

THE JOURNAL OF
Egyptian
Archaeology

VOLUME 52
DECEMBER 1966

PUBLISHED BY
THE EGYPT EXPLORATION SOCIETY
2 HINDE STREET, MANCHESTER SQUARE, LONDON, W. 1

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PRINTED IN GREAT BRITAIN
AT THE UNIVERSITY PRESS, OXFORD
BY VIVIAN RIDLER
PRINTER TO THE UNIVERSITY

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EDITORIAL FOREWORD

THE three substantial campaigns of excavation undertaken by the Society's field-workers during the season 1965-6 are reported in the pages of this volume. Professor Plumley's work at Qaşr Ibrîm has yielded the most dramatic results; his finds of documents at this stark site are among the most important achievements of the international campaign in Nubia. At Saqqâra and Tell el-Farâ'in the work has been less spectacular, but it is as well to remember that the importance of an excavation should not be estimated only by the obvious treasures that are found. The kilns of Buto have already quickened the pulse of the glass technologist; some of the secrets of faience-manufacture may be close to discovery.

Death has taken a particularly grievous toll among the ranks of Egyptologists this year. Of members of the Society who have died, special mention must be made of Professor Erichsen who has always worked closely with demotic scholars in this country. Mr. H. S. Smith writes:

The death of Professor Wolja Erichsen on 26th April, 1966, following closely on the death of his friends, Professor Askel Volten and Professor C. E. Sander-Hansen, is a severe blow to Danish Egyptology, and to demotic scholars everywhere. Indeed, every student of ancient Egyptian texts is under daily obligation to Professor Erichsen; for it is to him that we owe the marvellously clear and consistent autography of the five text volumes and volume II of the Belegstellen of the great Berlin *Wörterbuch*, a total of 3,258 pages. The steadfastness with which over the years he pursued this and his other labours on the Dictionary were typical of the man and his selfless devotion to Egyptian philology. Many might have rested content with such a monumental service to their colleagues. But Erichsen, on his return to Denmark before the war, set about making more accessible to Egyptologists, and more particularly to students, the most inaccessible, the most difficult, and the most neglected body of Egyptian texts, those in the demotic script. At that time there was no chrestomathy, sign-list, palaeography, or any dictionary since Heinrich Brugsch's pioneer work. By single-minded devotion Erichsen gradually filled these gaps as well as one man could, with his *Demotische Lesestücke* (1937), *Auswahl frühdemotischer Texte* (1950), and finally his *Demotisches Glossar* (1959). But to judge Erichsen by his published work was to miss what was best in the man, his unbounded generosity and magnanimity to all who shared his enthusiasm for Egyptian studies. In his later years it was to him that demotists turned in their difficulties as to an oracle, and it was rarely that he failed them; while to students he was unendingly kind. He was among the most modest and courteous of scholars and a true friend.

On July 7 Professor Hanns Stock died in a tragic road accident. He will be long remembered as an energetic and successful Director of the German Archaeological Institute in Cairo, the work of which had in recent years moved into a remarkably fruitful phase. He willingly sacrificed opportunities for pursuing his own studies in order to advance the work of his Institute. Another great loss from the Cairo scene has resulted from the death of Dr. Alexandre Piankoff on July 20 in Brussels. He has

earned the gratitude of all Egyptologists for his work on the religious texts in the royal tombs of the New Kingdom; his enthusiasm was at all times singularly free from special pleading in a field noted for eccentric interpretations.

Monsieur Pierre Monet, formerly professor at the University of Strasbourg, and later at the Collège de France, died on June 18 at the age of 80. To him came the rare excitement of the discovery of the royal tombs at Tanis, but he will be remembered for much else besides, not least his work at Byblos and his studies of scenes in Old Kingdom tombs.

Earlier in the year, on April 8, died Madame Militza Matthieu, the Curator of the Egyptian Department in the Hermitage Museum, Leningrad. Her published work was mostly in the fields of religion and art, and she was well-known to Egyptologists everywhere as a generous and helpful colleague. She had been in ill health for many years.

Mention should also be made of Mr. George William Murray who died on January 31 at the age of 80. He spent his working life in the Survey of Egypt and was finally the Director of the Topographic Survey. A keen amateur of Egyptology, he used his special knowledge to our great advantage in writing about geographical and related matters affecting the history and life of ancient Egypt. Another enthusiast who employed his particular skills on the elucidation of Egyptological problems, was Mr. Hollis S. Baker, an American furniture manufacturer, who died on June 12. His work on ancient furniture, and especially Egyptian furniture, had just come to fruition, but a cruel fate prevented him by a matter of a few days from seeing his volume in print.

From a position of relative austerity in Britain one contemplates with some envy the considerable expansion of opportunities for Egyptological studies in other countries, in particular the United States of America. The emphasis on science and technology in higher education in that great country has not prevented the establishment of new positions for Egyptologists in old and new universities there. When may we hope to see the same happening in one of the new British universities?

Notice

Miss Elizabeth Thomas, who has for many years been preparing a study of the royal tombs, has now completed her work *The Royal Necropoleis of Thebes*. A limited number of copies, privately printed, are available for purchase at a price of \$14.00. This sum includes postage for all countries, and also insurance for transmission in U.S.A. and Canada. Purchasers in other countries who wish their copies to be insured should add \$1.50 to the price. Orders should be sent *with remittance* direct to Miss Thomas, 308 Edgerstoune Road, Princeton, New Jersey 08540, U.S.A.

PRELIMINARY REPORT ON THE EXCAVATIONS AT NORTH SAQQÂRA, 1965-6

By W. B. EMERY

OUR excavations in the archaic necropolis of North Saqqâra were reopened on December 1, 1965 and were confined throughout the four months' period of work to the north-west area of the concession, following up the discoveries made last season (fig. 1). The staff of the expedition consisted of Mr. G. T. Martin (Christ's College, Cambridge), Miss A. Millard (University College, London), Mr. Ali El-Khouli (Egyptian Antiquities Service), my wife, and myself. Mr. and Mrs. H. S. Smith joined the expedition at the end of February.

As stated in our last report, the first objective was the discovery of the entrance to the ibis catacombs, access to which had only been attained through the shafts of archaic and Old Kingdom tombs. We confirmed, however, the experience of previous explorers of this labyrinth, that no satisfactory examination was possible by the method of first clearing a ten-metre shaft and bringing up debris from the galleries by pulley and basket. Examination of large dumps adjacent to the Abusîr-Serapeum road showed that they were not, as previously thought, the results of ancient plundering, but consisted entirely of masons' chippings which, to judge from the chisel marks, were of Saite-Ptolemaic date. From their positions on the lower levels of the valley, it was obvious that these were the spoil dumps of the builders of the ibis galleries, and large-scale clearance was commenced in this area. On December 22, rough stone walls were revealed, and in the days following a small chapel of Ptolemaic date was uncovered, with a wide courtyard in front of it. The whole structure was built of rough stone and mud brick faced with white gypsum plaster. Although rather disappointing in appearance, this find yielded objects of considerable interest, the most important of which were demotic and Greek ostraca (pl. I, 1 and 2). Mr. Smith and Professor Turner, who are now studying these ostraca, report as follows:

The ostraca found in and around the shrine in G 7 constitute twenty-three separate documents (19 demotic, 2 Greek, 2 bi-lingual). Of these, 3 large demotic documents (G 7-25, G 7-26, G 7-31) and one Greek (G 7-43) preserve substantially complete texts; nine others, while incomplete, yield something of value. Two documents (G 7-36, G 7-43) are dated to the joint reign of Ptolemy Philometor, Ptolemy Euergetes II and Cleopatra II (168-163 B.C.); two others (G 7-26, G 7-39) probably belong to the reigns either of Philometor or Euergetes II; most of the rest may very well belong also to the second century B.C.

G 7-25 and G 7-31 are dream-texts. The former is a report (*mkmk*) to the priests recording a dream of Isis crossing the sea from Syria to Egypt, which is interpreted as an augury that Ptolemy's eldest son shall succeed to the throne and his progeny to the fourth generation. G 7-31 records two obscure dreams apparently dreamt on a festival of Thoth. They are listed simply as 'The First

(dream)', 'The Second (dream)', as in Bologna papyri 3171 and 3173. G 7-26 is the report of an oracle or pronouncement of the god, auguring the good health of Ptolemy and the succession of his son. The Greek text G 7-43 includes several drafts of a letter or report addressed to the Ptole-

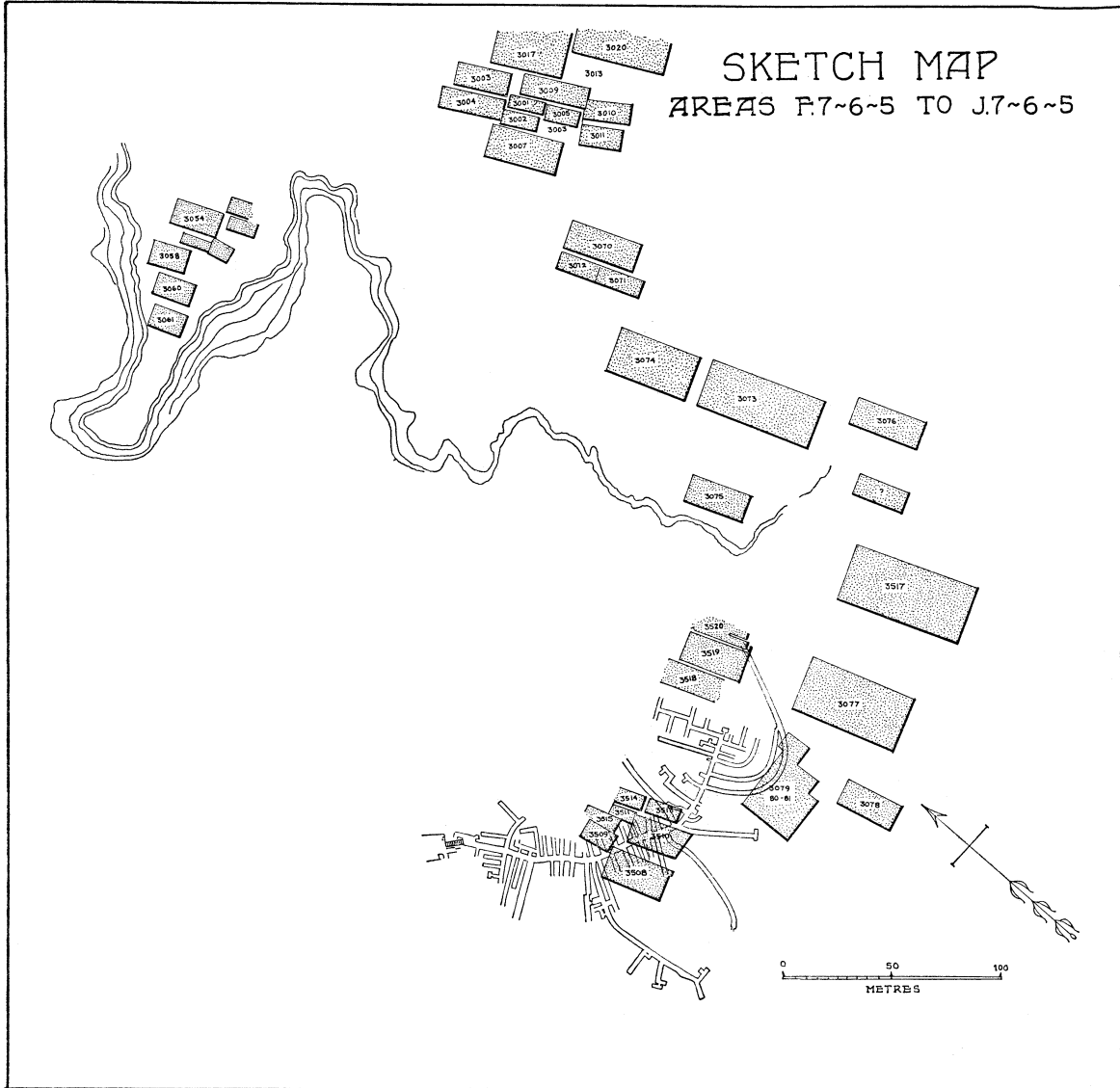
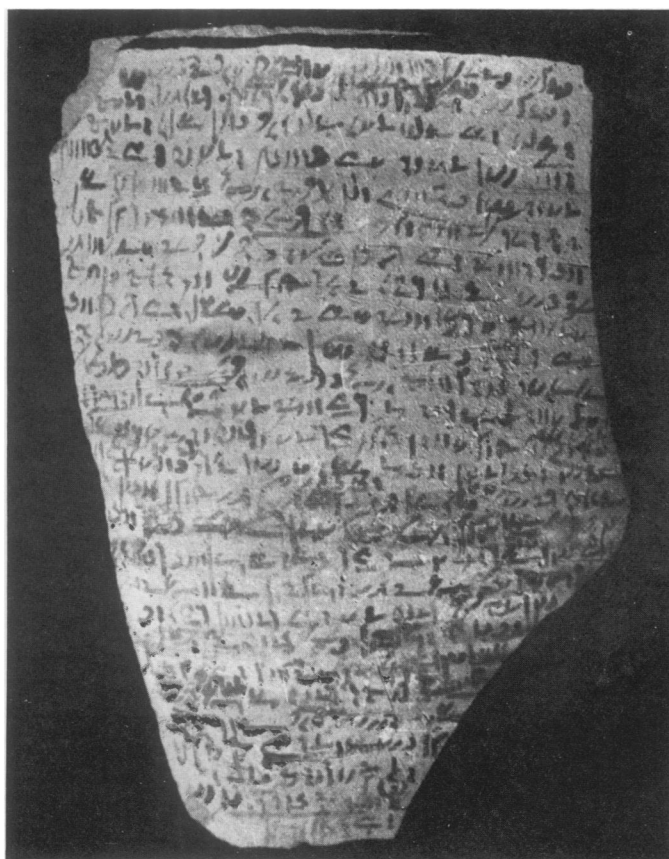
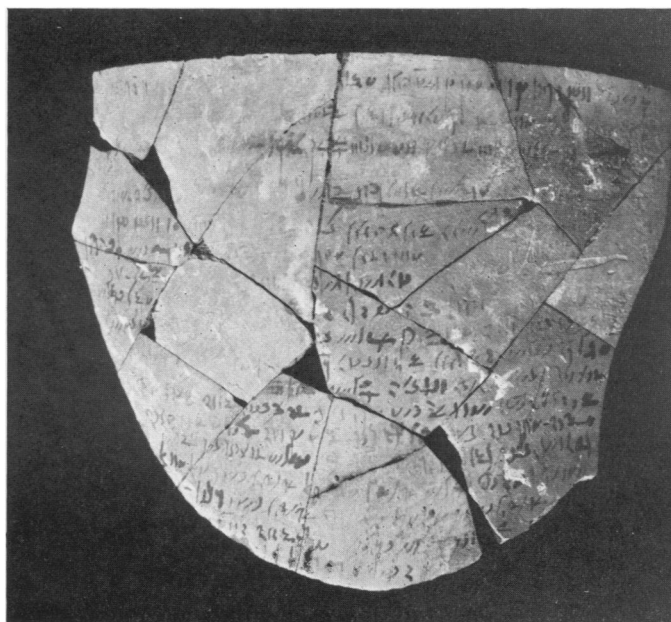


FIG. 1

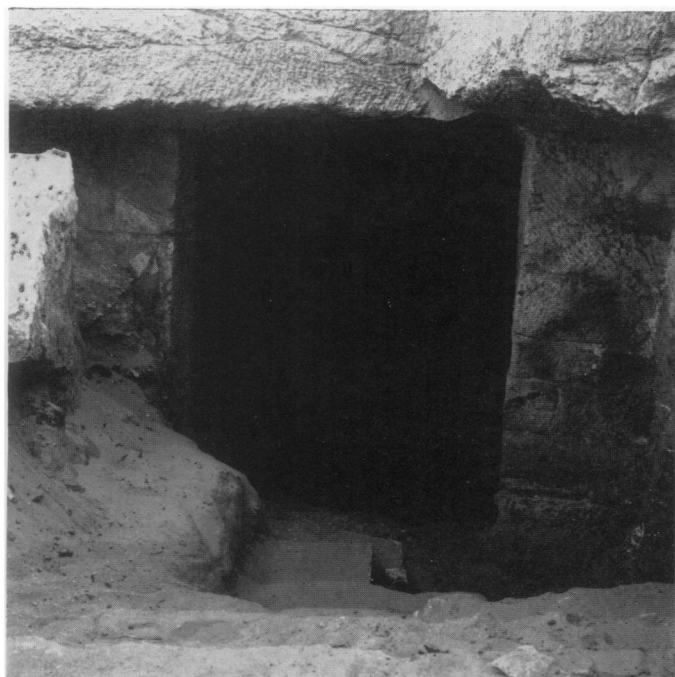
mies in the Thebaid, announcing some thing considered useful or favourable to them; it seems to emerge from the parallel text that this is in fact a decision or oracle of Hermes Trismegistos. The other texts appear to be mainly reports of complaints or requests to the god Thoth and the decision of his oracle concerning them. The fact that the texts are so consistently concerned with obtaining the decisions of the god should mean that there is no mere chance about their being found together near the chapel; the chapel must have contained, as its architecture suggests, a shrine image which was consulted as an oracle. The fact that Thoth occurs in no less than thirteen of the documents (all those which are well conserved), at least twenty-two times in all, and is directly appealed to in several of the documents, leaves little doubt that it was his image which dwelt in this particular



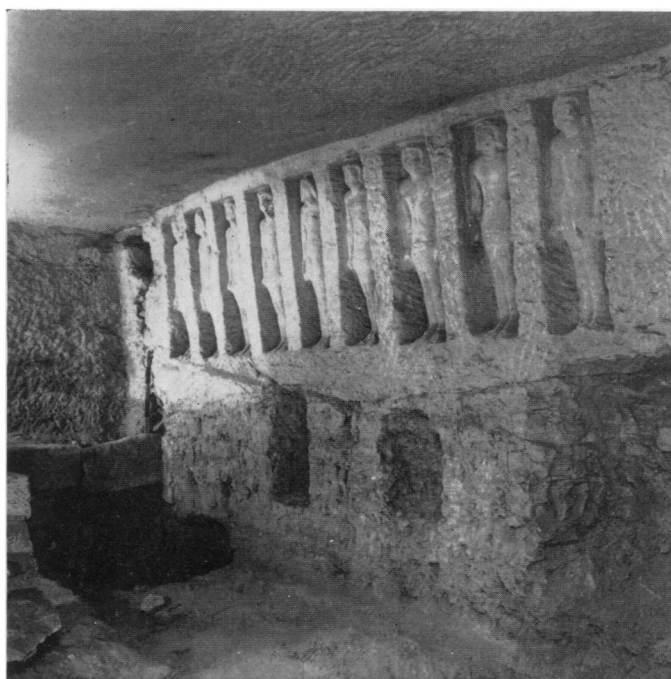
1. Demotic ostracon with Dream Text (G 7-25)



2. Demotic ostracon with Good Augury (G 7-26, obverse)



3. Entrance to the ibis-catacombs

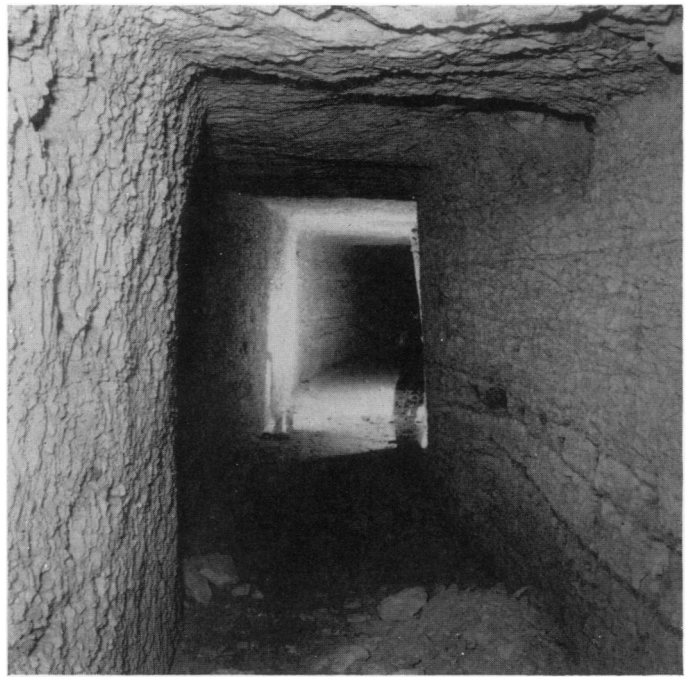


4. The statue room

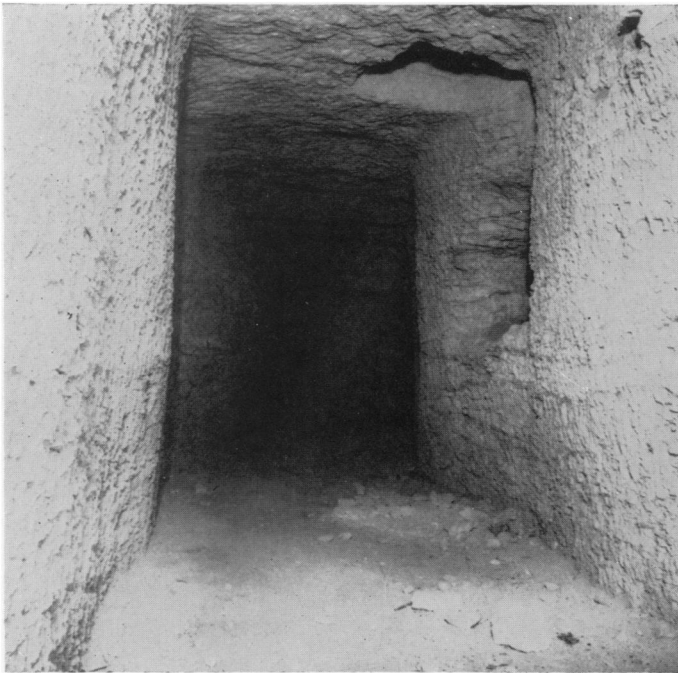
PLATE II



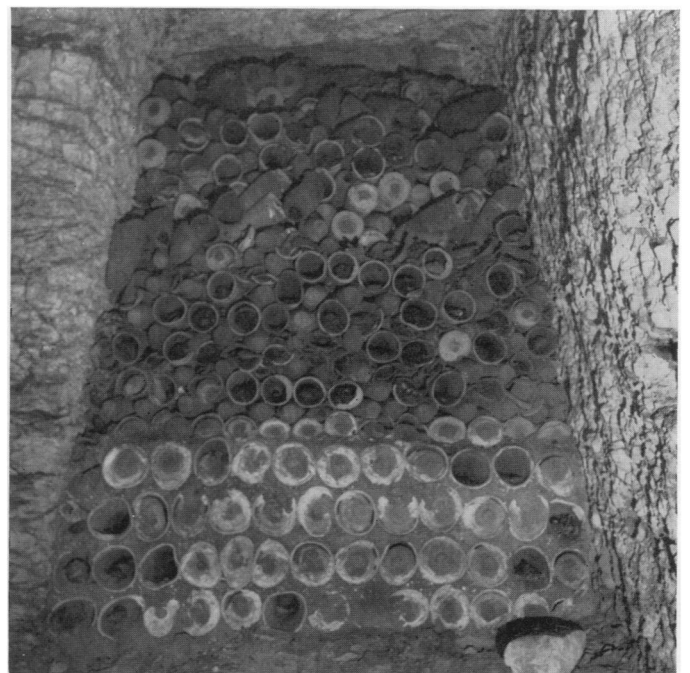
1. The main gallery half filled with sand



2. A side gallery cleared of debris



3. A side gallery which has penetrated an Old Kingdom burial shaft



4. An intact gallery filled with ibis-burials

shrine. Isis is the only other deity to play a major part, and she occurs only in seven documents. The fact that oracles were given for the Pharaoh suggests that this was a shrine of great importance.

Leading from the courtyard, and on the north side of the chapel, was a rock-cut stairway leading downstairs towards the east. When this was finally cleared on January 5, a rock-cut gate with stone jambs was revealed, giving access to the main gallery of the catacombs (pl. I, 3). Thenceforward the systematic exploration of the catacombs was continued at intervals throughout the rest of the season. To judge from the crude workmanship of the entrance gate, it would appear unlikely that this was the principal entrance which, I think, yet remains to be discovered. Its position so near the spoil dumps suggests that it may have been something in the nature of a 'works exit', and was only used later for ceremonial purposes when the chapel was built. So vast is the extent of the catacombs (Paul Lucas, during his visit to Saqqāra in 1716, claims to have traversed 4,000 metres of passage, and we have already explored a total distance of more than 1,800 metres) that a single entrance would obviously be inadequate for the working parties (fig. 2). There is little doubt that other entrances exist: some for work purposes, others—probably more pretentious in design—for the ceremonial burial of the ibis. The galleries, which have an average measurement of 4 m. in height and 2.50 m. in width, are blocked at intervals by hundreds of tons of sand that have fallen from archaic and Old Kingdom burial shafts which had been penetrated when they were cut in the soft limestone (pls. II, III, 1, 2). The builders of the galleries had blocked these shafts, but in many cases the retaining walls and roofing have given way under the pressure of debris or have been broken by tomb robbers. This makes exploration difficult and presents us with a great problem, for there is every indication that only a small part of the underground complex has as yet been revealed, and its complete excavation can only be achieved with greater resources than we at present command. In addition to this, the condition of the rock in certain areas is dangerous, and a considerable amount of shoring up with timber and stone will be necessary before large-scale clearance can be undertaken with safety. This particularly applies to the room with a frieze of rock-cut statues, discovered during the work of last season (pl. I, 4). This room, of early Old-Kingdom date and incorporated in the ibis catacombs, was cleared down to floor-level in which were two deep pits; but all work in this interesting area had to be suspended because of rock falls and the fragile condition of the roof. Another dangerous feature of the work lies on the surface above the catacombs, because the clearance of debris in the galleries frequently causes sudden subsidence in places where the mouth of some Old-Kingdom burial shaft is situated a few feet below the surface of the modern ground level. Many of the passages have been plundered and visited by explorers of the eighteenth and nineteenth centuries, as is shown by the walls and ceiling blackened by the smoke of their torches. But other areas notably gallery 76, are intact, and the presence of hundreds of thousands of mummified ibis in their pottery containers, stacked in their original locations, has been established (pl. II, 4). From gallery 38, some hundreds of ibis mummies were removed and examined, with profitable results; for many specimens were finely decorated with

elaborate, patterned bandaging and with appliqué figures of various gods and religious emblems of a unique character. More than a hundred varieties of patterned decoration

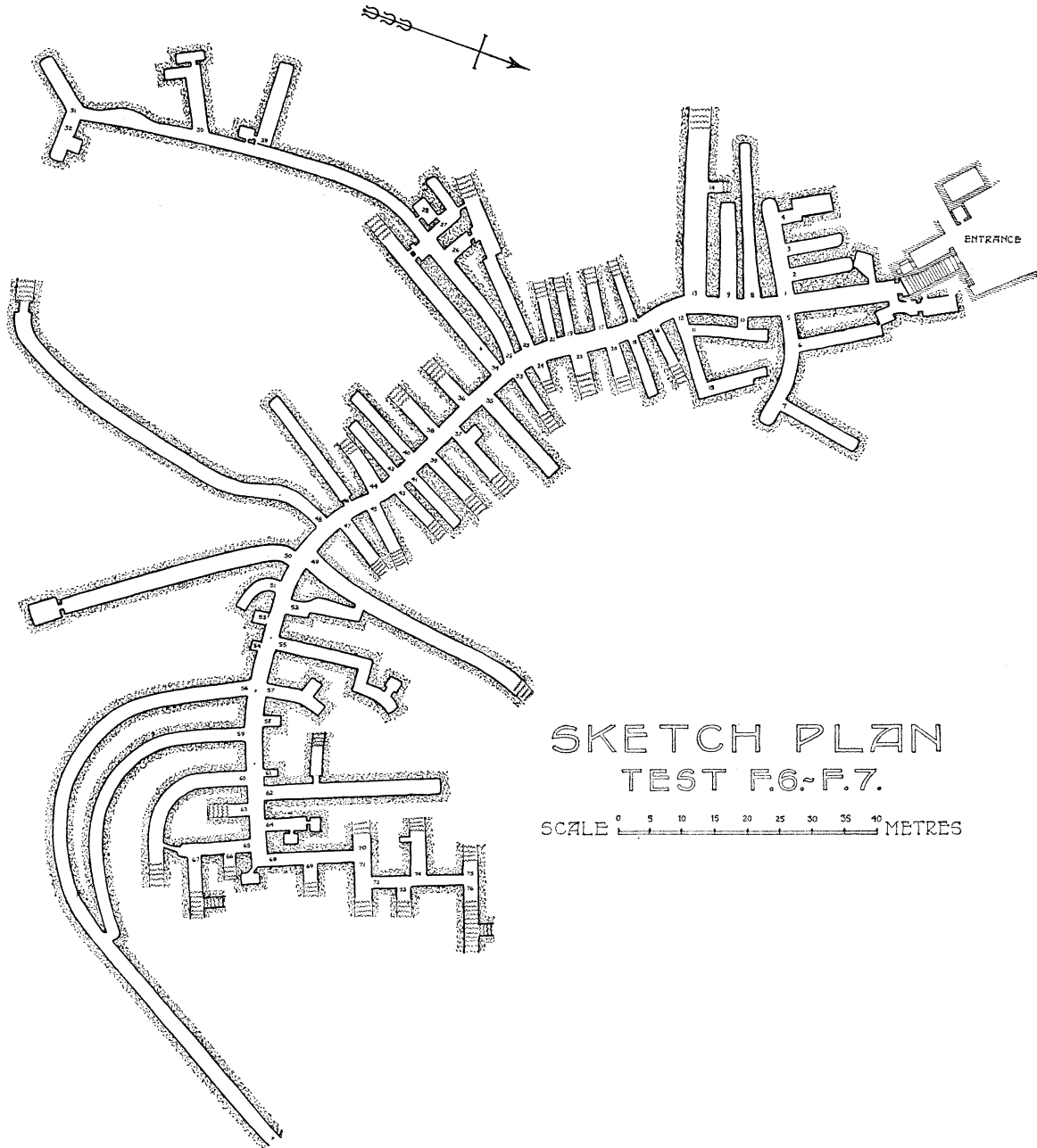


FIG. 2

on the bandages have been registered and appliqué figures of the following deities and their emblems have been noted: Thoth, Imhotep, Isis, Nefertum, Hathor, Ma'at, and the cynocephalus baboon.

On January 20, while test-trenching to find a safe dumping ground immediately east of Tomb 3077, the excavation uncovered what is perhaps the most inexplicable

discovery yet made in this area of the necropolis: a vast tomb of the Third Dynasty larger than any of this period hitherto found at North Saqqâra (pl. III). Many weeks were spent in its excavation, with most unrewarding results, for apart from damage by flood water, the mud-brick superstructure had been deliberately cut down in late times and levelled to a height of one metre above the original ground level (fig. 3).

TOMB N° 3517

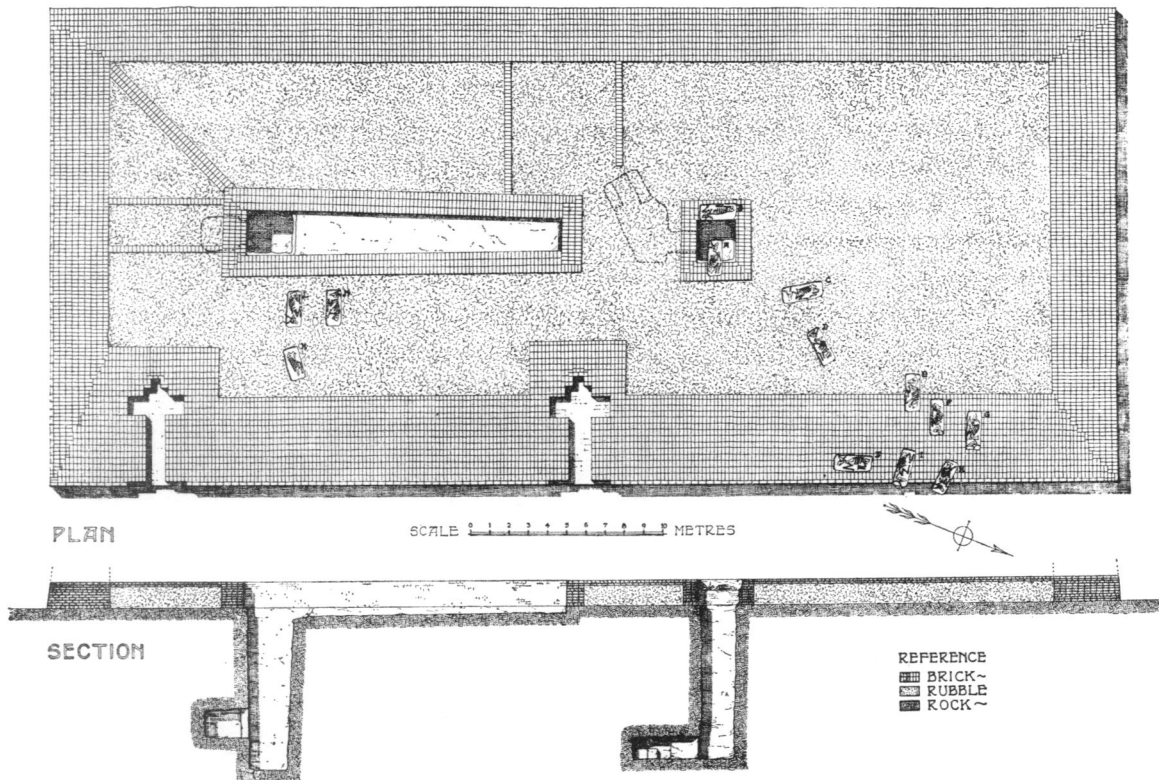


FIG. 3

Like other tombs of the early period in the vicinity of the ibis hypogeum, the two shafts and burial chambers had been systematically cleared out and filled with sand. On the east side of the head of each burial shaft were graves of sacrificed bulls of Ptolemaic date, and on the north end of the superstructure there appeared the barest traces of a building or pavement of this period. At a low level in the sand-filling of these shafts, a few fragments of stone vessels and tables of diorite and alabaster showed by their fine quality that the original owner of this big tomb was a person of very high rank. But no evidence of identification remained, and no explanation can be put forward why this particular tomb should have been treated in this manner during the Ptolemaic period, while its immediate neighbours were ignored (fig. 1).

The last two weeks of the season's work were devoted to a brick structure west of the entrance to the catacombs, which would appear to be another means of entry, perhaps more important than the one already discovered. Here in the disturbed debris, two more demotic ostraca and some tattered fragments of papyrus were found. The

latter included a scrap of a late hieratic religious book, a portion of a demotic account and the protocol of a legal document. Of little importance in themselves, these findings suggest that the site may in future yield papyri.

Various tests have shown that the area of the remains belonging to the ibis hypogeum

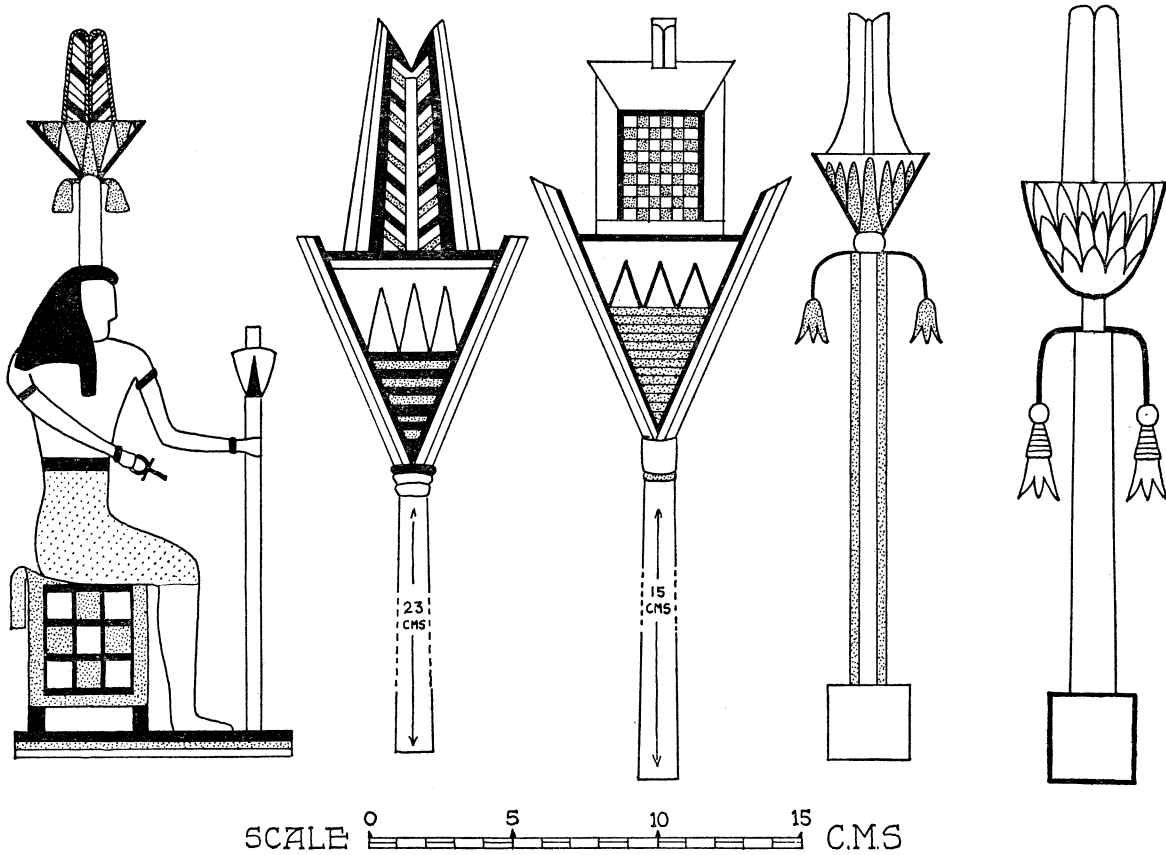


FIG. 4

is far larger than at first supposed and that operations on a bigger scale will be necessary if satisfactory results are to be obtained. In connexion with the possible identification of the site as part of the lost Asklepieion, one feature of the result of this season's excavation is significant. This is the marked predominance among the small amulets found, of figurines of the god Nefertum and of the emblems of this deity in the appliqué decoration on the wrappings of many ibis mummies (fig. 4). Nefertum, as the son of Ptah, was identified with Imhotep during the Saite-Ptolemaic period.

The Fifth-dynasty tomb of Hetep-ka discovered last season has now been completely restored under our direction by the Department of Antiquities and will be open to visitors to Saqqâra in the near future. As usual the expedition has received every possible assistance and co-operation from the Department of Antiquities who made available the decauville railway, and an electric generating plant for the work of exploration in the ibis galleries.



1



2

Stone sarcophagi at the bottom of Old Kingdom shafts which have been penetrated by galleries



3. The superstructure of Tomb 3517 from the south



4. The superstructure of Tomb 3517 from the north

QAŞR IBRÎM 1966

By J. MARTIN PLUMLEY

THE fourth season of the Egypt Exploration Society's work at Qaşr Ibrîm in Nubia was begun on February 1 and terminated on March 19, 1966. The staff of the expedition, which was directed by the Reverend Professor J. Martin Plumley of the University of Cambridge, included Mrs. G. A. Plumley, Dr. Violet D. MacDermot, Mr. Kenneth Frazer, Mr. Colin Walters, and Mr. Mohammed el-Hafiz Goma, Inspector representing the Antiquities Service. There were fifty-three workmen from Quft under Reîs Mohammed Ahmed Mahmoud el-Shammour. The staff was accommodated on the houseboat *Hathor*, and the tug *Seti* was in attendance during the whole of the season. The Egypt Exploration Society wishes to record its thanks to the Director General of the Antiquities Service for much valuable assistance, and more especially for the use of the houseboat.

The area selected for excavation was determined on the west side by the limit of the 1963-4 excavations of the so-called Podium; on the north side by the south wall of the Great Church; and on the east side by an arbitrary line drawn from the south-east corner of the Great Church which passed through the Bosnian house numbered 150 on the 1963 Survey Plan. The south side of the excavation area was defined by a second arbitrary line 1.50 m. south of the axis of the Podium (fig. 1).

The whole of the excavation area was covered with the remains of Bosnian houses from the second half of the sixteenth century A.D. (nos. 250, 295-310). The dismantling of the massive stone walls of these structures resulted in the recovery of a number of small funerary stelae of the Christian Period, inscribed either in Greek or Coptic. A small portion of a pharaonic temple wall decorated with four incised figures poorly executed was also found built into these walls (pl. IV, 1). In the subsequent excavation of the floor of house no. 299 a large pot, 55 cm. in height, was found. This had been sealed when buried, and contained a number of smaller receptacles each filled with Arabic manuscripts (pl. IV, 2). These appear to be letters, accounts, and legal texts of the late seventeenth and early eighteenth centuries A.D. There are between 250 and 300 manuscripts in all. Future study of the contents of this large documentary find may be expected to fill a gap in the history of Nubia during the Ottoman period.

The removal of the Bosnian houses 306-8 revealed that these had been built on top of a massive mud-brick complex. Some of the original vaulted roofs were still in position. Although the purpose of this structure is not entirely clear, the absence of doorways and windows would suggest that it was probably a storehouse or magazine, contemporary with the lower walls of buildings found on the Podium site during the 1963-4 season. The discovery of a pot containing four leather scrolls written in Old Nubian, concealed in a wall of this structure suggests that the building may be dated

PLATE IV



1. Pharaonic block with incised figures



2. Pot from House 299 with some of the document-containers found in it



3. Lion inscribed with the name of the Meroitic king Yesbêkhe-Amani



4. Wooden angel pectoral



5. Stela of Georgios, Bishop of Ibrîm

to the last period of organized Nubian Christianity. At a yet undetermined period rough openings had been hacked through the partition walls and it would seem that the complex had been used for some time as a dwelling house before the coming of the Bosnians in the early sixteenth century A.D. After their settlement on the fortress the main chambers were filled in with rubbish to make a foundation for the heavy stone houses which were constructed above.

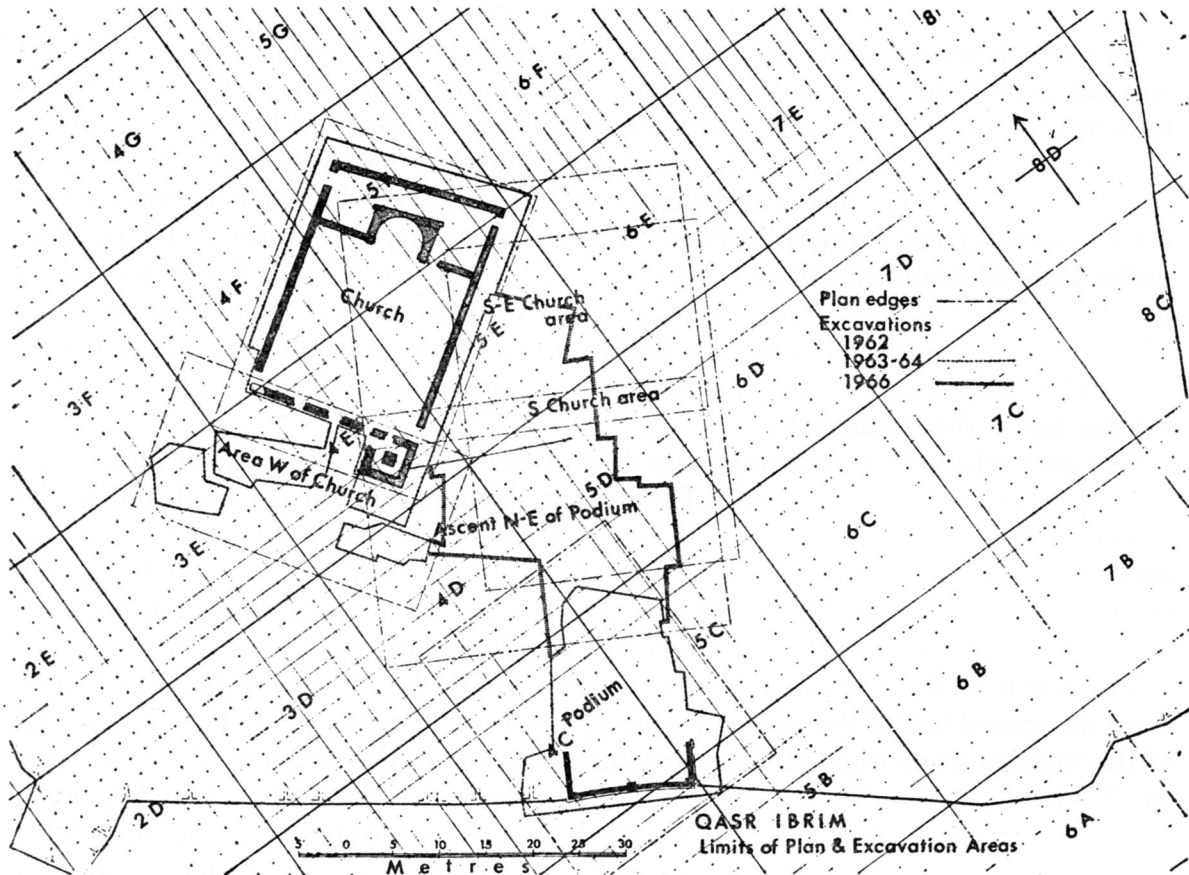
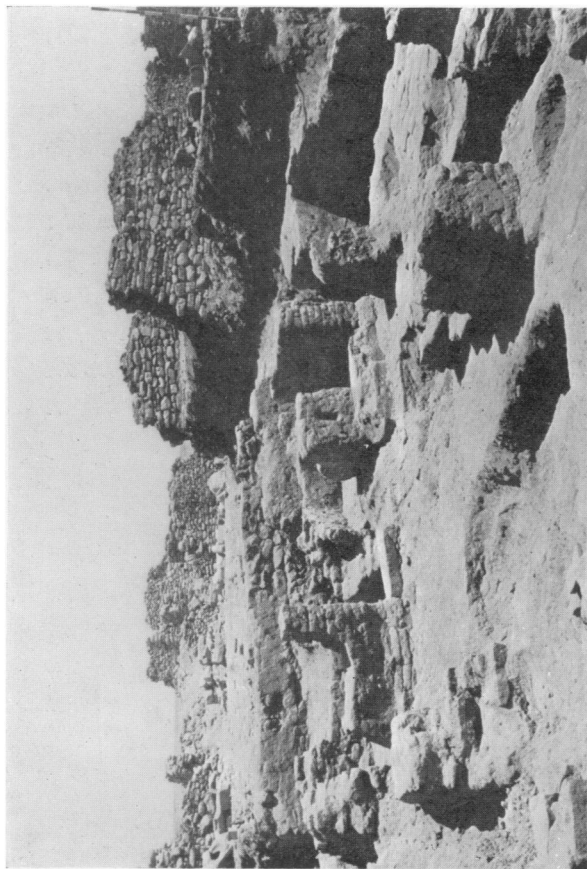


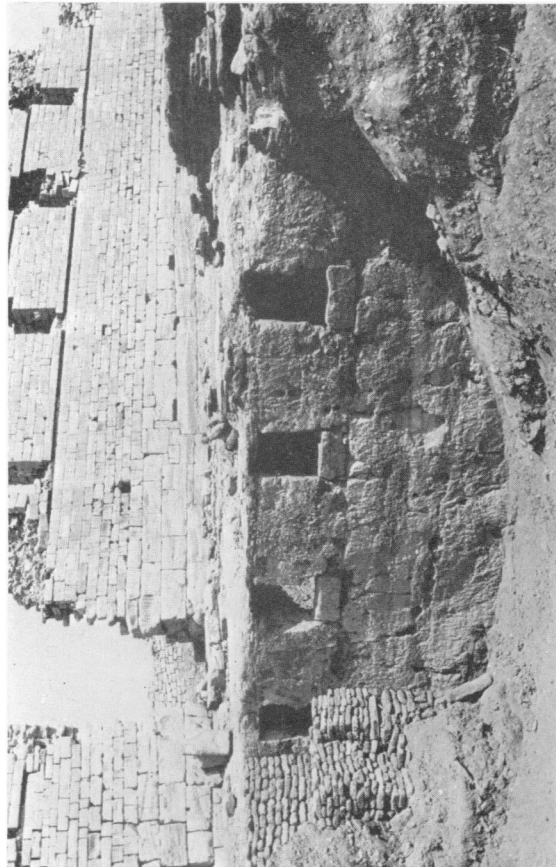
FIG. 1

The extension northwards of the excavation area revealed that the *gebel*, on which the major part of the Great Church stands, passed under the west tower and proceeded in a southerly direction, and that the top of the *gebel* had been levelled and paved.

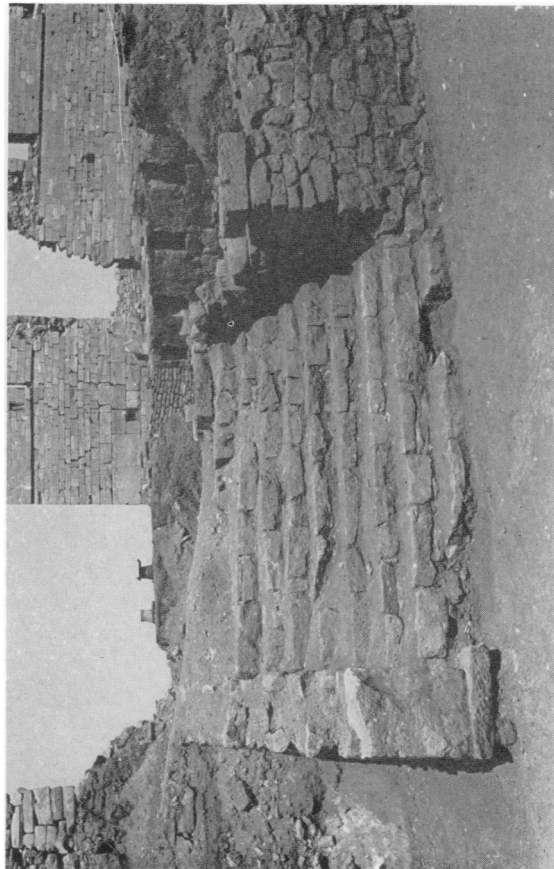
On this levelled area were the remains of a small church (pl. V, 1). Most of the lower stone courses of the outer walls and the lower parts of the interior mud-brick piers and apse were standing to an average height of 60 cm. Apart from one very fine, large, painted pilgrim bottle, and some fragments of pottery, very little was found in the interior of this building. To the west of this church lay a cemetery for the burial of important ecclesiastics. The foundations of several burnt-brick tombs were uncovered. One such burnt-brick tomb was complete, but as proved to be the case with all the other tombs discovered subsequently, this had been entered and robbed in antiquity. On the white washed interior walls of this tomb were three poorly written but legible texts in



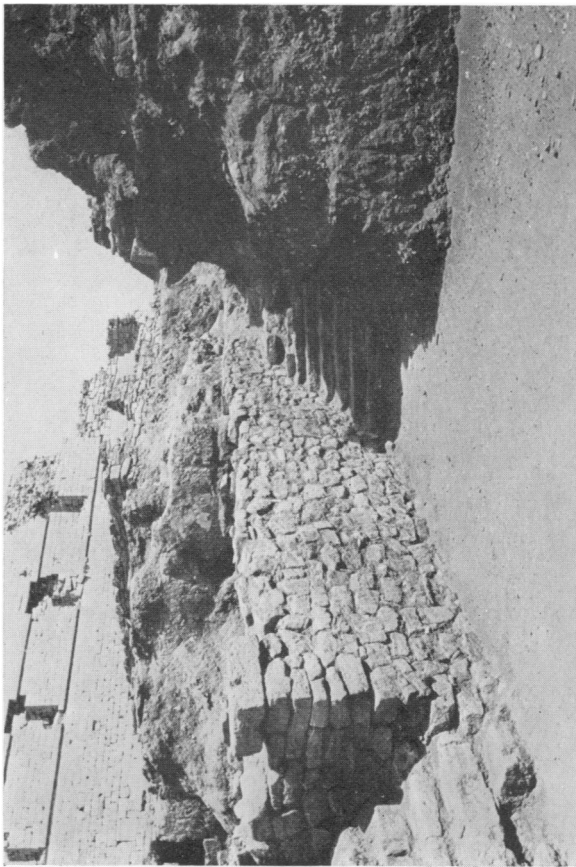
1. Remains of a small church



2. Entrances to the four tombs cut into the *gebel*



3. The north stair



4. The east stair

QAŞR IBRÎM 1966



1. Parchment sheet with part of a homily in Sahidic Coptic which quotes extensively from the Apocalypse



2. Parts of two manuscripts in Sahidic Coptic: on the left, parchment with Matthew xii. 32 ff.; on the right, papyrus with part of a homily on Saint Kyrikos

Sahidic Coptic. All that remains of a fourth text are two words. These texts which have been moved by the Antiquities Service are passages from the four gospels. Unfortunately they contain nothing which would give an exact date both for their writing and for the construction of the tomb. It is probable, however, that all these burnt-brick tombs date to the eleventh or twelfth century A.D.

Two metres to the south of the inscribed tomb lay a mud-brick tomb whose entrance had been sealed by a stone portcullis. This must be regarded as a most unusual construction for the period, but it did not prevent the entering of the tomb in antiquity and the disturbing of the two bodies within.

From the tombs it was possible to remove a useful collection of fabrics, footwear, etc. In one tomb a very fine carved wooden pectoral in the form of an angel was found on the body of its owner (pl. IV, 4).

Dr. MacDermot was able to study, record, and photograph all the human remains. The results of her work are likely to prove an extremely interesting study in comparative anatomy.

The *gebel*, upon which the small church and some of the burnt-brick tombs were built, terminated abruptly along a line running in a southerly direction from the west tower of the Great Church. Here the *gebel* had been cut to form a face descending perpendicularly for about 4 m. before meeting the level of the Podium area. It seems probable that this cutting of the face of the *gebel* was carried out during the Meroitic period, and that it may have formed part of a temple complex. At a much later period, probably during the tenth and eleventh centuries A.D. four large tombs were cut into the *gebel* (pl. V, 2). All had been entered and robbed, but the second tomb south of the Great Church yielded up five complete funerary stelae in Greek and a great quantity of manuscripts. One stela is illustrated on plate IV, 5.

The stelae which can be dated to the eleventh and twelfth centuries A.D. are those of bishops of Ibrīm and Korte, and some still preserve traces of their original colouring. A sixth larger and very important stela was found lying face upwards about 2 m. below the edge of the *gebel*. This is the stela of Marianos, Bishop of Faras, who died in A.D. 1037 (pl. IV, 5).

The manuscripts which lay on the floor of the tomb, and separated from the stelae above them by a few centimetres of debris, may well be the remains of one period of the library and archives of Ibrīm. Written for the most part on paper, there are examples of Greek, Sahidic Coptic, Old Nubian, and Arabic. Some of the fragments, especially those on parchment, are clearly ecclesiastical works, others are letters (pl. VI, 1).

The removal of the Late Nubian Christian mud-brick complex revealed that below lay remains of a period of short but intermittent occupation. Beneath these remains were the foundations of the strongly built walls of houses possibly dating to the Late Christian Period I. Everywhere there were signs of intense destruction by fire. This level was comparatively poor in remains, even potsherds being few in number. Beneath the most westerly of these walls, under an accumulation of debris and remains of burning, lay a large number of fragments of Coptic manuscripts, the greater part on

papyrus, together with the remains of leather book covers (pl. VI, 2). The fact that these manuscripts were written upon papyrus and were all in Coptic may indicate that they came from an earlier collection than that represented by the manuscripts from the rock-cut tomb referred to above. That their destruction may also have occurred at an earlier period remains to be determined.

Further excavation in this area revealed that the northern end of the Podium terminated in two flights of stone stairs (pl. V, 3 and 4). Both flights were well constructed possibly having been built in the seventh or eighth century A.D. At the intersection of these two stairways were found the two halves of a monumental lion of Meroitic workmanship, complete except for its front legs (pl. IV, 3). This is an important, and indeed, unique find. Since the monument bears two lines of Meroitic hieroglyphs giving the name of a king, Amani-Yeshbêhe, a tentative dating between A.D. 286 and 306 can be assumed. If this lion belongs properly to this site, the occupation of the fortress to a much later date than has been thought hitherto will have to be taken into account in future studies of Meroitic history.

One hundred and fifty-three objects and groups of objects were entered in the register. In addition a number of interesting individual finds, collections of fabrics, footwear, selected potsherds, etc., were made to illustrate the various periods excavated during this season.

It was not possible after the work was completed to arrange a division of the season's finds, but it is hoped that an early opportunity will be forthcoming for the division to be made.

The finds of the 1966 season have demonstrated once again the importance of this fortress site. While it is desirable that further excavations should be carried out at Qaşr Ibrîm, it must be recognized that all future work in Nubia will not only become increasingly difficult but also more expensive. Nevertheless, it is hoped that it will be possible for the Society to return to Qaşr Ibrîm which has proved consistently to be one of the richest sites in the Nubian section of the Nile Valley.

ABYDOS AND THE ROYAL TOMBS OF THE FIRST DYNASTY

By BARRY J. KEMP

AMONGST the most striking remains of the earliest dynasties at Abydos are those groups of graves called by their excavator the Tombs of the Courtiers.¹ They lie on the low gravel plateau behind the site of the early town of Abydos and the temple of Khentimentiu, and consist of three groups of small brick-lined graves, for the most part constructed in continuous trenches, and so arranged as to enclose large rectangular areas, the largest of which measures some 75 metres by 123 metres, and contains about 280 graves. On the analogy of similar burials, subsidiary to large mastabas, in particular around Saqqâra tomb 3504,² each of these small graves would originally have been marked on the surface by a low rectangular brick superstructure with a slightly vaulted roof, and perhaps with one or two small niches. From the contents of these graves, two of the groups were assigned to the reigns of Djer³ and Djet,⁴ and the third—largely on the basis of a pot-mark—to the period or reign of Merneith.⁵ The function of these rows of small graves has long been something of a puzzle for, while they seem very similar to the subsidiary burials which commonly surrounded royal and other large tombs of the First Dynasty, a search by Petrie in the area inside these rows failed to show any comparable structure to which these graves could have been subsidiary. There has, however, been no lack of explanations. According to Reisner,⁶ following a suggestion by Petrie,⁷ each group originally surrounded an enormous brick mastaba of the 'palace-façade' type similar to the royal tomb at Naqâda and without substructure. They would have served as Valley Temples to the royal tombs on the Umm el-Qaâb. This theory was subsequently dismissed as being incompatible with what is known of later Valley Temples.⁸ Helck later concluded that inside each rectangle was constructed an imitation palace built probably of matting and intended to house a statue of the deceased king, and saw this as a development of burial customs attributed to the earlier inhabitants of Upper Egypt of supposed nomadic origin. In the Second Dynasty brick replaced matting, resulting in the 'forts'. These conclusions

¹ Published by Petrie in *Tombs of the Courtiers and Oxyrhynchos*. I wish to record my thanks to Professor J. M. Plumley for kindly reading the manuscript.

² W. B. Emery, *Great Tombs of the First Dynasty*, II, 12, fig. 5, 13, pls. i-iii, viii, ixa, xv; cf. Abû Rawâsh tomb M VII, Montet in *Kêmi* 7 (1938), 42-43, pls. v, xi, 1.

³ Petrie, *op. cit.* I, and the tomb registers on pls. xx-xxi. One grave of this group had probably been discovered in 1904 during the excavation of the 'Middle Fort', and labelled μ 22. Its position slightly to the north-west of the east gate would place it probably in the south-west row of graves, cf. Ayrton, *et. al.*, *Abydos*, III, 46.

⁴ Petrie, *op. cit.* I, and pl. xxi.

⁵ Petrie, *op. cit.* I, and pls. xxi, xxviii.

⁶ G. A. Reisner, *Tomb Development*, 10-11, 246.

⁷ Petrie, *op. cit.* 3.

⁸ H. Ricke, *Bemerkungen*, I, 130, n. 164; Emery, *Great Tombs*, II, 2; J.-Ph. Lauer, *Histoire monumentale des pyramides d'Égypte*, I, 60.

were reached by an analysis of the later Osiris mysteries at Abydos, and by the assumption that the chief features of the mysteries originally belonged to a royal funerary ritual of the archaic period.¹ More recently a connexion with the *sed*-festival has been suggested,² while Lauer has put forward the idea that each of these groups of graves originally surrounded a series of imitation magazines or granaries similar to those found at Saqqâra on the north of Tomb 3357 of the reign of Aḥa of the First Dynasty.³ In all of these discussions, however, one significant piece of evidence has been overlooked.

While the complete clearance and recording of these graves was carried out in 1922 by Petrie, their existence had first been made known by Peet who excavated some of them in the course of clearing his cemetery S in the season of 1911 to 1912.⁴ One of the most significant of Peet's discoveries was a length of brick wall running inside and parallel to the north-west row of graves of the group later dated by Petrie to the time of Merneith. It was 180 cm. broad, rose from a low step 45 cm. broad, and displayed on its outer face a series of niches 35 cm. broad and 15 cm. deep (see pl. VII).⁵ The ground on either side of this wall was covered by mud paving. When Petrie excavated the same area ten years later, this wall seems to have been too denuded to enable its original form to be made out, but a further section of wall was discovered running inside and parallel to the north-east row of graves, though again presumably too denuded to preserve any details of the original finish of the outer surface.⁶ Petrie further discovered immediately to the south-west of this group of graves a large brick enclosure, named by him the 'Western Mastaba', which also had this simple panelling on the north-west as well as the south-west and south-east sides. However, on the north-east side, facing the cultivation, there was a more complex pattern with a second, deeper niche inserted between every fourth panel.⁷ This 'Western Mastaba', by its proximity and identical orientation, provides an obvious model from which the fragments of wall found by Peet and Petrie inside the Merneith group can be reconstructed. The resulting enclosure, whose sides are defined by the rows of graves, is then found to be similar in size also to its neighbour. In the case of the 'Western Mastaba' it seems that there was a deep and wide recess at the north-east end of the north-west wall reminiscent of the false doorways, set in recesses, around the sides of the large archaic tombs at Saqqâra and elsewhere. But whether this had existed in a similar position in the wall of the Merneith enclosure has unfortunately been obscured by later tombs which have been cut into the wall at this point. However, the squared end further to the south-west suggests that a true entrance may have existed originally in the west corner (see pl. VII).

A reconstruction of the Merneith group as a large rectangular brick enclosure with panelled walls containing probably at least one true entrance and surrounded on three sides by a row of small tombs, each, on the parallel offered by Tomb 3504 at Saqqâra,

¹ H. W. Helck, *Archiv Orientalni*, 20 (1952), 79 ff., also in Paulys, *Realencyclopädie*, 23, 2 (1959), 2172.

² Cf. *Chron. d'Ég.* 38, no. 76 (July 1963), 251.

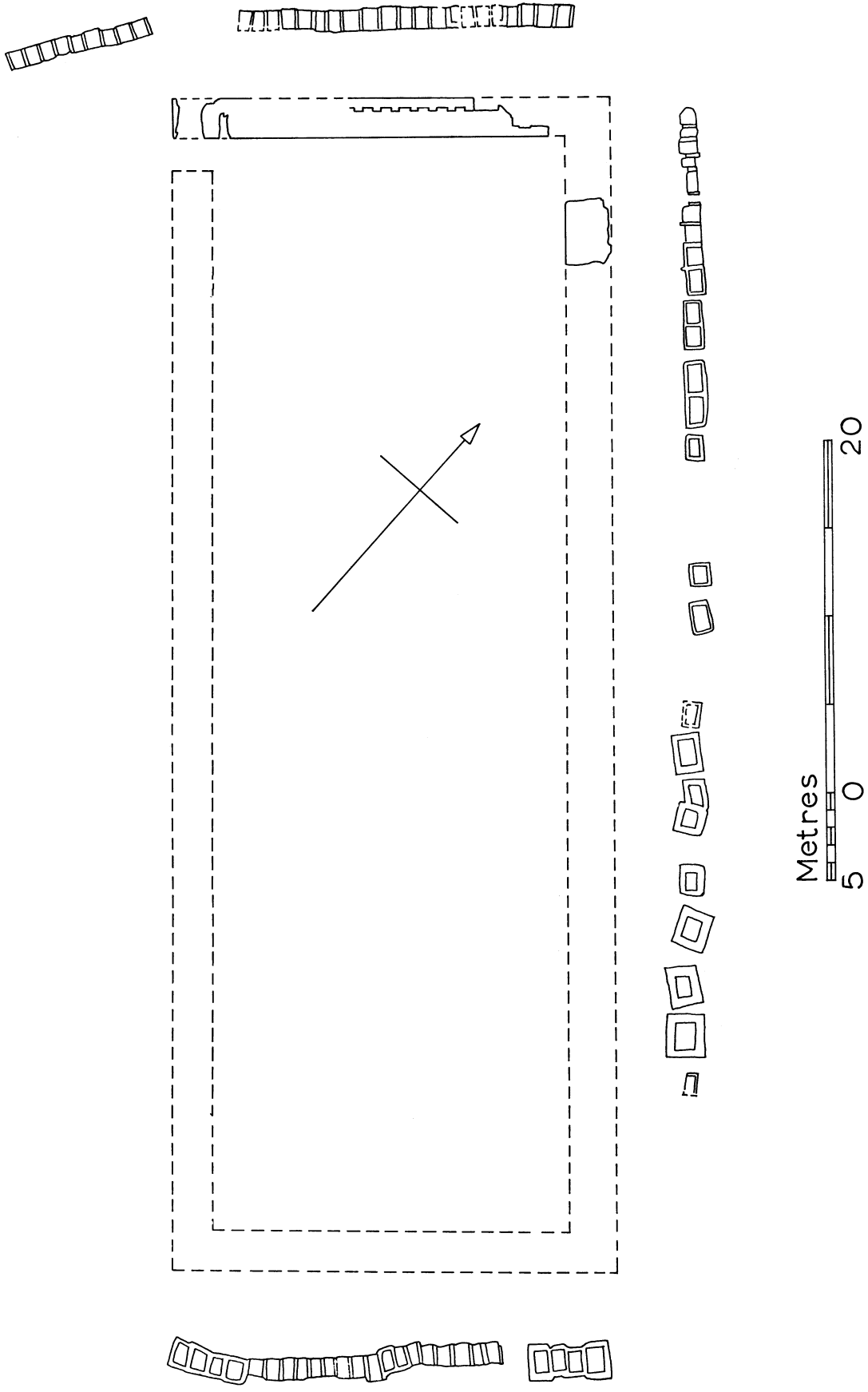
³ J.-Ph. Lauer, *MDAIK* 15 (1957), 163; id., *Histoire monumentale des pyramides d'Égypte*, I, 60.

⁴ T. E. Peet, *The Cemeteries of Abydos*, II, 30-34.

⁵ *Ibid.* 30, fig. 8, 31; pl. vi, 1 and 7; *Archaeological Report* (1911-12), 7-8, pls. v, 4; vi, 6.

⁶ Petrie, *Tombs of the Courtiers*, pl. xviii.

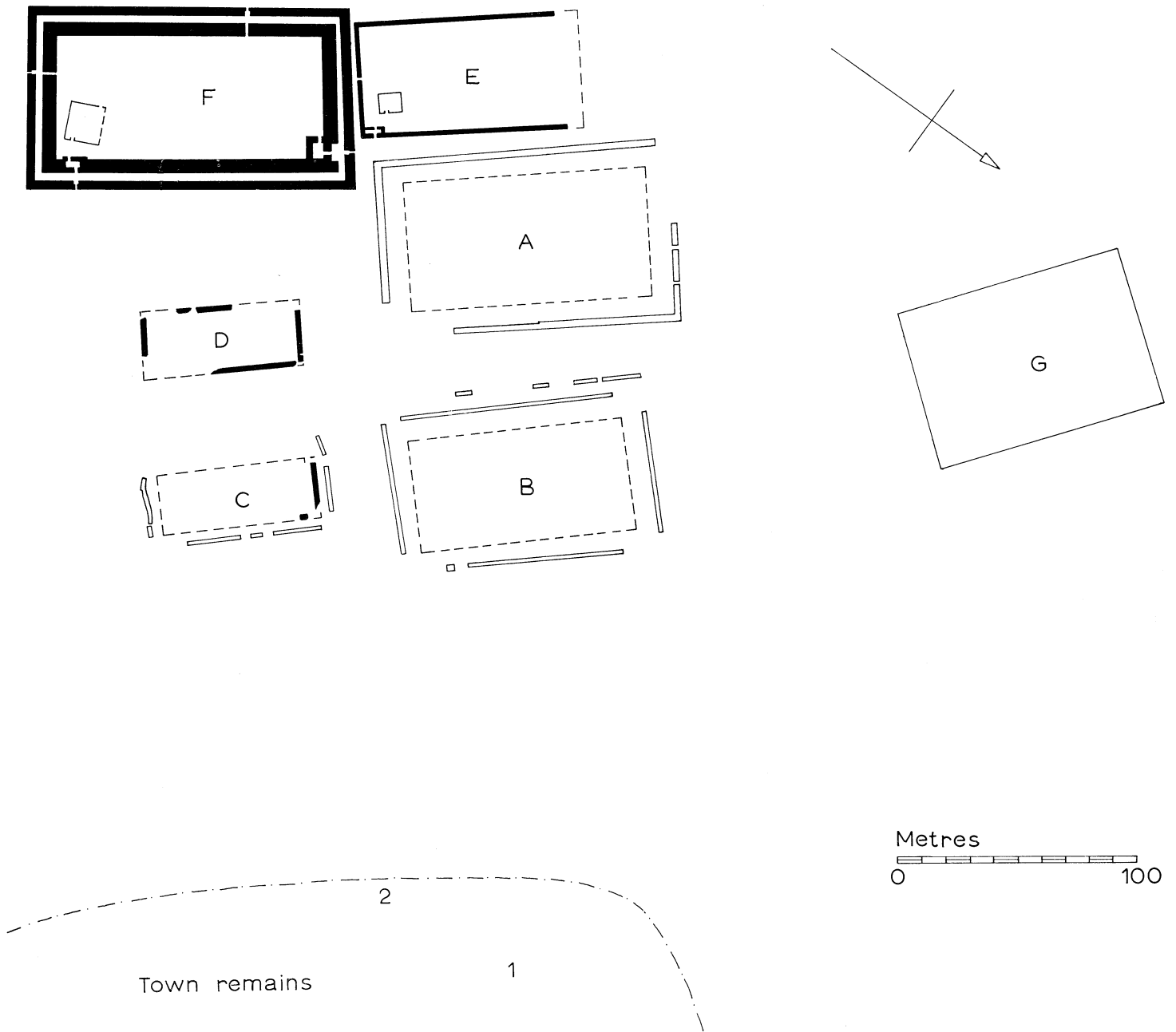
⁷ *Ibid.*, pl. xviii.



FUNERARY PALACE OF MERNEITH AT ABYDOS

(After Peet, *Cemeteries of Abydos*, II, 30, fig. 8, and Petrie, *Tombs of the Courtiers*, pl. 18)

PLATE VIII



PLAN OF THE FUNERARY PALACES BEHIND THE ARCAIHC TOWN AT ABYDOS

(After Ayrton, et al, *Abydos*, III, pl. 8, and Petrie, *Tombs of the Courtiers*, pl. 15)

- | | |
|-------------------------------|---------------------|
| A Funerary Palace of Djer | D 'Western Mastaba' |
| B Funerary Palace of Djet | E 'Middle Fort' |
| C Funerary Palace of Merneith | F Shûnet el-Zebîb |
| G Coptic Deir | |

Note: The limits of the town-remains are approximately fixed by:

1. Old Kingdom town debris found beneath the Kôm el-Sultân, cf. Petrie, *Abydos*, II, 7.
2. Second-dynasty sealings found in Old Kingdom town debris beneath portal of Ramesses II, cf. Petrie, *Abydos*, I, 24.

surmounted by a small, slightly vaulted superstructure, seems to be the one which most satisfactorily fits the evidence obtained in excavations.

If the original function of one of these groups of graves was to surround a large, panelled brick enclosure, it is only logical to extend this explanation to the two other groups of graves of the reign of Djer and Djet, since from the symmetry of the whole layout, it is clear that all three must be parts of the same series (see pl. VIII). That no trace of a brick enclosure has been found within these two other groups of graves need cause no surprise. In the first place, denudation has clearly been intense in this area and may well have been accelerated by deliberate destruction. Indeed, in the case of the Merneith wall, by the Twelfth Dynasty only a few courses of bricks, partly buried beneath a layer of sand, remained, on which small mastabas were constructed.¹ Furthermore, since Petrie was expecting a tomb in the centre, most of his test pits were concentrated there and large areas around the edges do not seem to have been so closely examined. It would seem, therefore, that some at least of the kings of the First Dynasty built on the Umm el-Qa'âb at Abydos tombs with small and rather rudimentary superstructures surrounded by subsidiary graves, and on the low plateau behind the town and temple of Abydos imposing panelled brick enclosures, likewise surrounded by subsidiary graves. Since the 'Western Mastaba', however, was without subsidiary graves, it suggests that it might date to the latter part of the First Dynasty when subsidiary burials were declining in number on the Umm el-Qa'âb, and had died out altogether at Saqqâra.² It may also be that the enclosure at present occupied by the Coptic village represents a further example. From the plan (pl. VIII) it is also clear that there would be ample room for other such enclosures in areas of which few have been excavated since the days of Mariette, and some not excavated at all, since the ground is occupied by a modern Coptic cemetery. As an indication of the possible extent of First Dynasty remains in the area it should be noted that in 1909 Garstang excavated, on a line north-west from the Shûnet el-Zebîb, a very large and deep tomb containing pottery of the First Dynasty;³ whilst to the north-west of the Coptic Deir, Amélineau excavated a cemetery in which the tombs consisted of oblong chambers in which the body was laid out directly on the ground without a coffin. He likened the objects found to those from the Umm el-Qa'âb, but noted that the bodies were in a different position.⁴ It is uncertain if this was a First-dynasty cemetery, but it was possibly such.

Returning again to the 'Western Mastaba', this building has the appearance of a smaller scale version of the neighbouring 'forts', the Shûnet el-Zebîb and the 'Middle Fort', both dated by mud seal impressions respectively to the reigns of Khasekhemwy

¹ T. E. Peet, *op. cit.* 31. The small panelled brick 'shrine' discovered by Petrie, situated partly over the south-western row of graves of the reign of Djer, *Tombs of the Courtiers*, 3-4, 8, pls. i, 5, xvi, xix, is presumably also a later building, erected when the original superstructures over these graves had been denuded away. Panelled brick walls were still in use at Abydos much later than the First Dynasty, as seen in the case of tombs E30 and E172, cf. J. Garstang, *El Arâbah*, 20, pls. xxx, xxxi.

² Reisner, *Tomb Development*, 10, suggested either the reign of Den or of Narmer for the date of the 'Western Mastaba'. ³ Tomb no. 859 A '09, unpublished except for a brief note in *Liverpool Annals*, 2 (1909), 125.

⁴ É. Amélineau, *Les nouvelles fouilles d'Abydos (1895-1896)*, 32-33, with a misprint of north-east for north-west, as is clear from his map, area 4.

and Peribsen of the Second Dynasty.¹ The similarity between all three is obvious: all had three walls with simple panelling on the outside, while the fourth, facing the cultivation, possessed a rather more complex pattern. It is also interesting to note that the 'false door' in the north corner of the 'Western Mastaba' occupies a position which, in the Shûnet el-Zebîb, was taken by a true entrance. The 'Western Mastaba' thus provides a link between the Second-dynasty 'forts' and the structure inside the rows of graves attributed to the period of Merneith. This latter, in turn, provides a link with the 'Tombs of the Courtiers', so that one can only suppose that all must belong to the same group, built for the same purpose. That this purpose was of a funerary nature seems certain for there is no reason or evidence to suggest that subsidiary burials had any purpose other than to provide service to a deceased person.² In the case of the 'forts' it is in any case unlikely that they were ever intended to be militarily effective or even inhabited. They were partly surrounded by a recent cemetery on to which their main gateways faced, it is difficult to see why one fortress should so quickly have been duplicated by another, and unless the 'Middle Fort' had been dismantled it would have rendered useless the fortifications on the north-west side of the Shûnet el-Zebîb. That it was not dismantled is indicated by the way in which the north-west outer wall of the Shûnet el-Zebîb has been built at an angle to the main wall in order to avoid the 'Middle Fort'. This had the effect of narrowing the passage running around the Shûnet el-Zebîb from *c.* 330 cm. to *c.* 120 cm., a curious feature if this passage was intended to enable troops to move round to man defences on the outer wall. Thus the relevance of these structures to the subject of military architecture must be merely the extent to which the Egyptians might have held in mind a military prototype when erecting them. The fortress-like character of these buildings is, nevertheless, striking, and, in the funerary context provided by the subsidiary burials, leads one to suggest that they served as dwelling-places for the kings' spirits, attended in the First Dynasty by the spirits of those buried around the buildings which themselves were modelled on the contemporary fortified palace. One might offer the term 'funerary palaces', and they would have served, therefore, a purpose similar to that of the normal tomb superstructure. The small brick buildings inside the Shûnet el-Zebîb and the 'Middle Fort' would therefore have been magazines,³ and there is, of course, nothing to guarantee that these were the only buildings originally existing inside. In the case of the Shûnet el-Zebîb Mariette noticed the remains of other walls in the east corner,⁴ and Ayrton later

¹ Cf. now P. Kaplony, *Die Inschriften der ägyptischen Frühzeit*, I, 163-6. The sealings from both 'forts' together totalled over 400, *ibid.* I, p. 163.

² On the custom of servant burials, cf. Reisner, *Tomb Development*, 108, 118; *id.*, *Kerma*, part III, 72-76 (= Harvard African Studies, vol. V, 1923); H. Frankfort, *Kingship and the Gods*, 53-54, chap. 4, n. 20; H. Junker, *Giza*, XII, 6-12; Kaplony, *op. cit.* I, 362-3.

³ P. Kaplony, *op. cit.* I, 166, concludes from a detailed study that the sealings found in the 'forts' indicate magazines with a special purpose. If, as suggested below, these funerary palaces are the counterparts of the 'palace-façade' superstructures of the Saqqâra tombs, they are then directly comparable with the Step Pyramid complex, the enclosure wall of which is clearly but an enlarged and slightly simplified form of the same architectural element. One might then expect these funerary palaces to have contained not only magazines but also buildings of a religious or symbolical nature, prototypes of those which occur inside the Step Pyramid enclosure.

⁴ A. Mariette, *Abydos*, II, 48, pl. 68.

found what seems to have been a foundation trench running north-west from here, parallel to the north-east wall.¹ It would also seem that Ayrton did not completely clear the inside of the building, for only four years later Garstang thought it worthwhile to excavate there, and discovered several jar sealings and numerous flint implements, though unfortunately the written records of this season's work are almost entirely missing.² Moreover, since the areas inside the 'Western Mastaba' and the lines of graves were later subjected to intensive use as a cemetery, similar buildings could easily have been destroyed. It is, however, interesting to note a deposit of First-dynasty pottery found by Peet to the north-west of the south-east line of the Merneith graves, and thus perhaps inside the original brick enclosure.³

For the form of the subsidiary burials, comparison has already been made with the large mastabas of the First Dynasty, and in particular Saqqâra Tomb 3504. The actual superstructure of these large tombs consists basically of a rectangular brick building whose walls were decorated with niches, sometimes simple, more often compound, and at intervals with doorways set at the back of deep recesses similarly decorated. If these door-recesses are absent, as in the case of the west wall of Saqqâra Tomb 3505 and tombs M I, II, VI, VIII at Abû Rawâsh,⁴ the building begins to take on the appearance of one of the funerary palaces at Abydos, and indeed the section of wall associated with the Merneith graves is extremely similar to the west wall of Saqqâra tomb 3505, in both design and dimensions.⁵ It has long been accepted that these tomb superstructures represent imitations of the royal palace, hence the name 'palace façade' often given to this style of architecture.⁶ It is also clear, in view of the small dimensions of the doorways, particularly in the few cases where the original height of the doorway can be determined from traces of the wooden lintel,⁷ that they are only small-scale models of

¹ Ayrton, *Abydos*, III, 4.

² Cf. *Liverpool Annals*, II (1909), 125-6. It is clear that his excavations included more than simply a re-excavation of the small building inside, first excavated by Ayrton.

³ Peet, op. cit. II, 34, pl. vii, 3.

⁴ Emery, *Great Tombs*, III, pls. 2, 15, 17b; Montet, *Kêmi*, 7 (1938), pls. ii, iv, v, viii, ix, 2. In the case of M II, the eastern wall, facing the valley, has received a more elaborate treatment.

⁵ One might compare also the strip of dark red paint along the outer wall of the 'Middle Fort', cf. Ayrton, *Abydos*, III, 3, with the dark-red strip with black edges which was sometimes painted around panelled tomb superstructures, such as Saqqâra tomb 3505, where it occurs at a similar height above ground, cf. Emery, *Great Tombs*, III, 8, pls. 6, 7, 8. However, since a similar feature occurs on the predynastic house-model from El-'Amra, this was perhaps a common way of decorating buildings, cf. E. Baumgartel, *Cultures of Prehistoric Egypt*, II, 132.

⁶ The close connexion between this style of architecture and the *serekh* for the king's Horus name is emphasized by the occasional simplified variant which occurs both in the *serekh* (e.g. W. Stevenson Smith, *Egyptian Sculpture and Painting in the Old Kingdom*, 134, fig. 49 (reign of Djoser); *ZAS* 71 (1935), 112, fig. 1, and pl. ii), and on the enclosure wall around the Step Pyramid complex at Saqqâra and elsewhere, cf. Lauer, *La pyramide à degrés*, II, pl. xxviii, 1, 82-92. The small rectangular depressions occupying the upper part only of this enclosure wall seem to represent the final abbreviation of the more complicated decoration of the upper part of the fully developed 'palace façade' type of wall, rather than the ends of wooden beams sunk into the brickwork which one would have expected, on the parallels afforded by the Nubian fortresses, to have occurred in the lower part of the wall also. For an intermediate stage in this process of abbreviation see M. Murray, *Saqqara Mastabas*, I, pl. xxvi.

⁷ The average width of these door-niches is, at Saqqâra, only about 35 or 40 cm., occasionally reaching 50 cm., as in the case of tomb 3507; the height, measured from the traces of wooden lintel, was 150 or 155 cm., cf. Emery, *Great Tombs*, III, 76, pl. 89; Lauer, *BIFAO* 55 (1955), 160, n. 2; cf. also *MDAIK* 15 (1957), 153, n. 3, pl. xvii, 3. At Abû Rawâsh the door niche of tomb M VII was about 33 cm. wide, and about 77 cm. high, judging from the information in *Kêmi*, 7 (1938), 40, pls. v, xi, 1. In the tomb of Hesy-rê^c at Saqqâra

a much larger original. The excavations of Professor Emery at Saqqâra have also revealed that some at least of these panelled and recessed superstructures were built over and around a much different type of superstructure, basically a tumulus of sand and rubble covered by a skin of brick.¹ In a tomb of the reign of Anedjib the brick skin covering the tumulus had been constructed in the form of small steps, an easier and perhaps more satisfactory method of constructing this feature on a larger scale.² It has even been suggested that here is the origin of the later step pyramids;³ and further links are perhaps provided by some later mastabas which have been given a stepped core, notably mastaba 17 at Maidûm,⁴ and also mastaba K2 at Beit Khallâf.⁵ It has also been suggested that this buried tumulus is, in fact, a copy of the type of simple tomb-superstructure supposedly characteristic of Upper Egypt, and included in the Saqqâra tombs for largely political reasons.⁶ Yet another reason which cannot be discounted is that this tumulus had in time acquired an important magical or religious significance.⁷ Whatever the true reason, and clearly it must have been a potent one to have produced such a curious architectural phenomenon, if a connexion with Upper Egypt be conceded one might expect to find this feature represented in the royal mortuary complexes at Abydos, whether they be actual tombs or cenotaphs emphasizing the king's association with Upper Egypt. The structures which appear most closely to fulfil the requirements are the actual superstructures above the royal tombs on the Umm el-Qa'âb, although unfortunately only in the case of the tomb of Djet have any actual remains of these superstructures been preserved.⁸ Here, around the edge of the pit the width of the door niche was *c.* 30 cm., and the height *c.* 86 cm. cf. Reisner, *Tomb Development*, 270-3. The earliest definitely dated monumental gateways that we possess are those in the Shûnet el-Zebîb and 'Middle Fort' at Abydos, and in the 'fort' at Hierakonpolis, all of the late Second Dynasty, and having widths of, respectively, *c.* 90 cm., *c.* 120 cm., and *c.* 160 cm. In the Step Pyramid complex at Saqqâra the main entrance measures *c.* 110 cm. by *c.* 380 cm., and where the entrance colonnade opens into the main courtyard the doorway measures *c.* 110 cm. by *c.* 260 cm., the heights in all cases being restored. In the latter case the doorway was part of a panelled surface, cf. Lauer, *La pyramide à degrés*, I, 129, fig. 121, and cf. also the doorway at E on, *ibid.* II, pl. xxxviii, elevation, pl. xlii. Another interesting comparison is from Mastaba T at Gîza, for which see n. 3, p. 19 below.

¹ Emery, *Great Tombs*, III, 73, 74, 77, pls. 85, 86, 92, 93; also Lauer, *MDAIK* 15 (1957), 155, pl. xix, 3.

² Emery, *op. cit.* I, 83-91, pls. 21-26, 31-35; cf. I. E. S. Edwards, *The Pyramids of Egypt*, 2nd ed. 24.

³ Notably by Emery, in *Archaic Egypt*, 144-5; tentatively by W. Stevenson Smith, *The Art and Architecture of Ancient Egypt*, 26.

⁴ A. Rowe, in *The Museum Journal* (University of Pennsylvania) 22, No. 1 (March 1931), 20, 22, pls. i, ii; *The Illustrated London News*, 22 March 1930, 461, figs. 1-8, 500; Reisner, *Tomb Development*, 221, 281, fig. 171.

⁵ J. Garstang, *Mahâsna and Bêt Khallâf*, II, pls. xvii, xviii. Was K1 built in a similar fashion, cf. *ibid.* 10, 37, pl. vi (e)?

⁶ Emery, *Great Tombs*, III, 73-74; *id.*, *Archaic Egypt*, 84, 144; cf. also H. Kees, *OLZ* 54 (1959), 568.

⁷ Edwards, *op. cit.* 24, 232-3; *The Early Dynastic Period in Egypt*, 50-51 (= *CAH*, 2nd ed., vol. I, chap. xi, § vii).

⁸ Petrie, *Royal Tombs*, I, 8-9, pls. lxi-lxiv; cf. also Amélineau, *Les nouvelles fouilles d'Abydos (1895-1896)*, 129-31, fig. 12. For attempts at reconstruction see Rieke, *Bemerkungen*, II, 14, 16, 17, figs. 1 and 2; Lauer, *BIFAO* 55 (1955), 159-60, and pl. iv, A; *MDAIK* 15 (1957), 156-7; *id.*, *Histoire monumentale des pyramides d'Égypte*, I, 44-49; Smith, *op. cit.* 24-25, fig. 7 (A).

This low wall around the tomb of Djet was clearly at ground level, and the depth of the burial chamber below ground level about 2.20 m., not over 5 m. as in the reconstructed sections offered by Reisner, *Tomb Development*, 326, fig. 173 and Lauer, *Histoire monumentale*, I, 46, fig. 14. If the plastered top of the wall around the burial chamber of Den, 8 in. above the plastered pavement surrounding the tomb, is original, and belongs not to the restorations carried out at a much later date, then the edge of the superstructure would have been a low step, 8 in. high, cf. Petrie, *Royal Tombs*, II, 10.

containing the burial chamber, ran a low brick enclosure wall, rather roughly finished, to retain a mound of sand and gravel which was itself presumably covered with a skin of brick to prevent rapid denudation, and to allow rainwater to run off easily.¹ It is important to notice that although all four sides of this wall seem to have been preserved fairly intact, there was no trace of any niche or other embellishment. The superstructures above the other tombs, although now totally destroyed, were presumably of a similar nature, though Emery has suggested that developments at Saqqâra, particularly of the stepped form of the tumulus, would also have been reflected at Abydos.² If, then, these brick-encased mounds acting as superstructures over the royal tombs at Abydos offer themselves as likely counterparts to the buried tumuli at Saqqâra it seems no less plausible to suggest that the 'funerary palaces' at Abydos also represent, on a similarly larger scale, the 'palace façade' type of superstructure at Saqqâra and elsewhere which, as has been mentioned previously, represents basically the same style of architectural design, though given more elaborate treatment, particularly in the multiplication of door-recesses. It is perhaps significant, therefore, that in the only example where the doorways attain a reasonably realistic and monumental size, Mastaba T at Gîza of probably the early Third Dynasty,³ the area covered by the superstructure approaches that of the 'funerary palaces', and being still preserved up to a height of 7.50 m., perhaps gives some idea of their original height. The suggestion made here is, therefore, that these 'double tombs' at Saqqâra in fact represent the combination in one building of two elements which at Abydos were constructed separately and on a much larger scale. This is not to say that the Saqqâra tombs are nothing more than miniature copies of what was constructed at Abydos, but simply that in the two places the combination of the two elements thought to be essential to a tomb, namely, a tumulus, and a building imitating a palace or other residence, was conceived in different ways. Perhaps a third⁴ method of combination is seen in the Naqâda royal tomb where the tumulus is represented by the central nucleus, thus actually containing the burial chambers instead of merely covering them.⁵

While the solution adopted at Saqqâra seems straightforward enough, that at Abydos needs some further discussion, and here one of the most important questions to consider is the reason why the kings of the First Dynasty chose the distant Umm el-Qa'âb area as the site for their tombs, or cenotaphs if this they be, when it was the common practice at this period to site large and important tombs on a suitable eminence overlooking the Valley. Ultimately some religious reason must have dictated the choice of such a comparatively remote spot as the site for a cemetery. But even by the First Dynasty tradition must have played an important part since it can be shown that, by this time, the Umm el-Qa'âb was already a cemetery of great antiquity. The

¹ Junker, *Giza*, XII, 31, and n. 1.

² Emery, *Great Tombs*, III, 74; id., *Archaic Egypt*, 130.

³ Width of door-niche: c. 67 cm. (after M. D. Covington, in *Ann. Serv.* 6 (1905), 201, fig. 3), c. 76 cm. (after Petrie, *Gizeh and Rifeh*, 8); height of door-niche 2.35 m.

⁴ For possible non-royal ownership see now Kaplony, *Die Inschriften der ägyptischen Frühzeit*, I, 66-68; II, 730 (286).

⁵ For a reconstruction of the superstructure see Lauer, *MDAIK* 15 (1957), 154-5, pl. xix; id., *Histoire monumentale des pyramides d'Égypte*, I, 17, fig. 8; 49-50.

evidence for this earlier use of the Umm el-Qa'âb comes in the first place from some excavations carried out by Peet on the low ridge which runs approximately northwards from this area.¹ At one end it is occupied by the so-called 'Heqreshu Hill', and at the other by Petrie's cemetery B,² which must have run diagonally across its south-east corner. Amélineau gives its dimensions as 261 m. by 91 m.³ In the course of 'carrying out some tests to discover the nature of the virgin soil in the vicinity of Umm el-Qa'âb'⁴ a small predynastic burial, U1, was discovered presumably within the confines of the area tested systematically, as given in the published plan of operations.⁵ Since the whole of the cemetery lay on this ridge, this particular tomb at least must have lain within the vicinity of Petrie's cemetery B. Altogether 32 graves were excavated, most being intact, but these were merely those remaining over from other excavations which had taken place some years earlier, and were mostly small ones situated amongst the larger ones which had been the chief object of the attention of the earlier excavators. These earlier excavations were, in fact, those of Amélineau, who examined the area during his first campaign in January 1896, and who, in the course of five days, opened up between 150 and 160 brick-lined tombs in this same area.⁶ Most of these tombs were single rectangular pits lined with brick, and of varying sizes, one attaining a dimension of 7.25 m. by 2.60 m.⁷ There were also some multi-chambered tombs. One consisted of no less than six contiguous chambers,⁸ and looked suspiciously like a group of subsidiary burials, as on the Umm el-Qa'âb, though rather larger. In five or six cases contracted burials were reported.⁹ Since Amélineau published so little information on the pottery and stone vases which seem to have been very abundant, if broken, it is difficult to define with any precision the dates of these tombs, particularly since some of the tombs of Petrie's cemetery B were included. Furthermore, since it now appears certain that the Hierakonpolis painted tomb belongs to the middle of the Naqâda II period,¹⁰ the use of brick in tomb construction is no longer a reliable criterion for a post-predynastic date. Furthermore, while the types of tomb which Amélineau illustrates can be paralleled in the First Dynasty,¹¹ one of these tombs contained several pottery jars each decorated with a wavy ridge around the upper part, three of them also with a painted design,¹² one an ostrich, another an animal, perhaps a bull, and the third a tree. While at Abydos the technique of painted decoration seems to have lasted into the First Dynasty,¹³ nevertheless this particular type of pot is completely absent from the not

¹ Peet, *The Cemeteries of Abydos*, II, 14-16.

² Petrie, *Royal Tombs*, II, 4-5, 7-8, pl. lix; Kaplony, *op. cit.* I, 62-64, 206-9; II, 897-900 (1165); W. Kaiser, in *ZÄS* 91 (1964), 96-102.

³ Amélineau, *op. cit.* 75. The area in question is that between his 'première butte' (= 'Heqreshu Hill') and his 'quatrième butte', over the tomb of Djer.

⁴ T. E. Peet, *op. cit.* 14.

⁵ E. Naville, *The Cemeteries of Abydos*, I, pl. xxi.

⁶ Amélineau, *op. cit.* 75-88; cf. also Kaplony, *op. cit.* II, 897 (1165).

⁷ Amélineau, *op. cit.* 77-78.

⁸ *Ibid.* 80.

⁹ *Ibid.* 82-84.

¹⁰ *JEA* 48 (1962), 5-11.

¹¹ E.g. at Helwân. Note that the small compartments built along one wall of a tomb excavated by Amélineau (*op. cit.* 81, fig. 7), occur also in two graves subsidiary to the tomb of Den on the Umm el-Qa'âb, Petrie, *Royal Tombs*, II, pl. lxii, south-east side.

¹² Amélineau, *op. cit.* 193-4, pl. xiii, lower row, nos. 1-3.

¹³ Cf. e.g. L. Habachi in *Ann. Serv.* 39 (1939), 770-3, pls. cxliii, cxlv; Baumgartel, *The Cultures of Prehistoric Egypt*, I, 2nd ed. 76 and n. 1, the pot referred to in this footnote being that from Habachi's excavations, cf. *op. cit.* 770, n. 2.

inconsiderable quantity of pottery from a definite First-dynasty context at Abydos (and at Saqqâra also), so that here at least it may well have been confined to a prior period, and so helps to date one at least of these brick-lined tombs to before the First Dynasty. However, the tombs which Peet excavated, and which apparently lay scattered amongst these brick tombs, help to fill in the picture, and from these it becomes clear that the use of this area as a cemetery began during the Naqâda I period,¹ continued during the Naqâda II period,² and, by further growth to the south and south-west, culminated in, yet simultaneously abruptly terminated with, the royal tombs of the first two dynasties.³ That this cemetery was of great importance before the First Dynasty is implicit in the choice of this place by the early dynastic kings as being suitable for their funerary monuments, and one may, perhaps, recognize here the cemetery of the predynastic ancestors of these kings.⁴

One can imagine, therefore, that the kings of the First Dynasty would have had a very strong incentive to construct their tombs on the ancient traditional burial ground on the Umm el-Qa'âb, even though, its remoteness and increasingly restricted space made it entirely unsuitable for the large and imposing superstructure which was now thought desirable. The expedient was then adopted, as later in the New Kingdom though for different reasons, of separating the superstructure from the actual burial chambers, and of constructing the former in a more prominent and suitable position on the low plateau behind the town and temple of Abydos. The burial chambers, however, continued to be marked by the same simple superstructure which, one assumes, had covered all the earlier tombs and had by now acquired some special significance. At Saqqâra where no factors of this nature were operative, it was possible to erect both superstructure and burial chambers together on the prominent escarpment overlooking Memphis, and to combine the old type of superstructure with the new in a single building, as had been done at Naqâda.

Before concluding, mention must be made of a recent ingenious attempt to solve the difficult problem of the ownership of the tombs in Petrie's cemetery B on the Umm el-Qa'âb, a problem created largely by the haphazard activities of Amélineau's workmen and the fanatical savaging of the site in the sixth century A.D. by the Copts, resulting in the widespread scattering of inscribed objects.⁵ Kaiser has proposed⁶ that the kings prior to Djer were in the habit of constructing tombs composed initially of two entirely separate chambers, as in the case of B1/2 and B7/9, the latter ascribed to Ka.

¹ E.g. U1 of SD 30-36, from pot type B22 *f*.

U11 of SD 34, from pot types P66 and B29 *a*.

U13 of SD 34, from pot type F96 *b*.

U16 of SD 35-36, from pot type B27 *g*.

U20 of SD 35, from pot type P61.

U21 also yielded two specimens of Petrie's 'white cross-lined' ware, types C26 and C44.

² E.g. U10 of SD 40-57, from pot type B75 *a*.

U25 of SD 40-43, from pot types B35 *d*, P34 *c*.

Probably near the First Dynasty: U22 of SD 60-76, from pot types L7 *d*, L40.

³ For the Second Dynasty note also, in addition to the tombs of Peribsen and Khasekhemwy, an apparently plundered cemetery 'out on the ridges beyond Umm el Qa'âb', mentioned by T. E. Peet, *op. cit.*, p. xv.

⁴ Cf. Ricke, *Bemerkungen*, 1, 56.

⁵ Cf. Kaplony, *op. cit.* 1, 62-64, 206-9.

⁶ In *ZÄS* 91 (1964), 96-102.

With Narmer the two chambers were built together, so B17/18. Aḥa reverted to the earlier custom and constructed his chambers separately, though on a much larger scale, and for the first time we find subsidiary burials, though in separate pits. Although Aḥa's tomb seems to consist of no less than three chambers, B10/15/19, this was perhaps not the original plan, and maybe the westernmost chamber, B19, in fact belongs to an ephemeral successor of Aḥa who died prematurely. By ascribing B19 to someone other than Aḥa it enables one to trace the idea of a double tomb on the Umm el-Qa'âb down to the reign of Aḥa, and thereby to establish a link with the practice, begun by his successor Djer, of constructing a second funerary monument on the edge of the cultivation, i.e. the 'Tombs of the Courtiers'. One of the starting points for this theory is the fact that the tombs of the northern part of cemetery B appear to occur in pairs. Since, however, cemetery B was but a part of a much larger cemetery containing at least 150 other brick-lined tombs there can be no guarantee that Petrie's plan records all the tombs in the area, so that if a complete plan could be produced, this grouping into pairs of B1/2 and B7/9 might conceivably appear less obvious. One would have expected, in any case, that if B1 and 2 were parts of the same tomb they would at least have been given the same orientation. It also seems a little strange that the layout should have undergone such a complete change of orientation under Aḥa, running east to west instead of north to south, and the attempts to explain away the third chamber of the Aḥa group as not part of the original plan seem unconvincing, particularly since nothing which can be definitely ascribed to his ephemeral successor has been found at Abydos.

Whatever the true explanation of the factors at work, there is no doubt that the royal funerary monuments of the First Dynasty at Abydos were constructed on a rather larger scale than is usually assumed when making comparisons of size and complexity between the royal tombs at Abydos and those claimed as royal at Saqqâra.¹ From the point of view of sheer size, the monuments at Abydos seem quite appropriate for actual royal burial places, and with their large open courtyards containing small buildings seem a much more obvious prototype the Step Pyramid complex.

¹ E.g. Lauer, *Histoire monumentale des pyramides d'Égypte*, I, 41-44; id., *BIFAO* 55 (1955), 157.

OLD KINGDOM EGYPTIAN ACTIVITY IN NUBIA: SOME RECONSIDERATIONS

By GERALD E. KADISH

EXCAVATIONS undertaken in conjunction with the international rescue campaign in Nubia have yielded valuable new information and focused scholarly attention on that region once again. Perhaps the most notable discovery was that, made by Professor W. B. Emery at Buhen, of an Egyptian copper smeltery which, according to the evidence of sealings and ostraca, was in continuous operation from at least as early as the reign of Chephren through at least that of Nyuserrē.¹ An evaluation of Emery's findings and conclusions must await the full publication of the material, but enough has been made public to warrant a reconsideration of some of the long-known evidence for Old Kingdom Egyptian activity in Nubia. Since no excuse is needed to return to the invaluable Sixth-dynasty autobiographies, I would like to discuss a number of points in the Harkhuf text.²

Scion of a noble family of Elephantine, Harkhuf was entrusted with several missions to Nubia during the course of his official career. Describing the first of these journeys, he says:³

The Majesty of Merenrē, my Lord, sent me, together with my father, the Sole Companion and Lector-Priest, Iri, to Yam in order to *wb* (the) road to this country. I did it within seven months (and) I brought back all the beautiful and exotic products therefrom. I was praised on account of it very greatly.

There are some interesting details here. The presence of Harkhuf's father arouses some curiosity. Of the elder courtier we know only what Harkhuf tells us. His titles are honorific rather than functional, and therefore not very instructive. While he cannot be positively identified with a certain Iri whose name appears in a graffito at Tomâs,⁴ certain points are suggestive.

This last inscription accompanies those of two other Sixth-dynasty officials, Sabi and Teti-ankh. The name of the latter provides only a chronological *terminus post quem* unless (1) he is the same Teti-ankh dated by Baer to the reign of Pepi I or later,⁵ or (2) we assume that he was a contemporary or successor of Sabi—whose graffito is roughly datable—on the grounds that Teti-ankh's inscription follows that of Sabi.⁶ Sabi notes that he was 'honoured before Pepi and before Teti'.⁷ Iri, whose name

¹ Editorial foreword, *JEA* 48, 1-3. These are only the attested limits. The life-span of the installation may in fact have been longer.

² Sethe, *Urk.* 1, 120-31.

³ *Urk.* 1, 124, 9-15.

⁴ Weigall, *Report*, pl. 58, no. 12; cf. Säve-Söderbergh, *Ägypten und Nubien*, 27, n. 5.

⁵ Baer, *Rank and Title*, 152.

⁶ Weigall, *Report*, pl. 58, nos. 9-11.

⁷ *Ibid.*, no. 9. I see no reason to suppose that this refers to Pepi II rather than Pepi I as Goedicke apparently does, *JEA* 46, 62.

follows these two worthies,¹ may very well have been a contemporary of theirs. If so, he could very easily have been Ḥarkhuf's father.

Another matter of interest is a title borne by Sabi, Teti-ankh, and Iri: the Overseer of Mercenaries.² It was also held by Ḥarkhuf,³ Pepi-nakht-ḥeka-ib,⁴ Sabni,⁵ and a number of others engaged in activities in Nubia.⁵ One wonders whether Ḥarkhuf's father might not have actually borne this title even though it is not included in Ḥarkhuf's account.

Why did Iri accompany Ḥarkhuf on the first trip? Surely the son did not include his father's name merely out of filial piety. Despite Ḥarkhuf's use of the first person singular in describing the achievements of that expedition, there is some reason to suspect that Iri was the actual leader of the venture, perhaps making his last journey to Yam in order to familiarize his son and successor with his responsibilities and with the route. In this connexion it is significant that, in the account of the second trip to Yam, Ḥarkhuf feels impelled to note that he went alone, thus emphasizing the difference from the first journey.⁶ If Iri had been the leader of the first venture, the title of Overseer of Mercenaries, borne by the Iri at Tomâs, would have been appropriate. That it does not appear in the Ḥarkhuf text may be explained by the probability that Iri had died by the time Ḥarkhuf had his own tomb decorated. It should be noted that it is the effective title by which Ḥarkhuf is addressed by the young king, Pepi II.⁷

Returning to the account of the first trip, we find that its main purpose was to 'wbꜣ the road to this foreign country (i.e., Yam)'. Our picture of Egyptian activity in Nubia during the Old Kingdom depends, in part, on how one interprets the verb *wbꜣ*, as Ḥarkhuf employs it. There have been numerous translations of this passage and different renderings of the word in question. The word itself is not rare and its fundamental meaning, 'to open, to bore', is quite well known.⁸ It was used to refer to drilling in stone and digging wells and, by extension, to mean 'to open up (in general), to unlock, to make something accessible'.⁹ As a transitive verb *wbꜣ* has been taken to mean 'to open doors, to open up or reconnoitre difficult places or roads, to march along a road or to make waters navigable'.⁹ On the basis of these extended meanings, Ḥarkhuf has been viewed as an explorer in an unknown country.

Weigall's earlier opinion was that Ḥarkhuf had been sent on the first trip 'to discover the best means of opening up communications with that country',¹⁰ understanding the word 'road' metaphorically. Some years later, having reconsidered the matter, he decided that it was to be read literally after all: 'in order to lay out a high road (across the desert) to this country'.¹¹ On the second trip, Ḥarkhuf *wbꜣ*-ed certain Lower Nubian districts on the way home from Yam.¹² Weigall changed his views on this too,

¹ Weigall, *Report*, pl. 58, no. 12.

² Goedicke, *JEA* 46, 60-64, appears to me to have offered a rather convincing argument.

³ *Urk.* I, 120, 14; 121, 10; 123, 9; etc.

⁴ *Urk.* I, 131, 16 and 17; 132, 5 and 9; etc.

⁵ Goedicke, *JEA* 46, 62.

⁶ *Urk.* I, 124, 17.

⁷ *Urk.* I, 128, 4. In addition to the *smr-wꜣty* and *hry-ḥb(t)* titles.

⁸ *Wb.* I, 290; Faulkner, *Concise Dictionary*, 58; Gardiner, *Eg. Grammar*³, 519, n. 2.

⁹ *Wb.* I, 290.

¹⁰ Weigall, *Report*, 6.

¹¹ *Id.*, *History of the Pharaohs*, I, 235-6.

¹² *Urk.* I, 125, 8-9.

from the definitive 'explore'¹ to the vague 'open up'.² The line-up of the scholarly supporters of the exploration-interpretation for *wbꜣ* is impressive: Reisner, Breasted, Drioton, Gardiner, Edel, and others.³ Yoyotte,⁴ however, sees a distinction between the use of *wbꜣ* in the account of the first trip and the way it is used to describe the events of the second. Using the occurrence of the phrase 'to *wbꜣ* a road' in the account of the Ḥatshepsut Punt expedition, Yoyotte, agreeing in effect with Weigall's earlier opinion holds that the first Ḥarkhuf trip aimed at opening up communications with Yam. On the other hand, the countries traversed on the return leg of the second journey were explored (*explorer*). Presumably the knowledge that Ḥatshepsut's men were not exploring Punt led Yoyotte to this view.

As for the countries (or places) traversed on the return from the second trip to Yam, there seems to be no disagreement that the countries named (*Irtt*, *Stꜣw*, etc.) are all to be located in Lower Nubia, between the First and Second Cataracts.⁵ Emery's new evidence makes it certain that Lower Nubia was well known to the Egyptians as early as the Fourth Dynasty. It is quite unlikely that Ḥarkhuf is trying to tell us that he explored those places. Support for this view is gained from another of the Tomâs inscriptions, one belonging to an official named *Hw-nis*, who says: 'I was sent to *wbꜣ* Irtjet for the King of Upper and Lower Egypt, Nefersaḥor.'⁶ A further inscription at Tomâs reads: 'I was sent to *wbꜣ* the southern foreign countries for the Horus, Mery-Tawy, Nefersaḥor.'⁷ Both these inscriptions belong to the early part of the reign of Pepi I, before he changed his praenomen to Mery-Rēꜥ. Nothing is known of the owner of the second inscription, but the owner of the first may be the man of the same name who was buried at Elephantine, and who is mentioned in Elephantine Papyrus 10523.⁸ Both bore the title Overseer of Mercenaries; both were sent on missions to Lower Nubia.⁹ Since both antedate Ḥarkhuf's claim to have *wbꜣ*-ed Irtjet, the latter cannot be considered an explorer. Lower Nubia was a land already well travelled.

In regard to the first trip, it can also be established that Ḥarkhuf was not the first to go to Yam or 'to open up communications with that land'. Leaving aside the question of the localization of Yam, the matter can be settled from the internal evidence of Ḥarkhuf's own words. In summarizing his second journey to Yam, he says: 'Never had it been found that it had been done by any Companion and Overseer of Mercenaries who went forth to Yam before.'¹⁰ One may quarrel over the grammar in the first part of the sentence and how it might best be rendered into a modern language, but the second half is quite clear: there were other Overseers of the Mercenaries who

¹ Weigall, *Report*, p. 6.

² Id., *History*, I, 236.

³ Reisner, *Sudan Notes and Records*, I, no. 1, 10-11; Breasted, *AR*, I, § 312 n; Drioton, *L'Égypte pharaonique*, 76; Edel, *Ägyptologische Studien* (ed. Firchow, 1955), 71; Erman, *ZÄS* 30, 79-80, translated *öffnen*, but was not certain.

⁴ Yoyotte, *BIFAO* 52, 173-4.

⁵ Gardiner, *Egypt of the Pharaohs*, 101; Edel, *Äg. Stud.* 70; Dixon, *JEA* 44, 43 ff.; Yoyotte, *BIFAO* 52, 176; Kees, *Ancient Egypt*, 312.

⁶ Weigall, *Report*, pl. 58, no. 29; *Urk.* I, 208.

⁷ Weigall, *Report*, pl. 58, no. 30; *Urk.* I, 209.

⁸ Säve-Söderbergh. *Ägypten und Nubien*, 12, n. 8; 13, n. 7.

⁹ One merely refers to the southern foreign countries. The location of the inscription in Lower Nubia suggests, but does not prove, that that is the region meant—at least in part. The other Tomâs inscription mentions Irtjet.

¹⁰ *Urk.* I, 125, 10-11.

went to Yam before Ḥarkhuf. Likewise at the end of the account of his third trip, he reports: 'I accomplished more than any Companion and Overseer of Mercenaries who was sent to Yam before.'¹ This time there is no grammatical debate; the statement is quite clear. Yet a third example exists. In the famous letter from Pepi II to Ḥarkhuf, the king is apparently paraphrasing a dispatch he had received from Ḥarkhuf reporting that the latter was bringing back a pygmy the like of which had not been brought by anyone who had visited Yam before.²

To refute this argument, one would have to take the view that Ḥarkhuf was merely boasting in the grand Egyptian manner. It was common for Egyptian nobles to assert that their efforts were so remarkable as to be unparalleled by those of any other servant of the king. Similarly their achievements received praise and rewards surpassing those bestowed on their predecessors or contemporaries.³ There is no doubt that Ḥarkhuf is bragging, but wherein lies the boast? When he says that no one had accomplished what he had, he may very well be exaggerating. But Ḥarkhuf was no fool, nor was he ignorant of the past, almost legendary accomplishments of important men like Ba-wer-Djed (an emissary of the Fifth-dynasty king Djedkare(-Isesi) who was reputed to have brought back a pygmy from Punt,⁴ just as Ḥarkhuf brought one from Yam. In order to lend substance to his boasts, he makes the comparison between what he has done and those things which had been done by men who went to Yam before. If Ḥarkhuf had been the first Egyptian to go to Yam, we may be sure he would have used half the available wall space to record the fact. There is no reason not to take this statement at face value, given the evidence. If, then, the notion of exploration is to be rejected, how are we to understand the word *wb* as Ḥarkhuf uses it and as it is employed in the two Tomâs graffiti mentioned earlier?

In the Old Kingdom texts, the verb *wb* is not commonly employed in its extended meanings. Consequently one has little with which to compare the usage in Ḥarkhuf. Nevertheless, there is a passage which, although difficult, may prove instructive. It occurs in the autobiography of Weni: 'I *wb*-ed the number of these troops; never had (it) been *wb*-ed by any servant.'⁵ This statement follows Weni's account of his efficient command of the army which was summed up by a line like 'His Majesty praised me for it more than anything'.⁶

Erman speculated that 'an unsere Stelle muss es eine Amtshandlung bezeichnen, die allein dem Könige zusteht, vielleicht die Musterung der Truppen'.⁷ Tresson, agreeing with Erman, translated: 'passer à travers le nombre de, c'est à dire, dénombrer, recenser, passer en revue'.⁸ Breasted used the word 'inspect' implying a formal review of the troops.⁹ The Berlin dictionary's definition 'to march along, to go along' can readily be extended to include the notion of inspection.¹⁰ Centering his interpretation around

¹ *Urk.* I, 127, 10-11.

³ *Ibid.* 83, 17-84, 1; 100, 1; 101, 4-7; 146, 15-16—just a few examples.

⁴ *Ibid.* 128, 14; 129, 1.

⁵ *Ibid.* 103, 4-5. A provisional translation for the sake of discussion.

⁶ *Ibid.* 103, 3, is almost entirely missing, but Sethe saw *ht nb(·t)* at the end of the line. Cf. *Urk.* I, 104, 4.

⁷ Erman, *ZAS* 20, 16.

⁸ Tresson, *L'Inscription d'Ouni* (Bibliothèque d'Étude) VIII, 4, n. 5.

⁹ Breasted, *AR* I, § 312.

² *Ibid.* 129, 2-4.

¹⁰ *Wb.* I, 290.

the *Wörterbuch*'s definition of *wb*: 'to open up' in the sense of 'divulge',¹ Gardiner rendered the Weni passage: 'There was revealed to me the number of these troops though it had never (before) been revealed to any servant.'² The chief problem is whether it makes any sense. Granted that Weni is boasting about some preferment and responsibility that was unusual, what would be the point of the sentence as Gardiner has it? One would have to envisage the king saying to Weni: 'I am putting you in command of this army. It consists of 1,254 soldiers; only you know the number of troops.' Clearly there is nothing unusual about a general knowing the size of the force he is commanding. It should be noted that Weni has boasted that he had all this authority even though his rank was not as high as was usual in such cases.³ Any military commander who feels it necessary to record as one of the significant events of his career the fact that he was informed of the number of troops under his command, must surely be something of an oddity—now or in ancient Egypt. Gardiner's translation appears to me unsatisfactory.

Professor J. A. Wilson has offered an alternate solution.⁴ Keeping to the basic meaning 'to open up', he rendered the passage: 'All these troops opened up (the way) for me, though (it) had not been opened for any servant (before).' Again it is difficult to see what Weni would be trying to say. If he is telling us that the soldiers cleared the road for him and that this had never been done for any servant before, the passage would appear to be just another boast. But it is a rather unusual statement and should be taken seriously. It is interesting that this sentence follows Weni's remarks about all that he did with regard to regulating the affairs of the army.⁵ There are also some grammatical problems.

To convey his understanding of the passage, Wilson has to posit an object for *wb*: 'the way'. We have seen that when this particular object was intended, it was specifically stated. The translations of Gardiner and Wilson at least avoid the difficulty involved in the *t* following the second *wb*: in the Weni passage. There is every reason to suppose that we have an example of the very normal form *n-sp* followed by *sdm.t(w)*.⁶ The question that remains concerns itself with the *n* that follows. The parallel examples given by Edel in the sections cited all take the *n* to be dative, as do the Wilson and the Gardiner translations. At first glance, there seems no reason not to view it as a dative. It should be noted, however, that the instances given or referred to by Edel⁷ are of the formula *n-sp ir.t n. . .* The verb is always *iri*.

A solution involving perhaps less difficulty *and* seeming to make sense is the following:

¹ *Wb.* I, 291, 7.

² Gardiner, *Egypt of the Pharaohs*, 96; cf. Fischer, *JNES* 16, 261, 267.

³ *Urk.* I, 102, 8-103, 2. Cf. Faulkner, *JEA* 39, 32-35. Weni's use of 'never' may constitute an exaggeration.

⁴ Wilson, *JNES* 20, 69.

⁵ One possibility should not be overlooked. It may be, taking Wilson's proposed translation, that Weni means that the troops opened up a path to honour him. While I cannot lay this interpretation completely to rest, I would suspect that mention of the honours paid to him by the troops, a unique occurrence, would follow the victory poem and that he might put it another way.

⁶ Edel, *Altäg. Gramm.* II, §§ 1081, 1086c, 1094.

⁷ *Ibid.*, §§ 1086c and 1094.

wbꜣ·n(·i) tꜣw tꜣ·wt pꜣn
n-sp wbꜣ·t(w) (i)n bꜣk nb

While it is not common, the writing of *n* for *in* is known from Old Kingdom texts.¹ Substituting Breasted's notion of 'inspection' for *wbꜣ*, I would offer the following translation: 'I inspected each of these detachments; never had it been inspected by any servant before.' It should be understood, from the line that precedes this one in the text, that Weni means by any official of his rank. He does not mean that generals were unaccustomed to inspect their troops before battle. Erman and Breasted correctly understood the reason for Weni's pride. And that was something to include in his autobiography.

By 'inspect' I do not mean to imply anything so rigorous and formal as that to which modern soldiers are almost too often subjected. Rather it would seem to be a kind of general review of the troops by means of which a field commander checks the preparedness of his forces and perhaps bolsters their *esprit de corps*.

The next step is to reverse the process followed by Wilson² and apply the results of the examination of the Weni text to the troublesome passage in Harkhuf. For the first trip, we would obtain:

The Majesty of Mer-en-rē, my Lord, sent me, together with my father, the Sole Companion and Lector-Priest Iri, to Yam in order to make the rounds along the road to this foreign country.

I use the expression 'make the rounds' rather than 'inspect' to avoid a too rigid notion of the latter term. In the same fashion, the account of the second trip would be rendered as:

I returned from the neighbourhood of the house of the ruler of Setjau and Irtjet (after) I had made the rounds of these foreign countries.

In the light of these translations, it may be possible to get a clearer picture of Harkhuf's mission in Nubia.

It was a twofold assignment, the two aspects closely intertwined. Egypt required and intended to have certain products which could be obtained only in the distant south.³ In order to make sure that there was no interruption in the flow of such goods to Egypt, the road to the trading place—presumably Yam was such a place—at which they could be procured had to be kept open. Harkhuf was sent—as undoubtedly were others—to get these southern products. This was clearly the primary purpose of his journeys. At the same time, he seems also to have had the responsibility of keeping the trade-route open. To do this he had troops with him and the authority to use them when and if necessary. That troops had been sent with Harkhuf is stated explicitly.⁴ The nature of Harkhuf's discretionary authority has to be extracted from the text.

On the third trip to Yam, Harkhuf encountered somewhat different circumstances: 'I found that the ruler of Yam had gone off to the land of the Tjemeh in order to

¹ Edel, *Altäg. Gramm.* II, § 756. It may be that the Tomàs inscriptions, *Urk.* I, 208–9, contain examples of *n* for *in* after *wbꜣ*, although the dative easily makes sense. If dative, the object in both cases is the king.

² Loc. cit.

³ See, for example, *Urk.* I, 126, 17–127, 3. Cf. Säve-Söderbergh, *Ägypten und Nubien*, 17.

⁴ *Urk.* I, 127, 6. The context suggests that it was a military force.

smite the Tjemeh as far as the western corner of heaven.¹ The ruler of Yam had gone to do battle with the Tjemeh, Libyans inhabiting the western deserts.² Harkhuf reports what he did: 'I went forth after him to the land of Tjemeh.'³ The two lines that follow in the text have engendered a good deal of controversy:

sh̄tp·n(i) sw
r wn·f hr dwꜣ ntr·w nb(w) n Ity

'I *sh̄tp*-ed him until he praised all the gods for the sake of the Sovereign.' With these lines the inscription to the right of the entrance to Harkhuf's tomb ends. The narrative resumes to the left of the entrance, but unfortunately it is broken at the beginning. It is clear, however, that Harkhuf is relating that he sent a message to the king and is in part paraphrasing it: '. . . after the ruler of Yam. Then I *sh̄tp*-ed that ruler of Yam.'⁴ There follows a completely unreadable line—a source of frustration—which seems, however, to belong to what comes after it rather than to what precedes it.

The focal point of the passage, of course, is the word *sh̄tp*.⁵ This verb is not uncommon and in fact appears in the Horus-name of Teti of the Sixth Dynasty (*Sh̄tp-tꜣwy*) and in the praenomen of Ammenemes I of the Twelfth Dynasty.⁶ Generally it means 'to cause to be at peace, to cause to be satisfied,' etc. The question is: What did Harkhuf do when he *sh̄tp*-ed the ruler of Yam?

An earlier generation of scholars understood the verb, as used by Harkhuf, as implying subjugation, forceful pacification or the like. Erman translated simply: 'Ich brachte ihn zum Frieden.'⁷ Breasted was somewhat more explicit: 'Harkhuf immediately went after him and had no difficulty in reducing him to subjection.'⁸ Weigall experienced a change of heart about this, too, and modified his earlier view that the Yamite ruler had been pacified (by force) and decided that he had been calmed down.⁹ More recent scholarship has continued to water down the meaning to the extent that it is thought of by some as betokening a kind of appeasement or buying off of the Yamite chief. Säve-Söderbergh translated *sh̄tp* in the Harkhuf passage with the none-too-precise word *befrieden*, and concluded that Harkhuf won the chief over, that is he persuaded him to desist from his purpose.¹⁰ Furthermore, he denied that the phrase *sh̄tp hꜣst rs·yt*, which occurs in a broken graffito at Tomâs,¹¹ is certain evidence for a military undertaking, because *sh̄tp* has the general meaning *zufriedenstellen*.¹² One can only agree that it does not constitute certain evidence. On the other hand the general meaning of a word does not necessarily impose stringent limitations on the formulation of extended meanings. One could argue, with as much justice, that the Tomâs graffito implies violent pacification. Unfortunately neither view settles the issue.

¹ *Urk.* I, 125, 14–126, 1.

² Hölscher, *Libyer und Ägypter*, 24 ff. Weni had Tjemekh Libyans in his army: *Urk.* I, 101, 12–15.

³ *Ibid.* 126, 2. ⁴ *Ibid.* 126, 10–11. The paraphrase is indicated by the line preceding these two.

⁵ A causative based on *htp*; the difficulties associated with translating the latter are well known to anyone who has worked through the *Pyramid Texts* or the *'Imy D(w)st*.

⁶ Gardiner, *Egypt of the Pharaohs*, 436, 439.

⁷ Erman, *ZÄS* 30, 80.

⁸ Breasted, *History of Egypt*, 138.

⁹ Weigall, *Report*, 7; *History of the Pharaohs*, I, 236.

¹⁰ Säve-Söderbergh, *Ägypten und Nubien*, 28. It should be recalled that he considers Egypto-Nubian relations during the Old Kingdom to have been essentially peaceful.

¹¹ Weigall, *Report*, pl. 58, no. 5.

¹² Säve-Söderbergh, *op. cit.* 10, n. 3.

Yoyotte has maintained that Ḥarkhuf did not go after the ruler of Yam, but went along with him, all the while trying to calm him down.¹ This view is derived, in part, from translating the expression *m-s:f* as 'along with him'. It also involves understanding the verb *sm* as a perfective active participle rather than as an Old Perfective.² On the latter point, it must be admitted that both interpretations are legitimate, although I am rather inclined to agree with Edel that it is an Old Perfective. Some indication of the sequence of events may be present in the lines that follow: 'in order to order to cause that the Majesty of Mer-en-rē, my Lord, know (that) . . . after the ruler of Yam. Then (*ihr*) I *shtp*-ed that ruler of Yam.'³ The use of *ihr* would seem to indicate that the *shtp*-ing took place subsequent to the going after the ruler. Furthermore, Ḥarkhuf shows a distinct preference for *hnc* when accompaniment is intended.⁴ While the argument is not airtight, it does seem most probable that Ḥarkhuf found that the chief had departed, went after him, and *shtp*-ed him when he found him.

Edel's interpretation goes still further. He holds that Ḥarkhuf went after the ruler of Yam in order to let him know that he had arrived in Yam for the purpose of trade.⁵ Edel rejects the idea that *shtp* here implies forceful pacification. His view rests on the line which follows 'I *shtp*-ed him', namely, 'until he praised all the gods for the sake of the Sovereign'. This, Edel argues, means 'to thank' as is clear in the inscription of Idu,⁶ where the artisans respond to being *shtp*-ed by the owner of the tomb by praising the god. Thus, Edel concludes, Ḥarkhuf went after the ruler of Yam to inform him of his readiness to do business. Having talked the ruler of Yam out of his immediate purpose—smiting the Libyans—Ḥarkhuf returned to Yam with the chief where the latter was amply contented by a good exchange of goods, so much so that he praised the gods on behalf of the king, thereby thanking the king, who was after all the one with whom the chief was really dealing, Ḥarkhuf being merely an agent.⁷ One wonders whether the chief was so well acquainted with the niceties of Egyptian theory on foreign trade.

Dixon has held that rather than going after the ruler of Yam to announce his arrival, Ḥarkhuf acted to forestall any hostilities which might be inimical to the continuance of trade and to the supply of mercenaries. He is, however, against interpreting *shtp* as an indication of the use of force, arguing that if Ḥarkhuf wished to preserve peace, violence would not be a suitable method and, in any case, he might not have been in a position to use it.⁸

Most recently, Sir Alan Gardiner indicated that he had accepted Edel's interpretation.⁹ Perhaps the only major voice continuing to uphold the idea of forceful pacification has been that of Hermann Kees.¹⁰

If we accept Edel's 'peace and trade' interpretation, we are faced with some problems. One is raised by the Tomâs inscription referred to above.¹¹ In all probability it

¹ Yoyotte, *BIFAO* 52, 177: 'réussit à l'apaiser'.

² Cf. Edel, *Altäg. Gramm.* 1, § 585.

³ *Urk.* 1, 126, 9-11.

⁴ E.g. *Urk.* 1, 124, 10; 127, 5-6; 128, 8-9. Likewise Pepi-nakht: *ibid.* 134, 10, 17; 135, 3-4.

⁵ Edel, *Äg. Stud.* 54. Dixon, *JEA* 44, 45, thinks that Ḥarkhuf went after the chief, but to dissuade him from battle, thus disagreeing with Edel's view of his purpose.

⁶ *Urk.* 1, 271, 10-11.

⁷ Edel, *Äg. Stud.* 53, with references; Dixon, *JEA* 44, 46, n. 6; Säve-Söderbergh, *Ägypten und Nubien*, 20.

⁸ Dixon, *JEA* 44, 46.

⁹ Gardiner, *Egypt of the Pharaohs*, 100, n. 1.

¹⁰ Kees, *Ancient Egypt*, 129.

¹¹ See above, p. 29, n. 11.

refers to something that happened in Lower Nubia, perhaps in the region in which the graffito was carved. There is also the inscription referred to by Edel in another context: 'I *shṭp*-ed all the foreign countries for the Residence.'¹ The formula is essentially the same and would mean, if Edel is correct about the use of *shṭp* by Ḥarkhuf, that there were two more Egyptian officials who found it noteworthy and deserving of being permanently recorded that they had contented Nubians by giving them favourable trade deals.² One wonders if the king was sending the right men on his trading missions; it would be reasonable to expect them to proclaim the bargains they got for the king rather than the ones that were disadvantageous to him. It is difficult to reconcile the idea of Ḥarkhuf, the one who instilled the dread of Horus in the foreign countries,³ with that of him as the travelling salesman who gives his customers good bargains, especially if they happen to be on the brink of war when he arrives to do business. Nor does it seem very likely that an Egyptian official would use the medium of a brief graffito to commemorate the fact that he had given the Nubians a good trade deal.

The expression 'one who instilled the dread of Horus in the foreign countries', or the like, should not be taken too lightly. The tone is admittedly boastful in the grand Egyptian style, but the expression is not common before the Sixth Dynasty.⁴ Two travellers, who brought products from the south, Henti and Ini-kaf, employ the same expression, although there is no direct evidence that military action was involved.⁵ Pepi-nakht, however, is of another case entirely. He employs the same phraseology,⁶ and there can be little doubt that he is to be taken literally.⁷ He was sent to hack up Wawat and Irtjet and he brought back the rulers of these districts and living cattle, information which comes on the heels of the statement: 'The Majesty of my lord sent me to *shṭp* these foreign countries.'

Another Sixth-dynasty official, Sabni, also claims to have *shṭp*-ed 'these foreign countries'.⁸ His father, Mekhu, had died (or been killed; it is not clear) in Lower Nubia and Sabni went thence to recover the body. A problem exists here because Sabni indicates that he took one hundred asses laden with oil, honey, etc. It has been thought that these commodities might have been trade goods to buy off the Nubians so as to be able to recover the body.⁹ It is unfortunate that the text is so badly damaged; assertions are not readily supported. The line which contained Sabni's purpose in taking these items is too broken to be translated. The line after the occurrence of *shṭp* is completely gone. Furthermore, the beginning of the text is likewise lost. If Sabni's father had simply died in the course of his return from a trading mission to

¹ Edel, *MDAIK* 13, 53. The text can be found in *Urk.* 1, 255, 4. Edel translates: 'Ich befriedigte alle Fremdländer für den Hof.' ² Kar indicates that he had been involved with Nubians: *Urk.* 1, 254-5.

³ *Ibid.* 124, 3.

⁴ Fakhry, *Ann. Serv.* 38, 41-42.

⁵ *Ibid.* 36, 38; cf. *Urk.* 1, 141, 180; Baer, *Rank and Title*, 57, 105.

⁶ *Urk.* 1, 132, 3. He repeats this expression after reporting his assault on the eastern Bedouin, *Urk.* 1, 133-4. Professor Wilson suggested to me that Pepi-nakht may have copied it from Ḥarkhuf.

⁷ Clear indication of military action is to be seen also in *Urk.* 1, 133, 9-134, 10.

⁸ *Urk.* 1, 136, 13.

⁹ Säve-Söderbergh, *Ägypten und Nubien*, 19 ff., makes much of the idea of *tongo*, payment to the natives for safe passage through their territory. I see nothing in the texts to justify such a conclusion. See below, p. 32, n. 7.

the south, why would it be necessary for Sabni to pacify the natives of Lower Nubia (especially) with so many gifts? In a period when Pepi-nakht employs force against Lower Nubia, is it reasonable to suppose that if the father had been murdered by the natives, the son would respond with gifts in considerable quantity? Would Sabni, in a situation that testifies primarily to his pious and praiseworthy behaviour with respect to his deceased father, take time to record that he had made a good trade agreement with the natives of Lower Nubia?¹

I am inclined to think that Kees and the older generation of Egyptologists have the better of the argument. Ḥarkhuf had as his primary mission the acquisition of certain commodities. He was, in addition, charged with the duty to make certain that the trade routes were kept open. It is more than likely that he was one of a number of officials so empowered. Arriving in Yam he found that Yam and Tjemeh were on the brink of war. He acted by pacifying the ruler of Yam, with the military force which he had with him, for the purpose of stopping something that threatened Egyptian aims in Nubia. The reason the ruler of Yam praised all the gods on behalf of the sovereign was that he was indeed grateful; Ḥarkhuf spared his life. Ḥarkhuf was the representative and agent of the King of Upper and Lower Egypt, whose tacit suzerainty had been acknowledged by those chiefs who appeared at the First Cataract to pay their homage to Mer-en-rē.² It seems far more comprehensible to view this and other *shṭp* texts referring to Nubia as evidence for the application of force or the *threat* of force. This is the stuff whereof boasts are made, and the tone of the biographies of Ḥarkhuf, Weni, Pepi-nakht, and of some of the graffiti is martial.

Both Edel and Dixon argued that the subsequent appearance of Yamite mercenaries in Ḥarkhuf's army indicated that there could not have been any use of violence by the Egyptians.³ Yamites had served in the Egyptian army under Weni,⁴ and are mentioned as serving in Egypt in the Dahshur decree.⁵ It is clear that Yamites had a history of mercenary service prior to Ḥarkhuf's trips to Yam. Furthermore, there is no reason to suppose, as do Edel and Dixon, that recently defeated troops would have posed a threat to Ḥarkhuf's forces rather than have acted as an aid. History contains enough examples of defeated forces being safely incorporated into the victor's army to show that this conclusion does not necessarily follow.⁶ That the Yamite troops returning with Ḥarkhuf from his third journey to Yam were newly recruited mercenaries emerges rather clearly from Ḥarkhuf's own statement: 'the troops of Yam which were returning with me to the Residence, together with the army which had been sent with me'.⁷ This was not just a bodyguard furnished by the ruler of Yam, but rather a body of Nubians going to Egypt under the command of an Egyptian official whose title—Overseer of

¹ See preceding note, p. 31.

² *Urk.* 1, 110–11. The appearance of Mer-en-rē at the First Cataract in Year 5 might very well have followed upon Ḥarkhuf's third journey. This is, of course, mere speculation.

³ Edel, *Äg. Stud.* 54; Dixon, *JEA* 44, 46 f.

⁴ *Urk.* 1, 101, 14.

⁵ *Ibid.* 209.

⁶ Roman policy during the Republic and early Empire and the policy of Alexander demonstrate the point.

⁷ *Urk.* 1, 127, 5–6. The reaction of the ruler of the Lower Nubian districts named in 127, 4 appears to be an attempt to speed Ḥarkhuf and his forces on their way to avoid giving them any excuse for causing damage rather than a case of the chief being thwarted in an attempt to collect *tongo*.

Mercenaries—indicates that he normally commanded such troops. No doubt they were to enter the Egyptian military or 'police' force when they arrived in Egypt.

Thus Ḥarkhuf was invested with considerable responsibility. He had troops under his command, he was empowered to recruit more from among the Nubians, and he had the discretionary power to use the force at his disposal should any circumstance arise which he deemed to be a threat to Egyptian trade routes.¹ The king of Egypt had ceased taking a personal hand in Nubian affairs and delegated authority to men like Ḥarkhuf and Pepi-nakht. The assertive, independent spirit of the provincial nobility—here the nobility of the Elephantine region, was, for the moment, being channelled into such activities. It was necessary that they be given initiative and a plenitude of discretion to act.² This is what was involved in the actions expressed by the verbs *wbꜣ* and *šꜣtp*.

¹ Ḥarkhuf may not have found it necessary to attack the Yamite ruler and his band; a convincing threat might have worked too. This would be backed up by the certainty that he would use force if necessary. *Šꜣtp* would still include the notion of force.

² In this connexion it is interesting to note that Ḥarkhuf did not feel constrained to await instructions from Memphis. He acted and informed the king of his actions.

VARIA GRAMMATICA

By A. M. BAKIR

EVERY teacher of Egyptian language is inevitably involved from time to time in a reconsideration of the linguistic views put forward in Gardiner's *Egyptian Grammar*, which is still the grammatical introduction in most frequent use. In fact, discussion with students puts every grammatical theory to a severe test, and the following observations are partly the result of such discussion. They will be divided into two sections, the first devoted to remarks on certain points in Gardiner's *Grammar*, and the second to a discussion of some relevant views which are partly propounded in my *Principles of Middle Egyptian Grammar* (in Arabic), 1954.

I

1. In § 194, p. 146 where 'Idiomatic phrases used as nouns' are discussed, the last two examples are:

(a) *šnyt m tp-ḥr-mst*

(b) *s nb m ḥr-m-ḥrw*

Gardiner translates (a) 'the courtiers were head-on-lap, i.e. in mourning', explaining the *m* as that of predication. It is more likely, however, that *m* here means 'with'. If it were the *m* of predication, one would expect the form of the *Identitätssatz*, the predicate being identical with the subject. This is clearly not so, although the predicate refers to a part of the body. The literal meaning is surely 'the courtiers (were) *with* head on lap'. Similarly Gardiner translates (b) 'every man is face-downcast, i.e. abashed', taking the *m* again as that of predication. But the prepositional *m* again makes better sense: 'every man (is) *with* face down'.

2. In § 163, 11, p. 126 (cf. p. 147 in the reading lesson), it is stated that *r mrr·f* means 'according as he desires'. One would expect the Egyptian for such a clause to be *mī mrr·f*, see § 170, p. 130, 5 b. The true meaning of *r mrr·f* is 'as much as he desires'.

3. On p. 58, n. 1, it is said that 'this thy humble servant' is to be rendered simply *bḥk im* 'the servant there'. *Im* here is almost certainly the *m* of predication in its emphatic form before a suffix, the meaning being 'the servant (is) I', i.e. 'I am your humble servant'; cf. § 162, p. 125 n. 6 (on the *m* of equivalence or predication): *it·tn im·i* 'your father is myself'. The fact that the suffix is not written in the phrase *bḥk im* corresponds with the Middle Egyptian practice of frequently dropping the first person suffix.

4. In § 444, 2, p. 357 the sentence *mrr·tn Wp-wḥwt . . . dd·tn* is translated 'if (or so surely as) you love Wepwawet . . . say ye'. Grammatically it is here possible for the pronoun *tn* to be the object, so that an alternative translation will be 'if (or, as) Wepwawet loves you', etc. Since a god is mentioned, there is a possible threat in the non-fulfilment

of the condition, and there is often an element of threatening in these appeals to the living; cf. statements of oaths in § 468.

5. In § 385, p. 300 (on the omission of the resumptive pronoun) the following sentence is quoted: *ink pw mrrw ntr snh:f wi*; Gardiner translates 'it is I whom the god wishes to preserve, lit. that he should preserve me', taking the last word as the resumptive pronoun. However, in another version, Pap. Hearst no. 78, we find $\text{𓂏} \text{𓂏} \text{𓂏} \text{𓂏} \text{𓂏}$, so that we are invited to see here, not a resumptive pronoun, but a determinative of the whole expression, while *snh:f* will be, not a *sdm:f* form, but an infinitive with a suffix pronoun. The literal translation will now be 'I was a man whom the god wished his being preserved'. If so, the writing *wi* in Pap. Ebers is an error of the scribe, and there will, therefore, be no need to talk, as Gardiner does, of a pronoun 'illogically and exceptionally substituted'.

II

In the following remarks it is assumed that a comparison of Egyptian grammar with that of Arabic has a special validity. The tendency of European scholars is to use the terminology and approach of Indo-European grammar. Egyptian, however, is a Hamito-Semitic language; its affinities with Hebrew are often acknowledged, but it is equally manifest that an affiliation with Arabic exists.

1. *The indirect genitive*

The genitive in Indo-European grammar is generally used to indicate possession. In Arabic, however, the term 'annexation'¹ is employed, and it is clearly a preferable term, since in Egyptian a meaning other than possession is often present. For example *ntr·i* is better understood as 'the god attached to me' rather than as 'my God,' i.e. 'the God whom I possess'. It should also be noted that the genitival *n* used to indicate what is termed 'the indirect genitive' implies, as in Arabic, a different meaning from the 'direct genitive'. Thus in Arabic the phrase *ibn Mohammed* implies 'the son (of) Mohammed'; but the phrase *ibn li-Mohammed* implies 'a son belonging to Mohammed'. In other words, the second phrase means that Mohammed has more than *one* son. It so happens that in the papyrus which I have been editing, Cairo Calendar no. 86637, *rt. i, 4*, there occurs the expression *sꜣ n Rꜥ*, used of Ptaḥ. Usually the expression is translated 'the son of Rēꜥ', but this makes no distinction between this phrase and its more common counterpart *sꜣ Rꜥ* used of the king. It may be suggested that *sꜣ n Rꜥ* means 'a son of Rēꜥ', with the same implication as in Arabic. A parallel instance occurs in the temple of Beit-el-Wâli, see Roeder, *ZÄS* 50, 126; cf. also the phrases *irt Hr* (*rt. i, 3*) and *irt nt Hr* (*vs. v, 11*) in the Cairo Calendar. In the latter case, it is clear that Horus has two eyes, and that *irt nt Hr* will refer to either of them, whereas *irt Hr* will mean the particular eye referred to in the context. In the case of *sꜣ Rꜥ* used of the king, it is clearly implied that the legitimacy of the king's enthronement depends on the idea that he is the *only* son of Rēꜥ, even to the exclusion of his brothers. Thus, the first noun must be regarded

¹ Cf. W. Wright, *A Grammar of the Arabic Language*, II (3rd ed. 1955), 225, § 92; also Bakir, *Principles of M. Eg. Grammar*, p. 19.

out again by the use with *pw*, e.g. *ntf pw m mst* 'it is he in truth' quoted by Gardiner in § 128, where he admits that 'the logical predicate may be an independent pronoun'. The predicative meaning of *in* is made clear by a comparison of the compound pronouns *in-ni*, *in-nakā* in Arabic, cf. Bakir, *Principles of M. Eg. Gr.*, pp. 24, 25.

3. *The function of 'Iw*

All grammarians hitherto seem to have regarded *iw* as a copula, or an auxiliary, whatever their views may be about its origin. This theory is plausible with regard to its use when followed by a pronominal or nominal subject, but it falls down badly when it seeks to explain the constructions *iw sdm·f* and *iw·f sdm·f*, as well as other constructions described as exceptional by Gardiner in § 468. That Gardiner himself had some doubts on the matter is shown by his observation 2 in § 461, where he says: 'There are grounds for thinking that, when *iw* was followed by a singular suffix-pronoun, the *w* was merely graphic, e.g. 𐎛𐎠𐎗 was pronounced *āf*.' An explanation which deals with all the facts more satisfactorily can be derived from a comparison of the varied usages of *waw* in Arabic.

(a) *Iw* with suffixes

The role of *iw* here seems to be one of accentuation or emphasis; just as *ink* can introduce a nominal sentence because of its compound nature, so the suffix pronouns in this position must have the element *iw*. In spite of this, *iw* + suffix can appear in subordinate clauses, and this function corresponds exactly to the use of *waw* in the Arabic circumstantial clause.

(b) *Iw* + noun

A similar accentuation is present here, and it corresponds to the *waw* of commencement in Arabic; see W. Wright, *Arabic Grammar*, II, 333 A.

(c) *Iw* + *sdm·f*

Gardiner's explanation (in § 461) of this usage is particularly unconvincing. He says that 'probably *iw* should be regarded as an impersonal statement "it is", i.e. "the situation is", the following *sdm·f*, *sdm·n·f* or passive *sdm·f* form being a virtual adverb clause (§ 215) serving as predicate of *iw*'. This procedure is so highly involved as to be quite incredible. The principles of accentuation and commencement can apply equally well to this construction and are consistent with the fact that the usage is often found in generalizations.

(d) *Iw* in statements of oaths

In § 468 Gardiner regards the use of *iw* in such sentences as exceptional, but it is a striking fact that *waw* in Arabic has a precisely similar employment, cf. Wright, *Arabic Gr.* II, 175, § 62.

4. *Some remarks on the sdm·n·f*

It has been customary to interpret this form as a counterpart of the present *sdm·f*, but with reference to the past. Sander-Hansen, *Äg. Gr.* (1963), §§ 311, 312 gives clear

examples where this *n* is not a sign of the past but part of the verbal stem, and so could equally give a present meaning; cf. the use of *mꜣꜣ*, *mꜣ* and *mꜣn*, Gardiner, *Eg. Gr.* §§ 299. 439. 448. Forms which include this *n* can even have a participial ending; see Sander-Hansen, *op. cit.*, § 312 who quotes: *nn wi ꜥnꜥ ꜣmꜣny*. In this and other examples cited by Sander-Hansen, it is true that the *n* follows the determinative; the reason for this may be that a differential nuance is added at the end. The *n* has therefore no connexion with the postulated sequence of participle + *n* forming a past tense.

Another use of the *sꜣm·n·f* claimed by Gardiner with the verb *rꜥ* is that, although *rꜥ·n·i* is a past tense, he regards it as a 'present' expressed in the form of a 'narrative' past, and so exceptional; cf. Gardiner, *op. cit.*, § 414, 4. I maintain that 'to know', 'to remember', 'to think', 'to doubt' express a 'mental state' and not an 'event' or an 'action' that takes place at a specific time. Examples could be quoted from Arabic to illustrate this point. To say 'knew' so-and-so, or 'knows' so-and-so means that knowledge has existed irrespective of time, cf. Bakir, *op. cit.*, § 174.

I would conclude with a plea for a more extensive use of Hebrew and Arabic for the elucidation of those points in Egyptian grammar which still remain problematic. Comparative grammar is always a useful study, but it is most useful when related languages are involved.

THE LETTER TO THE DEAD FROM THE TOMB OF MERU (N 3737) AT NAG' ED-DEIR

By WILLIAM KELLY SIMPSON

IN addition to the four lengthy account documents from tomb N 408, two of which have formed the subject of recent monographs,¹ there are several shorter texts of earlier periods from the site of Nag' ed-Deir in the Museum of Fine Arts, Boston. One of these, a letter addressed to the deceased Meru by his son Hēni, is the subject of the present study.² I am greatly indebted to Dr. William Stevenson Smith for giving me permission to illustrate and discuss the document, and for allowing me every opportunity to study the papyrus itself.³ My gratitude is also extended to Mrs. Caroline

¹ *Papyrus Reisner I: The Records of a Building Project in the Reign of Sesostris I, Transcription and Commentary*, Museum of Fine Arts, Boston, 1963; *Papyrus Reisner II: Accounts of the Dockyard Workshop at This in the Reign of Sesostris I, Transcription and Commentary*, Museum of Fine Arts, Boston, 1965.

² In Ancient Egypt letters were written to deceased individuals to request some sort of aid against real or imagined malign influences on the part of living or dead persons. Disease or other afflictions were considered a result of such interference. These letters were often written on pottery vessels placed in tombs. It was considered that this method of transmission was particularly effective, since the dead spirit would naturally be attracted to the food-offerings and would inevitably be forced to read the accompanying missive. The identifiable letters to the dead were collected and published by Alan H. Gardiner and Kurt Sethe, *Egyptian Letters to the Dead mainly from the Old and Middle Kingdoms*, London, 1928. Of this series of nine documents, three are assigned to the New Kingdom and six to the Old Kingdom, the First Intermediate Period, and the Middle Kingdom; there is some question as to whether two of the nine are actually letters to the dead. An important review of this publication was provided by B. Gunn, *JEA* 16 (1930), 147-55, in which he presents a succinct analysis of the genre and a valuable discussion of particular passages in the texts. Since the time of the publication, two additional letters have been added. The first of these is written on a jar-stand, now in the Oriental Institute Museum in Chicago, and was published by Gardiner, 'A new letter to the dead', *JEA* 16 (1930), 19-22. The second which, like the Chicago example is dated in the First Intermediate Period, is published by J. J. Clère and A. Piankoff, 'A letter to the dead on a bowl in the Louvre', *JEA* 20 (1934), 157-69. The Gardiner-Sethe volume, the Gunn review, and the two articles just cited provide the basic corpus for the study of the genre.

Three additional letters to the dead are known to me. One of these is the subject of the present study. The second was identified by Edward F. Wente, Jr., on a small stela or tablet seen and copied by Wente in the Egyptian Museum, Cairo, where it was being approved for export; its present location is unknown to Wente and myself. The third letter is an unpublished papyrus of the Old Kingdom from Nag' ed-Deir Tomb N 3500 in the Museum of Fine Arts, Boston. The letter is addressed to (or possibly sent by) an individual named Teti-sonbe, the first part of the name being written in a cartouche.

The letter to Meru here edited (M.F.A. accession no. 38.2121) has been discussed in part by Mrs. Peck in her doctoral dissertation, *Some Decorated Tombs of the First Intermediate Period at Naga ed-Dêr*, University Microfilms, Ann Arbor, [1959], 93, 121, 124-6, with a table of palaeography on p. 125, fig. 8.

A related genre is attested at a later period in the letters to deities: George R. Hughes, 'A Demotic letter to Thoth', *JNES* 17 (1958), 1-12. The letter, written in 502 B.C., is a complaint of extreme persecution at the hands of a fellow worker of the writer. Hughes notes the existence of several other letters of this nature addressed to deities, and notes the parallel to the early group of letters of complaint to dead relatives.

³ This study has been prepared during the first part of a sabbatical faculty fellowship from Yale University and during my appointment as a Fellow of the John Simon Guggenheim Foundation; I should like to express

Nestmann Peck for the generosity with which she consented to relinquish a project to publish the letter, a project close to her heart, which she had hoped to carry out at a later date, but one which she could not find the time to undertake in the near future.

The document was discovered during the excavation of the tomb of Meru (N 3737) in the course of the expedition at Nag' ed-Deir directed by Professor George Andrew Reisner and was assigned to the Museum of Fine Arts.¹ The papyrus consists of a single strip, 24 cm. high by 8 cm. wide, which has been pieced together from several fragments. Aside from the worn margins and lacunae it seems that the text is complete with five columns on the recto, the side with the horizontal fibres uppermost, continued and concluded with a sixth column on the verso.² The verso also has the titles and name of Meru, the addressee, and the name of Hēni, the sender, the latter written upside down in relation to the rest of the writing on the verso.³ Presumably the papyrus strip was folded once along its vertical axis and then the ends folded in toward the centre so that the resulting packet bore Meru's titles and name on one side and the name of Hēni on the other, with the top of the papyrus tucked in between. (See diagrams, fig. 1.) It was probably discovered folded, and the places where the material is damaged support this reconstruction of the method of folding.

Although it is conceivable that several columns are missing at the end of the recto, I believe that the letter consisted only of the six columns preserved. There is no trace of another column to the left of column five. The colour is a medium light tan, and the papyrus is so inscribed that the top of one side is also the top of the other. This is the only published early letter to the dead written on papyrus. With the exception of the Cairo Linen Document, the other letters are preserved on bowls and one vessel which seems to have been reused as a pot stand: the Qâw bowl (one letter on the inside and one on the outside), the Hū bowl, the Berlin bowl, the Cairo bowl, the Louvre bowl, and the Chicago pot stand. The unpublished letter from Nag' ed-Deir Tomb

my appreciation for these fellowships. The diagrams indicating the method of folding were drawn by Mr. Hossein Ziai, my student assistant at Yale, to whom I am indebted for this service. I have taken the opportunity of using the Boston letter in my classes, and I wish to acknowledge several suggestions by my student, Miss Virginia Davis. The expenses of preparing photographs and drawings have been borne by the fund for Research and Publication in Egyptology of the University, with aid from the Guggenheim Foundation.

¹ Peck, *Some Decorated Tombs*, 121.

² The account papyri from Nag' ed-Deir frequently have the earlier entries on the verso instead of the recto, a procedure contrary to general practice. In the New Kingdom letters regularly begin on the verso, as explained by J. Černý, *Late Ramesside Letters*, xvii-xx, whereas our letter begins on the recto. Similarly, our letter differs from those of the New Kingdom in that the top of the page is the same on both sides, whereas in the New Kingdom the top of one side corresponds to the bottom of the other.

³ The writing of the names in inverted fashion in relation to each other is explained by the method in which the papyrus was folded, as outlined in the diagrams. It is also a feature of the addressing system of letters. In the Old Kingdom the name and title of the address are inverted in relation to each other (P. Berlin 8869; *Berl. Hier. Pap.* III, pl. iii). Scharff notes, in respect to the Illahun letters, that when a man writes to an equal or superior the names of both appear in the address, inverted in relation to each other, with the sender's name following the preposition *m-r*, 'from' (*ZÄS* 59 (1924), 22). When a superior writes, however, only the name of the addressee appears, as in the case for letters of all types in the Old Kingdom. For examples of the inverted writing see Griffith, *Hier. Pap. from Kahun and Gurob*, pls. 29, 31, 35, 37, in each of which the preposition *m-r* precedes the name of the sender.

N 3500 with the name Teti-sonbe is also, however, on papyrus and may be the earliest of the series.

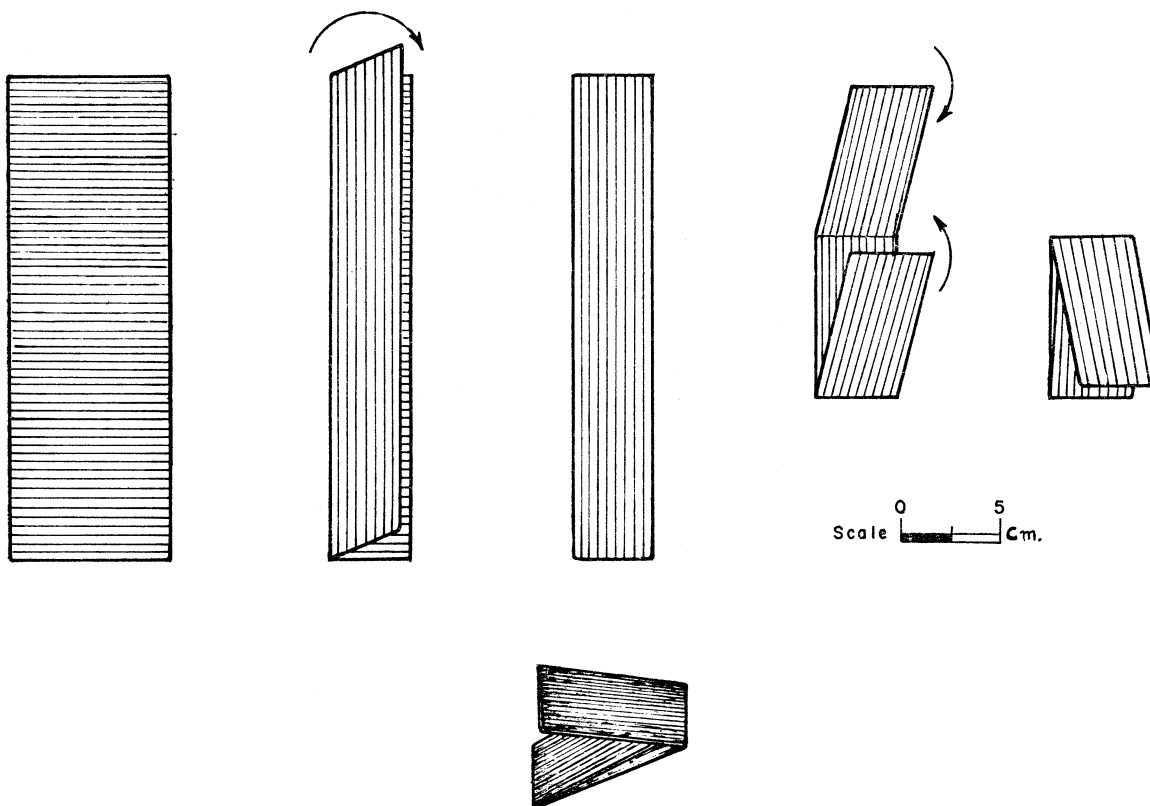


FIG. 1. Diagram of the method of folding the letter.

Translation

Recto: (1) A servant speaks before his lord, his [beloved?]. Hēni speaks. (2) Attention, many times! Attention for the one whom you have endowed (?) is beneficial, on account of this which your *dt*-servant Sen[i] does, in causing that this servant see him(self?) (3) in a dream in one city [together with] you. It is his character which damns (?) him himself. Now (4) there did not take place a summoning (?) which happened to his disadvantage by the agency of this servant. There is no end of anything which will happen (?). It was not I indeed (5) who first caused [his?] discomfiture (?); others acted before this servant. Grant, pray, the protection of his [. . . .]. Do not let [him] cause his protector (?) to be injured. **Verso:** (6) so that he will not see this [servant] ever <again>.

[Addressee]: The prince and count, the overseer of priests, Meru

[Sender]: Hēni

Commentary

b:k dd hr nb:f [mrrw]·f(?) [H]ni dd. 'A servant speaking before his [beloved] lord, Hēni speaking.' For *b:k dd hr nb:f*, see Wilson, *Mélanges Maspero*, 1, 903, and Hayes, *JNES* 7 (1948), pl. i, col. 1. The preposition *hr* is similarly used in the construction in the Cairo Linen Document: *snt ddt hr sn·s; s; dd hr it·f*.¹ There is too little space to

¹ Gardiner–Sethe, *Egyptian Letters to the Dead*, pl. i, 1.

restore *mrrw* with the quail chick, but some form of *mri* may be possible. Hayes points out, however, that *mrrw·f·nh·f* in the much copied letter of the Middle Kingdom means, 'whom he desires to live'.¹ As an alternative for our text one might suggest the restoration, *s·f·Hni dd*, 'his son Heni speaks'. The verb *dd* is generally taken to be a perfective active participle.² Following this column and separating it from the next is a short dividing line such as is represented in P. Berlin 8869 (Old Kingdom) and the Cairo Linen Document, a less exact parallel being the pair of such lines at the end of P. Bulaq 8.³

icnw h̄h n s[p]; ih icnw n m̄h̄nkω(?)·n·k. 'Attention many times. Attention is beneficial for the one whom you have endowed', or 'Attention many times is beneficial, attention for the one. . . .' The first problem here is *icnw*, a term recently discussed at some length by James and Baer.⁴ In our context the following meanings would be possible: 'help, aid, intercession, appeal, attention, and solicitude'. Gunn suggests this range of meaning with 'attention, *Achtung*' singled out.⁵ One might translate, 'Help! many times; help is beneficial. . . .' The next problem is *ih*, which could possibly be read at the end of the first phrase as an Old Perfective (. . . is beneficial) or as a participle in a nominal sentence (is what is beneficial), although it is perhaps more likely that it is a *sdm·f* construction at the beginning of the second phrase (attention is beneficial for . . .). The use of *ih* in any case is related to the usage in the cliché constructions of *tw n r ih n sch*, *nn nw m wrdt hr·s*, 'the breath of the mouth (the utterance of an invocation) is beneficial to a departed spirit, and this involves no fatigue (on the part of the speaker)', and *ihw n irr r irrw n·f*, 'it is more beneficial for the one who does it (the one who speaks) than for the one for whom it is done (the one on whose behalf the utterance is recited)'.⁶ Other constructions in which *ih* is similarly used have been collected by Janssen.⁷ Clearly, attention of some sort is solicited by the writer and is considered beneficial or profitable when and if extended to him. A completely different view is developed by Gardiner and Sethe in the close parallel of the H̄u bowl text: *icnw ih ih, icnw n m̄h̄nkω(?)·n·k*, which they render, 'A great, beneficial (?), (cry of) woe! Woe to him whom thou hast appointed as *m̄h̄nk(?)*'.⁸ Gunn has already criticized this reading and suggested 'will be profitable' for *ih*.⁹ Our new text effectively rules out Gardiner's view and supports Gunn's suggestion. The reading *m̄h̄nk*, however, also followed by Gunn, certainly deserves consideration, and I have adopted it for the lack of a better solution. In our text the sign is the simple arm as represented in the text of the verso in the titles of Meru, although elsewhere on the recto the sign is simplified without any

¹ *JNES* 7 (1948), 7.

² Gardiner, *Eg. Gram.*³, § 450, 1; James, *Hekankhite Papers*, 120.

³ Gunn, *JEA* 16 (1930), 148; *Berl. Hier. Pap.* III, pl. ii; *Egn. Letters to the Dead*, pl. i; Mariette, *Les papyrus égyptiens du Musée du Boulaq*, I, pl. 39.

⁴ James, *Hekankhite Papers*, 109; Baer, *JASOS* 83 (1963), 2-3.

⁵ *JEA* 16 (1930), 151.

⁶ Spiegelberg, *ZAS* 45 (1908), 67-71; Burchardt, *ZAS* 47 (1910), 115; Turajeff, *ZAS* 48 (1911), 161; Sottas, *Préservation de la prop. fun.*, 76-78; Blackman, *JEA* 21 (1935), 5, n. 1; Gunn, *Syntax*, 142; *BIFAO* 30 (1930), 799, 806; *Urk.* IV, 123, 415, 485, 1196, 1535; Engelbach-Gunn, *Harageh*, 28, n. 5, pl. 24, 3 and pl. 72, 3; I am indebted to Prof. J. J. Clère for these references. For an additional example see Fakhry, *The Monuments of Sneferu at Dahshur*, Vol. II, Part ii, 42, fig. 340 A, pl. 61. See also *Rec. trav.* 3 (1872), 119, xix, where the funerary invocation is explicitly cited. The general idea is that real offerings are preferred, but that in their absence the invocation is at least partially effective (cf. Clère and Vandier, *Textes de la prem. pér. int.* I, § 2).

⁷ *De traditioneele Egyptische autobiografie*, I, pl. 1.

⁸ *Letters to the Dead*, 5, pl. iv.

⁹ *JEA* 16 (1930), 152.

real indication of the palm of the hand. I do not know if one should attach much significance to this. One might indeed consider if an *m-* formation of *rdi* may be preferable to *mhnk*. The title *mhnk* with examples of its writings, none of which, perhaps significantly, correspond to those in the two letters to the dead, is discussed by Junker.¹ The reading of the text as *mhnkw·n·k* is thus clearly doubtful, if not impossible, a major objection being the presence of the determinative of the man with hand to mouth. An alternative deserves consideration: *icnw hñ n s[p] ðh icnw nmcw n·k* (or *nmcw·n·k*). The verb would then be *nmc*, 'to be partial, one-sided', of *Wb.* II, 267. 1–2, which is frequently determined by the sign of the man with hand to mouth and is attested without the writing out of the biliteral *nm*. The term *nmc* is used as a bad quality to be avoided, as in the partiality of a judge toward one of the litigants. It is possible, however, to envisage the letter writer as soliciting such partiality for his case. It is unlikely that we can treat the phrase as an imperative *nmcw n·k* 'be partial', in view of the terminal *-w*, a formation not attested for the imperative singular. The phrase *icnw nmcw·n·k* might be rendered, however, as 'the attention of the one you have favoured (the one to whom you have been partial)'. In the latter case there is some difficulty in understanding the text as 'the attention of the one you have favoured is beneficial' when we might expect 'attention for the one you have favoured', the latter almost requiring an additional *n*: *icnw n nmcw·n·k*. It is difficult to emend thus in both the Boston letter and the H̄u bowl. The matter cannot be said to have been solved.

hr nn irrw dt·k Sn[i]. 'On account of this which your *dt*-servant Sen(i) does.' The formula is similarly continued on the H̄u bowl: *hr nn irrw sst·(i)*. This is important, for it serves to underline the stereotyped nature of a formula which must have been fairly common in these letters of complaint to deceased relatives. It suggests that the local scribes were familiar with a genre and merely adapted a series of model letters to the special situations at hand. The parallelism is too close to be otherwise interpreted. It also raises a very difficult problem as to its interpretation, but one which perhaps can be solved. It will be useful to quote the text of the H̄u bowl in translation, the rendering which follows being that of Gunn rather than that of Gardiner and Sethe.²

A sister say to her brother: Great attention! attention to him whom you . . . ed will be profitable, on account of this which is being done <against> my daughter very wrongfully. I did him no harm; I did not consume his property; he did not (have to) give anything to my daughter. One makes funerary offerings to a spirit for the sake of protection of the survivor! Make your reckoning (quickly) with whoever is causing our (?) suffering, for I shall triumph against any dead man or woman who is acting thus against my daughter.

The opening lines of Gardiner's translation may also be cited: 'A great, beneficial (?) (cry of) woe! Woe to him whom thou hast appointed as *mhnk*(??) on account of that which is being done <against (??)> my daughter very wrongfully.' Clearly Gardiner and Sethe regarded the individual represented in the phrase *mhnkw·n·k* as the villain, as is also borne out by their rendering, 'woe to . . .'. The opposite situation obtains,

¹ *Die gesellschaftliche Stellung*, 16–18; also Montet, *Kémi* 4 (1931), 172. If we reject the reading *mhnkw·n·k*, it is just barely possible that we should read *mw·n·k*, which may be related to the *munf* of *Wb.* II, 55, 7–9.

² *JEA* 16 (1930), 152.

however, if the term *icnw* is translated as solicitude, help, or attention *for* a person. The matter at hand, however, concerns the rendering of *irrw sst·(i)*. Gunn follows Gardiner and Sethe in emending *irrw <r> sst·(i)*, ‘what is being done <against> my daughter’, instead of ‘that which my daughter does very wrongfully’. The reason for the emendation is clear. Since intercession is requested on behalf of the daughter, she cannot be regarded as the wrongdoer. If we follow Gunn, Gardiner, and Sethe, and then likewise emend the Boston letter, we will have cast Seni into the role of the maligned against, instead of the villain: ‘on account of that which is being done <against> your *dt*-servant Seni’. The problem can be explained in the following terms. In a stereotyped formula of this nature, one must consider both Seni and ‘my daughter’ either as the wrongdoers or as the ones wronged. It is extremely unlikely that in one text ‘my daughter’ is the one wronged and in the other Seni the wrongdoer. This is precisely the situation we have if we follow Gunn–Gardiner–Sethe for the Ḥu bowl and my translation for the Boston letter. Since their view involves an emendation in adding the preposition *r*, ‘against’, it is *a priori* suspect. Yet if we fail to emend, we have a situation whereby someone complains of a daughter’s wrongdoing at the beginning of the letter and then continues with a complaint against evil forces harming the same daughter. One could indeed read the letter with the daughter in the role of the wrongdoer, specifically by reading the end as ‘do this against my daughter’ instead of ‘who acts thus against my daughter’. Even though Edel cites this form of *ir(r)* as a writing of the imperative,¹ one senses that violence is being done to the rendering. A compromise solution is still possible. If we translate, ‘this which my daughter has to (is forced to) do very wrongfully’, it is no longer necessary to emend or to regard the daughter as the real wrongdoer. By a strange coincidence, this type of rendering is followed by Gunn for another passage in the same letter: ‘he did not (have to) give anything to my daughter’. If we accept the solution, both Seni and the daughter are, syntactically, subjects of *irrw*, the difference lying in the unwilling wrongdoing of the latter. I am not completely satisfied by this compromise, but I am entirely unwilling to emend the Boston letter as well as the Ḥu bowl. Obviously, complaint is being made against some malign influence in the Boston letter, and this evil-doer can only be Seni. For the identification of Seni, see below.

n rdit m3 szw b3k im m rswt m n3wt wct [hnc]·k. ‘In causing that this servant see him(self?) in a dream in one city [with] you.’ The phrase serves to specify and amplify the ‘things which your *dt*-servant Seni does’. The preposition *n* with the infinitive may mean, ‘because of’.² It is left to the context to determine whether or not *szw* is reflexive. The situation whereby a man sees himself in a dream is well represented in the *Dream Book* of P. Chester Beatty III = P. B.M. 10683.³ At the same time the last column of our Boston letter seems to imply that the writer wishes that his antagonist will not see him (the writer) again, although here there is no specific reference to the world of dreams. I think that *szw* may well be reflexive. I do not know of another example of *rswt*, ‘dream’, determined by the seated Seth. The determinative is used at this period for *mr(t)*,

¹ *Altäg. Gram.*, § 602.

² *Op. cit.*, §§ 1042, 1757.

³ A. H. Gardiner, *Hieratic Papyri in the British Museum*. Third Series, Vol. I, 9–11.

ind, and *nkm(t)*, which have the sense 'to be ill (pain), and to afflict (affliction)',¹ and *rswt* so determined may have the nuance of 'nightmare', 'cauchemar', or the like. There is indeed the remote possibility that the determinative characterizes the dream as Typhonian in the sense that a section of the dreams in the *Dream Book* has the heading, *Beginning of the dreams of the followers of Seth*.² For the use of the throw-stick as the third sign in *rswt*, see the example of *rsw* cited by Edel, *Altäg. Gram.* § 1152, and the remarks by Gardiner on the substitution of the throw-stick for the *rs* sign.³ Several years ago Edward F. Wente, Jr., kindly gave me a hand copy of a letter to the dead inscribed in hieratic on the back of a small stela or tablet of the First Intermediate Period, seen on the antiquities market and the location of which is unknown. It includes the phrase *ḥr·t ḥr·i m rswt*, 'may you (fem.) fight for me in a dream', without any determinative for *rswt*. It seems clear from this stela and the Boston letter that the world of dreams may have played a role in the background of the situations met with in these letters. A view of the afterworld may have been effected through a dream, and a letter to a dead relative would then have been sent as a consequence of a dream or series of dreams. We know from the *Dream Book* that the interpretation of dreams assumed considerable importance.⁴ The next two groups are ambiguous in that they can be read as *ht* and *ḥ(t)* instead of the reading adopted: *nwt wrt*. Since the latter phrase is represented elsewhere in the literature of letters to the dead, in the text of the Qâw bowl,⁵ I am inclined to read it here. From the point of view of the palaeography, one would expect dots or dashes filling the circle taken to be the *nwt* sign, and the form of the harpoon in *wr* lacks the usual downward curve on the right. The sign taken to be *nwt* is damaged, however, and there are forms of *wr* in the Old Kingdom without the downward curve.⁶ The expression *nwt wrt*, 'one city, a single (unique, solitary?) city, the same (?) city', must have reference to the world of the dead.⁷ Note the form *m* with a single aleph.⁸ I owe the suggestion that *hnc* be restored in the lacuna to Professor Baer. This would then be parallel to the text of the inside letter on the Qâw bowl: *mk sw hnc·k m nwt wrt* (*Letters to the Dead*, pl. ii, 8). I had originally conjectured that the text might have read [*s*]k^sk. There are two verbs *sksk*, one with the sense 'to destroy', and the other with a possible sense 'to be ill'.⁹ This reading would then yield, 'in a single city of destruction', or 'in a single city by reason of (my) illness'.¹⁰ Either reading would remove *sk* from the beginning of the next phrase. I have no doubt, however, that the true reading is *hnc*.

sk in is kd·f dr(?) sw ds·f. 'Indeed, it is his nature which damns (?) him himself.' I am uncertain as to the reading *dr*,¹¹ for it seems a curiously modern idea that one's

¹ Gardiner, *JEA* 16 (1930), 21.

² Id., *Hieratic Papyri in the British Museum*. Third Series, Vol. 1, 10, 21.

³ *Eg. Gram.*³, 513, under Sign List T 14, paragraph 3 (d).

⁴ On dream literature and dreams in the ancient Near East consult A. L. Oppenheim, *The Interpretation of Dreams in the Ancient Near East* (Philadelphia, 1956).

⁵ *Letters to the Dead*, pl. ii, cols. 8, 9.

⁶ *Berl. Hier. Pap.* III, pl. iv: Str. A 1, Be f, 1, 2; *Ann. Serv.* 25 (1925), pl. i, l. 6, facing p. 248.

⁷ *Letters to the Dead*, 4.

⁸ Cf. Edel, *Altäg. Gram.*, § 481.

⁹ *Wb.* IV, 319, 9. 14.

¹⁰ The use of the first person suffix may be suspect in view of the constant use of *b:k im*. The term *sksk* does not figure in von Deines and Westendorf, *Wb. der med. Texte*.

¹¹ The translation 'damn' is adopted from Faulkner, *Concise Dictionary*, 315.

character may destroy or damn one. The term *dr* is used in medical texts of driving away an illness.¹ There is a remote possibility that *kd·f* may refer to the Typhonian nature of Seni, although the dream, if Typhonian at all, seems to have been experienced by Ȝeni.

sk n ȝpr nis(?) ȝpr r·f n r n b:k im. 'There did not take place a summoning which happened to his disadvantage by the agency of this servant (the writer).' The traces in the lacuna may indicate the man with hand to mouth; they can hardly suit the negative *n*. The second occurrence of *ȝpr* may not be participial, and it is possible that we should read 'A summoning did not take place, but (nevertheless) it resulted to his disadvantage by the agency of this servant'.

n dr is pw n ȝprt nb. 'There is no end, indeed, of anything that may happen (?).' The particle *pw* appears to be written with an erasure separating the two signs. One expects *nbt* for *nb*, and this may indicate that the translation is incorrect. The meaning is not entirely evident. Does it mean, 'but difficulties indeed continued', or the like? Piankoff and Clère cite *dr* with the meaning 'obstacle',² and perhaps one can work out a sense along the lines of 'there was indeed no obstacle to anything which should (normally) happen'. Is *ȝprt* a *sdmt·fy* or *sdmt·f* form?

sk n ink is p: wdt st[?w? r·f?]; iw ir·n kw hr-ȝst b:k im. 'Lo, it was not indeed I who first caused his (?) discomfiture (?); for others acted before this servant.' The contrast between the two parts is clear. For *p:* with following infinitive, see Edel, *Altäg. Gram.*, § 903. It is difficult to restore the missing term following *wdt*. One should at least consider *st:w*, 'attempt at murder', or 'wounding, injury', respectively from *Wb.* IV, 355. 12 and 13, the first cited from the *Instruction of Ammenemes*, and the latter attested in the construction *wd st:w*. Compare also the *wd nkn*, 'do harm', cited in *Concise Dictionary*, 72, and the comments on *wdi* by Faulkner in *JEA* 45 (1959), 102–3. There is a *sts* cited in *Wb. der med. Texte*, 822–3, where the verb is used of the position of the patient during a medical examination, 'to lie stretched out (on one's back)'. The writer seems to be exonerating himself from any wrongdoing on his part against Seni, but he admits that he did take part, albeit not as the initiator, in inflicting some sort of harm. The suggested reading is *st:w·f* or *st:w r·f*, 'his discomfiture, un-doing, injury', 'injury to him', or else *sts·f*, 'that he lie on (his) back', although there might be room for a noun instead of the damaged suffix at the end of the damaged element. Edel cites *hr-ȝst* with a temporal sense in an adverbial usage.³

di hm s:wt nb[?]·f(?); im·f(?) sbgs w s:wt·f. 'Pray grant the protection (?) of his lord (?); may he not cause his protector (?) to be harmed.' The form Δ is a frequent imperative, (Edel preferring to read it as *im*),⁴ and the particle *hm* is well attested following the imperative.⁵ The term *s:wt* may be the 'watching-over' of *Wb.* III, 418. 5 (New Kingdom religious texts), although *s:w* in the Old Kingdom would be expected instead of *s:wt* (*Wb.* III, 416–17). The construction *im·f* with negational complement is discussed by Edel,⁶ and *sbgs w*, if thus correctly read, has the terminal *-w* characteristic of the form.⁷

¹ Von Deines and Westendorf, *Wb. der med. Texte*, 981–7.

³ *Altäg. Gram.*, in a passage quoted on p. 338.

⁶ *Ibid.*, § 1106.

² *JEA* 20 (1934), 160.

⁴ *Ibid.*, § 607.

⁵ *Ibid.*, §§ 839–40.

⁷ *Ibid.*, § 741.

Although *sbg*s is not attested in *Wb.*, it is clearly a causative of the *bgs* of *Wb.* II, 438. 4, with the meaning 'to injure', hence here 'to cause injury'. On *bgs*w, see Kuentz, *Griffith Studies*, 108.

r tmt·f mꜣꜣ bꜣk im r nhꜣ. 'That he may not see this servant ever <again>.' As mentioned earlier, it is conceivable that one or more missing columns separate this phrase from the preceding text on the recto; I consider this unlikely since there is no trace of ink to the left of col. 5 and since the method of folding reconstructed above suits the available amount of papyrus. The text suggests that Seni may well be deceased, that the writer is complaining of his malicious influence, and that Hēni is begging his dead father to prevent Seni from appearing in these dreams. For an example of *r tmt·f sdmw*, see Gardiner, *Eg. Gram.*³, § 408. The form *mꜣꜣ* is the expected form for the negatival complement (Gardiner, *Eg. Gram.*³, § 341). On the text of the recto the writer appears to complain that Seni has caused him, Hēni, to see himself or Seni in a dream. In this last phrase, on the verso, Hēni seems to wish that Seni may not see him, Hēni, ever again.

Three individuals appear in the letter: Meru, Hēni, and Seni. Their relations to each other can be ascertained with the aid of the representations in the tomb. Meru has the titles *iry pꜣt*, *hꜣty-ꜣ*, and *imy-r hm(w)-nꜣr*. He is clearly the owner of Nag' ed-Deir Tomb 3737 in which he is familiarly known as 'Iy the Long'. The letter-fragments were found within the tomb precincts.¹ The first of these titles is not represented in the preserved portions of the tomb, but the second and third are well represented there. In her careful discussion of the date of the tomb, Mrs. Peck suggests with some diffidence a date in the Ninth Dynasty.

Hēni I believe to be the writer. His name appears on the verso written upside down in relation to the rest of the text there, and his name is probably to be restored in *rt.* 1. He lacks any title or other indication of relationship, and he constantly refers to himself as *bꜣk im*, 'this servant'.² Gardiner notes the absence of specific identifications of relationship in the letters to the dead: 'The Chicago letter also displays that vague and allusive fashion of designating the *dramatis personae*, which is typical of the class. There can be no doubt but that the unnamed writer of the letter is addressing his dead father . . .'.³ In the paintings of tomb N 3737 there are two occurrences of the name Hēni:

smr wꜣty imy-r hꜣnty-š hm-nꜣr Hnꜣ sꜣ·f
Some Dec. Tombs, pl. xiii, p. 113;
 [sꜣ·f] *mry·f smr wꜣty hm-nꜣr H[nꜣ]*
Some Dec. Tombs, pl. xv, p. 117.




¹ Caroline Nestmann Peck, *Some Decorated Tombs*, 121. The fragments of papyrus were found 'just above' a pit in the courtyard of the tomb and hence may have been dispatched via this secondary burial. In view of this finding place it is just conceivable that we are dealing with a later generation, with Meru as the grandson of the tomb-owner and Hēni as the great-grandson of the original Meru. The tomb is cited by Schenkel, *Memphis, Herakleopolis, Theben*, § 227, where the titles of various individuals are listed.

² 'In formal letters to superiors and equals a fiction is established whereby the writer treats himself as the serf of the recipient.' James, *Hekanakhte Papers*, 128.

³ *JEA* 16 (1930), 20.

Thus there is every probability, and I would regard it as a certainty, that Ḥeni is Meru's son and is writing to his deceased father.¹ It seems likely to me also that Ḥeni is the father of the lady on a stela from Nag' ed-Deir.² On the stela the name is written with the same arrangement of signs as in our restoration in *rt.* 1, and he bears the titles *ḥsty-ꜥ*, *smr wꜥty*, and *ḥry ḥbt*. If the man is the same, he assumed the title of *ḥsty-ꜥ* after his father's death.³

Seni is mentioned in *rt.* 2 as *ḏt·k*, 'your (Meru's) *ḏt*-servant'. He is evidently the villain of the piece, that troubler of dreams about whom Ḥeni in desperation has undertaken to write to his deceased father. It is not entirely clear whether his malevolent influence, real or imagined, is effected from this world or the next. Possibly the dispute involved Meru's funerary service, for the *ḏt*-servant was in part thus occupied.⁴ From the tone of the letter Ḥeni seems apologetic for his own actions regarding Seni. By rare good fortune, I believe Seni can be further identified. On the west wall of the tomb a figure named Seni precedes a man leading a cow by a rope.⁵ This same wall, if my interpretation is correct, then shows all of our *dramatis personae*. Meru with his wife observes the agricultural life on his estate, and is approached by a dwarf leading a gazelle. Next in size to the owner and his wife is their son Ḥeni standing with staff and sceptre. To the right is a collection of offerings beneath which are the figures of Seni and another man. Here, at a presumably less troubled point in time, Meru, Ḥeni, and Seni are shown at peace on the productive estate. Of the ensuing troubles we know little except that Ḥeni lodges a letter of complaint to his father as a result of a bad dream which he has had.

One may point out several features which singly and collectively indicate that the letter is written in Old Egyptian in a hand which is close to the known examples of Möller's 'old hieratic'. The use of the non-enclitic particle *sk* at least twice (cols. 3 end, 4 end; perhaps col. 3 middle) is a construction typical of Old Egyptian. In the short letter from Saqqâra of the Sixth Dynasty it is used four times, twice introducing a principal sentence ('now . . .'), and twice used conjunctively ('and . . .').⁶ Other points of contact with Old Egyptian have been cited in the various references to Edel's grammar above. In terms of writings one may note the constant spelling  for *ḥpr*, well attested in the Old Kingdom.⁷ The usual Middle Kingdom spelling lacks the first two consonantal signs. Similarly, the writings  and  occur with con-

¹ Peck, *Some Decorated Tombs*, 121, describes the papyrus as 'a short letter to Meru, who was probably dead when it was written'.

² Lutz, *Egyptian Tomb Steles and Offering Stones*, pl. 15, no. 28; Dunham, *Naga-ed-Dêr Stelae*, 44, no. 31.

³ The identity of the two persons is not so certain that it can contribute to the question of whether *ḥsty-ꜥ* was an hereditary title. See Helck, *Zur Verwaltung*, 207 ff.

⁴ Bakir, *Slavery*, 37. References to the *ḏt*-servant, 'a term indicative of a status somewhere between that of a tenant and that of a serf', are collected by Hayes, *A Papyrus of the Late Middle Kingdom*, 30, n. 92. In P. Reisner III (unpublished) a distinction is made between the *ḥsbw*, 'the enrolled, the enlisted', and the *ḏt*.

⁵ Peck, *Some Decorated Tombs*, pl. xiii.

⁶ Gunn, *Ann. Serv.* 25 (1925), 250.

⁷ *Urk.* I, 22, 14; 38, 17; 39, 14; 60, 3; 61, 5; 61, 7; 63, 5; and *passim*. The fuller writings occur in Clère and Vandier, *Textes de la prem. pér. int.* I, §§ 16 and 23, but the shorter writing without the first two consonants written out is represented in the later text of § 32.

VERSO

RECTO

6

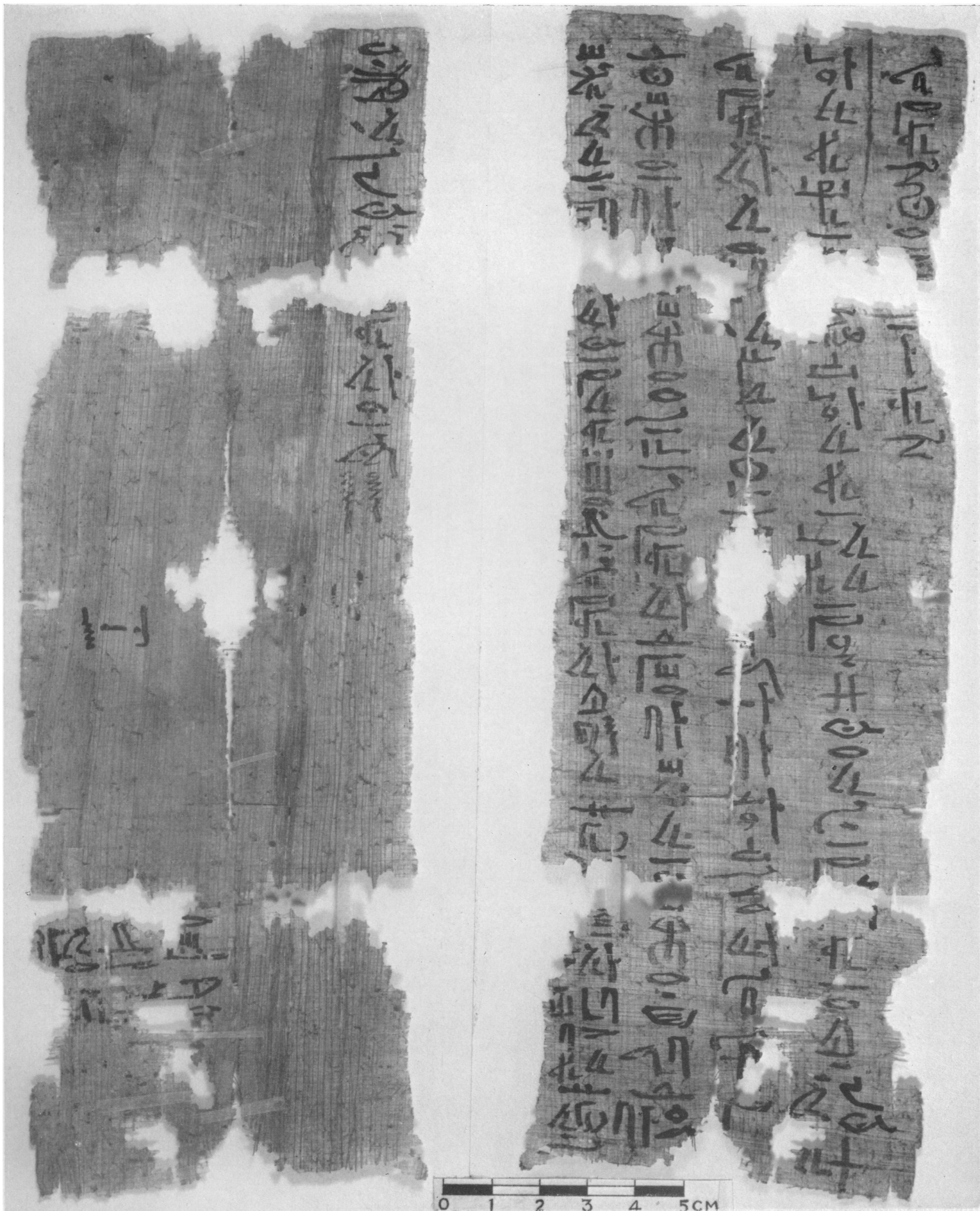
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4

3

2

1



A LETTER TO THE DEAD FROM NAG' ED-DEIR

considerable frequency in Old Kingdom hieratic texts,¹ whereas the sign Δ is absent from James's tables for the Ḥeḡanakhte and related documents, and is missing in Möller's columns under the headings of Hatnub, Illahun, Sinuhe, and Bulaq 18. In the hieratic texts of the early and later Middle Kingdom *rdi* appears in the form $\underline{\text{r}}\text{d}\text{i}$. This is a significant general rule.

The palaeography of the Boston letter is illustrated in the accompanying charts with the Gardiner Sign List number preceding the corresponding number for Möller's tables. Several signs are useful for dating purposes. Sign List A 1, the seated man, has the form typical of the Old Kingdom with the stroke at the top right not turning down to join the lap or legs, as in the Ḥeḡanakhte forms. Most characteristic of all is the forepart of the lion, Sign List F 4, a form well represented in the Old Kingdom and distinctive thereof.² Also characteristic of Old Kingdom hieratic is the *nḥ* bird of Sign List G 21 which has the exaggerated form of the tuft reaching way down below the normal base line. The form of the twisted wick of flax, Sign List V 28, with numerous twists at the top is likewise characteristic of the Old Kingdom; the form continues in the Ḥeḡanakhte papers with a final curve to the left at the base. Most notable of all is the use of the two forms of the basket with handle, Sign List V 31. In the term *b:k* the handle is here regularly on the left, as it is also in *mḥnkw·n·k* and *dt·k* in col. 2, *sk* in col. 3 (end), and *kw* in col. 5. Elsewhere the form has the handle on the right, the normal form in hieratic from the Old Kingdom onwards. Möller's tables show both forms in the Abusir and Elephantine columns, but the form with the handle to the left has disappeared as early as his Hatnub column, never to show up again. The letter to the dead on the Ḥu bowl, assigned to the First Intermediate Period,³ consistently uses the unusual form with the handle on the left. The Cairo Linen Document uses it once in a column where the editors suggest that it is used to avoid overlapping to the right.⁴ One could argue that the form is possibly a reflection of a semi-hieroglyphic style of writing; yet its implications for dating cannot be ignored, particularly in otherwise normal hieratic texts on papyrus.

The above considerations of language, writings, and palaeography, perhaps too heavily weighted in favour of an early date, nevertheless show closer ties to the end of the Old Kingdom, taking 2258 B.C. as the terminal date for the Sixth Dynasty, than to the reign of Sankhkarē Mentuhotpe at the end of the Eleventh Dynasty, taking 2001 B.C. as a representative date for the Ḥeḡanakhte papers. Hence Mrs. Peck's suggestion of the Ninth Dynasty (2160–2130 B.C.) for the tomb of Meru seems an eminently sensible solution. Slightly earlier or later dates are possible. It must be borne in mind that the palaeography exhibited by the Ḥeḡanakhte papers belongs to the very end of the Eleventh Dynasty, and is not typical for the early or middle part of the dynasty.

¹ P. Bulaq, 8; *Berl. Hier. Pap.* III, pls. 2 and 5, iv (left), 4 (twice) and 7, etc. Compare the writing with the conical loaf (Sign List X 8) in *Textes de la prem. pér. int.* 1, § 7, with the writing with the mouth and arm (Sign List D 21, 36) in § 14 and § 20.

² *Berl. Hier. Pap.* III, pl. ii, cols. 1, 7, and 11.

³ *Letters to the Dead*, 5, describes the cemetery as composed of shallow graves of Dyns. VI–VII, and pit tombs of Dyns. VI–XI.

⁴ *Letters to the Dead*, 5, n. 3.

Postscript

During the course of the discussion and commentary above, several alternative ways of translating certain passages have been presented. The following tentative translation embodies many of these alternatives, some of which have been rejected as less likely from our translation above. The alternative version is less literal, more coherent, and in consequence less reliable.

Alternative Translation

Recto: (1) A servant speaks before his lord; his [son] Ḥeni says: (2) A cry, many times! May the cry of the one whom you have favoured be effective in respect of these things which your *dt*-servant Seni does, (namely) in causing that this servant see him (3) in a dream in the same city [with] you.

Is it his nature which removed him himself? (4) A summoning did not take place which happened to his detriment through this servant, and there was no obstacle because of anything which might come to pass.

It was not I indeed (5) who first inflicted [his] injury(?). Others had acted before this servant.

Grant, pray, that his [ill-will?] be guarded, and do not let him allow his guardian(?) to be hurt.

Verso: (6) so that he may never again see this [servant].










































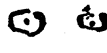
















[Addressee]: The prince and count, the overseer of priests, Meru

[Sender]: Ḥeni

A 1 33			G 1 192		
A 2 35			G 17 196		
A 47 47			G 21 229		
A 50 26			G 25 204		
C 7			G 29 208		
C 11 655			G 41 222		
D 2 80			G 43 200		
D 4 82			I 9 263		
D 21 91			I 10 250		
D 35 111			L 1 258		
D 36 99			M 17 282		
D 46 115			M 22 287		
D 58 124			M 23 289		
E 23 125			M 36 294		
F 4 146			N 17 317		



FIG. 2. Table of Hieratic Signs

N35 331			V 28 525		
N41 446			V 30 510		
O 34 366			V 31 511A		
O 49 339			V 31* 511B		
Q 3 388			W 11 395		
S 29 432			W 12 396		
R 8 547			W 24 495		
T 15 457			X 1 575		
T 21 461			X 8 569		
T 22 596			Y 2 538		
U 1 469			Aa 1 574		
U 15 489			Aa13- 14 327-8		
U 36 483			Aa28 488		
U 7 465			Z1 558		
V 13 528			Z2 562		

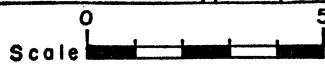


FIG. 3. Table of Hieratic Signs

DAS PRÄPOSITIONAL- UND ADVERBIALATTRIBUT DES ÄLTEREN ÄGYPTISCH, EINE APOKOINU-KONSTRUKTION

Von WOLFGANG SCHENKEL

DIE mittelägyptischen Konstruktionen, die man als Präpositional- und Adverbialattribute übersetzt und versteht, sind in ihrer Verwendung sehr viel eingeschränkter als etwa die entsprechenden Konstruktionen des Deutschen. Im Band 50 dieser Zeitschrift bin ich darauf zu sprechen gekommen¹ und möchte nun in dem folgenden Artikel die noch offenen Fragen weiter untersuchen.

Der Sachverhalt des Ägyptischen ergibt sich in aller Klarheit aus einem Vergleich mit den einzelnen Verwendungsweisen präpositionaler Attribute im Deutschen. Nach der Aufstellung H. Eggers'² gibt es hier der Entstehung und Anwendung nach drei Typen:

1. Genitiversatz durch 'von'.
2. Eine ursprünglich selbständige adverbiale Bestimmung wird einem Substantiv als Attribut zugeordnet (Gliederungsverschiebung). Z. B. 'Mein Haus steht an der Elbe' — 'Mein Haus an der Elbe steht nicht mehr'. — Dieser Typ ist älter als der Typ 1; er steht insbesondere bei Ortsbestimmungen.
3. Übernahme des Verbalbegriffs durch ableitende Wortbildung in eine andere Wortart. Was beim Verb als adverbiale Bestimmung steht, kann als präpositionales Attribut an das Verbalnomen gebunden werden. Z. B. 'glauben an Gott' — 'der Glaube an Gott'.

Das Ägyptische kennt mehr oder weniger genaue Korrelate des Typs 1 und 2; nicht aber den Typ 3. Das im *JEA* 50, 9–11, behandelte, nach Typ 3 konstruierte **sbꜣwt n hrdꜣw* 'die Lehre für die Kinder' ist durch diese Regel mit Sicherheit als unägyptisch erwiesen. Aber auch aus dem gut ägyptischen Typ 2 läßt sich eine solche Konstruktion nicht ableiten, da dieser Typ im Ägyptischen wie im Deutschen vorwiegend Ortsbestimmungen hervorbringt,³ kaum Besitzangaben oder ähnliches.

Bei genauerem Zusehen zeigt sich freilich, daß die ägyptischen und deutschen Konstruktionen des Typs 1 und 2 zwar im Ergebnis der Entwicklung gleich aussehen, ihrer Herkunft nach jedoch und somit in ihren Vorstufen charakteristisch anders sind.

Typ 1 entsteht im Laufe der ägyptischen Sprachgeschichte durch Erstarrung der

¹ *JEA* 50 (1964), 9–11.

² H. Eggers, 'Beobachtungen zum "präpositionalen Attribut" in der deutschen Sprache der Gegenwart', *Wirkendes Wort* 8 (1957/8), 257–67.

³ Die Präposition *m* ist die einzige, die geläufig verwendet wird; daneben *r* und *mj* (s. *JEA* 50, 10); auch lokales *hr* (s. Fischer, *Inscr. Coptite Nome*, nr. 43, z. 4; Fecht, *Persönl. Frömmigkeit*, 34 f., 15).

Nisbe *n.j.* Ansätze dazu mit einer Teilreduzierung der Formenbildung von *n.j.* reichen mindestens bis ins Mittlere Reich zurück. Im Neuägyptischen ist Typ 1 rein ausgeprägt. Er ist sprachgeschichtlich jünger als Typ 2.

Die ägyptische Entsprechung des Typs 2 leitet sich in der einfachsten Weise von sog. adverbialen Nominalsätzen her. *jmj-r mšc m t3 pn* 'Der Truppenführer ist in diesem Land' beispielsweise ist auf der Wortebene geradezu identisch mit einem *jmj-r mšc m t3 pn* 'der Truppenführer in diesem Land'. Die Sätze ohne (festes) Verbum finitum des Ägyptischen erreichen schon ohne Gliederungsverschiebung, was das Deutsche mit seinen Verbalsätzen in jedem Fall erst durch Gliederungsverschiebung gewinnt. Die Annahme einer im Ägyptischen ohnehin beliebten Apokoinu-Konstruktion¹ reicht aus, einen selbständigen Satz zum Korrelat des Typs 2 zu machen. Das Erstnomen/ 'Subjekt' des 'attributiven' sog. adverbialen Nominalsatzes ist dabei mit einem Substantiv eines anderen Satzes² identisch. So kann man verstehen:

dd·n b3k jm 'Es sagte der Diener' + 'Der Diener ist da'; oder besser: 'Es kam der Diener — ist da';

m33f t3 r dr·f 'Er sieht das Land' + 'Das Land reicht bis zu seiner Grenze'; oder besser: 'Er sieht das Land — reicht bis zu seiner Grenze';

jmj-r mšc m t3 pn 'Truppenführer — ist in diesem Land';

m3w m jtr·w, swr·tw·f 'Das Wasser — ist im Fluß —, trinkt man'.

Solche Apokoinu-Konstruktionen sind im Ägyptischen funktionsfähig. Sie können aber auch in einer einfachen Gliederungsverschiebung zu 'Attributen' umgedeutet werden, die dann mit dem deutschen Typ 2 genau übereinstimmen. Wird das gemeinsame Substantiv allein dem ersten Satz zugesprochen, so tritt an die Stelle der prädikativen Relation des zweiten Satzes eine subordinierende Relation, die man als 'adverbiale Relation'³ bezeichnen könnte. In deutscher Übersetzung läßt sich das etwa so darstellen: 'Es sagte der Diener — ist da' wird zu 'Es sagte der Diener — da'.

Daß diese Rekonstruktion der Realität entspricht, vor allem, daß zwei Stufen der Entwicklung anzusetzen sind, die sich der Geringfügigkeit des Schrittes entsprechend in der Sprachgeschichte nicht sehr scharf gegeneinander abheben, zeigen folgende Beobachtungen:

1. G. Fecht hat festgestellt, daß die als Präpositionalattribut verstandene Konstruktion sowohl in *einem* Kolon als auch in zwei Kola vorkommt.⁴ Als (apokoinu konstruierter) sog. adverbialer Nominalsatz müßte sie nach den Fechtschen Regeln⁵ zwei Kola haben. Mit *einem* Kolon könnte sie dann sehr gut die zum 'Attribut' verfestigte 'adverbiale Relation' sein; auch die einfachen adjektivischen 'Attribute' verhalten sich so.⁶ Es ergibt sich hieraus, daß die von G. Fecht notierte, mißliebige Ambivalenz der

¹ S. W. Schenkel, 'Beiträge zur mittelägyptischen Syntax, III: Sätze mit Pseudopartizip', *ZÄS* 92 (1965), 48–52, 67 f.

² Wobei 'Satz' auch das Einzelwort einer Liste, Überschrift o. ä. sein kann.

³ 'Adverbial' schließt wie im sog. adverbialen Nominalsatz Präposition + Substantiv mit ein.

⁴ G. Fecht, *ZÄS* 91 (1964), 34 f.; auch *MDIK* 19 (1963), 68 f. (Regel I, 5).

⁵ *ZÄS* 91, 33; *MDIK* 19, 67 (Regel G, 1).

⁶ *ZÄS* 91, 32; *MDIK* 19, 66 (Regel C, 1).

Konstruktion ein notwendiges Ergebnis der historischen Entwicklung ist und somit auch nie rein wird aufgelöst werden können.¹

2. Das Ägyptische hat in den von Präpositionen abgeleiteten Relationsnischen ausgesprochen 'adjektivische' Formen, die zur 'attributiven' Verwendung prädestiniert und in dieser Verwendung auch tatsächlich geläufig sind. Ein dauerhaftes Nebeneinander mit den sog. Präpositional- und Adverbialattributen ist nur denkbar, solange diese und die Relationsnischen sich inhaltlich ausreichend von einander abheben. Der geforderte Unterschied ist gegeben, wenn unsere Konstruktion nicht ebenfalls schlicht 'attributiv' ist, sondern eine (expressivere) Apokoinu-Konstruktion darstellt. Die Relationsnischen sterben im Laufe der Sprachgeschichte ab; schon im Neuägyptischen sind sie nur noch schwach vertreten. Ihre 'attributive' Funktion übernehmen die sog. Präpositional- und Adverbialattribute, denen man diese Funktion nun ohne weiteres zubilligen darf. Man wird in der Annahme nicht fehlgehen, daß sie bereits lange vor dem Rückgang der Relationsnischen in diese Rolle zu drängen beginnen; mutmaßlich doch schon seit dem Mittleren Reich.

3. Im Mittelägyptischen kann das Zweitnomen/'Objekt' nicht durch ein Präpositional- oder Adverbialattribut vom Erstnomen/'Subjekt' getrennt sein.² Die Regel ist leicht verständlich, wenn man es mit einem ganzen, in Apokoinu-Konstruktion zwischen beide Satzglieder eingeschobenen Satz zu tun hat, der den wesentlich in der Reihenfolge gegebenen Zusammenhang der Satzglieder verwischen könnte. Ein regelrechtes, etwa durch die Akzentuierung auch formal untergeordnetes 'Attribut' dagegen sollte sich wie jedes andere 'Attribut' einfügen lassen. Der im *JEA* 50 zitierte Beleg aus der Ptolemäerzeit,³ der nun gerade eine solche Konstruktion entgegen den 'klassischen' Regeln dazwischenschiebt, legt Zeugnis ab von dem späteren 'attributiven' Verständnis.

Die für das Ägyptische bis mindestens ins Mittelägyptische hinein anzusetzende Apokoinu-Konstruktion ist kaum weniger elegant und leichtfüßig, als die beliebten Präpositional- und Adverbialattribute des Deutschen es sind. Der Satz ohne (festes) Verbum finitum des Ägyptischen bietet ein Ausdrucksmittel, das in solcher Knappheit einem deutschen Satz abgeht. Etwas umständlicher und damit auch expressiver als die deutschen Konstruktionen werden sie freilich sowohl ihrer Akzentuierung als auch ihrer Opposition zu Nisbekonstruktionen nach schon sein.

Um die Apokoinu-Konstruktion für die ältere Zeit⁴ konsequent an die Stelle der sog. Präpositional- und Adverbialattribute zu setzen, ist die vermeintliche Konstruktion einiger Zahlwörter mit der Präposition *m* im älteren Ägyptisch aufzulösen. Als gewöhnliche Konstruktion von *h* 'tausend' gilt die häufig belegte mit *m*; bei *hh* 'Million' ist die Konstruktion neben der mit *nj* ebenfalls möglich. Nun ist bemerkenswert, daß bei 'tausend' die *m*-Konstruktion wohl überhaupt nur in den bekannten Opferformeln⁵ vorkommt, auch bei 'Million' nur in kultischem Kontext ('eine Million *sd*-Feste'). In

¹ Im Mittelägyptischen wird man, worauf auch die weiteren Argumente hinweisen, immer zunächst einmal mit beiden Möglichkeiten rechnen müssen.

² S. meinen Artikel im *JEA* 50, 9 f. Die Behauptung wäre noch weiter zu erproben.

³ Philensis I, 6 (*Urk.* II, 205, 3 f.).

⁴ Mindestens das ganze Alte Reich.

⁵ Z. B. *h* *m* *t* 'tausend Brote'.

allen übrigen Zusammenhängen steht, so weit die Belege reichen, *n·j*. So sagt man z. B. nur *ḥ·j n·j zḫ* 'tausendmal',¹ nicht **ḥ·j m zḫ*; ebenso *ḥḥ n·j zḫ* 'millionenmal',² nicht **ḥḥ m zḫ*. Desgleichen *ḥ·k pn n·j rnp·t* 'diese deine tausend Jahre',³ nicht **ḥ·k pn m rnp·t*; *ḥḥ(·w) n·j/n·jw rnp·(w)t* 'tausend(e von) Jahre(n)',⁴ nicht **ḥḥ(·w) m rnp·(w)t*. Weiter: *ḥ·j n·j jš·wt·f ds·f* 'tausend seiner eigenen Dinge';⁵ *ḥ·j n·j smd·t* 'tausend Grüße (o. ä.)'.⁶ Die Belege mit *m* weichen also von der Norm ab. Die Opferformeln insbesondere sind trotz ihres häufigen Vorkommens nur ein Sonderfall abseits des gewöhnlichen Sprachgebrauchs.

Die Opferformeln sind uraltes Gut; sie stammen aus einer Zeit, in der die 'Tausend' noch keine recht bestimmte, vielmehr eine ungeheuer große Zahl bezeichnete, wie später dann die 'Million'. Eine solche Zahl zunächst einfach für sich zu nennen, ohne Angabe des Gezählten, hat bereits einen Sinn, der unseren leichten Zahlen dieser Größenordnung ziemlich abgeht. Man kann zusätzlich daran erinnern, daß die Ägypter die Zahlen mehr als selbständige Größen behandeln als wir, indem sie ihnen, nicht unbedingt dem Gezählten, Possessivsuffixe, Demonstrativa und gar selbst 'Adjektive' zufügen.⁷ So sehe ich kein ernsthaftes Hindernis, die *m*-Konstruktion der Opferformeln als Apokoinu-Konstruktion zu lesen. So etwa: *ḥmsj jr ḥ·j m t ḥ·j m ḥnk·t* 'Setz dich zu dem Tausender — (er) ist Brot; zu dem Tausender — (er) ist Bier!'⁸ 'Ein Opfer, das der König gibt: *ḥ·k m rn n·j m·ḥd* dein Tausend — (es) ist (ein) Gazellenjunges'.⁹ So dann auch in den gängigen Opferformeln: 'Eine Opfergabe (*pr·t-ḥrw*): *ḥ·j m t* usw. ein Tausender — ist Brot usw.'. In allen Fällen, in denen *ḥ·j* nicht Satzteil eines anderen Satzes ist, oder zwar Satzteil sein kann, aber in einer Liste steht, die diesen Zusammenhang lockert, steht nichts dagegen, die Apokoinu-Konstruktion beiseite zu lassen und einen selbständigen sog. adverbialen Nominalsatz zu lesen. Z. B. 'Ihr sollt sprechen: *ḥ·j m t* usw. ein Tausender sei Brot usw.!' Bei allen Belegen außer den ältesten ist dann noch in Rechnung zu stellen, daß man es mit einer geprägten Formel zu tun hat, der man auch ein anderes Verständnis unterschieben kann; so könnte, nachdem die Formel verfestigt war, *ḥ·j m* ohne weiteres in diesem Kontext als Äquivalent des gewöhnlichen *ḥ·j n·j* verstanden worden sein.

Entsprechend kann man die 'Million *sd*-Feste' konstruieren, die mit dem jüngeren Wort für eine ungeheuer große Zahl, *ḥḥ*, gebildet ist: (*r*)*dj·n·j n·k jr·t ḥḥ m sd* 'Ich habe dir nun gegeben, zu machen eine Million — ist *sd*-Feste'.¹⁰

Daß die Konstruktion von *ḥḥ* und die von *ḥ·j* in der Opferformel im Zuge der allgemeinen Entwicklung dann später in ein weniger expressives 'Attribut' umgedeutet worden sein könnte (sagen wir: etwa im Mittleren oder Neuen Reich), ist wahrscheinlich; vorausgesetzt, daß die Formeln überhaupt noch mit der lebenden Sprache in

¹ Z. B. *Ḥekanakhte Papers*, I, vs. 6, 15.

² *Urk.* I, 180, 2; *Letters to the Dead*, I, 1; *Urk.* I, 61, 13; *Urk.* I, 185, 17 (s. Edel, *AäG*, § 399); *Ḥekanakhte Papers*, I, vs. 6, 15; III, 1; XII, 2; XVII, 2; vgl. *Wb.* III, 436, 7.

³ *Urk.* IV, 539, 5 (alter Text); *Urk.* IV, 1058, 15.

⁴ *Siût* IV, 22; *Ḥekanakhte Papers*, xx, B 6; *Urk.* IV, 306, 7 (alter Text).

⁵ *Deir el Gebrâwi*, I, pl. xxiii, Mitte, links.

⁷ Vgl. Edel, *AäG*, §§ 400–2.

⁶ *Ḥekanakhte Papers*, IV, 1.

⁸ *Pyr.* 214 b.

⁹ *Pyr.* 806 c; vgl. weiter *Pyr.* 1332 b; 2006 b–c; 2027; 2194 b (s. K. Sethe, *Von Zahlen und Zahlworten*, 47).

¹⁰ *Urk.* I, 115, 1; ähnlich *Urk.* IV, 303, 17–304, 1 (alter Text).

Verbindung gebracht wurden. Von Hause aus lagen jedenfalls keine 'Attribute' vor. Die allgemein anerkannte Konstruktion von *h* und *hh* mit *m* beruht auf einer Täuschung der Grammatiker durch die erdrückende Masse der Opferformeln.

Schließlich noch ein Wort zu *wj·w m* mit folgendem pluralischem Ausdruck. Ein Beispiel wie *wj·w m n; n·j jr* 'einer von diesen Eseln'¹ läßt sich ebenso gut als Apokoinu-Konstruktion verstehen wie als Präpositionalattribut: 'einer — war von/bei/unter diesen Eseln — tat dies und das'; im Gegenteil würde die stärker absetzende Apokoinu-Konstruktion noch besser zu einer ruhigen, 'umständlichen', Einzelzüge auskostenden Erzählung passen. Da man sich hier schon im Mittleren Reich befindet, stehen wohl beide Deutungsmöglichkeiten zunächst einmal offen.

Die Deutung der Präpositional- und Adverbialattribute als Apokoinu-Konstruktion mag noch manche Einzelheit der Syntax berühren;² ich möchte hier zum Schluß nur das Konglomerat der sog. Adverbien hervorziehen, das ohnedies eine Durchleuchtung nötig hätte.

Die Gruppe der sog. Adverbien, wie sie mit schwankender Umgrenzung in den Grammatiken bestimmt wird,³ ist recht disparat. Sie umfaßt Ausdrücke verschiedener Bauart und Funktion. Streichen läßt sich von vornherein die Untergruppe der mit Präpositionen zusammengesetzten Ausdrücke. Denn entweder werden sie als zusammengesetzt verstanden, dann sind sie Syntagmen, keine Wortart; oder aber sie gelten als einfache Wörter, dann sind sie in sich nicht weiter zu analysieren. Die überwiegende Mehrzahl fristet ihr Dasein nur im Hinblick auf die Übersetzung in eine moderne Sprache. Streicht man weiter die sicheren 'absoluten Substantive' sowie die Negation *n* (dazu auch unten) und läßt ein paar gelegentliche Besonderheiten einmal unbeachtet, so zerfällt der Rest der sog. Adverbien in zwei bedeutende Klassen:

- (A) solche, die als 'Prädikat' eines sog. adverbialen Nominalsatzes stehen können;
- (B) solche, die nicht in dieser Position vorkommen.

Die Wörter der Klasse A sind syntaktisch gleichwertig mit Präposition + Substantiv; diese und nur diese als eigene Funktionswortart 'Adverb' zusammenzufassen, hat in dem Variantenverhältnis seine gute Begründung. Aus Apokoinu-Konstruktionen mit solchen Adverbien entwickeln sich, wie oben gesagt, die Adverbialattribute, genauso wie aus solchen mit Präposition + Substantiv die Präpositionalattribute. Die sehr kurze Liste dieser Adverbien besteht nur aus Ortsadverbien: *jm*, *r* und wenigen seltener belegten.

Die Wörter der Klasse B stehen:

1. weitgehend wie Präposition + Substantiv, nur eben nicht als 'adverbiales Prädikat', darüber hinaus aber noch in Spitzenstellung im Satz (ohne Wiederaufnahme); oder besser: sie stehen wesentlich wie 'absolute Substantive';

¹ Bauer B I, 9; s. weiter Gardiner, *Eg. Gr.*³, § 262.

² So die geläufige Datierung 'Jahr soundsoviel unter NN.', die, jedenfalls in der älteren Zeit, am besten in zwei selbständige Syntagmen zu zerlegen ist: 'Jahr soundsoviel. Unter NN.'; damit steht eine Verteilung der beiden Angaben auf zwei Schreibzeilen in Einklang, vgl. Hayes, *Late M. K. Pap.*, pl. XIII (vs. Text A, 3-4).

³ S. Edel, *AäG*, §§ 750-4; Erman, *ÄG*⁴, §§ 438-40; Gardiner, *Eg. Gr.*³, § 205; Lefebvre, *Gramm.*², §§ 535-47; Westendorf, *Gramm. med. Texte*, §§ 332-4.

2. als Satelliten eines sog. Adjektivs (vor allem oder ausschließlich *wr·t* 'sehr').

Der zweite Unterfall bleibt der einzige Beleg für einen 'adverbialen' Satelliten eines Nomens im älteren Ägyptisch, nachdem die Satelliten des Substantivs auf Apokoinu-Konstruktionen zurückgeführt sind. Es liegt deshalb nahe, die Konstruktion von *wr·t* anders zu verstehen, zumal eine akzeptable Lösung bereits vorbereitet ist. Von den sog. Adjektiven stehen weder die Nisben noch das Wort *nb* als Nuklei des Satelliten *wr·t*, sondern nur solche, die sich, worauf ich hier nicht eingehen möchte, zu Partizipien stempeln lassen. *wr·t* dürfte ein Nomen oder spezieller sogar: ein Substantiv sein.¹ Ein Partizip kann verbaler Nukleus sein und daher zweifellos ein 'absolutes Substantiv' als Satelliten haben. *wr·t* ist wohl ein solches 'absolutes Substantiv', und damit sind die 'adverbialen' Satelliten des älteren Ägyptisch restlos beseitigt. Daß sich die Funktion von *wr·t* später nach der Umdeutung der Apokoinu-Konstruktion in Präpositional- und Adverbialattribute zu der eines 'Adverbs' hin verschoben haben könnte, ist sehr gut möglich.

Auch die sog. Adverbien des ersten Unterfalls wird man, worauf ihre den bekannten 'absoluten Substantiven' entsprechende Funktion hinweist, ebenfalls samt und sonders als Nomina oder genauer: Substantive einstufen; auch die Morphologie zeigt in zahlreichen Fällen in diese Richtung.² Gelegentlich liegt der Ansatz als 'absolutes Substantiv' auch aus anderem Grund auf der Hand; wenn man z. B. statt *mjn* 'heute' auch *m mjn* sagt, so liegt das auf der gleichen Linie wie *mj·tt* 'ebenso' neben *m mj·tt* oder *tr n·j rwhs* 'zur Zeit des Abends' neben *r tr·f* 'zu seiner Zeit'.

'Absolute Substantive' stehen manchen Grammatiken zufolge nur³ oder fast nur⁴ als Zeitangaben. In Wirklichkeit gelten sie auch für Angaben der Art und Weise (z. B. *mj·tt*). Sind die sog. Adverbien des ersten Unterfalls 'absolute Substantive', so erweitert sich ihr Bereich erheblich: zur Ortsangabe s. Westendorf, *Gramm. med. Texte*, § 151 c (jedenfalls das zweite Beispiel); Edel, *AäG*, § 754. Das Gros der neuen Belege sind Angaben der Art und Weise, einige auch Zeitangaben.

Diejenigen der sog. Adverbien, die weder zur Funktionswortart Adverb gehören, die man aber auch nicht als 'absolute Substantive' ansetzen kann, gehören in das große Reservoir der 'Satzadverbien', so z. B. die Negation *n*. Eine genaue Abgrenzung zwischen 'absoluten Substantiven' und 'Satzadverbien' wäre auszuarbeiten.

Nachtrag

Die Ableitung der Präpositionalattribute aus apokoinu konstruierten Sätzen berührt sich eng mit den Darlegungen J. Spiegels, *ZÄS* 71 (1935), 74–80, über eine durch die Präposition *m* eingeführte Apposition und ihren Zusammenhang mit sog. Nominalsätzen; zur Konstruktion des Zahlwortes *h3* vgl. Spiegel, S. 76. Von Interesse ist ferner ein Vergleich mit den Verhältnissen semitischer Sprachen; dazu vgl. J. Aro, 'Präpositionale Verbindungen des Nomens im Akkadischen', *Orientalia*, NS 32 (1963), 395–406.

¹ S. Westendorf, *Gramm. med. Texte*, § 333, 2.

² S. insbesondere die Schlußfolgerungen Westendorfs, *Gramm. med. Texte*, § 332.

³ Edel, *AäG* § 303; Gardiner, *Eg. Gr.*³, § 88; Lefebvre, *Gramm.*², § 132.

⁴ Erman, *ÄG*⁴, § 208 (die Belege selbst aber auch alle nur Zeitangaben).

OBSERVATIONS ON THE DENTAL PATHOLOGY SEEN IN ANCIENT EGYPTIAN SKULLS

By F. FILCE LEEK

UNTIL recently it was popularly thought that the diseases of teeth and other oral tissues were of very recent origin, an affliction only of modern society. They were believed to be the outcome of some of the so-called benefits of modern civilization, caused by the eating of refined foods.

It is now recognized that examples of caries, the most prevalent disease of the teeth, existed in skulls of much earlier populations, and there is growing evidence that caries was not so uncommon even as far back as the upper Palaeolithic times.

TABLE I.¹ *Frequencies of dental caries in some earlier human populations
(Adults only)*

<i>Approx. time span</i>	<i>Series</i>	<i>Author</i>	<i>No. of teeth examined</i>	<i>No. with caries</i>	<i>% caries</i>	
70000-35000 B.C.	European Neanderthal	Brothwell	259	0	0	
35000-10000 B.C.	M. Carmel (Skhul) and European Upper Palaeolithic groups	Brothwell	523	5	1.0	
	Rhodesian Man	Carter (1928) Brothwell (1959)	13	11		
10000-3000 B.C.	European and North African Mesolithic	Péquart, Boule, and Vallois (1937); Brothwell	1,148	88	7.7	
3000-1000 B.C.	French Neolithic	Hartweg (1945)	11,717	379	3.2	
	German Neolithic	Brinch and Møller-Christensen (1949)	1,589	27	1.8	
	Swedish Neolithic	Holmer and Maunsbach (1956)	6,402	91	1.4	
	Danish Neolithic	Christoffersen and Pedersen (1939)	3,612	56	1.6	
	British Neolithic	Brothwell (1962)	1,151	36	3.1	
	Predynastic Egyptian	Brothwell and Wood	1,742	40	2.3	
	Greece (3000-1000 B.C.)	Robinson and Carr				
	Crete (1750-1550 B.C.)	Angel (1944)	1,404	116	12.1	
	China (1766-1122 B.C.)	Carr (1960)	1,498	135	9.0	
	Total (3000-1000 B.C.)	Mao and Yen (1959)	884	38	4.3	
			29,999	918	3.1	

¹ Reproduced from D. R. Brothwell, 'The Macroscopic Dental Pathology of some Earlier Human Populations', in *Dental Anthropology* (Oxford, 1963), 271-8.

Egyptologists have long since reported on a common condition seen in predynastic and dynastic skulls, that of attrition. This has been correctly attributed to the grittiness of the diet, due possibly to the lack of water for cleaning purposes during several months of the year, but more probably due to the methods used for grinding the corn with which to make bread.

One of the three main types of occlusion (the relationship of the upper and lower teeth when the jaws are closed) is a vital predisposing cause of attrition. In Class III cases, in which the teeth bite edge to edge, a type commonly seen in negroid races, the wear of the biting surfaces is most pronounced and cases are frequent even today, with modern refined diet, in which most of the crowns of the teeth have been almost worn away. Another factor which influences the rate of destruction occurs after attrition has commenced. On the biting surface of the tooth islands of hard enamel appear surrounded by areas of soft dentine, and when such teeth come into contact during mastication, the softer dentine is worn more quickly, thus accelerating the rate of destruction.

Although Ruffer and others¹ have all commented on more severe abnormalities to be seen in dynastic skulls, it has been generally accepted that the dental tissues of the Ancient Egyptians show very few abnormalities. This may be due to the fact that only the best material is exhibited in museums, the teeth shown being well calcified and presenting an unbroken line, all regularly shaped and evenly spaced. At the same time there is no reference to suffering from dental pain to be seen in any of the representations in tombs which provide so much of our knowledge of the daily life of the people.

There is the classic exception of the findings of Elliot-Smith and Warren Dawson² in the skull of the mummy of the Eighteenth-dynasty Pharaoh, Amenophis III, which is in the Cairo Museum and shows a number of apical abscesses and other dental abnormalities. This example suggests that if such conditions existed amongst the ruling classes, who might be expected to have enjoyed a more carefully prepared diet than the workers, the same results of deterioration of the dental tissues might also be found amongst the common people.

Junker and others,³ from the study of texts, have produced evidence to show that the profession of the physician was recognized as early as the times of the Old Kingdom and that there were specialized 'toothists' in the same era. To Hesy-Rē, who lived in the reign of King Djoser, is accorded the honour of being the first illustrious member of the latter calling on the basis of a title of uncertain significance on one of the wooden panels from his tomb at Saqqâra.

¹ See Sir Marc. A. Ruffer, *Studies in the Palaeopathology of Egypt* (Chicago, 1921), 268-321; W. B. Emery, *A Funerary Repast in an Egyptian tomb of the Archaic Period* (Leiden, 1962), 7-8; G. Elliot-Smith, 'Incidence of dental disease in Ancient Egypt' in *Brit. Med. J.* 1932, 2, 760; D. E. Derry, 'Incidence of dental disease in Ancient Egypt' in *Brit. Med. J.* 1933, 1, 112; R. W. Leigh, 'Notes on the Somatology and Pathology of Ancient Egypt' in *American Arch. & Eth.* 34, no. 1 (Un. Col. Press), 19-32.

² See Elliot-Smith and Warren Dawson, *Egyptian Mummies* (London, 1924), 95.

³ See H. Junker, 'Die Stele des Hofarztes Iny' in *ZÄS* 63 (1927), 53-70; H. Ranke, 'Medicine and surgery in Ancient Egypt' in *Bull. Inst. Hist. Med.* 1 (1933), 237-57; B. W. Weinberger, *History of Dentistry*, vol. 1 (St. Louis, 1948), 62-67.

This evidence of a dental profession is supported by Junker's discovery in a tomb at Gîza of two lower molars linked together by gold wire and dated by him *c.* 2500 B.C.¹ Magical incantations for the relief of dental pains appear many times in the medical papyri, but there is very little else to support the contention that any form of dentistry was ever carried out.

Evidence of the practice of dentistry by the Etruscans in the latter half of the first millennium has been produced by Brothwell and Carr,² and it is possible that their inspiration was derived from a more developed civilization, possibly that of Egypt.

In one of the rear corridors in the Graeco-Roman temple at Kôm Ombo there are carvings on two blocks of stone which, it has been suggested, illustrate surgical instruments,³ but in its present condition many interpretations of the sculpture can be made. These carvings, however, have been well illustrated and described by Ghalioungui who suggests that they represent a votive offering of the foundation instruments which at that time it was customary to place beneath the foundations of a temple.⁴ Whilst this temple was commenced during the second century B.C. it was only completed at the end of the second century A.D. and is therefore at the very end of the period under consideration.

It has been shown that what has previously been accepted as evidence of surgical intervention for the cure of an apical abscess in a Fourth-dynasty mandible can no longer be regarded as valid,⁵ and neither fillings nor cavities cut in a carious tooth have ever been photographed and recorded.

One of the difficulties which beset an inquiry into the dental condition of the Ancient Egyptians is the scarcity of the material and the fact that it is housed in widely separated collections. In order to obtain an overall picture it is essential to examine as many dried skulls as possible and to make as much use of X-ray photographs as is practical.⁶ One can then correlate the pathological changes found and their causes, and see whether at any time during the course of the pathological disturbance human intervention, other than the use of magic spells, had taken place in order to alleviate the pain and discomfort attending on dental disorders.

Some time ago the author was able to examine in the Cairo Museum a number of skulls from Nubia and Egypt which were dated to the last few dynasties. These showed the common pictures of well-calcified teeth but with the occlusal surfaces of the molar teeth worn down by attrition, and in a number of instances this had so damaged the dental pulp that it had died, with a resulting apical abscess. Sinuses led from the infected area out through the outer or inner alveolar plate and as a result of this, there would have been a discharge of pus through the gingival tissue which would have relieved all the acute symptoms of pain.

¹ Cf. Junker, *Gîza*, 1, 256 and pl. 40c.

² See D. R. Brothwell and H. G. Carr, 'The Dental Health of the Etruscans' in *Brit. Den. J.* 113, no. 6, 207-10.

³ See A. Badawi, *Kom-Ombo. Sanctuaires* (Cairo, 1952), 41.

⁴ See P. Ghalioungui, *Magic and Medical Science in Ancient Egypt* (London, 1963), 101-3.

⁵ Cf. F. Filce Leek, 'Oral Surgery in the Old Kingdom. An Appraisal', in Brothwell and Sanderson (eds.), *Diseases in Antiquity* (in press, Illinois, 1966).

⁶ [For an examination of the teeth of the body found in Tomb 55 of the Valley of the Kings, see pp. 108-9 below. Ed.]

Inquiries were continued on skulls at the British Museum (Natural History), when a surprising proportion of dental abnormalities was seen. These included cases which showed extreme attrition, caries, teeth lost ante-mortem, fracture of the mandible and subsequent healing, abscesses, odontogenic and non-odontogenic cysts, irregular teeth, teeth crowded out of the arch, retained deciduous teeth, impacted lower third molars, and a buried inverted lower third molar.

It has been possible to undertake further investigations of Egyptian material which had originally been part of the Biometric Laboratory collection, University College, London, built up by Professor Karl Pearson. Unfortunately some of the material suffered damage during the Second World War, and this made it impossible to see the complete dental sample in certain instances, yet, as in most cases so much could be seen and charted, the inferences derived from the facts obtained can be regarded as conclusive.

During these investigations it became known to me that extensive work had already been undertaken on some aspects of early Egyptian dental pathology, and it is now planned to integrate all the data collected into a single detailed analysis of the dental health of early Egyptian populations (D. R. Brothwell, H. G. Carr, F. Filce Leek, and C. Wood Robinson). For this reason it would be out of place here to enter into a full consideration of the results. Using selected data, however, it is easy to indicate the extent of the dental troubles of these earlier peoples.

TABLE II. *Teeth present*

<i>Number of</i>	<i>Hierakonpolis</i>				
	<i>Pre- and Early Dynastic</i>	<i>Tarkhân Dyn. I and II</i>	<i>Qâw Dyn. IV-XVIII</i>	<i>Sidmant Dyn. IX</i>	<i>Qurna Dyn. XI</i>
Maxillae	95	114	128	48	48
Mandibles	27	32	129	42	33
Possible number of teeth	2,056	2,302	4,112	1,440	1,296
Teeth present	518	1,150	2,320	479	349
Teeth lost ante-mortem	137	129	269	39	49
Percentage lost ante-mortem	6.7%	5.6%	6.5%	2.8%	3.8%

Table II gives an indication of the amount of material available for some of these series. The difference between the number of maxillae and mandibles is due to the mandibles periodically becoming separated from the skull and one meeting a different fate from the other.

The difference between the possible number of teeth and the number of teeth actually present is due to the fact that in the dried specimen, teeth, especially those with straight or conical roots, become loose in their sockets and are frequently lost during excavation and removal to the laboratory.

From some jaws a single tooth had been lost ante-mortem, in some several, and in others many. Sometimes it is possible to see an osteoporitic condition of the surrounding bone indicating that the sepsis from the tooth had passed into the neighbouring structures. These would gradually soften and become absorbed until the tooth

became so loose that it would be easily dislodged from its socket. Where the tooth socket had healed normally, as in a few cases, no clue could be gained as to the method by which the tooth had been lost.

TABLE III. *Attrition and consequent pathology*

	<i>Hierakonpolis</i>	<i>Tarkhân</i>	<i>Qâw</i>	<i>Sidmant</i>	<i>Qurna</i>
Number of teeth	518	1,150	2,320	479	349
Number of teeth showing attrition	385	1,003	2,184	422	326
Percentage	74%	87%	94%	90%	93%
Pulp exposed by attrition	12	16	41	17	23
Abscess due to attrition	12	20	113	31	25

All teeth (except the newly erupted ones) from the material of every dynasty examined exhibit attrition of the cusps. This attrition increases with the age of the individual and all stages are to be seen from slight wearing of the cusps to almost complete loss of the crown structure. In the normal progress of attrition a protective deposition of secondary dentine is laid down, which safeguards the pulp. When the attrition is faster than the secondary dentine deposition the pulp is exposed, becomes septic, and finally dies. This is followed by an infection of the surrounding tissues and acute or chronic abscess formation. Although in Table III only abscesses due to attrition are included, some cysts, periodontal abscesses, and cases of osteomyelitis, can be directly attributed to the attrition.

TABLE IV. *Caries and consequent pathology*

	<i>Hierakonpolis</i>	<i>Tarkhân</i>	<i>Qâw</i>	<i>Sidmant</i>	<i>Qurna</i>
Single surfaced cavity	7	8	23	1	
Double surfaced cavity		8	25	1	3
Apical abscess		8	5	2	

At the present time, caries is responsible for most of the pathology seen in the mouth, but this was far less frequently the case among the Ancient Egyptians (Table IV). The cavities are usually single surfaced, but sometimes the caries progressed and reached the pulp and caused its death. This was followed by the inevitable apical and bony changes. It is interesting to note that the pathological changes which follow excessive attrition and excessive caries are identical. In both cases they are due to the same cause, namely death of the pulp.

TABLE V. *Periodontal and alveolar conditions*

<i>Area of alveolar destruction</i>	<i>Hierakonpolis</i>	<i>Tarkhân</i>	<i>Qâw</i>	<i>Sidmant</i>	<i>Qurna</i>
Buccal	33	28	46	11	10
Labial	1	8	3		
Periodontal abscess	12	3	7		

The bony structure surrounding the teeth is usually extremely strong, but in a number of instances there is a marked recession of the outer and inner alveolar plates. This gives rise to the condition commonly known today as pyorrhea. Only those cases have been noted where the bone loss has passed the bifurcation of the molar roots. In a number of instances the infection has given rise to abscess formation, whilst in others it was clearly seen that it was the unequal wear of the cuspal surfaces by attrition that gave rise to lateral pressure during mastication, and this induced alveolar changes.

TABLE VI. *Absence and Impaction*

	<i>Hierakonpolis</i>	<i>Tarkhân</i>	<i>Qâw</i>	<i>Sidmant</i>	<i>Qurna</i>
Absent	8	6	12	5	4
$\frac{8}{8} / \frac{8}{8}$					
Lower third molar impaction					

The normal growth of the jaws in almost every instance was quite adequate to accommodate the full number of teeth, and in no instance were impactions of third molars to be seen. A surprising number of cases, however, were noted in adult jaws in which no lower third molar tooth or teeth were present. There was no evidence that they had been removed (in many cases the inaccessibility would have prevented the successful conclusion of such an operation), and it was concluded that these teeth were congenitally absent. X-ray evidence confirmed these findings.

TABLE VII. *Osteopathology*

	<i>Hierakonpolis</i>	<i>Tarkhân</i>	<i>Qâw</i>	<i>Sidmant</i>	<i>Qurna</i>
Cysts	7	..	14	8	3
Osteomyelitis	16	11	10	4	1

When a dental abscess arises, providing there is adequate drainage, the result is a very localized osteomyelitis of the alveolar bone. In a number of cases the reaction by the surrounding tissues is to wall off the infection, and this eventually gives rise to a cyst, whilst in other cases, when the infection becomes more acute, it progresses to the surrounding cancellous bone with a resulting osteomyelitis.

Conclusions

Whilst the calcification and growth of the teeth and jaws of the Ancient Egyptians can be considered to be generally normal yet they show all the abnormalities to be seen in the dentitions of modern man.

It is, however, attrition and not caries which is the greatest cause of the pathological disturbances. In many cases this results in the death of the pulp, and the consequent formation of acute and chronic abscesses, cysts, and even widespread osteomyelitis.

In no instance was any interruption of the pathological sequence noted, and it is concluded that no human intervention other than the use of magical incantations took place for the relief of dental disease. It cannot be completely discounted, however, that a method of removing teeth was known.¹

¹ I would like to acknowledge with gratitude the inspiration and guidance given by D. R. Brothwell of the British Museum (Natural History) in the preparation of the paper. I must also thank Dr. Kenneth Oakley for permission to examine the Ancient Egyptian skulls housed in the British Museum (Natural History), and Dr. Trevor of the Faculty of Archaeology and Anthropology, Cambridge, for permission to examine skulls in the Karl Pearson collection. All the specimens illustrated on plates X-XII come from this collection, and the datings are those assigned by Pearson.

To Dr. David Hughes, formerly of Cambridge, and now of the National Museum of Canada, Ottawa, the author is indebted for much valuable assistance, and to Mr. L. Morley of the Museum of Archaeology and Ethnology, Cambridge, for the preparation of the photographs.



1. Widespread destruction of bone surrounding roots of first permanent molar. Causative factor was death of pulp of tooth through caries. *Qâw. Dyn. IV-XVIII*



2. Large cystic cavity extending from distal side of first upper left molar to posterior end of maxilla is visible; size c. 19 mm. \times 18 mm. \times 14 mm. On distal buccal sides of second premolar and first molar is a deposit of salivary calculus. *Abydos. Dyn. I*



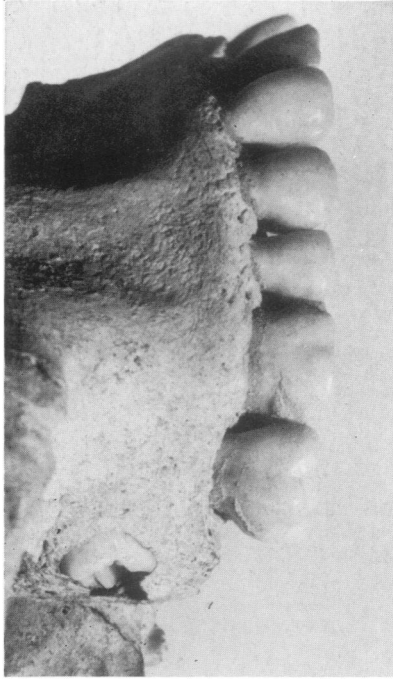
3. Extensive bone loss around apices of first permanent molar due to abscessed condition following death of pulp by attrition; also results of periodontal abscess on buccal side of anterior root of second molar. This could be due to spread of infection from anterior tooth or to infection caused by food packing because of irregular space between two molar teeth. *Hierakonpolis. Predynastic-Early Dynastic*



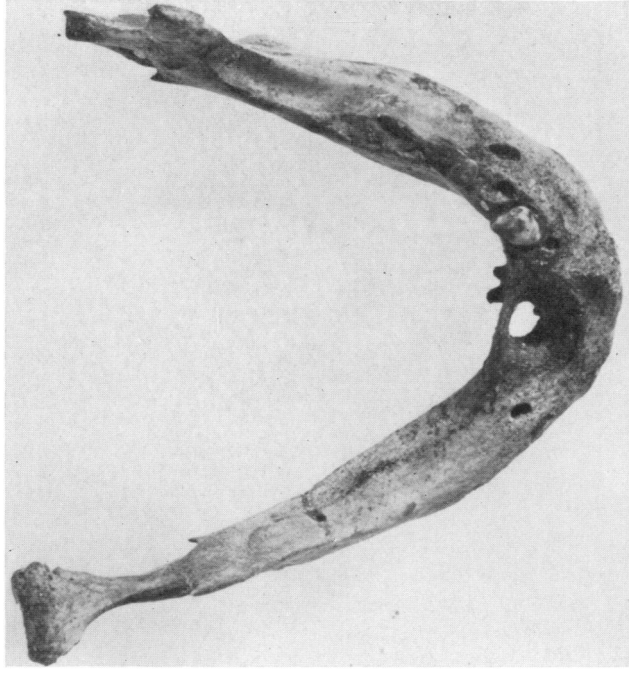
4. Widespread destruction of bone over first premolar root passing right through outer alveolar plate to palatal surface of maxilla. Similar destruction of bone over anterior apex of first molar root. In both cases death of pulp due to attrition. *Sidmant. Dyn. IX*



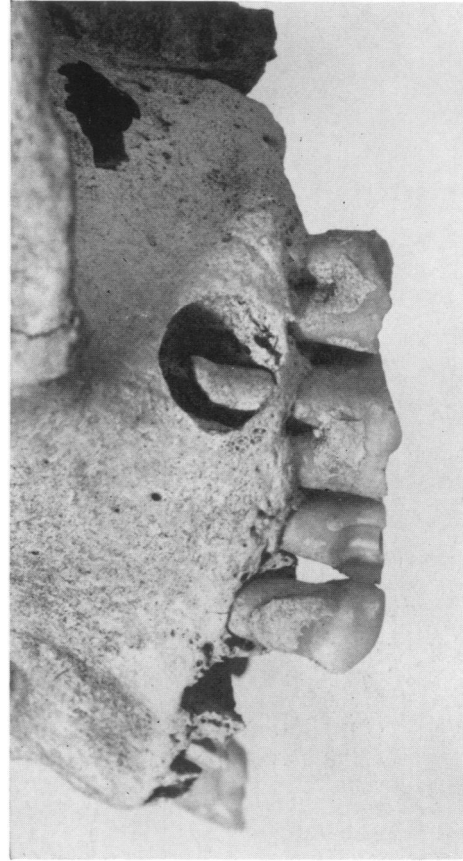
1. Bone loss around second molar could be caused by a periodontal abscess, but as everywhere else the alveolar bone is dense and unabsorbed, destruction most probably caused by an apical abscess. There is an unusual swelling below the mandibular notch. *Qāw. Dyn. IV-XVIII*



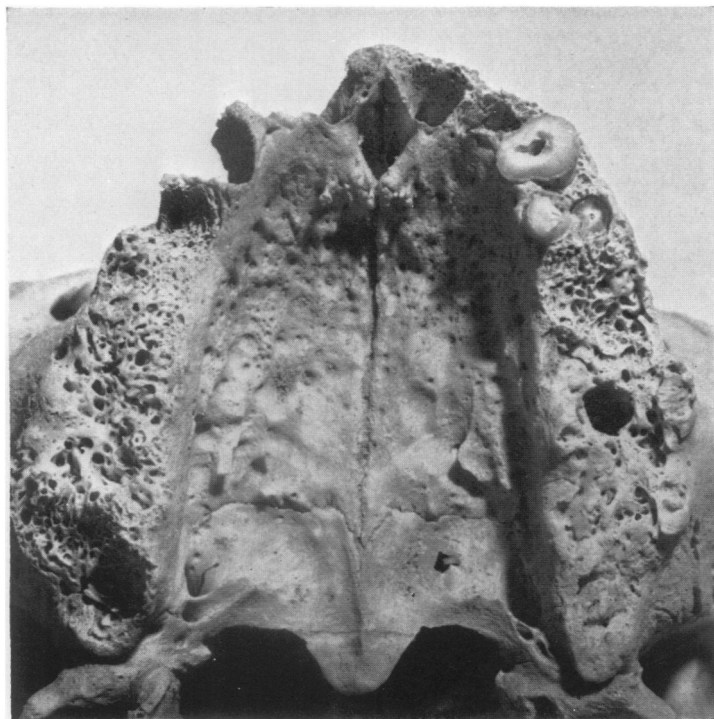
2. An upper right third molar erupting almost horizontally through posterior part of the tuberosity. *Tarkhân. Dyn. I-II*



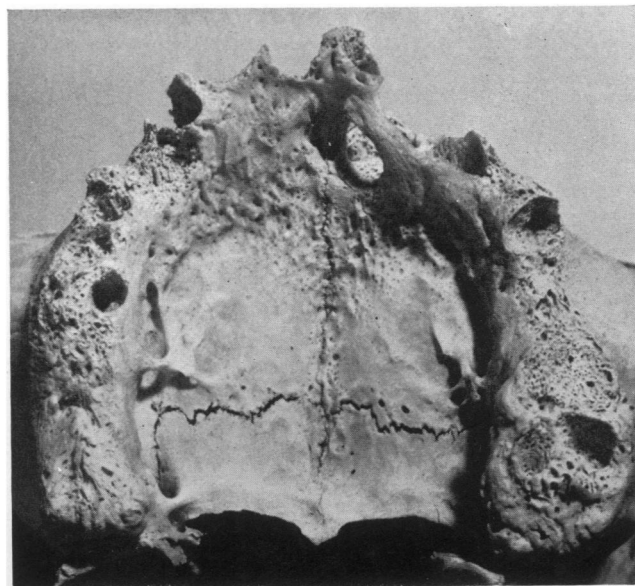
4. Edentulous mandible except for lower left canine that has turned labial-lingually, and a premolar tooth with pulp-chamber exposed by attrition. Alveolar ridges have been extensively absorbed by sepsis. There is marked destruction of bone in right incisor area resulting in a sinus penetrating the lower lingual border of mandible. *Abydos. Dyn. I*



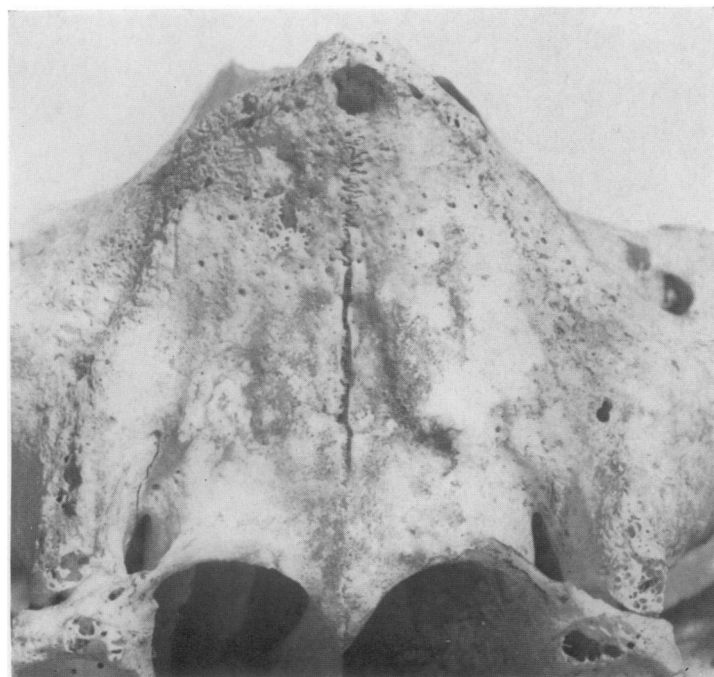
3. On buccal sides of posterior teeth excessive salivary calculus is seen. Posterior buccal root of first molar is standing in large cystic cavity. Death of pulp caused by attrition. Note almost circular orifice in buccal plate caused by cyst. *Tarkhân. Dyn. I-II*



1. Four incisor teeth have been lost post-mortem. Porous condition of alveolar ridges in both posterior regions of maxilla show the results of extensive osteomyelitis. *Sidmant. Dyn. IX*



2. Two incisor teeth were present at death, but maxillary bone shows extensive destruction due to osteomyelitis. In upper left lateral region, a bridge of bone below large cavity extends from palatal to labial side of maxilla. In region of first and second molars, curious bone bridges to be seen joining alveolar ridge and palate. Note also bone of palate and its continuation, the horizontal part of palatine bone, both of which are of tissue-paper thickness. *Sidmant. Dyn. IX*



3. Edentulous maxilla showing complete absorption of alveolar ridges. So much bone has been lost that the area previously occupied by the ridges and the palate now form almost a flat surface *Qáv. Dyn. IV-XVIII*



4. A maxilla showing results of a cyst formed above root of upper first premolar, the pulp of which died as a result of attrition. The cavity penetrates both buccal and palatal plates. *Sidmant. Dyn. IX*

THE TALBOTYPE APPLIED TO HIEROGLYPHICS

By RICARDO A. CAMINOS

'On the reader's capacity hangs the fate of books.'

TERENTIANUS MAURUS, *De litteris syllabis pedibus et metris*, 1286 (ed. Santen & Lennep, p. 57).

THE title of this article is borrowed from a slender Egyptological brochure which appeared in London in 1846, and to the production of which Samuel Birch, Joseph Bonomi, George Robins Gliddon, Anthony Charles Harris, and William Henry Fox Talbot contributed in various ways. The subject-matter of the brochure is that stately rock-stela of King Sethos I and his Nubian viceroy Amenemope which, prior to its complete and perennial submersion under the waters of Lake Nasser in July 1965, overlooked the Nile from a lofty cliff in the district of Ibrîm in Egyptian Nubia, on the east bank of the river, some 240 kilometres south of Aswân and 50 kilometres north of the celebrated temples of Abû Simbel.¹ The brochure contains the earliest published copy and translation of the stela in question, and although both copy and translation have in the nature of things been superseded, the brochure itself yet remains, for reasons which will be made apparent below, an item of no small antiquarian interest. It is, moreover, by no means readily accessible, and seeing that reprints, which are everywhere the order of the day, are not wholly unprecedented in this *Journal*, it has been thought fitting to republish it here, supplemented with a few remarks and essential notes, mostly of a bibliographical nature.²

On pls. XIII–XV below *The Talbotype applied to Hieroglyphics* is reproduced in its entirety and in facsimile, though on a slightly reduced scale. The original publication consists of three leaves which measure 22·5 cm. by 18·5 cm. each, written upon lengthways, on one side only, in a neat copperplate hand. It is not the work of a regular printing-press, but was produced by the early photographic method originally known as calotype,³ and subsequently styled, after its inventor, talbotype. And this is precisely what makes this little publication truly priceless because, having come out of Talbot's short-lived calotype workshop at Reading, it ranks among the incunabula of photography.

The Englishman William Henry Fox Talbot was born in 1800 and died in 1877. He was a country gentleman of independent means, a man of catholic interests and

¹ Porter and Moss, *Top. Bibl.* vii, 94 (top); also Hintze, *ZÄS* 87, 31 ff. with pl. 3; Caminos, *Shrines and Rock-inscriptions of Ibrîm*, pls. 39 and 40.

² Much information and many references have herein been included which, we feel sure, all Egyptologists are thoroughly conversant with, and which it would be only right to condemn as redundant in a strictly Egyptological paper. The subject-matter of the article makes us think, however, that it might prove of interest to students beyond our little circle, and it is for their sake that such information and references are given.

³ From *καλός*, 'beautiful', because of the beauty of the results. The term was coined by Talbot, who wrote he hoped that when the photographs obtained by his method became known, the term would not be found to have been misapplied. See Talbot, *Philosophical Magazine and Journal of Science*, Ser. 3, vol. 19, 89.

encyclopaedic knowledge who, as an active researcher, made his mark in such varied fields of scientific endeavour as theoretical physics, chemistry, mathematics, photography, linguistics, and archaeology—late in his life he turned, not unsuccessfully, to the translation of cuneiform texts.¹ His principal claim to fame lies, however, in his being one of the founders of photography, and it is one of his major contributions to it, the above-mentioned calotype or talbotype process which must now be described, however briefly, for it is pertinent to the matter in hand.²

The daguerreotype was an image on solid metal, a direct picture, a unique positive which could not be used for the production of copies. This limitation was brilliantly overcome by Talbot's invention of the calotype process in September 1840. It was the first negative-positive photographic process. By means of it a 'negative' was obtained, namely, an image which was the exact reverse, as far as light and shade were concerned, of the original object, and which could be used for the making of a virtually unlimited number of 'positives' or true copies—this being the principle on which all modern photographic techniques are based. Operationally the calotype process consisted of exposing to light in a camera a piece of writing-paper previously rendered sensitive by chemical treatment. In a few moments of exposure this paper would receive an impression or image which, being invisible but susceptible of being made to appear by adequate chemical handling, was aptly termed latent image. When removed from the camera after exposure the calotype paper was, then, apparently blank, and the latent image which it contained was gradually developed or made visible by washing it with a solution of gallic acid. In the resulting image the lights and shadows were reversed with respect to the actual object; in other words, it was a paper negative, the transparency of which could be increased by waxing, and from which any desired number of true copies or positive images could be obtained by simple contact printing upon another sheet of sensitized paper, as is still done today. Calotype made thus feasible, among other things and for the first time, the mass printing and publication of photographs.

Talbot patented his invention in February 1841, and in the autumn of 1843 set up a photographic printing establishment at 55 Baker Street, Reading, with a view to the commercial exploitation of his discovery on a large scale; he seems to have realized from the start its vast potentials, particularly with respect to book illustration. It was in fact there at Reading that the talbotypes or calotypes of the world's earliest photographically illustrated books were produced. The first of such books was one by

¹ Boase in *Dict. of Nat. Biogr.* XIX, 339 ff.; Cull, *Trans. Soc. Bibl. Arch.* 6, 543 ff.; Johnston, *Photographic Journal*, 87 (Section A), 3 ff.; Newhall, *Modern Photography*, 16, 86 ff. and 17, 66 ff.; idem, *Image*, 8, 60 ff.

² The calotype process, which was a great improvement on Talbot's previous method of photogenic drawing, has often been described. For the inventor's *ipsissima verba* see Talbot, *Abstracts of the Papers printed in the Philosophical Transactions of the Royal Society of London* (= *Proceedings of the Royal Society*), 4, 312 ff.; idem, *Philosophical Magazine and Journal of Science*, Ser. 3, vol. 19, 88 ff. and 164 ff.; plus Talbot's account appended to Tissandier, *History and Handbook of Photography*³, 359 ff. The process is also discussed in Eder, *Geschichte der Photographie*³, 242 ff.; Lécuyer, *Histoire de la photographie*, 49 ff.; Gernsheim, *History of Photography*, 68 ff.; id. in Singer, Holmyard, Holl & Williams (eds.), *Hist. of Technology*, v, 722 ff.; Newhall, *History of Photography*, 32 ff.; Thomas, *The First Negatives*, 9. For additional references see Boni, *Photographic Literature*, 31 (s.v. Calotype), 258 ff. (s.v. Talbot and Talbotype).

Talbot himself, *The Pencil of Nature* (London, 1844),¹ which contained 24 calotypes. The next was *Sun Pictures in Scotland* (London, 1845), also by Talbot; it had 23 calotypes but no letterpress except for a printed title-page and list of plates. Likewise from the Reading establishment—where they were completed shortly before it closed down in the latter part of 1847—were the photographs accompanying the first edition of William Stirling's *Annals of the Artists of Spain* (London, 1848); they formed a supplementary (fourth) volume of 66 talbotypes of engravings of paintings.² These are memorable titles, milestones in the history of book production that are quoted here because *The Talbotype applied to Hieroglyphics*, consisting as it does of three calotypes made at the Reading workshop in August 1846,³ is, in its own modest way, near of kin to them. These publications were issued in extremely limited editions. It is known that only 25 copies were produced of the supplementary volume of illustrations of Stirling's *Annals*. Of the other books there is no precise record, but an idea may be formed from the fact that of *The Pencil of Nature* 24 copies have been traced, while *Sun Pictures in Scotland* is a much scarcer book, and *The Talbotype applied to Hieroglyphics* the scarcest of them all. The British Museum copy⁴ was destroyed in the course of World War II, and that which Gliddon presented to the library of the Royal Society of Literature of the United Kingdom⁵ has long vanished from its shelves. The only extant copy we know of is Talbot's own copy. Lumped together with numerous other calotypes, sundry papers, and instruments of his, it lay for ninety years, probably unrecognized for what it was, or even altogether unseen, at Lacock Abbey, the inventor's country seat in Wiltshire. In 1937 his granddaughter Miss Matilda Talbot, C.B.E., presented to the Science Museum in South Kensington a large portion of that invaluable material. Included in her gift was the hieroglyphic brochure. It has since been kept in the Photographic Collection of the said institution,⁶ and from it have been prepared the half-tone reproductions on pls. XIII–XV.⁷ The possibility that other copies have survived cannot, of course, be ruled out. Be that as it may, as one of the rarest publications issued by

¹ Although the book came out in six parts between June 1844 and April 1846, the imprint on the cover of each part is 1844. In 1959 it was reprinted in full in *Image*, 8, 75 ff.

² On the Reading printing establishment and its output see Gernsheim, *History of Photography*, 125 ff.; Thomas, *The First Negatives*, 31 f.

³ August 1846 is the date of Gliddon's statement at the beginning of the brochure. The brochure must have been produced at Reading the same month and distributed forthwith, for on Saturday 5 September 1846 a London weekly commented on it circumstantially, and clearly referred to it as being already available; cf. the excerpt from *The Literary Gazette* on p. 68, n. 1 below.

⁴ British Museum, *General Catalogue of Printed Books*, xx, col. 736 (s.v. Birch); cx, col. 686 (s.v. Ibrim).

⁵ Announcement in *Proceedings of the Royal Society of Literature*, vol. 1, no. 17, 239.

⁶ Science Museum, London, Photographic Collection; the item bears no museum number yet.

⁷ *A tout seigneur tout honneur*. It was Mr. T. G. H. James who located the Science Museum copy. Aware that even after a long and fruitless search for the Talbot publication I still kept up a dogged interest in it, he kindly communicated his finding to me at once and supplied me with a xerograph of the brochure. Dr. David B. Thomas, Assistant Keeper in the Department of Chemistry, Science Museum, and author of an admirable monograph on Talbot entitled *The First Negatives* (London, 1964), was instrumental in securing the three photographs which illustrate this article and which are published by courtesy of the Science Museum, London. To the authorities of the Science Museum, to Dr. Thomas, and to my ever-obliging friend Mr. James, I herewith tender my heartfelt thanks. Miss Sara DeLuca and Professor Harry Glenn Brown, research librarians at Brown University, have been kind and efficient to a fault in answering my countless bibliographical inquiries and requests, and I am deeply grateful to them too.

Talbot's pioneering establishment at Reading the brochure is likely to remain a prized collector's item. For Egyptologists it holds, at all events, a special significance. Photography plays today an ancillary, yet enormously important, role in our discipline, and *The Talbotype applied to Hieroglyphics* is, without doubt, the very first instance of the use of photography in the field of Egyptology.

Not much need be said about the contents of the brochure; they may upon the whole be trusted to explain themselves.

Gliddon's opening statement lists the contributors and defines the part played by each of them. His own role may technically be regarded as that of editor. A contemporary London weekly begins a circumstantial account of the brochure in the following terms: '*Talbotype applied to Hieroglyphics*. — For an important specimen of this *art* (from a design by Mr. Bonomi), we are indebted to Mr. G. R. Gliddon, the most distinguished of American explorers of the antiquities of Egypt, previous to his leaving England (last week) for his native land.¹ One would almost expect him to have subsidized publication. It is at all events to Gliddon's credit that, probably mistrustful of his own powers of deciphering, he handed over to Birch for study the copy of the Ibrîm stela which his friend Anthony Charles Harris² had forwarded him from Egypt.

Harris's covering letter was dated from Alexandria on May 21, 1846. An excerpt from it on p. 3 of the brochure tells of his chancing upon the stela and recording it in December 1845. Thinking he had made a discovery, Harris carved his name and the date 'at the bottom of the Tablet in token of possession, and because it may thus be identified'.³ As a matter of fact the stela had previously been not merely spotted but also copied *in toto* twice: first by William John Bankes, either in 1815 or in 1819, and then again by Robert Hay, either in 1824 or in 1827.⁴ These two renowned early travellers never published their records, however, and not until 1951 were scholars interested in these matters aware that Bankes and Hay had ever had any business at all with the Ibrîm stela.⁵ It was therefore an understandable error on Harris's part to assume that he was the first European ever to see the monument in modern times.

¹ *The Literary Gazette and Journal of Belles Lettres, Arts, Sciences, &c.*, vol. 30, no. 1546 (London, Sept. 5, 1846), 771. 'Native land' is an error, for Gliddon was born in Devonshire in 1809. He appears to have been of American nationality, however. For nearly twenty-three years he resided in Egypt, on the antiquities whereof he subsequently lectured to vast audiences in the United States. He was a successful *vulgarisateur*. His book *Ancient Egypt* is said to have been sold in at least 24,000 copies. He died in 1857. Unlike Talbot and the other contributors to the Calotype brochure, whose names turn up constantly in our literature even now, Gliddon is a figure long fallen into oblivion. Information on him will be found in Wilson and Fiske (eds.), *Appletons' Cyclopaedia of American Biography*, II, 665; Caroline Ransom Williams, *Bull. Met. Mus. of Art*, Ser. I, vol. 15, 88 f.; idem, *New-York Hist. Soc. Quarterly Bulletin*, 4, 6 ff. On his reputation in France and England see De Saulcy, *Revue des Deux Mondes*, Per. I, Ser. 5, vol. 14 (1846), 989, and the *Literary Gazette* excerpt given above.

² London-born merchant of Alexandria, dealer in Egyptian antiquities and competent antiquarian. His vast collection, which included many important papyri, was purchased after his death (1869) by the British Museum. Cf. Dawson, *JEA* 35, 161 ff.

³ In the autumn of 1961 we found the legend A. C. HARRIS · 1845 neatly cut in capital block letters immediately beneath Sethos I's cartouches in the bottom horizontal hieroglyphic line of the stela; the graffito affected neither the text nor even the bottom frame-line.

⁴ Caminos, *Shrines and Rock-inscriptions of Ibrîm*, Part I, Exploration and recording (in press).

⁵ That they had concerned themselves with the stela was first disclosed, in print at least, in Porter and Moss, *Top. Bibl.* VII, 94, which appeared in 1951.

Harris's original design has not been found and can only be judged from the fair copy of it executed by Joseph Bonomi¹ and reproduced on p. 1 of the brochure. The drawing, as published, is a complete record of the monument, for it comprises the damaged scene at the top, the long royal inscription in twelve horizontal lines beneath it, and the viceroy's shorter vertical text and figure on the left corner. Faults can of course be found with the figures and the hieroglyphs, but they are neither so serious nor so numerous as to make the text incomprehensible or the record as a whole useless; nor will the reader fail to observe that Harris's verbal description (on p. 3) of the mutilated 'pictorial representation which heads the Tablet' is flawless. To anyone acquainted with the original monument one thing is, above all, quite evident, and that is that the drawing represents an earnest effort to achieve a faithful reproduction of the stela in every respect. Harris must have been well aware of the fundamental importance of full, exact facsimile recording, and Bonomi must have known that the accuracy and general quality of a facsimile are heightened by neat and fine presentation. Many of us have yet to grasp these principles.²

Considering the state of Egyptian philology at the time, Birch's³ attempt at coping with the difficulties of the text is very creditable.⁴

Harris's copy of the Ibrim stela and Birch's work on it were made known to scholars by means other than the talbotype brochure. At a meeting of the Royal Society of Literature held in London on June 11, 1846, exactly three weeks after Harris had dispatched his drawing from Alexandria, Birch communicated it to the company and briefly explained that the inscription recorded 'an act of adoration from the Ethiopian⁵ prince Amenemoph, in the reign of Sethos I'. At the next meeting of the Society a fortnight later 'Mr. Birch read a paper, by himself, upon the early relations of Ethiopia⁶ under the Egyptian monarchy, especially in connexion with Mr. Harris's tablet discovered at Ibrim'; the paper included a partial translation of the stela. Notice of these

¹ Bonomi, who was then 50, had travelled extensively in Egypt, Nubia, Syria, and Palestine. He had been a member of the great Prussian epigraphic expedition under Richard Lepsius. He was an accurate and painstaking draughtsman and had an admirable hieroglyphic hand. He became Curator of Sir John Soane's Museum in 1861, and died while still in office in 1878. See Simpson, *Transactions Soc. Biblical Archaeology*, 6, 560 ff.; S. Lane-Poole in *Dictionary of National Biography*, 11, 826.

² We seize this opportunity of stating our agreement with Cyril Aldred's 'gloomy prediction' in *JEA* 51, 199. Egyptian epigraphy which is concerned, or ought to be concerned, with inscriptions *and* scenes, for these are inseparable, is at a sad low ebb today. Accurate facsimile copying, fine draughtsmanship, and neatness of presentation are at a discount; it is sheer incompetence on our part that has led to this state of affairs, for at heart we still value sound recording.

³ Samuel Birch (1813-85) was then Assistant Keeper of the Department of Antiquities, British Museum. He was one of the leading Egyptologists of his day in England, and his scholarship extended to other areas of antiquity; he was also something of a sinologist. See Budge, *Transactions Soc. Biblical Archaeology*, 9, 1 ff.; and Dawson, *Who was Who in Egyptology*, 16, with additional references.

⁴ We cannot escape the feeling that he came back to this text and revised his translation years later. In his *Egypt from the Earliest Times to B.C. 300* (London, 1875), 118 f., reference is made to an inscription of Sethos I at Sesebi in Upper Nubia in which that king's dominion is said to have reached 'on the South the arms of the winds, and on the North the Great Sea'. We suspect that this quotation is from ll. 11-12 of the Ibrim stela and that Sesebi is a slip of the pen for Ibrim.

⁵ That is, Nubian or Cushite; see next footnote.

⁶ By Ethiopia Birch meant the country that is now generally designated as Nubia or Cush in our literature; some writers still use the term Ethiopia for Nubia or Cush, but only occasionally. See Dixon, *JEA* 50, 123.

activities as well as a good summary of Birch's paper were printed in the *Proceedings of the Royal Society of Literature*, vol. 1, no. 16, 235 and 236 ff.; and also in *The Literary Gazette and Journal of Belles Lettres, Arts, Sciences, &c.*, vol. 30, no. 1535 (June 20, 1846), 562, and no. 1540 (July 25, 1846), 669 with n.*. Furthermore, as pointed out above, on September 5, 1846, shortly after the appearance of *The Talbotype applied to Hieroglyphics* the London *Literary Gazette* carried an article under the same title which was a sort of abridged version in type of the autographed brochure; it lacked the drawing, but described verbally the damaged scene at the top of the stela and reproduced Birch's translation *in extenso*. Thus in a few months the bibliography of the monument had grown to six items. Admittedly, of the talbotype brochure very few copies must have been printed, no doubt; yet the work was not completely lost sight of, at least for some time, for we find it mentioned in a book, a pamphlet, and an article, all three published in London in 1886.¹ It should also be noted that the *Literary Gazette* was a weekly that had a very large audience, and was subscribed to by many libraries in the United Kingdom, on the Continent, and in America.

Proliferation and diffusion did not, however, prevent this early work of Birch and others on the Ibrim monument from being entirely overlooked by later scholars. Thus it came about that Archibald Henry Sayce laid claim to having discovered the stela while travelling in Nubia in the winter of 1893-4; he did not even atone for his error by producing a decent copy of it. Breasted relied exclusively on Sayce's inferior publication for his treatment of the stela in *Ancient Records of Egypt* (1906), which indicates that he was equally unaware of the existence of *The Talbotype applied to Hieroglyphics* either as a separate calotype brochure or as a regularly printed article.²

That little work which Gliddon promoted in 1846 was good, and worthy of a better fate. It was lame scholarship on the part of those for whose sake it had been produced that let it slip into oblivion. Terentianus Maurus put it tersely: *Pro captu lectoris habent sua fata libelli*.

¹ Ibrahim-Hilmy, *Literature of Egypt and the Soudan*, 1, 75 (s.v. Birch), 318 (s.v. Ibrim); Walter de Gray Birch, *Biographical Notices of Dr. Samuel Birch*, 92; Budge, *Transactions Soc. Biblical Archaeology*, 9 (Part 1, Dec. 1886), 26 (54). Note also the statement in Gliddon, *Otia Aegyptiaca. Discourses on Egyptian Archaeology*, 143, published in London in 1849.

² Sayce, *Rec. trav.* 16, 169 ff.; Breasted, *Ancient Records*, III, 56 n. c; 89, n. b.

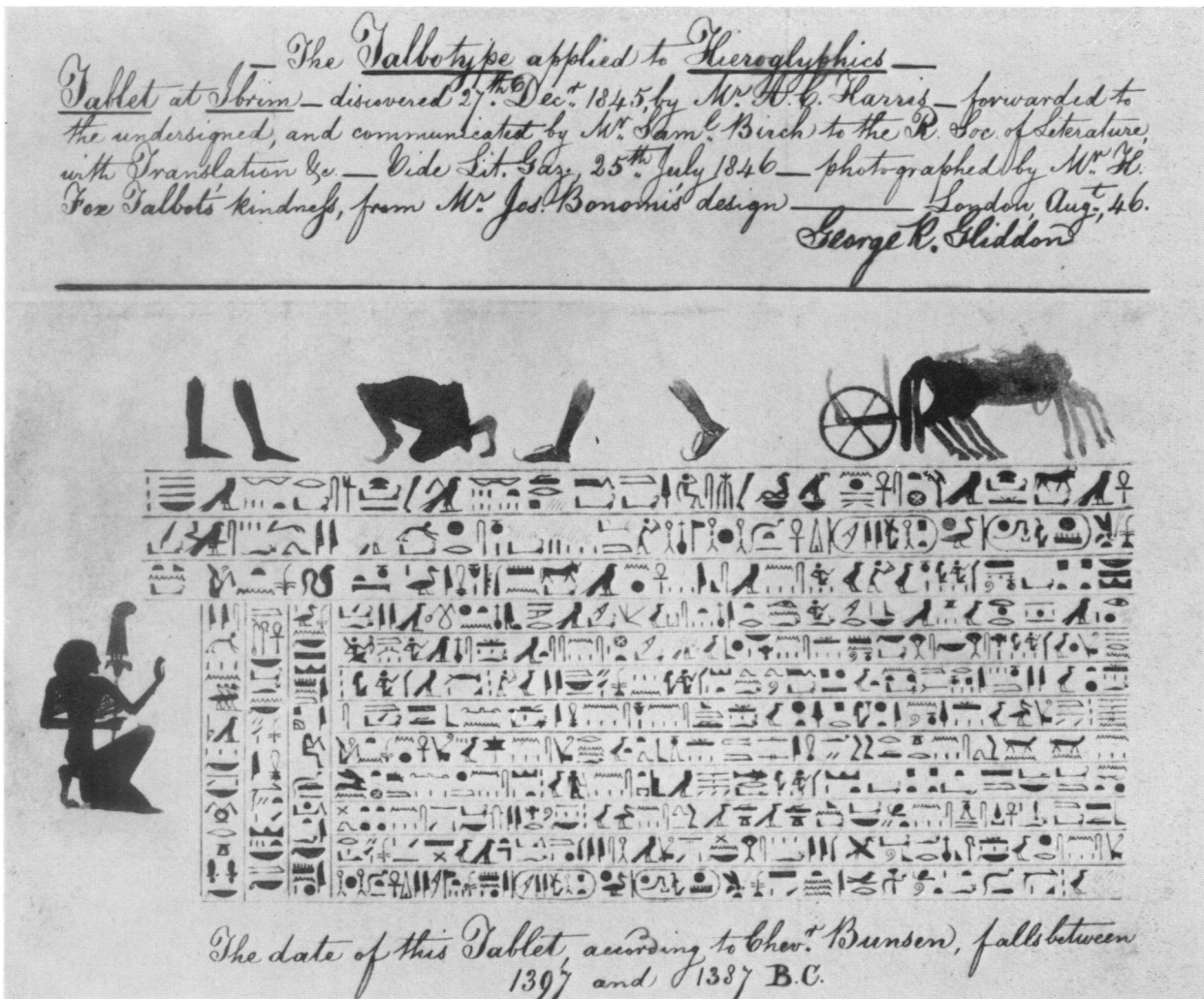


Photo. Science Museum, London

THE TALBOTYPE APPLIED TO HIEROGLYPHICS

Page 1 (slightly smaller than original)

Mr. Birch's Translation — "The Living Ra, the powerful bull, crowned in Nem-ma, giving life to the World, lord of the Upper and Lower World, the strength of the bow, the powerful establisher (?) piercer of the Naphit (Libyans): the golden Plawke, the strength of crowns, giving victory to bows in all lands, the king of the Upper and Lower world, the truth established by the Sun, whom the Sun has tried, the son of the Sun, Seti beloved of Ptaha, endued with life immortal for ever: the good god, the smiter of the fallen with his the hewer of flesh (Aethiopia) the hurler at the Reu-ten-nu (?) the living Ra, the powerful bull, establishing his heart like the son of Nu-pe, the victorious monarch making every place attached to him: who has made increase ploughing in the land of Egypt (?) through its length the Reu ten nu, through molesting their settlements and harrying their lands his commands before his archers with wine in the North (?) he has gone to the lands of the South, on their submission, the North

their heart, like the chief, none of the abominable can rise under his feet, as he has brought his name to them, the breath of life, the powerful monarch; the ruler of Egypt, the darter at foreign lands grasping all lands in their seats. striking their lands, the afflictors of Egypt, the abominable are in the power of his Majesty, established with life, led behind, the victorious lord their spirits are bound to thee, terrify thou their lands order thou them, thy fallen enemies, the good godlike ruler molest thou the lands of the Nakhsi (Negroes) on the day when he has extended the Southern frontier to and the North he declared to have troubled to the Lake of the Waters (the Caspian?). . . . the king of Upper and Lower worlds, the truth established by the King whom the Sun has tried, the Son of the Sun, Seti, beloved of Ptaha, beloved of Amenra king of the gods living for ever, and ever!"

Photo. Science Museum, London

THE TALBTYPE APPLIED TO HIEROGLYPHICS

Page 2 (slightly smaller than original)

The perpendicular lines of hieroglyphics before the Prince are:

"Amen-em-ap-t, royal son, (Prince) of Kesh (Aethiopia) says: thy father Amen-ra has ordered thee with all life, power, and endurance: he has conceded to thee the South as well as the North; all lands to be submissive to thy spirits, and every country to be under thy sandals."

Extract of Mr. Harris' letter, Alexandria 21 May 1846, to Mr. Geo. R. Gliddon.

"This Tablet is found a short distance above the Castle of Ibrim in Nubia. It is sculptured high upon a Rock which approaches so close to the River that there is no towing path, and from this circumstance boats seldom go near to the Eastern shore, and this Inscription has continued hitherto unnoticed by Travellers. Accident brought it to my view on the 27th Dec^r. 1845, and on my return from Wady Halfa I copied by means of a ladder on the 31st of the same month."

"The pictorial representation which heads the Tablet is almost effaced, in consequence of its greater exposure to the corroding effect of the wind, but there remain traces of a chariot and horses; the King stabbing or spearing a fallen enemy; and an approving Divinity of which the legs alone are visible. A representation somewhat similar to this is not uncommon on Egyptian monuments."

"I scratched my name at the bottom of the Tablet in token of possession, and because it may thus be identified." signed) A. C. Harris

Photo. Science Museum, London

CONSIDERATIONS ON THE BATTLE OF KADESH

By HANS GOEDICKE

No battle has echoed more in Egyptian inscriptions than that fought by Ramesses II in his fifth year against the Hittite king Muwatallish. There are the pictographic representations with their *Captions*, the official *Bulletin* of the event, and the *Poem*, proclaiming the king's valour, forming thus three closely related sources.¹ This abundance seems somewhat curious, if one considers that the meeting was not exactly an Egyptian victory, but from the military point of view was rather a defeat for Ramesses II, despite his personal bravery. Of course the Pharaoh had some justification for boasting about his deeds, but this concentration on the royal heroism could be done only at the expense of the royal followers whose behaviour must have seemed rather cowardly. With all due regard for the personal pride and the frequently quoted self-adulation of the Ramesside monarchs, there still remains an unexplained element in the emphasis given to the description of this event in the reign of Ramesses II, which contained other undertakings with more glorious results. Here attention will be drawn to this curious situation, with no attempt to supply any solution for it.

The following observations concern the question as to what saved Ramesses II from complete disaster. Before concentrating on details of the strategy of the battle, it is necessary to ask first about the motives for the king's campaign. The Prince of Kadesh appears to have become a vassal of Sethos I, thereby opening the door for Pharaonic dominion to expand into northern Syria.² Needless to say, such an action appeared dangerous to the Hittites who had other grievances against the Pharaoh dating from the reign of Horemhab.³ When Ramesses became sole ruler, after having served as co-regent with his father for probably three years, the Hittites apparently used the opportunity to try to remove the Pharaonic influence in Syria exercised through the vassalage of Kadesh. There is no direct information about how the Prince of Kadesh acted in this situation, but we are probably right to conjecture that he did not renew his vassalage, by not recognizing the newly crowned Ramesses II as his sovereign. It is furthermore likely that he ventured this step on account of support promised or received from the Hittites; therefore the campaign of the Year 5 has to be understood as an attempt

¹ The discussion is based on the standard edition of the texts by Kuentz, *La Bataille de Qadech* (MIFAO 55); the translations follow Gardiner, *The Kadesh Inscriptions of Ramesses II* (1960).

² Kadesh is mentioned in both lists of Sethos at Karnak, further at Qurna and the king's temple at Wâdi 'Abbâd; cf. Simons, *Egyptian Topographical Lists*, XIII, 28; XIV, 26; XV, 31; XVII, 3. The vassalage of Kadesh is also demonstrated by the fragment of a stela of Sethos I found there; cf. Rezard, *Syria* 3 (1922), 108.

³ Helck's hypothesis (*Die Beziehungen Ägyptens zu Vorderasien*, 186 ff.; 199 f.) that Horemhab was responsible for the disappearance of the Hittite prince Zannanza during his journey to Egypt to become prince-consort (or husband) of the dowager queen, best accounts for the course of relations between the two countries following the 'Amarna period. The punitive actions of the Hittites were suddenly ended by the outbreak of an epidemic of the black plague.

to restore the vassalage of Ḳadesh to Egypt and to stem Hittite influence in Syria. That the campaign was expected to lead to major military operations, is clearly indicated by the preparations taken by both parties. The Hittites are said to have secured the support of various people settled in Asia Minor, in addition to Naharin, Carchemish, Ugarit, and a host of others. The Egyptian report does not wish to acknowledge that the Hittites had a voluntary following, and thus ascribes the gathering of this alliance to great financial inducement offered by the Hittite king: 'He left no silver in his land, he stripped it of all its possessions and gave them to all the foreign countries in order to bring them with him to fight.' The preparations by Ramesses II were no less extensive and seem likewise to have required great expenditure. In addition to the Egyptian military forces organized in the four divisions, Amūn, Rēḥ, Ptaḥ, and Seth, there was also the levy imposed on the Pharaonic subjects in Palestine. This force was augmented by the hiring of mercenaries, the Shardanu, as indicated by an undated stela from Tanis:¹

The King Usimaḥrēḥ son of Rēḥ Ramesses-Miamūn, his fame crossed the Sea; the central islands were respecting him. They came to him with gifts, their great ones—might governed their plans. The Shardanu changed their hearts; one had not known how to fight them since eternity. When they came boldly, after they had sailed in battleships from the middle of the Sea, one did not know how to withstand them. He has taken them victoriously through his valiant strength and brought (them) to Egypt.²

While the Hittite alliance apparently assembled at Ḳadesh, there is no specification of where the Pharaonic force was gathered. According to the *Bulletin* (B4), Ramesses II was in Djahy prior to his advance against Ḳadesh. Although there is some ambiguity about the exact limits of the area so denoted, the presumable etymology of the term from 𐎠𐎢𐎩 'thirst' prompts the assumption that it refers to a parched region.³ This specification would hardly fit the coastal plain of Palestine, the actual Phoenicia, but would indeed apply to the mountainous hinterland. Only in the list of 'the *maryannu* from Djahy' in Pap. Petersburg 1116 A 67 is one coastal city (Ashkelon) mentioned under the heading of Djahy; otherwise all specified places are located inland. With this exception, the great Phoenician ports are not connected with Djahy. Less clear is the relation between Djahy and Retjenu; while Gardiner⁴ saw in them two distinct terms, Helck⁵ is probably right in considering them basically as synonyms, Retjenu being the older of the two terms.

The question is relevant to our discussion only insofar as it concerns the establishing of the place from which Ramesses II set out against Ḳadesh. This could provide a clue to whether the Pharaoh advanced along the coast, or inland. To identify Djahy, the starting point, with the mountainous hinterland, would make it probable that Ramesses II did not take the coastal road, but rather advanced inland, i.e. by either the Litanni or (less likely) the upper Jordan valley, continuing through the Beqa'.⁶ This assump-

¹ Tanis stela II A, ll. 13 ff., cf. Yoyotte, *Kemi*, 10 (1949), 62 ff.

² Of particular importance are the words 'the Shardanu changed their hearts' (cf. *Wb.* IV, 436, 14), clearly indicating the change from being marauders to paid mercenaries.

³ Eisler, *OLZ* 29 (1926), 3 f.

⁴ *Onomastica*, I, 145*.

⁵ *Op. cit.* 274 f.

⁶ First established by Sturm, *Der Hettiterkrieg Ramses II*, 65 ff. against Alt, *ZDPV* 55 (1932), 1 ff.; 66

tion is corroborated by the statement in the *Poem* (P 34) that the army proceeded 'on the narrow passes as on the roads of Kemet'.¹ Such reference to the difficulties of the road would not be made for an advance along the coast, but allows only the assumption that Ramesses II used the more difficult inland roads for his march against Kadesh, moving on the edge of the hill-country without descending to the Jordan valley or to the Mediterranean littoral. This specification of the route applies only to the royal party and does not necessarily cover any other troop movements.

As soon as we agree that Ramesses II took the inland road, the determination of the later part of his route is easy; it necessarily followed the Litanni valley, the Beqa' and the upper Orontes valley. The first specified place mentioned in this advance (P 35) is the *pꜣ dmi Rmssw-Myimn nty m tꜣ int pꜣ rš* which has been rendered as 'Ramessemiamūn, the town which is in the Valley of the Cedar'.² It undoubtedly has to be regarded as the last point before leaving the Pharaonic dominion and entering foreign land. Breasted,³ Alt,⁴ Bilabel,⁵ and Sturm⁶ saw in it a town on the coast near modern Beirut; Wilson,⁷ and Gardiner⁸ proposed a location in the Lebanon, while Edel⁹ placed it 'möglichst nahe an der ägyptischen Grenze am Nahr el-keleb'. All scholars tend to understand the indication as referring to a town which was either newly founded by Ramesses II or renamed by him.¹⁰ Because the time that had elapsed since the assumption of sole rule by Ramesses II seems too short to allow for the founding and building of a town, the second possibility appears more likely; also on account of the predominantly legal character of the indication, which refers to the limit of the dominion of Ramesses, and possibly to a place located there, rather than to a settlement in general. *Dmi* as 'border' or 'border-town' is repeatedly attested in the inscriptions of the Nineteenth Dynasty: so in the reign of Sethos I in *dmi n pꜣ Kꜣꜣn* or *dmi n Kꜣš*,¹¹ either one denoting the fortified place marking the frontier. In view of this usage, the designation is to be understood as 'frontier-town of Ramesses-Miamūn which is in the Valley of the Cedar', with the royal name serving as specification of *dmi*. This 'Valley of the Cedar',¹² where apparently the border of the realm of Ramesses II was located, was probably associated with the Lebanon, as the mention of cedars suggests. Of course,

(1943), 1 ff., and Dussaud, *Topographie historique de la Syrie antique et médiévale*, 105 ff. The approach from the south has been accepted in all recent studies, like those of Gardiner, Faulkner (*MDAIK* 16 (1958), 93 ff.), Helck (op. cit. 209) and Schulman (*JARCE* 1 (1962), 47 ff.).

¹ *Kmt* in this context does not necessarily refer to Egypt, but is better taken in its basic sense of 'fertile plain'; cf. *Urk.* IV, 4, 3, etc.

² Gardiner, *Kadesh*, 8; Wilson in Pritchard, *Ancient Near Eastern Texts*, 256; *et al.*

³ *Battle of Kadesh*, 11.

⁴ *Beiträge zur biblischen Landes- und Altertumskunde*, 68, 113.

⁵ *Geschichte Vorderasiens und Ägyptens*, 112.

⁶ Op. cit. 63: 'wohl im Küstenbereich . . . möglicherweise in der Gegend von Beirut'.

⁷ *AJS* 43 (1926), 287: 'the location of Shabtun roughly two miles from Kadesh, on the Brook el-Mukadiyeh'.

⁸ *Kadesh*, 16: 'its name clearly pointing to the Lebanon'.

⁹ *Geschichte und Altes Testament (Festschrift Alt)*, p. 63.

¹⁰ Cf. especially Gardiner, loc. cit.

¹¹ *Dmi n pꜣ Kꜣꜣn*: Wreszinski, *Atlas*, II, 39. It is not a specific town, but rather the place marking the border of Canaan, across which the Shasu were driven by Sethos I. Similarly *dmi n Kꜣš* (Wreszinski, op. cit. 53) does not concern Kadesh on the Orontes, but is rather to be understood as the border-fortress of *pꜣ tꜣ n Kꜣš*, 'the land of Kadesh'.

¹² The place is also mentioned in the *Tale of the Two Brothers*, 7, 2, referring there to a place outside the dominion of Pharaoh.

cedars in those times were likely to have grown on either face of the Lebanon range, as well as the Anti-Lebanon, so that we have here only a general, not a specific, indication. The fact that *t: int* 'the valley' is considered sufficiently distinct to need no further specification leads one to assume that this 'valley' was more than just one of the numerous crevasses cutting into the eastern face of the Lebanon. None of the *wâdis* in the vicinity of modern Beirût would seem distinctive enough to be labelled merely 'the valley', with the possible exception of that of the Nahr el-Kelb, which marked the limit of the Pharaonic dominion during parts of the reign of Ramesses II. The identification of 'the Valley of the Cedar' with the Nahr el-Kelb fails for several reasons: first, the Egyptian term implies that it was a source of cedar wood, which one would hardly expect from a frontier *wâdi*. Secondly, the Nahr el-Kelb itself is too narrow to provide space for a fortified town, which would then have to be explained as being *near* this creek, as proposed by Edel; this interpretation, however, is in direct contradiction to the Egyptian specification, according to which the place was located *in the Valley of the Cedar*. Thirdly, while the Nahr el-Kelb forms a clear and distinctive frontier towards the northern Lebanon, which at this time formed the land of Amurru, it certainly was not a suitable avenue for the advance of an army that had been marching north through the hill-country. The observation that the party of Ramesses had used the inland road really provides us with the proper identification for the 'Valley of the Cedar'. It can denote only that wide valley between Lebanon and Anti-Lebanon which is now called El-Beqa' and which in its southern part is watered by the Litanni and in the northern by the Orontes (Nahr el-'Asi) with the watershed at Ba'albek. This identification of the 'Valley of the Cedar' conforms with the subsequent information about the route of the Pharaonic army, so that we can assume that the frontier of the Pharaonic dominion of Ramesses II was in the Beqa'; the precise location cannot be defined with certainty, but the watershed at Ba'albek is at least a highly appealing conjecture.

By interpreting *p: dmi Rmssw-Myimn nty m t: int p: cš* this way, the significance of the following indication becomes clear; it refers to the passing of the border of the Pharaoh's dominion and the first penetration into enemy territory. The next point indicated in the various records (P 36) is the *hill-country south of Kadesh*. If we assume that the ruler of Kadesh controlled the entire course of the Orontes, this would have to be understood as the region from the watershed between the Litanni and the Orontes onwards, or possibly even this region, i.e. that of Ba'albek, in particular. From there the move was continued northwards, so that the invading party approached Kadesh from the south. This allows only one explanation, i.e. that the advance was along the Orontes, as Faulkner, Gardiner, and Helck justly propose, against Alt and Dussaud—accepted by Edel¹ and Noth²—who suggest an approach to Kadesh from the west through the Nahr el-Kebir. The stages after penetration into foreign territory are not indicated until Kadesh is fairly close. The *Bulletin* (B 8) mentions next *the region south of the town of Shabtuna*. The town itself has been identified by Breasted with Riblah,³ as is specified in P 38 and P 59 *covering the land* (of the Orontes) *in the neigh-*

¹ *Zeitschrift für Assyriologie*, N.S. 15 (1950), 195 ff.; 16 (1952), 253 ff. ² *Welt des Orients*, 3, 232, n. 55.

³ *Battle of Kadesh*, p. 22; this identification is almost universally accepted. Sturm (op. cit. 86) leaves the

bourhood north of the town of Shabtuna. It is at this point that Ramesses crossed the Orontes; Kadesh lies west of the river, while the road runs east of it. The distance from Riblah to Kadesh is approximately 10 miles, i.e., one day's journey.

It was at this point that the two Bedouin (*šsw*) met the king and delivered the false report that the Hittite army was still far away to the north of Tunip (B 8-17). Thus Ramesses II did not gather his forces but continued to proceed against Kadesh (B 29) with the division of Amūn which had been leading the advance. North-west of the town Ramesses II pitched camp (B 30), apparently intending to await the other divisions before dealing with Kadesh. There were certain advantages in this location for the encampment. One was the proximity of the lake of Homs, which was not only a source of water, but also acted as a defensive barrier. The position was additionally strengthened by the Mikiyeh river, which provided a natural line of defence against attacks from the west. The choice of this site suggests that Ramesses II expected an attack from the north or west and took precautions accordingly. However, the apparently ideal location for the camp was potentially a trap while the camp remained unfortified, and the main body of the army was still *en route*. Muwattalish had already assembled his troops *behind the town of Kadesh* (P 71) ready to attack.

There is some ambiguity about the exact location of the Hittite army. According to B 26 and B 51, it was *standing equipped and ready behind Kadesh the Old*. Helck¹ thinks that 'Kadesh the Old' is not identical with the Tell Nebi Mend but with the older (for him, abandoned) town-site to the north-east of Kadesh, east of the Orontes. Accordingly he places the Hittite camp there and supposes that the Hittite attack started there. This explanation agrees with that of Faulkner,² although he assumes that the Hittite king later moved the infantry further to the south-east of Kadesh—thus still on the eastern bank—while the chariotry attacked in broad formation south of Kadesh. Slightly different are Schulman's views,³ who thinks that the Hittite army attacked on a wide stretch south of Kadesh from its original position east of the river.

All these explanations disregard completely the difficulties of bringing a large body of troops—according to P 84 the Hittite attack was carried out by 2,500 chariots—across the Orontes swiftly enough to surprise the Pharaonic army. In modern times the Orontes (Nahr el-'Asi) is a wide watercourse which passes east of Tell Nebi Mend, the site of ancient Kadesh. As there is no specific reason to presume basically different hydrographical conditions for the past, it seems exceedingly implausible that an army of 7,500 men with 2,500 chariots could cross the broad river wherever it desired. That the crossing of the Orontes was restricted to fords is demonstrated by the route taken by Ramesses II's army which crossed at the ford of Shabtuna.

question undecided, except that he assumes *Šbtn* to be only 4 or 5 km. away from Kadesh, which is undoubtedly too close.

¹ *Beziehungen Ägyptens*, 212.

² *MDAIK* 16, 96. Strangely enough, Faulkner envisages difficulties and delays for the Pharaonic army in the crossing of the Orontes, but does not consider such problems for the Hittites. His explanation of the battle is greatly hampered by the figures of the distances he assumes between the different sections of the Pharaonic army. A distance of 1½ miles, which he presumes between the divisions of Amūn and Ptaḥ, could be covered in a quarter of an hour, and even the distance of 12 miles assumed by him from the division of Rē^c to that of Ptaḥ could easily be covered in three hours and even faster in an emergency.

³ *JARCE* 1, 53.

These considerations rule out the possibility of a sudden Hittite chariot attack across the Orontes. Thus it is more likely that the Hittite chariotry waited in ambush on the western bank of the Orontes in the plain north-east of the town. For the attack they passed Ḳadesh on the south side,¹ which led to their confrontation with the approaching division of Rēḳ. To attack from this side was also the best strategy for assaulting the encampment of the division of Amūn, whose natural defences, selected against an attack from the north, became a trap in the event of an attack from the south. Even if the Hittite chariotry assembled on the western bank of the Orontes in the shadow of Ḳadesh, this has no direct bearing on the question of the location of the Hittite camp. According to P 65–67, the body of the Hittite army with Muwattalish did not take part in the surprise attack. Thus there is no difficulty in locating the Hittite camp north-east of Ḳadesh on the eastern bank of the Orontes.

The battle proper has been discussed recently and there is no need to repeat its development here. We only have to investigate the position of the divisions of Pharaoh's army during the battle. The king was with the division of Amūn and had started to pitch his camp. The second group, the division of Rēḳ, was following the route the king had taken, but it was 1 *itr*, i.e. one day's journey,² behind the leading division of Amūn and the king. The division had started from the ford of Shabtuna on the day that it reached the camp site north-west of Ḳadesh, covering a distance of approximately 10 miles.³ Assuming, as seems only just, that the march had commenced in the morning,⁴ it can be conjectured that Ramesses with the division of Amūn reached Ḳadesh in the afternoon. The division of Rēḳ, following at a day's journey distant, was thus scheduled to cross the ford the following day, i.e., the day when Muwatallish attacked. After the arrival of Ramesses II at Ḳadesh, some reconnaissance was conducted, during which first contact with the Hittites was established, leading to the realization of the imminent danger.⁵ This explains the urgent orders given to the division of Rēḳ to proceed to Ḳadesh as quickly as possible.⁶ Assuming that the messenger was dispatched in the (late) afternoon, he had to cover the distance to Shabtuna, and then the troops resting there would immediately cross the ford and hasten to join

¹ P 68–72: 'He (Muwattalis) had sent men and horses exceeding many and multitudinous like the sand, and they were three men on a chariot and they were equipped with all weapons of warfare. They had been made to stand concealed behind the town of Ḳadesh, and now they came forth from the south side of Ḳadesh.' The formulation leaves no doubt that the attacking force was not the entire Hittite army.

² Borchardt, *Janus* 30 (1921), 119, on the basis of the Ptolemaic geographical lists, has calculated the *itr* as 10.5 km., which is accepted by Gardiner, *Egyptian Grammar*³, § 266. This figure is based on the indication that Egypt is 106 *itr* long; the result is somewhat small for the distances between the various places mentioned in connexion with the battle of Ḳadesh, but is certainly better than the 1½ miles for the *itr* insisted on by Faulkner which had been proposed by Breasted but since abandoned. It might be that *itr* was originally a rather approximate figure and only later became of definite size, a development which can be demonstrated with the German *Morgen*, originally denoting the area mown in one morning.

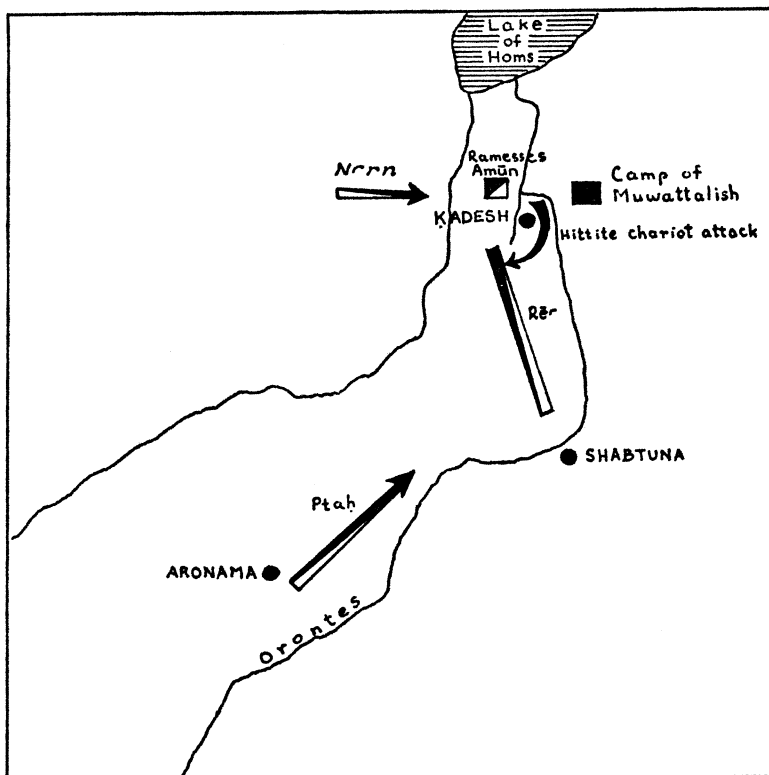
³ The camp site was north-west of Ḳadesh, which has to be taken into account when calculating the distances.

⁴ Cf. the movements of the army of Tuthmosis III, which started in the early morning (*Urk.* iv, 652, 13).

⁵ B 32–34: 'Then came a scout who was in the suite of His Majesty bringing two scouts belonging to the Fallen One of Ḳadesh' (translation by Gardiner).

⁶ B 72–75: 'Then command was given to the Vizier to hurry on the army of His Majesty as they marched on the road to the south of the town of Shabtuna so as to bring them to where His Majesty was.' *Mšr* 'army' could refer here to the divisions of Rēḳ and of Ptaḥ, the remainder of the army of Ramesses II, so to speak.

Ramesses II. Taking the distances into consideration (and the rather slow manœuvring of a large body of troops), one can conclude that the division of Rē^c probably advanced most of the night towards Pharaoh's camp. It thus would not have reached it before morning, and was probably attacked while still in marching order. As it is most likely that the Hittites started the battle in the morning, this would explain why the division of Rē^c was surprised *en route* just before reaching the camp.



The region of Ẁadesh

The next division, that of Ptaḥ, is described as having been south of Aronama, one *itr* behind the division of Rē^c, at the time Ramesses II recognized the threatening danger and his need to rally his forces. According to Albright,¹ this place is to be identified with El-Hermil. The distance between El-Hermil and Riblah is approximately the same as that between Riblah and Ẁadesh, i.e., one day's journey; in other words, the division of Ptaḥ was two normal days' journeys away from Ẁadesh when Ramesses II discovered the danger of an imminent attack.

In the account of the battle, the division of Ptaḥ is described as not having left the *forest of Rabawi (La-bi-wi)*. This forest has been differently located: Helck² and Schulman place it east of the Orontes and south of Shabtuna/Riblah; Faulkner, north of

¹ *BASOR* 130, 26 f.

² *Op. cit.* 212. He identifies Rabawi with *Lebwe* 20 km. south of Hermil. Such an identification requires the assumption of a forest stretching over more than 30 km.

Shabtuna and west of the Orontes. I think this point can be decided with some certainty. All discussions have neglected the fact that El-Hermil lies east of the Orontes, while the great road is west of the river. One could think that positions were indicated in relation to the nearest major settlement, but it is even more likely that the division of Ptaḥ moved along the western bank of the Orontes, while the divisions of Amūn and Rēḥ had taken the main road on the eastern bank. A number of circumstantial arguments can be advanced in support of this hypothesis. First, the location of El-Hermil (Aronama); second, the necessity of securing the entire Orontes valley and not only one bank; third, in addition to purely strategic considerations, the dividing of the troops and their advance on both sides of the river had considerable advantages for the supplying of the army which in all probability lived from the country through which it marched. This form of support restricted the number of troops to be moved along one route, as latecomers would not find any supplies. Fourth, the fact that the division of Ptaḥ was retarded could be attributed to the poorer roads on which it had to move. The conclusion to be drawn from these points is important for locating the forest of Rabawi. If the division of Ptaḥ advanced on the west bank of the Orontes, this 'forest' has to be on the same side, suggesting the slopes of the Lebanon. By accepting the hypothesis that the division of Ptaḥ advanced west of the Orontes, the reference to the forest of (the) Rabawi becomes understandable. It would seem only logical that Ramesses II not only summoned the division of Rēḥ, but also the more distant division of Ptaḥ. Since they were a two-day journey away from the king's camp, they could have tried to reach it by a short-cut in an approximately northerly direction, which would have led them through the north-eastern spurs of the Lebanon. The forest of Rabawi/Labawi thus most probably refers to the Lebanon or part of it.¹ This evaluation is supported by the mention of a hunt by Amenophis II 'on the Rabawi, in the forest', *Urk.* IV, 1304, 5. The context suggests a location south of Ḳadesh.

Support for this hypothesis can be derived from a cuneiform letter concerning the battle of Ḳadesh, probably written by Ramesses II to Khattushilish III after the peace treaty was completed. There are differences in the interpretation of the contents of the letter and the identification of its writer. Edel² sees in the four armies mentioned the forces of Ramesses II; Helck,³ on the other hand, prefers to apply the indications to the Hittite army. His main reason is the statement that one, i.e. the fourth, corps of the army was in Amurru. Leaving this question aside for the moment and turning to the information in the letter concerning the other three corps, we find it identical with the statements about the three divisions of Amūn, Rēḥ, and Ptaḥ in the hieroglyphic text. The first army is said to have been at Kinza, the camp site of Ramesses II, the third at a place whose name can be convincingly restored into *Hrnm—crnm—Hermil*. The position of the second army is indicated as Taminta, which Edel identified with *t3-mntyw* known from Pap. An. I, 22, 3 as a place in the vicinity of Ḳadesh. The latter is not an obscure place name, but can be easily explained as 'the west side'

¹ Of course, this is not to be understood as identifying Rabawi with the Lebanon proper, whose name is preserved in Egyptian records as Ra-ba-ra-na and Ra-me-na-na.

² *Zeitschrift für Assyriologie*, N.S. 15 (1950), 204 ff.; 16 (1952), 253 ff.

³ *Op. cit.* 214.

(*t3 imntyw*);¹ this implies that the second corps had already crossed the Orontes after its previous advance on the eastern bank.

We can now turn to the investigation of the movements of the fourth division, that of Seth. According to the *Poem*, the army of Seth *was marching on the road*; no attempt was made to summon these troops, nor were they expected. The version of the *Poem* (P 62–65) on the Luxor temple is most specific: ‘. . . the army of Seth was marching *en route*; when His Majesty made the first battle-line from the advance of his army, they were (still) at the shore (*mryt*) of the land of Amor.’

This agrees with the indication in the above-mentioned Hittite letter that the fourth army of the king was in the land of Amurru. From this coincidence it can be inferred that the Hittite letter describes the positions of the army of Ramesses II. It furthermore corroborates the fact that the division of Seth was not only absent from the battle, but that its advance was planned on a completely different route from that of the rest of the army. While the main body advanced from the south through the Beqa’, the division of Seth made its way through Amurru, the region north of Byblos. The most likely avenue for an eastward penetration is the Nahr el-Kebir, the Eleutheros valley, leading almost straight to the region of Kadesh. It seems quite obvious that Kadesh had been intended as an assembling place in the strategy of the campaign. It is uncertain, however, whether the division of Seth had advanced along the coast before penetrating through the Nahr el-Kebir, or had by-passed most of Amurru, having been transported north by ships.

While this part of the army did not take part in the battle at all, one group apparently arrived from Amurru late on the day of battle after the Hittites had stormed the Egyptian camp and begun looting. The relevant statement is contained in the following annotation to the battle-scene (R 11): *The coming of the ncrn . . . from the land of Amurru. . .* There can be no doubt that the designation *ncrn* is identical with Hebrew ‘youths’, commonly used in the Old Testament for soldiers, e.g. 1 Kings xx. 15. They are also mentioned in Pap. An. I, 17, 3–4 as the warriors of Phoenicia, and again in the great inscription of Merenptah as Canaanite mercenaries. It is in this sense that the term is to be understood in connexion with the Kadesh battle: the *ncrn* were Canaanite soldiers who had joined the Pharaonic army. The occurrence of the term is thus not to be explained as a philological fancy, as Schulman suggests;² the *ncrn* have rather to be taken as troops that did not belong to any of the four divisions of the Egyptian army, but represent an additional corps. One could conjecture that these *ncrn* were conscripts of Amurru which had become a vassal of Pharaoh. Such a view, however, fails in two points: first, the preposition *m* in the adverbial adjunct specifies only the move but not the people whose origin would have to be indicated by an indirect genitive; second, Amurru had just been forced to become a vassal of Pharaoh and might have been a rather doubtful ally, hardly willing to save the Egyptian camp from complete devastation. These *ncrn* are specified by *n(yw) c3 prwy*³ as being directly

¹ There is nothing to prove that *t3-mntyw* denoted a country or a specific geographical region; to understand the term as referring to the area west of the Orontes suits the context in Pap. An. I. ² *JARCE* 1, 52.

³ *c3-prwy* is a more likely transliteration for $\overline{c3}$ than the customary *pr-c3*, a point I hope to discuss

linked with Pharaoh, an indication which speaks against the possibility of their being conscripts from Amurru. In view of their obviously close connexion with the Pharaoh and also of their undoubtedly Semitic designation, it seems justified to equate these *nʿrn* with the unruly Semitic mercenary troops commonly described as ‘Apiru, who were settled as military colonists in various parts of Palestine. They had a very close, essentially personal relationship with the Pharaoh, and represented an integral part of his military forces. Having been in close contact with Egyptian military life and customs for a long period of time, it is only natural that they are depicted as if they were Egyptians.

These military colonists of Semitic extraction, usually known as ‘Apiru but denoted here by their self-styled designation *nʿrn*, apparently had advanced together, or in accord, with the division of Seth. They can thus be assumed to have advanced along the Canaanite coast, either by boat or on land, to Amurru, and not by the inland route taken by Ramesses II with the main body of the army. Through Amurru they presumably followed the Nahr el-Kebir (Eleutheros valley), and arrived at the battlefield just before Pharaoh’s army was completely routed. Their appearance seems to have changed the situation and caused the retreat of the Hittites. It was thus the ‘Apiru, i.e., the mercenaries of Palestine, who saved Ramesses II from fatal defeat. Of course this fact was hard on Egyptian–Theban national pride and might very well be the reason why the rescuers of the camp are labelled with the noncommittal term *nʿrn*, and why the scene showing their action occurs only once among the many records and is completely omitted from the *Poem* and the *Bulletin*.

Joining the various bits of information together, we can reconstruct not only the strategy of Ramesses II but also the circumstance which upset his plans. Ramesses II’s intention apparently was to approach in various columns and to assemble before Ḳadesh for an attack on the town. The main force advanced from the south through the Beqa’ in two groups on either side of the Orontes. The rest of the army came through Amurru by way of the Nahr el-Kebir. The second division of the main force, that of Rēḫ, was attacked *en route* and could not withstand the Hittite chariotry. It apparently sought shelter, as far as possible, in the yet unfinished camp set up by the division of Amūn, where the king was. The influx of the defeated soldiers of the division of Rēḫ seems to have caused great confusion, if not panic, and thus the Hittites were able to penetrate the camp. Only the unexpected appearance of part of the second western column seems to have saved Ramesses II’s army from complete disaster. While the surprise attack turned the situation in the favour of the Hittites, the exactitude of the Pharaonic strategy, with the precisely timed meeting of the two marching columns, counterbalanced the Hittite advantage, forestalling a disaster for Pharaoh’s army.

elsewhere. Despite the different rendering, the term should still be considered as referring to the institution traditionally called ‘Pharaoh’.

A TWENTIETH-DYNASTY ACCOUNT PAPYRUS¹

(Pap. Turin no. Cat. 1907/8)

By JAC. J. JANSSEN

IN 1925 T. E. Peet published in this *Journal* (vol. 11, 72–75) an article entitled ‘A Possible Year Date of King Ramesses VII’, in which he tried to prove that Ramesses VII reigned at least six years.² He derived his arguments from a passage in pap. Turin no. Cat. 1907/8, but did not endeavour to publish a transcription of the whole text, owing to what he called ‘the extreme cursiveness of the forms’ and ‘the slovenly handwriting of the scribe’. During the forty years which have passed since this article was written our knowledge of the hieratic of business papyri has greatly increased, and it seems therefore that the time may be appropriate to attempt a first publication of a transcription and translation of this difficult text. The hazards of such an undertaking will readily be appreciated, in that I have failed in fact to read many signs of parts of the verso texts. I should therefore have preferred to postpone the publication for some years, hoping that by continued study I might elicit more, were it not that the quite legible recto contained a series of price-notations which seem to merit publication now.³

The Pap. Turin no. Cat. 1907/8 is a palimpsest,⁴ measuring in its present state about 88.5 cm. × 40 cm. It consists of several strips, which do not quite join, leaving a small gap at the end of *rt.* col. I, and another straight through *rt.* col. II, while quite a width of the lower half of *rt.* col. III is missing. In addition, there are smaller holes in several places. The fragment at the bottom left of col. III has been added since Peet studied the papyrus, and the text here will be seen to confirm his conclusions admirably (see below, p. 91).

The recto contains the ends of the lines of a first column and two more or less complete ones. Since col. I has partly disappeared it remains uncertain how many lines it contained, but probably there were as many as in col. II, i.e. 21. Col. III consists of

¹ The author wishes to express his gratitude to Prof. S. Curto, the director of the Museo Egizio, Turin, for his permission to publish this text, and to the Netherlands Organisation for the Advancement of Pure Research (ZWO) and the Consiglio Nazionale delle Ricerche (CNR), which enabled him to study the original in the summer of 1965. He has also been able by the kindness of Prof. J. Černý to study his and Peet’s transcription of the Papyrus.

² An addition to this article is to be found in *JEA* 14 (1928), 60, where Peet was able to stress his argument by the replacement of a small fragment in the relevant lines of the papyrus.

³ The prices, had he known them, would have constituted a welcome addition to the fifth part of Helck’s *Materialien zur Wirtschaftsgeschichte des neuen Reiches* (*Abhandl. der geistes- und sozialwissensch. Klasse der Akademie der Wissensch. und der Literatur in Mainz*, Jhrg. 1964, nr. 4, Wiesbaden, 1965). Since a discussion of each separate item with Helck’s conclusions would unduly enlarge this article I have to refer the reader to my forthcoming study of commodity prices.

⁴ Traces of the older text are clearly visible, but I have nowhere been able to decipher even a single word. The position of the fibres on the recto is V/H.

15 lines, and from its shortness as well as from the contents of ll. 12–15 it appears that at this point the text was complete. The loss at the right hand side of the sheet cannot be measured, but I shall try to prove below that the first line of col. I was also originally the beginning of the text.

The verso too contains one incomplete and two more or less complete columns. Col. I is written far to the right, which means, since the writer turned the sheet vertically after filling up the recto, that this column is written upside down and almost on the back of *rt.* col. II.¹ Verso col. II is written far to the left of col. I, on the back of *rt.* col. III,² and again upside down in relation to it. Unfortunately, therefore, some portions of its lines are lost with the same fragment that is missing from the bottom of *rt.* col. III. With the recovery of the new fragment the end of the last two lines is retrieved, but the wide gap makes the connexion with their beginning uncertain.

Almost adjoining the last, longer, lines of col. II is a third column, the lines of which are broken off at the left-hand edge of the sheet. This proves that the papyrus was originally longer at this side as well, although I see no reason to suppose that it was long enough to contain yet a fourth column on the recto.³

Apart from these three texts there is a column of numbers just to the left of col. I, but written upside down. The meaning of these numbers escapes me completely.

The provenance of the papyrus is unknown, but from its contents it seems more than probable that, like so many other business papyri in Turin, it came from Thebes, and more precisely from the western bank, perhaps from the neighbourhood of Deir el-Medîna or its administrative centre, the temple of Medînet Habu.

Translation

Recto col. I

1. scribe of the treasury Pbēs.^a
- 2–3. (lost)
4. 1 (+x), makes 30 *deben*.
5. sweet *sgnn*-oil,^b 1 *hin*, makes 10 [*deben*].
6. necropolis.
- 7–8. (lost)
9. 4. Total: 17 *deben* of copper.
10. incense, 10 *hin*, makes 2½ *deben*.
11. 5 *hin*, makes 5 *deben*.
12. (lost)
13. makes 5 *deben*.
14. ¾ *oipě*,^c makes 2 *deben*.
15. barley as barley, 1 (*khar*), makes 8 *deben*; emmer, 1¾^d *khar*, makes 16 *deben*.
16. *wmnt*-loaves: 2.^e
17. 4 *khar*. Total altogether 25 various loaves, makes 5 *deben*.

¹ It is significant that to the extreme right the sheet is left blank, meaning, since the first parts of the lines of *rt.* col. I are lost, that originally quite a broad strip at the right edge of the verso was unused.

² The lines on the verso are much shorter than those on the recto.

³ Cf. also the end of *rt.* col. III, 14, which is broken off, but extended originally into the open space to the left of *rt.* col. III.

18. fishes, makes (?)^f 10, makes 3 *deben*.
 19. loaves, 8 *khar*(?);^g [fresh?] vegetables,^h 15 bundles, makes 4 *deben*.
 20. 1 *deben*; [1?] pair of sandals, makes 2 *deben*. Entered (?):ⁱ 1 *idg*-garment,^j makes 15 [*deben*].
 21. 1, makes . . . *deben*; 50 unguetted^k fish, makes 2 *deben*.

Recto col. II

1. Given to him in the fortress of the necropolis when he came to drink there
 2. together with the scribe of the treasury Pdēs;^l 12 *sdj*-garments^m of fine, thin cloth,ⁿ each 5 (*deben*), makes 60 *deben*; 1 *djyw*-garment^o of fine, thin cloth, makes 15 *deben*;
 3. sesame-oil, 2 *hin*, makes 4^p *deben*; 1 pair of enveloping^q woman's shoes, makes 2 *deben*;
 4. wine, 1 *mnt*-jar, makes 2 *deben*; bread, 40 fine *sb*-loaves;^r vegetables, 20 bundles;
 5. incense, 15 *hin*, makes 3½ *deben*.
 6. Given to him in the house of the scribe of the treasury Pbēs: 110 yokes of *mnk*-wood,^s makes 10 *deben*; remaining 30, makes 6 *deben*.
 7. (to be examined)^t Given to him in Year 4, the first month of the Inundation season, day 10, when he came <to> the House of Life:^u emmer, 2 *khar*, makes 8 *deben*;
 8. (to be examined) barley as barley, 3 *khar*, makes [2]4^v [*deben*]; vegetables, 20 bundles, makes 5 *deben*; flax, 50 bundles, makes 5 *deben*.
 9. *ibw*,^w 1 *oipě*, makes [2]½ *deben*; *rs*(?),^x 10 *hin*, makes 1 *deben*; *sty*,^y 10 *hin*, makes 1 *deben*;
 10. *tnr*(?)^z-fish,^z 20, makes 4 *deben*.
 11. Given to him in the *št*:^{aa} by Any: barley as barley, 2 *khar*, makes 16 *deben*; emmer, 1 *khar*, makes 4 *deben*; flour, 2 *oipě*, makes 4 *deben*.^{bb}
 12. Given to him in the village^{cc} byy: barley as barley, 3 *khar*, makes 26 *deben*; emmer, 3 *khar*, makes 12 *deben*.
 13. Given to him when he came [to enter?] in the House of Life: 5 mullet,^{dd} makes 3 *deben*; *rs*(?),^e 10 *hin*, (makes) 1 (*deben*); *sty*, 10 *hin*, (makes) 1 (*deben*);
 14. *ibw*, 1 *oipě*, makes 2½ *deben*.
 15. Given to him in Year 4, theth month^{ee} of Winter, day 6, by him: *ti-šps*-wood,^{ff} 20 (pieces), makes 5 *deben*; *nkpt* <and> *iwfyt*,^{gg} 1 *msti*-sack, makes 1 *deben*.
 16. Given to him in Year 4, theth month^{hh} of Winter, day 20, for the offering to Ptaḥ: emmer, 1 *khar*, makes 8 *deben*; barley as barley, 1 *khar*, makes 24 *deben*; 37½ *deben*; remaining 5 *deben*.
 17. Given to him in Year [5],ⁱⁱ theth month of Summer, day 5, by him: 1,^{jj} makes 3 *deben*, by P'okhet.^{kk}
 18. Given to him by [P]okhet: emmer, 1 *khar*, makes 4 *deben*; barley as barley, 1 *khar*, makes 8 [*deben*].
 19. Given [to him in] Year 5, the third month of Summer, day 3, by the vizier:^{ll} *ti-šps*-wood, 20 (pieces), makes 5 *deben*; [incense], 15 *hin*, makes 3 *deben*; remaining 10; bronze, 5 *deben*.
 20. Given to him b[y]:^{mm} honey, 7 *hin*, makes 5 *deben*.
 21. (to be examined) Given to hi[m by]nakhte(?): sweet *sgnn*-oil, 2 *hin*, makes 20 *deben*; sesame-oil, 3 *hin*, makes 6 *deben*.

Recto col. III

1. Given to him in the houseⁿⁿ: 1 chest,^{oo} makes 20 *deben*.
 2. Given to him when the ships of the treasury^{pp} came, by Pnūdjem:^{qa} salt, 2 *oipě*, makes 4 *deben*;
 3. natron, 30 bars,^{rr} makes x+2 *deben*; salt, 15 bars, makes 5 *deben*;

4. *ti-šps*-wood, 20 (pieces), makes 5 *deben*; *nkpt* <and> *iwyfyt*, 1 *msti*-sack, makes 1 *deben*; rushes,^{ss} 18 *deben*(?), makes 2 *deben*.
5. (taken)^{tt} Given to him by Pnūdjem, [the guardian?] of the necropolis: wine, 2 *mnt*-jars, makes 4 *deben*.
6. Given to him by the house^{uu} of the temple for the festival of Amūn: *ti-šps*-wood, 20 (pieces), makes 5 *deben*; incense, 10 *hin*, makes 2½ *deben*.
7. Given to him in Year 6, the second month of the Inundation season, day 5, by the lady Tmōrc: *ibw*, 1 *oipě*, makes 2½ *deben*;
8. *lubja*-beans,^{vv}, 2 *oipě*, makes 1 *deben*;
9. *isy*-wood,^{ww}, 1 *khar*, makes 120 *deben*.
10. Total given to him, consisting of all kinds of money,^{xx} except for loaves, fish, vegetables, and meat: 815 *deben* of copper.
11. Given to him by the scribe of the treasury Pbēs at one time^{yy} in exchange for the uninscribed coffin^{zz} and the (female) statue of Isis:^{aaa} 395 *deben* of copper.
- 12.¹ Total given to him [in goods of] every kind: 1210 *deben* of copper.
13. Given to him, from Year 5,^{bbb} theth month of the Inundation season, day 1, of King Nebmaꜥrēꜥ-mia[mūn] until Year 7, the third month of Summer, day 26, of the king of Upper and Lower Egypt
14. Usimaꜥrēꜥ-setpenrēꜥ-mi(amūn), L.P.H., our Lord, makes year various loaves:^{ccc} 100, makes it^{ddd} 14, makes various loaves: 1000+x
15. Total altogether: 1364 *deben* of copper.

Notes


a. The scribe of the treasury Pbēs occurs again in *rt.* II, 6 and III, 11, and in *vs.* II, 3 and 4. He is probably also mentioned in *vs.* I, 1 (written Bēs only). Several scribes of this name are known during the Twentieth Dynasty, e.g. the scribe of the necropolis Pbēs of Pap. Abbott, v, 17 and vi, 11; the scribe of the Turin Strike Pap., *rt.* 3, 21 (= Gardiner, *RAD* 58, 16), also mentioned without title *vs.* 6, 7 (= op. cit. 51, 6); the scribe of the vizier Bēs of O.IFAO 1275, 2 (unpubl.; dated to the reign of Ramesses III by the mention of the vizier To). It is not, however, possible to identify any of these individuals with certainty, since the name was common at that period (e.g. Gardiner, *The Wilbour Papyrus*, iv, Index 9). Bēs and Pbēs are variants, *p* being sometimes added and in other instances left out in names of the Twentieth Dynasty.

b. *sgnn* (also II, 21): used as ointment as well as for the lamps; cf. Helck, *Materialien*, iv, 700 ff.


c. The photograph shows what might appear to be 1¼ (⋅ ¼), but on the original the point is clearly part of the horizontal stroke of ¼. For the notation of *khar* and *oipě* cf. Gardiner, op. cit. II, 61.

d. The sign for ¾ *khar* or 3 *oipě* (⋅) is somewhat unusually shaped, but cannot very well be anything else. Černý read a large O, but this seems to me impossible.

e. *wmnt*: probably the rest of *tꜥ-wmnt*; cf. Helck, *Materialien*, iv, 677, no. 70. *tꜥ-wmnt* is a particular kind of loaf; cf. also *ꜥꜥw ꜥꜥ n wnm*, Pap. Harris, I, 17b, 8 and 72, 5.

f. : not certain. The gap is too wide for *iri-n dbn* 10, 'makes 10 *deben*' (of weight).

g. ¼: of unusual shape.

h. [*wꜥd*] *sm*: Helck, *Materialien*, v, 799 makes a clear distinction between *sm* and *wꜥdt*, in spite of the hybrid writing , frequently found in ostraca; cf. also Helck, *JARCE* 2 (1963), 67 f. If the restoration is correct, we have here *wꜥd sm*, as e.g. in *Hier. Ostr.* 61, 2, 4 and O. DeM. 408, 2. The distinction between *wꜥdt* and *sm* seems to me far-fetched, for though vegetables may

¹ The following lines, so far as they were then known, are transcribed and translated by Peet, *JEA* 11 (1925), 74.

well have been planted both in gardens and in the fields, I doubt whether the Egyptians would have been as careful in differentiating them as Helck presumes.

i. The form and appearance of Δ here are very strange. One would expect a word for some kind of cloth, such as $\dot{s}m\epsilon$, which is normally used for *idg*-garments. I cannot, however, recognize this, or any other sign except Δ .

j. *idg*: I am inclined with Budge (*Dict.* 104) to see in this garment a kind of headcloth. Jéquier (*BIFAO* 19 (1922), 54) was certainly correct in deriving *idg* from the verb *dg*, 'to cover' (*Wb.* v, 499). The only text known to me which gives any clue to the identification of the object is O. Cairo 25 218, 15 (cf. Müller, *Liebespoesie*, pl. 17 and p. 43), referring to 'the *bjk*-oil, which is on her *idg*'. Müller translates here 'Kopftuch', and I can think of no better explanation of this passage. For an actual object of this kind cf. Schiaparelli, *La tomba intatta dell'architetto Cha*, fig. 80 and p. 106 ('l'accappatoio di Mirit, una specie di ampio lenzuolo con frangie, tutto macchiato di olio').

k. *tm*: lit. 'complete', as against *wgs*, 'guttled'; cf. Janssen, *Two Anc. Egypt. Ship's Logs*, 32.

l. *Pds*: the reading is far from certain. The name occurs in several ostraca of the first year of Ramesses IV (e.g. O. DeM. 45, 3 and *passim*; O. DeM. 46, 1 and *passim*), but a scribe *Pds* seems to be unknown elsewhere.

m. *sdj*: for the different writings cf. Gardiner, *Hier. Pap. in the Brit. Mus.* 1, 25, n. 13 (Pap. Ch. Beatty III). It is also written $\text{||} \equiv \delta$, meaning, literally, 'tail'. Since *sdj* is rather common in the ostraca, the object will have belonged to the normal dress of ordinary men. It was probably a particular kind of apron, since it is said to be worn by the *Trk*-people (Pap. Koller 4, 6) and possibly also by the prince of Joppa (Pap. Harris 500, *vs.* I, 11; cf. Lefebvre, *Romans et contes*, 128). In Pap. Anastasi IV, 3, 1 (= Pap. Koller 3, 2) a skipper's apprentice is said to bind strips of cloth around his waist (??) in order to give the appearance of wearing a *sdj*-garment. For some references for *sdj* cf. Caminos, *Late Eg. Miscellanies*, 136.

n. $\dot{s}m\epsilon$: cf. Helck, *Materialien*, v, 923, where the word is explained as being derived originally from the $\dot{s}m\epsilon$ -plant. But see now Černý, *Inscr. from the Tomb of Tutankhamūn*, 10, n. 3.

o. *djyw*: the writing $\text{≡} \cup \delta$ is usual in business texts and ostraca, and ≡ and \cup are easily confused. For the different writings cf. Dévaud, *ZÄS* 49 (1911), 106 ff. The exact nature of the *djyw* is uncertain, the word being usually rendered as 'loincloth'. It is worn both by men and women (e.g. Pap. Cairo 65 739, 3 = *JEA* 21 (1935), 143, n. 3, and Brunner, *Die Lehre des Cheti*, 8, 5 = pp. 23 and 172). Brunner is probably correct in translating 'Arbeitsschurz' (op. cit., *passim*, particularly 6, 2 = pp. 22 and 126), and it seems indeed to refer to the triangular loincloth, which labourers are shown wearing in tomb paintings and which has been found in great numbers in undisturbed tombs of the New Kingdom (cf. Schiaparelli, *La tomba intatta dell'architetto Cha*, 92 ff.; Bruyère, *Rapport des fouilles de Deir el-Médineh*, 1934-5, 60 f.; Murray-Nuttall, *Handlist . . . of Objects in Tutankhamūn's Tomb*, no. 46 and *passim*).

p. Only two strokes are left, the rest being lost in a lacuna wide enough for two more strokes. Since in *rt.* II, 21, a *hin* costs 2 *deben* it seems probable that this is also the case here.

q. ϵfnw : Egyptian footwear normally consisted of a simple sole with one transverse and one longitudinal thong, but there was also another type, more like a boot or sock, which enveloped the foot and part of the ankle (cf. *MDAIK* 12 (1943), 66 and fig. 30). This type was worn by women and children (cf. Frankfort-Pendlebury, *The City of Akhenaten*, II, pl. 35, 4) and was made of leather. This is clearly what is meant by the word ϵfnw , from the verb ϵfn , 'to envelop'. For other instances cf. *Hier. Ostr.* 85, 1, *vs.* 16 and O. DeM. 213, 6.

r. *scb*: the nature and shape of this loaf or cake are unknown. For references cf. Helck, *Materialien*, IV, 674, no. 49.

s. *mnk nhbt*: cf. also *Hier. Ostr.* 28, 2, 9. The nature of *mnk*-wood is unknown (cf. *Wb. der aegypt. Drogennamen*, 248 f.). The very low price of the *nhbt* and the high numbers here and elsewhere

(O. DeM. 267, 2: 27 pieces; O. Gardiner 141, 1-2, unpublished: 31 pieces) cast some doubt on the translation 'yoke'. One eleventh of a *deben* for the yoke of an ox seems improbably cheap. Possibly some kind of carrying pole is meant (cf. *mꜣwꜥ*). Cf. also O. Cairo 25556, g.

t. smtr: one of the diacritical marks commonly found before the lines of business papyri; see Gardiner, *The Wilbour Papyrus*, II, 185, and Helck, *Materialien*, v, 903. To this class of marks belong also the big points in front of several lines.

u. pꜣ pr-ꜥnh: in spite of the town determinative it seems certain that here the reference is not to the town Pi-ꜥnhk (cf. Gardiner, *Onomastica*, II, 48* f.), but to the 'House of Life'. Indeed, in l. 13 *pr-ꜥnh* is written without the town determinative, and it is probable that in both instances the House of Life of the temple of Medīnet Habu was meant.

v. Though $\overset{\circ}{\text{h}}$ is completely lost in the lacuna, a price of 24 *deben* for 3 *khar* is in accordance with the other prices. For grain-prices see below, p. 93.

w. ibw (also in l. 14 and III, 7): as far as I am aware this word occurs in the same form only in Pap. Anastasi IV, 14, 7, in the expression *ibw n dnrg*, which Caminos (*Late Eg. Miscellanies*, 207), following Loret (*Rec. Trav.* 15 (1892), 123 f.) translates as 'pulp of carrots' (or 'gourds'? cf. Caminos, op. cit. 76)—though Loret gives no reason for his translation. Whether there is a connexion between this *ibw*, written with the heart-sign, and the plant *ibw* (cf. *Wb. der aegypt. Drogennamen*, 20 ff.), usually written with the kid (E8), remains uncertain. In O. Cairo 25 231, 7 (*ZÄS* 38 (1900), 37) *ibw*, written with a kid, is measured in *msti*-sacks and this might constitute a link between the present word and the plant. The nature of both, however, is equally unknown.

The quantity of *ibw* was probably $2\frac{1}{2}$ *oipē*, as in the other instances in this text.

x. ꜣš(?) (also l. 13): the two ticks at the end may stand for another sound, such as *m* (**ꜣšm*); cf. *nkꜣt* and *iwfyt* below, l. 15. From the writing it appears to be a foreign word for a plant or a kind of fruit.

y. sty (also l. 13): cf. Janssen, *Two Anc. Egypt. Ship's Logs*, 87 f. and *JEA* 49 (1963), 69. Note that in l. 13 the word is determined by 𓆎 . The combination of *ibw*, *ꜣš(?)* and *sty* strengthens the possibility that all three are kinds of fruit, kernel, or the like.

z. The reading tnr is very doubtful, *rnꜣ* or *trꜣ* being also possible palaeographically, though no fish with any of these names is known. Moreover, the transcription with 𓆎 is uncertain.

After the fish-determinative one or more signs are almost completely blotted out, leaving only a thick horizontal stroke. It may have been 𓆎 , *wgs*, 'gutted', or something similar.

aa. štꜣ: since essential parts of the word are lost in a lacuna, I do not know what is meant. The first signs after the gap look like 𓆎 , 'called', but what follows is so much interspersed with ticks, that it is impossible to read. The expression might point to a place or a building, *štꜣ . . .* being possibly *štꜣyt*, 'cellar'; cf. O. DeM. 112, 2; 198, vs. 3; Pap. Bulaq 10, vs. 5 and 10 = Spiegelberg, *Studien und Materialien*, 18-19; O. Louvre 2425, 3 and 5 = *ibid.* 29. A place called *štꜣ* in the neighbourhood of Thebes is unknown. *štꜣ* in Brugsch, *Dict. géogr.* 1337 (= Pap. Louvre 3226, xxxi, 3) is clearly the present word.

bb. The left stroke of the four has been added afterwards over the small ligature for dbn.

cc. dmy: unless the signs over l. 12, which I am unable to decipher, contain an addition to *dmy*, this word probably indicates the village of the necropolis workmen; cf. *pꜣ dmy* in Pap. Salt 124, vs. I, 15 (= *JEA* 15 (1929), pl. XLVI) and O. DeM. 297, 6.

dmy may be translated by either 'town' or 'village' (cf. Gardiner, *Onomastica*, II, 1*), and in view of the smallness of the settlement at Deir el-Medīna I should prefer 'village', despite the walls around it. Our European terms are clearly inadequate.

dd. ꜣdw: cf. Helck, *Materialien*, v, 834, with references.

ee. The remains of the signs exclude 𓆎 , but *šbd* 2, 3 or 4 are all possible.

ff. ti-šps(?) (also II, 19; III, 4 and 6): in all four instances we have the same word, determined

by the tree-sign, though the reading is not certain, particularly of the second sign. In III, 4 and 6 Peet and Černý read a simple 𓆎 , but 𓆎 seems to me more probable; cf. O. Cairo 25 677, *vs.* 36 and 25 678, 12, and Möller, *Hierat. Pal.* II, 2 (Lower Egyptian writing!).

For the meaning cf. Helck, *Materialien*, v, 810, and for the oil of this tree, Helck, *op. cit.* IV, 702. The *Wb. der aegypt. Drogenamen*, 550 f., translates hesitatingly 'cinnamon', following Loret and Ebbell.

The absence of a measure of capacity is usual with pieces of wood, charcoal, and the like.

gg. nk. . , iw. . (also III, 4): these words, written in full, occur e.g. in Pap. Harris I, 16a, 4-5 (cf. Helck, *Materialien*, v, 759 f.). They were measured in *msti*, but their meaning is unknown.

msti is a 'sack', sometimes said to be made of leather (e.g. *Hier. Ostr.* 63, 1, 4; O. DeM. 231, 5), but in other instances probably of basketry.

hh. Cf. the date in the preceding line and our n. *ee*. There is a small difference between the remains of the month number here and that in l. 15, so that one might incline to read the former as 𓆎 and the latter as either 𓆎 or 𓆎 . However, the offering to Ptah is mentioned in O. DeM. 401 as taking place in 'Year 2 (of Ramesses IV?), the third month of Winter, day 4', while a festival of Ptah is mentioned on III *prt* 1 (Botti-Peet, *Giornale della necropoli di Tebe*, pl. 50, l. 5); cf. for these references Helck, *Journ. Econ. and Soc. Hist. of Orient*, 7 (1965), 158. If in the present text the same Ptah-festival is meant, the date of the delivery cannot be other than the end of the second month, in which case 𓆎 was written both in ll. 15 and 16.

ii. The gap is too small for four strokes + 𓆎 , but the sign for 5 (cf. l. 19) would just fit in.

jj. Here I am at a loss. The first sign, or what is left of it, looks like 𓆎 ; the last might be either 𓆎 or 𓆎 . A word ending in an *f* is hardly likely; further, since the stroke is not sufficiently steep for an *f*, the rope-determinative is probably intended. A reading '100' is at best improbable, for then the word would have no determinative. Moreover, 101 pieces of some commodity for 3 *deben* would be very odd.

kk. pꜣ-rꜣ-ḥt: cf. l. 18. The same name occurs in Pap. Wilbour, A 70, 48, where Gardiner reads *pꜣ-rꜣ-ḥt*; cf., however, 'Okhet: O. DeM. 142, 11; 394, 4; 427, 3. 'He-whose-body-is-big' is very well suited for a name. For the alternative use of *pꜣ* in names cf. n. *a* above.

It is not clear whether P okhet delivered the unknown object mentioned before, since there is a contradiction between this and 'by him' earlier in the line.

ll. The name Tjati being rare (cf. Černý, *JEA* 26 (1940), 129, n. 4), it seems preferable to suppose that the vizier is meant. Possibly there is a connexion with the 'house of the temple' of III, 6; cf. n. *uu*, below.

mm. It is probable that in the lacuna and the first signs after it a name is lost, preceded by *m-drt*. A long stroke under the line is possibly part of the letter *f*. Černý read 𓆎 in front of *bit*, and took the stroke to be part of a *c*. There is, however, far too little room for a complete dating.

nn. rt: for the difference between *rt* and *pr* cf. Gardiner, *The Wilbour Papyrus*, II, 34 and Helck, *Materialien*, III, 340 f.

oo. grwt: some kind of box. Judging by its price (usually 10 *deben*) it was of a different type from the cheaper *fd*, and Helck therefore calls it a 'Truhe', 'chest' (*Materialien*, v, 911). In Pap. Harris I, 13b, 10 and Pap. Mayer B, 13 it is used for garments, in Pap. B.M. 10053, *rt.* 5, 6, and 13 for silver.


pp. rꜣ-ḥd: cf. Pap. Harris I, 5, 1. The sign after the ship looks like 𓆎 , but plural strokes of a somewhat unusual type are more probable.

qq. pꜣ-ndm (also l. 5): the reading is not completely certain, but the name Pnūdjem occurs frequently under the Twentieth Dynasty. The title in III, 5 may be *sꜣw n pꜣ hr* (cf. O. Cairo 25 831, 1-2), although *idnw* is also possible palaeographically. The usual expression, however, is *idnw n tꜣ ist*. If *pꜣ hr* means literally 'the (king's) tomb', the translation 'guardian' is almost certain.

rr. dbst: in *Hier. Ostr.* 20, 2, 4 six bars weigh 1 *oipě*, which is in accordance with the weight of a bar here (1 *oipě* salt = 2 *deben*; 1 bar = $\frac{1}{3}$ *deben*).





ss. gš: saccharum aegypt. Willd, according to Keimer, *OLZ* 30 (1927), 145, n. 2. Cf. Caminos, *Late Eg. Misc.* 128, and Helck, *Materialien*, v, 814.

The first *deben*, if correct, indicates the weight, the second the price.

tt. The diacritical marks in front of the line look like , 'taken'; cf. Gardiner, *The Wilbour Papyrus*, II, 71, and Janssen, *Two anc. Egypt. Ship's Logs*, 44.






uu. m-drt. The use of this expression here in connexion with *rt*, and not with a person, proves that it no longer meant 'by the hand of', but simply 'by'.

Exactly what kind of building 'the house (*rt*) of the temple' is I do not know—possibly part of the temple of Medînet Habu, which is called *tr hwt* (cf. Černý, *JEA* 26 (1940), 127 ff.). Cf. also Pap. B.M. 10383, 1, 6, where 'the house of the vizier (or: of Tjati?) in the temple' is mentioned. See n. *ll* above.

vv. ir. : probably *iry* = *iwry*, 'lubja-beans' (Copt. $\sigma\Upsilon\rho\omega$, $\alpha\rho\omega$: cf. Crum, *Copt. Dict.* 489a); cf. Helck, *Materialien*, v, 801, with references. The word occurs also in the writing    and var. in *Hier. Ostr.* 35, 1, 1, 8 and O. DeM. 121, 2-3; written with  it is fairly common. It is usually measured in *oipě*.

After *iry* there is room for another short word which I cannot decipher.

ww. isy: a common kind of wood, possibly the same word as *isr*, 'tamarisk'; cf. Keimer, *Gartenpflanzen*, 155. In O. Turin 9599, 2 (unpublished) it is used for a coffin, which costs 80 *deben*, an extraordinarily high price. Elsewhere one finds yokes (*nhbt*: O. Gardiner 141, 1; unpubl.) or beams (*sy*; *Hier. Ostr.* 63, 1, 13), made of it, the latter not at all expensive.




I cannot recognize the next word. The first sign looks like  or, less probably, . Could it be   , 'wood tar' or 'pitch'? Cf. Harris, *Lexicograph. Studies in Anc. Eg. Minerals*, 173.

xx. For *hd*, 'money', cf. Peet, *Griffith Studies*, 124 f.


The exemption of some food is explained e.g. by II, 4, where loaves and vegetables are not priced.

yy. hr sp: I know only the expression *m sp* (*Wb.* III, 438, 8-9), Copt. $\mu\sigma\pi$. The meaning in the present text is clear.

zz. wt: this word is normally masculine (e.g. O. Michaelides 14, 2 and 6; O. DeM. 73, 1). See also *vs.* II, 4.

As for the next word, I take it to be *ncc*, 'smooth', i.e. 'uninscribed'; cf. Pap. Berlin 10 496, *vs.* 13 (= Erman, *Sitzb. Pr. Akad. der Wiss.* (1910), 332): *wty, iw-f ncc* (  ), *iw bn sw mtnw hr rn n rmt nbt etc.* See also O. DeM. 49, 3 and O. Černý 20, 1 and 8 (unpubl.). For *ncc* cf. *MDAIK* 15 (1957), 297 f.

aaa. Note the absence of the determinative for goddess. I do not, however, think that *st* stands for *st-mst*, since in that case the article would have been used.

bbb. There is a stroke (tail) of the number visible above the next line, which proves that year 5 was written: cf. II, 19. The space for the season is very small, so that  seems most probable.

ccc. The tail of  is still visible.

ddd. The expression *iri-n st* is very unusual.

The three texts on the verso present far greater difficulties than that of the recto, and col. I is the most illegible and incomprehensible of all. Neither Peet nor Černý seems to have understood its meaning. The key-word seems to me to be *idr*, 'herd', which occurs several times. Moreover, in ll. 2, 5, 7, and all uneven numbered lines thereafter

a temple or other religious institution is mentioned, so that one is led to conclude that the text provides information about temple herds. The nature of this information seems to be hidden in the even numbered lines from l. 8 onwards, which, unfortunately, I have been unable to decipher. The text has what appears to be a heading in l. 1, but this also remains obscure. The opening words might be *nṣ ddḥw*, though the translation is uncertain. The verb *ddḥ* (*Wb.* v, 635) means 'to imprison', the basic meaning of which must be 'to confine', either by binding (determined by ⲉ) or shutting in (ⲛ). This would be fairly applicable to cattle, which are 'tethered' or 'corralled' (cf. O. Dem. 582, 4). A substantive from *ddḥ* is unknown to the *Wb.*

The mention of the treasury scribe Pbēs seems to be the connexion with the recto, where he also occurs. Possibly the papyrus was originally among his private papers, and he used it to scribble down, in an extremely cursive hand, some hasty notes.

The temples mentioned in this column are:

the House of Amūn (ll. 2, 7 and 19; in the second instance in the title of an official)

the House of Mīn (l. 5)

the Mansion of Usimaⲉrē-setpenrē in the House of Amūn (ll. 9 and 11)

the House of Onūris (l. 13)

the House of ⲉAnti (l. 15)

The House of Amūn is the temple of Medīnet Habu; the Mansion of Usimaⲉrē-setpenrē, the Ramesseum. The Houses of Mīn, Onūris and ⲉAnti are smaller temples in Egypt, the first two probably those of Coptos and This, the last possibly that of U-ⲉAnti.¹

Line 17 presents a particular difficulty. One is tempted to read, as Peet and Černý, *nṣ ḥtmw n Pth n Mr-n-[Pth]*, but what is meant by these fortresses? A 'fortress of Merenptah' is mentioned in Pap. Anastasi VI, 55 and 60 (cf. Caminos, *Late Eg. Misc.* 294), but Pth does not occur in the name. In any event it is impossible to read *pr n Pth n Mr-n-Pth*, the name of a temple in Memphis.² The real meaning of this entry remains obscure to me.

Since the reading of the text of verso col. I is so uncertain I prefer not to attempt a translation. Fortunately, col. II is more satisfactory. It is in fact written in a better hand than any other part of the papyrus, but this does not mean, however, that its contents are completely clear. Peet (*JEA* 11 (1925), 74) described it, from its writing and position, as a filing docket. If this is correct it may contain the clue to the understanding of the recto text, a point which will be discussed below. Here follows a translation:

Verso col. II.³

1. Year 7, the first month of the Inundation season, day 10. This day^a
2. took place the valuation of the goods of ⲉOnakhte^b^a
3. together with the scribe of the treasury Pbēs^c^a
4. Goods of the scribe of the treasury Pbēs to him the coffin,^d

¹ Cf. Gardiner, *The Wilbour Papyrus*, II, 52; id. *Onomastica*, II, 97* f.

² Cf. Gardiner, *The Wilbour Papyrus*, II, 153, § 232.

³ Translated, so far as it was then known, by Peet, *JEA* 11 (1925), 74.

5. together with all his goods:
6. 324 *deben* of copper. <Plus?> 60. Total 300 [+x]^e *deben* of copper given to the chief of the crew Nekhemmūt:^f 34 (sc. *deben* of copper)
7. Delivered to ‹Onakhte ‹and?› Nekhemmūt of the production Total: 350 [*deben*] of copper.
8. 336^g *deben* of copper. <Plus?> 30. Total 300 [+x]^g *deben* of copper . . .

Notes

- a. It is uncertain whether any words are lost and, if so, how many.
- b. ‹Onakhte does not occur on the recto as it stands. The name is well known in Deir el-Medīna from the Nineteenth Dynasty (e.g. Pap. Salt 124, II, 12) until the second half of the Twentieth Dynasty (e.g. *Hier. Ostr.* 43, 4, 6–7).
- c. Cf. *rt.* I, 1 and n. a.
- d. This is probably the coffin mentioned in *rt.* III, 11.
- e. This must of course be 384.
- f. This Nekhemmūt is the son of Khons and the grandson of Nekhemmūt the Elder, both of whom were also chief workmen. For the family relationships cf. Bruyère, *Mert Seger*, 14 f. and fig. 10 (stela from the rock chapel A). Nekhemmūt the Younger occurs as chief workman from Year 4 of Ramesses IV (O. B.M. 5652, *vs.* 6; O. DeM. 133, 7) until Year 17 of Ramesses IX (Bottipeet, *Giornale della necropoli di Tebe*, pl. 25, col. IX, 2).
- g. The reading of the first number (336) is uncertain, but probable. In consequence the second must be 366, though I am unable to recognize the 60, while the 6 is completely lost.

The third text on the verso is the most fragmentary of all, parts of the lines being lost at the left hand edge of the papyrus. The writing is again very cursive, although less so than that of col. I. It contains a list of receipts and deliveries, probably to temples or similar institutions. In ll. 9–10 the temple of Medīnet Habu may be mentioned, but any other temple of Ramesses III is also possible, since the words after the king's name are lost. It may be that this text was also written by or for Pbēs, but this is not borne out by such words as remain. There is no recognizable relationship between *vs.* col. III and the main text of the recto.

Verso col. III

1. Received from this^a
2. by the scribe ‹Anmosē^b
3.^c Total
4. Given for the festival hall^d
5. in the presence of this god in
6. Received
7. Given for the foundation^e of this god
8. by the 'great one of the throne'^f of King
9. Together for the Mansion
10. Usimaṛē-miamūn
11. for the fishermen in this House
12. scribe Pentwēre of the

Notes

- a. *py* might also be the beginning of a name or even a complete one.
- b. *en-ms*: I do not know a scribe of this name in any other text.

proof of his seventh year.¹ It is unfortunate that the break in the lower part of *rt.* col. III of the present text makes a definitive conclusion impossible. One may only hope that the relevant fragment will one day be found in the mass of scraps in the Turin Museum. It is obvious that should it be proved that Ramesses VI reigned less than seven years, all the texts mentioned above are to be dated to the time of Ramesses VII.

As to the precise day of the accession of Ramesses VII to the throne, I am unable to agree with Helck² who, following Peet's study of the present text, states that since the entry of *rt.* II, 19 is of III *šmw* 3 of Year 5 and that of *rt.* III, 7 of II *šht* 5 of Year 6, the accession to the throne took place between these dates. Now it is obvious that every day will always fall between a pair of other days, but this is of no consequence to us. Since these dates are recorded rather far apart in the text it is quite possible that the lapse of time between *rt.* II, 19 and III, 7 was more than a year. On the other hand, the series of dates in *rt.* II, 15-16-17-19 looks more like a single group. The first of them is probably of II *prt* 6 of Year 4; the next one of II(?) *prt* 20 of the same year; l. 17 of x *šmw* 5, but of year 5(!), after which follows in l. 19, III *šmw* 3 of the same Year 5. I cannot interpret these facts in any other way than that the change of the year-number, i.e. the day of accession to the throne, fell between II *prt* 20 and x *šmw* 5, x being either I or II, since III *šmw* 3 follows in l. 19.

This conclusion almost completely contradicts Helck's suggestion, from Pap. Turin 1885, *vs.* I, that the accession to the throne took place between II *šmw* 3 and I *šht* 18,³ the only (unlikely) possibility covered by both series being II *šmw* 4. My arguments are further strengthened by the dates of the Pap. Amiens,⁴ which I have supposed for various reasons to have been written also in the reign of Ramesses VII.⁵ Here the change of the year-number takes place between III *prt* 29 and I *šmw* 7, which is well within the range mentioned above.

B. *The prices*

The prices mentioned on the recto need to be studied in connexion with other prices found elsewhere, but it seems useful to attempt here some provisional conclusions. On the whole the prices are the same throughout for a given quantity of some commodity. Thus 20 pieces(?) of *tī-šps*-wood always cost 5 *deben* (II, 15 and 19; III, 4 and 6); 1 *oipē* of *ibw* costs 2½ *deben* (II, 9?; II, 14; III, 7); 1 *hin* of sweet *sgnn*-oil costs 10 *deben* (I, 5; II, 21). There are indeed small deviations from this uniformity. In I, 10, 10 *hin*

¹ Cf. Hornung, *Untersuchungen zur Chronologie und Geschichte des neuen Reiches* (Wiesbaden, 1964), 98, and Černý, loc. cit. They quote Varille, *Karnak*, I, pl. 68, but the copies of this book which I have seen do not contain a very convincing photograph. There seems at least to be something wrong with this text, since the name of the season and the sign © after it are left out by the sculptor. Moreover, the beginning is not clear. If it is to be read 'Year 7', as Varille states (op. cit. 22), the position of the signs { ☉ || } would be most unusual. I have not, however, been able to recognize the ||, and || seems more likely; cf. also Sauneron, *Rev. d'Ég.* 7 (1950), 56. In any event the stela needs an accurate collation.

² *Anal. bibl.* 12, 127.

³ Op. cit., 128; cf. also above, p. 91 n. 4.

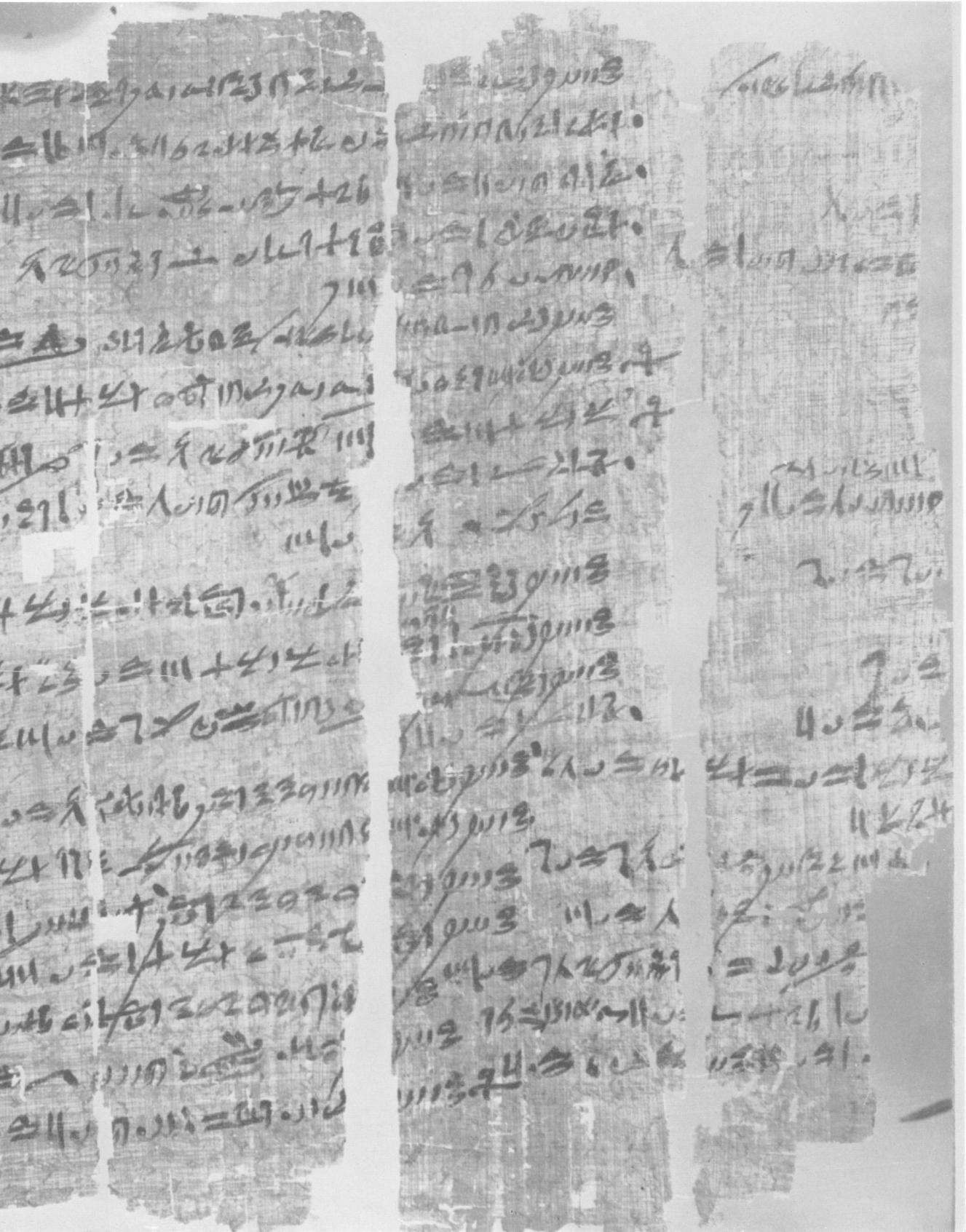
⁴ Gardiner, *Ramesside Administrative Documents*, 1 ff. The dates concerned are to be found in *vs.* 2, x+8 and x+9; cf. also Gardiner, *JEA* 27 (1941), 43.

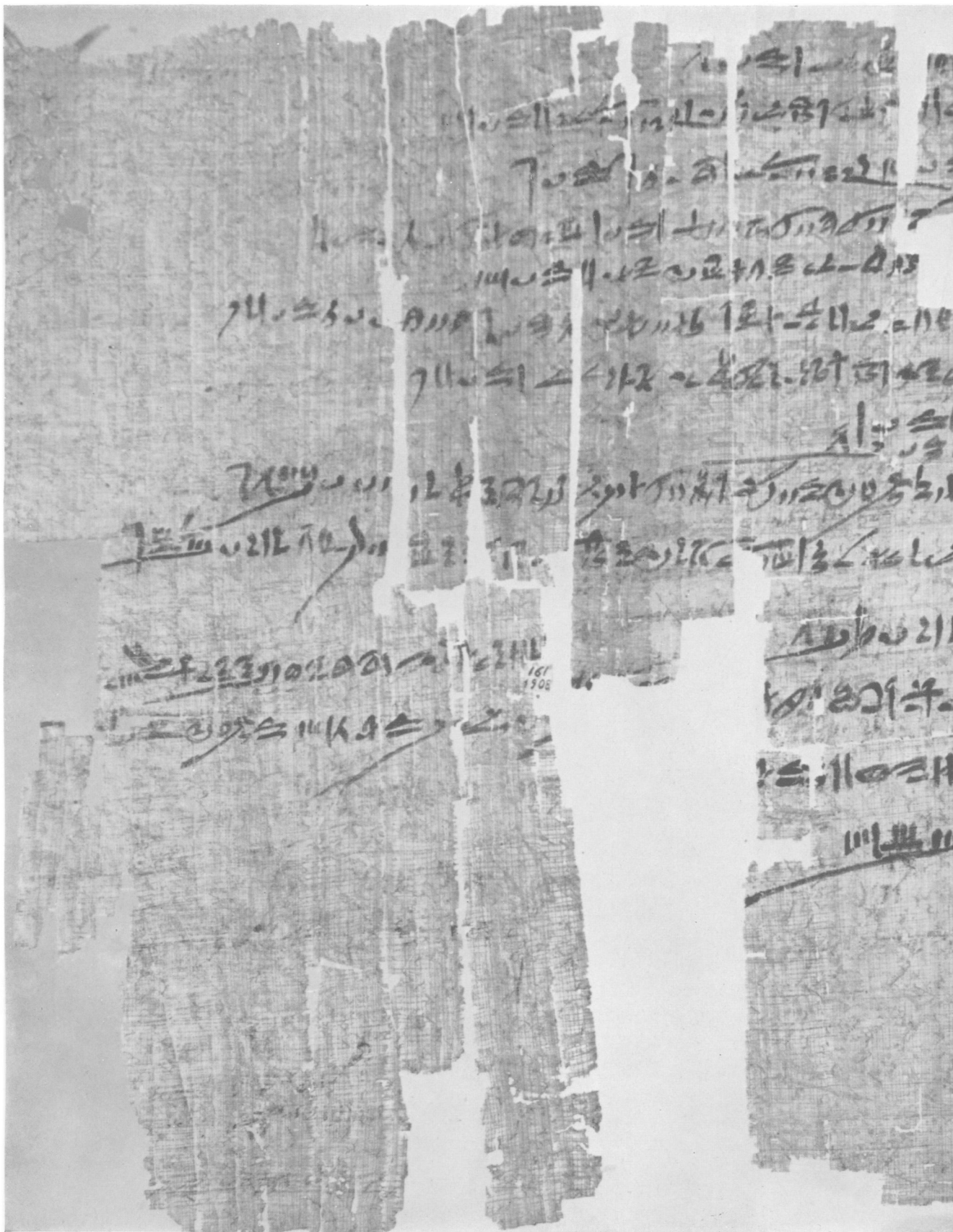
⁵ Cf. Janssen, *Two Anc. Egypt. Ship's Logs*, 56. I am no longer certain that my renumbering of the Ramessids there (after von Beckerath) is correct.

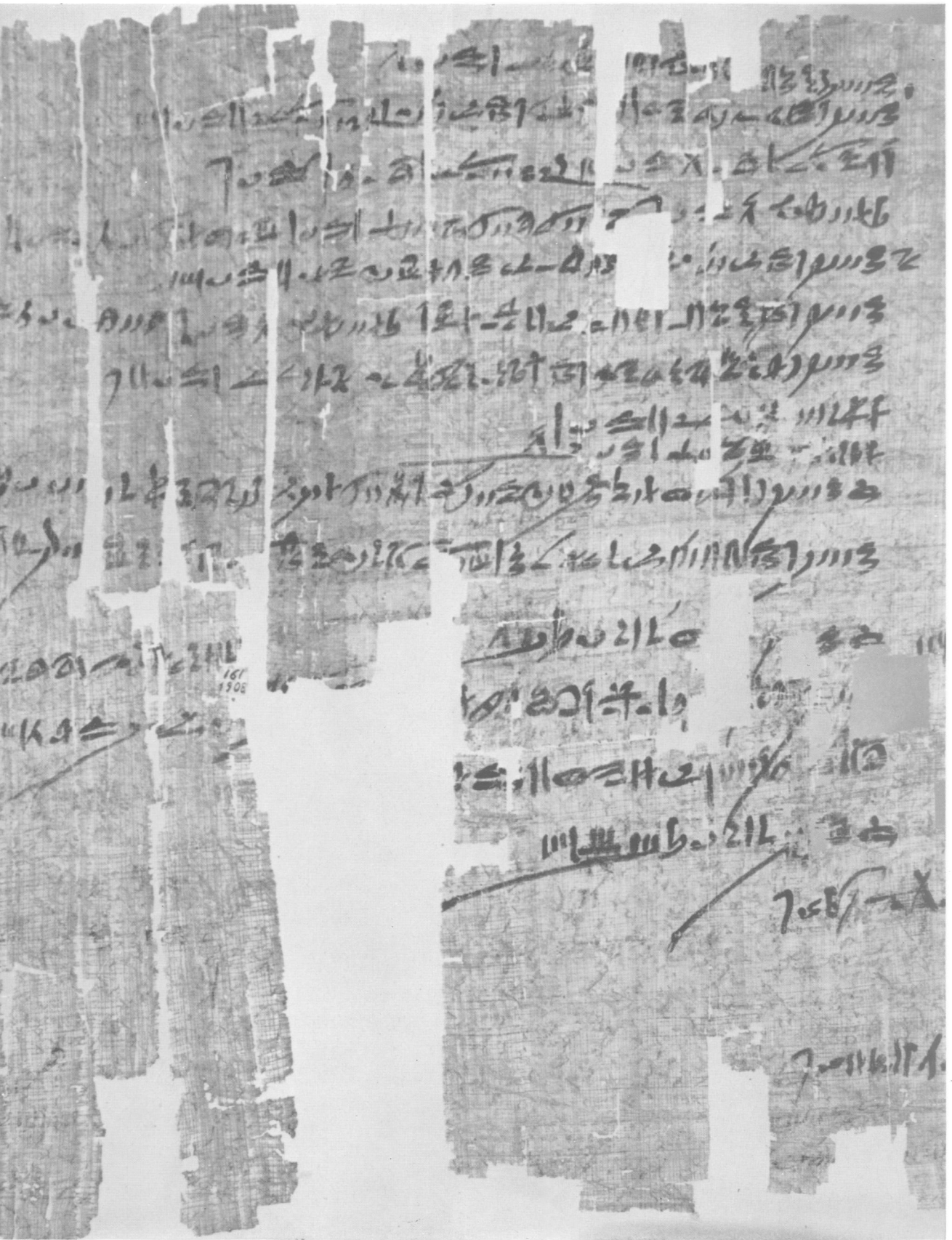
Fragment of papyrus with several lines of handwritten text in a cursive script, likely Demotic or Hieroglyphic. The text is partially obscured by damage and overlapping fragments.

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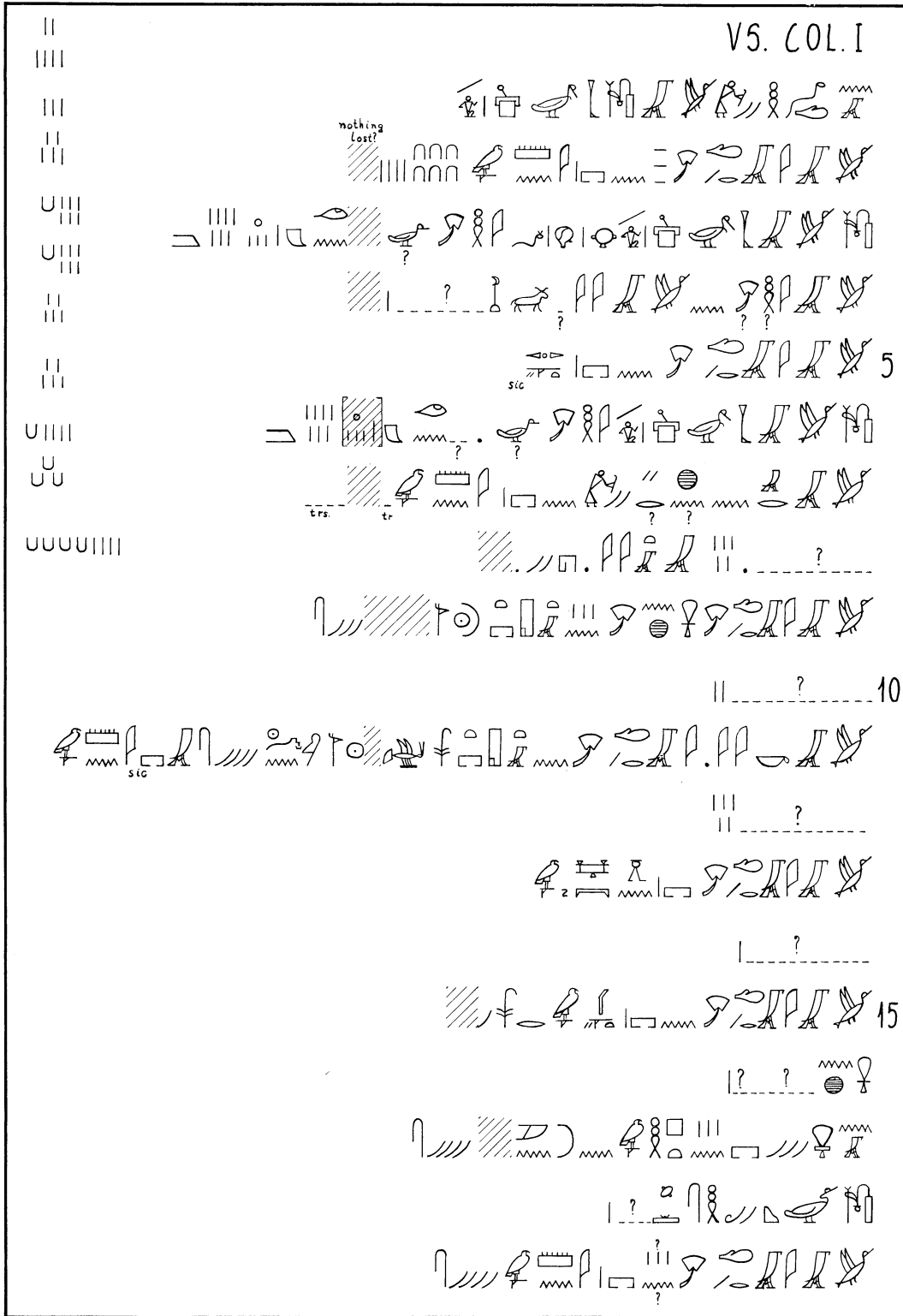




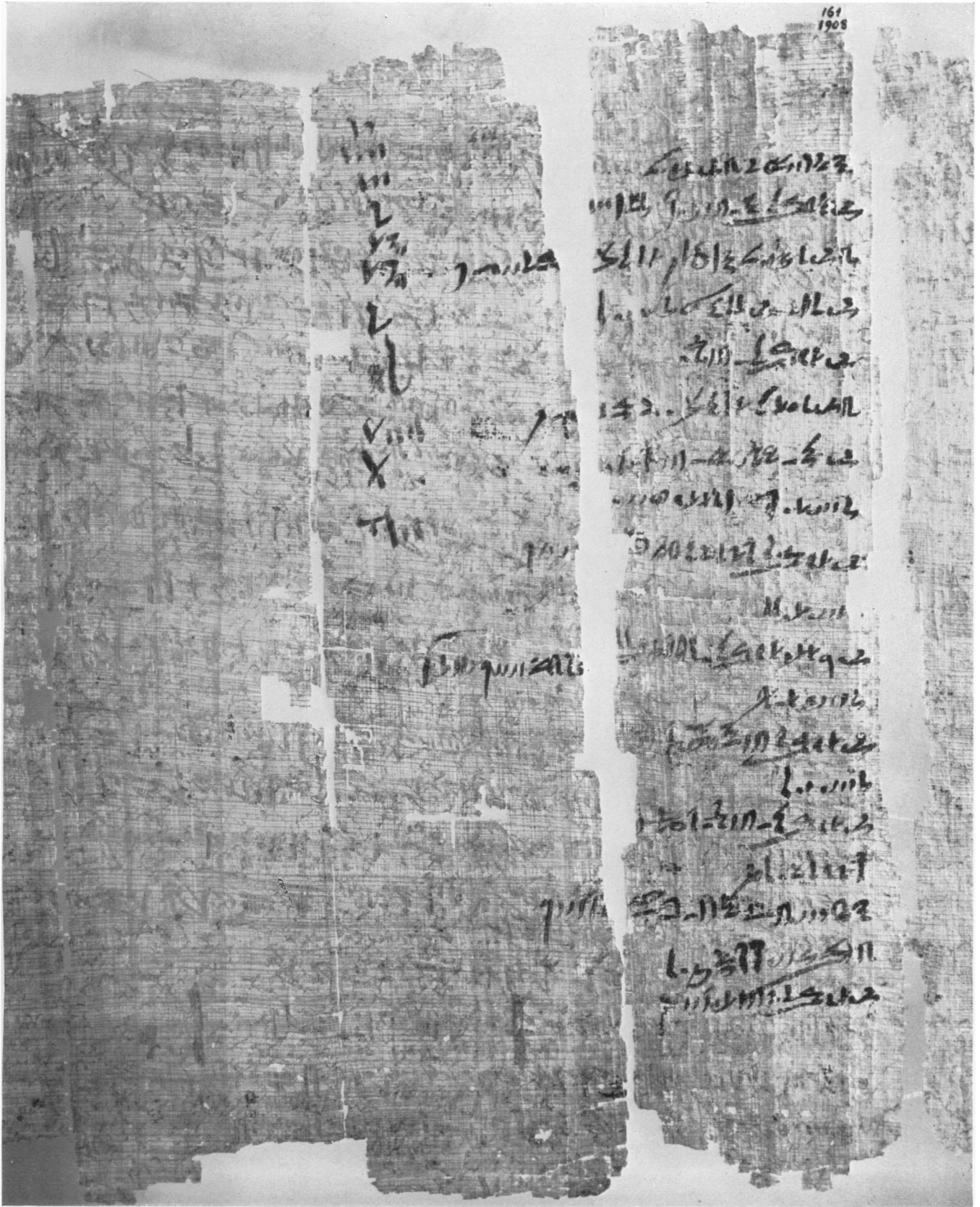


TURIN PAPYRUS CAT. 1907+1908

Recto III

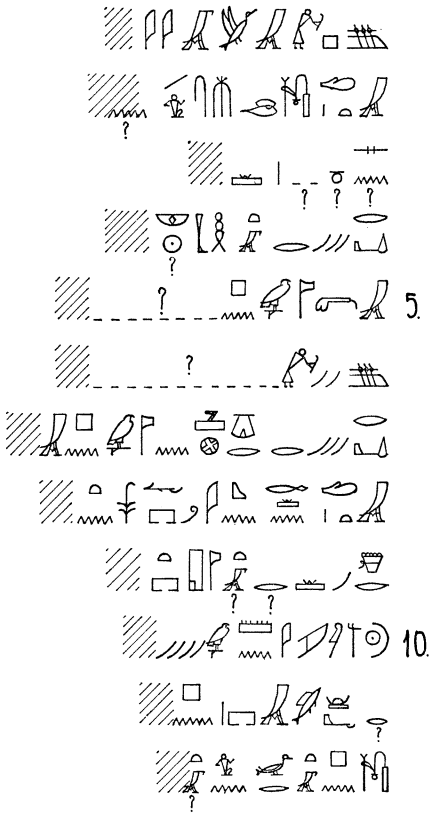


TURIN PAPYRUS CAT. 1907+1908
Verso I

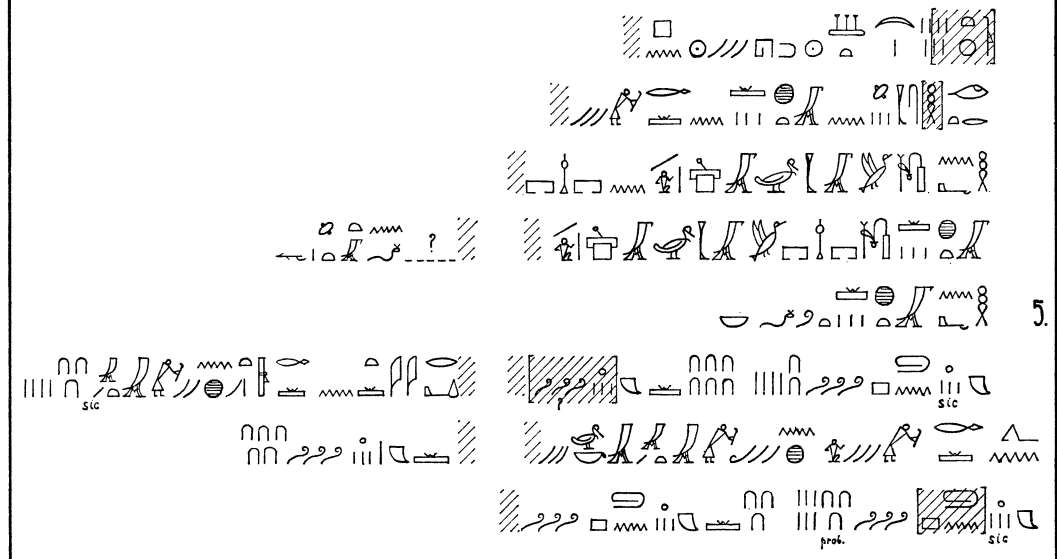


TURIN PYPYRUS CAT. 1907+1908
Verso I

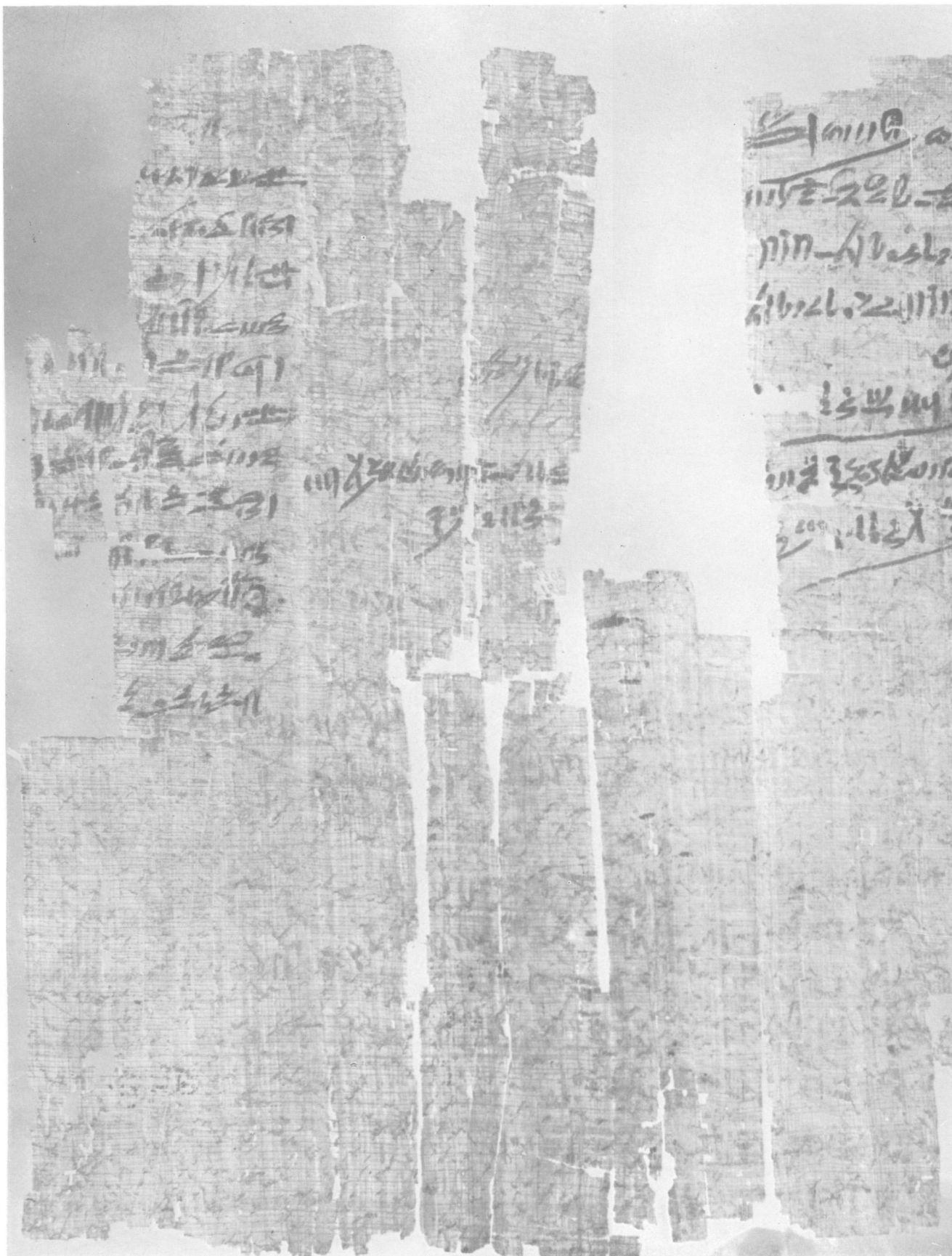
V5. COL. III

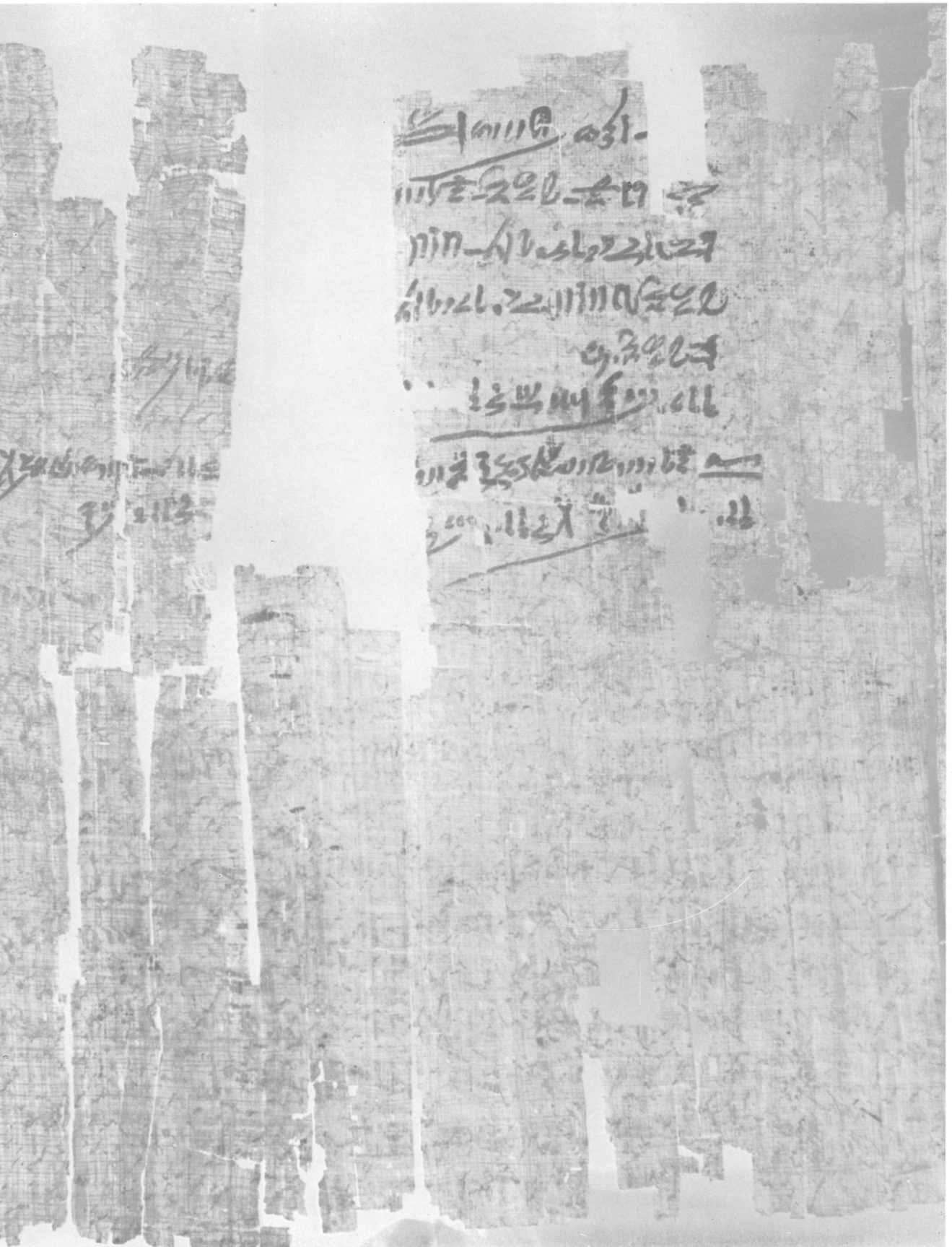


V5. COL. II



TURIN PAPYRUS CAT. 1907+1908
Verso II and III





TURIN PAPYRUS CAT. 1907+1908

Verso II and III

of incense cost $2\frac{1}{2}$ *deben* (i.e. 0.25 *deben* per *hin*, as also in III, 6), while in II, 5, 15 *hin* cost $3\frac{1}{2}$ *deben* (i.e. nearly 0.225 *deben* per *hin*), and in II, 19, 15 *hin* are 3 *deben* (i.e. 0.33 *deben* per *hin*).¹ As another example, 15 bundles of vegetables cost 4 *deben* (I, 19), while 20 bundles are also 4 *deben* (II, 8), and 110 yokes(?) are 10 *deben*, but 30 yokes(?) 6 *deben* (II, 6). It is not, however, certain that these differences are due to a real instability of prices during the years covered by our papyrus, i.e. to boom and/or decline. The differences are small enough to be explained at least partly by the characteristic Egyptian neglect of small fractions which occurs often in business papyri.

There appears, however, to be one exception to the price-stability, namely in the prices of grain. This fact, which is very obvious in our text owing to the many occurrences of barley- and emmer-prices, has been studied by Černý in his article 'Fluctuations in Grain Prices during the Twentieth Egyptian Dynasty'.² From the tabulated data Černý concludes a rise in Year 4 of Ramesses VII, and also, though hesitatingly, a fluctuation of grain prices during the year. More material is needed to see whether both or either of these conclusions can be proved, but as far as I am at present aware there is a strong probability that other price-notations of grain are in accordance with them.

C. General conclusions

As is the case with so many business papyri the nature of the texts here studied remains uncertain.³ We can but try to review the data in order to assess their implications. As stated above, *vs.* col. II looks like a filing docket, and its entries are dated to I *šht* 10 of Year 7, which is probably somewhat later than the date of the end of the recto text.⁴ Verso col. II would thus contain a summary of the recto text, which covers the period from Year 5 of Ramesses VI until Year 7 of Ramesses VII. However, the totals of *vs.* II are of a quite different order from those mentioned in *rt.* III, 10 and 12-15. Possibly this difference was explained in those parts of the lines of *rt.* III and *vs.* II which have disappeared. As it now stands the point cannot be satisfactorily explained.

Vs. II, 2 records 'the valuation of the goods of 'Onakhte'. Possibly this 'Onakhte was the 'him' to whom the commodities of the recto are delivered.⁵ *Vs.* II, 4, however, mentions the goods of the scribe Pbēs, but leaves us in the dark about the relationship between this man and 'Onakhte. To suppose that this connexion was explained in the first line of the recto text (*rt.* I, 1) is of no practical assistance.

¹ Since *dbn* is followed by $\frac{1}{2}$ it may be that $\frac{1}{2}$ *deben* has to be added, although the remains of the sign seem to exclude this reading.

² *Archiv Orientalní* 6 (1934), 173 ff., particularly 177 f. I do not agree with Černý in the reading of I, 15, where he saw $1\frac{1}{2}$ *khar*, and where I read $1\frac{3}{4}$ *khar*. The first price of emmer (Černý still translated 'spelt') in his table will not therefore be $12\frac{4}{5}$ *deben*, but $9\frac{7}{8}$ *deben* or, if the fraction is neglected, 8 *deben*, i.e. the same price as that of barley. One may add to Černý's table from I, 14: $\frac{3}{4}$ *oipē* (of barley?) makes 2 *deben*, i.e. 1 *khar* makes $10\frac{2}{3}$ *deben*, or, if here too $\frac{3}{4}$ *oipē* is in Egyptian arithmetic reckoned to be of the same price as 1 *oipē*: 1 *khar* makes 8 *deben* (as in l. 15).

³ Cf. Gardiner, *ZÄS* 43 (1906), 27.

⁴ If our conclusion that the accession of Ramesses VII to the throne took place between II *pṛt* 20 and II *šmw* 5 is correct, the date of *vs.* II (I *šht* 10) is later in the seventh year than that of *rt.* III, 13 (III *šmw* 26).

⁵ So too Peet, *JEA* 11 (1925), 74.

The reason why the goods are delivered 'to him' (‘Onakhte?') is mentioned twice, namely 'for the offering to Ptah' (*rt.* II, 16) and 'for the festival of Amūn' (*rt.* III, 6). The goods are delivered at various places, e.g. in the fortress of the necropolis (*rt.* II, 1), in the house of Pbēs (II, 6), in the village (II, 12), and in the House of Life (II, 13; also II, 7?); and by various persons, e.g. Any (II, 11), P^cokhet (II, 18; also II, 17?), Pnūdjem (III, 2 and 5), the lady Tmēre (III, 7), and Pbēs himself (III, 11). They consist of all kinds of commodities: grain, different kinds of food, and articles of clothing, but, except for a chest in III, 4, furniture is nowhere mentioned. In III, 11, a separate entry which follows the summary of III, 10, occur a coffin and a statue, and it looks as though this entry is of a different order from the rest of the text. It may be noted also that this is the place where Pbēs is the deliverer!

When the prices that are left are added together the total comes to $621\frac{1}{2}$ *deben*. This amount has to be increased somewhat by taking into account the numbers lost in illegible passages, so that it will not have been far below 650 *deben*. Compared with the summary of III, 10, viz. 815 *deben*, no more than 160–70 *deben* seem to have disappeared in the lost part of col. I, which now refers altogether to $126\frac{1}{2}$ *deben*. When one compares this with col. II (at least 321 *deben*) a loss of about 165 *deben* is consistent with the general trend of the text. The conclusion would then be that *rt.* I, 1 was indeed the first line of the text, as the surviving words themselves would lead one to suppose.

There remains one question, the heart of the matter: namely, why are the goods delivered (to ‘Onakhte?')? We do not know who this ‘Onakhte was, but it would seem improbable that he acted here as a private person. The papyrus has more the appearance of the rough draft of an official report. The role played by the scribe of the treasury Pbēs, on the recto as well as on the verso, points also to a semi-official document. The supposition that the contents of *vs.* col. I (and III?)—which seem to be unrelated to the rest of the papyrus—prove that Pbēs used the sheet afterwards for some (also semi-official) notes of a different nature also points to this. Could it be that our text belonged to the administration of the workmen's village in Deir el-Medīna? If so, the mention of the chief workman Nekhemmūt (*vs.* II, 6) would be explained.

It is unfortunate that the general conclusion reached is no more than a guess. Too little is known about the administration during the New Kingdom to resolve such questions. This should, however, be a stimulus to Egyptologists to publish more of these business papyri, incomprehensible though they appear in themselves, in order to enlighten bit by bit an important facet of life in ancient Egypt.

AN ANATOMICAL EXAMINATION OF THE PHARAONIC REMAINS PURPORTED TO BE AKHENATEN

By R. G. HARRISON

Derby Professor of Anatomy, University of Liverpool

IN a small unfinished tomb (no. 55) in the Valley of the Kings at Thebes the remains of a body were found in a damaged coffin in 1907. Since this tomb contained parts of a gilded shrine dedicated to Queen Tiye by Akhenaten, although his name had been erased, it is not surprising that attempts were made to identify the body with her. The first investigation of the sex of the remains attempted to demonstrate that they were undoubtedly female. When Sir Grafton Elliot Smith, at that time Professor of Anatomy in Cairo, later examined the remains, he was convinced that they belonged to a man. Since that time many attempts have been made by examination of the texts on the coffin,¹ or a re-assessment of the medical evidence,² to identify more accurately these remains.

Fairman in his review and re-examination of the inscriptions on the coffin and the 'magical bricks' found in the Tomb of Tiye concludes that the coffin was originally made for Meritaten.³ He presumes that her body was subsequently removed and the mummy of Smenkhkarē reinterred in it, 'the appropriate textual modification to the inscriptions being made at the same time, in places rather clumsily and maladroitly'. The reason for this, he suggests, is perhaps to be found in the fact that a substantial proportion of Smenkhkarē's funerary equipment had been taken over for the burial of Tutankhamūn.

There is no doubt that Smenkhkarē and Tutankhamūn were closely related to one another, perhaps even as brothers. Tutankhamūn married the third of Akhenaten's daughters; he was the immediate successor of Smenkhkarē and, as will be demonstrated later, there are certain physical resemblances between them.

It is apparent, therefore, that a re-assessment of the anatomical evidence for the identity of the remains found in the coffin discovered in the Tomb of Tiye is very necessary.

The basis of the anatomical examination

The only extensive anatomical examination of the remains excavated in 1907 by Mr. T. M. Davis was performed by Elliot Smith.⁴ At that time Elliot Smith was

¹ See H. W. Fairman, *JEA* 47, 25.

² See C. Aldred and A. T. Sandison, *Bull. Hist. Med.* 36 (1962), 293.

³ *Op. cit.*

⁴ G. Elliot Smith, *The Royal Mummies* (Cairo, 1912), no. 61075, pp. 52 ff.

influenced by opinions 'that the body . . . was either that of the heretic king (i.e. Akhenaten) or was believed to be his corpse by the embalmers'. After discussing the circumstances of the discovery he was convinced that 'we have the most positive evidence that these bones are the remains of Khouniatonou' (Akhenaten). Elliot Smith was therefore conditioned in his anatomical examination of the remains; hence, whilst determining that they belong to a man aged 25 to 26 years at the time of death, he proclaimed that 'if, with such clear archaeological evidence to indicate that these are the remains of Khouniatonou, the historian can produce irrefutable facts showing that the heretic king must have been 27, or even 30, years of age, I would be prepared to admit that the weight of the anatomical evidence in opposition to the admission of that fact is too slight to be considered absolutely prohibitive'. He 'did not think there can be any serious doubt that these are really the remains of Khouniatonou'. These firm opinions were probably occasioned by the fact that, at that time, the determination of the age and sex of skeletal remains could not be made with such a clear precision as modern methods and data allow.

One aspect of identification with Akhenaten, however, Elliot Smith did not investigate thoroughly. All monuments of this king display him as having an unusual physique, which Elliot Smith recognized. At the time of his examination of the remains, however, he was more concerned with establishing their male sex than with reconciling the anatomical evidence with Akhenaten's peculiar physique. When the burial chamber containing these remains was opened, Mr. T. M. Davis called in two medical men, one a 'prominent American obstetrician', and both 'instantly agreed' that they were a woman's. Not content with this instant recognition of the sex characters of the pelvis, the 'prominent American obstetrician' made a most thorough examination, and stated that the greater breadth of the pubic arch gives one of the most easily appreciable points of contrast between male and female pelvises, the pubic arch in the female forming an angle of 90–100°, whilst in the male it averages 70–75°. As Elliot Smith remarks,¹ this narrative cannot be considered completed until he adds that the angle of the pubic arch in the pelvis of these remains is actually 67°; in other words, judged by the standards invoked by Mr. Davis, the pelvis is 'ultra-masculine'. Although these remains were found in the tomb containing Queen Tiye's funeral furniture, Elliot Smith was convinced that the bones were those of a man about 25 years of age, perhaps a year or two more or less, but certainly of a man well under 30; at the very lowest estimate, he claimed, Queen Tiye must have been well over 40, and probably greater than 50 years of age at the time of death. He also quotes the degree of epiphyseal union in the long bones to suggest that this individual was more than 20, and probably more than 23, at the time of death; that the complete fusion of all scapular epiphyses indicates a probable age of at least 25; that the incomplete union of epiphyses of the clavicles, ribs, sternum, vertebrae, and hip bones indicates the improbability of the individual being much more than 25 years of age; and finally, the evidence afforded by the lack of eruption of one wisdom tooth, the fresh appearance and slight wear of the other teeth, was such as might be expected in a man of 25.

¹ *The Times*, Oct. 15, 1907.

Elliot Smith did, however, recognize that the fact that part of Queen Tiye's funeral furniture was found in the tomb containing these remains suggested that some confusion may possibly have occurred when the royal mummies were being transferred from the tomb at El-Amarna to Thebes.¹ He stated that if any such confusion occurred Smenkhkarē seems to be the only man whose mummy could by any reasonable chance have been substituted. Whilst considering the strange and pronounced anatomical peculiarities of Akhenaten, no doubt in exaggerated form, represented by artists of his reign, he was unable to detect in the skeleton any very pronounced resemblance to the pictures of Akhenaten, although he hypothesized that, when clothed with flesh, it would have presented some very exceptional features which, with comparatively slight exaggeration, might easily be brought into conformity with the Akhenaten peculiarities. He desired the best of both worlds, and was at pains to reconcile any discrepant anatomical features with an almost too obvious facility. Elliot Smith also claimed² that this individual had 'chronic hydrocephalus'; the enlarged cranium, he claimed, had become flattened and otherwise distorted, so that in profile it looked unduly long and the forehead rather low. He found the nose to be more prominent than in Amenophis III and somewhat deflected to the right; the jaws (and probably the hips also) he claimed to be rather prominent, with a somewhat pendent chin. The pelvis was so roomy, he claimed, as to have deceived Mr. Davis's two experts in regard to the sex. Because of 'irregular thickenings' on the inner walls of the cranium of this individual, he concluded that the disciples of Lombroso can claim that his abnormally big, perhaps epileptic, brain endowed him with one of the qualifications regarded by them as essential in the founder of a new religion.

It is most unfortunate that these fanciful opinions misled subsequent investigators. One exceptional and accurate anatomical account was, however, made by Derry,³ who restored the skull, and found no evidence of hydrocephalus. He described the skull as platycephalic, 'the very reverse of the shape produced by hydrocephalus', and concluded that the remains were from a man not more than 23, or at the most 24, at the time of death. Although Elliot Smith⁴ made a slight attempt to correlate his anatomical observations with contemporary artistic representation of Akhenaten by considering the possibility of dystrophia adiposogenitalis (Fröhlich's Syndrome), others, unfortunately, have been led to do so in much greater degree from a theoretical standpoint, without re-examining the remains. The most notable of such recent attempts is that of Aldred and Sandison.⁵ In a scholarly but purely theoretical treatise, they make extensive conclusions, based only on already published opinions, theories and data, and an examination of the Akhenaten monuments. These monuments depict a king with an elongated face, prominent and pointed chin, large full lips, coarse nose, large ears, wide pelvis, prominent abdomen, buttocks, thighs, and breasts. The thighs are large, but the lower legs and arms are slim, and the hands and fingers not excessively large. Aldred and Sandison regard these changes in the trunk and limbs as feminization, occasioned by a disorder of the pituitary gland. They hypothesize a transient phase of pituitary

¹ Ibid.² Ibid.³ *Ann. Serv.* 31 (1931), 115.⁴ *Tutankhamen* (London, 1923), 84.⁵ *Op. cit.*

hyperfunction going on to hypofunction; in the hyperfunctional phase acromegaloid changes were manifested in the skull and soft tissues of the face. They do not imply that Akhenaten was a true acromegalic, however, since there is no enlargement of the extremities or gigantism. They also consider that the changes in the trunk and limbs of the Akhenaten monuments are consistent with those occurring in dystrophia adiposogenitalis. From the published evidence of the remains, they conclude that the findings go some way to sustain a provisional diagnosis of pituitary cranial dysplasia, and that they are reconcilable with the evidence they have deduced from the monuments of Akhenaten.

Whilst agreeing with Aldred and Sandison that the monuments display an interesting physique, and that certain interpretations may be made from it, two investigations are essential before entering into extensive hypothesis concerning Akhenaten.

(a) It is important to ascertain whether his monuments depict a true likeness. Even if this is so, only limited and qualified clinical interpretations are admissible, in which case the comments of Aldred and Sandison provide an excellent analysis of the salient features to observe.

(b) A re-assessment of the remains which lie in the Egyptian Museum at Cairo, and interpreted by the Museum authorities as belonging to Smenkhkarē, despite Elliot Smith's conviction¹ that they belong to Akhenaten, is most desirable. If this examination reveals anatomical features which correspond to the Akhenaten physique, a scientific correlation is justifiable.

Such an anatomical examination, to be of value, must be entirely impartial, and must take account not only of the age and sex of the remains, but also such features as stature, bodily proportions, and any evidence of endocrinopathy, pituitary tumour, and hydrocephalus. With the collaboration of the late Dr. A. Batrawi, Professor of Anatomy in the University of Cairo, an extensive anatomical examination of the remains was performed,² and with the assistance of Dr. Mahmoud El Sayed Mahmoud, Professor of Radiology at the Qasr el-Aini Hospital, Cairo, a radiological examination was made of certain selected parts of the remains.³ In the ensuing pages a factual description of these findings, together with an analysis and assessment of the results, has been compiled.

Observations

The remains consist of a complete disarticulated skeleton, except for the manubrium sterni, which was absent. The pelvis comprises disarticulated hip bones and sacrum,

¹ *Royal Mummies*, 51.

² Without the generous assistance of the late Professor Batrawi this investigation would have been impossible, and I wish to express my lasting indebtedness to him. I also wish to express my gratitude to the Wenner-Gren Foundation for Anthropological Research, which kindly provided the necessary financial support for the investigation.

³ The remains were first examined in Dec. 1963 in the Egyptian Museum, and then removed to the Qasr el-Aini Hospital with the sanction of the Director General of the Antiquities Service, Dr. Anwar Shoukry, and the Curator, Mr. Mohammad Hassan Abdul Rahman, to whom many thanks are due for their ready co-operation and kind assistance.

while the skull is damaged in many places and shows evidence of reconstruction. The pubis of the left hip bone and both fibulae had been broken. Most of the bones were covered with shellac or some type of lacquer which was flaking extensively. The findings on examination of the remains are detailed in Appendix I, but it is necessary to consider the anatomical features of the remains in greater detail and compare them with the observations of Elliot Smith.¹

Sternum. The manubrium sterni was not present in these remains, only the body (mesosternum), the first segment of which was not joined to the remainder. It can only be presumed that these parts of the sternum had not joined in life. Elliot Smith entertained the possibility that the first segment of the mesosternum had been broken off, an opinion which is not confirmed by naked eye and radiographic appearance of the bone (pl. XX, 1).² Because the bone was separated in this way it was only possible to utilize the fused lower three segments in assessment of age and sex.

It is generally stated that the body of the sternum is narrower in breadth in relation to its length in the male, shorter and broader in the female.³ The length of the fused lower three segments in these remains measures 76 mm., and the breadth at the widest part 39 mm. Paterson gives the average breadth as 40.5 mm. in the male and 36.8 mm. in the female. On these grounds, therefore, the mesosternum approximates more to male than female breadth. Ashley⁴ has recently re-assessed the criteria for age and sex of the sternum and has measured the width of segments 1 and 3 in order to examine the claim of Paterson that the female sternum is broad in its lower part. Measurements (S1 and S3) were taken at the 'waist' of each segment since the maximum width of the mesosternum may be obscured by exaggerated costal processes; this is evident to a certain extent in the present remains (pl. XX, 1). It was possible to compare his measurement S3 for European sterna (male average 33.7 mm. (range 19-52); female average 30.5 mm. (range 19-49)) with that in the present remains (32 mm.); the measurement is therefore almost exactly intermediate between the male and female means.

The mesosternum also shows deep costal notches for the fourth and fifth ribs, and signs of recent union between segments 2 and 3 (pl. XX, 1). The facets for the fourth rib are still divided by a transverse cleft. This, and the non-union of the first segment of the mesosternum place the age at death in the period 19-20 years,⁵ with a maximum possible age of 23.

Clavicle. In both clavicles the secondary centres for the medial (sternal) end are not united with the shaft, and the surface of the medial end of the shaft is billowed. Todd and D'Errico⁶ state that the medial epiphysis begins to unite at 21-22 years, and is completely united by 25; the epiphysis for the lateral (acromial) end ossifies and unites

¹ *Royal Mummies*, 52 ff.

² I am indebted to the technical assistance of Mr. L. G. Cooper and Mr. D. L. Reeve, whose photographic skill has enabled an excellent and permanent photographic documentation of evidence to be made.

³ A. M. Paterson, *The Human Sternum* (Liverpool, 1904), 36 ff.

⁴ *J. Forens. Med.* 3 (1956), 27.

⁵ W. M. Krogman, *The Human Skeleton in Forensic Medicine* (Springfield, 1962), 216 ff.

⁶ *Amer. J. Anat.* 41 (1928), 25.

in the 20th year. Since the acromial epiphysis has united, it must be presumed that the age of the remains is in the period 20–22 years. McKern and Stewart¹ found no cases after the age of 22 in which the sternal epiphysis did not show some degree of union with the shaft. Elliot Smith² although noting the lack of union of the sternal epiphysis, minimizes any conclusion which may be adduced from this feature (see below).

Parsons³ found the average length of both right and left female clavicles to be 138 mm. (range 124–64 mm.), whereas the average length of right male clavicles is 151 mm. (range 126–73 mm.) and of left male clavicles 153 mm. (range 130–76 mm.). The lengths of the clavicles in the remains (Appendix I) are 154 mm. (R.) and 153 mm. (L.), *excluding the sternal epiphysis* in both cases. These measurements therefore suggest very strongly that the remains are from a male. Parsons also found a relationship between clavicular length and shoulder width, the male left clavicle averaging 0.387 of the shoulder width, the right averaging 0.382. Using the lengths of the clavicles without sternal epiphyses, it can be calculated that the shoulder width of the body belonging to these remains was *at least* 39.5 cm. (15.6 in.).

The claviculo-humeral index⁴ measures 47.82 for the right side, and 47.96 for the left side, using the lengths of the clavicles without sternal epiphyses. This index, however, shows considerable inter-racial overlap, and on these grounds alone its value is therefore limited.

Vertebral column. McKern and Stewart⁵ have found the presacral vertebral column to be completely ossified by the 24th year; noteworthy is the sequential pattern of ossification which shows the last signs of complete maturity occurring in the upper thoracic vertebrae, specifically the fourth and fifth thoracic vertebrae. Krogman⁶ also uses the fusion of the annular epiphyseal plates with the upper and lower surfaces of the vertebral bodies to mark the termination of his Period 4 of sequential age changes, which covers the period 20–24 years. It may be presumed, therefore, that the remains are from a person less than 24 years of age at the time of death, since not only are epiphyses not united with the bodies of the fourth and fifth thoracic vertebrae, but (Appendix I) the epiphyses of the fourth and fifth cervical vertebrae are not yet completely united. Elliot Smith had noted the state of ossification of the vertebral column, and found the axis completely ossified; he refers to the observations of earlier authors (Thomson; Piersol; Testut) that this bone is completely ossified at 20–25 years, and utilizes the single observation of Testut that the ossification of the vertebral column progresses with extreme slowness and is not complete until 25–30 years, to conclude that ‘this condition may indicate an age of not more than 30 years, or than 25 or even 20 years, according to different authorities’.

Clavelin and Dérobert⁷ have noted that the mean breadth of the male atlas is 83 mm. (range 74–90 mm.) and that of the female atlas 72 mm. (range 65–76 mm.). The breadth of the atlas in these remains is 75 mm., which falls within the male range. In a small

¹ Quoted by Krogman, *op. cit.* 51 ff.

² *Royal Mummies*, 53.

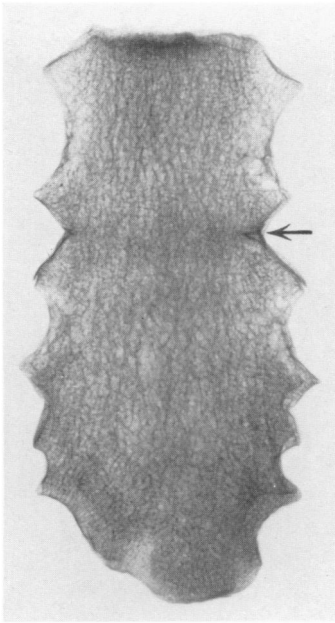
³ *J. Anat., Lond.* 51 (1916), 71.

⁴ See A. H. Schultz, *Human Biology* 9 (1937), 281.

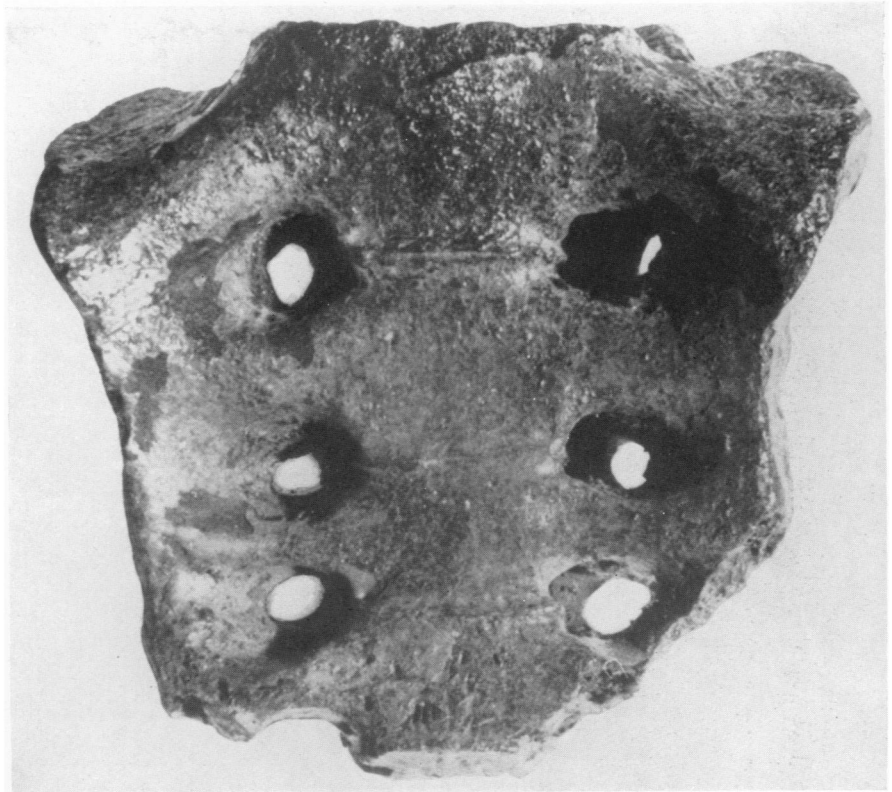
⁵ Quoted by Krogman, *op. cit.* 53.

⁶ Quoted by J. D. Boyd and J. C. Trevor, *Modern Trends in Forensic Medicine* (London, 1953), 133.

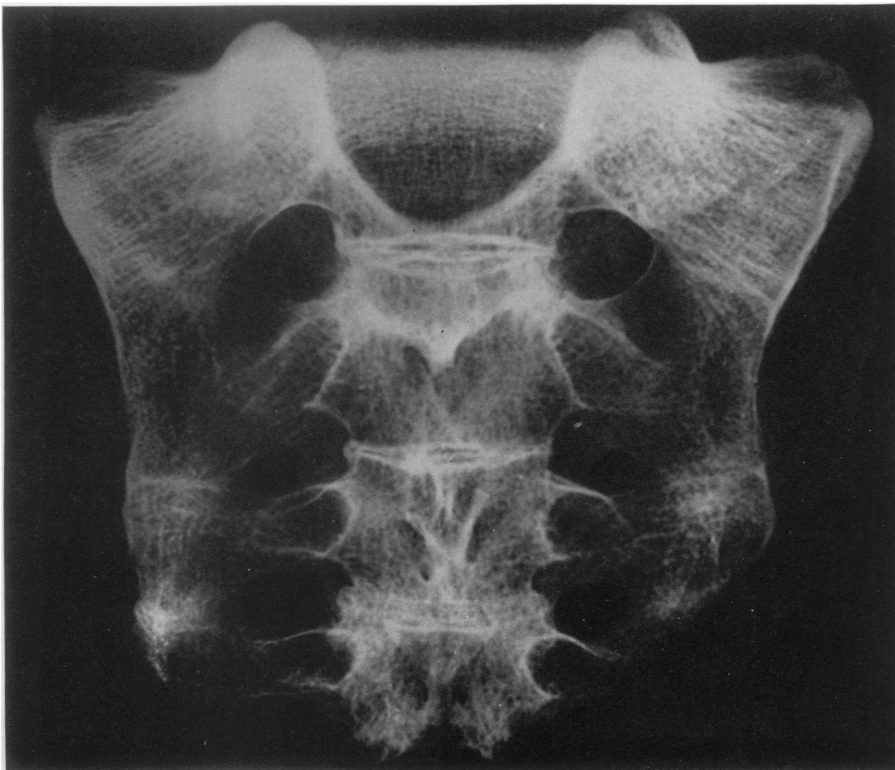
⁷ Quoted *ibid.*



1. Radiograph of mesosternum, showing the deep costal notches for the fourth and fifth ribs. The facets for the fourth rib are still divided by a transverse cleft, visible on the right of the figure (left side of sternum) as shown by arrow.
Natural size



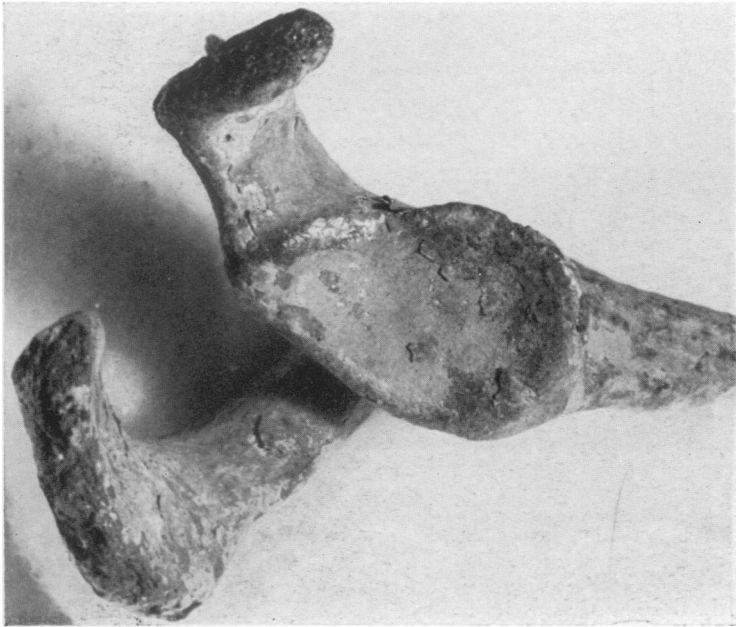
2. Anterior surface of the sacrum showing signs of recent union between the centra. Natural size



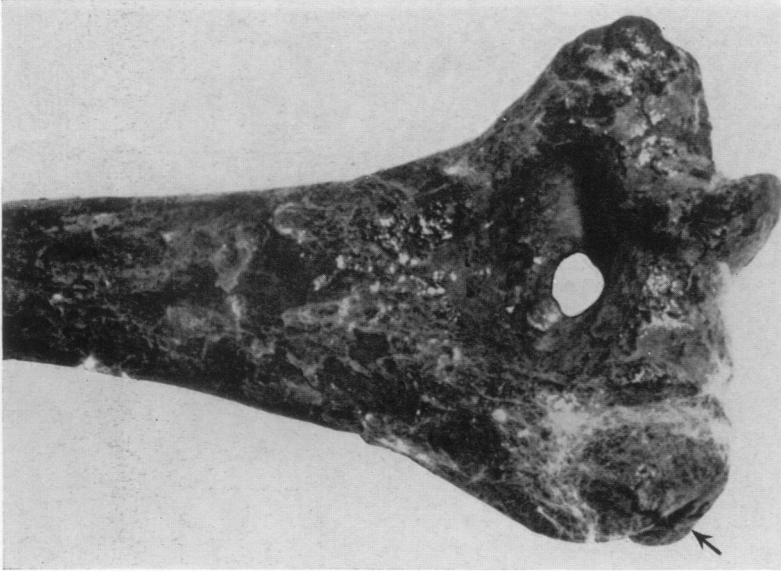
3. Radiograph of sacrum. The lack of complete union between the centra is clearly obvious. Natural size



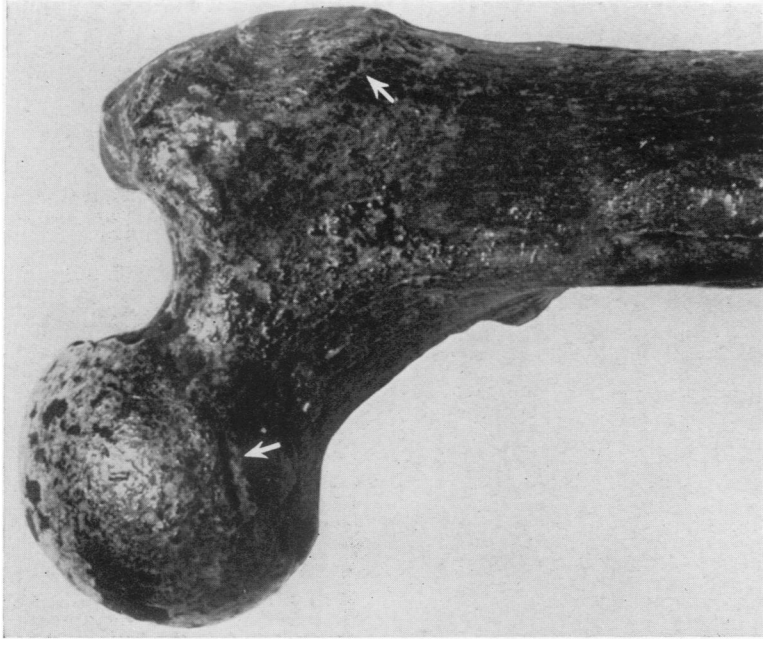
4. Left auricular surface of sacrum. Arrow points to dorsal elevation



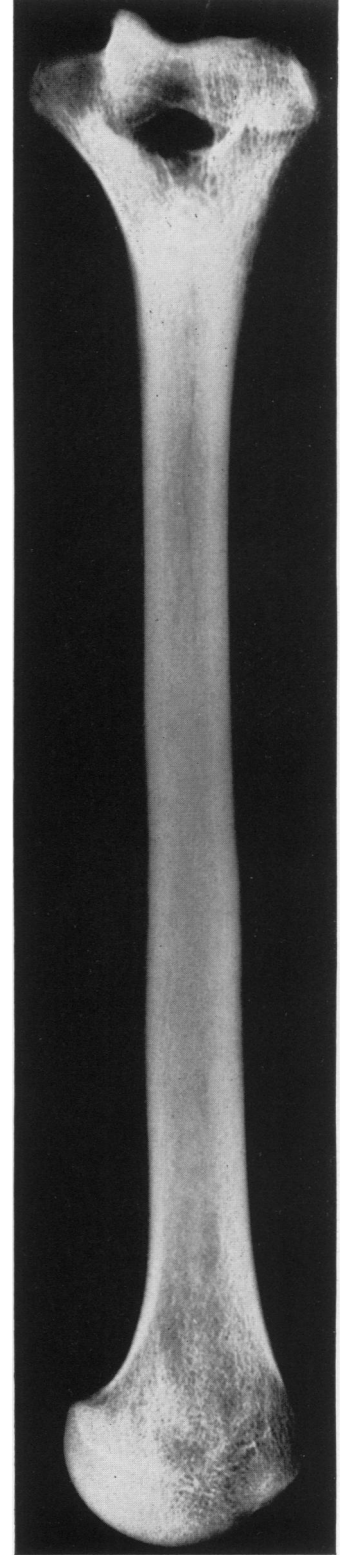
1. View of the glenoid cavity, acromion, and coracoid processes of the right scapula



2. Posterior surface of lower end of left humerus. Note the epitrochlear foramen. Arrow points to epiphysis of lateral epicondyle. Line of separation of epiphysis from remainder of bone is clearly shown



3. Anterior surface of upper end of left femur. Arrows point to grooves indicating recent union of epiphyses for greater trochanter and head of femur



4. Radiograph of right humerus. The epitrochlear foramen and epiphyseal scars are visible. ($\times 0.63$)

series of twenty atlases of each sex Boyd and Trevor¹ found no adult male atlas with a breadth of less than 76 mm. Harrison,² however, has found this conclusion unreliable, and this is confirmed in the present investigation.

Sacrum. This bone shows characteristics which are undoubtedly male. The sacral curve is uniform and straight, and does not show the sharp forward angulation in its lower part characteristic of the female. The auricular surface extends well on to the third sacral vertebra, which is characteristic of the male, for in the female only the upper two vertebrae are involved. The alae in the female sacrum constitute almost two-thirds of the base of the sacrum; in these remains the proportionate width of the alae is only approximately one-half that of the base, which is characteristically male. The female sacrum is shorter and wider than in the male; the mean sacral index (the proportion of the breadth at the base to the anterior length expressed as a percentage) in British females is 116, in males 112.³ In this sacrum only four pieces are present instead of the usual five in an adult, and therefore the sacral index cannot be calculated accurately. Nevertheless the index measures 111, and if the fifth piece had been present the index would have been less than this value, which would increase the male tendency.

The absence of the fifth piece of the sacrum can only be taken as evidence of incomplete ossification of this bone. This is further supported by the obviously recent union of the upper four pieces; the union appears to be most recent between the first and second centra (pl. XX, 2 and 3). Although the lower part of the left auricular surface and the lower aspect of the third and fourth pieces have been damaged (pl. XX, 4) it is not considered that this is a factor in the separation of the fifth piece. Girdany and Golden⁴ claim that the upper centra fuse by 16 years, the lower by 18 years, and all by 30 years. McKern and Stewart⁵ found that the elements comprising the sacrum begin to fuse from below upwards; by 23 years ossification is complete except often between the S1-2 centra where a gap may persist until the 32nd year. No such gap is present, although the lines of fusion are very prominent. It may be concluded that this sacrum is from a person less than 23 years of age.

Elliot Smith described the three intercentral fissures of the sacrum to be just closed or in process of closing, and takes this evidence to suggest that the sacrum is from a person less than 30 years of age.

Weisl⁶ has examined the shape of the dorsal border and eminences on the auricular surface of the sacrum, and has been able to utilize these in assessment of the age and sex. Ratios were assessed for the depth of the concavity on the dorsal border of the auricular surface (SR/NO) and the distance of the cavity from the cranial end of the dorsal border (NR/NO). These ratios in the present remains measured 18 and 47

¹ Ibid.

² *Medical and Scientific Investigations in the Christie Case* (London, 1953), chap. 5.

³ J. E. Frazer, *The Anatomy of the Human Skeleton* (London, 1948), 36.

⁴ *Amer. J. Roentgenol.* 68 (1952), 922.

⁵ In Krogman, *The Human Skeleton in Forensic Medicine*, 53.

⁶ *Acta Anat.* 22 (1954), 1.

respectively. In a recent analysis¹ the distribution of the SR/NO ratio in the sacra of adults is:

SR/NO Ratio	15	20	25	30	35	40	45	50
No. of males	4	12	8	3	5	1	—	—
No. of females	—	—	5	5	4	1	—	1

The following is the distribution of the NR/NO ratio:

NR/NO Ratio	20	25	30	35	40	45	50	55	60	65	70	75
No. of males	1	—	4	4	9	4	7	—	3	—	—	1
No. of females	—	—	1	—	2	3	4	3	—	—	2	1

The sacrum in the present remains is therefore more likely to be male than female. It is realized that Weisl's figures were obtained from sacra with intact articular cartilage, whereas the sacrum examined was devoid of such cartilage, and therefore some reservation must be attached to the findings.

The eminences of the auricular surface of the sacrum Weisl now regards as exostoses. In addition to cranial and caudal elevations, a dorsal elevation is sometimes present, and is clearly obvious in these remains (pl. XX, 4). Weisl² in examining a photograph (pl. XX, 4) of the auricular surface of the present remains concludes: 'In a child aged three the dorsal elevation was absent, in a twelve year old it was "feeble", in a 20 year old male it was "poor", and it was absent in a female of the same age; it was present in a female aged 23, feebly developed in a 32 year old female dying of puerperal dementia and poorly developed in a 31 year old male. Thus this dorsal elevation probably appears in the second decade. The elevation in this sacrum seems to be sufficiently low to be derived from a human in the second or third decade of life.'

Scapula. The secondary centre for the acromion is united with the remainder of the bone about the mid-18th year,³ whilst the secondary centre for the coracoid process has similarly united by the 18th year, and that for the vertebral border by the 20th year (Girdany and Golden).⁴ The first two of these centres have united (pl. XXI, 1) and this evidence suggests that the remains are 18 years of age or older. The centre for the vertebral border has not yet united, and Derry⁵ recognized this fact. Elliot Smith, however, claimed that the scapula is completely ossified. Stewart⁶ claims that lipping of the circumferential margin of the glenoid fossa commences at 30-35 years of age, and is first evident in the notch on the ventral margin of the glenoid cavity. This notch is clearly observable, with no evidence of lipping (pl. XXI, 1).

The scapula is also of some value for purposes of sex discrimination. Dwight⁷ has shown that the average glenoid height in male bones is usually greater than 36 mm., in female bones less than this figure. The left scapula of these remains has a glenoid height of 36 mm., the right 37 mm. The indication, although slight, is therefore towards the male.

¹ Weisl, personal communication.

² *Amer. J. Roentgenol.* 8 (1921), 721.

⁵ *Ann. Serv.* 31 (1931), 115.

⁷ *Amer. J. Anat.* 4 (1904), 19.

² Grateful thanks are due to Dr. Weisl for his analysis.

⁴ *Amer. J. Roentgenol.* 68 (1952), 922.

⁶ *Legal Medicine* (St. Louis, 1954), 407.

Hrdlička,¹ in measuring the dimensions of the scapula, examined Twelfth-dynasty Egyptian scapulae, and found them to be racially different from the scapulae of other groups investigated; sex differences were also present. His measurements for male and female 'old Egyptian' scapulae are compared with the dimensions of the right scapula in these remains in the following table.

	<i>Total height cm.</i>	<i>Infraspinous height cm.</i>	<i>Breadth cm.</i>	<i>Total scapular index</i>	<i>Infraspinous scapular index</i>
Old Egyptian males	15·56	11·48	10·61	68·2	92·4
Old Egyptian females	13·20	9·74	9·41	70·0	96·7
Present remains	13·1	10·5	9·6	73·3	91·4

These measurements are indeterminate apart from the total scapular height and index, which are clearly female in type, and the infraspinous scapular index which is clearly male. Krogman² has claimed that the scapula cannot be accurately classified with reference to racial differences, and this may account for the indeterminate nature of these measurements. Hrdlička, however, has shown that during the growing period the scapular indices diminish with age, which means that the scapula increases more in length than breadth. The very low infraspinous scapular index in a bone which otherwise demonstrates feminine tendencies, could be interpreted as reflecting approaching maturity in a male who has not yet reached mature adult proportions.

Long bones. The long bones show features of great interest and value in assessing the age at death and sex of the remains. In general all the long bones are fairly heavy with well-marked muscular impressions, characteristic of the male. Each of the bones will be assessed separately.

Humerus. Various estimates by previous authors³ have described the length of the adult male humerus to vary from 324·6 to 329 mm., whilst the adult female humerus measures 299–304 mm. The lengths of the humeri in these remains were 319 mm. (left) and 322 mm. (right) which indicates a tendency to maleness. The vertical diameter of the head of the male humerus is greater than 43, 45·5, or 46 mm., according to various authorities, whilst in the female it is less than 44, 41·5, or 45 mm. In these remains the vertical diameter of the head of the right humerus was 46 mm., of the left humerus 44 mm., which again indicates maleness. The transverse diameter of the head according to Dwight⁴ is greater than 43 mm. in the male, less than 41 mm. in the female: in these remains the right humerus is 42 and the left 40 mm. in transverse diameter, and therefore these measurements are indeterminate.

The state of epiphyseal union in the humeri is of great interest. In the right humerus the epiphysis for the lateral epicondyle has not yet fused with the remainder of the lower end of this bone (pl. XXI, 2). All authorities are agreed that this is usually complete by the age of 13 years, at the latest by 18 years. That of the left humerus was completely united. Radiography of both humeri shows the presence of well-marked epiphyseal

¹ *Amer. J. Phys. Anth.* 29 (1942), 363.

² *Op. cit.*

³ See R. J. Harrison, *op. cit.*

⁴ *Amer. J. Anat.* 4 (1904), 19.

lines, particularly at the junction of the head of the humerus with the metaphysis (pl. XXI, 4). Krogman¹ has criticized the validity of such epiphyseal 'scars' in radiographic assessment of age, since they can persist several years after demonstrable complete union on naked eye examination. Since union of all the humeral epiphyses with the shaft is normally complete by 21 years of age at the latest in the male, and one of these epiphyses has not yet fused, the evidence of the epiphyseal lines supports the conclusion that the remains are not much older than 21. This is supported by the radiographic appearance of the humeri as a whole (pl. XXI, 4). The internal structure of the epiphyses is radial, and of the diaphysis ogival, the medullary cavity being far removed from the surgical neck; these criteria would fit with an age of about 22 years according to Schranz.²

Both humeri showed the presence of epitrochlear foramina (pl. XXI, 2 and 4). Such foramina are quite common in ancient Egyptian skeletons. Macalister,³ in examining the Cambridge collection of Egyptian bones found that out of 692 humeri, 390 show such perforations while 292 are imperforate. Of the perforate humeri 192 were male, and 198 female. The propensity for such perforations to occur in modern females (Godycki)⁴ obviously does not apply.

Elliot Smith measured the length of both humeri and claimed that they are fully ossified and consolidated. He refers only to the upper epiphysis and its union with the shaft at 22–26 years according to earlier authorities (Testut; Thomson; and Poirier) and concludes that this 'neutralizes the suggestion of an age less than 25 or thereabouts, in support of which the data supplied by the clavicles and ribs might be adduced'. The clavicles have already been considered (see above), and Elliot Smith's observation that the epiphyses for the heads and tubercles of the ribs 'were just joining in most cases, but were separate in a few instances' can be confirmed. These epiphyses unite completely by the age of 24 (McKern and Stewart),⁵ the earliest union being observed at 17 years, which suggests an age within this range for these remains.

Femur. A bone with a femoral head diameter of less than 45 mm. is almost certainly female, whilst if over 47 mm. it is almost certainly male; similarly if the bicondylar width is of the order of 77 mm. it is probably male, whilst in the female it is of the order 67–68 mm. (Parsons;⁶ Harrison⁷). Pearson and Bell,⁸ in a more extensive analysis of approximately 1,000 femora of seventeenth-century Londoners, claim that the vertical diameter of the femoral head varies from 41.5 to 43.5 mm. in females, and from 44.5 to 45.5 mm. in males, corresponding figures for the bicondylar width being 72–74 mm. in females, and 76–78 mm. in males. In a later paper Parsons⁹ claimed that any measurement of the bicondylar width between 70 and 75 mm., although probably female, would be very unreliable. The measurement in these remains of the vertical diameter of the head (45 mm.) indicates maleness; the bicondylar width (70–71 mm.) measurement is,

¹ *Amer. J. Anat.* 4 (1904), 70.

² *Amer. J. Phys. Anth.* 17 (1959), 273.

³ *J. R. Anth. Inst.* 30 (1900), 14.

⁴ Quoted by Krogman, *The Human Skeleton in Forensic Medicine*, 146.

⁵ Quoted *ibid.* 218.

⁶ *J. Anat., Lond.* 48 (1914), 238.

⁷ *Medical and Scientific Investigations in the Christie Case*, chap. 5.

⁸ Quoted by Harrison, *ibid.*

⁹ *J. Anat., Lond.* 49 (1915), 345.

however, unreliable according to Parsons's data, and female in type according to Pearson and Bell. For these reasons Pearson and Bell also indicate that it is necessary to take other measurements of the femur before pronouncing the sex of the person from femoral measurements; the trochanteric oblique length should be in the range 430–450 mm. in male bones, and the popliteal length 132–45 mm. The trochanteric length in these remains (left: 434 mm.; right: 432 mm.) is certainly in the male range, as is also the popliteal length. This latter measurement is very difficult to estimate;¹ nevertheless separate estimates of this measurement by three different observers on the original bones and from photographs definitely place it in the male range (see Appendix I). On these grounds, and the general appearance of the femora, there is no doubt that they are from a male body. Parsons also claimed that a femur with a maximum length of 450 mm. or over is male; on these grounds, the femoral length in these remains being 453 mm., the male sex of the femora is confirmed. Parsons² found that the least transverse diameter of the shaft of the femur if under 25 mm. is probably female, and over 29 mm. male. In these remains this diameter is 26 mm. and, although of no assistance in sexing, demonstrates that the femora are not very robust. The angle formed by the neck axis with the shaft axis if low indicates masculinity, whilst if high indicates femininity. The angle in these remains (132°) is relatively indeterminate, but tends more to a feminine type.

The degree of epiphyseal union in the femora also provides evidence as to the age of the remains. There are well-marked grooves indicating recent union of the epiphyses for the greater trochanter and head of the femur (pl. XXI, 3), and the epiphysis for the lower end of the femur has only just completely united as judged by naked-eye and radiographic appearances. This would suggest an age at death of about 19 years.

Tibia. Dorsey³ found the dividing line between the two sexes for intercondylar breadth of the tibia to be 71–72 mm. The measurements in these remains (see Appendix I) therefore proved to be intermediate.

Radiographically some evidence of periostitis was present in the upper end of the tibia which was also obvious to a less marked degree in the humerus, radius, ulna, and clavicle. These radiographic appearances are similar to those occurring in syphilis, bejel, yaws, and certain haemoglobinopathies. Syphilis was, however, unknown in ancient Egypt (Ruffer and Rietti;⁴ Salib⁵), and without further information it is impossible to be specific about the cause of the periostitis.

Metacarpal bones. Borovansky⁶ measured the lengths of metacarpal bones in 18-year-old girls and boys in Prague. The data for such measurements of the lengths and breadths of these bones in the remains (Appendix I) approximate quite clearly to the male as regards length, but the breadth of the heads of the second to the fifth approximates more to the female, suggesting that these bones are masculine but slender. The secondary centres of ossification for all the metacarpal bones and phalanges had fused with the shafts of these bones, indicating an age of at least 18 years.

¹ See Harrison, *op. cit.*, for a discussion on this measurement.

³ Quoted by Harrison, *op. cit.*

⁵ *J. Bone Jt. Surg.* 44B (1962), 944.

² *Op. cit.* 345.

⁴ *J. Path. Bact.* 16 (1912), 439.

⁶ Quoted by Harrison, *op. cit.*

Skull. In comparing the skull present in the remains and its photograph in Elliot Smith's report, as shown in his Plate 37, it is clear that considerable reconstruction has been carried out. The original photograph demonstrates that much of the right half of the skull vault is missing, including the lateral wall of the right orbit and sphenoid bone, right maxilla and nasal bone. Presumably this part of the skull was later reconstructed with the remainder. Nevertheless, parts of the skull shown to be damaged in Elliot Smith's Plate 37 are still damaged, including the left ascending ramus of the mandible and the left nasal bone and frontal process of the left maxilla. Some incisor and canine teeth in the mandible, obviously broken in Plate 37 of the 1912 report, appear to be in a similar state. The reconstruction, although very commendable, is, however, slightly inaccurate. There was obvious difficulty in reconstructing the orbit owing to damage to the orbital surface of the right maxilla and zygomatic bone, and right half of the sphenoid bone. The right zygomatic bone has been largely remodelled (pl. XXII, 1), and the floor of the right orbit is misshapen (pl. XXII, 2). There is a crack in the right squamous temporal bone, and the right lambdoid suture, as a result of damage, or difficulty in reconstruction, is gaping (pl. XXIII, 1). The left pterygoid hamulus has been incorrectly reconstructed on to the posterior surface of the maxilla (pl. XXIV, 2). The tip of both mastoid processes is damaged, the left zygomatic arch is broken, and there is a hole in the base of the skull between the foramen magnum and right mastoid process about the same size as the foramen magnum (pl. XXIII, 2).

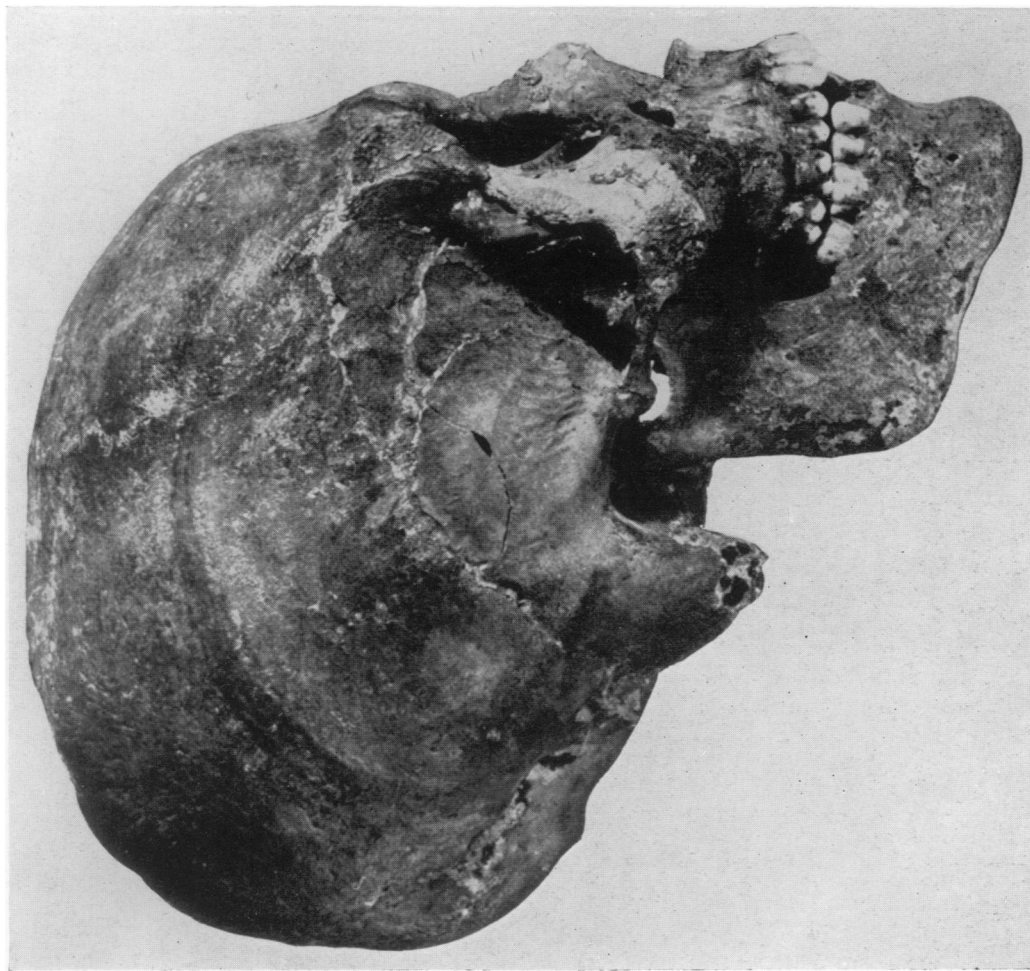
The general appearance of the skull is fairly light, but the muscular markings are well developed (for example, the temporal line, pls. XXII, 1; XXIV, 1), and the supra-orbital ridges prominent, a male characteristic. The superior nuchal line is prominent, but not the external occipital protuberance. The mastoid processes are large and pneumatized, which probably signifies life in a hot, dry atmosphere, but also maleness. The orbital outline is square, and the orbital margins fairly sharp. The mandible is moderate in size with a square mental region (pl. XXII, 2). These features point to maleness rather than femaleness, but in order to make more definite conclusions regarding the age and sex of the skull, various measurements are necessary (Appendix I).

None of the cranial sutures in the vault of the skull shows any sign of fusion on either the ectocranial or endocranial aspects, thus indicating an age of 22 years or less.¹ The occipito-sphenoidal synchondrosis has fused. The process of replacement of the cartilage separating the basi-sphenoid and basi-occiput begins at 17 years of age and is completed by 23 or 24 years in the male.² A line of dense bone indicates the site of fusion up to the age of 25; this line was still present in the remains (pl. XXIII, 2). Dutra also claims that the sphenoidal sinus may extend into the occipital bone after 25 years of age; this had not yet occurred on radiographic examination (pl. XXIV, 2). According to a more recent study,³ the occipito-sphenoidal synchondrosis closes between 13 and 16 years of age in males.

The palate is wide and has a breadth between the medial margins of the alveolar

¹ See T. W. Todd and D. W. Lyon, Jr., *Amer. J. Phys. Anth.* 7 (1924), 325 and 8 (1925), 23 for the data on cranial suture closure, its progress and age relationships. ² F. R. Dutra, *Arch. Path.* 38 (1944), 339.

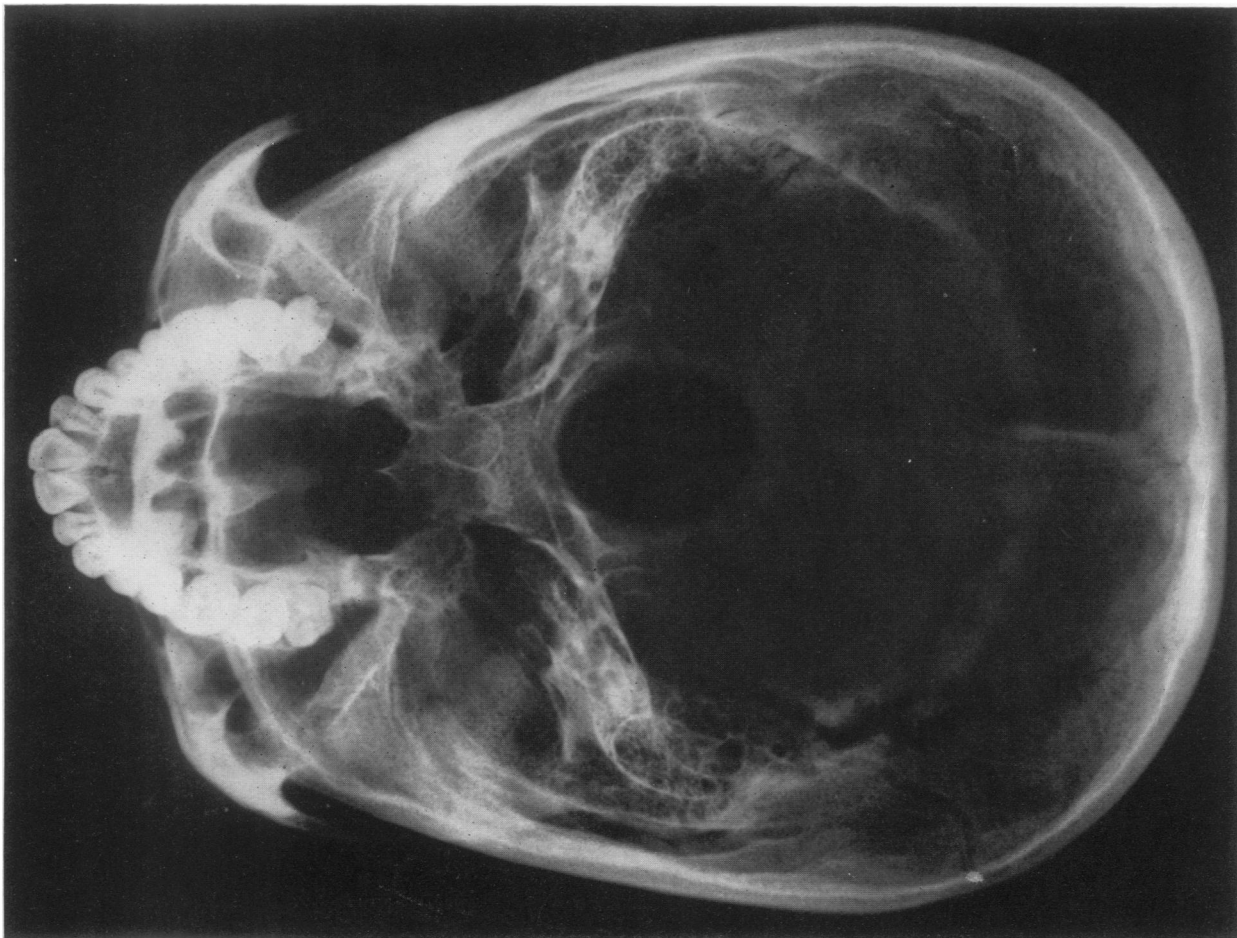
³ T. V. Powell and A. G. Brodie, *Anat. Rec.* 147 (1963), 15.



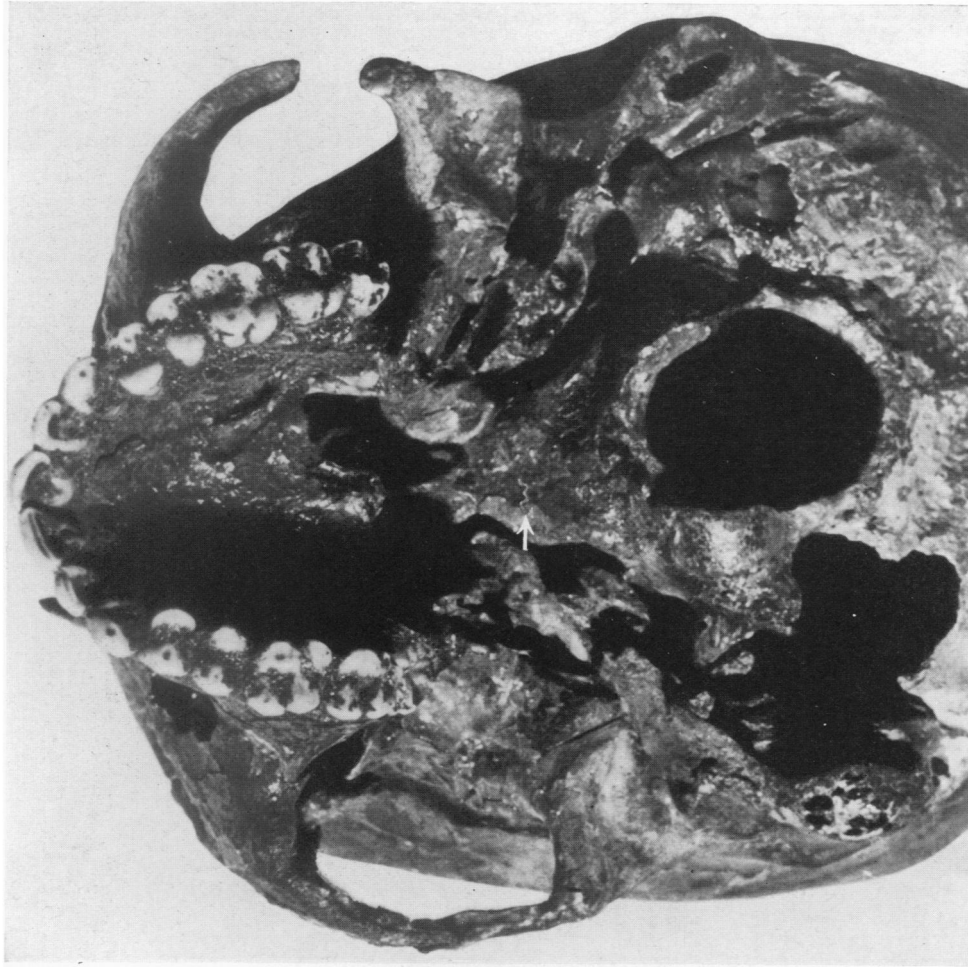
1. Right lateral surface of skull orientated in the Frankfurt plane. The remodelling of right zygomatic bone, well-developed temporal line, and large mastoid process should be noted. The extensive damage of parts of skull is clearly visible



2. Frontal view of skull, showing damage to floor of right orbit and maxillae, and post-mortem damage to some of the mandibular teeth. Skull was photographed resting on a horizontal surface with angle of mandible and occiput approximately in same plane, thus tending to accentuate the length of lower part of facial skeleton



1. Radiograph of basal aspect of skull showing gaping right lambdoid suture, a post-mortem phenomenon. The pulp cavities of maxillary canines are clearly visible. ($\times 0.69$)



2. Basal aspect of skull which is asymmetrical due to post-mortem damage and consequent difficulty in reconstruction. Arrow points to line of dense bone indicating site of fusion of occipito-sphenoidal synchondrosis. Post-mortem damage is clearly shown. Note slight wear of maxillary teeth. Foramen magnum is oval rather than circular in outline. Right maxillary third molar has not fully erupted

processes at the level of the second molars of 43.5 mm. This would indicate a male skull. Discounting the prominent incisors (see below) the palatal arch is U-shaped rather than parabolic (pl. XXIII, 2). The occipital condyles are relatively large, and the foramen magnum is large and elongated anteroposteriorly rather than circular (pl. XXIII, 2). The angle of the mandible is less than 125°, and the frontal and parietal eminences are poorly defined. All of these features point to a male skull.

According to the criteria of Hug¹ the cranial dimensions demonstrate it to be a long, broad, high male skull, with a large circumference and of average facial breadth and facial height. These dimensions (Appendix I) agree closely with those taken by Derry² and by Elliot Smith, who also claimed that the jaw is typically Armenoid, other signs of this alien influence being, he claimed, the prominence of the superciliary ridges and the sloping forehead. The skull is also brachycephalic, which would confirm this opinion. There are, however, certain objections to Elliot Smith's imputation of alien Armenoid influence. Brachycephalic skulls were not unknown in dynastic Egypt. In an extensive study of the individual measurements of the crania of Eleventh-dynasty soldiers from Thebes, Batrawi and Morant³ found cranial indices of over 80, indicating brachycephaly, in 6 out of 54 crania. The cranial dimensions are also very similar to those in skulls found in certain predynastic cemeteries excavated by de Morgan in Upper Egypt.⁴ In addition, the cranial dimensions of this skull fall well within the range of variability of Eighteenth-dynasty skulls estimated by Batrawi⁵ from his own data and those of other workers. As a result of this study Batrawi,⁶ in agreement with Morant, has shown that the direct evidence of variability of cranial measurements of Egyptian skulls refutes the contention of mixture with other foreign sources, such as an Armenoid substratum. The only possible conclusion from the craniometry of this skull is that it does not differ significantly from the crania of other male Eighteenth-dynasty skulls. On these grounds alone, and in the absence of any direct evidence for cranial deformation, the claims of Dingwall,⁷ based on the earlier observations of Elliot Smith and Weigall,⁸ that the skull is abnormally shaped such as to suggest artificial cranial deformation, cannot be accepted, nor the subsequent conclusions that production of this deformity by bandaging had its origins in Egypt, thus confirming Derry.⁹

The cranial capacity calculated according to the mean formula of Lee and Pearson,¹⁰ using auricular height, measures 1,718 ml., and by the formula using total height of the skull, 1,583 ml.; by the method of Froriep, 1,687 ml.; and by the method of Manouvrier, 1,701 ml. If the mean of these four values is taken as representing the cranial capacity, this value (1,672 ml.) indicates that the endocranial volume is large (megacephalic), but nevertheless within the normal range for European skulls. The data of Jørgensen,

¹ *Zeit. morph. Anth.* 38 (1939/40), 359. ² *The Tomb of Tut.ankh.amen* (London, 1927), vol. 2, 143-61.

³ *Biometrika*, 34 (1947), 18.

⁴ See G. M. Morant, *Biometrika*, 17 (1925), 1.

⁵ *J. R. Anth. Inst.* 75 (1946), 81.

⁶ *J. R. Anth. Inst.* 76 (1946), 131.

⁷ *Artificial Cranial Deformation* (London, 1931). Calvin Wells, *Bones, Bodies and Disease* (London, 1964), 165 has claimed that the skull of the 'presumed body of Akhenaten' is 'an unusual shape', and has some pathological condition which seems to be a mild and rather unusual form of acromegaly, and this would explain the 'curious shape'; he does not provide the authority for such conclusions, however.

⁸ *JEA* 8 (1922), 193.

⁹ *Lancet* (1927), 1376.

¹⁰ *Phil. Trans. A.* 196 (1901), 225.

Paridon, and Quaade,¹ for calculating cranial capacity in relation to cranial circumference, also suggest that this value is about 1,672 ml. This, and the lack of any naked-eye or radiological evidence of abnormal separation of sutures and thinning of the skull, excludes any possibility of the presence of hydrocephalus during life, an opinion also previously expressed by Derry.

On radiographic examination² the anatomical features found by naked-eye inspection, mentioned above, were confirmed, and other interesting features became evident. One significant observation was the apparent presence, on lateral radiography, of erosion of the sella turcica (pl. XXIV, 2). This misleading feature was undoubtedly occasioned by the extensive damage to the right half of the sphenoid bone, already mentioned (p. 106). Dr. El Sayed Mahmoud, Professor of Radiology in the University of Cairo, who took the skull radiographs has recently made a special and extensive study of the radiographic appearance of the sella turcica in health and disease, and its value in the morbid anatomical and topographic diagnosis of intracranial tumours, including tumours of the pituitary gland.³ On examining the lateral radiographs of this skull, he was of the opinion that they could be interpreted as demonstrating erosion of the sella turcica, but agreed with the author and the late Professor Batrawi that the damage to the sphenoid bone had undoubtedly been responsible for this erroneous appearance. It is, of course, impossible to exclude absolutely the possibility of the presence of erosion of the sella turcica in life, later obscured by post-mortem damage to the sphenoid bone, but all the evidence is against this. First, it is possible to examine with the naked eye the damaged body of the sphenoid through the right orbit, and observe that only the floor of the sphenoidal sinuses and base of the intervening septum are present; the edges of the sinuses are regular and appear as though broken through, not eroded. Secondly, there is no evidence in the remainder of the skeleton (p. 111) to suggest pituitary abnormality.

Radiography of the skull was also helpful in estimating the size of the frontal sinuses (pl. XXV, 1) which are large, but not unduly so, and typically male. Schuller⁴ has provided criteria for estimating the details of these sinuses. According to his methods of measurement, it is clear that the septum deviates to the left, and the upper border is markedly scalloped with five arcades to the right and two to the left. The sinuses are 38 mm. high, the left 39 mm. wide, and the right 41 mm. The excessive scalloping of the right sinus is probably an estimate of the dry, hot atmosphere to which it was exposed, rather than an expression of the sex of the individual.

The appearance and measurements of the mandible (Appendix I) are within the normal range for the male, and there is no evidence to suggest abnormal size of the mandible such as might result from acromegaly.

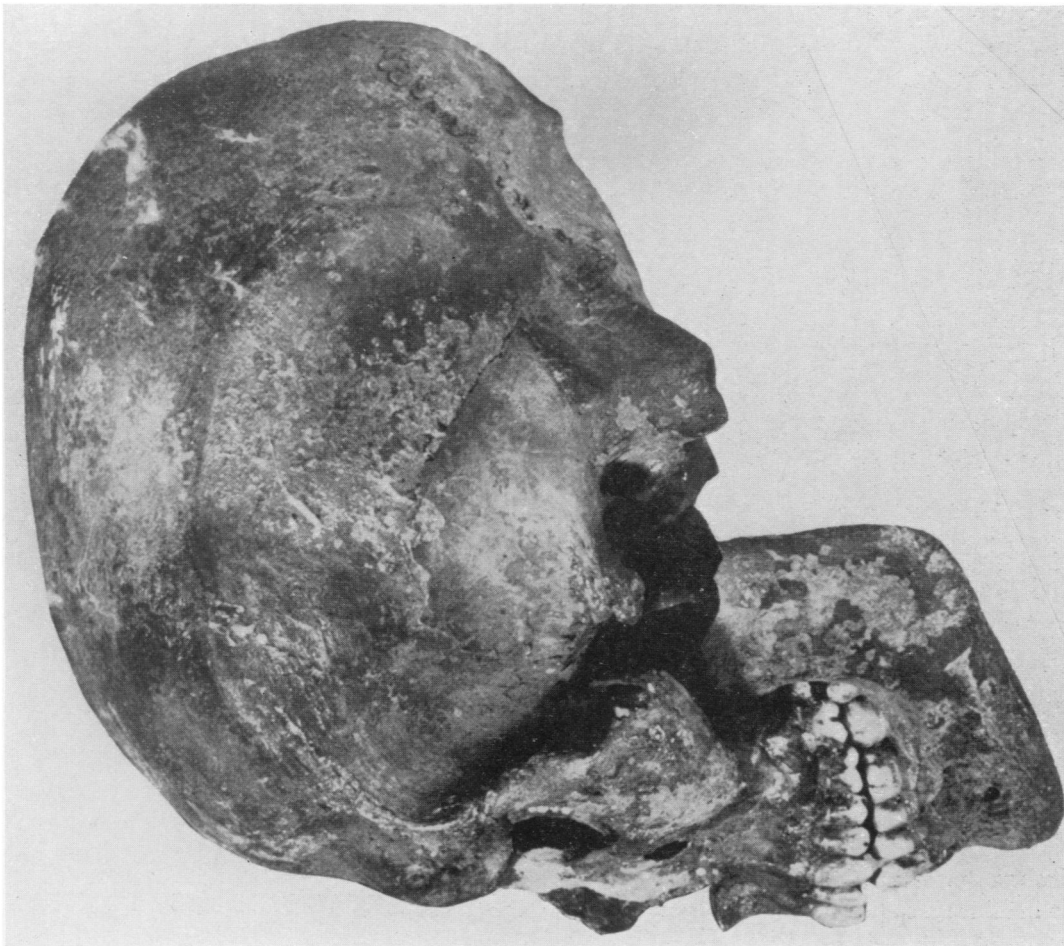
Teeth. Unfortunately there had been post-mortem damage to some of the teeth in the mandible. Those remaining in the mandible, and the maxillary teeth, are, however,

¹ *Amer. J. Phys. Anth.* 19 (1961), 317.

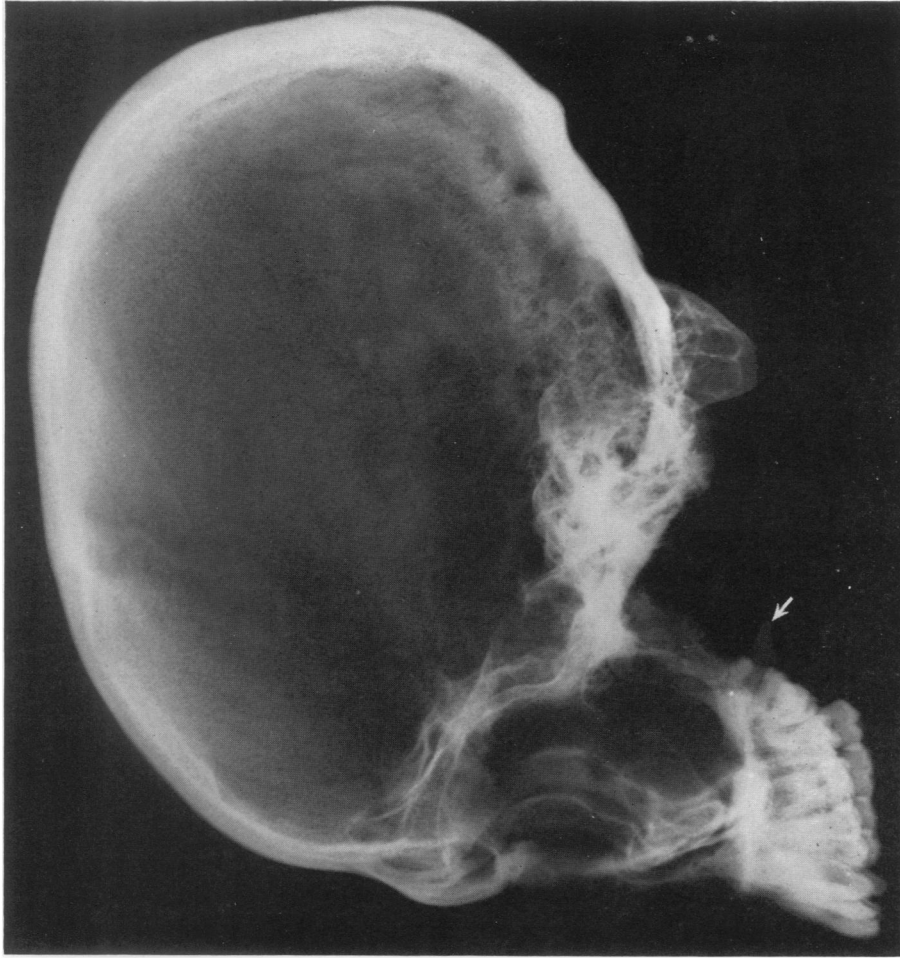
² This procedure was, to a certain extent, historic for, apart from a poorly documented examination with X-rays of the body of Tuthmosis IV by Dr. Khayat in 1903, this was the first time that the body of a pharaoh had been subjected to detailed radiological analysis.

³ M. El Sayed Mahmoud, *Brit. J. Radiol.* (1958), Supp. 8.

⁴ *Med. J. Aust.* 1 (1943), 554.



1. Left lateral view of skull orientated in the Frankfurt plane. The temporal line is prominent. Damage to left zygomatic arch and left ascending ramus of mandible is shown. The nasal bones have been partially reconstructed



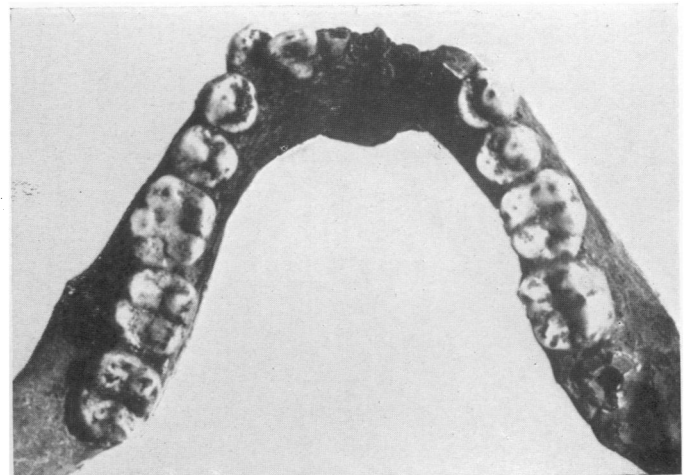
2. Lateral radiograph of skull showing apparent erosion of sella turcica, and extensive pneumatization of mastoid process. The frontal sinuses are well developed. The arrow points to pterygoid hamulus incorrectly reconstructed on to maxilla. The sphenoidal sinus does not erode the occipital bone. ($\times 0.57$)



1. Postero-anterior radiograph of skull, showing frontal sinuses. The septum between the two sinuses deviates to left, and the upper border of right sinus is scalloped. ($\times 0.68$)



2. Radiograph of right side of mandible. Crown of third molar has been broken post mortem. In the other two molars, and the pre-molars, there is no evidence of secondary dentine formation. Periodontosis, increased cementum formation, and root resorption are absent in all teeth shown here



3. Mandibular teeth. Post-mortem damage to three incisors, right canine, and third molar may be seen. Note imbrication between left lateral incisor and canine. Intact teeth show very slight wear

of considerable value in assessment of the age of the remains. One of the most significant findings is the fact that the right maxillary third molar is not fully erupted (pl. XXIII, 2), whereas the left third molar in the maxilla, and the mandibular third molars of both sides are fully erupted. There was no detectable bony tuberosity behind either of the maxillary third molars. This would place the age of the skull within the early part of the age period 18–22 years.¹ There is no recognizable diastasis of teeth (pl. XXVII, 3), which would tend to exclude certain endocrine abnormalities such as acromegaly.

The degree of attrition of the teeth has been investigated very thoroughly by Gustafson² and Miles³ as a criterion for assessment of age in skeletal material. In the maxilla the right incisors show some attrition within the enamel, more extensive in the first than the second incisor, which is not apparent in the left incisors (pl. XXIII, 2); radiologically there was no evidence of secondary dentine formation, periodontosis, increased cementum formation or root resorption in these or any of the other teeth in the maxilla and mandible (pls. XXIII, 1; XXV, 2). The canine teeth, which had not been broken post-mortem, and the buccal tubercles of the premolar teeth (particularly the first premolars), show very slight wear. The degree of wear in the molar teeth, both maxillary and mandibular, is very slight (pls. XXIII, 2; XXV, 3). This feature and the minor degree of wear in the other teeth would correspond to an age of 19–20 on the basis of modern standards. Racial differences in tooth attrition are of small degree,³ and the only acceptable alteration of this age assessment in these remains would arise from known marked differences in dietary habits between dynastic Egyptians and modern Europeans. It might be expected, however, that the dynastic Egyptian's diet would be coarser than that of the modern European, and therefore the degree of tooth attrition more severe; if this be accepted, the estimate of age from the naked-eye and radiological appearance of the teeth in these remains would certainly fall within the earlier part of the period 18–20 years. No evidence of caries was found in any of the teeth. There is imbrication between the mandibular left lateral incisor and canine (pl. XXV, 3) and slight imbrication between the central incisors in the maxilla (pl. XXIII, 2). The maxillary incisors appear prominent at first sight (pl. XXIV, 1), but their orientation to the Frankfurt plane is within normal limits.

Pelvis. Although the two hip bones were completely separated from the sacrum, and there was considerable damage of the pubis in the left hip bone, it was possible to reconstruct the right half of the pelvis (pl. XXVI, 1) and observe that it has the general characteristics of a male pelvis, the outline of the pelvic inlet being heart-shaped. In addition, a rough estimate of the subpubic angle showed that it is less than 90°. In both hip bones the greater sciatic notch is approximately 90°, and deep (pl. XXVI, 2), and the right ischopubic ramus everted (the left being broken); the right obturator foramen is ovoid. These, and other features of the pelvis (Appendix I) demonstrate it to be undoubtedly male.

An important feature on naked-eye and radiographic examination of the hip bone (pls. XXVI, 3; XXVII, 1) is the lack of complete union of the secondary centres for

¹ A. E. W. Miles and R. W. Fearnhead, *Medical and Scientific Investigations in the Christie Case* (London, 1953), chap. 6. ² *J. Amer. Dent. Ass.* 41 (1950), 45. ³ *Dental Anthropology* (Oxford, 1963), 191.

the iliac crests and ischial tuberosities. These should be completely united with the hip bone by the age of 21 according to the majority of investigators, and by 25 at the very latest according to earlier authorities. Acheson¹ found total ossification of the growth cartilage plate for the iliac crest and ischial tuberosity in 16 and 17 per cent. respectively, of male American and British children aged 18. Elliot Smith was of the opinion that the ischial tuberosities are consolidated, but close scrutiny coupled with radiographic examination (pl. XXVII, 1) demonstrates unequivocally the lack of complete union.

Some, but not all of the pelvic measurements (Appendix I) are within the male range. As a result of the reconstruction, however, it was possible to estimate the antero-posterior and maximum transverse diameters of the pelvic inlet, both of which are approximately equal and measure 101 mm. These measurements, and the estimated interspinous and intercrystal diameters (Appendix I) characterize the pelvis as being male, and mesatipellic.² Since the average pelvic index in the male is 100.5,³ the pelvic index of approximately 100 in these remains is in favour of the sex being male. The pelvic measurements are generally small in addition, which would tend to indicate immaturity.

The symphyseal surface of the pubis has been utilized by Todd and others as an age indicator, particularly valuable in the period 20–40 years. In these remains the right pubic symphysis was available for study (the left being broken) and is clearly rugged, and traversed by horizontal ridges separated by well-marked grooves (pl. XXVII, 2). There is no definite limiting margin, and no definition of the extremities; one ossific nodule is becoming apparent and fusing with the upper symphyseal surface. This appearance corresponds with an age period of 20–21 years.

Estimation of stature. The formulae of Dupertuis and Hadden, and the more recent formulae of Trotter and Gleser are now accepted to be the most valuable and accurate for the purpose of determining the stature from the lengths of long bones. The formulae of Trotter and Gleser⁴ for male whites, that of Dupertuis and Hadden⁵ for male whites, and their general formula, were all utilized (Appendix II); all agree in demonstrating the stature to be 5 ft. 7 ins.

Taking the mean stature as 171 cm., it is possible to estimate the degree of correspondence between the length of the long bones in these remains with those in German males⁶ and male Finns,⁷ as shown in the following table, in which bone length is expressed in mm.:

	<i>Humerus</i>	<i>Radius</i>	<i>Femur</i>	<i>Tibia</i>
Breitingger (1937)	331	249	466	379
Telkkä (1950)	335	232 (physiological length)	463	370
Present remains (mean of two sides)	321	241	453	374

¹ *Clin. Orthopaed.* 10 (1957), 19.

³ *Ibid.* 75 (1939), 289.

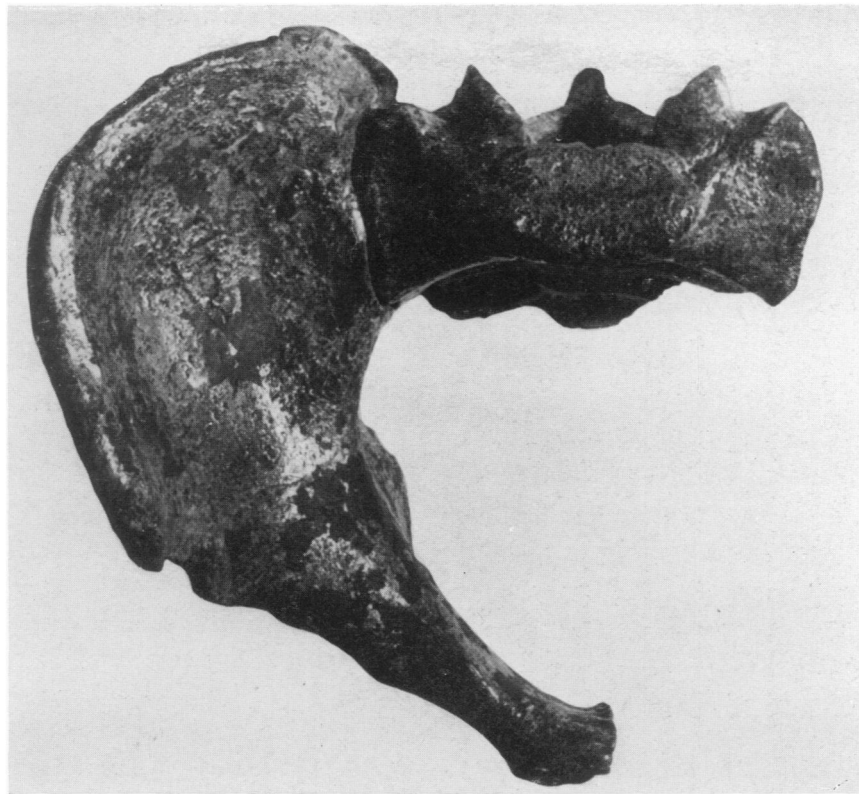
⁵ *Ibid.* 9 (1951), 15.

⁷ A. Telkkä, *Acta Anat.* 9 (1950), 103.

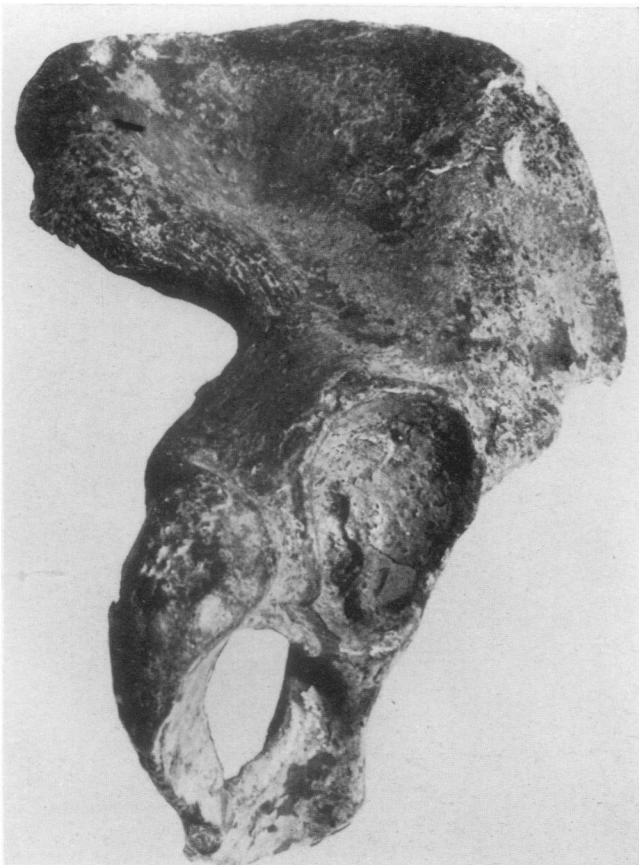
² W. W. Greulich and H. Thoms, *Anat. Rec.* 72 (1938), 45.

⁴ *Amer. J. Phys. Anth.* 10 (1952), 463; 16 (1958), 79.

⁶ E. Breitingger, *Anth. Anz.* 14 (1937), 249.



1. Reconstructed right side of pelvis. Outline of the inlet is clearly heart-shaped



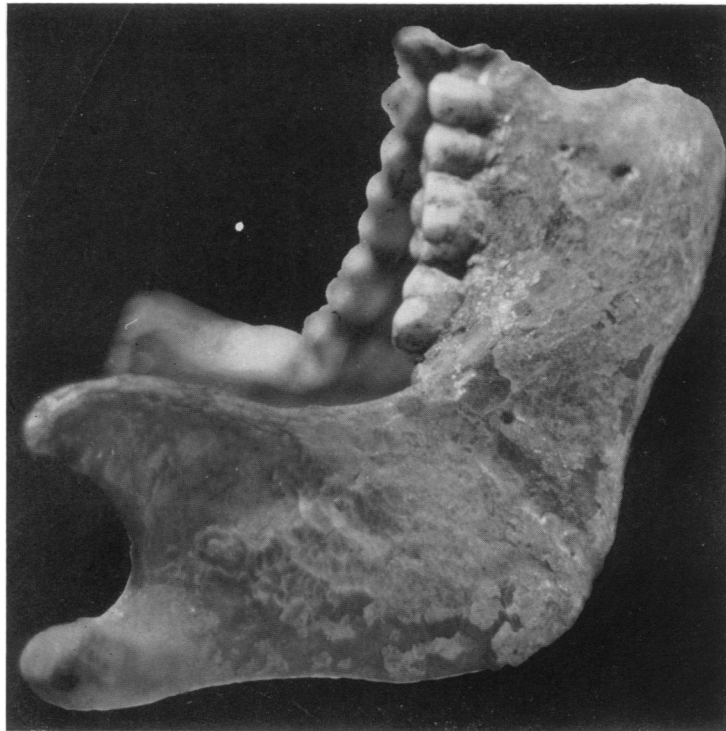
2. Lateral surface of right hip-bone, showing acute greater sciatic notch



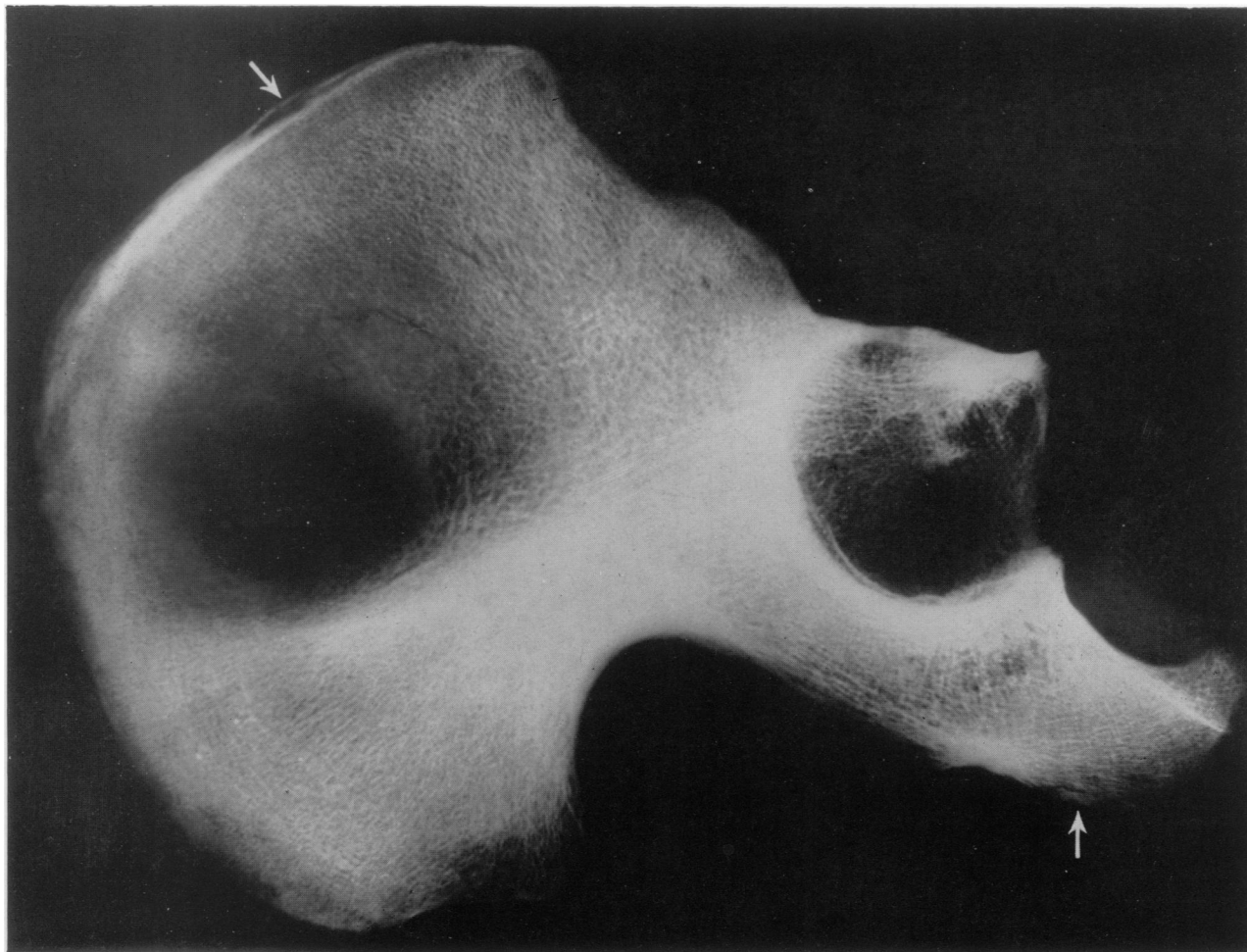
3. Medial surface of right hip-bone. Lack of complete union of the secondary centre of iliac crest may be observed



2. Symphyseal surface of pubis of right hip-bone. The surface is traversed by horizontal ridges separated by well-marked grooves



3. Right side of mandible. There is no diastasis of the teeth



1. Radiograph of left hip-bone showing absent pubis, owing to post-mortem damage, and lack of union of secondary centres for iliac crest and ischial tuberosity (indicated by arrows). ($\times 0.8$)

Such comparisons are dangerous, however, suggesting as they do a shorter humerus and femur length in comparison with modern Finns and Germans. If the brachial (radio-humeral) and crural (tibiofemoral) indices are calculated for these remains, they are found to measure 75.1 and 82.6 respectively. These values, and the lengths of the humerus and femur, agree very closely with data presented for male American whites by Krogman in 1955.¹ It may be concluded, therefore, that the proportionate lengths of the long bones in the extremities of these remains do not differ significantly from those in modern Americans. The low intermembral index in these remains, however, measuring 67.96, although falling within the range for male American whites, may be taken as suggesting a disproportionately short arm length, or long leg length.

Assessment

It is possible to conclude from the above observations that the remains are undoubtedly from a man, less than 25 years of age, and about 5 ft. 7 ins. in height at the time of death. If certain valuable anatomical criteria (for instance, the state of wear of the teeth, state of the symphyseal surface of the pubis, degree of union of clavicular, humeral, and other epiphyses) are to be utilized, it is possible to be more definite that the age at death occurred in the 20th year. Since epiphyseal union has been found generally to occur earlier in Egypt than modern European and American standards dictate,² and the data provided by Modi³ for another hot country confirm this tendency in East Indians, additional weight is given to this conclusion.

It is also possible to be certain that there is no evidence of hydrocephalus in the skull of these remains. There are minor variations in thickness in parts of the calvarium, but these are within normal limits. No abnormal separation of cranial sutures is visible either to the naked eye or radiologically. The cranial capacity, though large, is within normal limits. There is no abnormality in the cribriform plates of the ethmoid, and no erosion of the clinoid processes.

The presence of a pituitary tumour may also be excluded. The radiographic appearance mimicking erosion of the sella turcica (pl. XXIV, 2) is atypical of known pituitary disorders, and the extensive damage to this part of the skull has undoubtedly been responsible for this misleading radiographic feature.

The bodily physique and proportions are also within normal limits and unlike those which occur in established endocrinopathies. The shoulder width is at least 395 mm., which is much greater than the intercrystal diameter of the pelvis of 257 mm. This would exclude a gynaecoid pelvis (indeed the proportions of the pelvis do not suggest this) and would also exclude eunuchoidism, since there is a tendency in such persons for the breadth of the pelvis to exceed that of the shoulders. In hypogonadism the arm span greatly exceeds standing height, in addition to the relatively narrow shoulders and

¹ *The Human Skeleton in Forensic Medicine*, 200.

² G. Sidhom and D. E. Derry, *J. Anat.*, Lond. 65 (1931), 196. The late Dr. Batrawi was also convinced of this fact from extensive anthropological observations in the Faiyûm just before his death.

³ *Medical Jurisprudence and Toxicology* (Bombay, 1957).

wide hips; the arm span was calculated from these remains, assuming the shoulder width to measure 40 cm. according to the following table:

	<i>cm.</i>
Shoulder width	40
Total humerus length (R. + L.)	64.1
Total radius length (R. + L.)	48.2
Total greatest metacarpal length (R. + L.)	13.6
Total phalangeal length (assuming this to be 1.2 × greatest metacarpal length)	16.3
	<hr/> 182.2

The arm span may therefore be noted to exceed the mean standing height by 11 cm. (4.3 ins.). Further evidence of hypogonadal influence is provided by comparing the distance from the superior aspect of the symphysis pubis to the floor with the distance from the symphysis to the vertex of the skull; normally these are equal, but in hypogonadism the former is greater than the latter. The superior aspect of the symphysis pubis is approximately level with the top of the greater trochanter of the femur. The distance from symphysis to floor may therefore be estimated as in the following table:

	<i>cm.</i>
Mean trochanteric (shaft) length of femur (R. and L.)	43.3
Mean length of tibia (R. and L.)	37.4
Tarsal height	6.6
	<hr/> 87.3

Since the mean standing height is 171 cm., the distance symphysis—floor, is only slightly greater than the distance symphysis—vertex, i.e. by 3 cm.

Such estimates are, of course, approximate, and must be assessed very carefully. The arm span may exceed standing height by virtue of the shoulder width component rather than by the contribution of arm length to this measurement, and it is the latter which is of significance in hypogonadism, since diminished androgenic hormone production is mainly manifest in the skeletal system by a disproportionate elongation of the long bones. If the indication of the intermembral index (p. 111) is admissible, there is, in these remains, a disproportionately short arm length or long leg length. If the former, the above conclusion regarding the major contribution of shoulder width to arm span is warranted, whilst if the latter, the slightly greater symphysis—floor length, is confirmed.

The bodily proportions described above, nevertheless, would be consistent with a minimal effect of hypogonadism; evidence in certain parts of the skeleton of a trend towards femininity (for instance, the angle of the neck of the femur, the lack of robustness in certain of the long bones which are nevertheless undoubtedly male, slender metacarpal bones and feminine traits in certain of the measurements of the pelvis which is also undoubtedly male) would confirm this impression. It is, however, most unlikely that this hypogonadism has been severe enough to produce any more than an ectomorphic constitution in an otherwise normal male, and is certainly not sufficiently marked as to be consistent with dystrophia adiposogenitalis or eunuchoidism, and the sort of physique displayed in the Akhenaten monuments.

Indeed, the pelvis is undoubtedly male, certainly not 'roomy', and small in many of its dimensions. The transverse diameters of the pelvis are quite inconsistent with a person in life possessing wide hips. Much has also been claimed for the size of the jaws and evidence for prognathism in these remains. The mandible is not large (pl. XXVII, 3), and shows no evidence of acromegaloid influence. The incisor teeth in the maxilla, although slightly prominent, are not abnormally so. It is quite impossible that the form of the facial skeleton and mandible in these remains would be consistent with the appearance of the face and the form of chin represented in the Akhenaten monuments. In order to examine this further, a reconstruction of the skull was made (See below).

In view of the measurements of the arm span and proportions between standing height and sitting height mentioned above, the possibility of the remains belonging to a person with the Marfan syndrome¹ was also considered. Such persons may demonstrate a brachycephalic skull, but also third metacarpals measuring 7 cm. or more in length, a metacarpal index greater than 8.4, high arched palate, longitudinal striations or streaking of the metaphyseal region of long bones radiographically, and other skeletal features. None of these criteria, apart from the brachycephaly, could be observed in the remains, thus excluding the diagnosis of Marfan syndrome.

Reconstruction

The work of Kollman and Büchly,² which details the thickness of the tissues of the head and face at 23 points, providing minimal and maximal values at each point, enables a reconstruction of the contour of the head and face on the skull. Such an attempt at reconstruction was made with the skull of the present remains by Mr. D. J. Kidd, Medical Artist to the Faculty of Medicine, University of Liverpool. On photographic prints of lateral and frontal views of the skull, contours were outlined using the values shown in Table 4 of Kollman and Büchly's work, in which their data for males are compounded with the earlier figures of His; maximum and minimum values were used in order to obtain two outlines of frontal and lateral views (figs. 1 and 2). The position of the eyes relative to the orbits was estimated according to the criteria of Wolff.³ Since the resultant facial appearance was very similar in both contours, another was drawn from mean values and chosen as representative. Mr. Kidd was not aware of the possible identity of the remains before attempting the reconstruction, nor of the facial appearance of either Smenkhkarē or Akhenaten. Following the composition of the outline contours, however, it became apparent that the reconstruction could have no possible resemblance to Akhenaten as depicted in his monuments. Mr. Kidd therefore prepared half-tone illustrations of frontal and lateral views of the reconstruction for purposes of comparison (pls. XXVIII, 1; XXIX, 1), and in the course of their preparation the reconstructed remains were found to bear a striking resemblance to Tutankhamūn as depicted on the mummiform coffins, but no resemblance whatsoever

¹ See R. J. G. Sinclair, A. H. Kitchin and R. W. D. Turner, *Quart. J. Med.* 29 (1960), 19; and H. I. Wilner and N. Finby, *J. Amer. Med. Ass.* 187 (1964), 490 for a description of skeletal manifestations in the Marfan syndrome.

² *Arch. f. Anth.* 25 (1898), 329.

³ *The anatomy of the eye and orbit* (London, 1954), 23.

to Akhenaten (pls. XXVIII, 1-3; XXIX, 1-3).¹ It is also apparent that there is no facial resemblance between the reconstruction of these remains and the statue of Akhenaten in the Louvre often purported to represent Smenkhkarē (pl. XXIX, 4).²

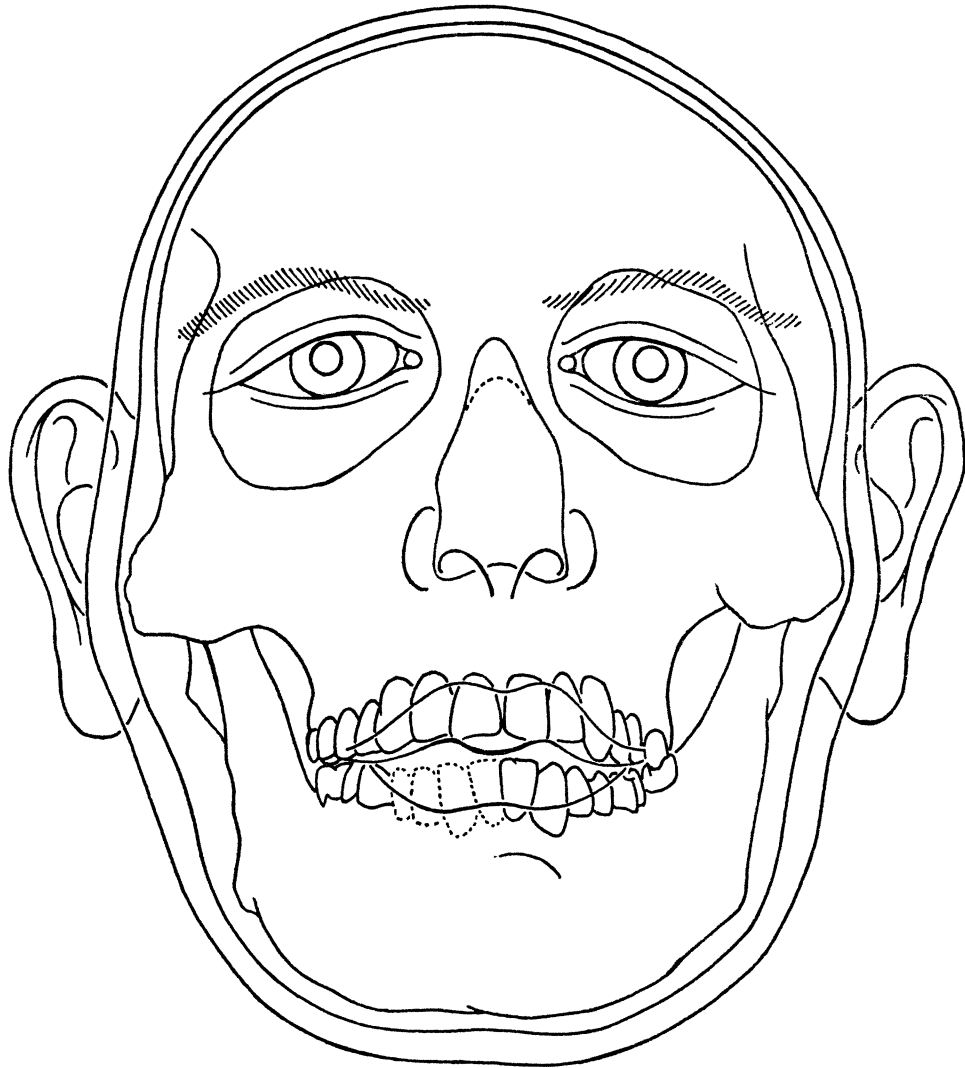


FIG. 1. Reconstruction of two outlines of the frontal view of the head and face on the skull according to the maximal and minimal values of Kollman and Büchly. Based on the photograph of the frontal view of the skull (pl. XXII, 2).

Such a resemblance between the present remains and Tutankhamūn has already been suggested on anatomical grounds by Derry, who found such a close correspondence between the measurements of the skull of these remains and the head of Tutankhamūn as to proclaim that they were brothers or at least closely related. When photographs

¹ The photograph of the face of Tutankhamūn shown on pl. XXIX, 2 is reproduced from K. Lange and M. Hirmer, *Egypt. Architecture, Sculpture, Painting in three thousand years* (London, 1956), pl. 184, by kind permission of Hirmer Verlag, Munich.

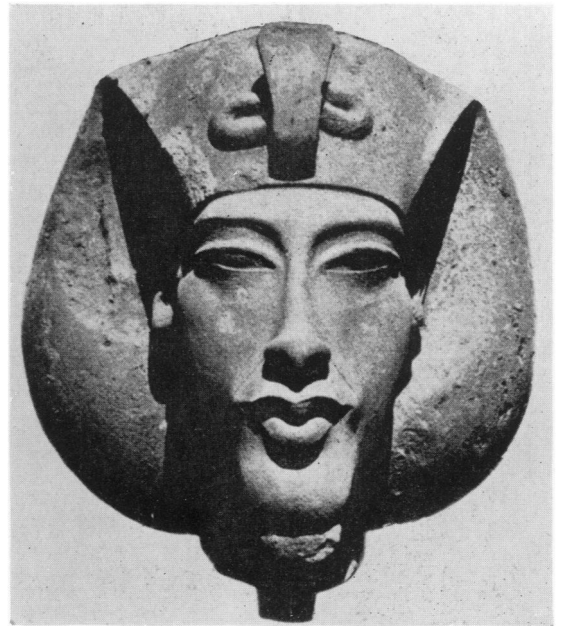
² Reproduced from op. cit., pl. 180, also by kind permission of Hirmer Verlag, Munich.



1. Reconstruction of frontal view of face according to mean of maximal and minimal values shown in text fig. 1

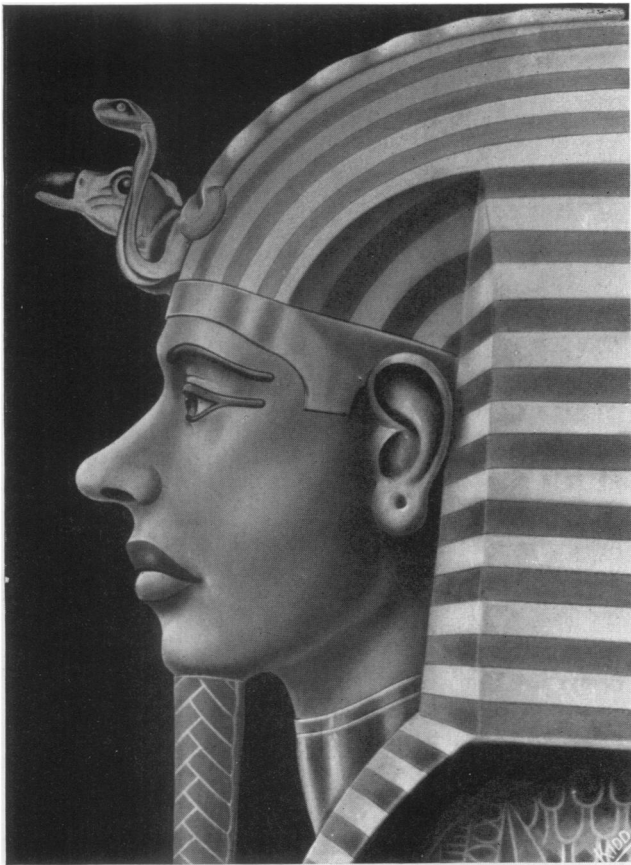


2. Frontal view of face of Tut'ankhamūn as depicted on the second mummiform coffin

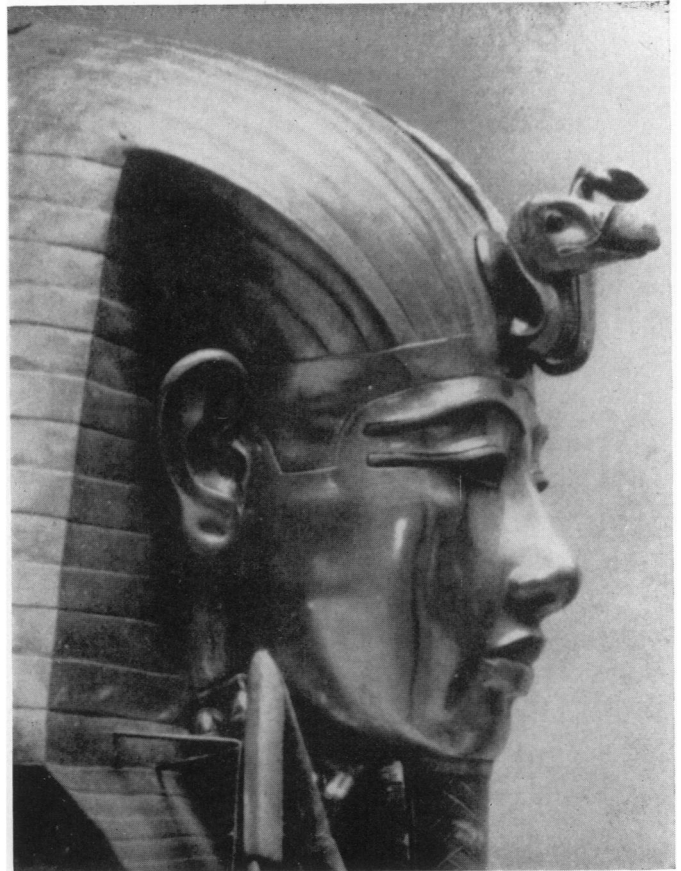


3. Frontal view of face of Akhenaten as depicted on one of the Karnak statues

ROYAL HEADS OF THE 'AMARNA PERIOD



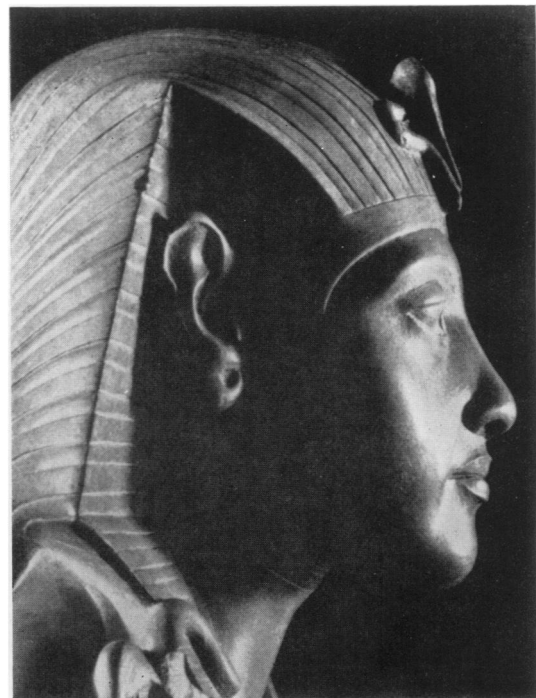
1. Reconstruction of lateral view of face according to mean of maximal and minimal values shown in text fig. 2



2. Lateral view of face of Tut'ankhamūn as depicted on the innermost gold coffin. By kind permission of Hirmer Verlag, Munich



3. Lateral view of face of Akhenaten as depicted on one of the Karnak statues



4. Lateral view of face of Akhenaten from a statue in the Louvre often said to be of Smenkhkarē. By kind permission of Hirmer Verlag, Munich

of the vertex of the two skulls are compared (pl. XXX), there is a similarity in their shape, both skulls being brachycephalic. Not only are the measurements of the skull vaults similar, but there is also a close correspondence in the width between the angles of the mandibles.

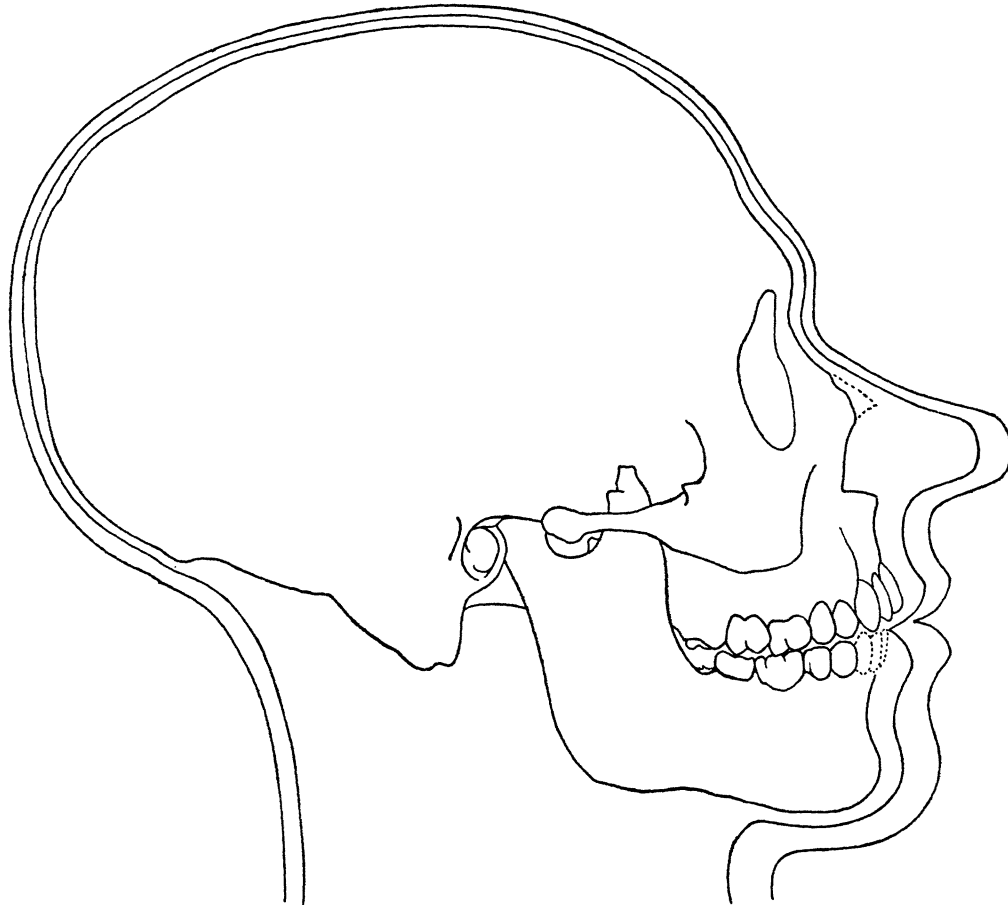


FIG. 2. Reconstruction of two outlines of the right lateral view of the head and face on the skull according to the maximal and minimal values of Kollman and Büchly.

The only qualification regarding the facial appearance in the reconstructions concerns the shape of the nose as seen in lateral profile (pl. XXIX, 1) owing to the damage to, and earlier reconstruction of, the nasal bones. With this qualification, however, it is believed that the reconstructions in pls. XXVIII, 1 and XXIX, 1 represent as accurate a likeness as it is possible to obtain.

The question of the identity of the present remains must therefore be carefully considered, and it is hoped that Egyptologists may be able to provide a more accurate and reasoned critique of the present findings. From considerations of physique, age at death, and facial appearance, however, it is impossible to concede a resemblance between the present remains and Akhenaten. The striking similarity in facial appearance with Tutankhamūn, and the age at death point much more forcibly to the

conclusion that these remains belong to Smenkhkarē. If this is so, a reappraisal of the anatomical and radiological features of Tutankhamūn is urgently necessary, and it is to be hoped that such an investigation may be made possible in the not-too-distant future.

APPENDIX I

Data from examination of the remains

CLAVICLE

Both clavicles show lack of union of the secondary centres at the medial end of the bone. The lengths, excluding these secondary centres, are:

Right: 154 mm. Left: 153 mm.

VERTEBRAL COLUMN

All the cervical vertebrae, including the axis, are completely ossified, but the inferior epiphyses of the fourth and fifth cervical vertebrae are not completely united with the body. All the thoracic and lumbar vertebrae are completely ossified except for the fourth and fifth thoracic vertebrae; the inferior epiphysis of the former and the superior epiphysis of the latter are not completely united with the body.

Breadth of atlas (male 74–90 mm.; female 65–76 mm.): 75 mm.

SACRUM

The sacrum consists of only four pieces, which have just united with one another.

Breadth at base: 107 mm. Anterior length: 96 mm.

Width of alae: 56 mm.

Proportionate width of alae (in female almost $\frac{2}{3}$ width of base): $\frac{56}{107}$ (i.e. approximately $\frac{1}{2}$).

The auricular surface extends over $2\frac{1}{2}$ vertebrae, and the sacral curve is straight. The coccyx is absent.

HUMERUS

	<i>L.</i>	<i>R.</i>
Length	319 mm.	322 mm.
Vertical diameter of head	44 mm.	46 mm.
Transverse diameter of head	40 mm.	42 mm.

An epitrochlear foramen was present in both humeri.

RADIUS

Length	240 mm.	242 mm.
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ULNA

Length	259 mm.	255 mm.
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The styloid process of the left ulna was slightly damaged.



VERTEX OF SKULL FROM TOMB 55 (on left) COMPARED WITH THAT OF TUTANKHAMUN (on right)

PHARAONIC REMAINS PURPORTED TO BE AKHENATEN 117

FEMUR

	<i>L.</i>	<i>R.</i>
Vertical diameter of head	45 mm.	45 mm.
Transverse diameter of head	45 mm.	46 mm.
Bicondylar width	71 mm.	70 mm.
Angle of neck	132°	132°
Length of whole bone	453 mm.	453 mm.
Trochanteric length (i.e. shaft length)	434 mm.	432 mm.
Popliteal length (apex of popliteal surface at bottom of linea aspera to centre of intercondylar line)	133-58 mm.	136-64 mm.

TIBIA

Length	372 mm.	375 mm.
Intercondylar breadth	71 mm.	70 mm.
Both fibulae were broken, and measurements were not taken.		

METACARPALS

	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>
<i>Length (mm.)</i>					
Right	44	68	67	57	53
Left	44	68	66	57	53
<i>Breadth of head (mm.)</i>					
Right	15	14	12	11	11
Left	15	13	12	11	11

SKULL

Greatest A.P. diameter (maximum glabello-occipital diameter): 190 mm.
 Maximum breadth = greatest transverse diameter: 154 mm.
 Basilo-bregmatic height: 136 mm.
 Height from opisthion: 142 mm.
 Upper facial length: 69 mm.
 Circumference (anteriorly over supraorbital ridges, posteriorly over upper part of occipital, so as to find maximum): 555 mm.
 Minimum frontal diameter: 98 mm.
 Maximum frontal diameter: 119 mm.
 Cephalic (cranial) index $\frac{(\text{Max. breadth} \times 100)}{\text{Max. length}}$: 81.05
 The skull is therefore brachycephalic.
 Bimaxillary breadth: 114 mm.
 Bizygomatic breadth: 137 mm. (This measurement is only an approximation since the left zygomatic arch is broken.)
 Nasal height: 50 mm. (It was not possible to measure the nasal breadth since the frontal process of the left maxilla is broken.)
 Breadth of right ascending ramus of mandible: 32 mm.
 Length of mandible: 74 mm.
 Depth of symphysis menti: 31 mm.

Height of ramus: 64 mm.

Angle of mandible: 120°

All the teeth are present and intact in the maxilla, but the right third molar is not fully erupted. All teeth are present in the mandible also, but the left first and right first and second incisors, the right canine and the right third molar are broken, with their roots preserved. There is minimal attrition of the teeth, and no exposure of dentine.

PELVIS

The left pubis had been broken. The secondary centres of ossification for both ischial tuberosities and iliac crests had not yet completely fused with the remainder of the bone.

Subpubic angle: $<90^\circ$

Pubic length (mm.) (Male range 65-83; female range 69-95): 76	} For right side only
Ischial length (mm.) (Male range 75-98; female range 69-93): 72	
Ischio-pubic index (Male range 73-94; female range 91-115): 94.7	

Greater sciatic notch (approximately): 90°

The pre-auricular sulcus is absent on both sides.

Depth of symphysis: 45 mm.

The right obturator foramen is oval in shape and measures 51×32 mm.

Distance between symphysis and acetabulum: 60 mm.	} For right side only
Width of acetabulum: 52 mm.	

There is slight eversion of the ischiopubic ramus on the right side.

$2\frac{1}{2}$ sacral vertebrae articulate with the ilium on both sides.

The ischial spines are not prominent and there is no flaring of the ilia.

Following reconstruction of the right hip bone with the sacrum it was possible to calculate the following measurements:

Interspinous diameter: 208 mm.

Intercristal diameter: 257 mm.

A.P. diameter of inlet: 101 mm.

It was also possible to determine that the outline of the pelvic inlet is heart-shaped.

APPENDIX II

Estimations of stature

TROTTER AND GLESER (1952). MALE WHITES

	<i>Minimum</i>	<i>Maximum</i>
$(3.08 \times \text{humerus} = 32.05) + 70.45 \pm 4.05$	165.014	173.214
$(3.78 \times \text{radius} = 24.10) + 79.01 \pm 4.32$	165.788	174.428
$(3.70 \times \text{ulna} = 25.9) + 74.05 \pm 4.32$	165.560	174.200
$(2.38 \times \text{femur} = 45.3) + 61.41 \pm 3.27$	165.954	172.494
$(2.52 \times \text{tibia} = 37.35) + 78.62 \pm 3.37$	169.372	176.112
$(1.30 \times \text{femur} + \text{tibia} = 82.65) + 63.29 \pm 2.99$	167.745	173.725
$(1.42 \times \text{femur} = 45.3) + (1.24 \times \text{tibia} = 37.35) + 59.88 \pm 2.99$	167.530	173.510
$(0.93 \times \text{humerus} = 32.05) + (1.94 \times \text{tibia} = 37.35) + 69.30 \pm 3.26$	168.306	174.826
$(0.27 \times \text{humerus} = 32.05) + (1.32 \times \text{femur} = 45.3) + (1.16 \times \text{tibia} = 37.35) + 58.57 \pm 2.99$	167.356	173.336
	Mean	166.958
		173.983

Average range = 166.958-173.983 cm.
 Mean = 170.47 cm.
 = 67.12 ins.

DUPERTUIS AND HADDEN (1951). MALE WHITES

	<i>Minimum</i>	<i>Maximum</i>
77·048 + (2·116 × femur = 45·3) ± 0·2308	172·672	173·134
92·766 + (2·178 × tibia = 37·35) ± 0·2436	173·872	174·358
98·341 + (2·270 × humerus = 32·05) ± 0·3094	170·786	171·404
88·881 + (3·449 × radius = 24·1) ± 0·2907	171·711	172·293
84·898 + 1·072 (femur + tibia = 82·65) ± 0·2508	173·248	173·750
87·543 + 1·492 (humerus + radius = 56·15) ± 0·2889	171·030	171·608
76·201 + (1·330 × femur = 45·3) + (0·991 × tibia = 37·35) ± 0·2173	169·512	169·946
82·831 + (0·907 × humerus = 32·05) + (2·474 × radius = 24·1) ± 0·2835	171·240	171·807
78·261 + (2·129 × femur = 45·3) - (0·055 × humerus = 32·05) ± 0·2328	156·844	157·310
88·581 + (1·945 × tibia = 37·35) + (0·524 × radius = 24·1) ± 0·2385	173·617	174·094
52·618 + (1·512 × femur = 45·3) + (0·927 × tibia = 37·35) - (0·490 × humerus = 32·05) + (1·386 × radius = 24·1) ± 0·1582	173·276	173·592
Mean	170·710	171·209
Average range = 170·710 - 171·209 cm.		
Mean = 170·959 cm.		
= 67·31 ins.		

DUPERTUIS AND HADDEN (1951). GENERAL FORMULA. MALES

68·089 + (2·238 × femur = 45·3) =	169·470
81·688 + (2·392 × tibia = 37·35) =	171·029
73·570 + (2·970 × humerus = 32·05) =	168·759
80·405 + (3·650 × radius = 24·1) =	168·370
69·294 + (1·225 × femur + tibia = 82·65) =	163·540
71·429 + (1·728 × humerus + radius = 56·15) =	168·456
66·544 + (1·422 × femur = 45·3) + (1·062 × tibia = 37·35) =	170·627
66·400 + (1·789 × humerus = 32·05) + (1·841 × radius = 24·1) =	168·105
64·505 + (1·928 × femur = 45·3) + (0·568 × humerus = 32·05) =	170·047
78·272 + (2·102 × tibia = 37·35) + (0·606 × radius = 24·1) =	171·387
56·006 + (1·442 × femur = 45·3) + (0·931 × tibia = 37·35) + (0·083 × humerus = 32·05) + (0·480 × radius = 24·1) =	194·272
Mean	171·278 cm.
	= 67·43 ins.

TROTTER AND GLESER (1958). MALE WHITES

	<i>Minimum</i>	<i>Maximum</i>
1·26 (femur + tibia = 82·65) + 67·09 ± 3·74 =	167·49	174·97
2·32 femur (= 45·3) + 65·53 ± 3·94 =	166·69	174·57
2·42 tibia (= 37·35) + 81·93 ± 4·00 =	168·32	176·32
1·82 (humerus + radius = 56·15) + 67·97 ± 4·31 =	165·85	174·47
1·78 (humerus + ulna = 57·95) + 66·98 ± 4·37 =	165·76	174·50
2·89 humerus (= 32·05) + 78·10 ± 4·57 =	166·16	175·30
3·79 radius (= 24·10) + 79·42 ± 4·66 =	165·76	175·08
3·76 ulna (= 25·9) + 75·55 ± 4·72 =	168·21	177·65
Mean	166·78	175·36
Average range = 166·78 - 175·36 cm.		
Mean = 171·07 cm.		
= 67·35 ins.		

A NEW FRAGMENT OF *THE WISDOM OF AMENEMOPE*

By BENG T JULIUS PETERSON

IN the winter of 1889–90 the famous Swedish anatomist Professor Gustaf Retzius travelled in Egypt as a tourist. He described his visit to Egypt in an entertaining book,¹ but a more important testimony of his travel was a collection of small antiquities which he brought home to Sweden. Later these antiquities were given to the Ethnographical Museum in Stockholm. Forgotten for several years they were in 1961 transferred to the Museum of Mediterranean and Near-Eastern Antiquities, *Medelhavsmuseet*, in Stockholm. During work with this Retzius collection in 1963 I found fragments of papyrus which contained passages of the well-known wisdom text called *The Wisdom of Amenemope*. The fragments may be identical with some contained in a mass of papyri, made up of small fragments, bought in Cairo but possibly originating from the Faiyûm;² but this is very uncertain.

The complete text of *The Wisdom of Amenemope* is found in a papyrus in the British Museum (B.M. 10474), containing a text of thirty chapters from beginning to end. Only a few parallel texts are known. In the Turin Museum there is a writing-board containing passages of chapters 24, 25, and 26. It was used by H. O. Lange in his work on the B.M. papyrus³ after an old transcription by Sir Alan Gardiner. An ostrakon with a few lines of text, mainly from chapters 1 and 2, in the Cairo Museum has been made known by a communication from J. Černý.⁴ A writing-board in the Louvre (E. 17173), the text of which corresponds to B.M. II, 15–18 (the prologue), has long been overlooked in the text-editions of Amenemope; it was published in 1886 by P. Virey.⁵ Another writing-board is preserved in the Pushkin Museum, Moscow, and contains passages from chapters 4 and 5.⁶ Further, the title *The Wisdom of Amenemope* is perhaps preserved in a graffito in Medinet Habu.⁷ As the B.M. text is not the best, especially from an orthographical point of view—it is rather carelessly written—every new parallel text which can elucidate it is of great importance. This is particularly the case since *The Wisdom of Amenemope* has in recent years been the object of much

¹ Retzius, *Bilder från Nilens land* (Stockholm, 1891).

² Ibid. 327–31.

³ Lange, *Das Weisheitsbuch des Amenemope aus dem Papyrus 10.474 des British Museum* (= Det Kgl. Danske Videnskabernes Selskab, Hist.-filol. Medd. XI, 2. Copenhagen, 1925).

⁴ Cf. G. Posener, 'Les richesses inconnues de la littérature égyptienne', *Revue d'Égyptologie*, 6 (1951), 43; R. J. Williams, 'The alleged Semitic original of the Wisdom of Amenemope', *JEA* 47 (1961), 106. Prof. Černý has most kindly sent me a hand-copy for which I am indebted to him.



⁵ Virey, 'Deux petits textes provenant de Thèbes', *Rec. trav.* 8 (1886), 169 f.

⁶ To be published by Posener, who has drawn my attention to this text as well as to the Louvre text. I thank him heartily.

⁷ Cf. Posener, *Revue d'Égyptologie*, 9 (1952), 119.

discussion,¹ and also since this *Wisdom* has been of general interest from the time when it was made known through the publications and commentaries of Sir E. A. W. Budge and H. O. Lange, and the translations of A. Erman and F. Ll. Griffith (among others).² Recently J. Ruffle has dealt with the *Wisdom* in a thesis presented at Liverpool University.³

The Stockholm fragment

The fragmentary sheet of papyrus in Stockholm (Inv. no. MM 18416) has been pieced together from several fragments of a golden brown colour. Only one small fragment does not join directly; it has, however, been placed in its correct place at the left edge. The whole fragmentary sheet (pl. XXXI) measures about 23.5 cm. × 14.5 cm.; the small loose fragment 3.2 cm. × 2 cm. The text has been written in customary columns. The actual remains of the text consist of parts of two columns; of col. I on the right there are just a few words, while the text of col. II is preserved to two-thirds of the original width. In each column there are sixteen horizontal lines. At the top and bottom of col. II there are single lines of corrections. The text of the sheet is written in a rather careless hand. The date is difficult to establish, but it is certainly not of a date before the Twenty-first Dynasty. The palaeography is similar to that of a group of texts from the Twenty-first and Twenty-second Dynasties, the oracular amuletic decrees collected by I. E. S. Edwards,⁴ e.g. B.M. 10083.⁵ Writings like  (col. II, 3) and  (col. II, 14) also point to such a date. The portion of the Stockholm text corresponds to B.M. x, 18–B.M. xiv, 5.

On the verso there are fragments of at least nine lines of text with irregular spaces between them, and of irregular length. Part of this text is blotted out, and it has not been possible to edit it here. It can be established, however, that this text does not belong to *The Wisdom of Amenemope*.

Translation and Commentary

One fundamental difference between the Stockholm fragment and the B.M. text of *Amenemope* is that the former is written continuously, while the text of the latter is divided into lines like poetry.⁶ The existence of corrections in the margins of the Stockholm text points to its being a school copy of *Amenemope*. As the right column has very few words preserved, I refrain from translating it. The text of col. II follows here in translation. What is printed in italics corresponds to the actual Stockholm portion; the rest is from the B.M. text. The division of the lines follows the B.M. text.

¹ Cf. the extensive bibliography compiled by J. Leclant in *Les Sagessees du Proche-Orient ancien. Colloque de Strasbourg 17–19 mai 1962* (Paris, 1963), 23 f.

² E. A. W. Budge, *Facsimiles of Egyptian Hieratic Papyri in the British Museum*, 2nd series (London, 1923), 9–18, 41–51, pls. i–xiv; H. O. Lange, *op. cit.*; A. Erman, *OLZ* 27 (1924), 241–52; F. Ll. Griffith, 'The teaching of Amenophis the son of Kanakht. Papyrus B.M. 10474', *JEA* 12 (1926), 191–231.

³ J. Ruffle, M.A. Thesis 1964, Liverpool University. (Unpublished.) I am very much indebted to Mr. Ruffle for his kindness in letting me see a copy of his thesis.

⁴ I. E. S. Edwards, *Hieratic Papyri in the British Museum*, 4th series (London 1960).

⁵ *Ibid.*, vol. II, pls. i–iii.

⁶ So also the tablets in Turin and the Louvre.

Col. II

B.M. XII

7. He makes a voyage among all the world;
8. he is laden with *false* words.
9. *He is like a ferryman [of snaring] speech;*
10. *he* goes and comes with quarrel.
11. *When he eats as well as when he drinks within,*
12. his conversation *is* outside.
13. *The day of establishing his crime*
14. *is a misery for* his children.
15. *Even* Khnum might come, *even* Khnum might come,
16. *the fashioner of the hot-tempered man,*
17. whom he has fashioned to . . . *hearts.*
18. *He is like a young wolf* in a farmyard;
19. he *turns one eye contrary to the other.*

B.M. XIII

1. *He causes brothers* to wrangle.
2. He goes before *every* wind *like* clouds;
3. *he destroys his character in the sun.*
4. He grasps at his tail *like a young crocodile;*
5. *he gathers himself together* like a maimed person (?).
6. His lips *are* *sweet, the tongue bitter;*
7. *the fire is burning* within him.
8. Do not leap to *embrace such a man,*
9. *lest a terror carry you away.*
10. Chapter 10.
11. Do not salute *the [passionate man] in forcing yourself,*
12. so that *you* grieve your own heart.
13. Do not *say praise [to] him in falsehood,*
14. *when there is a plot* within you.
15. Do not speak with *a man in falsehood,*
16. *the abomination of God.*
17. [*Do*] *not separate* your heart from *your* tongue,
18. *that all your plans will be successful,*
19. that *you will be important* before *others,*






B.M. XIV

1. *and you are safe in the hand of [God].*
2. God *hates* the falsifier of words;
3. *the great abomination is duplicity.*
4. Chapter 11.
5. *Do not . . .*




Commentary

Col. I, 1 = B.M. x, 18–19.


The version of this line in the Stockholm text does not exactly correspond to the B.M. text. *tw* at the beginning of the line ought to belong to a verb like the *wšd* of

B.M. x, 18. , however, could hardly be a corruption of  of B.M. x, 18, but must be an independent variant. The  is certainly prothetic before a following verb, which in B.M. x, 19 is , there written without .




Col. I, 2 = B.M. x, 21

 is part of *swdꜣ* of B.M. x, 21. The determinative is not quite clearly legible.  must be the beginning of .




Col. I, 3 = B.M. xi, 2

 is the end of *gm·k*. There is a trace of the *m* just before. Instead of the suffix *f* after *st* the B.M. text has the suffix *·k*.


Col. I, 4 = B.M. xi, 4

 corresponds to  of the B.M. text.  is the first sign of *hꜣpw*.

Col. I, 5 = B.M. xi, 6

. . . *tꜣw* is the end of *btꜣw*. There are only slight traces of . If the word-order of the Stockholm text corresponds to that of the B.M. text the *i* at the end of the line must be part of , the late writing of .

Col. I, 6 = B.M. xi, 8

Clear traces of the *nfr*-sign and the *f*; the group evidently without the -determinative.

Col. I, 7 = B.M. xi, 9

The *f* here must correspond to the last suffix of B.M. xi, 9.

Col. I, 8 = B.M. xi, 12

The *t* is in red ink and before it there is a trace, also in red, of the hieratic sign for 9. Here is the beginning of chapter 9.

Col. I, 9 = B.M. xi, 14

The only sign remaining here must be the determinative of the *sdd* of B.M. xi, 14.

Col. I, 10 = B.M. xi, ?

I am unable to identify the slight traces of this line with any word in the relevant part of the B.M. text.


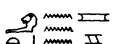
Col. I, 11 = B.M. xi, ?

The only sign preserved of this line, the piece of flesh (as a determinative), does not occur in the relevant part of the B.M. text.




Col. I, 12 = B.M. xi, ?

There are only some faint traces at the edge of the fragment.

Col. I, 13 = B.M. xii, 2


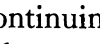

 is the end of  of B.M. xii, 2. There is a black dot at the edge above the waterlines.

Col. I, 14 = B.M. XII, 4






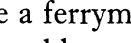
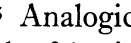
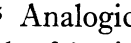


The *f* is the suffix in *dd.f.*  for  of B.M. XII, 4. The lower part of the sign  here forms a long stroke to the left.

Col. I, 15 = B.M. XII, 6

Col. I, 16 = B.M. XII, 8

 continuing with  in col. II, 1. A black dot at the right edge is presumably a trace of the preceding .

Col. II, 1 = B.M. XII, 8-10

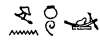
 is the continuation of  of col. I, 16, corresponding to the last word of B.M. XII, 8. For *iw* before the determinative cf. Erman, *Neuäg. Gramm.*, § 37; this spelling also in col. II, 12 and 13. : the corresponding text of B.M. XII, 9 has . From Lange's point of view the B.M. text is here erroneous. After *iry* he wants to read e.g. , 'eine *m*-Bildung aus *hnn* "streiten", also "er ist ein Streiter"'.¹ Griffith translates 'ferryman(?)'.² The parallel in the Stockholm fragment does not justify Lange's interpretation.³ The translation of the text of the Stockholm fragment is: 'He is like a ferryman ()'. A *m*-construction with *mi* for *m* could be possible⁴ but would seem extraordinary. Now, *mhnt*, 'ferry', also occurs without an *m* as, e.g., .⁵ Analogically a derivative  'ferryman' would not be unnatural. Also the lack of *iry* in the Stockholm text is in favour of the interpretation of *mi* as 'like' rather than of its being a variant of the *m*-prefix. A decisive argument for the interpretation of *mhnty* as 'ferryman' instead of the suggested 'Streiter' is to be found in the obvious parallelism between B.M. XII, 7 ('He makes a voyage among all the world') and B.M. XII, 9. It is not only a resemblance in form but also in matter. For an Egyptian 'making a voyage' implied using a boat. Therefore it was natural here to continue the literary metaphor with a reference to the ferryman who goes to and fro (B.M. XII, 10). Further, there is the parallel between B.M. XII, 8 and B.M. XII, 10; like the 'passionate man', who is laden with false words, the ferryman comes with troublesome words. This aspect of the ferryman's character seems not to be mentioned anywhere else in Egyptian literature.⁶ The four lines B.M. XII, 7-10 are a unity in the literary composition. The Stockholm text is corrupt;  must be added before . Presumably the signs above Col. II, 1 are the correction to the text here.

Col. II, 2 = B.M. XII, 11-12

In the B.M. text the line XII, 11 is written in red and 'seems to mark the beginning of the second half [of the chapter] numerically, without much reference to the

¹ Lange, op. cit. 67.

² Griffith, op. cit. 208.








³ This interpretation is due to Lange's attempt to render  of B.M. XII, 7 as a miswriting for *hnnw* 'Streit' ('tumult, uproar'), cf. Lange, op. cit. 66.

⁴ Cf. Grapow, *Über die Wortbildungen mit einem Präfix m- im Ägyptischen (Abhandlungen der Königl. Preuss. Akademie der Wissenschaften, Phil.-hist. Klasse. Nr. 5. Berlin 1914)*, 4 ff.





⁵ Ibid. 7, 11.

⁶ Cf. Grapow, *Die bildlichen Ausdrücke des Aegyptischen (Leipzig 1924)*, 152 f., where no debasing qualities of the ferryman are mentioned. But cf. also the Sumerian proverb 'The boatman is a man of belligerence', S. N. Kramer, *History begins at Sumer (London, 1961)*, 182.













⁷ The *m* omitted by haplography in B.M. XII, 9.

sense'.¹ This is not the case in the Stockholm text.  forms the end of  'eat'; in B.M. XII, 11 also with the determinative . At the left edge of the lacuna there are traces of ; but the lacuna is too broad for that group only. There is room possibly also for . To the left of the  there is a small hole. The *m* continues with a long stroke under the *hm*-group. Over the *nw*-group there is a space-filler. At the left edge of the fragment there is a trace of .



Col. II, 3 = B.M. XII, 13-14

hrw: for the spelling cf. Erman, *Neuäg. Gramm.*, § 50. *sr̥ bt̥w*, 'establish a crime', cf. Gardiner, 'A lawsuit arising from the purchase of two slaves', *JEA* 21 (1935), 144. After the *s* there are clear traces of  and ; *bt̥w* here with a direct suffix without the intermediate *tw* as in B.M. XII, 13. *ih̥mw* is clearly legible. The *ṣ* is blotted out, there is only a trace of *m*, and the  is small. *ih̥mw* corresponds to the  of B.M. XII, 14, and could be a scribal error for that word. Griffith's commentary on the content of this line: 'When he is accused, his children are involved in the heavy penalty',² is confirmed by, e.g., Pap. Lansing 7, 4.

Col. II, 4 = B.M. XII, 15-16

: B.M. XII, 15 has only . The *m* could hardly be the complementary *m* of *hnm*, for the name of the god written without a determinative would be extraordinary. The reading *m r̥-ṣ* confirms Lange's '*r-ṣ* zu Ende des Satzes bedeutet sonst wie  "auch"'.³ Ruffle's translation 'Even Khnum etc.'⁴ seems to be the most correct. The sign  is somewhat damaged; there is a hole on the right. The sign does not agree with the normal forms of . In the B.M. text there is a sign similar to , which is held by Lange to be 'ein misratenes '. Here the actual sign cannot be , which is written differently elsewhere, cf. col. II, 3. I am inclined to accept the actual sign in the Stockholm text as a form of . Of the *nhp*-bird there are clear traces. After that word there is a *n* in the B.M. text which is not in the Stockholm fragment. Lange and Griffith translate it as a preposition; it could, however, be the genitival adjective. The lack of *n* here points to its being the genitival adjective in the B.M. text. The word *t̥w* corresponds to ,  of B.M. XII, 16 (cf. B.M. v, 10) with the meaning 'enemy', 'fiery-mouthed'. The Stockholm fragment does not show a corrupt writing of that word but a synonym from the same stem *t̥*: 'be hot'. The word in the Stockholm text is identical with , 'hot-tempered', cf. *Admonitions* 5, 3. The change between the two words here may be due to an error of transmission.

Col. II, 5 = B.M. XII, 17-18

: the *b* with a long stroke to the right, is the end of a verb corresponding to  of B.M. XII, 17. Formerly the end of that line was considered corrupt, but Ruffle has rendered it clearly, taking *ngf* as a writing for *gnf* or *gfn*,⁵ which he translates 'plague'. Earlier translations took the verb as *gjf* 'bake, burn' leaving the *n* unexplained.

¹ Griffith, op. cit. 209.

² Ibid. 209.

³ Lange, op. cit. 67. Cf. *Wb*, II, 595, 6 ff.

⁴ Ruffle, op. cit. 117.

⁵ Ibid. 187.

The fragmentary word in the Stockholm fragment shows a variant. It supports Ruffle's interpretation insofar as the determinative 𐎎 points rather to a verb with the meaning 'plague etc.' than 'bake, burn'. Could $\text{𐎎} \text{𐎎} \text{𐎎}$ be the end of a verb like, e.g., *khh*, 'injure'? The suggestion by Lange, followed by Griffith,¹ that two lines have been omitted immediately after B.M. XII, 17 is not confirmed by the Stockholm text which has here the same sequence of text as the B.M. manuscript. *wn* at the end of the line is the beginning of *wnšw* of B.M. XII, 18.

Col. II, 6 = B.M. XII, 19–XIII, 1

$\text{𐎎} \text{𐎎} \text{𐎎} \text{𐎎} \text{𐎎}$: the *h* is clearly legible. The *n* is perhaps only a space-filler. Between *w* and 𐎎 there can have been no sign. This must be the end of the same word as B.M. XII, 19 $\text{𐎎} \text{𐎎} \text{𐎎} \text{𐎎} \text{𐎎}$, a writing of *msnh*, 'turn', where in the B.M. text the phonetically weak *h* has been omitted. In the Stockholm text there is a metathesis. 𐎎 is the younger form of 𐎎 . In the B.M. text 𐎎 only is common, cf. Erman, *Neuäg. Gramm.*, § 73. In my opinion this sentence, 'He turns one eye contrary to the other', characterizes the wolf as an insidious being. 'Turning one eye contrary to the other' is presumably an expression for squinting. The shape of the eyes of a wolf, fox, etc. and their place in the face of the animal can give the impression of squinting. Squinting has always been thought of as a debasing quality.² 𐎎 here, as in B.M. XIII, 1, an infinitive like the following $\text{𐎎} \text{𐎎} \text{𐎎}$ of line B.M. XIII, 2, cf. Erman, *Neuäg. Gramm.*, § 406. In $\text{𐎎} \text{𐎎} \text{𐎎}$ there are traces only of the *nw*-sign and no place for a complementary *w*.

Col. II, 7 = B.M. XIII, 2–3

The first word here *nbt* corresponds to *nb* of B.M. XIII, 2 being the adjective following $\text{𐎎} \text{𐎎}$. The *t* of *nbt* may be merely a space-filler. The word $\text{𐎎} \text{𐎎}$ in the Stockholm text must be an error of transmission for $\text{𐎎} \text{𐎎} \text{𐎎} \text{𐎎}$ of B.M. XIII, 2. The error is due to a misinterpretation of the hieratic script. The $\text{𐎎} \text{𐎎}$ of the B.M. text, for example, could easily be read carelessly as $\text{𐎎} \text{𐎎}$ instead of the correct $\text{𐎎} \text{𐎎}$. Further there is also a similarity in sound between the two words. The suffix *f* after *iw*n occurs also in the corresponding part of the B.M. text. Lange thinks that *f* 'ist zu streichen'.³ This proposal has been followed by most scholars. But with this suffix in two texts we can scarcely neglect it. The preceding *iw*n is not only 'colour' but 'character', 'nature' etc. also.⁴ If we read *sw hđ iw*n-*f* *n šw*, where *n* is *m* as is common in Amenemope,⁵ we can translate: 'He destroys his character in the sun.' The meaning is that the 'passionate' man, cranky in temper—he goes capriciously before every wind like clouds—is of bad character compared with his opposite, the silent man, who acts according to the firm qualities inherent in the *m*o*r*t-principle. But the sun, symbolizing this principle, defeats and destroys the bad man. Behind the literary metaphor, continued from B.M. XIII, 2, lies the actual observation of the natural power of the sun to disperse clouds.

¹ Lange, op. cit. 68; Griffith, op. cit. 209.

² Cf. J. A. Voth, 'Vom schielenden Blick', *Medizinischer Monatsspiegel* (Darmstadt 1955), Heft 7, 145. Cf. also the drawings on ostraca, *Doc. de fouilles IFAO*, II, 1 (1936), pl. 21. ³ Lange, op. cit. 68.

⁴ This meaning of *iw*n in the present context has also independently been pointed out by Ruffle, op. cit. 187 f. ⁵ Cf. A. Erman, *Neuäg. Gramm.*, § 603.

The word *šw*, here apprehended as a divinity,¹ is, however, not only the sun; in this word lies also the old conception of the divine being Shu, the lord of the air. An interesting parallel showing the sun as disperser of clouds is contained in a passage in the Israel stela, where Merneptah is likened to the sun (𓂏𓂛) which disperses (𓂏𓂛) the clouds (𓂏𓂛) from Egypt and makes the land see the sunlight.²

Col. II, 8 = B.M. XIII, 4-5

Above 𓂏 is a space-filler. At the left edge of the lacuna a trace of *n*. In the lacuna there is place for yet another sign which must have been an additional determinative for *twy*. The curious determinative of *mshwy*, the crocodile with the curved tail instead of the normal form with the straight tail, has perhaps been inspired by the content of the text: 'He grasps at his tail like a young crocodile.' Cf. the corrections at the bottom of this column. *d* . . . at the left edge is the beginning of *dmi*.

Col. II, 9 = B.M. XIII, 6-7

𓂏𓂛𓂛 is the end of *bnr*; there is a trace of 𓂏. 𓂛 is written with a dot above, as Möller, *Hierat. Pal.* II, 324, but beneath are some extra dots. At the left edge of the lacuna traces of 𓂏 and 𓂛. Some additional sign, a determinative of *bnr*, must be added in the lacuna. To *nst* may perhaps be added the suffix *f* as in the B.M. text. 𓂏𓂛𓂛𓂛, above *nw* a hole and a space-filler. The word corresponds to 𓂏𓂛𓂛𓂛 of B.M. XIII, 6 (cf. B.M. XXIV, 3). The writing in the Stockholm text could confirm Lange's proposal 'vielleicht dasselbe Wort wie *knj*',³ cf. *Wb.* v, 131, 'verdrossen sein o.ä.', as this word is also determined by 𓂏 (the 𓂛 determinative is of no importance). Its meaning must be the opposite of the meaning of *bnr*, 'sweet'. 'Bitter', as suggested by Griffith,⁴ or 'sour' would fit. *rk* . . . at the left edge is part of *rkḥ*.

Col. II, 10 = B.M. XIII, 8-9

pwfy (the *f* being quite clear), corresponds to *pfy* of the B.M. text. Only the left part of 𓂏 is preserved. There cannot have been any sign between 𓂏 and the preceding 𓂏; *hryt* here with the *t*, which is omitted in the B.M. text. After the last sign-group of this line there is a faint but clear trace of red ink, which shows that the rubric of a chapter followed.

Col. II, 11 = B.M. XIII, 11-12

The first sign, 𓂏, is the determinative of *wšd*. This word is followed in B.M. XIII, 11 by *tw* (so after neg. imperative) and *m*. Grammatically *tw* is not a necessary addition after a neg. imperative. The *m* of the B.M. text is possibly a confusion with the second *m* in the same line as pointed out by J. Ruffle.⁵ Therefore the Stockholm text is correct without the *m*. In the lacuna there is no place for the *pyw-k šmm* of the corresponding B.M. text, only for *šmm*. The group after 𓂏, determinative of *šmm*, must contain further determinatives but there are some holes in the papyrus. It seems

















































¹ Cf. the determinative in B.M. XIII, 3.

³ Lange, op. cit. 69.




























⁴ Griffith, op. cit. 209.

² Israel stela, 2-3.
























⁵ Ruffle, op. cit. 188.

clear that the group  consists of two different signs, my conjectural reading being .                             , which in B.M. XIII, 11 is                 , becomes the Coptic $\overline{\text{m}}\overline{\text{x}}\overline{\text{o}}\overline{\text{n}}\overline{\text{c}}$. *mt* . . . at the left edge is part of the *mtwk* of B.M. XIII, 12.





























Col. II, 12 = B.M. XIII, 13-14

dd·f must be emended to the *dd n·f* of B.M. XIII, 13, which, judging from the context, is the correct version. *izwt* here without the  of B.M. XIII, 13. For the writing of *cdziw* cf. above col. II, 1.                          , with clear traces of the *i* in the left edge, is *hryt*, as above col. II, 10. Ruffle here suggests the translation 'plot' which is better than the traditional 'terror'.¹

















































Col. II, 13 = B.M. XIII, 15-17

The first group of the line I have not been able to solve; the papyrus is partly damaged here and I can only reproduce a facsimile:                     . For the writing of *cdziw*, cf. above col. II, 1. At the left edge there is a small trace of . On the small fragment the upper line to the right is the lower part of a .














































































































Col. II, 14 = B.M. XIII, 17-19

 is the determinative of *nst*, B.M. XIII, 17.                            . The writing in the Stockholm text may be noted as an exception to Erman, *Neuäg. Gramm.*, § 25. The suffix of *shrw* here with the intermediary *tw*.

Col. II, 15 = B.M. XIII, 19-XIV, 2

                            is the end of                     of B.M. XIII, 19. The text of B.M. XIV, 1 omits the *k* after *izw*. The emendation with the *k* by Lange and Ruffle is now confirmed by the Stockholm text. Over the *p*-bird there is a red dot, probably accidental.

Col. II, 16 = B.M. XIV, 3-5

tz corresponds to *ty·f* of B.M. XIV, 3.                            is an error of transmission for the                            of B.M. XIV, 3, cf. above col. II, 13, where the Stockholm text has                           . To  the B.M. text adds                            which may presumably be added to the Stockholm text. At the end of the line there are clear traces of *m ir*.

Below this line are some miscellaneous signs all repeating signs which occur in the text of col. II. These signs may represent corrections and they seem to be written by another hand which formed its signs in a thicker and broader manner than those of the main text. The two crocodiles are interesting. As was pointed out above, col. II, 8, the crocodile with the curved tail was written as a determinative of *mshwy*, probably because of the content of the text, sportively. Here it has evidently been corrected, for the normal crocodile-sign has been written after the abnormal one. There are in this line five groups of signs; of a sixth, on the left, there is only a faint trace.

¹ Ibid. 189.

Handwritten symbol or characters at the top of the column.



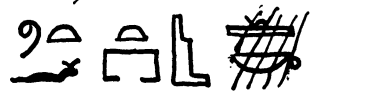



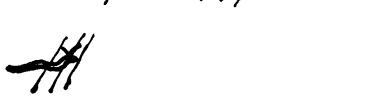





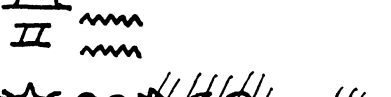


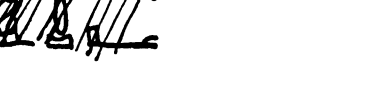
Main body of handwritten text, consisting of approximately 15 lines of dense, stylized characters.

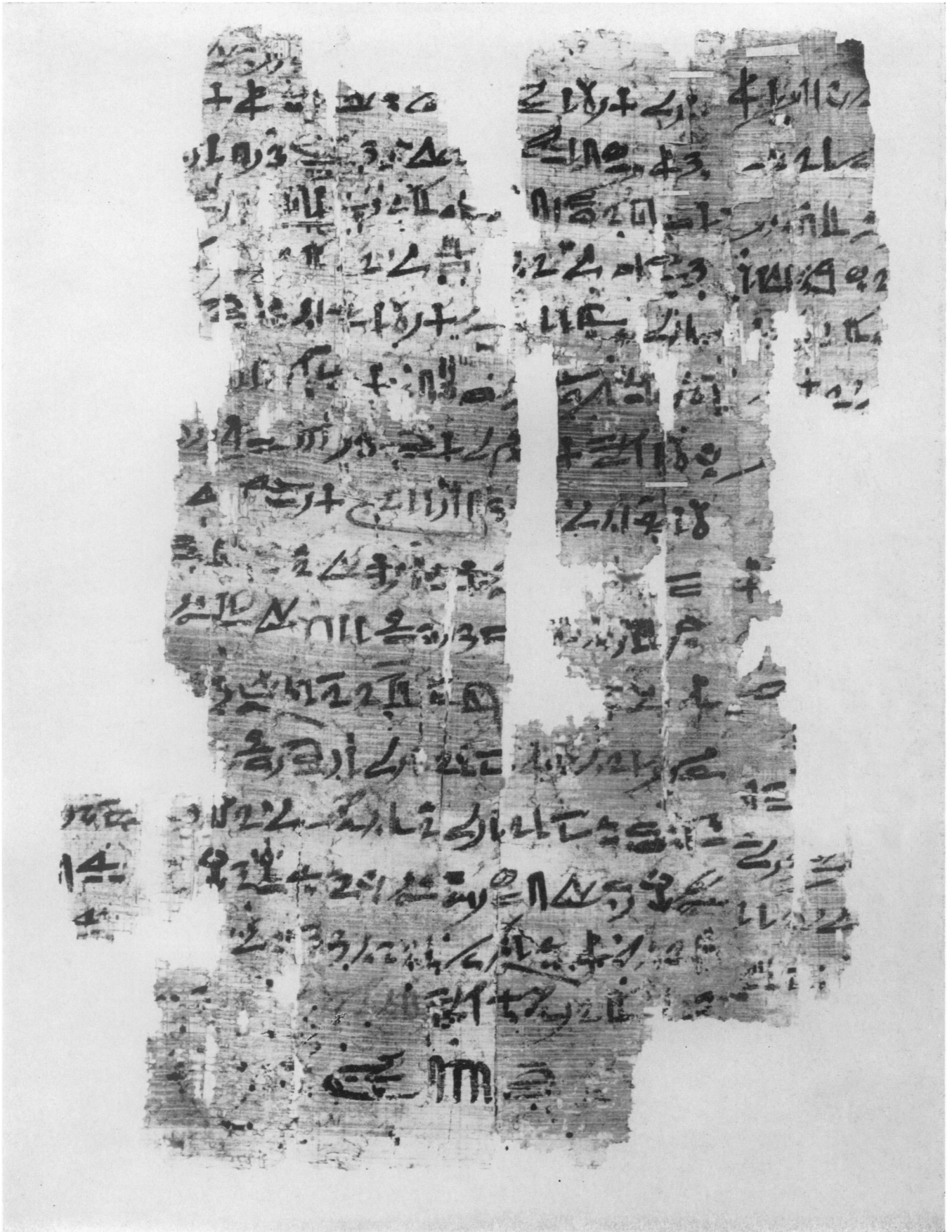
Handwritten symbols or characters at the bottom of the column.

Col. II

 1
 2
 3
 4
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 17

Col. I

 1
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 11
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 14
 15
 16



A NEW FRAGMENT OF *THE WISDOM OF AMENEMOPE*
Medelhavsmuseet, Stockholm, MM 18416 (slightly smaller than original)

FOUR PAPYRI FROM THE BRITISH MUSEUM¹

By REVEL A. COLES

THE four papyri here published form part of a group of eleven texts from the British Museum collection originally edited by myself as part of my doctoral thesis. Since then I have had the benefit of preliminary transcripts prepared some time ago by Sir H. I. Bell. The remaining texts in the group are to appear subsequently in this *Journal*.

A few brief general comments may conveniently be made here. All four texts originate from the Faiyûm, although acquired by the Museum at different dates and from different sources, and differing doubtless in exact provenance. All four are written on the recto of the papyrus. Lastly, all four have also been mounted so that it has not been possible to examine the verso of any of them: however, it may be presumed that each was blank.

I. ADVOCATE'S SPEECH (?)

P. Lond. Inv. 2910

22·2 × 22·0 cm.

A.D. 176?

The last twenty-four lines of a column from what seems to be an advocate's speech in a trial before a high official.² The latter is addressed as *ὁ κύριος* but is otherwise unspecified. The defendant, it seems likely, was Potamon, former strategus of the division of Heracleides of the Arsinoite nome; the present speech will have been for the prosecution. The date of the trial will perhaps be early in A.D. 176.

The point at issue would appear to be mismanagement in the collection of one of the farmed taxes, probably one of the taxes on sales (l. 8); the particular tax seems to be referred to in ll. 20 and 24, but only in general terms, and will presumably have been mentioned more explicitly in the earlier part of the speech, now lost, before the statement of the amount of the missing revenues in ll. 3-5. The history of the case was perhaps as follows. The tax-farmers had made serious losses over their contracts,³ and

¹ I should like to take the opportunity to thank Professor E. G. Turner, Mr. T. C. Skeat, Mr. P. J. Parsons, Dr. J. Rea, and Mr. W. Bennett for their valuable advice and suggestions on these texts; but especially Professor John Barns, who supervised me as a student, and to whom my debt is immeasurable. I should also like to thank the Trustees of the British Museum for the privilege of working on the texts and for permission to publish them here.

² Possibly this text should be classed as official correspondence, but the language in places has a marked rhetorical flavour (note esp. ll. 18-19). Is the document complete at the end? If so, this would strongly support its identification as a speech and not as official correspondence, and further as a copy of the advocate's speech itself and not of the trial as a whole: for such a papyrus containing only a single speech cf., e.g., P. Oxy. 472. As the text stands we have the lower portion of a column with a clear margin at the foot and on both sides. However, the appearance of completeness may be illusory; both palaeographically and contextually there could well have once been a succeeding column.

³ For similar difficulties at an earlier period, and the consequent troubles, cf. P. Oxy. 44. Mr. Skeat reminds me that the revolt of Avidius Cassius had taken place at the time with which the present text is concerned at this point.

so Potamon had allowed them a reduction in the amount of the revenues they were due to pay in.¹ The year specifically mentioned as that for which this reduction was granted was the fifteenth year (of Marcus Aurelius), i.e. A.D. 174/5. I suspect from *χρεῶσται* (l. 23) that this reduction may have been rather more than the circumstances justified: the tax-farmers have not simply made a loss, they are debtors—to the government—for the amount by which the reduction was excessive. Perhaps Potamon allowed them the reduction on condition that they passed a proportion of their consequent profits on to him. All this took place in the final year of the previous fiscal quinquennium.² In these circumstances the *κύριος* wrote to Apollonius, who had succeeded Potamon as strategus during the first year of the new quinquennium, with instructions to send him the accounts of the revenues from the sales-taxes for the present quinquennium so as to have a basis for comparison in assessing the fairness of Potamon's concession. Apollonius did not have these accounts, perhaps because he had only just come into office, and so wrote to Potamon asking for them. Potamon surrendered all the accounts except that he omitted to add what had been received for the present quinquennium as regards the particular tax at issue, lest it be only too clear to Apollonius that the tax-farmers had profited from the reduction, at the government's expense, and the tax in question be supervised by *ἐπιτηρηταί*; presumably because the tax-farmers would then have to repay the debt and so Potamon himself might have to return the profits he had made on the transaction.

The text is written along the fibres in a fine large semi-literary hand. On the left there is a margin of approximately 6 cm., and at the foot one of 3 cm. The margin on the right varies from just over 2 cm. to only about 0.5 cm.

.....[
 παρ' αὐτῶ[ν].....[].[
 νος τοῦ γραμματέως αὐτοῦ συγχωρ[ε]ῖ αὐ-
 τοῖς τὸν φόρον τοῦ ιε (ἔτους) δλόκληρον μὴ εἰ-
 5 σευγκεῖν ἀλλὰ παρὰ ρ (τάλαντα)· καὶ τοῦ[τ]ων
 οὔτως ἐχόντων, ἐν ᾧ κύριος ἔγραψας τῶ
 νυεὶ στρατηγῶ Ἀπολλωνίῳ ὥστε πέμ-
 ψαι σοι τῶν τελωνικῶν ὠνίων τὸν
 λόγον τῆς εἰσπράξεως τῆς ἀνὰ χεῖρα
 10 πενταετίας, τοῦτ' ἐστὶν τῶν ἀπὸ ιε (ἔτους),
 ἵνα κυρῶν τὰ ὠνία εἰδῆς πόσον εἰσηνέ-
 χθη ὑπὲρ ἐκάστης ὠνῆς καὶ οὔτως τὸν λο-
 γισμὸν μάθῃς. καὶ ὁ στρατηγὸς ἀκόλουθα

¹ It may be interesting to compare the troubles that arose over the Asian tax-contracts in the late Republic: see, for a discussion in brief, *CAH* IX, 508 and 518.

² A quinquennium at this period occurs frequently in P.Marmarica (= Norsa and Vitelli, *Il papiro Vaticano greco II Recto*: referred to especially by Reinmuth, *TAPA* 65 (1934), 259), where it is specified as being from 15 (ἔτους) ἕως κ (ἔτους), i.e. A.D. 175/6 to 179/80. These dates would accord very well with the data of the present text, and it is presumably the selfsame quinquennium which is referred to therein.

ποιῶν τῇ ἐνκελεύσει σου ἐπέστειλεν
 15 Ποτάμωνι τῷ πρὸ αὐτοῦ, διὰ τὸ αὐτὸν
 τοὺς λόγους μὴ ἔχειν, μεταδοῦναι αὐ-
 τῷ ὡς ἠθέλησας παρακειμένη[ς τ]ῆς
 εἰσπράξεως. ὁ γενναιοτάτος Ποτάμων
 τί ποιεῖ; μεταδιδούς γὰρ ὅλον τὸ βιβ[λίον],
 20 ἐπὶ ταύτης τῆς ὥνης οὐ προσγράφει τί
 εἰσπράχθη τῇ ἀνὰ χεῖρα πενταετία, ἵνα
 μὴ εὐδηλον γένηται τῷ στρατηγῷ ὅτι δὴ
 οὗτοι οἱ μισθωταὶ χρεώσταί εἰσιν καὶ
 παρατηρηθῆ ἡ ὥνη ὑπὸ ἐπιτηρη[τῶ]ν.

2. First letter after first lacuna: α or χ? 3. τοῦ added above the line. 18. Slight space
 after εἰσπράξεως. 19. Slight space after ποιεῖ. 21. ἵνα pap.

Translation

ll. 3 ff. '... allows them to bring in the revenue for the 15th year not in full but short by 100 talents. In these circumstances you, the lord, wrote to the present strategus Apollonius with instructions to send you the account of the revenues from the farmed taxes on sales for the present quinquennium, that is for those since the 15th year, in order that ratifying the taxes on sales you might know how much was paid in on account of each farmed tax and so learn the reckoning. The strategus, acting in accordance with your order, sent instructions to his predecessor Potamon, since he did not himself have the accounts, to hand them over to him as you required, with the amount paid in attached. What does our excellent Potamon do? While handing over the accounts in full, as regards this farmed tax he does not add what was paid in for the present quinquennium, lest it become obvious to the strategus that these tax-farmers are indeed debtors and the farmed tax be supervised by overseers.'

Notes

7. For the dates of the periods of office of Apollonius and Potamon (l. 15) see Henne, *Liste des stratèges des nomes égyptiens*, 55. The present text makes it clear that no other strategus held office between Potamon and Apollonius, although in the interval at present still undocumented (November 175 to July 176) this was perhaps unlikely in any case.

18, 19. See the critical notes above. Note that the pauses indicated come at a particularly dramatic moment in the speech. γενναιοτάτος (l. 18): here clearly not honorific but ironical.

34. ἐπιτηρηταί: note Wallace, *Taxation in Egypt*, 288. It is interesting to compare P. Oxy. 2472 with the present text.

2. UNDERTAKING TO RECOVER THE VALUE OF A DEPOSIT

P. Lond. Inv. 1969

10.3 × 22.4 cm.

A.D. 198

Affidavit addressed to the strategus wherein Soterichus, assistant to the sitologi, swears that he will recover for the collectors of the crown-tax the value of some wheat deposited by them with the sitologi and credited by the latter to the account of the state-cultivators of the village. As the στεφανικόν was a money-tax, this situation requires some explanation. Perhaps the πράκτορες had had to accept some payment

in kind, and the purpose of this operation was to convert the wheat so received into money so that their accounts might be set in order.¹

The text is written along the fibres in an untidy but practised upright hand which occasionally lapses into cursive. The subscription of Soterichus which follows is in a crude and very shaky upright hand, while the concluding date-formula is in a semi-cursive script which would appear to be by the same hand as the main body of the document and to be his more usual style.²

P. Lond. Inv. 1970 is a duplicate copy of this text. Variant readings are cited in the critical notes where they occur.

Βωλανῶ στρ(ατηγῶ) Ἀρσινοείτου Θ[ε]μίστ(ου)
 (καὶ) Πολέμωνος μερίδων.
 Σωτήριχος Δημοκράτους τοῦ Ἀπίω-
 νος ἐπικαλούμενος Μαριανὸς βο[η]-
 5 θὸς κειτολόγων κώμης Θεαδελ(φίας) γενή-
 ματος ς (ἔτους) ὀμνύω τῇ Αὐτοκρατόρων
 Καϊάρων Λουκίου Σεπτιμίου[υ Σε]ουήρου
 Εὐσεβοῦς Περτίνακος Ἀραβικοῦ
 Ἀδιβημικοῦ Παρθικοῦ Μεγίστο[υ] καὶ
 10 Μάρκου Αὐρηλίου Ἀντωνείνου Σεβαστῶν
 τύχην ἀπαιτήσιω ἕως Μεχεῖρ πεντε-
 καιδεκάτης ἀργυρίου δραχμᾶς
 χειλίας ὀκτακοσίας εἴκοσι οὔσας τρι-
 μῆν (πυροῦ) (ἀρταβῶν) κζζL μετρηθείσας ἡμεῖν
 15 ἐν θέματι ὑπὸ Χαρίτωνος καὶ τῶν κ[ο]ι-
 νωνῶν πρακτόρων στεφανικοῦ τῆς
 αὐτῆς κώμης Θεαδελφεις καὶ διαστει-
 λας ὑφ' ἡμῶν ὑπὲρ δημοσίων
 γεωργῶν τῆς αὐτῆς κώμης. ἐὰν
 20 δὲ μὴ ἀπαιτήσιω ἀποδώσω τῷ πράκτο-
 ρι τὸ ἐπιβάλλον μοι ἡμικυ μέρος
 ἀργυρίου δραχμᾶς ἑνακοσίας δέκα,
 μένογτος τοῦ λόγου τῷ Χαρίτωνι πρὸς
 τὸν κοινωνόν μου Ἰσᾶν περὶ τῶν τῆς ἄλλης
 25 ἡμίας (δραχμῶν) λγ· ἡ ἔνοχος εἶην τῷ ὄρκῳ.
 (2nd hand.) Σωτήριχος ὄμασα τὸν ὄρκον
 (1st hand) καὶ (2nd hand) ἀποδώσω καθὸς πρόκειται.
 (1st hand.) (ἔτους) ζ Αὐτοκρατόρων Καϊάρων
 Λουκίου Σεπτιμίου Σεουήρου Εὐσεβοῦς
 30 Περτίνακος Ἀραβικοῦ Ἀδ[ια]βημικοῦ

¹ It may be noted, in support of this explanation, that there is no mention of any *interest* at any point in the transaction; the operation appears to be one of simple exchange.

² A crease runs diagonally across the papyrus from top right to bottom left which was evidently caused in manufacture, as a gap is left in the writing in most lines where they cross the crease.

Παρθικοῦ Μεγίστου (καὶ) Μάρκ[ου Α]ῦρηλίου
 Ἄντωνίνου Σεβαστῶν, Φαῶφι β⁻.

1. 1. Ἀρσινοίτου 5. 1. *αιτολόγων* 6. 1. τὴν 9. 1. Ἀδιαβητικοῦ 11. 1. ἀπαιτήσειν
 13. 1. χιλίας, τιμὴν 14. 1. μετρηθειῶν, ἡμῖν 17. 1. Θεαδελφίας 17-18. 1. διασταλειῶν
 διασταλεῖσαι dupl. 24. ἴσαν pap. ἴσαν τρυφωνο(ς) dupl. 25. 1. ἡμικείας. της ημιας dupl.
 26. 1. ἄμοσα 27. 1. καθὼς πρόκειται 30. Ἀδ[ια]βητικοῦ: the lacuna should perhaps contain
 only one letter, the scribe having written Ἀδιβητικοῦ as in l. 9 above.

Translation

'To Bolanus, strategus of the divisions of Themistes and Polemon in the Arsinoite nome.¹

'I, Soterichus, son of Democrates son of Apion, surnamed Marianos, assistant of the sitologi of the village of Theadelphia for the produce of the 6th year, swear by the fortune of the Emperor Caesar Lucius Septimius Severus Pius Pertinax Arabicus Adiabenicus Parthicus Maximus Augustus and the Emperor Caesar Marcus Aurelius Antoninus Augustus that I will recover by the fifteenth of Mecheir one thousand eight hundred and twenty silver drachmae, being the value of 227½ artabae of wheat² measured to us on deposit by Chariton and his partners, collectors of the crown-tax of the same village of Theadelphia, and paid in by us on account of the state cultivators of the same village. If I do not recover it, I shall pay to the collector the half-portion pertaining to me, nine hundred and ten silver drachmae, without prejudice to the claim of Chariton against my partner Isas for the 910 drachmae of the other half; otherwise may I be liable to the consequences of the oath.

(2nd hand)³ 'I, Soterichus, have sworn the oath,' (1st hand) 'and' (2nd hand) 'I will pay as aforesaid.

(1st hand) 'The 7th year of the Emperor Caesar Lucius Septimius Severus Pius Pertinax Arabicus Adiabenicus Parthicus Maximus Augustus and the Emperor Caesar Marcus Aurelius Antoninus Augustus, Phaophi 2.'

3. REGISTRATION OF A MORTGAGE

P. Lond. Inv. 1975

10·8 × 20·0 cm.

A.D. 157

Notice of supplementary registration of a mortgage of catocic land addressed to the βιβλιοφύλακες ἐγκτήσεων of the Arsinoite nome, with above and below docketts of the record-office in a different hand. A parallel for this text is P. Heid.219 (publ. *Archiv*, 16 (1958), 158 ff.).⁴ The text is written along the fibres. The handwriting of the body of the document is, in the upper part, an upright semi-cursive; in l. 5 especially the writer has made a very clumsy attempt at the appearance of a book-hand. Further down the hand tends to slant to the right and becomes much more cursive. The docketts of the record-office above and below are probably both by the same hand, in a heavy slanting cursive.

¹ Bolanus: see also P. Teb. 330, 548; P. Lond. III. 1219 (p. 123).

² i.e. 8 dr. per artaba, not a particularly high or low rate at this period: cf. A. C. Johnson, *Econ. Survey of Ancient Rome*, II, 310 ff.

³ There is a rough mark at the beginning of the line before Σωτήριχος, perhaps made by the first hand holding the pen to the papyrus to indicate where Soterichus was to put his signature.

⁴ P. Heid. 219: here, most importantly, it seems [μεσι]ρείαν should be read in l. 9 (on filling the lacuna, note the wide μ in l. 5).

(2nd hand)] . [] [] . . . []
 [K]αίσαρ[ο]ς τοῦ κυρίου Ἐπειφ ἡ΄.

(1st hand) Ἰσιδώρω καὶ Ἄνταρίωνι τῶ καὶ Ἄρσινώω γεγυ(μνασιαρχηκόσι) βιβ(λιο-
 φύλαξι)

ἐγκ(τήσεων) Ἄρσι(νοίτου)

- 5 παρὰ Ταυήτιος (Σ)τοτοήτιος τοῦ Στοτοήτιος ἱερίας
 Σοκνοπαίου θεοῦ μεγάλου ἀπὸ κώμης
 Σοκνοπαίου Νήσου μετὰ κυρίου τοῦ ἀδελφ[ο]ῦ
 Στοτοήτιος ἐξ[ρ]έως τοῦ αὐτοῦ θεοῦ. χωρὶς ὧν
 ἀπεγρ(αψάμην) διὰ τῶν προτέρων βιβλ(ιοφυλάκων) προσαπογρ(άφομαι)
 10 καὶ ἦν ἔσχον μεσειτίαν παρὰ Ἀπίας ἀπά[τ]ορος
 μητρὸς Θερμουθαρίου τῆς Νίλου κατὰ διαγρ(αφήν)
 Ἀπίας τῆς Νείλου ἀναγρ(αφομένης) ἐπ' ἀμφόδ(ου) Μενδησίου
 ἀπογεγρ(αμμένης) διὰ [τ]ῶν πρὸ ὑμῶν βιβλ(ιοφυλάκων) μετὰ κυρίου
 τοῦ ἀνδρὸς Ἀπολλωνί[ου] τοῦ καὶ Ἄρτεμιδώ[ρ]ου
 15 Ἀπολλω[νίου], περὶ κώμην Ἀπιάδα Θεμίστου
 κ[λῆ]ρου κατοικικοῦ ἀρουρῶν τεσσάρων ἡμίσεω[ς]
 ἀ[πὸ] κοινῶν ἀρουρῶν ἕξ πρὸς ἀργυρίου κεφαλαί(ου)
 δ[ρα]χμὰς διεχιλία[ς] ἐ]ἰς ἐνιαυτὸν ἀπὸ τοῦ ἐνεσ-
 τ[ῶ]τος μηνὸς Ἐπ[ε]ἰφ τόκων δραχμειαίων ἐκάς-
 20 τη μνᾶ κατὰ μῆνα, [τ]ῶν συναγομένων τόκου
 τ[ο]ῦ ἐνια[υτο]ῦ δραχμῶν διακοσίων τεσ[σα]ρά-
 κ[ο]ντα. (2nd hand) Διόσκορος γρ(αμματεὺς) κατακεχώ(ρικα).
 (ἔτους) κ Ἄντωνίνου Καίσαρος
 τοῦ κυρίου Ἐπειφ ἡ΄.

5. ἱερίας pap. 1. ἱερείας 8. ἱερεως pap. 10. 1. μεσειτίαν 19. 1. δραχμιαίων

Translation

(Ll. 3 ff.) 'To Isidorus and Antarion also called Arsinous, ex-gymnasiarchs, keepers of the records of the Arsinoite nome, from Taetis daughter of Stotoetis son of Stotoetis, priestess of the twice-great god Socnopaeus, from the village of Socnopaei Nesus, with her guardian her brother Stotoetis, priest of the same god. Apart from what I have registered through the previous keepers of the records, I further return the mortgage I have received from Apia, who is illegitimate, her mother being Thermoutharion daughter of Nilus, in accordance with a contract made by Apia daughter of Nilus who is enrolled in the Mendesian quarter, and who has registered (property) through the keepers of the records preceding yourselves, with her guardian her husband Apollonius also called Artemidorus, son of Apollonius; (namely) of four and a half arourae out of six arourae held in common of a holding of catoecic land near the village of Apias (in the division) of Themistes, for the sum of two thousand silver drachmae for one year from the present month Epeiph, at the interest of one drachma per mina per month, the interest for the year amounting to two hundred and forty drachmae.' (2nd hand) 'I, Dioscorus, secretary, have registered this. The 20th year of Antoninus Caesar the lord, Epeiph 8.'

Notes

1. Only traces of occasional letters of this line remain.
2. With the reading here, in relation to the subscription at the foot, cf. the amendments to P. Heid. 219. 1 in *Archiv*, 17 (1960), 107.
- 2-3. A slightly wider space between these lines.
3. For Isidorus and Antarion cf. PSI 189 and P. Flor. 67; and also P. Varsov. 10, iii, 3-4 (see *Berichtigungsliste* III, 252). See *Actes Oslo*, 244.
- 8-9. *χωρίς ὧν ἀπεγραφάμην . . . προσαπογράφομαι*. Cf. Harmon, 'Egyptian Property>Returns', *Yale Classical Studies*, 4 (1934), esp. 178-80.
10. *μεσειτίαν*: note Mitteis, *Grundzüge*, 131. *ἀπάτορος*: note Calderini, *Aegyptus* 33 (1953), 358-69.
11. *κατὰ διαγραφὴν*: cf. P. Lips. 8. 7, 10; also 9. 15, 18. If I interpret the sense here aright, Tauetis has made the loan to the Apia of l. 10, but the *διαγραφὴ*, the contract of mortgage, has been made on behalf of the latter by her aunt, also called Apia, acting with her husband as guardian, so as to provide indirectly a legal *κύριος* for the illegitimate (and unmarried) Apia.
13. *ἀπογεγρ(αμμένης)*: or *(-αμμένην)*? The accusative would presumably refer to either *μεσειτίαν* in l. 10 or *διαγρ(αφήν)* in l. 11: but in either case the order of the clauses would seem strange and the resulting sense would be problematical. It may be better, therefore, to expand to *-αμμένης*, taking it as middle, and implying that Apia also has registered property previously and so will already have a place in the *διαστρώματα*. It would thus be equivalent in implication to *χωρίς ὧν ἀπεγραφάμην* in ll. 8-9; cf. the discussion of Harmon cited in the note on this above.
22. In P. Heid. 219. 24-25, read *Πτολεμαῖος γρ(αμματεὺς) κατακεχώ(ρικα)*? On the *γραμματεῖς* of the *βιβλιοφύλακες* note the P. Fam. Teb. archive.

4. CENSUS RETURN

P. Lond. Inv. 2196

9.5 × 19.8 cm.

A.D. 61/62

Declaration addressed to the *λαογράφος* of Kerkesis for the census of A.D. 61/62. Notification of this text, apparently the only return preserved for the census of that year, was sent to Hombert and Préaux, who, however, in their work *Recherches sur le recensement dans l'Égypte romaine* (1952) (hereafter referred to as H-P) had no opportunity to do more than include it in their list of returns together with a brief footnote (p. 173 and note, with p. 41 and n. 5). In 1957 a preliminary transcript by Bell was published by Braunert, *Symbolae Taubenschlag*, III, 66. The papyrus is here re-edited from the original and with a commentary.

In comparison with the rest of our material this text exhibits several peculiarities in formulae and other details (see the commentary below), but its relatively early date must be taken into account in any general assessment. Very few census-declarations are preserved from the first century A.D., the vast majority being of second-century date, and in view of the lack of homogeneity among the former and their differences from the latter it seems that at the date of the present text the style of census-declarations had not yet become stamped with the official and local forms that are regular later.

The text is written along the fibres in a documentary hand.

Ἡρακλήωι λαογράφωι¹
Κερκήσεωι

¹ There is a slight mark of ink before Ἡρακλήωι, perhaps from putting the pen to the papyrus before commencing writing. Cf. perhaps p. 133 above, n. 3.

παρὰ Κρονίωνος τοῦ
 Ἀρυώτου οἰκοδόμου τῶν
 5 ἀπὸ τῆς κώμης Κερκήσεως,
 καταγινομένου δὲ ἐν
 τῇ ὑπαρχούσῃ μοι οἰκίᾳ
 ἐν κώμῃ Τεβτύνει. ἀπο-
 γράφομαι εἰς τὸν τοῦ ἐνεστ[ῶτος
 10 η (ἔτους) Νέρωνος τοῦ κυρίου
 εἰκονισμόν·
 Κρονίων Ἀρυώτου μη(τρὸς) Θασῶτος
 οἰκοδόμος ὡς (ἐτῶν) νβ ρὺλ[ῆ]
 δακτύλῳ δευτέρῳ τῆς
 15 δεξι[ᾶ]ς χ[ειρός.]
 Κρονίων μη(τρὸς) Ταορσεύως
 (ἐτῶν) γ.
 Πάτρων μη(τρὸς) τῆς αὐτῆς ἐνι-
 αυσίς. θυγάτηρ
 20 Θενοκεύς μη(τρὸς) τῆς [αὐ]τῆς
 ὡς (ἐτῶν) ια.
 ἡ γυνή μου Ταορσεύς Πάτρ[ωνος]
 μη(τρὸς) Τεταρῶτος ὡς (ἐτῶν) λ.
 δουλικά μο[ν] σώματα
 25 []. φς. . [

9. ἐνεστ[ῶτος]: the edge of the papyrus is broken here, but the margin may have been narrow and possibly there would not have been room for the word in full; in which case read ἐνεστῶ(τος) (with ω set above the line)? 11. εἰκονισμόν set centrally, with space below. 12. Θασῶτος: the reading at the end is uncertain. There appears to be a letter (c?) set above the line. 14. δακτύλῳ: δ apparently corr. from κ. 18–19. 1. ἐνιαύσιος 21. Before ὡς: a slight mark of ink, perhaps just a slip of the pen. 23. A space between this line and the next. 25. After ς: c or ε? Last letter α? or possibly λ?

Translation

‘To Heracleus, registrar of Kerkesis, from Kronion son of Haruotes, builder, from the village of Kerkesis, residing in the house belonging to me in the village of Tebtunis. I register (the following persons) for the census of the present 8th year of Nero the lord: Kronion, son of Haruotes, my mother being Thasos, builder, aged about 52 years, with a scar on the second finger of the right hand. Kronion, his mother being Taorseus, aged 3 years. Patron, his mother being the same, one year old. My daughter Thensokeus, her mother being the same, aged about 11 years. My wife Taorseus, daughter of Patron, her mother being Tetaros, aged about 30 years. My slaves . . .’

Notes

1. For census returns from villages of the Faiyûm addressed to the *λαογράφοι*, cf. BGU 225

(Socnopaei Nesus, 161), 154 (Karanis, 161), and P. Flor. 301 (Socnopaei Nesus, 175). See H-P pp. 89-90, with p. 95.

2. This text is apparently the only return preserved registered at Kerkesis. See H-P p. 173.

4. *οἰκοδόμου*: statements of profession in the introductory section are infrequent (regardless of the provenance of the declaration). Most cases are of priests or similar; purely lay occupations as in the present text are especially infrequent. See H-P p. 103.

6-7. *καταγινόμενου δὲ ἐν τῇ ὑπαρχούσῃ κτλ.*: this phrase does not appear to be precisely paralleled. However, the whole introductory section of this return is very much conditioned by its being based on the *origo* of the declarant and not his place of residence. See on this Braunert, *Symbolae Taubenschlag*, III, esp. 61, n. 41.

9. The present text and SB 5661 (Philadelphia, 34 A.D.: see H-P p. 78) are the only declarations from the Faiyûm made during the census-year itself. (The regular formula of registration in Faiyûm declarations contains the phrase *τοῦ διεληλυθότος ἔτους*. See H-P p. 80, with pp. 81-84.) Note, however, my remarks in the Introduction above.

10. 61/62 A.D. See the Introduction above.

11. *εἰκονισμὸν*: the use of this word in the registration formula would appear to be unparalleled. The use of the word in this text is cited in LSJ⁹. However, it is frequent enough in other contexts, and one may also note usages such as *εἰκονισθεῖς*, P. Lond. 261 (II, 53) = *SPP* iv, 62, l. 10 and *passim*. See the discussion of Braunert in *Symbolae Taubenschlag*, III, 56 ff., esp. 61. (H-P p. 139.)

12 ff. On the list of persons returned, see H-P p. 118. Note here: 1. the absence of an introductory phrase. 2. The names of all persons declared are in the nominative. Besides BGU 154 (which is rendered less valuable as a parallel by grammatical errors) cf. the declarations from the Oxyrhynchite and Heracleopolite nomes: H-P pp. 120, 121. (Note also that some of the Oxyrhynchus returns are of first-century date.) 3. The absence of the phrase *ὁ προγεγραμμένος* accompanying the name of the person making the return; but see H-P p. 118 for parallels. (Note that this phrase is generally absent also in Oxyrhynchus returns: H-P p. 120.) 4. Grouping of persons declared by sex. This is unparalleled in returns from villages of the Faiyûm, and rare in returns from Arsinoe; but note it is regular in Oxyrhynchite returns. (H-P pp. 118, 114, 120.)

13. *οἰκοδόμος*: statements of profession in the list of persons declared are rare in returns from villages of the Faiyûm (and most examples are of priests), although not infrequent in returns from elsewhere. (H-P p. 119.) Cf. the note on l. 4 above.

13-15. The mention of distinguishing marks is infrequent in returns from the Faiyûm; see H-P p. 119, and also p. 118. Note, however, that it is a regular factor in Oxyrhynchite returns: H-P p. 121.

15. Restore *υἰοί* or *υἰοί μου* in the final lacuna? Cf. *θυγάτηρ* in l. 19 (see the note below), and *ἡ γυνή μου*, l. 22.

18-19. *ἐνιαύσιος*: with this in the sense of 'one year old' cf., e.g., Homer, *Od.* 16. 454; and note Jo. Eub. *innoc.* 3 (Migne, *PG* 96 1505 B: *ἐνιαυσιαῖος*).

19. *θυγάτηρ*: possibly read *θυγάτηρ μου*? Cf. l. 22 below, and note on l. 15 above. However, no traces of ink are visible after *θυγάτηρ* except one indeterminate spot below the *a* of *αὐτῆς* in the line above.

20. Note that the writer has attempted to place the name of each person declared at the beginning of the line, with the result that the last part of the preceding line is often left blank. Cf. ll. 16, 18 above; but the arrangement is particularly noticeable here with the break after *θυγάτηρ* in l. 19. The system has broken down though in l. 22.

24. Slaves occur only rarely in census-declarations from the villages of the Faiyûm. For the phrase, cf., e.g., PSI 1147. 19. See H-P pp. 118-19, with especially p. 119, n. 1.

EMBALMERS' 'RESTORATIONS'

By P. H. K. GRAY

THE art of embalming in ancient Egypt reached its pinnacle during the Twenty-first Dynasty, but as time passed deterioration of technique occurred, and in the later periods more and more attention was paid to the external wrappings and less and less to the treatment of the body. It is obvious that during the Ptolemaic and Roman eras many bodies were 'embalmed' when in an advanced state of decomposition. A good example of careless treatment is that of B.M. 21810 (Artemidorus).¹ This mummy, which dates from the Roman period, has a most elaborate exterior with a fine portrait-panel covering the head. Radiographs, however, reveal a very different state of affairs with regard to the treatment of the body, the thorax and spinal column being nothing more than a disjointed jumble of bones. X-rays have also disclosed other extraordinary manipulations of the later embalmers. Plate xiv in Moodie's memoir² depicts a composite radiograph of the wrapped mummy and coffin of a small boy named Pediamūn. In order to make the body fit the coffin it had to be shortened and made smaller. For this purpose both shoulders and arms were detached and discarded, the legs broken at the mid-thigh and the distal halves of both femora thrown away. The radiographs of a Late-Period child-mummy in a museum in Holland show an opposite manœuvre.³ In this case, the body was lengthened by the addition of two tibiae to the legs so that it might fill the coffin.

From time to time, however, it would appear that the consciences of some of the embalmers were pricked, for they set about to 'restore' the mutilated body. Elliot Smith and Wood-Jones report four such cases.⁴

Case 1. Mummy of a woman. In the body cavity were the right foot and both hands.

Some of the limb-bones were reversed, and others in disorder. The right foot and both hands were replaced by dummies made of linen.

Case 2. Mummy of a woman. In the body cavity were tarsal, metatarsal, metacarpals, and the left patella. The feet were dummies made of linen and resin with pieces of reed to represent toes. Hands also were dummies made of linen, resin, and a few bones. Both radii had been inverted.

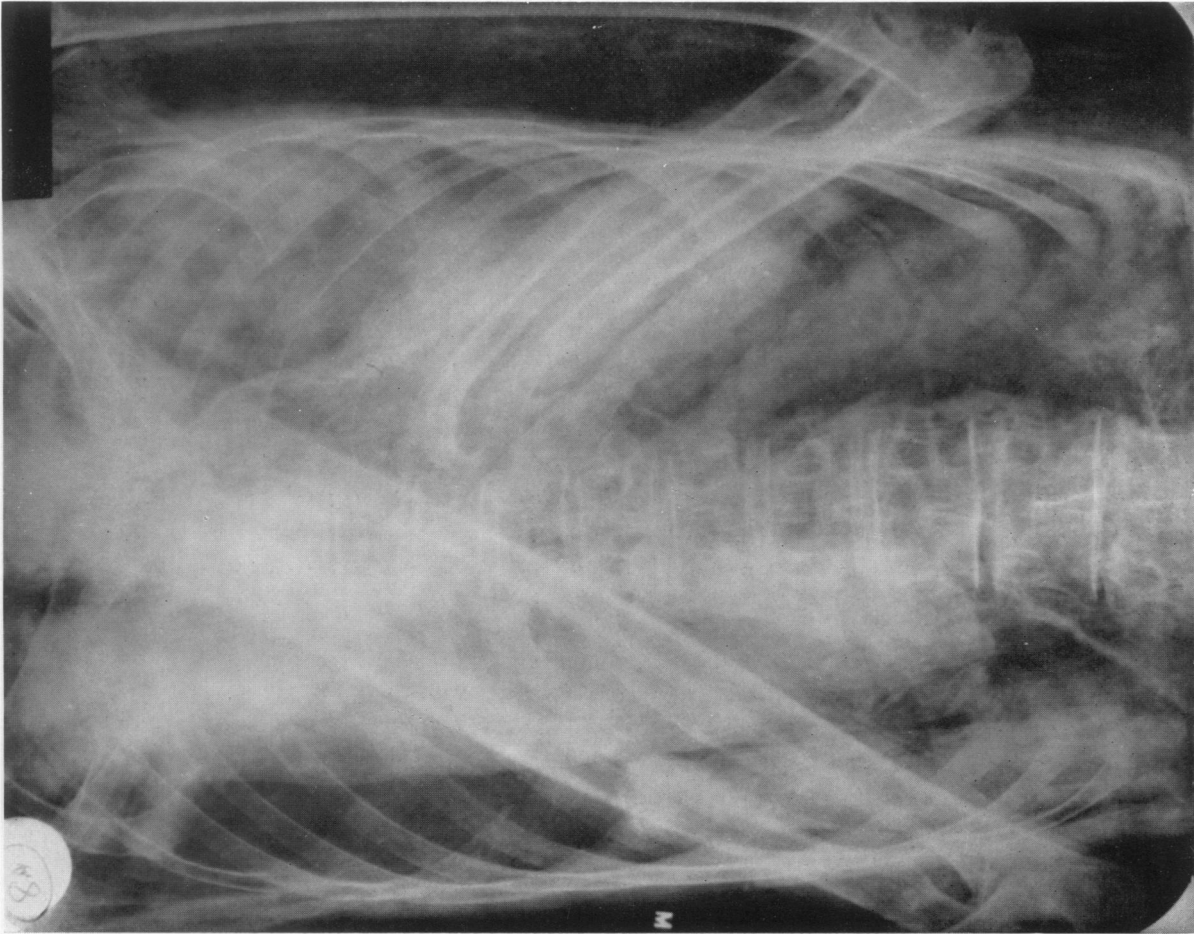
Case 3. Mummy of an elderly man. The body was quite complete, and no bones disordered. The right hand was missing, and had been replaced by a linen dummy.

¹ I should like to thank the Trustees of the British Museum for allowing me to X-ray this mummy, and J. G. Dain, Esq., of Kodak Ltd., for supplying and processing the radiographs.

² R. L. Moodie, 'Roentgenologic studies of Egyptian and Peruvian mummies' (*Field Mus. Nat. Hist., Mem.* 3, Chicago, 1931).

³ I should like to thank Dr. P. Vijlbrief of Leiden University for supplying me with the films of this case.

⁴ G. E. Smith, and F. Wood-Jones, 'Report on the Human Remains' (*Arch. Surv. of Nubia for 1907-1908*, vol. 2, Cairo, 1910), 214.

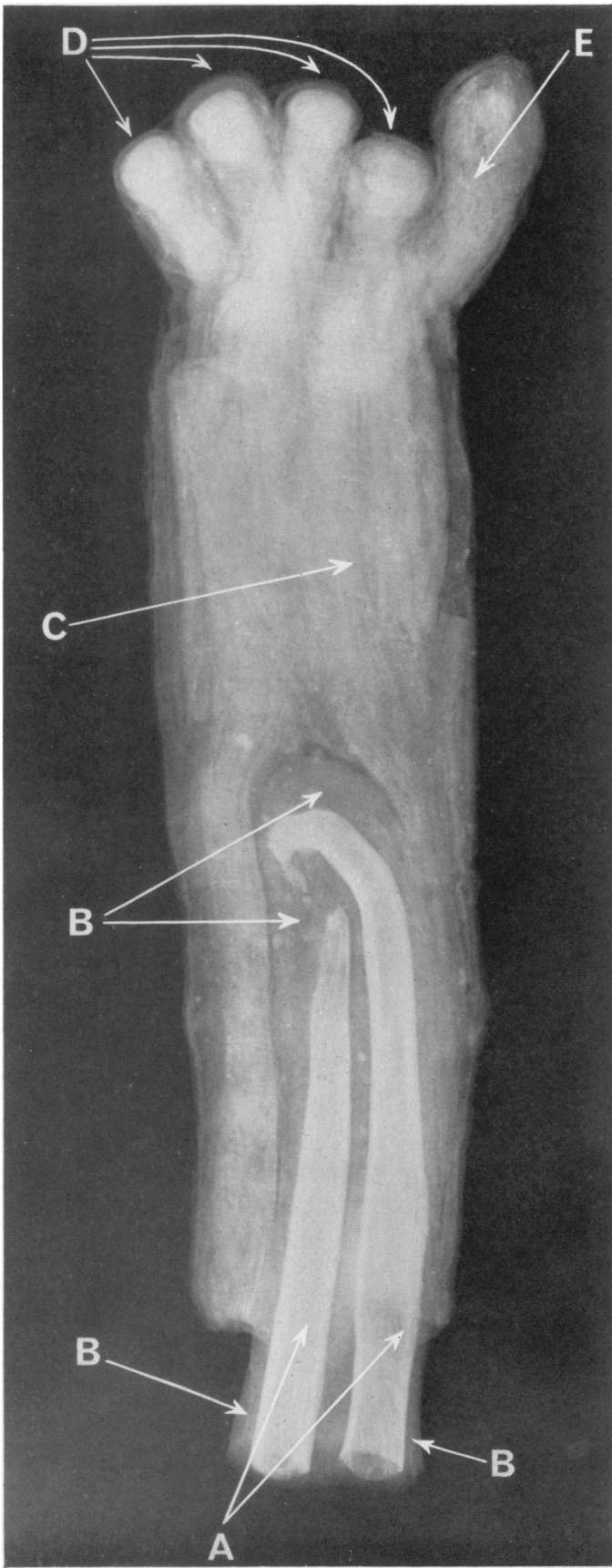


2. Radiograph of the thorax, showing the deformity of the left forearm and the attached artificial limb

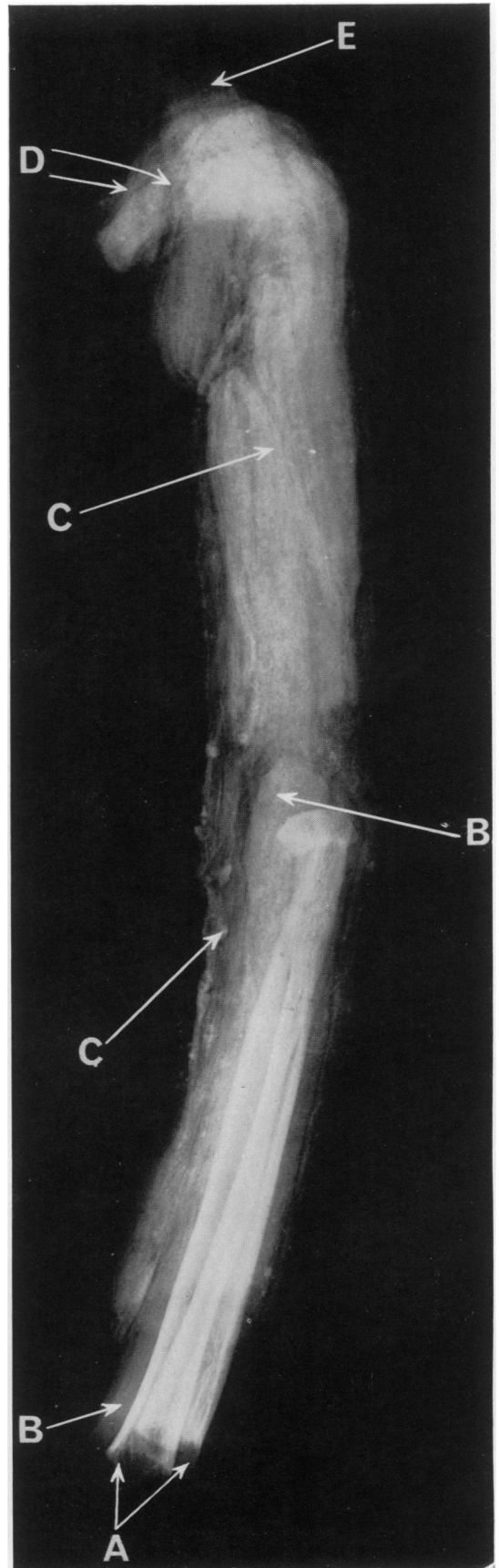
THE DURHAM MUMMY



1. Before the operation



1. Straight view



2. Side view

Radiographs of the the remnant of the left forearm with its artificial 'gauntlet'
(A=bones; B=flesh; C='gauntlet'; D='fingers'; E='thumb')

Case 4. Mummy of a woman. Many bones disordered and loose in the body cavity. No bones of the hands found. Both hands and forearms were false. The hands were made of linen and resin. The left forearm was composed of a left fibula, and the right of an inverted radius.

In all cases the outer wrappings and cartonnages were neat and undisturbed. All dated from the Ptolemaic age.

A case similar to the above has recently been detected by radiography in the Rijksmuseum, Leiden.¹ The mummy is that of a young child of uncertain sex. The outer bandages are covered with a thin coating of resin, much of which has flaked off. Radiographs showed the following bones to be missing:

- | | |
|-----------------------------------|---------------------------------------|
| <i>a.</i> The entire spine. | <i>d.</i> Majority of the ribs. |
| <i>b.</i> The pelvis. | <i>e.</i> Hands and wrists. |
| <i>c.</i> Scapulae and clavicles. | <i>f.</i> Portions of the long bones. |

The forearms were folded over the upper abdomen, and dummy hands had been fitted.

There is also a mummy fitted with a false arm in the Gulbenkian Museum of Oriental Art, Durham.² The specimen is on loan from the Darlington Public Library. Its exact provenance is unknown, but it is thought to have reached this country in about 1860. The mummy is that of an unknown person. It lies supine in the lower half of a wooden coffin, and has a detachable cartonnage mask, pectoral, apron, and boots. The outermost bandages, save those at the back, are covered with a thin layer of resin (pl. XXXII, 1).

The radiographs revealed a slight, well-embalmed male aged about 50-60. The mode of embalmment proved to be consistent with that employed during Ptolemaic times. There were several large resin-impregnated bandages within the body cavity, and a lateral view of the skull showed a mass of resin with a solidified 'fluid level' in its posterior aspect. The lumbar spine showed gross osteo-arthritic changes of the degenerative type, and there were several lines of arrested growth at the lower end of the tibiae, indicating an unhealthy childhood. The arms were crossed over the breast, right over left. The right arm appeared normal, but the left was most unusual (pl. XXXII, 2). At some time, many years before death, the left hand had been severed from the body a few inches above the wrist, and on to the stump had been fitted an artificial forearm and hand. The 'fingers' were flexed and the 'thumb' was extended (pl. XXXIII, 1, 2). As the general state of the body was in very good condition, it seemed possible the prosthesis might have been worn during life, and accordingly it was decided to examine it further. The mummy was carried to the diagnostic X-ray Department, Dryburn Hospital, Durham, where Dr. G. T. Holroyd made a detailed tomographic investigation, and the left forearm was explored during the summer of 1965. Dr. G. T. Holroyd, who was present at the investigation, reports as follows:

¹ I should like to thank Professor A. Klasens of the Rijksmuseum, Leiden, for allowing me to X-ray this mummy, and also Dr. P. Vijlbrief and the Staff of Kodak N.V., Den Haag, for their invaluable help.

² I am grateful to Mr. P. Rawson, Curator of the Gulbenkian Museum of Oriental Art, Durham, for allowing me to X-ray this mummy, and also to Dr. G. T. Holroyd, Consultant Radiologist, Dryburn Hospital, for his considerable help and interest in the case. I also wish to thank the Photographic Dept., Dryburn Hospital, Durham, for the pictures of the arm.

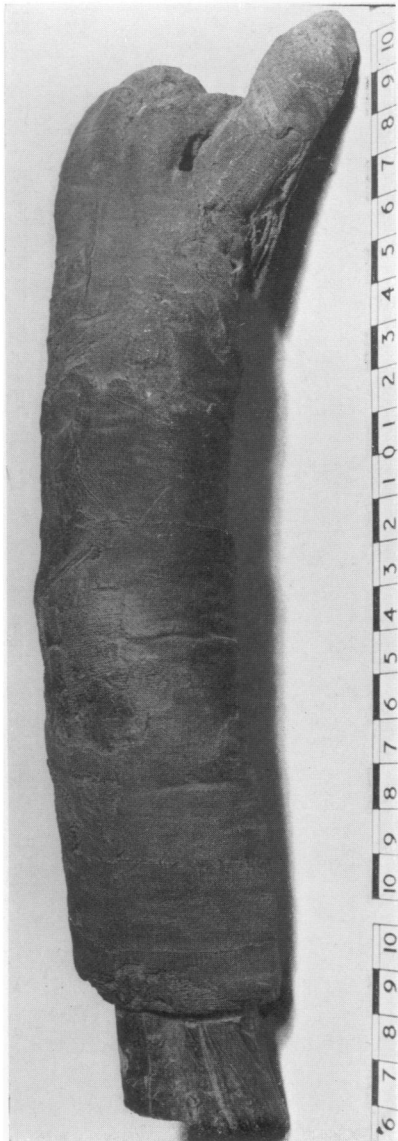
It was found necessary to remove part of the right forearm to gain access to the left. The forearm (left) was sawn through below the elbow and with some difficulty the false hand and part of the arm were removed. They confirmed the X-ray appearances in that the artificial part consisted of a gauntlet-shaped structure with the fingers partly flexed and the thumb extended (pl. XXXIV, 1-3).

The general structure was somewhat crude but not unrealistic. Further X-rays were taken of the forearm and this showed that the structure was made entirely of cloth and there was no wood present. It seems to have been built up by means of rolls of material laid around the forearm and extending up to the 'metacarpo-phalangeal joint level', further rolls were inserted amongst these to simulate fingers and thumb, and the whole wrapped by circular bindings and soaked in what appeared to the naked eye to be some form of resin.

The question whether the limb was fitted during life is still not settled, but it seems unlikely as, although the bones can be moved slightly in the cuff, they cannot be withdrawn, and in view of the shrinkage which would take place during mummification one would have expected that the arm would be freely removable. Careful measurement suggests that the inner width of the distal part of this cuff is a little greater than the proximal part, and if this is so it would account for the inability to withdraw the forearm. Furthermore, the general construction seems to be so flimsy that the hand would scarcely have stood up to even limited use.

It is quite clear from Dr. Holroyd's accurate description that the artificial limb was not worn during life, and that this is a further case of a conscientious embalmer's restoration.

The cause of the loss of part of the left arm will always be a matter of speculation, but it is almost certainly not due to disease or deliberate surgery. It is possible that some time during his earlier years the subject had become involved in a fight, and during this raised his left forearm, ulna aspect presenting, to ward off an oblique blow aimed at his head. If such be the case, he must have suffered considerable loss of blood, and the absence of modern treatment such as transfusion and antibiotics suggests considerable skill and devotion on the part of those who nursed him during the ensuing weeks.



1



2



3

Views of the part of the mummified left forearm over which has been fitted the artificial arm

THE DURHAM MUMMY

A LETTER OF SEVERUS ALEXANDER

By J. W. B. BARNES

THIS document, the property of the Egypt Exploration Society, by whose permission it is published here, was found in one of the numerous boxes of papyrus fragments from Oxyrhynchus at present stored in the Ashmolean Museum at Oxford. When the purport of the main part of it, *a* (14.3 × 14 cm.), was ascertained a careful search in the same box produced two more fragments, one of which joined *a* and helped to complete the text on the verso; the other, *b* (3.2 × 2.1 cm.), could not be related to *a* on the evidence of fibre patterns (see commentary, below), but seems safely placed on textual considerations. The quality of the papyrus is good, though not superfine.

The body of the letter is written along the fibres of the recto in a semi-literary hand with infrequent ligatures and only occasional cursive forms (ϵ , κ , σ). Most of its letter shapes can be paralleled in PSI 1094 (Scholia on Callimachus, *Iambi*), conjecturally assigned to the second century A.D. by ed. pr. (Vitelli, *Bull. Soc. Arch. Alex.* 24 (1929), 1 ff.); forms basically similar may be seen in P. Berl. 7094 (second/third century) and 6934 (third century), see *Berl. Kl. T.* iii, 5 ff. These are all professionally, if not elegantly written; the present text, however, is not. Its hand is by no means uneducated, but a certain irregularity in the shapes, slope, and spacing of the letters conveys an impression of inexperience—or of immaturity. The verso bears a single line of writing, along the fibres and at right angles to the recto text, in a wholly different style; it is a perfect example of the more formal type of chancery hand, for which see Gerstinger, *Wiener Studien* 47 (1929), 168 ff., and *Oxy. Pap.* xix, 81 f. (on P. Oxy. 2227), where further references are given. The closest parallels to our hand may be seen in the address (facsimile, art. cit. 172) of Gerstinger's P. Vind. 24473, dated by him conjecturally to about the time of Hadrian; P. Berl. 11532 (Schubart, *Pap. Berol.*, pl. 35, A.D. 209); and PSI 1247 (assigned by its editor to the second century).¹

The orthography of the letter is excellent—its only blemish is the correction in l. 10—and its composition seems good. Of its general import there can be little doubt. The nearly complete lines 5–11 show it to be a personal message to a body of people (they are addressed in the plural) by an emperor newly elected by 'the most noble soldiery'; he commends the loyalty of this body and promises a visit and appropriate benefactions. The identity of the emperor seems indicated by the beginning of l. 1: $\alpha\lambda\epsilon\xi\alpha\nu$ [²; with the generally agreed date of Severus Alexander's accession, March 6 or 11, 222, the date with which the letter concludes, 'Year 1, Pharmouthi (= March 27–April 25) the—', agrees well. The verso, however, directs the letter to an individual—'Apolinarius, senior (?) $\beta\omicron\upsilon\lambda\epsilon\upsilon\tau\acute{\eta}\varsigma$ '.

¹ A short writing exercise in the same style is P. Oxy. 2604 (3rd cent.), written (probably) by a government official on the back of one of his own documents.

² See, however, p. 144, n. 1.

I believe the difference between the hands to be highly significant. Were it not for the line of writing on the verso, the informal and unofficial style of the recto might suggest a copy of a public communication, made for some purpose by a private individual. But the address in its formal chancery hand seems to stamp the text as an original document, emanating from a government office. The almost childish informality of the letter, contrasting with the formality of its address, suggests to me that we have here not merely a personal message of the young emperor, but his actual autograph. His announcement was too momentous to be entrusted to a scribe; its authenticity had to be vouched for by his own handwriting. This is perhaps less improbable than it might at first seem. I am indebted to Mr. Fergus Millar for access to an unpublished study of his which convincingly refutes the widely held assumption that every emperor conducted all public business through a secretarial bureaucracy; especially relevant to the present case is evidence which shows us emperors writing part, or even the whole, of documents in their own hand.¹ He cites a number of instances from ancient sources which mention letters (mostly semi-official or private) written by emperors *αὐτοχειρίᾳ*. That this can have been the case here is made all the more credible from the account which ancient writers give of Alexander's personal character. The *Historia Augusta*, which represents him as a paragon, emphasizes his conscientious personal attention to public business (*Vit. Sev. Al.* 29, 4; 31, 1 ff.), as well as the excellence of his education, particularly in Greek (*ibid.* 3, 1 ff.; cf. 27, 5); the composition of this letter would perhaps not be beyond the capacity of a precocious lad of thirteen and a half years, though it may well have owed something to the guidance of his mother, which played such a large part in his life (*H. A. Vit. Sev. Al.* 14, 7; 26, 9; 60, 2; Herodian, VI, 1, 5. 9 f.; Aur. Vict. 24, 5; Eutrop. VIII, 23; Zon. XII, 5; Julian, *Symp.* 313 A-B).

Some problems remain to be solved. First, for whom precisely is the letter intended? The addressee of the verso, Apolinarius, is unidentified. The body addressed in the letter itself might have been specified in ll. 1 f. (see commentary, however); but I think that if τ[(rather than π[) is to be read in l. 12, the city which they represented will have been named there. Since the papyrus was found at Oxyrhynchus, the most obvious restoration would be τ[ὼν Ὀξύρυγχιτῶν, which would not be impossible if we supposed that the young emperor intended a general tour of Egypt; Oxyrhynchus was an important place in the Roman period (see Turner, *JEA* 38 (1952), 78 ff.; cf. Mertens, *Les Services de l'état civil et le contrôle de la population à Oxyrhynchus au III^e siècle de notre ère* (1958), v f.), and a letter to the authorities of each of the principal places to be visited would hardly be out of the question. But we have no record of such a tour by Alexander, or indeed of his presence in Egypt at any time;² had such a progress taken place, the absence of any record of it in literary or documentary sources would seem surprising. Possibly it was projected and promised, but abandoned. I suspect, however,

¹ A striking example is the story in Eutropius IX, 15, 2 of Aurelian's betrayal by a slave who showed a list of individuals marked down for execution in a forged imitation of the emperor's handwriting; if the latter were not familiar there would have been no point in forging it.

² The mention in *H. A. Vit. Sev. Al.* 28, 7 of malicious references to his Syrian origin by 'Antiochenes, Egyptians, and Alexandrians, on the occasion of a certain festival', even if it is to be taken seriously, need hardly imply a *visit* to Egypt.

Restored text

Ἀλέξαν[δρος (c. 13 letters)]ισενα.[
 καὶ παρα[(c. 17 letters)].ς ἐπικ[. .].[
 τὴν προ[(c. 16 letters) ἐ]μὲ εὐνοια[ἐν]
 τοῖς στέρνο[ις ἔ]τι φέρουτ[ε]ς τὴν σπ[ο]υδ[αίαν]
 5 γνώμην π[αρ]εμείνατε ἀφικνουμ[
 πρὸς ὑμᾶς τύ[χ]η ἀγαθῆ κεχε[ι]ροτονη[μένος]
 μὲν αὐτοκράτωρ ὑπὸ τῶν γενναιοτάτ[ων]
 στρατιωτῶν, ἐπὶ δὲ τὴν ἀρχὴν πα[
 αἰσίως πα[ρ]ελευσόμενοι[ς], καὶ ἀφ' ὑμ[ῶν τὰ]
 10 μάλιστα ἀρξάμενος τῆς τοῦ εὐ ποιειν [ἐξου-]
 σίας, ὅσον δίκαιον ἔστ[α]ι παρέχειν [τῆ μη-]
 τρώα πόλει .[
 (13) [
 [ἔτ]ους ᾧ Φαρμ[οῦθι—

Verso

[Α]πολιναρίωι X βουλευτ(ῆ) πρε[σβυτάτ(ω)?]

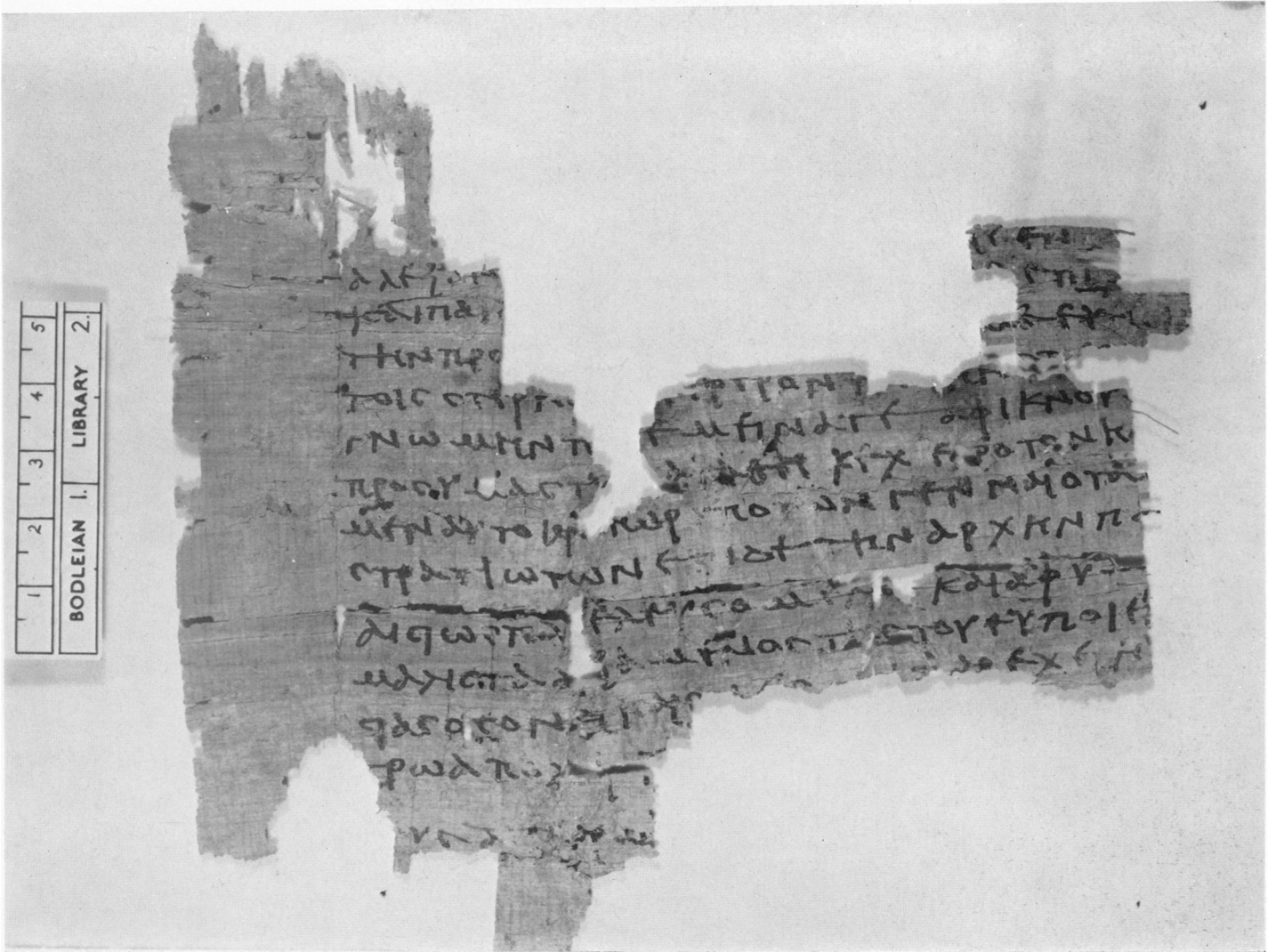
1-4. The position of fr. *b* cannot be determined by fibre patterns; the correspondence of its front (horizontal) fibres with those at the level of the first four lines of *a* is probable, but not distinct enough to be conclusive, and the right-hand half of *a* has been entirely stripped of back fibres. It seems impossible, however, to place *b* anywhere but in the first four lines; a position farther to the left seems excluded by the incompatibility of the traces of *b* 4 with anything in *a* 4 before *ην*; and it can hardly be placed farther to the right, since the length of the line is determined by some certain supplements.¹

Assuming that *b* has been rightly placed, σπ[ο]υδ[αίαν in 4 seems likely. The restoration of the first three lines, however, is uncertain. In l. 1 we must suppose the emperor's self-designation to have been short enough to have been lost in the first long lacuna: Καῖσαρ or Σεβαστός or both;² the letters in *b* cannot be part of his title. Turner suggests—I believe rightly—that the whole address was contained in the first line. Could it have run Ἀλέξαν[δρος Καῖσαρ Σεβαστός το]ῖς ἐν ἀρ[χ]ῆ χ(αίρειν)? (There would not be room for the full writing of the last word.)

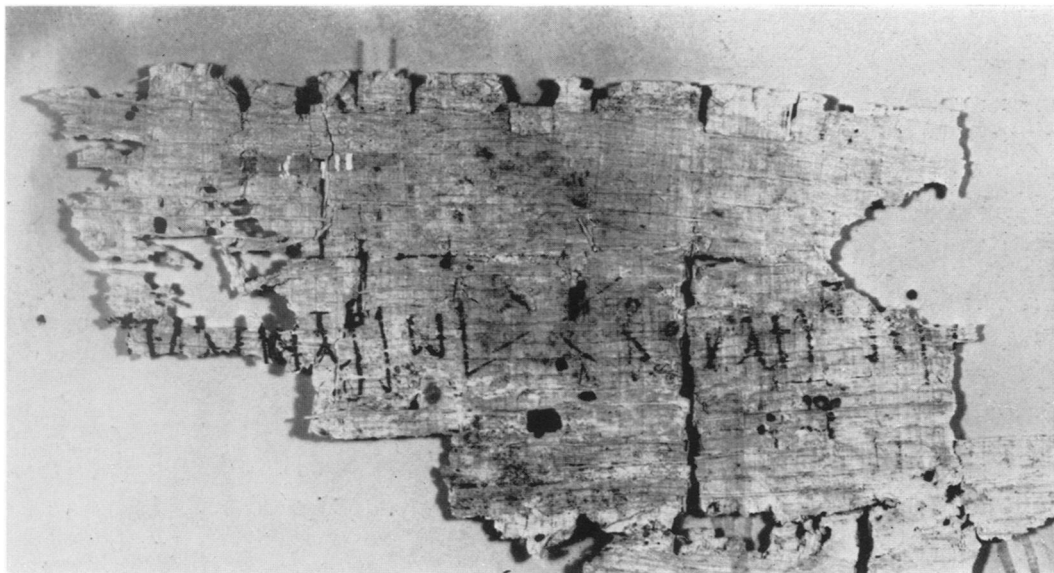
2. Turner, noting that letters not infrequently begin, after the address, with καί, suggests some openings, of which the one which best suits the traces and the space is: καὶ παρα [τῶν ἄλλων ἐπιστάμεν]ος. I cannot restore ἐπικ[. .].[after this; perhaps it was an adverbial expression.

¹ In view of the difficulty of restoring the first three lines it might be suggested that the letter had more than one column, and that *b* belonged to the preceding one; then ἀλέξαν[in l. 1 of *a* might (as Turner observes) be restored Ἀλέξαν[δρ(εῖς) or sim. Against such a suggestion I would say: (1) that the message as it stands seems almost complete; (2) that the left margin seems to have had a straight edge; (3) that this would make the document measure c. 36 × 14 cm., which seems disproportionately wide.

² Such a brief title would be consistent with what *H. A. Vit. Sev. Al.* 4, 1; 17, 4 says about his dislike of elaborate forms of address.



Recto



Verso

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3. Turner's suggestion τὴν προ[υπαρξασαν ὑμῖν πρὸς (or εἰς) εἰ]μέ suits both sense and space; alternatively τὴν προ[αίρεσιν might be restored at the beginning, but in this case I cannot complete the restoration. At the end, perhaps εὐνοια[ν ὡς (or ὅτι) ἐν].

4. (ἐν) / τοῖς στέρνο[ις εἴ]τι (or possibly ἀ]εὶ) φέροντ[ε]ς; cf. P. Oxy. 1664 (third century), 6 ff.: πᾶσα γὰρ ἡμῶν ἡ ἡλικία ἐν τοῖς στέρνοις σε περιφέρει, μεμνημένη τῆς ἀγαθῆς σου προαιρέσεως; the lacuna seems too short for πε]ριφέροντ[ε]ς here. For the expression ἐν στέρνοις ἔχειν, see *Wb.* s.v. στέρνον; adding P. Hermop. Rees 19, [3]; P. Lond. 1919 (Bell, *Jews and Christians*, 89), 17 f.

5. In view of the uncertainty of the restoration of the preceding lines it is not possible to decide between ἀφικνούμ[ενος] and ἀφικνούμ[αι].

6. τύχη ἀγαθῆ: with reference to an imperial accession (Trajan), cf. Dio Cass. 68, 3. 4. 7 f. ὑπὸ τῶν γενναιοτάτ[ων] / στρατιωτῶν: cf. (e. g.) Herodian v, 8, 10.

8 f. πα[: πα[ρ' ὑμᾶς? Turner would prefer this to πα[ρ' ὑμῖν on the ground that the latter would imply an inaugural ceremony at the place to which the letter was addressed. He observes, however, that one would expect κεχε[ι]ροτονη[μένος] / μὲν κτλ. in 6 f. to be balanced by a similar participle (chiastic) after ἐπὶ δὲ τὴν ἀρχήν. The composition here may of course be defective; but there is certainly no room for πα[ρ]εληλυθῶς (καί?) at the end of l. 8. Turner notes that it does not seem possible that τὴν ἀρχήν <τὴν> πα[ρ' ὑμῖν could be intended. With ἐπὶ δὲ τὴν ἀρχήν . . . πα[ρ]ελευσόμενο[ς], cf. P. Fay. 20, ii, 1 (copy of a letter of the same emperor and of the same year): ἐπ' ἐμοὶ παρελθόντι ἐπὶ τὴν ἀρχήν.

11 f. μη]/τρῶα? If so restored, this is perhaps only a more ceremonious equivalent of μητροπόλει—which might be thought too unimpressive a word, especially if the city in question is Alexandria. It seems just possible, however, that (again assuming the reference to be to Alexandria) we might restore instead πα]/τρῶα and understand an allusion to the obsessive emulation by Caracalla (whom Alexander regularly claimed as his father) of Alexander the Great; though to describe the latter's eponymous foundation as πατρῶα on such grounds would be very far-fetched. If so, perhaps π[α]ρέξω or sim. might be restored in l. 12. In any case a main verb of equivalent meaning must have been written in that line.

13. There is more than a normal line space after 12; ἔρρωσθε or the like probably followed below it, at the end.

14. This dates the letter a matter of weeks after Alexander's accession. It therefore seems natural to suppose that it was written from Italy. Note that this line is in the same hand as the foregoing text. Mr. T. C. Skeat notes that its dating in the Egyptian manner might be thought odd if, as we have suggested, it was not merely composed, but written by the emperor himself. I think that a letter of such a friendly nature would hardly have used a form of dating other than that familiar to its recipients; but that he should have dated it in his own hand, instead of leaving this to a scribe, is admittedly surprising.

Verso: βουλευτ(ῆ) πρε[σβυτάτ(ω)](?): a senator of Alexandria is so designated in P. Giss. 34, 2. 10 (third century).

The following translation, based in several places upon highly conjectural restorations, may now be offered with all reserve:

Alexander [Caesar Augustus to those (?)] in [authority, greeting (?):]

[Having learned from others of your (?)] goodwill [towards] me . . . [and how] you still (?) persevere in keeping in your breasts this good disposition, [I am (?)] coming to you, having with fortune's blessing been elected Emperor by the most noble soldiery, and being about to make my auspicious entry upon office [with a visit to you (?)], and having made a beginning with you especially of my opportunities for benefaction, as much as will be just to grant to the maternal (?) city [of the Alexandrians (?), I will bestow. Farewell(?).]

Year 1, Pharmouthi the [--].

Address: To Apolinarius, senior (?) senator.

POST-PHARAONIC NUBIA IN THE LIGHT OF ARCHAEOLOGY. III¹

By WILLIAM Y. ADAMS

Author's note: A paraphrase (in italics) of the history of the coming of Islam, as it is commonly understood, is first given. After a discussion of evidence now available, a revised historical summary (in italics) follows, incorporating those changes suggested by the record.

Epilogue: The Coming of Islam

The circumstances surrounding the conversion of Nubia to Islam are obscure. The process seems to have been an inevitable outcome of the century and a half of Egyptian invasion and intervention which began in A.D. 1171. By the end of the thirteenth century it would appear that large numbers of Nubians, including at least one of the local princes, had adopted the faith of the invaders. In the fourteenth century there were dynastic quarrels involving Christian and Moslem claimants, and the Mameluke sultans of Egypt intervened on several occasions on behalf of the Moslems. When the Nubian thrones passed finally into Moslem hands, the power of Nubian Christianity was destroyed at its head, and the conversion of the remainder of the population soon followed. By about A.D. 1400 there were no professing Christians left in Lower Nubia, although some Christian beliefs persisted.²

The current archaeological campaign in Nubia does not encompass the investigation of post-Christian sites, at least in the Sudan. In any case it is a moot question whether there are any such sites to be investigated. The archaeological surveys in the Wâdi Ḥalfa area have failed to reveal any remains attributable with certainty to the period between the disappearance of Christianity and the Egyptian conquest of 1821. This chapter of Nubian history, which ended less than 150 years ago, remains more obscure than any other period since the coming of the Tuthmosid armies 3,500 years before. For this reason we are obliged to close the systematic review of post-Pharaonic cultural history with the fall of the Christian kingdoms in the Middle Ages.

The coming of Islam is, of course, inextricably bound up with the disappearance of Christianity; it is generally assumed that there was a direct and immediate relationship between the two events. Islam is envisioned as a wave sweeping southward out of Egypt, destroying Christianity in its path.³ We know now, from the discovery of the scrolls at Qaṣr Ibrîm, that this was not literally true; there was still a bishop in Lower Nubia 50 years after the capital city of Dongola had officially adopted Islam.⁴ In fact,

¹ Part I of this study was published in Volume 50 of the *Journal*, pp. 102 ff.; Part II in Volume 51, pp. 160 ff.

² Sources: Arkell, *History of the Sudan*, 196-202; Crowfoot, *JEA* 13, 147-8; MacMichael, *History of the Arabs*, I, 155-87; Monneret de Villard, *Storia della Nubia cristiana*, 211-21; Shinnie, *Medieval Nubia*, 7.

³ Crowfoot, loc. cit.; MacMichael, loc. cit.; Shinnie, loc. cit.

⁴ Plumley, *Illustrated London News*, July 11, 1964, 51.

the whole question of the relationship between Christianity and Islam, and the circumstances leading to the replacement of the one by the other, appears to be much more complex than it has usually been made out. Active contact between the two faiths began less than a hundred years after the conversion of Nubia itself to Christianity, and continued for more than eight centuries before the triumph of Islam was complete. This long period of contact, as well as its final outcome, fall within the scope of our present review, and deserve a moment's consideration.¹

In the account of Ibn Selim we have historical evidence of Moslem Nubians, apparently recent converts, as far back as A.D. 985.² Their gradual spread up the Nile is marked by a series of dated Islamic tombstones: Tâfa in 832, Kalâbsha in 929, Qertassi in 933, and Derr in 1027.³ To these may now be added several Arabic tombstones from the Second Cataract region, dating between 913 and 1063.⁴ One tombstone from Meinarti is remarkable for its conjunction of a Coptic month with a Moslem year.⁵ It seems, therefore, that we must recognize the presence of a resident Moslem minority in Lower Nubia during the tenth and eleventh centuries.

As Christianity continued to flourish uninterrupted until three centuries later, we must assume that the Moslem communities existed on the sufferance of their neighbours, and generally lived at peace with them.⁶ The two groups must have been virtually identical in culture, for, aside from the tombstones, we find no remains specifically attributable to the Moslem settlers. The number of their known tombstones is very small, and it seems certain that Moslems never comprised a significant percentage of the resident Nubian population at any time during the Christian epoch. We definitely do not see evidence of a gradual increase in the number of Moslems which ends in their overthrowing the Christian kingdoms and establishing their own faith throughout Nubia.⁷ On the contrary, there are no Arabic tombstones at all after the eleventh century, which suggests that it may have been the Christians who turned persecutors.⁸ At all events it is clear that we cannot regard the disappearance of Christianity and the triumph of Islam as purely internal developments in Nubian history, and we are therefore compelled to look for external causes.

According to most scholars, it is to Egypt that we must look for the sources of Nubia's Islamization,⁹ as for most of the other religious impulses in Nubian history. We must therefore consider briefly the relations which prevailed between Christian Nubia and Moslem Egypt. The northern Nubian kingdoms were in direct and continuous contact with Egypt, and there was throughout almost the whole of the Christian period a lively and lucrative trade between the two regions.¹⁰ The only serious interruption seems to have

¹ It may be noted that Arkell (op. cit. 186-202) encompasses the entire 800-year period of Nubian Christianity within a chapter titled 'The Coming of Islam'. ² See Arkell, op. cit. 190.

³ Crawford, *Antiquity* 21, 13; Monneret de Villard, op. cit. 118-19.

⁴ *Kush* 12, 39, and 236; Adams, *Kush* 13, 172-3.

⁵ See Adams, *Kush* 12, 236, 249, and pl. liii.

⁶ Cf. Crawford, loc. cit.; Monneret de Villard, op. cit. 119-20.

⁷ Cf. Arkell, op. cit. 198; Crowfoot, *JEA* 13, 148.

⁸ A recently discovered inscription at Meinarti, in Arabic and Old Nubian, apparently commemorates the conversion of a Moslem to Christianity. See Adams, *Kush* 13, 172.

⁹ Crowfoot, loc. cit.; MacMichael, loc. cit.; Shinnie, op. cit. 7.

¹⁰ Cf. Arkell, op. cit. 188-91; Crawford, *Fung Kingdom*, 21-23; Shinnie, op. cit. 6-7.

occurred at the time of the Abbasid accession in A.D. 750,¹ but the trade was restored by treaty within a century,² and thereafter continued to flourish until the very end of the Christian period. It seems to have been little affected by the warfare of the twelfth and thirteenth centuries, and in the Late Christian houses we still find that a high percentage of the cooking pots were Egyptian-made, as they were centuries earlier.

We have, then, a constant economic interdependence between Egypt and Nubia which was largely independent of political developments. Turning to the latter field, we observe that peace generally prevailed during the Classic Christian period,³ but that there was renewed and finally almost constant warfare during the Late period.⁴ The nature of this warfare deserves close attention, for it bears comparatively little resemblance to earlier and later Egyptian invasions of Nubia. It takes the form of a long series of punitive expeditions and border raids, in which we seem to see a strong power succumbing to the temptation to intervene in the affairs of a weaker one. It is notable, however, that we do not see any systematic attempt at conquest, colonization, or the destruction of the native state or society. Each Egyptian invasion was a brief adventure, ending in a prompt retreat to Egypt as soon as affairs in Nubia had been settled to the satisfaction of the intruders.⁵

In the religious field, we cannot credit the Egyptians with a concerted attempt to destroy the Christian faith either in their own country or in Nubia. It is true that the expedition of Shams ed-Din in 1171 sacked the church at Qaṣr Ibrîm and slaughtered 700 pigs,⁶ but these acts were apparently incidental effects quite unconnected with the main purpose of the expedition. In general, the Egyptian invasions wrought comparatively little damage on the Christian monuments of Nubia, and it is impossible to regard them as motivated to any great extent by religious zeal. Their support of the Moslem claimants to the throne of Dongola⁷ was undoubtedly a matter of political expediency, as is shown by the fact that on one occasion the Mameluke influence was apparently exerted in support of a Christian king against a Moslem rival.⁸ There were persecutions of Egyptian Christians under the early Mamelukes, but no effort was made to suppress the faith, and Upper Egypt in particular remained heavily Coptic until modern times.⁹ In sum, although they undoubtedly played their part in the decline of Nubian Christianity, the Egyptian invaders of Medieval Nubia did not make a systematic attempt either to overthrow the existing faith or to propagate their own.

We must consider now the internal evidence pertaining to the disappearance of Nubian Christianity. From the middle of the twelfth century we find an increasing concentration of the Christian population in a few protected settlements, and the growth of defensive architecture.¹⁰ These circumstances accord well enough with the known record of Egyptian incursions in the Late Christian period, but both their character and their distribution have peculiar features which deserve closer scrutiny.

¹ See Adams, *Kush* 10, 280-1.

² Arkell, *op. cit.* 188-9; Shinnie, *op. cit.* 6.

³ Shinnie, *loc. cit.*

⁴ *Ibid.* 7.

⁵ See esp. Arkell, *op. cit.* 192-8; Monneret de Villard, *op. cit.* 211-21.

⁶ Arkell, *op. cit.* 192.

⁷ See esp. Arkell, *op. cit.* 195-6; Crowfoot, *op. cit.* 148.

⁸ Arkell, *op. cit.* 198.

⁹ Lane-Poole, *History of Egypt*, vi, 310-12.

¹⁰ The discussion which follows is based chiefly on the author's excavations. For preliminary reports see *Kush* 12, 216-48, and *Kush* 13, 148-76.

The strengthening of ordinary houses was general throughout Nubia, and took the form of thickening the walls, replacing flat roofs with brick vaults, and reducing as much as possible the number and size of external and internal doorways. Rooms were built which had no lateral entrances, and could only be entered through small hatchways in the roof. Where necessary, older doors were blocked up, and new passages made through the roof.

Such modifications have nothing to recommend them from the standpoint of armed defence; they would merely serve to trap the inhabitants in the event of a siege. They suggest much more an attempt to protect property and food stores from marauders. In particular, the massive communal buildings found at Meinarti and elsewhere,¹ with their complex of rooms entered only from above, are suggestive of fortified granaries. Probably the Nubians depended on mobility or outright flight for their own protection, and this may explain their failure to revive the use of girdle walls, at least in Lower Nubia.

The same cannot be said of the regions further south. Girdle walls are found in some of the very late Christian sites in the *Batn el-hajar*, and they are a regular feature of village sites in Upper Nubia. In general, the further south one goes, the more fortification is encountered, and the earlier in the Christian period it makes its appearance.² Among the recurring defensive features are look-out towers, which are also found at various points through the *Batn el-hajar*, as far north as Meinarti.³ A characteristic common to all of the towers of the cataract region is their situation so as to command a view southward.

Another point worthy of consideration is the location of the Late Christian communities. They show a strong preference for island situations; even when situated on the mainland, as at Serra East,⁴ their defensive orientations are toward the landward rather than the riverward side. Such precautions would have a limited effect against the Egyptians, who habitually travelled with boats, and would be certain to make use of them in an attack on the Christian settlements. On the other hand a water barrier might well have discouraged enemies to whom it was not a familiar element.

The cumulative effect of the archaeological evidence is to suggest that the enemies whom the Christian Nubians principally feared were not the Egyptians, but marauding nomads from the deserts, particularly in the south. Such peoples had been disturbing the peace in Nubia periodically for centuries, but until the Middle Ages their numbers were probably small and their capacity for mischief limited. Concurrent with Ayyubite accession, however, there was an enormous inpouring of Arab tribes into both Egypt and the Sudan, which continued through the thirteenth and fourteenth centuries.⁵ For the first time nomads formed not only a constant but possibly a numerically superior

¹ Adams, *Kush* 12, 222 and 232-3.

² This assertion is based on my admittedly unsystematic observation of Christian sites in the *Batn el-hajar* and Upper Nubia.

³ See *Kush* 12, 229-30. Other watchtowers have been identified at Abu Sir (*Kush* 11, 38-39), Abkanarti, Kasanarti (*Kush* 12, 222), Diffinarti, and Kulubnarti. As the names suggest, all of these are island settlements.

⁴ See Hughes, *Kush* 11, 124-6 and Plan II.

⁵ Arkell, *op. cit.* 200; Abdin, *Sudan Notes and Records* 40, 59-61; Musad, *Sudan Notes and Records* 40, 125.

element in the population. Through contact and intermarriage with the newcomers the older nomadic tribes of the Sudan were largely converted to Islam,¹ and it is possible that some of the Nubians themselves adopted a nomadic or semi-nomadic life at this time.² Over the next several centuries there seems to have been a definitive trend away from settled farming and toward pastoral migration—not for the first time in Nubian history.

Christian Nubia was, then, under simultaneous pressure from the north and from the south. The two processes were not unconnected: in the historical record there is a good deal to suggest that the Egyptian invasions of the thirteenth and fourteenth centuries were brought on less by the provocation of the Nubians themselves than by the inability of the Nubians to contain their nomadic neighbours to the south.³ As for the latter, the object of their depredations seems to have been as much Egypt as Nubia.⁴ Nubia was historically the buffer zone between Egypt and the predatory tribes of the desert; now, as a result of declining population and political weakness, she was unable to fulfill her role. Nomads marched across her territory at will, and Egyptian retaliatory expeditions followed. A good many Nubians may have been active among the nomads, but the settled population, at least, was more in the position of an innocent bystander caught between hostile forces.

If we return now to the specific question of the disappearance of Nubian Christianity, it is clear that we are not dealing simply with a linear advance of Islam from north to south, as has earlier been suggested.⁵ Christianity *per se* was seldom under attack. Instead, the Christian population of Nubia was caught between Moslem forces, Egyptian and nomadic, whose hostility was directed as much at each other as it was at the Nubians. If Nubian Christianity was nevertheless destroyed in the process, it was more by accident than by design. In fact, the chain of historical cause and effect took quite different forms in different areas.

The northernmost Nubians, on the frontiers of Egypt, were certainly the first to be converted to Islam, and to this extent the accepted version of the Islamization of Nubia is undoubtedly correct. These people, the present-day speakers of the Kenzi dialect, were in direct contact with Moslem Egypt for 500 years; moreover, in the Ayyubite period they were heavily infiltrated by Moslem Arabs.⁶ Possibly their conversion was complete by the thirteenth century, for we find very few churches of the Late type north of Maḥarraqa.⁷ Once converted, they participated along with the Arabs in the raids upon Dongola which led ultimately to the conversion of that region as well.⁸

¹ Arkell, *op. cit.* 189; Crowfoot, *JEA* 13, 148.

² Arkell, *op. cit.* 200; Crowfoot, *loc. cit.*; Musad, *loc. cit.*

³ Arkell, *op. cit.* 195–200; Clarke, *Christian Antiquities*, 11; Musad, *loc. cit.*

⁴ Arkell, *loc. cit.*; Musad, *loc. cit.*

⁵ Crowfoot, *loc. cit.*; MacMichael, *History of the Arabs*, 1, 155–87; Shinnie, *Medieval Nubia*, 7.

⁶ Arkell, *op. cit.* 200; Musad, *loc. cit.*

⁷ Churches north of Maḥarraqa which definitely show Late characteristics are those at Meḏīq, El Oqba, and Sheima Amalika (Monneret de Villard, *Nubia medioevale*, 1, 56–57 and 78–82). However, all of these buildings appear to be transitional between the Classic and the Late styles, and on typological grounds can be tentatively dated to the end of the twelfth century. For further discussion see Adams, *JARCE* 4, 87 ff.

⁸ Arkell, *op. cit.*; Millet, *Some Notes on the Linguistic Background of Modern Nubia* (MS.), 1; Musad, *loc. cit.*

The next part of Nubia to turn to Islam was not the middle region, immediately south of the Kenzi, but the Dongola Reach, between the Third and Fourth Cataracts. This relatively productive region had always been the 'heartland' of the Kingdom of Makuria (as it was earlier of the Napatan kingdom), and was probably the only area which remained under the effective control of Dongola in the fourteenth century. The process of conversion here was complex. There was already a substantial Moslem nomadic population in the region, as a result of Arab migrations and intermarriage.¹ Many of the Nubian farmers had perhaps also turned nomadic or semi-nomadic, although whether they had automatically become Moslems in the process is uncertain. The bulk of the settled population probably remained Christian,² and for a time they seem to have vied with the Moslems for control of the throne at Dongola.³ In the end, however, direct intervention by Egyptians and especially by Kenzi Nubians⁴ was sufficient to establish a Moslem dynasty, and the conversion of the Kingdom of Makuria was official. It is commemorated by a tablet recording the conversion of the church of Old Dongola—in effect, the royal cathedral—into a mosque in A.D. 1317.⁵

Middle Nubia, from Maḥarraqa to the Third Cataract, was little affected by these events. In the total deserts flanking the Nile on either side there was no possibility of nomadic subsistence, and the poverty of the river valley itself, bereft of its once lucrative trade, offered little inducement to plunder. Isolated in their island retreats, the remaining Christian farmers in this area were apparently left largely in peace as they watched the march and countermarch of armies on their way to Dongola or to Aswân. From the recent discoveries at Qaṣr Ibrîm we have learned that a bishop was consecrated there more than 50 years after Dongola became officially Moslem.⁶ As the region around Qaṣr Ibrîm had nominally been subject to Dongola, it is clear that the conversion of the royal family was not alone enough to ensure the Islamization of the whole kingdom, as suggested by earlier writers.⁷ On the contrary, the latest discoveries at Qaṣr Ibrîm suggest the survival of a Christian community here until late in the sixteenth century. Moreover, in the archaeological record of Middle Nubia we find the final manifestations of the Christian traditions in pottery and church architecture; manifestations which are not found either north of Maḥarraqa or south of Kerma.⁸

In fact, throughout Middle Nubia there is no evidence of direct conversion at all. From the archaeological record it appears that the Christian settlements were abandoned by the end of the fifteenth century, but there is no sign of a post-Christian population until centuries later.⁹ Of the 75 known churches between Maḥarraqa

¹ Arkell, *op. cit.* 189; Crowfoot, *op. cit.* 148.

² As evidenced by the absence of churches converted to mosques.

³ Arkell, *op. cit.* 199–200; Musad, *op. cit.*, Shinnie, *loc. cit.*

⁴ Cf. Millet, *loc. cit.*; Musad, *loc. cit.*

⁵ Crawford, *Fung Kingdom*, 35.

⁶ Plumley, *Illustrated London News*, July 11, 1964, 51.

⁷ Crowfoot, *loc. cit.* 147–8; Musad, *loc. cit.*; MacMichael, *op. cit.* I, 138–9.

⁸ This observation is based partly on published literature, partly on my reconnaissance of sites in Upper Nubia, and partly on the examination of sherd collections in the Sudan Museum, Khartûm.

⁹ Excepting the Bosnian garrison at Qaṣr Ibrîm; see below.

and the Third Cataract,¹ only the Basilica at Qaşr Ibrîm shows any traces of conversion to a mosque, and the transformation here was carried out by the Bosnian garrison in the sixteenth century, after the building had been abandoned as a church.²

In sum, archaeological evidence indicates that Middle Nubia was abandoned, not converted. Caught between hostile forces, and deprived of their once profitable commercial revenues, the last Christian inhabitants followed the trail of their C-Group predecessors 25 centuries before. Once again, we can hardly doubt that the direction of their emigration was to the south. The practice of nomadism had probably spread northward among the Nubians until it reached the last 'holdouts' in Middle Nubia. For them, however, there was no chance of livelihood in the neighbouring deserts, and they were therefore obliged to migrate southward into the rainfall belt. Thus, in the words of Ibn Khaldûn,³ 'Not a trace of kingly authority remained in the country, and the people are now become Bedu, following the rains about as they do in Arabia.'

In the far south of Nubia, whither the Middle Nubians probably retreated, there must have been a long period of religious anarchy, as in the X-Group period. The southern kingdom of Alwa is supposed to have been overthrown by the Fung in 1504,⁴ and its capital city of Sôba lay in ruins in 1523.⁵ However, Álvarez⁶ found some elements of Christianity persisting at this time and the practice of Islam was by no means general. Nevertheless, its final triumph was assured from the day when the Nubians forsook their homes and sedentary life, for the rigid and entrenched hierarchy of the Oriental Church could never be reconciled with a migratory life. Hence the nomads of the world have always found Islam better adapted to their cultural needs.

Chittick,⁷ following the *Fung Chronicle*,⁸ has recently put forward the suggestion that the Christians of Nubia made a last stand against their Moslem oppressors in the vicinity of the Sixth Cataract, in the course of which they were all slaughtered. The evidence is not very convincing, however, and it seems more probable that Nubian Christianity ended 'not with a bang but with a whimper'. Of the Middle Nubians we can only say that they were Christians when they left their homes in the fifteenth century, and Moslems when they returned in the eighteenth century.

A word must be said about the modern Nubian dialects, to which allusion has been made previously. The speech differences among the present population are believed to have originated not much more than 1,000 years ago,⁹ which means in all probability that they resulted partly from the varying experiences of different Nubian groups during the Christian and post-Christian periods. Thus linguistics may offer us an additional source of light upon the recent history of Nubia.

Among the Nile Nubians¹⁰ there are three main speech divisions, which will here be

¹ Based on detailed examination of the sites or their published plans. Cf. Adams, *JARCE* 4, 87 ff.

² Cf. Plumley, loc. cit.

³ Quoted in Crowfoot, op. cit. 148.

⁴ Musad, op. cit. 126.

⁵ Shinnie, op. cit. 8.

⁶ *Narrative* (tr. Stanley), 65.

⁷ *Kush* 11, 264-72.

⁸ MacMichael, op. cit. II, 359.

⁹ Trigger, *Languages of the Northern Sudan* (unpublished paper read at a Symposium on Contemporary Nubia, Aswân, 1964), 5.

¹⁰ No account is here taken of the Nubian dialects of Kordofan and Darfur. For general discussions of Nubian linguistic relationships see Greenberg, *Int. Jour. Amer. Ling.* 29, 85; Herzog, *Die Nubier*, 22-34; Hillelson, *Sudan Notes and Records*, 13, 137-48; Kirwan, *Sudan Notes and Records*, 20, 47-62; Trigger, loc. cit.

designated as Kenzi, Maḥasi, and Dongolawi.¹ The Kenzi are situated in the extreme north, between Maḥarraqa and Aswân. Alone among modern Nubians, they preserve in their domestic architecture many of the traditions of Christian Nubia, including mud brick construction, the barrel vault, and many decorative elements.² This, together with documentary evidence,³ suggests that the Kenzi have remained continually in the same habitat since Christian times.

South of the Kenzi, extending from Maḥarraqa to the Third Cataract, are speakers of the Maḥasi (or Maḥas-Fadidja) dialect. It is this dialect which appears to be most directly descended from Old Nubian.⁴ The Maḥasi speakers, as we noted earlier, seem to have occupied their present habitat until the fifteenth century, then migrated southward and did not return until the eighteenth century.⁵ South of the Third Cataract are speakers of the Dongolawi dialect, which is much more closely related to Kenzi than to the neighbouring Maḥasi. Linguistic scholars, in fact, normally speak of only two basic Nile Nubian dialects: Maḥas-Fadidja in Middle Nubia, and Kenzi-Dongolawi (or Dongola-Kenuz) to the north and south of it.⁶ As the archaeology of Upper Nubia has not been systematically investigated, we have no light from that source on the earlier history of the Dongolawi.

In Classic Christian times Kenzi and Maḥasi were subject to the same ecclesiastical and political authority, and there is no indication of any kind of cultural or tribal frontier at Maḥarraqa. Under the circumstances it is difficult to see why or how a dialect boundary should have grown up at this point before A.D. 1150.⁷ On the other hand, the 'Arabization' of the Kenzi,⁸ the possibly simultaneous migration of a wholly Arab, non-Nubian tribe into the region immediately south of them, along the Wâdi el-'Arab, and finally the outright emigration of the Maḥasi opened up first a cultural and then a physical gap between the two groups which might have been sufficient to produce the dialect difference of today.

Up to this point the historical reconstruction presents no difficulties. It is the Dongolawi, however, who do not conveniently fit the picture. The relationship of their speech to that of the Kenzi suggests a separation of less than 500 years. Since the Kenzi

¹ The proper names of the groups are respectively Kenzi, Maḥasi, and Dongolawi in the singular, and Kenuz, Maḥas, and Danagla in the plural. A fourth group, the Fadidja, are sometimes differentiated from the Maḥasi, although the dialect difference is slight. There is no consistent practice in regard to the use of singular or plural as a collective or attributive; throughout the present discussion I have used the singular for the sake of consistency.

² Modern Maḥasi and Dongolawi houses are basically African in type, with *jalus* walls and flat roofs.

³ See esp. Arkell, *op. cit.* 199–200.

⁴ Griffith, *Nubian Texts*, 68.

⁵ The date of their return is a pure guess. There are no archaeological remains and no reliable local traditions going back before the middle of the last century. However, the first European travellers at the beginning of the nineteenth century found a thinly scattered resident population in Middle Nubia (cf. Burckhardt, *Travels in Nubia*; Cailliaud, *Voyage à Méroé*; Linant de Bellefonds, *Journal*; etc.), and I have therefore assumed that the return was accomplished some time during the previous century.

⁶ Cf. Griffith, *loc. cit.*; Greenberg, *loc. cit.*; Trigger, *loc. cit.*

⁷ Nevertheless, glottochronology suggests that the divergence of Maḥas-Fadidja from Kenzi-Dongolawi began about A.D. 860, ± 200 years (Trigger, *op. cit.* 5). Moreover, Griffith (*op. cit.* 5–6) has noted that there are no known Old Nubian texts from the present Kenzi-speaking area. The possibility of a dialect difference going back to Classic Christian times, or even earlier (cf. Millet, *loc. cit.*), cannot be entirely ruled out.

⁸ Arkell, *op. cit.* 196, 200.

appear to have remained in their present whereabouts since much earlier times, we can only suppose that the Dongolawi split off from them and migrated southward in the fairly recent past—and probably during the absence of the Maḥasi.

In the light of all available evidence, the most likely explanation for the development of the modern Nubian dialects appears to be the following:

1. In Classic Christian times, Old Nubian was spoken throughout the Kingdom of Makuria, from the First to the Fourth Cataract.

2. In Late Christian times, the northernmost Nubians were heavily infiltrated by immigrant Arabs, and were converted to Islam. The resulting social separation from the Christian Nubians initiated a process of linguistic divergence between the two groups.

3. During the fifteenth century a large part of the remaining Christian population of Makuria turned nomad, leaving Middle Nubia and probably also considerable portions of Upper Nubia depopulated. Their separation from the Kenzi, who remained settled in Lower Nubia, accentuated the dialect divergence between the two.

4. Concurrently with, or subsequent to, the withdrawal of the Maḥasi from Middle Nubia, groups of Kenzi began filtering southward into the Dongola Kingdom, taking up the lands abandoned by their former neighbours. Their interference in the royal affairs of Dongola during the fourteenth and fifteenth centuries is well attested historically. Probably the immigrants passed over the inhospitable *Batn el-hajar* and established themselves in the more fertile regions above the Third Cataract. It may have been their interference which resulted in the accession of a Moslem king at Dongola; at all events, their immigration was undoubtedly encouraged by the fact that Islam was now the established religion of the kingdom.

5. As more and more Maḥasi moved out and more and more Kenzi moved in, the latter in time became the dominant element in the settled population, and their speech supplanted the older dialect. The Kenzi had in effect 'taken over' the remnants of the Dongola kingdom.

6. The Maḥasi speakers resumed a sedentary life in the eighteenth century, possibly because they were driven northward by the Fung. However, they found their old lands in Upper Nubia pre-empted by the immigrant Kenzi, while the Ababda Arabs had taken possession of the area in the north at the mouth of the Wâdi el-'Arab. Their re-colonization was therefore confined to the empty space in Middle Nubia, between Maḥarraqa and Kerma, which had been passed over by Kenzi and Arabs alike. The return of the Maḥasi severed whatever link remained between the northern Kenzi and the offshoot at Dongola, resulting eventually in the slight divergence of the Kenzi and Dongolawi dialects.

The above reconstruction is largely speculative, and indicates the need for more systematic investigation of the Nubian dialects. From the standpoint of solid documentary and archaeological evidence there is still little that we can say about the post-Christian period, and it remains one of the most obscure chapters in Nubia's 5,000-year history. To sum up the story of the coming of Islam, as it now appears:

The disappearance of Christianity and its replacement by Islam were not part of a continuous and connected process throughout Nubia. Islamic pressure was brought to bear on

the Christian population simultaneously from the north and from the south, with differing results in different areas.

The northernmost Nubians were undoubtedly converted to Islam as a direct result of Egyptian proximity and their infiltration by Arab immigrants. Elsewhere, however, the effect of Egyptian interference, combined with repeated nomadic deprivations from the south, was to drive the Christian population out of Middle Nubia. More than any concerted hostility, it was the abandonment of the sedentary life and the loss of all contact with other Christian communities which destroyed the underlying social and political fabric of Nubian Christianity, and made certain its ultimate disappearance.

For most of the populations in the south, including the newly arrived migrants from Middle Nubia, the transition to Islam was probably gradual. During the centuries of semi-nomadism many Christian beliefs persisted for a time. However, the Church as a viable social force was extinct, and in addition the Nubians were now surrounded by Moslem neighbours on all sides. Thus the ultimate triumph of Islam was assured from the time when they quitted their earlier homes. It was apparently complete when they finally returned to them in the eighteenth century.

Postscript: Philology, Archaeology, and History in Nubia

Having devoted a good deal of space to the discoveries and implications of Nubian archaeology, I would like to turn attention briefly to the study itself; i.e. as a field technique and a scientific discipline. I must once again claim an outsider's privilege of commenting upon the field from the point of view of one trained in a wholly different scholarly tradition.¹

To begin with, it seems that in Nubia archaeology has suffered at the hands of written history. Any kind of documentary evidence, no matter how fragmentary, ambiguous, or unreliable, has generally been accorded a higher place than archaeological evidence—a priority which it certainly does not invariably deserve. A conspicuous example may be cited in the case of the X-Group and Early Christian periods. Without disparaging the legitimate value of their work, we may nevertheless observe that Procopius, Olympiodorus, and Strabo have unwittingly dragged a great many red herrings across the trail of Nubian history. In the zeal and confusion of pursuit, the X-Group fox has all but got away.

In history there is no substitute for sound and unbiased written records. Unfortunately, the majority of our documentary evidence pertaining to Nubian history does not fit that category. The Egyptian and Nubian monarchs, and their viceroys, generals, and press-agents, stand convicted out of their own mouths as some of history's greatest braggarts and liars. The chroniclers of Classical and Medieval times had not quite the same motivation, but it is clear that they often felt compelled to exhibit a degree of erudition and encyclopaedic knowledge which they did not necessarily possess. Like many of their compeers in more recent times, they would not say 'I don't know', if they could avoid it. Writing about events and people at a distance of thousands of miles in space and sometimes hundreds of years in time, they were free to let their

¹ Cf. Adams, *JEA* 50, 102, n. 1.

imaginings fill in the lacunae in their data, with posterity, so they hoped, none the wiser.

It must be admitted that few scholars trust implicitly the written word, and students of Nubian history have long since spotted the more obvious exaggerations and distortions in the record. On the other hand, the significance of the written word, whether true or false, seems to be taken for granted. The suggestion that a documentary account, even if true, might be meaningless is perhaps heretical from the standpoint of philology, but it has to be considered in the case of history, and especially cultural history.

In Nubia, it seems to be proper names which hold an unaccountable fascination for the historian. A few score names, of persons, places, and tribes, lie embedded like so many fossils amid the debris of the centuries. Like natural fossils, their original context has long since been dissolved and transformed, and their own preservation owes more to the caprices of time than to any inherent significance. Within the contexts in which we now encounter them, names like Talakhamani, Blemmyes, and Alwa frequently tell us nothing at all. Yet an enormous effort of scholarship has gone into the attempt to invest them with tangible and meaningful attributes, after a lapse of thousands of years—something which their original chroniclers were sometimes unable to do at the time when they recorded them for posterity.

In the long run the attempt to retrace the course of evolution primarily from fossils is no more rewarding in the study of cultural history than it is in the study of natural history. Its inevitable result is to divert attention from the archaeological and historical matrix in which the fossils are preserved; a record which is often more informative than the fossils themselves. When this sort of preference is carried to its logical conclusion, the Egyptianization of Nubia in the New Kingdom is represented as a string of personally directed military adventures, the history of the Napatan and Meroitic periods reduced to a strictly hypothetical succession of pyramids, the X-Group upheavals identified with a couple of minor tribes, and the polity of Christian Nubia described in terms more suitable to nineteenth-century Europe. So much for proper names!

If the importance of documentary evidence in Nubia has sometimes been overrated, it is certain that the use of archaeological evidence in the reconstruction of history has fallen far below its potential. Undoubtedly, many of the field investigators of the past were not particularly interested in studying the humbler aspects of cultural history; their excavations were carried on for the recovery of inscriptions and *objets d'art* and for the unravelling of problems of dynastic succession. To this extent they cannot be unduly blamed for the meagre results which have sometimes emerged from their work.

A difficulty which has plagued scientific archaeology in all parts of the world—Nubia included—involves the criteria by which sites are selected for excavation. Left to their own devices, archaeologists show an understandable tendency to be content with the plums and leave the rest of the cake. They choose sites which offer the richest rewards, the simplest occupation history, and the fewest excavation problems. This means in the long run that they choose sites about which they already know the most, instead of the least. Their work soon begins to repeat itself; each new excavation

reaffirms the known without significantly diminishing the unknown. In short, there is a tendency to avoid 'problems', either cultural, methodological, or logistic.

The arbitrary and subjective choice of sites for excavation involves a double danger for the reconstruction of history. First, it produces an unbalanced picture in which a few of the highest, or most durable, expressions of a culture—temples, royal monuments, etc.—are made to seem representative of the culture as a whole. Second, it leads inevitably to a static view of ancient cultures. This results in part from a preference for 'pure' sites over those which are stratified or otherwise complex, and from deliberate specialization on the remains of specific periods, with a consequent disregard for other periods. In Egypt and Nubia, it has been greatly reinforced by a disproportionate concentration on graves and monumental remains, neither of which are sensitive indicators of cultural evolution. A grave is necessarily a static expression of culture, open one day and closed forever the next; a royal or religious monument is certain to be encumbered with formal tradition which is the enemy of change. It is chiefly in the humbler domestic sites that we are likely to find the combination of long occupation and continuous change which alone affords a dynamic view of the cultures of the past. By their avoidance of domestic sites and stratified sites, archaeologists cut themselves off from such a view.

In the history of archaeological endeavour, however, Nubia has been more favoured than most parts of the world as a result of the succession of dam projects at Aswân. The ensuing archaeological salvage programmes have gone a long way in correcting the distorted view of history which might have resulted from the usual practice of site selection—a practice which still obtains in Upper Nubia. Archaeologists all over the world owe an incalculable debt to the reclamation engineers, highway planners, and building developers who, by threatening the indiscriminate destruction of all archaeological remains, have forced them to investigate the nine-tenths of sites which would otherwise have remained forever untouched. The scientific benefit which has accrued from these programmes has more than compensated for the loss of the sites themselves. It is through them that we have been obliged to turn our attention to the inconvenient 'mixed-up' sites which do not accord with our preconceived ideas, and to the humble dwelling sites in which we can study culture not as a disconnected succession of static, 'classic' patterns but as the dynamic, ever-changing phenomenon which it is. Most of the important archaeological advances of the post-war period have come about as a result of salvage archaeology, and we are entitled to expect new ones from the current programme in Nubia. We might even go so far as to hope that some day a Third Cataract Dam Project will do the same for Upper Nubia.

In its preoccupation with cultural statics instead of dynamics, and its tendency to examine cultures as if isolated under glass, without reference to parallel and related external developments, the study of Nubian history clearly betrays its origins in Classicism. The very term 'classic' implies a static ideal, from which any deviation is regarded with a certain subjective disfavour. Thus, to one whose intellectual roots are embedded in Darwinian theory, the study seems to suffer from a want of evolutionary perspective and from a complete absence of the comparative method. We seem to

see cultural change represented as a succession of instantaneous leaps from one stable and integrated pattern to the next, and we have no consciousness of an evolutionary process at work. Since it is self-evident that culture by itself does not develop in this way, the archaeologists have had to postulate a major ethnic migration to account for each new level of Nubian cultural development.

In the long run, the reduction of cultural history to a series of static cross-sections simply divorces culture from any intelligible sociological context. The association and relationship between a specific culture and specific society often cannot be inferred at any single instant of time; both must be observed over a long period. Hence the importance of humble domestic remains whose patterns of growth and decay reflect no overall preconceived ideal, but simply shifting fortune and fashions. In a stratified house-site, no single level is important for itself; it is the changes from level to level, the destruction and modification and rebuilding, the processes of integration and disintegration, and the events that caused them, which spell out the story of Nubian history. It is here, if at all, that we should be able to follow the transitions from one 'classic' cultural level to the next.

We should not, however, make the mistake of presupposing any fixed relationship between any one society and any one culture. This assumption, or at least the failure to differentiate clearly between racial, linguistic, and cultural evidence, appears as another instance of lack of anthropological perspective in the study of Nubian cultural history. We have a number of historical reconstructions which attempt either to ascribe race, language, and culture to a common source, or to account for the distribution of one in terms of another, despite the fact that they are demonstrably independent variables in human history.

A final example of the unsophisticated handling of data may be seen in the unconscious ethnocentrism which has led both chroniclers and students of Nubian history to interpret social and political institutions by analogy with European models, instead of as being essentially tribal in character.

If we turn, finally, to the practice of archaeology in the field, it appears that the standards maintained in Nubia have generally been on a par with those in other parts of the world at the same time. Advances in procedure and technique have everywhere come about gradually, with the steady accumulation of results and experience, and an increasing awareness of specific problems. In Nubia, where working conditions are necessarily primitive, excavation controls have usually been adequate for the problems which were recognized at the time. Field documentation has been conscientious and detailed, although the prevailing form leaves something to be desired. The use of a diary in place of on-the-spot, categorical, field notes as the basic excavation record is an anachronism more appropriate to the day when archaeology was the diversion of gentleman travellers. It places an unnecessary burden on the memory, and virtually assures that no one but the excavator will be able to make much sense out of the record at a later date.

In some respects, however, Nubia has been ahead of the rest of the world in the development of field procedures. For this a good deal of the credit certainly belongs

to Reisner. The original Archaeological Survey of Nubia posed a challenge which at the time was entirely without precedent, and the methods and controls which he adopted to deal with it¹ have since become general practice in Nubia, and have influenced the strategy of salvage archaeology in many other parts of the world.²

Nevertheless, from the standpoint of science and history no excavation is better than its final published reports, and here the record of Nubian archaeology could hardly be worse. In the Sudan, only two important excavations in the twentieth century have been carried through to completion and fully published by the original investigators.³ Garstang, Griffith, Reisner, and Wellcome all died leaving major portions of their work unpublished; were it not for the dedication of men such as Addison,⁴ Dunham,⁵ and Macadam,⁶ we would have little to show for their half-century of activity in the field. As it is, we are still awaiting the full publication of such important digs as the town of Meroë, the temple of 'Amâra, and some of the Second Cataract forts.

In the field of publication, the paucity of preliminary reports and the tendency to disparage them are regrettable. Progress reports have an important place in all scientific research. Even if they are never read, they oblige the investigator himself to take stock of his work from time to time. There is nothing like the preparation of a formal statement, for the critical eyes of colleagues, to bring to light gaps in the data and fallacies in their interpretation—deficiencies which can otherwise go undiscovered for years. With the regular submission of preliminary reports, these shortcomings may often be recognized in time to make them good during the following season's field work. At the same time the systematic marshalling of facts and ideas will usually, in the process, suggest new hypotheses and avenues of investigation—again, hopefully, in time for probing in a later season.

More overtly, preliminary reports serve to keep colleagues *au courant* with the accumulation of new data and the evolution of new theories which may be important in their own work. Without them, it is usually necessary to wait a generation or more to achieve any synthesis of ideas arising from the related work of several scholars—which is undoubtedly why so few syntheses are achieved in the field of archaeology. In the interval between research and publication whatever intellectual enthusiasm was engendered by the work itself has vanished with the passage of time and the drudgery of preparing plates, tables, indexes, and the like. All this is avoided by the preparation of preliminary reports, which can and should be written before the initial glow of enthusiasm cools. Finally, preliminary reports must be accepted as a kind of insurance in case no final report ever reaches print—an eventuality which, on the record, can not be disregarded.

The definitive reports on the archaeology of Nubia, when they have appeared at all, often leave a good deal to be desired. Much of this is unavoidable in second-hand publication of notes and material which have been accumulating dust for twenty or thirty years. It has been amply demonstrated that a really satisfactory published

¹ See Reisner, *Arch. Surv. Nubia 1907-1908*, 7-13. ² Cf. Rowe, *American Anthropologist* 63, 1380.

³ Reisner's at Kerma and Woolley's and MacIver's at Buhen.

⁴ *Jebel Moya and Abu Geili*.

⁵ *Royal Cemeteries of Kush*, 1-v, and *Second Cataract Forts*, 1.

⁶ *Temples of Kawa*, 1-11.

report can only be produced by the original investigator, and then only within a reasonable period of time after the conclusion of field operations. Field notes, no matter how conscientiously written, always turn out to be addressed to the writer himself, and they invariably contain abbreviations, logical short-cuts, and *a priori* assumptions which are unintelligible to anyone else. There are, moreover, thousands of nuances of association and implication, of half-formed hypotheses and even flashes of intuition which never find their way into the documentary record, yet which are indispensable to the interpretation of archaeological evidence. They can never be transferred to the mind of another scholar, and they are not likely to remain fixed indefinitely in the mind of the original excavator.

It must be added that the form of publication appears intrinsically unsatisfactory in many cases. A lot of the so-called definitive reports in Nubian archaeology are little more than illustrated catalogues—masses of raw data presented without any attempt at summary, synthesis, or interpretation. It is this writer's conviction that the archaeologist who publishes his final report in such form leaves his job half finished, and his obligation to science half fulfilled. If no one else is ever as fully competent to publish archaeological data as is the original excavator, it is doubly certain that no one else is as fully competent to interpret them. Moreover, it is predictable that archaeologists who cannot find the time to organize and to analyse their own data, or even in many cases to publish them, are not likely to devote much time to one another's data. In short, the archaeologist who does not analyse and draw the relevant conclusions from his own investigations is very probably consigning them to perpetual limbo. Some of the secrets of Nubian cultural history are probably just about as effectively buried in the published reports, for example of the Archaeological Survey of Nubia, as in the unpublished reports of the Meroë Expedition.

We may look with confidence to the current Nubian archaeological campaign to make good some of the failures of the past and to throw much additional light into the still obscure corners of Nubian history. What is particularly encouraging about the present situation is that we have for the first time, among the various expeditions active in the field, a fair balance among philologists and field-archaeologists, prehistorians, and cultural anthropologists. It would be pleasant to report that a major interdisciplinary cross-fertilization was resulting from this conjunction of specialists. Unfortunately no such process is visible. The practitioners of the different disciplines are, as usual, going their own appointed ways, but they are at least cautiously observing each other, and a *modus vivendi* has been established in which the legitimate sphere of interest of each is recognized. The result is bound to be beneficial to the study of Nubian cultural history.

What is most notably absent from the current Nubian campaign is an overall strategy—a clear recognition of what we can and should hope to obtain from Nubia in the time that remains to us. It would have been a great service to science and history if the various advisory committees convened by Unesco had defined the objectives and priorities in the field of archaeology as clearly as they did in the field of conservation. As it is, we seem to have almost as many views of the proper approach to the problem as we have had expeditions in the field.

For those who are engaged in archaeological survey, or in a widespread and unspecialized excavation programme, the major strategic consideration is that of sampling. It is one of the most universal and least understood problems in archaeology. Granting that we can never find all of the archaeological sites, and cannot excavate more than a fraction of what we find, on what basis do we select sites for excavation? How shall we decide where to start and, once started, where to stop? How much is an adequate sample of a site, or a group of sites, or an area? Above all, at what point may we assume that what we have done is representative of what we have not done? These are questions of strategy which affect the organization of work and the disposition of forces from day to day, and sometimes almost from hour to hour, in the course of archaeological survey. The answers to them must depend on the excavator's own view of his problems and objectives.

In the surveys and excavations of the Sudan Antiquities Service, we have conceived as our primary objective the study and reconstruction of Nubian cultural history, rather than material conservation *per se*.¹ While recognizing an obligation to investigate all antiquities encountered, at least to a certain point of definition, our strategy has been to concentrate on those sites which give the most promise of filling in the gaps in the existing picture of history, and particularly on the types of sites which have received the least attention from others. Hence our emphasis on the post-Pharaonic periods, and our choice of habitation sites, whenever possible, in preference to cemeteries. The results have not always been enlightening, and we have probably made at least as many mistakes as our predecessors, but we have come up with a few new ideas, which I have tried to set forth in this series of articles.

¹ See Adams, *Kush* 10, 17-18, and *Kush* 11, 10-15.

THE TELL EL-FARÂ'ÎN EXPEDITION, 1966

By M. V. SETON-WILLIAMS

THE second full season at Tell el-Farâ'în lasted from May 2 to July 4. The Egypt Exploration Society is again indebted to the Egyptian Department of Antiquities for their assistance and that of the Inspector appointed to the Expedition, Mr. Gamal Bahy, and the reis, Hajj Ismayin Ibrahim Fayid of Gîza who has assisted us each season.

My thanks are due to the members of the Expedition: Miss Dorothy Charlesworth and Mr. Joseph Clarke, who supervised the furnace and temple sites respectively; Mr. Peter Darvall, engineer and surveyor and his wife Ann who was our housekeeper; Miss Angela Kenny who looked after the small finds and photography; Mr. Peter French who drew several hundred pottery types and supervised W8; Mrs. Stephanie Gee, my assistant; Miss Joan du Plat Taylor who did a fortnight's invaluable work on site; Mr. David Dyson who photographed continuously during the last three days of work;¹ and also to Robin Derricourt who assisted on the site for the first few weeks.

Site B: Gg 22.² Under the three Roman layers of Level I, found in 1965, a further two Roman layers were discovered. Room 3 turned out to be a passage-way with traces of a drain. The most interesting structure was Room 2 which had been cut into on the west side by a late Roman pit (6). This room, 4 × 2 m., had been abandoned with its furniture in position. It contained three amphorae crushed upon a beaten floor, two large storage jars and several small household pots and bowls, as well as a cluster of iron nails and some Roman coins. This was the most complete of the rooms, as Room 1 only contained two mudbrick floors, one 50 cm. beneath the other, but no built-in structures. All these rooms were used for domestic purposes and there were no traces of amulets or other votive objects. The Roman level overlay an area about 1 m. deep containing Ptolemaic material and Ptolemaic coins, which was in its turn above a large mudbrick platform which appears to be Saite in date.

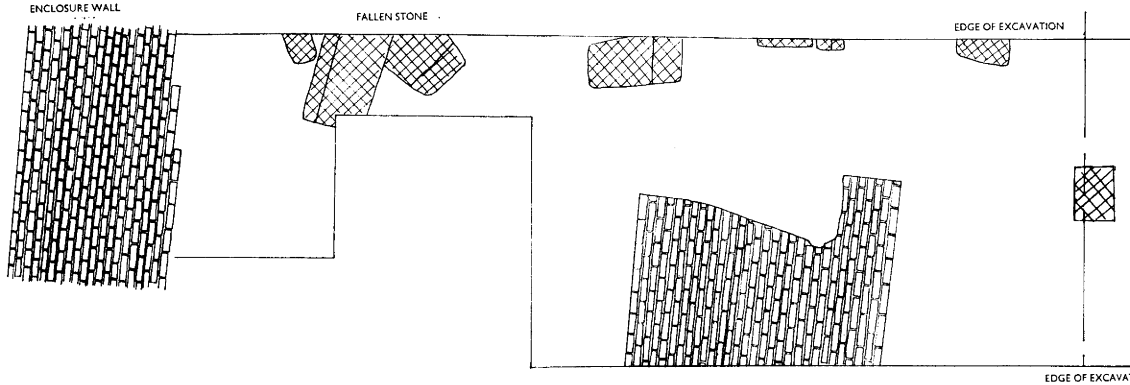
Hh/Gg 22 (see pl. XXXVIII, 3) The limestone platform or paved way, found in 1965, turned east and was overlaid by limestone blocks which had been partly broken up. This paved way was bounded on both sides by mudbrick walls 3 m. thick. Between the walls and the pavement was a mudbrick platform on which was set a small smelting hearth. This had been cut through by the foundation trench for the building of the limestone wells. These wells and the steps associated with them appear to be on a different alignment from the limestone paved way and to have been built at a later period.

¹ I am grateful to Stephanie Gee for help in preparing this report, and to Miss Charlesworth who has written the section on the Industrial Area.

² For the positions of the sites excavated this year the reader should consult the overall plan of the site, *JEA* 51, pl. VI.

Jj22

li 22

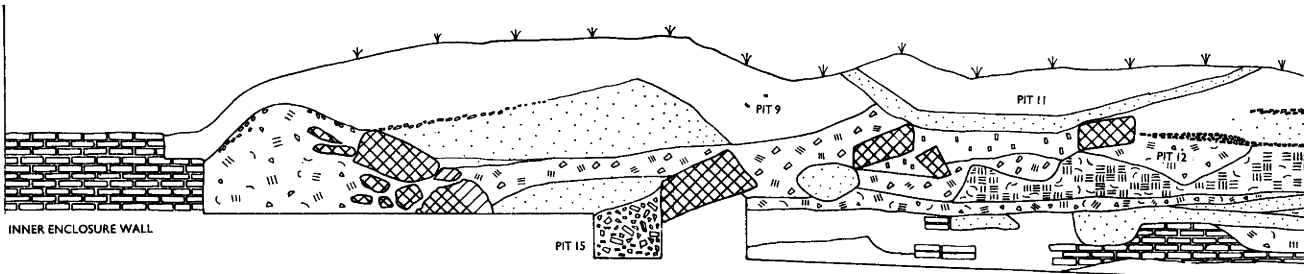


i. F

- | | | |
|----------------------------|------------------------------|-------------------|
| SURFACE DUMPING OR PIT | BLACK & BROWN MUD BRICK | THIN SANDY BRICKS |
| WATER LAID | GREEN MUD BRICK | GREY BRICKS |
| EARTH & LIMESTONE CHIPS | EARTH & POTTERY CHIPS | BROWN BRICKS |
| LIMESTONE BLOCKS | EARTH, POTTERY & STONE CHIPS | PLASTER LINES |
| SANDY PATCH OR LAYER | CHIPS | |
| GREY CLAY & POTTERY SHERDS | | |



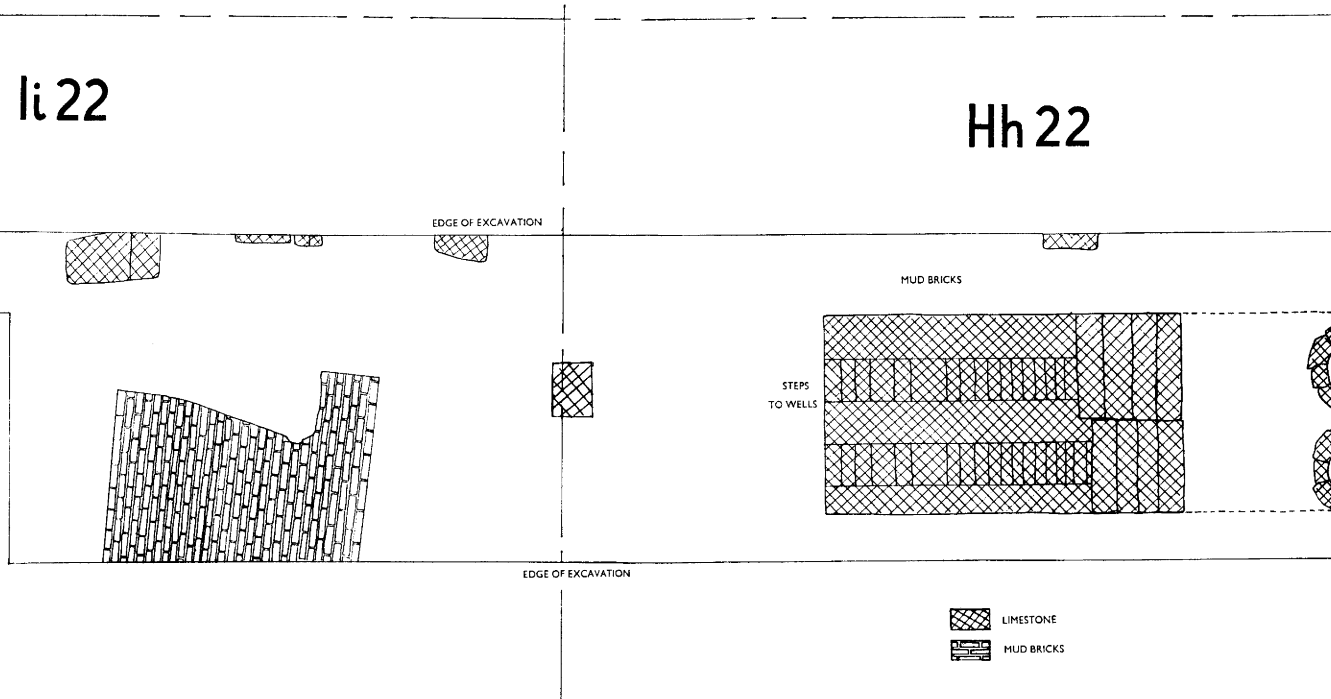
E



2. Gg, Hh, and Ii 22. S

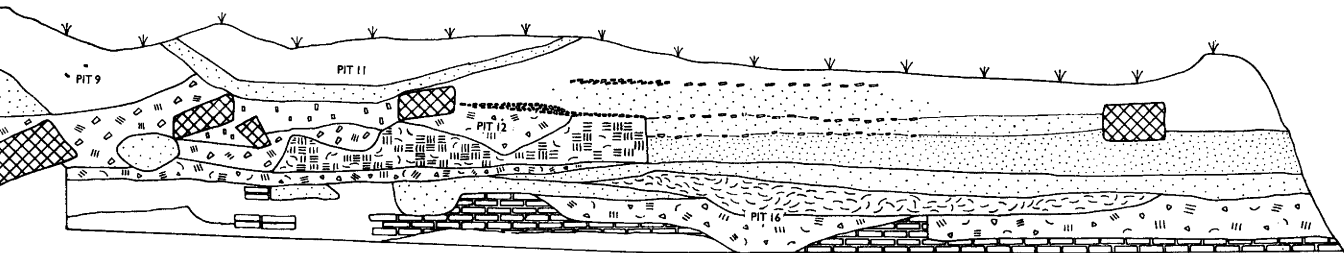
li 22

Hh 22

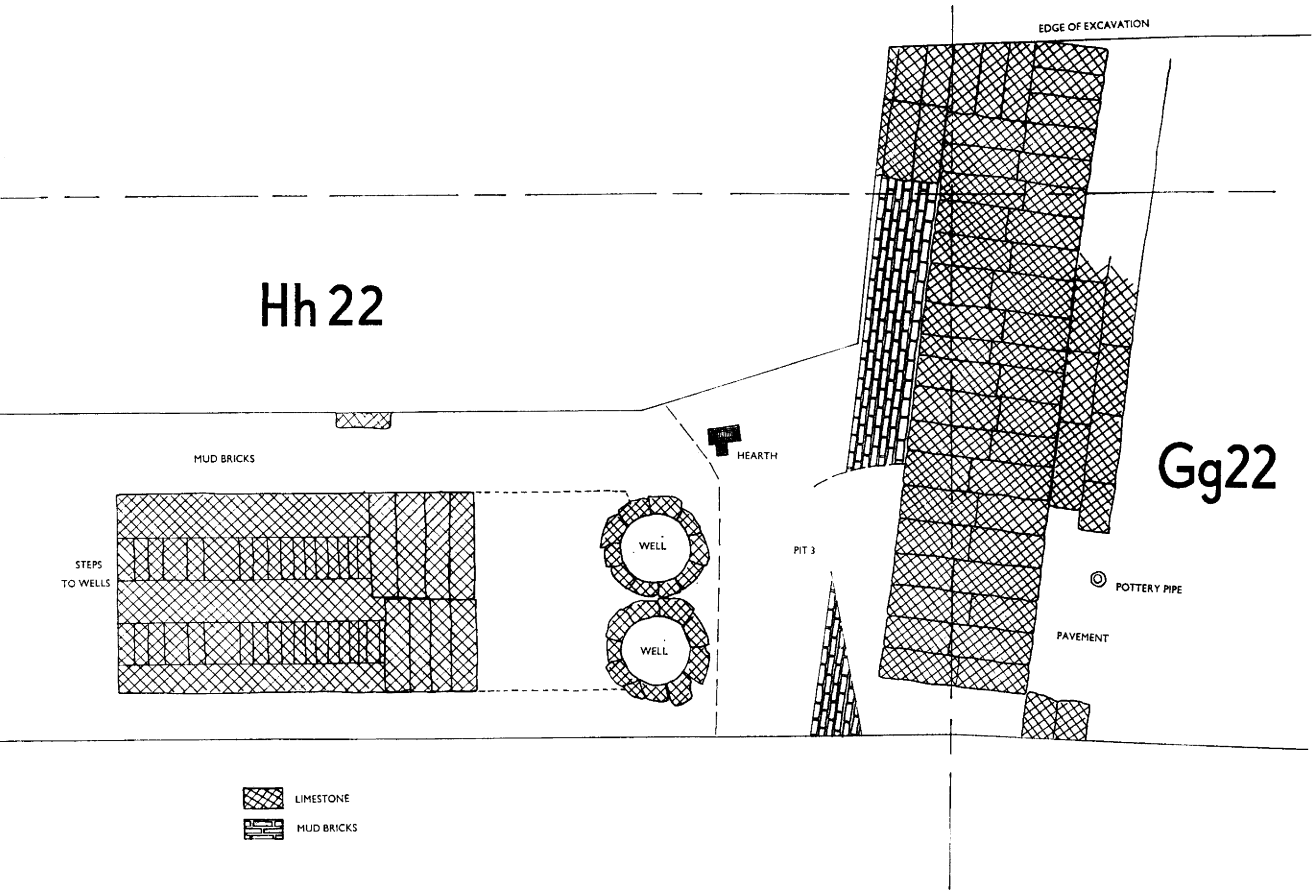


1. Plan of Site B.

- THIN SANDY BRICKS
- GREY BRICKS
- BROWN BRICKS
- PLASTER LINES

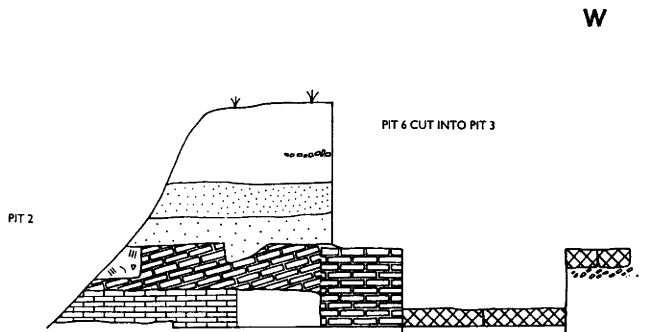
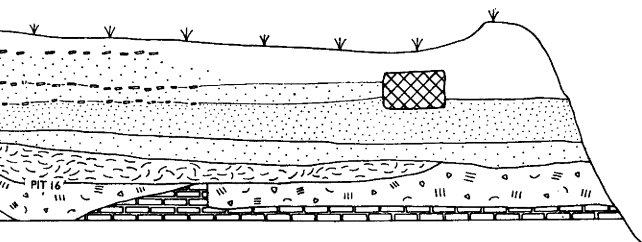


2. Gg, Hh, and Ii 22. South Face Section, July 1, 1966.



ite B.

4 5 Metres



ce Section, July 1, 1966.

In the structure leading to the well, eight to ten steps were cleared in 1965 but in 1966 the north-west well was pumped out, and twenty steps were uncovered ending in a sudden drop; several steps had been removed, leading to a hole 1 m. high in the side of the well, where the water could be obtained when the water-table was low. The material from this well consisted of fallen limestone blocks, some of which had been inscribed but were very water-worn, and Roman amphorae showing that the well continued to be used in the Roman period. The wells and steps were both cut through an earlier mudbrick platform not yet cleared.

ii 22 (see pl. XXXVI) In clearing the next 10 m. square, a large number of limestone blocks was encountered at about half the depth of the trench. This made clearing and moving them a very slow process. Included among them was a large column base. These stones were in Roman pits and it seemed that the Romans were robbing the earlier structure and breaking up the stone, but had apparently been interrupted before their final removal. The mudbrick platform on which this stone structure had stood had been cut into in various places by pits. It was from this area that two fragmentary statues were found not *in situ* (see pl. XXXVII).

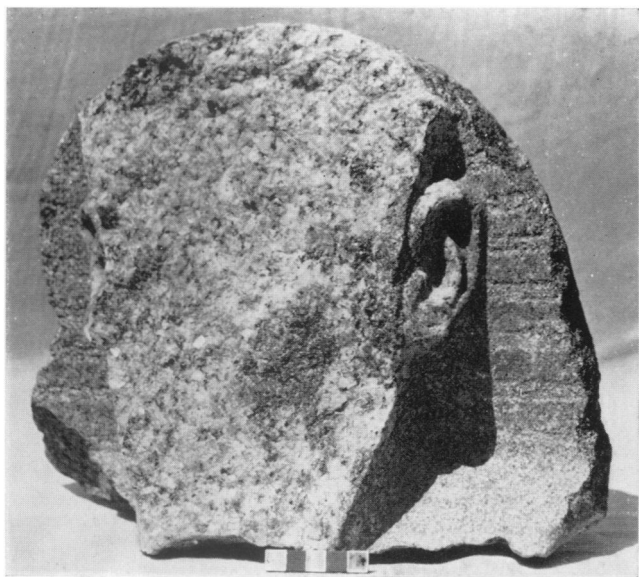
Continuing in an easterly direction the inner enclosure wall of the temple was reached. This was of mudbrick 3 m. thick and faced with a mud plaster. This wall was of two periods, the upper structure being still in use in the Roman period, and sitting clearly on the lower wall. In one place a dressed limestone block was cemented into it, probably all that was left of a small door. The upper wall, which was very decayed, seemed to have rounded bastions, but was much cut into by large late Roman pits. No floor level could be ascertained. The wall was cleared for a length of 22m.; the lower structure went down into the water level.

Inside the enclosure wall a number of pieces of pink plaster with raised reliefs were found. The appearance of these mudbrick platforms underlying the robbed stone is very similar to the mudbrick platforms found by the Americans at Mendes and considered by them to be Saite in date.

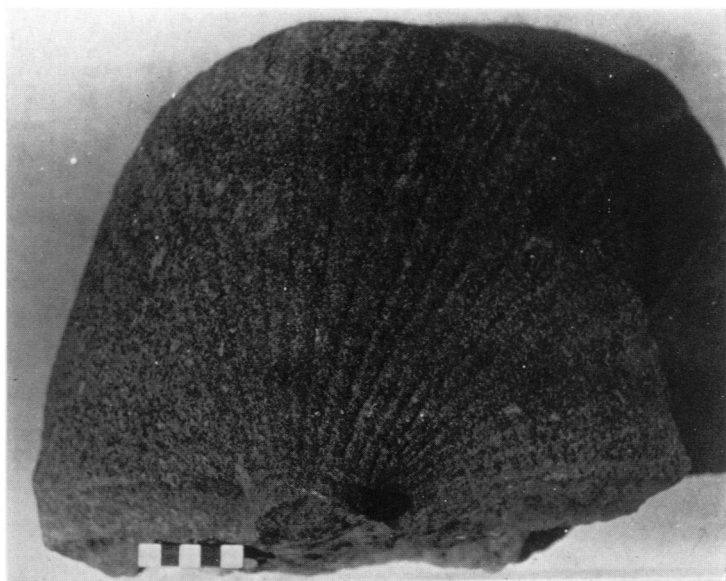
Site C: W8. The trial stratification pit on Site C was continued intermittently during the season. A further 2½m. were excavated, revealing two mudbrick houses under Structure 1, Structures 2 and 3. The pottery from this site equated with that from the lowest levels reached on Site B and contained a lot of imported material.

Industrial area: Cc 19, Dd 18, 19. (See pl. XXXIX). Three industrial areas form a conspicuous feature of the site. All are substantial mounds covered with vitreous slag, ashes, and brick rubble. It was decided to examine the mound, c. 60 × 40 m., near the the south-west entrance of the temple enclosure (site B) in order to establish the nature and date of the industries. During the two month's season north and east quarters of the mound were dug; nine kilns and one open hearth were excavated. Five of these kilns lay within a partly destroyed bathhouse, the north and east walls of which were still standing to a height of 58 cm. above the level of the floor of kiln 5, the best preserved of the rectangular kilns.

The site was well chosen. The prevailing north wind is funnelled between mound A



1. Front view

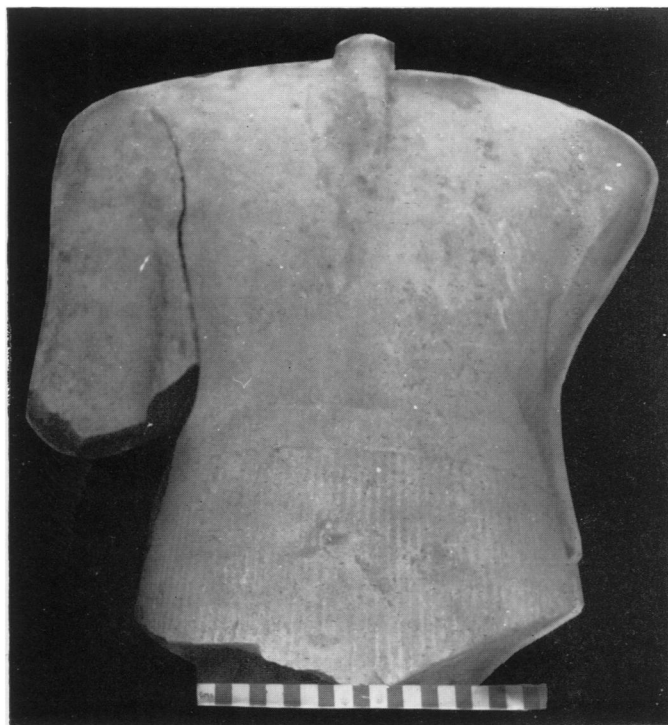


2. Back view

Granite Head



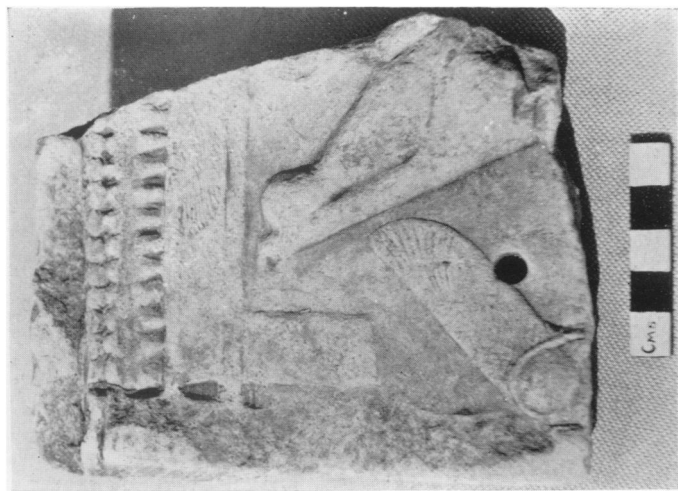
3. Front view



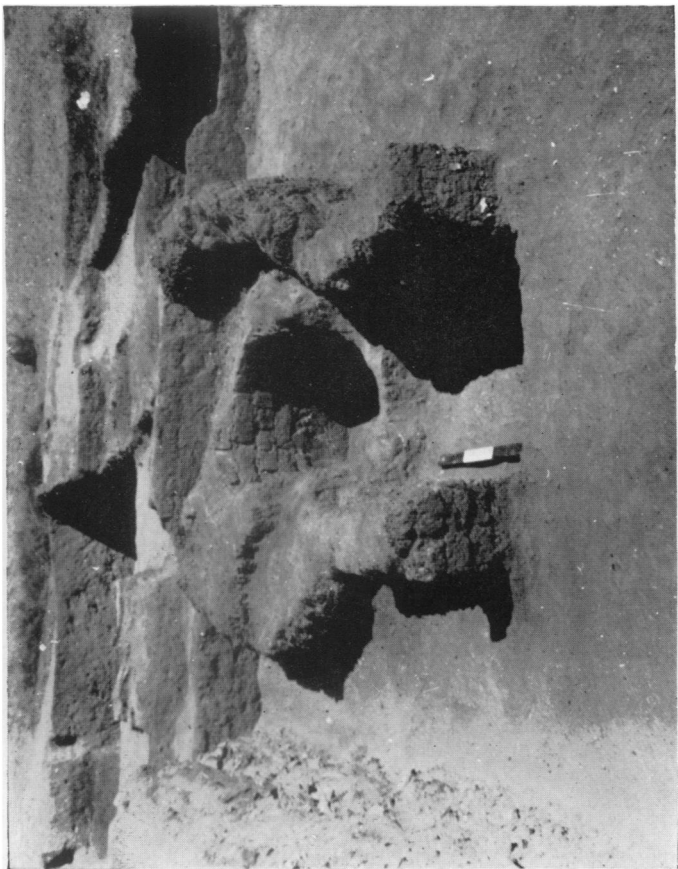
4. Back view

Limestone Torso

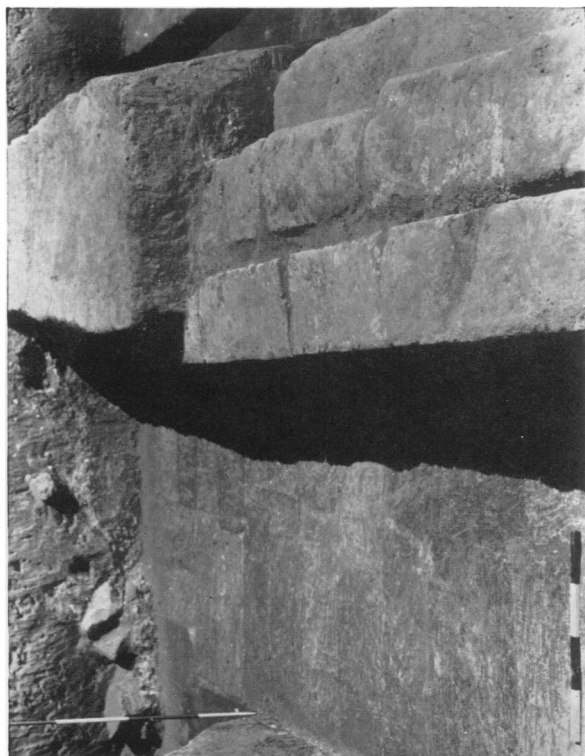
TELL EL-FARÂ'ÎN 1966



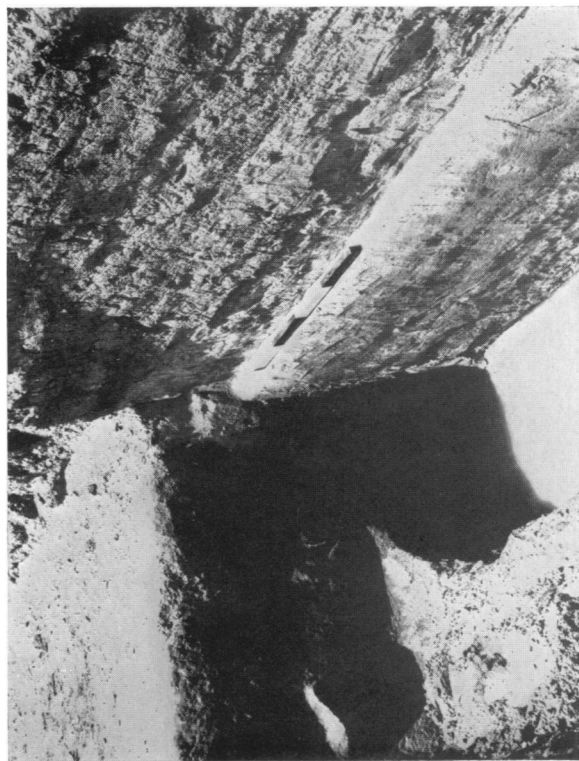
1. Inscription no. 12



2. Kiln no. 2



3. Limestone Pavement



4. Inner temple enclosure wall showing water level.

and the temple enclosure, mound B, thereby providing a constant strong draught for firing the kilns. Two of the circular kilns, both lying to the north of, and above the level of the ruined bathhouse faced straight into the wind. The third circular kiln, inside the ruined bathhouse, was in too damaged a condition for the direction of its flue to be determined. Two rectangular kilns faced west and a third probably faced south. Both types of kiln seem to have been used for the manufacture of faience vessels, but until the technical reports on samples from the site are available there can be no certainty. A large quantity of faience fragments was found, including some wasters, of which the most important example is part of a base to the underside of which is attached the top of the pottery support or spacer on which it stood in the kiln. Other fragments of base had circular marks on them where there was either no blue glaze or where the glaze was extremely thin, and which are assumed also to be the marks left by the supports. No complete example of a support was found, but there were several incomplete specimens in both pottery and faience. There was no evidence that objects such as beads, amulets, or figurines had been made here. Further evidence, which has still to be assessed, came from the pit at the south-east corner of the site where a broken crucible, thought to contain traces of copper (which was necessary to obtain the deep blue colouring of the glaze), and two fragmentary mixing pots with as yet unidentified contents, were found.

All the kilns date from the Roman period, as indeed does the bathhouse which preceded them. The few fragments of Hellenistic pottery are no more than residual material, pieces dumped in the pit and on the rubbish tips which covered the centre of the site, and into which kiln 2 (field number 11)¹ was built. A few sherds of painted Coptic pottery were also scattered in the upper levels to the north of the bathhouse, and a much eroded wall of that period lay near the north end of the area dug.

The kilns were built of mud brick, which had, of course, baked when the kiln was fired and had floors of limestone. Kiln 2 (FN 11) had at one stage been relined, reducing its diameter from 1.3 m. to 0.77 m.; at a third stage its floor had been stripped out and the flue rebuilt but the kiln abandoned without any subsequent firing. Kiln 1 (FN 1) may have succeeded it. This kiln, internal diameter varying from 80 to 73 cm. had been heavily fired. When its limestone floor was removed the mud brick sub-structure (which had not been fired) could be seen. There was no evidence of any burning under the floor, and in kiln 2 likewise the lower courses of mudbrick, below the level of the removed floor, were unbaked. Of the rectangular kilns the best preserved (FN 5) was represented by the remains of a limestone floor of 7 m. by 1.60 m. on which ash had accumulated in layers increasing in depth from west to east. No attempt seems to have been made to clear the floor; a new one was put down on top of the ash. None of the brick structure remained, but the plan is probably similar to that recorded at El-'Amarna by Petrie,² with an opening at either end to provide a horizontal draught

¹ In the field the kilns were numbered as they were, or were thought to be found. Subsequently some were found not to be kilns, and others, which appeared to be separate kilns, proved to be parts of a single large kiln. For the reports it is therefore necessary, for the sake of clarity, to re-number them.

² W. M. F. Petrie, *Tell el-Amarna*, 27, pl. XLII.

through the kiln and so give a regular temperature throughout its length. The long narrow plan is obviously designed to achieve a high temperature. On the first repair on the east half of the kiln floor was found a group of small circular marks, corresponding in diameter with the size of some of the fragmentary supports, and thought to be made by the bases of the supports of which the faience vessels would be standing in the kiln.

The bathhouse has not yet been fully examined. It had been largely destroyed shortly before the construction of the kilns, and two of the rectangular kilns had hypocaust tiles re-used in them. However, there remained a suspended floor of *opus signinum*, laid in blocks 79×73 cm. on which stood three baths on the three sides of a square, all built of brick and plastered inside and on three of the outer sides. In each case the long outer side of the bath was left rough, showing that it had originally butted against the wall of the room. The baths probably were filled by hand as, although they stood to full height, there was no signs of an inlet for a pipe; but they drained out on to the floor which itself must have had a drain in one corner. This is surely part of one of the public bathhouses. It is unlikely that any private bathhouse would be provided with three plunge baths; but it is probable that the public buildings of the Roman town were built in this comparatively level area between the three main mounds.

In excavating the area on which the dig house was constructed, a Roman well in square Y 14 was discovered. It was built of baked bricks and was 1 m. 10 cm. across. On clearing it, over eight hundred amphorae, pots, and jars were recovered. These were Roman in date and will form the subject of a separate study. This well produced an excellent water supply which was extremely useful for completing the house and washing the pottery. It also served in moments of stress when the normal water supply was cut off, although the saline content was rather high. The water supply problem was rather acute this year as the drinking water was cut off far more frequently than in previous seasons.

In association with the well was a small lined bath, almost square in shape. Close by on a plaster floor was found a pile of mixing vessels similar to those found in the industrial area associated with the kilns and thought to be used for mixing colouring materials.

In 1965 the archaeological evidence suggested the existence of three main periods. This year a further level IV has been added to the sequence. The main periods now are:

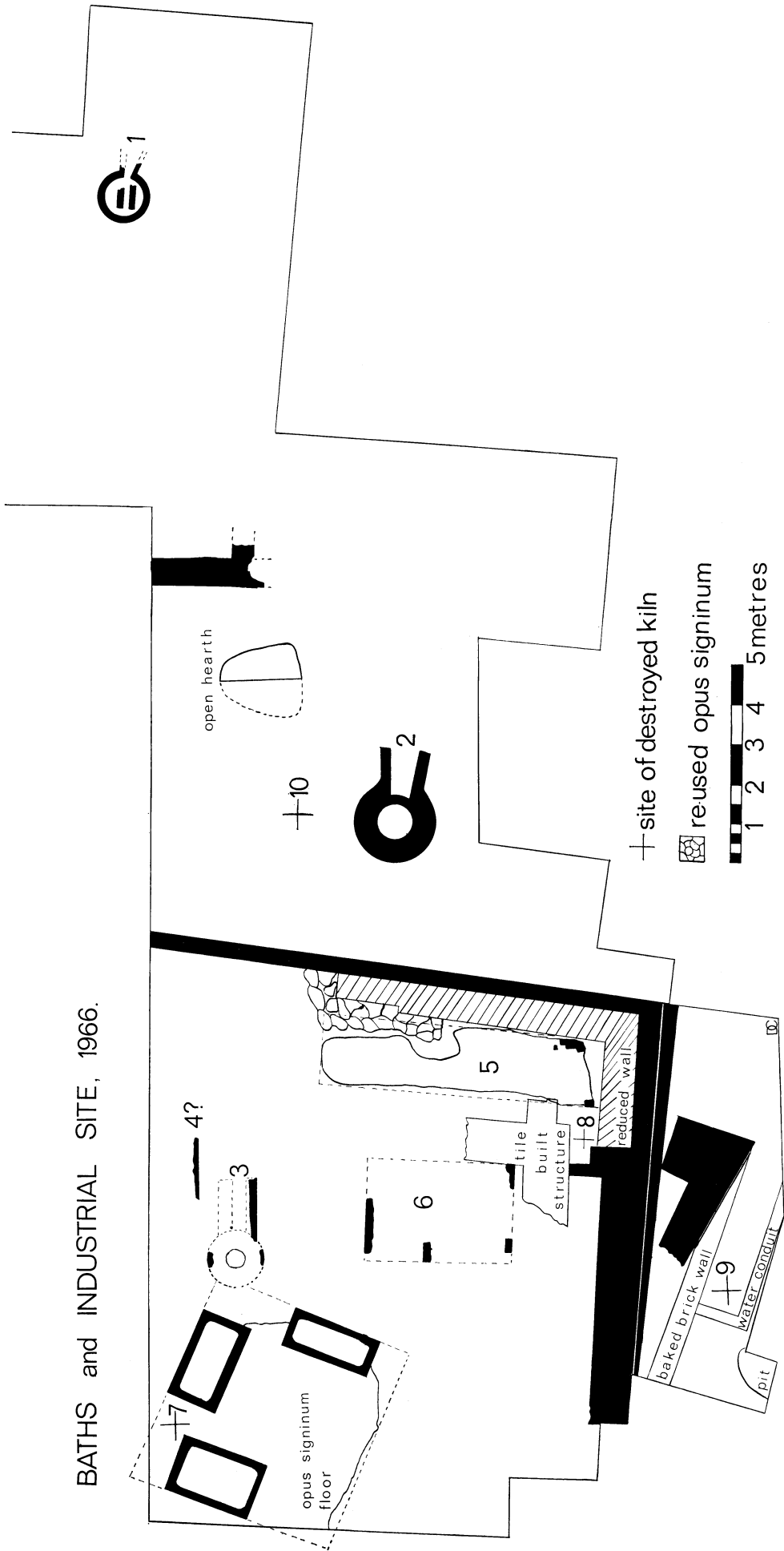
Level I. Roman: this will have to be sub-divided into at least four divisions once the pottery has been studied in detail. It includes buildings, kiln, baths on Sites B and C and in the area between them.

Level II. Ptolemaic: on Site B in squares Gg 22 and Hh 22.

Level III. Persian: W8, Site C, and Hh 22 and Gg 22, Site B, structures and pottery.

Level IV. Saite: mudbrick platforms in site B and mudbrick buildings in W8, Site C. Among the objects found intrusive in these late levels was a number of finely worked flints, several of which were predynastic in type.

The Pottery. Pottery was, as usual, the most ubiquitous material on the site. Although a few fragments of Coptic painted ware and two Islamic pipe bowls were found,



TELL EL-FARÂ'IN 1966

the remarkable thing is that even among the surface debris there was practically no late pottery to be found. The main part of the town would therefore appear to have been deserted by about the fourth or fifth century A.D.

With the vast increase in the number of forms found, particularly the pottery from the wells situated in Y 14 and Hh 22, a certain amount of time will have to be devoted to the study of parallels elsewhere than in Egypt. A promising start has been made with the Agora material which is offering some very useful references in the Roman, Hellenistic, and earlier phases.¹ However, it may be necessary to look further westward for some of our comparative material as our black wares of the Ptolemaic Period can be matched in Italy by the Campagnian of the Roman Republican Period.²

A mortar, Field Number 210, found in W8 in 1965, has proved to be a Corinthian Greek import, probably of the fourth to fifth century B.C. although they do occur later at the Agora.³

On Site B in the houses in Gg, a number of lamps were found but these are extremely difficult to date closely on typological grounds. Several of these were frog lamps or of the corn and palm type. The earliest frog lamps are thought to date from the second century A.D. but they do extend as late as the fourth century.⁴

An interesting feature of both seasons were the Hellenistic jar stamps. It is now known that these stamps are far more numerous in Egypt than in Greece, and it is hoped that the discovery of further types in a stratified sequence may assist with the establishment of the wine trade routes of the Hellenistic Period. The Farā'in examples came mainly from Site C in 1965 and from the industrial area in 1966. They include one Rhodian, 275-300 B.C., two so-called Cretan stamps probably from Samos of the second century B.C., and a Thasian specimen of the fourth century B.C. This year's stamps have not yet been identified.⁵

Foreign sherds, particularly Assyrian and Aramaean, continued to occur in Levels III and IV on Site C.

The Coins. Several hundred coins were found this season, mostly in the rooms of the Gg area on Site B. Many of them were in a very poor condition and could only be recognized as Roman by their size and the other objects associated with them. However, inside some of the lower rooms there were better specimens of an earlier date.

The coins described below represent the identifiable specimens found during the two seasons' work so far carried out.⁶ As is frequently the case with excavation coins from Egyptian sites they are mainly in very poor condition, the legends being badly effaced and often also the types. Those parts of the legends given in square brackets are either known from specimens in one of the major collections (see bibliography) or reasonable reconstructions of what one might expect to find. No weights are given in the

¹ I am indebted to Professor and Mrs. Homer Thompson for enabling Mrs. Gee and myself to examine the Agora material.

² Reference supplied by Miss Joan du Plat Taylor.

³ Sparks *JHS* 1962, 125, pl. IV: 4. Thompson, *Hesperia* 3 (1934), 416 and 470, type E 124, fig. 102. References supplied by Peter French.

⁴ From information supplied by Peter French.

⁵ I must thank Miss Virginia Grace of the Agora Expedition for identifying these specimens.

⁶ I am indebted to Mr. Peter Clayton for preparing notes on the Tell el-Farā'in coins.

descriptions since the coins had to be identified from photographs and weights were not available. Similarly with regard to the Ptolemaic coins, the size stated is as close as possible in consideration of the centimetre scale in the photographs.

On the present showing of coins from the site, there is a strong emphasis on the Roman period of the second half of the first and early second centuries A.D. Judging from the condition of the coins they remained in circulation for very many years after they were struck. By one of the vagaries of numismatic terminology, the coins of the Roman period in Egypt are referred to as Greek Imperial issues. The mint of Alexandria continued to strike coins upon the Greek standard after the conquest of Egypt by Augustus in 30 B.C., and this standard was used until the mint reforms of Diocletian in A.D. 296 when the mint of Alexandria changed to the Roman Imperial standard and types. Hence the Greek Imperial issues of Alexandria are a curious mixture of a Roman emperor's head as obverse type surrounded by a Greek legend and reverse types which may have their inspiration in Egyptian, Greek, or Roman mythology and iconography, or any mixture of these three elements.

Statuary. On Site B inside the temple enclosure a royal head, Field Number 925, in black granite wearing the *nemes* headcloth was found. It was not *in situ* as it was from Ii 22, Level II, and should almost certainly be attributed to Level IV. The bands of the headcloth were of regular width, and the ears were well set close to the head. Unfortunately the face was deliberately damaged but enough remained to show that originally it had been pink: this was not due to an artificial joining of two stones but the careful choice of a stone that naturally combined pink and black granite. Its extant height is 25.5 cm., width 35 cm. (pl. XXXVII, 1, 2). Near the head was found a limestone torso with head and limbs missing. Next day the upper part of the left arm was found at a little distance. The torso is much weathered and was perhaps originally plastered, but only this now remains in patches on the right shoulder. The chest muscles, medial line and navel are carefully delineated but with none of the over-softening of Ptolemaic work; this suggests an earlier date, probably Saite. Its extant height is 43 cm.

Faience. There were a number of faience amulets from W8, Site C. They were of a greenish-blue colour and varied considerably from those of the previous season. Some were inscribed on the pillar back and others under the base, resembling in this way amulets of the Twenty-second to the Twenty-fourth Dynasties from Lahun.¹ Most of the amulets came from Level IV. They included figures of Delta deities such as Ptah-Soker identical with Petrie's Twenty-sixth Dynasty type,² Isis and Horus, a form popular from the Saite Period onwards, Horus hawk-headed wearing the double crown,³ papyrus pillar amulets, the symbol of the goddess Edjo, sacred to Buto, and several *udjat*-eyes similar to those from Memphis of Twenty-second dynasty date with dark raised brow ridges.⁴ There were broken figures of Sekhmet or Edjo and numerous figures of Bes, Knum or the ram-headed god of Mendes, baboons, and Hathor cows.

¹ W. M. F. Petrie *et al.*, *Lahun II*, pl. LV a.

² W. M. F. Petrie, *Amulets*, pl. XXXI: 176 c.

³ W. M. F. Petrie, *op. cit.*, pl. XXXI: 180 h.

⁴ W. M. F. Petrie, *op. cit.*, pl. XXV: 138 s. Tufnel, *Lachish III*, pl. 35, 43.

Among the amulets were found some *Cardium Edule* shells pierced for suspension, and used to ward off the evil eye.

Other faience objects found included small bowls, platters, disks, and beads.

Conclusions. This year the inner temple enclosure wall was reached, and the existence of three different structures in the area was proved, Ptolemaic and pre-Ptolemaic. A large-sized industrial area was partially cleared and much information gained about the technical processes in use. It is hoped to pursue this work further next year. The stratification pit on Mound C has proved particularly useful as a check to the temple levels, and it may prove possible to reach earlier layers on this site before arriving at water level. It is planned to excavate within the temple enclosure next season, and to follow further the limestone pavement which at present runs under the section.

This year the water table was definitely lower than in previous years, and it is hoped that the new high dam will make further work in the Delta easier by limiting the amount of water available. There were lighter dews and less humidity than in previous seasons. A certain amount of time was lost owing to dust storms which were worse and more frequent than previously. The erection of the dig house, delayed by the lack of cement, was finished halfway through the season except for the building of the pottery store. Moving into the house greatly accelerated the work of the Expedition.

The Coins¹

Ptolemaic

1. Ptolemy III Euergetes, 247–222 B.C. AE 1·2
Obv. Head of Zeus Ammon right.
Rev. [ΠΤΟΛΕΜΑΙΟΥ Β]ΑΣΙΛΕ[ΩΣ]. Eagle standing left, on thunderbolt.
 Badly worn overall and no mint marks or monograms visible.
 FN 747. Gg 22 Room 2
2. Ptolemy IV Philopater I, 222–204 B.C. AE 1·5
Obv. Head of Zeus Ammon right, diademed.
Rev. ΠΤΟΛΕΜΑΙΟΥ ΒΑΣΙΛΕΩΣ. Eagle left, on thunderbolt, looking back; on left wing, cornucopiae. B.M.C.P. 35–38
 Pitted and worn, letter between eagle's legs illegible.
 FN 901. Gg 22 area A Level I
3. Ptolemy VI Philometer, 181–146 B.C. AE 0·7
 Period of reign with Cleopatra I as regent, 181–c. 174 B.C.
Obv. Head of Cleopatra I, right, as Isis, with long curls bound with corn.

¹ B.M.C.A. Poole, R. S. *British Museum Catalogue of Greek Coins. The Coins of Alexandria and the Nomos.* London, 1892.

B.M.C.P. — *British Museum Catalogue of Greek Coins. The Ptolemies, Kings of Egypt.* London, 1883.

Dattari. Dattari, G. *Monete imperiali greche. Numi Augg. Alexandrini.* 2 vols. Cairo, 1901.

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Karanis. Haatvedt, R. A. and Peterson, E. E. *Coins from Karanis: the University of Michigan Excavations 1924–1935*, edited by E. M. Husselman. Ann Arbor, 1964.

Milne. Milne, J. G. *Catalogue of Alexandrian Coins in the Ashmolean Museum, Oxford.* Oxford, 1933.

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- Rev.* ΠΤΟΛΕΜΑΙΟΥ ΒΑΣΙΛΕΩΣ Eagle left, on thunderbolt, looking back; on left wing, cornucopiae bound with fillet; to left, a monogram P (?) B.M.C.P. 2
Probably struck at Paphos in Cyprus.
FN 478 (1) Hh 22 Level II
4. Ptolemy X Soter II, 117–81 B.C. AE 1·2
With Cleopatra III, 117–111 B.C.
Obv. [ΒΑΣΙΑ]ΙΣΣΗΣ [ΚΛΕΟΠΑΤΡΑΣ] Head of Zeus Ammon right, diademed.
Rev. Two eagles left, on thunderbolt; in front, double cornucopiae; between legs of nearer eagle, monogram P B.M.C.P. 20.
Mint of Alexandria.
FN 748 Gg 22 Room 2
- Greek Imperial*
5. Nero, A.D. 54–68 AE tetradrachm
Obv. [ΝΕΡΩ ΚΛΑΥΚΑΙΣΣΕΒ ΓΕΡΑΥ] Bust right, radiate.
Rev. Illegible.
Worn.
FN 764 Hh 22 West Level I
6. Vespasian, A.D. 69–79 AE drachm
Obv. [ΑΥΤΟΚΚΑΙΣΣΕΒΑ ΟΥΕΣΠΑΣΙΑΝΟΥ] Head right, laureate.
Rev. Bust of Nike right, showing wings. Date letter effaced.
Known for years 1–3.
Worn. B.M.C.A. 246–50.
FN 753 Ii 22 West Level I
7. Vespasian, A.D. 69–79 AE diobol
Obv. [ΑΥΤΟΚΚΑΙΣΣΕΒΑΟΥΕΣΠΑΣΙΑΝΟΥ] Head right, laureate.
Rev. Bust of Isis right, wears horns, disk and taenia; to right L (?)=year 5.
Worn. B.M.C.A.–. Milne 427
FN 743 Cc 19 North-West Level I
8. Hadrian, A.D. 117–38 AE half drachm
Obv. [ΑΥΓΚΑΙ ΤΡΑΙ ΑΔΡΙΑΣΕΒ] Bust right, laureate, wears paludamentum and cuirass.
Rev. Nike advancing left. Date letters effaced but probably L 5 in front=year 16.
Worn. B.M.C.A. 721. Milne 1341
FN 478 (A) Hh 22 Level I
9. Antoninus Pius, A.D. 138–61 AE tetradrachm
Obv. ΑΝΤΩΝΕΙΝΟ ΣΕΒΕΥΣΕΒ Head right, laureate.
Rev. ΛΕΝΑ ΤΟΥ Eagle standing facing, on thunderbolt, wings open.
Year 9. B.M.C.A. 1002. Milne 1857
Worn overall, only odd letters may be read.
FN 750 Jj 22 Level I
10. Antoninus Pius, A.D. 138–61 AE tetradrachm
Obv. [ΑΝΤΩΝΙΝΟC ΣΕΒΕΥCΕΒ] Bust right, laureate.
Rev. Eagle standing right, wreath in beak, head left, wings closed. In field L Δ=year 14.
Obverse particularly badly effaced. B.M.C.A. 1003. Milne 2084
FN 476 (4) W8 Extension Level I

11. Antoninus Pius, A.D. 138-61

AE half drachm

Obv. ΑΥΤΚΤΡΑΙΑΑΔΡ[ΑΝΤΩΝΙΝΟC or possibly ΑΝΤΩΝΙΝΟCCEΥCΕΒ] Head right, laureate.

Rev. Emperor in quadriga moving left, raises right hand and holds sceptre (?) in crook of left arm. Date letter above but illegible.

This is apparently an unpublished example of this type with the Emperor's head right and quadriga left. It is not represented in B.M.C.A., Milne, Feuarent, Dattari, Voigt, or Karanis.

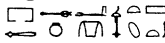



FN 478 (3)

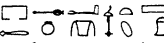
Hh 22 Level I

BRIEF COMMUNICATIONS


An additional note on ⲟ 'foreigner'

IN his recent book, *Inscriptions from the Coptite Nome*, Henry G. Fischer¹ contested the translation 'foreigner' I had earlier proposed for the term ⲟ, returning to the previous rendering 'interpreter'. There seems no need to repeat the arguments I have brought forth² concerning the fact that to be 'the speaker of a foreign language', in the sense of the Greek βάρβαρος, does not entail serving as an 'interpreter'. The graffito at Tomâs,³ listing eight *imy-r ⲟw*, would refer to an astounding gathering of *'interpreters' in one place, especially if one assumes that an 'overseer', by nature of his rank, is associated with a group of people whom he 'oversees'. To maintain the translation *'overseer of interpreters' in this inscription would require the assumption of a convention of 'interpreters' at Tomâs, which can hardly be considered seriously.

The main argument forwarded by Fischer in support of the rendering 'interpreter' for ⲟ is the medical title ,⁴ for which he refers to Peet's rendering 'Physician of the Great House, Interpreter of a difficult art'.⁵ A further occurrence of the element ⲟ is found in the title  which he does not mention. Leaving aside for the moment the problems connected with this second instance, a division into  and  as proposed by Junker, plus the translation reinstated by Fischer, yields a rendering 'interpreter of water in the interior . . .'. When we recall the basic meaning of ⲟ as established by Gardiner,⁷ 'to speak a foreign language' (βαρβαρίζειν), an ambiguity arising from the English usage of the word 'interpreter' becomes obvious. While a connexion between the speaker of a foreign language and a translator might still be plausible, to go further and play on the wider connotation of the English word 'interpreter' as one who elucidates or explains, would not only be stretching the interpretation of the term beyond its limits, but would introduce quite unwarrantable implications. This becomes obvious if the English word is replaced by its German equivalent, 'Übersetzer von Wasser im Innern . . .'. This now gives no sense.

With this limitation in mind we can now turn to the designation  quoted by Fischer as decisive evidence for a translation 'interpreter' of the word ⲟ. Fischer, like Peet and Junker before him, has overlooked the fact that *hmwt* is not attested with a determinative ⲟ; nor would such a determinative seem to make sense. The sign ⲟ is well attested in the Old Kingdom as determinative for *wṯ* 'embalmer',⁸ but I am unable to quote any other example of its use ideographically, as apparently here. As a compound expression, *hmwt-wṯ* is paralleled by the more frequently attested *hmwt-hry-hb*, 'the craft of the lector-priest';⁹ *štst* without a determinative can only

¹ Fischer, *Inscriptions from the Coptite Nome* (Analecta Orientalia, 40), 28 ff.

² Goedicke, 'The Title  in the Old Kingdom', *JEA* 46, 60 ff. For the reading ⲟ adopted here, see Goyon, *Wadi Hammamat*, no. 21, 5. ³ Weigall, *A Report on the Antiquities of Lower Nubia*, pl. lviii.

⁴ Quibell, *Excavations at Saqqara 1905-6*, 22, pl. xiv.

⁵ Junker, 'Die Stele des Hofarztes Irj', *ZÄS* 63, 53 ff., in particular pp. 67 f., Taf. II.

⁶ *PSBA* 37, 224. Fischer does not mention Jonckheere's 'rendering' (*Chron. d'Ég.* 25, 247) of the indication as 'celui qui connaît les organes du corps humain cachés aux yeux' or Read's (*BIFAO* 13, 142) 'Royal physician, scholar (or skilled practitioner) in a difficult science', which demonstrate the dangers of disregarding the literal meaning of a word. ⁷ *PSBA* 37, 117 ff., following Spiegelberg, *Rec. trav.* 14, 41-42. ⁸ *Wb.* 1, 379.

⁹ *Urk.* 1, 202, 16; James, *The Mastaba of Khentika, called Ikhekhi*, inscr. 7, A (5); Wild, *BIFAO* 58, 106 f.


datival *n* being well attested.¹ Once this is recognized, it becomes necessary to translate thus: 'The foreigner(s) were peacefully disposed towards (me) on the West and East of Wawat until I brought safely back (my) troop of soldiers.' Not only is a rendering 'interpreter' excluded here, but the *ꜥw* were not connected with the Egyptians in any way. The term applies rather to people living in the West and East of Wawat, who apparently were distinguished from the *Nḥsy* 'Nubians'. The passage is not conclusive, but might imply that the *ꜥw* were the settled population of the Nubian Nile valley, as contrasted with the *Nḥsy* in the mountain country.

It seems that Fischer himself lost confidence in his argument against my rendering 'foreigners' or 'foreign (mercenary) troops' in the title *imy-r ꜥw*. At the end of his argument he proposes: 'the safest conclusion is that the *ꜥw* were, in most cases, Egyptianized foreigners, who were used not only as interpreters but as scouts, spies, agents, couriers and foremen of mercenaries'. He thus comes close himself to my rendering 'foreigner'.

HANS GOEDICKE

Provenance and date of the stela of Amun-wosre (*JEA* 51, 63–68)

THROUGH the kindness of Mrs. Josephine H. Fisher I had the opportunity of publishing the upper part of a stela owned by her in the last volume of the *Journal*. Mr. Barry Kemp of Cambridge University has now been able to supply information as to its provenance and to suggest a reading of the first cartouche. I wish to acknowledge his thoughtfulness in communicating it to me. Mr. Kemp informs me that the stela derives from the excavations at Abydos conducted by Professor John Garstang on behalf of the Liverpool Institute of Archaeology in 1908. It was found as part of the contents of a tomb(?) designated as 368 A '07, the location of which was probably on the local west of the Shûnet el-Zebîb and 'Middle Fort'. The negative in the files of the School of Archaeology and Oriental Studies bears the number A 177. Many of the finds were assigned to the individuals who contributed to the costs of the excavations, and as a result the stela eventually found its way to the United States without any indication of its origin.

On the basis of the earlier photo Mr. Kemp has read the traces of the end of the first cartouche as *ꜥꜣt*: , the sickle (U 1) above the *t* (X 1), with the rear part of the platform (Aa 8) inside the curve of the sickle. This can only represent the prenomen of Ammenemes III, *Ny-ꜥꜣt-Rꜥ*, if the reading is correct. Mr. Peck has examined the original in Detroit and favours a reading with the sickle, although he rejects the *t* beneath it.

The consequences of this later date are several. The office of *imy-r šꜥꜣw* may have been reinstated as part of the administrative reforms attendant upon the curbing of the nomarchs under Sesostri III instead of being a continuation of the office in the latter part of the Eleventh Dynasty and the beginning of the Twelfth Dynasty. Secondly, the palaeographical points indicative of a date early in the Twelfth Dynasty, mainly the variations of the book-roll discussed by Schenkel, with single or double tie, placed horizontally or vertically, would have to be extended to the end of the dynasty and can no longer be regarded as absolute chronological criteria. The stela gives the impression that it follows an earlier model. A possible analogy lies in the stela of Montuḥotpe of the reign of Sesostri I (CCG 20539) and the similar stela of Seḥetepibre^c of the reign of Ammenemes III (CCG 20538).

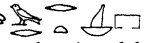
WILLIAM KELLY SIMPSON

Pyramid names

THE name of the pyramid of Ammenemes I, *Imn-m-ḥꜣt ꜥꜣ-nfr*, is translated by Gardiner 'The pyramid, Ammenemes is high and beautiful'.² But surely the adjectives refer to the pyramid, not the

¹ Cf. Cairo 20530; Sethe, *Übersetzung und Kommentar der altägyptischen Pyramidentexte*, III, 53. For the construction, cf. Edel, *Altägyptische Grammatik*, § 588.


² *Eg. Gr.*³, 495.


king. The royal name is not an integral part of the pyramid name, for it is sometimes omitted, as in the Story of Sinuhe, (Text R, l. 5). Similarly it does not occur in the name *Mn-nfr* 'Memphis', or in that of the pyramid *Hr* (*Urk.* 1, 18.) Thus the name of the pyramid must be 'The high and beautiful'. The king's name must be the genitive, as in *Ppy hwt-k3* 'Pepy's ka-chapel';¹ or *S:hwt-Rc*  'Sahurê's building (where) the *wrrt*-crown appears in splendour'.² Therefore the words should read 'Ammenemes' pyramid, the high and beautiful'.


Pyramid names are usually adjectival, e.g. *wr* 'great', *nfr-sw* 'most beautiful of places', and *k3-nfr* 'high and beautiful'. However, some names cannot be identified with certainty because they are probably abbreviated.³ Single adjectives are characteristic of the Fourth Dynasty. The accusative of respect, e.g. *nfr-sw*, starts in the Fifth. While two adjectives together, e.g. *k3 nfr*, appear first in the Sixth Dynasty.


Since the names of the pyramids have not been published since 1946,³ it seems a suitable moment to give here a revised list. The names can be verified by reference to Petrie's History; and Gauthier's *Livre des rois*; while the Twelfth Dynasty is also dealt with by O. Firchow in *Studien zu den Pyramidenlagen der 12. Dynastie*, (Göttingen, 1942). I owe this reference to Dr. Edwards.


Dynasty 4


Snfrw , 'Sneferu's pyramid which rises in splendour'.

Snfrw , 'Sneferu's southern pyramid which rises in splendour'.


Hw-f-w , 'Khufu's pyramid which is at the place of sunrise and sunset'.⁴ Gunn suggested that *hwt* was the *nisbe* form *hwtj*.⁵ That the pyramid was thought to be the place where the sun rose and set is suggested by the presence of solar boats. Here *Rc*, identified with the dead king, would have changed boats at dawn and sunset. But see *JEA* 41, 75 ff.

Dd-f-Rc , 'Djedefrê's pyramid which shines like a star'.⁶


Hc-f-Rc , 'Khafrê's pyramid, the great'.


Mn-k3w-Rc , 'Menkaure's pyramid, the sacred'.


Špss-k3f , 'Shepseskaf's pyramid, the pure'.


..... , '.....'s pyramid, the upper'.


Dynasty 5


Wsr-k3f , 'Userkaf's pyramid, holiest of places'. The word *st* 'place' seems to denote the pyramid site as a whole. This is suggested by the sentence *šsp st Špss-k3f Kbḥ* 'enclosing the site of Shepseskaf's pyramid, the pure'.⁸

S:hwt-Rc , 'Sahurê's pyramid (where) the (royal) soul rises in splendour'. This is possibly an example of the relative form with *im* understood. The phrase *hwt-b3* appears in the pyramid texts, e.g. *hwt b3 m ntr*, 'The (royal) soul rises in splendour as a god'.⁹

Nfr-iri-k3-Rc , 'Neferirikarê's pyramid (which belongs to) the (royal) soul'. The genitival adjective may perhaps be understood here, or even the direct genitive.

Nfr-f-Rc , 'Neferefrê's pyramid (which is) sacred to the (royal) souls'. Possibly the accusative of respect.

N-wsr-Rc , 'Neuserre's pyramid, most enduring of places'.

Mn-k3w-Hr , 'Menkaure's pyramid, most sacred of places'. A king *I-k3w-Hr* has a pyramid with the same name. Apparently the two kings are the same.⁹

¹ *Urk.* 1, 264.

² *Urk.* 1, 38. The construction may be the relative form with *im* understood.

³ *Revue d'Égyptologie* 5, 45.

⁴ Faulkner, *Concise Dict.* 5.


⁵ *Revue d'Égyptologie* 5, 45.










































⁶ *Kémi* 8, 223.

⁷ Faulkner, op. cit. 289.

⁸ *Urk.* 1, 239.

⁹ *ZÄS* 42, 8.

Issi †, 'Isesy's pyramid, the beautiful'.

Wnis †

and (ii) a few of the well-known Eighteenth-dynasty figure-vases, whose origin has been discussed at intervals,¹ some associated pots of very common Egyptian types, and two unique vessels which present certain points of comparison. The specimens were prepared by scraping a previously fractured edge or an area where a chip had already removed the surface.²

Specimens Analysed

- 1890.822 Handled alabastron (Furumark 87), Mycenaean II. Lahun, tomb of Maket. Petrie, *Illahun, Kahun and Gurob*, pl. xxvi, 44
- 1921.1436A Jug (Furumark 114), Mycenaean III. Sidmant, tomb 53. Petrie and Brunton, *Sedment*, II, pl. lix, 6; xlvi, 3
- 1909.130 Juglet (Furumark 134:9), Mycenaean III. Deir Rîfa, tomb 20. Petrie, *Gizeh and Rifeh*, pl. xxvii
- 1921.1290 Figure-vase. Sidmant, tomb 263, Tuthmosis III. Petrie and Brunton, *Sedment*, II, pl. xlvi, 25
- 1921.1291A Fish-vase. Sidmant, tomb 263, Tuthmosis III
- 1921.1279 Jug. Sidmant, tomb 263, Tuthmosis III. Petrie and Brunton, *Sedment*, II, pl. lxiii, 263L
- 1921.1282 Pot. Sidmant, tomb 263, Tuthmosis III. Petrie and Brunton, *Sedment*, II, pl. lxiii, 263D
- E.2408 Figure-vase. Abydos, tomb E178, c. Amenophis II. Garstang, *El Arabah*, pl. xix, 1 (top right)
- E.2415 Handled bottle. Abydos, tomb E178, c. Amenophis II. Garstang, *El Arabah*, pl. xix, 2 (centre)
- E.2427 Figure-vase. Abydos, tomb E178, c. Amenophis II. Garstang, *El Arabah*, pl. xix, 4
- E.2416 Pot. Abydos, tomb E178, c. Amenophis II. Garstang, *El Arabah*, pl. xix, 2 (middle left)
- 1913.394 Jug. Abydos, tomb D114, c. Tuthmosis III. Peet, *Cemeteries of Abydos*, III, pl. xii, 4 (bottom row, right of centre)
- 1923.533 Handled bottle. Qâw; in shaft of tomb 842, Second Intermediate Period or later. Brunton, *Qau and Badari*, III, pl. xvii, 90 h

Analyses

Museum No.	Provenance	Mg	Ca	Al	Fe	Na	Ti	Cr	Mn	Ni
1890.822	Lahun, Maket tomb	4.0	5.6	14.7	10.0	1.7	1.3	0.084	0.095	0.022
1921.1436A	Sidmant 53	3.8	15.2	11.6	9.9	1.2	0.80	0.072	0.079	0.061
1909.130	Rîfa 20	3.7	17.5	12.2	8.1	1.7	0.84	0.069	0.11	0.032
1921.1290	Sidmant 263	3.7	12.2	13.1	11.2	2.1	1.4	0.048	0.058	0.045
1921.1291A	Sidmant 263	2.8	8.5	10.7	10.4	1.8	1.4	0.050	0.048	0.087
1921.1279	Sidmant 263	3.2	6.4	14.1	13.8	3.4	1.7	0.070	0.11	0.11
1921.1282	Sidmant 263	3.3	4.9	14.7	14.2	2.6	1.7	0.050	0.13	0.040
E.2408	Abydos E178	3.2	9.4	12.7	10.6	1.4	1.4	0.043	0.13	0.029
E.2415	Abydos E178	3.1	5.8	12.5	10.6	2.7	1.5	0.046	0.14	0.077
E.2427	Abydos E178	4.0	25.2	11.2	7.3	1.5	1.1	0.044	0.081	0.032
E.2416	Abydos E178	4.2	22.2	14.0	11.1	1.2	1.2	0.056	0.11	0.058
1913.394	Abydos D114	4.5	23.8	10.6	10.3	1.6	1.6	0.056	0.11	0.037
1923.533	Qâw 842	4.0	23.2	10.2	8.1	1.7	0.99	0.037	0.058	0.090

¹ E. g. J. L. Myres in *El Amrah and Abydos*, 72-75; E. R. Ayrton, C. T. Currelly, A. E. P. Weigall, *Abydos*, III, 50; W. M. F. Petrie, *Gizeh and Rifeh*, 23; M. A. Murray, *Historical Studies*, 41; R. Englebach, *Harageh*, 18.

² This was most kindly done by Mr. J. B. Hennessy.

Results

The two imported Mycenaean III vessels, 1909.130 and 1921.1436A, are very close to Dr. Catling's Korakou group, which is one of the source groups making up his Peloponnesian type A.¹ The Mycenaean II alabastron, 1890.822, is not comparable with any previously analysed ware.

The remaining specimens selected can all be readily distinguished from all Dr. Catling's groups. The figure-vase, E.2427, typical of the class of light red, fine ware, figure-pots of the Eighteenth Dynasty, is close in composition to the very common local pot E.2416, and to the jug, 1913.394. The coarser ware, low calcium, pots and figure-vases are generally alike in composition, but are too variable for further comparison on the numbers analysed.

JOAN CROWFOOT PAYNE

E. Towry Whyte's List of the Kings of Egypt

EDWARD TOWRY WHYTE graduated from Pembroke College, Cambridge in 1870. He was an architect by profession, but held a strong interest in archaeology and more particularly in Egypt, where he travelled. He was elected a Fellow of the Society of Antiquaries in 1894 in recognition of his interest, and served on the council of that Society in 1901. He was the author of papers on Egyptological subjects in the *Proceedings of the Society for Biblical Archaeology* and elsewhere.

His main work, however, was never published. This was his book, *A List of the Kings of Egypt and Roman Emperors*, the manuscripts and notes for which passed recently into the hands of the Merchant Taylors' School Archaeological Society. The society has now presented them to the Library of Egyptology, Cambridge, to make them generally available.

The work is based on Lepsius's *Königsbuch*, with the addition of later material and an alphabetic index. The final manuscript copy of the book is dated 1902. It has 207 pages of references and transcriptions for Pharaohs, the Ptolemies, Roman emperors, and Ethiopian kings.

The book was advertized to be printed as soon as 150 copies were ordered. Publication however, was never realized—presumably because orders were not forthcoming. In 1908 Budge's *Book of the Kings of Egypt* was published in England, following the first volume of the more satisfactory *Livre des rois* by Gauthier in 1907. Whyte's work was now not needed. Nevertheless, a comparison of Whyte's and Gauthier's work reveals a number of omissions from the latter—about fifty entries of kings or their relatives in the first eleven dynasties alone. Thus not only is Whyte's work a memorial to the achievements and interests of the author: it could also well be of use in a revision of Gauthier's work, if such were undertaken.

R. M. DERRICOURT

No Ramesses III funerary estate in Pap. Louvre E 7845A

IN a brief communication in *JEA* 50 (1964), 180-1, Bryan G. Haycock compounds an error of mine and makes a wholly unwarranted deduction as a result. He not only removes my indications of doubt about reading 'Wsr-mꜣꜥ.tꜣ-Rꜥ' in a cartouche four times in a demotic land lease of Year 17 of Amasis, Pap. Louvre E 7845A, ll. 4, 5, and 6 (*Saite Demotic Land Leases*, pl. iii, pp. 28-29 and 36, § p), but indicates that final signs in the cartouche can additionally be read *mry-Tmn* with little difficulty as 'semi-hieratic'.

The correct reading of the whole is that given by Michel Malinine in *Revue d'Égyptologie*, 8 (1951), 136-8 and 140, (j). It is *dwt(t)-ntr*, 'the god's adoratrix', written Ⓞ Ⓜ Ⓝ Ⓜ in a cartouche as

¹ H. W. Catling, E. E. Richards, A. E. Blin-Stoyle, *op. cit.* 104-9. 1909.130 falls into the Korakou group except for a slightly high Na, which is, however, inside the total range of Peloponnesian type A; 1921.1436A fits except for high Ni, which can be explained by contamination in preparing the sample, a difficulty which has since been overcome.

was customary in the case of this title.¹ The writing is hieratic, not demotic, and as such the reading needs no demonstration beyond noting, as Malinine does, that the initial ☉ (or simply a dot) is abusive, as it not uncommonly is in names in cartouches in New Kingdom hieratic.

The god's adoratrix from the death of Nitocris in Year 4 of Apries to the Persian conquest under Psammetichus III, and thus throughout the reign of Amasis, was 'Ankhnesneferibrē daughter of Psammetichus II, and the high steward of her estate² was Shoshenq son of Peteneith.³ The lands mentioned in this lease as bounding the two plots of leased land were in her estate and under the over-all supervision of her high steward.

In any case, there is nothing in the lease that has the slightest bearing on late survival of New Kingdom funerary cults and their endowments.

GEORGE R. HUGHES

A fragment on the Ptolemaic perfume monopoly

(P. Lond. Inv. 2859A)

So little is known about the perfume monopoly in Ptolemaic Egypt⁴ that it may be desirable to publish the present exiguous fragment, acquired by the British Museum in 1930. This fragment apparently comes from an official notification from the farmers who had contracted for the perfume monopoly for the entire Arsinoite nome, including its metropolis, Ptolemais Euergetis (here carefully distinguished from the territory of the nome) addressed to the local concessionaire at Philadelphia. The writing suggests to me a date in the last century of Ptolemaic rule, and this is confirmed by the appellation Ptolemais Euergetis given to the metropolis. It was long ago shown by Grenfell and Hunt that this designation replaced the ancient name of Crocodilopolis towards the end of the second century B.C. In fact, while I have made no exhaustive search, the latest documents in which Crocodilopolis is so named appear to be P. Teb. 24, of 117 B.C., P. Teb. 113, of 114-113 B.C., and P. Teb. 740, of 113 B.C., whilst the earliest occurrences of the name Ptolemais Euergetis are in P. Teb. 14 and P. Teb. 26, both of 114 B.C. The change of name would thus appear to have been made some two years after the death of Ptolemy Euergetes II on 28 June 116.⁵ The twenty-second year mentioned in l. 3 of the present fragment must therefore be referred either to the reign of Ptolemy X Alexander and Cleopatra Berenice or to that of Ptolemy XII Neos Dionysos (Auletes), and the resultant date either 93-92 B.C. or 60-59 B.C., with perhaps a slight preference for the former.

The letter is addressed to the local concessionaire for perfume selling at Philadelphia, Seos, and confirms the grant to him of the (sole?) right to manufacture *μύρον* at Philadelphia. The general situation thus recalls P. Fay. 93 = Wilcken, *Chrestomathie*, 317, of 161 A.D., an application to lease a share in the perfume monopoly, addressed to one of the major contractors; this application bears a subscription by the major contractor, granting the application, and we may perhaps assume that this grant would have been embodied in a formal document resembling the present papyrus.

Once again, therefore, the procedure during the Roman period would appear to be a continuation of Ptolemaic practice; and it is noticeable that the technical term *ἐπιχωρεῖν*, used to denote the

¹ Cf., e.g., Pap. Abbott (B.M. 10221), 1, 6; 2, 5; 3, 17; 4, 7; 7, 4; but I cannot cite an instance of the title with the abusive ☉ at the beginning.

² That is, the *ꜥꜣ n pr n pr dwꜣ(t)-ntr n Imn-Rꜥ nsw ntrw*, to give him the complete title found in Pap. Abbott 2, 5; 4, 7; 7, 4.

³ Cf. Lichtheim, *JNES* 7 (1948), 165 f.; Kees, *Priestertum*, 266 ff.

⁴ Cf. C. Préaux, *L'Économie royale des Lagides*, 366-70; M.-Th. Lenger, *Chronique d'Égypte*, 23 (1948), 113-19; C. Préaux, *Chronique d'Égypte*, 28 (1953), 109-10.

⁵ Presumably the renaming was in honour of Ptolemy Euergetes II, though it is difficult to imagine why anyone should have wished to perpetuate the memory of this monarch.

granting of such a sub-contract in papyri dating from the early years of Roman rule,¹ here occurs, for the first time in this context, in a document of Ptolemaic date.

P. Lond. Inv. 2859A.

7·4 × 10·5 cm.

· · · · ·
] . . .
] γ
 [. .] φορτίων Πτολεμαίδος
 Ἐνεργείδος καὶ τοῦ νομοῦ
 5 τ[ὸ] κβ L Σεῶτι μυροπό(λη) Φιλα(δελφείας)
 χ[α(ίρειν).] ἐπικεχωρήκαμέν σοι
 μυρεψίν καὶ μυροποεῖν
 καὶ . [. .] . . .

1-3. Presumably the papyrus had something like οἱ ἐξειληφότες τὴν διάθεσιν τῶν ἀρωματικῶν φορτίων, although there is scarcely room for [-κῶν] at the beginning of l. 3.

7. μυροποεῖν. Apparently an addition to the Greek lexica.

T. C. SKEAT

¹ The earliest Roman examples known to me are P. Mil. 6, of A.D. 25, and P.S.I. 692, of c. A.D. 52-53.

REVIEWS

Die Inschriften der ägyptischen Frühzeit. By PETER KAPLONY. Ägyptologische Abhandlungen, ed. Wolfgang Helck und Eberhard Otto, Bd. 8. Wiesbaden, 1963. Pp. xxxv+1232, pls. 154. Supplement, Bd. 9. Wiesbaden, 1964. Pp. vii+47, pls. 32+photo. pls. 12. No price quoted.

Sixty-five years ago Griffith, in the preface to his notes on the early-dynastic inscriptions found by Petrie at Abydos, wrote: 'Progress in the reading of these very archaic inscriptions is likely to be slow.' Apart from some of the titles he considered their interpretation 'so impossible or so hazardous, that one is unwilling to venture a guess as to the meaning'. No one could deny that in the intervening years some progress has been made, but far less than in the understanding of texts dating from the subsequent periods of Egypt's history. It is against this background that the present book must be judged, and if its length and mass of detail may daunt many a would-be reader it must be remembered that the subject with which it deals is complex and a vast amount of reasoning and justification cannot, at this stage in our knowledge, be avoided. Since Griffith's initial studies the quantity of material available has been enormously increased, in no small measure by the excavations of Professor Emery in the early-dynastic cemetery at Saqqâra. Moreover, Dr. Kaplony does not confine himself to the texts of the first two dynasties, but includes those of the Third Dynasty on the ground that they belong in character to the earlier period rather than to the Old Kingdom. This division, which requires no elaboration, has the practical advantage of enabling him to use the inscriptions on vessels found in the subterranean magazines of Zoser's Step Pyramid, some of which are not closely datable, although manifestly they cannot be later than the time of Zoser, while others are indubitably assignable to the First and Second Dynasties.

The general plan of the book is logical and easily intelligible. Cylinder seals and their impressions provide the bulk of the inscribed material hitherto recovered from the early-dynastic monuments, and consequently their study is given pride of place; it occupies not only 175 pages of the text volume but also a further 200 pages of notes which are relegated to the second volume, an arrangement which is dictated by their extent. They are treated first according to the pattern of their decoration (single-line seals, seals of two or more lines, etc.) and secondly, within this general classification, according to their inscriptional content. In the latter respect they are divisible into nine categories to which the author gives the following names: Königs-, Prinzen-, Amts-, Verwaltungs-, Beamten-, Privat-, Kollektiv-, Fest-, and Tiersiegel. To quote the simplest of examples, seal-impressions consisting merely of the Horus-name of a king repeated perhaps several times in rows are Königssiegel within the general classification of row-seals (Reihensiegel). This classification, while not being necessarily indicative of date, may have a broad chronological significance: the row-seals bearing the king's name alone occur only from the earliest times until the end of the reign of Den. In most cases the student, if he can understand the inscription, can place a seal in its correct category, although the distinction between Amtssiegel and Verwaltungssiegel is somewhat fine: Verwaltungssiegel contain the names of buildings and localities as well as a title but not a royal name, whereas Amtssiegel, if they refer to a temple, palace or a branch of the administration, give the name of the king. The difference between the Privatsiegel and the Prinzensiegel is that the former give only personal names, but the latter give the Horus-name of a king and a personal name without a title. Kollektivsiegel in some instances imply filiation (although the relationship is not expressed); whether they do so or not they regularly indicate that the single title mentioned was borne by all the persons named, who may be as many as six or seven. Seals showing the 'lion and tent' design, of which he gives 37 examples, belong with only three exceptions to the Beamten-siegel because, in addition to the Upper Egyptian tent and its protecting deity, the lion with the curved bars, they mention the name of the person who was in charge of its administration; the latest examples date from the reign of Den.

One class of cylinder seal which the author places in a special category, while showing that in design it is

essentially not different from the other classes, is the seal which displays either the bird with its head turned backwards or the human figure seated at an offering-table; occasionally both the bird and the figure occur on the same seal. With only two exceptions (figs. 719 and 720) no impressions of these cylinders are known, the reason being that they were not used in daily life but were part of the funerary equipment. Both the bird and the figure at the offering-table represent titles of the dead written in a specialized way; the former, as Petrie suggested, represents the *iḥ*-bird and the latter probably the hieroglyph *šps*. Of particular interest are his comments (p. 40) on the changes in the details of representation of the figure and the offerings, which provide some indication of chronological order, and the later instances of this same group on the rectangular stelae. Also valuable is his analysis (pp. 41-45) of the criteria available for determining the probable chronological limits of the various inscriptional patterns on cylinders.

The second chapter of Part I is devoted to a statistical survey of the seven types of clay jar-stoppers according to their shapes and their inscriptions. All the major tombs from the earliest in Cemetery B at Abydos to the Third Dynasty mastabas at Beit Khallâf are examined individually and in chronologically related groups, the number of impressions of each class (royal, official, etc.) on the various types of stopper being listed at the beginning of the discussion of each tomb. The conclusions to be drawn from the statistics and observations on the relative occurrence and the significance of the different classes of seals are either interspersed between the nine sections (125 pages) into which the chapter is divided or appended to the accounts of the individual tombs. Not surprisingly there emerge some important deductions concerning the evolution of economic and political institutions during this early period and the parallel development of offices associated therewith.

In the second part of the book the author turns to (a) simple stelae and (b) rectangular stelae showing the offering-table scene and offering-lists. He begins with an index of all the examples of stelae, private and royal, which he has been able to trace. 258 of the private stelae, complete or fragmentary, come from the archaic cemetery in Abydos, 7 from the tombs of the courtiers and the forts, 8 from Abû Rawâsh (all from the time of Den), and 1 (the stela of Mery-ka) from Saqqâra. Two further indexes, the first comprising four entries including the two stelae of Meryt-Neith and the second comprising the nine stelae of kings, are given in volume II (pp. 892-5). In addition to the name of the owner the stelae of women may or may not include a title, and generally the name is followed by a representation of a seated woman as a determinative; the stelae of men may include both title and male determinative or lack one or both of these adjuncts. Stelae of men inscribed with the name only (i.e. without title or determinative) and stelae of women without title but with determinative occur throughout the First Dynasty. Stelae with a male determinative and either with or without a title and stelae of females with a title date mainly from the time of Den. Instances of stelae of men with title but no determinative occur from the time of Djet (Wadj) in the tombs of the courtiers but not in the royal tombs until the reign of Semerkhet. A complete analysis of the variants, together with the stelae of dogs and dwarfs, is given in tabular form (p. 224).

The rectangular stelae afford far more material for study than the simple stelae but, in contrast with the latter class, not one of the forty-three examples of the rectangular stelae now known (and listed at the beginning of this section) is dated to a particular reign, so that their chronological sequence must be deduced from internal evidence. The chief criteria are the different methods of representing the seat and the legs of the figure at the offering-table, the number and the form of the loaves of bread on the table, the position of the arms of the seated figure, the direction of the writing, and to a limited degree the numerals which accompany the offerings in the so-called 'magazine-list'. The earliest example, the stela of *Twr-irty-Ḳnpw-Nt* found at Helwan, shows the deceased seated at an offering-table furnished with bread only, while meat and drink are represented above. Later, the offerings on and near the table are supplemented by a list of commodities mentioned by name and constituting the 'magazine-list'. By the early Old Kingdom, from which time thirty examples of the offering-table scene are known, the magazine-offerings are confined to the names of one or more of four classes of commodities set out in separate registers: fabric (included in every case), corn, cattle and fowl, and household articles such as furniture and oil. Although the offering-lists in the archaic stelae are more simple and abbreviated they follow certain rules and it is possible, by studying the changes in their composition, to establish some reliable criteria for determining their chronological sequence. The constituents are bread, liquid beverages, corn, oil-vessels, and fabric. A hundred pages (242-342) are devoted to a most detailed study of these constituents, the different kinds which are represented, their

identification by name and an analysis of their order and arrangement in the various lists. From this study the stelae can be divided chronologically into three main groups, the earliest (14 and probably 15 examples), a middle group (16 examples), and the latest group (8 examples), thus leaving only 4 stelae, all damaged, unplaced. The earliest and the middle groups are capable of further chronological classification, each into three or four subdivisions, the second and third subdivisions of the earliest group being dated approximately to the end of the First Dynasty.

In spite of the very substantial increase in the number of stelae now known the evidence is still not sufficient to pass judgement on the conflicting theories put forward many years ago by Junker and Scharff. While it tends to confirm Junker's view that the simple stela developed into the rectangular stela it does not rule out Scharff's contention that the simple stela represented the Upper Egyptian counterpart of the Lower Egyptian rectangular stela. Another problem which remains unanswered is whether the size of tomb bears any significant relationship to the rank of the owner. When the plans of the tombs at Helwan, which yielded more than half the extant rectangular stelae, are published the material available for such a comparison will be greatly augmented. In the meantime much can be learnt about the various classes of persons who possessed tombs at the beginning of Egypt's dynastic history from the author's study of their titles and the offices which they held.

One of the most useful features of this book is the exhaustive index of names which forms the last part of the text volume (pp. 379-672). It may justly be described as a commentary on the persons indexed, for it includes not only a discussion on the name itself and references to the objects on which it is found, but frequently also genealogical details and lists of the titles attached to the names. Further indexes of titles and place-names and other proper names are given at the end of the volume of notes.

Of the many difficult problems which the author tackles perhaps none will command more general interest than his views on the location of the royal tombs of the First Dynasty. He leaves no room for doubt that he regards the tombs at Abydos as the only sepulchres of the kings and of Queen Meryt-Neith. The Saqqâra mastabas in his opinion must be assigned to princes and officials whose names occur on the seal impressions and other inscribed objects found among their contents. The Naqâda mastaba he believes is not the tomb of Queen Neithotep or of Menes but the tomb of the prince whose name is written on the sealings with three birds, perhaps to be read *Rhyt*. His arguments in favour of locating the royal sepulchres at Abydos are stated briefly in Volume II, p. 895, n. 1161, but the reader must also turn to his discussions on the individual Saqqâra mastabas to obtain a complete picture of his contentions. In a problem which has received so much attention from previous writers it is inevitable that most of the main elements should already be well known and decisions must rest in the final analysis on the importance attached to conflicting pieces of inscriptional, archaeological, and historical evidence. Dr. Kaplony has however introduced one new consideration which deserves mention, namely that the Abydos tomb of Den has yielded some later seal impressions than those found in the Saqqâra mastabas of his reign. Since chance plays so important a role in survival it is difficult to assess the value of this observation, but it certainly cannot be ignored.

It will be clear that early-dynastic studies have advanced appreciably as a result of Dr. Kaplony's exhaustive researches. A mere glance at the readings of the inscriptions in the volume of drawings, as interpreted in the list of captions in Volume II, will show how much he has been able to add to the understanding of these difficult texts. Throughout the book, and particularly in the volume of notes, textual puzzles which have hitherto defied all attempts at solution are submitted to the most detailed scrutiny and often he is able to offer explanations which carry conviction. To quote one only of the very many examples of his treatment of such problems, the hieroglyphic group consisting of three or four upright bars (sometimes with upright ovals as a variant) is shown (pp. 104-6 and 764-5) to possess the reading *nhb* and *nhn* according to varying circumstances. By its nature, however, the subject of this book is somewhat intractable, but the author could perhaps have done more to help the scholar who wishes to use the work mainly for reference purposes. His various indexes are indeed admirable, but it would have been most useful if he could have added an index of collections and also a sign-list. In order to locate his treatment of an inscribed object with a collection number it is often necessary to find the copy of the inscription in the volume of plates, turn to the list of captions in Volume II to obtain his reading of the owner's name (which sometimes differs from the reading hitherto accepted, e.g. Sekhty for Djer and Zekhen for Ka) and then consult the index of names for the page-reference. His numbering of the texts in the plates is not always easy to see at a glance because of

inconsistency in the position of the numbers; with a little ingenuity he could have devised a fixed position for the number in relation to the text. A more general criticism, which is certainly not applicable to much of the book, is that he could have made it easier to understand some of his views if he had summarized his final conclusions at the end of his discussion. It is a pity that he has not considered the annalistic labels suitable for inclusion, although he gives a concise index of the known examples in Volume II, pp. 980-4; perhaps they could form the subject of a second supplementary volume, the first having been devoted chiefly to material which he gathered during a visit to Egypt in 1963. For the most part the faults of the book are those of method; as the first major work of a young scholar it is a real achievement. I. E. S. EDWARDS

Accounts of the Dockyard Workshop of This in the Reign of Sesostris I. Papyrus Reisner II. Transcription and Commentary by WILLIAM KELLY SIMPSON. Museum of Fine Arts, Boston, 1965. Folio. Pp. 60, pls. frontispiece+24 collotype+24 lithographic (transcriptions). Price not given.

Papyrus Reisner II is a slender volume in similar format to Papyrus Reisner I. It contains the second of four papyri found at Nag' ed-Deir in 1904 by the Hearst Egyptian Expedition of the University of California and is reproduced at full size in a series of collotype plates each one companioned by a plate giving the transcription of the hieratic text. The original is sadly tattered and occasionally so blurred that it is impossible to read the otherwise excellent facsimile.

The plates are set at the end of the book and are preceded by sixty pages of introductory matter. This includes an analysis of the contents of the papyrus, but there is no continuous translation, only important headings and characteristic passages being rendered. Studies are made of the technical terminology, of officials, of geographical designations, and other symbols. There are tables illustrating the forms of the hieratic signs in so far as these are needed to supplement those already printed in the previous volume. Two beautiful photographs of Middle Kingdom model carpenter's tools form the frontispiece.

Since the vizier, Inyotef-oḳer, is named several times, this together with certain other evidence clearly fixes the years 15 to 18 with which this papyrus is concerned to the reign of Sesostris I. Both the recto and the verso are inscribed. The contents, which begin on the verso, revolve round the activities of the royal dockyard workshop at This and are divided by Simpson into twelve sections lettered from A to M. In addition there are five fair-sized fragments which almost certainly belong to the beginning of the papyrus. The entries, as in the case of Papyrus Reisner I, are not in any logical order. I think that the only possible explanation is that the scribe used the papyrus to assemble material prior to making fair copies in the official records of his department.

Sections A, B (in part), C, F, H, J, L, M make up a major section and comprise accounts concerned in the main with copper in the form of carpenter's tools and, in a lesser degree, with hides used to make binding material to attach the tools to their handles. The copper is being received and issued in the dockyard workshop, where the coppersmiths appear to have been remelting old copper tools to make new ones and probably sharpening and refurbishing generally. Although the accounts are entitled as of copper, it is as numbers of tools that they are totalled, except in the one case of section H where the weights are added instead. These weights appear as entries in red alongside the numbers of tools. The unit involved is not known, but the various tools have characteristic weights which accord proportionately with their sizes (e.g. axes 50, 40, 30, 20, 10; adzes 50, 40, 15; chisels 20, 14; etc.). Simpson tries to equate the unit with the *deben*, however, without very much success, as he himself confesses. Many tools are also provided with single hieratic signs in red which seem to be abbreviations for the names of certain towns. Simpson demonstrates that these abbreviations refer not to the tools but to the individuals named with the tools. As, however, a single individual can thus be connected with more than one town no simple explanation suggests itself.

It seems to me that in commenting upon these accounts Simpson allows himself to be too preoccupied with the word 'copper'. Of course the accounts are of copper, for that is what is being handled, but it is in the form of tools and in reality the accounts are tool accounts. Throughout Simpson translates D as 'units', R as 'hide units', and W as 'units (wood)'. In all cases I think that the sign is best rendered as 'tools', 'hides', or 'wood' as the case may be.

A considerable portion of the verso is taken up by section K, which is five pages long and contains a register of the royal dockyard running from Year 15, first month of Winter, day 21, to Year 16, first month of Inundation, day 3: a period of 228 days. Opposite a vertical column of consecutive dates each page has a series of columns containing figures. Unfortunately the column-headings are almost totally destroyed, but it would seem that the entries must represent the numbers of men working in various groups on the individual days. At the end of the fifth page there is a summary in which the monthly totals are set out and added together. The over-all totals thus obtained are used to compute amounts of bread, beer, barley, *bš*, and dates, and these in turn to calculate final quantities measured in grain. I believe that the final commodity, left unread by Simpson, must be *bnr* 'dates'. Clearly this is a register accounting for rations that have been issued. I feel it necessary to comment on several points in this section and to indicate certain slips that have crept into Simpson's commentary. This, I think, best done by giving a modified version of his final tabular statement (p. 33, col. 1, § 3) together with the necessary notes and corrections:

<i>Man-days(?)</i>	<i>Bread Loaf Units</i>	<i>Beer Units</i>
(5)96 ¹	5960 (10 × 596)	392
1718 ²	34360 (20 × 1718)	1718
130 ³	1970 (15 × 133 = 1995)	66½ (½ × 133)
88	880 (10 × 88)	44 (½ × 88)
163 ⁴	1630 (10 × 163)	—
181	3620 (20 × 181)	181
50 ⁵	980 (20 × 49)	50
6	60 (10 × 6)	—
45	900 (20 × 45)	45
18	180 (10 × 18)	—
Total	50565 (Correct fig. is 50540)	2495½ ⁶ (Correct fig. is 2496½)

1 Simpson says that he cannot ascertain any source for the figure 596. It occurs to me that it might be made up thus:

One year and three days	= 365+3 = 368 days
I Winter 21 to IV Summer 30 and three days	= 225+3 = 228 days
	Total 596 days.

It would seem that to the 225+3 days of section K the previous year+3 days has been added. Why I do not know.

2. I do not know why Simpson splits all the figures in this line into two. It seems to me that the scribe intended 1718, 34360, 1718. This figure of 1718 is then very close to the correct total of 1719 which Simpson erroneously gives as 1739.

3. Simpson has 65½ for the third figure in this line although the facsimile and transcription have 66½ which is correct. It will be further noticed that the scribe is operating with the correct 133 instead of the erroneous 130 which he had written at the beginning of this line.

4. The scribe's 163 is correct although Simpson says it should be 133.

5. Here again the scribe has operated with the correct total of 49, although only in the case of the second figure, despite his use of an incorrect 50 elsewhere in the line.

6. The facsimile and the transcription have 2495½ which is very close to the correct figure of 2496½. Simpson is in error in giving 2295½ and in saying that the correct figure is 2466½.

Copied down in the midst of these accounts are three orders (Sections D, E, G) sent by the vizier Inyotef-oker to the Stewards of the Palace in the Thinite Nome. The scribe has evidently felt these to be important enough for him to need to retain personal copies at least for his own office. The meaning of the orders is obscured by unknown words and lacunae. Only the third order contains anything that really seems important in that it includes a statement that if certain craftsmen happen to be requisitioned and the vizier is informed he will secure their release. Apart from this the orders, two of which are dated to successive days in the second month of Inundation in Year 17, are concerned with the transport or preparation for transport of various supplies, with the obtaining of crews for *imw*-boats and of a slave-girl, and perhaps with work on

boats. I would suggest that the curious idiom, 'see that you shave yourselves', 'let him be shaved', which occurs in the second order is equivalent to 'see that you bestir yourselves', 'let him bestir himself'.

In addition to the matters so far mentioned the papyrus contains an account concerning ships and ship's gear in the first part of Section B and a fragmentary pottery account on the verso of fragment 2.

Simpson confesses that the meaning of the accounts in this papyrus is still far from clear and that he realizes that his transcription is capable of improvement. He has, however, made a valiant pioneer effort with a difficult and tedious task.

C. H. S. SPAULL

From the expulsion of the Hyksos to Amenophis I. By T. G. H. JAMES. Vol. II, ch. viii of the revised *Cambridge Ancient History*. Pp. 30. Cambridge University Press, 1965. Price 3s. 6d.

Mr. James's contribution to the revised edition of the *Cambridge Ancient History* is a succinct and straightforward account of one of the most baffling periods in Egyptian history. His narrative includes a description of the text of the Kamose stela, discovered at Karnak in 1954, which is very valuable since the stela has not yet been adequately published. Recent work has also contributed greatly to our knowledge of events in Nubia, and a summary of this forms a section of the chapter.

The chapter is made up of a discussion of the campaigns of Kamose and Amosis in Egypt and Nubia followed by a description of the re-establishment of the royal administration in both countries. From extremely fragmentary sources Mr. James has succeeded in writing a coherent and up-to-date account. His narrative is however rendered a little one-sided by its brevity (23 pages of text and 4 of bibliography). Certain controversial problems have as a result not received as full a treatment as one would wish in a standard work. For example, the reference on p. 5 to the 'obscurity of language' in the Kamose stela which 'hinders a precise determination of whether much that is said refers to actual events in the past, or to the king's boastful intentions', is disappointingly vague since any historical reconstruction must be based upon interpretation of this text. Other problems which perhaps deserved fuller discussion are the southern boundary of Hyksos rule, and the adoption of the title 'God's Wife of Amūn' into the titulary of the queens of the early Eighteenth Dynasty, the latter particularly since a special section is devoted to the three famous queens of the period.

A certain lack of balance has been unavoidable since Mr. James has had to confine himself to a consideration of the Egyptian textual evidence for this period, the archaeological source material for the Second Intermediate Period having been already discussed in Volume II, chapter ii, while an account of the contemporary situation in Palestine and Syria, which is so vital for an understanding of events in Egypt at this time, has been left for a later chapter. It is also inevitable that a history of this period should appear unbalanced to some degree, since in order to write a narrative at all the historian still has to rely mainly on the surviving texts, which emanate exclusively from the Theban kings and their followers. Apart from these texts we know practically nothing of events in Egypt outside the Thebaid. We cannot, for instance, judge with any certainty how much, or of what kind was the opposition to the conquests of Kamose and Amosis. The absence of any historical material from the Delta is particularly important in this context. Mr. James points out the limitations of his sources and altogether his account provides a useful introduction to a most confusing period.

JANINE BOURRIAU

Amarna Reliefs from Hermopolis in American Collections. By JOHN D. COONEY. The Brooklyn Museum, 1965. Pp. x+110. 2 colour plates, 77 plates, 2 text-figs. Price \$8.00.

In the spring of 1939, towards the end of their tenth season at Hermopolis, the German Expedition under Günther Roeder uncovered in the foundations of a construction of Ramesses II over twelve hundred limestone blocks sculptured with reliefs of the 'Amarna Period'. This enormous haul could not be fully photographed and copied in the time remaining, and the Germans had to content themselves with hurriedly storing the blocks before they broke camp. The war prevented the Expedition from returning to the site and

their excavations have not since been resumed. During the war, when supervision was lax, the inevitable illicit digging took place, particularly by natives who had gained a shrewd knowledge of the German operations, and who not only uncovered again some of the hastily stored finds, but also brought to light fresh blocks in contiguous areas on the site. By 1942 some of the 'Amarna reliefs, now sawn into slabs and tricked out in modern pigments, were coming on to the market; and by the end of the war when purchasers of Egyptian antiquities were once more in full cry, the bulk of the plunder was in the hands of one dealer. It is from this source that some hundred reliefs have made their way to America and have been acquired by public and private collectors; others still await the buyer with the necessarily deep purse.

It is difficult to exaggerate the importance of these reliefs since although like the Karnak *talatates* very few are contiguous, they give us a glimpse, tantalizing in its incompleteness but nevertheless rewarding, of the temple sculptures of El-'Amarna which apart from some miserable fragments are entirely lacking on the site of the 'Heretic Capital' itself. For it is now clear that the abandoned city of Akhenaten remained virtually intact until the reign of Ramesses II when it was used as a quarry for constructions in the vicinity, at Hermopolis across the river, Asyüt further south, and elsewhere. It is in the foundations of early Ramesside buildings, in fact, that search will have to be made if the lost temple reliefs of El-'Amarna are to be recovered in part at least. The importance of the Hermopolis finds is that they provide an iconography for the 'Amarna period which has hitherto been entirely lacking in the absence of any systematic publication of the Karnak *talatates*. Until now we have had to depend upon the tomb reliefs at El-'Amarna with their circumscribed subject-matter, uneven execution, and poor state of preservation as virtually our only means of assessing the features of the age.

The timeliness of Jack Cooney's study, therefore, does not need to be stressed, and if it were no more than a picture-book of the newly discovered 'Amarna reliefs in American hands it would be heartily welcome. It is, of course, much more than this. There is no one better equipped to deal with these sculptures from their iconographical and artistic aspects than Cooney. He has spent a good deal of his time as an active and perceptive collector of Egyptian works of art examining 'Amarna and pseudo-'Amarna pieces: his eye misses nothing of importance and his judgements are supported by an extensive knowledge of the literature. Anyone who reads this book with close attention to the *obiter dicta* will learn much about 'Amarna iconography (e.g. the different appearances of the long robes worn by Akhenaten and Nefertiti, p. 23), as well as neglected aspects of the 'Amarna style (e.g. the management of space, p. 52), and will profit much from Cooney's explanations of the enigmatic, fragmentary, and unique details. His interpretations are at once sensible and ingenious.

The book is handsomely produced in a large but manageable format and consists of an introduction in which Cooney outlines the recent history of the pieces, and a historical summary in which he sketches in the background with admirable impartiality, though he is known as a firm adherent of the view that there was no coregency between Akhenaten and Amenophis III. Then follows the *catalogue raisonné* of seventy-three pieces of 'Amarna relief and one portion of Ramesside date from Hermopolis. In addition a torso from one of the hard limestone statues of Akhenaten, formerly in the Amherst Collection and now in the Brooklyn Museum, is published. Each entry gives details of the owner, dimensions, colour-traces where these have survived, and a bibliography. The subject-matter has been grouped into various sections such as the Royal Family, the Court Circle, Horses and Chariots, Other Animals, Musicians, Scenes of Daily Life, Flora, and Architecture. This book should now equip the student to form a juster appraisal of 'Amarna art than has hitherto been possible.

It was not to be unexpected that as soon as scenes from temple and palace decoration from 'Amarna came to light we should see subjects which were excluded from the repertoire of those tomb reliefs that have furnished to date almost all our knowledge of the life of the Royal Family and events at El-'Amarna. Large deductions have in the past been made from this lack of evidence. The new, yet traditional, subjects which now stand revealed should put our ideas in better perspective. Thus nos. 50 and 51 showing a kiosk on the royal barge with a decoration of Akhenaten smiting a foe should dispel for all time that 'pacifist' interpretation of the king's direction of foreign affairs. What Cooney has to say of no. 25 (pp. 45-47) also deserves to be studied in this context. Nos. 5, 6, and 21 suggest that traditional scenes connected with the Festival of Min were adapted for use in the Aten cult—another instance of that faithful observance of tradition that underlies so much of the superficial novelty of the king's religious ideas.

But among much that is familiar there are one or two representations that are unique and puzzling, though none is quite so bizarre as a fragment from El-'Amarna itself.¹ What is to be made for instance of the scene on a cabin of the Queen's state-barge (no. 51A), showing Nefertiti in the traditional pose of a Pharaoh smiting the captive foe? Cooney (p. 83) draws inferences from this which at a first view would seem to go a little too far, but which may well prove to be no less than just as more evidence comes to light. Other enigmas may be capable of a less subjective explanation. The 'notched sticks' held in the hand by attendants (no. 55) seem to this reviewer to be the branch-work of formal bouquets which are sometimes to be seen in the hands of bowing grooms who are greeting the king or one of his magnates. In this particular case, one visualizes the bouquets as resembling the stained ivory ointment-spoon in the Brooklyn Museum (no. 42.411), but since the relief is now free of all original paint, the detail of leaf, fruit, and flower has disappeared leaving only the stem-structure which was enhanced by incising (cf. the denuded branches of the tree in no. 49).

It is an unfortunate feature of 'Amarna studies that they are still in such a condition that there is wide latitude for disagreement and challenge between different students of the period. In view of Cooney's expert knowledge not only of the pieces illustrated in this work but also of the two hundred or more other reliefs still in the hands of dealers, it would be rash to question his conclusions. Nevertheless, there are points of detail on which this reviewer parts company with his American colleague. For instance, the epicene nature of 'Amarna representations makes it quite possible that no. 6 is of a woman not a man, and in point of fact this reviewer is tempted to identify the fragmentary features as those of Nefertiti. To a large extent Cooney has been somewhat handicapped by not having access to the still unpublished inscriptions from the Hermopolis *talatates*. Some of these texts unquestionably refer to the last years of the reign and must be coeval with the reliefs they explain.² It is reasonable to believe, therefore, that some, at least, of the scenes were carved towards the end of the reign. The group of about three hundred and fifty reliefs known to Cooney does contain inscriptions, but the references in every case are only to Akhenaten and Nefertiti, and this has naturally suggested to him that the reliefs were carved relatively early in the reign. The difference of style between some of these reliefs has led Cooney (p. 20) to postulate that in the scenes where royalty was represented, subordinate figures were the work of lesser hands. It may well be true that the carving of a relief was carried out by several craftsmen and their apprentices, the most skilled being concerned with the figures of the Royal Family: but the entire preliminary drawing, having regard to the integrated nature of 'Amarna compositions, must have been supervised and corrected by one master craftsman. To the reviewer's eye, three styles of work are apparent in these reliefs. There are, firstly, the exaggerated representations in the early revolutionary manner (e.g. nos. 18A, 18C). Then there are works in the mature and less flamboyant style of the later years of the period (e.g. nos. 3, 3A). Lastly there are a number of reliefs executed in a summary and even slipshod style which may have been conditioned by the difficulties of carving in inferior limestone, but which to this reviewer looks like the work of semi-skilled sculptors recruited in haste to complete the grandiose schemes of Akhenaten in the shortest possible time (e.g. nos. 47, 48, 55A). We therefore place no. 18B, which is known to be of Meritaten and shows her as an adult, in the second category of later carvings in the mature style; and we associate with this no. 13, which seems to represent the same person as the queen, the chin being a little too sharp and the lips too thin for Nefertiti. Similarly it is possible that no. 12 also shows Queen Meritaten, this time embracing her putative daughter Meritaten(-tasherit) who is known to have existed from an unpublished inscription. No. 10, also in the same late style, provides in the inscription mentioning Tia, Nurse of the Princess Ankhes-en-pa-aten, a welcome addition to 'Amarna prosopography. The reviewer is inclined to refer the inscription to the woman represented in the composition. There appears insufficient space between the legs of the larger figure to the left and the inclining figure of the woman on the right for inserting a representation of the royal nurse on all but a preposterously diminutive scale; and it is doubtful whether so minute a figure would have been distinguished by so large a label. Elsewhere the female attendants on the royal daughters are shown equal in stature to their charges. The royal nurses or tutors, as distinct from the royal nursemaids, would almost certainly have been women of high station, like the wife of Ay, and probably related to the Royal Family.

¹ *City of Akhenaten*, III, Pl. Ixix, 8.

² Cf. The inscription published by Brunner, *ZÄS* 74, 104-8.

These points of disagreement, of course, are not vital and some of them may be resolved one way or the other when the larger corpus of reliefs from Hermopolis, which Dr. Roeder is to publish shortly, appears. This can, however, only supplement Mr. Cooney's study which will remain indispensable for any serious student of 'Amarna art, iconography, and history.

CYRIL ALDRED

Abou-Simbel et l'épopée de sa découverte. By LOUIS-A. CHRISTOPHE. Éditions P. F. Merckx, Brussels, 1965. Pp. 275, pls. 34 (unnumbered). Price not given.

Abû Simbel is no longer quite what it was. Its temples are cut up and their rebuilding is well advanced on the heights above their old situation. This move, and all the issues involved in it, have provoked much thought, induced many books, and generated a deal of nonsense among Egyptologists, professional sentimentalists, opportunists who saw in the situation a fine chance of turning a dishonest penny, and even the general public. For many people Ancient Egypt and Abû Simbel are almost coterminous. It is a remarkable and shameful fact that the temples of Abû Simbel have never been the subject of a proper scientific study, and it would be a scandal if no adequate publication of their reliefs and texts emerged from the activity of the last few years.

There is, happily, a good chance that such a publication will eventually appear under the auspices of the Centre of Documentation and UNESCO. These organizations have sponsored a series of specialized studies of the temples, and carried out annual expeditions of an epigraphic nature throughout the years of the Nubian 'emergency'. Much of the credit for the success of the work at Abû Simbel, and indeed for all the work completed in these years in Egyptian Nubia, should go to Louis Christophe who has served as universal adviser to those who have worked in Nubia. Throughout this time the principal focus in Nubia has been Abû Simbel, and Christophe has lived with it constantly; it has been his problem and his inspiration. In this book he renders his thanks lovingly to the temples in the form of a survey of their history since they were first publicly reported in modern times, in 1813, by the Swiss explorer J.-L. Burckhardt. This history is strangely a record of short periods of feverish activity and long periods of neglect on the part of scholars and near-scholars, and of a multitude of curious visitors who marvelled at the temples and left their names carved on one or more of the exposed surfaces of the façades or on the reliefs within.

The story is largely told in the words of the people who worked on or visited the temples, beginning with Burckhardt himself, who was soon to be followed by Belzoni. The latter's account of his visits and attempts to open up the great temple is by far the most entertaining narrative quoted, but it is closely rivalled in interest by the rather more prim descriptions of Amelia B. Edwards whose stay at Abû Simbel in 1874 marked the climax of her visit to Egypt.

From the Egyptological point of view the visits of Champollion and Lepsius in the first half of the nineteenth century, and of Maspero and Barsanti at the end of that century and at the beginning of the present century, were crucial. Those of the former two resulted in the identification of the creator of the monuments and the recording of many of the scenes and texts; those of the latter two were devoted to the final clearing of the façade of the great temple, and the consolidation of the monuments. Christophe reproduces in part the records of their visits made at the time, and also those of many others who travelled in Nubia in the years between. His assiduity in tracking down and identifying the many shadowy figures whose visits are established by the graffiti they left at the site is extraordinary. Even the British soldiers who scratched their names as they passed on their way to fight the Mahdi are here recorded. It is satisfying, however, to learn that the painter David Roberts was 'un vrai gentleman' who did not memorialize himself in the same manner.

It is good to have a record as full as this of all that has happened at Abû Simbel since 1813, but it cannot be denied that most of the accounts here reproduced are of small interest except to the absolute devotee. Few of the visitors have anything original to say, and few are blessed with much literary ability. It is not long before the reader tires of the stupefaction commonly experienced when the newly arrived visitors penetrate the great temple and view the reliefs and halls by torch-light.

The last three chapters of the book are devoted to other matters. In the first of these (vii) Christophe describes the circumstances which led to the setting up of the Centre of Documentation, and the discussions

which preceded the launching of the campaign to save the Nubian monuments, particularly the Abû Simbel temples. Chapters viii and ix contain short essays on various aspects of the temples and certain historical problems. Some of the ideas put forward here will certainly not be readily accepted by many Egyptologists, and in some cases it is to be hoped that Christophe will elsewhere more fully argue them. His contention that the principal deity worshipped in the temples was Ramesses II himself, has much to commend it, although his arguments are not fully conclusive. On the other hand, the belief that the great temple was built to be ready for the celebration of the first *sed*-festival of the king and was orientated so that the rays of the rising sun should strike along the temple-axis into the sanctuary on 20 October, about 1260 B.C., the day of that celebration, is harder to accept. There are difficulties also in following Christophe in his view that Abû Simbel is 'vraiment le site familial où le souverain s'est complu à reprendre, autant qu'il lui est permis de le faire, les idées de Amenophis IV-Akhnaton'. Greatly attracted by 'Amarna ideas, but unable for political reasons to prosecute them in Egypt proper, Ramesses II at Abû Simbel indulged in a demonstration of monotheism in which the solar idea was identified with the person of the king, and also in a display of family attachment in the representation of members of his family in the reliefs and statuary of the temples which is reminiscent of the family scenes and sculpture found at El-'Amarna. To make such a demonstration in remote Nubia seems a singularly ineffective way for a powerful monarch to declare his attachment to the ideas of his proscribed predecessor. Such discretion is uncharacteristic of Ramesses II.

The book is completed with a substantial body of notes and indexes of names only. The plates are interesting, reproducing portraits of many of the visitors and some of the paintings and water-colours of the site made in the last century. A serious omission is a plan of the temples. T. G. H. JAMES

Medinet Habu, vol. VI. *The Temple Proper*, part ii. The Re chapel, the royal mortuary complex, and adjacent rooms with miscellaneous material from the pylons, the forecourts, and the first hypostyle hall. By the Epigraphic Survey. The University of Chicago Oriental Institute Publications, vol. LXXXIV. Chicago, 1963. Folio. Pp. xix, pls. 363-482+coloured frontispiece. Price 215s.

Medinet Habu, vol. VII. *The Temple Proper*, part iii. The third hypostyle hall and all rooms accessible from it with friezes of scenes from the roof terraces and exterior walls of the temple. By the Epigraphic Survey. The University of Chicago Oriental Institute Publications, vol. XCIII. Chicago, 1964. Folio. Pp. xl, key plans in 16 figures, pls. 483-590. Price not given.

These two volumes complete the publication of the temple proper at Medinet Habu. They are similar in all respects to the companion volumes that have already appeared and maintain fully the standards of scrupulous detailed accuracy and clarity of presentation that one has come to expect in this series.

The titles of these volumes, as will have been seen above, are so extensive that in themselves they give an excellent idea of the contents. In the main the complexes of rooms treated are those forming the rear half of the temple. They naturally include the innermost and holiest portions so that it is with particular regret that one contemplates the losses that have resulted from the use of the centre of this part of the structure as a stone-quarry. Some walls are reduced to no more than a course or two of stone.

The highlights of historical, calendrical, and festal texts and scenes having been disposed of in the initial volumes, the humdrum scenes of the king making offerings to various divine beings or being presented to them inevitably make up the main mass of what is depicted now. Nevertheless even here there is much to observe and study, as for instance the series round the inside of the top of the outer walls where, to quote Sir Alan Gardiner, 'the king was portrayed worshipping the deities of a large number of towns and localities arranged in strict sequence from south to north'.¹ There are, however, some texts and scenes of more than passing interest, especially in the Rē^c Chapel and the Royal Mortuary Complex. These include the establishment of the royal titulary (pl. 414), the birth of the sun with the hymn of the baboons (pl. 420), the king adoring the setting sun (pl. 422), a nome-list for Upper Egypt (pl. 452), chapter 110 of the *Book of the Dead* and its vignette (pls. 469, 470, 473), the vignette to chapter 148 of the *Book of the Dead* (pl. 474), and others besides. On plate 478 is published the astronomical ceiling from the Ramesseum for comparison with the fragments of that of Medinet Habu (pl. 476).

¹ *Onomastica*, 1, 53.

Volume VII is equipped with a set of plans and elevations on which are marked the plate numbers where each particular part of the temple is depicted. This enables any part to be located at once and is especially valuable where contiguous scenes do not happen to come on contiguous plates or even, sometimes, in the same volume.

Finally volume VII contains a concordance of the Berlin Zettel numbers with the plates of the *Medinet Habu* epigraphic publications. This most useful piece of work is necessitated by the fact that the Belegstellen use only the Zettel numbers from volume III onwards, and even in part from volume II.

C. H. S. SPAULL

Recherches sur les monuments thébains de la xxv^e dynastie dite éthiopienne. Institut français d'archéologie orientale, Bibliothèque d'étude, Tome XXXVI. By J. LECLANT. Cairo, 1965. Text volume, pp. xlvii+454; 35 text-figs., incl. folders. Plates volume, pp. xv, 88 plates. Price not stated.

This book, the summing up of years of patient and successful research, falls into two almost equal parts. The first (preceded by an introduction and full bibliography) is a compendium of summary descriptions of all the monuments and objects from the Theban area attributable to the Ethiopian Twenty-fifth Dynasty (716-656 B.C.), arranged topographically under forty-nine headings, with exhaustive references which, if followed up, will serve to show how much our knowledge of the period owes to the work of Professor Leclant himself. The second half comprises three masterly studies of the architectural, religious, and historical aspects of the material respectively. A concluding summary chapter is followed by full indexes. Eighty-eight excellent plates (presented in a separate volume), like the figures in the text volume show for the most part architectural features, scenes, or objects unpublished or not readily accessible elsewhere, or illustrate matters discussed in the text.

Despite its short duration, the Ethiopian dynasty which owed its ascendancy to the conquest of Egypt by the Kushite prince Pi'ankhy son of Kashta was vigorous enough to leave its mark upon history. The four Pharaohs who successively represented it, Pi'ankhy's brother Shabako, the sons of Pi'ankhy, Shebitku and Taharqa (the greatest name of the family), and Shebitku's son Tanuatamūn, though of alien origin, as kings of Egypt showed themselves to be pious traditionalists and active builders, particularly at Thebes; but their achievement was later obscured, partly through the animosity of Psammetichus II, of the next dynasty, who usurped some of their best buildings, systematically defacing their cartouches as well as one of the pair of uraei which regularly adorned their portraits. Leclant makes judicious use of the Theban evidence for these kings as rulers of Egypt, observing differences between their personal presentation of themselves to their Nubian and Egyptian subjects respectively. He also presents a detailed account of the line of virgin priestesses, the Divine Adoratrices of Amūn, who enjoyed a position which at Thebes was virtually royal, and, though they were closely connected with the reigning dynasty, was not wholly dependent upon its vicissitudes; thus Shepenwepet I, daughter of Osorkon III of the Twenty-third Dynasty, was not superseded after adopting the sister of Pi'ankhy and Shabako, Amenirdis I; the overlapping succession proceeded with the adoption by the latter of Pi'ankhy's daughter Shepenwepet II, who in her turn adopted Amenirdis II, daughter of Taharqa. The true status of the latter is demonstrated by the author, pp. 363 ff.; he had not, however, at his disposal the article in *JEA* 50 (1964), 71 ff., in which Caminos demonstrates that the adoption of Nitocris, daughter of Psammetichus II, founder of the Twenty-sixth Dynasty, as Shepenwepet III, was by Amenirdis II and not, as Leclant and previous authorities have believed, a second adoption by Shepenwepet III; which is further evidence for the inviolability of their office.

Leclant's account of the religious cults of Thebes under the Ethiopians confirms the accepted view of them as devotees of Amūn, but shows that the ancient prestige of Montu there is by no means eclipsed at this period; on the other hand, the worship of Osiris as god of the living as well as of the dead is seen by now to be well advanced. The author's enumeration of all the divinities figured in Theban monuments of the time, their titles and attributes, is made the occasion for some special discussions; for instance, on the relation of Amūn to the Nile flood (pp. 240 ff.), and on the significance of two titles showing the special connexion of Osiris with the persea tree, *isd* (pp. 274 ff.). Brilliant and convincing as these studies are, however, the supreme value of the book must lie in its record of the archaeological work, so much of it the author's own,

which made them possible. Not only has he been successful in identifying as Twenty-fifth-dynasty work temple buildings scattered over the whole of the Theban area in every stage of ruin and confusion with building of other periods, and in clearing and restoring them on the ground; he has had unique success in the reconstruction, on paper, by means of composite photographs, of reliefs which could never be physically reassembled, since they came from reused blocks from Twenty-fifth-dynasty buildings long since dismantled and replaced. Much of the material listed by him is seen to have been published in some form, though (as his frequent corrections of it show) often inadequately; but a great deal is still untouched. For example, the magnificent tomb of the great official Ḥarwa (part I, § 48, C 1) remains totally unpublished; the publication, by no means complete, of features from the comparable tomb of Mentuemḥêt, the Fourth Prophet of Amūn and Prince of the City (§ 48, C 3) to whom Leclant has already devoted a substantial book, gives us some idea of the quantity and quality of Twenty-fifth-dynasty architecture and art which still awaits research. There has been a tendency in the past to underestimate the work of this period; compare the remarks cited on pp. 221, n. 4 and 226. Examination of the material collected here shows such disparaging generalizations to be ill justified. Although the tendency of the period is to conservatism and imitative archaism, within its conventions it is capable of living and original artistic expression; its portrait sculpture in particular shows many pieces which impress us by their vigour, their realism, or their delicacy and charm. Its architecture too is not wanting in invention; to take a single example, the propylaea colonnade with low intercolumnar screen walls, such as Taharqa erected at each of the four cardinal points at Karnak, is a form of construction which makes its appearance in this dynasty and remains popular for centuries.

It is evident, in fact, that the achievements of the Twenty-fifth Dynasty deserve a more favourable estimate. The recognition of their merits will be largely due to Professor Leclant, who has made the period so much his own, and to this book in particular. One of its excellences requires a final remark. I cannot pretend to have consulted every reference it contains; but the compilers of the *Topographical Bibliography* confirm my impression that its standard of accuracy is exemplary.

J. W. B. BARNES

Thebes of the Pharaohs, Pattern for every City. Text by CHARLES F. NIMS, photographs by WIM SWAAN. Elek Books, London, 1965. Pp. 208, 80 photographic plates, map, 3 plans. Price 84s.

There can be scarcely anyone more qualified to write about Thebes than Charles Nims, for in him we find the ideal combination of scholarship and intimate local knowledge gained from many years of living and working there. And although from the preface the author seems to be writing chiefly for general readers this book should receive a welcome from students and scholars also as containing a detailed, reliable, and thoroughly up-to-date survey of Ancient Egypt's most important site. The book begins with a brief sketch of the history of Thebes from its beginnings to its eclipse in the Roman period. The latest chapter in its history, as a centre for antiquarian interest, is happily given a separate section, for this period is scarcely less fascinating than those earlier periods which the endeavours and contrivances of explorers and scholars have made available to us. The remainder of the book is devoted to a description of the monuments both great and small on either side of the Nile interspersed with historical details and information on the activities which once surrounded and often centred upon what are now often mere jumbles of ruins. Surprisingly all this wealth of information is contained in little more than one hundred pages of text, for almost one half of the book is devoted to illustration, judiciously chosen and finely executed photographs in colour and monochrome, plus a number of plans and a map. The necessary compression has been achieved by a very terse and economical style of writing which may well deter the less dedicated. But this is a small price to pay for such a handy compendium. A number of points have been reserved for more detailed treatment in a section of notes at the back, and in view of the author's familiarity with his material one could have done with more. A brief bibliography, chronological table, and index complete what is, with its handsome format an admirable attempt to produce a book which is neither an exercise in pure scholarship nor yet an emasculated popular account. One could wish for more books like it.

BARRY J. KEMP

Die Demotischen Tempelide. By URSULA KAPLONY-HECKEL. Ägyptologische Abhandlungen, ed. W. Helck and E. Otto, Band 6. Wiesbaden, 1963. Two volumes. Teil I: Text, pp. xix+484; II: Abbildungen, pp. 207. Price DM 70.

In contrast with the number of edited demotic legal texts on papyrus—the smallness of which has at times been a matter of merit, if perhaps smug, reproof by scholars working on the Greek sources from Egypt—there has been comparatively little attention paid to the demotic ostraca stored by their thousands in Egyptian collections. From the publications of Thompson, Mattha, Wängstedt, and Lichtheim in this field, one might have supposed that there was little to be won except tax-receipts. Dr. Ursula Kaplony-Heckel has personally searched through practically every ostraca collection of importance for copies of the oaths addressed to a god. Such oaths are characteristic of Egyptian legal and social practice in Upper Egypt for the settlement of disputes among families and between neighbours, where for the most part the truth of the matter could be determined only on the basis of the credibility of the opposing parties. The cull has exceeded all expectation, if one were to judge from the examples of this category of text already published. Dr. Kaplony-Heckel here edits 224 examples, giving transcription, translation, and commentary, and her own facsimiles made from the originals: only in the case of the American collections, and in a few other instances has she had to rely on photographs. Of this number 173 are new texts: of the other 51, many have been in need of a new edition from a study of the originals. The reading, for example, of *grgt* in BM 12602 (No. 19) is without doubt correct and the reference to this passage by Spiegelberg, *Rec. trav.* 26, 39, as an example of *bk*, from the erroneous copy in *Rev. ég.* 4, pl. 3, must be removed.

A number of other oaths are listed in a Catalogue (pp. 379–403) which gives, so far as their fragmentary state allows, the names of the parties concerned, the god addressed, the date, and particulars of the point in dispute. 279 of these are described for the first time. In an appendix (pp. 479–84) a further 58 examples from Berlin are listed, bringing the total number of oaths edited in full or signalled to 572. It is safe to say that there can be little which has escaped the diligent researches of the author.

In her introduction Dr. Kaplony-Heckel describes the characteristics of this body of texts. They come from a circumscribed area of Upper Egypt, stretching from Dendera in the north to Gebelein in the south, and date from the reigns of the later Ptolemies to the early Roman Period. All but seven, which seem to have been preserved with family papers, are written on ostraca. Examples in Greek known at present number only five. Though clearly Egyptian in origin, they have little in common with the oaths of the Pharaonic Period studied by Wilson, *JNES* 7, 129 ff.

The commentary to the texts is particularly concerned with the identification of the parties named with those of legal deeds on papyri, notably from the Theban region and Gebelein, both as an aid for a precise dating of the texts and for the better understanding of the point of dispute. Until the appearance of the demotic *Personennamenbuch* on which Professor Lüddeckens is engaged, the 45 pages of index of proper names and the references in the body of the notes will prove a valuable mine of prosopographical information and a useful tool in the time-consuming labour involved in the transliteration of demotic names.

Demotists may regret the absence of photographs of texts, at least of those that are better preserved, as has been the custom in other major publications of ostraca. In the nature of the script and of the subject-matter of the complaints, there are bound to arise points of detail in which it will be desirable to consult photographs. In their absence, more palaeographical information would have been welcomed. In no. 174, for instance, (II, 161) in line 1 the writing of *cnh* without the initial long stroke is not an unusual spelling: there is the loss of a fibre on the original and some indication of this, other than the blank gap in the facsimile, might have been given; in line 4, damaged at the end, the original suggests to me 'day 12' rather than '10'.

As a picture of everyday life among the humble individuals of the peasant economy of Egypt, this documentary evidence is revealing, unrivalled for the earlier periods, except for the Theban ostraca of the New Kingdom, which are not so readily accessible in translation. These rapidly written records on pottery sherds and limestone flakes illustrate in vivid fashion the mess which can be made of the conduct of private affairs when reliance is placed upon the goodwill of relations and neighbours. The texts are arranged loosely by the subject-matter of the complaints. Many, not unnaturally in a society of easy divorce, concern matrimonial disputes over property and inheritance. Loans, pledges, and arrangements over cattle are another source of constant friction. Petty theft is noticeable as well as personal violence. Dr. Kaplony-Heckel is

to be congratulated upon making these human documents accessible, the publication of which is a worthy addition to the *Ägyptologische Abhandlungen*.
A. F. SHORE

Das Geburtshaus des Tempels der Isis in Philä. By HERMANN JUNKER† and ERICH WINTER. With drawings by OTTO DAUM. Österreichische Akademie der Wissenschaften, Phil.-hist. Klasse, Denkschriften, Sonderband. Vienna, 1965. Pp. xv+429, pl. 1 and figs. 247. ÖS. 856.

Although this work is a sequel to Junker's *Der grosse Pylon des Tempels der Isis in Philä*, published in 1958, and is concerned with representations and inscriptions from a part of the temple which is closely associated with the great pylon, it differs in some important ways from its predecessor. One difference is a cause for regret: not only did Junker fail to witness its appearance, but he also missed a large part of the preparation, even if about two-thirds of the transcription derives basically from his hand. He was fortunate, nevertheless, to acquire the collaboration of Erich Winter in 1956, and it is Winter who has brought the task to its conclusion, being responsible for the whole of the translation. Other differences concern the method of publication. Instead of using printed hieroglyphs, as in *Der grosse Pylon*, a transliteration has been provided here, and each page of transliteration and translation usually faces the relevant representation which includes, in line-drawings, faithful reproductions of the hieroglyphs: the word 'Faksimile' is used. No printed hieroglyphic type can do justice to the variety and complexity of the Ptolemaic writing-system, so that the avoidance of printed hieroglyphs is a decided advantage. Dr. Otto Baum, who is responsible for the drawings, was able to use an ingenious method of achieving a facsimile, the diapositives of the Berlin photographs being projected on to a mirror beneath a glass-topped table on which transparent paper allowed the reflection to be traced. When the photographs proved difficult Winter resorted to the paper squeezes of a large part of the material which have been kept in the Berlin Academy, and he compared the results with the evidence of the *Wörterbuch* slips (which had, of course, been prepared by Junker). Since much of the *Geburtshaus* material is in inner rooms, in contrast with that of the great pylon, the photography had not always been so effective. Winter also took the vital step of checking readings *in situ*, an exacting task which was possible only in the heat of July and August since the island of Philae was submerged at other times. The necessity of this task was shown by the resulting numerous improvements in the texts—'literally thousands'—and by the opportunity, duly seized, of recording small areas both in the mammisi and the great pylon which the Berlin photographs had not covered. The additions to the material from the great pylon are published here on pages 405–27.

The provision of a transliteration of the text is a brave and helpful procedure. Here some inconsistencies are freely admitted. With participles and relative forms the verbal stem is denoted without any endings, following the usual practice of the hieroglyphic writing of the period. Occasionally, however, the 'historically correct' ending is added in brackets. To burden the text with a great number of such additions seemed undesirable, it is said, and so the additions have been restricted; thus, for instance, the feminine endings in the epithets of goddesses have been added in brackets only where the clarity of the grammatical structure is furthered thereby. In other cases the feminine endings are added *without brackets*, as in *ih·t nb·t nfr·t wsb·t*, because they are obvious and because the frequent bracketing would have been irksome. Clearly this procedure lacks objectivity, and it would have been better to avoid brackets altogether. Still, little trouble is caused by the inconsistency. More worrying is the fact that, occasionally, spatial exigencies have resulted in hieroglyphs that are not easy to read, as, for instance, on pp. 68–74. But this is exceptional, and in general the volume deserves high praise.

One might ask, reverting to transliteration, why *s* and *š* are distinguished here; a phonetic distinction had long since disappeared, and, in any case, *z* and *s* are neater differentiations of the spelling. A point about terminology is worth raising. In two consecutive sentences of his introduction Winter uses 'Transkription' to mean, first, the reproduction of a hieroglyphic text, and, secondly, its transliteration; the same term is of course used in other works for the representation of a hieratic text in hieroglyphic. Confusion may sometimes result. In English 'transcription' is often used of the first and third of these processes, but 'transliteration' is wisely employed for the second. It seems that in German a greater variety of terms has prevailed. Eрман, *Äg. Gr.* (Ed. 3, Berlin, 1911), § 36 uses 'Umschreibung' for transliteration; in § 137 he uses 'Transskription'

(*sic*). For the same process Edel, *Altäg. Gr.* (Rome, 1955), § 103 has 'Umschrift' and 'Transkription', but also, more than once, 'Transliteration' (as well as the verb 'transliterieren'). I venture to suggest that for this meaning the last-mentioned term is the best, since it is the most precise, and that the other terms should be reserved for the other processes.

Winter's translations are commendable. In these texts one can run, after stretches of plain sailing, into very rough waters. Uncertainties are here frankly denoted, and it is clear that finality is not claimed, especially when the reading is still in doubt. There is a slip on p. 355, 13, where *Hrw pꜣ hrd* is rendered by 'Harendotes' instead of 'Harpokrates' (likewise in the sub-heading). For the form 'Arhensnuphis' on p. 355, 6 one would expect 'Arsenuphis', though the form given is nearer to the Egyptian. 'Der Selige' for *mꜣ hrw* as an epithet of Onnophris (p. 129, 13) is not quite satisfactory; triumph or vindication is probably denoted.

One hopes that the plans for saving Philae which are now being discussed will be implemented. In any eventuality the scientific publication of the texts is of the highest importance, and that is why this volume is especially welcome. Quite apart from such a consideration, it would serve, were it not for its price, as an admirable introduction to the hieroglyphic texts of the Graeco-Roman era. Mythologically its content is richer than that of its predecessor, and recent studies by Daumas and Brunner have illuminated its main theme.

J. GWYN GRIFFITHS

Ptolemais, City of the Libyan Pentapolis. By CARL H. KRAELING with contributions by D. M. BRINKERHOFF, R. G. GOODCHILD, J. E. KNUDSTAD, L. MOWRY, and G. R. H. WRIGHT. University of Chicago, Oriental Institute Publications, Vol. XC. Chicago, 1962. Pp. xviii+288, 64 plates, 22 plans, 74 text figures. No price given.

Ptolemais of the Libyan Pentapolis was founded in the second half of the third century B.C., probably early in the reign of Ptolemy III, on a site that had already been occupied for almost three centuries by the anonymous 'harbour at Barca'. Virtually nothing is known of the city's history, but it no doubt fared much as did Cyrenaica as a whole, prospering at first under the direct control of Egypt, languishing under the semi-independent rulers of the late Hellenistic period and under the Roman republic, and prospering again under the Roman empire, apart from the set-back of the Jewish revolt. It was only in late antiquity that Ptolemais enjoyed a period of relative fame, when Diocletian made it the capital of Libya Pentapolis, a position it held until supplanted by Apollonia in the second half of the fifth century. Demotion was followed by a rapid decline, and according to Procopius scarcity of water had reduced it to only a few inhabitants by the early sixth century. Justinian seems to have brought a last brief flicker of life to the place, but long before the arrival of Amr ibn el-'Asi, it was finally extinct.

Extensive remains of Ptolemais have always been visible and a careful plan of them was made by the Beechey brothers in 1822. Between 1935 and 1942 a number of sites including the Street of the Monuments, the Square of the Cisterns, the Palazzo delle Colonne, the Fortress Church, and the Tower Tomb were cleared and studied by the Italians under Professor Giacomo Caputo; and three more buildings, a villa of the early Roman period, a public building on the Street of the Monuments and the City Bath of the Byzantine period, were examined in detail by the Oriental Institute of Chicago under Professor Carl Kraeling between 1956 and 1958. The purpose of the present volume is twofold: to present detailed excavation reports on these last three buildings, and also to attempt a survey of Ptolemais as a whole on the basis of a thorough re-examination of the site carried out by the Institute. This was something well worth doing and generally speaking it has been well done. Even if some of the conclusions reached here must inevitably be regarded as provisional, we now have a broad and solid foundation on which future investigators can build, and for which they will be grateful.

Detailed criticism would be out of place in this brief notice, but one point must be mentioned since it has important consequences. The prevailing winds in North Africa are not northeasterlies, as Professor Kraeling asserts, but northwesterlies; it is therefore almost certain that the harbour of Ptolemais lay to the east of the promontory jutting out to sea from the city's northwest corner, not to the west of it. The book is well-produced (in Germany) and most of its many misprints are harmless. But 'bouleutereion' (*passim*) and 'Miss Joyce Reynolds of Newham College, Oxford' should not have got past.

D. E. L. HAYNES

Coins from Karanis (The University of Michigan excavations 1924-35). By ROLFE A. HAATVEDT, ENOCH E. PETERSON, ed. by ELINOR M. HUSSELMANN. Ann Arbor, Kelsey Museum of Archaeology, 1964. Pp. 399, pls. 11. No price quoted.

The site of ancient Karanis (mod. Kôm Aushîm) on the northern edge of the Faiyûm was excavated over a period of eleven years between 1924 and 1935. Some of the coins found there have been known to specialists previously, but the main body of the material remained uncatalogued. Of the coins found, the total here listed shows a total of 26,796 specimens, a number of further pieces being illegible and thus impossible to account for. The coins range from the Ptolemaic period—where however specimens are quite few owing to the destruction of the most ancient parts of the site in modern times—to Byzantine times: the paucity of material after the fourth century A.D. attests a steep decline in the prosperity of the site.

Most of the coins (over 24,000) consist, as might be expected, of specimens from the Roman mint of Alexandria which produced base silver tetradrachms and bronze drachms and smaller pieces, inscribed in Greek, from the reign of Augustus until that of Diocletian, when the mint was entirely reorganized to produce coins of the standard type uniform throughout the empire. Before Diocletian, the number of Roman coins of the other imperial mints is very small (only 1,321 specimens), and this fact seems to illustrate tellingly the way in which Egypt was kept insulated from the rest of the world by the Roman administration. After Diocletian, coins of the new imperial type are present but not in very large numbers, those of the new Alexandria mint being most numerous, but those of Antioch and Constantinople not far behind. Most of the coins of all periods were found in hoards and not as single specimens. Of the hoards the most noteworthy is hoard 4, containing sixty second-century Roman aurei of Antoninus Pius, Faustina I, Marcus Aurelius, and Faustina II: this was, it is suggested (p. 14) perhaps 'the treasure of a Roman officer who brought it with him when he left Italy for his station in Egypt. He may have concealed it awaiting the day of his return to his homeland, since it could not be spent locally.' The point here raised is an interesting one, since in fact several other hoards of Roman gold have been unearthed in Egypt, though they are not properly published: on this we may refer to West and Johnson, *Currency in Roman and Byzantine Egypt*, ch. vii, though it is true that those authors tend to take much the same view as is implied in the above quotation.

The basic compilation of the present work is by Haatvedt, and was originally prepared as long ago as 1939, being later revised by Peterson and finally edited for publication by Husselmann. We can only admire the vast amount of patient work which it must have entailed for all concerned, in spite of certain disadvantages in the arrangement such as the grouping together of Alexandrian and imperial specimens of the same emperor. The recent final redaction has brought with it the advantage that the late Roman coins have been furnished with references to the work by Carson, Hill, and Kent which now represents the most authoritative arrangement. One or two points may be added regarding the Ptolemaic coins, which, as already remarked, are not numerous. The Karanis specimens are fairly typical of what may be found on any Ptolemaic site and raise no problems. With regard to no. 15, this would be better attributed to Ptolemy V; nos. 30-33—there is no real reason why the monogram appearing on these should be of Paphos. Nos. 44-46, given to Ptolemy Auletes, and no. 47, given to Cleopatra VII, are so attributed on the basis of the work of Svoronos, to which some necessary amendments were long ago made by Regling; in fact the 'Auletes' coins should be Cleopatra, and no. 47 should be Auletes. It is also somewhat misleading to say 'struck at Paphos' for no. 47, although this piece, like nos. 44-46, has the letters *IIA*: it is strange but true that these mint letters, originally indicating the Paphos mint, were also used from the time of Euergetes II on silver coins which must for good reasons be of Alexandria, and it is certainly from this mint that these specimens come.

The work is illustrated by eleven plates, mainly of coins in good preservation, including the Roman aurei, and also giving maps and plans of the find spots of the various hoards. Full indexes are provided of Roman mints and emperors, of the coin types and coin inscriptions both in Greek and Latin. G. K. JENKINS

Corpus des Ordonnances des Ptolémées. By MARIE-THÉRÈSE LENGIER. Académie royale de Belgique (Classe des Lettres et des Sciences Morales et Politiques)—Mémoires LVII, Fasc. 1. Brussels, 1964. Pp. xxiv+368.

No less than the *principum placita* of the Roman empire, the legislative aims of the Ptolemaic monarchs were realized in a variety of forms. As against νόμοι, διαγράμματα, etc., Mlle Lenger has concerned herself

with *προστάγματα*, those royal provisions—whether of a general scope or of more specific application—which were made, sometimes *sua sponte* sometimes in response to applicants, in a manner generally not dissimilar to *rescripta*. The range is a wide one—a census of the priests of Dionysos (no. 29), a ban on advocates pleading in fiscal cases to the prejudice of the treasury (no. 23), the grant of privileges to a gymnasium (no. 48), an amnesty (nos. 53, 54), the validity of safe-conducts for agricultural workers (no. 74), to take a few examples—though there seems usually to be some link with the royal treasury, if only in the imposition of penalties, often in the form of confiscation.

As was only to be expected of the distinguished Belgian scholar, the compilation has been most carefully prepared on the following plan. First come the undoubted *προστάγματα*, nos. 1–76, which constitute the largest part of the work; nos. 77–91 are more doubtful instances; 99–113 comprise *testimonia* indicative of *προστάγματα*; finally, 114–23 demonstrate the existence still in Roman times of recourse to Ptolemaic *προστάγματα*. The treatment is admirably methodical. Within each group of texts, the order is chronological. A brief introduction furnishes the date and provenance of the document, and its author, intended recipient, and probable purpose; next comes a note of existing editions—if any; for there are not a few documents which see print for the first time in this Corpus—and bibliography of commentaries on it; then the text itself is set out, followed by a sage commentary which notes and comments judiciously upon suggested alternative readings or proposals for restoration of the sadly all too frequent gaps and lacunae; to the commentary Mlle Lenger brings added authority from having read all the texts, however authoritative existing editions, in the original or in photocopies.

In addition to some excellent photographic reproductions, the substance of the work is followed by a comprehensive papyrological bibliography and a rich series of indexes—the origin and present location of each document, concordances with previous editions, a calendar of the reigns of the Ptolemies, a geographical index, list of proper names and of Greek words, and, after the table of contents, a final note of the texts emended.

The quality of the scholarship exhibited in this collection is matched by that of its mechanical production. To the Académie our gratitude for, to Mlle Lenger unstinted admiration of, a volume that will be indispensable to any library.

J. A. C. THOMAS

Paramoné und verwandte Texte: Studien zum Dienstvertrag im Rechte der Papyri. By BERTRAND ADAMS. Neue Kölner Rechtswissenschaftliche Abhandlungen, Heft 35. Walter De Gruyter & Co., Berlin, 1964. Pp. x+201.

Dr. Adams's premature and deeply regretted death occurred before he had completed the preparation of this work for publication. It was seen through the press by Professor E. Seidl whose pupil he was and to whom, at the time of his death, he was Assistant at Cologne University. The book is a study of the service contracts of free persons and of the role of free labour in Egypt. As the author notes, slavery has been thoroughly discussed in literature but the documents provide abundant evidence also of the importance of free labour.

After distinguishing the other instances of *παραμονή* (security in litigation and for liturgies, on manumission, etc.) and also initially dissociating educational and nursing contracts and those for the services of dancers and musicians, the author makes a careful presentation of the documentary evidence and proceeds to his view of *παραμονή*. To Adams *παραμονή* was a service contract, characterized by the dominant position of the employer and the requirement of a remuneration, *μίσθος*, though the relation was not a consensual contract. *Παραμονή* is a service relationship; *παραμένειν* denotes 'to serve'.

The author's discussion of the element of predominance, economically speaking, of the employer in the service relationship and his rejection of any necessary unfree subjection of the servant or his diminished or restricted state of liberty are wholly to be accepted. But the identification of *παραμονή* with service may be queried. One may, of course, delimit one's inquiry as one chooses; but, in the full range of use of the terms *παραμονή* and *παραμένειν*—as emerges in A. E. Samuel's recent survey, 15 J.J.P. 221–311—the general picture is of an obligation of 'staying/remaining with'; though not—as Adams rightly points out—necessarily of residence, so to speak, as a member of the employer's household. One would see *παραμονή* as an obligation

arising out of a service contract, as it does from other relations, rather than as the service contract. In short, *παραμονή* connotes an obligation arising in what may be conveniently styled a *locatio operarum* (not, however, confined to *munera sordida*), an obligation understandable in this contractual relation in view of the employer's superior position but of a type with the obligation in other relationships which do not necessarily import a remuneration for the person who is under the obligation.

This would seem in fact to be confirmed by the parallel, rather than equiparation, which Adams subsequently makes between *παραμονή* and the nursing contract, and the somewhat tenuous relevance for *παραμονή* of the contracts for the services of musicians and dancers in the final sections of his work, after a sound discussion of *διδασκαλικαί* as a kind of sub-species of the genus *παραμονή* as a service contract.

Reluctance to accept the central thesis in no way diminishes one's appreciation of the thorough treatment and careful documentation which make the publication a work of real consequence for those interested in the legal regulation of service and labour relations in the ancient world. J. A. C. THOMAS

Prosopographia Ptolemaica V (Studia Hellenistica 13). By W. PEREMANS and E. VAN 'T DACK. Publications universitaires de Louvain, 1963. Pp. xl+192. Price not stated.

The fifth instalment of the *Prosopographia Ptolemaica* (cf. on Part IV *JEA* 47 (1961), 170-1) brings up to 14,478 the total number of names analysed into categories of profession, office, or occupation. The 2,000 appearing in this part are drawn from the areas of 'commerce, industry, land-transport, the fleet, persons in domestic service'. The compilation is in fact a classified directory to Ptolemaic Egypt and its overseas possessions carried out *according to the names of persons involved*. It is of course often a delicate matter to decide in which category a person should be classed, and how to separate homonyms. The compilers set out their principles in an introduction. 'We consider as slaves not only those who are called *ἀνδράποδα* or *δοῦλοι*, but equally all who are mentioned in their quality as *παῖς*, *παιδίσκῃ*, *παιδάριον* or *οἰκέτης*, unless the context is clearly opposed to this interpretation.' The person with an interest in New Comedy will be delighted to note the slave-names *Λόναξ* (14344: it was Wilcken in *Archiv* 6, 399 who first suggested that the word should be given an initial capital, and even he does not quote Terence; now one would add Menander, *Dyskolos* 959) and *Θράκων*, and will be charmed by the names *Σαφφώ* and *Ἰσπώρα*. E. G. TURNER

Les Archives de Sarapion et de ses fils. By JACQUES SCHWARTZ. Institut français d'archéologie orientale. Bibl. d'étude, Tome xxix. Cairo, 1961. Pp. 382, 9 half-tone plates. Price not stated.

The late appearance of this notice is the fault of the reviewer, for which he presents his apologies. The work itself is most definitely deserving of the attention of economic historians as well as students of Greek papyrus documents. Professor J. Schwartz, by attending to the inventories of collections of papyri, the family relationships of persons concerned in them, and the handwritings of the texts themselves, has succeeded in putting together a set of papyrus documents belonging to the time of Domitian, Trajan, and Hadrian which were divided up by their finders in the 1890's, and here publishes some 122 texts (something like three-quarters of that total for the first time). No doubt more texts of this archive still await identification. The reassembly succeeds in bringing into sharp focus the affairs of a farming family, Sarapion, his wife Selene, their five sons and one daughter who lived in Hermopolis between A.D. 90 and 130, and offers a corpus of important economic data (prices of corn related to wages and prices) over this period, the more reliable in that the data are not given in isolation. Sarapion had a peasant's hard-fistedness over money matters, and was not popular in his district: but he observed the customs of his time, and when he advises a son 'to consult the oracle as to the right time for sowing at Psothis' (no. 83a, 14 ff.) we may perhaps think that he has in mind the scruples of his workmen as much as his own superstitions. E. G. TURNER

Apologie de Philéas. By V. MARTIN. Papyrus Bodmer XX. Bibliotheca Bodmeriana, 1964. Pp. 58. 15 half-tone plates. Price not stated.

I should be sorry not to notice in this *Journal* the last important papyrus publication by the Swiss scholar Victor Martin. The text he publishes is of an interest quite out of proportion to its small size. It consists of the interrogation of Phileas, Bishop of Thmouis, by the prefect of Egypt, Culcianus, before his martyrdom. The date given by Halkin is 4 February, A.D. 306, but the year may be wrong; much of the argument depends on the dates assigned to Culcianus, the extreme limits for whom are A.D. 303 and 308 (cf. A. Swiderek, *Eos* 54 (1964), 164). The record is sober and factual, for the most part simple question and answer without rhetorical colouring. The Roman governor is no monster, but a public servant faced with a disagreeable task, and is at pains to try to save Phileas from the consequences of an obstinate adherence to his faith.

The text has had to be reconstituted from fragments (a few of which, in the possession of Sir Chester Beatty, were identified by T. C. Skeat, and made available to Dr. Bodmer). From them Martin has assembled the greater part of four double leaves of a papyrus codex. He argues that the text was comprised in a quire of five (or possibly even more) sheets. After the colophon *ἰρήνη τοῖς ἀγείοις πᾶσει* is set a chrism reminiscent of an *ankh* in form. The handwriting (which cannot be earlier than 303 and may be up to half a century later) is a fair-sized regularly executed round capital, in which the letters are well spaced and usually even in size (though β, κ, and ζ may have a much lengthened horizontal extension). μ formed in four strokes is of a noteworthy form. At the ends of lines letters are more crowded and reduced in size. Ligatures are rare, but a horizontal stroke (the crossbar of τ or γ, the horizontal of ε) may connect with a following ο, ι, or ρ. The handwriting can be studied in detail in the complete facsimile generously provided by the Bodmer library.

The appearance of the hand is such that several experienced palaeographers of my acquaintance tell me they would have assigned it without hesitation to the middle of the third century A.D., and it is easy to see why: the mode of forming individual letters (e.g. the broadly based β; ε is well-rounded and has its crossbar high) and the layout and general appearance could be readily paralleled from third-century documents or from codices, such as the Bodmer St. John (P. Bodmer II) which have been assigned to the third century. It is salutary to be reminded of the precarious nature of palaeographical judgements, and to have in this specimen an excellent test piece of fourth-century bookhand to add to others that have recently become available, notably, P. Cair. Isid. 2 (Plate II—the facsimile is reduced in size) and P. Hermop. Rees 4 and 5 (Plates II and III). It seems doubtful whether any of the pieces assembled to form the conglomerate codex published as P. Bodmer V, X, XI, VII, XIII, XII, XX, IX, and VIII should now be dated in the third century. The reviewer would be inclined to assign them all to the fourth.

In his reconstruction Martin has had useful assistance from F. Halkin. The latter has drawn important inferences for the early history of the *Martyria* in an article in *Rev. d'hist. ecclésiastique* 58 (1963), 136–9, and given a revised text of both Greek and Latin versions in *Analecta Bollandiana* 81 (1963), 5–27. Let it not be thought that the work has all been done. There is an unfortunate misreading on p. 9 l. 11 of the manuscript (p. 38 of Martin's edition), where μ has been taken as λλ, and a sentence constructed οἱ Ἰουδαῖοι πρὸς Ἑλλήνων τὴν κάθοδον αὐτοῦ κ[ατήγγειλαν, from which it is inferred that Greeks as well as Jews foreshadowed the second coming of Christ. But Greeks have no place here, either linguistically or philosophically; Phileas is not appealing to Plato as prophet of the fate of Christ or of Christian doctrine. When Plato is mentioned a little later it is in an ironical question by the Roman governor, and it seems that he is being contrasted with St. Paul.¹ The correct reading here may be restored as οἱ Ἰουδαῖοι προεμήνυ[σαν] τὴν κάθοδον αὐτοῦ (the Latin has *praedixerant*).

E. G. TURNER

Papyri from Panopolis in the Chester Beatty Library. Ed. by T. C. SKEAT. Dublin: Hodges Figgis & Co., 1964. Pp. xlv+194, with 3 plates. Price 120s.

The two long papyri which form the subject of the present publication are worthy to be ranked among the most important documentary evidence to have survived from Roman Egypt. There is inevitably some element of fortune in all papyrological finds, but in this case fortune has been especially kind to us. Some forty years after they were written the two papyrus rolls, now evidently mere waste paper, were cut into sheets which were glued together in pairs, with the writing on the inside, and then folded over to form a rough

¹ I have developed this point in *JTS* 1966, 55.

codex. This was used to record tax receipts and it was in this form that the papyri were discovered. By chance some of the writing on the inside was visible and, since it promised to contain more interesting matter than that on the outside, the sheets were carefully unglued. The results exceeded all expectations. The reconstructed rolls were found to be from the archives of the *strategus* of the Panopolite nome, one of them belonging to A.D. 298 and the other to A.D. 300. They thus give us a fascinating glimpse into the workings of the imperial bureaucracy in its middle, and sometimes in its higher, echelons at this crucial period of transition under Diocletian.

The first papyrus, which is just over 400 lines long, contains copies of letters despatched from the office of the *strategus* in September A.D. 298. Recipients higher up the administrative ladder than the *strategus* are his immediate superior the *procurator* of the Lower Thebaid, the *magister rei privatae*, and the *catholicus*. Possibly we should add the prefect of Egypt, if he is to be identified with the ἡγεμόνι to whom 272–5 is addressed; there is some doubt, however, as to whether the prefect had any authority in the Thebaid at this date (cf. Skeat's remarks on p. xxi). There are several letters to the senate of Panopolis, normally addressed via its *πρόεδρος*, and not a few signs of friction between it and the *strategus*, e.g. 109–19 and 230–40. Many of the entries are instructions issued to a host of minor officials concerned with taxation and the collection and distribution of the *annona militaris*. Easily the most interesting information in the document is that a visit from Diocletian is imminent, and it is with preparations for this that a number of letters are concerned, e.g. 381–4 on the provision of sacrificial animals. It is also valuable to have proof that the city and the nome were required to share the tax burden in the proportion 1:2 (379; cf. Skeat's note). I would further draw attention to letters seeking the requisition of labour, especially 213–16 concerning a smith, and 342–6 concerning makers of armour, both of which suggest the use of police-state methods.

The second papyrus, though about one hundred lines shorter and less varied in content, is possibly even more important. It is a record of correspondence received by the *strategus* from the *procurator* of the Lower Thebaid in January to March A.D. 300. Many of the letters relate to the payment of donatives, etc. to the soldiers. Apart from the useful military information they contain, they prove that the date of Diocletian's accession was 20 November. Other particularly interesting letters are 43–50, which mentions the conveying down the Nile to Alexandria of special stone columns, 68–71, where the *strategus* and *decemprimi* are accused of deliberately seeking to defraud the treasury, and 215–21, which shows that gold was regularly exacted each month even at this early date; the price paid is forty talents per pound (equivalent to 60,000 denarii). As well as letters the papyrus gives us copies of four proclamations issued by the *procurator*: the first, 92–99, condemns payments to bankers made *προφάσει βαλλαντίων*, an obscure expression on which Skeat has an important note; the second, 156–60, which should be compared with a letter from the *catholicus* included at 134–44, regulates the procedure for auctioning land *ἀπὸ ὑπολόγου ἀφόρου*; the third, 222–8, is an exhortation to all concerned to take proper care of the dykes; the fourth, 229–44, deals at length with the malpractices of tax-collectors. The dominating position of the *catholicus*, who completely overshadows the *praeses* of the Thebaid, is one of the most striking things to appear from the two documents. In 2 he is Pomponius Domnus, who had been *magister rei privatae* in 1. It is to the *catholicus* that the *strategus* must send his monthly accounts, 1, 63–71, and it is by him, so it would appear, that he is appointed (1, 90 f.). Throughout 2 we find him putting pressure on the *procurator* to perform his job properly, just as we see the *procurator* in his turn putting pressure on the *strategus*, sometimes accompanied by strongly worded threats (e.g. 2, 106–8). The necessity for this continual prodding, the evidence of the corrupt practices of officials, and the poor relations between the *strategus* and the local senate all combine to give the impression that the administrative machine was far from running smoothly at this time.

If we have been fortunate in the discovery of these papyri, we have been no less fortunate in that their publication was entrusted to an editor of Skeat's ability. The editing of papyri written in several different hands and containing many passages to which no parallels exist cannot have been easy; nor was it made easier by the fact that the rough codex had been trimmed at the edges, thus causing a great many small lacunae in the rolls (cf. plate iii). Skeat has rightly concentrated on producing as reliable a text as possible, and has performed this task with exemplary thoroughness. He has, however, found time to comment in the introduction and notes on many of the more important topics raised in the papyri. My only complaint is that he has not included a subject index. This would have been particularly valuable in an edition of papyri whose texts, and in consequence the comments on them, range over such a wide field.

The major historical points discussed in the introduction are the date of the revolt of Domitius Domitianus (almost certainly A.D. 297/8) and the part played by Diocletian in the Persian war. A further section deals with the organization of Egypt at this period, since the papyri make it clear that the Thebaid was already a separate province subdivided into an Upper and a Lower division under two *procuratores*. Skeat argues very plausibly that the separation of the Thebaid can be traced in P. Oxy. 43 of A.D. 295, and that the *ἐπίτροποι* who occur in papyri of this period may often be *procuratores Heptanomiae*, not purely financial officials. The papyri also tell us that the Lower Thebaid in A.D. 300 consisted of nine nomes, from the Hermopolite to the Thinite, among which the Antinoite, Cussite, and Hypselite are noteworthy. Elsewhere in the introduction Skeat treats of the military information contained in the documents, which is especially valuable for what it tells us of the strength of the various units in the Thebaid, and the geography of the Panopolite nome, including a republication of P. Got. 7 which he shows to have come from Panopolis. There are also brief sections on taxation and finance, and on the language and style of the documents; in the latter Skeat draws attention to the large number of ἀπαξ λεγόμενα, many of them being transliterations of Latin words.

Most of the notes are short, but some important exceptions deserve mentioning. In the first papyrus: 64 on the *catholicus*; 128 on the *ἐπίσταθμος*; 155 on τὸ πασσαλιωτικόν, which Skeat tentatively refers to proscribed persons; 219 on *cellarium*; 230–40 on the appointment of municipal tax officials (cf. 276–331 n.); 252 on μετὰ with the accusative. And in the second papyrus: 30–31 on the equivalence of ἀττικάί and denarii; 33–34 on περιφορά meaning ‘period of grace’; 41 on the ὀπινιάτωρ, a military officer; 137–40 on auctions; 148 on reckoning by indictions; 153 on Maximianopolis; 299–308 on *follis*. For good measure Skeat has added a brief appendix in which he publishes two unedited papyri from the British Museum which bear on the administration of Egypt at this period.

A few less important points may be added. ‘The clue’ on p. xxviii, n. 1 is a slip for ‘no clue’. The ending]ιαδος in 1, 114 suggests a proper name; one possibility is Ἀσκληπ]ιάδος, which opens up an interesting line of speculation: for Ἀσκληπίας might well be an alternative for Ἀσκληπιιάδης, and we know that Aurelius Asclepiades was *catholicus* at approximately this date (cf. 1, 64 n.). Instead of διεπεμ]ψάμην in 1, 200 it would be possible to restore a compound of -εγρα]ψάμην; this would mean that the *bibliophylax* was not being despatched, but merely informed. On the Latopolite nome, cf. 2, 97 n., reference should be made to A. Calderini, *Studies presented to D. M. Robinson*, II, 450–8. On the term πρακτῆρες, 2, 230, see Turner’s article in *Language and Society*, 165–9.

It has been possible here to mention only a selection of the vast number of interesting points contained in these papyri, but I hope it has been made clear that they are not documents which will be of interest only to papyrologists. Undoubtedly economic and military historians will get most assistance from them, but the material they include is important enough to make them required reading for all students of the history of the Empire at this critical time. It is encouraging too that the papyri should have come to light at this stage in the history of papyrology, since they provide a conclusive refutation of the pessimistic view that the Egyptian sands have long since given up their most valuable treasures. J. DAVID THOMAS

Papyrus Bodmer XVI, Exode I–XV. 21; en sahidique. By RODOLPHE KASSER. Bibliotheca Bodmeriana, 1961. Pp. 198, pls. 43. Price not given.

Papyrus Bodmer XVIII, Deutéronome I–X. 7; en sahidique. By RODOLPHE KASSER. Bibliotheca Bodmeriana, 1962. Pp. 228, pls. 49. Price not given.

Papyrus Bodmer XIX, Évangile de Matthieu XIV. 28–XXVIII. 20. Épître aux Romains I. 2–II. 3; en sahidique. By RODOLPHE KASSER. Bibliotheca Bodmeriana, 1962. Pp. 256, pls. 48. Price not given.

It is little more than ten years since a diverse series of Greek and Coptic books, on vellum and papyrus, containing biblical and other Christian writings as well as classical Greek texts, became known. The Coptic manuscripts are some of the earliest of their kind to have survived, dating from the fourth and perhaps fifth centuries. The greater proportion of this material was acquired by M. Bodmer for his library at Coligny-Geneva: other volumes or parts of them are in the Chester Beatty Library, Dublin, and in the University of Mississippi.

Dr. Kasser has performed a notable service to Coptic and biblical studies by the speed, diligence, and scholarship with which he has edited the Coptic volumes of the Bodmer collection. Two, Bodmer III (the Gospel of John and part of Genesis, in an early form of Bohairic) and Bodmer VI (Proverbs, written in an archaic script in a mixed sahidic-achmimic dialect) appeared in the Louvain *Corpus Scriptorum Christianorum Orientalium* in 1958 and 1960 respectively. The three volumes reviewed here are the first of the rich treasure of sahidic texts to be edited.¹

Bodmer XVI, a vellum codex in good state of preservation, only slightly affected by damp, is a complete volume with its original binding; there are 84 folios of text consisting of the book of Exodus, beginning at i. 1 and ending abruptly at xv. 2 after which the scribe has strangely added the title 'The first part of the Law'. Bodmer XVIII, a papyrus volume, contains in its present state 96 inscribed folios with text of Deuteronomy. The last leaf, a fragment only from the top of the page, reaches x. 7. The editor does not believe that much has been lost from the volume; he suggests that it probably ended with the end of x. 7 or perhaps x. 11. Bodmer XIX is the remains of a thick codex treasured and repaired more than once in antiquity. Kasser gives a careful and detailed history of the codex. In its present condition the text begins with the gospel of Matthew in the middle of xiv. 28, on the recto of a leaf bearing the Coptic pagination of 77. At the end of the Gospel (p. 166) there follows, with pagination beginning at 1 again, the Epistle to the Romans, of which three leaves (six pages) survive, written in a different hand, but in the same style as that of Matthew, with two columns to the page (as in the Crosby Codex at Mississippi). These leaves served as a guard for the final rebinding of the surviving portion of Matthew. In its original form, the volume may have contained the whole of Matthew and Romans, making an exceptionally thick volume of about 3 cm., which would have been susceptible to damage if in constant use. On a blank leaf preceding the text of Matthew has been added in another hand the title 'The last part of Matthew'. The same hand has also gone over a number of letters, mostly on the hair-side, which time had already made indistinct. Yet another hand has been responsible for a certain number of corrections in the text of Matthew.

In contrast with the two texts already edited in the Louvain series, the present volumes contain a complete photographic record of the original texts, except in the very few instances, as in the verso of the last leaf of Romans, where the making of it might have proved damaging to the original. These plates will be of the greatest value for the study of Coptic palaeography. The printed text contains as faithful a reproduction of the appearance of the Coptic original as the ingenuity of modern typography allows: only the superlinear stroke is standardized. Facing each page of text is a careful line by line translation into French in which the editor seeks to give not 'un texte littéraire, mais un texte aussi littéral que possible, sans outrager, toutefois, trop grossièrement, les constructions de la syntaxe française; une quantité exagérée d'idiotismes coptes, transcrits mot à mot, aurait pu rendre notre version inintelligible au non-coptisant: nous espérons n'avoir pas trop gravement péché dans ce sens-là par excès de fidélité littérale'. The principles are excellent in theory, their application in practice described in detail in the introductions. In view of the editor's disclaimer, it is perhaps churlish to remark that the method is unnecessarily elaborate. To take page 3 of Exodus: ⲉϣⲱⲛⲉ ⲙⲉⲛ is translated 'si' with a footnote 'litt. si d'une part (μὲν)': ⲟϣϣⲓⲙⲉ (LXX θῆλυ, of an animal) is translated 'une femelle' with a footnote 'litt. femme': ⲙⲓⲛⲟϣⲉⲓⲣⲉ 'elles n'agirent' with a footnote 'litt. faire': ⲕⲁⲧⲁ ⲑⲉ is translated 'de la manière' with a footnote 'litt. selon (κατά)'. To the student ignorant of Coptic, the frequent reminder through the footnotes of the literal meaning of common Coptic idiomatic expressions can be of little practical value, unless he intends to use the text, as some have used Horner, as a crib to learn the language. The Coptic student would be better served with an index of Coptic words. In common with the general practice of editors, there are indexes only of proper names (XVI in Coptic letters, XVIII and XIX in Greek) and of the so-called loan words. Many of the latter, particularly particles like γάρ, δέ, ἦ, had passed into the vernacular, as non-literary texts show, and the division between Coptic and loan words is in some ways an arbitrary one.

Each volume contains an introduction in which the editor describes in detail the physical appearance of the codices and discusses the palaeography, the dialectical peculiarities, and the nature of the version itself. On the provenance of the manuscripts Kasser rightly insists upon the absence of assured information, or archaeological indications. There is, as he says, the possibility, even probability, that they have come from

¹ For Bodmer XXI and XXII see *JEA* 51, 226-9.

Other books received

1. *Grundzüge der ägyptischen Geschichte*. By Erik Hornung. *Grundzüge*, Bd. III. Darmstadt, 1965. Octavo. Pp. 140+map.
2. *Physical Conditions in Eastern Europe, Western Asia and Egypt before the Period of Agricultural and Urban Settlement*. By K. W. Butzer. *Cambridge Ancient History*, revised edition of vols. 1 and 2, fasc. 33. Cambridge, 1965. Octavo. Pp. 39, figs. 6. Price 6s.
3. *Douanes et Commerce dans les ports méditerranéens de l'Égypte médiévale d'après le Minhādij d'al-Makhzūmi*. By Claude Cohen. Offprint from *JESHO* 7, 3. Leiden, 1964. Octavo. Summary+pp. 218-314.
4. *La Religion et la Morale dans l'Égypte antique*. By Jacques Pirenne. Neuchâtel, 1965. Octavo. Pp. ix+176.
5. *Mysteries of Demeter*. By Thomas Karel Ficek. Albany, N.Y., 1965. Octavo. Pp. 71. Price \$5.
6. *Abu Simbel*. By William MacQuitty. London, 1965. Quarto. Pp. 192. Numerous plates and illustrations both in colour and in black and white. Price £4. 10s.
7. *High Dam at Aswan, the Subjugation of the Nile*. By Tom Little. London, 1965. Octavo. Pp. xiv+242, maps and diagrams 4, pls. 12. Price 30s.
8. *Egypt in Nubia*. By Walter B. Emery. London, 1965. Octavo. Pp. 264, figs. 49, pls. 32+frontispiece. Price 50s.
9. *Four Aspects of Egypt*. By John Marlowe. London, 1966. Octavo. Pp. 303, pls. 32, maps 3. Price 45s.
10. *Rescued Treasures of Egypt*. By Max-Pol Fouchet. London, 1966. Quarto. Pp. 270, pls. 154.
11. *Nous partons pour l'Égypte*. By Serge Sauneron. Paris, 1965. Sixteenmo. Pp. viii+239, pls. 16, maps and plans 26.
12. *Il torinese colosso di Osimandia*. By Silvio Curto. Offprint from *Bollettino della Società Piemontese di Archeologia e Belle Arti*, New Series, yr. XVIII. 1964. Pp. 16, pls. 4.
13. *Kalabsha*. By S. Curto, V. Maragioglio, C. Rinaldi, L. Bongrani. *Orientis Antiqui Collectio* V. Rome, 1965. Quarto. Pp. 99, pls. 20, figs. 20.
14. *Assyria and Babylon, c. 1370-1300 B.C.* By C. J. Gadd. *Cambridge Ancient History*, revised edition of vols 1 and 2, fasc. 42. Cambridge, 1965. Octavo. Pp. 36. Price 6s.

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PRINTED IN GREAT BRITAIN
AT THE UNIVERSITY PRESS, OXFORD
BY VIVIAN RIDLER, PRINTER TO THE UNIVERSITY
AND PUBLISHED BY
THE EGYPT EXPLORATION SOCIETY
2 HINDE STREET, MANCHESTER SQUARE, LONDON, W. 1

ALSO SOLD BY BERNARD QUARITCH, 11 GRAFTON ST., NEW BOND ST., W. 1;
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