical theories can, in fact, be transposed into the argot of detective fiction and lose nothing in the process. As illustration, I append here a detectival rendering of a few of our celebrated 'cases', admittedly drawn from rather dusty archives and more typical of another age than of our own.

The case of the wooden horse[7]

'Heinrich,' I cried, 'you are a wizard. How could you have known that Homer was telling the truth when no one else believed him?'

'My dear professor, it is simplicity itself,' he replied. 'In his description of the crime Homer had told us the general area in which it took place, and that the city of Troy was completely destroyed. He could not, as you know, recall the exact address, but he was sure it was somewhere about the eastern shore of the Hellespont, and he distinctly recalled a small stream nearby. Now, when I went in person to the Asiatic shore of the Hellespont, and actually found at Hissarlik, by a small stream, a ruined city which had been destroyed not once but several times, it is surely not too farfetched to suppose that this is the very city of which Homer wrote, and therefore that he was telling the truth.'

'But are there not other ruined cities in the area?' I objected.

'Of course,' he replied, 'but a simple process of deduction will tell you that as Homer spoke only of one city, it needed only one to prove him right.'

'How could you be so sure that Agamemnon and Achilles were the culprits?'

'In order to answer that, I had first to establish the time of the crime. Homer, of course, was unable to say, but from certain peculiarities of his text I had already made up my mind that it was probably

[6] For further development of this thesis see Adams 1968.
[7] For the background of this case see Blegen 1964; Childe 1964, 46-47, 73-7; Mellink 1965, 120-121; Piggott 1965, 159.

some time in the 12th century BC. It is also true that the individual destruction levels at Hissarlik could not be dated very accurately, but with so many to choose from it seems virtually certain that one or another of them belongs to the 12th century. Now, when you find evidence of an act of complete and wanton destruction in the 12th century, and when you know that such a notorious pack of scoundrels as the Agamemnon gang were in the neighborhood during the same century, it is surely too monstrous a coincidence to suppose that they are not cause and effect.'

'What about the other destruction levels at Hissarlik?'

'My dear professor, your own good sense will suggest to you that the Agamemnon gang could not have destroyed the site more than once. Therefore, no matter how improbable it seems, we must suppose that the other destructions were due to some other cause. After all, Anatolia is an area of frequent earthquakes, and has also been the scene of almost continuous warfare, so that we really need not look very far afield for the causes of the other destructions at Hissarlik.'

'Yes, I see it all quite clearly,' I replied. 'Only one question remains: how could you ascertain which of the destructions of Troy was the work of Agamemnon and his accomplices?'

'Ah, that is another question, and one which may never be answered with certainty. Perhaps some day, with the aid of radiocarbon dating, we shall be able to say for certain which of the Trojan crimes was committed in the 12th century, and therefore which was the work of the Agamemnon crew. For now we can only say that, with so many to choose from, one of them must surely be the right one.

'There is, however, one telltale clue. When I bent to examine the fallen walls at Level VI, my eye fell on a few fragments of broken Minyan pottery. Now, you have often heard me discourse on the deduction of a man's trade from small but revealing signs; the recognition of a weaver by his tooth or a compositor by his thumb (Conan Doyle 1953, 363). In just such a way, I am able to tell prehistoric peoples by their pottery.

'I knew that Agamemnon and his fellows were Achaeans, and I knew that Achaeans made Minyan pottery, because I found some of it when I was investigating their activities at Mycenae last year. When I found a few pieces of the same pottery at Troy, then, it seemed probable that the culprits had brought it with them, and that some one of them in his haste had dropped his wine goblet.'

'Heinrich,' I said, 'there can be no doubt that you have averted a grave historical scandal, and have saved from ruin one of the most honored names in literature.'

The case of the fugitive craftsman[8]

'Sir Arthur, it was simply splendid the way you traced those fugitives from Egypt to Crete by the traces they left in the arts and crafts of the island.'

'Well, it was really not difficult. There is no doubt of Egyptian influence in Cretan art around 3200 BC, and when we find Egyptian influence we find Egyptians. As our good friend Herodotus has long since remarked (Histories, Book II, Sec. 35), who else can think like an Egyptian?'

'But to realize that they were the very fugitives whom Menes was seeking?'

'It was surely not a farfetched supposition. Manetho told us that Menes had unified Egypt by military conquest, and we have already established that it can only have been some time between, say, 3500 and 2500 BC. Now, Manetho said nothing about it, but as men of the world we know that one does not unify a country without making enemies. I therefore deduced that there would have been, in the time of Menes, certain Egyptians with the strongest possible motives for wishing to leave the country. When I find at the same time, or within a few centuries of it, Egyptians actually showing up in Crete, I have no hesitation in saying that these were none other than the refugees from Menes' campaigns.'

[8] For the background of this case see Childe 1964, 19; Clark 1965, 134-135; Hutchinson 1962, 53; Pendlebury 1965, 54.

'But how could they escape?' I demanded. 'Surely the Egyptians of those days had not the vessels nor the navigational skills to undertake the voyage to Crete.'

'Of course not; it was the Cretans who were the seafarers. But Cretan traders were always lurking about the coasts of the eastern Mediterranean, and it is not improbable that a Cretan ship arrived off the shores of Egypt providentially in time to rescue the beleaguered enemies of Menes.'

'Why should the Cretan sailors welcome aboard a disorderly rabble of fugitives?'

'Who knows? Perhaps they felt compassion for the sufferers. Perhaps they saw a chance to build up their own civilization at the expense of Egypt. Perhaps, like Cretans in all ages, they succumbed to the temptation of a bribe. Whatever the reason, we can only be sure that they laid the foundation of one of the world's great civilizations.'

The case of the wandering Semites[9]

'Sir Matthew,[10] how could you be so certain that civilization was brought to Egypt by Semitic invaders, and was not developed by the people who had been there all along?'

'Well, first of all there is the simple logic of the thing. The Predynastic Egyptians had lived in the country for several thousands of years without making any progress, so it was clear to me that they were a backward race with no capacity for the arts of civilization.

'Then there is the matter of language. When I came to examine the language of Pharaonic Egypt, I found that it contained both Semitic and Hamitic elements. Here, then, was clearly a case where a Semitic ruling class had imposed parts of its language on a subject Hamitic population.

'By this time, you see, I had already a pretty clear idea of how civilization got to Egypt. To be absolutely sure, however, I turned to my friends Grafton and Douglas in the C.I.D.,[11] and asked them to examine the skulls of the Predynastic and of the Dynastic Egyptians. Their findings bore me out absolutely. The skulls of the Predynastic Egyptians show the taint of the Negro; those of the Pharaonic nobility are the skulls of Semites.'

'One moment,' I interjected. 'Did you tell them beforehand which were the pre-dynastic and which were the Dynastic skulls?'

'Of course,' he rejoined testily. 'How else could you expect them to define the differences between the two?'

'What do you mean in saying that the skulls of the nobility were Semitic?'

'Well, I mean that they were of an eastern Mediterranean type, and of course we know that the eastern Mediterranean peoples included Semites.'

'What are the characteristics of the eastern Mediterranean skulls?'

'They are long-headed.'

'Are not the skulls of the Predynastic Egyptians also long-headed?' I objected. 'How do you tell them apart?'

'Ah, that is a difficult question. It is often easier to say that you know something than to say how you come to know it. There are certain subtle peculiarities, certain nuances. Individually perhaps they amount to little; collectively their weight is considerable.'

'Is that not a rather subjective procedure?'

[9] For the background of this case see Aldred 1961, 69-71; Clark 1965, 107-109; Deny 1956; Edwards 1964, 35-40; Emery 1961, 38; Kees 1961, 36.

[10] Sir Matthew [sic]; Sir William Matthew Flinders Petrie (1853-1942). The other individuals referred to in the author's archaeological detective stories are Heinrich Schliemann (1822-1890), Arthur John Evans (1851-1941) and Andrew Ellicott Douglass (1867-1962) [ed.].

[11] Criminal Investigation Department of the police, United Kingdom [ed.].

'My dear professor, intuition and inspiration are the essence of our profession. Without them we should be common drudges, like our friends at Scotland Yard.'

The day the ground shook[12]

'Sir Arthur,' I asked, 'what made you believe that the entire town of Knossos had been destroyed by an earthquake, when you had in fact only excavated a part of it?'

'Well, there was certainly a great deal of violent destruction in places.'

'What sort of destruction?'

'Walls all tumbled down, roofs fallen in, no effort to clean up the mess, and so on.'

'You encountered that sort of thing all over the site?'

'No, just in places, as I said. In other parts of the site I had to suppose that the debris had been cleared away in the course of later re-buildings.'

'But you assumed that the destruction had once engulfed every building at Knossos?'

'I could not imagine an earthquake damaging some of the buildings and leaving others intact.'

'What made you certain it was an earthquake?'

'Well, it stands to reason, does it not? Earthquakes are frequent and violent in the eastern Mediterranean, and at Knossos there was no evidence of military destruction at this time.'

'Had you any other reason for thinking that destruction was general over the whole site, even though you did not find evidence of it everywhere?'

'Yes indeed, and a good one. Wherever we did find evidence of destruction, we found also that a different kind of pottery ware was being used after the town was rebuilt. It was not a very big difference, mind you—not the sort of thing that would make you think of a new people coming in. Still, it was perceptible, and it was consistent. Now, we found that same pottery change all over the site, wherever we dug. Since the pottery change was clearly, in my mind, associated with the destruction, I argued that wherever we found the pottery change, there also must have been destruction. Do you follow me?'

'I think so.'

The case of the empty house[13]

'Andrew,' I said, 'You have undoubtedly saved the Athabascans from the noose. Had it not been for your intervention they would surely have been convicted of driving the Anasazi from their homes. How did you discover that a drought was really responsible?'

'Well, of course the alibi of the Athabascans, that they were still in the north at the time of the crime, was weak, and could not be proven since nobody saw them. Still, the case against them was far from conclusive. I had to consider the possibility that there might have been other reasons which would induce the Anasazi to leave their homes. I began, then, systematically reviewing the alternatives. When my examination of the tree-rings showed me that there had been 23 years of continuous low rainfall just at the time when the Anasazi were leaving, I felt that my faith in the Athabascans was vindicated.'

'Why should low rainfall drive them out of their homes?' I objected.

'My dear professor, it is surely obvious that they had to go in search of water.'

'But was not the drought general over the whole region?'

'Yes, and I confess that that puzzled me for a time. However, I soon perceived that a good many of the Anasazi had gone to the Rio Grande Valley, and so it became clear to me that they had been obliged to seek a permanent stream.'

[12] For the background of this case see Childe 1964, 21-27; Hutchinson 1962, 137-98; Pendlebury 1954; Pendlebury 1965, 146-149; Weinberg 1965, 301-312.

[13] For the background of this case see Jennings 1968, 276-279; McGregor 1965, 401-403; Willey 1966, 210-211.

'But are there not permanent streams closer at hand—the San Juan and the Dolores and the La Plata? What about them?'

'My dear professor, I feel that you ask too much when you expect me to enlighten every corner of what still remains in some ways a mystery. I can only be sure that many of them did go to the Rio Grande, which you must acknowledge is indeed a permanent stream.'

'Did the Anasazi all go to the Rio Grande?' I inquired.

'No, some of them went to other canyons. There must have been special factors operating there, which we shall probably never know.'

'You have not yet explained what became of the Kayenta,' I remarked.

'Ah, yes, they moved to the other end of Black Mesa and became the Hopi,' he replied.

'But is that not an environment essentially similar to the one they left?'

'Yes, it is, and that continues to puzzle me. However, you must have observed that there are sand dunes there which retain moisture for a long time, and it is in fact in these dunes that the Hopi do most of their farming.'

'Do you think they moved there for the sake of the sand dunes?' I asked. 'Why didn't they go to the lower Chinle Valley, which is practically nothing but sand dunes?'

'I fear you ask more than I can answer.'

'Andrew,' I cried, 'I have an inspiration. Forgive my temerity, but is it not possible that these people, who had been foregathering in larger and larger groups because of the demands of their social and ritual life, simply found that they had exhausted the possibilities of their environment in this direction? Is it not possible that they were obliged to shift over from milpa to floodplain agriculture in order to support their nucleated settlements, and so they had to come down out of the mesas and onto the floodplains? This would have happened even if there had not been a drought; after all, it was really soil and not water which was in short supply.'

'What makes you say that?' he asked sharply.

'Why, I continued, 'they had been impounding soil in little terraces all over the mesas and canyons in the 12th and 13th centuries. We found a lot of them in the side canyons just upstream from the San Juan river (cf. Lindsay 1961) but not on the river itself, so we knew it was soil and not water that they were concerned about.'

'Brilliant, my dear professor,' he exclaimed sardonically. 'I must congratulate you on a most ingenious theory. There is really only one flaw in it, and that is that you have put the cart before the horse.'

'What do you mean?' I protested.

'Complex social and ritual institutions are the effects, not the cause, of nucleated settlements. Growth and complexity are the consequences, initially, of material and technological progress. You cannot have social progress in advance of economic progress.'

'Andrew,' I said, 'I can see that you are as dedicated a materialist as Marx himself. You will not believe that an Anasazi farmer might be willing to change his way of life, or at least to walk a little farther to his field each day, in order to gain the advantages of town life. How can you possibly be so sure about this?'

'Because, my dear professor, there are certain things in this world that even a skeptic should not question: one must have faith in something. I place my faith in Rational Man.'

'But must Rational Man always be Economic Man? Is the pursuit of comfort and material gain the only form of rational behavior?'

'In our civilization, yes. The doctrine of economic determination is one of our most sacred tenets; it lies at the root of our whole theory of cultural evolution; we could not possibly dispense with it. Still, it is a pity. One would prefer to believe man capable of higher things.'

'Well, I am not sure he is not,' I replied, 'but perhaps the higher things are only different ways of seeing his own best interest. Are there not, at any rate, large religious communities in Europe and the Near East that are drawn together in spite of environmental disadvantages by their desire for a fuller ritual life, and that have suffered material and physical privation rather than forego it?'

'My dear professor, I really cannot be responsible for what happens in the Old World; I have quite enough to do in my own hemisphere. That is the province of the philologist and the classicist; let them explain, if they can, what they find in their own gardens.' 'Well,' I said, 'at any rate the Athabascans would seem to be exonerated.'

'Quite so,' said he.

* * * *

There are of course problems of fact as well as of interpretation in each of these cases. Under more critical examination, their protagonists would surely suffer the same discomfiture as Sherlock Holmes. Foregoing such a leisurely procedure here, it will be sufficient to identify a few common deductive errors which these classic 'solutions' exemplify, and which are widely prevalent in archaeological explanation.

1. *The argument from pottery to people.*

So much historical theory rests upon this one proposition that to cast doubt upon its validity almost amounts to heresy. Nevertheless, its probability value is high only for Neolithic-type societies, in which pottery making is a part-time domestic craft of the womenfolk. With the coming of the potter's wheel, the craft very soon becomes industrialized and passes into the hands of small groups of highly specialized artisans. Decorated wares, in particular, are mass-produced and widely, even internationally, traded from a few centers. In these circumstances given wares may be closely associated with given factories, but rarely with specific ethnic groups or nationalities. Pottery distributions in the Bronze Age are evidence of trade networks, not of population movements. Moreover, they develop and change in response to competitive demands and market conditions which have little to do with other aspects of culture or art. Commercially successful wares are certain to be widely imitated.

This *caveat* surely applies to the equation of the factory-made Minyan Ware with the coming of the Achaean Greeks (cf. 'The Case of the Wooden Horse'). Minyan Ware appears more or less simultaneously at several points in the Aegean, apparently following on the destruction of major sites (Childe 1964, 73-77). Destruction of such a wholesale nature, if it really took place (see below), would surely have disrupted any existing long-range trade networks, and have opened the door to the development of new ones. Even if it was the Achaeans who destroyed the early Aegean sites (a proposition which still rests largely on our interpretation of Homer), it was not necessarily they who spread Minyan pottery around. Their antecedents do not suggest that they would have had the time or the skill to build up their sedentary industries to such an extent. More probably some other and more sophisticated group of potters took advantage of the trade vacuum created by the Achaean piracies to build up a market for their own product. At this point we have to find out where Minyan Ware was actually made before we can begin speaking about the identity of its makers.[14]

2. *The argument from stratigraphy to military destruction.*

An initial fallacy here lies in the equation of general destruction with violent destruction. Combat activity,

[14] For further discussion of this point see Adams 1968, 202.

no matter how violent, is essentially uncoordinated, and, once inside the fortifications, its primary thrust is directed at personnel rather than at installations. Even after the cessation of hostilities, the types of destruction which occur during the initial phase of pillage are sporadic and limited. Some of the most celebrated captures and sacks in history, such as those of Rome (AD 410), Baghdad (1258), and Constantinople (1453), probably left little or nothing in the stratigraphic record by which an archaeologist of today could recognize the tremendous military and political significance of those events.

In contrast to the type of random destruction usually resulting from military activity, the complete and systematic destruction encountered at Hissarlik ('The Case of the Wooden Horse') and Knossos ('The Day the Ground Shook') is evidence par excellence of organized, determined, and coordinated effort. Where a human agency (rather than fire or earthquake) is suspected, the real deductive choice is between voluntary 'urban renewal' on the one hand and a calculated act of retribution by a victorious enemy on the other. We must recognize, though, that in either case the evidence will look about the same once the town has been rebuilt. It takes a good deal of violent and coordinated effort to push over a brick or stone wall, whatever your motives.

Considering the regularity with which layered stratigraphy is encountered in the Near East, and the infrequency of total leveling in recorded cases of military conquest, we conclude that 'urban renewal' is a much more probable explanation of stratification than is enemy activity in the great majority of cases (cf. Adams 1968, 207-208). We should, in fact, probably never attribute mere leveling to enemy activity without strong supporting evidence such as widespread fire, defaced monuments, or above all, the presence of *corpora delicti*.

3. *The assumption of causality from concurrence.*

The inflated probability values common in archaeological explanation are nowhere more apparent than in the assumption that events roughly contemporaneous in time must somehow be historically related. This is a patent logical absurdity: how many historical events in any society at any given time are in fact causally related—particularly if they are purely local in occurrence?

The interpretation of Egyptian influence in Crete ('The Case of the Fugitive Craftsmen') clearly illustrates this fallacy. Here, as in a great many similar cases, the fact of contemporaneity itself is far from clear (see below). Even if we accept it for the sake of argument, and if we further accept the historicity of Menes' campaigns (about which we are told nothing except that they happened), how much logical basis have we for assuming that there is a causal connection between a given campaign in Egypt and the influence of Egyptian art in Crete? The record of emigration from Egypt suggests that there have always been good and sufficient reasons for wanting to leave that somewhat oppressive climate, and there have usually been greener pastures abroad. And why should the erstwhile fugitives from Menes' vengeance have included artisans and craftsmen—the very people who depended most on royal patronage for their well-being both in this world and in the next?

As further proof of logical fallacy in this particular case, we may note that the artistic traditions characteristic of Pharaonic Egypt are traced from Egypt to Crete at the actual moment of inception of the Pharaonic state. In fact, however, the type of Egyptian artistic influence which is first perceived in Crete is not even manifest in Egypt itself in the time of Menes, or in fact throughout most of the Archaic period.

Here is a case where ingenuity has clearly triumphed over probability. It illustrates, as does a great deal of archaeological explanation, that passion for tying together as many loose ends as possible which is also a conspicuous feature of detective fiction.

4. *The unfounded assumption of concurrence.*

Here we encounter further evidence of a passion for tying up loose ends: the tendency for dates which fall within the same general range of time to assimilate to each other in order to facilitate explanation. As in the case of the Egyptians in Crete ('The Case of the Fugitive Craftsmen'), it will usually be found that assumptions of contemporaneity are used in conjunction with assumptions of causality to build a circular chain of probability. The notion of contemporaneity is necessary to buttress the notion of causality, and vice versa. As the logician knows, this is an argument that you cannot have both ways.

Quite aside from the problem of causality, let us consider simply the chances of temporal coincidence between the campaigns of Menes on the one hand and the appearance of Egyptian artistic influence in Crete on the other. Menes' accession is conventionally put at around 3200 BC, but it could in fact have fallen anywhere between outside limits of, say, 3400 and 3000 BC (Emery 1961, 28). The date of the first Egyptian influence in Crete is similarly imprecise, but can be fixed roughly within the same limits (Hutchinson 1962, 53). The longest reign ascribed to Menes is 62 years (Emery 1961, 29): although almost certainly exaggerated we will have to accept it for the sake of argument. We will also assume that his campaigns of unification might have occurred at any time during his reign, although logically they ought to have occurred at the beginning. We have then a span of 62 years, falling some time between 3400 and 3000 BC, during which the fugitive Egyptian artisans might have decamped for Crete.

We must further assume that, once arrived in Crete, it might have been some years before they began to practice their trades, and still longer before their influence became at all apparent. It must nevertheless have been within their own lifetimes, so we can perhaps set a further limit of 40 years on the time which might have elapsed between their arrival in Crete and the visible establishment of their influence there. If, then, we allow for the least probable assumption—that the craftsmen fled Egypt during the last year of Menes' reign, and that their influence in Crete did not become apparent until they were on their deathbeds—we have a time span of just about 100 years (the 62 years of Menes' reign plus the 40 years of their residence in Crete) during which a causal relationship between Menes' campaigns and Egyptian influence in Crete is a logical possibility.

The rest is a matter of simple mathematical probability, or lack of it. If either event could have happened any time within a period of 400 years, the chance of their both happening even within the same century is only 1 in 4. This is the most liberal probability figure which can be allowed mathematically. There are, however, practical factors which greatly reduce even this small likelihood, such as the probability that Menes' campaigns were confined to a short period at the outset of his reign, and that his reign itself was shorter than the 62 years recorded by Manetho. In sum, the idea that Menes' unification of Egypt was responsible for an emigration of Egyptians to Crete, with consequent influences in Cretan art, is an ingenious suggestion with a remote mathematical possibility of being correct.

Similar false assumptions of coincidence are involved in the hypothetical destruction of Troy by the Achaean Greeks ('The Case of the Wooden Horse'). Here, too, they are part of a logical circle of concurrence and causality.

5. *The confusion of race, language, and culture.*

This is of course a legacy of the primitive world-view: the idea that each people has gone forth complete with its appropriate language and culture from the hands of its creator. Anthropologists are nominally proof against such simplistic equations, but a surprising number of them are still with us in spite of our best efforts. Anyone who has attempted to sort out the proto-history of Mesopotamia in evolutionary terms must have despaired at the indiscriminate juggling of racial, linguistic, archaeological, and historical evidence which has been practiced by scholars in that area.[15]

The case of Egypt is much simpler than that of Mesopotamia, and therefore easier to discuss here.

[15] For a recent example see Woolley 1965, 54-65.

Only one race-language-and-culture migration is invoked to account for cultural change, at the outset of the Dynastic period. The supporting racial, linguistic, and cultural evidence has been cited earlier ('The Case of the Wandering Semites'). A little clear thought will show that the different categories of evidence do not in fact reinforce each other, because none of them necessarily implies the others. Moreover, the evidence in each category is subject to alternate explanation. The racial-cultural argument of limited capacity for civilization on the part of the Predynastic Egyptians is of course an unacceptable one to anyone but a racist, since it rules out the demonstrated possibility of diffusion in history. The idea that the Semitic elements in the Egyptian language are evidence of a Semitic increment to the population is likewise unacceptable, ignoring as it does the genetic relationship between Egyptian and Semitic and the fact that Egyptians were in geographical contact with Semites for thousands of years. Again, linguistic diffusion is arbitrarily and unjustifiably ruled out. The argument for a difference in skulls between the Predynastic and Dynastic Egyptians, if true, is of course evidence of the coming of a new group, but does not prove that they added anything to the established language and culture of the country. The Roman Empire in its time absorbed thousands of barbarians whose skulls can be distinguished from those of the Italians, but who contributed nothing to the civilization or the language of the realm.

The handling of anatomical evidence from Egypt is not without its disturbing overtones. A recent re-examination of Predynastic and Dynastic skulls has made it pretty clear that there is really no significant difference between them (Berry *et al*. 1967). This experience has been repeated in Nubia (Mukherjee *et al.* 1955, 73-92; Greene 1967, 57) and, a generation ago, in the American Southwest (Seltzer 1944, 32-33), where successive populations which were once thought to be genetically distinct have proven on re-examination to be the same. There is no escaping the conviction that the methods of the early comparative anatomists were so subjective and intuitive that they could find in their material confirmation for whatever they wished to believe. In the case of both Egypt and Nubia it is perhaps sufficient to add that the original analysis was carried out very largely by Elliot Smith.[16]

6. *Excessive materialism***.**

This is a charge to which American archaeologists are much more liable than are their Old World colleagues, reflecting, I suppose, the dominant values of contemporary American life. If anything, it has been aggravated by the recent and sometimes exaggerated preoccupation with cultural ecology. Since I have already voiced my objections through the mouth of the mythical 'Professor Watson' ('The Case of the Empty Houses'), no further elaboration of the point is required.

* * * * *

The recurrent thread which runs through all of the deductive fallacies so far discussed is that of circularity. It seems that archaeologists, who are obliged in their work to deal with nothing but probabilities, do not understand the central theory of probability. Along with much of the rest of mankind, they seem to believe that the probability of a whole string of assumptions being true is equal to the sum, rather than to the product, of their individual probabilities. (Since probabilities are fractions, their product is of course less than any individual probability, whereas the sum is greater). Individually improbable statements are believed to reinforce each other when taken in conjunction.

The five 'cases' which have formed the nucleus of this discussion all date back at least 50 years. The highly trained field worker of today may perhaps smile at these clumsy efforts of our youth, secure in the conviction that we have long since outgrown such naive credulity. It is true that sweeping generalizations

[16] Grafton Elliot Smith (1871-1937). See further Todd 1937; Waldron 2000 [ed.].

and simplistic deductions are characteristic of the formative stages of every science, when solid facts are few and the imagination is obliged to substitute for empirical evidence. Nothing has been said or implied here about archaeological theory which could not be said with equal justice about the early theories of biologists, geologists, physicists, or what have you.

We could, then, simply make fun of our early follies, except that they are still around to haunt us. Every one of the five 'explanations' which have been considered here will be found solemnly repeated in books printed within the last decade; books not only by popularizers and laymen but by reputable professional archaeologists.[17] The changing demands of the academic and literary marketplace have recently produced among archaeologists an eclecticism that was once conspicuously lacking. Many of us are wandering for the first time beyond familiar pastures. Once off our home ground, we are often continuing to accept the 'detective' fantasies of yesteryear.

Our infancy does not seem to be entirely behind us yet. Why, then, do we remain theoretically naive, when our sister disciplines (including ethnology) have been casting off the mythology of their childhood? My own feeling is that, like Sherlock Holmes, we have been putting too much of our effort and emphasis in the wrong place: on observations rather than deduction. Our graduate training programs for archaeologists are more heavily vocational and methodological than those in any other human science. As a result our highly trained archaeologist (i.e. field technician) may be a very poor anthropologist, and no historian at all.

This last point deserves a moment's reflection. Archaeology involves the reconstruction of culture patterns and culture processes of the past, but it also and abundantly involves the reconstruction of particular events. The study of particular events, *per se*, is the province of the historian rather than of the anthropologist. The archaeologist, however, cannot avoid them, and in practice he is therefore apt to be as much historian as anthropologist, whatever his inclinations may be.

Should not the archaeologist, then, be obliged to study history for the same reason that he is now obliged to study anthropology: to provide him with deductive propositions in his work? When he finds a certain type of fetish and deduces from it the presence of a particular religious cult, he justifies the deduction not on abstract logic but on parallel cases and association drawn from the ethnographic literature. When he similarly deduces from material evidence a particular historical event (say, the coming of a new people as evidenced by a new pottery type), should we not be entitled to ask him how often in recorded history such a thing is known to have happened? Under present circumstances, the chances are that he will not even ask himself the question.

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[17] It will be noticed that, with two exceptions, the works which I have cited as background sources for the five 'cases' discussed in this article have all been published or re-published in the 1960s.

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