

# SUDAN & NUBIA

The Sudan Archaeological Research Society



*Bulletin No. 17*

*2013*





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Front cover: The descendency of Tomb IV T 1 near Sedeinga under excavation (© V. Francigny / SEDAU).

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## From Nubia to Arizona – and Back; or, Reisner comes Home

*William Y. Adams*

In 1908, George A. Reisner (Plate 1) was a well established and respected figure in the field of Egyptology, having conducted excavations for a decade on the Giza Plateau. His field methods, inspired by the earlier work of Flinders Petrie, were considered to be models for their time, with insistence on the precise recording of everything found. Reisner was also, in that year, just at the start of his long and illustrious career in Nubia and the Sudan. He had just completed the first year of the Archaeological Survey of Nubia, a project which he designed and initially directed (published in Reisner 1910). In the Nubia survey he encountered for the first time the remains of non-Egyptian peoples, and was forced to think about cultural determinants and cultural relationships.

Reisner must have had archaeological methodology much on his mind when in 1909 he returned for one of his



*Plate 1. George A. Reisner.*

infrequent trips to the United States. In that year he taught a course on the subject at Harvard University, where he was an adjunct professor. In one of the happy accidents of history, the students in the class included 24-year-old Alfred Vincent Kidder (Plate 2), destined in his own time to become the dean of both North American and Mesoamerican archaeology.

At this point in time Kidder's only experience in archaeology had been a season of what passed for survey in the American Southwest, for which he received no training at all

(Givens 1992, 11-28). North American archeology was then in what Thomas Kuhn (1970) would call a pre-paradigmatic stage, much like Egyptology half a century earlier—it had not yet decided on what questions to ask. Methodology in the Southwest consisted of little more than locating, drawing, and photographing the numerous ruined pueblos that were scattered all over the territory. They were all considered to be remains left by the same people (ancestors of the present-day Pueblo Indians), and not very old.

Reisner's course was a revelation to the young Kidder; he later described it as the most enjoyable class he ever took (Givens 1992, 25).

'Reisner explained the aims of archaeology and how to attack a problem, how to determine a culture's chronological relationships to other cultures and its trade contacts with its contemporaries, gave a lot of stratigraphic theory, recommended leaving test columns or sections for later checking, explained the proper disposal of backdirt, taught a classification of various kinds of debris, described details of cataloguing, and discussed 'the organization and house-keeping problems of an expedition. . . .'

(Wauchope 1965, 151).

Among the lessons learned by Kidder from Reisner was the importance of pottery types as cultural determinants, and, at least by implication, the importance of collecting potsherds. It was a consideration that was to dominate all of his later excavations in the Southwest. It was, in effect, the "tail wagging the dog" in his twelve-year excavation at Pecos Pueblo (1915-1929), the dig for which he first became



*Plate 2. Alfred Vincent Kidder (courtesy of Faith Kidder Fuller).*



famous (Givens 1992, 29-76). Kidder and his team spent more time and effort in trenching through the enormous, deeply stratified refuse deposits surrounding the pueblo than they did in digging the village itself. They were looking above all for ceramic sequences.

By the time of the Pecos dig, Kidder and a few of his colleagues had begun to recognize that the ruined pueblos were not all the same age; they represented a trajectory of development, of unknown length. For one thing, the pottery in the pueblos was not all the same. Some yielded sherds with black-on-white decoration, but no glazed wares; others produced glazes but not (at least on the surface), black-on-whites; a few had no sherds at all. (They became, and are still known, as Basket-maker sites.) An actual example of ceramic stratigraphy, involving a succession of four types, had been found in a site not far from Pecos (Nelson 1916). Kidder, more than anyone else, became fixated on the idea that once a ceramic sequence had been developed, it could be used to date, or at least to range in chronological order, all the ruined pueblos of the Southwest. Hence the concentration on the trash mounds at Pecos.

Kidder's hopes were achieved when, in 1927, he together with a few colleagues published a seven-stage periodization for the development of puebloan culture (Kidder 1927). The stages were marked by significant changes in architecture, *kivas* (ceremonial chambers), and other features, but the *sine qua non* for recognition in all cases was ceramic. The scheme was formulated in the course of an archaeological conference at Pecos, and has been known ever since as the Pecos Chronology. In later years it was found to contain a good many over-simplifications, but it remains a useful heuristic device, and as such is still widely cited.

Kidder however was no ceramicist. Pottery was only a means to an end, and he took little interest in wares which proved to have no chronological significance, nor did he offer a formal typology. It was left to others, at a much later date, to study and to publish in appropriate detail the pottery from Pecos (Shepard 1942). One of the analysts, Anna Shepard, went on in time to become one of the world's leading authorities on archeological ceramics (see especially Shepard 1956).

Among the many colleagues following Kidder's lead, it was Harold S. Colton (Plate 3) who was to carry ceramic description and classification to heights undreamed of by Kidder, and to wrest all kinds of new information from the sherds. Colton, independently wealthy, established the Museum of Northern Arizona (MNA) at Flagstaff in 1928. Its object was to sponsor all different kinds of scientific research in the upland Southwest, but the primary emphasis was always on archaeology. Colton, trained in zoology, did not feel competent to lead digs in person; over the years they were conducted, under MNA sponsorship, by a great many colleagues and subordinates, eventually including the present writer.

Although Colton did not dig, he was fascinated by the finds from the MNA digs, and above all the abundant and

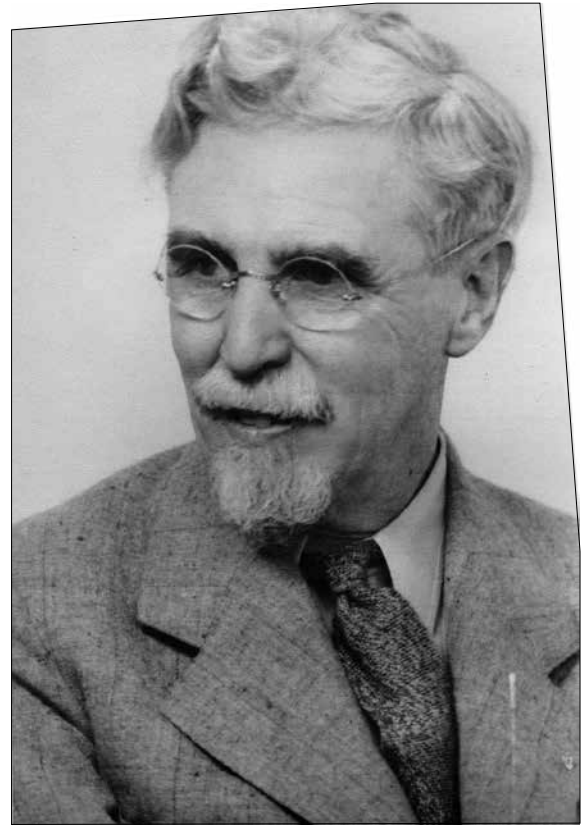


Plate 3. Harold S. Colton  
(Courtesy of the Museum of Northern Arizona).

highly varied pottery. Because the archaeological work ranged over a very large territory, including not only northern Arizona but parts of three adjoining states, the Museum's collections of sherds came to include a much greater variety of wares than did those of any other institutions. Colton, with the eye of a trained taxonomist, recognized that existing classifications were wholly inadequate to deal with such an abundance and variety of material. For the remainder of his life he devoted himself, not wholly but largely, to the task of classifying the Southwestern pottery wares in a properly scientific manner. In time he and a colleague produced the *Handbook of Northern Arizona Pottery Wares* (Colton and Hargrave 1937), which became and to a large extent remains the Bible of Southwestern ceramicists. The title is misleading, for the volume includes a great many pottery wares traded into northern Arizona, but made elsewhere.

Colton's analytical methodology began with a precise definition of each ware (designated by him as a type), including not only decorative designs if any, colors, and surface treatment, but also internal features of paste and temper. This proved to be important, because Southwestern potters borrowed designs widely from one another, and there are cases in which identically decorated wares from widely separated regions can be distinguished only by the use of different tempering material. Analysis of pigments also proved important, because potters in the most easterly puebloan region produced black designs using iron oxide, while further

west they used carbon.

The Colton classification made it possible to recognize for the first time regional variants. It had been realized for a good many years that prehistoric Puebloan culture at its highest stage of development had exhibited three somewhat different regional variants, as reflected primarily in architecture. They were presumed to be indicative of different but related population groups. It was Colton who demonstrated that these subcultures were reflected also in slightly differing pottery types. Thus, it became possible to identify a site as belonging to the Chaco, the Mesa Verde, or the Kayenta subculture even in the absence of architectural remains.

Following in the tradition of zoological taxonomy, in which he had been trained, Colton for the first time introduced a hierarchical dimension into his classification. On the basis of similarity or difference, types were grouped into wares, and wares into series. In the best tradition of biological taxonomy, this was based on internal characteristics exclusively, without reference to archaeological context.

The Museum of Northern Arizona was my own 'archaeological alma mater'. My first digs under its sponsorship were undertaken in the spring and summer of 1949. The work however was carried out in small, one-phase sites, in which dating was not an issue. It conformed to a pattern already well established throughout the region. My partner and I dutifully collected potsherds, but I am not certain that we ever sorted or tallied them.

It was otherwise when, in 1957, the Museum hired me to direct salvage operations in the canyon of the San Juan River, soon to be inundated by the Glen Canyon Dam. The adjacent highlands had been the scene of intensive archaeological activity for half a century (including a survey carried out by me for the Smithsonian Institution in 1952), but the canyon bottom was terra incognita. We all supposed, however, that any remains we found would be comparable in date to those on the mesas above.

During the season of 1957-58 my wife and I surveyed the full length of the threatened area, and recorded a total of 83 sites (Adams and Adams 1959). Only 33 were habitations, the remainder being rock pictures, lithic workshops and the like. The sites were all small, but potsherds were fairly abundant and clearly of puebloan type. We could deduce from the locations and the very insubstantial structure of the buildings that they were no more than summer farming colonies, 'spun off' from the much larger pueblos on the nearby highlands.

The sherds nevertheless presented two unexpected surprises. The first was that the entire occupation within the canyon had taken place in the interval of years from about AD 1050 to 1200, even though occupation at the "mother pueblos" had begun much earlier and continued later. (This has subsequently been found to be true throughout the northern periphery of the Southwest, and still remains unexplained.) The second was that while the south side of the river had been occupied by people of the Kayenta subculture, the north side had been occupied by the Mesa Verde subculture.

This despite the fact that the river is no kind of a barrier; during the low-water season it is quite easy to wade across. This remains the only instance familiar to me of an apparently negotiated boundary in the Southwest. The important point to note is that these conclusions were derived entirely from the study of potsherds; the structural remains were far too insubstantial to present any chronological or cultural diagnostics. So unexpected were they that I had to check and recheck my sherd collections against the descriptions given in the Northern Arizona handbook, and I paid far more detailed attention to pottery identification than I had ever done before.

Such was the situation when, in 1959, I came to the Sudan to begin salvage operations in Nubia. I brought with me very little in the way of background that was useful in the Nile Valley, but I did bring a conviction of the value of fine-tuned ceramic analysis as a basis for site identification and dating. But I was not in any sense a 'ceramic junkie' at this time.

By another of the nice accidents of history, the very first site I encountered, when we began on-the-ground survey in Nubia, was a ruined pottery factory where, a millennium earlier, some of the most elaborate wares of the Christian Nubian period had been produced. The site had already been excavated in 1911-12 by the Oxford University Expedition (Griffith 1926, 63-5), but in the intervening years it had once again blown so full of sand that only the wall tops were showing.

After the lapse of half a century I cannot remember for sure what it was that drew me to this already-excavated site, to which I devoted the whole of my first Nubian excavation season (Adams 1961). The published description (Griffith 1926, 63-5) was superficial in the extreme, and some instinct must have told me that the Faras Potteries still had a lot of information to yield.

Indeed they did. I soon found that the previous excavations had been very limited, in considerable areas clearing only the wall tops, for mapping purposes. In few if any areas did they seem to have reached the original floors, for here and there I found sizable caches of partly-completed vessels, some thrown but not yet painted, others painted but not yet fired. I found also that the buildings had a long and complicated history, with many rebuildings not noted by my predecessor (for the fullest published account see Adams 2005, 71-89). Most importantly of all, as it turned out, I found that the earliest pottery wares produced at the site were vastly different from the latest – so much so that, in the heyday of migrationist theory, they would probably have been attributed to different peoples (see Adams 1977, 4-5). The earliest wares, which I designated Early Christian, were overwhelmingly red, with simple or no decoration. The latest ones, which I called Classic Christian, were mostly white or cream, with highly elaborate decoration in black and/or red.

So I had, at the conclusion of my first season, the beginnings of a ceramic sequence. Many of the pottery types I found had not previously been described, and in the process of writing up the season's dig I found myself obliged, for the



first time, to write my own ware descriptions. (Throughout my Nile Valley work I was obliged to use the term ‘ware’ rather than ‘type’ to designate minimum units of classification, because other archaeologists in the area were using ‘type’ exclusively to designate vessel forms.)

My interest was sufficiently piqued so that, during the following season, I dug three more pottery-making sites. One, kindly ceded to me by the Scandinavian Joint Expedition, in whose concession it lay, had been used exclusively in the production of the later ‘X-Group’ wares, already well known from cemetery excavations. Also represented were a few wares never found in graves, which I took to be the earliest Nubian pottery from Christian times. I called them Transitional wares. A second site produced very early Christian wares, which filled in the gap between the X-Group finds and the earliest Faras finds from the previous season. (For these digs see Adams 1962b, 62-75).

After two seasons I had, through sheer luck, a 500-year ceramic sequence, without benefit of any of the tedious trenching required by Kidder. At this point I felt emboldened to publish my first ‘Introductory Classification of Christian Nubian Pottery’ (Adams 1962a), embracing 27 wares. I was now inescapably hooked on pottery, though it always remained for me, as for Kidder, a means to an end rather than an end in itself.

During the next two seasons I investigated a number of Late Christian sites. The pottery was obviously different from anything in my sequence – overwhelmingly orange – but I could not find any way to connect it to my sequence until, in 1963-4, the great stratified mound of Meinarti ‘fell into my lap’. There, painstaking excavation of its eighteen occupation levels, all loaded with potsherds, provided me with an unbroken sequence of pottery development from late Meroitic to Terminal Christian times, a span of 1400 years (see Adams 1964, 222-50; 1965b).

My sequence was now complete, for the Christian period, and I was ready to bring out my own version of the Pecos Chronology, which I called ‘The Seven Ages of Christian Nubia’ (Adams 1964, 241-7). (Note that the Pecos Chronology also has seven stages!) The stages in my scheme were marked by differences of house type, church type, settlement size and site distribution, but the diagnostic as always was pottery. (I have since subdivided my Late Christian period into two phases, so that the scheme now encompasses eight ‘ages.’) There had been, up to this time, no attempt at an internal periodization of the Christian period; the whole 900-year interval was treated as unitary. A site was either Christian, or it was not. Authors since 1964 have regularly availed themselves of the ‘Adams chronology,’ at least to the extent of designating a site as Early, Classic, or Late Christian.

I updated my pottery classification in 1968, with the addition of the later wares (Adams 1968), but this like its predecessor was a journal article in which descriptions and illustrations were necessarily abbreviated. It was not until 1986 that I was able to extort the necessary funds from the

University of Kentucky (by threatening to take a job that had been offered at a rival institution) for a complete, formal publication. The two-volume *Ceramic Industries of Medieval Nubia* (Adams 1986) has minutely detailed descriptions as well as illustrations of 106 Nubian and imported wares, as well as 640 vessel forms and over 2600 decorative designs.

My methodology in describing wares is in all its essentials the same as that of Colton, who was my acknowledged inspiration. I did, however, have to introduce one additional dimension of variability, for my collections included both hand-modeled and wheel-thrown wares – the latter unknown in native America. I also made use of two descriptive tools not available to, or not used by, Colton, the Munsell color scale and the Mohs’ hardness scale.

In the hierarchical dimension of my classification, wares are grouped into ware groups and these in turn into families. This usage departs from that of Colton by tying the ware groups more specifically to chronological periods, and families to places of manufacture. This latter feature is possible because medieval Nubian (and Egyptian) wheel-thrown pottery had been mass-produced at a small number of factories, sometimes specifically identifiable, while prehistoric American pottery had been locally made by women in every village. I believed, and still believe, that this was also true of the Nubian hand-made wares, which remain impossible to localize.

Before my arrival in Nubia the only aspect of Christian Nubian archaeology that had received significant attention was church architecture. Every sizable village had had at least one church, and some had several. Being generally well built, and carefully maintained during their lifetimes, they were commonly the best preserved buildings in their villages.

In the earlier years of the 20<sup>th</sup> century there had been already four major surveys of the Nubian churches (Mileham 1910; Clarke 1912; Griffith 1926-28; Monneret de Villard 1935). These authors had noted a considerably variety of building types, but, lacking any chronological key, they were forced to treat them as so much synchronous variability. Only Clarke (1912, 31-2) had attempted a rudimentary classification – into three types.

So much simultaneous variability seemed inherently unlikely, given the well marked trajectory of development in European and Near Eastern church architecture, and in the West Bank survey, where I investigated half a dozen churches, I began to recognize clear evidence of chronology. My feeling was reinforced when, at Meinarti, I found actual superposition of church types (Adams 2003, 7-40). Seeing another chance to bring some order into medieval Nubian archaeology, I then compiled a complete list of all the known churches in Nubia, and sent questionnaires to colleagues for information about others that had not yet been published. When I matched the church plans against the available evidence of associated potsherds, a chronology and typology simply ‘jumped out at me.’ I published it initially in 1965 (Adams 1965a), and much more fully in Adams 2009, where I have been able to include



a good many additional churches. These are of course quite a few buildings for which no potsherd evidence is available, but the scheme holds so well that I now feel confident in dating buildings on typological grounds alone. The scheme as it now stands comprises 14 church types, in five chronological subdivisions.

There you have it. My lasting (I hope) contributions to Christian Nubian archaeology consist of a pottery typology, a church typology, and a periodization. In one way or another all of them owe their inspiration directly or indirectly to Kidder, who in turn owed his inspiration to that class at Harvard in 1909. When I came to work in Nubia half a century later, Reisner came home with me!

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## Gabati

A Meroitic, Post-Meroitic and Medieval  
Cemetery in Central Sudan.

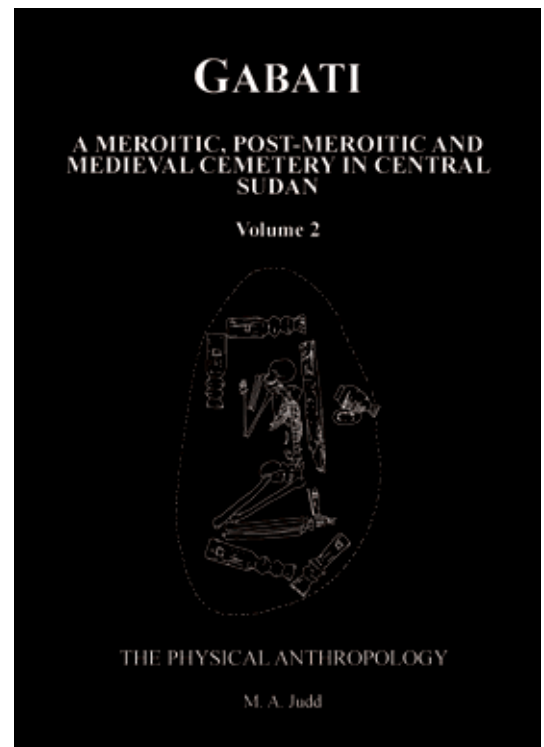
Vol. 2: The Physical Anthropology

by Margaret A. Judd,  
with a contribution by David N. Edwards  
London 2012

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## Sudan's First Railway The Gordon Relief Expedition and The Dongola Campaign

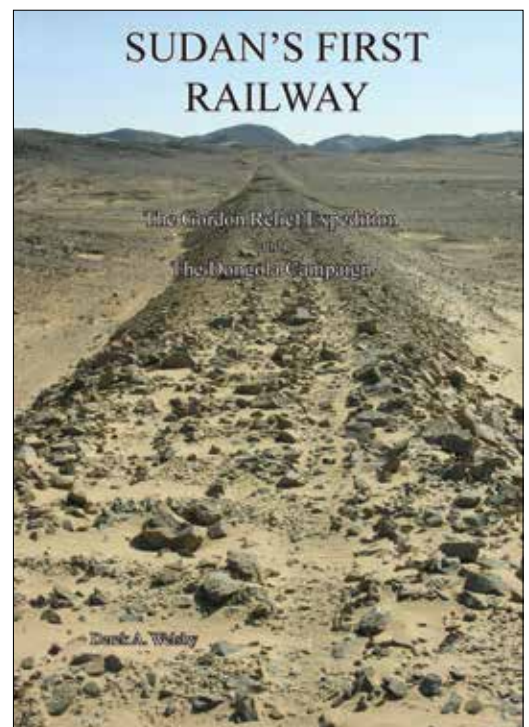
by Derek A. Welsby

London 2011

*149 pages, 6 tables, 47 figures, 173 colour and 19 b&w plates*  
ISBN 978 1 901169 1 89

Begun in 1875 by the Egyptian khedive, Ismail Pasha, the railway played an important role during the Gordon Relief Expedition of 1884-5 and Kitchener's Dongola Campaign in 1896. It was abandoned and cannibalised to build other railways in Sudan during the first decade of the 20<sup>th</sup> century. For much of its course it runs through the desert and in those areas the roadbed, the associated military installations and the innumerable construction camps are extremely well preserved. This book is the result of a photographic survey of these installations together with the detailed archaeological surveys undertaken within them. A report on the artefacts, which includes personal equipment, ammunition, fragments of rolling stock, bottles, tins and ceramics, completes the volume.

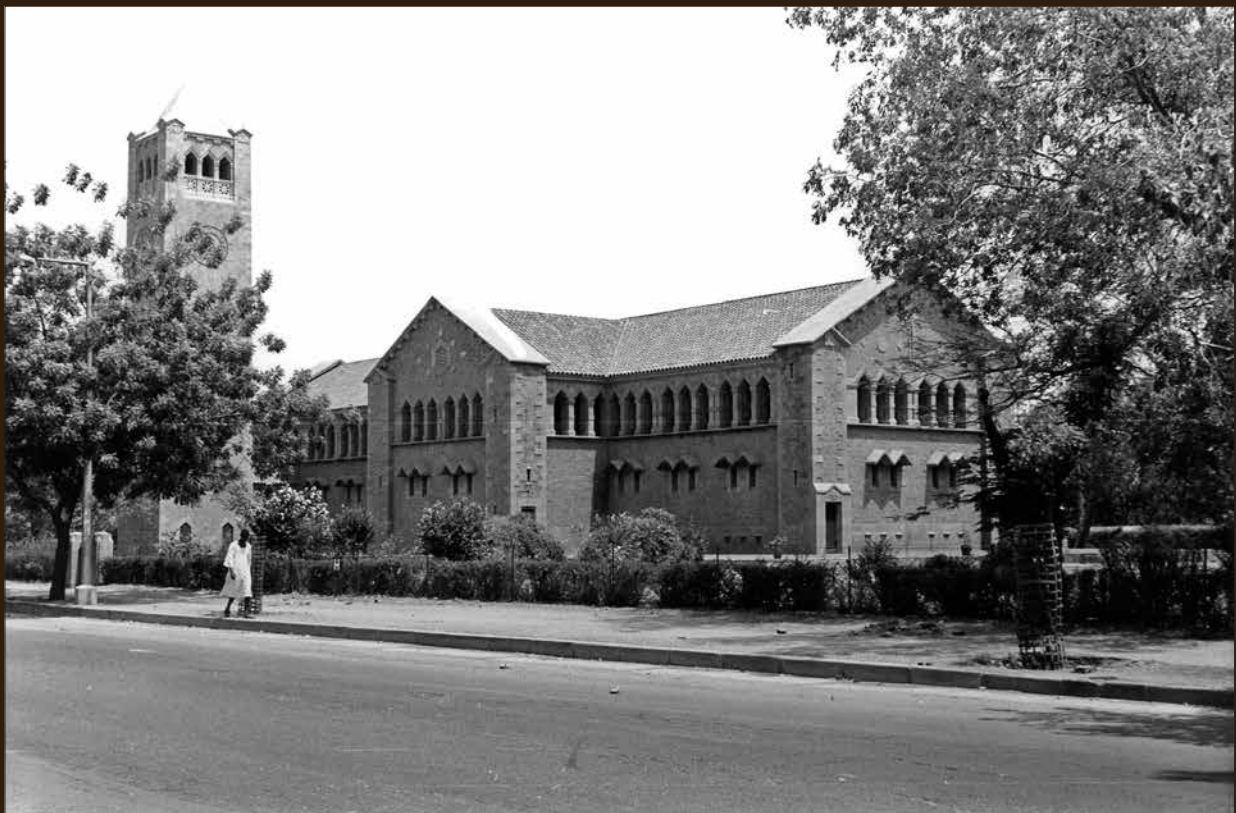
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*Khartoum. The Republican Palace, once the Governor General's residence, in 1968 (photo SARS Hawkes Archive HAW P091.01).*



*Khartoum. The Anglican cathedral in 1968. Now minus its bell tower it houses the Republican Palace Museum (photo SARS Hawkes Archive HAW P090.01).*