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ABBREVIATIONS USED IN THIS JOURNAL

BIFAO — — — — Bulletin d’Institut Français d’Archéologie Orientale.
JEA — — — — Journal of Egyptian Archaeology.
LAAA — — — — Liverpool Annals of Archaeology and Anthropology.
PPS — — — — Proceedings of the Prehistoric Society.
SASOP — — — — Sudan Antiquities Service Occasional Papers.
SNR — — — — Sudan Notes and Records.
ZAS — — — — Zeitschrift für Aegyptische Sprache.
ZDMG — — — — Zeitschrift der Deutschen Morgenländischen Gesellschaft

N.B. The Editor is not responsible for the opinions expressed by contributors

Contributions to this Journal are welcomed and should be sent to:—
THE EDITOR, KUSH, P.O. Box 178, Khartoum, Sudan.
Editorial Notes

SINCE the publication of KUSH III, the Sudan Antiquities Service has undergone considerable changes. Mr P. L. Shinnie left as a result of the judgment of the 'Sudanisation Committee' that his post, Commissioner for Archaeology, was one of political influence, and Sadik Eff. Nur has succeeded Mr H. N. Chittick as Curator of Museums.

At the time of writing, important modifications are also taking place in the activities of the Service, as a result of the danger to Sudanese archaeology caused by the Sudd-el-Aaly scheme, which threatens to submerge more than seventy-five sites in that part of the country which is richest in antiquities.

Thanks to the strength with which it has been endowed by Dr A. J. Arkell and Mr P. L. Shinnie, the Antiquities Service has been able to overcome the two-fold crisis which it has had to face. The publication of KUSH IV is in itself evidence that the Service is alive and active, and enables me, in the name of the Antiquities Service as a whole to thank Mr Shinnie, who was the initiator of KUSH and its inspiration. The best way we can show him our gratitude is to carry on his work, and this we hope to do, with the help of all those scholars who take an interest in the archaeology of the Sudan, which is so complex and as yet so little known.

We very much regret to record the death, since going to press, of Mrs G. M. Crowfoot, one of the contributors to this issue.

J. VERCOUTTER.
Second Thoughts on Jebel Moya

by F. Addison

DURING the period 1910–1914 the late Sir Henry Wellcome (or Mr H. S. Wellcome as he then was) carried out extensive excavations at Jebel Moya and certain other sites in the Sudan. These excavations were interrupted—and as it turned out, ended—by the outbreak of World War I and remained unpublished at the time of Sir Henry’s death in 1936. In 1937 I was invited by Sir Henry’s trustees to undertake publication of his excavations and my report on those at Jebel Moya, delayed by the outbreak of World War II, finally appeared in 1949. Since then Dr A. J. Arkell has criticised my findings in various journals, and in 1955 the belated report on the human remains from Jebel Moya was published. The time seems opportune, therefore, for some reconsideration of the views I originally expressed.

I may recall that the site of the excavation lay in a basin or valley high above the plain in the north-east corner of the Jebel Moya massif. On the floor of this basin there had accumulated over the centuries a layer of deposit, between two and three metres thick, which consisted mainly of debris of occupation. The excavation covered about ten acres, roughly two-fifths of the area of the floor of the basin, and yielded an embarrassingly large quantity of miscellaneous material. There were several tons of potsherds, most of which seemed to be of Meroitic date; many thousands of locally made lipstuds, beads and other ornaments; hundreds of stone implements such as polished celts, rings, mace-heads, grinders, and flaked implements of all kinds including arrow heads. There were also a few imported beads and amulets of Napatan date, an assortment of scarabs, and a number of objects of iron and copper. In addition some 2792 graves, which had originally been dug from various levels in the deposit, were excavated. The skeletons in these graves showed little evidence of regular burial customs; they were orientated to every point of the compass and displayed a wide variety of burial attitude. About half the graves contained only human remains and no other objects of any kind whatever. In most of the others the occupants had been buried with only the few personal ornaments they had worn during life—beads of ostrich egg shell or zeolite, nacre ornaments, lipstuds, ivory bracelets and the like. In a few graves, however, beads and amulets of obviously Napatan date were found, also copper hair ornaments and an occasional copper or iron bracelet.

These Napatan objects, both from the graves and from the general debris of occupation, were almost the only ones which could be dated with any approach to accuracy, yet they create considerable difficulties in the dating of the settlement as a whole. They were found in the same strata, and were, therefore, apparently contemporary with, the incomparably greater bulk of other material, especially pottery, which seemed to be of very much later date. In my report I wrote [p. 251]:—

‘The evidence for dating the site is conflicting. We have on the one hand the mass of pottery which on the face of it might be of comparatively late date, and, on the other hand, the beads, amulets and scarabs which are unquestionably of

1 The Wellcome Excavations in the Sudan, vols. 1 and 11, ‘Jebel Moya’, by Frank Addison.
SECOND THOUGHTS ON JEBEL MOYA

Napatan date. These Napatan objects are distributed throughout the B stratum, the lower part of the A stratum, and the upper part of the C stratum, and, as these strata cannot have been deposited at a date anterior to that of the objects enclosed in them, it is clear that the site cannot be "prehistoric" in the sense which Sir Henry Wellcome at first supposed. If the beads and amulets be assumed to have reached Jebel Moya at, or soon after, the time they were made, they fix the date of the upper levels of the site without further argument. But if objects of this kind had not been present on the site at all, and the pottery formed the sole criterion of date, it is to be doubted if anyone familiar with the results of other excavations in the Sudan would have ventured to date the Jebel Moya pottery as early as 500 B.C. It would with some justification have been assigned to the Meroitic period and not very early Meroitic at that; and the occupation of the site would be considered to have extended well into the early centuries A.D. So strong, indeed, is the impression of lateness conveyed by the pottery that there is a temptation to dismiss the beads and amulets out of hand as the loot of a later age. In this way the discrepancy between the apparent date of the pottery and that of the Napatan objects could easily and simply be resolved. The beads and amulets are not, however, to be so lightly disposed of. It is a question for argument as to whether they are likely to have reached Jebel Moya actually during the Napatan period or were taken to the site at a later time.

I went on to review the evidence and came to the conclusion at that time that the Napatan objects probably were contemporary trade goods and that most of the debris of occupation had accumulated during the Napatan period. It followed that there could have been no occupation in middle or late Meroitic times and that the settlement must have been abandoned soon after 400 B.C. The apparent Meroitic date for the pottery could be accounted for by supposing that this type of ware must have been made in the Jebel Moya area long before it appeared on sites further north. The date of the beginning of the occupation could not be determined by archaeological evidence alone because there was nothing in the lowest occupational stratum which could be dated. But by means of a graphical construction based on the rate of deposition of the debris I worked out that the site could not have been occupied before about 1000 B.C.

The vertical distribution of the human remains (see Fig. 1) shows without any doubt that the occupation must have been continuous with a gradually increasing but not necessarily homogeneous population. Certain types of objects, such as stone implements continued to be made without any change or modification during the whole of the period of occupation; they were distributed throughout the debris from top to bottom. But there were many types of pottery, not found in the lowest strata, which seemed to have been introduced into the settlement at different times. Also there were variations in such customs as the wearing of lipstuds, the removal of teeth, etc. It therefore seemed possible that the population of the settlement might have included more than one ethnic type. It also seemed possible, when I wrote my report, that the physical anthropologists might be able to distinguish these types. Had they not, I thought then, the skeletal remains of several hundred individuals to work on? I may add that the anthropometrical records made at the time of the excavations showed a considerable preponderance of female over male burials, and this suggested that the male inhabitants of Jebel Moya were polygamous and may have raided the surrounding tribes for women.

So much, then, for my original conclusions and suggestions; now for the publications I mentioned at the beginning of this article.
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The report on the human remains makes melancholy reading. It shows that, owing to the long period which had elapsed between the excavation and the final examination of the skeletal remains, and to the number of times these remains had perforce been moved from one storehouse or repository or another, very few crania remained in a condition for accurate measurement. It was, in consequence, impossible to distinguish any differentiated ethnic groups in the settlement itself. The report showed, moreover, that, while the anthropometrical records made during the 1911-1912 season by Dr Derry could be depended upon, the very large number made during the last two seasons were quite unreliable. Many of the bodies, according to the authors of the report, had originally been wrongly sexed, and there was, in fact, no such preponderance of females in the population of the settlement as the records at my disposal had indicated. It follows, of course, that any of my tentative conclusions based on this supposed preponderance are no longer tenable.

One of the Appendices to the report, written by Mr Mukherjee, is entitled "A Quantitative Analysis of Some Cultural Traits of the Jebel Moyans". In this he suggests that the differences noted between the different strata in such matters as burial attitudes and the wearing of lipstuds and beads may be explained by "an influx of an immigrant population" and a later "complete assimilation of the immigrants by the autochthonous population". The fact that he postulates immigration supports my own conclusion that the introduction of distinctive types of pottery was due to this cause, though I had thought that the immigration was perhaps stimulated by the polygamous males. The words "influx" and "immigration" refer, of course, to comparatively small numbers of people and not to mass movements.
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I turn now to Dr Arkell’s criticisms. Those which I have seen are to be found in three reviews, the first in The Listener of 13 October 1949; the second in The Archaeological News-Letter for January, 1950; and the third and longest, somewhat belatedly, in The Journal of the Prehistoric Society for 1954. In general he disagrees with my dating and with my view that no reliable conclusion could be drawn from the orientation of the burials. On the evidence he is prepared to accept Reisner’s opinion that the occupation of the site had come to an end in the time of Aspelta (592–568 B.C.); yet he considers there is evidence of trade from Meroë to Jebel Moya in the Meroitic period, and draws attention (as indeed I had done myself) to the Meroitic character of many of the finds. He regards Jebel Moya as a ‘multi-period site’ and considers it improbable either that there was only one continuous period of occupation or that many of the burials did not post-date the latest occupation. It is a curious fact, to which I drew attention, that a number of the few whole pots recovered during the excavation were devoid of ornament while a high proportion of the mass of loose potsherds were decorated in some way. Dr Arkell considers that this fact presumably indicates that most of the graves were not contemporary with the occupation. As to that I would merely observe that less than one per cent of the graves contained a whole pot and that not all these pots were plain. I may as well dispose of this matter at once by saying that there is no real evidence that any of the graves post-dated the occupation. However, if there were such graves they must have been the latest on the site and must have been dug from the highest level in the deposit. Manifestly, some of the latest burials on the site are those at the highest levels in Fig. 1, and amongst these graves is No. 1577 which is one of those singled out by Dr Arkell as ‘certainly Napatan’!

Although I think Dr Arkell is right in insisting that many of the objects are of Meroitic date, it seems to me that some of his other views are based on a fundamentally false assumption. Certain passages in his longest review make it clear that he has too readily, and erroneously, assumed that Jebel Moya was a site similar to those which he himself had excavated in the neighbourhood of Khartoum, and that what happened on those sites must also have happened at Jebel Moya. He has, in consequence, considered the problems of Jebel Moya not in the light of the recorded evidence from that site, which he has entirely ignored, but in the light of his own experience of sites completely different. He has studiously avoided any reference to the stratification of the site although it is of fundamental importance in any discussion of the dating; and he has either evaded, or failed to see, the main point of my argument, namely, that the archaeological evidence for a Meroitic date and that for a Napatan date are mutually exclusive. The acceptance of the one automatically entails the rejection of the other. He has, moreover, both misunderstood and misrepresented the ‘mathematical’ method I used to determine the limits of the occupation. Nevertheless, in view of his criticisms I have re-examined the record after a lapse of years with a mind comparatively fresh and I will re-state the evidence here in terms which, I hope, cannot possibly be misinterpreted. Passages within quotation marks are from my original report and references in square brackets are to the text figures or plates which accompany that report.

The argument is earth-bound throughout; it is concerned almost exclusively with the stratification of the site and archaeological considerations are only remotely involved.

The deposit which had accumulated to a considerable depth on the floor of the basin at Jebel Moya was seen to consist of four main layers differing somewhat in colour and texture. They were denoted by the letters A, B, C, and D, in that order from top to bottom and they formed the basis of all the recording during most of the excavation. Below the lowest or D stratum was a layer of disintegrating granite passing down into solid rock.
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In the opinion of the geologists who either worked on, or later reported on, the site, the D stratum was deposited before human occupation began; it contained no objects but was penetrated by a number of graves. The surface of this stratum was the floor of the valley when the first settlers appeared, and the C, B, and A strata were all formed during the occupation. The late Dr G. A. Reisner who visited the site during the excavations subsequently wrote [p. 30]: 'The whole deposit of debris in all parts of the excavation consisted of very thin layers. This thinly laminated deposit showed all the characteristics of "debris of occupation" and was obviously a growing village floor laid down for centuries and reaching in places a depth of 3 metres'. I may here observe that the accumulation of deposit was continuous though not necessarily uniform in rate. There is no evidence—and nobody connected with the excavation ever suggested—that the accumulation was interrupted by periods of erosion or non-occupation. It is, of course,
obvious that whenever graves were dug they must have been dug from the level on which people were living at the time; and since, owing to the accumulation of debris, the living floor continued slowly to rise, the graves must have been dug from progressively higher levels. Also, after they had been filled in, the graves were covered by an ever-deepening layer of debris. This is not mere probability, it was an observed fact. To quote Reisner again: 'The level on which the grave was dug would be discovered by uncovering the floor in which it was dug by gradual excavation from above' and further [p. 35] 'I proceeded to ascertain the level of the graves in one quarter of the area under examination. Here I had the workmen clear out about a centimetre at a time, sweeping up and looking out for grave outlines on the swept surface. At about one metre down they exposed three graves all opening on the same floor.'

Now although the main excavation was notoriously not carried out with this meticulous care it was nevertheless done in roughly horizontal layers, and the records show that graves were constantly encountered at every level. They were numbered progressively as soon as their outlines were seen and hence, in any of the squares into which the excavation was divided, the graves dug from the highest levels were allotted smaller or earlier numbers than those lower down. The outlines of a number of graves low down in the debris had been traced, and the graves numbered but not excavated, when work was suspended. They are shown dotted in the grave plan and account for the blank spaces in the Register of Graves. The point of all this is that a grave dug during a late period of the occupation must have been dug from a higher level than one dug during an early period, and that graves of different dates cannot possibly have been dug from the same surface. It may seem to be labouring the obvious to state this, but it is a fact of crucial importance on which the entire argument hinges. It is a fact, moreover, which Dr Arkell has either failed to appreciate or has chosen to ignore.

Although no erosion of the site took place during the period of occupation, considerable denudation of the strata has occurred since the settlement was abandoned. There is a gap in the eastern wall of the basin through which at the present day the summer storm water drains away into the plain below, but while the site was occupied this gap was blocked up in some way at present unexplained. Since the blockage was removed the storm water has worn out a series of khors as shown in the plan, FIG. 2, and between any two adjacent khors the ground now forms a convex ridge as shown in the section, FIG. 3 [see also plate xxii]. It will be seen that, on either side of these khors one or more of the original strata have disappeared, and it is probable that many human remains which may have been buried in these strata have been swept away during the erosion. The graves are noticeably fewer in the vicinity of the khors.

Owing to this post-occupational erosion the present ground surface is useless as a reference datum, so to overcome this difficulty I adopted, as fully explained in my report, the surface of the c stratum ('c surface') as a reference datum. Since the strata were
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built up from the bottom, levels at the same distance from this datum must be roughly contemporary in date.

That the erosion took place after, and not during, the occupation is shown by the position of the large pot O.C.2303, FIG. 4 [see also plate cxi]. A pot of this size could never have been buried in a grave and it was, in fact, found in situ 'embedded up to its middle in the highest "paving" in square M.5, N.6 with the top collapsed and fallen inside'. It must, then, belong to a late period of the occupation. Its position is shown in the section, FIG. 3, and it is obvious that a good deal of the stratum had accumulated above it before any erosion took place. It follows, of course, that the date of the end of the occupation and that of the beginning of the erosion are both later than the date of the pot. Dr Arkell has pronounced the pot to be of Meroitic date, and he may well be right; but in that case the occupation must have continued at least into the late Meroitic period and the erosion must be post-Meroitic. And here it is pertinent to note that Mr G. W. Grabham, the geologist, in his report on the Jebel Moya strata wrote: 'The subsequent erosion may be a matter of 500 years—since the Arabs came'. It will be observed that Dr Arkell's acceptance of the view (see above) that the occupation ended in the Napatan period is entirely incompatible with his acceptance of a Meroitic date for the pot.

Now that the important question of the stratification has been, I trust, adequately explained, I can deal with the conflict between the Napatan and Meroitic evidence which is the main theme of this article. My original arguments were based on the vertical distribution of various objects throughout the strata, but Dr Arkell takes the view that on an occupation site so much disturbance of the ground is caused by the daily activities of the inhabitants (and on any site by the post-occupational activities of burrowing animals) that no reliance can be based on the stratification of small objects. There is some substance in this view though I think he exaggerates the amount of disturbance. If the debris of Jebel Moya were continually being churned up to the extent Dr Arkell would like to make out, how have the hundreds of fine laminations noted by every observer, including Dr Reisner and myself, managed to remain unbroken? It is not for me to answer this question and I will not pursue the matter; I propose instead to confine
SECOND THOUGHTS ON JEBEL MOYA

my argument to objects which cannot possibly have been disturbed since they were buried, namely, to objects in graves. It will make no difference to the conclusion.

I would draw attention to two graves, Nos. 1577 and 2000 which lay not far apart in the same part of the site; they are marked on the plan, FIG. 2. That they were in the same part of the site rules out, for a start, any idea that they may have belonged to separate 'cemeteries' of different periods. Suspended from the neck of the body in grave 1577 was a plaque of early Napatan (xxvth Dynasty) date, and the grave, as already noted, was amongst those selected by Dr Arkell as 'certainly Napatan'. In front of the face of the body in grave 2000 was a decorated pot, FIG.5, R.3 [plate cxi] of a kind dated elsewhere in the Sudan to the period Meroitic B (100 B.C. – A.D. 100). This, surely, is a Meroitic grave. Now since the accepted definition of the Napatan period is 750–538 B.C. (unless it has recently been modified) there is a difference in the apparent date of these two graves of about 600 years—six centuries during which the living floor of the settlement was slowly rising. Yet the 'Napatan' body in grave 1577 was found only 10 centimetres below the modern surface (or 90 centimetres above C surface) while

Fig. 5. OUTLINES OF POTS FROM GRAVES

the 'Meroitic' body in grave 2000 lay 175 centimetres below the modern surface (or 35 centimetres below C surface). Grave 2000, to judge from the photograph [plate xxxiv, 4, 5] was a shallow oval grave dug from a level low down in the B stratum. Grave 1577—600 years earlier in date?—was dug from a level in the A stratum, a level so high that at the time of the excavation it had been eroded away although the ground in its vicinity had not been extensively denuded. The relative positions of the two bodies may be seen in FIG. 1 which shows the number of bodies recorded at different levels. (During the earlier part of the excavation this level was measured to the nearest 5 centimetres and during the latter part to the nearest 10 centimetres which accounts for the preponderance of bodies at 10 centimetre intervals.) It will be seen that grave 1577 must have been one of the latest burials on the site while grave 2000 was probably one of the earlier ones.

Now my long dissertation on the stratification of the site has, I hope, shown that it is physically impossible for an early, or Napatan, grave to have been dug from a higher level than (or even from the same level as) a late, or Meroitic one, and here we face that direct conflict of evidence to which I have already referred. If grave 1577 is Napatan then grave 2000 cannot be Meroitic and vice versa; there is no possibility of reconciling the apparent dates of these graves. Let us examine this grave 2000 a little more closely. The body in it was in a crouched position, a rare 'pre-dynastic' attitude associated with the earliest levels at Jebel Moya; only twelve bodies out of nearly three thousand were found in this burial attitude. To quote Mr Mukherjee again (loc. cit. supra), 'the
extended type of burial grew more common in the later part of the settlement and the other forms, particularly the contracted or crouched, became obsolete'. This is a statistical finding and gives grounds for accepting grave 2000 as one of the early graves of the occupation. But suppose, in our anxiety to escape from the dilemma, we accept a Napatan date for grave 1577 and regard grave 2000, against all the previous evidence, as a deep, intrusive, post-occupational grave. There is no escape along that line. Had 2000 been a post-occupational grave it must have been dug from a level on or above the modern surface, and in that case its outline would have been detected at an early stage of the excavation of the square (1.9, 1.10) in which it was situated. Yet of the 257 graves actually excavated in this square, 163 were given numbers less than 2000. This indicates a strong probability that the grave to which this number was allotted must have been dug from a fairly low level. Further, if this grave had been dug from the modern surface it would have been at least 175 centimetres deep. There would be nothing remarkable about this except for the fact that the greatest depth of grave recorded at Jebel Moya, i.e., the depth from the surface in which the grave was dug, was only 140 centimetres, and this was exceptional. It is true that the depth of only about 10% of the graves was recorded, nevertheless the figures are against the probability of very deep graves at Jebel Moya whatever may have happened elsewhere. All the evidence, from different sources, points to the fact that grave 2000 was one of the earlier graves of the settlement and that it must have been an older grave than 1577. There is a further point: grave 1577 must have been dug from a level very much the same as that of the ‘paving’ in which the large pot already referred to was embedded, and hence the grave and the pot cannot differ widely in date. A Meroitic date for the pot precludes a Napatan date for the grave. The inescapable conclusion is that the ‘archaeological’ evidence for the date either of grave 1577 or grave 2000 is misleading and must in one case or the other be rejected.

I have focused attention on these two typical graves because they illustrate the conflict of the evidence for dating the site in a manner which cannot be circumvented. The issue must be squarely faced. It cannot this time be evaded by unfounded accusations that I have ‘relied on artificial data’ or on ‘arbitrary assumptions’. I have merely drawn attention to certain facts which are on record and which are published in my report for all to read. As I have already noted, this conflict extends throughout the A and B strata of the site and occurs in individual graves. What, for instance, is the date of a grave in which were found iron (?Meroitic) bracelets and Napatan beads? And the pot in grave 2000 was not the only one of its period found on the site. There was a similar one, FIG. 5, R.2, in grave 2193 at a slightly higher level than 2000, and another was found at some unknown level in the B stratum [plate cxiii, 4, 5]. There were also, of course, graves containing Napatan objects at much lower levels than that of 1577; some of them were at levels even lower than that of 2000, but the difference in level was not such as to account for the 600 years difference in date.

In my original report I set out what seemed to me at the time to be valid reasons for accepting the Napatan objects found at Jebel Moya as evidence for an occupation of the site in the Napatan period. This, as it happens, was also Reisner’s view in 1914, though it should be noted that, when Reisner visited the site, graves 2000 and 2193 had not been excavated and he had not seen the pots they contained. There is, however, no actual proof (nor as far as I can see is there any possibility of proof) that the objects in question really did reach Jebel Moya during the Napatan period. All I did was to argue that the balance of evidence was in favour of their having done so, and, faced with a

\[^3\]SNR, vol. ii, p. 65.
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choice, to accept the result. My argument has nowhere been challenged, although it is wide open to challenge. If the conclusion I reached has solved some difficulties it has created others, and in particular it has involved assigning a Napatan date to a good deal of characteristically Meroitic pottery as well as to iron and other objects. There is a case to be made out for accepting the Meroitic evidence and rejecting the Napatan, and since nobody else has put it forward I propose to do so myself. This does not mean that the actual date of the Napatan objects is in question; it simply means that they are to be regarded as 'the loot of a later age', i.e., they reached Jebel Moya at a date very much later than 500 B.C. They are no longer to be regarded as evidence for an occupation during the Napatan period itself but simply as evidence of a connection with Napata at some indeterminate time. They are no more use for dating than were the scarabs found on the site, regarding which I wrote [p. 119] 'the collection as a whole has the appearance of casual loot'. I may recall that amongst these was one of Sheshonq I (945 B.C.) which was found in the A stratum at the same level as, and quite close to, the large pot (Fig. 4)

Now that the Napatan evidence for dating has been rejected there remains only the Meroitic. The stone implements are, in a sense, neutral, for they do not help to date the site at all. However old they may typologically appear to be (and Dr Arkell says that the trapezoidal chisel-type arrow heads occurred in the Capsian of North Africa) they were in fact made on the site and most of the flaked implements were found in the A and B strata. They are, therefore, of the same date as the other objects found in those strata whether these are taken to be Napatan or Meroitic.

I ought, perhaps, to say at this point that the rejection of the Napatan evidence involves no disrespect for Reisner's view. As it happens I wrote to him in 1938 soon after Mr Kirwan and I had started work on the material from Jebel Moya and when the Meroitic (or later ?) date of much of the pottery had become apparent. In his reply he advised me to take no notice of anything he may have written in 1914 and to be guided by my own judgment.

To return to the question of dating, I observed in my report that the vertical distribution of the Meroitic and metal objects was the same as that of the Napatan ones. In fact, all the imported objects, whatever their apparent dates, can be considered together as evidence of trade, or at least of human intercourse, between Jebel Moya and sites further north. This seems to have begun when the living floor of the settlement was a few centimetres below c surface, and the most likely period for its inception was during the first century B.C. Jebel Moya is not the only southern site on which pots such as those in Fig. 5 have been found. They were found not far away across the Blue Nile in a cemetery accidentally discovered (and, incidentally, destroyed) during the building of the Sennar Dam and which I have already published elsewhere. Also in this cemetery were found bronze bowls (some bigger than any found elsewhere in the Sudan) which must have been imported from Egypt, and pottery and beads which could be exactly duplicated in the Meroitic cemeteries at Faras and the 'Romano-Nubian' cemetery at Buhen in the extreme north of the Sudan, as well as at Meroë itself. The cemetery is dated to the period 100 B.C.—A.D. 100 and the presence of the objects so far south is an indication of the freedom of communications which must have obtained during that period. It does not, then, seem improbable that the imported objects, whether via Napata or via Meroë, first reached Jebel Moya about this time.

Another link, this time with Abu Geili (not far from the cemetry just described but having no connection with it) is provided by the 'dry scratched' ware which occurred so abundantly at Jebel Moya and which I have called 'typical' Jebel Moya pottery. Many fragments of this were found on the floors of the rooms in the village site at Abu Geili. Indeed, the only complete pot of this ware was found at Abu Geili and not at Jebel Moya at all, a fact of which I was unaware when I wrote my Jebel Moya report. In the account of Abu Geili I have given reasons for supposing that this ware did not originate on either of these sites but reached them independently from a common source. The village site at Abu Geili is dated to the early centuries A.D. (from just before the beginning of our era to anything from A.D. 200 to 600) and the dry scratched ware at Jebel Moya started at a few centimetres below C surface and occurred most abundantly at the highest levels. On the evidence of this pottery the B and A strata at Jebel Moya could be of much the same date as the Abu Geili site, namely, the first three or four centuries A.D.

This dating is confirmed by a consideration of the iron objects. These again started at a level about 20 centimetres below C surface and continued through the B stratum into the A stratum. Ornaments such as bracelets and rings appear to have reached the site earlier than arrow heads and implements, for these latter were confined to the highest levels. As I observed at the time, it was difficult to fit these iron objects into a Napatan context, and now that this constraint is removed they can be assigned the Meroitic date which seems more reasonable in view of the evidence of extensive iron working at Meroë. The early centuries A.D. is also the period to which Mr Kirwan and I, on our preliminary inspection of the material from Jebel Moya, were inclined to assign the mass
of potsherds recovered during the excavation. They do not fit very happily into a Napatan frame.

If the B and A strata of the site were formed during the early centuries A.D. it would be reasonable to date C surface to the beginning of the period; i.e., the surface of the C stratum was the living floor of the settlement about that date. It must, then, follow that the C stratum was deposited during the last centuries B.C.; and as the living floor when the first settlers appeared was the surface of the D stratum (or the bottom of the C stratum) the number of years taken for the C stratum to accumulate is manifestly the same as the number of years B.C. for the foundation of the settlement. The thickness of the C stratum is only about 70 centimetres so this date cannot be very remote. The A and B strata together have a total depth at their deepest point even to-day of 220 centimetres, and this depth (plus whatever has since been eroded away) must have been deposited between the beginning of the period A.D. and the end of the occupation. How long would be required for the formation of a mere 70 centimetres in the years B.C.? I have observed elsewhere [p. 33] that the rate at which deposit is accumulated at any time is probably roughly proportional to the number of inhabitants at that time, and using this principle I have worked out the time scale shown on the right of FIG. 6. This shows the approximate date of any level of the deposit and it seems to fulfil most requirements reasonably well. It allows grave 2000 to have been dug before A.D. 100; it permits the beginning of imports from the north during the last half century B.C.; and it gives a date of about A.D. 250 for the large pot, FIG. 4, a date which is probably correct to within a century or so. It shows that the occupation probably extended into the fifth century A.D. and it allows a period of 500 years for the accumulation of the C stratum, which is, perhaps, too much. But even this generous allowance means that the occupation of the site cannot have begun before 500 B.C. at the earliest. These dates are, of course, only rough, but they are both possible and credible.

It will be seen that the solution to the problem of dating the site varies according to the view taken of the Napatan objects. For myself, after reviewing the evidence again (including that from Abu Geili which I had not examined when I wrote my Jebel Moya report) I think the more acceptable solution is the one I have just propounded. I think I should have saved a good deal of time and trouble if I had followed my first impulse and rejected the Napatan objects for dating purposes at the very beginning. After all, the date of an assemblage of miscellaneous objects is the date of the latest object in that assemblage, and that date at Jebel Moya appeared to be Meroitic. However, I have at least shown both sides of the argument and nobody can say that the subject has not been fully discussed.

My position now is that I think it probable that the occupation of Jebel Moya covered roughly the whole of the Meroitic period, early, middle and late, and that it did not begin until after the Napatan period had ended. There was, that is, no occupation at all in the Napatan period. The matter is open to argument, but it is one which can only finally be decided by laboratory experiments using one or other of the new techniques; it cannot be settled by purely typological considerations. One thing at least should by now be clear, namely, that the Meroitic and Napatan dates are mutually exclusive throughout. Accept the Napatan objects as evidence of date and there can logically be nothing Meroitic on the site: reject these objects and there is no evidence for a Napatan occupation. It does not seem to me to be possible to construct a chronology for Jebel Moya which will allow an occupation in Napatan times extending into the Meroitic, and if there are objects of this latter period on the site they must be evidence of occupation during that period. Dr Arkell's attempt to evade the issue by
accepting a Napatan occupation, and postulating post-occupational graves to account for the Meroitic objects, breaks down (apart from its intrinsic improbability and lack of evidence) on the question of the pottery. There was, as I have said, an enormous quantity of potsherds; 'every basketful of the thousands of tons of debris removed during the excavation contained some fragments of pottery' [p. 199], and this pottery was in every sense of the term 'debris of occupation.' It cannot be accounted for in any other way. It must be of the same date as the occupation levels from which it came; if they were Napatan then the pottery must be Napatan. Dr Arkell has pronounced some of the selected sherds of this pottery to be Meroitic and I think he is right; but, if so, they must indicate an occupation in Meroitic times. He has also said, again rightly, that the large pot, Fig. 4, is Meroitic, and no theory of mourners bringing their dead for burial on a deserted site can possibly account for that pot; it obviously must belong to a period of occupation.

It will have been observed that such matters as the burial attitudes of the bodies and the orientations of the graves are quite irrelevant to this question of dating. Nevertheless, as Dr Arkell has dealt with the question of orientations at some length, it would be well for me to refer to it here. In the table below I give the recorded orientations for all the graves and also of one or two selected groups. It might have been interesting to compare the orientations of males and females separately, but since the sexing of the bodies is unreliable this has not been possible.

<table>
<thead>
<tr>
<th>Type or Group</th>
<th>Orientations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>N</td>
</tr>
<tr>
<td>All bodies</td>
<td>251</td>
</tr>
<tr>
<td>Bodies with lipstuds in situ</td>
<td>42</td>
</tr>
<tr>
<td>Graves with Napatan beads or objects</td>
<td>11</td>
</tr>
<tr>
<td>'Cow’s foot' burials</td>
<td>1</td>
</tr>
<tr>
<td>Result of spinning marked disc</td>
<td>4</td>
</tr>
</tbody>
</table>

These figures show that there was a preponderance of westerly orientations in the graves as a whole, which may be partly explained by the even more marked preponderance amongst the group of graves with lipstuds in situ. The proportions between these orientations did not vary much at different levels showing that there was no correlation between orientation and date.

The small group of graves containing Napatan objects gives completely indeterminate results, which is what might have been expected. Dr Arkell's 'reasonable preliminary hypothesis that an easterly orientation may have been introduced in Napatan times' seems to me to rest on some extremely questionable assumptions. The first of these is that, because graves in Napata had an easterly orientation, therefore graves in far away Jebel Moya with an easterly orientation must be Napatan. The second is that burial customs are exportable with trade goods. There is nothing to show that the people of Jebel Moya were at any time affected by Napatan or Meroitic influences; on the contrary, all the evidence is that they retained their own primitive culture until the end of the occupation. The presence of a few imported beads at Jebel Moya is...
indication that the inhabitants had adopted Napatan or Meroitic burial customs. In any Central African village in the middle of the nineteenth century there could be found beads and wire of European manufacture, yet the inhabitants had adopted neither European culture nor Christian burial customs. The third assumption, reasonable but not established, is that there was an occupation of Jebel Moya in Napatan times at all.

One of the few signs of burial ritual observed at Jebel Moya was to be seen in the ‘cow’s foot’ burials [p. 59], where one or more feet of a cow (ox, calf) were found in the grave, indicating that the body had probably been covered with, or wrapped in, the skin of an animal. There were 36 of these graves distributed widely over the site and at many different levels. If the orientation of the grave were considered to be an essential part of the ritual to be followed at these distinctive burials it might have been expected to display some uniformity. The figures given in the table (the orientation was not recorded in five cases) show that the orientations were, in fact, completely random. To check this I spun a disc, marked with the various compass points, the requisite number of times, noting where it came to rest. The result is also given in the table and it shows that the figures for the ‘cow’s foot’ burials are completely without significance.

It is, of course, possible to select and tabulate the orientations of the graves of many other selected groups, e.g., those of a particular burial attitude; those with ivory bracelets; those with shell pendants, etc., but I do not know what the value of such an exercise would be. It is the sort of investigation Mr Mukherjee might have undertaken, but he has not done so, possibly because he thought it would be unrewarding, as I do myself.

I do not share Dr Arkell’s optimistic belief that further excavation would help to solve the problems of Jebel Moya. He himself speaks with two voices on the matter. One of these stresses the value of a careful excavation with knife and brush, the object of which, surely, is to establish the exact vertical distribution of any objects buried in the ground. Yet the other voice denies that such a distribution has any value as evidence owing to the disturbance caused by ‘the activities of mice and men’ (sic). Further excavation, he says, will enable us to ‘ascertain which objects are true grave goods and which come from occupation debris’. Here again are some unwarrantable assumptions. The first is that the objects (when there were any) found in the graves already excavated were in some way not ‘true’; that, in fact, the excavators were incompetent. The second is that the objects (again if any) to be found in a new excavation will be different from those already found—or why trouble to find them? The third and most doubtful assumption is that such objects will be datable. The uncertainty in the dating of the site is due to the lack of two vital pieces of information, (1) the precise levels from which the graves were dug, and (2) the date when the Napatan objects reached Jebel Moya. This lack could not possibly be made good by any excavation, and further digging would, in my view, be a complete waste of time; it would only produce more material of the kind we already have in super-abundance and which we cannot with complete certainty evaluate.

Dr Arkell expresses an important truth when he writes: ‘In conservative Africa cultural traits may persist for a thousand years or more’. This means that such expressions of a culture as stone implements, pottery and the like cannot in themselves be dated to within a thousand years or so. Hence it is no use attempting to date either the beginning or the ending of the occupation of the Jebel Moya site by reference to implements or objects made on the site itself.
KUSH

My conclusion that the occupation of Jebel Moya was entirely Meroitic may or may not be correct, but it cannot be invalidated simply by asserting that such and such a type of implement could not have been made in the third century A.D. or, alternatively, that it must have been made before 500 B.C. The arguments on which the conclusion is based must be examined and proved wrong using the same basic facts of the stratification from which I started. No reference to other sites is necessary and no archaeological expertise is required other than the knowledge of the meaning of the terms ‘Napatan’ and ‘Meroitic’.

In conclusion, I wish to thank the Wellcome Trustees for permission to make such extracts from the text and figures of my original report as I have found necessary.
Die Fiktion der ‘Kuschitischen’ Völker

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Im Verlaufe der archäologischen Verarbeitung ägyptischer und ägyptisierender Denkmäler Äthiopiens durch Lepsius stellte sich dann, wie bekannt, heraus, dass es in jenem sudanischen Großreich neben Ägyptisch auch eine damals noch unverständliche, einheimische Sprache gegeben hatte. Ihre Entzifferung schien anfangs mangels jeder

1 Die Genesis kennt drei ‘Kūš’ genannte Länder, resp. Völker. Das eine weist auf ein Land in Südarabien (Gen. II, 11); das andere (Gen. x, 8 ff.) bezieht sich auf das Land des Assyrerkönigs Tukulti-Ninurta I (Nimröd) 1243–1207 v. Chr.; das dritte Land dieses Namens aber war Äthiopien, das Reich des Königs *Te-arḵō von 671 v. Chr. (2 Kön. xix, 9; Jesais xx, 3–5, etc.) welches die Genesis (x, 6) als Bruder Ägyptens ansah. Die letztere geogr. Richtung bildete dann in der Folge die Dominante für die antike Länderkunde von Afrika.
KUSH

Bilinguis ziemlich aussichtslos. Gleichwohl erblickte man in dieser Unbekannten den Schlüssel zum philologischen Verständnis der 'kuschitischen' Sprachen, ohne sich aber noch klar zu sein, welche der heute noch im Sudan gesprochenen Sprachen als Nachkommen jener rätselhaft schriftsprache des Äthiopenstaates in Frage kommen könnten. Lepsius riet auf Grund der Meinung des 'Kitāb el-Fihrist', dass nur die Begä-Sprache als Grundlage in Betracht käme; Reinisch dagegen (und später noch H. Schäfer) hielten Nubisch für die Sprache der antiken Äthiopenkönige. Solange man die damals 'Meroitisch' genannte Sprache aber nicht lesen konnte, war solcher Widerstreit keineswegs verwunderlich.

Die ganze Sachlage änderte sich sofort, als F. Ll. Griffith die 'meroitische' Schrift auf Grund von Namensgleichungen mit der ihm eigenen Akribie der sicheren Entzifferung zuzuführen vermochte. Ihm als Ägyptologen und Spezialisten für Demotische Litteratur standen zudem die Materialien demotisch-ägyptischer Äthiopen-Inscriptions und Graffiti Unter-Äthiopiens zugebore, so dass neben ihm kaum jemand Aussicht gehabt hätte, die Aufgabe der Entzifferung in derart befriedigender Weise durchzuführen. Auf Grund der so gewonnenen Einsicht konnte dann 1911 die erste sprachliche Skizze (Karanog, vol. vi) zur Einführung in die Textpublikationen aus den beiden äthiopischen Nekropolen Karanog und Shablul zustande kommen.

Das ziemlich gleichförmige Schema äthiopischer Epitaphe hatte nun zwar für das Erfassen des Textinnes viel Vergleichsmaterial an die Hand gegeben, doch blieb das so gewonnene Bild der Sprache dadurch immerhin noch fühlbar einseitig. Es fehlte alles Lebendige darin; aber dafür waren es ja auch Toten-Inscriptions. Das Wenige, was dabei an Wörtern und Formen erfasstbar wurde, genügte jedoch durchaus, die obgenannte Streitfrage nach der sprachlichen Natur des Schrift-Idioms Alt-Äthiopiens dezidiert ad acta zu legen. Es war nunmehr klar, dass es sich weder um Bedauye, noch auch um Nubisch handelte. Je mehr an Texten zur Publikation kam, desto deutlicher wurde diese für Manche überraschend gekommene Tatsache.

Wer sich unter den Sprachwissenschaftlern die Hoffnung gemacht hatte, die neu entdeckte 'meroitische' Sprache würde das sprachgeschichtliche Bindeglied für die sogenannten 'kuschitischen' Sprachen sein, der musste angesichts der völlig andersartigen Wortstämme und Formen jenes Idioms gründlich enttäuscht bleiben. Nicht nur Bedauye und Nubisch fehlte jeder innere Kontakt mit der Morphologie des 'Meroitischen', sondern es gab überhaupt keine sogenannte 'Kuschiten'-Sprache, bei welcher sich ein irgendwie plausibler Zusammenhang mit der Inschriften-Sprache von Alt-Äthiopiens hätte herstellen lassen. Eher fand sich so manches innere Band ursprünglicher Sprachverwandtschaft zwischen einzelnen Gruppen auf dem heutigen Sprachfeld der erythräisch-nilsudanischen Völkerstämmen, als auch nur die Spur eines solchen Bandes mit dem nun greifbarer gewordenen Idiom des vermuteten Stammsvaters Kusch. Soviel war damit klar sichtbar, dass die Bezeichnung 'kuschitisch' sozusagen nur ein philologischer Verlegerheits-Ausdruck geworden war, der verschleieren sollte, was man eben nicht wusste. Man hatte einfach keinen Anlass mehr, Sprachen dadurch zu charakterisieren, dass man sie 'kuschitisch' nannte.

Bei näherem Zusehen erweist sich zudem, dass das Bild einer gewissen 'Verwandtschaft' dieser z.T. morphologisch heterogenen wirkenden Idiome der Gegenwart überwiegend durch Lehngut ganz bestimmter Art vorgetäuscht wird. Die als breites Superstrat, lokal entsprechend massiert, das heutige Sprachfeld der Nil-Sudan-Völker durchsetzenden Sprachen sind neben Sudan-Arabisch hauptsächlich Tigré und Amharisch. Weil diese Fremdsprachen gegenüber den Einheimischen nun selber genetisch zusammenhängen, borgen sie also ihren sprachlich Hörigen den Anschein von

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Verwandtschaft der Sprachen, welche solches Lehngut *en masse* nostrifiziert haben. Der geschilderte Zustand erwuchs aber sichtlich fortschreitend aus dem sudanischen Mittelalter her bis heute, und hat also mit der antiken Sprache des alt-äthiopischen Reiches nicht das Mindeste zu tun. Eine Rückprojektion der neuzzeitlichen Sprachlagerung in die Antike wäre sowohl unhistorisch als auch entwicklungsgeschichtlich unmethodisch. Für die ältere Zeit also bleibt dann nichts Anderes übrig, als sich doch lieber mit dem annoch rätselhaften Inschriften-Idiom Alt-Äthiopiens bekannter zu machen.

Zuvor jedoch muss man sich erst informieren, wie weit sich jenes Alt-Äthiopien der Geschichte überhaupt auf dem Felde der heutigen Sprachen ‘kuschitischer’ Völker ausgedehnt hat, um sich klar zu werden, welches Gebiet des heutigen Sudan als antike Einflusszone eigentlich in Frage kommt, der geographische Heimatboden gegenwärtiger Eingeborenen-Sprachen gewesen zu sein.


In dieser Weise lässt sich die geographische Lage des Äthiopenstaates noch an Hand der einheimischen Quellen rekonstruieren. Dieser Staat war es, den die Antike seit der homerischen Zeit als ‘Äthiopien’ gekannt hatte und der bei den Ägyptern *K’ash*, äthiop.–äg. *eK’ōshi*, bei den Babyloniern ‘mat Kashi’, bei den Assyren ‘mat *Kōši*’ und zuletzt bei den Aksyiten ‘*Kası*’ sowie bei den Alt-Nubiern ‘Kası’ geheissen hat. All diese Bezeichnungen im Ausland stammen noch aus der Zeit vor den Meroticen-Königen, als die Residenzstadt der Äthiopen nach Napata im Lande ‘*Kaś*’ gewesen war.2 Statt also immer gedankenlos von ‘Kuschiten’ zu sprechen, werden wir besser den geographisch, politisch und national genauerem Terminus ‘Kaschiten’ gebrauchen und damit das bezeichnen, was es historisch besagt: die Bevölkerung des

2 Der scheinbare Lautwechsel äg. k: äth. k in dem Namen von ‘Kasch’ verrät eine charakteristische Eigenart für die Sprache. Bekanntlich artikulieren heute noch alle südarabischen Erythräer das emphatische k des Semitischen als stimlosen Dorsal k mit unmittelbar nachfolgendem Stimmeinsatz. Von den Ägyptern war dieser Stimmeinsatz aber als *Radikal* aufgefasst worden und so forderte die ägyptische Aussprache einen Vorschlagsvokal.
alten Äthiopenstaates im Nil-Sudan, welche die Träger der Herrschaft daselbst gewesen waren.


Angesichts dieser erheblichen räumlichen Ausdehnung des kaschitischen Äthiopenreiches zwischen Nil und der Küste des roten Meeres könnte die Frage auftauchen, ob es sich bei jenen Kaschiten etwa um ein Volk gehandelt habe, dessen Massen die Träger der Macht im Sudan des zweiten Jahrtausends v. Chr. gewesen sein mochten. In solchem Falle könnte man doch noch an die Existenz eines ‘kuschitischen’ Sprachstamms für jene Zeit denken, von dem aus die heutigen Sprachen ‘kuschitischer’ Völker sich in so seltser Diskrepanz von einander abgespalten hätten.


Das Bemerkenswerte an dieser Aufzählung ist nun, dass besagtes Land ‘K’s’, respektive ‘K’sh’ lediglich als einer der vereinten Feinde Ägyptens erscheint, nicht aber

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6 Beim Aufzählen von beherrschten Fremdländern bestand in Ägypten die Regel, vom Fernpunkt beginnend bis heran zum Nahpunkt des Aufzählenden anzuordnen.

7 Noch die jung-kaschitischen (merotischen) Texte verraten, dass ihre Sprache kein eigentliches, dem Ägyptischen entsprechendes sh besass. Man unterscheidet dort zwei s-Laute: der eine lag etwa zwischen s und sh, der andere mag etwa die Aussprache des spanischen z gehabt haben. Der oben aufgefallene Wechsel in der ägyptischen Schreibung kann als erstes Argument für die historische Konstanz dieses Zustandes im Kaschitischen dienen.
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Mit der kaschitischen Sprache hatte es aber, wie schon erwähnt, etwas auf sich. Die meröitische Schrift repräsentiert nämlich auf den ersten Blick bereits ein sogenanntes ‘halb-defekives’ Alphabet von 20–(ursprünglich 21)–Konsonantenzeichen und zu diesen drei Hilfselemente zu gelegentlicher Vokalandeutung. Aus den wenigen und zufälligen Vergleichs-Namen bei der ersten Entzifferung entsprang nun die Vorstellung, dass die äthiopische Schrift nur die drei Vokale ē, e, i, unterschied, während graphisch unausgedrückt blieb. Von der Existenz eines o und u und entsprechender Länge aber zeigte jene Schrift nicht die geringste Spur. In Ermangelung weiterer Schlussmöglichkeiten über diese seltsame Erscheinung blieb nichts Anderes übrig, als sich ein akademisches Lese-System zurechtzulegen, ‘als ob’ jene Sprache nur a, ē, e, i als Vokale gekannt hatte, und dementsprechend zu transskribieren. Für Aussenstehende allerdings, welche die Vorstellung von der wirklichen Sprache brauchten, musste solche akademische Abstraktion höchst bekreuzlich wirken. Der Anblick von Namen wie z.B: Mala-tekeli, Kazigē, Kelqell, Makeshakhe, Mapētawe, etc. etc., konnte ja unwillkürlich die Vorstellung aufkommen lassen, die alten Äthiopen seien vielleicht irgendwelche urzeitlich nach Afrika verschlagene Indianer oder dgl. gewesen. Von dieser Sprache führte sichtlich kein leitender Faden zu den Sprachen des heutigen Sudan.

Bei all dem musste jedoch jeder klar werden, dass das kaschitische Schriftsystem bestimmt einen anderen Zweck gehabt haben musste, als den, Vokal-Färbungen graphisch festzuhalten. Nach der akademischen Lesung hätte ja der Gott Usirë bei den Äthiopen Ashēri geheissen; Rom müsste dort Arēme genannt worden sein; auch das einzige damals bekannte Äthiopenwort ettō ‘Wasser’ erschien in der Schrift konsequent als atē; der Name der Göttin Marāk (Mutter des Gottes *Manēū-i) – zeigt das Schriftbild Mrēk, u. dgl. Somit verbirgt sich hinter dem graphischen ē fallweise auch die Reihe der Vokale i, ō und ū. Damit erweist sich von selbst, dass jenes Schriftsystem sich gar nicht um Vokalfärbungen gekümmert hatte, sondern höchstens um Andeutung der betreffenden Konsonanten.

Nachdem es sich bei den ‘Kuschiten-Sprachen’ um eine ganz besonders typische syntaktische Struktur handelt, welcher, wie erwähnt, nicht nur Amharisch, sondern auch Nubisch erlegen war, so müsste man bei Kaschitisch erwarten, dass eben hier die Wurzel jener Erscheinung zu suchen sei. Darum ist die Erkenntnis dieser Sprache von Hauptinteresse für vorliegendes Thema.
der Vokal-länge bemüht war, im Gegensatz zu den Kurzvokal-Andeutungen der beiden andern Zeichen\(^9\). Auf jeden Fall aber erschien es gewiss, dass derart überliefertes schriftliches Sprachgut von jeder Sprachvergleichung ausscheiden musste. Transkriumbles Kaschitisch gab es nicht und damit schien die Griffith'sche Entzifferung praktisch für die Sprachwissenschaft wertlos.

Eine der phänomenalsten Entdeckungen für die Weiterentwicklung an der Materialien Griffith's war nun, wie wir heute ermess en können, die von G. A. Reisner beobachtete Erscheinung, dass innerhalb der hieroglyphisch wiedergegebenen Namen der alten Äthiopenkönige von Napata, die natürlich vielfach einheimisch waren, gewisse Wörter mit ägyptischem Sinn-Determinativ versehen worden sind. So erkannte Reisner alsbald, dass die ägyptisch geschriebene Lautgestalt eines Wortes, welches das ägyptische Determinativ \textit{ufr} 'gut' nach sich hatte, lautlich zu einem schon von Griffith mit dieser Sinnbedeutung erschlossenen, häufigen Epithet der meroitisch geschriebenen Texte passte\(^10\).

Untersuchung weiterer Äthiopennamen auf solche Schreibgewohnheit förderte dann noch drei Nomina und zwei verbale Ausdrücke zu Tage, welche durch ihr ägyptisches Determinativ sinngemäß zu erfassen waren. Es waren das die Wörter für 'Kopf', 'Sohn' und 'Nachkommenschaft', sowie die Ausdrücke für 'bleibend, dauernd' und 'liebend'. Ihre Lautgestalt lag ägyptisch geschrieben fest. Da die so gewonnenen Vokabeln auch in meroitisch geschriebenen Personennamen lautlich aufschienen und dort sinngichtig wirkten, so war durch die Reisner'sche Entdeckung erwiesen, dass die Sprache der älteren Äthiopien \textit{identisch} war mit der der Meroiten-Zeit.

Da sich die Königs- und Adels-Namen beider Perioden als vielfach theophoren Charakters und syntaktisch meist als kurze Nominal- oder Verbal-Sätze erwiesen, kam die Natur der Sprache und ihrer Elementarstruktur lautlich mehr und mehr ins Gesichtsfeld, so dass sich eine gewisse \textit{Copia verborum} ansammelte. Mit diesen versehen wurde es einerseits möglich, die grossen zusammenhängenden Texte epigraphischen Inhalts anzugehen, andererseits aber dem sprachlichen Zusammenhang der gewonnenen Einzelwörter mit dem lok. Sprachgut von heute nachzuspüren. Mehr und mehr stellte sich auch heraus, dass das \textit{Altnubische} zahlreiche Ausdrücke der Verwaltung und des staatlichen Lebens als \textit{Lehn} von Äthiopien aufgenommen hatte, was bis dahin unbekannt war. All diese Dinge wirken zusammen, um schliesslich inhaltlich mehr und mehr Verständnis textlicher Zusammenhänge zu gewinnen, so dass es bald möglich wurde, den Grundriss der Grammatik des Kaschitischen zu erhalten\(^11\). So ist es heute bereits so weit, in vielen Fällen schon rein sprachlich zu beurteilen, was am Kaschitischen 'Kuschitisch' ist, und was nicht.


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Im Nachfolgenden soll nun eine kursorische Skizze der Hauptmerkmale dieser bisher ungreifbaren Sprache Altäthiopiens vorgeführt werden u. zw. mit Rücksicht auf den aussenstehenden allgemeinen Sprachwissenschaftler in approximativ erschliessbarer Lautgestalt.

(a) Lautliches

Nachdem das vermittelnde Schrift-Instrument der Meroitenzeit aus der demotischen Schrift Ägyptens entwickelt worden war, gab es da keine Möglichkeit, stimmhafte Dentale und Dorsale graphisch zu unterscheiden von stimmlosen Lauten entsprechender Art, weil die Ägypter selbst nur Stimmlose kannten. Gelegentliche Transskriptionen, etc., verraten aber, dass das Kaschitische Stimmhafte gar wohl besessen hat. Da ferner die ägyptische Schrift kein Mittel besass, Konsonantenlänge (d.i. Doppelkonsonanz gleicher Art) graphisch zu markieren, so musste auch die Meroitenschrift auf solche Markierung verzichten. Die wirkliche Sprache jedoch besass auch gelängte Konsonanten. Wir müssen uns also damit abfinden, dass die Schrift uns bloss 17 einfache Konsonanten zeigt, vermehrt durch Markierung von Vokalstellen, wo es die Fixierung des Wortbildes erfordert hatte; ansonst gilt, wie schon erwähnt, das Prinzip defektiver, d.h. vokalloser Wortschreibung.

Was den Vokalismus selbst betrifft, so sind wir nach Ausweis vereinzelter Transskriptionen imstande festzustellen, dass das Schema des Schriftbildes mit dem gesprochenen Lautbilde, was Vokalposition und Quantität betrifft, konkurriert. Da es sich aber, wie man deutlich merkt, um eine Sprache mit labiler Vokalbehandlung -(also ähnlich wie Ägyptisch, Semitisch, etc.)- dreht, mit Stammvokalwechsel, Enttonung u. dgl., so ergeben sich naturgemäß auch leichte Abweichungen von dem, was das starr scheinende Schriftbild vermuten lassen würde. Zudem genügte in bestimmten Fällen nicht, den Ort des Vokals zu wissen, sondern man benötigte noch einen zweiten, u. zw. syntaktischen Vokal-Index, den man graphisch durch das Zeichen der Interjektion ‘*i!*’ andeutete, um Missverständnisse der Lesung zu vermeiden. Zu einem einigermassen klaren Sinnausdruck des Kontextes gehörten in der Sprache fünf unterschiedliche i-Suffixe, welche dem syntaktischen Zusammenhang erst verständlichen Ausdruck gaben. Diese Fälle sind:

(1) Vokativischer Anruf.
(2) Subjekts-Index im Nominalsatz.
(3) Indirektes Objekt.

(5) Adjektivische Ableitung (Nisbe).

Ohne solche Markierung wäre Vieles dem Leser syntaktisch zwei- oder mehr-deutig geblieben, da diese vokalischen Suffixe zum Verständnis unerlässlich waren.

(b) Nomina

Eine Auswahl bereits greifbarer Nominal-Bezeichnungen kann gleichzeitig als Unterlage für Nachweis der Sprachverwandtschaft dienen:

*apa 'Vater'
*ende 'Mutter'
*taḍōha 'Gattin'
*terik.ke 'Gatte'
*taḍōha-li 'Mutterkind'
*terik.ke.li 'Vaterkind'
*mate 'Sohn'
*mate-kandi 'Tochter'
*wil 'Bruder'
*wik-kandi 'Schwester'

*abar 'Mann'
*kandi 'Weib'
*ayi 'Seele'
*hamō 'Kopf'
*biti 'Gesicht'
*bel 'Auge'
*ab 'Mund'
*uluk 'Ohr'
*iy 'Hand'
*tak 'Fuss'
KUSH

*šah 'Kuh'
*le 'Schaaf'
*ḥa 'Milch'
*šar 'Korn'
*lōt 'Gemüse'
*ata 'Brot' (āg.)
*erpe.ke 'Wein'

*əd 'Land'
*əd-bali 'Wüste'
*wa 'Territorium, Boden'
*ṭal 'Stadt'
*yer 'Wasser'
*(y)ettō 'Flusswasser'
*anđē 'Katakrtk'

* kèrē 'König v. Meroë' ('Rufer')
*nīs 'König von Kash' (āg.)
*psintī 'Nachkomme d. Prinzen von Kash' (āg., -āth.)
*akrūrī 'kgl. Amtsträger'
*agendē 'Prokurator'
*urō 'Basiliskos' (āg.)
*ponn.ena 'Gouverneur' (āg., -āth.)
*mete 'Phylarch'
*te.ide 'Anordnung, Gesetz'

Adjektivische Ausdrücke jeglicher Bildung treten unmittelbar hinter das näher zu bestimmende Nomen. Es heisst also *ettō mlū 'gutes Wasser' *mak kende 'fürsorgender Gott', *Pi.tak kō 'das heilige Philae', *Aritinyi Tergi-li 'der zu Tergis gehörende Gott A'.


Determiniert wird das kaschische Nomen durch ein (ursprünglich demonstratives) Suffix -l: *masa 'Sonne', det. Masā-l 'der Sonnengott', *(y)ettō-l 'der Nil', *əd-bali-l 'das Wüstenland'. Beim adjektivisch bestimmten Nomen tritt dieser Artikel hinter das Adjektiv, wie *pawide mlū-l 'der gute Herrscher', *wīse kō-l 'die hehre Isis', *ōr awide-l 'der ältere Hor' (Haroeris).


Rektion in der Genetivverbindung besteht einzig nach der Folge Regents—Rectum. Das Rectum ist syntaktisch ein adjektivisch stehender Ausdruck mit dem (rückweisenden) Relativ-Suffix -s (-sa), welcher an sich selbst als Nomen gebraucht werden kann. Im Falle der genetivische Rektion steht dieses relat. Suffix enkliatisch reduziert, z.B. *mas kērī-s 'Offizier des Königs', *mantar wa-s 'Grenzort des Landes', *mate ank-ābe-s 'ein Sohn von Adeligen'.

(c) PRONOMINA


12 Überrest dieses kaschischen Plurals dürfte die heute noch bei Bedja und Nubiern gebräuchliche Endung -āb als Bezeichnung des Stammkollektivs sein, wie: Ḥannik-āb, Mitkin-āb, Gezn-āb, Bodjw-āb etc. etc.
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Bemerkenswert hieran ist ausser der sehr archaischen Form (vergleichen mit den ältesten ägyptischen Typen), die Unterscheidung des grammatischen Geschlechts\textsuperscript{13}.

Bei possessivem Gebrauch trat das Personalpronomen (wie im Ägyptischen) enklitisch hinter das zugehörige Nomen unter entsprechender lautlicher Verkürzung; Beispiele: *mak-i ‘mein Gott’, *taka-k ‘dein Leben’, *ayi-ś ‘seine Seele’ u.s.w. (z. plur. einmal *-kana oder *-kna belegt).

Demonstrativ-Pronomina kennen wir bisher nur singularisch u.zw. p ‘dieser’ und t ‘diese’ (fem.), vermutlich *po und *to vokalisiert.


Ein indefinite Pronomen *ye fungiert häufig als Kopula in Nominalsätzen, wie z.B. *Wise Mikhe ye ‘Isis ist Mikhe’ (d.i. Göttin des Reichtums); *Hara Mandū ye ‘Hara ist Mandū’ (d.h. identisch mit *Mandū-l). Es dient also sichtlich zum Ausdruck der Identität.

Hierher gehört auch die häufige Kopula *lō, zur Feststellung einer Tatsache, wie z.B. *mlū-lō ‘er ist gut’, *a-kra-lō ‘es ist, ich habe rezitiert’. (Diese kaschitsche Partikel ist auch in den, wie wir heute sehen können, stark kaschitsierten Litteratur-Dialekt des Alt-Nubischen eingedrungen.)

(d) Konjugation

Das Grundsystem des verbalen Ausdrucks im Kaschitschen basiert auf der Präfigierung eigener Verbal pronomina vor die Verbalwurzel. Dieselben sind:

<table>
<thead>
<tr>
<th>Singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td>3.m. y-...</td>
<td>3.m. y-... -b</td>
</tr>
<tr>
<td>3.f. t-...</td>
<td>3.f. (unbelegt)</td>
</tr>
<tr>
<td>2.c. t-...</td>
<td>2.c. (unbelegt)</td>
</tr>
<tr>
<td>1. *...</td>
<td>1. n-...</td>
</tr>
</tbody>
</table>


Unter der Decke des defektiven Schriftsystems bleibt, bis auf vereinzelte Sonderfälle, verborgen, dass es sich dabei nicht um ein einziges Thema der Konjugation handelt, sondern dass wir es zumindest mit drei modalen Themen bei labiler Vokalisation zu tun haben. Eines dieser Themata, u.zw. mit optativisch-jussivem Sinn, war durch ein betontes -ō am Schlusse des Verbalstammes gekennzeichnet; zum Verbem *take ‘leben’ lautete die 3.p.m. des Singulars *yi-tkō ‘er soll leben’; desgleichen im Passiv – (sogar in der Schrift gekennzeichnet !) – *yi-ni,tkō ‘er soll belebt sein’. In den beiden andern Themen werden wir wohl nur die Formen des (vokalisch unterschiedenen) perfektischen und imperfektischen Ausdruckes zu sehen haben.

Wie im Semitischen erscheinen auch im Kaschitschen die verbalen Grundstämme durch bestimmte Präfixe erweitert, so Kausativ durch *ši-, Passiv durch *ni- und Reflexiv durch *t-. Diese Stammerweiterungen sind schon seit 25 Jahren bekannt. (Meinhof).

\textsuperscript{13} Zu bemerken ist hier, dass bei den Kaschiten Götter und Tote ohne Unterschied des Geschlechts als Maskuline behandelt werden. (Letztere, weil Jeder, Mann oder Frau, nach dem Tode Osiris geworden ist !)
KUSH

Das Kaschitische besass noch eine spezielle Form für den Ausdruck der Dauer (Permansivum) durch Konstruktion des Nomen actionis mit den possessiven Person- suffixen als Prädikat von Aussage-Sätzen, z.B. *öröse.li taka-k 'ruhmreich (ist) dein Leben', *dimme-š ahide 'sein Zufriedensein (ist) vorteilbringend' u. ä., wo wir eher sagen würden: 'ruhmvoll hast Du gelebt' resp. 'Ist er zufrieden, so ist es gut' (scil., für die Umwelt).


Am besten zeigt sich der Gegensatz zum streng gebundenen Verbalstamm des Semitischen darin, dass z.B. das Objekts-Pronomen der 1. Person sing. *-ni 'mir, mich' im Kaschitischen noch durch solche syntaktische Elemente von der eigentlich Verbalform getrennt auftreten kann, wie z.B. *yi-bahe-sa-ni 'er hat mir darzubringen' (Meroë 9, lin. 12 und bei pluralenum Subjekt *yi-bahe.b-sa-ni 'sie haben mir darzubringen' (Meroë 8 a, b, lin. 4–5).

Derartige Merkmale verraten, dass das Kaschitische noch aus einer vorsemitischen Entwicklungstufe gestampft hat, in welcher der starre Trikonsonantismus noch nicht fixiert gewesen war. Wir haben es also mit einem ansonsten historisch nicht mehr greifbaren Frühstadium semitischer Sprachentwicklung zu tun.

Um ein ungefähres Bild vom eigentlich Sprachlichen im Kontext vor Augen zu führen, sollen hier noch zwei einfache, strukturell durchsichtige Sätze zur Anschauung kommen:

*šad.atTmit Wise tew-webbi-te-l lō. (Griffith, Inscr. 75–9–10).
' (Steuer-) Herrin des Nordlandes ist die Isis vom heiligen Berg'.

*twist-i Pakhöpe a-so . . . . .
yindi Aarendt Etebe-te-l-le a-кра lō. (Ibid. Inscr. 125).
'Dieses Proskynema, das ich P. geschrieben, . . . . habe ich heute vor Harendotes von Bigga rezitiert'.

Schon der äusserliche Habitus der kaschitischen Sprache verrät auf den ersten Blick, dass diese Art Sprache heute weder im Gebiete des Nil-Sudan, noch auch im Raume der nordabessinischen Hochländer existiert. Sie kann daher in keiner Weise als historisches Bindeglied für die 'Kuschitisch' sprechenden Völkerstämmchen gelten. Fehlen ihr ja doch nahezu alle Kriterien, welche den Typ der seit Reinisch immer als
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Mit der Entdeckung des kaschitischen Sprachtypes gewinnen wir nun für den erythräischen Raum des Nil-Sudan eine Reihe von Aspekten bezüglich Sprach- und Völkergeschichte daselbst.

I. Alt-Epoche (ca. 3000–2270 v. Chr.)

Das der Halbinsel Arabien gegenüberliegende afrikanische Küstengebiet, besonders aber die Landschaften des rechten Nilufers, wies dazumal sicher eine relativ dichte Bevölkerung von Jägerstämmen, etc., unterschiedlicher Art auf. Ihre, uns heute nicht mehr greifbare Sprache resp. ihre Sprachen hinterliessen in den Idiomen späterer Neuankömmlinge den bisher rätselhaften ‘Erythräischen Komplex’ (Voranstehendes Adjektiv—Genetiv : Rektum—Regens—Postpositionen —Starres Nominalverb mit Modal—Suffixen) Ähnlich lagen die Dinge auch im Raume der südlichen Hochländer.

II. Kaschiten-Epoche a (ca. 2270–1970 v. Chr.)


III. Kaschiten-Epoche b (ca. 2000–1085 v. Chr.)

wicht jedoch dem Denksystem der eingeborenen ‘Erythräer’. Ihre Nachkommen wurden die nachmaligen Buga und heutigen Bedja einerseits, sowie die heutigen Sahō-Stämme andererseits. Erwerbszweig blieb Viehzucht; zur ‘Kultur’-Entwicklung war kein Raum gegeben.


IV. Kaschiten-Epoche c (ca. 1085 v. Chr.–361 n. Chr.)


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Aksùm feindliche Koalition verbot\textsuperscript{14}. Nicht lange vorher war ja der Blemmyerfürst *Hara-Maṇḍič-ye durch den König von Meroë als Hüter der Flusenge Báb el-Kal’a-Bšē und zum Prokurator des Nordlandes im Raume von Primis—Tergis—Selele eingesetzt worden, so dass Äthiopien nun von Römisch-Ägypten isoliert und gesichert war. (Griffith, Inscr. Nr. 94.) Solche Manöver wünschte sich Aksùm nicht. Als dann aber die bekannte Nubier-Politik des letzten Königs von Meroë einsetzte, kam es zum Feldzug Aïzāna’s von Aksùm gegen Meroë (360–61), welcher dem kaschitischen Sudanreich ein schnelles und nachhaltiges Ende machte\textsuperscript{15}.

Schon dieser noch sehr lückenhaft Überblick über die Geschichte des altäthiopischen Nil-Sudans verrät uns, dass das kaschitische Reich und seine Sprache ganz und gar keinen Schlüssel für die Konstruktion eines einheitlichen Ursprungs der \textquote{Kuschitisch} sprechenden Völker zu liefern imstande ist. Dieses ehrwürdige Grossreich bildete sogar durch die Zeit seines Bestandes hindurch ein politisches Hindernis dafür, dass der \textquote{kuschitische} Sprachtyp, wie er sich am Rand und auswärts des Äthiopenreiche vielfach herausgebildet hatte, zum herrschenden Typ im Raum der Nil-Sudan-Länder geworden ist. Wie stark der geistige Druck des \textquote{kuschitischen} Komplexes gewesen war, das erkennt man noch daraus, dass das ganz andersstämmige Nubisch zur Zeit des Altnubischen noch die Genetivstellung Regens—Rectum besessen hatte, dieselbe aber alsbald zugunsten der Position Rectum—Regens aufgegeben hat.

Es steht also zu erwarten, dass bei hoffentlich mehr und mehr steigendem Interesse für die Erforschung des Kaschitischen zwei sprachgeschichtlich-ethnologische Probleme einer endlichen Erkenntnis und Klärung zugeführt werden können: Das eine Problem ist die Verarbeitung der litterarischen Denkmäler des Kaschitischen zur Klärung der historischen Stellung dieser Sprache zum Semitisism. Das zweite Problem aber ist die Konkordanz-Untersuchung jener \textquote{Kuschiten’}-Sprachen, welche noch deutliche Spuren eines tragenden oder weichenden Substrats alter Semiten-Invasoren mit Semitischer Stufe 1 zeigen. Was dann bei dieser Sonderung übrigbleibt, kann hernach als Basis für die eigentlich afrikanistische Frage nach dem historischen Träger des hier soggennanten \textquote{erythräischen Komplexes} verwendet werden.

\textbf{Summary}

In accordance with Reinisch and Lepsius, Africanists are accustomed to use the term \textquote{Kushitic} to designate the indigenous languages of some north-eastern tribes inhabiting the countries along the Red Sea coast and some northern districts in the highlands of Abyssinia.

The use of that term seems at first to suggest a well-founded connection with the ancient land-name \textquote{Kūṣh}—called \textquote{Ethiopia} by the Greeks—according to our biblical and historical tradition. The biblical \textquote{Kūṣh} was a \textquote{son of Ham} and therefore it seemed to be convenient to suppose that all languages, hitherto called \textquote{Hamitic}, southwards from Egypt may have been of \textquote{Kushitic} origin.

\textsuperscript{14} Conti Rossini, \textit{Storia di Etiopia} 1, Milano, 1928, pp. 130–31.

\textsuperscript{15} Die Träger des gross-äthiopischen Staates mit ihrem rein familiar ausgebautem Stände-System waren wieder bis auf ihr altes Stammland Barkal zusammengeschürmt, und führten nach 361 nur mehr ein Schattendasein als verarmte ehemalige Herrenleute gegenüber ihren vormaligen nubischen Prätorianern. Sogar der Titel \textquote{König von Äthiopien} wurde von da an durch den König von Aksum in Anspruch genommen und weiter vererbt.
KUSH

In the time of Lepsius, a number of remarkable inscriptions were found in the area of ancient Ethiopia, which seemed to be written in the still unknown idiom of 'Kush'. According to Lepsius these new materials could perhaps have represented a kind of Old Beja language. In opposition to this hypothesis, Reinisch preferred to see in those fragments of literature remnants of an early Nubian dialect. Both opinions remained at that time without any certain philological support.

In spite of the uncertainty in the points mentioned above, the terms 'Kush' and 'Kushitic' have been willingly accepted by most scholars who were not conversant with the true problem.

Since the beginning of the 20th century, new data have been made available from the ancient Ethiopian inscriptions excavated in Meroë, Karanog, Shablul, Philae, etc., which were then ingeniously deciphered by the famous Egyptologist F. Ll. Griffith (1911–12).

Both the problem of the writing and the problem concerning the contested nature of the language spoken in the ancient 'Kush' were then solved, though with quite a negative result: neither Old Nubian appeared there in the Ethiopian inscriptions, nor Old Bedauye. What one could learn from it was merely a theoretical skeleton of a new and still entirely unknown language, enriched by Egyptian expressions, but unfortunately written in what seems to be a semi-defective alphabet.

Of momentous interest was the impression given by the newly discovered inscriptions that the unknown language could have possessed only the vowels a, e, ë and i, as Griffith had supposed. Comparison of the personal and divine names, newly found, with their equivalent in Meroitic letters, brought out the evidence that in the spoken language of Ethiopia vowels like o, õ, and û must also have existed although they were not indicated in the written language. ('Das meroitische Sprachproblem': Anthropos 1930, by the author of the present essay.)

Proof was given too in the same essay, mentioned above, that the special and local stem-name of Ethiopia had evidently been Kîš (egypt. Kśš). Therefore we should not wonder at the fact that Aksumitic kings were calling the Ethiopians of Meroë by the name Kâś-û; nor should we be surprised at the substitution of the Aksumitic ã for the vowel û in the Canaanite land-name 'Kûš', which shows the well-known change of ã to û, a development which normally took place in Egyptian pronunciation, especially at the end of the New Kingdom. Indeed the historical root-vowel in the national stem-name of Ethiopia must, according to that observation, have been an a. The pronunciation Kash seems, therefore, more suitable than the foreign Asiatic form Kûš.

The third and most promising attempt in the above-mentioned essay was to try to find out at least the theoretical tenor of a longer, and apparently narrative, offering text from Philae. Attention was paid first of all to the verbal expressions used in this text, which showed some well-known Semitic characteristics (though the general context of the inscription could hardly reflect a real Semitic language), as they are so far known to historical Semitists today.

In addition to this view, it must be stressed here that one could see from the very syntax in the above-mentioned text from Philae, that the language used in it was not a 'Kushitic' one. Thus it offered no grounds for the general belief that the native idiom of ancient Ethiopia could have been the ancestor of the so-called 'Kushitic' languages.

By reason of these facts, and in order to avoid a further confusion with the fictive term 'Kushitic' languages, it seems to be better to avoid henceforth the term 'Kushitic' altogether and to prefer instead the more correct term 'Kashitic' and to distinguish in this way the native language of Kash from that quite different linguistic group.
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The points discussed in the first part of the essay are continued by a brief grammatical sketch, selected from the results of twenty years' special research on the longer Ethiopian texts, as far as it could be tested by the author of the present essay*. This sketch is to inform the reader of the most important characteristics shown by the Kashitic language. These characteristics are rendered with theoretical vocalization as much as possible.

Our increased knowledge of such matters arose from a continually growing stock of words gathered from the examination of Ethiopian proper names, the foundation of which was the important discovery of G. A. Reisner that some names of early Ethiopian kings were written in Egyptian hieroglyphs, showing the use of common Egyptian determinatives behind some components of these theophoric names. ('Excavations at Nuri VII, H.A.S., 1918.) Further comparison of such names to the numerous proper names of the Meroitic epoch proved clearly the fact that the idiom of both was identical. This was due to the fact that such components appeared in the Meroitic names as, for instance, the early determined expressions for 'son', 'head', 'good', 'loving', etc. A number of Kashitic nouns, adjectives, etc. were in this manner ascertained from their syntactical position in nominal sentences. The result of this investigation was the unexpected evidence that the literal dialect of Old Nubian had been largely enriched by Ethiopian terms, especially concerning political life, administration, theology, etc. Such foreign expressions used by writers of Old Nubian literature let us see really vocalized forms of Ethiopian origin.

We hope that this newly gathered Kashitic material may encourage the student to look now for other signs of connections between the recently emerged language of Kash and, for instance, the so-called 'Northern Kushitic' group of languages such as Bedauye, Saho, Afar, etc. However, with regard to the latter group, one must be careful not to rely on single specimens of their vocabulary as the final test of affinity, especially if one considers the very different grammatical features which separate each member of that group from Kashitic grammar. If there was really a kind of former relation, it must remain limited to the Pre-Semitic features of both philological types, concerning which very little is so far known.

What we do know is that the area of the so-called 'Kushitic' tribes lies right outside the area belonging to the excavated sites of ancient Kush.

What we further know is the fact that the Kashitic territories had always lived in continual defence against the greedy dwellers of the deserts. (See, for instance, the official function, in Meroitic times of the 'pelmis ad-bali-to'—'Commander (strategos) of the deserts'.)

Thirdly, we can conclude that since the roads connecting the deserts along the whole Red Sea coast and the countries of the Middle Nile passed through the kingdom of Kush, its ancient history must be inseparably bound up with that of the Red Sea districts and their tribes, of which we have only little documentary evidence.

In view of these points, there is appended to the present essay a brief historical summary of the main periods of ancient Sudan history, from the first Kashites (ca. 2270 B.C.) up to the total destruction of their kingdom (360–61 B.C.).

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* Die Kaschitische Sprache von Napata-Meroe. Attention of the German Academy of Sciences has been drawn to these matters by the recommendation of the excellent Egyptologist Herm. Junker, to promote if possible publication of the work done. (1952). Lack of sufficient means has delayed for the present completion of the task, which will prove very expensive.
The Sudanese Camel Girth

by G. M. Crowfoot

In *Sudan Notes and Records* for June, 1951, I gave an account of the making of a double weave camel girth in Khartoum by Sitt Zeinab, a weaver from the Butana, and expressed my surprise at the discovery that so complicated a weave was actually carried out on the simplest of looms with only the aid of one rod heddle, a shed rod, a pair of camel ribs, and a pointed wooden pin. Here I should like to carry the enquiry further and show how the weave compares with other instances of double weaves made on primitive looms, in the past and at the present day. On *Plate II* I show a photograph of the Sudanese weaver and a diagram of the way the rib beaters are placed to hold the front and back sheds, which, with the weaving directions on p. 37, should make the character of the weave clear. The warp is set up in pairs, one of each colour, and the pattern appears in reverse on the back of the weave. There is only one weft.

The closest early parallel to the Sudanese weave that I know of is the piece of a girth shown on *Plate I*. 1, 2, from The University of Michigan Excavations at Karanis (Kom Aushim) in Egypt, an ancient site on the East bank of Birket Karun, the Lake Moeris of the Greeks. This piece was published with other textiles from Karanis in 1933 as 'a band of dark brown and yellow hair with a few threads of red (dyed) woven in bars and checks'. The weave was not described and seems to have been something of a puzzle as the suggestion is made that it might have been 'an ornament, probably for a caparison or hanging'. Struck with the resemblance between the illustration and Beduin weaves, I wrote to the late Dr Orma F. Butler of the Museum of Classical Archaeology at Michigan to ask if the band had a pattern on the back and if so, if it could be a double weave. I received the excellent photograph shown here which leaves no doubt as to the character of the weave. It is rather coarser than the Sudanese example shown with it *Plate I*. 3, 4), but the resemblance of the weave is exact and so is part of the pattern, the bars and checks. In the Karanis piece variety is given to the pattern by the four red threads in the warp mentioned above; these cannot be distinguished in the photograph, but are shown in the drawing, *Fig. 1*. The warp, as in the Sudanese piece, is S-spun, Z-plied, but the weft is a thick S-twist of two Z-plied threads.

The textiles of Karanis are not from robes treasured in the coffers of the tombs of ancient kings or from the burial clothes of Coptic Christians, but from the rubbish heaps in the corners of rooms, just pieces of rag, gathered up with care and studied since. Their value is that they have revealed what was being made and worn in that part of the town dated to the late third and early fourth centuries A.D. At this time wool had become more popular than linen; 3450 woollen pieces are recorded, 350 of linen and 100 of hair. There were some of the well-known linens with tapestry borders and roundels in purple or in colours, dark blue, red, green and shades of 'rose, lavender or amethyst'. In addition there were examples in twill, and patterned pieces, listed as drawloom weaves, but which Mr Thomas Midgeley of Bolton, who also examined them,

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1 L. M. Wilson, *Ancient Textiles from Egypt*. University of Michigan Studies, pl. x. 121, p. 47.
PLATE II

1. SITT ZEINAB WEAVING A CAMEL GIRTH

SUDAN DOUBLE WEAVE

THE CAMEL RIB BEATERS ARE TURNED ON EDGE CAUSING THE LOWER THREADS TO FALL AND MAKE THE BACK SHED.

1. WHOLE OF HEDDLE THREADS.  2. WHOLE OF SHEDROD THREADS
   1.a. HALF HEDDLE THREADS  1.b. HALF HEDDLE THREADS.

2. DIAGRAM TO SHOW FRONT AND BACK SHEDS
THE SUDANESE CAMEL GIRTH

could have been more easily woven on multiple heddles. It was a time when Egypt was prosperous and industrious, famous for the export of glass, paper and textiles, and Milne tells us that 'no occupation, save that of husbandman, is so commonly mentioned in the papyri as that of the weaver.' But what of the camel girth? It seems incongruous in company with these more sophisticated products. But Karanis was on the desert edge, like others of the Greek cities, Dimeci to the north and Bacchios to the east; they were points of departure for caravans to the Libyan oases, or the Sahara. This is proved by the lists of customs dues and charges for maintenance of the desert roads which include an ad valorem charge of 3 per cent on all goods coming in or out at Karanis. And charges for camel tickets were made at the head of all the desert roads to the Fayum. Among the tolls on a stele at Koptos the actual figures paid are given and among them is: 'camel tickets, one obol.' So we may imagine that in that distant past also it was the dwellers in tents who wove the camel girths and not the craftsmen of the town—the torn end of the girth is a relic of their passing.

Much later in time the incoming Arabs no doubt brought their craft to the Sudan to add yet another weave to those which had been practised there on the same loom from Ancient Egyptian days.

The resemblance between the weave of the modern girth and that of ancient Karanis is exact and easy to understand, but to make a comparison between it and double weaves made elsewhere it is necessary to mention the chief classes of the weave. They are:

A. With warp and weft visible (a) with no exchange of thread, (b) with exchange of threads.

B. With warp alone visible (a) with no exchange of threads. (b) with exchange of threads.

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2 Karanis was richer in glass than any other single site.


When there is no exchange of threads for pattern, the weave will be hollow, joined only along the edges; and even when there is exchange of threads, some portions of the pattern may be hollow, while some are tightly interlocked.

The Sudanese camel girth is in Class B b, for the warp alone is visible and it has exchange of threads to make a pattern. It may seem strange that the most famous double weave of antiquity, the Girdle of Rameses⁵, in fine linen of several colours, also falls into this category, though it is a much more complex weave. Although in parts it has four warp threads to form the two layers of the weave, in others it has five, and by means of the extra thread the pattern is enriched, and instead of being in reverse at the back as in the camel girth is identical with that on the front.

We have to go to those master weavers, the people of ancient Peru, to find anything to touch this unique piece. There the first double weaves appeared in the early centuries A.D. among the amazing textiles and embroideries laid with the dead in the Paracas Cavernas. They are of all classes but mainly that in which double warp and double weft make the pattern; later, as well as double, triple and quadruple cloths were made. Attractive in their variety of designs, they were prized in museums and much discussed long before the method of their making was understood. In 1937, Kurt Hentschel published his study of Peruvian double weaves with examples of his own making⁶. He describes how he saw these textiles in the Berlin Museum für Völkerkunde he could not imagine how they had been woven. He could have produced them himself on a Jacquard, but as he remarks, that loom was not invented till 1805. But on a visit to Scandinavia he found that double cloth had been made there in olden days, and countrywomen in Sweden and Finland still knew the art. They used four heddles on their treadle loom, together with the aid of three wooden rods, one, the ‘ploca’ to pick up pattern threads, another, flat, to hold the shed open, and the third, round, to divide off groups of threads. On his return to Berlin, he looked at the textiles there with new eyes. Among them was a modern Huichol loom from Mexico set up for double weave and he realized that of the outfit in place, three rod heddles and a shed rod were the equivalent of the Scandinavian four heddles, and a pointed and a flat stick of the ‘ploca’ and the shed opener. He then set up model looms and wove many pieces in imitation of the ancient ones, illustrations of which are in his study together with practical directions for working.

A yet simpler method appears to have been in use among the Mayas. There is a small loom in the British Museum of the Chimu period⁷, with a double weave in brown and white cotton showing an amusing design of cats and scrolls. The loom has two rod heddles and a shed rod and no doubt other rods were used as well. I tried to weave a piece in imitation of this but found it very difficult to manage a pattern that was in both warp and weft instead of only in the warp as in the camel girth.

Designs as complicated as this are still woven in parts of Mexico on the back-strap loom, in which one end of the warp is fastened to a post and the other to the belt of the weaver, and many examples are to be seen in the Pitt Rivers Museum at Oxford. In an

⁵ *LAAA*, vol. v, nos. 1 and 2, Thorold D. Lee, ‘The Linen Girdle of Rameses III’; *LAAA*, vol. x, nos. 1 and 2, G. M. Crowfoot and H. Ling Roth, ‘Were the Ancient Egyptians conversant with tablet-weaving? ’


⁷ T. A. Joyce, ‘Note on a Peruvian loom of the Chimu period’, *Man*, vol. xxii, 1 Jan. 1922. This period is before the coming of the Spaniards.
occasional paper on the collection there Laura Start gives photographs and descriptions by Elsie McDougall of the warping and weaving of double cloth by women at Zinapan, Hidalgo, Mexico, in 1936. The loom has three rod heddles and a shed rod as well as two other rods, one pointed, the other flat. The author comments on the ability of the women to make these complicated patterns in reverse without any aids and says that they 'seem readily to visualize figures in either of two positions,' which makes me blush to think how often I had to look at the backs of my Peruvian cats to see if they were in the right place.

It is now apparent in what ways the Sudanese camel girth weave differs from the others cited, and in what its special interest lies. To get the four warps needed for the double web the Scandinavians used four heddles, the Huichol Indians and those of Zinapan three rod heddles and a shed rod, the Maya only two heddles, and all had extra rods to pick up threads and open sheds as required. Probably for more complicated weaves the workers had more rod heddles, the Ancient Egyptians five or four and a shed rod, the later Peruvians six or more. The Sudanese method is then the most simple and no doubt the most primitive as the fingers play so important a part, reminding us that there was once a day when all the weaving was done by fingers alone without the help of any heddle.

Although the weave is so simple it yet can give patterns that are very attractive and can be varied at the pleasure of the weaver within the compass of the breadth of the material, and be made in any colour desired. All the patterns of the Western Desert and Trans-Jordan tent weaves can be carried out, for they also are set up with a double warp on one rod heddle and a shed rod and chosen out in the same way with the fingers, only that the unwanted threads fall to the back. Once when a Sudanese friend saw an Amria rug in my room at Khartoum and we were examining it together she took one look at the floating threads at the back and said: 'Poor things, they only know half the pattern!' Of course, double weave is limited in its uses, it is chiefly good for belts and bags, and girths or any such article subject to hard wear. I wondered if it was still practised in the Sudan and asked Mr Basil Waterfield, during his time there, to make enquiries for me. He brought me back a fine girth to see, presented to him by Sheikh el Taib el Sillihabi of the Jaalin, it was procured near Kassala but was believed to have been made in the Butana. My weaver friend, Sitt Zeinab, also came from Abu Deleig in the Butana. She was of the Batah, and it would be interesting to know if other tribes also have this craft.

At the end of my study I feel as Kurt Hentschel did the desire that the beautiful old handicraft should not cease, but continue to be useful and to give pleasure to the workers, that they should have, as he says: 'ein Stückchen Freude mehr im Leben'.

APPENDIX

Weaving Directions

The warp is set up double in two colours. Two threads, one of each colour, go through each loop of the rod heddle, and two over the shed rod. To get the pattern the threads are chosen by eye, lifted in the fingers, and held by a beater. The following gives the order of the weaving:—


The heddle threads are raised and held by beater B. Half these threads are chosen, beater A inserted, beater B removed, a throw of weft made and beaten up and beater A left in position in the shed.

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2. **Second Throw. Back Shed. Shed rod raised.** (See Plate II. 2).
   (a) The shed rod threads are raised and beater B inserted. Both beaters are turned on edge. Result, rejected half of heddle threads fall and make shed at X.
   (b) Beater A is removed, inserted at X, a throw made and beaten up, beater A is then removed and beater B left in position.

3. **Third Throw. Front Shed. Shed rod raised.**
   Half the shed-rod threads are chosen, beater A inserted, beater B removed, a throw made and beater A left in position.

4. **Fourth Throw. Back Shed. Shed rod flat.**
   (a) The heddle threads are raised, beater B inserted. Both beaters are turned on edge. Result, the rejected half of shed-rod threads fall and make shed at X.
   (b) Beater A is removed, inserted at X, throw made and beaten up. Beater B is left holding heddle threads in preparation for 1.
Varia Grammatica
par Werner Vycichl
(Paris)

I. LA FORMATION DU PLURIEL NOMINAL EN VIEUX NUBIEN

Le problème

Parmi les problèmes que pose encore la grammaire du vieux Nubien figure la formation du pluriel nominal du nom déterminé. Zylharyc cite dans sa grammaire (Grundzüge, § 79, b et § 80, a, b) trois formes différentes selon la position des éléments de la détermination (-l, -il) et du nombre (-gou):

1. des pluriels de noms déterminés en -(i)l-gou (§ 79, b),
2. des pluriels déterminés de noms indéterminés en -i-gou-l (§ 80, a) et
3. des pluriels déterminés de noms déterminés en -(i)l-gou-l (§ 80, b).

Nous nous trouvons donc en présence de trois formes dont la signification ne doit pas être la même. Il s'agit précisément des cas suivants:

1. pan-il-gou, 'les affamés' (Stauros xxiv, 9-10).
   odd-il-gou, 'les malades' (Stauros xx, 3).
   dio-l-gou, 'les morts' (Stauros xix, 4).
2. oukr-i-gou-l, 'les jours' (Ménas xv, 4).
   ouran-i-gou-l, 'les scribes' (Matthieu II, 4).
   meddijn-i-gou-l, 'les esclaves' (Ménas xv, 15).
   ta-ii-gou-l, 'les jeunes filles' (Ménas II, 8 et xiv, 9).
3. ani-gou-l, 'les vivants' (Stauros iv, 1).
   psall-il-gou-l, 'ceux qui chantent' (Canons xxix, 9-10).
   dio-l-gou-l, 'les morts' (Stauros iv, 1).
   pisteuo-l-gou-l, 'les croyants' (Stauros xii, 6).

L'interprétation des formes

La distinction subtile que fait le vieux Nubien entre noms et désinences déterminés et indéterminés est un fait unique qui ne se retrouve dans aucune autre langue connue.

Mais ce n'est pas à cause de cette singularité que je me permets d'émettre des doutes sur l'interprétation donnée de ces formes, mais :

—parce que le sens des passages ne la justifie pas, et
—parce qu'il semble que chaque mot cité ne puisse former qu'un seul pluriel déterminé (jamais des types 2 et 3).

En effet, nous trouvons ta-ii-gou-l à deux reprises, de même deux fois oukr-i-gou-l (Griffiths, p. 112), mais jamais deux pluriels déterminés différents du même nom.

On peut même aller plus loin. Dans la liturgie de la Croix (Stauros, voir Griffith, p. 44-6), voisinent les pluriels du type (1) avec des pluriels indéterminés dans les mêmes conditions grammaticales. En effet, cette liturgie comporte 47 phrases de la même construction grammaticale p. ex. :

(a) istauros-il kaue-il-gou-na kitting-alo, 'la Croix est le vêtement des nus (en grec : stauros gymnōn skēpē) no. 27 (zz).
(b) istauros-il poksid-il-gou-na soudd-a-lo, 'la Croix est le bâton des paralysés' (en grec : stauros khōlōn baktērēta) no. 5 (g ?).
KUSH

Si le génitif de la phrase (b) est indéterminé, il n’y a aucune raison pour qu’il soit déterminé dans la phrase (a) qui présente la même structure grammaticale. Il est certain, qu’il faut dire, en français, ‘la Croix est le vêtement des nus’ et ‘la Croix est le bâton des paralysés’ mais, à mon sens, dans ce cas le texte Nubien a été calqué sur le modèle grec, où les génitifs sont indéterminés (angelos-ri-gou, pokod-i-gou).

Parallèles grecs et nubiens

La liturgie de la Croix contient, en dehors des deux exemples précités, encore plusieurs cas de noms du type (1):
—istauros-il di-o-l-gou-na ēmarr-a-lo, ‘la Croix est la résurrection des morts’
—istauros-il teu-o-l-gou-na dau-a-lo, ‘la Croix est le chemin des errants’
  (grec : stauros peplanēmenōn hodos). 3 : c.
—istauros-il odd-il-gou-na iatorōs-a-lo, ‘la Croix est le médecin des malades
  (grec : nōsountōn iatōs). 8 : uu.
—istauros-il diñar-o-l-gou-na sauatan-a-lo, ‘la Croix est le rempart des combattants
  (grec : stauros polemoumēnōn teikhos). 12 : r.
—istauros-il pān-il-gou-na parou-a-lo ‘la Croix est le pain des affamés
—istauros-il oei-tak-o-l-gou-na esogister-a-lo, ‘la Croix est la consolation des
  opprimés (grec : stauros thlibomenōn ānēsis). 42 : w, etc.

La traduction française ne tient pas compte de l’indétermination des noms nubiens et grecs. Je pense, qu’il s’agit effectivement, dans tous ces cas, de noms indéterminés ou plutôt de participes indéterminés :
(a) participes actifs :
  (1) oddi-l, ‘malade’ du verbe oddi, ‘être malade’ (nubien moderne, j’ai souvent entendu oddi-bā-l comme participe duratif).
  pān-il, ‘affamé’ littéralement ‘ayant faim’ (moderne fān).
  (2) di-o-l, ‘mort’ (participe passé).
  teu-o-l, ‘errant’ littéralement ‘ayant perdu le chemin.’
  diñar-o-l, ‘ayant combattu.’
(b) participes passifs :
  (1) participes passifs du présent : néant.
  (2) participes passés : toshki-tak-o-l, ‘assoiffé’ ou ‘desséché’ (toskhi).
  oei-tak-o-l, ‘opprimé’ [d’après le grec].

Les mots du type (1) ne sont donc pas des pluriels de noms déterminés, mais des
pluriels indéterminés de participes.

Oukr-i-gou-l, ‘les jours’

Les formations du type (2) sont, par contre, des pluriels déterminés : man eit-il-lon
  ta-n kok-kane-n oukr-i-gou-l kir-i-n-ou-an-ni, niss-ou Mēna-Kisse-lō djor-a ki-s-na
  Mareōthē-ō ‘mais cette femme, quant les jours de son accouchement furent passés, se
  rendit à l’église du Saint Mēnas (située) dans la Maréotis’ ((Mēnas xv, 4).

Dans ce cas, il s’agit bien des jours (déterminés) de l’accouchement.

Ań-il-gou-l ‘les vivants’

Les exemples de ce type (3) sont des participes simplement déterminés. Un exemple
convaincant se trouve dans la légende de St. Mēnas : di-o-l-gou-l-de ań-il-gou-l-de
kel-ka, ‘(qu’il allait juger) aussi bien les morts que les vivants’.
VARIA GRAMMATICA

La forme *psall-il-gou-l-dal*, 'avec ceux qui chantent, cum cantantibus' se trouve dans un passage dont le sens se laisse reconstituer approximativement comme suit: 'si quelqu’un ne chante pas *(psall-e-men-en)* avec ceux qui chantent Allilouia . . . .' (Canons xxix, 9–10).

*Pisteu-o-l-gou-l* se trouve dans un passage que Griffith traduit comme suit (p. 48): *and all who have believed in the Cross with their whole heart, shall dwell under it.*

Conclusion

Il ressort, de ce qui précède, que le nom nubien forme le pluriel à l’aide de la particule -gou. Le pluriel déterminé ajoute encore l’article -l p. ex. *ouran-i-gou-l* ‘les scribes’.

Par contre, les prétendues formes du nom déterminé (en -il, -l) et suivies de gou ou *-gou-l* n’existent pas; il s’agit, dans ces cas, de participes (voir Lepsius, p. 499 et Reinisch, p. 95, § 280).

La présente étude ne concerne que les rôles des éléments -l (participes), -gou (formatif du pluriel) et -il ou -l la détermination. Toutefois, il semble que l’étude des autres éléments intervenant dans la formation des différentes expressions (-a, -i, -n, -ni, -ri, -ou) doive également être reprise.

Note:—Les formes nubiennes ont été translittérées (lettre par lettre) et non transcrites phonétiquement.

BIBLIOGRAPHIE


II. VIEUX-NUBIEN orpê: ÉGYPTIEN érêp ‘VIN’

(Étude sur la forme nominale nêfèr)

Introduction

Malgré les éclaircissements que nous apportent le copet et les transcriptions (cunéiformes et autres) de mots égyptiens, il reste dans ce domaine encore bien des problèmes à résoudre. Le preuve en est qu’on n’a jamais essayé de reconstituer un texte égyptien vocalisé ou d’écrire une grammaire au moins partiellement vocalisée. C’est la raison pour laquelle toute indication concernant la phonétique égyptienne nous est précieuse, non seulement parce qu’elle sert à éclaircir l’histoire d’un mot isolé, mais surtout parce qu’elle permet de reconstruire toute une gamme de formes nominales (ou verbales) et d’établir des relations grammaticales et sémasiologiques qui nous ont jusqu’à présent échappé.
Les formes nubiennes

—artos-a djolam orp-a djolam-a, ‘it is simply (?) bread (grec : artos), and simply (?) wine’ (p. 16, K 19, 11),
—orp-a en-kan eille en-kan, ‘whether it be wine (or) whether it be wheat’ (p. 16, K 19, 17 et K 21, 8),
—artos-i ouer-oui orpê sarpê ouer-a lo, ‘one loaf (artos), one finger (?) of wine’ (p. 18, K 31, 12).

La forme du nom est simplement orp (parallélisme artos-a;orp-a). Par contre artos-i ouer et orpê sarpê ouer ne constituent pas un parallélisme parfait et il serait hasardeux d’attribuer à la forme orpê une signification déterminée. En effet, la terminaison -ê (orpê) peut : —
—faire partie du mot (et être tombé devant a dans artos-a, orp-a),
—représenter une terminaison nominale, ou
—constituer le résultat de la contraction d’une voyelle radicale et d’une désinence vocalique.

Toutefois, c’est la voyelle o des formes orpê et orp-a qui présente un intérêt particulier.

L’origine égyptienne des formes

C’est déjà Griffith qui a reconnu l’origine égyptienne de ces formes (p. 70 et p. 110) qu’il compare à l’égyptien y-r-p et au copte ḫr-p.


En effet, nous savons que les voyelles écrites ê et i du copte peuvent remonter à une ancienne voyelle u : abût ‘mois, pl. abêt (B); apôt ‘vase à boire’, pl. aphêt (B); mêt ‘dix’ : cunéiforme mutû; ke (ket) ‘autre’, pl. kouwwe; hyê ‘chemin’, pl. hyowwe; usê ‘nuit’, pl. usowwe; tehnê ‘animal de charge’, pl. tebnowwe; remyê ‘larne’, pl. remyowwe, kiahk ‘le mois Choiak’ : égyptien k’-hr-k’ et cunéiforme ku-iḥ-ku ; trîr ‘four’, comp. arabe tannûr, etc.

Dans ces cas on a certainement prononcé abût, aphôt, môt, kôt, hyô, uô, tébnô, rémyô, kiahk et trûr. Les formes du mot désignant la femme, hîme, pl. hyome s’expliquent par *hyûme, pl. hyóm(u)e.

La forme nominale nêfer

Il y a, en égyptien, une série de noms formés d’après le type *nêfer : —
—vêrêp ‘vin’ (yrp),
—hêhe ‘pauvre’ (hqr), copte hko ‘avoir faim’ (hokêr, f. kkayt),
—mêrê ‘rouge-orange’ (mrê),
—mêše ‘peuple, foule’ (msê),
—pêre ‘calice’, aussi péra (prê),
—wêb ‘prêtre’ (wô), copte wop ‘être pur’ (waab),
—şêre ‘fils’ (šry de šr), verbe šrr ‘être petit’.
La forme mēēše est régulière (comme pōōne ‘verser’ de *pn*). La forme pēra à côté de pēre correspond à sōneh, sōnah ‘hier’. Weēb ‘prêtre’ à voyelle (graphiquement) redoublée correspond a šōōt ‘couper’ de šēd. 4 parmi ces 7 formes désignent des qualités (pauvre, rouge-orange, prêtre et fils comme ‘petit’). Aussi les formes kēm ‘noir’ (Arphkhēmis ‘Horus le noir’ *Har p-Kém), f. Kēme ‘Egypte’ et wēr ‘grand’, f. wērē (Esouēris ‘Isis la Grande’: *Es-Wērē) appartiennent probablement au même type nominal (copte kmm ‘être noir’, égyptien wrr ‘être grand’); or, il semble que Kēme ‘Egypte, la (terre) noire’ soit issue d’une forme *Kummat ou sim. dont la voyelle brève s’est rallongée à une certaine date pour compenser la simplification du groupe mn. La simplification des géminées peut être étudiée facilement aux formes qattāl (p. ex. hako ‘sorcier’, de ḥakkā, plus anciennement ḥakkā, etc.). Aegyptus, 1954, p. 85.

La qualité de la voyelle u du type nēfēr

Chacune des 3 voyelles primitives a, i, u apparaît en copte sous deux formes longues :

A—la voyelle ā
(a) hūn ‘intérieur (hnow) (b) čōm ‘vigne’ (k’m)
B—la voyelle i
(a) htēf ‘son coeur’ (h’ty-f) (b) sēf ‘poix’ (*sft)
C—la voyelle ū
(a) hīme ‘femme’ (hymt) (b) Kēme ‘Egypte’ (Kmt)


ad A (b) : arabe karm, ad B (a) : i de l’adjectif nisbé, ad B (b) : arabe zift ; toutefois la forme sft n’existe pas, à ma connaissance, écrite en égyptien, ad C (a) *hyūme, pl. hyom(w)e, ad C (b) kmm ‘noircir’, d’où Kēme de *Kumme, *Kōmme.

Parmi les 6 formes types ci-devant, il y en a 3 dont on peut déterminer la quantité primitive à l’aide d’étiologies. Dans un autre cas, pour Kēme, on peut rétablir la voyelle brève d’après la forme nominale. On peut donc déduire que la voyelle ū (A, a) correspondait primitives à un *ū long et i (C, a) à un *ū long.

Les formes du type nēfēr (ērēp, ḥēkē, etc.) avaient donc primitives une voyelle *u (brève) à la première syllable et la prononciation réelle était probablement *ērēp, *ḥēke à l’époque copte.

Formes nominales et verbales

Comme il a été exposé au chapitre précédent, les formes du types nēfēr se trouvent associées, dans la morphologie égyptienne, au verbes du type hko(r), kmm, wop (c’est à dire *wōb).

La vocalisation du pseudoparticipe de hqr peut être rétablie à l’aide des deux formes survivantes qui représentent (a) la 3-ième personne m. sg. et (b) f. sg. ; la forme hokēr peut être ramenée à un *hagrey tandis que hakit pose des problèmes. Dans ce cas il faut tout d’abord compter avec le passage de r à Aleph (dr.t ‘main’, d’.t-f ‘sa main’ comme en copte tōrek, tootef) etc, ensuite avec le passage du groupe it, en passant pas ē, à la diphthongue ai (ḥāirtē, ḥāitē, ḥēt, ḫait).

Le deuxième phénomène décrit s’est produit également dans fai ‘porter’ (B) pour fiyēt, pai ‘celui’ pour pi’, udjai ‘être en bonne santé’ pour wdi (formes intermédiaires fēye, pē, udjē).
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La forme *nēfēr a servi primitivement, comme il semble, à l’expression d’une qualité (pauvre, rouge-orange, pur, petit, grand, noir), probablement en opposition avec une autre forme d’adjectif (ou de participe) du type *nēfēr comme bōn ‘mauvais’ (hbn), nūtēm ‘doux’ (nām), nūfe (nfr) ‘bon’, wōbēš ‘blanc’ (wbb), etc. Cette dernière forme semble correspondre à la forme du participe sémitique (qāṭil).

Résumé

Le nom Ṣṛp ou Ṣrēp ‘vin’ ne dérive pas des dialectes coptes connus, mais présente une forme plus ancienne. D’après d’autres noms appartenant à la même classe nominale, il s’agit probablement d’une expression d’une qualité. La forme copte Ṣrēp ne doit pas être ramenée à une plus ancienne forme *yērēp, mais à une forme (y)dērēp qui repose, de son côté à une forme nominale yurēp (ou sim.).

La forme Ṣrēp citée par Zyhlarz (Grundsüge, p. 180) et par Till (Koptische Grammatik, Leipzig, 1955, § 62) n’est pas attestée.

III. SUR DEUX PLURIELS NUBIENS


Dans le cas de la désignation du chameau il peut s’agir d’un mot d’origine nubienne car nous savons parfaitement que le chameau arrive, à l’époque historique, tardivement dans la vallée du Nil. La nombreuse littérature à ce sujet est réunie chez R. Walz : ‘Zum Problem der Domestication der altweltlichen Camelen’ (ZDMG, 1951, p. 38, Note 1).

Afin de déterminer la provenance de la désignation kam, je groupe ci-dessous les différents noms du chameau en usage au Nord-Est africain :

—copte čamūl (du démotique gml d’origine sémitique),
—nubien kam, bedja kām, pl. kam,
—arabe čamāl,
—geez gamal, tigré gamal, bilin gīmilā, qwaṛa gamal, khamir gīmil,
—sahō gala, afar gala, galla galā, somali gel.

Il est évident que les formes du nubien et du bedja appartiennent au même groupe. Ce qui les distingue des trois groupes suivants est l’insonorisation de la consonne initiale (g:k) et l’amputation de la fin du mot qui peut être due à différentes causes.

Or, il est parfaitement établi où cette insonorisation s’est produite en égyptien (Vergote, Phonétique historique de l’Égyptien, 1945, p. 29). Quant à la chute de la voyelle u, il ne semble pas qu’elle se soit produite en copte ou en nubien. Probablement elle est imputable à une tierce langue, peut-être au méroïtique.

On peut se demander si le deuxième mot formant son pluriel en -li n’est pas aussi un emprunt. En égyptien, il y a le mot Kbn.t, plus tard aussi Kpnyy.t ‘bateau, bateau d’expédition’ (lit. ‘bateau de Byblos’) qui a dû être prononcé *Kubli(t), *Kupli(t). Bien que nous n’ayons pas la certitude, d’après les textes, que le terme Kbn.t, Kpnyy.t ait jamais été utilisé à désigner les bateaux du Nil, nos connaissances dans cette matière sont trop restreintes pour pouvoir prononcer un jugement définitif à ce sujet.
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IV. UN VERBE PERFECTIF EN ÉGYPTIEN ET EN BEDJA

L'égyptien connaît un verbe pî qui sert à l'expression d'une action terminée (Gardiner, *Egyptian Grammar*, § 484: *have done in the past*). Ce verbe accompagne un infinitif: n zp pî tw yr.t s.t dr hsw N-sw.t By.t-y Snfrw mi+hâw 'ceci n'a jamais été fait depuis le temps du roi Snofrou le justifié' (Sinouhe 139, 10–11).


Le nom verbal de pî est pî.t 'antiquité (primeval times, Urzeit), d'où pîw.t-y 'dieu primaire' (comme dpî.t 'bateau', cum suff. dpîw.t-f, etc.).

L'existence d'un verbe perfectif est connu dans beaucoup de langues. Je ne cite que sudah (malay), liao (chinois) et le formatif du passé swahili me qui remonte à une forme bantuë mala (avec -i).

Dans les langues sémitiques, il ne semble pas qu'un tel verbe existe. Par contre, on trouve une parallèle en bedja (parlé entre la vallée du Nil et la Mer Rouge). Il s'agit de l'auxiliaire bêj, bêa 'terminer, to finish, complete, end' (E. M. Roper: *Tu Bedawie*, Hertford, 1928, p. 84–5; § 271):

—ô-ôja harid-ti b'ânîék ãîgânë 'when I have finished slaughtering the cow, I am going'.

—ane kitib-ti bambse yaki kâade 'until I have finished writing (lit. I not having finished writing) I will not get up'.

Le deuxième exemple correspond à la construction égyptienne bsw yr-t-f sdm qui a abouti, en copte, au temporalis negativus (W. Till: *Koptische Grammatik*, Leipzig, 1955, p. 161). En bedja, l'emploi de ce verbe est encore plus étendu qu'en égyptien. On l'ajoute souvent à une forme conjuguée pour marquer l'action terminée: aktib besan 'j'ai écrit' (lit. 'j'ai écrit j'ai terminé')

L'existence d'un verbe perfectif dans deux langues voisines pourrait bien être le produit d'un hasard. Toutefois les rapports peu nombreux, mais indéniables qui existent entre ces deux langues (p. ex. l'article défini, l'emploi de la tournure h'-u-f, hâôf, biyê-s 'ses membres' pour 'lui-même,' etc.) font penser qu'il s'agit d'un trait commun remontant à une haute antiquité.

Quant à l'interprétation proposée par Erman qui l'identifie avec un autre verbe, pî 'voler, fliegen', je ne pense pas qu'elle soit à retenir. Il n'y a, à ma connaissance, dans aucune autre langue un cas analogue où un verbe 'volez' ait abouti à une signification 'terminer'. De plus, il se pourrait que pî corresponde étymologiquement à be'a, tandis que pî 'volez' ait des rapports avec bir, fir, et avec le sémitique (hébreu pârâ).

V. NOTE SUR LA TRANSCRIPTION DE L'ARABE

Il y a actuellement deux systèmes-types servant à la transcription de l'arabe: celui de la Deutsche Morgenländische Gesellschaft adopté presque intégralement par la Normalisation Française (FD Z 46-002) et le système de l'Association Phonétique Internationale employé par A. Worsley (*Sudanese Grammar*) et J. Spencer Tringham (*Sudan Colloquial Arabic*).

Ces deux systèmes correspondent parfaitement aux besoins des arabisants et des phonéticiens et il ne peut être question de les remplacer. Le seul inconvénient qu'ils présentent, à mon avis, est le nombre de caractères surchargés de signes diacritiques (D.M.G.) ou spéciaux (A.Ph.1.) qui ne se trouvent que dans des imprimeries spécialisées. Or, il arrive assez souvent que des articles de valeur sont imprimés dans des imprimeries moins bien équipées et présentent, par conséquent, les noms et mots arabes de façon
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défectueuse ; la même transcription peut rendre deux mots différents : haram (harâm) ‘crime’ et (hâram) ‘pyramide.’

Ce n’est que pour ce dernier cas que le système exposé ci-après est destiné. D’ailleurs, la plupart des consonnes ne posent pas de problème. Les consonnes emphatiques peuvent être écrites à l’aide de l’apostrophe (d’, t’, z’, s’, r’, l’, mais q à la place de k’). Quatre consonnes sont transcrites par des combinaisons de deux lettres : dj, gh, kh (ou : x), sh. Alif initial n’est pas noté. Au milieu du mot, on écrit 3 (qarasa). Le son de l’ain est noté, au début du mot, par un apostrophe (’amal) tandis qu’il est noté par le chiffre 6 au milieu et à la fin du mot (fa6al, mana6). Les sons dh (anglais that) et th (anglais think) ne se trouvent pas dans tous les dialectes.

Les voyelles sont notées a, i, u (brèves) et aa, ee, ii, oo, uu (longues). La transcription à l’aide d’un accent circonflexe est facultative (bâb = baab).

Textes


1


2

Ya uhad’saay, ane eetbaab siibaab andieek, daay maalhi idarab itkteen tesuurawa, khe haatnia ?

Imaalhiwa tamint kamwa daay-g3iib ba arideewa baafooriwa haams3aaheeb leheeyt baakaayt fadjil.

Ane himeniit yak andeey, saawiitaki eekam shibiba andeey, daay door kissee haamsaa, baa dilleeliia emb3i walla nhadeey.

Idjamal thawaatee biishitshihikna, ixaddaamaan ee yam djaabiriibaay, tsiiibaabtuun male terigayyaai.

Un uu kaam uun male tergaayeeet siibaab adgiriini ? hare kiikey, kaam weer hanyii-kaab haamsaa !

*

Bien que cette transcription ne présente pas tous les avantages des systèmes dits scientifiques, elle rend parfaitement les sons de l’arabe (classique ou moderne) ainsi que du bedja. Son indéniable avantage pratique est la possibilité d’être imprimée dans toute imprimerie non spécialisée. En outre, elle peut être employée avec toute machine à écrire, sans adjonction de signes diacritiques.

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Summary

Two Nubian Plurals

Nubian nouns ending in a consonant take the plural ending -i, e.g. kitāb 'book', pl. kitāb-i. There are but two exceptions: kam 'camel' has pl. kamli and kub 'ship' has pl. kubli. All these examples are taken from the Kenūz dialect (Northern Nūbia).

Nubian kām as well as Beja kām, pl. kam cannot be derived from Arabic djāmal, Ge'ez gamal, Bilin gimila, Saho gala and Somali gel. They probably go back to a pre-Coptic kaml, the Coptic form being iaml. The loss of -ul or -u- may be due to the influence of Meroitic. In any case, l in the plural belongs to the stem (kaml-i).

Kub 'ship' is a loan word from Egyptian k-b-n.t 'ship of Byblos (Gubla)', vocalized *kubli.t as a nisbe form. In the singular -l has been lost.

A Perfective Verb in Egyptian and in Beja

There is a perfective verb in Egyptian, p' 'to have done in the past' (Gardiner, Grammar § 484). Erman explains it by p' 'to fly'. It is interesting to see that Beja too has a perfective verb be', bé'a 'to finish, complete, end' (E. M. Roper, Tu-Bedawie, § 271). O-s'ā harid-ti b'anyēk gigane 'when I have finished slaughtering the cow I am going'. Perfective verbs are well known in many languages such as Chinese, Malay, etc., but they are unknown in Semitic. Egyptian p' and Beja be'a seem to be one of the features common to both these languages. There may be an etymological relation too. P' 'to fly' belongs to Hebrew pārāh (p-r-y) and Berber yebru 'he flew' (Nefusi dialect, Tripolitania), stem f-r-y, and has nothing to do with Beja be'a.

Note on the Transliteration of Arabic

There are two main types of transliteration systems of Arabic, that of the Deutsche Morgenländische Gesellschaft, almost identical with that adopted by the French Normalization and that of the International Phonetic Association. Both use special phonetic signs available only in some specialized printing houses. The following recommendations will help to write Arabic words and sentences correctly without diacritic signs:

— emphatic sounds are marked with an apostrophe (d', t', z', s', even l' and r' when necessary, but q instead of k')
— letter groups are used for djim, ghain, shin, but kha is written x (xabar, shayx, t'abraax), the strong h is h' (Ah'mad)
—'ain is written ' at the beginning and 6 (six) in other cases, e.g. 'amal, fatb, mana6, alif is 3 (three): qursaan
— long vowel may be written aa, ee, ii, oo, uu or ā, ē, ī, ṓ, ū

Dh (as in English that) and th (as in English think) occur in classical forms (dhaaliqa, thalaatha). This system may be used for other languages too, e.g. for Beja, in any printing house and with any typewriter.

Old Nubian Plural Forms

According to Zyhlarz (Grundzüge) there are determinate plurals of indeterminate nouns and determinate plurals of determinate nouns in Old Nubian. As a matter of fact, there are only indeterminate plurals (odd-il-gou 'the sick ones') and determinate plurals (oukr-i-gou-l 'the days'). Forms like pisteu-o-l-gou-l 'who have believed' are not determinate plurals of determinate nouns but determinate plurals of participles (e.g. pisteu-o-l 'one who has believed'). The participles ending in -l (odd-il 'being sick') are frequently found in modern Nubian dialects (Kenūz, Fadīčta, Dongolawī). The use of determinate and indeterminate form follows the Greek original (e.g. istauros-il di-o-l-gou-na naierr-a-lo 'the Cross is the resurrection of the dead', Greek: stauros nekrōn anastasis).
Land Tenure during the Time of the Fung

by Sadik Nur

In Sudan Notes and Records, Vol. xvi, 1933, Part I, as appendix to an article on 'Fung Origins,' Mr (now Dr) A. J. Arkell published two documents granting land to a certain religious chief, Ya’qub Wad Mohammed Zein, of the Ya’qubab tribe.

Since 1933, more documents have come to light, having been produced by owners before Land Settlement Officers as proof of ownership of a specific stretch of land.

The significance of these, and any other such documents which may be discovered at a later date, lies in the fact that they form not only the first recorded evidence of land tenure in Fung times, but the oldest known in the history of the Sudan, as, in the absence of written records, nothing is yet known for certain of land policy in the periods that preceded the Fung.

A common feature of all the five documents, which are the subject of this article, is the thick undergrowth of titles and words of praise, which varies only slightly in each instance, and which it is difficult to put into English. An attempt will, however, be made to give the nearest meaning.

Three of the documents in question deal with land donated by the King to two different persons, one a tribal chief and the other a religious chief. The remaining two documents are merely records of judgments given in land disputes and they, therefore, show a different aspect of the matter. Documents in the former category will be considered first, as they all emanated from the King’s office and bear the Royal seal, whereas those in the second category were drawn up by some sort of viceroy or representative and bear no seal.

Document 1. Headed: In Thy name O Generous—in the Name of God, the Compassionate, the Merciful. Thanks be to God who has inspired the Apostle whose message we believe.

Seal

A Sultanic Title Deed and Royal Document written in the city of Sennar, the well-guarded and protected, may God make it honoured in the eyes of its ruler, the Sultan of the Muslims and the Caliph of our Lord, who looks after temporal and spiritual affairs, who protects the interests of the muslims and is the supporter of Mohammedan Law, the promulgator of justice and kindness among the human race, through whom the Almighty made peace between the people, and shed light on the universe. It is he who repressed the infidel, the cunning, the stubborn, the iniquitous, and the corrupt—God’s mercy be upon the living and those who have passed away, and the one who has a covenant with the Almighty, the Sultan, son of the Sultan, the Victorious (with the Grace of God) Sultan Badi, son of the late Dekin, son of Sultan Badi, may God make him victorious through the Koran and the Prophet, Amen Amen, O Lord.

To all who ascertain the contents of this document and understand the facts contained therein; whereas the Sultan, the protected, the righteous, the victorious one,—Sultan Badi has completed the gift to Sheikh Awad el Kerim Abu Sin bin Ali bin Abu Ali bin Mohammed el Ideighim, Chief of the Shukria Tribe, of rain and river land on
LAND TENURE DURING THE TIME OF THE FUNG

the East bank of Bahr (river?) El Adeik and to the East of the river Rahad. It is a wide stretch of land with the following boundaries:—

On the South    Ein El Luweiga
On the East     River Atbara to Sherif Hassaballa
On the North    The land of the pious Sheikh Ali Alu and of the pious Sheikh Deleig, Hassan Wad Hisouna
On the West     The east bank of the Adeik and Rahad rivers.

The land in question is to be lived on and utilized by the Shukria tribe and any other people chosen by Sheikh Awad el Karim who is hereby given the usufruct of the land bounded as above, excluding the land of the Abdallab Tribe. Sheikh Awad el Kerim’s sons, grandsons, and great grandsons will enjoy the same privilege to the day of Resurrection. Let no one claim this land or show resistance to him, and whoever obstructs him after this my document will render himself liable to severe punishment. A strict warning is hereby given against failure to comply with this order, and he who disobeys will have only himself to blame.

This document has been drawn up in the presence of, and witnessed by:—
The Vizier Sheikh Nasir son of Sheikh Mohammed Abu Lkieilik
El Amin El Sheikh Haroun Walad Younis
The soldier Ali Walad Shawwal, the market guard
Sheikh Diab Walad Abdel Raziq, Lord Chamberlain to the King’s maternal uncle
Sheikh Badi son of Musmar, Sheikh of Gerri
Sheikh Omer Gur son of Hamad El Zeer, Sheikh of Taka (Kassala)
Sheikh Agit son of Hakit, Sheikh of Atbara
Sheikh Ibrahim son of Abdel Ati, Sheikh of Beila
Sheikh Subahi son of Adlan, Sheikh of El Bahr
Sheikh Ali son of El Nur, Sheikh of Kordofan
Sheikh Gasim son of Idris son of Nail, Muqaddam (chief) of El Sawakra
Sultan Abdalla son of Sultan Badi, Sultan of the Fur Musabbaat
King Ahmed son of Adlan, King of Birsag, groom of the carpet (=chief of religious sect)
Sheikh Medani son of Shanbul, Sheikh of Arbegi
Sheikh Ali son of Mahmoud, Sheikh of El Gawaria
The Muazin (announcer of the hours of prayer) Osman son of Billi
The Qadi (judge) Sherif Omer
The Khatheeb (preacher) Nuwar son of Ammar
and The scribe, the slave of God, Khidr Ibrahim Yaqub Hemeira.

GOD IS THE BEST WITNESS

Written at noon on Monday, the 13th of Rabie El Awal 1206 A.H. (October, A.D. 1791)

Document II. Headed: In Thy name O Generous. In the Name of God the Compassionate, the Merciful. Thanks be to God who has inspired the Apostle whose message we believe.

SEAL

A Sultanic Title Deed and Royal Document from the presence of him who is victorious with the support of the Almighty, and who is eminent and protected; he who gives shelter to the poor; the one who is happy in the world and who, with the Grace of God, will die a martyr; the one who is honest and true, our lord the Sultan, son of the Sultan, Sultan Badi son of Sultan Nol; may God the Compassionate, the Merciful, make him victorious through the Koran and the Prophet, Amen, Amen, O Lord.
KUSH

To all chiefs, sheikhs and sub-sheikhs within the boundaries of my kingdom, who see this document and understand the facts contained therein, especially Sheikh Badi son of Sireir and whoever succeeds him, whereas the Sultan, the protected, the righteous, the victorious one, has approved the continued ownership of land by Fiki Mohamed son of Sheikh Abu . . . ., such land as is known by the name of Daggagat, all the cultivation belonging to his family, and also the piece of land purchased by Fiki Hamad from Ahmed son of Hamad, the boundaries of which are well defined: on the east Wad Habib, on the west El Zein, on the north Hamid, on the south Hileiw. This land has now become the property of Fiki Hamad, his sons, grandsons and great grandsons, etc., until the day of Resurrection. The land is free from charge, and nobody should come near it or touch it. All are hereby warned against disobeying this order, and he who fails to comply will have only himself to blame.

Written in the presence of and witnessed by:—

Sheikh Yunis
The soldier, Mohammed . . . . .
Sheikh Ismail,
Sheikh Diab Sheikh of . . . . .
. . . . . Shatir Sheikh of Alices
Sheikh Badi son of Hakit
Sheikh Awad, sheikh of El Taka,
Sheikh Mohamed Geili, sheikh of Beila
Sheikh Osman, Sheikh of El Garbein,
Sheikh Abdalla, sheikh of El Sawakra
Khamis Sultan of Fur
Arbab Eido grandfather of the king
Sheikh Rajab, head groom
Sheikh Nail groom of the trappings and saddles
Sheikh Badi, sheikh of the river
Sheikh Mahmoud, sheikh of the Gawaria
Sheikh Shalanky, groom of the carpet
King Mohamed of Birsag
Judge Abdel Hameed
Preacher Abdel Hafeez son of Nono
El Arbab Mohamed Bakoma
El Arbab Dafaalla son of El Ghannami
Fiki Imad bin Fiki Abu El Naqa
Fiki Yaqub Himeira
and the scribe Mohamed son of Abdel Ghani.

GOD IS THE BEST WITNESS

Written this month of Rajab 1191 A.H. (August, A.D. 1777)

Document No. III. Headed: Thanks be to God, and prayers and peace upon the Prophet.

SEAL OF SULTAN BADI SON OF NOL

A Sultanic Title Deed and Royal Document from the Sultan of the Muslims, the supporter of Mohammedan Law, and the promulgator of justice and kindness among the human race; it is he who protects the country and helps the people; the one who repressed the iniquitous, and the corrupt. God's mercy be upon the living and those who have passed away, our lord the Sultan, son of the Sultan, the victorious one, Sultan
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Badi son of the late Sultan Nol, may God increase the days of his justice and prolong the nights of his happiness, through our prophet and his companions, Amen.

To all who see this document and who understand its contents, whereas the Sultan, the protected, the righteous, the victorious one, Sultan Badi, has completed the gift to Fiki Mohamed son of Fiki Ahmed, son of Sheikh Abdel Rahim Bayaa el Matar (seller or donor of rain) of the land that belonged to his grandfather the aforesaid Sheikh Abdel Rahim, such land as has been in the family since the days of their grandfather, the boundaries of which are well defined and known: on the north the land of Osman son of Dol to Um Esheira and the Azaza where the liberated slaves are camping and hafir Kirai, on the east hafir Wad el Marqoub, on the west the gravel, and on the south hafir . . . . Such are the boundaries of the land that belonged to Sheikh Abdel Rahim Bayaa el Matar, which the Sultan has given to the family for God's sake and in the hope of salvation on the day of resurrection. Let no one approach this land, or oppose or resist them. It is free of all claims and complaints. Whoever quarrels with them over this land will render himself liable to severe punishment, especially the Sheikhs of Wad el Malik estate. All are warned against disobedience, and he who disobeys my order will have only himself to blame.

This document has been drawn up in the presence of and witnessed by:—
Sheikh Rahma, son of Sheikh Younis
The soldier Younis
Sheikh Ismail, the King's maternal uncle
El Arbab . . . . .
Sheikh Abdalla, son of Sheikh Ageeb
Sheikh Mohamed, son of Ali
Sheikh Abu Shawa, sheikh El Sawakra
Sheikh Sha'el Din, Sheikh el Bahr (river)
Sheikh Mohamed, groom of the carpet
Sheikh Hamad, sheikh el Garbein
Judge Ibrahim
El Arbab Mohamed Wad Bakoma
Fiki Ammar, son of Fiki Abu el Naga
Ahmed, son of Fiki Babiker

and the scribe, the slave of God, Mohamed, son of Abdel Ghani.

GOD IS THE BEST WITNESS

Written in Shaaban, 1145 A.H. (January, A.D. 1733)

Documents IV and V. These are in a fragmentary state, and the writing is partly destroyed. No. IV probably emanated from the office of a vizier, whose name is illegible, but his father was the well-known Mohamed Abu Likeilik. Both documents bear no seal at all, and deal with land disputes.

Document IV. Headed: In the name of God, the Compassionate, the Merciful. Thanks be to God, and blessings and peace be upon our lord the Apostle.

A Sultanic title deed, and royal document, drawn up at Timeid town, the well-protected and guarded, may God the Almighty, make it honoured in the eyes of its ruler, who has a strong hold over every single inch of it, and who knows its past and present; it is he Sheikh . . . . (name not clear) son of the late Sheikh Mohamed Abu Likeilik, may God make him victorious. Amen.

Whereas it has been found necessary to write a proclamation on account of a dispute between Hag Hamid son of Awonda and Obeid son of Idris over the land known as Dar
KABOKNI AND SEIF EL DIN. El Hag says that the land belongs to the Halawin (a tribe), but Obeid maintains that Dar Kabokni and Seif El Din is his own property and has produced witnesses who have corroborated his statement . . . . (the rest of the document is obliterated.)

Document V. Headed: In the name of God, the Compassionate, the Merciful.

Here is a regent’s title deed, drawn up for fear of guile and quarrel. Whereas El Hag Hamid and Obeid, son of Idris, have had a dispute over a piece of river and forest land before Gasim son of Mohamed, Sheikh of the Halawin, Obeid claiming that he was originally a partner and as such should not be denied the right to cut wood from the forest or to use the crossing place on the river. El Hag holds that both his father and grandfather had some title to the land in question and . . . . (words not clear). Obeid has sold his share to El Hag in the presence of Sheikh Gasim. This has taken place in the presence of and been witnessed by several people, among whom were:—

El Arbab1 Zein El Abdin, son of Fiki El Sayed Doleib
El Arbab Hamad, son of Ahmed, son of Abu Zig
El Arbab Mohamed, son of Said, son of Abu Fru
Arbab Mohamed son of Likeil
Mohamed
Fiki El Mardi
El Daw son of Fiki Mohamed son of Fiki Qorashi
Abdel Samad, son of Fiki Midian
Mohamed son of El Maqal
Mankorokuna2 Idris, son of Isa
Mankorokuna Mohamed, son of Aqab El Agag

and myself, the scribe, the slave of God, Ali son of Fiki Qorashi . . . .

GOD IS THE BEST WITNESS

Written in the year 1204 A.H. (A.D. 1789)

It may be concluded from these documents that all land must have belonged to the state and that the king had the sole right to dispose of it.

Wide stretches of land were given to tribal chiefs who were allowed only the usufruct. Not only is this fact borne out by document No. I, but by similar documents now in the possession of the Abdallab tribe and others. The political significance of the system lies in the fact that the tribe was kept within the boundaries of its land, as far as possible, thus avoiding disputes with neighbouring tribes over cultivation and grazing rights, and rendering possible the collection of taxes through the sheikhs. This is probably the reason why so many tribal sheikhs act as witnesses, and it appears that they formed some sort of commission and dealt with the distribution of land. The boundaries of these stretches seem to be clearly demarcated, as in the majority of cases they are natural or artificial landmarks—rivers and hafirs (water storage reservoirs with earth banks). Furthermore the system acted as a security measure, discouraging the encroachment of neighbouring tribes.

Turning to the two documents concerning land donated to religious chiefs, it is clear that they were allowed to retain land which their forefathers had acquired, perhaps by way of a gift from a previous king or through purchase from another chief. The piece

1 Arbab = Prince, or a descendant of the royal family.
2 Mankorokuna is probably a title.
of land involved was usually small and its utilization was left entirely to the Sheikh or Fiki who relied on his followers to cultivate it and harvest the crop for him. Despite the wording of the deeds, which gives the impression that such land was only given as a symbol of respect to the Sheikh and for 'God's sake,' it is beyond doubt that there was a political idea behind it. These chiefs had a wide following and were very influential among their students. To ensure their loyalty to the ruling king, and to win their confidence, they were given land, obviously freehold, and this appears to have been successful.

There are more documents of this nature in the family of Sheikh Abdel Bagi of Tayiba in the Gezira, which the writer had the opportunity of examining, but was not allowed to take away as they are looked upon as an heirloom. The wording of them is almost identical with that of documents II and III.

The last two documents, Nos. IV and V, deal with land disputes, and the only salient fact is that oral evidence, as opposed to documentary, played an important rôle in deciding the ownership of land. One document, No. IV, having no date (this was probably contained in the missing part of the script) and the other being difficult to read and understand, there is an element of doubt as to their authenticity.

An interesting fact, to be noted in passing, is that on the advent of the late Condominium Government, the owners of the five documents in question were able, on the strength of these, to establish a claim to more land than any of their own people.
The Nubian Kingdom of the Second Intermediate Period

by T. Säve-Söderbergh

One of the most important recent discoveries in Egypt is without doubt the second stela of Kamose, found in the first court of the temple of Karnak where it had been re-used in the foundation of a statue of Ramses II.

The text starts without the usual introductory formulae and is thus probably a direct continuation of the famous first stela of which Chevrier found some fragments in the third pylon and the beginning of which is known to us also from the so-called Carnarvon tablet. This first stela had already thrown some light on the contemporaneous history of Nubia, clearly showing that the territory which earlier had been united under one government was now divided into three independent kingdoms: northern Egypt under the Hyksos king, Upper Egypt between Cusæ and Elephantine under Kamose, and Kush under a Nubian ruler.

It could also be deduced from this earlier text in combination with some contemporary stelae from Buhen, that in all probability one single ruler dominated the whole of Lower Nubia, that one of them had the name ḫnḥ and that he had Egyptians in his service, which is perhaps the explanation of the rapid Egyptianization of the Nubian C-group.

The second Kamose stela gives us some more valuable information on this Nubian kingdom of the late Second Intermediate Period, and clearly demonstrates that these earlier conclusions were correct.

The difficulty in this new text is to make a distinction between past and future tenses and thus between what describes the real acts of Kamose and what he only threatens to do. I shall not here enter upon this problem in detail but only give some outlines of the context in which the passage referring to Nubia occurs.

After some introductory phrases characterizing the general situation and the defeat of the Hyksos king there follows a narrative section describing the attack of Kamose’s fleet on the territory of the Hyksos ruler Apophis. Kamose seems to have made a raid

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1 Labib Habachi, *Ann. du Serv.*, 53, 1955, 195 ff., with a photograph and a summary of the contents. Hammad, *Chron. d’Eg.*, 30, 1955, 198 ff., also published a copy, a photograph, and a preliminary translation. I am very much indebted to my friend Habachi for his kindness in putting at my disposal an excellent photograph and an accurate hand copy, which he authorized me to use for this article, and I remember with gratitude the long and interesting discussions we had in Uppsala on this extraordinary text and its many problems.


5 I suggest the following interpretation: ‘It will be a bad report in your town when you are driven back at the side of your army, your authority being too restricted to make me a vassal, yourself being the ruler, or (even) to ask for which slaughtering place you shall fall to (ḫrt.k prospective relative form). Your wretched back will be seen, when my army will be behind you, and the women of Avaris will not conceive (ḫn ivw, that is ḫn sḏm.f which practically always refers to future events; Gardiner, *Eg. Grammar* § 457), their hearts not open in their bodies, when the (battle) cry of my army is heard.’
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FIG. 1

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up to the Hyksos capital Avaris, as his ships are said to have ‘uprooted the field of Avaris (d’t Ht-wr’t)’, and in a speech he addresses the women who were ‘on the top of his (Apophis’s) palace, looking out through the windows towards the harbour’. He says that he will destroy the dwelling of Apophis, cut down his trees, and ‘your women shall be dragged to me to the ships’ holds and I shall seize the chariots (ti-nf-htrt)’.

Kamosse then enumerates the booty taken during this raid, presumably near Avaris, as this section ends with the words: ‘I did not leave anything of Avaris, it being empty’, and speaks of the wretched Asiatic who had said: ‘I am a lord without equal from Hermopolis to Pr-Hathor... and to Avaris with the two rivers’. Kamosse tells us that he left this territory destroyed and empty of people. ‘I hacked up their towns and set fire to their dwellings’, so that they were made into red tells eternally, because of the damage they had done in Egypt, when they gave themselves to serve the Asians forsaking Egypt, their mistress’. This passage is a valuable support for my view that the opponents of Kamosse were not only the Hyksos, but also Egyptians who had sided with the foreigners.

Then we read (see FIG. 1 above, p. 55):

‘I captured his message on the upland Oasis route (cf. Urk. 1, 105) proceeding southward to Kush in a written letter. I found in it as follows in writing from the ruler of Avaris:

‘Awwoserrē, the son of Rē Apophis greets my son the ruler of Kush (or, the son of the ruler of Kush)”’. Why have you risen as king without letting us know?’”

This introduction is a complaint that the Nubian king had not acted according to diplomatic courtesy and informed his colleague of his enthronement, a habit which is well-known from the El Amarna letters. It is interesting to see this kind of diplomacy at so early a date in Africa and in the correspondence between the Egyptianized Hyksos and the kinglet of Lower Nubia. It is possible that this is due to an Asiatic influence, and that the Hyksos king (who was called ‘chieftain of Retenu’ by Kamosse; l. 4) had learnt those courtesy rules in his diplomatic correspondence with his colleagues in the Near East. However, the fact that Apophis could ever have expected the Nubian to act according to these rules, throws in itself an interesting light also on this Nubian kingdom, which was so well established as to survive more than one ruler.

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6 Again we have a series of sentences with mn śdm.f referring to the future, just as the construction mk swr.i m trp etc. ‘I shall drink of the wine of your vineyard’ (Gardiner, Grammar, § 234); and grm n.t hmrw.t.k is in my opinion the passive śdm.f of future events (Gardiner, op. cit., § 422, 2); the hitherto unknown word grm is perhaps to be combined with Arabic ḫ mét ‘drag’ (cf. Vergote, Phonétique, p. 135, for the phonetical possibility).

7 The word may be a wrong restoration of an original ʕalālōn.

8 JEA, 37, 69 f.

9 Fig. 1: cf. Habachi’s translation, Ann. du Serv., 53, 201.

10 The first rendering implies that Apophis, being the elder ruler, addressed his younger colleague as ‘my son’ instead of the usual ‘my brother’ (and the writing rather supports this view); if we translate ‘the son of the ruler of Kush’, Apophis would have stressed that the new ruler was a legitimate successor of his father.

11 cf. Bilabel, Gesch. Vorderas. u. Äg., S. 89; e.g. EA, 29, 61 ff. (from Tuṣratta of Mitanni): ‘But when Naṣib[uru], the great son of Nimmurua by Tyi his wife... [wrote to me]: “I will enter upon my reign”, I said: “Nimmurua is not dead.” Now Naḥūriya, his great son by Tyi, his great wife [has placed himself] in his stead... Now I say [in] my [heart]: “Naḥūriya is my brother. We love one another in our hearts”’; EA, 33, 9 ff. (from the king of Alasia: ‘I have heard that you have placed yourself on the throne of your father’s house’.

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But if the diplomatic rules were the same as those of later times, Apophis himself had not acted as he should either. When a new king had been enthroned, his colleagues were expected to send him gifts of congratulation\(^{12}\), and this Apophis had not done. It is perhaps some kind of an excuse when he continues:

'Do(n’t) you see what Egypt has done against me? The ruler there, Kamose, given life, is attacking me (thm prop. "butting" like a wild-ox) on my ground. (And yet) I had not attacked him in the same way as everything he had done against you. He chooses these two lands to harass them, my land and yours, he has destroyed them'.

This passage shows that Kamose had waged war against the Nubians, too, an event of which there was no certain evidence so far. The fact that Egyptians served under the Nubian king, the sudden Egyptianization of the C-group in contrast to its anti-Egyptian attitude during the earlier Egyptian occupation, the peaceful immigration into Upper Egypt of the Nubian pan-grave people, the use made of them by Kamose as mercenaries under the name of Mdyw, all this has been interpreted as signs of peaceful conditions between Egypt and Nubia at the beginning of the 'war of liberation'\(^{13}\) and the first Kamose text gives the same impression. There the grandees try to calm down Kamose’s irritation over the fact that he has to share his power in 'this Egypt' with an Asiatic and a Nubian, by pointing out to him that 'Elephantine is strong'. We have reason to believe that there was a movement of people going on northwards from Kerna in the south into Lower Nubia at this time (some pure Kerna graves occur in Lower Nubia, and Kerma ware is found in the late C-group), and the pan-graves show that this movement did not stop at the Egyptian frontier, where the immigration was, however, in all probability under Egyptian control. If Kamose, as is said by Apophis, had attacked Nubia, it was perhaps just a raid to make his control over this Nubian northward movement more effective. In all probability this happened before his third regnal year, when, according to his first stela, the war against the Hyksos started. To judge from the words of the grandees just quoted, it did not lead to any great results, and the frontier of the Nubian territory remained at the first cataract.

Near Toshke in Lower Nubia the royal names of Kamose occur in a rock inscription, which it is tempting to connect with his raid, if we are to believe Apophis. But immediately below them are the names of his brother and successor Ahmose, the king who conquered Lower Nubia, and it is doubtful, whether the text was written before Kamose's death, despite the fact that Kamose is called 'given life', an epitheton which as a rule is applied to living rulers. It is, however, not impossible that Kamose's names were incised there during his raid and that the names of Ahmose were written under those of his brother when the Egyptian army returned to the place during the later conquest, just as Thutmosis III tells us that he put up his stela on the Euphrates next to that of his predecessor Thutmosis II\(^{14}\).

\(^{12}\) cf. Bilabel, loc. cit., especially the letter from Ḥattušil to Ramses II (ZDMG 72, 1918, 44 f.):
‘I have seized the reign, [but] you have not sent any messenger; and, whereas it is the custom among kings, [when somebody seizes the] reign that the kings send their colleagues beautiful gifts, royal dresse and fragrant [oil] of anointment, you have not done so this day’.

\(^{13}\) cf. my Agypten und Nubien, pp. 129 ff., 141 ff.; JEA, 35, 56 f.

\(^{14}\) cf. my Agypten und Nubien, pp. 141 f.; Weigall, Report, pl. 65, 4, had made a rapid copy only, and the text should be re-examined. Labib Habachi has told me, that he is trying to do so. It is still more doubtful, if some scarabs with the name Wid-hpr-R* found at Faras (LAAA, 8, pl. xvi) should be connected with the raid of Kamose.

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Apophis then comes to the main purpose of his message:
‘Come, journey downstream! Don’t be afraid!¹⁵ Lo, he is here with me and there is nobody who will stand up against (?)¹⁶ you here in Egypt and, behold, I shall not give him (free) road until you arrive. Then we shall divide the towns of Egypt (between us), and our [two lands] shall thrive¹⁷ in joy!’

This offer to join forces and attack Kamose from two sides shows undoubtedly that the Nubian kingdom was strong enough to be a political factor to be taken into consideration, and the fact that Kamose used the Nubian Mdrëw in his army, where Apophis had experienced their military capacity, may also have inspired him to try to induce other Nubians to side with him. This help was badly needed, despite the fact that the Hyksos had now imported from Asia and used against the Thebans improvements in military technique such as horse-drawn chariots¹⁸, bronze weapons¹⁹ and new types of daggers, swords and bows.

From the next section of the text, which starts with some more general phrases in praise of Kamose, it seems as if this message had been sent before Kamose had reached the neighbourhood of Avaris.

‘He became afraid of me, when I proceeded northward, before we had fought (with one another), before I had reached him. When he saw my fire, he sent as far as Kush to seek somebody to save him. But I seized it (the message) on the road, and did not allow it to arrive. And I had it brought back to him, putting it on the eastern gebel towards Atfih’.

A terror seized Apophis, Kamose tells us, when he heard of what Kamose had done in the territory of the Cynopolite nome, and then we hear about Kamose’s last exploit before returning to Assiut, the starting point of the campaign according to the first stela.

‘I sent a strong troop overland to destroy the Bahria Oasis, when I was in Sako (El Keis) in order to prevent rebels from being behind me’.

It is not clear whether this implies that he had moved his frontier to the north of the Cynopolite nome, the part of the Nile valley which is opposite the Bahria Oasis, possibly up to Atfih near the entrance to the Fayum, or even to the neighbourhood of Avaris (which is less probable), or whether the purpose of the raid was simply to cut off every possible communication between the Hyksos and the Nubian king.

The text of the new Kamose stela ends with a description of the triumphal return to Thebes and a command that the stela should be placed in Karnak.

I cannot here enter upon all the problems connected with this extraordinary text and shall restrict myself to a few points.

First the Nubian king. In an earlier article (JEA, 35, 57) I suggested that the ruler of Kush Ngh, known from the Khartoum stela No. 18, is possibly the one alluded to in the first Kamose stela. If, as is plausible, the second stela describes the continuation of the war, the beginning of which is the subject of the first text, this same Ngh should also have been the addressee of Apophis’s letter. On the basis of a stela belonging to the same group as Khartoum No. 18 and Philadelphia Nos. 10983 and 10984 I later drew

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¹⁵ ‘i’ is a new word; cf. ‘y Wb. I 169, ‘von angsterfüllten Menschen’?
¹⁶ nnyt ‘h’ nkr for r.k; then, properly, ‘in this Egypt’.
¹⁷ read wn [t’wy], n nfr.
¹⁸ The word tt-n-r-hš occurs for the first time in the new Kamose stela; cf. (p. 56, n. 6, (above) and JEA, 37, 59 ff.
¹⁹ Among the booty taken during the raid against Avaris were also, according to the new stela, ‘axes of bronze without number’.
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the further conclusion, however, that the Nubian kingdom lasted more than one generation. The new Kamose stela also supports this view, but the fact that we have at least two Nubian kings during the period of freedom from Egypt lessens of course the probability that Nåh was the one to whom Apophis wrote his letter. He can quite as well have been the father of the latter, and this is perhaps more plausible, as it was the grandfather of Iah-woser who served under Nåh and this Iah-woser almost certainly put up the stela before the Egyptian conquest under Ahmose. However, no certain conclusions can be drawn from the evidence now available.

The most extraordinary new fact which we learn from the second Kamose stela is the name of Kamose’s Hyksos opponent, ‘Awoserré’, the son of Ré’ Apophis’. According to practically all scholars who have tried to reconstruct the history of the Hyksos this ruler, with the exception of Khian, was practically the only one who could not possibly be dated to Kamose’s time. According to Stock’s reconstruction, based on the literary sources and on an analysis of the stylistical development of the scarabs, ‘Awoserré’ Apophis belonged to the ‘great Hyksos’ of the xvth Dynasty, which was followed by the xvth Dynasty of Hyksos rulers in Lower Egypt contemporaneous with the xviith Dynasty in Upper Egypt. I cannot here enter upon all the consequences of the new fact that ‘Awoserré’ Apophis was one of the last Hyksos rulers or the possible solutions of the problem. I shall here restrict myself to some aspects linked up with Nubian history.

The best starting point for the reconstruction of the history of Nubia in the Second Intermediate Period is the trade factory of Kema. There it is possible to follow an uninterrupted development of the native civilization, which was under a strong Egyptian influence, from, perhaps, the end of the xith Dynasty down to the New Kingdom. The latest phase is as yet unpublished, but, as I have tried to show elsewhere, the richest native tumuli with their many imported Egyptian objects, are probably to be dated to the period after the xith Dynasty, presumably the xith Dynasty and the beginning of the Hyksos period. This is shown not only by the scarabs but also by the occurrence of the widely spread Tell el Yehudieh ware, which indicates a vivid trade between Kema and Egypt, and between Egypt and Syria-Palestine during the period after the xith Dynasty. The so-called Deffufa of Kema was without doubt the Egyptian trade factory through which the natives received the foreign goods deposited in their tombs, and in a room in its western part a large number of seal impressions were found by Reisner, all accumulated there during a rather short period, datable through names (Jacob-her, Mä’etibré’ and Sheshi) and ornamental designs to the Hyksos period.

21 Whether the Ahmose-Intef, mentioned on a scaraboid found in Ermenne, is one of these Nubian rulers also remains doubtful; cf. my Ägypten und Nubien, p. 131.
22 Ägyptol. Forsch. 12; I accepted it in its general outlines in my article on the Hyksos rule (JEA, 37, 53 ff.; cf. also Bi. Or. 6, 87 ff.), which by many was regarded as too sceptical—but at least regarding this problem, the chronological order of the Hyksos, it was not sceptical enough.
23 Stock tells me that he will soon publish a revision of his earlier reconstruction; he now wants to date the first rulers to about 1675. The ‘small Hyksos’ (his xvith Dynasty) are contemporary with the ‘great Hyksos’, and those with Semitic names (such as Jaqob-el), whose scarabs have not as yet the characteristics of the later period, would be the forerunners.
24 Ägypten und Nubien, pp. 110 ff.
25 op. cit., pp. 125 f.
26 Kerma III-IV, pp. 75 f., 81, 558.
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little later the Kerma factory was set on fire—according to Reisner it was taken by storm and the Egyptian garrison put to the sword. As there is no sign of any interruption of the trade before this date, but indications of isolation from Egypt in the later tumuli, and as we have every reason to believe that the trade was to the benefit of the natives, we thought we were justified in assuming that the interruption of the trade was due to developments in Lower Nubia, where the native population had been oppressed by a hated Egyptian occupation and used the first opportunity to set themselves free. As the Kerma trade could hardly be upheld by an Egyptian government which did not rule Lower Nubia, it seemed reasonable to correlate the interruption of the Kerma trade and the liberation of Lower Nubia, and the latter event was thus dated to the period immediately after that of the Hyksos kings whose names occur on the seal impressions found in the Kerma factory.

This reconstruction which I first published in my Ägypten und Nubien, seems logical enough, but may have to be revised in the light of the new evidence.

The assumption that the occurrence in the trade factory of seal impressions with names of Hyksos rulers implied that the trade was in the hands of these kings is no longer necessarily correct, as we learn from the new Kamose stela that letters were sent from the Hyksos to the Nubians. The seal impressions are perhaps simply those with which such letters were sealed, and hence do not indicate a political domination or even influence. Moreover, the fact that the free rulers in Lower Nubia were highly Egyptianized for perhaps two generations, and had Egyptians in their service, is in favour of the possibility that trade with Egyptian goods, passing through the hands of Egyptians or Nubians in Lower Nubia, could be upheld in Kerma even if Lower Nubia was no longer under the rule of the Egyptian government, whether this had been taken over by the Hyksos or not. Thus the date of the Lower Nubian liberation is no longer necessarily dependent on the interruption of the Kerma trade which may, after all, be due to unknown local circumstances. The dating of these Hyksos rulers occurring in Kerma is therefore, from the Nubian point of view, no longer of such fundamental importance, and we cannot know for certain if the trade in Kerma was, during its last stage, in Egyptian hands or in the hands of some Egyptianized natives, like the rulers in Lower Nubia.

The assumption that 'Awoserrē' Apophis had upheld the Kerma trade and dominated Lower Nubia, was based on the date assigned to him, earlier than the rulers mentioned in Kerma, but we now know that this date cannot be correct, and his restricted political power during the later part of his reign rather makes it improbable that he could control the trade in the Sudan.

However, this Apophis reigned for at least 33 years and, even if the Nubian evidence regarding the geographical extension of the territory under his control is now of less value, there are some indications that conditions were different before Kamose's third regnal year, when Upper Egypt down to Hermopolis was independent, a fact which is clearly acknowledged also by Apophis in his letter to Kush. The names of 'Awoserrē' Apophis and of Khian occur on some blocks from a monument at Gebelein south of Thebes, and we have no reason to believe that they have been brought there secondarily. This finds an indication of political influence also in Upper Egypt, and the

27 To judge both from the official texts, the geographical distribution of fortresses in the neighbourhood of C-group settlements, and the archaeological evidence of the C-group.
28 The mathematical Papyrus Rhind is dated to his 33rd regnal year.
29 This is implied by his words 'Kamose is attacking me on my ground', without any protest against Kamose's rule of Upper Egypt.
30 cf. JEA, 37, 63; Stock, op. cit., p. 65.
famous tale of Pap. Sallier on the Hyksos Apophis and the Theban king Seknenrê's supports it. This Seknenrê is certainly identical with Kamose's father Seknenrê Ta'o II. When we now know that Kamose's opponent was 'Awoserrê Apophis, who had a very long reign, and much of this reign can hardly fall after Kamose, who in all probability was rather soon succeeded by his brother Ahmose, who expelled the Hyksos, it is reasonable to conclude that the ruler in the Hyksos capital Apophis of Pap. Sallier should be 'Awoserrê'. According to the tale the entire land was tributary to him which is in perfect agreement with the evidence of the Gebelein blocks. Thus this detail of the tale may be correct, implying that the predecessors of Kamose were more or less vassals of the Hyksos, despite the fact that the main trend of the tale, the Hyksos contempt for the Egyptian god Rê, is hardly historical, a conclusion now borne out also by the new stela, where Kamose, when quoting the Nubian letter, adds the determinative of the slain foe to the name of Apophis, but without reaction writes out his title 'son of Rê'.

Consequently, if 'Awoserrê Apophis was perhaps the acknowledged ruler of the whole of Egypt at the beginning of his reign, the possibility remains that Lower Nubia had also acknowledged his suzerainty. When Apophis reproaches the Nubian kinglet that he had not been notified of his accession to the throne, this may be something more than just a complaint of lacking diplomatic courtesy. Even if Nubia was now free and the communications with the Hyksos were difficult owing to the rise of Kamose, the Hyksos king could so much the more have expected the Nubian king to notify him of the changes in Nubian politics if Apophis had at an earlier date been the suzerain of Nubia, and so much more natural would his demand for Nubian help be, if they had had a close political collaboration before.

But all this remains pure conjecture, as well as the possibility that the Kerma trade was perhaps at some time in the hands of Apophis, of which there is no longer any positive evidence.

Few texts have to such an extent revolutionized our preconceived ideas as the new Kamose stela, and among the many problems connected with it I have here only wanted to stress its great importance for the reconstruction of the history of Nubia during a period which is still very imperfectly known, but which is from many points of view the most fascinating.

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31 For an interpretation of the main theme see my article in *Hermes Søderblomiana*, III.
33 cf. *JEA*, 37, 64 f.
Scientific Archaeology in the Near East

by Oliver H. Myers

There are moments in the development of a discipline when all advances steadily, when there is no introspection and no need for introspection: such was, perhaps, the state of Near Eastern Archaeology in the years between the two World Wars. It is my contention that we have now reached a period when we should do some stocktaking.

Science has been and is being more and more applied to archaeology with immense benefit; people are beginning to use statistics, though not nearly enough; there has been some fusion of techniques developed in different areas and under different circumstances, though this fusion is admittedly inadequate. Nevertheless, one can say that technique is improving steadily; but it will not reach its full capacity till some university has the courage to open a chair of scientific archaeology in its faculty of Science—but this is something that will probably come of its own account before long.

It is something more fundamental that needs examination, the strategy of archaeology, and I hope to suggest that with a new strategy the major difficulty of to-day, finance, may perhaps be overcome.

Firstly, I assume that we are all agreed that the object of excavations is to provide the raw material of history—directly, where everything except inscriptions are concerned, and at second hand through the philologist when we find texts. (Of course an inscribed object has two aspects, its position as an archaeological find and the contents of the inscription, and the two are complementary.)

So far so good, but if we leave politics and finance aside for the moment, where should we dig first? What problem would you first set out to solve, or, since personal preferences may well guide the answer, what problems would you send teams to solve if you were Director of a United Nations Fund for Archaeological Excavations?

The answer is not easy, for the instantaneous answers are not necessarily the correct ones. Seldom have I been so moved by an account of an excavation as when I read that they had found the palaces of Nestor and Maenaleus; Nestor's bath, where he doubtless chanted some ancient half-forgotten war-song as he scrubbed himself, has been exposed; you may step in the echoing portico of Maenaleus where Telemachus and Peisistratus slept; you can examine household accounts of Nestor or even lie in the very bedroom where Helen slept. But many readers, not brought up on Homer, may well pass such news almost unnoticed, whereas to see the terrace of Tao Tse, or of Confucius, the pleasure garden of the young Siddartha, or again the site where the Prophet pitched a tent would move them profoundly. In part a regional division solves this problem and clearly all problems except that of earliest man are regional in nature, though the regions will vary with each age and with each problem. But, more than this, we may, perhaps, question the emotion of the finder or his readers and listeners as a criterion of importance. We may well argue that some subject, which will arouse few to enthusiasm and none to tears of emotion, such as the origin of plant domestication, of the wheel or pot, or of iron smelting, is more important to history than our sites of literary or theological association.
It is, perhaps, worth carrying the matter ad absurdum and asking what we would stop to look at if we had H. G. Wells’ Time Machine at our disposal. It is surely clear that there is much we would not need or wish to stop and question, activities which we know must have taken place at all times everywhere, eating and sleeping, drinking, love-making, and quarrelling. True, if we were trained anthropologists, there might be some details of all these activities which we might wish to study, but we would only want to witness the act once for all the period of time and for all the people who complied with the form observed.

In fact the total possible knowledge of the past may, perhaps, be described schematically as four-dimensional, by regarding the surface of the earth and the people on it as two-dimensional, their activities as the third, and time as the fourth (this will not muster in the philosophic world but will do for our purpose). Since we could hardly observe at a speed greater than four times that of nature we could, at best, each of us in his working life of thirty years observe the complete activities of a small village for about 150 years and, even allowing for the large numbers of people interested to-day and the small population of ancient times, we should not get far without the most rigorous selection. As a reaction against the King-and-Battle-Date school of thought, we have, perhaps, been inclined lately to over estimate the value of the Pot and Pan. No-one wants to spend more than a few months observing the material culture of a primitive village and, except at a few sites, this is all that archaeological remains can show us (robbed of the dramatis personae) till about 6000 years ago, i.e. for the first million years (or is it half-a-million?) of human history. In fact, however moving it might be to a man with imagination to watch his ancestors of that period—his great, great, $\times 10^4$ grandfather, eating and hunting, fighting for life or pursuing the mate of his choice, there can be little doubt that the scenario would be of only slightly more interest than that provided by a troop of cynocephali. I fear that most of us, after the first few days, would welcome the sight of Lollabrigida on the silver screen for a change. However that may be, time and destruction have saved us from the ‘acres’ of boredom such a spectacle calls forth. Nevertheless, there are clearly periods and places which it would be fascinating and instructive to visit. Would you not like to see whether the Trojans were really asses enough to pull in the wooden horse? What about the movements of the Hamitic cultures in the Sahara? Lunch with the queens of Meroë? Many such things are tempting and some of great value to the historian, and, since archaeological material must be used by historians, and since histories must be read to be of any value, there will always be some place in each region for what we may, perhaps, call ‘associative’ digs. Khartoum may well say to their archaeological sons, ‘We are of Khartoum, tell us about the mounds of Soba across the river, what kind of people lived there? What rulers had they? Why did they disappear and their capital fall?’ They may even be prepared to put forward money for this though that is—alas, always—another matter.

But, leaving aside politics, which are always detestable until they have been eroded by the sands of time for at least 500 to 1000 years, what great drama has been played through history in the Near East and is still being played to-day? There can be only one answer, the growth and recession of deserts and man’s part in it. To-day, science is taking a hand and many an august body, seated in Rome, New York or Paris, casts from time to time a Jovian glance towards our deserts, semi-deserts and steppes, a glance that even becomes jovial as the northern climate degenerates in mid-winter. Seldom, however, do plans include archaeologists and pre-historians in their purview, although practically all knowledge of the past of these deserts springs in the first place from the work of archaeologists, even though the present state of the knowledge has been moulded by
people of other disciplines. I believe this to be because we have not ordered our studies in a scientific manner. Partly this has been due to exigencies outside our control—a road must be built through an old fort or palace, a railway through a cemetery, and quickly, like lightning, without costing any one more than a penny, or anyway not much more, the archaeologist must drop what he is doing and save such remnants of history as are to be found there.

We may hope that, in the future, this sort of task will be 'looked after' by some sort of flying squad, which should also serve as a splendid training ground for young archaeologists, with new problems and new periods cropping up all the time, but the major effort should be elsewhere. It is not entirely the fault of destruction or constructional development that work has been so unco-ordinated. It has to some extent been the reverse of the medal of the golden age of Near-Eastern archaeology between the wars when money flowed from interested private individuals and exciting finds were being made in all sorts of fields everywhere. This work was then, and the tradition continues to-day, entirely quantitative. What kind of people lived here? What kind of instruments did they use? What kind of jewellery did they wear? etc. etc. This was exciting and interesting to do, and not very difficult either; but the value of the results was strictly limited; it had the value of a poem or a sonata, a per se value which could and can be maintained but only as long as people are prepared to pay for it—either in time or money.

The Near East is full of interesting sites, it is indeed littered with them, and though there is doubtless exaggeration in the statement that there are 10,000 tells in Syria alone, this is no more than justifiable hyperbole. Some way must be found of saving this archaeological treasure from destruction, for there is much destruction by local digging, especially when tells are still occupied. Then a selection may be made with some end in view that will gain the widest possible support, while other sites remain inviolate. (Education is probably the only answer to the need for conservation.) It is suggested that the work should be quantitative and ecological so that we may learn from future excavations about ancient populations and their economy, and about the ecology of the area at that time. To some extent techniques have been developed to enable climatic judgments to be made, such as by conchology—a good example being Arkell's work on Old Khartoum; but these can be much further developed if statistical methods be employed. It is necessary that complete sites be cleared so that all the evidence from them is available. I may well be wrong, but I do not believe that a single town of the scores that have been excavated in the Near East has been completely cleared! We have little idea of how old towns were laid out and hardly any of their populations. Take for example the magnificent discoveries at Jericho where Miss Kenyon has found a city older than the discovery of pottery with a stone reveted defence wall! Surely here is a site crying out for complete excavation, the ancestor and perhaps the prototype of all cities. Probably if every city in the world were to give £10 this site could be cleared, but the present financing admits only of trenches and sondages.

Such sites require international co-operation so that we can squeeze the orange dry. When we finish we should be able to say that at x years B.C., when the rainfall in the area was y, the city supported n±n' people, who mostly made their living by such-and-such an occupation. The area was wooded with trees of this-and-that species. All this apart, of course, from the usual details of pottery, implements, furnishings, burial customs, to which we would add town planning and lay-out, and, with luck, the sociology of the population, at least in its more material aspects.

Returning then to our pipe-dream of the Director of a U.N.E.F.O.A. and benevolently regarding the Near East with a view to helping its archaeology we can, perhaps, now say
in what projects he should be prepared to participate financially and with technical assistance.

(We are not concerned here with the very urgent problems of restoration and repair which need treating separately, but we may mention in passing that whereas no-one in the world can find enough money to preserve with glass fronts the incomparable wealth of the tomb walls of Egypt, this would not require more glass than is to be found in one skyscraper.) We should give technical assistance and some financial aid to:

(i) A project for instructing children in schools at every level from the kindergarten upwards in the necessity of preserving the country’s and region’s cultural heritage and its monuments.

(ii) To any university prepared to set up a full chair in scientific and statistical archaeology.

(iii) To any excavations or series of excavations designed to shed light on the problems of the formation of deserts and semi-deserts and on their utilization, and replanting.

(iv) To an excavation or series of excavations designed to solve a historic problem of more than local significance.

It will be noticed that whereas we here leave the local problems to the locals to solve themselves, there is a sufficient degree of latitude in subsection (iv) to allow the Director latitude to help with the resolution of an associative problem as light relief. And since most of us have to enjoy our archaeology by proxy, let us enjoy a little in fantasy form. What, reader, is your major serious project for which, when he is appointed, the Director of the U.N.F.F.A. will grant several thousand pounds, and what your minor associative one? I shall not give away my major one in case some heartless reader steal it, but my minor one (at the northern end of the arid zones) I will confide in you: in a favourable light I noticed two years ago that the ‘misty cavern half way up the crag, facing the West . . . the home of Scylla, the creature with the dreadful bark’ is still to be seen, walled up it is true, perhaps for safety. (It is more than half way up but we cannot expect mathematical accuracy in a Bronze Age document.) I will unwind it and look for the remains of those twelve horrid feet and those ‘six long necks each ending in a grisly head with the triple rows of teeth, set thick and close and darkly menacing . . .’—or for what gave rise to the story. And the great fig-tree overhanging Charybdis? Yes, I shall look for a piece of that for radio-carbon dating. You may think that the gigantic pylon which now stands there will have destroyed its last traces, but I think not, for if indeed it did overhang Charybdis then it must itself have been larger than the Eiffel Tower.

Finally, lest the Editor, who so kindly asked me to contribute, or a reader may accuse me of levity I would like to recall G. K. Chesterton’s reply to an accusation that he was not serious about religion because he made so many jokes about it. G.K. said that this attack confused two things, seriousness and solemnity. He (G.K.) could afford to laugh about religion because he was serious; leader writers, politicians and company directors were all solemn—and he hoped no-one would suggest that they were serious.
New Egyptian Texts from the Sudan

by Dr. J. Vercoutter

When Lepsius came to the Sudan in 1843, he recorded nearly all the inscriptions he could see along the banks of the Nile. Those are now part of his large and admirable *Denkmäler aus Aegypten und Aethiopien*. This publication is all the more important inasmuch as a certain number of texts have deteriorated, or even have been destroyed, since Lepsius' visit to the Sudan. But, however precious Lepsius' readings may be, they need now to be published again: our knowledge of Egyptian philology and epigraphy has much improved since 1843 and the *Denkmäler* are no longer up to the standard of a modern publication of texts and, furthermore, since this date a lot of archaeological work has been achieved in the Sudan leading to the discovery of new inscriptions which have not always been fully published. It is the hope of the Sudan Antiquities Service to undertake as soon as possible the publication of a 'Corpus' collecting all inscriptions found in the Sudan: Egyptian and Meroitic as well as Greek, Coptic, old Nubian and Arabic.

This projected 'Corpus', for want of money, will, I am afraid, take a long time to be converted into fact and in the meantime I do think it could be useful to give in Kush, from time to time, all the epigraphical material which comes to light during our daily work in the Antiquities Service; accordingly this is the first of these papers to be devoted to Sudanese epigraphy.

Since the only link between the texts I am publishing here, is to be found in the Sudan, I will follow the geographical order from south to north. References are to the 1/250,000 maps of the Sudan Survey.

Khartoum. While working in the store-room of the Sudan Museum I noticed part of a basalt kneeling statuette¹ of a man holding a figure of Osiris within a shrine. As far as I know this statuette is still unpublished. I will deal here only with the texts which cover: I, the base; II, the border of the shrine of Osiris; III, the dorsal pillar.

¹ Khartoum Museum, no. 2782, height 37 cm., breadth 12.5 cm., length 25.5 cm. The card index gives the following indications: 'found abandoned in upper room of a house in Sirdar Avenue, Khartoum, that had once been a hotel. Place of discovery unknown. Presented by Messrs Lorenzato Bros.'
STATUE OF PATEFEMAOUYNE (Khartoum Mus. 2782)
Ph. J. Morhange
FRAGMENT OF STATUE, SAI (Inscr. 9)
PLATE IX

FRAGMENT OF STELA, JEBEL ABRI (Inscr. 29)
NEW EGYPTIAN TEXTS FROM THE SUDAN

1. I. The socle: the base is rounded at the front (see FIG. 1, for the position of the texts on the socle); the right part of base is destroyed:

II. The shrine (cf. FIG. 2). In the middle of the shrine Osiris is standing wearing the atef crown, the flagellum and the hook. The right part of the border is destroyed.

III. The dorsal pillar (see PLATE VII). Only the lower part of two columns are left:

The priest of Osiris, Isis and Neit Patefemaouyne is the son of Hathoremhat who is known to us by an Apis stela in the Louvre Museum. It can be seen from the text and the titles that the statuette comes from Northern Egypt; how did it reach Khartoum? Since this small monument is of very late date, xxvith Dynasty or even later, it is improbable that it could have come from an Egyptian site within the Sudan. Alternatively it could be like the Augustus head found in Meroë part of an ancient pilferage from Egypt, or a more or less recent import in the Sudan.

Hagar-el-Merwa. (Map 45 C.) In May 1956, I had the opportunity to pay a quick visit to Hagar-el-Merwa where Egyptian inscriptions were found some years ago which were partly published by A. J. Arkell. The inscriptions are difficult to read: the rock, a kind of white quartz with blackish veins is very hard and the hieroglyphic signs have only been slightly hammered. To publish them comprehensively will need a special expedition with scaffoldings and the use of artificial light. One must be grateful to A. J. Arkell for what he has done with such difficult material.

2 (←→) means that on the original the text is disposed in horizontal lines, hieroglyphic signs to be read from left to right (←→) = horizontal lines, signs to be read from right to left; (↓) is for column to be read from left to right, and (↑) for column, signs to be read from right to left.

3 SNR, 9, 22.

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The inscriptions occur on a small peak of quartz (see Fig. 3), the main group, including the royal stelae, are on the south-eastern part of the rock at A; a second group is at B, a little north of A. A third one, mainly defaced, is at C, on the south-western face of the rock. I have numbered more than twenty different inscriptions, mostly of the xviii Dynasty, some of them possibly Ramesside. In group C the very good inscriptions seen by Arkell\(^5\), painted in red, must be read:

2.

\[\text{The great (?) royal wife Amosis, may she live}\]

some twenty centimetres lower is another inscription in red ink:

3.

\[\text{The Harem's Child (or Servant)\(^7\), who follows the King wherever he goes, Iry.}\]

The two inscriptions seem to have been painted by the same scribe and at the same time; however, they are separate as is shown by the big space between them. Their main interest is to show that Tuthmosis the First went as far as the 4th Cataract. The qualifying 'nh.ti' affixed to the name of Tuthmosis' wife, daughter of Amenophis I and mother of Hatshepsut,\(^8\) manifests that she was still living, if not actually in the Sudan, at the time of the expedition which must have occurred at the beginning of the reign, most probably in the second year (see inscription 7 under). Moreover, the presence of a hrdw n kp, and the mention that he was following the king, seems to point to the fact that the campaign was headed by Pharaoh himself and not by one of his generals.

In group A the reading \[\text{in front of one of the lions}\] is certain. I think that under the right hind leg of this lion is a small and much defaced inscription which runs:

4.

\[\text{The thirty-five (?) year under the Majesty of . . .}\]

The cartouche is illegible, only the wjst is certain, this added to the fact that we are dealing with a date of over thirty years, rules out the possibility of a campaign by Tuthmosis I, as one would have expected from the position of the text under the lion referring to Tuthmosis I. The campaign referred to, on the inscription, must then refer to a king (a) using wjst in his cartouche, and (b) who reigned for over thirty years.

These two tests taken together rule out Ahmosis, Amenophis I, Tuthmosis I, Tuthmosis II, Hatshepsut, Tuthmosis IV, Amenophis II and leave us two possibilities only, Tuthmosis III himself and Amenophis III. Although the qualification hqs wjst is

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\(^5\) Arkell, l.c., p. 39.

\(^6\) The Hr instead of wr is certain.

\(^7\) Gauthier, BIFAO, 15, 1918, p. 197; foreigners could be 'Harem's Children'.

\(^8\) See Gauthier, Livre des Rois, 2, p. 306 sq.
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much more frequent in the cartouches of Amenophis III (nearly always \[\text{[image]}\]),

than in those of Tuthmosis III, nevertheless the same epithet does occur in the two 'cartouches' of Tuthmosis III\(^9\), and especially on the Gebel Barkal Stela of this pharaoh\(^10\)

where we find: \[\text{[image]}\]. Taking into consideration the fact that it was Tuthmosis III

who erected one of the boundary stela in Hagar-el-Merwa itself\(^11\), I should be inclined

to attribute inscription 4 to this king.

A little to the north of inscription number 4, there is a small graffito in red paint

and good handwriting:

5.

\[\text{[image]}\]

*The Overseer of the southern and northern Palace (?) Horiu\(^{12}\).*

The 'title' is unusual, it is not mentioned either in the Titles Indices of the Catalogue
général des Antiquités égyptiennes du Musée du Caire, nor in Sir Alan Gardiner's Onomastica.

The nearest title known to me is the usual \[\text{[image]}\] *Superintendent of works of Upper and Lower Egypt\(^{13}\).*

Among the group B I noticed one inscription:

6.

\[\text{[image]}\]

*The Priest of Amon . . . Amenemhet.*

The excellent painted inscription read \[\text{[image]}\] 'The ouab-priest of Re,

Senhotep' by Arkell\(^{14}\), belongs too to the group B. I am not altogether sure that it is

R\(^4\) which is to be read. I wonder if it is not \[\text{[image]}\] 'Imn-R'. All other inscriptions refer
to Amon's worship, see for instance the bull figurations in the group A, which must be

understood \[\text{[image]}\] 'Amon-Re, the Bull of his mother', where

the word \(k\)\(_t\) is represented by a full sized bull and it seems that the rock of Hagar-el-

Merwa was specially devoted to the Theban god Amon-Re (see too inscription 7 below,

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\(^9\) Id., ibid., p. 239, VIII; 241, XVI; 260, XXVII.
\(^10\) See Helck, Urk. 18 Dyn., heft 17, pp. 1227 and 1228.
\(^11\) See Arkell, l.c., p. 36 and fig. 4.
\(^12\) On this name see Ranke, Personennamen, p. 245, no. 21, who translates 'Horus ist

gekommen'.
\(^13\) See for instance Couyat-Montet, Hammamat, p. 134, s.v. imy-r \(k\)\(_t\) ts \(\ddot{s}\)m etc.
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line 2 of text). As soon as possible we will try to spend more time in this important place. As far as I could see in natural light the main inscription (group A) reads:

7. [Image of hieroglyphs]

As for any Nubian . . . 15 who shall trespass the stela16 which my father Amon has given to me, [his head (?)] will be cut . . . . he will . . . . for me. The . . . (?), his cattle will not . . . (?), for him; he will not have any successor. The first day (?), . . .

The date at the end of the inscription is difficult to read being nearly obliterated. It is possible that the sign hst (Gardiner, Sign-list, M, 4) escaped me when I collated the text. From my copy, there is obviously enough space for it between lww.f and either hrw or sp. In this case we ought to read: hst-sp 1 (or possibly 2) . . . tpy 'year 1 (or 2), the first. . . .' The inscription continued obviously under the fourth line, but I am unable to read further. It seems most probable that the inscription refers to the well-known campaign of the second year of Tuthmosis the First17.

Sabu (Map 45 H). When on my way from Dongola to Wadi Halfa, I stayed a few minutes in Sabu, a little north of Nauri, where Arkell noticed a number of rock engravings and inscriptions both Coptic and hieroglyphic 18, I observed a graffito:

8. [Image of hieroglyphs] 19

For the Ka of Userhat from the country of Am.

The chief interest of this small inscription is the name of Am, which, as far as I know appears here for the first time in situ, south of the Second Cataract; the translation is not altogether sure. We have two possibilities: we read either, as I propose, Wsr-hst

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15 Space for another name or, alternatively, for determinatives of the collective Nh3yw.
16 Or, possibly 'who shall transgress the decree'; for wdw meaning 'decree' see Zaba, Archiv. Orientalni, 24, 1956, p. 272, but the word may simply refer to the rock itself (see Wb, 1, p. 398, 17).
18 JEA, 36, 1950, p. 35.
19 The hieroglyphs are in cursive.
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n 'm, Userhat being a frequent proper name, or alternatively Wsr hst n 'm. In the first case we have the name of the country following the proper name—which is a current feature in New-Egyptian; in the second case we have the proper name User, frequent too, followed by a title hst n 'm 'commander (?) of Am'. In Egyptian, usually, the titles precede the name but there are cases where the title follows the name.22 Another difficulty for the second interpretation is that hst is not a usual title—if it is a title at all; hsty- 'mayor'23 is well known and from the Nauri Decree we are sure that there were hstyw- in the Sudan but neither the Onomastica nor the Wörterbuch mention such a title as hst so written. However we know the title h3wty25, a military function and there were h3wtyw n tj-sti26; I wonder if hst could not be a defective writing of h3wty and anyway the title or qualification hst written in the same manner as in the inscription 8 exists at the xxiind Dynasty27 so that the alternative translation 'For the Ka of User, Commander (or possibly "the First one") of Am' cannot be ruled out. However owing to the difficulties (a) of the rare title, (b) of the fact that the title follows and does not precede the name, I prefer the translation 'For the Ka of User-Hat of Am'.

The mention of Am is, of course, in favour of the localisation in Northern Sudan of this country rich in gold.28 If we follow the 'List of Mines' engraved in the Luxor Temple,29 we have from South to North: Nsw-Tswy (Gebel Barkal), Am and Kush. Am, according to inscription 8 being in the 3rd Cataract area, Kush in the 'List of Mines' ought to refer to the gold mines situated between the 2nd and the 3rd Cataract.

Sai. (Map 35 J.) The island of Sai, where a French Archæological Mission has been working since 1954, has recently yielded a certain number of inscriptions. In 1948, A. J. Arkell, during an inspection tour found, not far from the modern Muslim cemetery, south of the fort, a cache filled with fragments of broken statues.30 Those fragments are now stored in the Khartoum Museum. They are mostly of granite which has been burnt and are in very bad condition. However, if it is impossible to restore the statues themselves, part of the inscriptions at least can be interpreted.

On the back of a kneeling statue can be seen what is left of four columns of finely engraved text (cf. PLATE VIII):

20 See Ranke, Personennamen, s.v., Wsr-hst.
21 See Erman Neuägyptische Gramm., § 190, p. 84.
22 See Id., ibid., p. 84, where Erman mentions 'Pathay the Trumpeter', but one must remark that this quotation is pointed out by Erman as anm (irregular).
23 Gardiner, Onomastica, 1, 15*, A. 101.
24 JEA, 13, 1927, p. 200, n. 6.
25 See Wb, 3, 29, 9 and Gauthier, RT, 39, 1921, p. 237.
27 See RT, xxxi, p. 6 and Wb, 111, 20, ex. 21.
29 Daressy, RT, 16, 51 and Max Müller, RT, 32, p. 68.

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This inscription is too fragmentary to be worth a comprehensive translation. It is obviously a biographical text referring to a Royal Son of Kush: see line 6+x the mention: 'I was appointed as King's Son and Overseer of Southern Lands'. From the wording of the inscription and the use of I for the pronoun first person singular it can be seen that the text dates from the beginning of the xviith Dynasty. During the course of the forthcoming excavations it is hoped that the missing parts of the inscription will be recovered.

A similar kneeling statue, but smaller, was found in the same cache. Only part of the inscription is left. It reads:

... of Horus, the Bull Lord of Nubia, for the altar ... in presence of the Lord of the Two Lands (Egypt) when on earth ... the ... of the King of Upper Egypt in the length of every day, for the Ka of the King's Son, Usersatet.

The style of the hieroglyphs is not as good as in the preceding inscription, and it seems to be of later date. The King's Son (of Kush), Usersatet, who was Viceroy under Amenophis II, is well known from monuments from Amara32, Semna, Buhen and generally from lower Nubia33. It is the first time that his presence is mentioned at Sai.

31 We give here the text assuming that ... d.i hsy belongs to the column immediately after the one beginning with hr. But, of course, it is possible that one or more columns existed between those signs and this is why we add + X to the other figures.
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A granite fragment of the same provenance bears the inscription:

11. \[\text{Shāt Horus, the Bull (Lord of Nubia).}\]

This fragment is important because of the mention of \[\text{Shāt} \]. This name which appears in another inscription of Sai (see below under 13), is well known by a text of Semna Temple dating from the time of Tuthmosis III which runs: ‘I built this temple anew with fine white stones of Shāt’\(^{35}\). It was supposed, from the Semna text, that Shāt was in the vicinity of Semna\(^{36}\), but the Sai inscriptions seem to show that Shāt was none other than Sai itself. This is all the more important since Shāt appears in the ‘Exeptional Texts’ of the Middle Kingdom where it is mentioned among Egypt’s enemies\(^{37}\), it appears too among the Nubian captives in the Wadi-Halfa inscription of Mentuhotep who lived under Sesosiris I\(^{38}\). Moreover the island is full of sandstone quarries and this fact is in favour of the location of Shāt in Sai itself, since the Semna Temple, which according to the Tuthmosis text was built of ‘stones of Shāt’ is actually of sandstone in a granite district.

The mention of ‘Horus the Bull Lord of Nubia’ in inscriptions 10 and 11 is interesting since it seems that it was one of the gods specially worshipped in Sai\(^{39}\). In Ellesiyah grotto this god is represented as a hieracosphalic deity with the legend \[\text{Horus, the Bull Lord of Nubia who resides in Thebes}^{40}\]. The oldest name of the god seems to have been Horus alone but very quickly there appears to have been an identification between this Horus and Amon-Rē, and so, in Semna on a fragment of sandstone door-jamb ‘later than the beginning of Dynasty XVIII’\(^{41}\), one can read:

\[\text{A boon which the King gives (to) Amon-Rē, Horus the Bull Lord of Nubia, where the name of Horus seems to be in apposition with the name of Amon. During the Napatan period we find a god}^{42}\]

\[\text{which could be the old ‘Horus of Nubia’}.\]

\(^{34}\) Only the lower part of the \(\text{s} \) is visible, but the restoration is certain.

\(^{35}\) Cf. Lepsius, Denkm., III, 57 a = Sethe, Urk. IV, 212, 1.


\(^{39}\) See below under 23, p. 78.


\(^{41}\) Diary of Reisner (Manuscript) for the 25 January 1928, page 2 (Archives of the Antiquities Service, Khartoum).

\(^{42}\) Read \[\text{by mistake by Reisner, ibid.}\]

\(^{43}\) Id., ibid.

\(^{44}\) Dows Dunham, Nuri, II, p. 97.
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During two excavation campaigns in the fortress of Sai the French Archaeological Mission found some inscribed stones of which I give here the most important.

Pillar of sandstone. The pillar is inscribed on its four faces. On one of them Amenophis III (Fig. 4, A) is represented standing in front of a goddess now destroyed; on the second one to the right of Amenophis III (cf. Fig. 4, B), one can see Tuthmosis III in front of Amon; the third one (Fig. 4, c), represents again Tuthmosis III in front of a deity much defaced. Under this representation and accompanying a kneeling figure of a scribe with arms uplifted, one can still read:

12. [Image of hieroglyphs]

'Give adoration to (Amon-RE) ... (by) the King's Herald the Overseer of the Granary, the King's son, Nehy'.

On the fourth face of the pillar (Fig 4, d), under the figuration of Tuthmosis III in front of a goddess, are the remains of an important text:

13. [Image of hieroglyphs]

1. 5 sq. lost...
2. 6 sq. lost...
3. 4½ sq. lost
4. 6 sq. lost...
5. 7 sq. lost...
6. 7½ sq. lost...
7. 8½ sq. lost...
8. All line lost except...

---

45 Excavation no. S. 1.
46 Breasted, photograph no. 3264.
47 Restored from Steindorf, Anibe, II, pl. 18, lintel no. 1.
48 Owing to the fact that all the figurations on the pillar represent Amon, I restore here the name of this god.
49 Seen and photographed in 1905 by Breasted Expedition (Photogr. no. 3263), and in 1939 by Kirwan, Oxford Univ. Exca. at Firtha, p. 29 and pl. vi, 1–2, see Porter-Moss, vii, p. 165. It is by mistake that this text and the Amenophis III representation are mentioned in Porter-Moss under different headings, they belong to one and the same monument.
NEW EGYPTIAN TEXTS FROM THE SUDAN

... Menkheperre, son of Ré, Djehtymes-Nefer-Kheperu (Tuthmosis III), May he live for ever! King's Son ?) Nehy to build a temple in . . . the fortress of Shât. Thereupon the King's Son, Overseer of Southern lands (Nehy) did . . . the 'sheseru' to the Temple of Amon . . . In the year 25, the third month of winter, the second day . . . (in) stone (what) was built in brick . . . (adorned) of paintings . . .

This text seems to show: (a) that a new temple was built in the fortress (?) of Shât (line 2-3); (b) that another temple of bricks was restored in stone at the same place (line 6); (c) that the new temple was to be dedicated to Amon (line 3); (d) that Nehy, the well-known King's Son of Kush who worked in nearly every place in Nubia, was put in charge of the work and that he did it in the 25th year of Tuthmosis III, that is to say at the beginning of the personal reign of this Pharaoh (lines 2 and 5 of text).

That Tuthmosis III was not, by a long way, the first Pharaoh to build a temple in Sai is shown by a number of monuments found on the spot. For instance a much defaced stela in quartzite was found inside the fort:

14.

(N.B. Under the vertical text was a long horizontal one which is entirely defaced. It seems that at the beginning there was a date.)

Horus, 'the Bull who put in chains the (foreign) lands'; the Good God, Nebti 'Great of Fear'; the Golden Horus 'Enduring of Years like Ré'; the King of Upper and Lower Egypt, The Lord of the two lands 'Djeser-Ka-Ré', May he live for ever!

We have here the complete 'titulary' of Amenophis I, second king of the xviith Dynasty.
A fragment of door-jamb, in sandstone, gives us the well engraved text:

15.

Nebti: 'Enduring of Kingship like Ré'; the Golden Horus: 'Sacred of weapons'.

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50 One must restore here the beginning of the orders given to Nehy by Tuthmosis III.
51 We restore here which is in accordance with the traces subsisting.
52 Understand doubtless: 'Moreover he reconstructed in stone'.
53 Read [hpp]k.ti or perhaps [sh[p]k.ti, for shhr m d rew, see Belegst., v, 601, 6; some of the stones from the temple are actually painted (see below, p. 76, no. 17).
54 At the end was probably a list of festivals.
57 Excavation no. S. 3; seen by Blackman in 1937 (dimensions 1.53 m. x 0.86 m. x 0.24 m.).
58 Excavation no. S. 12 (0.53 m. x 0.43 m. x 0.64 m.).
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It is the same titulary which figures too on a big sandstone architrave:\n
16.  

A: Nebti: 'Enduring of Kingship like Ré in the sky', the (golden) Horus . . . .
B: (Horus): 'the Bull who rises in Thebes'; Nebti 'Enduring of Kingship'; the Golden Horus: 'Sacred of weapons'; the good god . . . .

This titulary belongs to Tuthmosis III.

The temple in Sai was of a good workmanship as is shown by parts of a wall with painted reliefs and hieroglyphs, such as:\n
17.  

Foreign countries (come) to thee bowing down . . . he has (overthrown) the Iunu . . . . his terror is among the Hau-nebut . . . . (enduring) of years (when) he appears, stability . . . .

A similar text, set out in the same manner can be seen in the Buhen Temple.

When Lepsius visited Sai in 1843 he saw 'in the middle of the fort' two door-jambs still standing, with the cartouche of Tuthmosis III. On the inside they had the figuration of an official standing; in front of him the inscription

(a) Lepsius suggests the reading $\text{ɪ\text{ɪ}ɪ\text{ɪ}}$

In 1954 was found a fragment of a door-jamb with the following text:

18.  

For the Ka of the Overseer of priests of all the Gods, the deputy of Kush . . . .

---

59 Excavation no. S.2.  
61 Excavation no. S.4; sandstone, the hieroglyphs are sculpted and painted (0.53 m. x 0.57 m. x 0.48 m.).
62 Randall-MacIver, Buhen, pl. 21 and pp. 57-8.  
63 Lepsius, Denkm., Text, v, p. 226.
64 Excavation no. S. 11 (0.60 m. x 0.20 m. x 0.40 m.).
65 Our reading $nw$ confirms the interpretation $ldnw$ of Lepsius. An anonymous fragment gives $\text{ɪ\text{ɪ}ɪ\text{ɪ}}$ with again the title 'Deputy of Kush', on this official see Säve-Söderbergh, Ägypt. u. Nubien, p. 182.
NEW EGYPTIAN TEXTS FROM THE SUDAN

We suspect this fragment to be either part of the same door-jamb which Lepsius saw still erected, or another door-jamb dedicated by the same official. In either case we ought to complete the newly discovered text: 'the Deputy of Kush, User-maat-Re-di-khau'. This inscription gives us the proof that the temple was still in use during the Ramesside period.

Nevertheless it is evident that the temple reached its acme during the xviiith Dynasty as is shown by the numerous stones with the cartouches of kings of this period, such as:

19. 

The great [Royal Wife], who joins the Nefret [the crown of Upper Egypt], Amosis-Nefertiry, May she live

Amosis-Nefertiry was the wife of Amosis I. Since her name is followed by the wish 'nh.ti', which shows that the queen was still living at the time when the temple was dedicated, it is probable that the first temple in Sai was erected by Amosis I himself (see also below under 24).

Two small stelae were found in 1954:

20. In front of the king wearing the khepesh crown: The King stands before Amon, between the two figures:

Djeser-Ka-Rê, beloved of Amon-Rê.

The other stela gives:

21. At the top under the sun-winged disk = Behedety, the Great God.

In the middle the King stands in front of the Goddess Nut with the inscription

The good god Djeser-Ka-Rê, given life, beloved of Nut.

Djeser-Ka-Rê is the name of Amenophis I who erected in Sai an important stela (see above under 14) and a seated statue (see below under 25).

A small fragment of a wall gives the cartouche:


68 Other fragments found in Sai showing distinct xix-xxth Dynasty style confirms this point.
67 Excavation no. S.64, sandstone block (0.77 m. × 0.44 m. × 0.40 m.). In front of the text a female figure is standing.
68 Excavation no. S.52. Broken in five pieces; sandstone (0.38 m. × 0.25 m. × 0.09 m.).
69 Excavation no. S.50, sandstone; broken in two pieces (0.40 m. × 0.27 m. × 0.08 m.).
70 Excavation no. S.48, sandstone (0.44 m. × 0.31 m. × 0.22 m.).
KUSH

The name ‘Aa-kheper...Re’ could be, alternatively: ‘r-hpr-ki-R’. Tuthmosis I, or ‘r-hprw-R’, Amenophis II. Both kings\(^\text{71}\) have left inscriptions in Sai Temple; Amenophis II, for instance, seems to have built a temple to the north of the fortress. During the 1955–1956 excavations we found broken in many pieces the upper part of a pillar or door-jamb\(^\text{72}\):

23. At the right: A

Facing A: B, at the top remains of a vulture grasping a \(\textit{snw}\) and

A: Horus the Bull Lord of Nubia who resides in ...  

B: Horus, ‘the Bull, great of strength’ the good god master of rites, Aa-kheperu-Re, the son of Re Amenhotep, the god ruler of Heliopolis (Amenophis II).

Besides the inscriptions just mentioned, three statues or parts of statues found in Sai are now in the Wadi Halfa Museum, they belong too to the xxi\(\text{th}\) Dynasty. The more ancient one was found in 1937 by the late Professor Blackman and Prof. Fairman\(^\text{73}\). The head is in Khartoum Museum; only the lower part is in Halfa. On each side of the throne on which the king is seated, his feet placed on the Nine Bows, runs the same inscription (see FIG. 5).

24. The good god, Lord of the two lands (Egypt), Nebpehetet; the real son of Re (lit. (the son) of his body), Amosis, beloved of Amon-Re the Lord of the Thrones of the Two Lands (the Karnak Temple).

Three years ago when surveying Sai Island prior to the French excavations, the Senior Inspector of Antiquities, Thabit Eff. Hassan, found a seated statue and a head of the same rough style as the Amosis statue:

\(^{71}\) In February 1955, we found in Sai a big granite stela with the name of Tuthmosis I (Excavation no. S.58, 1·20 m. \(\times\) 0·70 m. \(\times\) 0·30 m.); unhappily it is much defaced and I am afraid that it will prove impossible to decipher it.

\(^{72}\) Excavation no. (provisional) S.1000. The monument was found in seven pieces re-used in the Turkish wall from 28th November to 17th December, 1956, we still hope to complete it in the course of the forthcoming excavations.

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25. I: on the back of the head

\[\text{He is the head of the Kas of all the livings, rising in . . .}\]

II: on each side of the statue (see FIG. 5, p. 78), two columns:

\[\text{N.B.—a-a. The word } \text{Imn has been erased, then clumsily re-engraved.}\]

The good god, Lord of the Two Lands (Egypt), Master of the Rites, Djeser-Ka-Rê, the real son of Ré, Amenhotep, given life, beloved of Amon-Rê, Lord of the Thrones of the Two Lands.

Same text from the beginning up to the second cartouche included, then:

var. column B: (Amenhotep, given life) beloved of Dedun, Head of Ta-Seti (Nubia).

A small fragment of a statue in black granite kept in the same Museum, bears the inscription:

26. \[\text{Horus, the Bull of Ta (-Seti) (=Nubia), for the Ka of the Prince, Count . . .}\]

This fragment could belong to one of the statues of the Sai cache. It confirms the importance of the God Horus 'The Bull, Lord of Nubia' in Sai Island.

I come now to the lower part of a double statue, in black granite, of a king and a goddess seated on a square chair. According to A. J. Arkell, followed by Porter-Moss\textsuperscript{74}, this statue comes from Sedeinga. I very much doubt this provenance. For one thing the statue was found in Abri which is much nearer to Sai than Sedeinga; for another a Wilkinson drawing of a statue which he saw in Sai\textsuperscript{75}, refers most probably to our statue. Except that the left hand corner of the socle is now destroyed, it is the same as the statue which was sketched by Wilkinson (see FIG. 6).

At the back of the seat are the remains of a rather long inscription which runs:

\textsuperscript{74} Cf. Porter-Moss, Topogr. Bibliogr., vii, p. 166.

\textsuperscript{75} From Wilkinson MSS x1, 8t bottom. This sketch was copied for me from the Wilkinson MSS through the Griffith's Institute, which I should like to thank.
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27.

on the side of the seat:

right

left

...[the fear of him] circulates in the waters (?) of the chiefs of the enemies... building as a wall on the... like the obelisks... among the Aamou of the Retenou, prisoners of his Majesty. One had never done [the like]... I conducted the lower Egypt Kingship according to the projects... for his son who is faithful to him (lit. who is on his water), the Lord of the Two Lands (Egypt), Nebmaatre, who has made (his?) name famous among... of the vile Kush. The same was never done [before]. This is what has done the Son of Re (the son) of his body, his beloved the Lord of... every... Amenhotep ruler of Thebes, Horus ‘Bull mighty of strength’, the Lord of the Two Lands who reaches the Confines (of earth) by the victories he accomplishes. May he be granted of life, stability, durability, health, his heart being joyful with his Ka (when) he rises on the throne of Horus like Re, eternally.

on the side of the seat; right: All the Rekhyt, all the Henememet are at the feet of this good god, the praised one of Amon-Re.

left: [all the... are] at the feet of this good god, as the praised one of his father Amon-Re.

In connection with the attribution of this statue to the site at Sai, it can be said that Amenophis III was one of the principal builders in Sai, as is shown not only by the

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scene on the pillar (see above p. 74 and 13) but also by a fragment from a wall found in 1955 in Sai which runs:

28.

The fifth (or sixth) year, the second month of Akhet, the twenty-fourth (or twenty-fifth) day, under [the Majesty]. . . . The Golden Horus, 'Great of Khepesh who strikes the Asiatics', the King of Upper [and Lower Egypt] Neb-maat-Rê (Amenophis III). Lo! His Majesty was in\(^77\) . . . . the enemies from Ta-Seti (Kush)... The soldiers (\(?)\(^79\) of his Army... .

Gebel Abri (Map 35 J). Some two kilometres from the river and on the east bank of the Nile, stands Gebel Abri, due east of the Island of Sai. At the foot of the Gebel, on a big fallen rock, A. J. Arkell found a rough engraving of a king, without text, which he ascribes to the Meroitic Period\(^79\) (see FIG. 7, a).

The top of the gebel dominates the sandy desert plain at over 200 m. and its slopes are sometimes nearly vertical, making access to the summit all the more difficult. During a short excursion I found fragments of at least two Egyptian stelae nearby two small cairns (FIG. 7, c and d) which stand on the highest point of the small plateau at the top of the gebel. I intend to publish these fragments more thoroughly, but for the time being I give here the main one (PLATE IX):

29.

[of the] King's Palace, may he live, prosper and be healthy, who approaches the God's body (i.e. Pharaoh) . . . . in the King's Palace, l.p.h., causing all the Rekhyt to be satisfied . . .\(^80\) . . . . excellent of the Horns of the earth, eyes [of the King] . . . Overseer of the southern countries.

\(^77\) Restore probably 'in his Palace'.

\(^78\) Read possibly 'nhw nt ms' hr dd.

\(^79\) A. J. Arkell, JEA. 36, 1950, p. 32.

\(^80\) Restore here the name of a material, presumably a stone.
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The rounded top of the stela, much defaced, still bears the name of Menkheperre. Accordingly the stela of Gebel Abri was dedicated (see line 4) by one of the viceroys of Kush living at the time of Tuthmosis III. On stylistic grounds I think that this viceroy was none other than Nehy himself.

Aksha (Serra west) (Map 35 I). During an archaeological survey of the west bank of the Nile between Wadi Halfa and Faras in June 1956 I had the opportunity to spend some hours in the Temple of Aksha (Serrah of ancient writers). At the base of the remaining wall are two well known geographical lists, one relating to the south (on the west wall, south wing, see FIG. 8, A); the other to the peoples of the north (west wall, north wing, FIG. 8, B). Those lists were published a long time ago by Max Müller. To check the state of the walls I cleared the north part of them to the north of B (FIG. 8) and was surprised to find that the list of the northern countries continued on the other side of the small door and on the north wall (FIG. 8, c). Unhappily there has been a kind of chemical reaction in the sandstone at the level of the list so that the lower parts of the name-rings are destroyed. This is what can now be seen:

![Diagram]

N.B.—An eleventh name is destroyed after Mw-ked.

Part of these names can easily be restored from other sources. At first sight it seems evident that this list has much in common with the lists of Sulb and Amara, our no. 9 for instance is the no. 5 of the Sulb list. Our no. 3 could be the no. 21 of Amara; our no. 5 confirms the reading Amara, no. 28) and so on. On the south wall (FIG. 8, d), the list of the southern countries extended similarly, but the building has deteriorated badly here and only a few signs are left.

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81 Max Müller, *RT.*, 19, p. 74.
82 Fairman, *JEA*, 36, p. 66.
Notes

THE MAKING OF MAIL AT OMDURMAN

When I was Commissioner for Archaeology and Anthropology in the Sudan, Sir James Mann, the distinguished Curator of the Wallace Collection, suggested to me in 1939 that I should try to find out the origin of the suits of mail used in the Sudan, of which a number had been brought to England as trophies from the battlefield of Omdurman.

I soon discovered that the expert on Sudanese mail was Hamid Idris, then a venerable figure of well over seventy, but with all his wits about him. He had worked for the Mahdi and his successor Khalifa Abdullahi as silversmith and craftsman, and, unless my memory deceives me, he told me that he had first made mail as a young man under the Egyptian Government before the Mahdia. In February 1940 I went with him to the Khalifa's House Museum in Omdurman and examined all the suits of mail there. These are all made up of individually riveted rings, and for that reason Hamid Idris pronounced them as all having been made outside the Sudan and imported into it 'from the north', i.e. via Egypt before 1885—some of them possibly many years before 1885. He had no idea where they were made. He told me that in the time of the old Egyptian Government there were very many of these suits of mail in the Sudan, every important tribal chief or melik having 200 to 300 of them.

Although mail had been made in Omdurman during the Mahdia and before, no one in the Sudan knows how to rivet rings, and Hamid thought that they never had had that knowledge. Sudanese craftsmen had usually used butted rings, which were imported and which they bought by weight from the merchants Kyriazi and Sirkis. About 10 kilogrammes of rings were needed to make one suit, and a good suit of that kind sold for £25 in Omdurman, and more in the provinces. Suits of riveted mail fetched no higher price—which shows clearly that by that time mail was worn merely for show and not for serious defence. There was also a third kind of mail called 'Huksawi' after the ill-fated General Hicks who fell at Sheikan early in the Mahdia. This was made out of imported split rings.

Of the suits of armour in the Khalifa's House Museum, Hamid Idris said that K 99 is of good quality, K 100B of very fair quality and K 100A of poor quality. Suit K 100B has a seal (maker's mark ?) attached, of which the design is an upward-looking crescent supported by a vertical line, on either side of which is another vertical line.

A few days later, having agreed that Hamid Idris should make me a good suit of mail as made for emirs in the Mahdia, I visited his house, where he demonstrated the various stages in making mail. He has taught his eldest son all the secrets of the art. He needs about half-a-dozen assistants, and these include several of his sons, who, though merchants of some standing, closed their shops and came to work for him at his summons. The men were well practised in the work. In 1940 they were still making and selling a few suits of mail for Arabs, particularly the Baggara of Kordofan and Darfur. They were also making small pieces of mail like cavalry shoulder-guards for merchants who sold them to tribes like the Ingessana and Nuba. They made my suit in twelve days, probably not working all day, and I paid them £15 for it as agreed. It is shown in PLATE XII, a. They also made a suit for the Ethnological Collection in the Khartoum Museum. Their tools are shown in PLATE X, a.
KUSH

The various stages in the making of a suit are as follows:

1. The wire is drawn down to the size selected by the customer through a draw plate of European manufacture. The drawing tongs and chain are also probably European. Only the draw bench has been made locally (Plate X, b.)

2. The wire is now wound round an iron core or rod giving the desired diameter of ring. This rod is thrust through two blocks of wood fixed vertically to a plank on which the winder stands (Plate XI, a.) The iron core rod has a ring forged at its end through which passes another rod, probably an alternative size of core. The wire is being wound on to the underside of the core. As it passes on to the core, it rests on a small shelf fixed outside the pipe which lines the hole in the blocks of wood. This keeps the coil tight and the winder does not have to drag backwards on the wire with his left hand. In this way he produces a right-handed coil. In this case the direction of the coil is not important, but in riveted mail the rings have almost always been cut from right-handed coils*.

3. A large pair of top cutters is then used to cut the coil into rings (Plate XII, b.)

4. Three men then work together, squatting round a small table. The first man, using two pairs of pliers, closes the gap in many of the wire rings. He first brings the ends of the wire together, and then gives the ring a slight squeeze to restore it to a circular shape. (Another man widens the gap in a smaller number of rings.)

5. The second man fastens the rings together in groups of five, by clipping an open ring through four closed ones (Plate XI, b.)

6. The third man (the master) clips the groups of five rings together by passing an open ring through a pair of closed rings from each of two groups, and then closing it (Plate XI, b.) It is in this way that the suit is built up.

Subsequently, I made a number of enquiries aimed at ascertaining whether the riveted mail which had been imported into the Sudan 'from the north' had been made in Egypt, Tripoli, Tunis or elsewhere in North Africa; but everywhere I drew a blank, and am now of opinion that it was not made anywhere in Africa (or in Europe, where it would have been too expensive for the Sudanese market). I am, therefore, inclined to think that it may have come from India, for I am told by Sir James Mann that Lawrence of Arabia failed to trace its source anywhere in Syria or Arabia, although he sought hard for it. I hope that this article may encourage someone else to track this riveted mail down to its source.

A. J. ARKELL.

SOME NOTES ON THE SUDANESE NEOLITHIC

Under the above heading Dr O. G. S. Crawford makes on pp. 88–90 of Kush II some 'exploratory remarks', some of which, if not corrected will, I fear, make what must appear to many people a confusing subject only worse confounded. It is a pity that he has neither mastered the pottery typical of Early Khartoum or Esh Shaheinab, or the chapters on pottery in the publications of those sites. I have already pointed out in Kush II, p. 91, that he is wrong in attributing the site at El Damer to Shaheinab. There is no doubt that it is an Early Khartoum site, or as I prefer to call it, Khartoum Mesolithic. Shigla (Shilqa) which he also mentions as being allied to Shaheinab, is, judging from the sherds which he collected there, and which are now catalogued under No. 9575 in the Antiquities Collection in Khartoum Museum, typical neither of Early

a. THE TOOLS USED IN MAKING MAIL AT OMDURMAN

b. THE DRAW BENCH, WITH EUROPEAN TYPE DRAW PLATE AND TONGS
a. COILING THE WIRE

b. TWO MEN MAKING GROUPS OF FIVE RINGS, WHILE THE MASTER (R) JOINS THE GROUPS TOGETHER
NOTES

Khartoum nor of Shaheinab. The sherds from Shigla do include a few slightly burnished examples which may be related to Shaheinab. The rest, including a few full of mica, look to me nearer a late form of the Khartoum Mesolithic (Early Khartoum).

This brings me to his statements: 'Mica-ware which is extremely common on Shaheinab sites' . . . 'At sites of the Shaheinab type . . . mica-ware sherds are very abundant'. This is quite incorrect. Mica-ware sherds do not occur in the Khartoum Neolithic (Shaheinab type sherds). I do not think Dr Crawford will find the word 'mica' in Chapter 8 of Shaheinab if he will read it. When I published Early Khartoum I was not sure whether the rare Micaceous ware found there was contemporary with the sherds typical of that site. But as I have not found a single sherd of Micaceous ware on a Shaheinab type site, I am now satisfied that it is one of the earlier Khartoum Mesolithic wares. Presumably Dr Crawford's statements are based on the sherds he collected at El Damer and Shigla, neither of which are sites of Shaheinab type.

Dr Crawford writes, 'How much if at all (my italics) earlier Early Khartoum is than Shaheinab is a matter of opinion'. I hope that Chapter 12 of Shaheinab shows that Early Khartoum is definitely earlier than Shaheinab, with the Dotted Wavy Line ware coming between them; but if it does not convince Dr Crawford, will he not reflect that the change in fauna reported by the late Miss D. M. A. Bate can only have taken place gradually, and must imply a very considerable time? I interpret it in the light of the much greater depth of occupation deposit at Early Khartoum than at Esh Shaheinab as indicating that the Khartoum Mesolithic lasted a very long time, before slowly developing the Dotted Wavy Line ware, and then giving place to the Khartoum Neolithic with its new ideas of burnishing pottery and grinding stone implements, etc. In any case, surely the actual evidence of animal remains is more weighty than any arguments based on the typology of pottery, especially since it is admitted that impressed ware has been made in the Sudan from Early Khartoum to the present day? And incidentally, is Dr Crawford right in stating that the Early Khartoum pottery is technically advanced, since none of it is burnished?

Dr Crawford objects to my names Khartoum Mesolithic and Khartoum Neolithic for the two main cultures. I have given my reasons for proposing these names in Shaheinab, p. 102. If after considering all the facts he still objects, he is entitled to his opinion. A rose by any other name would smell as sweet. But I must point out that though he is the protagonist of the use of maps in archaeology, he does not seem to have studied my maps of the distribution of Khartoum Mesolithic sites on p. 116 of Early Khartoum and of Khartoum Neolithic sites on p. 106 of Shaheinab. If he had, I do not think he could have written that the name 'Khartoum Neolithic' was an 'even more unfortunate' change. He would have seen that Khartoum is the centre of the distribution of some twenty sites, all of which are confined to Khartoum Province. Omdurman Bridge is one of these sites, although I did not excavate it as Dr Crawford states. I only rescued the contents of two protodynastic graves dug into the edge of the earlier site and partly excavated by the military, see Early Khartoum, c.10.

Perhaps I may be excused for pointing out that on p. 69 of Shaheinab I have already used the term Impressed Ware for this general kind of pottery, and was feeling my way towards it in Early Khartoum on p. 82, where I used the term, 'Other impressed patterns,' i.e. other than 'Wavy Line'. May I end with a protest against Dr Crawford's proposed term Dot-and-Drag ware? This term is not self-explanatory, and is liable to confusion with a particular kind of decoration which I have already described as 'lines of dragged dots' in Shaheinab, p. 71. If another term of this nature is required to describe the ware typical of Shaheinab, why not Dot-and-Vee ware?—not that I recommend it.

A. J. ARKELL.
KUSH

'SPATHE' SHERDS

In Mr P. L. Shinnie's interesting article in Kush II on the excavations at Tanqasi he mentions on p. 80 sherds with one or more edges worn away; one is illustrated in FIG. 10, 1.

These were found frequently in the Armant excavations and we named them 'spade-sherds' for they were almost certainly used for excavation, particularly by tomb robbers. Our own workmen did the same thing and produced an identical result.

At Armant there exists the remains of a wide road which had undergone only the first stages of preparation, that is, the boulders had been rolled aside and the surface stones, etc., had been scraped into little piles ready for removal. This road led straight up to the foot of the High Desert Cliff and up its centre wound a narrow donkey path to an old Coptic Monastery. A photograph of it can be seen in The Bucheum, Vol. III, pl. x, 5. We suspected for a number of reasons that need not be gone into here, that this road was planned to lead to a new valley of the Kings to be attached to Akhetaten, which, it will be remembered, was originally projected near Thebes and was only later removed to the well-known site at Tell el Amarna.

We tested this hypothesis by spreading the workmen and boys in a line across the road and telling them to collect anything archaeological they could find. They covered the length and brought back a large number of sherds. Most of these were Coptic, clearly belonging to the Monastery path, but a considerable number were Grey-Yellow ware of the XVIII-XIX Dynasties. Of the former only one was a spade sherd but of the latter almost everyone was such*. This showed clearly that the scraping operations of which the little piles bore mute witness, were carried out in the Early New Kingdom, and this was very strong supporting evidence for our identification of the road.

OLIVER H. MYERS.

TWO MEROITIC POTTERY COFFINS FROM ARGIN IN HALFA DISTRICT

In 1950 the news came to Mr A. E. D. Penn, then District Commissioner Wadi Halfa, that villagers at Argin (north of Halfa Town) had, in the course of digging the foundations of a house, unearthed two pottery coffins, one whole one (in two parts), see PLATE XIII, and the top part of another, see PLATE XIV.

The coffins were taken to the District Headquarters and subsequently to the Halfa Museum where they were kept in storage until 1952 when the writer loaded them on an Antiquities Service truck and brought them back to Khartoum where they now are (Museum Nos. 10045-6).

The complete coffin when repaired measured approximately $180 \times 46 \times 13.7$ cm. On the top is depicted a human face and arms as if the figure was swimming; but this theory is ruled out by the fact that on the lower part are two feet with the toes pointing upwards. The face is decorated with a set of three longitudinal incisions, below the orbit and on either side of the nose, and the arms have two incisions on each. Such incisions on the face occur nowadays among the Nuba in Kordofan as a tribal mark, and in certain parts of the Central and Northern Sudan where it is said they have some medicinal value in the treatment of eye diseases. This belief is gradually dying out.

The coffins in question do not seem to have had any lids, considering on the one hand the fact that the features that normally appear on the lid—the face, arms, etc.—are shown on the wall of the coffin itself, and the fact that the upper part of the wall is

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* Full statistics exist but are not available at the moment of writing.
PLATE XIII

FRONT VIEW OF MEROITIC POTTERY COFFIN FROM ARGIN

facing p. 86
FRAGMENT OF MEROITIC POTTERY COFFIN FROM ARGIN
NOTES

rounded off, so that no lid could fit in securely, on the other. The hair is done in a pure Sudanese fashion, in the same way as that of the female sandstone statue from Meroë No. 538 (published in Guide to the X1th Annual Exhibition, pl. ix).

It is suggested that they are of Meroitic date, but no stress can be laid on this as we have seen no parallel. It is a pity there was no trained archaeologist on the spot when the discovery was made. However, in view of the absence of grave goods, the size of the coffins (the maximum internal depth being only 10.5 cm.) and the fact that no bones were found in them, and that the red burnishing is in perfect condition, it is thought that their purpose was merely ritual.

SADIK NUR.

NOTE BY THE EDITOR

I should like to add a few notes concerning the interesting remarks of Sadik Eff. Nur about the pottery coffins of Argin. Professor L. Keimer recently published a small pottery head of a Sudanese woman wearing facial scars (see L. Keimer, ‘Une petite tête romaine en terre cuite représentant une Soudanaise à cicatrices faciales’ in Bulletin Société Archéol. d’Alexandrie, no. 40, 1953, pp. 1–3 and plate). Professor Keimer ascribes the head at least to the Roman period, if not earlier (op. cit, p. 3) and quite reasonably uses it as proof of the existence in the Sudan in ancient times of facial scars (sa’llkh, plur. selukh ‘incisions’) which consist on the Roman head, as well as on the Argin coffin, of three vertical strokes.

During the archaeological survey of the Northern Province in June this year, I was able to visit the spot where the coffins were found. There are obviously other graves in the vicinity, but we were unable to excavate them, owing to the fact that a modern cemetery has been established in the same place. Not very far from there is a cemetery of shaft-graves, where definite Meroitic sherds have been collected. This supports the date ascribed to the coffins by Sadik Eff. Nur, if, as I suppose, all the graves belong to one and the same cemetery. But one must keep an open mind and a slightly earlier date is not to be rejected, not only on the basis of the small Roman head published by Keimer, but also because pottery coffins with human figures, of which the Argin coffins are only an interesting variation, are well known in the Nile Valley from the Ptolemaic and Roman periods. (See, for instance, Jéquier, Ann. du Serv., 29, pp. 160–1; ibid. 30, pp. 111–12 and pl. iv; Reisner, Archaeological Survey of Nubia, I, Report 1907–1908, pp. 76, 178, 200, 201, 203, 205, 207, and plates 11, 12 and 14.)

J. VERCOUTTER.
Reviews


The second volume of the Royal Cemeteries of Kush concerning the Nuri pyramids appears after volume III of the series, but the importance of the new volume is in itself an explanation of the delay. Archaeologists will be most grateful to Dows Dunham, who has unselfishly undertaken—and completed—one of the most arduous and thankless tasks in archaeology, namely that of publishing archaeological material from excavations supervised by a scholar who is now dead, and collected from files which he did not entirely compile himself.

The excavations here recorded were carried out between 1916 and 1918: the final survey and revision were done in 1920—thirty-six years ago. Moreover, since that time the finds have been mainly divided between the Boston Museum and the Khartoum Museum, while minor presentations have been made to various museums: the Cairo Museum, the Musée d’Art et d’Histoire de Bruxelles, the British Museum, the Ashmolean Museum, Oxford, etc.: this did not help the editor. Furthermore, the pyramids themselves were in a far country and much of the material had been left in situ. Notwithstanding these drawbacks, Dows Dunham has produced an excellent book and thanks to him the significant material is now available for study.

For a description of the site and for the history of the excavation he refers to the first volume of the series, and the Nuri volume deals only with a description of the tombs (pp. 7–253) preceded by a few but most important pages which treat of the chronology; with a chronological list of the Tombs at Nuri (pp. 2–3) and the ‘plundering of the Nuri cemetery’ (p. 4). This is of special interest since it explains convincingly ‘the finding in one tomb of objects inscribed with the name of the owner of a different one’, and demonstrates that some of the plundering took place while the cemetery was still in use, while most of it took place in Coptic times.

All the descriptions of tombs follow the same pattern: description of the superstructure, enclosure, chapel, foundation deposit, substructure, and burial. A list of the finds is given together with illustrations of the most important objects. There are also plans and sections of each tomb.

At the end of the volume there are descriptions of the surface finds (pp. 253–4), a graphic study of the types and inscriptions of Shawabtis for each reign (figs. 197–208 and pp. 255–64), and, finally, excellent facsimiles of the stelae found during the excavation. Most of the information concerning the actual building of the pyramids (superstructure, enclosure, etc.), as well as the composition of the foundation deposits is summarized in three charts which enable us to follow the evolution of burial customs throughout the period. An index showing the present location of objects found at Nuri—so far as this is known (Location Index)—and a General Index complete the volume, which is illustrated by 141 photographic plates.

The Nuri volume will prove, I am sure, invaluable to archaeologists, who have now a mine of archaeological information on this little known period of history.

When checking some of the material which is in the Sudan Museum, I noted some minor points which I give here, not as criticism but as a token of admiration for the achievements of the book:—
p. 53, figs. 34: The green stone scarab no. 18–2–236 (Nu. 81) of which Dunham notes (p. 54). 'This scarab is in Khartoum: the field record includes no photograph and no copy of the inscription: only the back was drawn'. I checked the object and found that the inscription is exactly the same as that on the heart scarab of Queen Yeturow (see no. 18–2–243, fig. 22, p. 37), with the cuts for lines at the same points.

p. 83, Alabaster vase no. 16–4–74b (= Khartoum 1386 a). The cartouche is clear on the original. At the end of the inscription read \( \text{\textit{\text{nfr}} \text{ is certain.}} \)

p. 105, fig. 79: In the location index no. 18–3–589 (sw Deposit, 6 cups) is given as now in Khartoum under no. 1556. Under this number we have in Khartoum only four cups, with a note ‘2 sent to British Museum, 1 to Ashmolean, 1 to Brussels’. The ones we have now are: 1556 a ‘King’s mother Nasalsa—\( \text{(N.B. more probable than } \text{: see Dunham under 18–3–541, same reference)} \); 1556 b ‘Nasalsa, \( \text{sic} \); 1556 c ‘Nasalsa, \( \text{; 1556 d ‘Nasalsa} \); 1556 d ‘Nasalsa}. Inside Khartoum 1556 c is a small label with the note ‘from Pyr. xxiv, n.w. 19, 18–3–545’ and inside 1556 d a label with ‘from Pyr. xxiv, n.w. 19, 18–3–542’; in the Location Index, p. 293, Nos. 18–3–541–545 are given as being in the Cairo Museum, numbered 46534. So it seems that some confusion has arisen.

p. 142, fig. 106, no. 17–1–656 (= Khartoum 1386 b) read \( \text{\textit{\text{?}}} \)

p. 152, fig. 114, no. 17–3–131 (= Khartoum 1377), line 7, read \( \text{\textit{\text{?}}} \); line 9, read \( \text{\textit{\text{?}}} \) and line 10 in fine \( \text{\textit{\text{?}}} \) (\( \text{\textit{\text{?}}} \) and \( \text{\textit{\text{?}}} \) are clear on the original). The material of which this scarab is made has been identified by G. Andrew, late Sudan Government Geologist, as ‘probably epidiorite’.

p. 156, fig. 117, no. 17–1–280 (= Khartoum 1375): another part of this vessel, which is not described, is in the Khartoum Museum and completes the inscription which runs:

\( \text{\textit{\text{i}}} \)

p. 170, fig. 127, no. 17–2–15 (= Khartoum 1358 b): the design is not ‘four snake-heads’ as described on p. 169, but four ram-heads. The incurring horns are clear in the original. It is, I think, the well-known god Amon with four ram heads, which appears in the late period on magical objects, e.g. Hypocephali
KUSH

(cf. for instance, Daressy: *Textes et Dessins Magiques*, no. 9444, pl. xiii and p. 51) and, if I am not mistaken, among the Decan gods. These are very minor remarks indeed, and we owe a great deal to Dows Dunham, not only for collecting and publishing Reisner’s material, but also for providing such a good tool for the study of Napatan archaeology.

J. VERCOUTTER.


In the first millennium B.C. the kingdom of Napata and the metropolis of Meroë can be looked upon as bridging the gap between Egypt and Negro Africa. A number of similar cultural institutions such as the ritual killing of the king, brother-sister marriage, the position of Queen-mother, matrilineal inheritance, etc., are also found among modern sudanic tribes. After the destruction of Meroë, A.D. 350, meroitic culture seems to have flowed into the South (Sennar) and the South-west (Kordofan and Darfur) with immigrations of meroitic refugees. It may be assumed that the first impulses for the foundation of states emanated then. They may have lead also to the foundation of the kingdoms of Kafa, Monomotapa and Kuba (between Kassai and Sankuru).

W. VYCIHL.


This very interesting article deals with the question of king killing in the Sudan. We know of king Ergamenes who cut the throats of the priests instead of letting them kill him. We also have a story of Frobenius about the custom of king killing in Kordofan and how one king was able to avoid his fate. This custom is in practice, or now in a dormant form, among the Shilluk, the Burun and other tribes and even the Nasrallāb Arabs as the viceroys of the Fung kings in the North are said to have practised it. The Dar Fung tribes and the Nuba of Southern Kordofan represent an older stratum of the Sudan, which is due to their isolation and the poverty of their present lands. On the contrary, the Shilluk are a relatively young tribe.

W. VYCIHL.

ERRATUM