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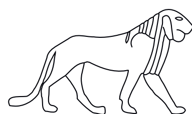
DOWN TO EARTH ARCHAEOLOGY

WILLIAM Y. ADAMS

EDITED BY JULIE R. ANDERSON



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Cover: Professor William Y. Adams documenting the medieval pottery kilns at Faras in 1960 (photo SARS Adams archive ADA).

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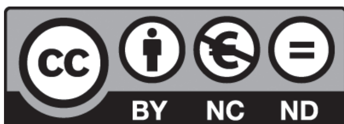

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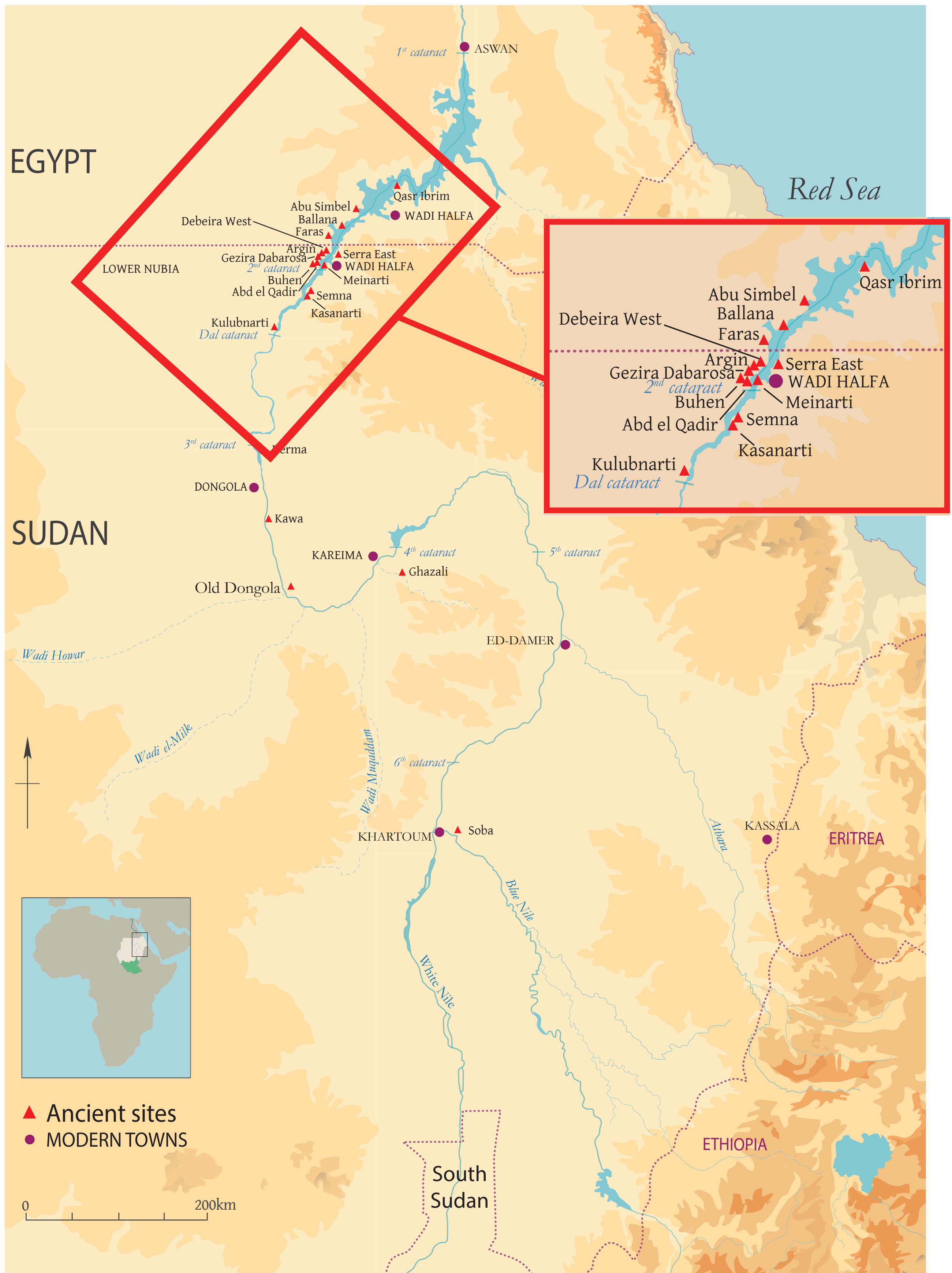

Map with places in Sudan mentioned in the volume.

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),

[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

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[6] See Plate ii [ed.].

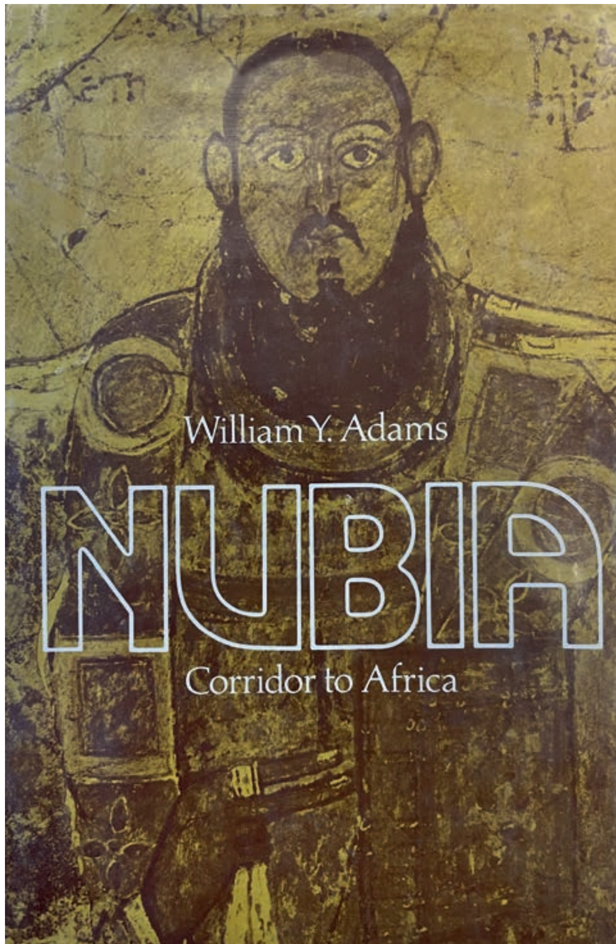

Plate ii. Cover, W. Y. Adams. *Nubia Corridor to Africa.* Princeton (1977).

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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