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B. J. Kemp, *Amarna Reports*, 1 (London, 1983), 57.

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CONTENTS

	PAGE
EDITORIAL FOREWORD	v
THE SAQQÂRA NEW KINGDOM NECROPOLIS EXCAVATIONS, 1986	I
MEMPHIS 1985	11
THE AMARNA WORKMEN'S VILLAGE IN RETROSPECT .	21
REMARQUES SUR LES REPRESENTATIONS DE LA PEINTURE D'HIERAKONPOLIS (TOMBE N° 100)	51
THE ARCHAIC STONE TOMBS AT HELWAN	59
TECHNIQUES AND TERMINOLOGY OF ROPE-MAKING IN ANCIENT EGYPT	71
PRACTICAL RELIGION AND PIETY	79
THE LOCATION OF IREM	99
THE CURIOUS LUXOR OBELISKS	137
ON THE LOCATION OF THE ADMINISTRATIVE OUTPOST OF THE COMMUNITY OF WORKMEN IN WESTERN THEBES	149
ON STYLE IN EGYPTIAN HANDWRITING	161
A PIOUS SOLDIER: STELE ASWAN 1057	169
APPENDIX TO NAQADA EXCAVATIONS SUPPLEMENT .	181
MUSEUM ACQUISITIONS 1985	191
BRIEF COMMUNICATIONS	
FURTHER RELIEFS FROM THE CHAPEL OF <i>Rc-htp</i> AT MEYDUM	197
A SLAB OF <i>Int-kis</i> IN THE FITZWILLIAM MUSEUM .	200
EIN KÄFIG FÜR EINEN GEFANGENEN IN EINEM RITUAL ZUR VERNICHTUNG VON FEINDEN	202
REMARKS ON THE BEINGS CALLED <i>mrwty</i> OR <i>mrwryt</i> IN THE COFFIN TEXTS	206
AMENOPHIS III'S VIZIER AMENHOTEP AT SILSILAH EAST	207
PILLOW STUFFINGS FROM AMARNA?	211
THE VIENNA STELA OF MERYRE	213
TWO WALKING-STICKS WITH TEXTS MENTIONING MEMPHIS	216
A WEST SEMITIC TITLE ON AN EGYPTIAN STELA IN RIO DE JANEIRO	218
A STATUE OF PREHIRWENMEF, SON OF RAMESSES II .	220
THE TAKHATS AND SOME OTHER ROYAL LADIES OF THE RAMESSIDE PERIOD	224
A NOTE ON THE FAMILY OF MONTEMHAT	229

A SEATED MAN WEARING A CLOAK (NAPLES NO. 237)	E. Kerrn Lillesø	230
CHATBY RECONSIDERED	William D. E. Coulson	234
NOTES ON A COPTIC LETTER FROM ANTINOE	Anthony Alcock	236
A NOTE ON 'Ineditum Campioneum Nottinghamense'	J. D. Ray	237
ST CHRISTOPHER AND THE LUNAR DISC OF ANUBIS .	Dom Bede Millard OSB	237
SOME REMARKS ON DEER (CERVIDAE) IN ANCIENT		
EGYPT	Patrick F. Houlihan	238
CORRECTIONS IN THE IDENTIFICATIONS OF THE		
ALABASTER IBEXES IN TUTANKHAMUN'S TREASURES	Dale J. Osborn	243
ANCIENT EGYPTIAN PLANT-REMAINS IN THE MAN-		
CHESTER MUSEUM	Renate Germer	245
REVIEWS		
WILLIAM KELLY SIMPSON AND WHITNEY M. DAVIS		
(eds.), <i>Studies in Ancient Egypt, the Aegean and the</i>		
<i>Sudan</i>	Reviewed by Charles C. Van	
	Siclen, III	247
C. A. NELSON, <i>Financial and Administrative Docu-</i>		
<i>ments from Roman Egypt</i>	Revel Coles	253
JAN ASSMANN, <i>Re und Amun</i>	J. Gwyn Griffiths	257
BRUCE WILLIAMS, <i>Excavations between Abu Simbel</i>		
<i>and the Sudan Frontier. Part 5</i>	A. J. Mills	259
JOHANNES KRAMER, <i>Glossaria Bilinguia in Papyris et</i>		
<i>Membranis Reperta</i>	J. David Thomas	262
A. KACZMARCZYK AND R. E. M. HEDGES, <i>Ancient</i>		
<i>Egyptian Faience</i>	M. S. Tite	263
J. B. SEGAL, <i>Aramaic Texts from North Saqqara</i> .	H. G. M. Williamson	265
L. KEIMER (ed. R. GERMER), <i>Die Gartenpflanzen im</i>		
<i>alten Agypten, II</i>	D. M. Dixon	269
MOHAMMED SALEH, <i>Das Totenbuch in den Thebanis-</i>		
<i>chen Beamtengrabern des Neuen Reiches</i>	Leonard H. Lesko	270
L. J. CRAVEN, <i>Aboudi Man of Luxor</i>	C. N. Reeves	271
MICHEL GITTON, <i>Les Divines Epouses de la 18^e</i>		
<i>dynastie</i>	Gay Robins	272
N. KANAWATI <i>et al.</i> , <i>Excavations at Saqqara north-</i>		
<i>west of Teti's Pyramid, I</i>	Nigel Strudwick	276
BARRY J. KEMP, <i>Amarna Reports I-II</i>	David Jeffreys	278
Other books received	281

EDITORIAL FOREWORD

THE Society has had successful seasons at three sites during the past year:

Memphis: The Memphis–Saqqâra 1986 season lasted from 1 September to 31 December. Staff members were David Aston, Signe Biddle, Heather Bleaney-Tait, Janine Bourriau, Sarah Buckingham, Nicole and Ellis Douek, Peter French, Helen Ganiaris, Dr Lisa Giddy, Julia Harvey, Dr Howard Hecker, David Jeffreys, Dr Jaromir Malek, Ian and Padi Mathieson, Helen McKeown, Margaret Nutt Moore, Paul Nicholson, John Ray, Professor and Mrs H. S. Smith, Dr John Tait, and David Tunnicliffe.

Excavation at Kôm Rabî'a continued with a seven-week digging season. Early hopes of recovering substantial occupation of the Second Intermediate Period were frustrated: beneath the earliest Eighteenth Dynasty occupation, a deep layer of coarse sand showing signs of only sporadic and temporary occupation proved to seal well-defined architecture in the northern part of the exposure, associated with Middle Kingdom pottery. A thin demolition or destruction rubble over this architecture marks a distinct break in the building history of the site, since there is a marked difference between the structural orientations of the Middle and New Kingdoms. The Middle Kingdom level is still remarkably high-lying, being some 3 m above that of the Ramesside temple floors further east, and less than a metre below the ground level at the First Intermediate Period cemetery a little to the north-west on Kôm Fakhry. It raises our expectations that occupation levels of the Old Kingdom might be accessible here. As usual a wide range of objects of daily use was found, largely in the early New Kingdom levels in the west and south-west parts of the exposure where the coarse sand deposit was shallower. Glass and faience rings and ear-rings of some refinement were recovered: inscribed scarab seals and ring bezels range in date from Amenophis III to Ramesses I and are all from the later New Kingdom levels in the south-west corner of the excavation.

The Epigraphic Section prepared and collated records and copies of the previous six seasons' work, and completed photographic coverage of the limestone colossus of Ramesses II. Professor Smith, Mrs Smith, and Dr Tait continued with the preparation for publication of demotic texts from the Sacred Animal Necropolis at North Saqqâra, and Mr Ray proceeded with his study of demotic ostraca. Mr French began the photography and collation of paper documents from Qasr Ibrim stored temporarily at Saqqâra, as well as continuing his analysis of pottery from the Anubieion.

Saqqâra: The Expedition discovered and entered the substructure of the tomb of Maya, treasurer of Tutankhamun, on 8 February 1986. This season our efforts have been concentrated on the superstructure of the tomb, and most of it has been cleared. The Expedition worked in the field from 6 January until 19 March 1987. The staff comprised Dr G. T. Martin (Field Director and Epigraphist), Mr K. J. Frazer (Surveyor), Mrs R. S. Walker (Anthropologist), Miss B. A. Greene (Pottery recorder), Miss J. Harvey (Student), Dr M. J. Raven (Site Supervisor and Objects Recorder), Dr J. van Dijk (Philologist), Mr P.-J. Bomhof (Photographer), Miss I. Blom (Student), Miss Amal Samwel (Inspector, Egyptian Antiquities Organization).

The mud brick superstructure resembles the plan of the tomb of Horemheb, except that Maya's tomb was not provided with a colonnade on all four sides of its outer courtyard. The architectural elements exposed so far are: (1) an outer court with a single row of columns on

the west side. This court is unpaved, and a very low mud ramp leads from it to the paved floor of the west colonnade. Three doorways on the west of the court give access to two long magazine-like chapels and a large room for statues, of which only the plinths for the latter remain. (2) The statue room was originally decorated with scenes painted on mud plaster, only exiguous traces remaining. This room and both chapels were vaulted with mud tiles, similar in size to those found in the tomb of Horemheb. Some bricks stamped with the name and titles of Maya were also found. A doorway on the west side of the statue room gives access to (3) an inner courtyard, originally decorated with relief blocks of superlative quality. Some blocks remain *in situ*, but most were stripped away in antiquity. Some of the blocks seen and recorded by Lepsius have also disappeared since last century and only fragments of these have surfaced in museum collections. The court, which was colonnaded, had twelve columns, of which one incomplete example remains. It bears an inscribed panel. West of the court lies (4) the offering room, flanked by two side chapels. All were originally vaulted. The side chapels, like the statue room, once bore painted scenes on mud plaster. The offering room had a decorated revetment of limestone.

Lepsius saw part of the inner court in 1843, which he recorded in the *Denkmäler*. The Expedition found a number of other blocks not seen by Lepsius, on the reveals of the doorways leading into the chapels and on the west wall of the inner colonnaded court. Many loose blocks were found in the debris of the tomb, but their original location is not immediately apparent. The ancient architect built much of the pavement of the inner court and statue room from relief blocks taken from Old Kingdom mastabas and from the upper courses of the *enceinte* wall of the pyramid of Djoser.

The blocks recorded by Lepsius and rediscovered by us this season are in raised relief of the highest quality. Some of the loose blocks are in sunk relief, and it is probable that these came from the west wall of the outer court, once protected by the roof of the colonnade. A complete photographic record of the tomb was made during the excavations, and most of the inscribed and decorated material was recorded in facsimile, a task that will be completed when work resumes next year. By the kind permission of the Director of the Cairo Museum we were able to photograph the Maya blocks found by Quibell in the Monastery of Apa Jeremias, and these will serve as the basis for facsimile line-drawings in the eventual publication of the monument. Drawings have already been made of the few blocks from the tomb of Maya in museum collections.

Many intrusive objects and pottery sherds were found in the debris of the tomb. None bears the name of Maya or of his wife Merit. A few fragments of uncontexted Mycenaean wares were found. A curious discovery this season was part of a humerus of an elephant. Its age has yet to be determined. Work continued on the recording and analysis of the skeletal material from the shafts of Iurudef and Ramose, excavated in 1985 and 1986.

The Expedition completely cleaned and conserved the tomb of Maya, and replaced in position a number of blocks and fragments found loose or shattered. A double statue of Maya and Merit, seen by Lepsius *in situ* on the south side of the inner court, was found thrown face downwards, revealing the fact that the underside of the base was carved with a fine bas-relief of the Old Kingdom. The block from which the statue was made was at least 1.33 m thick, and may possibly have come from the mortuary temple or causeway of Unas. In addition to the work in the tomb of Maya, we restored and roofed the tomb of Pabes, found last season, and repaired the pillars and columns in the tomb of Tia and Tia. The ramp in the colonnaded court of the latter monument was also restored, as well as the columns and jambs in the antechapel. Next year it is hoped to finish the excavation of the outer court of Maya's monument, where it is expected the remains of a pylon will be revealed, and to begin the clearance of the substructure where so many fine reliefs are still in position.

El-Amarna: The director and part of the team assembled in Cairo on 17 January, and excavation started on the 26th. The staff comprised: Ann Bomann, Ahmed Galal, Christopher Kirby, Michael Mallinson, Angela Tooley (site supervisors); Maire Brison (registrar), Andrew Boyce, Fran Weatherhead, Pamela Rose, Paul Nicholson, Gillian Vogelsang-Eastwood, Willemina Wendrich, Delwen Samuel, Ian Mathieson, Robert Miller, and Anthony Leahy. The representative of the Egyptian Antiquities Organization was Ahmed Galal. The work was located in the Main City, just north of the principal wadi which crosses the city from east to west. Two areas were selected for investigation.

Late in the 1986 season, the site known as Q48.4 was identified as the most likely source of water for the Workmen's Village. It lies on the north edge of the large wadi, separate from the Main City, and had not been previously investigated. More than 2 m of fill were cleared over the whole surface of the well, whilst on the western side a trench was dug through the accumulated fill to the present water-table, reached at a depth of 8.70 m. The well had been made in two parts. The upper was a four-sided cut into the desert bedrock, with steeply sloping sides. At the top it measured approximately 9 m square. Lower down, its profile changed sharply, a narrow shelf running around the edge of a roughly circular shaft with irregular sides. This descended to the modern water-table. For the last 2 m the natural bedrock was soft grey sand (presumably the aquifer), easily undermined by adjacent water. Herein probably lies the explanation for the change of profile. The original well-diggers had to allow for some collapse of the central shaft, and therefore cut the upper part (in much firmer strata) wide enough to provide for several years of operation before the collapse reached its sides. As a delaying measure they packed an irregular and probably sloping layer of marl and other material over the sand to shore it up. Even so, by the time the well was abandoned, the process of decay had almost reached the stage at which it would have become dangerous.

The inward slope of the sides of the upper part made it impossible to raise water simply by lowering containers down the sides, and the distance across the upper part makes it unlikely that a well-head construction was built. The most likely method of use, therefore, was direct descent of the upper part, and letting containers down the shaft from the lower shelf. On the east side the configuration of the rock towards the top seems to include a narrow ramp sloping down from the desert surface to an upper shelf near the north-east corner. This continued round the north side, inclining downwards, with traces of a brick wall running down the edge. This is evidently a continuation of the access ramp which would have reached the lower shelf in the north-west corner of the well. Towards the bottom of the deep trench, large stone blocks were set into the marl lining as if to form a rough staircase. A surprising find was part of a limestone door-jamb bearing part of a hymn to Akhenaten. Whether it had fallen from a doorway above or was simply waiting to be used in the well lining remains to be seen. Substantial quantities of pottery in the fill and on the surface to the east confirm previous observations that water was drawn and transported in amphorae, many of them Canaanite in type. Furthermore, the discovery of sections of limestone conduit blocks in the adjacent building holds the possibility that water was also channelled into it.

Excavation in the adjacent building revealed a corner of a buttressed brick enclosure wall which probably surrounded the whole site, with the exception of the well which lay just outside on the north. A series of rooms had been built against the inner face of the wall, surrounding the open central space. Manufacturing of various kinds, including glazing and pottery making, was indicated by one (possibly two) puddling pits, a potter's wheel, several hundred mud sherds from unfired vessels, and two pottery kilns. In the adjacent areas several ovens were also found, but their purpose is uncertain. The mud sherds show a surprisingly wide range of vessel types, including cobra bowls and female figurines. Most of

the sherds had been made from Nile silt, but some seem to have been made from desert marl clays. Red and yellow colouring was also found on some. This discovery not only clarifies the manufacturing process of pottery, it also provides a basis for the recognition of other kilns. One likely example from the old German excavations was subsequently identified from the published plans and re-excavated, revealing a simple pottery kiln within the grounds of the private house P47.22. The pieces of vitrified kiln which occur widely over the whole site suggest that pottery making was quite common at Amarna.

The excavation of the building Q48.4 also yielded valuable chronological information. Several faience ring bezels were recovered, all bearing the prenomen of Tutankhamun. One lay at the bottom of the rubbish layer beneath the later, and probably principal, phase of building. Another from a good early context adds the epithet 'chosen of Amen-Ra' to the king's prenomen. This situation echoes that at the Workmen's Village, and implies again that in Tutankhamun's reign parts of the city were seen to have a definite future.

A long-term project is the study of a broad strip running from the old German/British excavations on the east to the edge of the cultivation on the west, based on a combination of excavation along the lines developed at the Workmen's Village, surface survey and limited sampling. Over much of the Main City the spaces between houses were used in ancient times for the dumping of household refuse. These dumps can often be identified from the surface, and detailed planning sometimes reveals the origin of a particular dump. Since the original contents of a house will often lie outside it, the old method of concentrating on the interior of a house can give only a limited picture of the archaeological characteristics of a given household. The first sample area lies adjacent to the west enclosure wall of the large estate Q46.1. A detailed contour and archaeological map was started, as was excavation on one medium-sized house mound, and surface survey of the pottery on the ancient dumps. A block of four 5-m squares was completely excavated, exposing the greater part of one house together with outbuildings which contained a quern emplacement and an oven. Part of the staircase to the roof, many fragments of the collapsed roof, and a charcoal hearth were found. The house also produced much broken pottery and some lumps of what appears to be iron.

Ian Mathieson continued the resistivity survey which he began in 1983. His main project was to survey the strip of cultivated land between the Great Palace and the present river bank to detect any surviving architectural features and the line of the original waterfront. It proved possible to detect a regular dip in the underlying sediments, which probably reflects an ancient water's edge, but no ancient architectural elements were discovered.

A resurvey of the small Aten temple (*ḥwt 'Itn*) was started by the architect Michael Mallinson. This building was completely cleared by Pendlebury and a small-scale, outline plan published in *City of Akhenaten* III. Clearance between the front brick pylons brought to light an elaborate gypsum foundation for a limestone platform which had survived remarkably well since its first exposure in 1931. Impressed into the gypsum were over seventy mason's marks originally carved on the now vanished pavement blocks. Further examination of the foundations, and clearance of the large brick altar in the middle of the first court, showed that Pendlebury's plan is a composite of perhaps three different building phases. In the earliest, the principal element was a large brick altar, later demolished. The final phase, by which time the pylons had been erected, saw the addition of the stone-paved platform, ramps, and pavement between the pylons. A ring-bezel dates this last alteration to the reign of Smenkhkara.

Amarna is unusual in the number of its wells, visible on the surface as rounded depressions, and variously located within administrative buildings, within private gardens, or as public wells serving groups of small houses. Dr Robert Miller began a surface survey, concentrated in the zone west of Q46.1, to establish basic data on their distribution and frequency in a sample area. Two projects at the dig house were the making of pottery bread

moulds by Paul Nicholson and the production of loaves by Delwen Samuel. Replicas of the moulds discovered last year in Chapel 556 were made using local marl clay and firing them in a simple kiln based on that in Chapel 556. A quern emplacement, stone mortar, and bread oven of ancient type were also constructed. Using Eighteenth Dynasty equipment and techniques, it proved possible to grind flour of reasonable quality. One batch of loaves was also successfully baked.

Pamela Rose and Paul Nicholson continued recording the pottery, with increasing emphasis on reconstructing vessels with a view to creating a fresh corpus of drawn shapes. Fran Weatherhead completed the restoration of a second panel of wall paintings from the Main Chapel in the Workmen's Village. Andrew Boyce concentrated on publication drawings of material for the final Workmen's Village volume, and has finished a fine bead corpus. Willemina Wendrich catalogued the large collection of basketry, matting, rope, and other fibre from the Workmen's Village. Gillian Vogelsang-Eastwood completed her work on the remaining Workmen's Village textiles. Delwen Samuel studied soil samples for botanical remains, and began a reference collection of plants currently growing in the neighbourhood. Finally, Dr Anthony Leahy worked for a week on the 130 hieratic jar labels discovered since 1982, most from the Workmen's Village, and containing well-known formulae referring to regnal years and commodities.

Egyptology has suffered its quota of losses in the past year, including those of Shehata Adam, formerly Director-General successively of the Centre of Documentation and the Egyptian Antiquities Organization, and Professor Alexander Badawy, whose three-volume *History of Egyptian Architecture* is his monument. We have also lost Klaus Baer, Professor of Egyptology at the Oriental Institute, Chicago, who will be remembered for his wide-ranging contributions to the subject, and particularly his seminal *Rank and Title in the Old Kingdom*; Dr Eve (Jelinkova-)Reymond, former Reader in Coptic at the University of Manchester, who produced many books and articles (latterly specializing in demotic), of which her *The Mythical Origin of the Egyptian Temple* perhaps reached the widest audience; and Elizabeth Riefstahl, author of *Thebes in the Time of Amunhotep III*, who contributed much to the growth of the Wilbour Library in the Brooklyn Museum, where she was formerly Librarian and Associate Curator of Ancient Art, and who has died at the age of 97.

Finally, Dr C. N. Reeves contributes the following appreciation of Elizabeth Thomas:

News of the death of Elizabeth Thomas, on 5 December, 1986, in Jackson, Mississippi, will be greeted with sadness by all who had the good fortune to know her. Born on 27 March 1907, in Memphis, Tennessee, Miss Thomas came to Egyptology relatively late in life: she first visited Egypt as a tourist in 1938, and shortly afterwards enrolled as a student at the Oriental Institute, Chicago. During the war, she worked as a cryptographer for the US government, and was not able to resume her studies until 1947. In 1953, she received her MA for a study of Cosmology in the Pyramid Texts. This was followed by a further visit to Egypt, during which she became interested in the Theban royal tombs. Several months of feverish activity in Luxor in 1959/60 resulted in the publication, in 1966, of an indispensable reference work, *The Royal Necropoleis of Thebes*. It is good to know that a second, revised edition was substantially complete at the time of her death, and that we may hope to see it published in due course. Miss Thomas's single-minded devotion to her chosen field of study

was truly remarkable, and served as a constant source of inspiration to her many friends and co-workers. She will be very much missed.

We should also like to notice the retirement of Mr Graham Halsey, for many years Commercial Office Manager at the University Printing House, Oxford, who, in the regular publication of the *Journal* and other EES publications, has ever kept the interests of the Society in mind and has done much to ensure the maintenance of the high standards expected from Oxford.

THE SAQQÂRA NEW KINGDOM NECROPOLIS EXCAVATIONS, 1986: PRELIMINARY REPORT

By GEOFFREY T. MARTIN

Three tombs are described, all excavated by the EES–Leiden mission at Saqqâra in 1986. The first was built for Khay, ‘goldwasher of the Lord of the Two Lands’, and his wife, ‘the chantress of Amun’ Tawerethetepi. Most of the scenes are extant. The second, somewhat weather-damaged, is that of his son Pabes, ‘chief of bowmen of the tradesmen’. Both tombs are new architectural types for the Memphite necropolis. The third tomb dates to the end of the Eighteenth Dynasty, and was erected for Ramose, ‘chief of bowmen of the army’. One of the two shafts in the tomb leads, by way of a robbers’ breakthrough, into the subterranean part of the tomb of Maya, treasurer of Tutankhamun, and his wife Merit, where a fine decorated chamber was found. The report concludes with details of work carried out on the skeletal material found in the 1985–6 seasons.

THE joint EES–Leiden expedition continued its programme of research in the New Kingdom Memphite necropolis in 1986, working on the site from 8 January until mid March. The staff comprised G. T. Martin (Field Director), K. J. Frazer (surveyor), D. A. Aston (Birmingham University, pottery recorder), and Barbara A. Greene (University of California at Berkeley, pottery recorder). Representing the Leiden Museum were M. J. Raven (objects recorder), R. van Walsem (Leiden University, objects recorder), J. van Dijk (Groningen University, philologist), and P.-J. Bomhof (photographer). All staff members assisted in the day-to-day supervision of the excavations. The anthropological team consisted of Roxie S. Walker, Professor R. Heglar (San Francisco State University), and Kathleen O. Arries. We owe a continuing debt of gratitude to Dr Ahmed Kadry and to the members of the Permanent Committee of the Antiquities Organization. On this occasion we thank Professor Gaballa A. Gaballa, Dr Mohammed Saleh, and other colleagues who authorized and participated in the division of antiquities that took place on site at the end of the season. At Saqqâra itself, we worked in close co-operation with our colleagues and friends of the Antiquities Inspectorate, of whom we especially thank Mohammed Ibrahim Aly (Director of Saqqâra), and our inspector this season, Mahmoud Abu el-Wafa Ahmed.

Having exhausted the possibilities in the tomb of Tia and Tia and the tomb chambers of Iurudef in the previous seasons, we now turned our attention to a rectangular area immediately west of the latter and close to the tomb of Horemheb, in the expectation that a small group of monuments would emerge that would throw light on the status of the officials who were permitted to erect their tombs near those of members of the royal family and the highest courtiers of the land. Immediately west of Horemheb’s tomb, we had already excavated and published two tomb-chapels of persons of relatively minor rank.¹ Our predictions this year were fulfilled,

¹ Martin *et al.*, *The Tomb-Chapels of Paser and Racia at Saqqâra* (London, 1985).

three more chapels coming to light, again of persons of comparatively humble status. Whether such officials were related to the owners of the great tombs, or whether they were dependents, members of their entourages, or concerned with the maintenance of their funerary cults, has yet to be revealed. At any rate, we are rapidly accumulating details of Memphite families of all ranks, especially those who lived in the New Kingdom, which was one of my original intentions in initiating the work in the necropolis in 1975. Further work should reveal some aspects of the 'settlement' pattern of the Memphite city of the dead.

Our first discovery in 1986 was the limestone chapel of the *irw n nb tswy Hcy*, '(gold)washer of the Lord of the Two Lands, Khay', and his wife, 'the lady of the house, chantress of Amun, Tawerethetepi' (see pl. I and figs. 1-3). Among the other titles or variants of titles held by the tomb-owner are *irw nbw*, *irw nbw n pr-hd*, and *hry pdt n šwyw n pr-hd pr-ḥ-ḥw-wdḥ-snb*. Other members of the family are named on the reliefs and in the inscriptions, including a son Pabes, whose tomb is located immediately behind the chapel of his parents.

Khay's chapel is a simple structure consisting of a forecourt, anteroom, and two rooms for the cult. The front of the building was open, and the exterior and interior

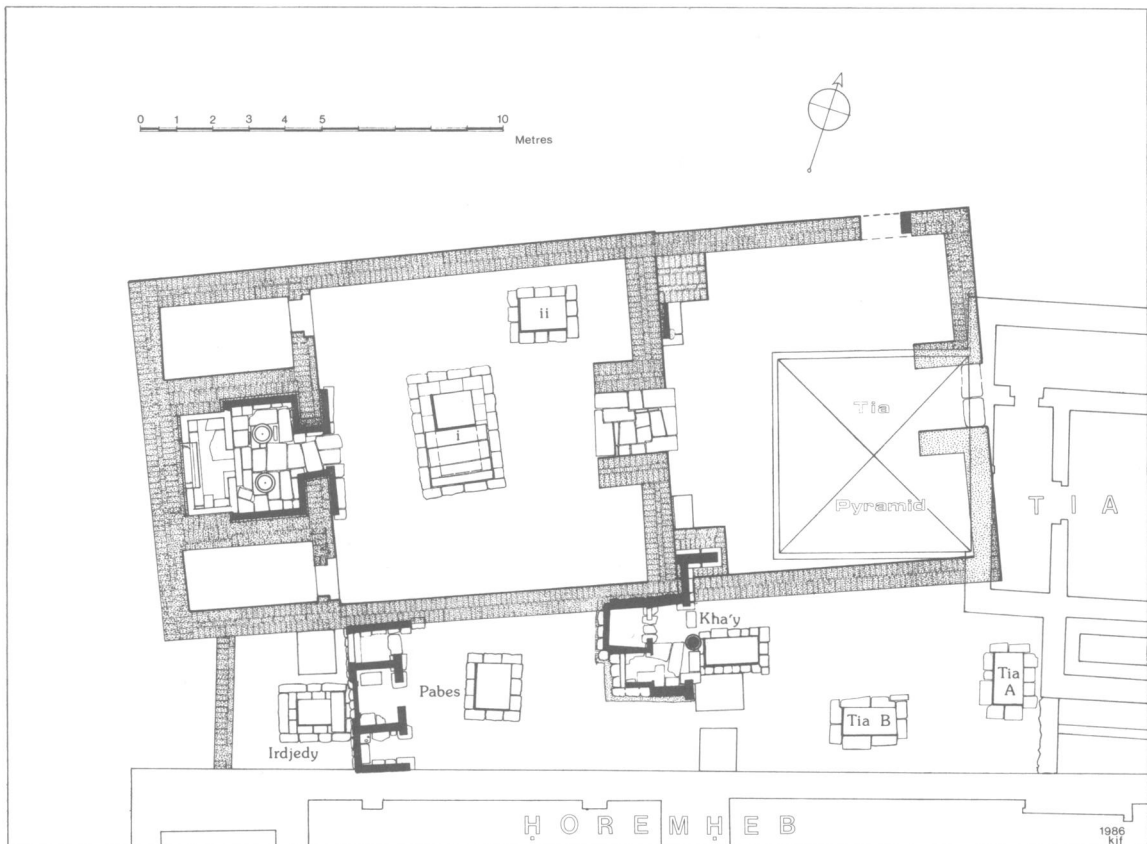


FIG. 1. Plan of tomb of Ramose, showing position of tomb-chapels of Khay and Pabes

walls, and a column half-way across the entrance, carried the weight of the roofing slabs, which themselves may have supported a brick pyramid and stone pyramidion (see below, scene 3), although no remains of these latter features were found. The tomb-chapel is a new type for the Memphite necropolis.

The scheme of decoration is almost complete, though somewhat weather-worn. Much colour is preserved. A synopsis of the principal scenes may be given here:

Scene 2. Two registers. Above, Khay and his wife kneeling and offering to Osiris. Below, the couple receive the sacred oils (five, rather than the usual seven) from their son Khay-amun.

Scene 3. Two registers. Above, Osiris seated with Isis; Nephthys behind. Below, mummy of the deceased supported by a priest, with widow mourning in front. Behind the priest is a depiction of the tomb-chapel with a pyramid on the roof. In front of this scene a man censuring before an offering table, followed by four mourning women.

Scene 4. The central stela, comprising (a) a decorated frieze showing Khay kneeling and adoring a recumbent Anubis jackal; (b) cavetto cornice; (c) recessed

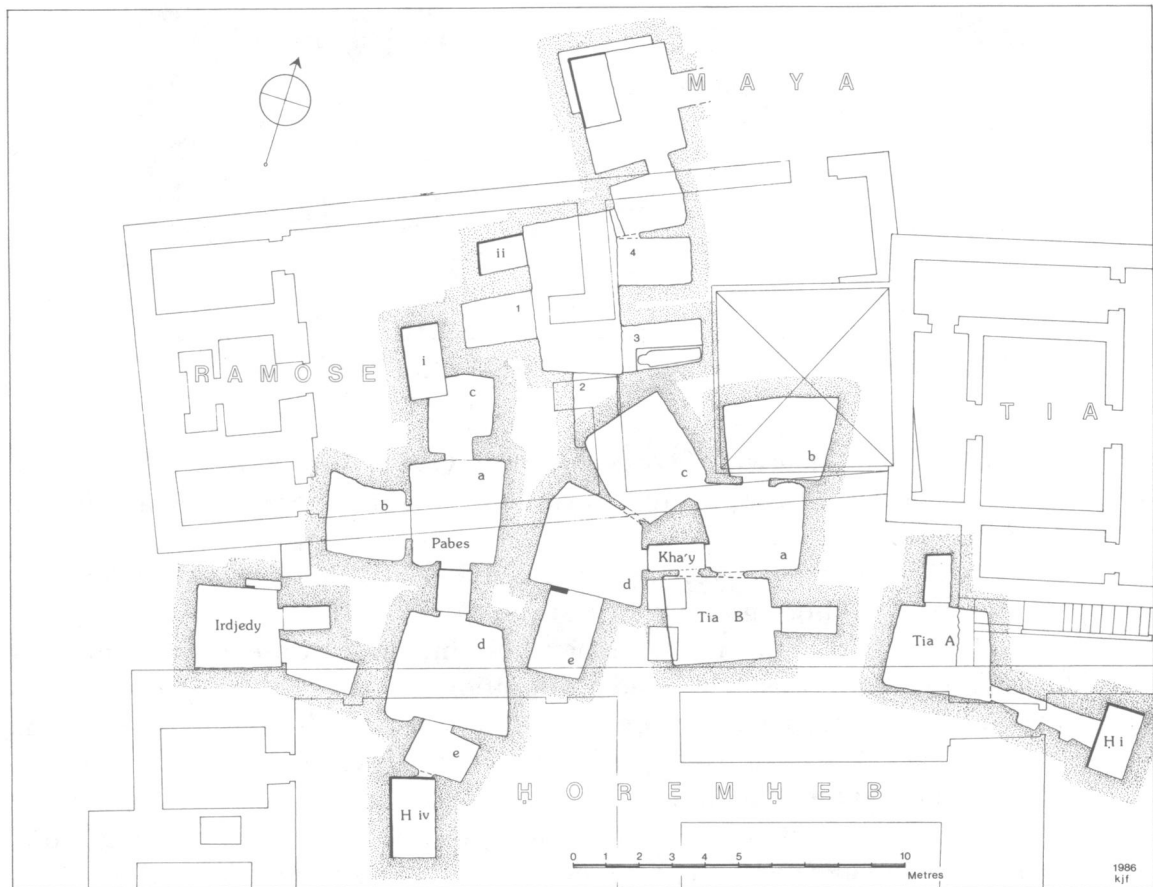


FIG. 2. Plan of substructures

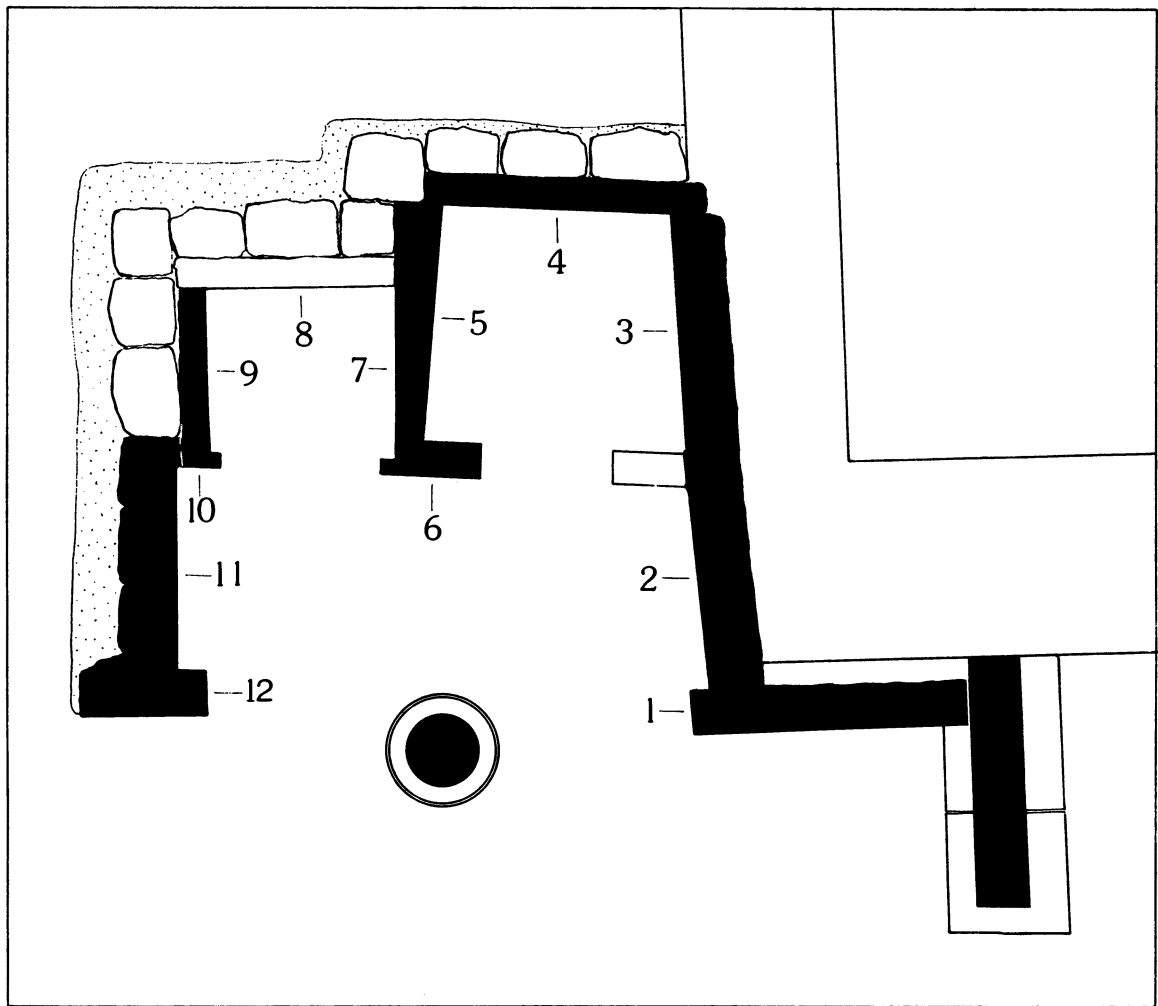


FIG. 3. Plan of tomb-chapel of Khay

stela in two registers showing Khay adoring Osiris and offering flowers to Re-Harakhty. The seated couple are shown below, being offered to by a man and woman; (d) inscribed frame.

Scene 5. Two registers. Above, vignette from the Book of the Dead. Below, the seated couple receive offerings from two of their sons.

Scene 7. Three registers. Above, Khay and his wife adore Osiris and Re-Harakhty. Middle, Khay raises his hand in adoration to the Hathor cow, and with his left hand supports a bowl positioned on his shoulder. Below, Khay superintends six workmen who are shown busying themselves with various activities in the goldworker's atelier. One man works bellows with his feet.

Scene 9. Three registers. Above, Khay and his wife behind an offering table, being censed and libated. Middle, nine persons standing, some carrying papyrus stems. Below, eight persons standing, some likewise carrying stalks.

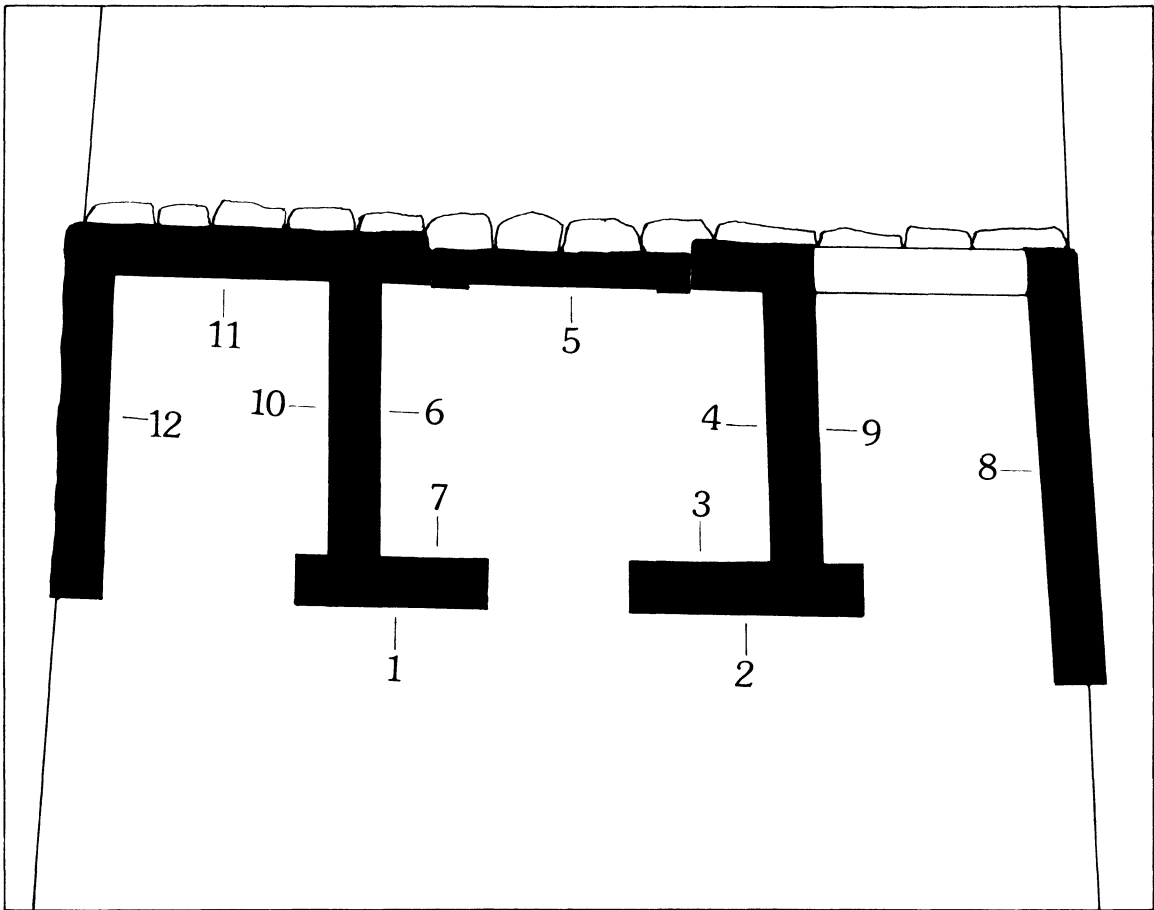


FIG. 4. Plan of tomb-chapel of Pabes

We date the tomb-chapel to the Nineteenth Dynasty, probably *temp.* Ramesses II. The walls of the monument were consolidated and built up with stone by the expedition to support a protective roof. Likewise, all the reliefs were cleaned and freed of salt incrustation, the work being carried out by the conservation staff of the Antiquities Inspectorate at Saqqâra. A full photographic, epigraphic, architectural, and descriptive record was made for publication, as also in the case of the two other monuments to be described. The substructure yielded a number of funerary objects, closely datable, including the remains of a coffin decorated with gold leaf, and many shabti figures. Skeletal material was plentiful.

The tomb of Khay's son Pabes lies immediately to the west (see pls. I, 1 and II, 1 and figs. 1, 2, 4). It is a simple tripartite chapel of limestone, preceded by a flagged forecourt, and is greatly weather-worn and fractured. Again, it is a new architectural type for the Memphite necropolis. The owner's main titles are *sš nsw mꜛ n nb tꜣwy*, *hry pꜣt šwyw*, 'true royal scribe of the Lord of the Two Lands', 'chief of bowmen [= overseer?] of the tradesmen'. A full description of the reliefs and texts on the

walls will be reserved for the publication. In the interim, scene 4 may be singled out for mention. Only the lower register is preserved, divided into two sub-registers. Above, four men carry objects on their shoulders, including an ingot.² Before them a man sets the scales of a balance. Beneath are three boats with masts down. Clearly, the receipt of materials is depicted doubtless taking place at the dockside in Memphis. We date the tomb-chapel to a period slightly later than the reign of Ramesses II. The substructure yielded funerary objects and skeletal material in abundance.

The forecourt of the tomb of Khay abuts on the south wall of the court of another tomb, already in existence when the two just described were constructed. This is a mud-brick tomb-chapel of simple type, consisting of an open court with two shafts, and a centrally positioned offering room on the west flanked by side chapels (see pls. I, 1 and II, 1 and figs. 1-2). An outer courtyard was partly masked or obliterated when the pyramid of the tomb of Tia and Tia was erected under Ramesses II. The owner, who was identified from the remains of a stela set against the north side of the west wall of this court, lived in the late Eighteenth Dynasty: the *hry pꜣt n pꜣ mšꜥ Rꜥ-ms*, 'the chief of bowmen of the army Ramose'. A stela in E. Berlin (no. 7306)³ evidently comes from the offering chapel, which was the only part of the tomb to have an interior revetment of limestone, now stripped away except for the foundation course. The chapel may have been unfinished in any case, to judge from a flake of limestone giving the name and titles of Ramose, 'true royal scribe, beloved of him, chief of bowmen', outlined in red paint but never carved. Part of another stela, found in the subterranean room E of the tomb of Khay, also names Ramose and his wife, 'the lady of the house, Wiat', and it was doubtless originally positioned, as a counterpart to the one already mentioned, on the south side of the doorway leading into the main courtyard.

The main shaft of the tomb, which contains scattered skeletal remains, probably all intrusive, is currently under investigation. Concurrently we were working the second shaft, destined probably for members of Ramose's family, and situated in the north-east corner of the court. At a depth of 10.7 m the shaft opens into a large, rough-hewn chamber with side emplacements for burials and containing a stone sarcophagus of late date, much skeletal material, and hundreds of mass-produced shabtis of Thirtieth Dynasty types. No trace was found of any late Eighteenth Dynasty material. The extant skeletal remains, though scattered (apart from those in the sarcophagus), are closely datable, and are being recorded within the framework of our anthropological programme.

A robber's breakthrough at the back of the chamber leads via a rough passageway into the subterranean parts of an adjoining tomb, evidently (like that of Horemheb shaft iv) on more than one level. A large rough chamber, partly altered by the addition of rock-cut loculi of late date, is encountered first, with other rooms leading

² On the question of ingots see A. Nibbi, *DE* 4 (1986), 41-65.

³ PM III², 733. I am indebted to Dr K.-H. Priese for supplying a photograph of the stela. The owner's titles are *sš nsw, hry pꜣt n nb tꜣwy*, and *idmw n pꜣ mšꜥ*.

off. The late date of the intrusive burial emplacements is confirmed by the presence of faience shabtis curiously, and perhaps uniquely, plastered into a corner of the room. An empty shaft 7.0 m deep descends from this room, and a door at the bottom gives access to a stairway, encumbered with debris, leading to a rectangular chamber, entered by me and Dr van Dijk for the first time on the morning of 8 February.

Although we had already realized, from the scale of the subterranean parts, that the tomb we had entered was that of an important official, we hardly expected his name to be forthcoming, since we have no New Kingdom precedents at Saqqâra for inscribed or decorated tomb chambers. Even the tomb of Horemheb, presumably the most important of its period (*temp.* Tutankhamun) in the Memphite necropolis, had no inscriptions below ground, and the wall and ceiling decoration were mere linear patterns. It was therefore with surprise that we found the antechamber (see pl. II, 2) to be lined with fine relief slabs of limestone in good condition (though partly smashed by robbers) with decoration including representations of the tomb-owner adoring various deities. This room is certainly not the burial chamber proper, but its decoration gives an inkling of what we may expect to encounter when the interior parts of what must be an extensive underground complex are reached. The texts accompanying the reliefs immediately revealed that the tomb⁴ is that of Maya, Overseer of the Treasury in the government of Tutankhamun and his two successors on the throne, Ay and Horemheb. His wife Merit, chantress of Amun, is also depicted and named. The reliefs are on a large scale and in good condition considering the prevailing humidity. Yellow, or yellow-gold, is the predominant colour, with small details picked out in blue and black. Monochrome decoration of this kind, found later at Deir el-Medîna,⁵ is not otherwise known in the Memphite necropolis at this period, and the overall effect is decidedly regal. Shortage of space precludes more than a brief description of the discovery, the tomb itself awaiting excavation in the future.

As long ago as 1975⁶ we had been given permission by the Organization of Egyptian Antiquities to relocate the tomb of Maya, which had last been seen in 1843 by K. R. Lepsius. His plan of the area south of the causeway of Unas was utilized by Mr Frazer in 1975 in our initial reconnaissance, and the location of the tomb narrowed down to a rectangle of the desert. As we suspected when the tomb of Horemheb was discovered we were within metres of the tomb of Maya. A part of the superstructure of the latter monument had been seen and recorded by Lepsius, but it is certain that the substructure has not been entered since antiquity. The lack of inscribed objects of Maya and Merit⁷ in museum collections gives rise to the hope that some may still be in the tomb, although it is virtually certain that the burial

⁴ LS 27, PM III², 661-3, adding E. Graefe, *Porticus*, 4 (1981), 1-8 (block in Memorial Art Gallery, University of Rochester, NY, no. 42.55); J. Berlandini-Keller, *Connaissance des Arts*, 413-14 (July-Aug. 1986), 62-9 (including two photographs of the new reliefs discovered by the EES-Leiden expedition, one in colour).

⁵ B. Bruyère, *Tombs thébaines de Deir el Médîneh à décoration monochrome* (Cairo, 1952).

⁶ Martin, *JEA* 62 (1976), 6.

⁷ Cf. PM III², 663 (cubit-measure). For the three statues found doubtless in the superstructure, loc. cit.

chambers have been plundered. On one side of the anteroom a doorway (see pl. II, 2), originally masked with carved relief slabs, gives access to a long corridor, full practically to the ceiling with rubble deposited after the interments to seal the area. The ancient robbers removed a minimal amount just under ceiling level in order to crawl over the top to the burial chambers. Since they did not go to the trouble of removing the entire fill, any large objects, such as sarcophagi, are still probably in position in rooms at the end of the corridor. We were able to glimpse from a distance, over the rubble, two more painted rooms, which may well be the burial chambers. It should be emphasized that our initial exploration of the substructure was only tentative, partly because of the potential danger (at a depth of some 20 m or more) and the probability of disturbing material, partly because we did not wish the new discovery to deflect us from our original programme of work for the season, namely, the recording of the tombs described above.

There is hardly need to emphasize the enormous potential interest of the tomb of Maya, particularly if it yields new historical material,⁸ which is probable on the analogy of the adjacent and contemporary tomb of Horemheb. The superstructure will doubtless prove to be on a par with the latter monument, and to judge from the reliefs recorded by Lepsius (some of which must still be *in situ*), material of great artistic and iconographical interest will be forthcoming.

Finally, Mrs Walker reports on the work of the anthropological team in 1986 as follows:

In the course of the 1986 season we continued the sorting, conserving, and standard anthropometric recording of the human material excavated from the tomb of Iurudef in 1985. Owing to the failure of the X-ray machine left in Cairo over the preceding year, radiological studies could not be done this season. We are, however, now able to characterize the two populations buried in the tomb of Iurudef in terms of date, probable social status, and probable lifestyles and health, as well as the more basic data on age, sex, and demographic profiles. Owing to space constraints detailed analyses are not included in this report. All of the skeletal material has been returned to storage as a permanent reference collection for future population biology studies of ancient Memphis.

The burned bone fragments from the upper chambers of Iurudef yielded four immature, one adult female, and three adult male burials, as well as fragments of six additional individuals which were too incomplete for age and sex estimations. The principal male burial appears to be that of a very large adult with little muscle-pull relative to the population in general, suggesting a fairly sedentary life consistent with the known titles and duties of Iurudef.

The water-damaged burials from the lower complex yielded a final count of twelve immatures, six adult females, and two adult males. On the whole, these Nineteenth Dynasty burials in both upper and lower complexes showed less severe physical attrition (such as marked muscle-pull, arthritides, dental attrition, and healed fractures) than the later intrusive burials (in the upper complex), suggestive of fairly distinct class or occupational differences in the two populations.

After the anthropology team had left Saqqâra, the skeletal material excavated from the shaft (as opposed to the chambers) of the Iurudef tomb in 1982, and stored in the field

⁸ A recently published stela fragment in Liverpool sheds new light on his administrative activities, year 8 of Tutankhamun; see Amin A. M. A. Amer, *RdE* 36 (1985), 17-20.

magazine, became once again accessible. The material was moved to Horemheb Magazine B for definitive study in 1987.

A great deal of new skeletal material was excavated in the course of the 1986 season. Owing to the unexpectedly large number of skeletons and the greatly disturbed condition of most of them, the vast bulk of the material has simply been stored in the Horemheb Magazine for sorting, conservation, and study in 1987. Two complete burials were found and have been separately catalogued. A preliminary assessment of this material, most of which came from the burial chamber and niches of Shaft ii of the tomb of Ramose, suggests a probable count of 150 individuals.

A final development from the anthropological viewpoint has been the initiation of negotiations between R. Walker and the Organization of Egyptian Antiquities for a permanent anthropology research centre to be based at Gîza, using Reisner's storehouses and adding a conservation laboratory, radiography facilities, study rooms, a research library, and residences for visiting scholars. A proposed auxiliary facility at Saqqâra is planned as a resource centre for all excavators working in the Saqqâra necropolis.



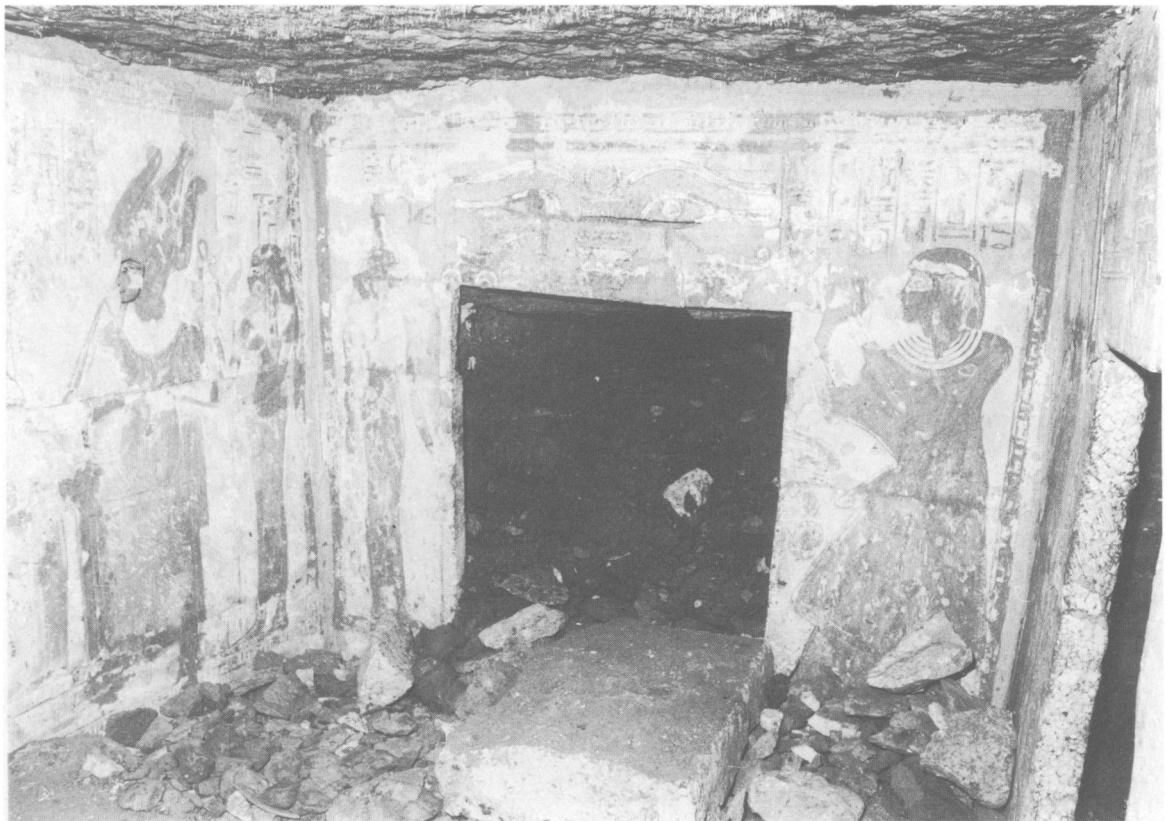
1. Tomb of Khay (reconstructed) with tomb-chapel of Pabes behind. In foreground, tomb of Ramose; in background, tomb of Horemheb. From north



2. The same, from west. In background, Tia and Tia



1. Tomb-chapel of Pabes to left, with tomb of Ramose beyond, showing position of subsidiary shaft (arrowed) giving access to tomb of Maya. From south



2. Scene in subterranean chamber of tomb of Maya, showing corridor leading to burial complex

MEMPHIS 1985

By D. G. JEFFREYS, J. MALEK, and H. S. SMITH

The survey of Memphis continued its excavation at Kôm Rabî'a in the south-west quadrant of the Memphite ruin field, finding evidence of terracing up to the north and west and a substantial rebuilding of the central property not earlier than the reign of Tutankhamun. The Epigraphic Section began its survey of the Ptah temple with a study of the colossal limestone statue of Ramesses II at the south gate. Work proceeded at Saqqâra on publication of the Mother of Apis stelae from the Sacred Animal Necropolis, and on the ceramic collection from the Anubieion.

Project review

As the Memphis project has now been running for five years, a brief review of its aims, achievements, and future prospects is presented here as a general background to the results of the 1985 season.

The science of Egyptology has reached a point at which, although there is available a rich store of textual, archaeological, and environmental evidence, it is difficult to study Egyptian culture as it was at any particular time because of the incomplete state of our knowledge. Only rarely is the documentary and archaeological source-material available from one site, and even when this is so (as in the case of Deir el-Medîna for the late New Kingdom), the community illustrated is often of an exceptional character.

One way to improve our resources is to concentrate on a particular region of Egypt and to study it from every possible aspect through its entire history. The region of Memphis stands out as one of exceptional importance because of its commanding position immediately above the southern apex of the Nile delta, which made it a centre of government in ancient Egypt for three thousand years. Although much work has been done at Memphis, some of it on quite a large scale, this has (with honourable exceptions) been poorly published and totally uncoordinated, yielding little coherent account of the city. Yet the surviving ruin field is one of the largest and best-preserved of Egyptian city sites.

Our initial objectives were simple and practical ones: archaeological survey and mapping; environmental survey; epigraphic recording; study of contemporary Egyptian documents, and of medieval and modern topographical sources; excavation within the valley site.

By December 1983 the whole of the central ruin field (with the exception of areas occupied by military camps and modern housing) had been mapped and over 150 previous excavation sites located and recorded. The full account of this survey is now available,¹ comprising maps, site descriptions, and topographical discussions. One finding in particular is of far-reaching importance: that, contrary to common

¹ D. G. Jeffreys, *The Survey of Memphis*, 1 (London, 1985).

opinion, the Nile has been moving eastwards at Memphis, probably throughout its history. This is based on resistivity-meter survey² and core samples, which provide evidence of riverine fauna,³ and other environmental and sedimentary evidence. With the assistance of a grant from the Leverhulme Trust Jeffreys will be pursuing this project over the next three years.

Limited stratigraphic excavation was begun in October 1984 at a site on Kôm Rabi'a in the south-west quadrant of the ruin field. An artisans' quarter was discovered outside the south-west corner of the great Ptah temenos wall, contemporary with the construction of the surviving buildings in the mid thirteenth century BC. Since the level of this settlement was as much as 3 m higher than the floor of the main Ptah temple and other contemporary sanctuaries to the east, the excavation has confirmed indications from the survey that the early city had grown into a *tell* before the late Eighteenth Dynasty, and that the new temple quarters erected by Ramesses II represented a huge eastward extension, probably on virgin land reclaimed from the receding river. Excavation down to levels of the early Eighteenth Dynasty has revealed a slope downwards both to the east and to the south, perhaps in relation to the contemporary river-front (and tributaries?). The excavation provides the first detailed stratigraphy for this and earlier periods, and its completion over the next two or three seasons will revolutionize our knowledge of the development of the city. The excavation and survey records have been stored by Jeffreys on computer at University College London (thanks to a research grant from the British Academy), and the ceramic data from the site (stored on computer at Cambridge) will form the basis of a new analytical corpus of Memphite pottery under the direction of Janine Bourriau (Fitzwilliam Museum).⁴

Although there has been partial publication of many of the decorated and inscribed monuments from Memphis, it is a sad fact that few have received a full epigraphic treatment. This large and difficult task is now being undertaken by Jaromir Malek (Griffith Institute, Oxford). So far the small Ptah temple east of the excavation site, the Ramesside temples along the south sacred way, the limestone colossus of Ramesses II ('Abû'l-Hôl'), and the throne-base and other blocks from the palace of Merneptah, have all been recorded, in addition to a number of smaller pieces. These will be published shortly in the *Survey of Memphis* series by Dr Malek and Helen McKeown, and the entire body of material is being kept on a computerized archive (SCHISM) at Oxford. Future tasks include the recording of the west gate of the Ptah temple. Work on other documentary sources is now in its early stages: evidence for cults of Amun is being studied,⁵ and work on demotic texts of Memphite provenance found at Saqqâra is in progress.

This work means that we now have a considerably better basis for understanding

² Undertaken by Mr and Mrs Mathieson from 1982 to 1985.

³ The faunal remains are being identified by Dr Eric Robinson, Department of Geology, University College London, to whom thanks are due.

⁴ A system of analysis using random sampling techniques has been devised to deal with the vast quantities of sherds.

⁵ By Mariam Kamish (Ph.D. thesis, University College London).

Memphis and its history than four years ago. The present projects and forthcoming enterprises will contribute much to the scientific investigation of the city. The results so far achieved have been greatly aided by generous support from the Institute of Egyptian Art and Archaeology at Memphis State University, Tennessee, whose Director, Rita Freed, and her students are participating in the project: we were particularly pleased to welcome Dr Freed and Earnestine Jenkins to the site this year. Large excavations of monumental buildings are beyond the Society's present resources, but settlement archaeology has a vital role to play in the investigation of Memphis. The excavators hope that the Egyptian Antiquities Organization, whose collaboration is so crucial, the Egyptian universities, and other foreign missions will be encouraged to undertake work at Memphis and elsewhere, as the American Research Center is currently doing at the Apis House site.⁶ To such missions the project will extend the fullest co-operation, and will welcome also co-operative epigraphic projects on Memphite pieces in Egypt or elsewhere: Memphis is too large and important a site for any one mission to be able to accomplish more than a small part of the work needed. Our aim is to serve the international scholarly community, and we welcome its response.

Memphis 1985

The Society's fifth season at Memphis took place from 8 October to 17 December 1985. The Society wishes to express its gratitude to Dr Ahmed Kadry, Head of the Egyptian Antiquities Organization, to Drs Mahmoud Abd el-Raziq and Nassif Hassan and Mr Ahmed Moussa, the officers responsible for the Gîza and Saqqâra areas, and to Mr Fouad Yaqoub, and their staffs for assistance in facilitating the work. Mohammed Ibrahim Aly, Director of Antiquities for Saqqâra, Mohammed Nagib, Chief Inspector at Mît Rahîna, and his Inspectors Mohammed Rashid and Ahmed Farghaly, all gave much courteous assistance in the day-to-day running of the expedition. The staff were H. S. Smith, D. A. Aston, J. D. Bourriau, R. Freed, P. G. French, H. M. Hecker, D. G. Jeffreys, E. Jenkins, J. Malek, H. McKeown, and I. M. E. Shaw. Mrs H. F. Smith undertook management of the camp as usual, as well as photography, H. S. Smith and C. A. R. Andrews studied Mother of Apis documents from the Sacred Animal Necropolis, and Mr French continued with analysis of the pottery from Anubieion. Boyce Driskell undertook microscopic examination of flint use wear in Cairo and London, and Ian and Padi Mathieson continued their resistivity-meter survey. Dr Thomas von der Way of the German Archaeological Institute in Cairo kindly lent drilling equipment for two weeks after the excavation. Mrs Deborah Keirle, the Society's honorary agent in Cairo, once more had great success in raising funds and obtaining material, while Mr C. J. M. Keirle kindly administered the Society's finances in Cairo. We also appreciate the help and constant support given by Mr Bryan Vale, Representative at the British

⁶ Under the overall direction of Professor B. V. Bothmer; Field Directors, Michael Jones and Angela Milward Jones.

Memphis

RAT 1985

Contours inferred (m ASL)

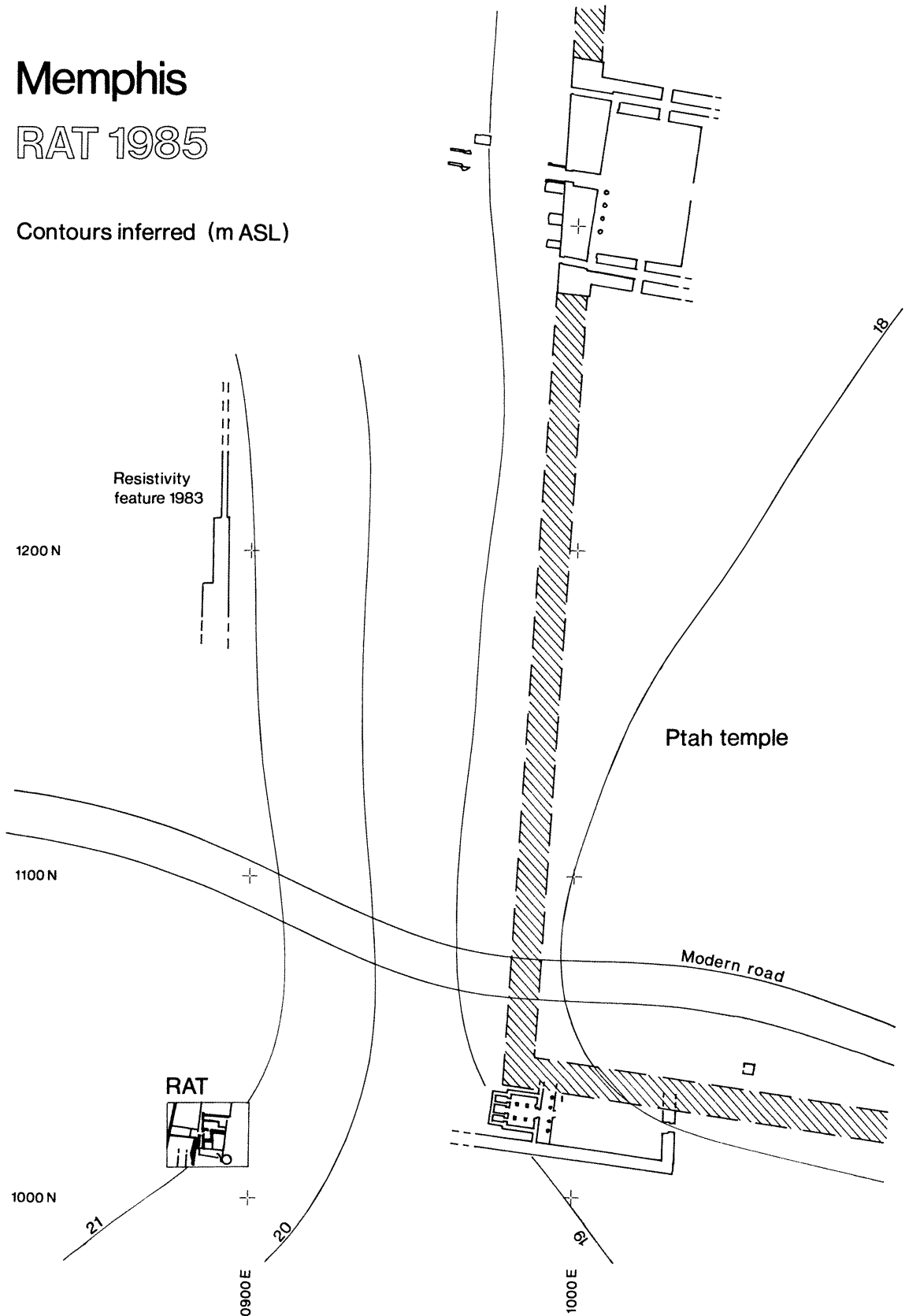


FIG. 1

Council, and his Deputy Mr Peter Mackenzie Smith. During the season the expedition built a small workroom at Mît Rahîna, adjoining the EAO magazines, which will enable recording and analytical work to be done in more sheltered and commodious conditions. We again thank the Memphis Inspectorate and Mr Ramsy of the Saqqâra architect's department for facilitating the construction, and *ra'îs* 'Abd al-Kreity for the contract work.

At the end of the season a discretionary award of small finds and study material was made by the Egyptian Antiquities Organization. The Society is indebted to Dr Muḥammad Şâleḥ, Director of the Cairo Museum, and his staff for arranging this. A number of small objects were granted to the Society as well as flint and sherd collections for study in the United Kingdom, and a number of the objects have been assigned to Memphis State University in recognition of their substantial support of the project.

Archaeological Section

The Archaeological Section of the Memphis Survey continued its excavation at Kôm Rabî'a (site RAT) in the south-west quadrant of the ruin field. The first season in 1984 had established that an undisturbed New Kingdom site lay here between a cemetery of the First Intermediate Period on Kôm Fakhry (see *Survey of Memphis (SoM)*, 1, fig. 8, FAC) and Ramesside temples to the east (*SoM* 1, BAA, RAB/C, RAG/H), and at a level consistent with the theory of a steep slope or bank parallel to the west side of the Ptah temple (see *JEA* 72 (1986), 4, fig. 2). Our main aim in 1985 was to determine the nature of this slope and assess the likelihood of reaching occupation of relatively early date (late Old Kingdom?) in future seasons.

Excavation with seven staff, twenty local workmen, and three specialist foremen began in October and lasted six weeks. Walls of Level II (see *JEA* 72, 5), now dated to the late Eighteenth Dynasty by a ring bezel of Tutankhamun from the central property, were removed to reveal a similar underlying ground plan (Level III) of the early to mid Eighteenth Dynasty (fig. 2), dated by scarabs and amulets of Tuthmosis III and Amenophis II. The layout of III differs only in its more open appearance, the community still being dependent on the silo in the south-east corner of the site, reached by a curving stair around its west side. Pottery recovered from the fills of the silo was identified as being as late as the ninth/eighth century BC, giving the feature a remarkable lifespan of as much as 600 years (though not necessarily always as a silo).

The south-west corner had been comparatively low-lying throughout III, and this was confirmed by a ring bezel of Sethos I from an early spill over the wall to the east. The only sign of settled occupation here was a series of stoves built in the north-east corner of the area (probably an open yard) which all post-date Sethos I, and pig bones and a coprolite, some of the bone showing signs of butchery or carving. The spills filling the yard covered a group of wall remains which pre-date III but still follow the same general alignment.

In the north-west corner the details of terracing down the slope to the east began to emerge: the broad wall along the west side of the exposure is now dated no earlier

Memphis RAT 1985 Level III

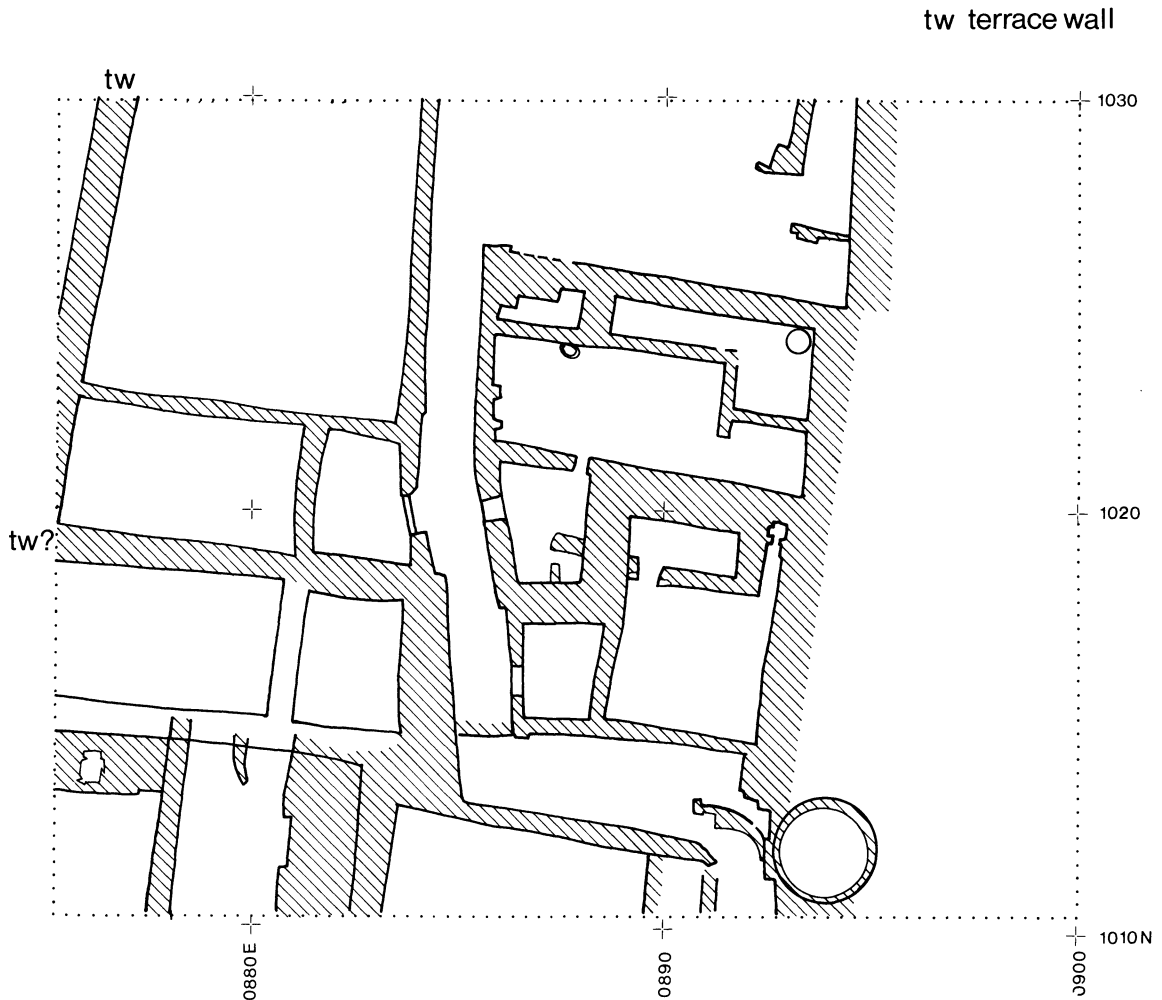


FIG. 2

than Tuthmosis III by a cartouche amulet buried in the foundations (perhaps an actual foundation deposit?), and retained a bank of rubble and earth containing pottery of the early Eighteenth Dynasty. A scarab of Amenophis II in the latest Level III floor in the central property (before the rebuilding in II) is from a near-contemporary context, as may be one of Tuthmosis I from the courtyard to the north. These deposits all give a clear indication of terracing up to the north-west (fig. 2). Beneath the western wall a deep (30 cm) layer of coarse sand was found, with small installations in the surface, including a pottery jar of Second Intermediate Period/early New Kingdom type (fig. 3). If contemporary, this would indicate a floor of *c.*1550 BC at an absolute level some 20 cm higher than the fourteenth-century

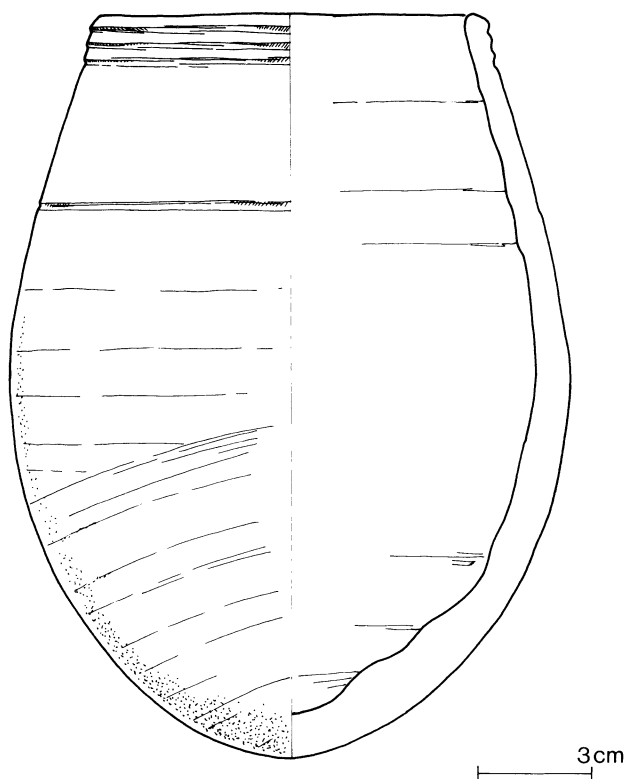


FIG. 3

floors to the east. The west-to-east slope at least is, therefore, an artificial feature not confined to the Ramesside period, and its nature will hopefully become clearer when we investigate it in 1986. In particular, it will be of interest to discover whether the sand layer is of natural deposition and represents a break in occupation in this area during the early part of the sixteenth century.

As an auxiliary project, a series of test drillings was carried out across the southern part of the ruin field from west to east, using a mechanical auger kindly loaned to the expedition by the German Archaeological Institute. The most westerly of these still produced artefacts (chiefly pottery fragments) from as deep as 88.6 m SD (10.85 m ASL), some 11 m below ground level. Since natural silts and sands were found more than 2 m higher than this at the excavation site, it is possible that an early waterfront existed between these two locations. To the east the natural alluvium was found at progressively higher levels, suggesting that the gradual recession of the river might be traced with some confidence by a closer grouping of drill sites.

A visit to the south-eastern part of the ruin field (Kôms Helûl and Qal'a) with Michael Jones and Angela Milward Jones of the Apis House project (New York University/American Research Center in Egypt) provided an opportunity to examine an area that had been under military occupation during the survey project in 1982-3. To the west-south-west of the modern 'Ezbet al-'Arab on the east side of

Helûl, a former excavation site was noted, with much fired brick-earth and spoil dumps covered with faience slag and many vessel fragments, some spoiled. The site is almost certainly where a group of faience kilns was noted by Petrie in 1886 and excavated by him in 1908.⁷ The data published in *SoM* 19, figs. 7, 8, should therefore be emended so that site HAA lies some 30 m west of the position shown.

Epigraphic Survey

Large royal statues had probably always been a characteristic feature of Memphite temples and their approaches. During the reign of Ramesses II, in particular, the city must have presented an awe-inspiring sight, with colossi of the king set up at the gates and perhaps even at important crossroads. A study of these sculptures and their distribution can provide clues to the town's ancient topography and its chronology, as well as elucidate the special aspects of these statues. This is the current task of the Epigraphic Section of the expedition. For the purpose of this study, we define a colossal statue as one which is at least twice life-size, i.e. over 3.50 m tall. This definition excludes private statues⁸ as well as a number of royal statues which, although over life-size, still differ markedly from real colossi.⁹

The preliminary list of Ramesside royal colossi known from Memphis contains fourteen entries, all except one standing (where the type can be established). Several of these are fragments which may eventually prove to belong to the same sculpture, while some of the debris scattered in the compound behind the Museum at Mît Rahîna may belong to colossal statues and will have to be added to the list (we have not examined this material and so it is not fully included). We have been unable to study *in situ* some other sculptures mentioned in early recordings and descriptions of the site as their present location is uncertain and the reports are too vague, but a figure of twenty colossi at Memphis during the Ramesside Period is probably not unduly optimistic.

*Limestone*¹⁰

L1 = SCHISM 51 ¹¹	'Abû'l-Hôl', Mît Rahîna Museum ¹²	Ramesses II
L2 = SCHISM 2654	Mît Rahîna Museum compound, double crown only ¹³	?

⁷ W. M. F. Petrie *et al.*, *Historical Studies* (London, 1911).

⁸ e.g. the female torso in quartzite found, according to J. Hekekyan, north of colossus G3, now in the museum at Mît Rahîna.

⁹ e.g. the standard-bearing statue of Ramesses II, Smith, Jeffreys, and Malek, *JEA* 69 (1983), 39; idem, *ASAE* 69 (1983), 94; the text discussed by Malek, *JEA* 72 (1986), 109-10; PM III², 837 (disregard the reference to Ramesses VI). Another, with two(?) standards, is listed by Hekekyan, MS 37452, 245^v, and another standard-bearing statue of Ramesses II was found at Hôd el-Wissâda, M. el-Amir, *ASAE* 42 (1943), 359-63, pls. xxi, xxii; PM III², 863. Note also the torso of a red granite seated statue of Ramesses IV, A. M. Moussa, *ASAE* 68 (1982), 119-20, pl. i; S. G. Gohary, *OrAnt* 17 (1978), 194-6, pl. xvii; PM III², 837 (disregard the reference to Ramesses VI), and the red granite head copied by Hekekyan and Wilkinson, PM III², 837 (there dated to the New Kingdom).

¹⁰ Petrie recorded a fragment of a limestone colossus in front of the West Pylon, in *Memphis*, I (London, 1909), 5. This is not included in our list.

¹¹ SCHISM = Systematic Corpus of Hieroglyphic Inscriptions and Sculptures from Memphis. Each monument from Memphis has been assigned an identifying number.

¹² PM III², 836-7.

¹³ Perhaps Ramesside.

*Red/pink granite*¹⁴

G ₁ = SCHISM 9	Mît Rahîna Museum compound ¹⁵	Ramesses II
G ₂ = SCHISM 10	Mît Rahîna Museum compound, in fragments, but almost complete ¹⁶	Ramesses II
G ₃ = SCHISM 1222	Cairo, Ramsîs Square ¹⁷	Ramesses II (secondary names of Ramesses IV)
G ₄ = SCHISM 1233	North side of the road from Mît Rahîna to Badrashein, in fragments (fist, BM 9) ¹⁸	?
G ₅ = SCHISM 3701	Mît Rahîna, Ptah temple, at the south side-entrance of the West Pylon, pedestal with feet ¹⁹	Ramesses II
G ₆ = SCHISM 2655	Mît Rahîna Museum compound, White Crown only ²⁰	?
G ₇ = SCHISM 702	Cairo Mus. CG 643, head, ²¹ found in front of the West Pylon	Ramesside
G ₈ = SCHISM 703	Cairo Mus. CG 644, head, ²² found in front of the West Pylon	Ramesside

*Quartzite*²³

Q ₁ = SCHISM 2653	Mît Rahîna Museum, fist	Ramesses IV(?)
Q ₂ = SCHISM 1057	Near the eastern entrance to Ptah temple when seen by Alice Lieder in 1853, fist ²⁴	Ramesses VI (perhaps just secondary)

Alabaster

A ₁ = SCHISM 3702	Ptah temple, in front of the West Pylon, probably seated, fragment ²⁵	? (presumably Ramesses II)
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¹⁴ Petrie, *Memphis*, 1, 5 mentioned a companion piece to colossus G₅, but did not publish any details, and as we have not yet been able to study it, it is not included in the list. Hekekyan listed a granite colossus near the north-western corner of the Ptah enclosure, which we have not located.

¹⁵ Recorded by us in 1981, Smith, Jeffreys, and Malek, *JEA* 69 (1983), 38; PM III², 846; illustrated in J. Baines and J. Malek, *Atlas of Ancient Egypt* (Oxford, 1980), fig. on p. 136.

¹⁶ PM III², 846. Already known to Hekekyan in 1852.

¹⁷ PM III², 840; in particular L.-A. Christophe, *BIE* 37/1 (1954-5), 5-19, pls. i-iv.

¹⁸ Smith and Jeffreys, *JEA* 71 (1985), 5-6, pl. I (1); BM 9, see PM III², 837 (provenance to be corrected). No doubt Ramesside.

¹⁹ Petrie, *Memphis*, 1, 5, 9-10, pl. xxiii (top and middle right).

²⁰ Perhaps Ramesside.

²¹ L. Borchardt, *Statuen und Statuetten*, II (Cairo, 1925), 188, pl. 118.

²² Ibid. 189, pl. 118.

²³ Hekekyan mentions three colossal royal statues 'in red breccia', perhaps including Q₂ (see next note), to the north of colossus G₃. Fist-fragment Q₁ may belong to one of these.

²⁴ The text published by J. Malek, *JEA* 72 (1986), 106-7. It is probably the same as Wilkinson's 'lost colossus' (Wilkinson MS xiii.31 (right), reproduced by Christophe, *BIE* 37/1 (1954-5), pl. vii), and may be Hekekyan's 'colossal statue in fine sandstone breccia in Horner's excavation', not far from the find-spot of granite colossus G₃.

²⁵ Petrie, *Memphis*, 1, 5, 10, pl. xxiii (middle left).

*Material not known*²⁶

XI = SCHISM 2652 Present location not known, probably fragmentary²⁷ Ramesses II (usurped by Ramesses VI)

In the 1985 season J. Malek and H. McKeown carried out a detailed recording of 'Abû'l-Hôl' (L1 = SCHISM 51). The field-work had been preceded by a study of early reports and old drawings and photographs (pl. III, 1).²⁸ The limestone statue was discovered in 1820 by G.-B. Caviglia, who worked on behalf of C. Sloane and H. Salt. Because of its size (10.95 m at present) and weight (estimated to be some 100 tons), no attempt has ever been made to remove it from Mît Rahîna. In 1852 J. Hekekyan cleared the area around the colossus and discovered the platform on which it originally stood. In 1887 Major A. H. Bagnold, of the Royal Engineers, successfully raised the colossus above the inundation level and turned it over. The present museum which houses the statue was built in the late 1950s. Although this colossus had thus been known for a long time, ours was its first systematic recording. Photographic documentation was undertaken by Mr David Tunnicliffe (British Embassy, Cairo).

The distribution of the royal names²⁹ on the statue follows a clear pattern with the texts reading outward. The only exception is the prenomen of Ramesses II on the pectoral which reads from left to right, and which may provide a clue to the original position of the colossus outside the southern gate of the Ramesside temple of the Ptah precinct; it probably stood to the right (east) of it.

Various details of the king's anatomy, as well as his regalia, dress, and accoutrements, were studied and recorded for comparative purposes. Some features, such as the noticeable downward tilt of the face,³⁰ may be due to the size of the statue and serve to compensate for the angle of viewing. The traces of the texts accompanying the reliefs of a prince and a queen by the left leg of the statue were examined. From the remains of a *sem*-priest title it is almost certain that the prince is Khaemwese.

²⁶ A basalt pedestal of another colossus of Ramesses II stood in front of the Pylon, *ibid.* 10, pl. xxiii (bottom), but it is omitted from our list for the time being.

²⁷ Recorded by B. G. Gunn in the early 1920s, MSS R.4.1-9 at the Griffith Institute, Ashmolean Museum, Oxford.

²⁸ The photograph on pl. III, 1 pre-dates Bagnold's work in 1887. It is reproduced from a print in the library of the Department of Egyptian Antiquities of the British Museum, and was brought to our attention by Dr C. N. Reeves.

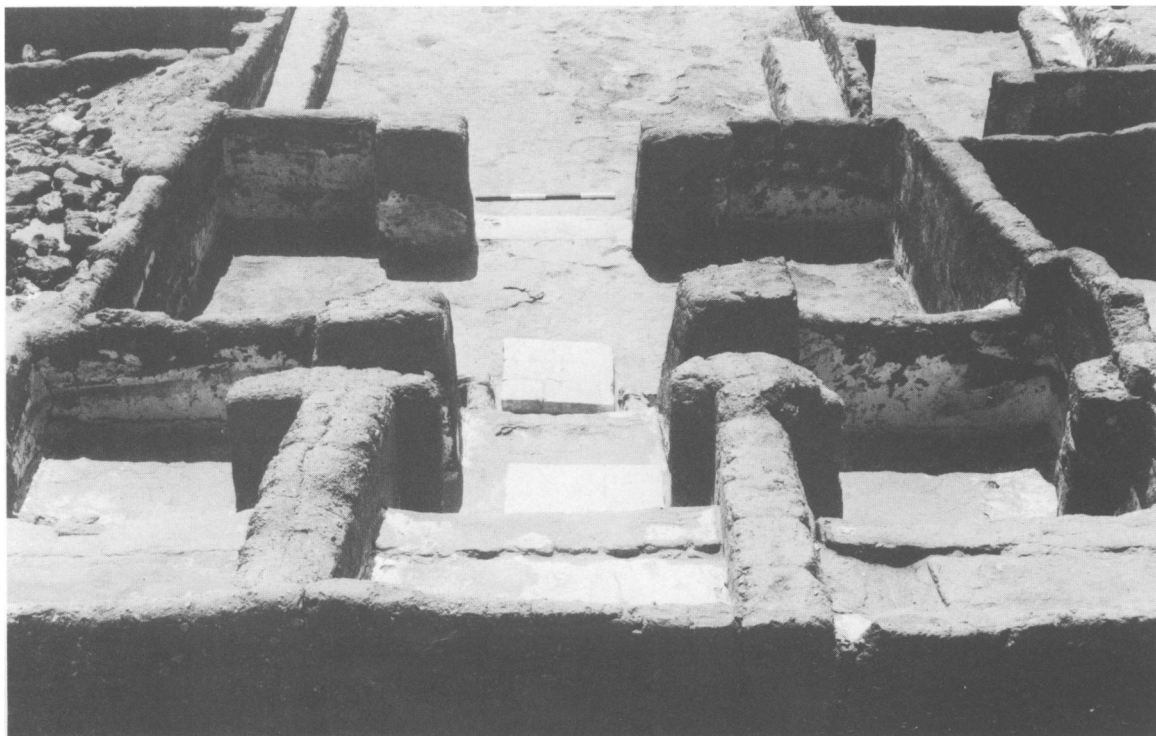
²⁹ The text described by Kitchen in *KRI* 11, 494 (191, A, iii) as being on the left fist is, in fact, one of the two cartouches on the right bracelet, and the texts given by him are incomplete.

³⁰ This feature is not confined to over-life-size Ramesside sculptures, as pointed out by V. Solia, *Artibus Aegypti Studia in Honorem B. V. Bothmer* (Brussels, 1983), 148 n. 3, but the reason for its introduction in smaller statues may have been different, B. V. Bothmer, *BMFA* 47 (1949), 44-5.



1. Mît Rahîna. Limestone colossus 'Abû 'I-Hôl', looking north over the Ptah enclosure, before 1887. *Courtesy the Department of Egyptian Antiquities, British Museum*

MEMPHIS, 1985 (p. 20)



2. General view of the Main Chapel from the east. The scale is one metre long

AMARNA WORKMEN'S VILLAGE (p. 30)

THE AMARNA WORKMEN'S VILLAGE IN RETROSPECT

By BARRY J. KEMP

The excavations of the Egypt Exploration Society at the Workmen's Village at el-Amarna began in 1979 and were completed in 1986. The eight seasons saw extensive investigation of the ground outside the walled village as well as limited excavation inside. A wide range of evidence has been gathered, relating to the life and economic basis of this community, with particular reference to animal-keeping. Although an annual report has made available a summary of each season's results, this article offers a general survey of the village in the light of the new work, and sets the latter within a broader framework of research on ancient Egyptian society.

THE Society's excavations at el-Amarna were begun on a small scale in 1979 at the Workmen's Village and have since proceeded annually. For the first four seasons it was enough to prepare preliminary reports for the *Journal*, and these appeared in volumes 66, 67, and 69 for 1980, 1981, and 1983. Subsequently, the range of findings both on the ground and from research on various categories of material proved too extensive for a short illustrated *Journal* report. Although a brief summary of the expedition's activities has continued to form part of the *Journal's* editorial, the annual report on the season's work has been transferred to a monograph series, entitled *Amarna Reports*, forming part of the Society's Occasional Publications series. Four volumes have so far been published (1984, 1985, 1986, and 1987). With the 1986 field season the excavations at the Workmen's Village have ended, and it seems appropriate to return to the pages of the *Journal* to offer a retrospective summary. In the ensuing account, each time a fresh part of the excavations is introduced the season of excavation is given in brackets.¹

The general setting and approach to the village

The square walled village and the various elements which lay immediately outside its walls occupy the sloping floor of a short south-facing valley in the low plateau which runs out from the cliffs in the middle of the Amarna plain. The full limits of the site, however, do extend further, although they take in elements which are, archaeologically, somewhat unconventional. Excavators inevitably concentrate their attention on ground containing buildings, but one of the attractions of this site is the way that less substantial signs of human activity have survived on the desert surface (fig. 1).

¹ With the following publication equivalents: 1979: *JEA* 66 (1980), 5-16; 1980: *JEA* 67 (1981), 5-20; 1981 and 1982: *JEA* 69 (1983), 5-24; 1983: *AR* I (London, 1984); 1984: *AR* II (London, 1985); 1985: *AR* III (London, 1986); 1986: *AR* IV (London, 1987). The abbreviation *AR* is used for *Amarna Reports*; *COA* for *The City of Akhenaten*.

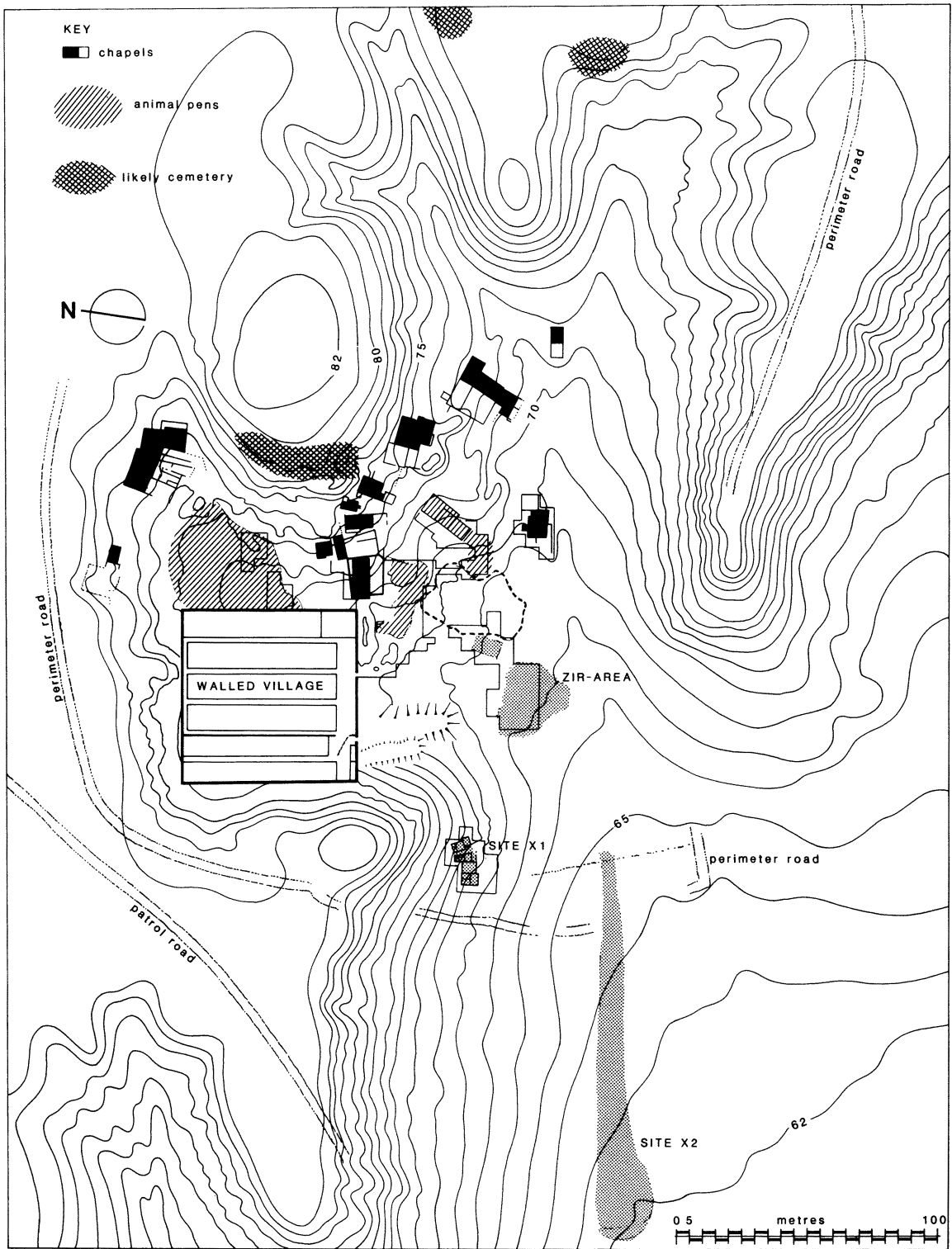


FIG. 1. Map of the area around the Workmen's Village. Some of the contouring, especially on the east, is provisional.

It has been known since the surveys of Petrie, Timme, and Davies² that a network of ancient roadways connected with Akhenaten's city survives on the desert. Some belong to the rock tombs, some running north-south were probably to assist the regular patrolling of the desert plain by police, whilst others form local concentrations. One of these occurs at the Workmen's Village. The paths or roads always have the same appearance: a strip of desert of varying width has had its cover of small stones swept to the sides into low ridges. Some of the stretches of roadway near the village are very clear and were noted by past surveys, but others are so faint as to be properly visible only when the sunlight falls on them at an advantageous angle. When all are considered together, it can be seen that they form a virtually complete circle around the village. On the west and north the line runs close to the walled village, and on the south takes the only line possible to keep it on high ground. On the east side, however, it runs much further away and takes in a large area of plateau and valley. Within its circuit are to be found all traces of the villagers' activities except for some isolated sherd dumps on the south. The area designated Site X2 also lies outside, but, as will be discussed shortly, this was probably not created by the villagers themselves. Whether or not this encircling pathway was ever patrolled, it formed a territorial perimeter to the village, leaving ample space on the east for the development of the cemetery. That the pathway was actually seen as an effective perimeter is shown by the way it relates to Site X1 (1979).

Site X1 and the adjoining area on the west, Site X2 (1986), are probably to be understood by their relationship to this perimeter line, since they occupy the ground where it intersects the natural route to the village from the main city. The *Zir*-area (1982-4) occupies a corresponding position on the inside of the perimeter. The intersection was emphasized by a line of larger stones running along the perimeter between site X1 itself and the easterly turn of the perimeter track. It was not a wall or even a serious obstacle since it would have been easy to skirt around its southern end. It acted simply as a particularly prominent territorial marker. Site X1 was a building with a plan which was not that of a house. It consisted of a well-built unit of three rooms, attached to which were other rooms in which animals (including goats) had been kept. Its position at the exact place of intersection between road and perimeter explains its function: it housed those whose job it was to guard the village and to check anyone who sought to cross the boundary.

Site X2 and the *Zir*-area should be considered together, for they appear to be complementary both in the character of their pottery and in their relationship to the boundary line at Site X1. Site X2 is simply an irregular scatter of sherds which begins about 150 m to the west of the boundary line and then follows a linear course towards it, eventually becoming confused with the spread of sherds which have been washed by natural agencies down from the village. It is broadest at its western end, where it is probably associated with a short east-west stone alignment. A careful sampling and study of the sherds reveals an overwhelming preponderance of

² W. M. F. Petrie, *Tell el Amarna*, 4-5, pl. xxxv; N. Davies, *Rock Tombs* II, 5-6, pl. i; IV, 11, pl. xiii; P. Timme, *Tell el-Amarna vor der deutschen Ausgrabung im Jahre 1911* (Leipzig, 1917), 24 ff., and maps.

Canaanite amphorae and their locally made imitations. Some peripheral sherd clusters derive from the breakage of individual vessels, and the material implies on-the-spot breakage rather than the deposition of sherds broken elsewhere. The distinctiveness of the Canaanite type, which is otherwise rare both at the village and in the main city, has helped in pinpointing the other end of the trail of breakages. This is on the edge of the main city, beside a well depression which is at the closest point to the village itself. This greatly enhances the view that these amphorae, doubtless re-used from some surplus government source, were the prime means of transporting water to the village, and that Site X2 was connected with the transfer of vessels from supply train to villagers.

At the *Zir*-area amphora sherds are also frequent, but are matched in number by sherds from *zirs* (the colloquial Arabic word for a large pottery vessel used for storing water). The area also possessed constructions, in the form of irregularly spaced emplacements, roughly built from stones, rubble, and re-used pot bases, and primarily intended to support large pottery jars, especially *zirs*. Only a portion of the site (15 from a total of probably 28 five-metre squares) was excavated, but detailed surface planning of the remainder implies an original total of around fifty jar emplacements. This, as we shall see, is roughly the number of houses in the basic labouring part of the Village. The linear sequence from well depression in the main city to *Zir*-area is probably the scattered remains of the whole water-supply operation, and it is hoped gradually, as analysis of existing data and excavation in the main city proceed, to clarify the logistics of water supply at Amarna. The villagers had at least three separate water requirements: for domestic use within their houses, for their animals kept in the pens on the east and south-east of the village, and for watering the plants in the small growing-plots. The *Zir*-area would have supplied all three, and its location presumably struck a compromise amongst the various distances involved (including that from the Site X1 boundary line) and between these and the wish to position it outside the zone in which refuse was dumped.

Away from this area the perimeter track provided a clear and permanent boundary to the village's territory. That this was its main purpose (rather than actually assisting an outside agency to keep the village under observation and guard) is evident not only from the configuration around Site X1, but from the fact that it avoided the single most advantageous observation point overlooking the whole valley, and the extra-mural area in particular. This is the headland which rises to the east, above the chapels. By swinging some distance to the east it left a substantial tract of desert for the villagers' sole use, doubtless in time to be developed into a cemetery.

The walled village and its houses

One of the reasons for working at Amarna is to clarify the meaning of many standard features of domestic life already recorded, so that the existing mass of data from previous excavations can be better and more confidently used. In the case of the Workmen's Village houses the most important structural results (as distinct from

results of analysis of material found, as yet uncompleted) pertain to the roofs and upper structures, and to the fixtures for food preparation. The excavations of 1921 and 1922 revealed about half of the interior of the village, and showed it to have consisted of similar houses arranged in a simple plan of straight north-south rows. The predictability of results from further work inside the village led the present expedition to a policy of very restricted sampling. The houses selected were Long Wall Street 6 (1979), Gate Street 8 (1985) and 9 (1986), and West Street 2/3 (1986).

Whereas the village enclosure wall is constructed throughout of Nile-mud bricks mixed with gravel—the standard building material in the main city—everything built outside the village was either of bricks made from locally quarried marl clay, or stones set in a mortar of the same material. The houses inside the village, however, display a mixture of all three materials. Evidently there was a surplus of Nile-mud bricks provided for the enclosure wall and these were shared out amongst the various house-builders. In three of the four houses excavated these bricks were found to have been used very selectively, often for the bottom foundation course, and for the partition wall between the Middle and Rear Rooms.³ The one house where none was used was West Street 2/3. The curious preference for one partition wall perhaps relates to a feature in a presumed upper storey (e.g. a dais) which required additional strengthening.

In the main city the common pattern of wall preservation is that whereas the thicker walls of large houses often stand to considerable heights, the thinner walls of smaller houses and other buildings are normally only a few courses high. The location and dense construction of the Workmen's Village, however, led to a unique degree of preservation of walls for houses of this size. This, in turn, helped to preserve information about the original roofing and elevations of the houses. The modern robbery which had affected the contents of many of the houses hindered this aspect of the work, but some positive results were nevertheless forthcoming. The most significant came from Gate Street 8 (1985), where much of the contents of the Middle Room and Rear Room South were found undisturbed. Their roofs had been constructed from a mixture of acacia beams and tamarisk poles on which the bark had been left. In the Middle Room it was possible to determine that the beams had been set at intervals of roughly 70 cm, the average length to which the tamarisk poles had been trimmed before being laid across them. A thick layer of marl mud had then been spread over the upper surface, retaining the impressions of the wood. It is clear that this thick and heavy roof spanned the whole width of the house without the need for a central columnar support. This was not the only kind of roof construction in the various houses. Other materials survive, partly as impressions in fragments of the mud roof cover and partly as actual specimens of the substances involved. The study of these is not yet complete, but one set of data from Gate Street 8 deserves to be singled out. Mixed in with the rubble in the Middle Room were fragments from a second roof made with a cover of mud of a slightly different colour and bearing the impressions of parallel reeds. The greatest concentration lay immediately above the

³ For a further use in a rear wall, see *JEA* 66 (1980), 11, fig. 4.

lowest level of rubble (which was obviously undisturbed and in contact with the floor) and renders it unlikely that it had been introduced from an adjacent house. When this is added to observations made in the 1920s about fragments of painted plaster found high in the filling, it is a fair supposition that, in the case of Gate Street 8, the Middle Room possessed a second storey. Such a room would have increased the roofed living-space by a significant amount, and would have provided a different order of privacy and comfort from the gloomy and smoky main room below. One group of fragments of painted wall plaster found in a high position in 1921 belonged to a pilaster on which was painted the convolvulus and papyrus design basic to the New Kingdom scenes of women suckling infants.⁴ This subject matter, perhaps belonging to a painted panel behind a real bed, would fit ideally into a more private upper room, which offered a setting for feminine activity. Also in the rubble, and lying as if it had fallen from high up in the eastern wall of the Middle Room of Gate Street 8, were the remains of a small wooden window-frame and pivoting shutter. This could have lit and ventilated the upper room.

Two further sets of observations relate to the front and rear rooms of the houses. Again in Gate Street 8, Rear Room South, when fully cleared, was revealed as a probable bedroom, with a low dais along one side. Above the floor lay rubble from the collapsed roof. Towards the top a distinctive ashy layer spread into the rubble from the south-east corner, and in the uppermost layers, somewhat disturbed in modern times, fragments of a cylindrical oven occurred. The ash must have derived from a concentration on the roof, in one corner, and it is very tempting to postulate an upstairs oven, which would have been feasible since the standard oven was fired at only a very low temperature.⁵ This conclusion would also explain the fact that ovens have been found in less than half the total number of houses excavated in the past.

With regard to the front rooms, in both Gate Street 8 and 9 (1986) they were subdivided, but only by one-metre-high partition walls. The avoidance of using these partitions as roof supports is part of the evidence for thinking that some Front Rooms were open to the sky, serving as forecourts. The front part of Gate Street 8 also contained a small structure identified as an emplacement for a quern, of the type frequently depicted in baking/brewing scenes in funerary art and tomb models. A second example occurred in the annexe to West Street 3. In both cases the top of the plinth which supported the quern contained a layer of ash. This ties in with observations of the same kind, including patches of actual burning on the wall above, made by Peet and Woolley about constructions of identical appearance. They interpreted them as a type of oven or hearth. But what we really seem to have is evidence for an elementary practice of hygiene to reduce insect infestation around the quern, for ash (and periodic burning) is effective in destroying insects and eggs.

These various suggestions modify the way that houses of this kind should be

⁴ *COA* 1, 60, 80, pl. ix. 2, cf. *AR* III, 25. For a recent discussion of this material, see G. Pinch, *Orientalia* 52 (1983), 405-14.

⁵ Evident from unburnt rope found in one case still wound around the outside of one which had seen use, *AR* III, 21.

reconstructed. But it must be born in mind that just as the ground plans of the houses vary a good deal in detail, so too might the arrangements at roof level. If we accept the evidence for a second storey over the central room of Gate Street 8, we are not obliged to reconstruct a similar room over all the houses. In its final state the village could well have possessed quite a varied roofscape.

The evidence from the recent work also makes it more profitable to analyse the occurrence of features in all of the excavated houses within the village, looking not so much for the 'typical' house (which Peet and Woolley described reasonably well) but for the range of variation. A short summary of the results of doing this is contained in *AR* IV, Chapter 2. A visual summary, which plots the occurrence of features as a house-by-house score, is given in fig. 2. Its purpose is to illustrate the degree of internal differentiation, reflecting the fact that although the village was planned to accommodate a set of families of common status, in actuality the circumstances of these families varied somewhat, and this was reflected in their houses (and even more so, in the chapels and other extra-mural constructions).

The excavation of West Street 2/3 (1986) was part of a wider exploration in the south-western corner of the village where the plan departed from the regular spacing of unit houses. The area which could have been filled by West Street houses 1 to 3 was in fact divided into two more-or-less equal portions: an open area and a house (West Street 2/3) which occupied one-and-a-half of the normal house ground plots. Both occupy ground which rises steeply from the main gateway of the village, giving it a commanding position almost like a small acropolis.

Along the south side of the open space, and thus against the inside of the village enclosure wall, a line of five rectangular rooms had been built, the most easterly subsequent to the rest (1985). The remainder of the area had remained open, and had gradually accumulated a deposit of organic rubbish, laid down in a series of finely divided strata. The purpose of these rooms is not fully established. The contents had been badly disturbed in modern times, but to judge from several fragmentary or complete limestone animal feeding-/drinking-troughs found in the loose sand fill, they could have housed animals. It is interesting to note that the 'house' in the north-west corner of the village, West Street 23, was also an animal pen (equipped with quern emplacement), although for small animals or birds to judge from the small size of the entrance to an inner pen.

A separate examination was undertaken (1986) of the southern enclosure wall at the site of the west gate, and the equivalent portion of the north side of the village. The purpose was to check the Peet and Woolley interpretation that the western section of the village was an addition to the main part. The pattern of brickwork confirms this. They also thought⁶ that the west gate had been blocked up during the life of the village and before the build-up of rubbish in the south-west corner. This, however, was found not to have been the case: the west gate was in use throughout the life of this part of the village. Peet and Woolley went one step further, and argued that the whole western part was abandoned before the rest, citing as reasons the lack

⁶ *COA* 1, 54; *JEA* 7 (1921), 176; *JEA* 8 (1922), 51.

of objects, and in particular of remains of roofing. They saw the lack of roofing material as a sign that it had been carried away when this part was prematurely abandoned. The excavation of West Street 2/3 did not support this latter idea. A more plausible interpretation is that West Street was occupied by a group of families somewhat poorer than the remainder, for this seems to be reflected in structural features of the houses (see fig. 2) as well as in numbers of artefacts. The possibility that the village housed two communities will be returned to later.

Utilization of the ground outside the walled village

Most of the expedition's work has been devoted to the ground which lies outside the village walls. The general spread of ancient remains was apparent from surface indications at the beginning, although excavation subsequently showed that important features extended further to the south and south-east. This still means, however, that only the ground to the south and east of the village was utilized.

Over the sector south of the village two factors shaped the pattern of ground utilization: the hill slope to the north-east and the marl quarries on the valley floor. Since from the latter came the raw material for making the bricks from which parts of the houses and other structures were built, they must have been one of the first man-made features. The Main Quarry began about thirty-five metres south of the village main gate. Its full extent was determined partly by excavation (1981-3) and partly by means of a resistivity survey (1983). Three smaller quarry pits were located in the ground much closer to the village. These pits subsequently became convenient places into which the village refuse could be thrown. The smaller ones were almost completely filled in this way, and with the Main Quarry the northern half was filled with a great sloping bank of rubbish from dumping from the north and north-west. Because of the considerable depth of deposits (one of the pits excavated in the Main Quarry in 1982 reached a depth of three metres, as did one in square L17) several different stages in the processes of filling were clearly differentiated, providing a unique record of stratigraphy and chronology. Comments on this aspect are provided in the section below on chronology. As the pits filled up, the dumping of rubbish spread over neighbouring ground. Eventually the building closest to the village gateway, animal pen 350, was abandoned, and its ruins came in time also to be filled with rubbish. Later still, the surface of this zone was levelled off to provide a stretch of clear ground running between the village gateway and the Main Chapel. It accommodated a line of T-shaped basins which in effect extended the territory of the chapels. A separate area where rubbish was dumped lay beside the east wall of the village (1985). Again, its close proximity to one of the animal pens (building 250) led to changes in this building's layout.

The composition of the rubbish deposits varied, reflecting different origins. If these could be properly determined, they would be an important source of information on activities carried out in other parts of the site, especially within different kinds of buildings. The reason for saying this is that it was common practice regularly to clear the floors of houses, chapels and, to some extent, animal

West Street av. 4.4		Long Wall Street av. 6.5		Main Street av. 7.45	Gate Street av. 7.5	East Street av. 7
2	6	6	8	9	8	
3	3	7	8	6	6	
4	7	8	7		7	
4	7	7	9			
6	6	4	9			
5	7	7	9			
			7			
			8			
			6			
			4			
			5			
			6			
						(10)

numbers represent scores in terms of structural features present

bracketed pairs of houses indicate possible single enlarged properties

'crossed' houses were probably not normal residences

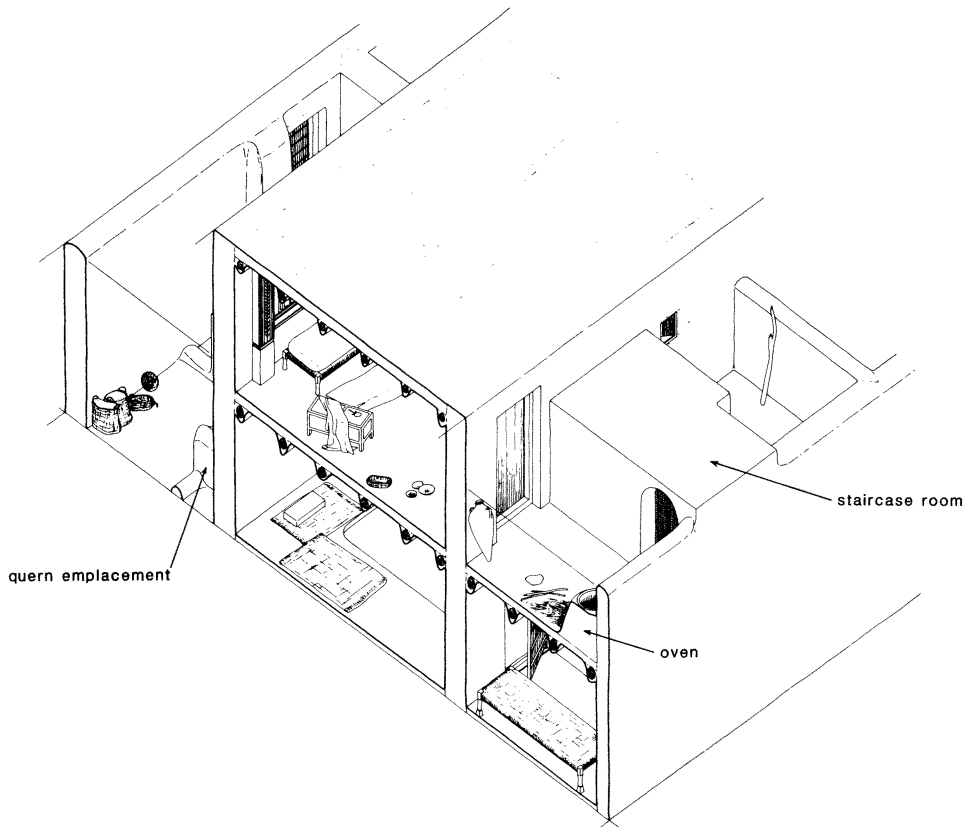


FIG. 2. Above: outline plan of the walled village. The numbers are scores based on the presence of structural features, as follows: stairs to roof from rear of house; stairs from Middle Room (two points); shelf supports in rear room; north-south partition walls two bricks in thickness; bed alcove; wall painting; painted plaster in fill; manger; loom-beam socket; stone bath; quern emplacement; mortar; hearth; bench; column base; limestone cavetto cornice. Because of disturbance and robbery subsequent to 1922 no scores are given for the four houses newly excavated. Below: reconstruction of the house Gate Street 8, excavated in 1985.

pens. Thus, very little of the debris of actual use survives *in situ* in houses and chapels: most was transferred anciently to the rubbish heaps. In practice, however, it is difficult to determine point of origin, and at the time of writing the integration of data from these deposits and the various groups of buildings has hardly been attempted. Nevertheless, some remarks on specific deposits will appear later in this text, in the sections on chapels and animal pens.

With quarries and rubbish occupying the ground in front of the village, the building activities of the villagers were for the most part kept to a peripheral zone on the hill slopes facing west and south. This zone displays a simple order of preference: chapels on the higher slope, animal pens on the lower, with the Main Chapel a major exception. Since the chapels would, if the village had been occupied for longer, have come increasingly to serve the tombs which were dug in the desert to the east, their location is more likely to be a result of preference rather than of chronology. We can also consider another possible factor: that chapels and animal pens belonging to the same groups of people were also built in proximity to one another. This would explain the association between the isolated group of pens (building 300) and chapels (570 and 571) at the south end of the site, although it would leave the most easterly group of chapels (532-6) without its share of farm buildings. On the other hand, it is possible that not all families were engaged in the same range of activities.

The overall result of the excavations is to show that groups of households had allocated to themselves territories outside the walled village and beyond the presumably common area of rubbish dumping on the south side: territories marked by chapels, animal pens, and plant beds, and individual sites in the *Zir*-area. If texts written by the villagers had survived, these elements would doubtless have each been distinguished by a separate word (e.g. *hnw* perhaps for 'chapel'). All of them have in common the use of locally derived materials, informality of arrangement, and diversity of design. It is a far more natural interpretation to see them entirely as products of the villagers' own initiatives, rather than as things bestowed on the community by an outside agency of the state.

The chapels

The groups of mud-brick chapels excavated by Peet and Woolley make an important contribution to our knowledge of private religious architecture in the New Kingdom. Their designs were straightforward and had a marked element of repetition, and the published reports cover them in some detail. The recent expedition discovered and excavated three more (the Main Chapel and nos. 570 and 571), but re-examined a selected number of the older ones. In two cases (523 and 540/1) it was suspected that they were not chapels at all but parts of the animal farms. These will be dealt with in the next section. In the other cases (528-31 and 556) the purpose was to check certain features not fully explained by the older work.

The Main Chapel (1979, 1983-4), also called Chapel 561/450, proved to be a particularly productive building (fig. 3, pl. III, 2). Sheltered from the wind by having been built on a shelf cut into the hillside, the walls in some places rose to a

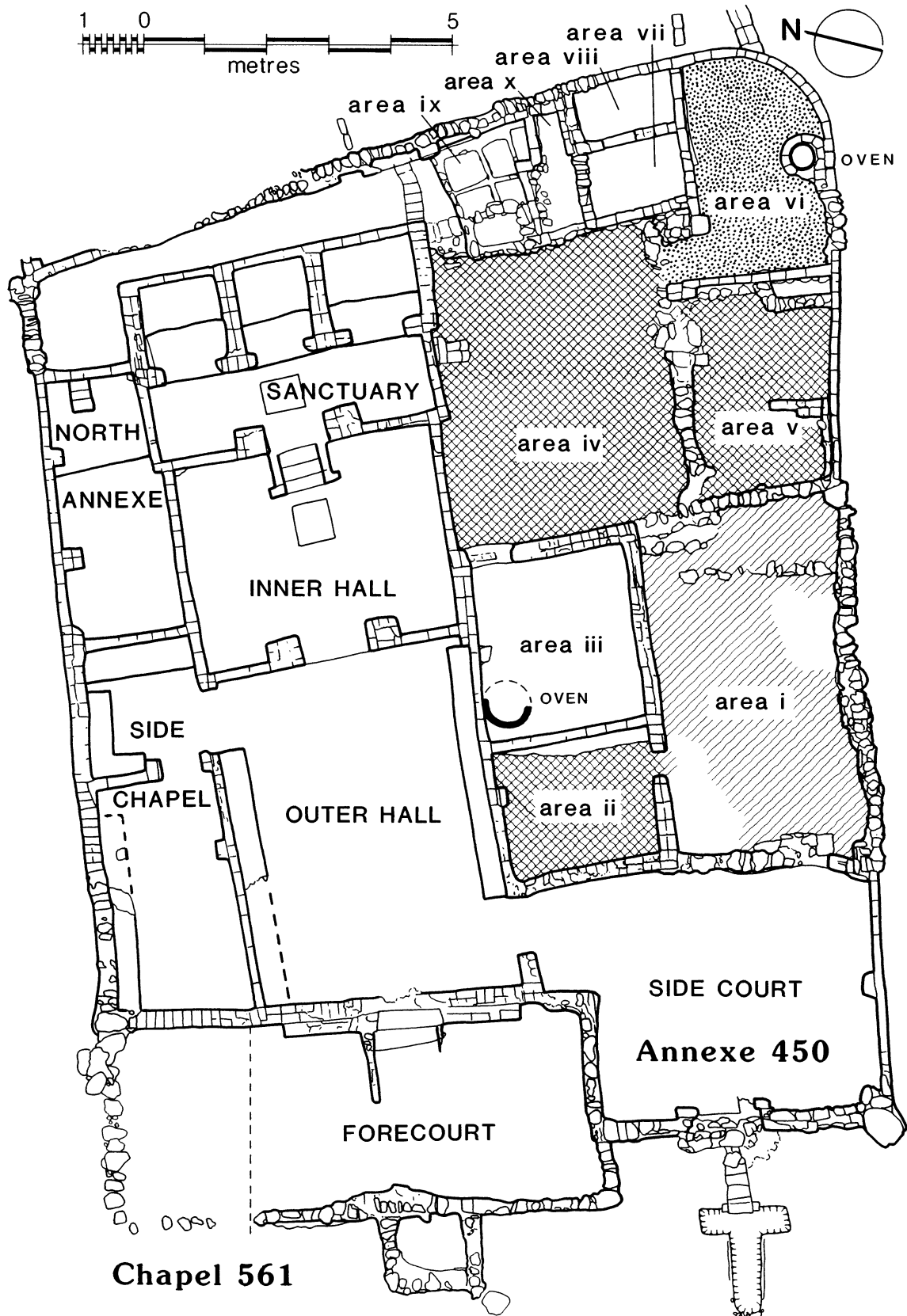


FIG. 3. Outline plan of the Main Chapel, no. 561/450. Organic floor deposits are shaded: simple hatching for thin deposits, cross-hatching for thicker deposits. Ash deposits are stippled.

height of 1.40 m. It had also largely escaped modern robbery because, lying further down the hill than the chapels dug in 1921, its site had been covered by the spoil heaps from the older work. The monument is really a complete little temenos, or sacred enclosure, of which the chapel itself occupies more than half. The other half (450) consists of an annexe which runs around the back of the building and includes the part labelled North Annexe. The chapel proper (561) is a larger version of the standard design: a sequence of two halls followed by a sanctuary with three shrines, the whole arranged symmetrically on a central axis. Low square offering-tables of brick stand in the centre of the floors of the Inner Hall and Sanctuary; benches line the two principal walls of the Outer Hall. Additional to this standard arrangement is a side chapel on the north, provided with a single shrine and two rooms containing benches. Well-preserved architectural evidence illustrates several specific features. The principal entrance to the chapel, on the west, was flanked by a pair of upright wooden poles, the bases of which were found set in gypsum. It is tempting to reconstruct them as flag-poles of the kind regularly set in front of Egyptian temples.⁷ The walls which subdivided the chapel in an east-west sequence were screen walls, rising to a height of about 1.20 m, and capped with a cavetto cornice leaving windows above. There was thus continuous intercommunication from Outer Hall to Sanctuary in addition to the doorways. All areas inside the chapel were roofed, including the Outer Hall. We know this because the rubble from the mud layers covering the roofs themselves had collapsed on to the floors and had yet retained ample indications of the wood and other organic materials which they had originally covered. As with the houses inside the walled village, the cross timbers were of sufficient strength to bear the weight of the roof without needing an intermediate pier or column. Amongst the fragments were some from circular holes in the roof, white plastered on the inside and evidently intended to improve the interior lighting (1984).

The inside surfaces of the chapel proper—walls, floors, and ceilings—were entirely painted. Mostly this was a coating of white gypsum, but in the Sanctuary, Inner Hall, and Side Chapel coloured schemes of decoration had been applied. Numerous fragments of these were recovered in the rubble, and their full study and reconstruction is not yet complete. Nevertheless their general character is apparent enough, although the preserved fragments represent only a fraction of the original painted areas. Most designs were on a yellow background, and many consisted of geometrical friezes running along the tops of scenes or elaborate floral groups or bunches of grapes. In the Sanctuary a large multi-coloured winged disc spread across the space above the doorway leading in from the Inner Hall, whilst two vultures grasping *šn*-symbols and ostrich-feather fans occupied corresponding spaces over the windows above the screen walls (1983). In the Inner Hall the long side walls had carried very formal scenes of men and women in probable offering-ceremonies, accompanied by short hieroglyphic texts which gave the names and titles of the figures, and brief prayers. In one or two places the name 'Amen-Ra'

⁷ The feature was repeated in front of Chapel 522, *AR* 11, 3, fig. 1.2; 13.

could well have occurred. The sole personal name that can be certainly identified is Sen-nefer, and the only title that of 'scribe'. Unfortunately, no trace was found of decoration from the insides of the shrines themselves.

The Main Chapel is the only one in which significant quantities of painted wall plaster have been found. We must consider it likely, however, that this is a consequence of the unusual state of preservation here. The excavations of the 1920s produced a small quantity of painted plaster fragments from other chapels, and our own re-clearance of Chapel 529 brought to light two small painted fragments: one from a human face and the other with a trellis-and-vine pattern, small in themselves, but pointers to a substantial degree of decoration.⁸ The way we see the chapels now—as ruins above a ground plan—is perhaps a very impoverished impression of how they looked originally.

The floors of the Main Chapel provided evidence of a behavioural kind, in two areas. The more puzzling was in the Sanctuary. The surfaces of the bench-like altars as well as parts of the floors bore groups of narrow scratches or grooves (1983). They were frequently located towards edges and corners, though not exclusively so. They did not occur in the other chapels cleared, although something not too dissimilar was found on the floors of small gypsum compartments in Building 541 (see below). The latter circumstance raised the possibility that such scratches are the result of subsequent animal activity, the claw-marks of a large dog, for example. The difficulty with this explanation at the Main Chapel is that they are found on the two levels of floor and bench top, including along the very edge of the latter, and must have been made before the walls and roof collapsed to bury these parts in rubble. The marks cannot have been the result of digging through a covering of sand; they must have been made on the surfaces as they lay exposed to the air. There is an alternative explanation. This makes them the result of human activity, from deliberately rubbing the surface with a narrow blade, perhaps to obtain dust from a sacred source for secondary use.

The other behavioural evidence lay in a restricted part of the floor of the Outer Hall, in front of the bench against the south wall. Embedded in the mud floor were numerous tiny pieces of debris: bird and animal bones, short lengths of thread, wood shavings, seeds, etc. These were excavated in a separate operation (1985). They probably represent debris trampled into the floor made damp and soft at this point from the proximity of a water jar. So whereas most debris from the use of the chapels was regularly swept outside, leaving the floors clean, here we have a sample fortuitously left which provides a window on what went on inside. It implies that the Outer Hall was used by people sitting on the benches, eating meals, and carrying out simple craft activities, such as spinning and the shaping of wooden objects. This is welcome direct evidence for the chapels having been used for more than simply occasional worship.

The nature of the cult performed in the Main Chapel (and other chapels) was only slightly illuminated by the current work. The only obvious items of chapel

⁸ *AR* 11, 47.

furnishings were pottery vessels which had been coated with gypsum. From the Outer Hall also came two small painted wooden oars from a model boat. This context brings with it the implication that the model boat was probably a piece of chapel furniture, a miniature portable shrine, in fact.⁹ In this connection it is worth noting that a pottery bowl with an outline painting of a boat in black was found actually within the Inner Hall of the Main Chapel (*AR* III, 103, 105, fig. 7.3). It is not so easy to explain why a wooden model boat and a model steering oar were found in the 1920s in house Gate Street 12 (see *COA* I: 74, objects 21/272 and 21/400), and oars and other parts from model boats in Long Wall Street 12 (*ibid.* 85, pl. xix, fig. 2, object 22/62), Main Street 2 (*ibid.* 75, object 21/393), and Main Street 12 (*ibid.* 82, pl. xix, fig. 2, objects 22/42 and 22/45). By far the most intriguing item, however, was the top of a wooden military standard found in the Sanctuary (1983; fig. 5). The significance of this for identifying the users of the chapel will be discussed further below, but the fact that it was found here at all suggests that the chapel Sanctuary may have been seen as a suitable place for the safe-keeping of the standard.

The annexe on the south side of the chapel possessed a very different atmosphere. Its surfaces were not gypsum plastered (with the exception of a pair of small enclosures at the back) and on its floors lay a deposit of organic debris. Much of it was also open to the sky. Its complex of little spaces had provided food for the chapel proper, emphasizing the importance which the taking of meals had in the chapel. Animals had probably been penned in two areas (i and ii) to judge from the organic floor deposits and the finding of a limestone tethering-stone in the loose debris above area ii, a room with a particularly narrow doorway. Area iii had contained an oven, as had area vi, which stood in a little court with a deep deposit of ashy debris on the floor. Area ix, raised up on a podium, supported a set of five square plots for the growing of flowers or vegetables (pl. IV, 1). These had been made with particular care. Each plot was a square receptacle about 35 cm deep which had been partially filled with sand to act as a water reservoir, over which a layer of black alluvial soil had been spread. Beside them, and also on the podium, were two of the small rectangular enclosures, originally gypsum plastered, which resemble similar ones in some of the animal pens, and which may have been used in butchery.

The description of the Main Chapel should really include the strip of ground in front, which ran westwards towards the main village entrance. As noted above, this had been cleared and smoothed at a time well into the village's history. On the line of the path linking the village entrance to the Chapel's side entrance a series of six T-shaped basins had been cut into the ground and lined with bricks and mud plaster (1979, 1980, 1981, 1982).¹⁰ Marks showed that they had been filled with water on at least one occasion, and subsequently were carefully filled in with soil or mud. Not all were used simultaneously, and some were replaced by new ones. Although the

⁹ Cf. the New Kingdom model boat on wheels from a tomb at Medinet el-Ghurab, G. Brunton and R. Engelbach, *Gurob*, 17, pl. lii.

¹⁰ A T-basin also lay in the little garden in front of Chapel 528, *AR* II, 42, fig. 4.3; 45.

T-shaped basin is a familiar element in Egyptian culture, there is no way of knowing of what particular ceremony they formed a part, or how frequently that ceremony occurred.

The other chapels newly discovered by the expedition were given the numbers 570 and 571 (1983, 1984). They lay differently from the others, in that, instead of occupying the slopes of the surrounding plateau, they were built on the floor of the valley, near its centre, on the edge of a low shelf which projects southwards. Perhaps they were almost the last to be built. In plan they were somewhat different from the standard design, and more irregular than most. Even so, no. 571 possessed benches around the walls of the main room, and its little shrine, on a small podium, had been subdivided into three compartments.¹¹ The shrine of no. 570, however, was unique. It consisted of a small square room originally covered by a brick barrel-vault, which communicated with the chapel's little hall by means of a low window.

Particular interest attaches to this chapel group on account of a tomb found cut into the ground beside no. 570 (1984). It was empty, probably unfinished, and almost certainly never used for a burial (as distinct from having had its burial subsequently removed when the village was abandoned). Yet the adjacent chapels had seen use, as apparent from the pottery in no. 571 as from patches of burning on the floors. This helps in clarifying the order of priorities involved: chapels were built for independent use; burial and funerary cult came afterwards.

The re-clearance of chapel group 528-31 (1984) revealed that no. 528 was not an independent chapel at all, but an elaborate forecourt serving the group of three chapels, nos. 529-31. A small garden had lain in the centre, initially provided with a T-shaped basin.¹² A bench had run around the north, east, and western sides, but different from the kind used as seating inside the chapels. This one was higher, and the upper surface bore at close intervals a line of receptacles moulded into the brick and mortar construction, the whole coated with gypsum plaster. It is hard to think of any purpose for these receptacles other than that of holding holy water poured from the large jar that seems originally to have stood on a projecting podium in the centre. But whatever the purpose, the uniqueness of the design shows that although chapel architecture is very uniform, a variety of religious practice existed at the village, differing amongst family groups.

The remaining chapel work consisted of re-clearing no. 537 at the far eastern end of the site (1984, planned 1985), and no. 556 in the north-eastern corner of the site (1986). The former was done simply to recover the plan for the first time, since it was omitted from the *COA* I report.¹³ This chapel was unfinished, and so was perhaps the last one of them all. With no. 556 excavation showed that the front part of the chapel consisted of an extensive roofed hall containing two brick pillars for the roof, wall benches and a charcoal hearth, the whole serving not only no. 556 but the

¹¹ Apparent more from the revised plan of this part in *AR* II, 30, fig. 3.1.

¹² A little group of trees to form a garden in front of a desert tomb was found at Abydos in the North Cemetery, T. Peet, *Cemeteries of Abydos* II (London, 1914), 74-5, figs. 37, 38, pl. xi. A small ornamental garden was also found in the front of Chapel 531 (1984).

¹³ Cf. *COA* I, 101; *AR* II, vii-viii.

adjacent no. 553 as well. Along the east side ran an annexe consisting of a narrow open space containing an oven, a small garden, and a square oven/kiln still filled with its last consignment of about seventy pottery bread-moulds. These were of two kinds: the normal New Kingdom cylindrical, and hemispherical with a foot. This is a most useful discovery since it must represent a complete baking or firing session.

Although the chapels were, in the first instance, religious buildings, this term does not adequately cover their full role in the village's life. For people living in the tightly packed village, the chapels offered a periodic escape to a more salubrious setting.

The evidence for farming

Despite the bleakness of the environment and the necessity to import all water and most foodstuffs, the villagers made determined efforts at self-help. They attempted to grow plants, and they succeeded in raising animals. This was done on a belt of ground generally below the chapels, although without significant separation between them. As noted above, whilst a general zoning of agriculture and religion can be detected, the arrangement of the various elements could also reflect a subdivision of the land into plots 'owned' by groups of families, on which they both farmed and worshipped.

The first sign of attempts to grow plants (presumably vegetables) was found in 1984 beside and beneath Chapel 570. It consisted of a small group of roughly rectangular growing-plots, constructed from brick edgings to contain thin beds of dark alluvial soil brought up from the Nile floodplain. The remains of others lay nearby, but were badly damaged by the building of chapel group 570 and 571 over part of the same area. The full limits were not determined. They could have extended further to the east, where some modern disturbance of ancient material is evident; they definitely extended northwards up on to the low hilltop, for the remains of another set of growing-plots was found beneath Building 541 (1985). However, the principal evidence for attempts at crop cultivation appeared only during the 1986 season, at a site to the east of the walled village. Part of this consisted of a set of animal pens, but on the south and west the ground had a deep cover of almost black ashy soil. This could easily have been dismissed as more village rubbish, had it not been for the fact that on one part, a block of twenty-one rectangular growing-plots had been laid out, on the same level surface, implying that growing was also done outside the prepared beds. The black deposit extended for some distance further to the north, as is shown by patches which have been brought to the surface through modern digging. Beyond lies the north-eastern group of chapels, and one of them, no. 551, also had a small set of growing-plots outside its front entrance (to judge from the plan in *COA* I, pl. xxv, and cf. p. 107), always assuming that this was not simply a peripheral part of the area around Building 200.

Buildings for animals have emerged as almost equal to the chapels in their number and ubiquity outside the village. Seven different sets have been discovered, each identified by a number: 200 (1986), 250 (1985), 300 (1985, 1986), 350 (1980, 1981),

400 (1983, 1985), 523 (1921, 1983), and 540/1 (1921, 1985). They show differences in layout, and these probably reflect differences in use as well as the results of preference or of changes in design over time (fig. 4; pls. IV, 2, V, 1).

The most distinctive feature is a courtyard containing, or with access to, a smaller pen. The pens can be rectangular to square, or rounded. In the latter case, they could be made by running a wall across one of the courtyard corners. Building 300, built down a steep slope, showed an interesting variant. The pens for two courtyards were small caves cut into the hillside. This shows a preference for total shelter, including a roof, and it is possible that simple roofs were general. The pens cannot have been very high. One example, brick-built at Building 300, was probably preserved to virtually its full original height, at 90 cm. Both pens and courts had distinctive gateways: very narrow (often about 30 cm) and flanked by outward projections of masonry. Those for the courts were sometimes long enough to have acted like funnels, whilst those for the pens were used to support wooden poles placed across the threshold at a height of around 5 cm. The best preserved example (Building 300) also showed that poles had been placed across the doorway higher up, 45 cm above the threshold. In several cases, too, the original top of the doorway was preserved, giving an original height of around 45 cm. The floors of both pens and courts were normally covered with an even layer of dark brown organic matter, rich in grain husks. The courtyards were also provided with troughs for water: carved limestone ones, cruder versions built up from rubble and plastered with gypsum, and the bases of large pottery vessels sunk in the floor.

These pairs of courts and pens were sometimes served by larger courts, and also by small enclosures, carefully built of brick, with well-laid floors and all surfaces coated with gypsum. In the case of Buildings 523 and 540/1, these latter elements formed the basis of separate buildings. Some uncertainty surrounds their purpose, although we must accept that it was central to animal farming. A fitting explanation is that they were butchery chambers, the gypsum plastering (which is matched only by the interiors of the chapels) being a sign of hygiene.

The animal buildings belong to various phases in the village's history, and together show that animal raising was a feature of life throughout the village's existence. Impressive records of continuity were obtained from two sets of pens: 250 and 300. At the former, situated beside the eastern enclosure wall, rebuilding and a degree of re-siting seems to have been bound up with a rapid accumulation of village rubbish (pl. V, 1). Its proximity to the enclosure wall also provided a direct stratigraphic link with the village itself, making it very likely that this set of pens was first built shortly after the village was founded. Building 300 lay next to the Main Quarry, and in consequence could be linked to the important stratigraphic sequence of quarry deposits. This showed that part had been built before the last two major deposits (the clean sand and the upper midden layer) had formed. Another event in the history of the animal buildings was the abandonment of Building 350 in front of the village, and its partial replacement further up the hill by Building 400. This fact of continuity through all the village phases, coupled with the uniqueness of the type

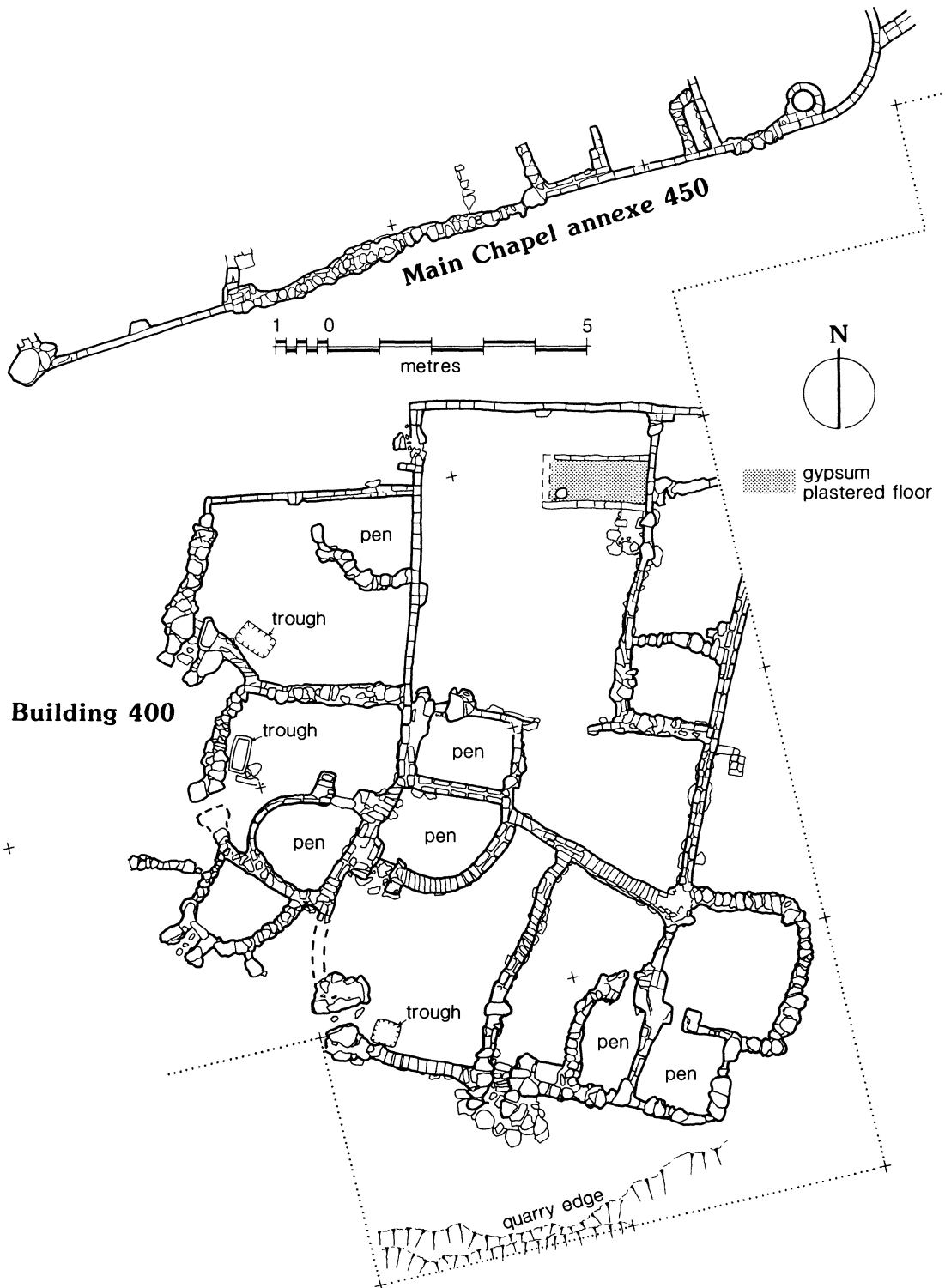


FIG. 4a. Outline plan of the animal pens, Building 400.

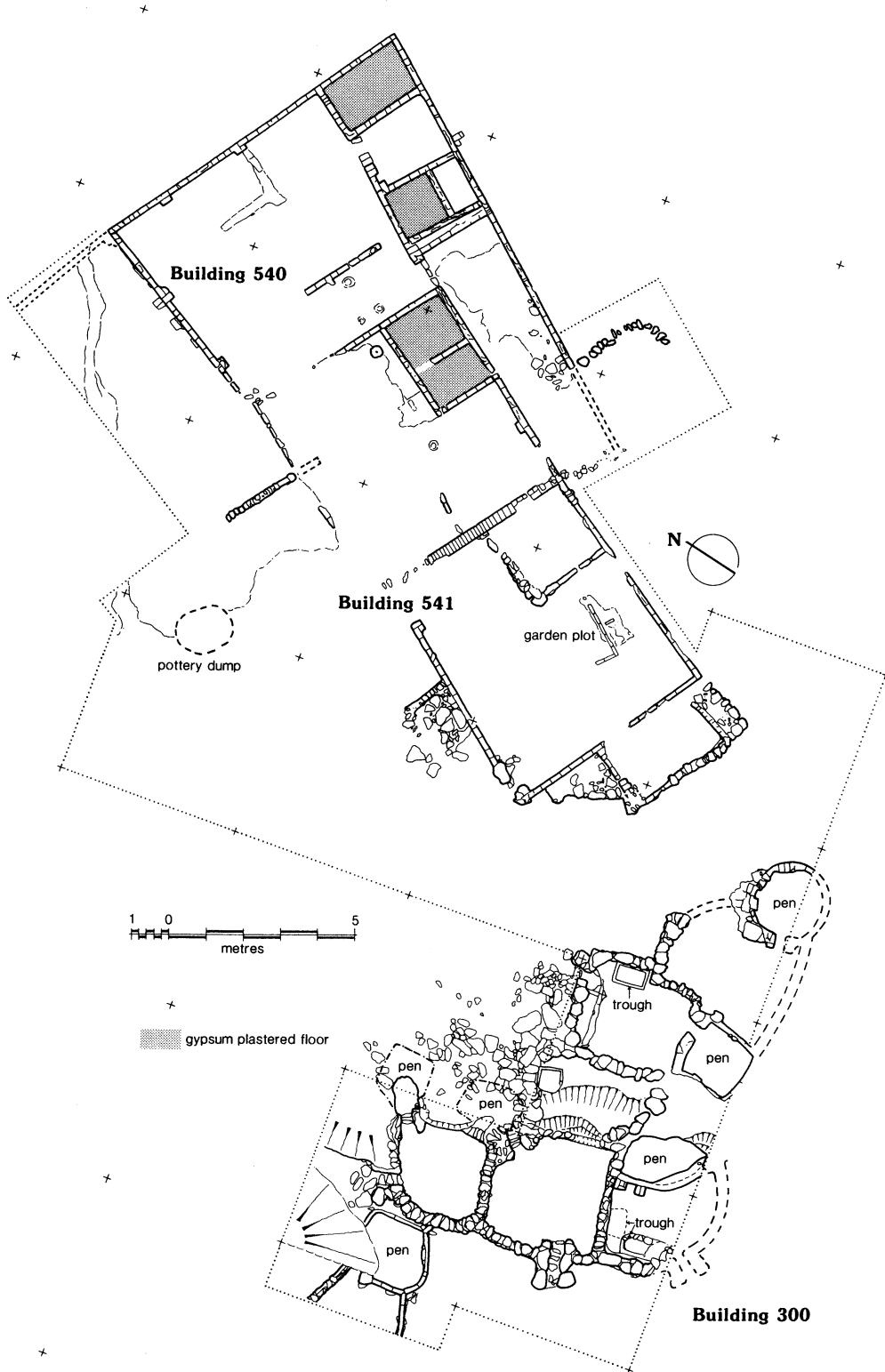


FIG. 4b. Outline plan of the animal pens, Building 300, and the related Building 540/1.

of building itself, is strong evidence for the same community having lived there from start to finish.

It is important to identify the species of animal for which these buildings were intended. Study of the extensive collection of bones recovered mostly from the rubbish deposits reveals the significant presence of three largish mammals (1983): oxen, goats, and pigs, and the choice really has to be confined to these. The first species, oxen, is easily excluded on the grounds of size. The pen doorways have very small dimensions. The choice between goat and pig brings other evidence into play. The organic soil on the floors contains a limited quantity of coprolite which has retained its original form. These bear more resemblance to those from pigs than from goats (the latter, more pellet-like, having been separately found in other parts of the village), and reveal a diet of grain (whereas analysis of a small sample of potential goat coprolites identified only green plant remains). Tests for preserved parasite eggs have also been positive, and although the egg casings, of microscopic size, have suffered change in the course of three millennia, tentative identifications have been made of *ascaris*, a parasite which infests both man and pig (other samples have revealed the presence of *taenia*, the common tapeworm, evidently from human or possibly dog faeces). Welcome confirmatory evidence of a very positive kind was provided by the identification of specimen coarse black bristles, which occur in large quantities in a great many of the deposits outside the village walls, as hog bristles (1985). The direct evidence, therefore, points to pig rather than to goat. The structural details of the pens themselves tend to confirm this. One factor in pig breeding is the vulnerability of newly-born litters of piglets. They and their sow need to be kept within their own small sheltered space, and whilst the mother will leave the sty to forage, the piglets will need to remain within their shelter. The layout and construction of the pens admirably suits the requirements of pig breeding. A study of a sample of 89 pig mandibles from the site reveals a clear pattern of slaughter—mid-way through the first year, and throughout the second year, with few surviving beyond—which is reasonable for animals raised for meat consumption.

The animal pens represent a considerable expenditure of effort on the part of the villagers, and so far are without parallel in the main city at Amarna. Pig raising was evidently a serious matter. Strictly speaking it was, as were the attempts to grow vegetables and to keep goats, an illogical activity. It added greatly to the demand for water, pigs having a particularly high water requirement. And although pigs have a natural tendency to scavenge (and the extensive rubbish dumps at the village offered plenty of scope for that), it is clear from the deposits within the pens that they were, at least for a time in their lives, fed on grain, which, like the water, had to be brought from the city. A modern balance-sheet approach would query the rationale behind it, since the few prices that we have for pigs (from Deir el-Medina) show them to have been sold reasonably cheaply.¹⁴ The subject is intimately bound up with the

¹⁴ J. J. Janssen, *Commodity prices from the Ramessid Period* (Leiden, 1975), 177–8, 525. They were, however, more expensive than sheep and goats.

broader issue of the relationship between state and private sectors of the economy in ancient Egypt. The determination of the villagers to raise their own animals in unsuitable surroundings is an important pointer in itself to the limited role of the state as supplier of peoples' needs, leaving much room for local initiatives to broaden the economic base. Within this framework we can find a better economic balance by postulating that the villagers paid for the extra water and grain by means of a proportion of the animals that they raised, leaving the remaining livestock either for home consumption, or perhaps also for sales within the city, via the same porters who brought the village supplies.

Chronology

The village has not quite the straightforward history that one would expect at Amarna. The grounds for disquiet at the idea that the village simply reflects the history of the Amarna Period as generally perceived are twofold: archaeological relationships at the site, and certain categories of written and iconographic evidence. With some of the evidence it is, of course, possible that we are simply seeing differences between modern and older styles of excavation, but a proper perspective on this will have to await the results of modern excavation on a significant scale within the main city.

The archaeological relationships that bear on the village's history are stratigraphic, with the exception of a piece of evidence from the house West Street 2/3 (1986). From the rubble of the front room came fragments of bricks painted with the figure of a royal personage not in the Amarna style, more likely to be Turankhamun than any other. A close study of the bricks and plaster has shown that the painting is on a carefully prepared surface that was probably done when the house was first built. This implies that the West Street extension to the village was not built until Tutankhamun's reign.

The oddness of the stratigraphy, as principally visible in the Main Quarry deposits, was outlined in the report in *JEA* 69. Subsequent work has added more detail and variations but has not significantly altered the basic picture, or brought an understanding much closer. The quarry sequence shows an accumulation of rubbish with two major interruptions. One is represented not by a deposit but by a weathering-step in the quarry face at a level within the thickness of the lower organic deposit. This could be a local feature, reflecting a change in the direction from which the quarry was being filled. The other interruption is not local, but is represented by a sloping bed of clean wind-deposited sand which seems to cover the whole quarry area. The absence not only of archaeological material but also of ashy dust and chaff which inevitably accompanies active human life points strongly to the fact that the site had, at this time, been abandoned. It was during this phase that at least two buildings began to collapse: the walled courtyard beside the Main Quarry itself, and one of the lower courts in the animal pen group, Building 300 (1986). Subsequently, however, deposition of organic rubbish resumed, but the deposit was now of a

different and distinctive character: sloping layers of dusty or ashy sand interleaved with sloping beds of densely packed grain husks.

The contrast with the wind-blown sand both below and above is so marked that we must accept that this upper organic deposit is the result of human activity at the site. The most likely kind of activity is the cleaning of the animal pens and disposal of ash from the fires used in the processing of the carcasses. It is not normal domestic rubbish, the pottery and small finds content being minimal. However, such material as was recovered is no different in style from the rest of the material from the site. Neither it nor any significant body of finds from elsewhere in the village suggest a significant occupation or use of the site following its abandonment at the end of the Amarna Period.

This last term, Amarna Period, does, however, require some better definition provided by the local Amarna archaeology. Two datasets are available from the village, hieratic jar labels and ring bezels with royal names, of the same two kinds which underpin the chronology for the whole of Amarna. The jar labels so far studied (material from 1979 to 1982, *AR* II, Chapter 6) bear a similar range of dates to those found in the main city. They are spread between years 7 and 17, and years 1 and 2, the presumption being that the latter group belongs to a successor of Akhenaten, most likely Tutankhamun. As a guide to the chronology of occupation of Amarna, however, they are probably of very limited value. They show only that after the 'year 2', commodities from certain official estates were no longer being sent to Amarna. But labelled and dated amphorae were not the norm. The amphorae (probably second-hand) used to transport water to the Workmen's Village, for example, were normally left unlabelled.

The ring bezels from the village, by contrast, have a profile of their own.¹⁵ If we exclude uncertain examples but include the 1921-2 material, we find that 71 per cent belong to Tutankhamun (as distinct from 19 per cent from the British excavations within the South Suburb, used as the control sample for the main city). For Smenkhkara, on the other hand, the figures are closely comparable (24 and 22 per cent respectively), whilst for Akhenaten, Nefertiti, and Meritaten the proportions are reversed (in total, 5 and 37 per cent respectively). Ring bezels are a more sensitive guide, being fragile, easily broken, and thus less subject to prolonged use. Both the absolute figures and the comparison with those from the main city imply that the occupation of the village was primarily in the late Amarna Period and the reign of Tutankhamun, much of whose reign had passed before the village was abandoned for the last time. The impression is greatly strengthened by the presence of Tutankhamun bezels in the lower midden strata of the Main Quarry. The accumulation of the sand layer as well as of the upper midden layers had yet to take place.

Interpretation of the evidence yields the following summary picture. The village was established as part of the administrative processes that accompanied the

¹⁵ The initial study of the bezels and their contexts is by I. M. E. Shaw, in *AR* I, ch. 9. The figures have since been modified slightly by subsequent finds of bezels, studied in *AR* IV by E. Shannon.

foundation of Amarna as a major royal city. A time in Akhenaten's reign is strongly implied by the basic character of the village, even though poorly supported by on-site evidence. During the early part of Tutankhamun's reign, perhaps when the king returned his court to Memphis, the village was abandoned. Subsequently, but still during this king's reign, it was reoccupied for a while by a community which kept up the practice of organized pig-keeping, and may even have been augmented by the building of the West Street sector in an expansion of the scope of its activities (see below). By the time of Horemheb the village had been finally abandoned. The apparent reoccupation is distinctly odd, but alternative hypotheses are harder still to support. It has broader implications, namely, that sufficient administration was left in, or returned to, the main city to provide the outside support on which the village depended, and that the end of the Amarna Period may have been more complex and protracted than at present appears from other sources. Herein lies an opportunity for further investigation. For whilst it is hard to imagine that further work at the village will provide clarifying evidence, we do know from which part of the main city the village was anciently supplied.¹⁶ One can only hope that excavation here and in other areas will enable the village record to be tied into that of the main city, and will at the same time provide relevant evidence of a less ambiguous kind.

The purpose of the village

Peet and Woolley, on circumstantial grounds, identified the village as 'the home of the tomb-diggers and grave-tenders of the royal city, a class of men whom for various reasons it was well to keep at a distance and under discipline' (*COA* I, 52). This was written before there was a full awareness of the nature of the village at Deir el-Medina. In terms of location and layout it can now be seen that the two villages possess a striking similarity. As is very well known, Deir el-Medina in the Ramesside Period was occupied by a community of skilled labourers and artists responsible for cutting and decorating the royal tombs at western Thebes. It is thus natural to transfer this identification to the Amarna village, and to see it occupied by people who, although their professions were indeed as envisaged by Peet and Woolley, were of higher status and greater independence.

The parallel between the two sites is sufficient to make it natural to look to the written evidence from Deir el-Medina for a better understanding of how the Amarna village worked, particularly in the ways that it might have been sustained by an agency of the state. But the parallel is not so close as to imply that we are dealing with the very same community. The recent fieldwork has brought to light features—the animal pens and *Zir*-area—which have no counterpart at Deir el-Medina.

The location of the village places strict constraints on the possible range of interpretations. For what reasons would a royal administration have created and

¹⁶ During the 1986 season, the site of an ancient well with associated building was discovered just outside the eastern limits of the city, beside the wadi which has cut the southern part of the city in half. A trail of sherds from Canaanite amphorae links the well to the Village.

supplied a community in so isolated and vulnerable a place? Only two come to mind: tomb work and policing the deserts, the latter suggested by the network of desert roads created at this time. By 'tomb work' we can include work on the South Tombs, towards which the village actually faces. The matter is complicated by the existence of a second village lying further to the east, the 'Stone Village'.¹⁷ Neither the recent excavations nor those of the 1920s discovered artefacts which help to answer the question. No quantity of stonemason's tools, artists' trial pieces, or weapons were left behind. Fragments from a carefully gridded outline painting of a king were found in house West Street 2/3 (1986) and had been made by someone very practised in outline draftsmanship, but it is hard to know how much weight to give it.

A crucial question which has to be faced is whether the Amarna village housed the same community which, in normal times, had lived in Deir el-Medina. For this we need to know if Deir el-Medina was occupied during the Amarna Period. The evidence is, however, ambiguous. Shabti-figures and a coffin mentioning the Aten and Akhetaten show only that the owners were alive at this time.¹⁸ They could have lived and died at Deir el-Medina, or at Amarna, their burials being subsequently transferred to Thebes by relatives when Amarna was abandoned. One source, however, is excluded from this interpretation. This is an inscribed chair belonging to a 'Servant in the Place of Truth on the west of Akhetenaton, Nakhy'.¹⁹ The significance of this spelling of Akhenaten's 'city', with the inserted genitival element *ny*, has been clarified by work on the Karnak *talatat*-blocks. The term Akhetenaton was applied in the early part of Akhenaten's reign to Thebes itself; it was never used for Amarna.²⁰ Here, then, is evidence for one Deir el-Medina workman definitely resident at Thebes in the Amarna Period. At first sight this weakens the case for supposing that they were one and the same community. But one could always argue that this man died during the first years of Akhenaten, before the move to Amarna took place.

The case for identity between the two communities rests essentially on their similar locations and general character, and especially on the similarity of their chapels. But the general utilization of the surrounding ground is very different. In particular the scale and persistence of farming outside the village walls sets the two communities apart and makes it unlikely that they were identical. There is a further possibility. Černý²¹ cites evidence for the existence of other places which possessed a 'Place of Truth', one of them at Memphis, to which carpenters and possibly a goldsmith were attached. This proves nothing by itself, but it does broaden the basis of the discussion. Saqqara's extensive and grandly designed New Kingdom cemetery presupposes the existence of a permanent community of necropolis

¹⁷ This is an unexcavated site, but it is clearly much smaller than the Workmen's Village, and apparently lacks the extensive peripheral activity areas which are such a conspicuous feature of the latter.

¹⁸ Conveniently cited by J. Černý, *A community of workmen* (Cairo, 1973), 51; D. Valbelle, «*Les ouvriers de la tombe*» *Deir el-Médineh à l'époque ramesside* (Cairo, 1985), 12.

¹⁹ *Ibid.* 51.

²⁰ D. Redford, *The Akhenaten Temple Project 1* (Warminster, 1976), 60, and n. 38.

²¹ *Op. cit.* 63-4.

workers in the vicinity. Perhaps it is no coincidence that in the late Eighteenth Dynasty the temple of Ptah at Memphis owned a large herd of pigs.²²

One problem in comparing the Amarna village with Deir el-Medina is compatibility of evidence. The proliferation by the workmen of records of themselves is a phenomenon of the Ramesside Period. It has no equivalent in the Eighteenth Dynasty, when it is very difficult to identify the individuals who made up the community.²³ We do possess a limited prosopography for the Amarna village, supplied by the older and by the recent excavations. The most important source is the Shed and Isis stele found in 1921 in Chapel 525.²⁴ It bears the names of the family of a man named Ptahmay: his sister, his wife(?), probably four daughters, and one son. None of them bears a title (except for 'Lady of the House' in the case of the two women), an interesting fact in itself, given the rarity of inscribed material from the site. A wooden statue base from Chapel 529 (also 1921 excavations) bears a dedicatory text from one Nehem-maatiu.²⁵ Here a title is preserved: 'Servant in the Place'. This, of course, is very close to the standard title adopted by the Deir el-Medina workmen, 'Servant in the Place of Truth'. The only other possible personal reference (a title) found by the 1920s expedition occurs on fragments of mud plaster from one of the houses, Main Street 10.²⁶ At the top of one column is the word 'herdsman', and at the top of the next the sign *sn*, perhaps from the word 'brother'.²⁷ Gunn assumed that the word 'herdsman' occurred in a prayer and was perhaps being used figuratively of the Aten. The layout of the inscription, however, insofar as this is visible from its very fragmentary state, better belongs to short lines of text accompanying a scene in which the main subject is one or more persons, so that 'herdsman' is more likely to be a title. In view of the extensive animal-keeping outside the village this seems very appropriate. The recent excavations have added a little more: an occurrence on a potsherd of the name Ptahmay (again without a title), the name Sennefer and the title 'scribe' (not of the same person) from the Main Chapel wall plaster.

This admittedly very limited evidence fits a community of people in which very few of the men possessed titles at all, or at least of distinction. In this they appear to have been like the pre-Amarna Period community at Deir el-Medina. The classic sequences of men with the titles 'Servant in the Place of Truth' and 'draftsman' and related terms begin with the reigns of Tutankhamun and Horemheb in a way that suggests that the community really was reorganized, as is brought out in a statement dated to year 7 of Horemheb. Thereafter we see a community with a much greater degree of self-consciousness as to its unique status, as well as the means to match this in material terms.²⁸ However, ostraca from western Thebes dated to the mid-Eighteenth Dynasty record, not surprisingly, the activities of gangs of workmen sometimes organized in a manner similar to that of Ramesside Deir

²² *Urk.* IV, 1797, 2, see *AR* I, 161, n. 12.

²³ Cf. Černý, *op. cit.* 50-1; Valbelle, *op. cit.* 23-6.

²⁵ *Ibid.* 101, fig. 15.

²⁷ A good photograph of the key pieces is in the EES archive, negative no. 1922/187.

²⁸ Černý, *op. cit.* 41, 50-2, 101, 124, 290; E. Bogoslovsky, *ZĀS* 107 (1980), 89-116 (Parennefer and Mia are of the time of Tutankhamun); C. Keller, *JARCE* 21 (1984), 119-29, esp. 119, n. 4.

²⁴ *COA* I, 96-7, pl. xxviii.

²⁶ *Ibid.* 146-7.

el-Medina.²⁹ We do not know if any of them actually lived in Deir el-Medina itself for the reason that they seem to have lacked the pretentiousness of their Ramesside successors, and have left few permanent memorials at Deir el-Medina or anywhere else. They give the impression of being more a pool of semi-skilled labour not given to the assertiveness of later times.

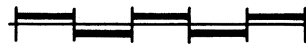
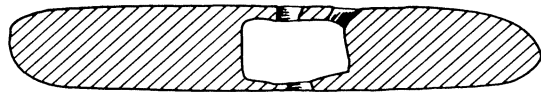
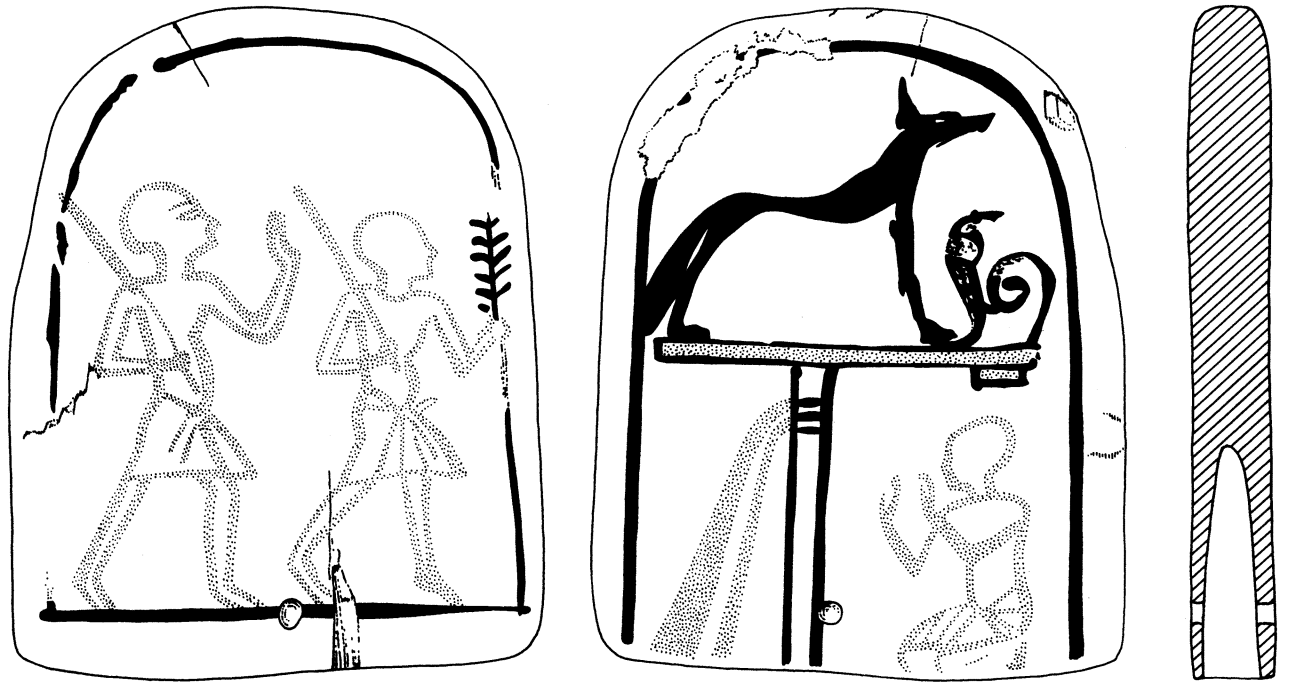
It is helpful to ask ourselves: supposing the occupants were engaged in tomb construction, what kind of work was mainly involved? The bulk of the work was the removal of soft rock to create the tomb chapels and the burial chambers (and in the case of the Royal Tomb, only the latter). The rock was removed partly by chipping and dumping the chips immediately outside; but where appropriate by more careful quarrying, removing the stone in small blocks for use elsewhere. As the chapels progressed supports were left for conversion into carved columns. As soon as wall surfaces became available one or more outline draftsmen started to lay out the wall decoration, closely followed by the sculptor who rendered the designs in relief. Sometimes the additional job of covering the wall with a thin skim of gypsum plaster was necessary before the artists went to work. Much of the tomb work was therefore done by people with labouring rather than artistic skills. The time spent by an artist within any one tomb was probably not great, and was presumably broken into several sessions as individual parts of the walls became available for decoration. Furthermore, the artists were fully conversant with the new art style and were able to lay out elaborate scenes of the life of the royal family as it could be observed in the context of the Central City. There is thus a case for separating the labouring from the artistic work. If we continue to see the village as housing a pool of tomb labour we are not obliged to include the few tomb artists amongst them. They could well have been drawn from the artists living in the main city, men of higher status who also worked on royal projects. We can, of course, make a direct comparison of size of house between the largest village house, East Street 1, and houses within the main city, using the index compiled by Piers Crocker.³⁰ This places East Street 1 below any of the houses to which names and titles are attached, including those of sculptors and builders.³¹ Yet if we people the village with tomb workers at all, we only emphasize the paradox in chronology, for the nearest group of tombs, the South Tombs, was begun fairly early in Akhenaten's reign.

One piece of evidence fails to fit the view that the village, like pre-Amarna Period Deir el-Medina, simply accommodated a pool of relatively unskilled labour (and a scribe) mainly for necropolis work. This is the decorated top to a wooden military standard found within the Sanctuary of the Main Chapel (fig. 5). This object had been left there when the village was finally abandoned. It could be argued (cf. *AR* 1, 3-4, 11) that the second phase of occupation was by a different group, namely guards looking after the tombs. But the continuation of the distinctive pig-raising industry

²⁹ W. Hayes, *Ostraka and name stones* (New York, 1942); Hayes, *JEA* 46 (1960), 29-52; M. Megally, *Studia Aegyptiaca* I (Budapest, 1974), 297-311.

³⁰ Deposited at the University of Cambridge; cf. *JEA* 71 (1985), 52-65.

³¹ With the uncertain exception of O47.16A and .20.



5 cms.

5239

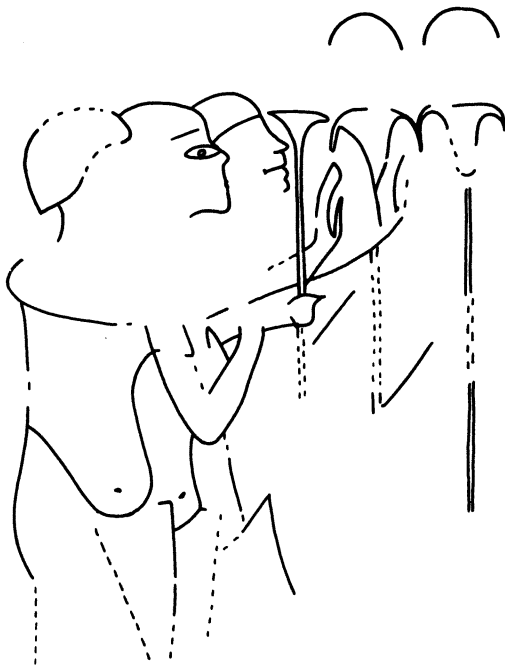


FIG. 5. Wooden top to a military standard, object no. 5239, found in the Sanctuary of the Main Chapel. Below left: military men holding standards in the tomb of Panehsy, el-Amarna. Below right: standard bearer in the tomb of Thanuny (Thebes no. 74).

throughout the entire history of the village argues also for an identity of population. One way of explaining this is to postulate the existence of a policing unit within the village population, one whose job it was, for example, to man the checkpoint at Site X1 and to supervise the exchanges between the two groups of people: the villagers and the supply columns and other outside visitors, who used the corridor of ground now represented by Site X2 and the *Zir*-area.

It was pointed out above that West Street differs somewhat from the rest of the village in showing signs of a greater degree of poverty, and may have been built significantly later, well into the reign of Tutankhamun. It would also have appeared to be a separate entity to those who lived in the Village. The main entrance to the village lay close to a major change of slope: very gently inclining from the east, sharply rising to the west. Anyone entering the village by the main gate anciently and looking left would have seen the edge of West Street 2/3 well above him, almost as if it lay on an acropolis. This sense of separation was emphasized by the rough stone walling that helped to cut off West Street from the rest of the village, and even more by the existence of the west gateway, which allowed the occupants of West Street their own independent entrance and exit. Perhaps in this we can find an explanation for the western quarter's separate existence. If we look at the contour map of the site as a whole, in terms both of linear distance and degree of slope, the west gate provides a favourable direct route to sites X1 and X2, the former already identified as a police post manning the natural access to the village, and overseeing the activities related to the supply of the village centred on the line between Site X2 and the *Zir*-area.

We have insufficient evidence for how many men constituted a unit of this kind. Evidence related to the Ramesside army sets the smallest likely unit at fifty men,³² but this is too large for our village since if each soldier/policeman was also a householder, they would have formed the majority of the population. The evidence from Western Thebes is also not very helpful. The Medjay-police numbered 60 in the time of Rameses IV, but another list comprised 24, 6 of them 'chiefs', whilst other records imply even fewer.³³ However, they were, as a group, not accommodated in Deir el-Medina, and this itself could form one of several ways in which the two villages differ. Černý's evidence also implies that there was a 'chief' for every three Medjay-men, but other sources imply that a 'chief' could also be a person of greater standing than this. The scenes of the 'Chief of Medjay of Akhetaten' Mahu in his tomb at Amarna (no. 9) certainly convey this. If West Street accommodated a police unit, and each policeman was there with his own household, a force of around twenty men is involved, with a chief in West Street 2/3. This would reduce the labouring element to around forty-seven households. We would then be able to explain the unequal dualism of the village: the larger, slightly more prosperous eastern sector entered via the main gateway and looked after by the occupant (the 'scribe' of the Main Chapel) of East Street 1; the poorer West Street sector, with its

³² A. R. Schulman, *Military rank, title and organization in the Egyptian New Kingdom* (Berlin, 1964), 26-9.

³³ Černý, *op. cit.* 261-3.

own independent gateway, looked after by the occupant (a police officer) of West Street 2/3, a person lower down the hierarchy than the owner of East Street 1, though still with access to the Main Chapel. It is likely that this western sector was built significantly later, in Tutankhamun's reign. Did it mark a return to the village in troubled times, which now required the presence of a larger military guard than before?

The overall strategy of research

The excavations at the Workmen's Village have had a modest share of discovery of things new. But 'discovery' is no longer an adequate reason for field archaeology. Excavation, even on a limited scale, produces a wealth of relatively routine information, and it is the handling of this which offers the real challenge for the future. The social sciences, of which Egyptology is a somewhat peripheral branch, but a branch all the same, are built on the profiling of societies through the abstraction of patterns from large masses of near-repetitive data. Only archaeology which is directed towards this end offers anything like the proper basis for a serious study of ancient Egypt at the socio-economic level. Many will object that only texts can do this, and there would be much truth to this assertion if there were enough of them and they were geographically and socially representative. But it is now abundantly clear from the slow rate of discovery and restricted range of provenances that this is unlikely ever to be the case. Their role has to be one of illuminating primarily social and economic mechanisms, whilst leaving it to other kinds of evidence to provide the basis for judging how widely and in what proportions those mechanisms acted at different periods.³⁴

The isolation of the Workmen's Village has provided a particularly valuable start at Amarna, for it defines the parameters that we can expect to recur: domestic and non-domestic architecture and a range of fittings related to domestic economy, the complex relationship between activity areas and the deposits of waste cleared from them, a far larger and more varied and informative pottery record than was ever visible from the older reports, and similarly more impressive records of other categories of finds, including cloth, basketry, and wood, as well as still only vaguely charted riches of an environmental kind. We know what one community, occupying a particular social and economic niche, left behind, and thus what occupied most of the people for most of the time. At this basic level we can cope with the evidence without knowing for certain what particular work the male heads of households did for the state as their employment, whether tomb cutting, policing or something else. We may lament the absence of other kinds of evidence—the more valuable objects that would have established levels of absolute wealth, the written records of daily life and administration which would instantly solve the problem of why the village was there at all. But we could wait for ever for these to be unearthed in significant

³⁴ See Kemp, *Archaeological Review from Cambridge* 3:2 (Autumn, 1984), 19–28, written in a pessimistic mood.

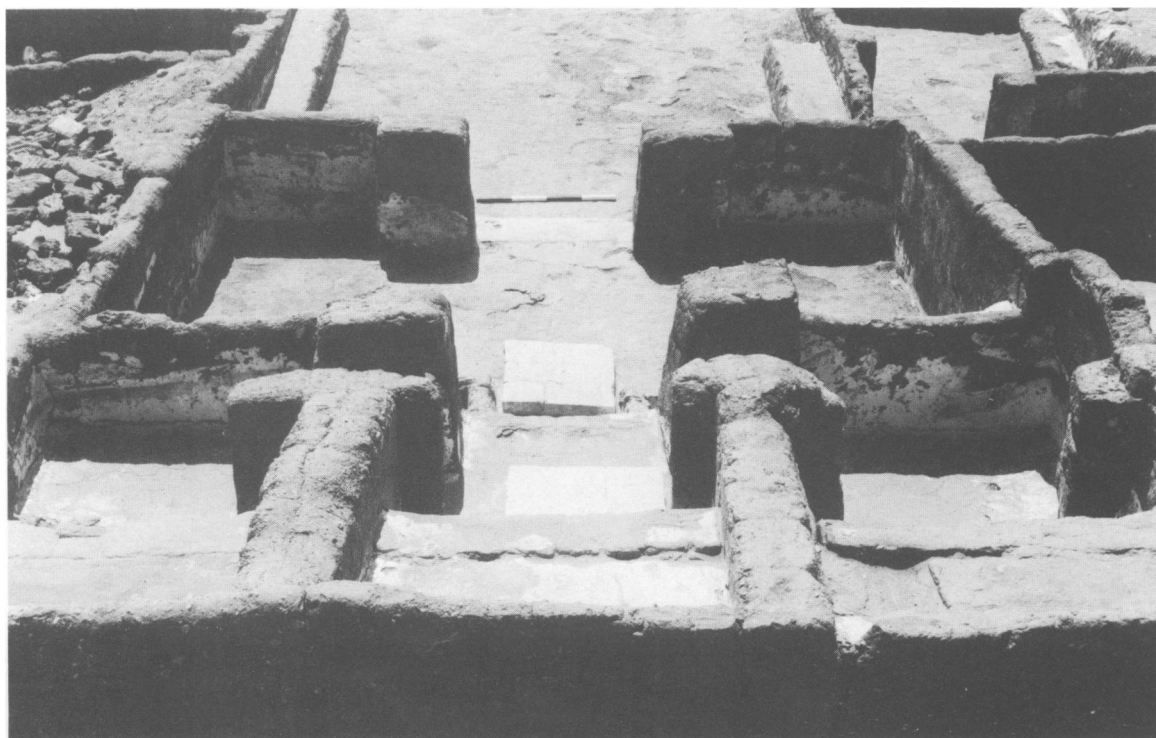
numbers. The way forward lies in being able to make ever broader comparisons with other communities similarly documented on the basis of what material we have.

To realise this ambitious goal we have to recognize two major difficulties: the framework of study and the sheer scale of the undertaking. The former exists because we have only the most general and intuitive ideas of how to profile a society through non-written evidence. For the latter, after eight seasons at the Village we have a database (or rather, a series of them) of no small size, but in terms of the city as a whole it is minute, even on a scale that accepts that carefully planned sampling will always have to suffice. Furthermore, progress in constructing the framework of understanding is only going to come from the empirical handling of ever larger databases. For these reasons, the current work should be seen within the broader and longer-term strategy of building up sample databases of equivalent size and type which represent the whole city, and in so doing, of taking more steps along the hard road towards the goal of profiling ancient societies in a meaningful way through their archaeological evidence.



1. Mît Rahîna. Limestone colossus 'Abû 'I-Hôl', looking north over the Ptah enclosure, before 1887. *Courtesy the Department of Egyptian Antiquities, British Museum*

MEMPHIS, 1985 (p. 20)



2. General view of the Main Chapel from the east. The scale is one metre long

AMARNA WORKMEN'S VILLAGE (p. 30)



1. Main Chapel annexe: artificial garden with growing plots, looking south (p. 34)

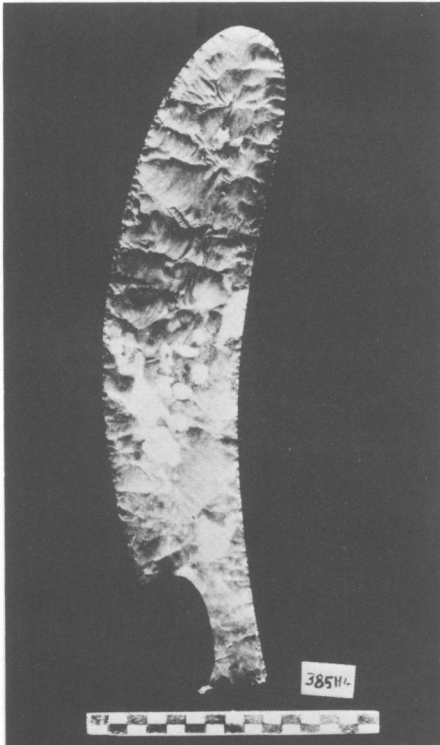


2. Building 300 (animal pens): view of one of the pairs of courtyard and pen, looking north-east (p. 37)

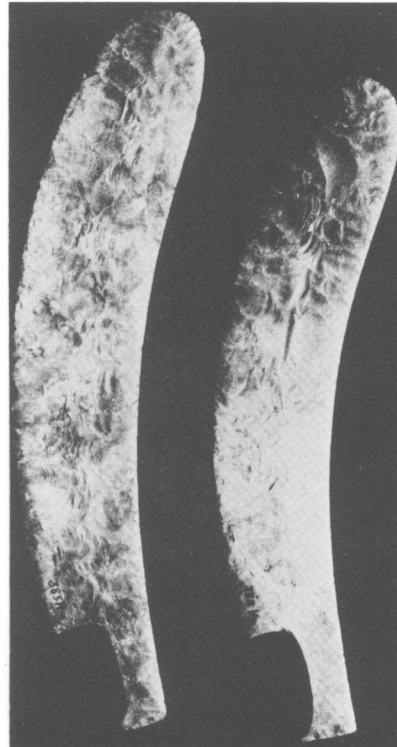


1. Building 250 (animal pens): the entrances to two adjacent pens showing the wooden cross poles over the thresholds, and changes in layout during a long period of use. View to the east

AMARNA WORKMEN'S VILLAGE (p. 37)



2



3

Three flint knives from tomb 385 H.4

THE ARCHAIC STONE TOMBS AT HELWAN (p. 66)

REMARQUES SUR LES REPRESENTATIONS DE LA PEINTURE D'HIERAKONPOLIS (TOMBE N° 100).

By JANINE MONNET SALEH

Reconsideration of the Hierakonpolis Painted Tomb in the light of the author's recent study of the so-called 'boats' found on Gerzean pottery. It is argued that the similar representations in the tomb are also incomprehensible as boats but perfectly intelligible as temples. The diagnostic characteristic is an entrance between two huts, but the prototype of the *sh-ntr* naos hieroglyph is also identified. The main difference from the pottery—the absence of supporting 'stilts'—reflects the desert environment of Hierakonpolis. Comparative study suggests that the decorative repertoire of the pots is here enlarged and placed in a human context, in which warfare is a prime element. An association is also postulated between the triple-vaulted hut of the tomb and the *wrmt* tent used in the funerary rites of Old Kingdom pharaohs under the patronage of Horus and Seth. This and other motifs, such as 'smiting', confirm the status of the tomb owner and his links with the later pharaonic tradition.

LA PEINTURE murale retrouvée à Hiérakonpolis par Green¹ en 1899, a été abondamment commentée, et les études les plus récentes, de Kaiser,² Case,³ Payne⁴ et Kemp,⁵ ont permis d'établir que cette peinture murale appartient à une tombe, portant le numéro 100 dans les archives de la fouille de Hiérakonpolis. De plus, ce qui reste du mobilier funéraire, et les dimensions relativement vastes de la tombe, impliquent que son occupant jouissait d'une position prépondérante par rapport à la masse de ses contemporains, plus modestement inhumés. Case et Payne employent même les termes de 'one of the legendary kings of Upper Egypt',⁶ dont par ailleurs la date est située autour des SD 48-53.

Si un nouvel examen des représentations de cette peinture murale semble nécessaire, c'est en raison du fait que les éléments construits avaient jusqu'ici été identifiés à des 'bateaux', par analogie avec les pseudo-bateaux figurés sur la panse de la poterie gerzéenne. J'ai exposé ailleurs⁷ les arguments en vertu desquels cette hypothèse stérile de 'bateaux' devrait être abandonnée. Ces représentations semblent bien être celles de plates-formes, juchées sur des pieux, et supportant des huttes jumelles et une enseigne, permettant d'identifier l'occupant des huttes jumelles à une divinité: Neit, Min, Horus, Hathor, Ré, ou toute autre divinité

¹ Cf. J. Quibell et F. Green, *Hiérakonpolis*, II, pl. lxxv-lxxix.

² *MDAIK* 16 (1958), 183-92.

³ *JEA* 48 (1962), 5-18.

⁴ *JEA* 59 (1973), 31-5.

⁵ *Ibid.* 36-43.

⁶ *JEA* 48, 18.

⁷ J. Monnet Saleh, *BIFAO* 83 (1983), 263-96. Je dois préciser, au sujet des représentations retrouvées à Gebelein, sur tissu, que dans la mesure où ces 'bateaux' sont pourvus de *rames*, terminées par un paleron arrondi, et représentées *traversant* la coque du navire pour atteindre les mains des rameurs, il ne peut pas être question d'assimiler ces vrais bateaux aux représentations des plates-formes de temples surélevées, des vases gerzéens. Cf. G. Galassi, *Rivista dell'Istituto Nazionale d'Archeologia e Storia dell'Arte*, nuova serie, IV (1955), 9, tav. i = fig. 12, p. 16 et fig. 10, p. 14.

primitive et oubliée. L'évolution du motif m'avait amenée à envisager les deux huttes comme l'origine possible des pylônes des temples pharaoniques.

En tenant compte de cette interprétation nouvelle, il faut examiner les éléments semblables de la peinture d'Hiérakonpolis. Et sur cette peinture, il faut distinguer les cinq éléments semblables et peints en blanc, du sixième, qui est peint en noir.

Les cinq complexes aux plates-formes blanches

Considérées dans leur ensemble,⁸ les cinq plates-formes blanches offrent, tout comme celles représentées sur la poterie gerzéenne, des différences de courbure, d'épaisseur et de longueur. De plus, alors que sur la poterie, si les deux groupes laissent subsister un espace libre entre eux, la plate-forme par contre était unie.⁹ Or sur la peinture de la tombe de Hiérakonpolis, les pieux ont disparu, mais trois des plates-formes comportent une zone médiane d'une couleur contrastée, qui peut sans difficulté être interprétée comme un passage. Ces éléments se présentent donc comme des plates-formes bâties à même le sol, et comportant une zone médiane d'accès.

Les ornements floraux¹⁰ suspendus aux extrémités des plates-formes de la poterie gerzéenne sont ici évoqués, sous des formes dont la nature exacte n'est pas facilement identifiable. Cependant, les éléments des bouts de plates-formes, sur la peinture murale, ne sont pas *verticaux*, mais leur 'tige' est plus ou moins *oblique*, et même presque horizontale, dans le cas de l'ornement placé sous la 'vigie' du bateau de la partie supérieure gauche de la peinture. Il ne semble donc plus y avoir d'*espace libre* entre la plate-forme et le sol proprement dit. Ce qui va dans le même sens que l'absence de représentation des pieux qui, sur les vases gerzéens, indiquent la surélévation des plates-formes par rapport au sol.

Autre différence enfin: les plates-formes de la peinture murale sont recouvertes de peinture blanche, par opposition à l'ocre jaune du fond, et elles comportent une bande peinte à l'ocre rouge, aboutissant entre les constructions jumelles figurées sur la plate-forme. Autrement dit, les seules couleurs indiquent clairement que les plates-formes sont faites de quelque chose de différent du sol sur lequel elles reposent, et une zone de passage (ocre rouge) permet d'aller de l'extérieur (ocre jaune) à la plate-forme, le passage conduisant *entre* les deux huttes, à cette porte quelques fois résumée par son linteau.

Remarquons encore que l'absence de pieux sous les plates-formes indique bien un changement dans l'environnement de ces plates-formes, par rapport à celui des vases gerzéens: elles ne sont pas construites dans un paysage montagneux, et l'eau ne s'amasse pas non plus en flaques, en marres ou en torrents,¹¹ mais elles sont placées dans un pays au sol meuble, *sans eau* apparente, et sans risque d'arrivée subite d'une

⁸ De bonnes reproductions de l'ensemble sont données par J. Vandier, *Manuel*, I¹, figs. 375-6 et 377. On trouvera également des reproductions de l'ensemble, mais aussi de détails et de personnages dans l'article de H. Case et J. C. Payne, *JEA* 48, 5-18.


⁹ *BIFAO* 83, 275-6 et figs. 7-8.

¹⁰ *Ibid.* 289.

¹¹ *Ibid.* 267-8 et fig. 3.

inondation qui, dans la Vallée, se produit régulièrement et annuellement, et ne dépasse pas une limite marquée dans le paysage par la ligne de végétation. Or, cette végétation est justement absente du paysage de la peinture d'Hiérakonpolis, et les constructions évoquées, tout comme la tombe elle-même, devaient être situées sur la bande semi-désertique, surélevée de quelques mètres par rapport au fond inondable de la Vallée du Nil, et sur laquelle, à Hiérakonpolis comme à Abydos, des gens s'étaient installés.

En ce qui concerne les deux huttes (ex-cabines), placées sur la zone centrale des plates-formes blanches, elles sont en tout point comparables à celles que reproduisent les scènes de la poterie gerzéenne:¹² même silhouette rectangulaire, même double boucle en haut de chaque angle des parois. Quant aux petites annexes qui flanquent ces huttes vers l'extérieur, elles se trouvent déjà sur quelques exemplaires des vases gerzéens.¹³ Mais l'enseigne figurée sur l'une des huttes des vases gerzéens manque ici. À vrai dire, la peinture d'Hiérakonpolis, dans un état moins délabré, aurait certainement montré ces enseignes: les deux motifs¹⁴ les plus à gauche, en bas, comportent encore quelques traces qui sont vraisemblablement les restes d'enseignes disparues.

En revanche, quelques éléments de la peinture murale semblent nouveaux, par rapport aux représentations des vases: il s'agit de la petite construction placée sur la gauche, au bout des plates-formes (n° 5 de Case et Payne), à l'abri de la haute palme qui, elle, figure régulièrement déjà sur les vases. Cette petite construction est aisée à identifier: elle se présente comme la 'façade de sanctuaire, de naos', de l'hiéroglyphe , le signe *sh-ntr* de la liste des hiéroglyphes. Si l'absence d'enseigne pouvait faire hésiter sur le caractère d'habitat divin du complexe des plates-formes de la peinture, ce petit *sh-ntr* nous ramène péremptoirement à la notion de temple.

Une autre remarque s'impose: quatre des plates-formes blanches et les constructions qu'elles soutiennent ne sont pas mises en rapport avec les êtres humains. Elles sont en quelque sorte désertes. Seule la plate-forme blanche la plus à gauche,¹⁵ semble 'en activité' en raison des personnages qui lui sont associés. Non seulement une 'vigie' est assise sur l'extrémité droite de cette plate-forme, mais trois personnages (n° 13 de Case et Payne), vêtus de jupes longues et blanches, effectuent une sorte de danse rituelle, bras étendus horizontalement. Les trois personnages ne sont pas directement sur la plate-forme, mais à quelque distance derrière, ou au dessus d'elle. Par contre, deux autres silhouettes humaines (n° 9 et 11 de Case et Payne) semblent se tenir sur le toit de la hutte de droite. L'une de ces silhouettes regarde vers la gauche (n° 11); elle est abritée sous une sorte de dais ou de naos (n° 9 de Case et Payne); elle tient quelque objet rituel dans sa main droite, tandis que la gauche est ramenée sur la poitrine. Est-elle debout? Les traces de la peinture sont peu explicites, dans la zone inférieure de cette représentation. S'agit-il

¹² Ibid. 279-84 et figs. 7 à 9.

¹³ Ibid. 280-1 et fig. 10.

¹⁴ Vandier, *Manuel*, 1¹, fig. 375, p. 563.

¹⁵ Cette plate-forme se trouve partiellement sur la fig. 375 de Vandier, *Manuel*, 1¹. L'extrémité de cette plate-forme et la 'vigie' qui s'y trouve, sont reproduites à la fig. 376 du même ouvrage.

d'une divinité? ou d'un personnage effectuant ou subissant des rites? On peut simplement constater qu'il s'agit plus vraisemblablement d'un homme que d'une femme, en raison d'une fausse barbe que sa tête, vue de profil, semble porter. Quant au personnage qui lui fait face, à l'extérieur du naos, il évoque, en dépit du mauvais état de la peinture, un orant ou un prêtre (n° 11 de Case et Payne). On aimerait pouvoir identifier la divinité adorée dans ce temple: malheureusement, le relevé de la peinture ne montre que quelques vagues traces au dessus du naos, et s'il existait une enseigne, elle a disparu.

Néanmoins, la représentation de ce temple en activité nous donne des renseignements précieux parce qu'uniques sur ce qu'était alors l'entourage d'une divinité. Ce dieu était logé dans un temple aménagé sur une épaisse plate-forme. Sur cette plate-forme, les bâtiments se composaient de deux huttes jumelles, d'une porte d'entrée ménagée entre ces huttes, d'une cour(?) délimitée par une barrière s'appuyant de part et d'autre des huttes, et enfin d'un naos, un *sh-ntr* abrité sous une haute palme, et déjà éloigné des deux huttes jumelles. Si l'on exclut le personnage abrité par le naos central et dont la nature et le rôle ne sont pas clairs, le personnel du temple semble comprendre un prêtre, trois danseurs ou danseuses, et une 'vigie'. Enfin, les quatre ibex représentés un peu au dessus de la plate-forme, sont couchés sur une ligne de sol tracée sous les animaux (n° 8 de Case et Payne); ce sol si précisément évoqué n'est pas n'importe quel endroit de la terre, symbolisée par l'ocre jaune du fond, mais quelque chose d'aménagé, à rattacher certainement au temple tout proche, et destiné aux animaux que l'on est également tenté d'inclure parmi l'entourage du dieu.

Les personnages et les animaux

Les animaux représentés sur la paroi sont presque tous mis en relation avec l'homme: les pièges¹⁶ tendus aux animaux sont de plusieurs sortes, et les bêtes ne s'en échappent pas. L'homme encore utilise le lasso pour capturer le taureau,¹⁷ et il est plusieurs fois aidé dans son travail de capture par le chien, qui semble bien être domestiqué et déjà un actif auxiliaire de son maître. La liste des animaux aux prises avec l'homme, sur cette paroi, n'est pas différente de celle des animaux représentés sur les vases gerzéens:¹⁸ gazelles, antilopes, oryx, ibex, chèvres, taureaux, ânes et volatiles — un oiseau noir à aigrette, un oiseau sans aigrette, une oie(?). Mais à la différence des vases gerzéens, des lions figurent aussi sur cette paroi: deux sont combattus par un homme armé d'un gourdin,¹⁹ en haut à gauche, et deux autres sont tenus par leur crinière et par un seul homme, comme le sont les fauves²⁰ du motif asiatique bien connu de héros Gilgamesh. C'est le seul élément de toute la décoration

¹⁶ L'étude et la comparaison de ces pièges avec des pièges africains a été faite par J. Leclant, cf. P. Huard et J. Leclant, *RdE* 25 (1973), 141 et fig. 1. Voir aussi Vandier, *Manuel*, I¹, 564.

¹⁷ Ibid., tableau n° 11, p. 568, et tableau n° 9, p. 567, pour les deux chiens qui participent à la capture de bouquetins.

¹⁸ *BIFAO* 83, 271-4 et figs. 5 et 6.

¹⁹ *JEA* 48, fig. 4, n°s 6 et 12 = Vandier, *Manuel*, I¹ tableau n° 1, p. 562.

²⁰ Ibid. 562 tableau n° 3 = *JEA* 48, fig. 4, n° 1.

de la tombe qui ait une connotation asiatique, tous les autres thèmes trouvant leurs analogies dans des documents typiquement locaux.

Autre particularité de cette tombe n° 100: les combats acharnés d'homme à homme,²¹ et la mise à mort²² de plusieurs hommes par un seul, thèmes qui n'apparaissent jamais sur les vases gerzéens.

En quelque sorte, les éléments figurés sur les vases gerzéens et repris sur cette peinture d'Hiérakonpolis, sont enrichis, développés, mis à jour, traités avec plus de détails, et placés dans un contexte humain différent de celui des vases. L'homme capture et domestique les animaux, et combat sans merci les autres hommes. Ce contexte de violence généralisée — qui ne semble pas être celui de la SD 40 — semble bien avoir prédominé dans la Vallée du Nil quelques temps *avant* l'unification: palettes et têtes de massues décorées ne relatent pas autre chose: il n'est pas sans intérêt de constater que le domaine funéraire est également envahi, dès avant l'Histoire, par ces thèmes, dont les tombes postérieures reproduiront maintes variantes: chasse au désert,²³ chasse dans les marais²⁴ etc.... La mise à mort de l'homme, par contre, n'aura pas de suite dans la décoration des tombes; mais elle figurera sur certaines tablettes retrouvées dans les tombes des souverains thinites,²⁵ où le sacrificateur, comme sur la peinture d'Hiérakonpolis, ne portera pas les attributs royaux; alors que la taille héroïque et les gestes seront les mêmes, sur la peinture de la tombe n° 100, et sur ce qui sera le motif classique du Pharaon assommant un ou plusieurs de ses ennemis, motif développé tout d'abord sur les palettes, dont la grande palette de Merynar conservée au Caire,²⁶ et ensuite sur tous les pylônes des grands temples d'Égypte. Cependant, dans la tombe n° 100, il s'agit vraisemblablement de la représentation d'événements contemporains,²⁷ car les combats et mises à mort qui y sont représentés s'insèrent parfaitement dans le contexte de luttes acharnées relatées par la palette aux Vautours, la palette du 'Tribut libyen', la tête de massue du Scorpion,²⁸ c'est à dire à une date beaucoup plus proche de l'unification que de la SD 40.

²¹ *JEA* 48, fig. 4, n° 3d-4-11 = Vandier, *Manuel*, I¹, 566 tableau n° 5. Il s'agit de deux 'duels' et dans les deux cas l'un des adversaires est figuré en état d'infériorité, très près d'être vaincu par l'autre. Le tableau n° 12 de Vandier, s'il n'était pas aussi mal conservé, aurait probablement montré un autre de ces duels à mort.

²² *JEA* 48, fig. 4, nos 2-3a-3b-d-8 et 12 = Vandier, *Manuel*, I¹, 562 tableau n° 2. C'est aux archéologues anglais que revient le mérite d'avoir analysé correctement ce motif, prototype lointain du sacrificateur — montré en taille héroïque — assommant un ou plusieurs ennemis, plus petits, et déjà subjugués.

²³ L'étude de ces représentations, dans les tombes pharaoniques, constitue le chapitre X *ibid.* IV, 787-833.

²⁴ Le même ouvrage, cité dans la note précédente, traite des chasses dans les lieux marécageux, au chapitre IX, 717-86.

²⁵ *Ibid.* I², 835, figs. 559 et 845, fig. 565.

²⁶ *Ibid.* I¹, 595-9, fig. 391-2. Le même thème, sous les rois de la Ière et de la IIème Dynasties, se trouve à la fois sur des palettes (*ibid.* I², fig. 573; W. B. Emery, *Archaic Egypt*, 60, fig. 23), et sur un graffito rupestre du Sinaï, daté de Semerkhet (Vandier, *Manuel*, I², 857, fig. 572).

²⁷ Sur la valeur historique d'une représentation, qui ne deviendra que par la suite un symbole stéréotypé, cf. mon article: *Interprétation globale des documents, et hypothèses de travail concernant l'unification de l'Égypte*, *BIFAO* 86 (1986) 237.

²⁸ Pour la palette aux Vautours, cf. Vandier, *Manuel*, I¹, 584-7, figs. 384-5; pour la palette du 'Tribut libyen', *ibid.*, pp. 590-2, fig. 388; la tête de massue du Scorpion figure aussi *ibid.* I¹, 600-2, fig. 393.

Le complexe de la plate-forme noire

Le dernier élément construit représenté sur cette peinture est le soi-disant ‘bateau noir’,²⁹ figuré au milieu de la paroi, vers le bas. Il y a de fortes chances pour qu’un tel bateau, posé sur l’eau, fasse naufrage avant d’avoir navigué, irrésistiblement tiré au fond de l’eau par le poids de cette proue (ou poupe?) verticale, dont la hauteur est supérieure à la moitié de la longueur de la coque! Aucun drakkar nordique, aucune gondole vénitienne n’a jamais été amputé d’une seule de ses extrémités relevées, et si les bateaux ‘étrangers’ figurés sur le manche du couteau du Gebel el Arak³⁰ ont aussi des extrémités haut relevées, chaque bateau possède *deux* de ces parties hautes, une à l’avant, l’autre à l’arrière de la barque, afin que celle-ci soit en équilibre, et puisse flotter et non pas couler! Parler de ‘convention du dessin’ ou d’‘erreur’ de la part de l’artiste ne résoud pas le problème, et il est plus sage de ne pas identifier ce motif à un bateau, et de noter seulement que ce qui soutient ici les constructions est peut-être une plate-forme, mais peut-être aussi quelque chose de différent, qui reste à déterminer.

Mais quoi qu’il en soit du support, les constructions placées sur lui sont comparables, sinon pareilles, aux huttes figurées sur les plates-formes blanches.

Semblable, la petite hutte coiffée de la ‘gorge égyptienne’, placée à côté d’une haute palme, dans l’angle gauche de la représentation. Semblable aussi, la hutte légère, aux deux boucles chevauchant les angles supérieurs, et flanquée sur sa partie gauche d’un élément moins élevé, qui peut être une barrière. Semblable également, le trait horizontal qui joint la hutte que nous venons d’examiner à la hutte de droite, et qui évoque certainement une porte. Toutes ces constructions semblent bien indiquer que la plate-forme noire sert de base à des éléments d’habitat divin, de temple, peut-être provisoirement transformé.

La hutte aux trois voûtes

Cette hutte de droite est différente de toutes les autres, et son intérêt est considérable: elle est formée de trois traits parallèles et continus,³¹ qui semblent indiquer une construction au toit arrondi. Des traits horizontaux remplissent la partie centrale, et cachent ce que contenait cette curieuse hutte. Le *profil* de cette construction évoque une fois encore celui de la hutte de l’hiéroglyphe *sh-ntr*.³²

On connaît une autre hutte rituelle, représentée comme celle-ci par trois traits parallèles et non pas par un seul: c’est la construction figurée sur la tablette en ivoire retrouvée à Naqada,³³ et si magistralement étudiée par Grdseloff,³⁴ qui a pu l’identifier comme la tente funéraire du Pharaon dont le nom de *Nbty* était *Mn*. Les

²⁹ Vandier, *Manuel*, I¹, fig. 376 et p. 570 = Case et Payne, *JEA* 48, fig. 5, n^{os} 5 et 10 et pp. 13-14.

³⁰ Vandier, *Manuel*, I¹, figs. 359-60 et pp. 538-9.

³¹ Ces trois traits sont plus nettement lisibles sur la reproduction de Vandier, *Manuel*, I¹, fig. 376, p. 565, que sur celle donnée par Case et Payne, *JEA* 48, 14, fig. 5, en raison des conventions adoptées par les archéologues anglais pour rendre les couleurs originales sur un dessin en noir et blanc.

³² Lefebvre, *Grammaire de l’Égyptien Classique*, liste des signes hiéroglyphiques, n^o O 21.

³³ Vandier, *Manuel*, I², fig. 556 et pp. 828-9.

³⁴ *ASAE* 42 (1943), 279-82, fig. 27; et Sethe, *Die altägyptischen Pyramidentexte*, II, § 2100.

Textes des Pyramides fournissent un troisième exemple de cette construction particulière, mentionnant explicitement qu'un *sh* est tressé par Horus, puis ensuite recouvert par le dieu Seth de trois voûtes de jonc, les *wrmt*. L'ensemble est une tente funéraire à l'intérieur de laquelle Néferkaré défunt — le Pharaon dans la Pyramide duquel ce texte est inscrit — subira les rites qui le transformeront en *ntr*.

Ainsi donc le complexe à plate-forme noire se trouve mis en rapport avec les rites funéraires, et plus précisément avec les rites dont les Pharaons seront, à la Ière puis à la VIème Dynasties, bénéficiaires. Rites dont on voit mal le déroulement sur un quelconque bateau...

On doit encore se poser la question de savoir si, à l'époque de la tombe n° 100, la tente aux *wrmt* était ou non déjà placée sous le patronnage d'Horus et de Seth. En d'autres termes, le document a-t-il, à si haute époque, les mêmes implications qu'à la fin de l'Ancien Empire? Car si les deux dieux étaient déjà associés dans les soins à donner à un Chef défunt, cela signifierait, en clair, que ces deux divinités et leurs dévôts étaient à l'époque assez nombreux et assez puissants, et apparentés ou alliés, pour présider en commun au rituel funéraire d'un Chef défunt.

En ce qui concerne le dieu Horus, il est déjà attesté sur les enseignes³⁵ des temples à plates-formes surélevées des vases gerzéens, ainsi d'ailleurs que sur les palettes décorées³⁶ protodynastiques, et le premier Pharaon de l'Histoire, Merynar, sera un Horus, l'incarnation sur terre du dieu faucon. Dès lors, il est vraisemblable qu'Horus ait été l'un des principaux officiants qui accomplirent les rites destinés au Chef ou au roi mort, et évoqués tant sur la peinture de la tombe n° 100 que sur la tablette de Naqada.

Quant au dieu Seth, on considère généralement que la plus ancienne représentation de son animal a été trouvée à Mahasna,³⁷ et il figure déjà sur un vase décoré gerzéen, ainsi que le regretté Labib Habachi³⁸ l'avait bien montré. Seth est représenté aussi sur la tête de massue du Scorpion, à deux reprises perché sur un pavois qui sert aussi de potence à un vanneau pendu par le cou. Mais il faudra attendre le milieu de la IIème Dynastie pour trouver Seth réellement et étroitement associé au Pharaon: Peribsen et Khasekhemoui³⁹ lui feront partager avec Horus le parainnage royal.

Mais dans le contexte de la tombe n° 100, et en l'absence de toute enseigne qui permette d'identifier les occupants des temples sur plates-formes figurés dans cette tombe, on peut seulement constater que la tente aux triple nattes sera, un peu plus tard, mise en rapport avec Horus et Seth, divinités dont on sait par ailleurs qu'elles étaient bien attestées parmi les dieux révéérés par les contemporains de cette tombe n° 100.

Le présence de cette tente aux *wrmt* renforce, cependant, l'hypothèse selon


³⁵ BIFAO 83 (1983), 287.

³⁶ Voir l'article cité dans n. 27 ci-dessus, pp. 231, 233-5.

³⁷ Voir à ce sujet Vandier, *Manuel*, I¹, 389 n. 10.

³⁸ ASAE 39 (1939), 770-4 et pls. cxliv et cxlv.

³⁹ Emery, *Archaic Egypt*, 95-7 pour Peribsen, et 101-3 pour Khasekhemoui; J. Yoyotte, *Histoire Universelle*, Encyclopédie de la Pléiade, I, 117; Drioton et Vandier, *l'Egypte*, 142-3.

laquelle l'occupant de la tombe était un Chef important. Était-il un  plutôt qu'un Horus? Portait-il la couronne blanche dévolue par Nekhabit le Vautour aux Chefs des populations se réclamant d'elle?⁴⁰ Rien ne permet de répondre à ces questions, mais une chose reste sûre: le propriétaire de la tombe n° 100 fait sûrement partie des 'legendary kings' dont les traditions funéraires et religieuses se transmettront fidèlement à ses successeurs et jusqu'à celui qui sera le *Nbty Mn*, autrement dit un Pharaon. De plus, les temples sur plates-formes de la décoration de cette tombe permettent de faire remonter cette lignée jusqu'aux peuplades nomades et semi-nomades qui pérégrinèrent aux confins de la Vallée, dans l'actuel désert, mais qui n'en était pas un avant la SD 40. Et donc de constater aussi que les racines du pouvoir pharaonique remontent très loin dans le temps, mais restent localisées à la Vallée et à ses confins.

⁴⁰ Voir la seconde partie de l'article cité dans n. 27 ci-dessus, à paraître dans le *BIFAO*.

THE ARCHAIC STONE TOMBS AT HELWAN

By WENDY WOOD

From 1942 to 1954, Zaki Saad excavated an ancient Egyptian cemetery at Helwan. He claimed to have found a series of stone tombs spanning the Archaic period that enable us to study the development of stone construction. A re-examination of the material yields more accurate dating, largely by typological comparisons of the substructure plans with those of more important tombs at Abydos and Saqqâra. The rarity of the stone tombs at Helwan, not their construction or development, is remarkable, especially in view of the concern for security that resulted in deeper substructures and limestone portcullises. Although the evidence is meagre, it suggests that stone construction developed more rapidly in Archaic temples, perhaps because brick had symbolic value for tombs. In view of all the factors in favour of using limestone for tombs, religion was probably the only force powerful enough to retard development.

FROM 1942 TO 1954, Zaki Saad excavated a cemetery at Helwan across the Nile from the necropolis at Saqqâra.¹ He claimed to have found a series of stone tombs spanning the Archaic period that enable us to study the development of stone construction in ancient Egypt.² His methods of analysis were faulty, however, and his claims overstated. A re-examination of the material yields more accurate dating; it also leads to a different assessment of the historical significance of the Archaic stone tombs at Helwan and the use of the new construction material.

Saad nowhere mentioned the total number of stone tombs at Helwan. In his preliminary reports I have counted nine tombs in which limestone appears to have been used extensively, one tomb with a burial chamber cut in soft rock, and one very large tomb which, on the basis of limestone slabs lying in the burial chamber, was probably partially lined with stone. Saad singled out five of these tombs for his proposed sequence of development, using tombs 1 H.3 and 40 H.3 as representative of the first half of the First Dynasty, tombs 1390 H.2 and 385 H.4 as representative of the second half, and tomb 287 H.6 as late Second or early Third Dynasty. The remainder of the 10,258 tombs excavated by Saad at Helwan in twelve seasons were cut in gravel and built of mud-brick.

Tomb 1 H.3 is first in Saad's sequence. 'Very few traces' of the superstructure remained, and their nature was not reported.³ An L-shaped stairway flanked by L-shaped magazines cut in gravel, two on each side, leads directly to the burial chamber. The stairway walls are mud-brick with a lining of stone, and the steps are stone. Two limestone portcullises were found *in situ* along the north-south stretch of the stairway. The rectangular burial chamber was cut in gravel and paved with

¹ Zaki Saad, 'Royal Excavations at Saqqâra and Helwan (1941-1945)', *Supplément aux ASAE*, Cahier 3 (Cairo, 1947), 1-178; 'Royal Excavations at Helwan (1945-1947)', *ibid.*, Cahier 14 (Cairo, 1951), 1-84; *The Excavations at Helwan* (Norman, Oklahoma, 1969).

² Saad, *Excavations*, 28-9.

³ *Ibid.* 164.

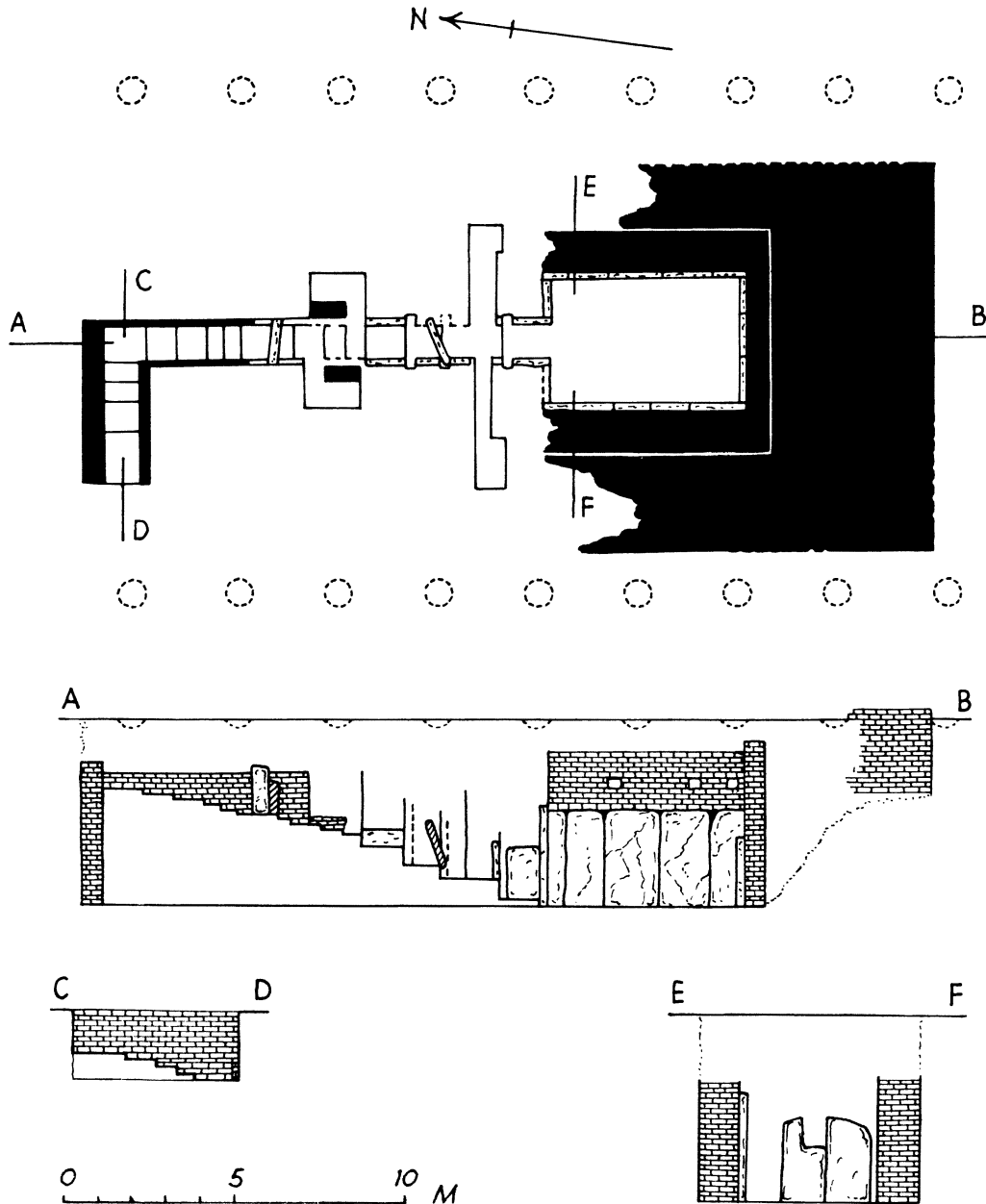


FIG. 1. Plan and sections of tomb 1 H.3 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3 (1947), pl. lxi. (Redrawn by Marian Cox)

limestone. The walls are mud-brick with a thin lining of limestone blocks, and the roof was wooden. No dimensions were reported, but from the plan and sections (fig. 1) it is possible to estimate the size of the burial chamber as approximately $6 \times 4 \times 3$ m high. The limestone blocks are placed on their ends and extend upward far enough to make coursing unnecessary. Tomb 1 H.3 had been plundered. In the preliminary report Saad stated: 'We were able to restore one

tubular alabaster jar from Tomb No. 1 only.⁴ No illustrations or further descriptions of the restored jar, which is not mentioned in the final publication, were given. It was evidently not used to date the tomb. Saad dated tomb 1 H.3 to the first half of the First Dynasty, prior to the reign of Den, without stating his reasons. The dating was apparently based on his interpretation of the stone lining as a primitive precursor of stone walls.

The stone lining appears in a different light after the use of typological comparisons to date tomb 1 H.3. Reisner, in his comprehensive study of early Egyptian tombs, concluded that kings' tombs led the main line of formal and technical development in the early Archaic period.⁵ Within the court's geographical sphere of influence the nobility and other members of the elite tended to construct tombs that imitated more prominent tombs that were contemporary or slightly earlier in date. The ground plans of the imitative substructures generally resemble those of the great tombs of Abydos and Saqqâra. Junker has cautioned against dating tombs solely by typological comparisons.⁶ Factors that undoubtedly influenced development, such as family and workshop traditions and personal taste, are not taken into account. Nevertheless, the method yields a *terminus post quem* and an approximate date.

The two great monuments at Abydos and Saqqâra that tomb 1 H.3 at Helwan most closely resembles in plan have both been dated to the reign of Ka'a, the last king of the First Dynasty. The largest mastaba on the escarpment at Saqqâra is 3505, the tomb of a high official named Merka. Its substructure consists of an L-shaped ramp with a limestone portcullis and a rectangular burial chamber. Although only two magazines, rock-cut and walled with mud-brick, adjoin the ramp, their peculiar double-L form might have been the prototype for the northern magazines of tomb 1 H.3 at Helwan. Emery dated tomb 3505 to the reign of Ka'a by jar sealings bearing his name and by the formal similarity of the substructure to that of Ka'a's tomb Q at Abydos.⁷ The similarity of tombs 1 H.3 at Helwan, 3505 at Saqqâra, and Q at Abydos yields an approximate date for the Helwan tomb at the end of the First Dynasty or slightly later.

In addition to dating tomb 1 H.3 at Helwan, the radial pattern of development suggests an interpretation of the stone lining of the burial chamber. If the rock-cut burial chamber of tomb 3505 at Saqqâra had a lining, it was destroyed by fire, but we know that the mud-brick burial chamber of tomb Q at Abydos was lined with wood. The lining in the mud-brick burial chamber of tomb 1 H.3 at Helwan is probably a stone imitation of a wooden form.

Derivative tombs can be expected to show reductions and substitutions dictated by the lesser resources of their owners. The burial chamber of Helwan tomb 1 H.3 is less than half the size of the burial chamber of Abydos tomb Q. The financial

⁴ *Supplément*, Cahier 3, 165.

⁵ G. A. Reisner, *The Development of the Egyptian Tomb Down to the Accession of Cheops* (Cambridge, Massachusetts, 1936), 5-6 and *passim*.

⁶ Hermann Junker, *Giza IX* (Vienna, 1950), 23.

⁷ W. B. Emery, *Great Tombs of the First Dynasty*, III (London, 1958), 5.

resources of the king were, of course, greater. Was limestone substituted for wood in the burial chamber at Helwan for the sake of economy?

The relative value of construction materials during the Archaic period must be deduced from their availability, their tractability, and the extent and manner of their use in architecture. Sizeable pieces of imported wood would have been very costly, probably surpassing even hard stone in value. Various woods were evidently imported as early as Predynastic time, but only small pieces have been identified.⁸ There is no evidence that the material was used for construction. Lucas pointed out that Egyptian reliance on imported timber was probably less than has been supposed.⁹ Construction wood of the Archaic period was probably all domestic wood, and it was available throughout the Nile valley. For most of Egyptian history even domestic wood of good size and quality has been a luxury. But the value of domestic wood was probably somewhat lower during the First Dynasty when the climate was more moist and trees were more abundant. At the end of the dynasty the climate began to change toward the more arid conditions that still prevail.¹⁰ The value of domestic wood must have begun to rise after the First Dynasty.

Why did the Helwan tomb owner use limestone to line his burial chamber but wood to roof it? Split trunks of date palms were used for roofing, an unsophisticated and probably relatively inexpensive form. The wooden linings that improved the appearance of mud-brick walls were pieced together by skilled carpenters and joiners. It was probably the cost of skilled labour more than the cost of domestic wood that made a wooden lining too expensive for the Helwan tomb owner. The limestone lining in the Helwan tomb would have been less costly than a wooden one. The hills near Helwan are rich in limestone, and, judging from the sections of the burial chamber (fig. 1), the blocks are thin and irregular. No remarkable expenditure for transport or refined cutting was necessary. Although the motive for lining the burial chamber of tomb 1 H.3 at Helwan with stone was probably primarily economic, a desire for security cannot be entirely discounted. Fear obviously prompted the installation of stone portcullises to block the stairway, and fear might have made limestone doubly attractive as a material for lining the burial chamber.

Another instance of the substitution of materials in a derivative structure can be identified at Helwan in tomb 40 H.3. The substructure (fig. 2) consists of a tapered stone stairway with two stone portcullises *in situ* and a rectangular burial chamber cut in gravel. At the north end the chamber is subdivided to form two small magazines on either side of the entrance. The chamber is paved and 'walled up with big white limestone blocks in the same manner as in tomb No. 1'.¹¹ No dimensions were reported, but from the published plan and sections it is possible to estimate the size of the burial chamber as approximately 5.0 × 3.5 × 2.5 m, and the largest limestone block as approximately 3 × 2 m × 50 cm.

⁸ A. E. Lucas, *Ancient Egyptian Materials and Industries*, rev. edn. (London, 1962), 432.

⁹ *Ibid.* 429.

¹⁰ K. W. Butzer, 'Physical Conditions in Eastern Europe, Western Asia, and Egypt Before the Period of Agricultural and Urban Settlement', in *Cambridge Ancient History*, 1, rev. edn. (Cambridge, 1970), 68-9.

¹¹ Saad, *Supplément*, Cahier 3, 164.

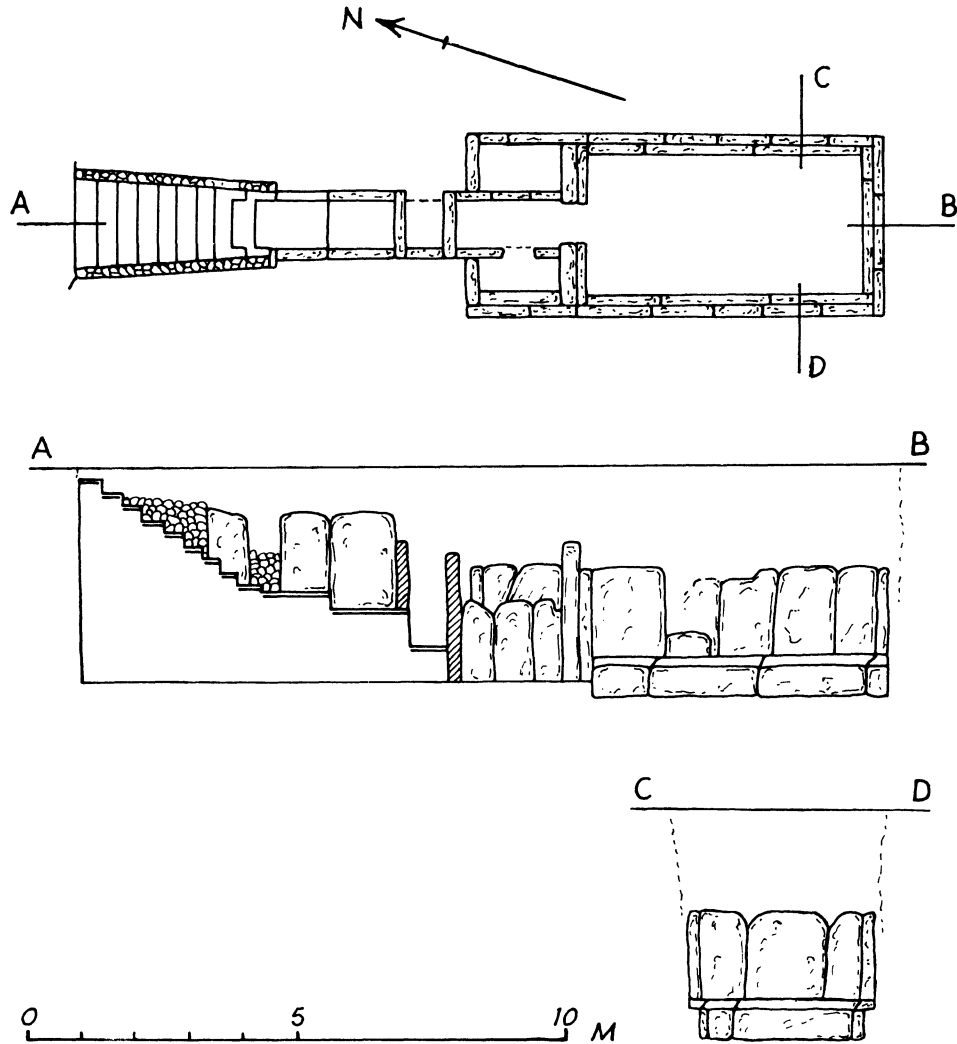


FIG. 2. Plan and sections of tomb 40 H.3 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3 (1947), pl. lxii. (Redrawn by Marian Cox)

The tomb had been plundered, and Saad reported: 'The only items we found were a very few alabaster fragments and broken pottery jars, but from these fragments we were able to date the tomb as belonging to the First Dynasty.'¹² He placed the tomb in the first half of the First Dynasty, prior to the reign of Den but after tomb 1 H.3. This more precise dating was apparently based on his interpretation of the stone kerbs around the edge of the floor in the burial chamber and the magazines (pl. VII, 1): 'The stone blocks walling both the burial chamber and the two magazines are reinforced by blocks of the same stone placed on their sides to prevent the standing blocks from falling.'¹³ Saad apparently viewed the so-called reinforcement as an advance in stone construction.

¹² *Excavations*, 26.

¹³ *Supplément*, Cahier 3, 164-5.

The stairway suggests that tomb 40 H.3 probably is not prior to the reign of Den. The closest formal parallel among prominent tombs of the First Dynasty is the granite-paved tomb T of Den at Abydos.¹⁴ The burial chamber of tomb T was entirely of wood with the lining separated from the nucleus to create magazine space between the wooden walls. No subsidiary rooms flank the entrance inside the chamber, as in tomb 40 H.3, but there is a wide bay that might have constituted such a space. On the basis of formal similarity to tomb T at Abydos, tomb 40 H.3 at Helwan can be dated approximately to the reign of Den, reversing the chronological order of the first two tombs in Saad's sequence. According to Emery, Archaic burial chambers with wooden floors commonly have wooden baseboards, also.¹⁵ The stone kerbs in tomb 40 H.3 are probably a translation of wooden baseboards. Economy again appears to be the principal reason for using limestone.

No plan was published for Helwan tomb 1390 H.2, which was not mentioned in the preliminary reports but which Saad placed third in his proposed sequence and dated to the second half of the First Dynasty. From his brief description and one photograph (pl. VII, 2), the substructure consists of a burial chamber with magazines at either end.¹⁶ Although the arrangement recalls the separate spatial units of early First Dynasty tombs, the main chamber communicates with the magazines by doorways. The burial chamber is built of limestone slabs measuring 2.5 m × 80.0 × 20.0 cm and laid horizontally in two courses. One of the slabs is cut to serve as a door lintel. Saad apparently based his dating on his interpretation of this crudely tailored stone as evidence of technical progress.

The plan of the cemetery bordering the El-Khashab Canal reveals that tomb 1390 H.2 is a modest single-chamber substructure.¹⁷ Presumably, Saad intended to refer to the nearby tomb 1389, which has a shallow chamber with end magazines and communicating doorways. The plan is similar to that of the mud-brick tomb 649 H.5 (fig. 3), which, however, is deeper and has a tapered stairway. Tomb 649 H.5 appears to be a variant of the type exemplified by tomb T at Abydos, and tomb 1389 can therefore be dated by association with tomb 649 H.5 approximately to the reign of Den, contemporary with tomb 40 H.3 at Helwan.

Saad dated tomb 385 H.4 at Helwan, the fourth in his sequence, to the second half of the First Dynasty, apparently on the basis of pottery fragments. The single limestone slabs laid horizontally on the north and south walls of the rectangular burial chamber measure 4 × 2 m × 40.0 cm. The east and west walls each have one additional block measuring 1.17 × 2.17 m × 40.0 cm to span the long sides of the rectangle. Saad's description of the slabs as 'double the size of the blocks used in previous tombs' is a little exaggerated, but he was clearly impressed by the increase in scale and the greater care taken in dressing the stone.¹⁸ He thought the substructure was probably roofed with stone.

¹⁴ W. M. Flinders Petrie, *The Royal Tombs of the Earliest Dynasties* (London, 1901), II, 9-11.

¹⁵ Emery, *Archaic Egypt* (Baltimore, 1961), 187.

¹⁶ *Excavations*, 28-9.

¹⁷ *Supplément*, Cahier 3, folding plans at end.

¹⁸ *Excavations*, 11 and 32.

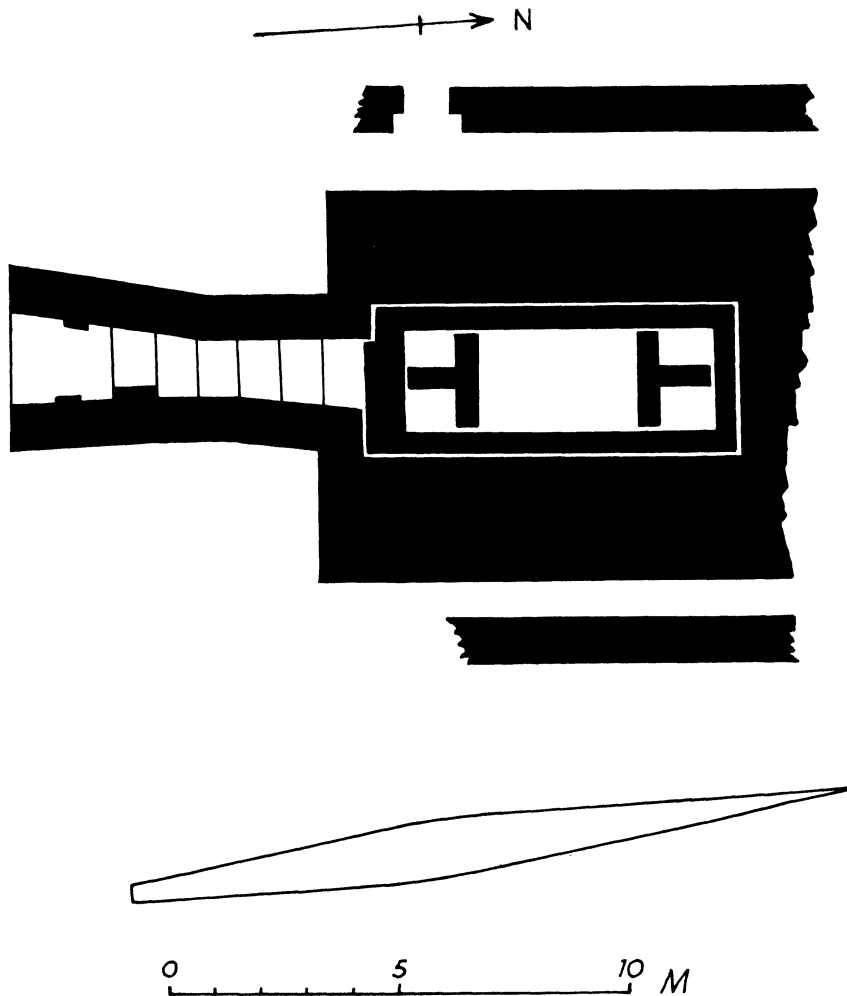


FIG. 3. Plan of tomb 649 H.5 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 14 (1951), plan 16. (Redrawn by Marian Cox)

This tomb is the first in Saad's sequence to display any true evidence of technical advance in stone construction. But while he is correct in seeing the tomb as a step forward, the plan and finds suggest that the tomb does not belong to the First Dynasty but rather to the end of the Second. For the later date the technical advance is not remarkable. By the end of the Second Dynasty, which is generally believed to have been much less stable than the First, stone was probably used primarily for security and permanence.

The substructure of Helwan tomb 385 H.4 (fig. 4) has an axial stairway with flanking magazines, but it has neither the spatial unity of the tombs of Den's reign nor the L-shaped approach and integrated magazines of the reign of Ka'a. The plan of the Helwan tomb is apparently related to a new type that appeared at Saqqâra in the early Second Dynasty. The substructure of the new type was subdivided, with

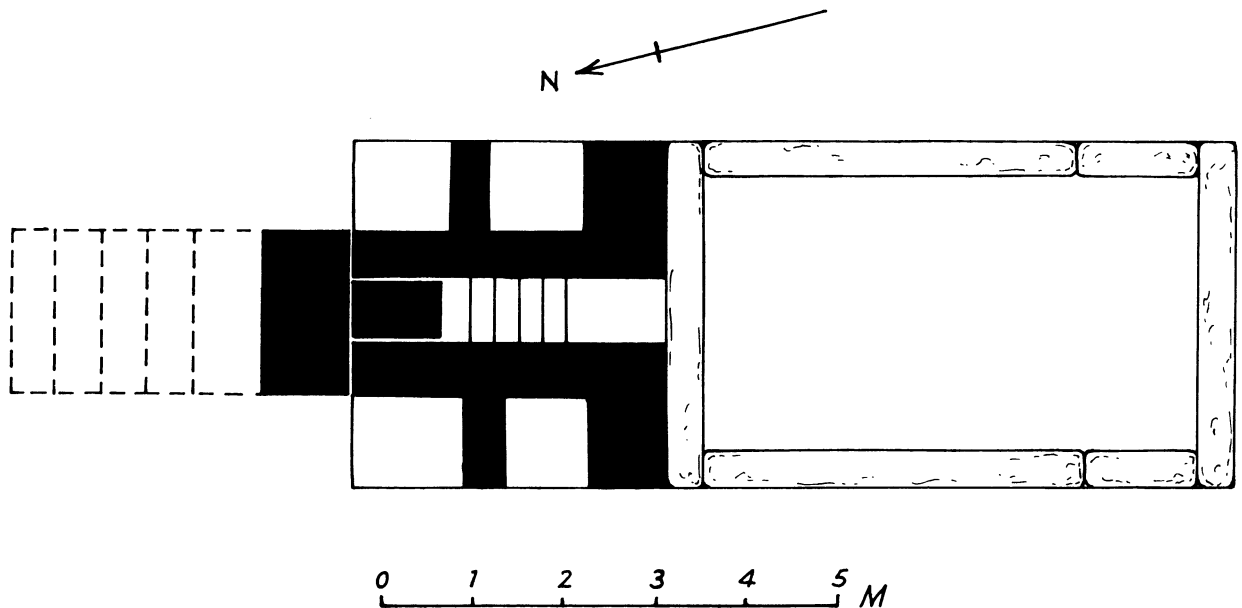


FIG. 4. Plan of tomb 385 H.4 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 14, pl. 8. (Redrawn by Marian Cox)

the burial chamber on the west to correspond to a master bedroom in domestic architecture, creating a more literal interpretation of the tomb as a house. The stairway, usually on the north side of the burial chamber, no longer entered on axis but was shifted eastward, balancing the westward expansion of interior space. Stone flags began to replace timber as a roofing material, for wood was more scarce and the times were troubled. In spite of its conservative rectangular burial chamber, axial stairway, and magazines discrete from the burial chamber, tomb 385 H.4 can be related to the house type through association with Helwan tomb 355 H.4 (fig. 5), which has the same type of stairway with one magazine but is a simplified house plan with the stairway off axis and a burial chamber that expands westward.

Additional evidence for dating tomb 385 H.4, not used by Saad, is provided by three flint knives (pl. V, 2-3) found in the intact north-west magazine. The shallow blade curvature, rounded tip, and the shallow cutting out of the handle with a relatively short handle-spur are characteristic of this type of knife in the late Second or early Third Dynasty, as can be seen by comparison with examples found by Petrie in tomb V of King Khasekemui at Abydos.¹⁹ A close dating of tomb 385 H.4 on the basis of the knives would be unsound. Needler has pointed out one very late find of the curved type at Naga-el-Deir and another find at Saqqâra in which various stages of the type were represented.²⁰ Together with the plan of the substructure, however, the knives support a late Second Dynasty date for tomb 385 H.4.

Saad dated the fifth tomb in his sequence, 287 H.6, to the late Second or early

¹⁹ Petrie, *Abydos* (London, 1902), 1, pls. xiv and xv.

²⁰ Winifred Needler, *JEA* 42 (1956), 41, n. 6.

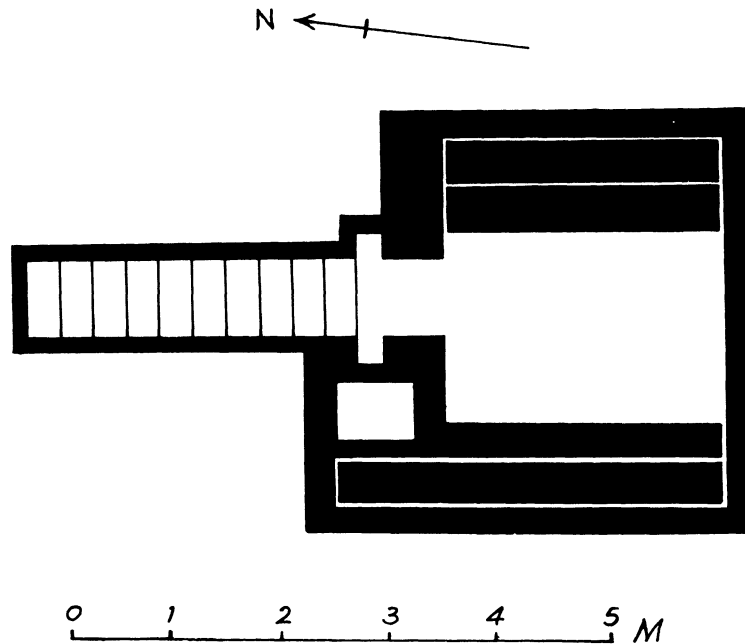


FIG. 5. Plan of tomb 355 H.4 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 14, pl. 7. (Redrawn by Marian Cox)

Third Dynasty. The tomb is a ruined stone mastaba with a rectangular shaft to the burial chamber (pl. VI, 1-2). No evidence suggests that this conventional shaft tomb is earlier than the end of the Third Dynasty.

Re-examination of the archaeological evidence thus leaves only three tombs from Saad's sequence that appear datable to the First Dynasty. I would place the three First Dynasty tombs in a different chronological order: tombs 40 H.3 and 1389 approximately from the reign of Den, and tomb 1 H.3 approximately from the reign of Ka'a. The remaining tombs in Saad's sequence, 385 H.4 and 287 H.6, are probably of the late Second and late Third Dynasties respectively, somewhat later than his dating. None of the tombs has any feature that is not consonant with the use of stone at other sites in tombs of the same dates, and the sequence is not a reliable indicator of technical development.

Little can be said about the remaining stone tombs at Helwan. The highest ground, almost invariably occupied by large tombs at an early date, and the direction of the cemetery's growth are not recorded. Few plans were given and formal descriptions are either too brief to be helpful or are omitted altogether. Tomb 9 H.1 has a limestone substructure, and tombs 589 H.1 and 601 H.1 have burial chambers lined with limestone.²¹ The burial chambers of tombs 1371 H.2 (fig. 6) and 1502 H.2 (fig. 7) are paved with limestone, the surface in the latter being covered with a layer of whitewashed plaster.²² Both substructures are of the same type as tomb 1 H.3 (fig. 1) and can therefore be dated approximately to the end of the First

²¹ Saad, *Supplément*, Cahier 3, 28.

²² *Ibid.* 109-11.

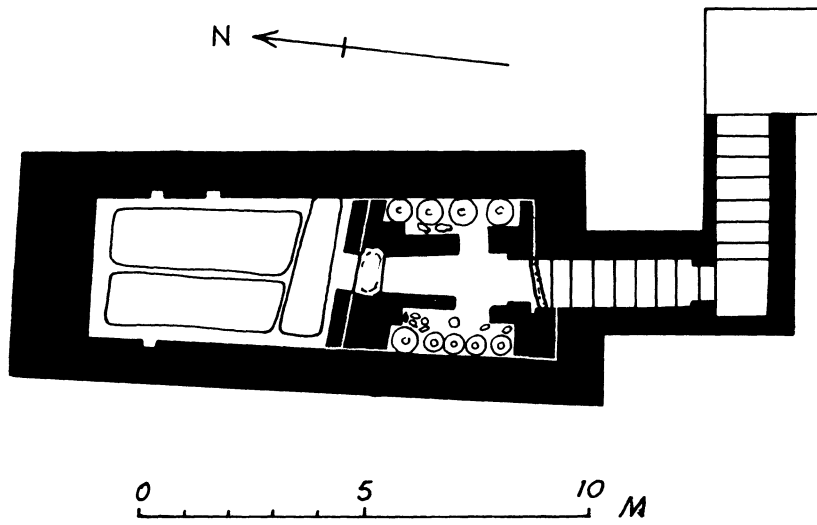


FIG. 6. Plan of tomb 1371 H.2 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3 (1947), pl. xxxviii. (Redrawn by Marian Cox)

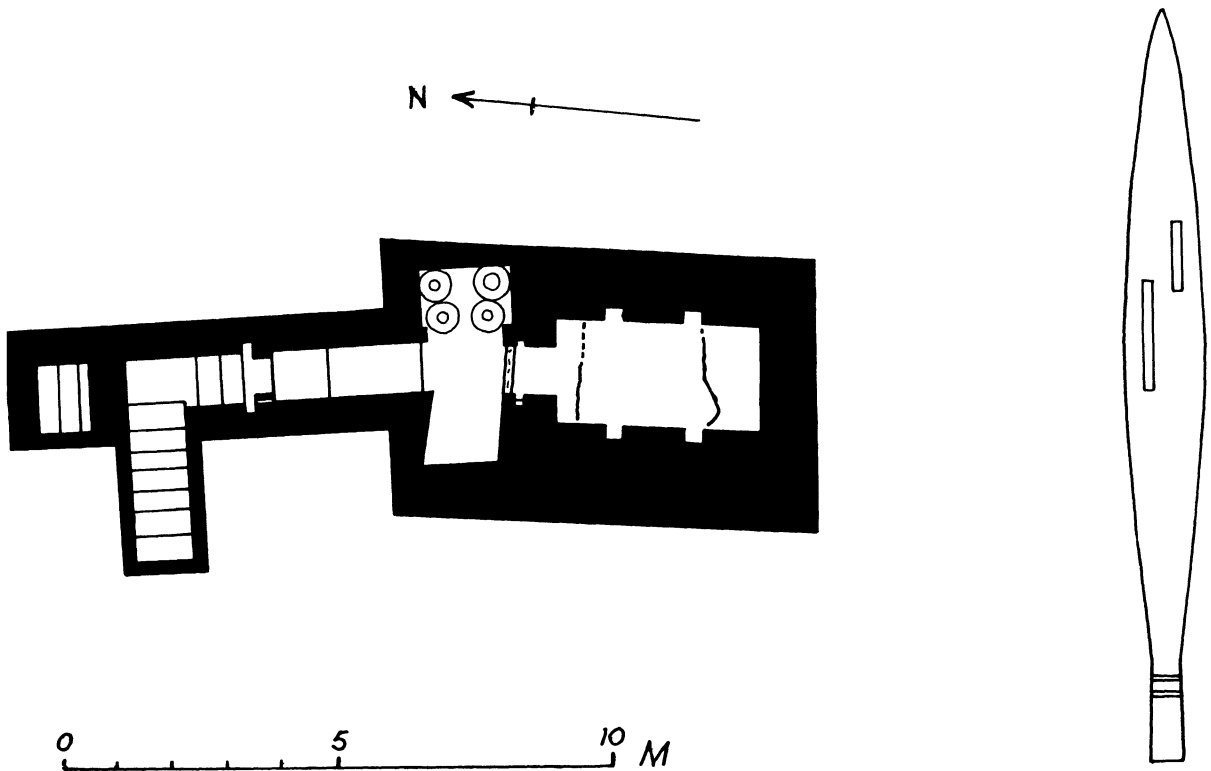


FIG. 7. Plan of tomb 1502 H.2 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3, pl. xl. (Redrawn by Marian Cox)

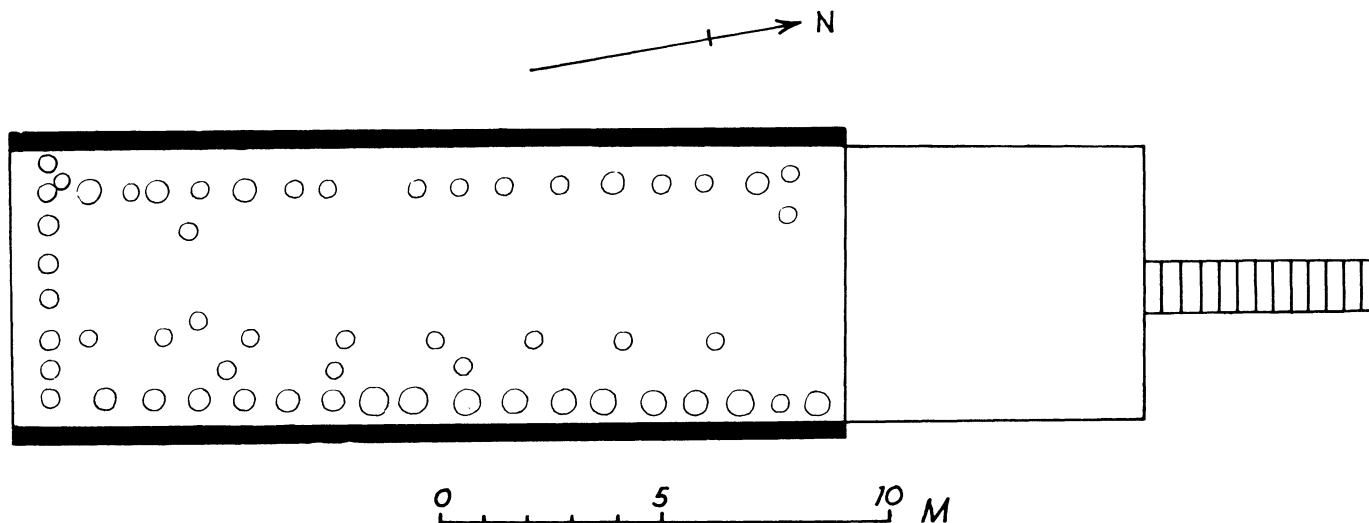


FIG. 8. Plan of tomb 653 H.4 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 14 (1951), pl. 12. (Redrawn by Marian Cox)

Dynasty. Tomb 653 H.4 probably had a burial chamber lined with limestone, for slabs were found on the floor.²³ Saad presumably dated the tomb to the First Dynasty on the basis of pottery sherds and fragments of stone vases. The substructure (fig. 8), however, suggests a late Second or early Third Dynasty date. The elongated plan, the entrance bay, and traces of wooden magazines and a stone-lined burial chamber recall tomb V of King Khasekemui at Abydos.²⁴ Tomb V differs principally in its irregularity, occasioned by the flow of mud-bricks, and in its sunken burial chamber nearly 2 m below the level of the magazines. No features in any of these briefly mentioned tombs at Helwan suggest noteworthy technical progress in stone construction.

It is the rarity of Archaic stone tombs at Helwan, not their construction or development, that is remarkable. Among more than 10,000 tombs Saad found less than a dozen of stone—that is, about 0.1 per cent. This statistic is his most important discovery, for it must be seen in the context of a concern for security that resulted in deeper substructures and stairways blocked with stone. The large and well-made limestone portcullises amount to an endorsement of stone as the most effective material for protection, and yet the use of stone was minimal. The motivation and technical ability for the construction of tombs built entirely of stone existed in the First Dynasty, but the development did not take place until the end of the Archaic period or the beginning of the Old Kingdom.

Even after allowing for lost monuments—and an astonishing number have survived—nothing in Archaic tomb architecture is adequate preparation for the technical mastery that suddenly appears in the heart of the Third Dynasty Step Pyramid complex at Saqqâra. The complex contains two granite burial chambers

²³ Saad, *Supplément*, Cahier 14, 19.

²⁴ Petrie, *Royal Tombs*, II, 12–15.

and a granite plug weighing approximately 3 tons that was used to seal the principal chamber under the Step Pyramid. The storeroom that housed the plug was probably limestone, but it must have had an extraordinarily strong roof to support the rubble in the shaft above it—a boldness of design that bespeaks confidence.

We have tantalizing hints of the technical development that preceded the Step Pyramid—the granite reliefs and other architectural fragments from Hierakonpolis and el-Kab, and the record on the Palermo Stone of a temple of stone built during the reign of Khasekemui.²⁵ Although it is meagre, the evidence suggests that stone construction developed more rapidly in Archaic temples than in tombs. Since both temples and tombs are sacred, inherently conservative architecture, why would a change in material progress more rapidly in temples? I have elsewhere suggested that alabaster, greywacke, and sycamore had symbolic value for sculpture during the reign of Mycerinus in the Fourth Dynasty.²⁶ It is probable that architectural materials, too, sometimes had symbolic associations by virtue of their geographical sources or natural properties. The inundation deposited the alluvium from which bricks were made. The Egyptians might have regarded bricks as manifestations of divine beneficence and as assurance of regeneration and eternal life. If mud-brick was sacred in the context of tombs, a lag in the use of stone would be understandable. The sanctity of the material would also account for the fact that limestone is treated almost as if it were mud-brick at the Step Pyramid complex.

Behind Archaic tomb construction is a complex fabric of economic considerations, religious beliefs, and technical possibilities. Limestone was abundant, tractable, and reassuring to tomb owners who wanted both durability and security. In view of all the factors in favour of using limestone for tombs, religion was probably the only force powerful enough to retard development. De Cenival commented on the transformation of monumental religious architecture in the Fourth Dynasty, and on the inextricable relationship between architectural development and religion: ‘This represents a basic change of aesthetic which cannot be explained merely by a modified approach to the question of forms and their adaptation to a new material . . . fundamental changes in theology and the royal cult must have intervened.’²⁷ Stone seems to have been adopted for tombs reluctantly during the Archaic period, which makes the achievements of the Pyramid Age all the more remarkable.

²⁵ J. E. Quibell, *Hierakonpolis* (London, 1900), 1, pls. ii and iii; Heinrich Schäfer, *Ein Bruchstück Altägyptischen Annalen* (Berlin, 1902), 26, Jahr x + 2.

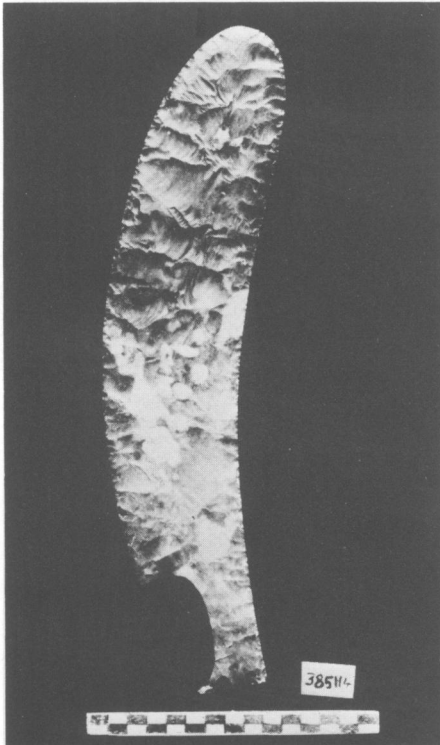
²⁶ *JEA* 60 (1974), 82–93.

²⁷ Jean-Louis de Cenival, *Living Architecture: Egyptian* (London, 1964), 175.

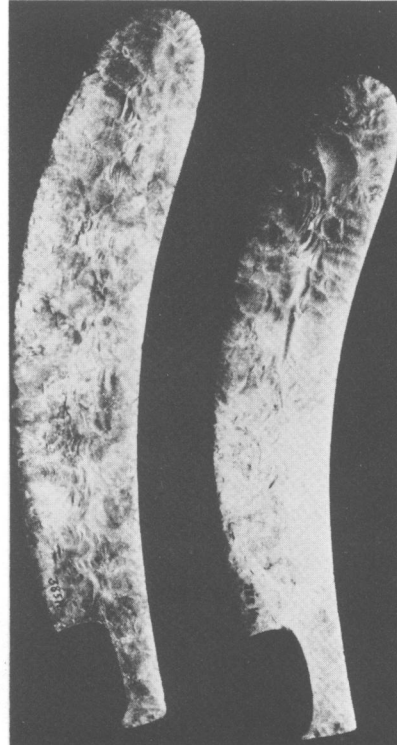


1. Building 250 (animal pens): the entrances to two adjacent pens showing the wooden cross poles over the thresholds, and changes in layout during a long period of use. View to the east

AMARNA WORKMEN'S VILLAGE (p. 37)



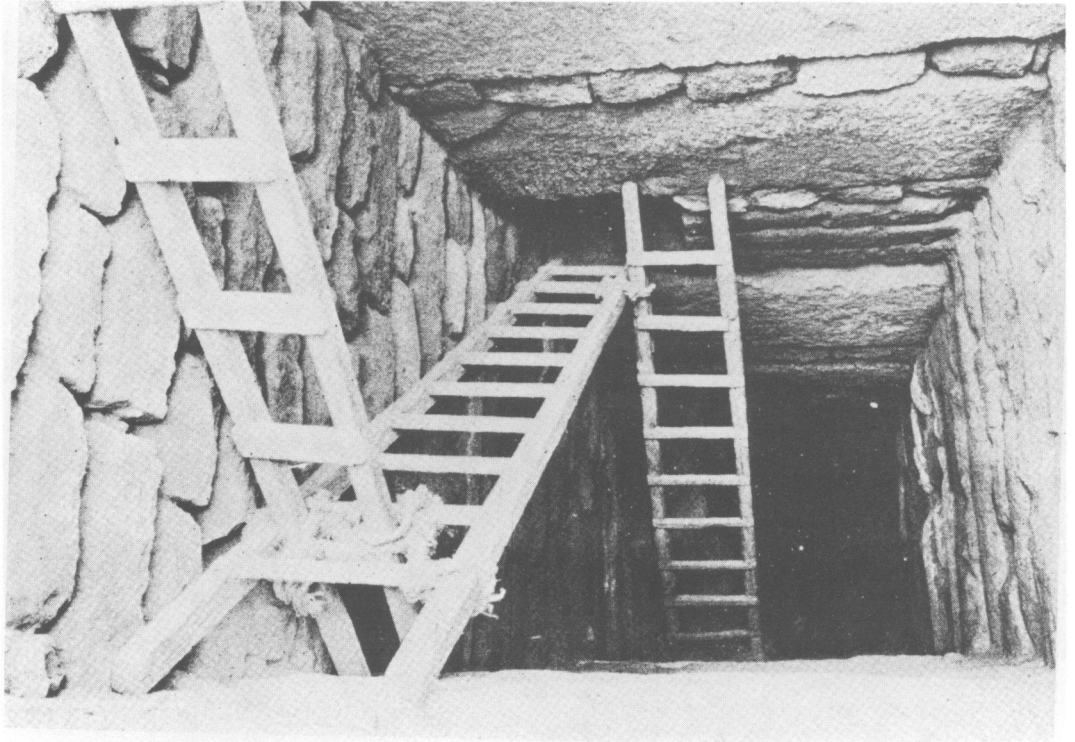
2



3

Three flint knives from tomb 385 H.4

THE ARCHAIC STONE TOMBS AT HELWAN (p. 66)



1

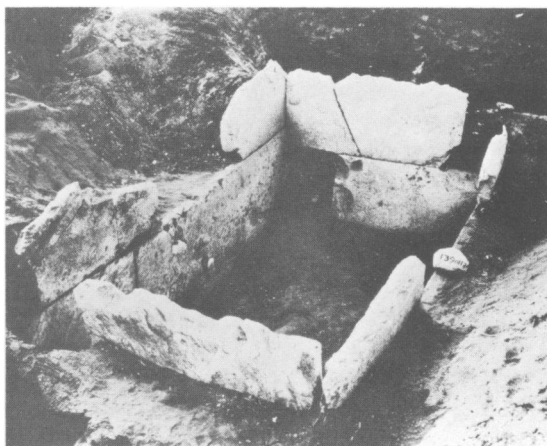


2

Interior of Tomb 287 H.6 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 14 (1951), pl. ii a-b

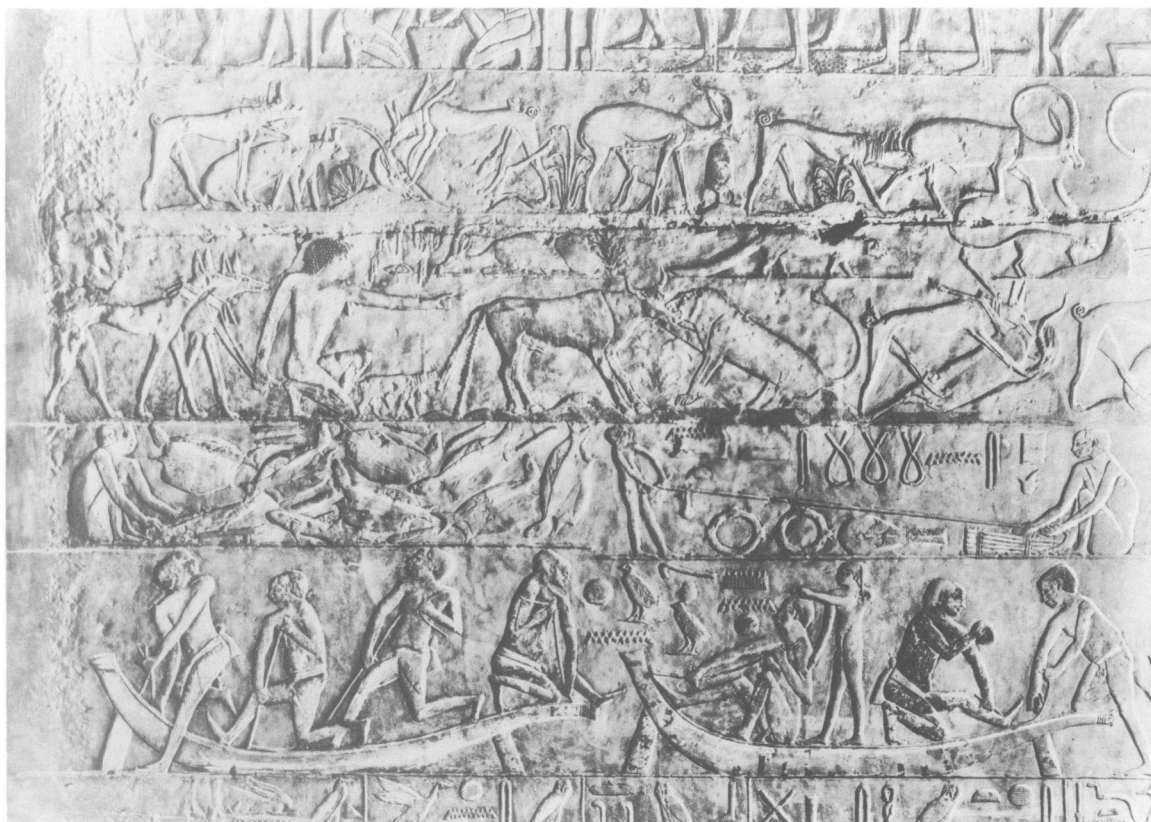


1. Interior of tomb 40 H.3 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3 (1947), pl. lxxix



2. Interior of tomb '1390' (probably 1389) at Helwan from Saad, *The Excavations at Helwan*, p. 102, pl. 16.

THE ARCHAIC STONE TOMBS AT HELWAN (pp. 63-4)



3. Twisting ropes for boatbuilding, tomb of Ptahhotep and Akhethotep

TECHNIQUES AND TERMINOLOGY OF ROPE-MAKING (pp. 71-7)

TECHNIQUES AND TERMINOLOGY OF ROPE-MAKING IN ANCIENT EGYPT

By EMILY TEETER

Eleven scenes are examined for information concerning the techniques and terminology of making rope. The definition of certain technical terms is refined from the earlier work of Junker and Vandier, and several scenes are added to Vandier's corpus of rope-making scenes.

DURING most periods of pharaonic Egypt, it was customary for non-royal tombs to be decorated with scenes of daily life and industry. These are invaluable for our reconstruction of the techniques employed by the ancient craftsmen. Among the many industries depicted is the manufacture of rope, which is recorded in nine different tombs and on one loose block in the Cairo Museum.¹ With the exception of one representation in the tomb of Kaemnofret, all scenes show a remarkable uniformity, which allows a reconstruction of the methods used, and a clarification of certain technical terms used by the ancient craftsmen.

Rope-making techniques

Virtually all examples of rope from ancient Egypt were made employing one basic technique which involves twisting and counter-twisting fibres. In the first step, yarns or cords are created of bundles of individual fibres, each yarn being twisted in the same direction. The yarns are then twisted around each other in the reverse direction. The laying of the yarns' fibres upon each other in the opposite direction creates a tension that keeps them together in the final rope shape. A very few examples are made with a more complicated process which involves plaiting.² The most common type of ancient rope, like its modern counterpart, is made up of multiple yarns without a core section. Great variety is attested in diameters and lengths,³ and in fibres. Recent botanical studies of the samples of rope in the

¹ *Kahif*: H. Junker, *Giza*, VI (Vienna, 1943), 68-9, fig. 43, pls. xiva, xxiiB. *Nyankhnesut*: in the Nelson-Atkins Museum of Art, Kansas City. PM III², 695 (the correct accession number is 30-14). *Nefer*: Junker, op. cit., fig. 17. *Akhtihotep*: Mastaba du Louvre: J. Vandier, *Manuel d'archéologie égyptienne*, v² (Paris, 1969), pl. xxix, fig. 197. Ptahhotep: N. Davies, *The tomb of Ptahhetep and Akhetetep*, I (London, 1900), pls. xxi, xxv. *Rekhmire*: N. Davies, *The Tomb of Rekh-Mi-Re at Thebes* (New York, 1943), pl. lii. *Khaemwaset*: E. Mackay, *JEA* 3 (1916), 125; L. Klebs, *Die Reliefs und Malereien des neuen Reiches* (Heidelberg, 1934), fig. 118. The tomb is erroneously referred to as TT 260 in *LÄ* v, 828. *Kaemnofret* (2 scenes): D. Dunham, *AJA* 39 (1935), 300-9. *Iymery*: *L D Ergänzungsband* (Berlin, 1913), pl. vi. *CG 1697*: Vandier, op. cit., pl. xxx, fig. 209. This block is very badly damaged, but traces of an inscription may be made out, most probably *nct smc*. PM III², 294 (1) cites a cordage scene from the tomb of Khuenre. The photographs at the Boston Museum of Fine Arts show the block to be so badly damaged that the rope scene cannot be made out.

I would like to thank the Committee of the Egypt Exploration Society for their kind permission to reproduce the rope-making scenes from the tombs of Khaemwaset and Ptahhotep (pl. VIII, 2; VII, 3).

² Cairo JE 56282B (Eleventh Dynasty, from Thebes) is one of the rare examples of plaited leather rope.

³ See, for example, J. Janssen, *Commodity Prices from the Ramessid Period* (Leiden, 1975), 439, for references

collection of the British Museum have identified dom palm, halfa, and papyrus. Other studies cite camel hair, flax, and leather rope.⁴

Each of the steps involved in the manufacture of rope can be documented in the ancient Egyptian reliefs.⁵ The variety of materials employed is reflected in scenes depicting the first step, the collection of fibres. The representations in the tombs of Ptahhotep, Khaemwaset, Akhtihotep, Iymery, and Nyankhnesut, as well as the scenes on Cairo CG 1697, show rope-workers near swamps, and presumably gathering papyrus. In the tombs of Kahif and Nefer, agricultural settings suggest a different type of fibre. One example, from the tomb of Rekhmire, shows the manufacture of rope adjacent to leather-workers and it has been suggested that this scene shows the manufacture of leather cordage.⁶

The next stage in making rope, the sorting of the raw materials, is recorded in the tomb of Kaemnofret, where two men sit facing each other, placing the fibres in a bundle (pl. VIII, 1). The result is shown in the tombs of Ptahhotep (pl. VII, 3) and Khaemwaset (pl. VIII, 2), where a neat stack of fibres awaits the ropemakers' attention. A block from the Cairo Museum shows a similar sorting scene in conjunction with basketry work.⁷

The actual twisting of the rope fibres is not shown with the same clarity as are the scenes of picking and sorting, since most of the representations combine the two twisting procedures. In almost all examples, two workers are involved, and in the majority of cases (Kahif, Akhtihotep, Kaemnofret, Iymery, Khaemwaset, and Ptahhotep), one is a young boy. The clearest representation of rope-makers and their tools is in the tomb of Khaemwaset (pl. VIII, 2). There, three men are engaged in the two separate twisting processes. The man to the right produces the initial twist in the yarns by manipulating a tool composed of a handle with a short lanyard, to which a round weight is attached.⁸ The rope fibres were apparently attached to the lanyard between the handle and the weight while the opposite end of the yarns was anchored in place by another workman. As the handle was turned in a tight circular motion, the weight swung around the handle itself, imparting a twist to the yarns. The tool is usually shown hanging at the upper end of the yarns away from the worker who feeds

to rope in lengths of 1,400 cubits, and idem, *Two Ancient Egyptian Ship's Logs* (Leiden, 1961), 87, where ropes of 1,000 cubits (523 m) and 500 cubits (261.5 m) are mentioned. For reference to very thick rope, see that recovered from Tura (2.5 inches in diameter), in A. Lucas, *Ancient Egyptian Materials and Industries* (London, 1948), 160-1, and the five-strand rope recovered from the Khufu ship in M. Zaki *et al.*, *The Cheops Boat* (Cairo, 1960), pl. xxxviii.

⁴ D. Ryan and D. Hansen, *A Study of Ancient Egyptian Cordage from the British Museum* (in press). I am indebted to Mr Ryan for his considerable work in locating scenes of rope-making. Also see Lucas, *op. cit.* 160-1 and C. Singer *et al.*, *A History of Technology*, 1 (Oxford, 1954), 451-5.

⁵ Also see Vandier, *Manuel*, v, 479-82.

⁶ Davies, *op. cit.*, pl. lii; Singer *et al.*, *op. cit.* 453; R. Drenkhahn, *Die Handwerker und ihre Tätigkeiten im Alten Ägypten* (Wiesbaden, 1976), 13 no. 28. However, the juxtaposition of scenes does not necessarily prove that the rope is made of leather.

⁷ Vandier, *Manuel*, v², pl. xxix, fig. 207.

⁸ Some confusion concerning the exact description of the tool stems from Mackay's ethnoarchaeological reconstruction. The fellahin that he studied used a tool composed of two pieces of wood, one socketed into the other on a swivelling joint. The tool was roughly L-shaped, and the asymmetrical shape of the tool itself provided the centrifugal motion that the ancient Egyptians achieved through the use of a stone weight.

them into the final twisting process. In the tomb of Khaemwaset (pl. VIII, 2), it is shown alongside other rope-making tools, and has the shape of a paddle. In the tomb of Rekhmire, the binding which holds the weight on to the lanyard is clearly shown. To my knowledge, no examples of this tool have been recovered. Considering the materials—a stick, rock, and binding—it would not be surprising if it had been overlooked in the publication and exhibition of small finds, or even not recognized by excavators.

The final step is the twisting of the yarns against each other. As with the initial twisting, two workers face each other, one anchoring the end of the yarn bundle while the other turns the yarns in the opposite direction. The scene from the tomb of Ptahhotep (pl. VII, 3) may show this second process. There a worker holds a straight stick to which the yarns are attached, and which probably provided some mechanical advantage and a more secure grip on the yarns. The scene in the tomb of Khaemwaset (pl. VIII, 2), which may also record the twisting of the yarns into rope, shows a man, seated between two workers, regulating the tension of the final twist. According to Singer and Mackay, this was done by inserting a marlinspike between the yarns at the point where the fibres encountered the reverse twisting motion.⁹ The scene from the tomb of Rekhmire shows a variation in how the final twist was put in the cord.¹⁰ There, the man who imparts the final counter-directional twist maintains the turn in the yarns by keeping them under tension. This is done by attaching the yarns to a bridle, which is looped around the standing worker's hips. Thus the yarns were stretched under tension between himself and his co-worker, leaving his hands free to rotate the rope-making tool. However, the representation in this tomb is not entirely clear. It appears as if the yarns are attached to the handle of the tool itself rather than to an attachment point between the handle and the weight. Yet the yarns cannot be attached to the handle, for the circular motion would then be transferred to the bridle as well, twisting it and gradually tightening its loop around the man's waist.

Junker has suggested that the scenes from the tomb of Kaemnofret and Kahif do not show the production of cordage, but the manufacture of lighter twine for tying sheaves.¹¹ I do not believe that such a delicate differentiation can be made on the basis of these scenes. The technique shown for Junker's twine scenes also appears alongside boat-building scenes, where one can assume the finished product was stouter rope.¹² The presence of coils of finished rope shown above the supposed twine-manufacturing scenes also speaks against the assumption that short lengths of cord, suitable only for tying sheaves, are being produced. The short pieces of wavy line shown underneath the workers in the tombs of Rekhmire and Kahif might be representations of fibres which have yet to be worked.

A very curious scene related to the manufacture of cordage appears in the tomb of Kaemnofret. There, a man whose outspread legs are represented frontally holds a

⁹ Singer *et al.*, *op. cit.* 454; Mackay, *op. cit.* 125–6.

¹⁰ Davies, *op. cit.*, pl. lii.

¹¹ Junker, *op. cit.* 136–7.

¹² See, for example, the scenes from Nyankhnesut and Ptahhotep, n. 1 above.

piece of fibre taut over his right shoulder¹³ (pl. IX). The end of the fibre is threaded between the toes of his left foot, and a trail of fibre may be seen passing behind his foot, visible in the rise under his knee. Vandier has suggested that this is an unusual cordage scene where one man alone produces rope.¹⁴ It is difficult to classify this representation with others which show the manufacture of rope, because none of the usual features is present, and because a standard cordage scene appears in the same tomb-chapel, suggesting that a distinction is being made between two different activities.¹⁵ Furthermore, I doubt if the single man in the scene from Kaemnofret could manipulate a twist into fibre by himself, particularly in such a contorted position. His arm and hand positions provide a clue to his activity. His hands are clasped around the fibre, his arms extended over his bent right knee. I suspect that this scene portrays the measuring of a standard length of twine or rope, measured perhaps from the toe to the shoulder, much in the manner of the British and American 'yard' measurement. Another, less likely, explanation is that the man is engaged in the twisting and production of light twine without the help of tools.¹⁶ This twine would then be passed to his companions who tie the sheaves. The inscription is so distinctly different from those which narrate the rope-making scenes (see below), that I strongly suspect the scene shows a different activity.

A similar scene appears in the mastaba of Hetepherakhty, now in Leiden.¹⁷ There the ropeworker sits with his left leg extended, the other bent sharply at the knee. A short coil of rope is seen under his left foot, leading up over his outstretched left hand, down to his right hand which is positioned at the back of his right ankle. The cord terminates in a block-shape space which may be the workman's low seat or an abstract rendering of a stack of rope (?). The cord above the right ankle is scored with oblique lines which may be an indication of a twist in the fibre. This scene, unlike that of Kaemnofret, may indeed depict a single man making light twine, here imparting the twist to the fibres by rolling them against the back of his ankle.

Types and date of scenes

The production of cordage is most commonly shown adjacent to scenes of boat-building or in conjunction with swamps, presumably in reference to the source of the fibre (Ptahhotep, Khaemwaset, Akhtihotep, Iymery, Nyankhnesut, and Cairo CG 1697). It is also found with scenes of agricultural activity or adjacent to fields

¹³ W. Wreszinski, *Atlas zur altägyptischen Kulturgeschichte*, III, pl. 55 (top); Vandier, *Manuel*, VI, pl. viii (top).

¹⁴ Vandier, *Manuel*, VI, 76. A scene in the tomb of Akhtihotep depicts a man in a similar position, although portrayed in a more conventional perspective. He, too, uses his toes, but apparently more in the manner of the third man in the scene of Khaemwaset who regulates the feeding of the yarns into the final twisting process.

¹⁵ The conventional rope-making scene occurs on the north wall of the tomb-chapel (PM III², 467 (7)), while the anomalous scene appears on the east wall, south of the entrance door (loc. 3).

¹⁶ While in Egypt in 1985, I observed lightweight rope for donkey hobbles being produced by a single worker, but his stance (sitting with his feet together and his hands near his feet) was far different from that shown in the tomb of Kaemnofret. The rope was made of two yarns of palm fibre.

¹⁷ This scene was kindly brought to my attention by Ann Macy Roth. The scene occurs in sequence with (left to right) cleaning fish, flattening reeds, man sitting with a pile of reeds (?) between his knees, beating mats and the cordage scene.

where the cord was employed to bind sheaves (Kahif and Nefer). Less common contexts include manufacture in a workshop alongside other specialized crafts, such as leather-working (Rekhmire). As most of these scenes appear alongside agricultural or boat-building scenes, one might assume that rope-making was considered to be more of a practical function than one of craft or art status. This is evident also in the absence of rope-making scenes among the crafts shown in certain Eighteenth and Nineteenth Dynasty tombs.¹⁸

The great majority of these scenes date to the Old Kingdom. It is curious that it is not shown in the tombs of Beni Hasan, where so many other related industries are depicted.¹⁹ In view of the number of New Kingdom tombs that depict agricultural and craft themes, one might expect to find more than the two scenes (Rekhmire and Khaemwaset) which date to that period.

The inscriptions

Of the eleven scenes showing men making rope, only six are accompanied by captions which describe the processes involved. The following phrases occur:

(a) *sš smc* 'preparing (sorting) the fibres'. This phrase is used to narrate the first stage, shown in the tomb of Kaemnofret (pl. VIII, 1). The word *sš* is known from the Turin Papyrus, where it is used in the context of preparing a bier or bed.²⁰

(b) *nc̄t smc* 'twisting the fibres'.²¹ All inscribed rope-making scenes, with the exception of the above-mentioned scene from Kaemnofret, are narrated by the caption *nc̄t smc*. The phrase is also found in Coffin Text Spells 189G and 195N and P in reference to shipbuilding.²²

The word *smc* , not listed in the *Wörterbuch*, is consistently written with the flesh determinative (𓂏), while in the Coffin Texts it shows the variations 𓂏, 𓂏, and 𓂏. In spite of the variety of determinatives, the word appears in the same context throughout and is consistently paired with *nc̄(t)*. Dunham defined *smc* as 'a technical term for the rind of the papyrus stem used in rope-making',²³ whereas Vandier preferred the more general 'fibres'.²⁴ The contexts in which the word appears leave little question that *smc* is the fibre that is tensioned and twisted into rope. I would,

¹⁸ Note the absence of rope-making scenes in Theban tombs known for their representation of crafts: TT 39 Puimre, TT 49 Neferhotep, TT 66 Hapu, TT 79 Menkheperresoneb, and TT 181 Nebamun.

¹⁹ P. Newberry, *The Tombs of Beni Hasan*, II (London, 1893), pls. iv, xiii. The tombs of Baket (15) and Khety (17) show a full spectrum of other industries, including weaving, dyeing fabric, folding cloth, and the manufacture of mats.

²⁰ W. Pleyte and W. Rossi, *Papyrus de Turin*, II (Leiden, 1869-76), pl. 98.11.5, where it appears as . The geminated form that is displayed in the rope-making scene helps clarify the writing in the Turin Papyrus. A. Gardiner, *JEA* 31 (1945), 25 no. 5, suggested the reading *sš hnkyt*, while F. Schmitz, *Amenophis I* (Hildesheim, 1978), 26-7, more recently proposed that the third radical of *sš* is actually the first radical of the word *šs* 'bier' (*Wb.* IV, 543.2). The reduplicated form shown in Kaemnofret supports Gardiner's reading. The verb would be classed as *secundae geminatae*, in these examples to be rendered 'preparing the yarns' and 'preparing a bier'.


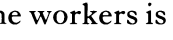
²¹ Kaemnofret, Cairo CG 1697, Nyankhnesut, Akhtihotep, and Kahif; cf. n. 1 above.


²² A. de Buck, *The Egyptian Coffin Texts*, III (Chicago, 1947), 97, 113.

²³ Dunham, op. cit. 304, no. 1.

²⁴ Vandier, *Manuel*, VI, 79.

however, be wary of assigning Dunham's very specific definition to the material of *sm^c*, for, as the scenes indicate, rope-making may not always take place near a source for papyrus. Considering the variety of fibres used and the diversity of locations where the manufacture of rope and the word *sm^c* is shown, it is safe to suggest that *sm^c* is a generic term for fibres which are to be twisted into rope, without reference to the particular material.

The verb  *n^c* is attested from the Old Kingdom on and has been defined as 'to twist (rope)' (*Wb.* II, 207.1). In the cordage scenes, it takes the direct object *sm^c*. A variation occurs in the tomb of Ptahhotep and Akhethotep (pl. VII, 3), where a child and adult stretch fibre between them as a tool hangs from the uppermost end of the fibre. Between the workers is the caption  *n^ct šszw n sp*, 'twisting the ropes for boat-building'. Here the context is identical to the *n^ct sm^c* scenes, but the more specific *šszw n sp* is used. Underneath this register is a scene of craftsmen using the finished rope to bind reed ships. An exceptional use of *n^ct* appears in a boating scene in the tomb of Merhotep at Sakkara. There a boatman advances the *smh*-boat by pulling the ship's tow-rope.²⁵ In this context, the term has very much the same connotation of 'tensioning' as in the cordage scenes, although the idea of twisting is absent. All three uses of *n^ct* refer to tensioning rope fibre. Since the great majority of the uses of the terms specifically refer to the stretching of rope fibres between two persons in order to impart a twist, the primary definition of *n^ct sm^c* must be 'to tension and twist rope fibres'. In Coffin Text Spell 189 and 195, *n^c(t) sm^c* describes one of the stages in boat-building: *š^r dyt:f* (its fibres are cut); *n^ct sm^c:f* (its ropes are twisted); *sp smh:f* (its hull is bound).²⁶

(c) *szš sš*  'drawing the fibre'. This inscription appears only in the unusual scene in the tomb of Kaemnofret (pl. IX). The difficulty in the scene's interpretation stems from the unusual verb *szš*. Vandier, assuming that this scene depicted the manufacture of cordage, assigned the meaning 'to bind, plait, or weave' ('tresser'),²⁷ apparently regarding the otherwise unattested *szš* as synonymous with *n^ct*. Vandier's definition assumes, firstly, that rope is being produced, and, secondly, that rope was plaited, which is not the case in the vast majority of examples. Junker suggested the meaning 'to draw' and relates the following word, *sš(w)*, determined with a coil of rope, to *szšt* (*Wb.* IV, 76.4 'Stricke'), giving the sense of 'drawing the twine'.²⁸ A comparison of this scene with other rope-making scenes favours his suggestion, if his definition is understood as simply drawing or pulling. I suspect that this is the true difference between the terms *szš* and *n^ct*; only the latter involves twisting under tension to form yarns or finished rope. The accompanying inscription may be translated *dm: mh* 'binding the flax'; *szš sš dm: mh* 'drawing the cord in order to bind the flax'.

²⁵ L. Borchardt, *Denkmäler des alten Reichs*, II (Cairo, 1964), 18, pl. 61.

²⁶ De Buck, *CT* III, 97g. Also see de Buck, *CT* VI, 268 for an unintelligible passage which Faulkner emends to: *in wrt i n^ct n^ct . . .* 'it is my *wrt* crown which twists the *n^ct*-snake' . . . *The Ancient Egyptian Coffin Texts*, II (Warminster, 1977), 223, no. 8.

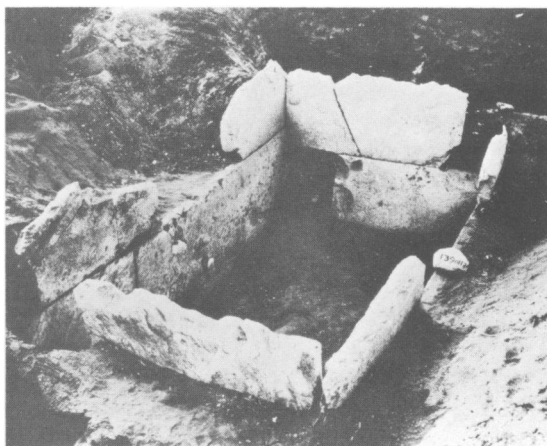
²⁷ Vandier, *Manuel*, VI, 79.

²⁸ Junker, *op. cit.* 137.

An examination of rope-making scenes as preserved in the Egyptian pictorial record provides an intriguing look into ancient technology. Although such scenes are not numerous, they contain much valuable information. Not only do the representations help us reconstruct the technique used to manufacture rope, but the captions, through their specific nature and consistency, enrich our knowledge of one aspect of ancient technical terminology.

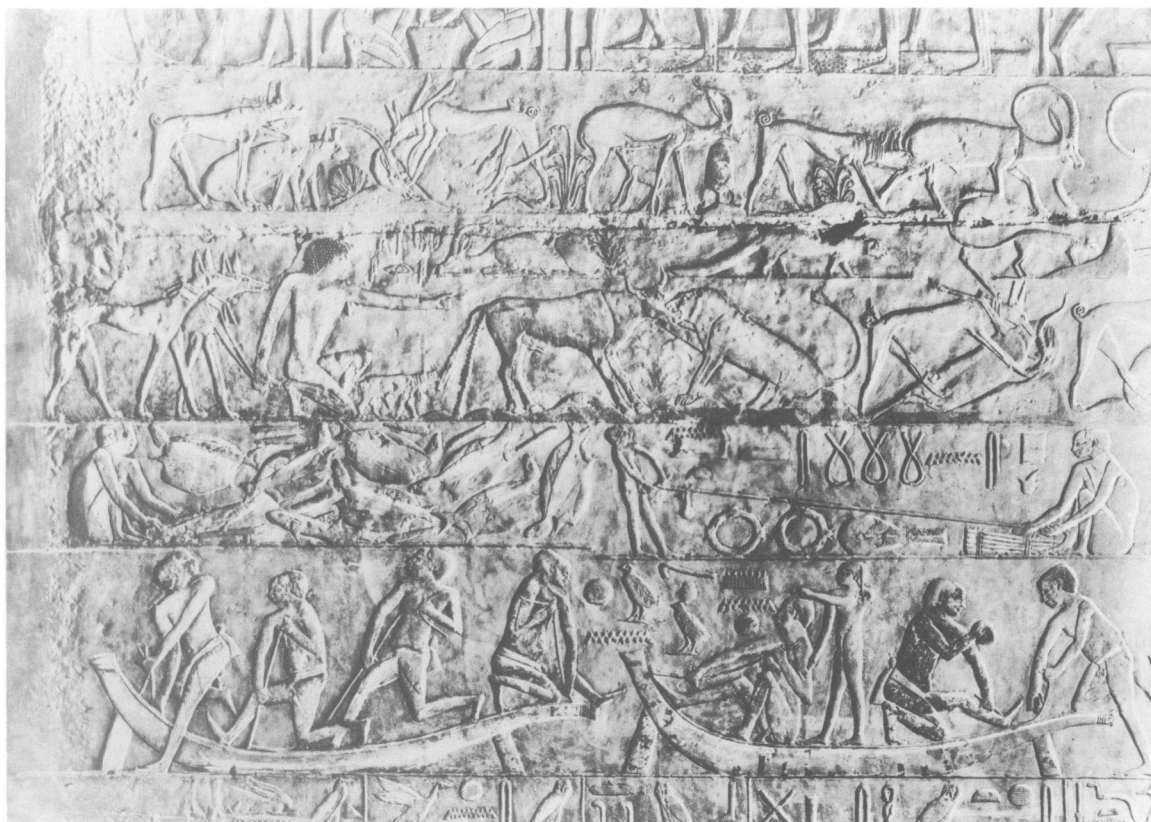


1. Interior of tomb 40 H.3 at Helwan, from Saad, *Supplément aux ASAE*, Cahier 3 (1947), pl. lxxix



2. Interior of tomb '1390' (probably 1389) at Helwan from Saad, *The Excavations at Helwan*, p. 102, pl. 16.

THE ARCHAIC STONE TOMBS AT HELWAN (pp. 63-4)

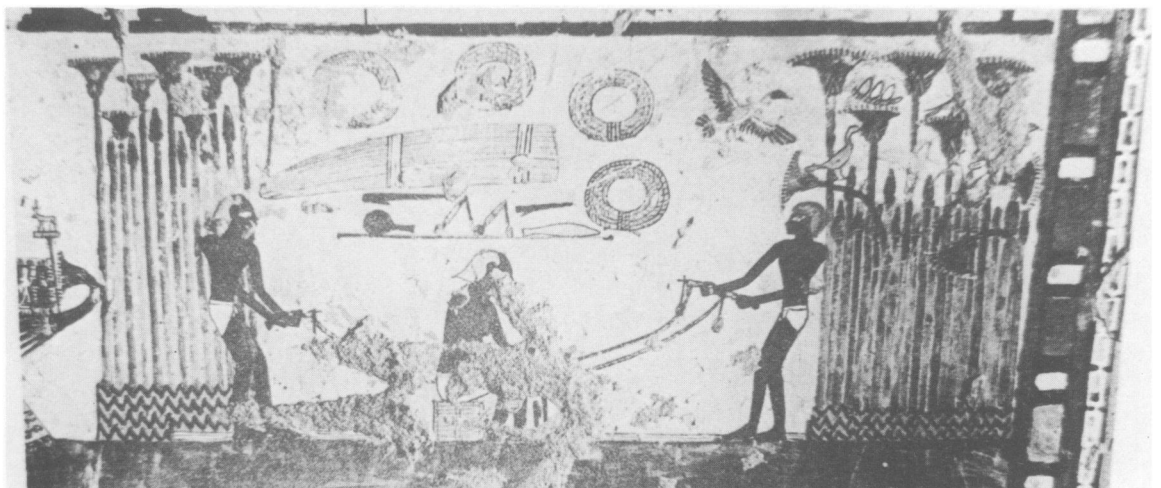


3. Twisting ropes for boatbuilding, tomb of Ptahhotep and Akhethotep

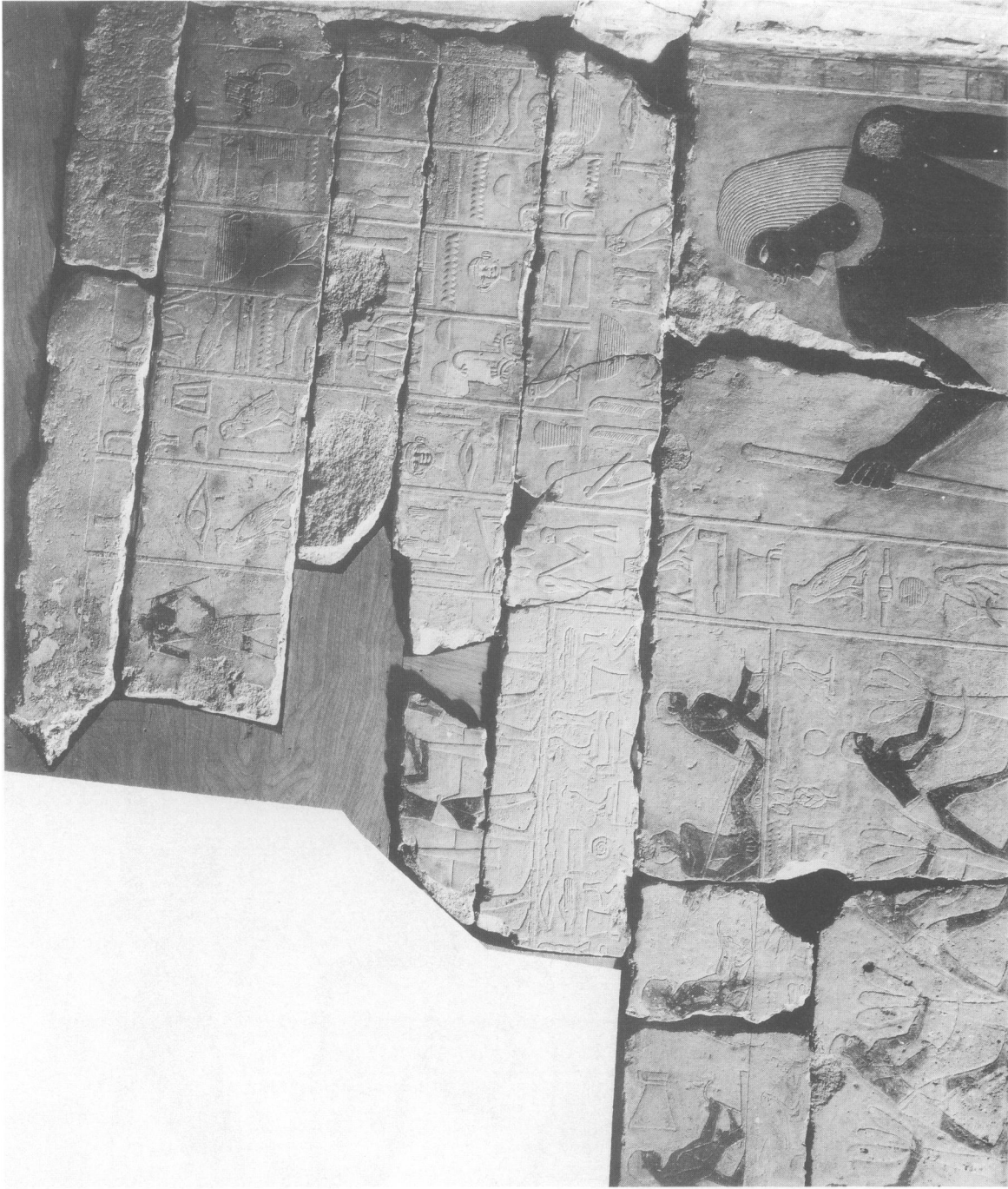
TECHNIQUES AND TERMINOLOGY OF ROPE-MAKING (pp. 71-7)



1. Sorting and twisting rope fibres, tomb of Kaemnofret (*photo courtesy of Boston M.F.A.*)



2. Rope-making tools, tomb of Khaemwaset



Drawing rope fibres, tomb of Kaemnofret (*photo courtesy of Boston M.F.A.*)

TECHNIQUES AND TERMINOLOGY OF ROPE-MAKING (pp. 71-7)

PRACTICAL RELIGION AND PIETY¹

By JOHN BAINES

Official religion is presented as centring on royal-divine relations; decorum excludes human non-funerary religious concerns. For want of evidence, pre-New Kingdom personal religion must therefore be approached through constructing hypotheses rather than accumulating evidence. A biographical model suggests that practical religion—religious action in an everyday context—may focus on affliction, to which responses include communication with the dead—letters to the dead among the literate—and perhaps divination through oracles and consulting seers. These approaches may precede further, unknown actions. The use of intermediaries to deities and the deification of non-royal individuals does not certainly extend beyond the élite. Piety—personal relations with deities—is most clearly attested in personal names, while the élite display of personal religious involvement implies some general aspiration to divine contact. Later Egyptian society, in which practical religion and piety are more visible and integrated, had different rules of decorum and perhaps a different organization, in which values and religious action were less local in their focus.

Introduction: the élite context

EGYPTIAN religion before the New Kingdom is poorly known in all but its funerary aspects. In this paper, I present hypotheses on the character and spread of non-funerary religious practice in earlier periods. My chief purpose is to suggest ways of filling some gaps in the pre-New Kingdom record from the perspectives of practical religion—religious action in an everyday context—and piety—here loosely defined as relationships between individuals and deities. My suggestions have the character of designs for research, but some designs may be feasible and others not. Even if a particular line of enquiry cannot be pursued, hypotheses in that area may still be worthwhile. Any attempt to round out understanding involves interpretative assumptions: hypotheses provide the context for detailed research. Less abstractly put, intuitive ideas about the general shape of religion in society are part of any approach and are best formulated explicitly. It is worth considering the general shape of religion because the alternative of taking the material we have as being truly representative, particularly in its statistical distribution, is implausible. Odd hints of religious practice may help to illuminate gaps in knowledge and to formulate more general models of the context into which such evidence can be fitted.

This strategy implies a looser, more complex picture of religious practice than has

¹ Revised version of a paper delivered at the Fourth International Congress of Egyptology, Munich, August 1985, and at the University of Copenhagen and American University Cairo. I am grateful to Christopher Eyre, Geraldine Pinch, and John Tait for criticisms of drafts, to Paul Frandsen and David Silverman for corrections suggested during the discussion in Munich, and to Edward Brovarski and Jan Quaegebeur for information. Geraldine Pinch's forthcoming book on New Kingdom popular religion has influenced my thinking on the general character of earlier religion. The published abstract of this paper (International Association of Egyptologists, Fourth International Congress of Egyptology, Munich 1985, *Abstracts of Papers* (ed. S. Schoske), 4-7) is a short version of the whole; not all the points covered in it are repeated here.

For a valuable summary of several of the issues discussed in this paper see H. Brunner, *LÄ* 11, 479-83. Brunner emphasizes usefully the extent to which the Egyptians viewed the cosmos and the individual's fortune as being under threat and uncertainty.

been the norm, and a broad, non-evaluative conception of what counts as religion. Religion need not be seen as a unitary, well-defined phenomenon. In considering these possibilities, I use as a point of departure a theoretical study in which I considered religion as a mode of discourse whose unified application proliferated greatly in the late New Kingdom and Late Period, in a process I termed 'sacralization'.² Here, I explore for earlier periods other forms of action that are considered 'religious' in the study of most societies, but are not so centripetally organized and have tended to be minimized in Egyptology. My approach contrasts, for example, with those of Morenz³ or Assmann,⁴ who see religion as completely pervading early Egyptian civilization and, it seems, as being ultimately unitary, so that the attested élite forms would be a reliable guide for the society as a whole. If, however, religion need not be a single, homogeneous mode of discourse, there is no reason, apart from some overarching theory, for insisting that a single form of it pervaded all of life from the beginning of history; diversity and sacralization become real possibilities. In this context, the 'practical religion' I discuss could be termed more loosely and explicitly 'non-material modes of action and response'.

Early religion is, I suggest, less well known, and probably less knowable, than is often assumed. Its complexity and diversity may be related to the complex society of Egypt, but there is no easy equation between society and religion, and no society is truly 'simple'. A stratified, politically centralized society is, however, unlikely to be equivalent in its religious beliefs and practices to a homogeneous, local, small-scale society. But it remains worth asking whether elements or traces in religion find their best analogies in such small-scale societies, a possibility that is relevant to much of my discussion. First, however, the model and evidence to be considered should be set in the centralized institutional context.

Old and Middle Kingdom monuments, especially the former, focus on the king, whose mortuary complex is altogether larger than divine cult temples. Although temples have been more subject to destruction than pyramids, the record is probably not completely misleading here. The king dominates human society and appears to stand between mankind and the gods—but this appearance may be in part an illusion created by centripetal sources. The focus of society appears political and institutional as much as religious, in part because the king's role as sole protagonist of the cult pre-empts the religious burden. Religious work is delegated to priests (the king's own cult responsibilities were probably heavy), but most priests are themselves at most part-time officiants. Among the élite, their chief appointment is in the political and institutional sphere; the remainder are perhaps prominent members of local communities. The king is a general ideological focus as well as a religious one. The most pregnant formulation of this point is in the treatise on the king's role in the solar cult, a text probably dating to the Middle Kingdom, which states that the king is on earth 'for ever and ever, judging mankind and propitiating

² *GM* 76 (1984), 25–54.

³ *Ägyptische Religion* (Stuttgart, 1960) (= *Egyptian Religion*, tr. A. E. Keep (London, 1973)), 6–15.

⁴ *Ägypten: Theologie und Frömmigkeit einer frühen Hochkultur* (Stuttgart, 1984), 9–14.

(*štp*) the gods, and setting *mꜣt* in place of *ꜣꜣft*. He gives offerings (*ḥtpwt*) to the gods and mortuary offerings to the spirits (*pꜣt-ḥrw n ꜣḥw*).⁵ This gives a place to living humanity only in so far as the king 'judges' them. Parallels quoted by Assmann⁶ suggest that 'judging' here means creating justice and well-being among people, but the word also has repressive associations and does not depict the king as a primarily beneficent force. In addition, the text emphasizes aggressive action by the king in relation both to the potential aggression of the gods and to this world. For anyone but the king, it makes a bleak, centripetal statement that largely disregards the questions I ask, even if it does speak of justice and mentions the gods and the spirits.

Within the centripetal, politicized world of the élite, mortuary provision serves the prestige of the living to a great extent. If the dead were accepted in official ideology and among the élite in general as being a continuing force in the life of the living, one might expect their tombs to be better protected, so that they would be less universally and quickly robbed, and to contain more evidence for interaction between the living and the dead. In fact, the texts and representations in Old Kingdom tombs are very much concerned with the deceased's status in this life and much less with his aspirations for and activities in the next.⁷ An exception to this is in formulae where the recipient of offerings says that he will intercede in the hereafter on behalf of whoever pronounces an offering text—the texts only imply what the intercession might be for, conceivably judgement after death or acceptance in the next world; threats of action and litigation are also part of the formulae.⁸ But even if most of the dead did not retain much attention beyond, say, a generation after their demise (and that may be a liberal estimate), they are an essential part of Egyptian society, which consists of the four categories of the treatise quoted above: gods, king, and dead, as well as, far behind, mankind (the text omits further possible, less auspicious categories, as well as any form of being 'below' humanity, such as animals). A similar integration of the dead in the society of the living is well attested in other cultures;⁹ what is Egyptian is the inequality, to which the apparent lack of reciprocity between living and dead might be related.

Outside these ideologically central sources, there are multiple relations possible between human beings and the three main categories of the treatise. Human relations with the king are not my concern here, except if kings are deified. There remain human relations with the gods or the dead, either direct or mediated by the king, by other human beings, or by specialized modes of communication, of which

⁵ J. Assmann, *Der König als Sonnenpriester* (Glückstadt, 1970), 22; idem, *Sonnenhymnen in thebanischen Gräbern* (Mainz, 1983), 48–9; idem, *Ägypten: Theologie und Frömmigkeit*, 11.

⁶ *Der König als Sonnenpriester*, 58–65.

⁷ Cf. J. Spiegel, *Die Idee vom Totengericht in der ägyptischen Religion* (Glückstadt, 1935); J. Assmann, in A. Assmann et al. (eds.), *Schrift und Gedächtnis: Beiträge zur Archäologie der literarischen Kommunikation* (Munich, 1983), 71–9. For a more absolute and rather problematic formulation see F. Junge, in J. Assmann and G. Burkard (eds.), *5000 Jahre Ägypten. Genese und Permanenz der pharaonischen Kunst* (Nussloch/Heidelberg, 1983), 43–60.

⁸ See in general E. Edel, *MDAIK* 13 (1944), 1–90; for judgement see p. 17. See also R. J. Demarée, *The ḥꜣꜣ n Rꜣ-stelae: on Ancestor-Worship in Ancient Egypt* (Leiden, 1983), 204–12.

⁹ See I. Kopytoff, *Africa* 41 (1971), 129–42; his views have been much discussed, cf. C. J. Calhoun, *Man* NS 15 (1980), 304–19, with refs.; correspondence ending with M. Fortes, *Man* NS 16 (1981), 300–2.

later examples are dreams and oracles.¹⁰ Direct contact with deities is seldom the norm for a religion; where it occurs, it falls in part under the heading of piety, which I consider later. It is virtually absent from the Egyptian record—except where it is wished for or assumed to take place in the hereafter—but this exclusion may be partly a function of the system of decorum,¹¹ which specifies in hierarchical terms what may be depicted in what context, as well as probably affecting the content of many textual sources: a topic that is out of place or not of official concern is not a legitimate topic. Despite the bleakness and aggression in the formulation of the treatise, a vital additional feature of decorum is that what is negative is not presented (which could relate to the text's not being attested in normal public contexts¹²). The gods in their beneficence created the world and its riches and call forth rapturous acclamation from mankind¹³—sweetness and light dominate official sources. But much of religion in the world at large, and perhaps also in Egypt, is concerned with the negative and untoward; in Egypt this is visibly the case for private affairs in later periods and decorum probably masks its fundamental importance for many contexts, creating severe obstacles to an integrated view of society.¹⁴ Decorum and élite ideology go together here in ignoring suffering along with the rest of society. Because of all these factors, a better, or rather less optimistic, balance of beliefs and practices should be posited in relation to gaps in the record (see n. 21 below). Texts like the treatise and the declaration by the deceased in Book of the Dead 125 acknowledge that this applies to the gods too by terming the cult 'propitiation'.¹⁵

¹⁰ For dreams see P. Vernus, *LÄ* vi, 746–9. Dream books may date back to the Middle Kingdom, cf. A. H. Gardiner, *Hieratic Papyri in the British Museum Third Series: Chester Beatty Gift* (London, 1935), 9–11. This dating is disputed by S. I. Groll, in eadem (ed.), *Pharaonic Egypt, the Bible and Christianity* (Jerusalem, 1985), 71–118.

¹¹ For this concept see Baines, *Fecundity Figures: Egyptian Personification and the Iconography of a Genre* (Warminster and Chicago, 1985), 277–305. The term was devised for phenomena in iconography; for textual extensions see Baines, *JEA* 72 (1986), 41–53; idem, 'The Stela of Khusobek: Private and Royal Military Narrative and Values', in J. Osing and G. Dreyer (eds.), *Form und Mass: Festschrift für Gerhard Fecht* (Wiesbaden, 1987), 42–61.

¹² For discussion see J. Assmann, *Re und Amun: die Krise des polytheistischen Weltbilds im Ägypten der 18.–20. Dynastie* (Freiburg/Göttingen, 1983), 24–5 (his assumption that one copy of the text was deliberately made unintelligible is not necessary). Despite its ideological importance, the text is known from only four very restricted temple sources; it later achieved wider currency as a 'hymn' (which it is not). The same pattern of attestation applies to the hymns of the hourly solar ritual, which have a more violent, problematic character than more widely disseminated texts: *ibid.* 33–53.

¹³ Cf. E. Hornung, *Conceptions of God in Ancient Egypt: the One and the Many* (Ithaca and London, 1982/3), 196–205 (= *Der Eine und die Vielen* (Darmstadt, 1971), 192–200).

¹⁴ On the importance of the untoward see, for example, C. Geertz, in idem, *The Interpretation of Cultures* (New York, 1973), 87–125; P. L. Berger, *The Social Reality of Religion* (Harmondsworth, 1973) (= *The Sacred Canopy* (Garden City, NJ, 1967)), 61–87. On the central significance of misfortune to the analysis of societies see, for example, M. Douglas, *Evans-Pritchard* (Glasgow, 1980), esp. 12–13. In these terms, Egyptian ideology deliberately suppresses something that is crucial to society.

¹⁵ E. Naville, *Das aegyptische Todtenbuch der XVIII. bis XX. Dynastie*, II (Berlin, 1886), 316; see, for example, T. G. Allen, *The Book of the Dead or Coming Forth by Day* (Chicago, 1974), 99. If the arguments of Assmann on the dating of the treatise (*Der König als Sonnenpriester; Re und Amun*, 24–5), and Yoyotte on that of BD 125 (in *Le Jugement des morts* (Sources orientales 4, Paris, 1961), 58–65) are followed, the two texts will be of similar date, perhaps from the early Middle Kingdom. Like the treatise, BD 125 may document 'secret' élite knowledge of uncertain validity for society as a whole. For a hypothesis on its original context of use see R. Grieshammer, *ZDMG Suppl.* 2 (1974), 19–25.

Public official sources say little about supernatural beings other than deities. Such beings are known from the Pyramid Texts on and pervade the next world. For this world, they are amply attested in magical texts and are mostly malevolent—which is why they are excluded from official contexts. The margins of the cosmos are beset with ambivalent forms and beings, who may also appear within the ordered sphere at any time.¹⁶ Demons and marginal beings have a position in the hierarchical order of the world analogous with that of magic among modes of response to the supernatural.

Individual biographies and affliction

In order to circumvent the distortions of decorum, I now present a model of contexts for religious action, and to a lesser extent experience, and evaluate modes of response of living human beings and others. The model is in continuous form, organized around an individual's biography, and draws on studies of living religion in small-scale societies.¹⁷ Not all the possibilities it implies need have been realized in Egypt, and societies vary greatly in the extent to which they sacralize social life or individual biographies and experience. I then discuss some types of religious action that may fall within the areas suggested by the model.

In an individual's life, vital points of transition are birth, puberty and the assumption of an adult role, marriage with parenthood (which starts the cycle over again), and death. These experiences involve most of the minority of people who survive long enough.¹⁸ Less regular and orderly, but nearly universal, events disturb this pattern. These include illness, sudden or premature death, loss through aggression or machination, disasters such as are termed in Anglo-Saxon law 'acts of God', and so forth. Many people also become temporarily or permanently unable to sustain their roles in society and are now said to suffer breakdowns or be mentally ill.

The 'regular' transitions of life may be the subject of rites of passage. The 'irregular' disturbances may be termed afflictions¹⁹ and have as their rarer counterpart benedictions, cases where an individual or a group experiences favour. Our sources say much about the latter, especially if the source of favour is the king—they are an essential subject of biographical texts—but little about the former.

Personal religious practice may be concerned with major transitions, in rituals that are sparsely and unevenly distributed throughout a life. Perhaps because of the liminal character of these experiences, several of the non-human beings directly

¹⁶ See, for example, E. Hornung, *Altägyptische Höllenvorstellungen* (ASAW 59, 3, 1968); idem, *Conceptions of God*, 172–85 (= *Der Eine und die Vielen*, 166–79); D. Meeks, in *Génies, anges et démons* (Sources orientales 8, Paris, 1971), 18–84; H. te Velde, *LÄ* 1, 980–4.

¹⁷ For the biographical organization see, for example, P. L. and B. Berger, *Sociology: a Biographical Approach* (rev. edn., Harmondsworth, 1976). For studies of religion see, among many others, E. E. Evans-Pritchard, *Nuer Religion* (Oxford, 1956); M. Fortes, *Oedipus and Job in West African Religion* (1959; repr. with an essay by R. Horton, Cambridge, 1983); C. Geertz, *The Religion of Java* (1960; repr. Chicago, 1976); R. G. Lienhardt, *Divinity and Experience: the Religion of the Dinka* (Oxford, 1961).

¹⁸ For the context of life-expectancy see Baines and C. J. Eyre, *GM* 61 (1983), 72–4, with refs. (figures probably too low for the élite, but valid for the wider population).

¹⁹ For the term in studies of religion see, for example, Berger, *The Social Reality of Religion*, 76; V. W. Turner, *The Drums of Affliction* (Oxford, 1968).

involved in birth, for example, are not normal deities but shadowy beings with anomalous characteristics.²⁰ Some individuals—an unquantifiable proportion of the population but surely a minority—also participate in the major cults.

Official ideological sources in Egypt implicitly acknowledge the socially destructive effects—and causes—of affliction by asserting that reciprocity and the interlocking solidarity of successive generations are vital in the proper order of things. This is a bland, élite presentation of one side of the truth.²¹ When later sources are more open in their presentation of loss and suffering, this may reflect both their generally greater loquacity and the loosening of decorum. They provide the most informative accounts of affliction, but have no earlier parallels, so that analysis is forced back on more theoretical considerations.

Where religious practice relates to affliction, it may be prophylactic, a complex of observances aimed at keeping influential forces well disposed, while also celebrating their benignity, or it may be a corrective response to affliction. In the former case, observance could be of any type, from official cult and participation in festivals to wearing amulets and making superstitious gestures: 'religion' blends into 'magic' and the two should not be separated.²² Magic is well attested in texts from the Pyramid Texts on and enough is preserved to show its prominence among the living, at least for the élite, but there is relatively little archaeological evidence for it from early periods, except from tombs. I do not study it here because élite magic cannot easily be related to the more widespread practices which probably existed, and it warrants extensive consideration in its own right. Texts integrate magic and religion explicitly by stating that magic is one of the resources given by the creator to mankind.²³ Magic is the main relevant area where the chief thrust of activity is forestalling or countering affliction, so that this legitimation offsets, in rather ambivalent fashion, the bleakness and irrelevance of the human condition as seen in central sources. It is the opposite of the belief in mankind's origin in tears and the creator's bald statement that theodicy is a problem brought into being by mankind (even if he did create the world in a beneficent fashion before it was spoiled).²⁴

For pre-New Kingdom times, there is no reason why the whole range of practices

²⁰ See W. Westendorf, *LÄ* II, 459–62; G. Pinch, *Or* 52 (1983), 405–14, with refs. For a new interpretation see L. Bell, in Munich *Abstracts* (n. 1 above), 9. The royal context is presented by H. Brunner, *Die Geburt des Gottkönigs* (Wiesbaden, 1964).

²¹ Thus the analysis of J. Assmann, in *Studien zu Sprache und Religion zu Ehren von Wolfhart Westendorf*, II (Göttingen, 1984), 687–701, reports on an élite view but does not set it in a broader social context. For the related concept of person and social role see idem, *LÄ* IV, 963–78; and, in general, M. Carrithers *et al.* (eds.), *The Category of the Person: Anthropology, Philosophy, History* (Cambridge etc., 1985).

²² Apart from the analytical importance of this point, the attitude is part of general Egyptian belief, as is shown by the fact that the archetypal magician was the chief lector priest, the central figure in normal temple cult. This does not imply a lack of awareness of differences between magical and other activities, since such an awareness is clearly visible in stories. Cf. J. F. Borghouts, *LÄ* III, 1146, with refs.

²³ See, for example, Hornung, *Conceptions of God*, 207–10 (= *Der Eine und die Vielen*, 203–6); idem, *LÄ* II, 790; Borghouts, *LÄ* III, 1141.

²⁴ Hornung, *Conceptions of God*, 212–13 (= *Der Eine und die Vielen*, 208–9). For the implied attitudes to the past see Baines, 'Ancient Egyptian Concepts and Uses of the Past: 3rd–2nd Millennium Evidence', in R. Layton (ed.), *Who needs the Past? Indigenous Values and Archaeology* (London, in press). For theodicy see also n. 13 above.

I have enumerated should not have occurred—as well as others—but their relative frequency cannot be estimated. Response to affliction should ideally include discovering its cause—in the broadest terms, divination—and suitable action to overcome it. A host of agents may cause affliction, from snakes against which an appropriate charm has not been used to the anger of a deity.²⁵ Not all agents are supernatural, but most problematic ones are; non-supernatural agents may belong in an overarching supernatural context that shows why a particular person is struck at a particular time—an exalted case would be Ramesses II at the Battle of Qadesh.²⁶ Divine affliction may not be altogether bad, because the deity singles out the sufferer, who is transformed by the adverse experience, and especially by overcoming it, into one of the select:²⁷ in a harsh world, affliction and benediction are not always distinct. An example of this in a this-worldly élite context is the early Fifth Dynasty case of Rewer, whose king's inadvertent near-condemnation of him was a sign of favour because of the king's corrective action.²⁸

The complexity of beliefs on this subject is well exemplified in the oracular amuletic decrees of the Third Intermediate Period, where the concern to protect children leads to enumerations of almost any conceivable agent, including gods, demons, a particular kind of spirit called a *wr(t)*, magicians, dreams, and personal destiny; random afflictions like the collapse of a house wall were also to be guarded against.²⁹ To all of these can be added the dead, who are attested as harming the living from the Old Kingdom on,³⁰ and living human beings, whose malice may be

²⁵ Such possibilities are known chiefly from later times and from spells to forestall or counter them or from such sources as the calendars of lucky and unlucky days (which go back to the Middle Kingdom). The area has been little studied; for a summary see E. Brunner-Traut, *LÄ* vi, 153–6. See also I. E. S. Edwards, *Oracular Amuletic Decrees of the Late New Kingdom*, Hieratic Papyri in the British Museum, 4th series (London, 1960), xix–xxiii. For an additional related text see P. Vernus, *RdE* 33 (1981), 89–106.

²⁶ For example, T. von der Way, *Die Textüberlieferung Ramses' II. zur Qadeš-Schlacht: Analyse und Struktur* (Hildesheim, 1984), 302–7. The same essential point is much discussed in the ethnographic literature (see especially E. E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande* (Oxford, 1937)), and has been at the centre of the debate on rationality; see material cited in *GM* 76 (1984), 50–4; J. Overing (ed.), *Reason and Morality* (London, 1985).

²⁷ This attitude is visible in 'personal piety' stelae from Deir el-Medina, see, for example, J. Assmann, *Ägyptische Hymnen und Gebete* (Zurich/Munich, 1975), 349–407 (nos. 148 and 150, among many others). It is prominent in redemption in Christianity from the conversion of Paul on. For an ethnographic analogy compare the ambivalent material discussed by C. Lévi-Strauss, *Anthropologie structurale* (Paris, 1958), 183–203. In two cases, the appearance of Hathor in a dream, in a context similar to that of other personal piety documents, was a pure 'benediction': J. Assmann, *RdE* 30 (1978), 22–50; H. Satzinger, in *Mélanges Gamal eddin Mokhtar*, II (Cairo, 1985), 249–54. Such instances might be much more widespread than the documentation suggests. The occurrence of benedictions in a class of material mainly concerned with affliction is typical of the idealizing monumental record.

²⁸ This addresses only one aspect of the text, which is much cited but has not been discussed *in extenso* (*Urk.* 1, 232; A. Roccati, *La Littérature historique sous l'Ancien Empire égyptien* (Paris, 1982), 101–2).

²⁹ See Edwards, *Oracular Amuletic Decrees*, xix–xxiii.

³⁰ For the dead see G. Posener, *MDAIK* 16 (1958), 252–70; W. Spiegelberg, *ZÄS* 65 (1930), 130–1; A. H. Gardiner and K. Sethe, *Egyptian Letters to the Dead* (London, 1928), 12; Gardiner, *The Attitude of the Ancient Egyptians to Death and the Dead* (The Frazer Lecture 1935, Cambridge, 1935), 16, 18–24, 33; J. Černý in R. A. Parker, *A Saite Oracle Papyrus from Thebes in the Brooklyn Museum* (Providence, RI, 1962), 39 (the deceased Neskons should be kindly disposed to her widowed husband and not seek to do him any harm); P. Kaplony, *LÄ* vi, 648–56. For material in the medical texts see H. Grapow, *Kranker, Krankheiten und Arzt* (Berlin, 1956), 31–5. Note also the belief that one's death could be under the control of a particular god: J. Černý and G. Posener, *Papyrus hiératiques de Deir el-Médineh*, I (Cairo, 1978), 5–8.

implied by the listing of both foreigners and Egyptians in execration texts of the Sixth Dynasty and later.³¹

Responses to affliction

In seeking out the agent of affliction, one may look to disturbances in one's social surroundings, which are often more easily tackled and comprehended than, for example, the medical cause of an illness. Despite the execration texts just cited, human malice, whose manifestation might be witchcraft or sorcery, is not prominent in Egyptian material; this may be a chance of the record.³² Attempts to find human agents of affliction are likely to involve divination rather than detective work—although the two are not incompatible—and suitable people must be consulted or special techniques used. If the agent is outside this world, an informant should have privileged access to another world or a mode of enquiry should lead reliably in that direction. Although it is difficult to be sure that one's source has produced the right answer, definite action is needed, so that doubts may be suppressed.

I consider three among many possible modes of reaction to affliction or prophylactic measures: communication with the dead; oracles and the status of intermediaries in official cults; and human beings with special powers.

Communication with the dead

The letters to the dead show that people who felt that they had suffered injustice could turn to the dead for assistance,³³ probably after mundane attempts had failed or where they had little chance of success. There is thus a very close parallel between an Old Kingdom text recording the verdict of a court over an inheritance and a contemporary letter to the dead.³⁴ The supplicants could expect the dead to act on their behalf unless they had been slighted. The dead could feel wronged by actions of others when they were alive or after their death and the sense of wrong could spill over into litigation in the hereafter.³⁵ The living might feel afflicted by the dead, whether they were at fault or not (of course, they claim not to be). All those approached appear to have been only a few years dead and some may have been reached through intermediaries who had died still more recently;³⁶ intercession accompanies offerings and may be prefaced by a reminder of favours or evocation of

³¹ J. Osing, *MDAIK* 32 (1976), 133–85, with refs.

³² Cf. Borghouts, *LÄ* III, 1142–4.

³³ R. Grieshammer, *LÄ* I, 864–70, with refs.; J. Černý, *A Community of Workmen at Thebes in the Ramesside Period* (Cairo, 1973), 369–70, with refs.; E. F. Wente, *OLP* 6–7 (1975–6), 595–600. There is another possible letter from Naga' el-Deir, knowledge of which I owe to E. J. Brovarski.

³⁴ Gardiner and Sethe, *Letters to the Dead*, no. 1 (translation: Roccati, *La Littérature historique*, 296–7); Sethe, *ZÄS* 61 (1926), 67–79.

³⁵ As noted by Grieshammer, *LÄ* I, 867–9, several letters allude to such litigation, while related spells in the Coffin Texts (38–41) make play with the same possibility; see also idem, *Das jenseitsgericht in den Sargtexten* (Wiesbaden, 1970), 12–30; *OLP* 6–7 (1975–6), 231–4, with refs. Litigation could be between this world and the next or wholly in the next world. It dramatizes the tension between generations, in which the elder feels displaced by the younger and the younger feels that the elder thwarts its assumption of a full and independent role; cf. Fortes, *Oedipus and Job*, 11–31.

³⁶ Gardiner and Sethe, *Letters to the Dead*, 12 (Additional Note).

a gesture of moral solidarity by the deceased.³⁷ One early letter asks that the deceased 'intercede' on the writer's behalf, perhaps in a next-worldly tribunal,³⁸ while only the latest preserved letter uses the deceased explicitly as intermediaries to gods.³⁹ Is this interaction between the living and the dead the literate tip of a non-literate iceberg? Written form seems unnecessary to such communication, and the letters might in any case have been said out loud at the time of deposition. Comparable oral practice could be extremely widespread. The dead who were addressed could be of either sex and may have been people of authority in families, but two New Kingdom wives who may have died quite young are unlikely to fit such a description.⁴⁰ For the high proportion of people whose relatives may have had no proper burial, there could still be some contact with the recently dead. Alternatively, some might have appealed to figures of higher authority, such as local 'deified' dead (but see below). Thus, despite the poor attestation of these letters, they may point to a widespread and socially significant practice. They may also be continuous with cults of ancestors (however recently dead), as attested at Deir el-Medina and elsewhere in the New Kingdom.⁴¹ When integrated with regular offerings to the recent dead and practices like the Beautiful Festival of the Valley,⁴² recourse to the dead in affliction may be both prophylaxis and response. Like the gods, the dead can be capricious and unpredictable: if there was divination, it must have come before the letters were written; only one letter plays on uncertainty about the agent of affliction.⁴³

Whereas there is almost no archaeological material that might fill out the record of the letters to the dead, Old and Middle Kingdom cults of the deified dead are relatively well known,⁴⁴ including those of Hardjedef,⁴⁵ Ptahhotpe,⁴⁶ and Kagemni⁴⁷

³⁷ Ibid. 16. See also the fundamental review of B. Gunn, *JEA* 16 (1930), 147-55.

³⁸ n. 35 above; see also Wentz *OLP* 6-7 (1975-6), 595-7 with n. f.

³⁹ Even there, this is not the main message of the text (Černý, *Community of Workmen*, 369-70). The letter is formally addressed to Akhtai's coffin, not directly to the deceased.

⁴⁰ The author of P. Leiden I 371 seems to have been married to his deceased wife for relatively few years (Gardiner and Sethe, *Letters to the Dead*, no. 6). Butehamun's wife Akhtai probably died young (Černý, *Community of Workmen*, 366-9). The affliction caused by deceased women may have been different from that caused by men.

⁴¹ See, for example, J. L. Keith-Bennett, *BES* 3 (1981), 43-71; Demarée, *The šh ikr n Rr-stelae*; F. Freedman, *JEA* 71 (1985), 82-97. In addition to Keith-Bennett's often poorly provenanced material, an 'ancestor' bust has been found in the Egypt Exploration Society's excavations at Memphis; it is clear that the practice was not restricted to a single community.

⁴² E. Graefe, *LÄ* VI, 187-9, with refs.

⁴³ Two letters mention dreams (W. K. Simpson, *JEA* 52 (1966), 45; Wentz, *OLP* 6-7 (1975-6), 599-600), one perhaps as a premonition which prompted its writing (cf. G. Fecht, *MDAIK* 24 (1969), 111), but it is not clear whether divinatory dreaming is involved. Wentz assumes that something of the sort is present in the letter he publishes. There seems to be no reason why divination should not have preceded letter-writing, and dreaming is one of its possible forms. Since a strong approach is desirable, it would not normally be appropriate to dwell on divination in the letter itself. The letter in which the writer shows uncertainty is Gardiner and Sethe's no. 4.

⁴⁴ For deification of Old Kingdom individuals see in general W. Helck, *WZKM* 63-4 (1972), 18-19; for other examples see next three notes; H. Goedicke, *JEA* 41 (1955), 31-3, is to be rejected. See also in general H. de Meulenaere, *LÄ* VI, 973-4; H. Goedicke, *ibid.* 989-92.

⁴⁵ K. Baer, *Rank and Title in the Old Kingdom* (Chicago, 1960), 74-5.

⁴⁶ H. Junker, *Giza*, VII (Vienna, 1944), 26-7; *idem*, in *Studi in memoria di Ippolito Rosellini* . . . , II (Pisa, 1955), 131-40; H. Goedicke, *ASAE* 55 (1958), 35-55.

⁴⁷ C. M. Firth and B. Gunn, *Teti Pyramid Cemeteries*, I (Cairo, 1926), 1-30.

at Giza and Saqqâra, of Izi at Edfu⁴⁸ and Heqaib at Aswan.⁴⁹ A comparable phenomenon is the deification of King Wenis and the partly consequent cemetery around the west end of his causeway, which continued in use in the Middle Kingdom.⁵⁰ Despite its apparent wealth, this material remains problematic and its significance for personal religion hard to assess. The cases from the capital are known only from inscribed necropolis finds, and the same applies to Izi. Only Heqaib is known to have had an urban monument (although Izi's tomb is very near the town). Almost no small finds or votive objects are reported in connection with any deification, so that worship of these people cannot be proved to extend beyond the lower élite who could afford nearby burials and whose titles show that they were personally involved in the cult; the general quality of Heqaib's monuments is also high, including several royal statues. If the excavation reports are adequate, the cults must have been conducted with perishable offerings only. Large quantities of votive pottery, for example, would probably be mentioned in publications, and for Heqaib it is clear that they were not present.⁵¹

Since the analogy for these cults is communication with the dead, the necropolis is an obvious focus for them, but parallel worship in settlements is possible. A funerary forerunner of Heqaib's sanctuary is known in the settlement on Elephantine Island,⁵² so that mortuary cults of late Old Kingdom notables may have been conducted both in the town and in the necropolis; something similar is attested in the Middle Kingdom text of Djehutihotpe at el-Bersha, who had a 'lower [chapel]' in the town, which was some distance away.⁵³ The general lack of evidence from early urban sites renders any hypotheses about the cult in them fragile; some popular, semi-mortuary activity could have occurred. None the less, the evidence for deification is not impressive testimony to religious activity that reached out beyond the élite and cannot be compared with what is known for the Late Period.⁵⁴ This seemingly promising area of interaction between the living and the dead and the human and the divine is of demonstrable relevance only to local élites; any extension to the rest of the population is speculative.

Oracles and intermediaries

Oracles are a form of divination or of delegated decision-making and may constitute another kind of prophylaxis;⁵⁵ in discussing them, I also consider the problem of

⁴⁸ M. Alliot, *Rapport sur les fouilles de Tell Edfou (1932)* (Cairo, 1933); idem, *BIFAO* 37 (1937-8), 93-160.

⁴⁹ Labib Habachi, *Elephantine IV. The Sanctuary of Heqaib* (2 vols., Mainz, 1985). The cults of both these men continued into the Second Intermediate Period.

⁵⁰ Ahmed Mahmoud Moussa, *MDAIK* 27 (1971), 81-4; H. Altenmüller, *SAK* 1 (1974) 1-18; Moussa and Altenmüller, *MDAIK* 31 (1975), 93-7; P. Munro, *GM* 63 (1983), 81-109. Compare also the Middle Kingdom cult of Snofru at Dahshur, cf. R. Gundlach, *LÄ* vi, 971-2, with refs.

⁵¹ Habachi, *The Sanctuary of Heqaib*, 19-21.

⁵² W. Kaiser, *et al.*, *MDAIK* 32 (1976), 98-107 (F. Junge).

⁵³ P. E. Newberry, *El-Bersheh*, 1 (London, n.d.), pl. 12 col. 12; K. Sethe, *Aegyptische Lesestücke*² (Leipzig, 1928), 78 l. 2, restored [*hwt-kꜣ*]-*hrt*; the wording is uncertain but the sense is clear.

⁵⁴ See E. Otto, *MDAIK* 15 (1957), 193-207; S. Morenz, *Religion und Geschichte des alten Ägypten* (Weimar, 1975), 263-80 (= *ZÄS* 84 (1959), 132-43); D. Wildung, *Imhotep und Amenhotep: Gottverdung im alten Ägypten* (Munich/Berlin, 1977); idem, *Egyptian Saints: Deification in Pharaonic Egypt* (New York, 1977).

⁵⁵ See in general L. Kakosy, *LÄ* iv, 600-6; Černý, in Parker, *Saite Oracle Papyrus*, 35-48.

general access to temples. They are well known in New Kingdom and later sources, but are generally assumed not to have existed earlier.⁵⁶ The context of major oracles is cult temples of deities, but the deified, of whom Amenhotpe I at Deir el-Medina is an important intermediate example,⁵⁷ can also be approached; in later times animals also played a significant part as intermediaries or approachable manifestations of deities. I see no reason why oracles could not have been consulted in earlier times; the argument that they should not be sought because they are ‘completely alien’ to earlier religion (see n. 56) assumes that its general character is better known than I believe it to be. Both accidents of preservation and the operation of decorum may have robbed us of concrete evidence. I discuss three possible examples in texts.⁵⁸ Two are probably fictitious, but are no less relevant.

The first case is where Ankhtify of Moalla claims that Horus led him to conquer Edfu.⁵⁹ This is probably a rationalization after the event and part of its function is to replace the normal royal sanction or initiative of an Old Kingdom biography. None the less, the formulation should be meaningful and morally persuasive in its own terms. If Ankhtify was inspired to conquer Edfu or dreamed that he should, and such experiences were accepted guides to action, this legitimation would be coherent, but it would not be subject to public involvement or social sanction. An oracle would have the advantage of bringing in an outside agency and displaying both the protagonist’s accountability and his prestige. It is irrelevant whether Ankhtify actually consulted or received an oracle. In legitimating his action, his text refers to divine authorization. Such authorization is best seen against a background of divinatory practice, one of whose possible realizations is the oracle.

The next example is the late Thirteenth Dynasty Haremkhauuf of Hierakonpolis, who states that Horus sent him to Itjtawy to fetch his new cult image.⁶⁰ This journey is the sole significant event narrated on Haremkhauuf’s stela and should thus be seen as a privilege or benediction. Some process should lie behind the selection of Haremkhauuf, who is unlikely to have been the only person eligible for the mission. If the text meant merely that the person in authority decided to send Haremkhauuf, the formulation is very grandiose. If there was a genuine choice among candidates, an oracle would be a suitable method of making it, because it is public and detached from the persons involved. It seems to me to be the best way of understanding the passage. As with Ankhtify, the oracle would to some extent replace a central or royal authority. It is appropriate to local or minor concerns, but cannot be addressed to the king, because he can answer back in person; consultation with him has a different

⁵⁶ For example, Assmann, *Ägypten: Theologie und Frömmigkeit*, 188.

⁵⁷ See Černý, in Parker, *Saite Oracle Papyrus*, 41–3, with refs. The legal aspects of the cult are being studied by Andrea Newman (University of Pennsylvania).

⁵⁸ Texts are not necessarily a good source of evidence for oracles, so that the problem of bias is acute here. See also related discussions further below.

⁵⁹ J. Vandier, *Moralla* (Cairo, 1950), 20; G. Fecht, in W. Helck (ed.), *Festschrift für Siegfried Schott zu seinem 70. Geburtstag am 20. August 1967* (Wiesbaden, 1968), 54 with n. 5; M. Lichtheim, *Ancient Egyptian Literature*, 1 (Berkeley etc., 1973), 85. The oracular interpretation was originally suggested tentatively by Vandier and seemingly endorsed by Fecht. For an alternative view see Assmann, *Ägypten: Theologie und Frömmigkeit*, 188.

⁶⁰ W. C. Hayes, *JEA* 33 (1947), 3–11; Lichtheim, *Ancient Egyptian Literature*, 1, 129–30.

character. In the New Kingdom, when the king states that he himself consulted oracles, this may tell us something about the changing status of kingship and of the gods or about the changing quality of the record. We cannot exclude the possibility that kings consulted oracles in earlier times. From the Middle Kingdom on they state that they received divine commands, and these could have come in the form of oracles.⁶¹

The last example is avowedly fictitious.⁶² Sinuhe makes much play of a god's having caused his flight to Palestine, although he does not even know which god it was. One reason for dwelling on this point is Sinuhe's casuistic self-exculpation—that is, in one sense a reaction to affliction long after the event. His recourse to a god is not an oracle, but it is relevant here because of the assumptions it implies about the role of the gods in human affairs and the difficulty of knowing who the god was and what he did. In contrast with the other cases, it is explicitly polytheistic and aleatory. It would fit well in a context where divination was normal. Sinuhe's motivation may have been direct inspiration—if one thinks his narrative is presented as being in good faith—but the putative search for its meaning and source belongs better in a context of divination.⁶³

The use of divine oracles need not be distinct from other modes of access to gods. The most obvious area of potential overlap between oracles, approaches to the deceased through letters and other activities that centred on their burial places, and other modes of action, is in the use of intermediaries: most people had no direct access to the gods in temples and would have had to use priests who deposited their offerings or presented their questions and returned the answers. From the New Kingdom on, intermediary statues are attested both from inscriptions that invite passers-by to make use of them and from the largely undatable signs of wear on the pieces themselves. Here, two scribe statues of the Twelfth Dynasty vizier Mentuhotpe from Karnak are of interest.⁶⁴ These are in a pose known elsewhere for intermediary statues.⁶⁵ They were restored and given new heads in the Nineteenth Dynasty, so that they must have been openly accessible for some 700 years. They are not heavily worn and cannot have been in a very public place, but they could have been venerated by priests who penetrated the inner parts of the temple (the public

⁶¹ Cf., for example, Hornung, *Conceptions of God*, 211 (= *Der Eine und die Vielen*, 207); Vernus, *LÄ* VI, 745–9. In a fragmentary context, Senwosret I appears to have been influenced by a dream to build a temple at Elephantine (W. Schenkel, *MDAIK* 31 (1975), 116, 118 n. a; W. Helck, *MDAIK* 34 (1978), 70–1).

⁶² Cf. Baines, *JEA* 68 (1982), 40–2 with n. 39. For different views see Assmann, *Ägypten: Theologie und Frömmigkeit*, 188; J. G. Griffiths, 'Intimations in Egyptian Non-Royal Biography of a Belief in Divine Impact on Human Affairs', in T. G. H. James *et al.* (eds.), *Edwards Festschrift*.

⁶³ Deities need not be the only dispensers of oracles. Other possible oracles are ancestors, the deified dead, kings, or 'sacred' animals (compare, for much later times, J. D. Ray, *The Archive of Hor* (London, 1976), 130–1). There could also have been non-supernatural oracles such as are well known from the Azande (Evans-Pritchard, *Witchcraft, Oracles and Magic*). (For oracular names see nn. 83 and 86 below.)

⁶⁴ S. Sauneron, in *Karnak V 1970–1972* (Cairo, 1975), 65–76; [J. F. Romano], *The Luxor Museum of Ancient Egyptian Art, Catalogue* (Cairo, 1979), nos. 31, 34; for Mentuhotpe see also D. Franke, *Personendaten aus dem Mittleren Reich (20.–16. Jahrhundert v. Chr.): Dossiers 1–796* (Wiesbaden, 1984), 183–4 no. 262.

⁶⁵ For example, statues of Amenhotpe son of Hapu and Paramesse found by the tenth pylon at Karnak: PM 11², 188 (584); *Urk.* IV, 1832–5; 2175–6; see comments of J. Yoyotte, in *Les Pèlerinages* (Sources orientales 3, Paris, 1960), 42–3; J. J. Clère, *JEA* 54 (1968), 141–8; H. te Velde, *LÄ* IV, 161–3.

areas from which later intermediary statues come had not been constructed when these ones were made). Such objects could therefore be forerunners of later intermediary practices, but used by fewer people. The Karnak of the Middle Kingdom was a much smaller institution than its New Kingdom successor and served a smaller local population, so that a more restricted earlier practice could have had a proportionately larger impact. People could also have had intermediaries to intermediary statues. But, despite these extra possibilities and factors, these statues are no more than possible examples of pre-New Kingdom individuals acting as intercessors before the gods.

If prominent pre-New Kingdom individuals could act as intermediaries—and that is a major proviso—this role might be visible in other material: as something that demonstrated their status and provision for others, it could be another stage in the process that led to private deification and could be worthy of report in biographies. This does not seem to have happened, perhaps in part because decorum would not allow private religious matters and cult activities to be displayed, but, from the late Old Kingdom on, biographies do give importance to people's position in the cult of provincial deities.⁶⁶ Here, their actions, which one suspects are modelled on the king's role (specified in the treatise quoted earlier), cannot be compared directly with royal activity, because no royal inscriptions are concerned with such matters before much later times. The general context is the assertion that the protagonist had an ideal life in his career and in his exemplary performance of duties, as well as in his moral concern for his fellow men, which includes the non-élite 'hungry' and 'naked' to whom he 'gives' (see n. 6 above). He does not claim in so many words that he performed the cult on behalf of the local people as a whole, but that is probably implied. Other motifs, such as the prestige of his access to a deity—denied to most people—are, however, also involved. This does not point directly to an intermediary role, but the attitudes are compatible with one or it might grow from them. The ruler or the superior takes on responsibility for cult in implied return for the labour given him by the people. The superior's view is thus that the rest of humanity need not be concerned with the cult and the maintenance of sweetness and light, so that there is a social division of care, but the rest of society need not have agreed about this. Apart from evidence pointing in another direction, such as personal names, both the fact that a superior presents the cult as an object of prestige, and the reality of human ambition, argue against there being general agreement. With the disappearance of central authority in the First Intermediate Period and a probably looser social hierarchy, local involvement in religion could have become more significant. This may then apply more strongly to the Second Intermediate Period, which came after a time when rules of decorum had loosened and religious material was a little more freely available for private monuments than it had been in the Old Kingdom.

⁶⁶ The best Old Kingdom example is the biographical inscription of Pepyankh the Middle at Meir (*Urk.* 1, 221–4; Roccati, *La Littérature historique*, 234–6). Such concerns are commonplace in Middle Kingdom stelae from Abydos, e.g. Sethe, *Aegyptische Lesestücke*², no. 15.

These arguments cannot prove anything; they are meant to show how gaps in knowledge leave space for more religious action and participation related to the cult than is mostly assumed (apart from part-time minor priesthoods, which are documented in quite large numbers by the Abusir and Illahun papyri⁶⁷). There could be a continuity between practices like oracles, intermediary action, and public festivals, of which the last appear to have been common to all periods. But the arguments remain tentative and cannot be related to specific or archaeological evidence—as is true of most of the problems I consider.

One major difficulty here is the small scale of temples in pre-New Kingdom times. This does not imply that the cult was unimportant, but it shows that the centrality of the king in ideology had its economic counterpart and it renders religious action in and around the temples still less accessible. In any case, material from such minor sites as Medamud⁶⁸ and Tod,⁶⁹ as well as possible early objects from Karnak,⁷⁰ suggests that temples may have been more significant than would otherwise appear from the record. There are a few deposits of votive offerings, many of high quality, from Early Dynastic and Old Kingdom sites,⁷¹ so that their absence from Middle Kingdom sources is the more striking. It remains clear that, in comparison with the New Kingdom, private and non-élite participation must have been small and indirect. Material like the stelae from Abydos, which show religious involvement but place most of their emphasis on the next world, probably also symbolize and present indirectly something of religious action and belief in this world, and part of the reason for their reticence may be restrictions upon what may be said publicly about such action. Here, for example, the stela of Ikhernofret reports only cult actions that took place during a public performance that was partly oriented to the next world, and otherwise deals with the tending of the god's shrine and statue.⁷² His account is coherent and meaningful in its own terms, but its focus on social prestige and the externals of religion does not exclude the existence of underlying personal concerns. In comparison with Ikhernofret, Pepyankh the Middle of the Sixth Dynasty, who states that he entered and saw Hathor in the cult (n. 66 above), is a little more informative. His high local status as nomarch and the placing of his inscription in his tomb, away from cult areas, may be significant as separating the statement from a context in which it could be contrary to decorum (such an argument is particularly hypo-

⁶⁷ Abusir: P. Posener-Kriéger, *Les Archives du temple funéraire de Néferirkarê-Kakai* (*Les papyrus d'Abousir*) (Cairo, 1976); the organization of the temple personnel is being studied by A. M. Roth (University of Chicago). Illahun: U. Kaplony-Heckel, *Ägyptische Handschriften*, 1 (Wiesbaden, 1971); work by U. Luft in progress, see *Oikumene* 3 (1982), 101–56.

⁶⁸ C. Robinson and A. Varille, *Description sommaire du temple primitif de Medamoud* (Cairo, 1940).

⁶⁹ D. Arnold, *MDAIK* 21 (1975), 175–86; C. Desroches Noblecourt and C. Leblanc, *BIFAO* 84 (1984), 81–90.

⁷⁰ Romano, *Catalogue*, no. 10; B. V. Bothmer, *SAK* 6 (1978), pl. 12; J.-C. Goyon and C. Traunecker, in *Cahiers de Karnak VI 1973–1977* (Cairo, 1980), 132–8, pls. 36–40 (prefer later dating).

⁷¹ Material valuably surveyed and discussed by G. Dreyer, *Elephantine VIII. Der Tempel der Satet: Die Funde der Frühzeit und des Alten Reiches* (Mainz, 1986).

⁷² H. Schäfer, *Die Mysterien des Osiris in Abydos unter König Sesostri III* (Leipzig, 1905); Sethe, *Aegyptische Lesestücke*, no. 14; Lichtheim, *Ancient Egyptian Literature*, 1, 123–5.

thetical because of the absence of private temple inscriptions from the Old Kingdom).

This line of reasoning could be extended for Middle Kingdom material. Texts like the contracts of Hapidjefai at Asyut show the deep involvement of the local élite with the priesthood in this life and beyond,⁷³ while the tomb of Wekhhotpe at Meir, in which there is no biographical text but widespread allusion to the cult of Hathor, renews the religious focus in that of Pepyankh the Middle.⁷⁴ On another level, religious material from houses, slightly better attested than that from temples, includes objects of types that occur in new Kingdom temples and may suggest a continuity of practice between the two spheres and periods.⁷⁵

Seers

My third illustration is the seer.⁷⁶ The most definite evidence for using a seer—a person who has privileged insight or techniques of insight into events and their causation—comes from Deir el-Medina documents. In one, a man writes to a woman, perhaps his wife, about what should be done on the death of two children, and refers to possible consultations with another woman who is called *ṭꜣ rḥt* ‘the wise woman’. Two further texts report on what ‘the wise woman’ said about affliction suffered by questioners through the ‘manifestation (*bꜣw*)’ of deities, while another records that an oracular(?) movement by the deified Amenhotpe I in a procession was predicted by the ‘wise woman’. The cases appear to record consultations over divine afflictions or other matters involving divination and, significantly, include prior consultation about something that was itself a consultation. The divinatory process might thus be many-layered. The allusions all appear to be matter-of-fact, suggesting that recourse to the ‘wise woman’ was normal. It is unlikely that evidence for such practices would be preserved from earlier times, so that it is worth considering whether seers or mediums may have been a feature of practical religion of the New Kingdom and earlier times. Recourse to seers would fit well with material like the letters to the dead and has many parallels in other cultures, where such specialists are often women.

All these illustrations have been drawn from literate, and therefore élite, material; only by extension can such practices be posited for the rest of the population. They are, however, of types compatible with widespread use, even for relatively minor concerns, as oracles were used in later times. Because the earliest examples of oracles that have often been discussed are royal and involve major matters of succession or of the waging of war, it is generally assumed that they filtered down from there

⁷³ For example, Sethe, *Aegyptische Lesestücke*², 92–6.

⁷⁴ A. M. Blackman, *The Rock Tombs of Meir*, vi (London, 1953).

⁷⁵ For example, W. M. F. Petrie, *Illahun, Kahun and Gurob* (London, 1891), 11; G. Brunton, *Qau, and Badari*, III (London, 1930), 7, pls. 9–10; B. Fayolle, *Le Livre du Musée Guimet de Lyon* (Lyon and Paris, [1958]), 81. I am very grateful to Geraldine Pinch for these references.

⁷⁶ Penetrating discussion by J. F. Borghouts, in R. J. Demarée and J. J. Janssen (eds.), *Gleanings from Deir el-Medina* (Leiden, 1982), 24–7. See also B. Letellier, in J. Vercoutter (ed.), *IFAO, Livre du Centenaire* (Cairo, 1980), 127–33. Both authors consider the possibility that *ṭꜣ rḥt* is a name; this can almost certainly be discounted.

to minor, everyday concerns. This is not necessarily correct; although no early evidence can be cited for frequent or 'trivial' use of oracles, the possibility is worth considering. The variety of religious and divinatory practices can be seen as integrating a variety of approaches to comprehending and coping with problems of normal existence that centre around the individual or the social group. These approaches can extend into different contexts: oracles into the official cult; letters to the dead into mortuary practices and in some cases into ancestor cults; other forms of divination into religious action in local communities; dreams relate to individual actions and to any other of these possibilities. In another sense, all of these modes of action may overlap with 'official' and secular ones such as recourse to courts of law. What seems to have changed over the centuries is the nexus of everyday religious action in local, non-official, individual concerns (see below).

Early piety?

I now turn briefly to the 'piety' side of my title. Whereas so far I have asked what the range of religious action might have been, the background to considering piety is the problem of understanding the significance of the large numbers of Egyptian deities for society as a whole and for its individual members, in periods from which there is very little evidence for interaction with them. Only recently have scholars produced evidence for pre-Amarna period piety.⁷⁷ Can their arguments be extended or complemented? Purists may say that oracles, and even much use of intermediaries, are manipulative, but for the user they involve active response by the deity and positive engagement by the supplicant. The deity plays a direct part in people's lives, at least at moments of decision, affliction, and change. If the possibility that there were oracles in earlier times is admitted, this may provide an additional stimulus to searching for traces of piety.

Before considering the sparse evidence from outside the élite and participants in the main cults, the implications of the latter should be reviewed. For much of the time, the regular cult may be 'performative' and almost mechanistic,⁷⁸ and there is no access to the officiants' attitudes, which need not include personal involvement. Such an orientation suits especially the function of propitiating potentially hostile and capricious deities. Votive offerings tend to be evaluated in the opposite sense, as witnesses of positive personal involvement.⁷⁹ But this contrast is probably too sharp. The strong social buttressing of regular cults does not exclude personal involvement. Such evidence as accompanies New Kingdom votive offerings,⁸⁰ however, could be thought to show that they too were mechanistic and manipulative, for the record reveals almost nothing of accompanying attitudes. Here, decorum may constrain the material so that it is virtually mute. Neither regular cult nor irregular

⁷⁷ G. Posener, *RdE* 27 (1975), 185-210; P. Vernus, *RdE* 34 (1982-3), 115-17.

⁷⁸ See Assmann, *Re und Amun*, 33-53; for a general interpretation, including public aspects, see S. J. Tambiah, *A Performative Approach to Ritual* (London, 1979), (= *Proceedings of the British Academy* 65 (1979), 113-69).

⁷⁹ For example, S. Morenz's discussion of animal burials: *Religion und Geschichte des alten Ägypten* (Weimar, 1975), 304-12 (= *ZÄS* 88 (1963), 42-7).

⁸⁰ This material is gathered and analysed by Geraldine Pinch in her study mentioned in n. 1.

votive evidence displays the attitudes of the participants. They are likely to have been diverse and not necessarily very different in the two contexts; it is best to allow for the possibility that personal relationships with deities might occur in both. The late New Kingdom change to displaying such relationships—which is confined to relatively few contexts—may be to a considerable extent a change in decorum.

There is ample evidence from all periods that the most fundamental transition of all, birth, was related to deities by the parents, who named the majority of children for gods.⁸¹ Names of the gods themselves are not common as personal names; instead, names record a relevant aspect or action of the deity, or state that the holder belongs to a deity or to one of his or her manifestations. One must be cautious in assessing this material and bear in mind the story of a Zande child whose name meant ‘God has sealed my lips’ because, the father explained, he couldn’t think what to call him.⁸² On the other hand, anyone who called a child Djeddjehutyufankh, ‘Thoth said he would live’, would hardly do so just because he liked the sound of the name;⁸³ the fundamental seriousness of naming and of Egyptian attitudes to names need not be doubted. In default of appropriate studies, my comments here are impressionistic.⁸⁴ The frequency of names that assert that a god is gracious, great, and so forth, is significant here. The obvious explanation of such a pattern is that in the birth of a child the originator of a name saw a token of the god’s graciousness, greatness, etc.; most later users of the name may have chosen it to commemorate elder or deceased kin, but they would probably have rejected it if its content was distasteful. Apart from prophylactic names directed at capricious deities, mostly goddesses, almost all theophoric names show the gods in a favourable light. If one was dissatisfied with the gods, it was probably because the child died or was crippled or deformed; one might then choose a non-theophoric name, an option that was always available in any case. Apart from its inauspiciousness, the choice of a ‘deviant’ name might be rendered impossible by social pressures or by decorum.

Few names express or recount interaction between the namer and a deity; rather, birth is a signal of divine presence and involvement. Because so many infants and mothers died, the gods were not simply beneficent in birth, which was an occasion of potential tension, affliction, and divine caprice as much as, or more than, others. Mediated contact with the gods before birth, for example through votive offerings or indirect temple involvement, is poorly attested before the New Kingdom,⁸⁵ and seems to be separate from the experiences evoked in names, which should thus show

⁸¹ Cf. P. Vernus, *LÄ* IV, 334–6; H. Brunner, *LÄ* IV, 951 with n. 2; J. Assmann, in E. Hornung and O. Keel (eds.), *Studien zu altägyptischen Lebenslehren* (Freiburg/Göttingen, 1979), 15 with n. 9 (I have not seen the work of R. Albertz referred to there).

⁸² E. E. Evans-Pritchard, *Essays in Social Anthropology* (London, 1969), 175–7.

⁸³ Note the comment of Edwards, *Oracular Amuletic Decrees*, xx with n. 1 (citing Černý), that oracular names like this one are especially common in the period of the oracular amuletic decrees, in which a wide range of forces acting on human destinies is named.

⁸⁴ Ranke, *PN*, is not organized around thematic concerns, and probably includes no more than half the accessible names. For a valuable analysis of Early Dynastic names see Hornung, *Conceptions of God*, 44–9 (= *Der Eine und die Vielen*, 32–8). H. Junker, *Pyramidenzeit* (Einsiedeln etc., 1949), 26–40, collected valuable material, but his discussion is not very illuminating.

⁸⁵ For New Kingdom material see Pinch (n. 1 above).

piety, as I have defined it, in a non-official context.⁸⁶ It is therefore significant that most theophoric names refer to major deities, not to the liminal figures like Bes and Taweret. If names focused exclusively on birth itself, these figures should be more prominent. None the less, the involvement of major deities in birth could be an experience and affirmation of centripetal religious concerns that left the rest of life relatively untouched, or it could have a broader reference. Here, the sparse evidence for religious practices, such as festivals, that might reach out to many people, is relevant, but hardly decisive. It is easier to assume that divine involvement could be felt at least at times of elation and of crisis and affliction—not just in birth and death—than to believe in a separation of the different domains; after all, the name one received at birth was mostly kept for life. Some important transitions in life, such as marriage, were secular, but others certainly were not. If in transitions major deities were invoked, their involvement may have been felt to some degree at other times.

Two points remain. First, how far do hints of concern for or involvement with central religion imply an aspiration to personal experience—an orientation toward piety? In pre-New Kingdom times, rules of decorum probably make it impossible to answer this, but if names and other hints suggest that it may have existed, I see no persuasive argument against its occurrence, and religious material from the élite implies it could have been favourably evaluated. Second, the plurality of deities argues against any simple or unitary understanding of their roles. Unlike the few gods in some societies in which practices like the ones discussed earlier in this paper are dominant, whose presence is almost irrelevant to human concerns, Egyptian gods are attested in names—our only evidence—at all social levels and in all periods.⁸⁷ Major deities appear to have a distribution that encompasses both the higher reaches of religion and its middle and lower ground, where they vie with, or provide valid alternatives for, other non-material forces.

This argument for early piety competes with that of Assmann,⁸⁸ who maintains that the content and phraseology of New Kingdom pious texts points to an origin in instruction texts and in the ‘loyalist’ texts of the Middle Kingdom. If this literary derivation reflected accurately the extent of the phenomenon, it would be necessary to exclude other possible manifestations of piety from consideration and to assume that piety arose in the core élite and was a diversification of their religious orientation. In presenting his case, however, Assmann minimizes the significance of

⁸⁶ Earlier naming patterns are notably different from those of the Third Intermediate and Late Periods which refer to oracular consultation during pregnancy, the evil eye, etc. The attitudes of the two groups are not, however, incompatible. Changes in patterns do not have to reflect changes in religious belief in any straightforward way, but could be complicated by fashion, decorum, and other factors. In particular, the earlier absence of oracular names is not necessarily evidence for an absence of oracles, because the context of use of oracles could have changed, and in any case the new name types did not appear until some centuries after oracles become frequent in the New Kingdom record.

⁸⁷ Valuable material in B. L. Begelsbacher-Fischer, *Untersuchungen zur Götterwelt des Alten Reichs im Spiegel der Privatgräber der IV. und V. Dynastie* (Freiburg/Göttingen, 1981); Hornung, *Conceptions of God*, 66–74 (= *Der Eine und die Vielen*, 56–65).

⁸⁸ In Hornung and Keel (eds.), *Studien zu altägyptischen Lebenslehren*, 11–72.

evidence from personal names and does not seek to broaden his enquiry in other directions (which would be irrelevant to his purpose). The difficulty with his approach is that it does not provide a clear point of departure for religious diversification. His literary arguments are, however, persuasive. I would prefer to interpret his material as showing the use of specific literary models for a new genre of literary formulation. Any literary novelty is a departure that will either seek to be a nearly 'pure' innovation or look to a model from which it may depart; these texts could use loyalism as such a model. Among the further constraints on the creation of new literary forms is decorum, which might well affect the presentation of personal religious matter in public. If considerations of genre and of decorum affect the development of personal piety texts, their literary antecedents may tell us little about the origin of the attitudes to which they give an artistic form, and I suggest that this is the case here.⁸⁹

Conclusion: change and diversity

Why should the general image projected by religion change so much in different periods? All the practices and beliefs I have reviewed are pushed to the margin of the preserved record of early periods. In the New Kingdom, and much more in the Late and Graeco-Roman periods, from which analogous practices are altogether better known, they are fairly well integrated into the major cults, so that, for example, letters to gods may replace letters to the dead.⁹⁰ This change can be seen occurring in the New Kingdom; it could be apparent or substantive, or more probably both. If it is apparent, the record must be distorted. Distorting factors are the system of decorum, the slowly increasing spread of writing and probably of wealth, and specific archaeological anomalies, such as the preservation of major temples but not minor ones. If decorum is invoked here, it implies that most people were deprived of the official mode of expression, as is evidently the case; but were they deprived by poverty or by deliberate exclusion? The rigid hierarchies of official presentation—which ignores humanity or, where humanity is shown, ignores most of religion—were surely oppressive in some measure,⁹¹ but alienation, I suggest, breeds aspiration to belong with the élite rather than rejection of them. The élite and the rest may be united by everyday religious practices that are not part of official ideology and are concerned with problems of comprehending, accepting, and

⁸⁹ It would even be possible to turn Assmann's argument round and propose that the loyalist phraseology of Middle Kingdom texts derived from spoken conventions in statements about the gods.

⁹⁰ This has been suggested by several writers on demotic letters to gods, cf. Grieshammer, *LÄ* 1, 868–9; R. A. Caminos, *LÄ* 1, 860 nn. 5–6. The earliest letters to gods, which date to the late New Kingdom, overlap with the latest letters to the dead, of which the last includes an address to gods (n. 33 above). The neat transfer of practices that could be deduced from this transition is, however, complicated by other factors such as the New Kingdom prominence of intermediary statues (nn. 64–5 above), which appear to have come into their own after the deaths of their owners, whose mortuary interests and continuing status among the living were enhanced by the approaches they received from the faithful.

⁹¹ Cf. Baines, 'Literacy, Social Organisation and the Archaeological Record: the Case of Early Egypt', in B. Bender *et al.* (eds.), *State and Society: The Emergence and Development of Social Hierarchy and Political Centralization* (London, in press).

responding to the world, to loss, and to suffering that are treated in religious terms by very many cultures. The whole people is also united by the general absence of explicitly religious material on the monuments. The official cult and ideology are refined aspects of religion's role in comprehension and presentation, and serve more than simply religious ends. They are short on general appeal, for which they are not designed; they are hardly concerned at all with loss and suffering. In participating in them, the élite may satisfy only one side of their religious needs and aspirations, while sharing the remainder with the rest of society.

In the New Kingdom, and especially later, this picture is more homogeneous. The reasons for this levelling may be in part social. New Kingdom and Late Period Egypt was probably more urban and sophisticated than the Old and Middle Kingdoms. The small-scale, strongly group-oriented character of material like letters to the dead might be less appropriate to this society than something at once more individual and more anonymous. Here, the analogy with small-scale societies I proposed at the beginning is most relevant: earlier Egyptian religion seems to encompass, but only weakly to integrate, practices typical both of smaller and of larger societies, while later religion belongs more clearly to a large society. A social analysis along these lines implies that some changes in religion may not be as great as is often thought—a theme of much of what I have said. Later religion is better known, as well as perhaps more religious, just as the later wisdom texts are more overtly, but not necessarily more fundamentally, religious.⁹² Apart from possible continuities I have emphasized, there was substantial change, for which areas that I have not considered—like discussion in the core élite or the proliferation of practices relating to animals—provide ample evidence. For earlier times we must allow not just for polytheism and its variety of official observances and contradictions, but also for many other practices, analogous with later ones but different in focus. For the actors, this diversity was not bewildering because they encountered each element in its place and exploited the potential it offered, but it may still bewilder us.

⁹² Cf. Assmann, in Hornung and Keel (eds.), *Studien zu altägyptischen Lebenslehren*, 11–72. For my arguments, which tend in the opposite direction, see Baines, *GM* 76 (1984), 47–50.

THE LOCATION OF IREM*

By DAVID O'CONNOR

Since Priese's well-argued article of 1974, it has been accepted by many scholars that the southern toponym Irem probably lay in Upper Nubia, although Kitchen (1977) and Vercoutter (1980) dissented from that view. After a critical review of Priese's argument and the relevant evidence, it is suggested here that Irem is rather to be located somewhere within a triangle defined by the Shendi reach, the northern Butana, and the Atbara. Historical reconstruction must, at the least, take both locational possibilities into account. The implications for New Kingdom campaigning in the Sudan are that an Upper Nubian locale for Irem indicates an insecure hold over that region, whereas a more southerly location indicates an aggressive Egyptian policy corresponding to those followed in Western Asia.

Introduction

ANCIENT Egyptian texts record many toponyms which lay within the borders of the modern Republic of the Sudan, but the more specific locations of these toponyms within this vast area (about $2\frac{1}{2}$ million sq. km) are for the most part unknown.¹ Indeed, we do not even know in most cases whether a given toponym represents a large country, a small territory, or simply a place along some itinerary.² However, a few Sudanese toponyms which were evidently important to the Egyptians and were presumably countries of considerable size are mentioned more frequently and in more detailed contexts than the others. On the basis of the relevant data a substantial—although not unanimous—scholarly consensus has developed about the locations of this minority of the toponyms. This consensus leads to a certain set of historical conclusions about the extent and nature of Egyptian activity within the Sudan and about the history and character of various peoples encountered as a result of this activity.

It is my purpose here to show that alternative locations can be suggested for one of the toponyms in question, for reasons which are at least as valid as those upon which the present (partial) consensus rests. In my view, the alternative is preferable to that which at the moment receives majority support. Of course, a substantial shift in the

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¹ Virtually all the known toponyms are collected and discussed in K. Zibelius's valuable compendium *Afrikanische Orts- und Völkernamen in hieroglyphischen und hieratischen Texten* (Wiesbaden, 1972).

² D. B. Redford, *JSSSEA* 12 (1982), 55–74, has persuasively argued that the list of Asiatic toponyms drawn up under Tuthmosis III was based upon 'a group of itineraries for western Asia as far north as the Euphrates, well known to the Egyptian Department of State in the 15th century, and used by its couriers' (ibid. 60). Similar itineraries—developed by trading, military, and diplomatic expeditions—may well underlie the New Kingdom lists of Sudanese toponyms and perhaps even their predecessors in the Middle Kingdom Execration Texts. It follows, therefore, that the longer lists of 'African' toponyms may consist of several itineraries, each of which, according to the route which was followed, groups together large and small regions, major and minor settlements, and also named or anonymous geographical features such as hills, streams, and valleys (cf. ibid. 57, 59, 64, and 'Conspectus' on 74). The shorter 'African' lists—like the shorter Asiatic lists—appear to contain mainly countries or states of considerable size and power.

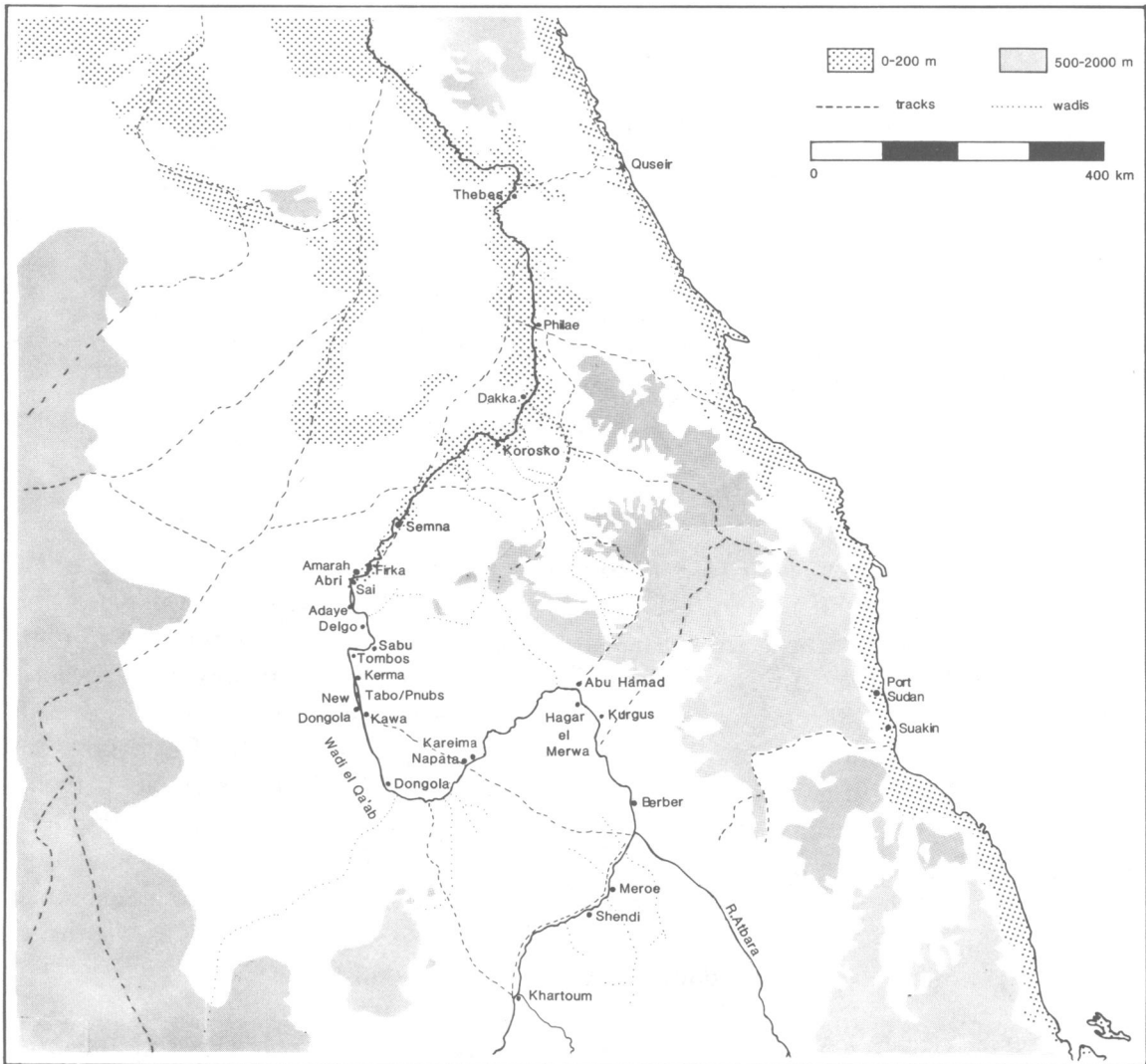


FIG. 1

location of a historically significant Sudanese toponym also creates major changes in the historical picture. In a concluding section, I therefore explore the historical implications of the different locations that can reasonably be suggested for the same toponym. The modern place-names mentioned, and the possible locations of ancient toponyms, are shown on figs. 1 and 2. Like fig. 3, these were drawn by Sarah Iams.

I. Irem

Irem is a particularly significant toponym in two ways. First, it had important relations with New Kingdom Egypt; and second, Irem may be the New Kingdom form of the Old Kingdom country name Yam, and establishing the location of Irem might therefore help to fix the location of Yam also. In 1974 K.-H. Priese in a richly

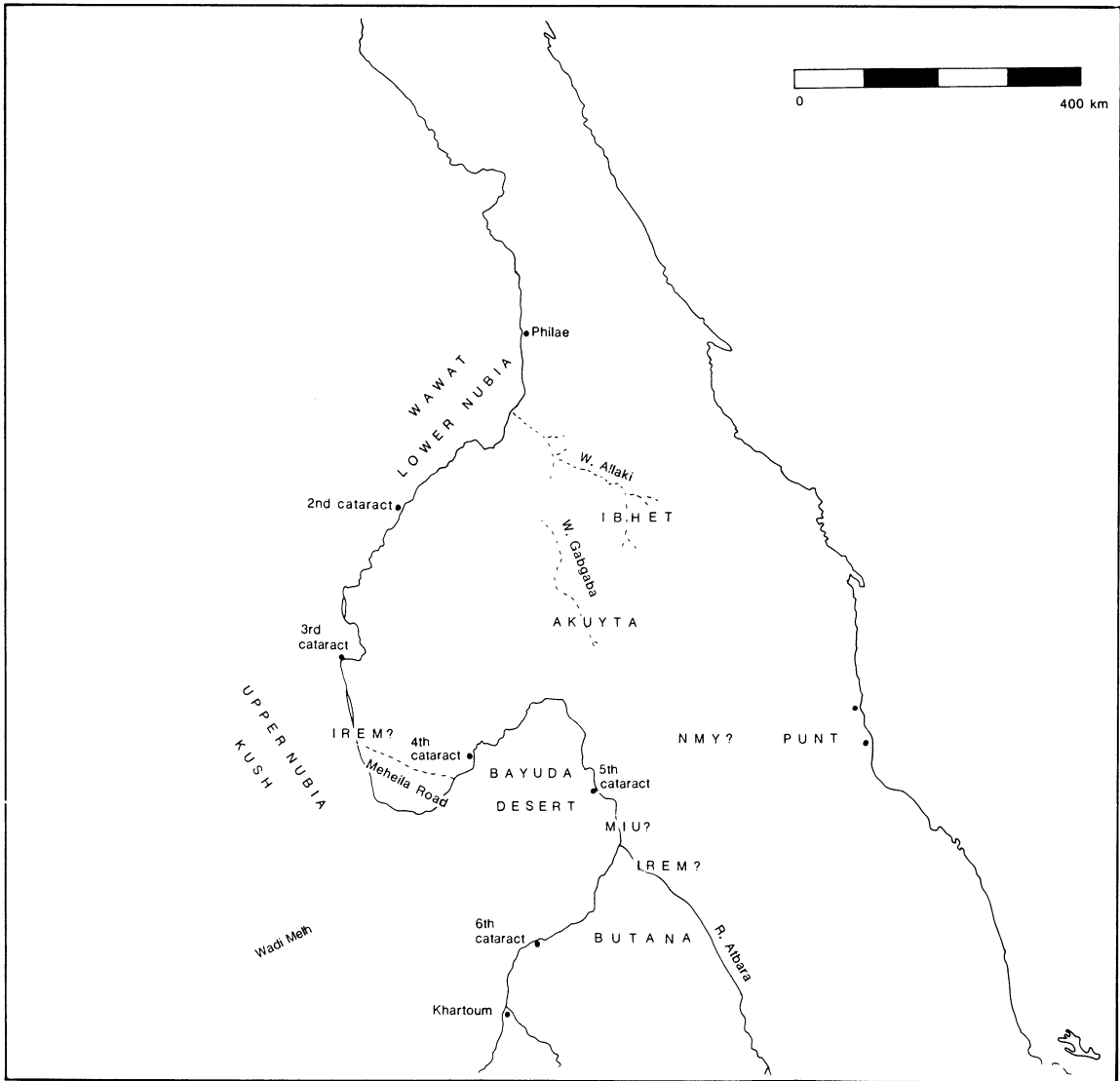


FIG. 2

documented study concluded that in the New Kingdom and in Napatan and Meroitic times Irem was located in the Upper Nubian valley, more specifically in the area defined by New Dongola, Kawa, and Tabo. This conclusion has received support from Trigger and Kemp.³ However, more recently alternative locations for Irem have been suggested. Kitchen has argued that 'at least part of the land of Irem in Ramesside times' lay west of Upper Nubia, approximately in the Wadi el Qa'ab,⁴

³ K.-H. Priese, *Altorient Forschungen*, 1, 7-41 (abbreviated Priese, 'Irame'); B. G. Trigger, *Nubia under the Pharaohs* (Boulder, 1976), 112; B. J. Kemp in P. D. A. Garnsey and C. R. Whittaker (eds.), *Imperialism in the Ancient World* (Cambridge, 1978), 29.

⁴ K. A. Kitchen in E. Endesfelder *et al.* (eds.), *Ägypten und Kush* (Berlin, 1977), 217-18.

while Vercoutter has concluded that the whole of Irem lay 'Sud-Sud-Ouest de la vallée nubienne du Nil'.⁵ I have suggested yet another location for Irem, namely 'south of the Fifth Cataract' and including 'not only a section of the Nile valley, but also semi-arid or even savannah lands east of the Nile',⁶ while Störk has argued that Irem lay east of the Abu Hamed reach of the Nile (Kareima to Berber) and extended as far as the Red Sea coast.⁷

As I shall now try to demonstrate, a careful review of the data which are most critical for the locating of Irem shows that in fact none of them provides *compelling* support for any of the proposed locations. Therefore, at the least, each location proposed is approximately of equal validity. Some of the data do indicate that one of the locations proposed above—i.e. placing Irem along the Berber—Shendi reach of the Nile and/or in the northern Butana—is the most likely; but the evidence is not entirely conclusive.

Kawa IX

Priese emphasized particularly four pieces of evidence in arguing for an Upper Nubian location for Irem, but none of them, in my opinion, compels us to accept this location. The first datum is a long inscription upon the walls of Temple T at Kawa. This, and the even later graffiti discussed below, are of course far removed in time from the New Kingdom, the principal period of interest in this essay. By the time of these data, the locational and other meaning of the toponym Irem may have changed considerably. Nevertheless, information on Irem's location is so rare, that Priese's conclusions from these later data must be carefully considered. Known as Kawa IX, this text narrates events occurring in the first two regnal years of King Irike-Amonate (c.431–405 BC), whose acts included giving to the Amun temple at Kawa a gift of 'lands' and 'people' (*ḥwt* and *rmṯ*). The 'lands' are specifically identified as four toponyms while the 'people' include 'families of Irem'.⁸ A little earlier in the narrative Irike-Amonate is described as making a similar gift (of 'lands' and 'people' different from those allotted to the Kawa temple) to the Amun temple at Pnubs (Tabo). Both gifts are introduced by virtually identical passages in which the god of the relevant temples asks that the gift be made and the king states that he is making the gift.⁹

Macadam, the editor of Kawa IX, proposed three theories about these gifts, all three being relevant to the location of Irem, but each in a different way. In Macadam's translation the gifts are made because the 'lands' and 'people' had been

⁵ J. Vercoutter, *MIFAO* CIV (Cairo, 1980), 161–71.

⁶ D. O'Connor, 'Egypt, 1552–664 B.C.', in J. D. Clark (ed.), *The Cambridge History of Africa*, 1 (Cambridge, 1982), 934–40.

⁷ L. Störk, *Die Nashörner* (Hamburg, 1977), 263–4.

⁸ Kawa IX, cols. 65–8; for the entire text, see M. F. L. Macadam, *The Temples of Kawa, I. The Inscriptions* (Oxford, 1949), 50–67 (text), 17–24 (plates). 'Families of Irem' (col. 66) is Macadam's translation; Priese, 'Irame', 9, points out that the phrase could be translated in other ways, i.e. 'Angehörige/Familien des Stammes/von Bewohnern des Landes' *'rm'* or 'der Stamm (namens) *'rm'*'.

⁹ Kawa IX, cols. 60–3. On the identification of Pnubs with Tabo, see H. Jacquet-Gordon, C. Bonnet, and J. Jaquet, *JEA* 55 (1969), 109–11.

captured by means of the god's 'aid'.¹⁰ In commenting on this, Macadam suggested:¹¹

(i) that the 'lands' (and implicitly the 'people') 'might be in the neighbourhood of Pnubs' (and implicitly Kawa). This theory he thinks less likely than theory (ii);

(ii) that the 'lands' (and implicitly the 'people') given to Pnubs (and implicitly Kawa) had been captured 'from the *Rhrhs* north of Meroe (Cols. 26 ff.) or from the Medja near *Krtn* (Cols. 45 ff.)'. Macadam is here referring to events—described also in Kawa IX—which preceded the making of the gifts to Pnubs and Kawa temples. Upon the death of the king—Irike-Amonate's father—the *Rhrhs* had attacked the province of Meroe, where Irike-Amonate was residing. The latter's troops drove the *Rhrhs* away; he then proceeded overland from Meroe to Napata, where he was crowned, and then made a royal progress downstream through Upper Nubia, putting the affairs of its provinces in order. During this progress *Mdd* tribesmen raided the town *Krtn*, where Irike-Amonate happened to be staying for a period; the *Mdd* fled when they discovered the presence of the king, whose troops pursued and harassed the *Mdd*.¹²

Macadam's theory (ii) therefore—assuming it applies to the 'people' as much as the 'lands'—would require that the Irem 'families' were captured from the *Mdd* (who occupied the desert east of Upper Nubia)¹³ or from the *Rhrhs*, who were desert-dwelling tribesmen located either in the northern Butana or (less probably) west of the Berber–Meroe reach.¹⁴ Irem therefore would appear to have lain east of Upper Nubia or east (or west?) of the Berber–Meroe reach.

(iii) Macadam also makes a remark which in effect is yet a third theory by saying—without giving any reason—that perhaps the presence of the king's army was not necessary at the capture of the 'lands', a remark presumably also applying to the capture of the 'people'. By this I assume Macadam means that the news of the royal victories over the *Rhrhs* and the *Mdd* was in itself sufficient to make other lands submit voluntarily to the king or to reaffirm an already existing subjection. In these circumstances Irem would be located virtually anywhere in the general region of Upper Nubia and the Berber–Meroe reach.

Zibelius offers an interpretative theory about the 'lands' and 'people' making up the gifts to Pnubs and Kawa temples which is similar to theory (iii) of Macadam. Zibelius does not believe the 'lands' and 'people' were connected with the defeats of the *Rhrhs* and the *Mdd* and does not think Irike-Amonate would have had sufficient time to win these two victories *and* conquer some ten or eleven *other* lands (i.e. 'lands' and the domiciles of the 'people' identified in the gifts). These ten or eleven lands therefore must have been subject to Irike-Amonate before the events narrated in Kawa IX, and by giving them to the temples Irike-Amonate was ritually ensuring that his authority over them—as a *new* king—would be recognized by the gods.¹⁵

¹⁰ Macadam, *op. cit.* 60–1.

¹¹ For the three theories, see *ibid.* 61 n. 110.

¹² Kawa IX, cols. 1–48.

¹³ Zibelius, *op. cit.* 133–7 (under *mdr*).

¹⁴ *Ibid.* 144 (under *rhrhs*) where a location 'zwischen Nil und Atbara' is preferred.

¹⁵ *Ibid.*, 57–8 (under VII A a 30).

Zibelius's theory, like Macadam's theory (iii), provides no indications on the location of Irem.

Priese explicitly rejects Macadam's theory (ii), does not mention (iii), and believes Macadam's theory (i) is the correct one. He has two principal reasons for this belief. First, he points out that the term *šhwt* used for the 'lands' would be more applicable to arable, riverine land than to the desert or semi-desert territories occupied by the *Rhrhs* and the *Mdd*. Second, Priese believes that the stated reason for which the 'lands' and 'people' were given to the gods of Kawa and Pnubs is not to be translated 'captured by mine/thy [i.e. the god's] aid' but rather 'm'ont/t'ont été enlevées'. In other words, Priese argues: 'Ce que demandent les dieux au roi, c'est... la restitution de propriété en terres et en hommes, qu'ils avaient perdus pendant une période ignorée par nous.' In these circumstances, Priese concludes: 'Damit wird es aber wahrscheinlich, dass die aufgeführten Ländereien und die Wohnsitze der Menschen in der näheren oder weiteren Umgebung von Tabo und Kawa, auf jeden Fall aber im Niltal, und doch wohl südlich des III Kataraktes zu lokalisieren sind.'¹⁶ Irem, therefore, is in his opinion probably located in Upper Nubia.

However, in my view there is no compelling reason to consider this more likely than Macadam's theories (ii) and (iii) or than Zibelius's theory. Priese's point that *šhwt* implies arable land is good, but can apply to both Upper Nubia and the Berber-Meroe reach. Priese himself notes that the 'lands' owned by a temple can be scattered over very wide areas,¹⁷ so there is no reason to assume that all, or even any of, the 'lands' given or restored to Pnubs and Kawa temples were located in Upper Nubia. Even if some of the 'lands' can be shown to be in Upper Nubia¹⁸ others may have been located elsewhere. In any case, the specific reference to Irem in Kawa IX involves not the 'land' of Irem itself, but people from Irem; and a location outside Upper Nubia for the original homes of these and the other 'people' mentioned is quite possible, for 'people' are mobile whereas 'lands' at least are immobile. New Kingdom examples of foreigners being settled as servants at Egyptian temples could be cited, but it is sufficient to note that Kawa temple itself had enjoyed similar benefits. About 250 years before Irike-Amonate's reign Taharka had settled at Kawa, as temple servants, children of Tjehenu (i.e. contemporary Libyan) chieftains, gardeners from Bahariyah Oasis, Lower Egypt, and the Mentiu of Asia and the wives of Lower Egyptian rulers.¹⁹ About seventy years after Taharka the Napatan ruler Anlamani made prisoners of war from the land of Belhe²⁰ man- and maidservants of the god at Kawa.²¹

Finally, there are *slight* indications that Macadam's theory (ii)—and more

¹⁶ Priese, *Études et travaux* 7 (1973), 156–8; 'Irame', 7.

¹⁷ Priese, *Études et travaux* 7 (1973), 158. For good New Kingdom evidence of widely scattered temple lands see W. Helck, *Materialien zur Wirtschaftsgeschichte des Neuen Reiches*, II (Wiesbaden, 1960), 216 ff. and especially 218–21 (items d and h).

¹⁸ Priese tries to demonstrate this for *Irtkr* and *Mrkr* (both given to the Kawa temple), but his arguments are not convincing; cf. *Études et travaux* 7 (1973), 162; 'Irame', 7 and 71.

¹⁹ Kawa III, line 22; Kawa VI, lines 15, 20–1; cf. Macadam, op. cit. 9 and 35–6.

²⁰ In the deserts east of the Nubian Nile? See Zibelius, op. cit. 108 (under *brhj*).

²¹ So to be inferred from Kawa VII, line 20; cf. Macadam, op. cit. 47.

specifically a link between the 'families of Irem' and the defeat of the *Rhrhs*—is to be preferred above the other theories. First, Macadam's theory (ii) is at least based on explicit statements made elsewhere in Kawa IX, namely the descriptions of the defeats of the *Rhrhs* and the *Mdd*. His other theories, and those of Zibelius and Priese, all have to assume circumstances which in fact are not described explicitly in Kawa IX. Second, the victory over the *Rhrhs* seems more likely to have resulted in the capture of 'lands' and 'people' than the victory over the *Mdd*. The *Mdd* raid on *Krt̄n* was comparatively unimportant; it receives only three columns of description (the conflict with the *Rhrhs* occupies at least sixteen columns),²² the *Mdd* avoided battle and fled when they discovered the king's presence, and they were so few or so panic-stricken that the pursuing royal troops were able to 'slaughter' them without losing a man. By contrast, the *Rhrhs* were rebellious, i.e. deliberately risked conflict and appeared in large numbers—'more numerous than the sand'.²³ They raided the northern part of the province of Meroe, carrying off men and herds of cattle; and one or two weeks later they were still 'surrounding' or 'circulating through' the province, perhaps threatening Meroe itself. It took a battle to defeat the *Rhrhs*, and then as they fled they were pursued by royal troops, slaughtered, and plundered. Could the 'families of Irem' have been captured at this time? Were the *šhwt* given to the temples of Upper Nubia arable lands in the Berber–Meroe reach which had been reclaimed from the *Rhrhs*? It is noteworthy that the aid of Amun was specifically invoked against the *Rhrhs*, but not against the *Mdd*, and booty and prisoners from the *Rhrhs* would therefore be appropriate gifts to the Amun temples of Upper Nubia. However, the case is not fully convincing. The taking of prisoners is not mentioned in connection with the *Mdd*, but nor is it with the *Rhrhs*; the above suggestion about the source of the *šhwt* is perhaps too ingenious; and victory gifts should also have been given to the main Amun temple at Napata (Gebel Barkal), whereas such gifts are not referred to in Kawa IX.²⁴

The preceding discussion therefore leads to the following conclusions. First, all of the theories relevant to the original location of the 'families of Irem' referred to in Kawa IX are—at the least—of approximately equal validity. So far as Kawa IX is concerned, Irem could have lain in Upper Nubia, in the deserts on its east, or somewhere in the general region of the northern Butana. Second, a *weak* case can be made for linking the 'families of Irem' to the *Rhrhs*, suggesting a location for Irem approximately in the northern Butana.

The Meroitic graffiti at Kawa

The second datum which Priese finds particularly significant for the location of Irem consists of five Meroitic graffiti²⁵ inscribed upon the walls of Temple T at Kawa.

²² Kawa IX, cols. 5–9, 22–32.

²³ *Ibid.*, col. 25; cf. Macadam, *op. cit.* 55.

²⁴ This would be expected, for example, in the fully preserved section Kawa IX, cols. 35–43, when the king—fresh from the *Rhrhs* victory—presents gifts to Amun of Napata.

²⁵ Macadam, *op. cit.* 103 (no. 31), 104 (no. 36), 105 (no. 41), 107 (no. 49), 115 (no. 115); Priese, 'Irame', 9–10. Other examples of Irem may occur in other Meroitic texts and in Egyptian texts of the Ptolemaic Period (cf. Priese, 'Irame', 10–12, 26–8), but none is directly relevant to the problem of Irem's location.

Each graffito appears to refer to a different individual, but all seem to come from a region called *Arme* or *Armi*, which Priese believes are forms of the name Irem. Each one of the five graffiti is of a type which refers to the adoration of the temple's god by the individuals named,²⁶ a type which is typical of at least 50 per cent of the approximately 107 Meroitic graffiti found in the temple of Kawa. Priese concludes from these circumstances that: 'Die Bewohner von 'rm haben nämlich offenbar noch in meroitischer Zeit in Kawa ihr Kultzentrum gesehen',²⁷ the implication being that Irem itself, like Kawa, must have been in Upper Nubia.

This is not a compelling reason for locating Irem in Upper Nubia, because different, equally valid, interpretations can be made of these graffiti. First, since Meroitic is not yet fully deciphered, it is possible that the graffiti are not referring to Irem.²⁸ Second, even if we assume the references are to Irem, it does not necessarily follow that Irem included Kawa or was located in Upper Nubia. In Meroitic times Upper Nubia was a corridor linking the Butana and the Berber-Shendi reach to Lower Nubia and ultimately Egypt; it must have been frequently traversed by groups or individuals coming from further south or north on official or private business. Such travellers were likely to visit important temples along their route and commemorate these visits in dedicatory texts of graffiti. During Meroitic times, Philae was noted as 'a place of pilgrimage alike for all classes and all nationalities; Greeks, Romans, Egyptians, Meroites and desert nomads',²⁹ and other temples, while less popular, would surely have been affected by the same custom. Indeed, a demotic prayer for a safe return to Meroe was found at the temple of Dakka,³⁰ and it has recently been noted that a significant minority of the dedicatory demotic graffiti left at Philae especially, but also at Lower Nubian temples (on behalf of both Egyptians and Meroites), were probably due to travellers and pilgrims.³¹

Some of the Kawa graffiti may then have been left by visitors from elsewhere, and indeed regions other than Irem are referred to in them. One graffito was on behalf of a man from 'Tape' (alternatively perhaps to be translated as 'the Bowman'), another for a man from 'Nale'(?); why should not these indicate that Kawa was in 'Tape' or 'Nale' (?) rather than Irem? Another graffito—not of the standard adoration type and untranslated—refers to Meroe and hints at visitors from that region.³² In short, once it is recognized that there is a good probability that travellers visited Kawa temple in Meroitic times the five graffiti referring to Irem can be interpreted in two equally

²⁶ Macadam, *op. cit.* 94–5.

²⁷ Priese, 'Irame', 9.

²⁸ Priese (*ibid.* 10–12), however, does effectively dispose of Macadam's theory, *Allen Memorial Art Museum Bulletin* 23 (1966), 66, that the *Arme/Armi* of the Kawa graffiti is to be translated 'Rome', i.e. the Roman section of Lower Nubia.

²⁹ W. Y. Adams, *Nubia, Corridor to Africa* (London, 1977), 338; cf. also V. Monneret de Villard, *Storia della Nubia Cristiana* (Roma, 1938), 19–22.

³⁰ F. Ll. Griffith, *Meroitic Inscriptions*, II (London, 1912), 25; on visitors from Meroe to Philae, see especially p. 45. See also *idem.*, *Catalogue of the Demotic Graffiti of the Dodecaschoenus* (Oxford, 1937); inscriptions Ph[ilae] 410, pp. 112–13, left by Meroitic agents visiting the temple, and also Ph[ilae] 411, pp. 113–14, and especially Ph[ilae] 416, pp. 114–19, praying that the petitioner may be 'taken (back) to Meroe'.

³¹ A. Burkhardt, in Endesfelder *et al.* (eds.), *op. cit.* 99–106 (especially 99–100).

³² For these three graffiti, Macadam, *Temples of Kawa*, I, 105 (no. 42), 106 (no. 48c), 108–9 (no. 57).

valid ways; Irem may have been located in Upper Nubia but, with equal probability, it may have lain outside of it.³³

The 'Kusch-Liste' of Tuthmosis III

The third datum upon which Priese lays particular emphasis is a list of twenty-two toponyms found at the beginning of two of the African toponym lists of Tuthmosis III at Karnak. These twenty-two toponyms open with the word 'Kush', are placed in the same order in both examples, and are followed immediately by the word 'Wawat';³⁴ the 'Kusch-Liste' is assumed to end with the last toponym before Wawat. This so-called 'Kusch-Liste' occurs (only partially preserved) earlier, during the co-reigns of Hatshepsut and Tuthmosis III, and was frequently reproduced—in whole or in part, and usually with the same sequence of toponyms—in later New Kingdom lists of southern lands.³⁵

Irem occupies position no. 11 (actually tenth) on this 'Kusch-Liste', a roughly 'central' position which suggests to Priese that Irem 'etwa in der Mitte des erfassten Teiles des Niltales anzusetzen ist... in der Umgebung von Neu-Dongola'. His reason is his belief that the 'Kusch-Liste' is 'eine Aufzählung von Ländern im Niltal zwischen Semna und Hagar el Merwa'.³⁶ Priese's reasons for this belief, however, are not compelling.

First, Priese notes that the fourth entry on the 'Kusch-Liste' is *Mrkr*, identical with one of the 'lands' given by Irike-Amonate to the temple of Kawa.³⁷ *Mrkr*, Priese implies, must therefore have included Kawa or, at least, have been also located in Upper Nubia; but, as I have shown above, Kawa IX does not provide a compelling reason to locate any of the 'lands' given to the temples (Kawa and Pnubs) in Upper Nubia. Second, Priese argues that five of the other toponyms of the 'Kusch-Liste' can be identified with ancient or modern place-names certainly located in Upper Nubia. These include one near the beginning of the list (no. 2) and one near the end (no. 21), the implication being that the others, in between these two and including Irem, must also lie in Upper Nubia.³⁸

However, Priese's identifications are by no means certain.³⁹ It is true that some modern place-names in Nubia are of demonstrably very ancient origin; modern Sai and Adaye, for example, can be traced through Meroitic and Christian period references to the ancient names *Šꜣt* (Middle Kingdom) and *ḥwt Tiy* (New Kingdom).⁴⁰ Priese, however, can cite no similar chains of evidence for the five

³³ Alternatively, it might be suggested that the Iremites of the Kawa graffiti were descendants of the 'families of Irem' given to the temple by Irike-Amonate, in which case the graffiti would not be relevant to the location of Irem itself. The graffiti, however, are at least 400 years more recent than the reign of Irike-Amonate (Priese, 'Irame', 11) and it seems most unlikely that a group of temple servants would maintain their original ethnic identity over such a long period.

³⁴ *Urk.* IV, 796.1-797.23. Despite the numbering there are in fact only twenty-two toponyms, no. 6 being non-existent.

³⁵ O'Connor in Clark, op. cit. 932, fig. 12.29.

³⁷ Ibid. 9 and 17.

³⁹ No. 2 *itr*/modern Atiri, no. 15 *grb*/modern Kulb, no. 8 *srnyk*/modern Sedeinga, no. 4 *miw*/ancient (E)meae or (E)meeum near modern Firka, and no. 21 *qdi*/ancient Karoy, in the region of modern Abu Hamed.

⁴⁰ *Šꜣt*, Zibelius, op. cit. 154-5; Adaye/*ḥwt Tiy*, Griffith, *Meroitic Inscriptions*, II, 8.

³⁶ Priese, 'Irame', 17.

³⁸ Ibid. 17.

toponyms with which he deals, and a quite different location has been suggested for one of them; whereas Priese places Miu (no. 4) near Firka in Upper Nubia, Kemp notes evidence suggesting that 'The important kingdom of Miu should be located in the Berber-Shendi area'.⁴¹

Given these difficulties, three equally valid interpretations can be made of the 'Kusch-Liste', assuming that it is in some way geographically related to Upper Nubia. 'Kush' may be a rubric introducing twenty-one sub-regions of Upper Nubia, in which case Irem would be located in that region. The 'Kusch-Liste' may cover both Upper Nubia and regions further south,⁴² in which case Irem could lie in Upper Nubia *or* further south. Finally, 'Kush' may refer to the whole of Upper Nubia—as it usually does in the New Kingdom—in which case all the remaining twenty-one toponyms (including Irem) would be located outside Upper Nubia and generally to its south.

At this point another equivalence between Irem and a modern place-name suggested by Priese should be noted. Priese suggests that the name Kerma may consist of the Nubian word for 'house' combined with the word Irem, in which case Irem would include Kerma ('the house of Irem') and of course be in Upper Nubia.⁴³ Priese makes the suggestion cautiously and indeed there is little to support it, for it requires us to create a word—*kirm* or similar—which is not found in any relevant ancient text.

The reference to Irem in the Annals of Tuthmosis III

The fourth datum which appears to be particularly important for Priese in establishing Irem's location is a passage in the Annals of Tuthmosis III which states that in year 34 the *b'kw* sent from Kush (Upper Nubia) consisted of gold, sixty Nehasyu as [male and female servants], four sons of the ruler (*wr*) of Irem, long- and [short-horned] cattle, bulls, [ships] laden with ivory, ebony, and 'all the products of this land' and the harvest of Kush.⁴⁴ Logically, there appear to be two alternative ways of explaining the presence of the four sons of Irem's ruler in the *b'kw* or 'impost' of Kush:

(i) Irem lay outside Upper Nubia, but was either obliged or wished to send the four men to Egypt. Perhaps there had been an Egyptian attack upon Irem, and the men were prisoners,⁴⁵ or Irem was under Egyptian domination, and the men were hostages for its continuing good behaviour; or Irem was independent of Egypt and in contact with it, and wished its ruler's sons to be educated at the Egyptian court.⁴⁶

⁴¹ Kemp in Garnsey and Whittaker (eds.), *op. cit.* 290 n. 68.

⁴² It is noteworthy that the full African lists of Tuthmosis III seem to contain no segment other than the 'Kusch-Liste' that could refer to the Berber-Shendi reach and the Butana. No. 24, 'Wawat', introduces a group of toponyms which are not associated with Upper Nubia in any known source, and thereafter the list seems to move off to Punt (no. 48), Medja (no. 78), and even Tjehenu (no. 88) and to toponyms presumably geographically close to these three areas.

⁴³ Priese, 'Irame', 41; followed by Trigger, *op. cit.* 57.

⁴⁴ *Urk.* IV, 708.9-709.3.

⁴⁵ Suggested by E. Zhylarz, *Kush* 6 (1958), 11.

⁴⁶ The latter two possibilities noted in Priese, 'Irame', 18, but in the belief that Irem was in Upper Nubia. Störk, *op. cit.* 262-3, however, suggests the four men were hostages from a region outside Upper Nubia.

(ii) Irem lay in Upper Nubia. The four sons of its ruler might be sent to Egypt both as hostages and as trainees who would eventually return and fill positions in the administration of Nubia—a well-attested phenomenon during the New Kingdom in Wawat and presumably therefore in Kush.⁴⁷ Alternatively, Irem may have rebelled, and the sons of its ruler might be in fact prisoners of war. Or, finally, the future of these four men might lie in permanent assignment to the retinue of the Egyptian king, the fate of other selected groups of male Nehasyu recorded in the Annals.⁴⁸

Of these various possibilities, Priese believes the most likely is that Irem did lie in Upper Nubia, because the four men in question are included in the *b:kw* of Kush and this comes from the Upper Nubian province itself; and he further suggests that the sons are indeed hostages being sent to Egypt to be educated. Again, his reasons for these conclusions are not compelling. He argues that Irem must be under direct Egyptian control and hence in Upper Nubia, because *b:kw* is paid only by regions which are in this political condition; independent or semi-independent powers (specifically those of Western Asia, including Syria–Palestine) send *inw*, not *b:kw*. Priese is probably incorrect in attributing such precise political significance to the two terms,⁴⁹ but in any case the relevant passage does not state directly that Irem was subject to the *b:kw*, only that the sons of its ruler, in one particular year, were included in that *b:kw*. The *b:kw* of Kush regularly included materials such as ebony and ivory which were produced not by Upper Nubia but regions further to the south.⁵⁰ Therefore, conceivably some of the Nehasyu servants, and the cattle which were regular items in the *b:kw* of Kush, could also have come from further south and so, by analogy, might the four sons of Irem's ruler.

Moreover, nowhere else in Egyptian references to Nubia is the dispatching of such hostages singled out for specific reference, so why should an exception be made in this particular case? Indeed, the four men in question occupy a peculiar position for such high-ranking hostages, for they are listed *after* the gold and the servants of the

⁴⁷ Suggested by Priese, 'Irame', 19; on this phenomenon, T. Säve-Söderbergh, *Ägypten und Nubien* (Lund, 1941), 184–6, and the more recent literature cited in O'Connor in Clark, *op. cit.* 970, as well as S. I. Hodjache and O. D. Berlev in Endesfelder *et al.* (eds.), *op. cit.* 183–8.

⁴⁸ Cf. *Urk.* IV, 695.12, 728.2 (from Kush), and 703.7 (from Wawat, and probably for the *šmsw* or royal retinue). I do not agree with Priese that these individuals were necessarily of high rank amongst the Nehasyu and sent as hostages ('Irame', 19); rather, one thinks of the Nehasyu included in tribute because of their striking appearance, mentioned specifically in P. Koller; cf. Priese, 'Irame', 23 and references cited there.

⁴⁹ Cf. the fairly frequent use of the verb *b:k* and the nouns *b:kw* and *b:k(w)t* in connection with the 'tribute' sent to Eighteenth Dynasty Egypt by states such as Khatti and Babylon as well as the city-states of Syria–Palestine; examples are quoted in D. Lorton, *The Juridical Terminology of International Relations in Egyptian Texts through Dyn. XVIII* (Baltimore and London, 1974), 90–105. For Priese's argument, see 'Irame', 18–19; see also E. Bleiberg, *JSSSEA* 11 (1981), 107–10.

⁵⁰ The contents of the Kushite *b:kw* are conveniently summarized in Säve-Söderbergh, *op. cit.* 210–11, 218–20, 223–7. That the trees producing ebony grew in Upper Nubia is most unlikely (on the distribution of *Dalbergia melanoxylon*, the source of Egypt's ebony, see Kitchen, *Orientalia* 40 (1971), 187), and there is no evidence that the elephant lived in Upper Nubia during historic times; the ivory used for artefacts found in this region was derived from trade with regions farther south, and the occasional depictions of the elephants (e.g. at Kerma in the Second Intermediate Period, cf. G. A. Reisner, *Excavations at Kerma*, pts. IV–V (Cambridge, Mass., 1923), 265 and 267) in Upper Nubia no more mean that elephants actually lived there than the painted Meroitic pottery of Lower Nubia means that giraffes lived in that region! (Cf. C. L. Woolley and D. Randall MacIver, *Karanog*, Plates (Philadelphia, 1910), pls. 41–3.)

b:kw, whereas when high-ranking prisoners or hostages from Asia are mentioned in the Annals, they nearly always precede all other items listed.⁵¹ The implication is that the four sons of Irem's ruler have an inferior status, more in keeping with disgraced prisoners (after a rebellion in Upper Nubia *or* a conflict outside it) than distinguished hostages.

There are, in fact, slight indications that the four men might be prisoners of war produced by Egyptian activity to the south of Upper Nubia. It is notable that the specific amounts of gold, servants, and cattle annually dispatched from Wawat during the years covered in the Annals adhere fairly closely to the average amounts for each that can be established by reference to the surviving figures. The amounts of gold, servants, and cattle included in the Kushite *b:kw*, however, fluctuate more markedly above and below the average. Two periods of markedly high returns are to be noted for Kush: first, for gold (years 33, 34), cattle (years 31/2, 33), and servants (years 33, 34); second, for gold (year 41) and for servants (year 39).⁵² Of course, internal factors of unknown nature within Upper Nubia could have been responsible for these above-average figures. However, one could suggest they reflect two periods (years 31/2–34, years 39–41) when Egyptian forces were particularly active to the south of Upper Nubia, securing booty which abnormally increased some items of the *b:kw* of Kush during those years. The campaigns were led not by the king—who was personally preoccupied by large-scale military activity in Asia—but by his lieutenants, and the plunder was simply incorporated into the annual *b:kw* instead of being given special prominence. In this context, the four sons of Irem's ruler would then be disgraced prisoners of war⁵³ coming from a region—Irem—outside Upper Nubia.

However, this speculative argument should not be urged too strongly, for there is no independent proof for such campaigns and the fluctuations in the amounts of individual items are not totally consistent within the two periods suggested (e.g. gold increases in years 33 and 34, while cattle are more numerous in years 31/2 and 33, but not 34, when they drop below average). Nevertheless, the preceding arguments do show that because of the laconic nature of the text *all* interpretative theories about the significance of the occurrence of the four sons of Irem's ruler in the Annals are equally speculative. The Annals therefore do not contribute significantly to solving the problem of Irem's location.

The Campaign of Seti I in Irem

The two surviving records (Amarah, Sai) of this campaign show that it offers significant indications about the location of Irem, which do not suit an Upper Nubian location. Unfortunately, the texts were not fully published at the time of

⁵¹ *Urk.* IV, 665.6 ff., 669.1 ff., 690.6–10, 698.4–8.

⁵² For the figures, Säve-Söderbergh, *op. cit.* 210–11, 224, 226–7. Note also that at Hagar el Merwa there is a graffito of year 35 of Tuthmosis III(?), perhaps another indication of Egyptian campaigning in the south at this time; cf. Vercoutter, *Kush* 4 (1956), 68 ff., and Störk, *op. cit.* 256–7.

⁵³ Like the son of a 'ruler of vile Kush' captured during a campaign of Tuthmosis II during the subjugation of Upper Nubia; *Urk.* IV, 140.12.

Priese's article and he was therefore unable to analyse them in detail. However, in view of the conclusions he had reached from other data Priese believed that Seti had suppressed 'gelegentliche Aufstände' in Upper Nubia.⁵⁴ The relevant texts have now been published⁵⁵ and two commentators have noted that they indicate clearly that at least part⁵⁶ and perhaps all of Irem⁵⁷ lay outside Upper Nubia in a desert or semi-desert environment.

Kitchen suggests that the Egyptian army which Seti sent to foil Irem's rebellious plans left the Nile in the Third Cataract region and moved south-west into the Wadi el Qa'ab, which has wells and in recent times supported an appreciable population of cattle-raisers.⁵⁸ This conclusion fits the desert location of the campaign and also Kitchen's belief that the campaign involved a round trip of seven days,⁵⁹ averaging 12-15 miles per day. Vercoutter, however, believes that the text in itself does not give any certain indication about the location of Irem, for several reasons. Desert environments, including wells, can be found throughout 'tout l'arrière-pays au Sud et au Sud-Ouest du Nil, entre III^e et IV^e Cataractes' and Wadi el Qa'ab is therefore only one of several possible locations. Moreover, the Egyptian forces may have left the valley at a point *not* in Upper Nubia and the seven days which the campaign took are not a good indication of how far away from the Nile the Egyptians went, because before they began the campaign they had already reached a fortress which was located (at an unknown distance) outside the Nile valley.

However, Kitchen does satisfactorily establish that the Egyptian forces 'went up (into the desert)' *after* they reached the fortress mentioned;⁶⁰ and there is in any case no good reason to locate this fortress in the desert. Vercoutter's sole reason for doing so is the presence of the club determinative in the name of the fortress, which is partially illegible and restored by Kitchen as *Sgr[h]-T'wy*, 'Pacifying the Two Lands'. However, it is possible that the name could be restored rather as 'Pacifying the Rebellious Lands'⁶¹ or even, as Vercoutter suggests, 'Pacifying the Nehasy Lands'.⁶² On the other hand, Vercoutter is correct in pointing out that the place of departure from the valley is nowhere explicitly stated; it was probably in Upper Nubia, but not necessarily in the Third Cataract region as suggested by Kitchen. The actual route taken by the Egyptians therefore remains uncertain. Nor is it necessary to assume, as Kitchen does, that the campaign involved a round trip

⁵⁴ Priese, 'Irame', 22.

⁵⁵ *KRI* I, 102-4.

⁵⁶ Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 217-18.

⁵⁷ Vercoutter, *MIFAO* CIV, 161-3. The principal reasons cited by both scholars for a desert location for the campaign are the use of chariotry by the Egyptians (*KRI* I, 103.8), the capture of six wells during the campaign (*KRI* I, 103.11-16), and the bringing of the prisoners and cattle captured to the river bank (*KRI* I, 103.15-16). Vercoutter also argues that the terms applied to the attack planned by Irem—the *casus belli*—and the description of its repulse show that Irem was penetrating into Egyptian territory and therefore lay outside Upper Nubia; cf. Vercoutter, *MIFAO* CIV, 161.

⁵⁸ A. E. W. Gleichen (ed.), *The Anglo-Egyptian Sudan*, 1 (London, 1905), 204-6; K. M. Barbour, *The Republic of the Sudan. A Regional Geography* (London, 1961), 132, 138-9.

⁵⁹ Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 218.

⁶⁰ Kitchen, *ibid.* 217 and n. 19, citing a good parallel from the Kubban stele of Ramesses II which overrides Vercoutter's objections (Vercoutter, *op. cit.* 163) to Kitchen's interpretation of the relevant phrase.

⁶¹ Taking 𓆎 as a misreading of the hieratic writing of 𓆎; cf. *sbi*, 'rebel'.

⁶² Vercoutter, *MIFAO* CIV, 162 and n. 3.

of seven days. The relevant passage could be interpreted as meaning that the suppression of the Irem-people took seven days to complete (during which a series of wells were captured, perhaps on successive days), with a further period of time being needed to return to the river. Kitchen also suggests that the round trip covered about 100 miles, but this assumes a marching rate of 12–15 miles per day.⁶³ While this is probably an appropriate speed for the large and complex forces that Tuthmosis III and Ramesses II (both cited by Kitchen) led into Canaan and Syria, the force sent against Irem was certainly smaller and perhaps organized for rapid movement. Even though it contained infantry as well as chariotry, such a select force might have covered well over 20 miles a day.⁶⁴ In sum, we do not know the point of departure from the valley, the route taken, or the depth of penetration into the desert.

In the circumstances, several routes and destinations would seem to have approximately equal validity:⁶⁵

- (i) Starting in the Third Cataract region or thereabouts, the Egyptians moved south-west into the Wadi el Qa'ab, where the rebels were suppressed.
- (ii) Starting further south in Upper Nubia, the Egyptians moved south-west along the Wadi Melh towards (but not into) Kordofan and Darfur.
- (iii) Starting from Napata, the Egyptians moved south-east along one of the well-known routes across the Bayuda to the region of Meroe (about 150 miles one way) or, further north, to modern Berber (about 100 miles one way). Whether the Egyptians actually reached Meroe or Berber is a moot point; the campaign may have been concluded at some place along one of the routes, rather than at their terminations.

So far as the relevant text alone is concerned, all three possibilities seem equally possible; but it is clear from the preceding discussion that at least some of Irem lay outside Upper Nubia.

The Punt reliefs of Hatshepsut

This is the last datum significant for the location of Irem. Part of the pictorial and textual narrative contained by the Punt reliefs of Hatshepsut's temple at Deir el Bahari is made up of three elements (fig. 3, VIII–IX); on the right a representation of Hatshepsut's names (IX), in the centre a text in vertical columns (IX), and on the left two horizontal registers. The lower one of these depicts a procession moving towards the royal names and carrying various items, as well as driving or leading baboons and short-horned cattle; the procession culminates in four figures, prostrating themselves before the queen's names and identified as the 'great ones (*wrw*) of Punt' (VIII a). Most of the upper register has been lost, but sufficient

⁶³ Kitchen in Endesfelder *et al.* (eds.), op. cit. 218.

⁶⁴ Cf. the estimates for the mileage covered by such selected, highly mobile forces, as well as an entire army, given in D. W. Engels, *Alexander the Great and the Logistics of the Macedonian Army* (Berkeley, 1978), 153–6.

⁶⁵ While theoretically possible, the important desert route linking Kawa to Napata—the Meheila road—has been excluded, on the assumption that Egyptian control of Upper Nubia would make it difficult for hostile forces to occupy this particular route. On all of the desert routes discussed here, see Kemp, op. cit. 26–9 and fig. 3, and Adams, op. cit., index under appropriate name.

survives to show it contained a similar procession culminating in another four men prostrating themselves before the royal names. The lower pair are called 'the great ones of Irem', the upper pair 'the great ones of *Nmy*' (VIII b). The vertical columns of text (in IX) separating these 'great ones' from the royal names refer to the obeisance made to the queen 'by the great ones of Punt, [Irem, *Nmy*], the *Iwntyw Sty* of Khenethennefer and all foreign countries south of [Egypt]'.⁶⁶

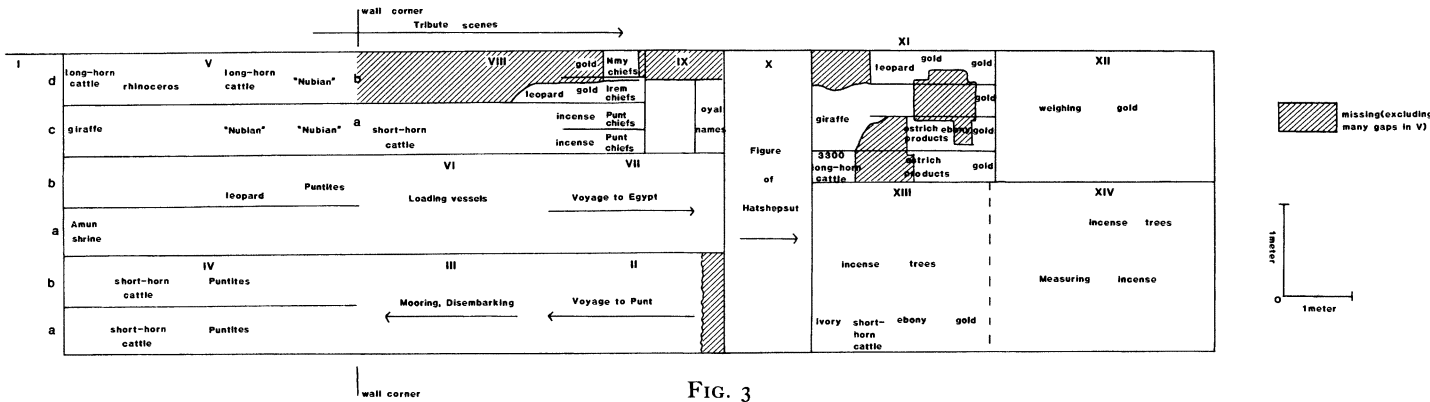


FIG. 3

This conjunction of Punt, Irem, and *Nmy*, has suggested to several scholars that Irem was located close to Punt,⁶⁷ but Priese firmly rejects this idea.⁶⁸ For him, Punt is not geographically close to Irem and the depiction of the tribute of Irem is not connected with the Punt expedition. The reasoning which leads Priese to these conclusions is as follows. First, in an adjoining scene (fig. 3, x-xiv) depicting Hatshepsut presenting southern products to Amun at Thebes, the products of Punt are placed in the lower register (fig. 3, XIII-XIV), while the upper register (XI-XII) deals with non-Puntite products, specifically in fact the tribute of southern foreign lands, Kush and Nehasy land. Priese infers that since VIII b (tribute of Irem and *Nmy*) is also above that of Punt (VIII a), VIII b must be equated with XI-XII; and argues that the regional names linked to the tribute depicted in XI-XII most probably 'vor allem auf das ägyptische Herrschaftsgebiet im Nordsudan beziehen'. Therefore, he implies, Irem must also lie within the 'Herrschaftsgebiet', i.e. probably in Upper Nubia, and certainly not south of it, since direct Egyptian control never extended south of the Fourth Cataract. Even on its own terms, however, this argument is unconvincing. It is true that in XI-XIV the products of Punt are clearly differentiated from those of other southern lands, but the latter are not so

⁶⁶ E. Naville, *The Temple of Deir el Bahari*, III (London, 1896-7), pl. lxxvi; *Urk.* IV, 331.3-8. The restoration 'Egypt' is suggested by Sethe, but not that of 'Irem, *Nmy*'; however, in view of the immediately adjacent scene depicting the events described in the vertical columns and carried out by the 'great ones' of Punt, Irem, and *Nmy* the restoration seems the most reasonable.

⁶⁷ Priese, 'Irame', 12-14 cites Chabas, Mariette, and Müller. To these citations he could usefully have added J. Krall's careful study of the question, in which he concluded that the Deir el Bahari material showed that the people of Irem were neighbours of the land of Punt, *Das Land Punt* (*SÖAW*, Phil.-Hist. Klasse, 121) (Vienna, 1890) 19-21, 77. Cf. also the similar conclusion of Störk, op. cit. 262-4.

⁶⁸ For Priese's discussion, quoted extensively below, see 'Irame', 19-22.

specifically described that they must be restricted solely to regions under direct Egyptian control. The latter inference is reasonable for 'Kush', but the more generalized references to 'southern foreign countries' and 'Nehasy land' could clearly include areas outside permanent Egyptian control with which the Egyptians traded or which they raided. Indeed, Priese contradicts his own argument, by suggesting that *Nmy* (which is equally—on his reasoning—to be equated with the regions related to XI–XII) actually represents an 'Element der Völker, die im Sudan ausserhalb des direkten ägyptischen Machtbereiches lebten'.

The tribute depicted in x–xiv is probably best interpreted as a recapitulation or summary of all the southern 'tribute' (i.e. enforced dues, plunder, and trade goods) secured throughout the first ten years of Hatshepsut's reign. It is likely that the Puntite products of XIII–xiv are those secured as a result of the Punt expedition of year 9, and that the products depicted in xi include some obtained from Irem and *Nmy* in the same years (on this, see further below). For example, xi includes a 'live leopard of *šmꜣw* [*sic!*] brought to her majesty from the [southern] foreign countries',⁶⁹ which suggestively recalls the live leopard depicted as part of the tribute of Irem and *Nmy* in VIII b. More generally, however, the tribute depicted in XI–XII seems to have come from a very wide geographical area which included Upper Nubia (referred to as 'Kush' in x), Lower Nubia or Wawat (gold is assigned great prominence, especially in XII, and most gold came from Wawat),⁷⁰ and the eastern and western deserts (ostrich eggs and feathers depicted in xi). Moreover, in at least some cases the tribute must represent totals of materials collected over many years. For example, xi includes '3,300' long-horned cattle, an enormous number hardly likely to be collected as a result of a single raid or a single payment of annual tribute. For example, some ten to twenty years later, the annual tribute in cattle of Lower and Upper Nubia combined averaged only about 450 head.⁷¹ In the context of the decorative scheme of Deir el Bahari itself the tribute scenes of x–xiv probably refer back not only to the contacts with Punt, Irem, and *Nmy* of VIII a and b, but to earlier contacts with southern lands described in the southern half of the *Lower Colonnade*.⁷² At the southern end of the latter is a depiction of southern foreign lands (the 'Kusch-liste' of Tuthmosis III) being led as prisoners to Hatshepsut; and above this we should probably restore a fragmentary text describing an early campaign of Hatshepsut's against southern foreign lands.⁷³ It is also important to note that Hatshepsut's predecessors had already secured permanent control over the

⁶⁹ *Urk.* IV, 336.17.

⁷⁰ Säve-Söderbergh, *Ägypten und Nubien*, 210 ff.; Vercoutter, *Kush* 7 (1959), 130.

⁷¹ Calculated on the basis of the figures for years 31/2, 33, and 38 of Tuthmosis III; cf. Säve-Söderbergh, *Ägypten und Nubien*, 224.

⁷² E. Naville, *The Temple of Deir el Bahari*, VI (London, 1908), pl. clii.

⁷³ Discussed in Redford, *History and Chronology of the Eighteenth Dynasty* (Toronto, 1967), 58–9. The fragments were collected by Naville from the debris along the Lower Colonnade; Naville, *op. cit.*, VI, 8 and pl. clxv. They would appropriately be illustrated by a captive scene, hence the suggested restoration above the scene of the captured lands. That the Nubian wars referred to in the Lower Colonnade occurred early in the reign is suggested by the adjoining scenes (Naville, *op. cit.*, VI, pls. xliii–clvi) dealing with Hatshepsut's 'earlier' pair of obelisks. Cf. P. Barguet, *Le Temple d'Amon-Rê à Karnak* (Cairo, 1962), 100 n. 1; L. Habachi, *The Obelisks of Egypt* (New York, 1977), 60.

Abri-Delgo reach of Upper Nubia and had established some form of domination over the Dongola reach.⁷⁴ From the beginning of her reign, Hatshepsut therefore could well have been receiving tribute from both Wawat and (at least part of) Kush and was well situated to trade with or raid regions beyond these.

A second argument of Priese should also be noted. In ix there is a reference to the obeisance of the *Iwntyw Sty* of Khenethennefer, a term which in this context could be interpreted as referring to Irem, as Priese in fact concludes. However, his further observation that Khenethennefer is amongst those regional names applicable only to areas under direct Egyptian control (and therefore that Irem must lie within these) is not necessarily correct. The geographical significance of this term is not yet securely established.⁷⁵ Priese's conclusion that Irem appears in the Punt reliefs as the representative of 'die nubische Provinz' is therefore not based on very secure arguments. More importantly, his conclusions that Punt was not near Irem, and that the Punt expedition had no direct connection with any contact between Egypt and Irem, appear to me to be directly contradicted by the evidence of the narrative of the Punt expedition itself.

This narrative can be analysed in the following way. First, it is definitively marked off from preceding and succeeding material. It is preceded by a long text (fig. 3, 1) which apparently recapitulated the events of the expedition, laying particular emphasis upon the statues of Amun and the queen which were set up in Punt.⁷⁶ The narrative is followed by x-xiv, the presentation of southern products at Karnak, an incident opened by a figure of Hatshepsut which spans the entire height of the decorated registers and has its back turned to the Punt narrative of II-VIII. Pictorially, the narrative is marked off from 1 and x by wide, boldly marked vertical dividers. Within these dividers, however, are a set of horizontal registers, uninterrupted by vertical dividers, through which the narrative of the Punt expedition flows by means of a clear sequence of skilfully interlinked events which establishes 'the complete unity of the whole representation'⁷⁷ and which has the 'factual nature' of an 'accurately depicted record'.⁷⁸ In other words, II-VIII consists of a self-contained,

⁷⁴ This conclusion is based on evidence suggesting that Tuthmosis I occupied and fortified the Abri-Delgo reach, specifically the location of his Tombos inscription near the Third Cataract (*Urk.* iv, 82-6), a large stela of his(?) associated with a fort on Sai island (Vercoutter *et al.*, *Études sur l'Égypte et le Soudan anciens* (CRIPEL 1), 28-9) and retrospective comments under Tuthmosis II (*Urk.* iv, 138.15-139.7). Tuthmosis I also appears to have traversed the Dongola reach (he had a frontier stela inscribed near Kurgus, well upstream of the Fourth Cataract; see A. J. Arkell, *JEA* 36 (1950), 36-9, and Vercoutter, *Kush* 4 (1956), 68-70) and to have broken its political integrity by dividing it up into five chieftainships (*Urk.* iv, 139.4-7).

⁷⁵ C. Vandersleyen, *Les Guerres d'Amosis* (Brussels, 1971), 64-8, equates Khenethennefer with Upper Nubia, but this is only certain in 'some' of the cited examples. Vandersleyen's observation that in geographical lists Khenethennefer is sometimes placed in apposition to foreign regions of the extreme north (ibid. 65 n. 2) suggests that the term could apply to the extreme south, i.e. regions south of Kush/Upper Nubia. If H. Goedicke (*Kush* 13 (1965), 111) is correct in his conclusion that Khenethennefer applied to regions beyond Egyptian administrative control but subject to Egyptian domination then it is likely that the term could be applied to regions beyond Upper Nubia.

⁷⁶ K. Sethe, *ZÄS* 42, 91-100; *Urk.* iv, 316.15-321.17.

⁷⁷ G. A. Gaballa, *Narrative in Egyptian Art* (Mainz, 1976), 53.

⁷⁸ W. S. Smith, *Interconnections in the Ancient Near East* (New Haven, 1965), 138. For Good commentaries on the Punt expedition narrative, see Gaballa, *op. cit.* 50-3; W. S. Smith, *op. cit.* 137-9; texts, see *Urk.* iv, 322-333.12.

uninterrupted narrative; it begins in the lowest registers (II–IV), moves upward through V, continues on through VI–VII, and concludes with VIII–IX. Second, the narrative itself indicates very strongly that Egyptian contact with Irem and Nmy—on this occasion—was directly related to the expedition to Punt.⁷⁹ The sequence of events runs as follows. The expedition sails south to Punt (II) and disembarks there (III); its members are greeted obsequiously by the ruler of Punt and his retinue, which at the same time shrewdly assesses the trade goods displayed on the shore by the Egyptians (IV a). In this same locale the Puntites then bring various regional products to the Egyptians, thus completing the trading process which is shortly to be celebrated at a banquet of (to the Puntites) exotic Egyptian foods, set out in a nearby tent or booth (IV b).

The expedition then enters upon a second phase of activity, the direct collection of items for transportation back to Egypt; accompanied by Puntites, the Egyptians gather *ꜥntyw*-incense, uproot and basket young *ꜥntyw*-incense trees, cut ebony, and capture or acquire wild animals.⁸⁰ As Kitchen has observed, these activities probably took the Egyptians far inland from coastal Punt; weather conditions (necessitating at least a two-to-three-month stay in Punt until winds shifted to the north and facilitated the homeward journey) and the information provided in section V of the narrative indicate a long stay and a journey inland that may have penetrated as deep as 250 miles.⁸¹ On the basis of Kitchen's suggestions, I would make several additional observations. First, the four registers of section V form a kind of rough 'map', depicting two significantly different geographical zones. The 'map' begins at the sea-shore (depicted in V a) of Punt itself, this locale being fixed by the presence in V a (left) of the shrine dedicated to Amun and Hatshepsut, set up 'in the midst of the terraces of *ꜥntyw*-incense of Punt'.⁸² The 'map' then moves westward and far inland, for in V c (left) there occurs a wild giraffe (i.e. not a captive animal), which would almost certainly not have been found in the coastal regions but rather in the semi-desert and savannah lands west of the coastal ranges. A rhinoceros, depicted in V d (left), is also an unlikely animal for a coastal region on the Red Sea shore of the Sudan or Ethiopia.⁸³ There are other consistently maintained differences between

⁷⁹ For an initial analysis of the material along the lines followed here, see O'Connor in Clark, *op. cit.* 935–9.

⁸⁰ *ꜥntyw*-incense gathering and *ꜥntyw*-trees: fig. 3, V a and b, and *Urk.* IV, 327.3–4, 6, and 328.3–7; ebony cut or carried away: fig. 3, V b, c, and d, and *Urk.* IV, 326.17 and 327.1; 'trapped' leopard: fig. 3, V b; and leashed animal: fig. 3, V d.

⁸¹ Kitchen, *Orientalia* 40 (1971), 202–3.

⁸² *Urk.* IV, 319.17.

⁸³ M. Hilzheimer, *ZÄS* 68 (1932), 113–14, asserts that the giraffe would be unable to penetrate the rugged coastal ranges or the Ethiopian highlands and so could not have appeared on the (modern) Sudanese and Ethiopian coasts. Even in less rugged Somalia giraffes still remained about two days' journey inland from the coast; and Punt was probably located well north of Somalia, probably in the general area of Port Sudan–Suakin (Kitchen, *Orientalia* 40 (1971), 184–207; G. Posener in Endesfelder *et al.* (eds.), *op. cit.* 342). Störk, it is true, argues that giraffes could have reached the Sudanese coastline (*op. cit.* 227–8), but cites no specific record of them being observed on the coast. Gleichen's detailed notes (*op. cit.*, I, index under 'giraffe') on the giraffe's distribution in the Sudan make no reference to coastal occurrences of the animal. The northernmost occurrence noted is 'in various parts of the district of Kassala' (*ibid.* 98), marked on the map (*ibid.*, at end) as in those days not extending farther than 17° N. latitude and, of course, located well inland. Elsewhere it is noted that the distribution of the giraffe is 'approximately that of the Elephant' (*ibid.* 308), and the latter is not documented as ranging farther north than the Wadi Gash (or Gash delta?; *ibid.* 307), in any case not farther north than about latitude 16° N. Störk also claims that the rhinoceros could have occurred on the Sudanese coast, but again can

the eastern (coastal) and western (inland) sectors of the 'map'; the cattle of coastal Punt are short-horned (IV a and b), but those in v d are long-horned, while in v c and d occur dark-skinned individuals wearing clothes and decoration different from that of the lighter-skinned Puntites of IV and v a and b.

The second important observation to be made is that the 'map' of section v does not cover Punt only (as Kitchen thought),⁸⁴ but also Irem and *Nmy*. I base this conclusion on the fact that the scenes of the tribute of Punt (VIII a) and Irem and *Nmy* (VIII b) run directly out of v c and d, without any interrupting vertical divider, just as the scene of loading the ships (VI) runs directly out of v a and b. As Smith acutely observed: 'The usual border is omitted at the corner of the walls; nothing breaks the action. The line of men carrying trees in slings on the south wall therefore continues up the gang planks of the ships being loaded. The men with trees and logs in the upper two registers of the south wall [i.e. v c and d] are continued by processions of similar figures on the west wall [i.e. VIII a and b].'⁸⁵ The logical consequence seems inescapable: the forwarding of the tribute of Irem and *Nmy* to Hatshepsut is as much a result of the expedition as the delivery of the Punt tribute itself.

Moreover, whereas IV and v a (and part of b?) are to be equated with Punt, v b, c, and d are to be equated with Irem and *Nmy*. This conclusion is based on the observation that the differentiation of two geographically distinct regions (IV and v a, part of b(?) on the one hand, and v b (part?), c, and d on the other) is subtly but clearly reaffirmed in VIII a and b. Section VIII b (tribute of Irem and *Nmy*) is placed above that of Punt (VIII a), just as the western region of v (b, c, and d) is placed above the eastern (IV, v a, part of b(?)). Moreover, only short-horned cattle are shown in the Puntite tribute of VIII a; perhaps they were balanced by long-horned cattle in the missing part of the Irem/*Nmy* tribute of VIII b. The 'great ones' of *Nmy* at least (those of Irem are defaced; fig. 3, VIII b, right) are of Negroid type never represented amongst the Puntites of IV and v a and b but probably identical with the dark-skinned men of v c and d. Note also that the tribute of Irem and *Nmy* includes a captive leopard, a significant item linking VIII b directly back to v b, in which a leopard (presumably the same one as in VIII b) is shown.⁸⁶ The tributes of Punt and Irem/*Nmy* differ in another significant way, although this is one not reflected in v; for Punt, pictorial emphasis is used to indicate that incense was a chief product (VIII a, right) whereas for Irem and/or *Nmy* gold is the item emphasized (VIII b, right).

cite no specific proof for this (Störk, op. cit. 229 and 503, the latter reference to the route between Berber and the coast, but not necessarily the coast itself); while Gleichen's valuable compendium again makes no reference to coastal occurrences (Gleichen, op. cit., I, 308 and see also index under 'rhinoceros').

⁸⁴ For example, Kitchen assumes that the giraffe is indigenous to Punt (*Orientalia* 40 (1971), 187) and that the gathering activities of the expedition all took place in 'Punt itself' (ibid. 203).

⁸⁵ W. S. Smith, op. cit. 139.

⁸⁶ Naville, *Deir el Bahari*, III, pl. lxx; the leopard in fig. 3, v b is turning back, to bite its foot (hinting at a snare, although none is shown?). This leopard is not mentioned in the list of items being loaded on the ships (VI; cf. *Urk.* IV, 328.17-329.11), a datum inconsistent with my arguments, but not sufficiently strong to outweigh the other indications of definite interrelationships between Punt, Irem, and *Nmy*.

These numerous logical interconnections between IV–V and VIII are interwoven with the narrative proper. According to the latter, after the collecting phase (V) was over the vessels were loaded (VI) and sailed back to Quseir or thereabouts (VII). The expedition then travelled by land and river to Thebes (events described textually, not depicted), at which place the ‘great ones’ of Punt, Irem, and *Nmy* prostrated themselves before the queen’s names and presented their tribute (VIII a, b and IX). That VIII–IX depict and describe the last events of the narrative is indicated both by their position in the uppermost register (the narrative throughout has moved not only from north to south, and south to north as appropriate, but also from lowermost to uppermost registers), and by more specific references. The description of the return trip (VII) states that the expedition arrived at Thebes with ‘the great ones of this foreign country accompanying them’ and in IX the ‘great ones of Punt, [Irem, and *Nmy*]’ are said to perform obeisance at, and bring tribute to, ‘the place where her majesty is’,⁸⁷ i.e. Thebes.

The conclusions reached as a result of the preceding analysis are three.

(i) The penetration of Irem and *Nmy* by Egyptians and the presentation of the products of these regions to Hatshepsut are events which are integral parts of the Punt expedition narrative. They cannot be dissociated from the expedition, as Priese argues.

(ii) The narrative indicates strongly that Punt, Irem, and *Nmy* were contiguous or, at least, in very close proximity to each other. Irem must be located close to Punt, whether the latter is to be located in the Port Sudan–Eritrean border zone or further to the south.

(iii) The narrative makes an Upper Nubian location (or one south-west of this region) for Irem unlikely. Irem and *Nmy* appear to include semi-arid or savannah environments which I doubt existed in Upper Nubia by this period, although the evidence on this point is admittedly not definitive.⁸⁸ However, on logistical grounds it seems improbable that an expedition using the coast of Punt as its point of departure (say, roughly at Port Sudan) could have then travelled as far as Upper Nubia or beyond to its west (to the Wadi el Qa’ab locale suggested by Kitchen for Irem), covering in either case a distance much greater than the maximum hypothesized by Kitchen. And if such a feat had been achieved, it would surely have been referred to in the narrative.⁸⁹

⁸⁷ *Urk.* IV, 329.17–330.2 and 331.9–10. In the former ‘this foreign country’ must cover Irem and *Nmy* as well as Punt, to judge from the evidence of VIII–IX.

⁸⁸ The following data at least suggest modern conditions of aridity for the region of Upper Nubia. In the reign of Ramesses II, Akuyta (Zibelius, *op. cit.* 95–6), east of Lower Nubia, appears to have been quite arid (*KRI* II, 17, 355.1–7; 356.15–357.4). Later, in the Twenty-sixth Dynasty, the hill country of Punt itself—at least at certain times of the year—was characterized by ‘l’extrême aridité’ (Posener in Endesfelder *et al.* (eds.), *op. cit.* 342). For further discussion, O’Connor in Clark, *op. cit.* 925–8.

⁸⁹ Störk has argued that Irem occupied a large desert region east of the Abu Hamed reach, stretching to the Red Sea and north of Punt (*op. cit.* 262–5). His reasoning, however, is not very convincing. First, he claims that Irem must have lain north or east of Miu (a country in which Tuthmosis III suppressed a rebellion) because the four sons of the ruler of Irem were presumably captured in an attack of year 34, whereas Miu was not invaded until year 35. But it is by no means certain that the attack on Miu is to be linked to year 35—Störk only assumes

II. *Nmy*

So far I have treated Irem and *Nmy* together, and I believe the evidence cited does strongly suggest that both lands were contiguous with Punt, or at least comparatively close to it, and therefore lay west of the roughly Port Sudan–Suakin location that is most likely for Punt (see n. 83 above). I have also argued that at least one, if not both, of these regions must have included semi-desert or savannah-type environments, and that the totality of the evidence makes it likely that both lay considerably further south than the region or the latitudes of Upper Nubia. But to what degree can we distinguish *between* the two regions of Irem and *Nmy*? On the basis of the Deir el Bahari material, hardly at all, for no clear distinction between Irem and *Nmy* is provided there, and the toponym *Nmy* occurs nowhere else (except, dubiously, in a topographical list of Seti I).⁹⁰ We might therefore argue that in the circumstances, the actual topographical relationships between Irem and *Nmy* are irrecoverable and irrelevant, and that what is important is the location for Irem suggested by the Deir el Bahari data. However, we should briefly examine a suggestion made by Müller and tentatively supported by Zibelius, that *Nmy* is actually a miswritten form of the more common toponym *ʿm*;⁹¹ should this be the case, do the data on *ʿm* provide any indication of the location of *Nmy* (= *ʿm*) and of its topographical relationship to Irem?

These data have recently been reviewed by Posener who, while reaffirming that *ʿm* was noted particularly for the gold it produced, observed an apparent contradiction in the data relevant to locating *ʿm*. On the one hand, *ʿm* seems accessible from the Nile valley, and specifically from Upper Nubia. It is included with several other southern sources of gold in a list prepared under Ramesses II and here the scribes responsible seem to have linked *ʿm* to the river valley. In fact the graffito of a certain ‘Userhat of *ʿm*’ has been found at Sabu, in the Third Cataract region a little north of Nauri. However, on the other hand, gold from *ʿm* also reached Punt, suggesting that *ʿm* was also accessible from Punt. Yet Punt, in Posener’s view, was located ‘dans la zone côtière de la mer Rouge qui a pour centre approximatif Port Soudan’, quite far away from the Nile valley in general, and very remote from the Third Cataract region in particular. Posener proposed three hypotheses to reconcile these data. *ʿm* may have been a vast territory reaching from Sabu far to the east of the Nile valley; or, *ʿm* was a smaller region located in the eastern desert but accessible from the Third this—and in any case his location of Miu (along the Nile from Abu Hamed to the Fifth Cataract, op. cit. 260) is farther north than that favoured by other scholars (e.g. Kemp in Garnsey and Whittaker (eds.), op. cit. 290 n. 68). Second, he argues that Irem could not be south or south-east of Punt, because then Egypt would not be able to capture or to compel Irem to send hostages to Egypt; this might be true of a southern location (although later Punt itself sent ‘ruler’s children’ to Egypt, under Ramesses III, cf. Säve-Söderbergh, *The Navy of the Eighteenth Egyptian Dynasty* (Uppsala/Leipzig, 1946), 28–9), but a country south-east of Punt might well yield hostages or prisoners, just as later (Nineteenth Dynasty), in fact, Irem supplied visually striking people as part of its tribute to the Egyptian king (P. Koller; Priese, ‘Irame’, 23; also cited by Störk, op. cit. 262). His arguments about the ethnic character of the people of Irem (ibid. 263) are in my opinion not relevant to the discussion, since we cannot reliably reconstruct the distribution of ‘Negroid’ and ‘non-Negroid’ peoples for the period in question.

⁹⁰ Zibelius, op. cit. 139.

⁹¹ Ibid.

Cataract and, implicitly, Punt; or, finally, that ζm itself lay on the Nile but that its gold-producing 'mountain' was some distance away to the east.⁹²

Posener's suggestion that ζm was accessible from both the Nile valley and Punt, on the Red Sea coast, seems to me reasonable, but none of the available data compels us to bring it as far north as the latitude of the Third Cataract. The Sabu graffito was left by an inhabitant of ζm , but he may have been quite far away from ζm itself at the time, perhaps *en route* to or returning from Egypt. Certainly we cannot equate ζm with the ancient gold-producing area of the Third Cataract region, for it is most unlikely that Punt could obtain gold from this (to Punt) remote area, already under Egyptian control by Hatshepsut's reign (see below), to which period the first known references to the 'gold of ζm ' belong.⁹³ The mineral-countries list of Ramesses II, mentioned above, is also ambiguous in its topographical implications. Vercoutter deduced from this list that the 'mountain of ζm ' referred to 'the gold mines situated near the Third Cataract, north and south of Sabu', basing this conclusion on the equivalence he had suggested for the southern gold-producing countries or places cited in this list. The equivalences suggested were as shown in Table I (given in the order in which they occur in the list).⁹⁴

TABLE I

Place	In or near Nile valley	Eastern desert
Napata	Mines near Abu Hamed OR in Fourth Cataract region	
Mountain of ζm	Mines near Third Cataract, in vicinity of Sabu	
Mountain of Kush	Mines near Semna	
Foreign land/desert of Ta Sety	Mines north of Semna	OR some mines in Wadi Allaki OR mines of Wadis Allaki and Gabgaba
Mountain of Khenethennefer	Dittology for Ta Sety OR gold-mines nearer to Lower Nubian valley	
Pure Mountain (Wadi Hammamat)		Wadi Hammamat
Mountain of Edfu		Mines of Baramieh and vicinity
Mountain of Coptos	(Possibly did not refer to gold)	

However, as Vercoutter himself indicates, the precise location of each of the gold-mining areas is hard to infer from this list (see the alternatives he proposes for the sources of the gold of Napata, Ta Sety, and Khenethennefer), and I believe an

⁹² Posener in Endesfelder *et al.* (eds.), op. cit. 337-42.

⁹³ *Ibid.* 337-9. ⁹⁴ Vercoutter, *Kush* 7 (1959), 128-33.

alternative interpretation of the topography underlying the list is legitimate. Although Vercoutter emphasizes that in general the list enumerates the gold-producing lands from south to north, his own analysis indicates that within this generally true framework the list also veers out into the eastern desert. This is natural, since the gold deposits of interest to the Egyptians occurred both in or very close to the Nile valley and also comparatively far out into the eastern desert. If we consider that the graffito at Sabu does not necessarily link ϵm to the Third Cataract, then we could suggest the equivalences shown in Table 2.⁹⁵

TABLE 2

Place	In or near Nile valley	Eastern desert
Napata	Mines in or near Abu Hamed reach, close to Napata and perhaps equivalent to the 'gold of Karoy'* mentioned elsewhere	
ϵm		Mines further east of the above at the same general latitude but considerably deeper into the desert; these approximate to the southern fringe of the gold-bearing complex, and would be the closest to Punt
Kush	Mines distributed throughout the Third-Second Cataract region	
Ta Sety/Khenethennefer		Mines of the deserts east of Lower Nubia (Ta Sety) and northern Upper Nubia (Khenethennefer), generally in the upper reaches of Wadis Allaki and Gabgaba
Wadi Hammamat/Edfu		Mines in the deserts of southern Egypt

* On Karoy, Zibelius, op. cit. 162-3.

This interpretation would suggest that $\epsilon m/Nmy$ lay north of Irem, since it was $\epsilon m/Nmy$ that included, or had closest access to, the southern fringe of the gold-bearing complex as defined by Vercoutter. It is therefore possible that $\epsilon m/Nmy$ straddled the transition between desert and semi-desert, and included both desert and savannah-type environments, while Irem belonged more firmly to the semi-desert zone, perhaps even extending down into true savannah land. However, it would remain true that both $\epsilon m/Nmy$ and Irem would lie far south of Upper Nubia, and be in comparatively close proximity to Punt, given the most likely location of

⁹⁵ With reference to the map of the gold-bearing complex and gold-workings, cf. *ibid.* 129.

the latter. In closing, I would remind the reader that the equivalence of *ʿm* with *Nmy* is by no means certain, and should it prove mistaken the preceding arguments would no longer be relevant to the location of *Nmy*.

III. Miu

Before proceeding to a discussion about the historical implications of the various theories about the location of Irem, it is necessary to discuss briefly the location of another African toponym of the New Kingdom, namely Miu. As I shall show below, the location of this toponym is important for our understanding of New Kingdom activity in the south-lands; and Miu may have lain not far from Irem, according to one of the theories about Irem's location. There are considerable data about Miu, which appears to have been a country of some importance.⁹⁶ These have most recently been reviewed by Störk, who concluded that Hagar el Merwa was located in Miu and that Miu's extent could be perhaps described as bordered on the west by the Nile between Abu Hamed and the Fifth Cataract, and extending for a significant distance into the eastern desert.⁹⁷ In other words, Störk equated Miu with the upstream half of what is sometimes called the Abu Hamed reach, between the Fourth and Fifth Cataracts. There is, however, by no means a consensus on Miu's location; Zibelius, for example, preferred a 'nördliche Lage' for Miu, apparently (she is not entirely clear on this point) in Upper or Lower Nubia,⁹⁸ while Kemp placed Miu south of Upper Nubia but 'in the Berber-Shendi reach' rather than the Abu Hamed reach.⁹⁹

The two data most critical for Miu's location are unfortunately not conclusive. One datum is the Edfu stela of Emhab, who served his 'lord' in 'year 3' in the course of military campaigning which extended as far south as Miu and as far north as Avaris (Tell el Da'aba in the eastern delta). The 'lord' in question is assumed, with good reason, to be Kamose, who certainly reconquered Lower Nubia from the 'ruler of Kush' at the end of the Second Intermediate Period.¹⁰⁰ Zibelius was clearly influenced by the Emhab stela in preferring a northern location for Miu, presumably assuming that Miu actually lay in Lower Nubia and was one of the reconquered areas or that a thrust into Upper Nubia by Kamose may have followed his reconquest of Lower Nubia. Störk, however, ingeniously suggests that Kamose dispatched an army on a forced march along the desert 'Korosko road' from Korosko to Abu Hamed in order to distract his opponent, the kingdom of Kush, by presenting it with an Egyptian force in its rear; in this way, Emhab would have reached the region in which Störk believes Miu was located.¹⁰¹ Unfortunately, there is really no way of choosing between the two alternatives unless some other datum provides more conclusive evidence for the location of Miu.

⁹⁶ Zibelius, *op. cit.* 118-20.

⁹⁷ Störk, *op. cit.* 241-85; conclusion on 260.

⁹⁸ Zibelius, *op. cit.* 120.

⁹⁹ Kemp in Garnsey and Whittaker (eds.), *op. cit.* 290 n. 68.

¹⁰⁰ J. Černý, *MDAIK* 24 (1969), 87-92; J. Baines, *JEA* 72 (1986), 41-53.

¹⁰¹ Störk, *op. cit.* 279-80. Störk also suggests that Tp-Miu, a place in Lower Nubia not to be confused with Miu proper, was the beginning of the desert route to Miu; *ibid.* 258-9. On Tp-Miu see also Zibelius, *op. cit.* 120.

Störk and Kemp place greater emphasis, for locational purposes, upon the reference to Miu on the Armant stela of Tuthmosis III. The text describes two roughly parallel sets of events which (amongst others, described later) demonstrate the prowess of the king and the vast area that was affected by this. One set of events is set in Asia—an elephant hunt in Niy (in Syria), a raid over the Euphrates into Naharin, and the setting up of a ‘victory stela or inscription’ on the banks of the Euphrates. The other events occur in the southern lands—a rhinoceros hunt in the ‘southern foreign land of Ta-Sety’, the seeking for a rebel in Miu, and the setting up of a stela or rock inscription ‘as he (the king) had done at the [lacuna]’.¹⁰² It is on the basis of this text that Störk concludes that Hagar el Merwa (where a frontier inscription of Tuthmosis III is found) was in Miu,¹⁰³ while Kemp comments that *if* the inscription referred to on the Armant stela is identical with the Hagar el Merwa inscription ‘then presumably the important kingdom of Miu should be located in the Berber-Shendi area’.¹⁰⁴

However, it is by no means clear whether the inscription referred to on the Armant stela was set up in Miu or the ‘southern foreign land of Ta-Sety’. The parallelism of the southern events with the events described as occurring in Asia is a general one, not one of detail. The description of the Asiatic events is specific and informative as to the sequence and the locales of the events described. The king was returning from Naharin when the elephant hunt took place in the country of Niy, which was west of the Euphrates; during the campaign against Naharin he had crossed the Euphrates and attacked and burnt towns along its banks; and the ‘victory stela or inscription’ was clearly a celebration of his military success and explicitly placed ‘upon its (the Euphrates’) [lacuna] bank’.¹⁰⁵ The sequence and the locales of the events in the south are less clear. The rhinoceros hunt took place ‘after he (the king) had proceeded to Miu seeking him who had rebelled in that land’, so the time sequence is clear, but the geographical sequence is not. Did the king entirely traverse Miu, and then—moving further south or east—penetrate into the ‘southern foreign land of Ta-Sety’; or did the hunt in that region occur during his return journey from Miu, i.e. somewhere north of or at least downstream from Miu?¹⁰⁶ Moreover, the adverb ‘there’ in the phrase ‘he set up his stela or inscription there’ might apply to either of the two regions. The phrase occurs immediately after the reference to seeking the rebel in Miu, so Miu might be its locale; however, the stela or inscription is not one of ‘victory’ as might be expected (cf. the one on the Euphrates) and one might argue that the reference to Miu, while it parallels generally the attack on Naharin, in this specific context merely sets the *time* of the rhinoceros hunt. In this case, the ‘there’ might refer back to the ‘southern foreign land of Ta-Sety’ and this

¹⁰² Text republished (from A. de Buck, *Egyptian Reading Book* (Leiden, 1948), 64 ff.) in Störk, *op. cit.* 242–3; with translation 243–4. The relevant lines are 7–9.

¹⁰⁴ Kemp in Garnsey and Whittaker (eds.), *op. cit.* 290 n. 68.

¹⁰³ *Ibid.* 259–60.

¹⁰⁶ Armant stela, lines 8–9. Thus, I do not agree with Störk’s judgement that the rhinoceros hunt ‘must’ (in parallelism with the elephant hunt in Niy) have occurred on a return journey to Egypt (Störk, *op. cit.* 271) but rather with Zibelius’s observation that ‘*mju* entweder nördlich von dem Süden von *tj sty* . . . oder *mju* liegt im Süden von *tj sty*’ (Zibelius, *op. cit.* 119).

region would be the locale of the stela or inscription. After campaigning in Miu, the king might have passed into a comparatively poorly known region, rarely entered by Egyptians, and hence be stimulated to have an inscription set up recording his presence.¹⁰⁷

If we are not sure whether the stela or inscription was in Miu or in the 'southern foreign land of Ta-Sety', we are also not sure that it is identical with the inscription of Hagar el Merwa. The latter certainly recalls significant aspects of the Euphrates inscription of Tuthmosis III; both were carved in emulation of earlier frontier inscriptions of Tuthmosis I and both lie in obvious frontier zones, the Euphrates bank on the one hand and the virtually uninhabited Abu Hamed reach on the other.¹⁰⁸ However, there is nothing in the text of the Armant stela that specifically links the inscription referred to there with that of Hagar el Merwa and it is not impossible that an entirely different inscription, made on the occasion of a later (and deeper?) incursion into the southern lands, is referred to.¹⁰⁹ The point of the Armant text could be that the southern inscription described was the southernmost existing at the time it was written, and therefore it was the appropriate pendant to the northernmost marker of Egyptian activity; but it does not follow that, at that time, the Hagar el Merwa inscription itself was the southernmost and therefore the one in question.

There is therefore no reasonable way of deciding whether 'the southern foreign land of Ta-Sety' lay north or south (or indeed, east or west) of Miu: in which of the two regions the inscription referred to was set up; or whether Hagar el Merwa was located in one of the regions or lay outside both. At best, one might argue that the various references to Miu give the impression that it lay beyond the southernmost region of permanent Egyptian control, i.e. beyond Upper Nubia. If this impression is correct, then some part of the Berber-Shendi reach (suggested by Kemp) is a more likely one than the Abu Hamed reach (suggested by Störk). The Berber-Shendi reach is highly productive and today at least is the 'most populous portion of the Northern Sudan';¹¹⁰ the Abu Hamed reach, by contrast, is today one of the 'least populated along the whole of the river's length',¹¹¹ because of its intrinsic unattractiveness to settlement, and not because of change in historical circumstances, for it does not figure importantly in any phase of Nubian history.¹¹²

IV. Historical implications

The preceding discussion has reviewed three principal theories on the location of Irem. One assigns Irem a location in (Priese) or quite close to (Kitchen) Upper Nubia; a second favours a more southerly location, either an undefined distance south-south-west of Upper Nubia (Vercoutter) or stretching eastwards along the

¹⁰⁷ Armant stela, line 9; I disagree with Störk's belief that 'der Vermerk „dort“, womit nach dem Gesagten nur *Mjw* gemeint sein kann' (Störk, op. cit. 259).

¹⁰⁸ On the Hagar el Merwa inscription, Arkell, *JEA* 36 (1950), 36-8.

¹⁰⁹ The date of the Hagar el Merwa inscription is unknown, although it may be of year 35 of Tuthmosis III (Störk, op. cit. 256-7). The Armant stela text covered events at least as late as year 29 (Armant stela, line 17) but continued on to an unknown extent, so its date of composition is unknown.

¹¹⁰ Barbour, op. cit. 135.

¹¹¹ Ibid. 137.

¹¹² Adams, op. cit. 32.

latitude of the Abu Hamed reach as far as the Red Sea (Störk); and the third advocates an even more southerly location, with Irem lying somewhere along the Berber–Shendi reach of the Nile and extending eastward into the Butana, perhaps even beyond the Atbara river (O'Connor). The second theory I will not deal with further on the grounds that Vercoutter's suggestion is based on highly tenuous data and that Störk's is vitiated by the uninhabitable nature of the Abu Hamed reach and the adjoining deserts to the east. Both the other locations of Irem are better supported by the data, but they provide very different perspectives on the nature and scope of Egyptian activity in the south-lands and, to some degree, on the character of some of the indigenous peoples they encountered. Let us then briefly examine the historical implications of these two theories of, respectively, 'Upper Nubian' and 'north Butana' locations for Irem.

Only the period of the New Kingdom is relevant, for Irem—in this form of the name at least¹¹³—does not occur earlier than this and is rare after it. Irem during the New Kingdom appears to have been of some significance, judging in part from specific historical references discussed below and in part from its frequency in the African topographical lists of the period. Irem occurs in at least 52 per cent of the known examples, including the shorter lists,¹¹⁴ which consist of a few regions presumably selected because they were important to the Egyptians or, at least, were considered in some way as especially typical of the southern lands. For the period of the New Kingdom, three specific sub-phases provide useful historical frameworks for a discussion of Irem's significance.

Irem in the conquest period

The conquest period, in my view, extended over a long span of about eighty years, or even more, for Upper Nubia was probably not fully under permanent Egyptian control until the reign of Hatshepsut, or even the sole reign of Tuthmosis III. Kamose reconquered Lower Nubia, but not beyond Buhen and, despite the subsequent 'Kushite' campaigning of Ahmose, Amenhotep I, and Tuthmosis I, Egyptian control expanded only slowly, extending no further south than Sai island at the end of the reign of the last named.¹¹⁵ The reasons for this comparatively slow expansion are no doubt complex, including a concern for internal Egyptian stability as well as expansion, and limited military resources necessarily divided between two fronts, an 'Asiatic' and a 'Nubian'; but we can also discern indications that the once powerful state of Kush, which had dominated Lower and much, if not all, of Upper Nubia in the late Second Intermediate Period, was stubbornly and not ineffectively resisting the Egyptian advance into Upper Nubia.¹¹⁶

¹¹³ Priese has suggested that Irem is but a later writing for the Old (and Middle?) Kingdom toponym Yam (cf. 'Irame', 33), but cannot demonstrate this conclusively, as Vercoutter points out in *MIFAO* CIV 171 n. 4. In these circumstances, it is better to keep locational discussions of Irem and Yam separate from each other.

¹¹⁴ Conclusions based on the data collected in Zibelius, *op. cit.*

¹¹⁵ On Kamose, H. S. Smith, *The Fortress of Buhen. The Inscriptions* (London, 1976), 206–7; on Tuthmosis I, cf. n. 74 above.

¹¹⁶ Might we conclude that the *Ywntyw Sty* opposing Amenhotep I (*Urk.* IV, 7.3), the 'ruler of wretched Kush' slain under Tuthmosis I (*ibid.* 139.4 and 140.12), and the two children of this ruler who instigated an attack on

Tuthmosis I at least 'softened up' Upper Nubia by traversing it completely (cf. his frontier stela at Hagar el Merwa, upstream of the Fourth Cataract) and dividing up this once unitary kingdom between five, presumably vassal, rulers.¹¹⁷ This system of indirect rule over Upper Nubia south of Sai was probably replaced by one of permanent occupation during the co-reigns of Hatshepsut and Tuthmosis III. The period of occupation was marked by the three (at least) campaigns the former organized against Upper Nubia;¹¹⁸ its success is indicated by the absence of any reference to Kushite hostilities in the Annals of Tuthmosis III, covering the next twenty-two years after Hatshepsut's death. Napata, the future 'frontier town' at the downstream end of the Fourth Cataract, was certainly under Egyptian control within twenty-seven years of Hatshepsut's death, if not much earlier. Thereafter, throughout the Eighteenth Dynasty, we have a number of references to hostile contact between Egypt and 'Kush', but the historical significance of these is obscure, because in the New Kingdom 'Kush' eventually 'bezeichnet vor allem das Niltal vom ersten Katarakt bis Khartoum'.¹¹⁹ Unless more specific evidence is available, a 'Kushite' campaign could be located inside Lower or Upper Nubia or well south of the latter; this point will become important in subsequent discussion.

For the conquest period, references to Irem are rare and none survives earlier than Hatshepsut. However, it should be noted that Kamose's troops, even before the beginning of the Eighteenth Dynasty, had reached Miu, possibly with hostile intent, although this last suggestion is based on indirect evidence only.¹²⁰ As noted above, admittedly highly tenuous evidence indicates that Miu was somewhere along the Berber-Shendi reach, in which case this early penetration into the Sudan was strikingly deep. The route by which Kamose's troops reached Miu is unknown, and it is hard to tell if the operation was simply a part of a pincer movement against Kush/Upper Nubia or whether the Egyptians feared that Miu would support the resistance of Kush/Upper Nubia to the Egyptian reconquest of Lower Nubia. In either case, the event shows Egyptian awareness of, and contact with, kingdoms well upstream of Upper Nubia very early in the New Kingdom. The next surviving reference to Miu (excluding the African topographical list at Deir el Bahari, cf. above) is undoubtedly hostile, for late in Hatshepsut's reign Tuthmosis III 'journeyed to Miu to find him who had rebelled against him in that land',¹²¹ once again demonstrating a serious Egyptian interest in regions apparently well south of Upper Nubia.

the Third Cataract fortresses under Tuthmosis II (ibid. 139.4 and 140.12) were all successively ruling descendants of the earlier 'rulers of Kush', struggling to hold together a Kushite polity gradually disintegrating under Egyptian pressure?

¹¹⁷ Ibid. 139.4-7.

¹¹⁸ Campaigns of Hatshepsut: Redford, *History and Chronology*, 57-62; W.-F. Reineke in Endesfelder *et al.* (eds.), *op. cit.* 269-376 for a hitherto unknown campaign.

¹¹⁹ Zibelius, *op. cit.* 167.

¹²⁰ Stela of Emhab, cf. n. 100 above. The inference of hostile intent on the Egyptians' part is based on the fact that Emhab was 'beating drum every day' (as a military musician) during the period that Miu was contacted; that his 'lord' (the king) 'killed'; and that Miu is placed in apposition to Avaris, to which the Egyptians were undoubtedly hostile (Emhab stela, lines 8-14).

¹²¹ Cf. n. 102 above.

It is within this framework that we must briefly consider the only three references to Irem surviving from the conquest period. We have already seen that Hatshepsut's Punt expedition made (apparently peaceful) contact with Irem, while Tuthmosis III found four sons of the ruler of Irem included in the tribute (*b:kw*) of Kush one year. If the second argument for Irem be accepted (implying that Irem was contiguous with or close to Punt), an Upper Nubian location for Irem seems too far away from Punt; while as I have pointed out above, the evidence of the Tuthmosis III datum in itself does not require us to locate Irem in Upper Nubia. Possibly, therefore, another penetration of the kingdoms well upstream of Upper Nubia is indicated. Alternatively (following Priese) one could argue that the pacification of Upper Nubia was continuing and it was this process that involved an Upper Nubian Irem, but the argument against this derived from the Deir el Bahari material remains. The third datum is the occurrence of Irem in the list of captive 'Kushite' regions being led to Hatshepsut by the god Dedwen at Deir el Bahari. This context itself, as well as the possibility that a text describing hostilities against southerners once stood above the scene, suggests that Irem was considered to be amongst the places hostile to Egypt. Whether Hatshepsut's forces ever actually campaigned in Irem is, of course, quite uncertain, since the list in question may extend well beyond the locale of any campaign.

Irem in the period of stabilization and secondary expansion

This second phase extends, roughly speaking, through the remainder of the Eighteenth Dynasty after the death of Tuthmosis III. By 'stabilization' I mean that during this period a permanent Egyptian presence is well documented at least as far south as Kawa, not to mention Napata, suggesting that an effective system of military and political control permitted Egypt to exploit the resources of Lower and Upper Nubia. This security also permitted Egypt to expand its activities, at least towards the east; here lay the 'gold of Wawat', the well-known deposits of the Wadis Allaki and Gabgaba in particular. The (presumably nomadic) inhabitants of these regions did not welcome Egyptian expansion, and perhaps interfered with Egyptian efforts to exploit the gold. Tuthmosis IV launched a surprise attack on an enemy force that had deliberately concealed itself in the 'eastern desert' of the 'region of Wawat'; a viceroy of Amenhotep III engaged in a campaign of attrition against the people of Ibhet (definitely to be located in the eastern desert) when they were preoccupied with their harvest; and Amenhotep IV/Akhenaten sent troops against the eastern desert land of Akuyta because its inhabitants had been 'seizing' the food supplies of Egyptians (possibly gold-miners?).¹²² This aggressive policy was apparently successful, since references to Egyptian activity in Akuyta continue into late Ramesside times, but without any indication that further campaigning was

¹²² Tuthmosis IV, *Urk.* IV, 1545.1-1548.6 (cf. especially 1545.11 and 1547.17); Amenhotep III (ibid. 1659.1-1661.5); on Akhenaten, see H. S. Smith, *op. cit.* 124-9; A. R. Schulman, in *L'Égyptologie en 1979*, 11 (Paris, 1982), 299-316; W. Helck, *SAK* 8 (1980), 117-26. On the locations of Ibhet and Akuyta, see Zibelius, *op. cit.* 74-5, 95-6.

required.¹²³ Additional campaigning along a section of the Red Sea coast inhabited by Nehasyu (Nubians) indicates an even more adventurous policy, at least on occasion.¹²⁴

The apparent stability of Egypt's control and its expansionist policies, at least to the east of Lower Nubia and probably northern Upper Nubia (the bulk of the known ancient mines, at least, cluster along the latitudes of the latter),¹²⁵ form an important context for significant historical questions raised by the different locations suggested for Irem. The questions are generated by hostilities between Egypt and Irem after the conquest of Upper Nubia, hostilities possibly documented in the Eighteenth Dynasty and certainly attested in the Ramesside Period.

As well as his campaign east of Wawat/Lower Nubia Tuthmosis IV *may* have fought an additional Kushite campaign,¹²⁶ unless the latter is in fact identical with the former. It is worth noting, at least, that on the chariot of Tuthmosis IV an Asiatic list of important regions or peoples is balanced by an interesting African list, the sequences being:

Naharin	Babylonia	Tunip	the Shasu	Kadesh	Takhesy
Kush	Karoy	Miu	Irem	Gwerses	Tiurek. ¹²⁷

Tuthmosis IV certainly did campaign in Asia, not only attacking Gezer in Palestine but also fighting as far afield as Naharin, east of northern Syria.¹²⁸ Might this suggest that the African list on his chariot also indicates aggression against some or all of these southern lands? The evidence that Amenhotep III campaigned against Irem is a little stronger, but by no means conclusive. In his fifth regnal year Amenhotep fought a 'first campaign of victory' provoked by a rebellion plotted by the 'Fallen One' of Kush. This campaign is not identical with that fought in Ibhet,¹²⁹ being on a much larger scale; 30,000 prisoners were taken, a figure which, even if hyperbolic, at least indicates a very substantial campaign.¹³⁰ One of the stelae commemorating the campaign depicts four captive countries, namely Kush, Irem, Tiurek, and Weretj or Weresh,¹³¹ a list short and specific enough to suggest that the last three were in fact the targets of the Egyptian attack. Under Akhenaten no 'Kushite' campaigns are reported, but Horemheb fought a campaign in the south-lands on Tutankhamun's behalf and yet another against the 'land of Kush' after he had become king. The exact locales of these campaigns are, however, unknown.¹³²

¹²³ Cf. the documents VI B b 20 and VI F 90 cited in Zibelius, *op. cit.* 96, and described 48 and 55, but note also n. 144 below.

¹²⁴ *Urk.* IV, 1734.1-1736.7.

¹²⁵ See the map of the known ancient gold mines given by Vercoutter in *Kush* 7 (1959), 129.

¹²⁶ *Urk.* IV, 1556.15; also, *ibid.* 1617.17-18 ('the battlefield' from Naharin to Karoy).

¹²⁷ *Ibid.* 1560.15-19.

¹²⁸ *Ibid.* 1556.10-11 (Gez[er]); *ibid.* 1554.17-18 ([Nahar]in); *ibid.* 1617.17-18 (Naharin).

¹²⁹ *Contra* Redford, *Akhenaten. The Heretic King* (Princeton, 1984), 39, and J. Leclant, in C.-A. Julien *et al.*, *Les Africains* (Paris, 1977), 90-2.

¹³⁰ *Urk.* IV, 1661.6-1666.20; the figure of 30,000 prisoners, *ibid.* 1666.10.

¹³¹ *Ibid.* 1661.17.

¹³² Redford, *Akhenaten*, 219; *Urk.* IV, 2138.14-2139.2. Note that Tuthmosis III also conducted a campaign in the south-lands (locale unknown) in his fiftieth regnal year; *Urk.* IV, 814.1-815.2.

With regard to the Irem campaigns of Tuthmosis IV (??) and Amenhotep III (?), placing Irem and presumably the contiguous regions of Gwerses, Tiurek, and Weretj (Weresh)¹³³ in Upper Nubia (as Priese's theory requires) or at least near it, Kitchen recalls Kemp's theory that the Upper Nubian valley between Kawa and Napata was not settled with Egyptian temple-towns and that the active trade routes virtually by-passed this region, moving instead from Kawa via Sikket el Meheila to Napata and from thence south-east towards the Berber-Shendi reach.¹³⁴ Might one infer (Kemp does not) that the Kawa-Napata stretch was in fact rather loosely controlled by Egypt, and that it was along it that the rebellions of Irem, Gwerses, and Tiurek broke out? Against this we should note that the evidence for an absence of Egyptian settlement along this stretch is essentially negative, namely the absence of reported archaeological remains of Egyptian temple-towns or even fortresses.¹³⁵ The Kawa-Napata reach, however, has not yet undergone comprehensive archaeological survey and such remains may yet emerge, as Vercoutter has emphasized.¹³⁶ In any case, Kemp himself notes that if Napata was so isolated, this shows 'how secure the Egyptian presence in Nubia was'; and, moreover, tentatively places Irem, Gwerses, and Tiurek in the Kawa-Amara area, i.e. precisely in the area containing a number of well-attested Egyptian temple-towns which are, comparatively speaking, widely scattered and provided with fortifications of a 'perfunctory', even symbolic, nature.¹³⁷ This last datum also does not accord well with the picture of an Upper Nubia subject to recurrent and serious rebellion.

If Irem, Gwerses, and Tiurek were both rebellious and located either in the Kawa-Napata stretch or the Amara-Kawa stretch, then the possible campaigns listed above (and, even more, the Ramesside campaigns discussed below) would suggest either that the Egyptian system of control over Upper Nubia was surprisingly unstable or loose, or that certain indigenous peoples retained a striking degree of resilience and capacity for resistance even while under Egyptian rule. These circumstances are not impossible to visualize (and gain in credibility if Irem is to be shifted westwards into the area of Wadi el Qa'ab as Kitchen suggests), but do not fit very well with the impression of Egyptian security in Upper Nubia created by much other data. It is true that a serious rebellion in supposedly more secure Wawat broke out as late as the reign of Merenptah, but this is the *sole* record of such surviving from the New Kingdom and occurred in what might have been unique circumstances.¹³⁸

If, however, we place Irem, Gwerses, Tiurek, and Weretj (Weresh) much further south, along the Berber-Shendi reach and in the northern Butana, then the possible campaigns of Tuthmosis IV and Amenhotep III would reflect not only stability in

¹³³ Nothing specific or, at least, reliable is known about the locations of these three toponyms; cf. Zibelius, *op. cit.*, under *wrt* (104), *grss* (170-1), and *trk* (177-8).

¹³⁴ Kemp in Garnsey and Whittaker (eds.), *op. cit.* 26-9.

¹³⁵ *Ibid.* 26-8.

¹³⁶ Vercoutter in *MIFAO* CIV, 164.

¹³⁷ Kemp in Garnsey and Whittaker (eds.), *op. cit.* 28 and 29. On the nature of the Amara-Kawa region towns see especially B. J. Kemp, in P. Ucko, R. Tringham, and G. Dimbleby, *Man, Settlement and Urbanism* (London, 1972), 653.

¹³⁸ Kitchen in E. Endesfelder *et al.* (eds.), *op. cit.* 222-4.

Upper Nubia (which would not be involved) but also a bold and aggressive foreign policy which would be in keeping with the expansion in the eastern desert described above, not to mention that which periodically carried Egyptian armies as far afield as Naharin. In fact, if we assume that the campaigns in question (as well as the others which are more ambiguously 'Kushite') were confined to Upper Nubia or its environs, then Egyptian policy upstream of Napata not only appears atypically timid for the period but appears to include no conflict with kingdoms south of Upper Nubia.

Yet such conflict appears inherently likely. Many of the 'southern' items desired by the Egyptians came from these kingdoms, or via them from even more southerly areas,¹³⁹ and while no doubt these products were often (perhaps typically) derived through trade, periodic Egyptian campaigning would ensure Egyptian dominance over some at least of the trade and also produce booty. Moreover, we need not assume that these southern kingdoms were always passive partners in interaction with Egypt; it is reasonable to imagine that they tried to prevent Egyptian encroachment upon the trading routes and ultimately upon the independence of some at least of these kingdoms, and that they were tempted to raid the prosperous towns of Upper Nubia. Our prime candidates for these southern kingdoms lying upstream of Upper Nubia would be then precisely Irem, Gwerses, and Tiurek.¹⁴⁰

The Ramesside Period

The discussion concluding the preceding section is even more relevant to the Ramesside Period, when there are quite explicit references to conflict with Irem. Thanks to Kitchen's recent study,¹⁴¹ these data can be rapidly reviewed. In his eighth regnal year Seti I, after seven years of vigorous campaigning in Palestine and Syria (with a foray into Libya), launched a campaign against the 'enemies in the land of Irem' (details discussed above). The scale of this campaign was comparatively small; although, as Kitchen notes, it reaped 'a body of prisoners and cattle, certainly in excess of 600 and possibly more', the humans were in a minority since the 'body' included at least 420 cattle.¹⁴² Seti's successor, Ramesses II, subsequently cam-

¹³⁹ Cf. Kemp in Garnsey and Whittaker (eds.), op. cit. 28; O'Connor in Clark, op. cit. 925-8 and 939.

¹⁴⁰ Does the chariot of Tuthmosis IV provide a slight clue here? The Asiatic toponyms listed on it are comparatively remote, beyond the area of secure Egyptian control (Tunip, Kadesh, Takhesy, all in Syria) or even more remote, large and independent states (Naharin, Babylonia). The Shasu were a nomadic people found in both Syria and Palestine. Might we assume from this that the pendent southern toponyms also represent relatively remote and independent regions, and therefore upstream of Upper Nubia? Of these toponyms Kush is, as I have noted, topographically ambiguous; Karoy is specifically the frontier zone, 'den äussersten Bereich der ägyptischen Verwaltung von *kš*' and in fact upstream of Napata, the 'frontier' town (Zibelius, op. cit. 163); the remainder—Irem, Gwerses, and Tiurek—we might suggest, by analogy with the Asiatic list, to be even farther away, i.e. farther upstream.

¹⁴¹ Kitchen in Endesfelder *et al.* (eds.), op. cit. 213-25.

¹⁴² *Ibid.* 217; slightly misleadingly, Kitchen refers elsewhere to a 'list of booty' including 'well over 600 captives', implying all involved were human, whereas animal 'captives' may have been the majority: cf. Kitchen, *Pharaoh Triumphant* (Warminster, 1982), 31. The total preserved figures are over 220 humans and over 420 cattle (combining *KRI* 1, 104.3-5 with the better-preserved part of line 4 [Sai] published by Vercoutter, *MIFAO* CIV, 159 = Sai line 10), a total of over 640 'captives' in all, of which about 65 per cent are cattle. Commentary on the Irem texts, cf. especially Kitchen in Endesfelder *et al.* (eds.), op. cit. 214-18; Kitchen,

paigned against Irem (somewhere between his fifteenth and twentieth regnal years). This was 'no mean expedition', for the impressive figure of 7,000 prisoners is recorded.¹⁴³ More tenuous evidence hints at a later conflict with Irem under Ramesses II, for Setau, appointed viceroy of Kush between Ramesses' thirty-fourth and thirty-eighth years, refers to Irem and the land of Akuyta in terms which, at the least, have been noted to include 'some warlike allusions' and elsewhere have been translated as definitely referring to a campaign.¹⁴⁴

Thereafter, no clear reference to conflict with Irem has survived, suggesting to Kitchen that after the Ramesses II campaign 'Irem sank again into enforced quietude, never again to challenge Egyptian rule'.¹⁴⁵ However, he does note that a rhetorical text of the reign of Ramesses III places in apposition to references to clashes with Libyans, sea-peoples, and easterners (all reflecting, however generalized, real and contemporary conflicts) a statement that Ramesses had caused the south-land to be repelled, specifically the 'Nehasyu', the people of Irem and Tirawa. Kitchen surmises that a 'viceroy may have had to crush summarily some revolt before it took hold'.¹⁴⁶

Other textual references to Ramesside triumphs over southern lands refer only to 'Kush', by now a very generalized term. There are also a number of scenes depicting warfare against Nubians.¹⁴⁷ It has generally been assumed that these scenes have little historical value¹⁴⁸ and are only 'conventional', not based on actual events.¹⁴⁹ Kitchen, in a recent discussion, identifies a Beit el Wali scene of Ramesses II as of a 'real event', chronologically distinct from the Irem campaigns of Seti I (year 8) and Ramesses II (years 15/20),¹⁵⁰ but agrees that the scenes of Ramesses III are probably unhistorical.¹⁵¹

It cannot be denied that, in general, the scenes in question belong to well-established, conventional pictorial modes and evidently also draw on specific, but earlier, representations of the same or similar subjects.¹⁵² Nevertheless, they also show a degree of originality and of independence from each other that is sufficient to suggest (though not prove) that real historical campaigns, in the reign in question,

Pharaoh Triumphant, 30-1; Vercoutter, *MIFAO* CIV, 157-66; note A. J. Spalinger, *JARCE* 16 (1979), 42-3. On the Asiatic campaigns of Seti I, cf. conveniently Kitchen, *Pharaoh Triumphant*, 20-5, and, in more detail, Spalinger, *op. cit.* 29-43; most recently, W. Murnane, *The Road to Kadesh* (Chicago, 1985).

¹⁴³ Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 220 with commentary 220-1.

¹⁴⁴ *KRI* III, 93.9-11. Compare Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 221, and *Pharaoh Triumphant*, 138, with Helck, *SAK* 3 (1975), 91, who translates the relevant sections as referring to the 'doing away with' Irem by the pharaoh and the taking prisoner of the 'ruler of Akuyta' and others in the course of military operations involving Setau, and hence later than the years 15/20 clash with Irem. Similar translation, E. Wente, *Mélanges Gamal Eddin Mokhtar* (Cairo, 1985), 351.

¹⁴⁵ Kitchen, *Pharaoh Triumphant*, 72.

¹⁴⁶ *KRI* V, 91.8-9; Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 224-5.

¹⁴⁷ Conveniently reviewed in Säve-Söderbergh, *Ägypten und Nubien*, 169-74.

¹⁴⁸ *Ibid.* 172 and 174.

¹⁴⁹ R. O. Faulkner, 'Egypt: From the Inception of the Nineteenth Dynasty to the Death of Ramesses III', in I. E. S. Edwards, C. J. Gadd, N. G. L. Hammond, and E. Sollberger (eds.), *The Cambridge Ancient History* (3rd edn., Cambridge, 1975), 230 and 244.

¹⁵⁰ Kitchen in Endesfelder *et al.* (eds.), *op. cit.* 220.

¹⁵¹ *Ibid.* 224.

¹⁵² Well demonstrated in Säve-Söderbergh, *op. cit.* 170-2, 173-4.

provided new details for the artist. This is most evident in the battle scenes, particularly of Ramesses II. The dominant theme of those at Beit el Wali and Derr (the Nubian war scenes of Abu Simbel do not include a battle) is of pharaoh's chariot relentlessly riding down (with only a token supporting military escort) helpless and fleeing Nubians. This is a theme going back to, at least, a box of Tutankhamun.¹⁵³ But the Ramesside artists have added the elements of a wounded Nubian leader being helped from the field of battle and of a Nubian village where villagers carry out their daily tasks, unaware of the impending catastrophe. Unanticipated in any surviving scene of similar type from earlier reigns, these elements suggest they were based on incidents and scenes observed during a contemporary campaign of the period. This possibility is reinforced by the fact that at the Beit el Wali and Derr temples respectively, the Nubian leader and his supporters, as well as the village and its surroundings, are depicted in markedly different ways. It is as if two sets of artists were working independently of each other (and of any prototype) in depicting data reported to them about the campaign.¹⁵⁴

The Nubian battle scene of Ramesses III, in its turn, is certainly not a slavish copy of those of Ramesses II at Beit el Wali and Derr.¹⁵⁵ Although the motif of the unsuspecting villagers at the fringes of the fray may be hinted at, there is apparently no 'wounded leader', although the scene is admittedly badly damaged and incomplete. More importantly, the 'battle' (or rather slaughter) proper is arranged quite differently from those of Ramesses II, discussed above. Pharaoh charges in his chariot, but he is accompanied by a real army (chariotry and infantry) and the slaughter is carried out by Egyptian troops, including Sherden and Philistine mercenaries.¹⁵⁶ In these original features, do we detect again the impact of real contemporary events? Of course, even if we accept these Ramesside scenes as reflecting and even, to some extent, being based on real events, we do not know if all or any relate to the conflicts with Irem discussed above. However, since all *specific* Ramesside references to southern campaigns (except for that of Merenptah in Wawat) refer to Irem, sometimes in association with other toponyms, it is a good possibility that the scenes, as well as the generalized references to 'Kushite' campaigns noted above, do in fact reflect the same or similar conflicts with Irem.

To return to the specifics, while the reality of Ramesside campaigning in the Sudan has begun to take firmer shape in recent years, scholarly opinion continues to regard it as of minimal importance in the overall pattern of foreign relations during the period. Thus Kitchen, a well-established authority on the Ramessides, describes the Irem campaign of Seti I as 'a very modest affair'; the clash recorded at Beit el

¹⁵³ Compare H. Ricke, G. Hughes, and E. Wente, *The Beit el Wali Temple of Ramesses II* (Chicago, 1967), pl. 7, and A. Blackman, *The Temple of Derr* (Cairo, 1913), pl. xv, with N. Davies, *Tutankhamun's Painted Box* (Oxford, 1962), pl. ii.

¹⁵⁴ Compare H. Ricke *et al.*, *op. cit.*, pl. 7, with Blackman, *op. cit.*, pl. xvi. Note also that the Beit el Wali temple is considerably earlier than that of Derr (Kitchen, *Pharaoh Triumphant*, 40 and 177), so for the artists of the latter a prototype (i.e. the Beit el Wali scene) existed, and yet was not followed in important details.

¹⁵⁵ No Nubian war scenes have survived from the Ramesseum.

¹⁵⁶ H. H. Nelson, *Medinet Habu I. Earlier Historical Records of Ramesses III* (Chicago, 1930), pl. 9.

Wali as a 'minor revolt', a 'little Nubian war . . . kids' stuff'; the later Irem campaign of Ramesses II as provoked by the rebellion of a 'local chief'; and the possible Irem campaign of Ramesses III as a 'minor conflict'.¹⁵⁷

Such opinions, however, may to some degree underestimate the importance of Ramesside military activity in the Sudan. I would suggest two reasons for this. First, the more spectacular conflicts of the period—with urbanized Syria–Palestine and the Hittites, and with the unruly sea-peoples and Libyans—probably did involve substantially greater and more extended phases of military activity than did the Nubian campaigns; and certainly these other campaigns received much greater prominence in the pictorial and literary record, further diminishing the 'significance' of the Nubian campaigns to the eye of the modern scholar. And yet, smaller-scale and less spectacular as they may have been, these Nubian campaigns may have been quite important for the maintenance of Egyptian dominance in the northern Sudan. Second, if one believes (as Kitchen, for example, does) that Irem, the toponym frequently involved in the campaigns, was located in or, at least, within the nearer environs of Upper Nubia then the campaigns will also seem incidental affairs because the Egyptian position in Upper Nubia seems to have been a secure one throughout the Ramesside Period. But, if there is a legitimate case for arguing that campaigns against Irem involved Egyptian forces moving down into the Berber–Shendi reach and into the northern Butana, then we can envisage the campaigns as being more serious in scope and intent. As pointed out above, Egyptian efforts to maintain some kind of dominance in the regions upstream of Upper Nubia and the barren Abu Hamed reach are inherently likely to have occurred throughout the New Kingdom (paralleling similar efforts beyond the securely controlled areas in Syria–Palestine). If, for the Ramesside Period, we exclude the Irem campaigns from this extramural campaigning we would have no specific references to such campaigning left; and must also assume that Upper Nubia and its environs were unstable to a degree that does not match well with Egypt's apparent stability within Upper Nubia.

Of course, the Ramesside references to Irem offer little direct evidence on its location. However, the campaigning of Seti I (for which we have the most information) clearly implies a non-riverine location and, on internal evidence, can be reconciled with a northern Butana location for Irem, as easily as with the Wadi el Qa'ab locale suggested by Kitchen. The desert environment, the use of chariotry and infantry, the duration of the campaign, and the capture of a series of wells or water-holes are details that would fit an Egyptian strike along the well-known routes running across the southern Bayuda desert and linking the Napata region to the Berber–Shendi reach.¹⁵⁸ Not only were these routes known and used in antiquity, they were also carefully surveyed by British army officers in the later nineteenth century. These officers noted that cavalry (and hence chariotry) could traverse some

¹⁵⁷ Cf. Kitchen, *Pharaoh Triumphant*, 31, 40, and 72, and idem in Endesfelder *et al.* (eds.), *op. cit.* 224.

¹⁵⁸ On the routes, cf. n. 65 above; also, N. Chittick, *Kush* 3 (1955), 86–92; Gleichen, *op. cit.*, I, 208–10; II, 172–87.

	← UPPER NUBIA AND/OR ITS IMMEDIATE ENVIRONS →									
	UPPER NUBIA/NSUDAN IN GENERAL	UP-STREAM FRONTIER ZONE, UPPER NUBIA	← BERBER-SHEND/NORTHERNATBARA REACH / BUTANA / RIVER →							
	KUSH	KAROY	MIU	IREM	GWERSES	TIUREK	WERESH	TIRAWA	EASTERN DESERT	PUNT
KAMOSE			X							
AHMOSE	X?									
AMENHOTEP I	X									
THUTMOSIS I	X									
HATSEPSUT	X?									
"										X
"	X									
"			X							
THUTMOSIS III										X
"										X
"				X?						
"										X
"	X									
AMENHOTEP III										X
THUTMOSIS IV									X	
"	X?	X?	X?	X?	X?	X?				
"										X
AMENHOTEP III									X	
"	X			X		X	X			
"										X
AKHENATEN									X	
"										X
TUTANKHAMUN	X?									
HOREMHEB	X									
"										X
SETI I				X						
"										X
SETI I/RAMESSES II	X									
RAMESSES II				X						
"				X?					X?	
"										X
RAMESSES III	X									
"				X?				X?		
"										X
TOTALS	12	1	3	7	1	2	1	1	4	12

FIG. 4

of these routes.¹⁵⁹ In view of the indications that Seti's campaign was provoked by an intrusion by Irem on Egyptian territory, it is possible that the narrative of the campaign describes the decimation by the Egyptians of hostile forces that had been gathering at the water-holes along the route, either preparatory to a thrust into Upper Nubia or to enjoy profitable control over trade routes which were peculiarly important to the Egyptians.

V. Conclusions

The overall historical picture sketched above can be summarized diagrammatically, as can Egypt's continuing, peaceful relations with Punt.

The implications of the first, 'Upper Nubian' locale are clear from fig. 4. Once the earliest rulers of the Eighteenth Dynasty had brought Kush proper, in the sense of Upper Nubia, under control its usage becomes so generalized that it may refer to Upper Nubia or regions upstream of it. If we assume that the other toponyms involved are Upper Nubian, then it follows that throughout its entire history New Kingdom Egypt periodically engaged in hostilities against kingdoms in, or very near to, Upper Nubia. The New Kingdom southern 'empire' would then appear as distinctly unstable, particularly as some of the campaigning was on a large scale (30,000(?) prisoners for Amenhotep III, 7,000 for Ramesses II). Moreover, we would then have no indication of hostilities against regions upstream of Upper Nubia and the Abu Hamed reach, a striking contrast to Egypt's aggressive policy in Palestine and Syria during the Eighteenth Dynasty and Ramesside times.

The second locale suggested for Irem changes our picture of the scope and intent of Egyptian activity very considerably. After the pacification of Upper Nubia we now see the Egyptians moving out aggressively into the Berber-Shendi, Butana, and Atbara region, paralleling the aggression shown in Palestine and Syria and the interest in the 'further south' documented more pacifically in the continuing contact with Punt. There were no doubt strong psychological and economic motives for their campaigns on the Egyptian side but there are also indications that southern kingdoms in these remote regions displayed provocative aggression against Egypt and its Upper Nubian province.

I believe that this second historical picture best fits the locational evidence. At least, it should be seen as a viable alternative to the theories that locate Irem and other toponyms—and therefore nearly all New Kingdom campaigning—in Upper Nubia and its immediate environs.

Postscript

J. C. Darnell has recently published a brief discussion of Irem ('Irem and the Ghost of Kerma', *GM* 94 (1986), 17–23) in which he points out that the archaeological history of Kerma makes it unlikely that Kerma lay in Irem (17–18) and that the Seti I texts suggest that Irem 'is a minimum of seven days journey from a remote southern outpost of Egypt, far from *i'm* and Kerma, and east of the Nile' (20). This last point is based on the appearance of

¹⁵⁹ Gleichen, *op. cit.*, II, 179–80.

rulers of Irem in connection with Hatshepsut's expedition to Punt (23 n. 21), a point discussed in some detail by the present writer in 1982 in a publication not noted by Darnell (D. O'Connor, 'The Toponyms of Nubia and of Contiguous Regions in the New Kingdom', appendix to ch. 12 in J. D. Clark (ed.), *The Cambridge History of Africa*, 1 (Cambridge, 1982), 934-40).

THE CURIOUS LUXOR OBELISKS

by MARTIN ISLER

When ancient building projects are carefully surveyed, they sometimes reveal curious anomalies. Because we know so little of Egyptian building methods, it is often difficult to decide if the imperfections are deliberate or accidental. This article addresses the unexplained curvatures on the longitudinal and lateral faces of the Luxor obelisks, and argues that they are an accidental result of the ancient quarrying method.

The ancient Egyptians left overwhelming testimony to their sculptural and building skills, but almost no indication of the methods they used. We are therefore in the unenviable position of trying to piece together a sequence of steps that could yield the results we see, from the meagre supply of tools and instruments in their possession. In doing this, it is sometimes an advantage to concentrate on building anomalies, simply because a perfectly executed monument does not disclose the building method as readily as one that is imperfectly built. As an example of this, let us consider the puzzling aspect of the Luxor obelisks. Both are slightly bowed and two of their opposing faces swell out. Of the one now in Paris, Gorringe said:

A close examination revealed that all the sides of this monolith are not planes; the N.W. and S.E. faces as it stood at Luxor (turned respectively to the Seine and to the Madeleine in Paris) have a double curvature. Laterally both are convex, and versed sine of the convexity of the former being $1\frac{1}{8}$ inches, and of the latter $1\frac{1}{3}$ inches; in other words, these two sides are rounded out, the middle of the rounding being $1\frac{1}{8}$ and $1\frac{1}{3}$ inches from an imaginary straight line across from edge to edge. The longitudinal curvatures are remarkable in that the N.W. face is convex, and the S.E. concave, in consequence of which all four longitudinal edges are curves convex to N.W.—to the river Seine as the obelisk now stands. The versed sine of this curvature is very small, being only four-fifths of an inch for the N.W. face, and half an inch for the other.

It is a very curious fact that the sides of the other obelisk, still at Luxor, present the same peculiarity, the convexity of its edges also being turned toward the Nile. This can hardly be attributable to accident, or to the imperfection of the work of quarrying and dressing, but must be considered one of the many questions connected with these wonderful monuments that have yet to be solved.¹

Gorringe's description of the obelisks' peculiar configuration is shown graphically in fig. 1. The north-west face is both longitudinally and laterally convex, while the south-east face is longitudinally concave and laterally convex. To clarify, the side elevation of fig. 2 shows the north-east and south-west sides with flat faces, while fig. 3 clearly shows the north-west and south-east faces bowed. The imaginary straight line from edge to edge in the sectional view of fig. 4 shows the swelling or convexity of both the north-west and south-east faces.

Engelbach seemed to have been aware of its convex longitudinal face but not its

¹ Henry H. Gorringe, *Egyptian Obelisks* (New York, 1882), 83.

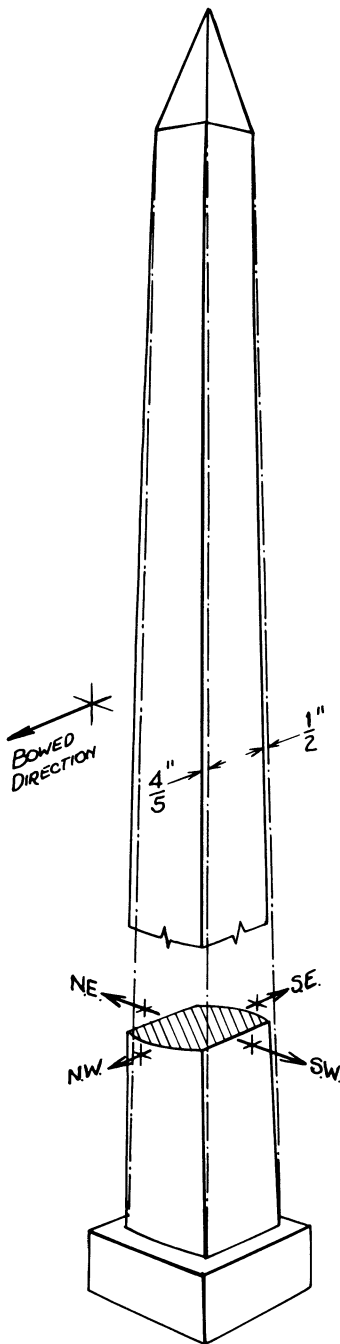


FIG. 1. Perspective view of the Luxor obelisk, presently in Paris.

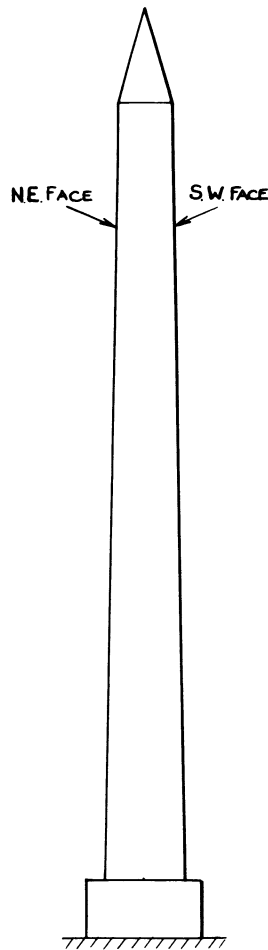


FIG. 2. Side elevation, showing the flat north-east and south-west faces.

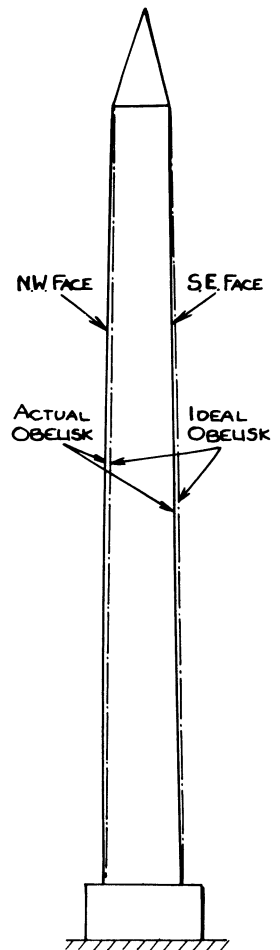


FIG. 3. Side elevation, showing the bowed north-west and south-east faces.

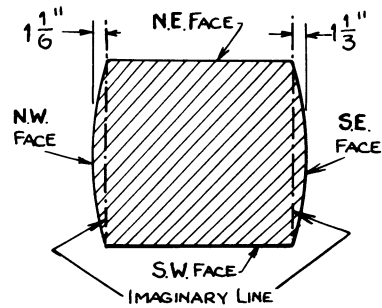


FIG. 4. Lateral section through the obelisk, displaying two convex and two flat faces.

opposing concave face, for he considers it an indication that the Egyptians used 'entasis', a term used in Greek architecture for pillars intentionally carved with a swelling in the centre, in order to obviate perspective distortion and make them more pleasing to the eye.² While Engelbach gave a reason for the convexity, and Gorringer did not, both felt the obelisk was deliberately so carved. The motive is insignificant, however, when compared to the question of how the ancient Egyptians, with only the most primitive measuring instruments, could possibly have put two almost imperceptible parallel curves in an 82 foot, 284 ton monolith.

Consideration of means requires a review of ancient quarrying techniques, for which we will rely heavily on Engelbach's study of the unfinished obelisk at Aswan.³ This represents an unsuccessful attempt by the Egyptians to extract a monolith of 137 feet in length, with a weight of 1,168 tons. When it was found that fissures in the stone prevented them from reaching their goal, the projected size was reduced, then, because of further difficulties, the project was abandoned entirely. By this time, however, they had achieved a fairly flat top surface, and had cut trenches around three sides of the perimeter. According to Engelbach, after selecting a possible quarry site, the Egyptians quickly broke down the surface of the granite by using fire, perhaps in combination with water, until they arrived at an area that seemed both sound and large enough for the obelisk. As this procedure neared the surface of the proposed monument it was stopped, and a slower, more precise carving method was introduced. At Aswan, when this moment was reached, it was realized that, while the area was large enough, it sloped, and the resultant pyramidion would be lying at a higher level than the base. As a consequence, it would be necessary to carve the top surface at an oblique angle.

It is important to note that the initial, and most important step, in extracting a monument such as an obelisk, is to establish a first planar surface, which, once achieved, becomes a reference for all other surfaces. The evidence at Aswan, clearly shows that the stonemasons did achieve a top plane on a sloped surface. I suggest they did this by cutting two shallow troughs at right angles to the longitudinal axis as a first step (see fig. 5). In this way, although the long axis of the pyramid is at an angle to the horizon, the troughs can be made level to the horizon becoming parallel to each other. If they are then joined, a first plane is formed. While it might be possible for water to have been used as a levelling means for the troughs, Dieter Arnold feels⁴ that the small fissures and the porosity of the stone makes it impractical. He therefore suggests the use of a builder's tool, such as that from the tomb of Sennedjem,⁵ to level the channels. This is pictured in fig. 5.

The manner of joining the channels is of great importance and the consequences of a possible mistake will be disclosed shortly. Engelbach stated that the upper surface was almost certainly obtained by the use of boning-rods, but instead of using the string stretched between them as a guide, as shown in the example of the tomb

² R. Engelbach, *The Problem of the Obelisks* (New York, 1923), 37.

³ *Ibid.* 32-51.

⁴ Personal discussion in 1985 at Metropolitan Museum of Art.

⁵ W. M. Flinders Petrie, *Tools and Weapons* (London, 1917), pl. xlvii, 59.

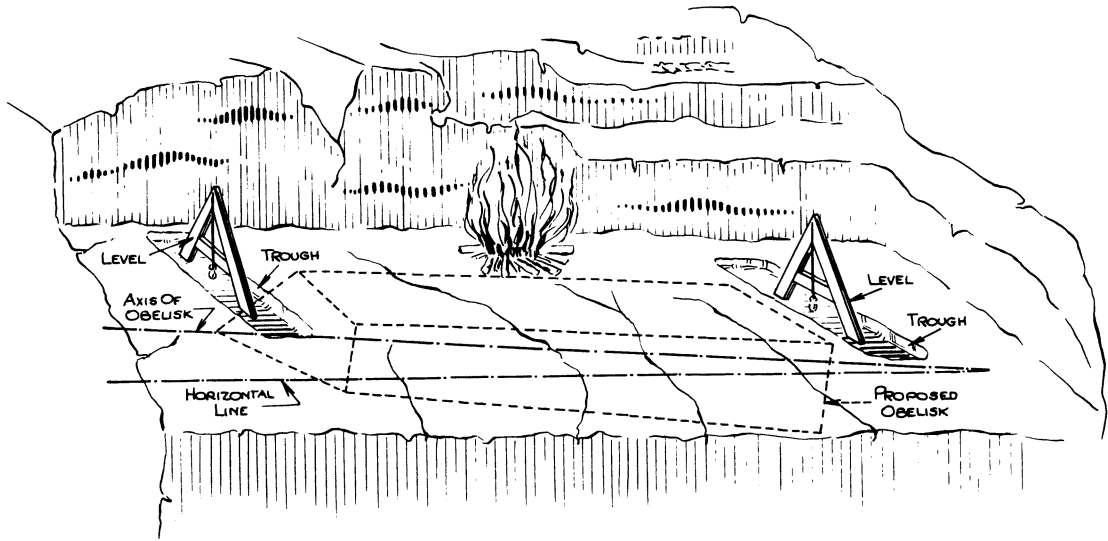


FIG. 5. Quarrying procedure, showing the axis of the obelisk inclined to the horizon, with the levelled troughs, at right angles to the axis.

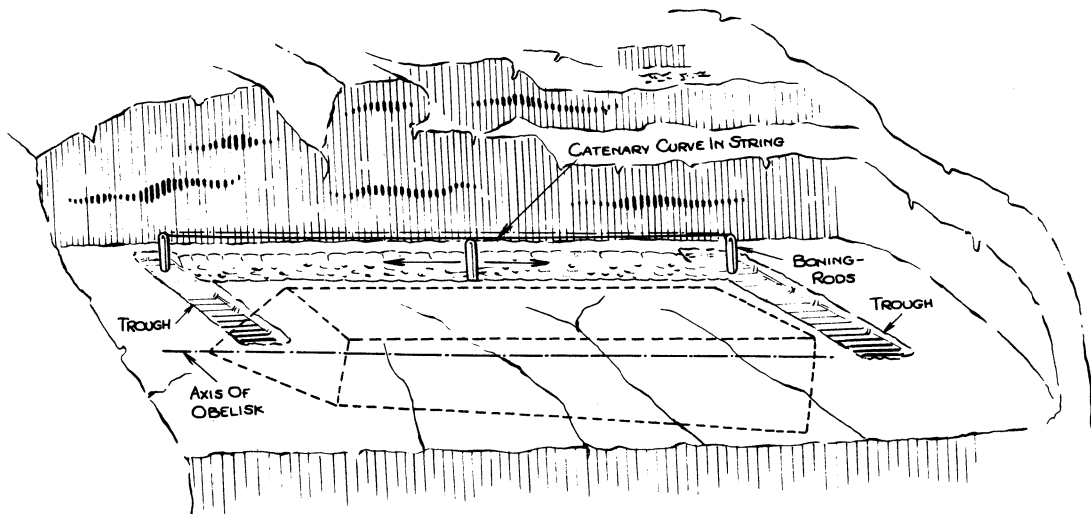


FIG. 6. A view showing both levelled troughs, joined axially, with the aid of boning-rods.

drawings,⁶ he proposed the use of a visual method, whereby a man standing at one end could see if a third intermediate rod was in line with the others. He felt that a *string would sag and produce a concave error*. Therefore, he claimed, the visual method, without the string, which is as simple and obvious, seems a legitimate assumption. Suppose, however, contrary to Engelbach's supposition, that in the case of the Luxor obelisks (and perhaps others as well) the stretched string *was used and did sag*. This would cause all of the stone surfacing guided by the string to assume its curve. We might then have our longitudinal concave south-east face.

⁶ S. Clarke and R. Engelbach, *Ancient Egyptian Masonry* (London, 1930), figs. 113, 265e.

The drawing of fig. 6 shows a pictorial view of the above description, while the sections of figs. 7 and 8 show in detail how the centre rod, which is the same length as the other two, by having used the sagging stretched string to transfer measurements, caused the centre to fall below the ideal position of the upper face of the obelisk. A continuation of this method would result in the upper face of the obelisk having the same downward bowing that is shown by the catenary curve in the string.

Having established a first, albeit curved, plane, the next step in the quarrying procedure would be to mark the outline of the proposed obelisk on the surface of the rock and then to cut a trench around the perimeter of the monolith. Before that, however, it might be of interest to describe the cutting tools which the ancient masons employed. Engelbach found a great number of dolerite balls around the Aswan obelisk and, by investigation, decided that the stonecutters reduced the rock by using the balls to smash the surface. These balls are a very tough greenish-black stone, 5 to 12 in. in diameter, with an average weight of 12 lb., and are found naturally in some of the valleys of the eastern desert. Engelbach found that by bringing the ball down with great force, the surface of the stone to be worked is bruised and reduced to powder. While it seems very primitive, Engelbach felt it to be effective. We might imagine that a great many men would work in unison, either timing their blows to a chant as teams of men often do, or by having someone beat a cadence.

A comparison of the lateral cross-sections of figs. 9 and 10, shows how the perimeter trenches were cut, first down and then under the monument in order to separate it from the rock beneath. Attention should be given to the manner of transferring the outline of the obelisk which was drawn on the top surface, down the two vertical sides. The use of a plumb line,⁷ ensures that both faces will be flat in a vertical direction—this would account for the north-east and south-west sides of the obelisk. A general view of the partially undercut obelisk is shown in fig. 11. The side view of fig. 12 shows the method of transferring the measurements from the top surface (south-east side of the obelisk) to the bottom surface (north-west side of the obelisk). There are two ways of accomplishing this; by taking measurements directly from the top surface and transferring them to the bottom, or by stretching a string in the appropriate place and measuring equally from either side of the centreline. Once again, careful attention should be given to the procedure. The curved bottom given by direct measurements is obvious, less so is the tautly stretched string used for placing the centreline, for this string will assume the same catenary curve as the string used with the boning-rods. The result, if followed, is a longitudinally curved convex face on the north-west side of the obelisk. We can therefore account for the manner by which the two long curves shown in figs. 1 and 3 were produced.

The quarrying method that produced the unusual lateral curves on the north-west and south-east side of the monolith remain to be discovered. The use of dolerite balls would obviously have created a roughened surface on the faces. In order to bring them to the high polish that was eventually attained, these would have to be

⁷ Petrie, *op. cit.*, pl. xlvii, 57.

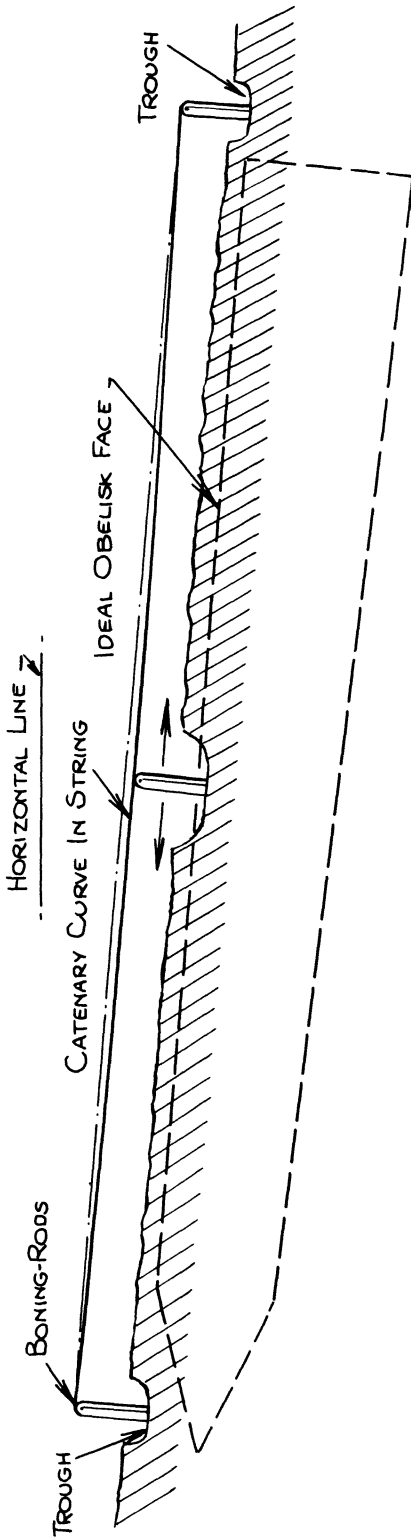


FIG. 7. Sectional view, showing the manner in which the catenary curve in the string, causes the centre boning-rod to fall below the idealized obelisk face.

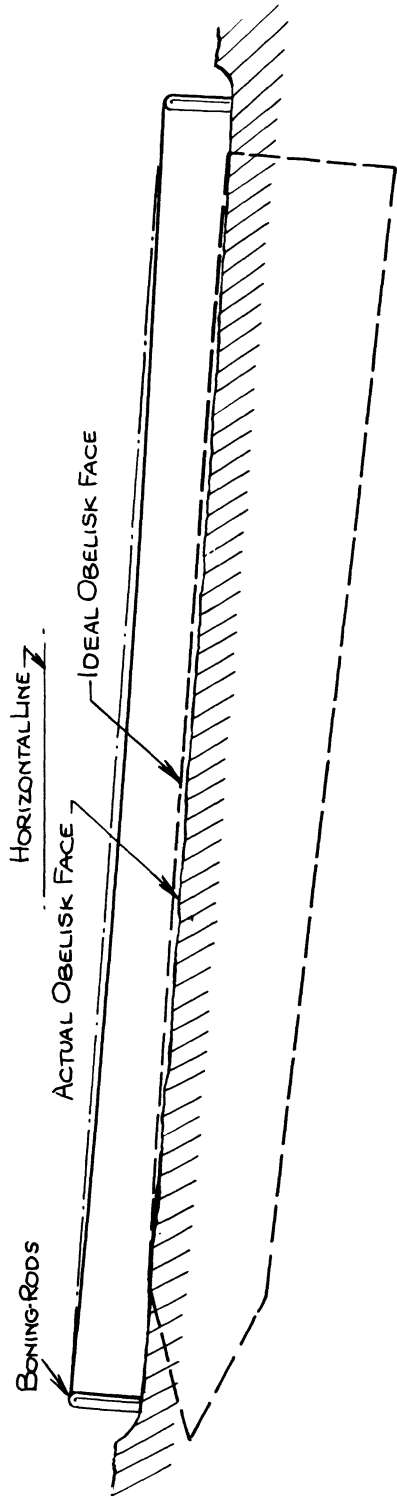


FIG. 8. Sectional view, showing how continued use of this method would result in a curved top surface.

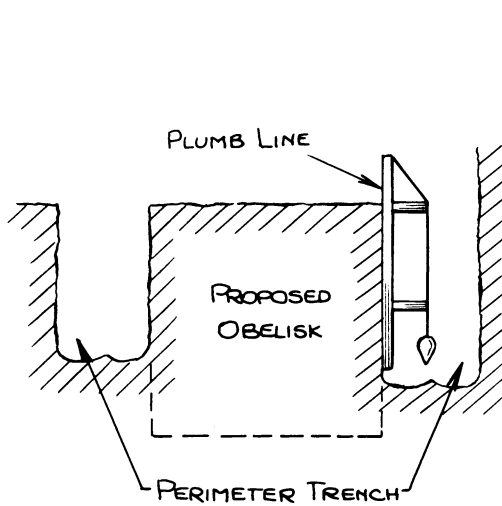


FIG. 9. Section, showing the perimeter trenches and the method of using a plumb line.

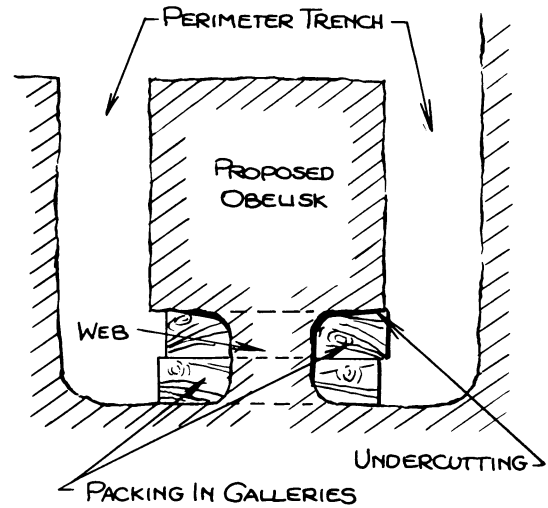


FIG. 10. Section, showing the partial undercutting of the obelisk and the packing in the galleries.

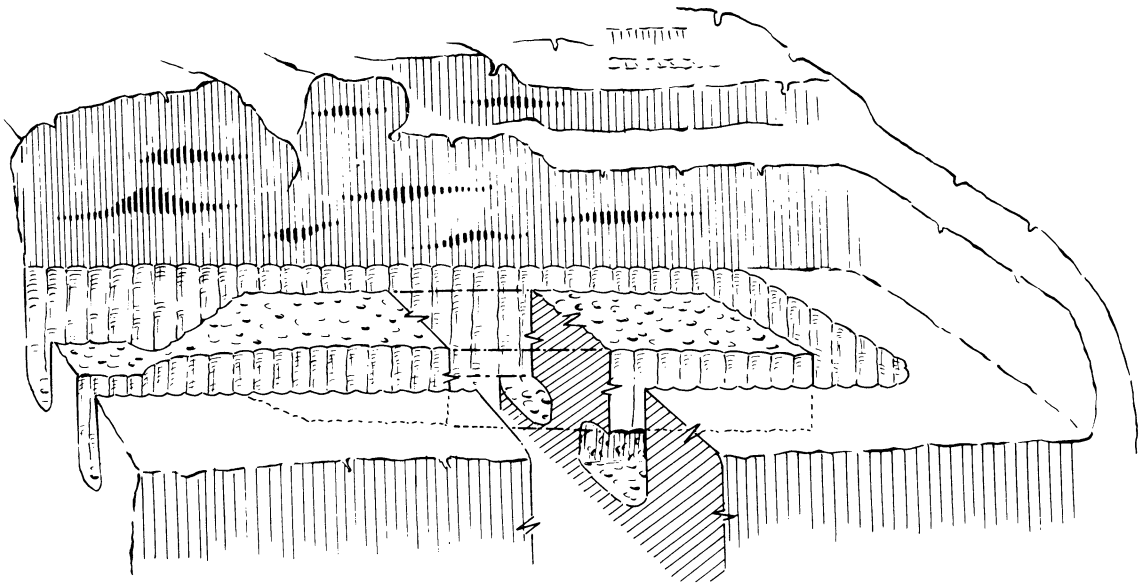


FIG. 11. A partially sectioned pictorial view of the obelisk, undercut, and displaying the perimeter trenches.

smoothed and then polished. When we consider that Hatshepsut had a pair of obelisks carved in seven months,⁸ it is likely that, to save time, the dressing of the top face was started as soon as the rough carving was completed, and while the masons were cutting the perimeter trench.

Here, let us examine a method which has not changed in thousands of years—the polishing procedure. As a sculptor, the polishing methods I used were the same as

⁸ Engelbach, *op. cit.*, 49.

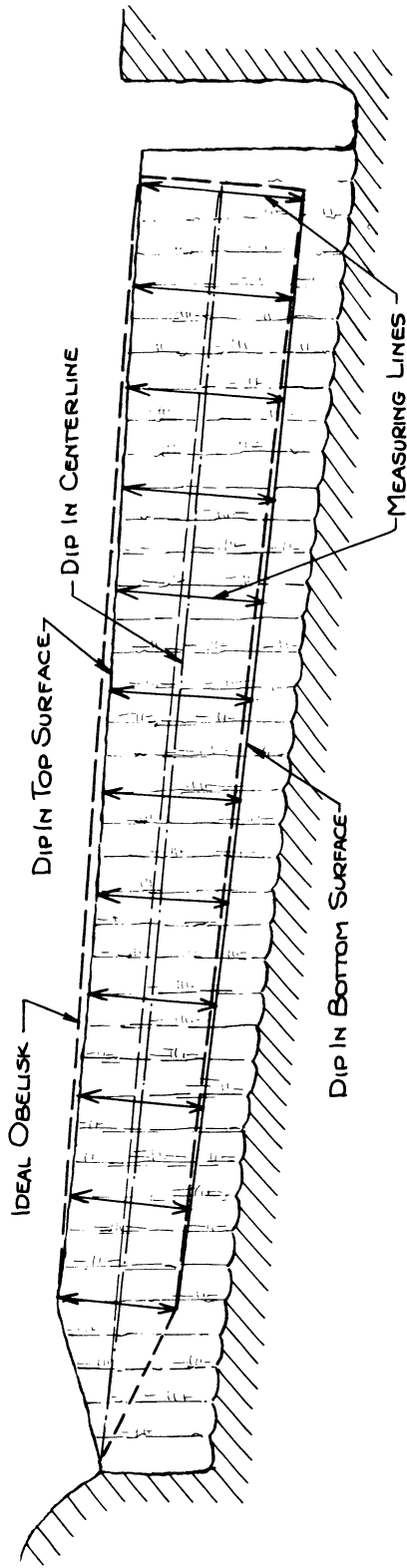


FIG. 12. Longitudinal view of the obelisk, showing a manner of laying out the lower face, by either measuring directly from a curved top surface, or by the use of a curved centre-line laid with the aid of a string.

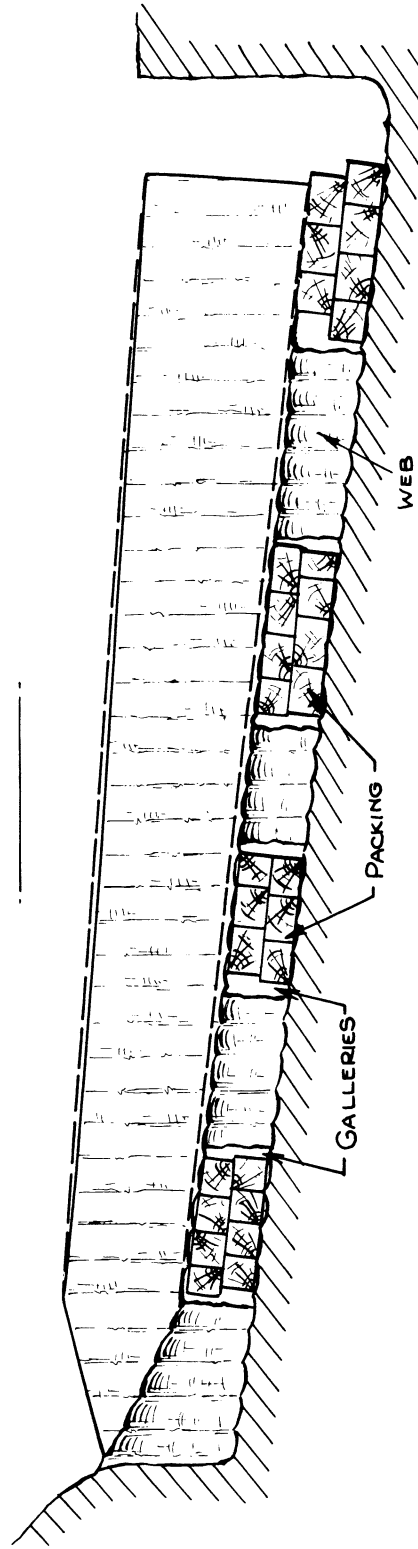


FIG. 13. An advanced quarrying stage, showing the remaining webbed portion of the undercut obelisk broken through, to form galleries, which contain supportive packing.

those shown in the ancient Egyptian tomb drawings,⁹ that is, simply rubbing a stone against the sculpture. A lubricant such as water or oil may be used with it, according to the degree of polish desired. An examination of the polishing motion of fig. 14 will show that as the stone reaches the edges of the top surface, gravity pulls it down and slightly more stone is taken off the edges than the centre. This dressing, would require a great many back and forth motions, and would, I believe, produce the lateral convexity on the south-east face shown in fig. 4. This is a common problem in dressing sculpture and the only way to avoid it is to place a piece of waste stone against the edge, to support the polishing stone and prevent it from dropping. Anyone who has cut the edge of a board with a wood plane would have had the same problem, but the ultimate example is the method of grinding a parabolic lens for a Newtonian reflector telescope,¹⁰ in which a glass blank called a tool is fixed to the end of a barrel and another glass blank which is to be fashioned into a lens is rubbed back and forth across it, while moving around the barrel. The result, if done with care, is a perfect parabolic lens for the top blank. That is, the upper glass or lens becomes concave, while the lower glass, fixed to the barrel, becomes convex.

It should be clear from fig. 15 that no such problems arise when dressing the vertical sides, for gravity assists in keeping the polishing stone on a true course. This produced flat faces for the north-east and south-west sides of the obelisk. The final steps are to separate the monument from the quarry bed and then to dress the north-west face. Engelbach described a method of driving galleries through the underside, packing them with wood or stone, and then removing the remainder of the rock that ties the obelisk to the quarry-bed. This is shown in figs. 10 and 13. All that remained was to turn the obelisk over, so that its north-west face was on top. If this face were dressed in the same manner as the south-east (fig. 16), clearly, the result would be another laterally convex face, as shown in fig. 17. The method by which the obelisk was raised will not be pursued here.

The process of measuring and dressing the obelisk would have had a modifying effect on the surfaces of the obelisk, which can be seen in fig. 1, where the longitudinal bow of the north-west side is $\frac{4}{8}$ in. and that of the south-east side $\frac{1}{2}$ in. Fig. 4 shows the lateral dimensions as $1\frac{1}{8}$ and $1\frac{1}{3}$ in. Considering the roughness of the surface that had been dressed, and the inconsistencies of dressing by hand, the dimensional variations on opposing sides are remarkably small.

Using the evidence from Aswan, I have set forth what I believe to be the method of quarrying which produced the two curious Luxor obelisks. While it can be presumed that all obelisks were quarried in this manner, to my knowledge, no others have been measured with enough care to display these peculiarities. Although it is possible, as Engelbach argued, that, at Aswan, the masons used the optical method of measuring the surface, I believe that, there too, the string method was used. It would be interesting to see whether a careful survey of the top surface of the unfinished obelisk discloses a flat or concave face.

⁹ Clarke and Engelbach, *op. cit.*, figs. 232, 240, 241.

¹⁰ Allyn J. Thompson, *Making Your own Telescope* (Cambridge, Massachusetts, 1947), 38-50.

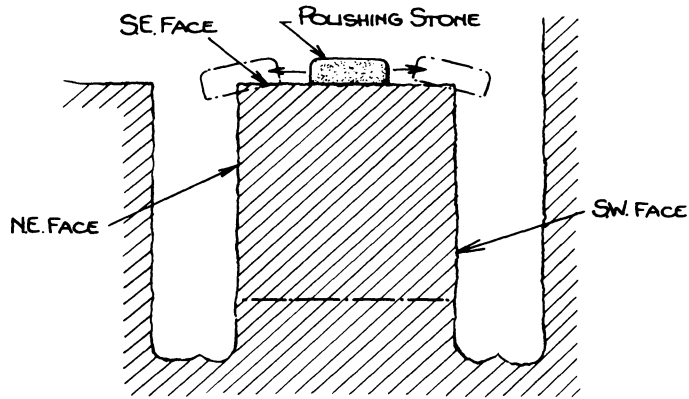


FIG. 14. Lateral section, showing the method of dressing the top surface of the obelisk and indicating how the polishing stone would drop as it overshot the edges.

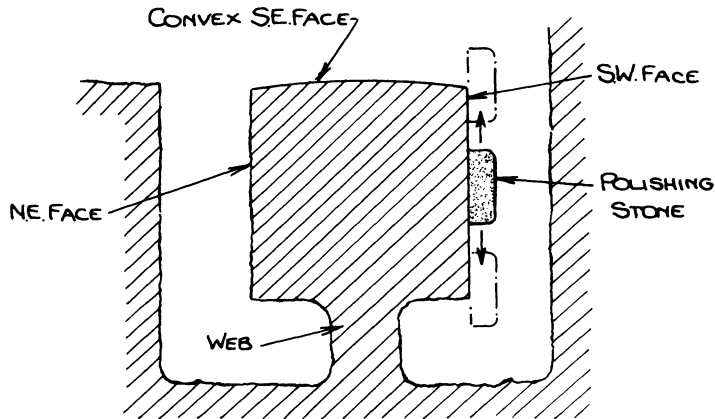


FIG. 15. Method of dressing the vertical sides, with the polishing stone keeping to a true course.

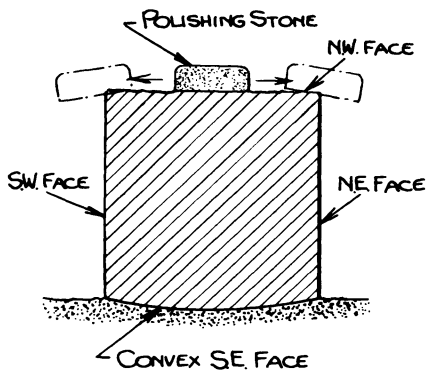


FIG. 16. View of the obelisk, removed from the quarry, turned upside down, and being dressed.

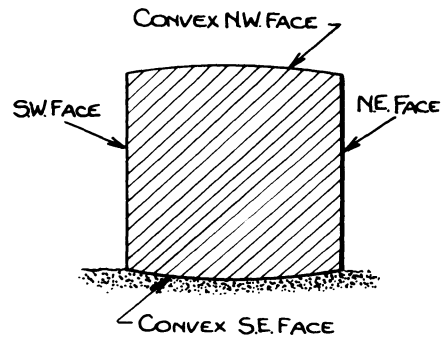


FIG. 17. Lateral section of the finished obelisk, with two flat faces and two convex faces.

I am uncertain whether all the polishing was completed at the quarry site or if a final polish was given at its destination. Damage might occur in the course of transporting the finely finished monument, yet there is the example of the unfinished obelisk of Sethos I, which still lies in the quarry of Gebel Simaan on the west bank opposite Aswan, with its pyramidion *decorated on three sides*.¹¹

Since publication of my earlier paper on the concave faces of the Great Pyramid,¹² in which I argued that sagging masons' line would cause the faces to be dished deepest near the bottom, gradually becoming level as they neared the top, I have come across a reference by Maragioglio which seems to confirm this supposition. He reports that Mycerinus has a concavity on all its faces which appears greater in its lower part and diminishes as it goes upward.¹³

The evidence of the pyramids and the Luxor obelisks suggests that the ancient architects made frequent use of the stretched line in their building procedures. The Great Pyramid, that of Mycerinus, and the two Luxor obelisks may not have been perfect—but they were good enough!

¹¹ Labib Habachi, *The Obelisks of Egypt* (New York, 1977), 32.

¹² Martin Isler, *JARCE* 20 (1983), 27-32.

¹³ V. Maragioglio and C. Rinaldi, *L'architettura delle piramidi menfite*, VI (Rapallo, 1967), 36.

ON THE LOCATION OF THE ADMINISTRATIVE OUTPOST OF THE COMMUNITY OF WORKMEN IN WESTERN THEBES

By RAPHAEL VENTURA

In a recent publication (R. Ventura, *Living in a City of the Dead*), it was claimed that *pꜣ ḥtm n pꜣ ḥr* was the administrative outpost of the community of workmen at the Royal Theban Necropolis, and that its location should be sought near the Ramesseum. The following article provides additional evidence for the location of the *ḥtm*, which substantiates that conclusion. It is based upon P. Turin 1923 + fragments *rt.* 2-8 which mention *pꜣ ḥtm n pꜣ ḥr* in connection with the digging of a well. Combining the numerical data from altitude measurements given in the papyrus with other documentary and topographical pieces of evidence, the author concludes that: (a) the well being dug (under Ramesses VI) was Bruyère's 'Grand Puits', near the northern entrance of the wadi of Deir el-Medina; (b) the *ḥtm* was situated on the slope leading from the Ramesseum to Deir el-Medina, within 300 m of the south-western corner of the temenos of the Ramesseum.

DURING the Ramesside Period, the village of Deir el-Medina housed the members of an important branch of the Egyptian administration known by the name *pꜣ ḥr*.¹ Their function was to prepare the royal tombs and to look after their safety. *Pꜣ ḥr* was directly answerable to the Southern Vizier and consequently its officials were in frequent contact with the Vizier himself, his personal staff and other high-ranking figures in the Theban area, particularly in Western Thebes. In order to meet the special needs of *pꜣ ḥr*, tight co-ordination with other branches of the administration was essential. A site for the royal tomb had to be selected, plans had to be approved, the pace of work had to be monitored, security measures had to be adapted to conditions, visits of high officials to the work area (which, being secret, was out of bounds for all but a very few) had to be pre-arranged, provisions for the workmen and their families had to be sought and their release authorized, breaches of discipline had to be investigated and their perpetrators punished, accounts of progress and information regarding special events had to be submitted. In periods following the death of a king, funerary furnishings had to be transported to the tomb, unfinished work had to be accelerated, last minute problems had to be overcome.²

In order to manage effectively this complex administration, despite the obvious limitations of access that were imposed for reasons of security, a special group

¹ For *pꜣ ḥr* as the name of the administration, rather than a term denoting uniquely the royal tomb in course of construction, see R. Ventura, *Living in a City of the Dead* (Fribourg, 1986), 13 ff. Recent publications on Deir el-Medina have expressed increasing doubt regarding Černý's categorical identification of *pꜣ ḥr* with the royal tomb, e.g. J. J. Janssen, in R. J. Demarée and J. J. Janssen (eds.), *Gleanings From Deir el-Medina* (Leiden, 1982), 135 (c); J. F. Borghouts, in *ibid.* 95 n. 60; D. Valbelle, 'Les ouvriers de la Tombe'; *Deir el-Médineh à l'époque ramesside* (Cairo, 1985), 87-8.

² For some of these activities see J. Černý, *A Community of Workmen at Thebes in the Ramesside Period* (Cairo, 1973), 255-9 and *passim*; Valbelle, *Ouvriers*, 90, 135-47 and *passim*.

of officials was singled out to take care of the important liaison between *pꜣ hr* and the other branches of the administration. It consisted of two scribes of *pꜣ hr* and two chiefs of police, and was identified in the documents of the necropolis by one of the collectives *rwdw (n bnr)*, *hryw* or *hwtyw n bnr*, used interchangeably.³ The village of Deir el-Medina was ill-suited for the activity of the *rwdw*, for it was relatively distant from the centres of administration of Western Thebes, which were situated at the royal funerary temples; in addition, the permanent presence of workmen and members of their families, whose contact with outsiders was severely restricted, would greatly hinder their activity. For these reasons, it was judged practical to have the *rwdw* operate from a more conveniently located site, an outpost, termed *pꜣ htm n pꜣ hr*. Had it not been for the need to keep an eye on the comings and goings from and to the region of *pꜣ hr*, the centre of activity of the *rwdw* could have logically been incorporated within the administrative centre of Western Thebes.

Much has been written about *pꜣ htm n pꜣ hr*,⁴ which is abundantly mentioned in the documents of the necropolis, but the building itself has not been discovered and the question of its whereabouts has remained unsolved. In a recent publication I reviewed the extant textual information on its nature, function, and location.⁵ The evidence points consistently in favour of a site in the general vicinity of the Ramesseum. Since this conclusion diverges widely from what has been published hitherto, and in view of its importance for the understanding of the real function of the *htm* and, through it, of the overall organization of *pꜣ hr*, I present an additional piece of evidence which confirms that result while taking us one step closer towards the determination of its exact location. In order to make proper use of the new information, this document will be treated independently, i.e., without any preconception regarding the nature and location of the *htm*, since these are still in debate.

Most of the *recto* of P. Turin 1923 (+ fragments) deals with the activity of a master builder of the estate of Amon who came to make some measurements and calculations for the digging of a well. The papyrus itself remains unpublished, but a transcription, based upon Černý's reading, has been made available in Kitchen's *Ramesside Inscriptions*.⁶ The pertinent section consists of seven lines, from *rt.* 2 to *rt.* 8, and is independent in its content, though probably not in its date, from the remaining entries of the papyrus. The passage was written in red ink.

³ These officials belonged formally to *pꜣ hr*, but were *n bnr* 'of the outside'. Černý (op. cit.) has discussed their function but has not made a clear distinction between them and officials of other administrations with whom they were in frequent contact. See Ventura, *LCD*, 69-70, 78, 98-9, 178.

⁴ T. E. Peet, *The Great Tomb-Robberies of the Twentieth Dynasty* (Oxford, 1930), 12; E. Otto, *Topographie des thebanischen Gauces* (Berlin, 1952), 64; A. Massart, *MDAIK* 15 (1957), 182 n. 1; Černý, *Community*, 18, 97, 162; idem., *The Valley of the Kings* (Cairo, 1973), 16 n. 4, 19; E. Thomas, *JEA* 49 (1963), 62 and n. 4; idem., *The Royal Necropoleis of Thebes* (Princeton, 1966), 50-1; J. J. Janssen, *Commodity Prices from the Ramessid Period* (Leiden, 1975), 560; idem. in *Gleanings*, 137 (g); Borghouts, in *Gleanings*, 89, 95 n. 60; Valbelle, *Ouvriers*, 90.

⁵ Ventura, *LCD*, 83-106, 178.

⁶ *KRI* vi, 367-8.

Translation

(2) Regnal Year 2(?)^a, second month of the Third Season, day 15. (The) day of arrival by the master builder //////////////

(3) of the estate of Amon^b to measure the well of the front^c of [the village of]

(4) *pꜣ hr*^d to the canal/lake^e of the temple of *Wsr-mꜣrt-Rꜥ-stp-n-Rꜥ* (Ramesseum), l.p.h.^f (down) to (the) surface of (the) water^g.

(5) From the canal/lake to *pꜣ htm n pꜣ hr*: Height^h- //////////////ⁱ cubits.

(6) From^j *pꜣ hr*^k to the well: 26 cubits 5 palms. Total: 60+ cubits //////////////

(7) Remainder to (complete) 22 cubits 5 palms to the surface of the water: 10,^l and (therefore) 10 cubits^m should be dugⁿ to the surface of

(8) the water. Total: 22 cubits 5 palms.

Notes to the translation

(a) The exact regnal year is not clear. Kitchen notes that it was probably 2, or possibly 3. The *verso* mentions Year 2, II *šmw* of Ramesses VI (*vs.* 11) and the two sides of the papyrus have been treated by Kitchen as being contemporary though dealing with different subjects.⁷ That date, however, does not necessarily indicate the time of inscription of the entry in the *verso*, since it is preceded by *r-šꜣ m*, ‘starting from’. An examination of the contents of the *verso* leaves the possibility of year 3 as the time of inscription open.⁸ The king, at any rate, seems to be Ramesses VI, on the evidence of the *verso*.

(b) What is lost in the lacuna, at the end of line 2, must have been the name of the master builder of the estate of Amon. The sequence ‘Title + name + *n* + administrative unit’ is very common in New Kingdom administrative documents.⁹ If the name of a specific institution were lost in the lacuna, we would expect an *m* instead of the genitival *n*. The generic *n Pr-ꜣImn* simply affirms that the master builder in question belonged to the administration of Amon rather than to the royal administration. Normally, *hryw-qdw* could be assigned to individual temples among the major ones of the period (see W. Helck, *Materialien zur Wirtschaftsgeschichte des Neuen Reiches*, I, 43, 45, 102, 103, 113; II, 156, 161, 168 for examples) but it may be assumed that by the time of Ramesses VI, though still active, the administration of the Ramesseum would have been reduced to a skeleton staff of primarily religious character.

(c) *Pꜣ hft-hr* as a masculine substantive occurs also in Ostr. Gardiner 7, *rt.* 2, 10; *verso* 5 (A. H. Gardiner and J. Černý, *Hieratic Ostraca* (Oxford, 1957) I, pl. xxii, 1) in connection with the construction of the temple of Hatshepsut at Deir el-Bahari. There, the *inb n pꜣ hft-hr* ‘wall of the front’ is in contrast to *inb rsy* and *inb ꜣꜣ imnty* etc., which are also mentioned. Wallet-Lebrun has argued that *hft-hr* in building inscriptions refers to the processional axis of a temple.¹⁰ Her conclusions, however,

⁷ See the heading in *KRI* VI, 367.1 where Kitchen states: ‘Year 2 (2 *šmw x* to 15).’

⁸ I shall discuss several passages of the *verso* which are of particular interest in a forthcoming publication.

⁹ See, for example, A. H. Gardiner, *Ramesseid Administrative Documents* (London, 1948), 6, 13, 14; 58, 14-15; 59, 6, 9.

¹⁰ Ch. Wallet-Lebrun, *GM* 58 (1982), 75-94.

are based uniquely on hieroglyphic sources, whereas the present example, and probably the one mentioned above as well, argue for the more straightforward rendering 'front, façade'.¹¹

(d) The short(?) lacuna preceding *pꜣ hr* presents a problem. The temptation to read *pꜣ htm n pꜣ hr*, based upon the occurrence of that term further on in the text should be resisted in view of the general context of the passage, which will be discussed presently. We obviously need a topographical term pertaining to *pꜣ hr* at the front of which a well was being dug. My proposition, *tꜣ whyt pꜣ hr*, 'the village of *pꜣ hr*', will be shown to meet the requirements of the numerical data of this passage. The term occurs on the *verso* of the papyrus (line 18). Valbelle has rightly criticized Černý's interpretation of *tꜣ whyt (n) pꜣ hr* (*Community*, 92, nn. 1-5) which stemmed from an erroneous understanding of the term *pꜣ hr* itself.¹² In her opinion, *tꜣ whyt* was a more comprehensive term than *pꜣ dmyt* 'the village (of Deir el-Medina)' and included, besides the village proper, its surroundings (*Ouvriers*, 89, 121). Elsewhere, she emphasizes that *tꜣ whyt* was not synonymous with *pꜣ dmyt* and that its location is still problematic (Valbelle, *Mélanges Gamal Eddin Mokhtar II* (Cairo, 1985), 316). It is my belief that *tꜣ whyt (n) pꜣ hr* was a more official term for the settlement of Deir el-Medina than the colloquial *pꜣ dmyt* which was the intimate term, used only by its inhabitants (Ventura, *LCD* 184, n. 4). *Pꜣ hft-hr n tꜣ whyt (n) pꜣ hr* would therefore mean the front of Deir el-Medina, i.e., the region that lies immediately to its north, near the Ptolemaic temple of Hathor.

(e) The *mr* of a temple can be either the canal leading to its quay, or the temple's sacred lake (B. Gessler-Löhr, *LÄ* v, 792, 800 n. 8). Though, presumably, both a canal and a sacred lake existed at the Ramesseum, since they were required by ritual, they have not been excavated, nor is their exact position known.¹³ This shortcoming, however, as will be shown, need not affect our understanding of the passage.

(f) The addition *ꜥnhꜣ wdꜣ snb* after the name of a dead king is very common in the Ramesside Period (cf. Gardiner, *RAD*, 54, 11; 57, 2; 58, 4).

(g) *Hr n (pꜣ) mw* is mentioned three times in this passage (lines 4, 7, 8). Its literal translation, 'face of the water', and the context of the passage leave no doubt regarding the meaning of the term, even though the only parallel I can quote is from Coptic (W. Crum, *Coptic Dictionary*, 650 𐩀𐩢𐩣𐩠𐩪𐩠𐩮𐩠𐩰). It should be translated 'the water surface' since the reason for the measurements was to calculate the depth to which the well ought to be dug in order to strike water. Crum translates similarly, and so does W. Vycichl (*Dictionnaire étymologique de la langue copte* (Leuven, 1983), 286). Interestingly, even though the construction *hr n (pꜣ) mw* is an indirect genitive, the term is not defined in both instances (the third is fragmentary). One would expect to find **r pꜣ hr n (pꜣ) mw* or **r hr pꜣ mw*. A similar phenomenon is

¹¹ Wallet-Lebrun (ibid. 94 n. 65) admits that the customary translation 'devant', 'avant', should be preferred in certain contexts.

¹² Černý's proposition that *tꜣ whyt (n) pꜣ hr* denoted habitations of the workmen in the Valley of the Kings stems from his basic theory that *pꜣ hr* could only refer to the royal tomb in course of construction which was situated in that valley. See n. 1 above.

¹³ See, however, B. Gessler-Löhr, *Die heiligen Seen ägyptischer Tempel* (Hildesheim, 1983), 115 ff.

encountered in the idiom *r qʿy n pt* ‘to the height of heaven’ (BM 807, *HTBM* 9, T. G. H. James (ed.), London, 1970, pl. 33/1; Ostr. Turin 57517, J. Lopez, *Ostraca Ieratici IV* (Milan, 1984), pl. 170; Berlin Museum, *Ägyptische Inschriften II* (Leipzig, 1924), 160, no. 20377 l. 1). The words *r-hr n mw* were added above the line as an afterthought; the reason for the additional specification is obvious, since what mattered was not just the elevation of the well compared to that of the lake/canal, but to the surface of the water in it.

(h) The word *hy* normally means ‘height, vertical elevation’, even more so than *qʿy* which has been used in some measurements to denote length (Ostr. Cairo CG 25581 *rt.* 2, 4). It cannot mean simply ‘rise’ which could be interpreted as a diagonal measurement of the distance between two sites of different elevation. That much is made clear by a passage in P. Anastasi I where both heights and diagonals are mentioned, the word *hy* being used for vertical distances only (Anastasi I, 14.3; 15.4-5 = Gardiner, *Egyptian Hieratic Texts*, I, 25, 27, 16*-17*, 32*).

(i) Judging by the total which follows the addition of two successive elevations, one may obtain the approximate value of the number that was lost in the lacuna. By subtracting 26 cubits 5 palms from the total of 60+ cubits, one obtains 33 cubits 2 palms as a minimal value and 43 cubits 1 palm, as a maximal value. In view of Černý’s remark (see footnote 11a in *KRI* VI, 368) that the traces might be read as a ‘10’ or a ‘20’, which can be a misreading for ‘30’¹⁴ but not for 40, we may limit the range of the missing elevation to 33c. 2p.-39c. 6p.

(j) As a marker of the starting point in a measurement (whether in space or time) *(r)-šʿr* is equivalent to *(r)-šʿr m* ‘from, starting from’ (cf. Černý and Groll, *LEG*, 123. For a parallel to *r-šʿr m*, see P. BM 10054, *verso* 2, 3). In calculations of distance, the Egyptians could use either one of the compounds *(r)-šʿr (m)* . . . (*nfryt*) *r* and *r-iwd* . . . *r (-iwd)* (for the latter, see Ostr. Cairo JE 72460 in *KRI* II, 855-6).

(k) Obviously *pʿ htm n pʿ hr* was intended here since this was the intermediate point in the measurement, as follows from the addition of the two distances. Anyway, *pʿ hr*, as a topographical term, was too large and vague to serve as a measuring point (Ventura, *LCD*, 18-22). *Pʿ hr* should be regarded in this example as an abbreviation rather than an omission by error, inasmuch as the point in question was identified by its full name in the previous line, and the passage was totally unambiguous for the people involved.

(l) Kitchen, following Černý, has transcribed *wdʿt.s*. ‘Her/its remainder’, however, is hard to fit into the syntax of the sentence or into the context of the calculation. The short lacuna at the end of line 6 presents an additional uncertainty, but it probably contained no more than the missing part of the numeral representing the total. Megally (*Notions de comptabilité a propos du Papyrus E. 3226 du Musée du Louvre* (Cairo, 1977), 69-78) remarks that in various types of accounts the term *wdʿt* followed by a suffix is particular to the period prior to the New Kingdom

¹⁴ The little that can be seen of the hieratic sign produced in *KRI* VI, 368.11 can, indeed, be interpreted as a ‘30’.

(p. 72). In the New Kingdom the suffix is generally omitted (p. 73; for exceptions see p. 76 n. 5). Whenever the suffix does occur, it is invariably masculine. In the present case, we do not have a feminine antecedent for **wḏꜣt·s*, unless one assumes that it was lost in the lacuna, which is highly improbable. Moreover, one normally expects a numeral to follow directly the term *wḏꜣt*, particularly when it is written in its abbreviated form in a text involving calculations; such numeral is lacking in Kitchen's transcription. There is yet another point which argues against the feminine suffix. Following the total of line 6, we should expect a new sentence to start, serving as the 'opening sentence' for the conjunctive which follows, and therefore independent, whereas **wḏꜣt·s r mh 22 šsp 5 r hr n pꜣ mw* is not a sentence. One way out of this difficulty would be to supply a short verb in the lacuna; for instance, **ḥsb·i wḏꜣt·s . . . mtw·tw . . .* 'I have calculated its remainder . . . and (therefore) one should . . .' could be acceptable, but for the question of the feminine antecedent and the fact that such rendering does not seem to fit the style of the passage. Since we know, from what follows, that the remainder was 10 cubits, and since the sign for '10', if written carelessly, can be mistaken for an *s*, the proposed reading *wḏꜣt 10* may be justified. However, so long as the hieratic version of the text remains unpublished, our suggestion can only be conjectural and subject to verification. Whether or not the reading '10' is accepted does not affect the interpretation of the text, since that figure occurs once again in the same line. The absence of the unit in my proposed reading *wḏꜣt 10* should not be regarded necessarily as an omission since *wḏꜣt* frequently denotes the absolute, mathematical difference, which is devoid of units (cf. Megally, *Notions*, 80).

(m) The signs *mh*, 'cubits', denoting the unit, have been omitted in Kitchen's publication (KRI VI, 368.12). They figure, however, in Černý's transcription of the text (Černý MSS 3.549; 17.16.49). I am indebted to Dr Jaromir Malek of the Griffith Institute for his kind co-operation in providing photocopies of the pertinent pages from Černý's manuscripts.

(n) It is rare to have a conjunctive following a nominal opening sentence. Groll gives only one example, containing a negated conjunctive (Černý and Groll, *LEG*, 450, ex. 1225; see also Borghouts, *ZÄS* 106 (1979), 15 n. 11).

Commentary

This text, as is often the case with ancient documents, does not contain as much information on the subject as one would desire. It was composed for the benefit of the team which was responsible for the digging of the well, so data already known to them, or unnecessary for their purposes, were left out. Thus, for instance, the text does not tell us explicitly whether the excavation of the well had already begun at the time of measurement, and, if so, to what extent it had progressed. Similarly, it does not clarify the source of the number 22 cubits 5 palms which occurs twice, nor does it explain why they should dig only ten cubits to reach water despite the fact that the well was at an altitude of 60+ cubits above water level. Fortunately, the main purport of the text is clear and if one combines the given information with our

knowledge of the region and with pertinent clues from other documents, the probability of error in reconstructing the missing facts is minimized.

We may safely deduce from the data at hand that the master builder had been summoned to measure elevations in connection with the digging of the well, because the workmen had not struck water even though their excavation had progressed to a considerable depth. Moreover, we have to assume that this was not the first attempt to reach the water level in that well, which means that the digging had been carried out in at least two stages. The bulk of the excavation, nearly 40 cubits, had been performed on an earlier occasion. During the current season, an additional 12 cubits 5 palms had been excavated without attaining the desired result, at which stage it was decided to obtain expert advice regarding the amount of work that lay ahead. Having made his measurements, and having concluded that the elevation of the well was 60+ cubits above water level, the master builder subtracted what had already been done on the earlier occasion and obtained the difference of 22 cubits 5 palms. He then subtracted from that difference the work already done during the current instalment, and concluded that 10 additional cubits would be required to reach water level, and in all, 22 cubits 5 palms for that specific season of work. There seems to be no other way of reconstructing the facts in view of the information at hand, nor need there be one, in the light of corroborating evidence which will be presented later on.

To sum up the stages of the digging according to this reconstruction of events:

Initial depth [prior instalment(s)]	c.40 c.
Last instalment, prior to measurement . . .	12 c. 5 p.
Additional depth required to water surface	<u>10 c. 0 p.</u>
Subtotal	<u>22 c. 5 p.</u>
Total:	60+ cubits

Before we proceed with further calculations, we should consider the meaning of the numbers involved. A depth of 60+ cubits amounts to more than 31.5 m.¹⁵ In order to achieve such a depth safely, one needs an aperture several metres in diameter. In view of the systematic excavations that were performed at Deir el-Medina and its immediate surroundings,¹⁶ including its 'front' in the region of the Ptolemaic temple, the existence of a pit of this magnitude would be bound to attract the excavator's attention. The only artificial depression in that vicinity which is comparable to the well of the present passage, is Bruyère's 'Grand Puits'.¹⁷ Bruyère dated the excavation of the 'Great Pit' to the Ptolemaic Period,¹⁸ but he admitted that the evidence of its contents allows for any date from the Twentieth Dynasty onward.¹⁹ He stated repeatedly that the purpose of the pit was to reach water rather than to serve as the shaft for a prestigious tomb.²⁰ *Tḥ šdt n pḥ ḥft-ḥr n pḥ dmyt in the

¹⁵ One cubit is 0.525 m and one palm is one seventh of a cubit, amounting to 0.075 m. Five palms, which figure repeatedly in the text, are 0.7 cubits. See W. Helck, in *LÄ* III, 1200.

¹⁶ Particularly by Bruyère who published his findings in seventeen volumes of *Rapport sur les fouilles de Deir el-Médineh* (Cairo, 1924-53).

¹⁷ PM I², 691-2; B. Bruyère, *BSFE* 5 (Dec. 1950), 69-86; idem., *CdE* 26 (1951), 67-72; idem., *Rapport (1948-51)* (Cairo, 1953), 17-29, 129-30.

¹⁸ Ibid. 26.

¹⁹ Idem., *CdE* 26 (1951), 71-2; and *Rapport (1948-51)*, 129-30.

²⁰ Ibid. 25.

colloquial, or *tꜣ šdt n pꜣ ḥft-hr n tꜣ whyt pꜣ hr* in the language of an outsider (as would have been the scribe accompanying the chief builder of the estate of Amon), is a most appropriate name for the ‘Great Pit’, and the measurements of P. Turin 1923 leave little doubt that it was, in fact, the well in question.

Returning now to the details, we learn from our text that an attempt was being made to reach water level in that pit in Year 2 or 3 of Ramesses VI, and that this was merely a further effort, after some 40 cubits had already been dug at an earlier period. Fortunately, an echo of this earlier attempt has come down to us in Ostr. Deir el-Medina 92.²¹ The inscription is very short, only three lines, but suggestive:

(1) *Hꜣsbt 15, ꜣbd 4 Prt, sw 12. Rḥ(t) bꜣkw nb i-iry m tꜣ šdt:*

(2) *Hr-ḥꜣt: mḥ 36½*

(3) *Bꜣkw hr-sꜣ: mḥ 6½*

Dmd: 43

‘Regnal Year 15, fourth month of the Second Season, day 12.

Amount²² of all work that has been done in the well:

Formerly:²³ 36½ cubits

Work subsequently:²⁴ 6½ cubits

Total: 43 (cubits).’

The ostrakon has been dated to the reign of Ramesses III.²⁵ The overall depth involved is so substantial (22.4 m), that we may safely assign it to this same enterprise. Valbelle,²⁶ assumed that the workmen had been employed away from *pꜣ hr*, since the evidence of this short passage alone would be too weak to invalidate Bruyère’s dating of the ‘Great Pit’, and no other well of these dimensions is known to us from *pꜣ hr*. The possibility that some other well, away from *pꜣ hr*, is referred to in Ostr. Deir el-Medina 92 cannot be ruled out categorically, but the familiarity implied by the mere *tꜣ šdt*, as well as the extraordinary coincidence of its depth with what we would expect for the initial stage in the Turin Papyrus are hard to brush aside. From the ostrakon we learn that even under Ramesses III the digging had not been done in a single stage. This is an encouraging fact, which shows that our theory of several phases has a precedent to rely on. We may also conclude, in view of the 43 cubits attained under Ramesses III, that there had not been any further attempt to deepen the pit until the reign of Ramesses VI.²⁷ Most important of all, it supplies us

²¹ Černý, *Catalogue des ostraca hiératiques non-littéraires de Deir el-Médineh* (Cairo, 1935), pl. 54.

²² For *rḥt* = ‘list/amount’ see L. Lesko, *A Dictionary of Late Egyptian II* (Berkeley, 1985), 68.

²³ For a parallel use of *hr-ḥꜣt* see Megally, *Notions*, 45, 86. Cf. Černý, *Late Ramesside Letters* (Brussels, 1939), 46.2.

²⁴ This takes *hr-sꜣ* as an adverb. Alternatively, *bꜣkw hr(y)-sꜣ* ‘subsequent work’ would imply an adjectival use (‘nisbe’ form) of the preposition. For the meaning of the preposition *hr-sꜣ*, see J. Černý and S. Israelit Groll, *A Late Egyptian Grammar* (Rome, 1975), 119 ex. 415.

²⁵ See Valbelle, *Ouvriers*, 51, table I.

²⁶ *Ibid.* 92 n. 6.

²⁷ In theory, there is room for an additional short instalment, prior to the one mentioned in P. Turin 1923. If we take the absolute maximum value for the height of the well (69 cubits 6 palms) and subtract from it the 22 c. 5 p. of the Turin Papyrus stage, we obtain 47 c. 1 p. This means that the pit could have been, at the very most, 4 c. 1 p. deeper than the 43 cubits mentioned in the ostrakon. Even if such an instalment did take place,

with the datum which led to the number 22 cubits 5 palms in the Turin Papyrus, and enables us to fill in the information that was lost in the lacunae.

By adding the 43 cubits of the ostrakon to the 22 cubits 5 palms of the papyrus, we obtain the exact elevation of the well from the water surface, 65 cubits 5 palms. This was the total of the addition of the two measurements made by the master builder. Consequently, the elevation of *pꜣ ḥtm n pꜣ ḥr* (the intermediate point of the measurement) from the water level in the lake/canal of the Ramesseum, the value of which was lost in the lacuna of line 5, turns out to be 39 cubits (65 c. 5 p. – 26 c. 5 p.). This number is well within the range we had assigned to it by internal calculation (33 c. 2 p. to 39 c. 6 p.). The history of the excavation of the pit, down to the reign of Ramesses VI, may be summarized as follows:

Initial stage(s), presumably under Ramesses III	36 c. 3.5 p.
Second stage, presumably under Ramesses III	6 c. 3.5 p.
First subtotal, presumably under Ramesses III	43 c. 0 p.
Third stage, under Ramesses VI	12 c. 5.0 p.
Fourth required stage to water level	10 c. 0.0 p.
Second subtotal under Ramesses VI	22 c. 5 p.
Grand total measured by the master builder	<u>65 c. 5 p.</u>
This grand total was arrived at by the following measurements:	
Elevation of the <i>ḥtm</i> from the water level at the Ramesseum	39 c. 0 p. ²⁸
Elevation of the well from the <i>ḥtm</i>	<u>26 c. 5 p.</u>
Total elevation of the well from the water level	<u>65 c. 5 p.</u>

In reality, the overall depth of the 'Great Pit', as measured by Bruyère, is much more than that, amounting to 52 m which are approximately 100 cubits, and even at this depth, water was not reached. Hence, it turns out that the calculations of the master builder were wrong, after all. However, since we know by which means he obtained his result of 65 cubits 5 palms, we may conclude that the principal reason for his error does not lie in the accuracy of his measurements, but rather, in his erroneous assumption that the underground water in the region of Deir el-Medina could be found at the same level as in the region of the Ramesseum.²⁹ Such an error would have been natural for a person who was accustomed to well digging in the vicinity of the royal funerary temples of Western Thebes or the large temples in Eastern Thebes. One may assume that, having realized that the prediction of the master builder had been erroneous, workmen went on digging, in one or more attempts until they finally gave up, leaving the bottom of the pit unfinished.³⁰ There is no way of telling when the final stage of the digging took place, and it may possibly have been as late as Bruyère has suggested.

The next step in this discussion, which will eventually lead us to the location of the which I do not believe, the result of our calculations would not be altered in any significant way, since it will be shown that our margin of error is higher than the two metres involved in such an instalment.

²⁸ This number is obtained by subtracting the elevation 'ḥtm-well' from the total which has been calculated exactly: 65 c. 5 p. – 26 c. 5 p. = 39 c.

²⁹ In Bruyère's 'coupe géologique' (*Rapport (1948-1951)*, pl. ii) one can see clearly how much deeper than the Nile water level (which was 10 m lower than in the diagram during the Ramesside Period, see n. 35) the pit ultimately progressed without striking water.

³⁰ Ibid. 24, pl. viii.

htm, will attempt to estimate the real elevation of the 'Great Pit' above the water level of that period. We shall thus be able to check whether the proposition that the well was the 'Great Pit' is valid. We would have liked to check the accuracy of the measurements performed by the master builder as well, but this will not be possible since the margin of error in our calculations will exceed by far any mistakes that a qualified measurer could have made.

Our first problem is caused by the fact that the actual opening of the 'Great Pit' is situated in a depression which, according to Bruyère, is natural.³¹ The extent of the original depression seems to me impossible to calculate, in view of the innumerable excavations that were performed on that spot, both in antiquity and in modern times. Since the depression, as mapped by Bruyère, is quite localized and abrupt,³² passing from an absolute altitude of 100 m to c.90 m in a matter of 15 m distance, I am inclined to believe that most of it is due to the excavation of the pit. At any rate, the logical course of action would have been to measure altitudes to the elevation of the rim of the depression, counting whatever lay in it as part of the depth of the pit. Our maximal starting point should therefore be the absolute altitude (i.e., above sea level) of 100 m, with a probable margin of error of 5 m.³³

The other extremity of our measurement is the absolute altitude of the water level of the Nile in the Ramesside Period. As said before (p. 152), the actual site of measurement of that datum at the Ramesseum is not available at the present. On the other hand, Hoelscher's excavations at Medinet Habu did reveal the ancient quay and go down to the water level.³⁴ Moreover, Hoelscher calculated that the low water of the Nile and the canal at Medinet Habu in the Ramesside Period was almost 66.50 m above sea level.³⁵ The margin of error, there, may be of no more than 20 cm, in view of the fact that its change is about 10 cm per century.³⁶ Thus, the net elevation of the mouth of the pit from water level can be calculated by subtracting 66.50 from 100 which amounts to 33.50 m or 63.7 cubits. This result is impressively close to 65.7 which was calculated from the evidence of the documents. When one combines this confirmation with the description *n pꜣ ḥft-ḥr n ||| ||| | pꜣ ḥr*, and with the lack of any other deep pits in that region, one is left with practically no alternative than to identify our well with the 'Great Pit'.³⁷

We may now proceed to the harder task of localizing *pꜣ ḥtm n pꜣ ḥr*. Several statements can be made a priori regarding the *htm*. It should have stood on the

³¹ *Rapport (1948-1951)*, 10.

³² *Ibid.*, pls. i, ii.

³³ For a convenient map with marked altitude lines and including the funerary temples as well as the region of Deir el-Medina, see: G. and D. Castell, D. Meeks, *Deir el-Médineh 1970* Fasc. I, *Gournet Marcei Nord* (Cairo, 1980), plan 2.

³⁴ U. Hoelscher, *The Excavation of Medinet Habu IV, The Mortuary Temple of Ramesses III*, Part II (Chicago, 1951), 11-13.

³⁵ *Ibid.* 12.

³⁶ *Ibid.*

³⁷ In view of Černý's identification of the term *pꜣ ḥr* with the royal tomb in course of construction, it may not be superfluous to point out that the absolute elevation of the entrance to KV 17 (tomb of Seti I), for instance, is 178 m or 338.2 cubits (K. Weeks, *The Berkeley Map of the Theban Necropolis, Report of the Second Season, 1979* (Berkeley, 1979), 19).

way between the Ramesseum and the 'Great Pit'; it could not be situated within the confines of the Ramesseum or be appended to it from the outside, for in such case, the master builder would have chosen a more prominent point at the rear of the Ramesseum for his measurement. On the other hand, it could not be close to the 'Great Pit', in view of the 26.7 cubits of altitude which separate it from the pit.

The *h_{tm}* was at an altitude of 39 cubits, or 20.28 m, from the Nile water level (p. 157), but only 10.77 m from the level of the quay platform, if one may judge from the parallel at Medinet Habu,³⁸ since the two temples seem to be constructed at approximately the same levels.³⁹ Thus, almost half of the altitude of the *h_{tm}* from water level is taken up by the rise from that level to the platform of the quay. The rest should be attributed to the inclination of the terrain as one proceeds west.

The absolute altitude of the quay was 76.01 m, that of the *h_{tm}* was *c.*86.50 m,⁴⁰ whereas that of the rim near the pit was 100 m. If, in antiquity, the slope leading from the quay of the Ramesseum to the 'Great Pit' had been at a steady angle, these numbers would mean that the *h_{tm}* would have stood near the southern corner of the western temenos wall of the Ramesseum.⁴¹ In reality, the inclination must have been easier in the vicinity of the Ramesseum, and steeper in the region of the pit, which would tend to place the *h_{tm}* further west than the corner of the back temenos wall, at a distance which cannot be determined unless careful excavations are undertaken.

Even though the exact location of *p₃ h_{tm} n p₃ h_r* cannot be pinpointed at the present state of our knowledge, the general region of its location has been conclusively demonstrated. It was close enough to the Ramesseum to be easily attainable by officials who had business with *p₃ h_r* and, being a solitary landmark on the slope, it could dominate the access to *p₃ h_r*, as is implied by its name. The parallelism between *h_{tm}* and *m_{ryt}* in the documents of *p₃ h_r*⁴² ought to warn us against pushing its location too high up the slope, and should advocate for a site within a radius of 300 m from the south-western corner of the encompassing wall of the Ramesseum.⁴³

The following is a theoretical reconstruction of P. Turin 1923, *rt.* 2–8, in which the lacunae have been filled in, and the 'missing' information has been supplied.

³⁸ Its elevation, calculated from data given by Hoelscher (op. cit. 12), is 76.01 m.

³⁹ Cf. Castel/Meeks, op. cit. plan 2. An elevation measurement on the pavement of the palace of the Ramesseum (Hoelscher, op. cit. 78 fig. 52) gives 76.60 m, which shows that our measurements are essentially correct.

⁴⁰ Measuring from below, we add 20.28 m and 66.50 m, obtaining a total of 86.78 m. Measuring from above, we subtract 26.7 cubits (13.9 m) from 100 m, and obtain 86.1 m. The average value is *c.*86.50 m.

⁴¹ The net altitude of the well from the quay being *c.*24 m (100–76.01), and the distance between the two (assuming an approximate site for the quay of the Ramesseum) being *c.*1600 m, the *h_{tm}*, whose elevation from the quay was only 10.5 m (86.50–76.01), would be at a distance of *c.*700 m from the quay. This distance takes us approximately to the south-west corner of the enclosure wall of the Ramesseum.

⁴² Ventura, *LCD*, 89–93 where it is shown that the suggestion proposed by Černý (*Community*, 40) and upheld by Borghouts (*Gleanings*, 89) that *p₃ h_{tm} n p₃ h_r* and *p₃ h_{tm} m m_{ryt}* were two distinct institutions, is unlikely.

⁴³ This margin should be roughly correct whether the measurements were related to the canal or to the sacred lake.

Date: Regnal Year 2/3, second month of the Third Season, day 15.

Heading: (The) day of arrival by the master builder NN of the estate of Amon to measure the well of the front of the village of *pꜣ hr* to the lake/canal of the Ramesseum, down to the water surface.

Measurements: From the lake/canal to *pꜣ htm n pꜣ hr*: Height of 39 c. 0 p.

From *pꜣ htm n pꜣ hr* to the well: Height of 26 c. 5 p.

Total: 65 c. 5 p.

Calculations: What was dug under Ramesses III: 43 c. 0 p.

Remainder: 22 c. 5 p.

What was recently dug: 12 c. 5 p.

Conclusion: Remainder: 10 c. 0 p. to complete 22 c. 5 p. to the water surface.

Suggestions: 10 cubits should be dug to the water surface; total: 22 c. 5 p.

ON STYLE IN EGYPTIAN HANDWRITING

By JAC. J. JANSSEN

The author attempts to demonstrate, on the basis of the hieratic forms of *p'* in the Late Ramesside Letters, that it is possible to distinguish the handwriting of various scribes. Although many similar forms occur, particularly very cursive ones, there appears to be a statistical difference between the forms used by Dhutmose, Butehamun, and the scribe of the General Kenkhnun. When extended to other groups of signs and other manuscripts, the method described may help to date otherwise undatable texts.

I HAVE recently expressed my scepticism regarding the possibilities of dating a hieratic text, other than very roughly, on the basis of individual signs.¹ Valuable as Möller's *Hieratische Paläographie* certainly is, particularly as a tool for students learning to read hieratic, it was not intended for dating letters or account papyri. Most of the texts used by Möller are written in literary characters, and more cursive forms are noted only incidentally. Moreover, Möller illustrated what he considered to be the most typical shapes, usually also the most elaborate ones, without indicating their frequency in the text concerned. This is not meant as a criticism of Möller's splendid achievement; I merely want to indicate the unavoidable limitations of his publication.

I was once more confronted with the matter when studying some Late Ramesside Letters in the British Museum which Černý did not include in his well-known publication, *Late Ramesside Letters* (Brussels, 1939).² This correspondence dates from the very end of the Twentieth Dynasty³ and generally shows the type of hieratic script of that time. However, comparing some original papyri in the British Museum with photographs of letters preserved in other collections,⁴ I became aware of two phenomena: first, the clear distinctions in individual handwriting between the various scribes, which may enable us to ascribe, with a reasonable degree of certainty, a letter (and also an account papyrus) to a particular scribe when, through loss of the address, it is not clear who wrote it; and, secondly, the variety of forms particular signs and ligatures take within a single text, even when it was written

¹ *BIFAO* 84 (1984), 305, repeated in *Varia Aegyptiaca* 1 (1985), 112.

² He discovered them after publishing the book.

³ The date of Letter 41, the only one mentioning Ankhef (as the addressee), who is very probably Butehamun's son Ankhefenamun (see E. Wente, *Late Ramesside Letters* (Chicago, 1966), 15), and also the only one, so far as is known, preserved in the Cairo Museum (CG 58061), is slightly uncertain. It may be later than the rest of the correspondence. See Niwinski, *SAK* 11 (1984), 142.

⁴ I am most grateful to Professor Donadoni-Roveri for photographs of some letters in the Turin Museum, to Dr Chappaz for those in Geneva, and to Dr Müller for those in Berlin. Mr James kindly provided me with a photograph of P. Phillipps (Letter 15), now in the J. Paul Getty Museum, Malibu (no. 83.AI.46). My thanks also go to Mr James and Dr Bierbrier for permission to study the originals in the British Museum and help during my work there.

The plates in Spiegelberg's *Correspondances* are poor, hardly sufficient as a basis for conclusions. Moreover, only P. Bibl. Nat. 196, 11 (Letter 10) was written by Dhutmose, and this text contained on the recto (the verso was perhaps written by Kenkhnun) too few instances of *p'* to allow reliable conclusions.

within a few minutes. Clearly, the latter point hampers our attempts to date a manuscript on account of the shapes of particular signs, although the difficulties may be less severe in the case of rare signs.

My attempts to distinguish individual hands in the letters⁵ led me to consult the Laboratory of Forensic Physics (Gerechtelijk Natuurwetenschappelijk Laboratorium) at Rijswijk, Netherlands.⁶ The first thing I learned was that one has to compare the most common words. The reasons are obvious: since they occur frequently in almost every text they offer a large amount of evidence, and since they are so common, the scribe wrote them almost unconsciously. In noting unusual words or signs he may have been careful, attempting to approach the forms he had been taught, which are the more literary characters, but in the case of very common words he was inclined to write them quickly in a cursive form. This should be kept in mind, since it may mean that, when a common word was carefully written, there was a special reason for this.

After several attempts, one group appeared to fulfil all requirements, namely *pʹ*, in the masculine article itself, and also in the demonstrative pronoun *pʹy* and in the possessive pronouns *pʹy·i*, etc. Nowhere could I establish that what followed *pʹ*—usually *ⲛ* or *Ⲛ*—influenced its shape. Obviously *pʹ* is particularly suitable for our purpose because it has a conspicuous shape which allows several variations, such as the indication or omission of the wings of the *ⲡ*-bird, and the possibility of ligaturing it to the following *Ⲛ*. Moreover, since the majority of persons occurring in the letters, whether as senders, addressees, or otherwise, are men, masculine possessive pronouns are far more frequent than their feminine counterparts. That *Ⲛ* presents less scope for variation adds to the preference for *pʹ*. Of course, other, less frequent, words should also be studied in order to establish whether the argument developed here can be strengthened. For the present purpose, to show how the question of individual handwriting can be tackled,⁷ *pʹ* may suffice.

The most prominent authors of the correspondence known as the Late Ramesside Letters are Dhutmose, also called Tjaroy, and his son Butehamun. Hence, it seems appropriate to begin our study with the shapes of *pʹ* in their letters. Although father and son, which may mean that Dhutmose himself taught Butehamun and thus strongly influenced his style of writing, their *pʹ*'s appear to show clear and consistent differences. Possibly this was due to Butehamun's (other?) teachers—there was a school at Medinet Habu⁸ where the necropolis workmen lived at that time—but individual character may equally well have caused the difference.



⁵ This problem is not too dissimilar to that of recognizing the hand of painters. See for this matter K. Keller, *Newsletter ARCE* 115 (Summer 1981), 7–21.


⁶ Contact with Ir. H. Hardy of the laboratory was made for me by Dr Demarée, to both of whom I offer my thanks for their kind help.





⁷ This seems never to have been done systematically, although some scholars, such as Gardiner and Černý, identically note that (part of) a particular text was, or was not, from the same hand as another text (e.g., in the descriptions to *Hieratic Ostraca* (Oxford, 1957), 1–30). They nowhere indicate on what they based their opinion.

⁸ See *LRL* 10, 13–14 (Letter 5, verso 4–5).

Starting with some letters written by Dhutmose, of which I possess clear photographs,⁹ I quickly realized that the forms of *pʹ* can be divided into three types:

1. carefully written literary signs, mostly occurring in the headings of the texts (e.g., Letter 1,¹⁰ recto 1: *pʹ hr*), which approach the shape of Möller's sign no. 221, particularly that of P. Abbott 2, 3. They are ligatures, the two bird-signs being connected by a curved stroke, whilst the wings of the -bird are indicated by two very short strokes, made afterwards. (.

2. Very cursive signs, never ligatured, which occur throughout the texts, constituting in some the majority. Their exact shape varies, but generally they look like .

3. The type in which the left-hand signs are made more carefully in two strokes of the brush, formed as  and , and generally connected as in . This form of connecting the  with the wings appears to be characteristic for Dhutmose. In Letter 5, for instance, there are ten instances of type 3, as against four very cursive ones; in Letter 1 the ratio is 4 : 5; in Letter 4 it is 4 : 9; in Letter 9, 16 : 14.¹¹

Two conclusions can be drawn. The scribe used, in one and the same letter, three variant forms of *pʹ*. Type 1 is restricted to the conspicuous place of recto line 1, the other two occur indiscriminately, sometimes in the same line.¹² At the end of the letter, on the verso, and in cases where the scribe wished to fit in several words at the end of the line, there is a tendency towards the very cursive forms of type 2, but this is only a general preference, in accordance with the well-known tendency of showing less care at the end of a page than in the first lines.

A second, negative, conclusion is that the ratio of literary and cursive forms is not influenced by the person of the addressee. All four texts quoted above were written to Butehamun, but they show clear variations in the ratios of more or less cursive shapes. Whether Dhutmose wrote more carefully in his correspondence with influential persons such as Hori, the Deputy of The Estate of Amun (Letter 2), I cannot establish since I do not at present possess photographs of this papyrus.¹³ The possibility should be considered since, as will appear below, Butehamun's letters to his superiors show more care than those to his father.

⁹ Apart from Letters 1, 4, 5, and 9, I also used two unpublished letters in the British Museum, nos. 10419 and 10440, but I have not referred to them since I hope to publish them in the future.

¹⁰ Since almost every Egyptologist has Černý's publication at hand, I cite only the numbers of the letters in his book.

¹¹ Some unclear instances, or ones partly lost in lacunae, are not included.

¹² E.g. Letter 5, verso 10. The first two instances (*pʹ tš* and *pʹ Rʹ*) are cursive, the third one (again *pʹ tš*) shows type 3. The two different forms occurring as article before the same noun demonstrate that this use does not exercise any influence. Even the following name of a god (*Rʹ*) did not persuade the scribe to write more carefully.

¹³ After completing the article I received photographs of P. Berlin 10494 (Letter 12), for which I am grateful to Dr Müller.

The nine instances of *pʹ* that occur in this text definitely prove that it was written by Dhutmose, and not by the second sender, Pentahutnakht. No instance of type 1 occurs, but four clear ones of type 3, the other five being more or less cursive. Although the letter is fairly neatly written, the suggestion that it shows a conspicuous contrast in writing to the letters to Butehamun appears to be incorrect. In this respect it differs from P. Phillipps (Letter 15), written by the prophet Amenhotpe to Dhutmose, the recto of which is in an almost literary hand.

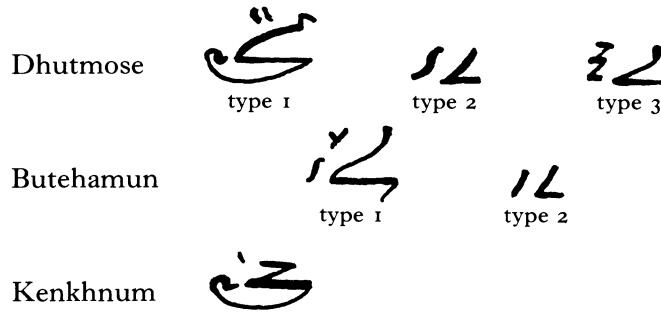


FIG. 1

All four texts studied here, as well as two unpublished examples from the British Museum (see n. 9), date, according to Wente,¹⁴ from years 6 or 10 of *Whm-mswt*. The timespan is too small to allow for any development in Dhutmose's handwriting, the more so since he was already in year 6 a mature man. Although in Letter 4, of year 10, the less cursive form (type 3) occurs approximately twice as frequently as type 2, in the only slightly later Letter 9 the numbers are roughly equal. Letter 5, again, contains more than twice the number of instances of type 3 than of type 2, but it is scarcely later than Letter 1 in which the numbers are equal. We may therefore conclude that the occurrence of either type 2 or type 3 is coincidental.

Before we turn to Butehamun's letters, it may be appropriate to compare our conclusions with the text of the Turin Taxation Papyrus,¹⁵ which records quantities of grain collected by Dhutmose in an area south of Thebes and sent by him to the city. Although he is the author of the text, this does not necessarily mean that he was also the scribe. Moreover, the papyrus dates from year 12 of Ramesses XI, more than twelve years before the correspondence, so that differences in the handwriting might be expected. Although Dhutmose was not a really young man anymore, his writing may have changed during his lifetime. More crucial, however, is the fact that the Turin Taxation Papyrus is an official document, whereas the letters are written to his son. A neater hand is therefore to be expected in the former.

Its recto¹⁶ contains, in the five columns, about sixty recognizable instances of *p'*, others being lost in lacunae or too faint to identify their exact shape. About twenty-five are ligatures, of the form called above type 1 (𓂏) which occurs in the letters only in the first lines. The other thirty-five *p'*'s are more or less cursive, mostly without a connection between the *ʿ* and the wings of 𓂏. Type 3 (𓂏), characteristic of Dhutmose's letters, appears rarely.¹⁷ The forms in verso col. 2, all in


¹⁴ *LRL*, Introduction. For convenient reference, see his table on pp. 16-17.





¹⁵ P. Turin 1895 + 2006 (transcription: Gardiner, *Ramesside Administrative Documents* (Oxford, 1948), 35-44). I possess photographs of this text which prove that Gardiner's remark (op. cit. xiii) that the facsimiles in W. Pleyte and F. Rossi, *Papyrus de Turin* (Leiden, 1869-76), are 'reasonably good' is correct, although there are minor variations in the details of the hieratic signs.

¹⁶ Pleyte and Rossi, op. cit., pls. lxxv, c, clv-clvii, in this order covering most of cols. 1-5 of the recto; pl. xcvi bears the left hand side of col. 5, and pl. ci part of col. 3, which is more complete on pl. clv. Of the verso Pleyte and Rossi, op. cit., gave only a facsimile of col. 2 (pl. xcvi).

¹⁷ Examples are recto 3, 1b (but the first *p'* in this line is of a more elaborate shape) and recto 4, 5a (but the next *p'* is more cursive). (I indicate by a and b the *p'*'s as they occur in one line.)

names, are fairly carefully written but show no ligatures, while those in verso col. 3, also in names, are generally slightly more cursive. They constitute a sort of 'middle' class between types 1 and 2.


The question is whether the few instances of type 3 are proof that the text was actually written by Dhutmose. Obviously we should expect the more literary type 1 to occur more frequently than in the letters. We may also note that the number of very cursive writings increases in the later columns, and the same tendency recurs on the verso. In recto col. 5 only three ligatures occur as against twelve more cursive forms, and there is not a single careful writing in the second and third paragraphs (recto 5, 5-11). It seems to me that the differences between the nature of letters and a record, as well as, perhaps, the distance in time, are sufficient explanation for the variations in the shapes of *p*; that is, Dhutmose was indeed the scribe of the Turin Taxation Papyrus. For an additional argument, I would cite a few intermediate forms between types 2 and 3 (), with the wings and the *p* just not joined up.¹⁸ Whether the habit of connecting them constitutes a later development in the scribe's handwriting remains uncertain. The discussion of the Turin Taxation Papyrus clearly demonstrates the difficulties of handwriting analysis. It is obvious that a single group, distinctive as it may be, is not sufficient evidence definitely to ascribe a text to a particular scribe, certainly not when there is no explicit internal evidence. The wealth of variation, sometimes found in a single line, should warn against too rash conclusions.

As regards the letters written by Butehamun, I have been able to study four instances from photographs.¹⁹ They show two forms of *p*: a more carefully-made shape, used not only in the first line, as type 1 in Dhutmose's letters, but also later in the text, and a very cursive one. Not a single instance of ligature occurs. The former type clearly indicates the wings by two, usually connected, short strokes (), but the  is usually abbreviated to a fairly vertical stroke.²⁰ Another characteristic is that the lower horizontal stroke of  usually ends in a curve downwards (), which is almost never found in Dhutmose's cursive forms. The differences are small, only discovered by careful comparison, but their regular occurrence seems to be decisive.

In Letter 28, written by Butehamun on behalf of the necropolis officials to the General Paiankh, the number of cursive forms is very small in comparison with the more elaborate ones—less than one in every three—and again, they mostly appear at the end of the lines and at the bottom of the recto. The short Letter 29, addressed to the Prophet and Troop-commander Shedsuhor, uses a full form at the beginning of line 4 (*p* *mr mšr*), but further on only cursive ones (five instances). In Letter 8 and 16 verso, both to his father, Butehamun is fairly careful, although cursive *p*'s number about half the total. We may conclude that Butehamun made an

¹⁸ E.g., recto 3, 4a. Similar forms are found in Butehamun's letters, however.

¹⁹ Letters 8, 16 (only the *verso*, since I do not possess a photograph of the recto), 28, and 29. I also used the unpublished P. BM 10411.

²⁰ An exception is the elaborate form at the beginning of Letter 8, verso 4a ().

effort to write neatly when he addressed himself to his superiors, but even here cursive forms are employed.

Comparing these letters with those of Dhutmose, we find clear differences in the forms of *pʿ*. The latter used the characteristic type 3 (𐀓), which is never present in the former's texts, whereas many instances of 𐀔 occur here which are absent from the letters of the father. A single example does not tell us anything useful to our study. It is the statistical preponderance of one form, and the absence of another, that indicates who wrote a particular text.








The hypothesis that the hieratic writings of *pʿ* in the texts written by Dhutmose differ clearly from those of his son, can be tested with the help of three other Late Ramesside Letters, all with roughly the same message. It is the well-known affair of the two policemen who were to be killed.²¹ Of the three, Letter 21 is addressed to Dhutmose (called Tjaroy), Letter 34 to the *rwḏw* Payshuben, and Letter 35 to the Lady Nodjme, the widow of Herihor. The last one begins with a short courtesy formula, but the contents are similar. In the address on the verso the titles and name of Nodjme are followed by *n sš n mr mšr* Kenykhnum, which Wenté, translating *n* by 'to', conceives to be the 'forwarding address'. In Letter 21 Kenykhnum (here called Kenkhnum) is mentioned in the same way, but—although it is a letter to Tjaroy—the first name is that of Payshuben. It seems to me more plausible that all three are written by Kenkhnum (*n* to be translated as 'from'), who was one of the General's secretaries. The almost identical wording of the three letters—although in that to Tjaroy some sentences are added—suggests that they were written immediately after each other, without being exact copies. For our present subject, it is of importance that they show throughout the same characteristic form of *pʿ*, clearly differing from those of Dhutmose and Butehamun. The basic form is 𐀕, with a horizontal beginning at the top of the right-hand part and always a ligature, while a few indicate the wings of 𐀖 by a point or a very short stroke (𐀕'). The latter occurs in the first words of each letter (*pʿ mr mšr*), and occasionally within the text. A ligature of this kind is never employed by Butehamun, and only in recto 1 of Dhutmose's letters. Moreover, whereas Dhutmose writes the wings of the *pʿ*-bird as 𐀗 (in type 3 connected with the ʿ to 𐀗), and Butehamun indicates them by 𐀘, Kenkhnum abbreviates them, if written at all, to a point or a single stroke.²²

Another letter, addressed by the General to Tjaroy, Letter 22, does not show the characteristics of Kenkhnum's handwriting. Here *pʿ* occurs, at least in the beginnings of some lines (recto 1 and 2, verso 1), in an elongated form (𐀕'), the wings indicated as by Butehamun, the ʿ as by Dhutmose. There are also more cursive forms, but these remain nearer to the literary style than in the other letters. None of the three scribes shows all the characteristics of this form. The words of the text mention no scribe, only the General as its author, but it is improbable that he

²¹ First published by A. Erman, *Ein Fall abgekürzter Justiz in Aegypten* (*Abh. Kön. Pr. Akad. d. Wiss.*, phil.-hist. Kl., 1913, Nr. 1).

²² This seems to imply that the last lines of Letter 4, although containing words addressed by Kenkhnum to Butehamun and Amenpnufe, were actually written by Dhutmose. None of the three *pʿ*'s shows the typical form here pointed out.

wrote the message himself. Even this negative result is of some value, for further study of the hands occurring in the correspondence of the General may lead to identification of the scribe.

A final example may be the letter addressed by Henuttawi to the scribe Esamenope (Letter 37). The forms of *p* here vary considerably, but it is conspicuous that in a few instances the  is written more elaborately as .²³ This occurs almost nowhere in any of the texts discussed above.²⁴ On the other hand, in most instances *r* is abbreviated to , while the wings of  are indicated as , , or simply as . All three forms are used by other scribes, but not one of those discussed above uses them all. This does not mean in itself that the lady wrote the letter with her own hand, although neither is there evidence that she used the service of a professional scribe.²⁵ Further study of the handwriting may lead to more definite conclusions on this problem.

The above constitutes a first exploration of a method not hitherto, so far as I know, applied in Egyptology. Its value cannot as yet be measured. I have only attempted to indicate how basic material can be collected. For our insight into Egyptian civilization and its development, the matter seems of small importance, the results providing no more than a means to recognize the actual scribes of some letters which by chance have been preserved. Whether that in future may lead to significant conclusions no one can estimate. However, no scholar should attempt to draw broad lines of history, whether social, political, or economic, or whatever, without from time to time occupying himself with the humble aspects of our science, such as the transcription and translation of unpublished texts.

²³ E.g., recto 5a; 7a; verso 7a.

²⁴ The only instances are Letter 28 (by Butehamun). recto 7 and 10a.

²⁵ I think that literacy was more widely spread in the community of necropolis workmen than C. J. Eyre has suggested (*GM* 61 (1983), 86-91), particularly among women, as some letters by them attest. Nevertheless, Henuttawi certainly played an exceptional role (see Janssen, *Wepwawet* (UCL), 2 (1985), 30-1).

A PIOUS SOLDIER: STELE ASWAN 1057

By J. D. RAY

The stele discussed in this article was found by Reisner during the first survey of Nubia, but, apart from a small published photograph, has been neglected. The narrator, a soldier and priest of Isis at Philae named Petiesi, describes his role in the administration of Ptolemaic Nubia, and the rich donations he made to the temples of his native province. The text is partly in hieroglyphic (with unusual writings), partly in demotic, and the stele is iconographically of interest. It also sheds light on religious sentiment in Late Period Philae.

NEW demotic texts tend to come from excavations, or from the reserves of major museums. But occasionally a text can be on display in a museum, and yet still fail to enter the literature. The stele published here can be seen in the Aswan Museum, on the island of Elephantine, where it bears the number 1057. I am very grateful to Dr Alfi Henri Qaddis, Director of the Museum, who gave me every facility to study the inscription during a visit to Aswan in March 1986, and I also owe a debt to Dr Adel Farid, who has recently completed a detailed bibliography of demotic inscriptions; thanks to him, it was easy to discover that a photograph of the stele was published by G. A. Reisner in *Archaeological Survey of Nubia 1907-8*, pl. 15(a). This photograph, although reduced in size, is so clear that most of the text is readable; indeed, it puts modern technology to shame. Other details become clearer when I was able to inspect the original in Aswan. An enlarged photograph appears on pl. X.

The stele is funerary, from the cemetery of the priests of Isis on the island of El-Hesa in the first cataract.¹ Apart from the brief details in the original excavation report, there is no further mention in the literature. The stele is of sandstone, measuring 98 × 53 cm. Its layout has parallels elsewhere,² but is more elaborate than most. The inscription was found in two separate areas, broken into eight pieces; these have been reconstructed in the museum, and the stele now lacks only a portion to the left of the middle register. At the top of the stele appears the conventional winged disc and two jackals described as Anubis and *Wpy*, or Wepwawet. The motif of the two jackals is frequent in the stelae from this cemetery. The upper register shows the deceased on the right, worshipping eight of the gods of Heliopolis and the great ennead. His name is given as *Wsir P3-di-3st s3 P(3)-Hnm*. In the middle register there is a scene of mourning and embalming; two of the 'sons' of Horus, Hapy and Qebhsenuf, are visible on the right; the other two probably stood in the missing left-hand portion of the scene. These figures wear robes which make them

¹ There is a brief description in *Archaeological Survey of Nubia 1907-8* (for the stelae) and 88-9 (for the burial or burials in question). On the human remains see *idem.* vol. 11 (Cairo, 1910), 66-70, although none are mentioned in connection with this particular burial. For the site in general see PM v 258, and A. Bernard, *Inscriptions grecques de Philae 1* (Paris, 1969), 122-4. A. Weigall, *Antiquities of Lower Nubia* (Oxford, 1907), 56, dates the cemeteries to the Roman period.

² See P. Munro, *Die spätägyptischen Totenstelen* (Glückstadt, 1973), 76-9 on the Aswan stelae in general. Note p. 77 on the use of Osirian motifs and the 'Sons of Horus'.

read either *nhm* or *nb* (S. Sauneron, *L'Écriture figurative dans les textes d'Esna* (Cairo, 1982), 122); hence the suggestion here, which makes a contrast with the previous *ikbtyt*.

(f) *ꜣt* is a possible reading, but the epithet *mnḥt ntrw* is standard for Nephthys (cf. R. Lanzone, *Dizionario di Mitologia egizia* II (Turin, 1881), 363).

(g) The group with the falcon holding a flail can be read either as *ntrw* or as *bꜣw*; for the epithet see the note above. The word recurs later in the line, and either *bꜣw* or *ntrw* is a possible reading for the inhabitants of a necropolis, but I have chosen *ntrw* in each case (cf. Sauneron, *Écriture figurative*, 140).

(h) *Dw* is written with the sign for three desert hills (*ḥꜣst*), but the same occurs earlier in the line in the epithet *tpy ḏwꜣf*, and there can be no doubt about the reading.

(i) The group is $\overline{\text{om}}$; the values *t* and *m* for the first two signs are occasionally attested, and the reading *tmt* gives good sense.

(j) Imsety and the two final 'sons' of Horus have feminine determinatives. For Imsety as occasionally female, see M. Heerma van Voss, *L'Ā* III, 53, but here the idea seems to have been extended to at least two others. The lack of a feminine determinative for Hapy alone is strange and is probably an omission. In the vignette above, Hapy appears with Qebehsenuf wearing a female(?) robe, and it looks as if we are dealing with four 'daughters of Horus'. Could this be a local tradition, imposed by the importance of Isis at Philae?

(k) For the writing with the *ḥnw*-barque see P. Derchain, *Sacrifice de l'oryx* (Brussels, 1962), 31 ff.

(l) This is the name of Petiesi's locality, and looks as if it is to be read *Wꜣt*, but the demotic equivalent (lines 1, 7) clearly has *Spe*. This suggests that we should read the hieroglyphs as *spꜣt* 'nome'. This is unlikely to be a specific place name, but is presumably the home province of the narrator; hence perhaps the lack of the definite article. For *ḥꜣt-spꜣwt* as a name of the province of Aswan, see H. Ricke, *Die Tempel Nektanebos' II* (Cairo, 1960), 49.

(m) In Middle Egyptian this *iw* would be hard to explain, but in Late Egyptian it could be the prefix of the past 'participle', elsewhere written variously as *i*, *r* or occasionally *iw*. Unfortunately the demotic equivalent (line 1, end) is missing.

(n) Written as a monogram ($\overline{\text{d}}$!).

(o) The group is $\overline{\text{ow}}$, which looks like *m-ḥꜣ(w)ꜣs*, but *m-ḥnwꜣs* gives better sense. The pronoun should refer to *spꜣt*.

(p) The form *irḥ* is unusual (either phonetic or a false archaism). The position of the *nsw* after the first word shows that the normal title *rḥ-nsw* cannot be meant; *rḥ-pr-nsw* is what is intended. It is interesting that such an important title should be held by a native Egyptian: see commentary below.

(q) *sty* is clear, but the form of *tꜣ* is strange. No other reading suggests itself, and the title *irḥ-pr-nsw* should be followed by an important toponym. It presumably corresponds to *Pꜣ-tꜣ-nḥs* in line 2 of the demotic version, which seems to be a vague paraphrase of the hieroglyphic.

BIFAO 30 (1931), 128, publishes some evidence for an early cult). But the presence of these gods in the middle of a list of epithets honouring Isis is difficult to explain otherwise. For Amun as an original god of Philae see E. Winter, *LÄ* IV, 1025.

(d) See textual note above.

(e) The area in Biggeh associated with the burial place of Osiris; H. Junker, *Das Götterdekret über das Abaton* (Vienna, 1913), 35; P. Montet, *Géographie de l'Égypte ancienne* II (Paris, 1961), 20, 28; Gauthier, *Dict. géog.* VI, 125. However, since the list of gods is arranged in descending hierarchy, and since Biggeh (*Snmt*) has been mentioned at the beginning, it is possible that *Dw-q* here refers to El-Hesa itself, the findspot of our stele.

(f) See textual note above for the feminine genders implied. On the 'sons' of Horus in general see P. Munro, *Festschrift 150jährigen Bestehens des Berliner ägyptischen Museums* (Berlin, 1974), 195–204.

(g) Reading confirmed by the demotic equivalent. On *stn*, corresponding to the Greek *σημείον*, see E. Lüddeckens, *Festschrift S. Schott* (Wiesbaden, 1968), 82 n. c, and E. Van't Dack, *AfP* 19 (1969), 155 ff.

(h) See textual note (l) above.

(i) See textual note (m) above.

(j) See textual note (p) above.

(k) See textual note (q) above.

(l) See textual note (t) above.

The demotic text follows at the beginning of the next line. Note the skill with which the artist has arranged a line-division between *n k n* and the name and titles of the deceased (lines 4/5), and has succeeded in 'justifying the margin' at the end of the hieroglyphic text. The whole stele is a thoughtful piece of work.

Demotic text

1. [*Wsir P₃-di-3st s₃ Pa-Hnm*] *mwt·f Ta-Hnm gl-šr^a*
Yb₃^b fy stne h₃ Spe^c hr h₃ p₃ 5 stnew [i·ir šms]^d n₃ ntrw
2. *P₃-t₃-nh₃ iw·w dit n·f šhy^e hn n₃y·w irpyw iw*
Pr-^c p₃ i·ir hn-s w₃h ir nfr n-[drt]^f
3. *P₃-di-3st dd·f rmt nb n₃-gm mdt^g nty iw·w iy r*
smne(?)^h n₃ hbw n₃ hsw r·di 3st cy·w r·i·iryⁱ mdt·ntr
4. *my p₃y·tn r-IO (n) n₃y·tn ntrw m·ir dlh₃ n₃ mdt·w*
n₃ ntrw [ir·i(?)] iwne(?)^j n P₃-t₃-nh₃ ir·i šms
5. *Pr-^c 4 nhm-t(y) n₃ ntrw r t₃y·w wt^k in·w-*
t(y) r p₃y·(i) dmi (n) rn n₃ mdt·w·ntr r·i·ir·i^l
6. *mnqe w^ct bynt nbw r Yb₃ kt r Pr-iw-lq*
wb₃ wn-hr·s(?)^m [tw·i mn]qe w^c grg
7. *hbyn r p₃ cy·-cš Hthr tw·i h^dweⁿ w^ct wmt₃(t)^o*
nbw Spe n₃y(?)^p ir·i-s
8. *di·i mnq·w^q w^ct ph-^ch(?)^r [r(?) ht]-ntr Pr-iw-lq.*

Notes to demotic text

(a) The reading *gl-šr* is clear, but there is a determinative (ϣ) which is absent from the examples in Erichsen, *DG* 588, except for the final one, which is exceptional. The same sign, however, appears in *Mdw/Mty* (*DG*, 195), from which it may have been borrowed.

(b) The form *Ybʿ* is regular in this text. For a similar form *Ybe* at the cataract see F. Ll. Griffith, *Catalogue of the Demotic Graffiti of the Dodecaschoenus* (Oxford, 1937), Glossary no. 890. It may reflect a local pronunciation, although Greek forms such as *Xνομωνεβιηβ* do not suggest a final vowel.

(c) See note (l) to the hieroglyphic text above. The reading is clear here and in line 7. For the forms *spʿ* and *speʿ(?)* see *DG*, 427. It is noticeable that the demotic does not make use of the more common and more general word *tš*.

(d) Reconstruction based on line 5 of the hieroglyphic text, but tentatively.

(e) Suggested by the traces; the hieroglyphic text does not have a parallel. For the phrase *di·w | di·i n·k ihy* cf. P. Dodgson verso line 9, recto line 5 (Griffith, *PSBA* 31 (1909), 104 n. 26).

(f) Again suggested by the traces, although I cannot parallel the use of *n-drt* after *ir-nfr*.

(g) *mdt* is clear, but *tʿy mdt* is an unlikely reading and makes for bad syntax. *gm* is therefore preferable. *nʿ* for the first future is unusual, but the writing may be influenced by the similar prefix of adjective verbs. See also note (f) to the translation below.

(h) The initial sign looks like *h*, but this is not promising, unless we read *hbn* for *šbn* (*DG*, 499), which is unlikely. If the first sign is really a badly formed *s*, we may read either *sbte* or *smne*; for the similarity, see Griffith, *Dodecaschoenus*, Glossary no. 302.

(i) Here the form *r·i·ir·y* should be imperative (*ⲁⲓⲓ*), in view of the following *my* and *m·ir*; in line 5 an identical form appears, but the syntax must be different. See note (l) below.

(j) Another difficult group. The surface of the stele is damaged here, and even *ir·i* is uncertain. The word resembles *iwn* 'colour' or *iwn* 'cargo', but with a house-determinative. There are hieroglyphic writings of *iwnn* 'sanctuary' which are close to the present example (*Wb.* 1, 55), but this is not completely convincing. *Swnt* 'market' is less likely.

(k) For *wtt* *ⲟϣⲏⲧⲉ* 'lightning' see Griffith, *Dodecaschoenus*, Glossary, 83; for the metaphor see Philae 244, end (the 'lightning' of Isis as a punishment). The construction *nḥm r* does not appear in *DG*, but cf. Crum, *CD*, 244a.

(l) This group is written identically to the imperative in line 3 above, but this is not appropriate here. The natural interpretations are either past relative ('the acts of piety which I made') or second present governing *mnqe* in the next line. The relative interpretation goes fairly well with the writing, but leaves *mnqe* unattached (it would be better to assume that *tw·i* has been left out in the change of lines). The second

present explains *mnqe*, but leaves us with an unusual writing, and the fact is that the first present, not the second, is used in the following clauses. In the translation I have assumed the first explanation.

(m) A difficult group. *wbʿ* seems clear enough, as does the following *wn*. After this is a group which could read *hr*, although the ‘flesh-determinative’ is rather elaborate. After *wbʿ* we would expect a noun or an infinitive; for *wnh* ‘reveal’ written *wn-hr* see H. Brugsch, *Thesaurus* 972 (Serapeum), and W. Spiegelberg, *Mythus* (Strassburg, 1917), Glossar, 263, but the first element is always written as *wn* ‘open’. Text offered with reserve.

(n) So the traces, although there is slight damage to the stele at this point. The word probably corresponds to *hd* ‘vergolden’ (*DG*, 282); note the writings *hd* and *hd̄* in Griffith, *Dodecaschoenus*, Glossary, 223 and 255; the latter writing occurs in graffito Philae 254 (id. 84), a text which has several points in common with our stele.

(o) This word is also difficult to identify. *Ššt* ‘window’ would be possible if it were not for the determinative; the traces are too long for *wṯ* ‘stele’, which is unlikely to be gilded. If the preceding *wrt* is correct, *wmṯt* is the most likely reading; for the determinative see *DG*, 87.

(p) There is a horizontal stroke before this word which may be a space filler. *Gy* is a possible reading in view of the following words (for *Ḡἶρῶν* etc. see W. Vycichl, *Dictionnaire étymologique* (Louvain, 1983), 346), but the object pronoun *-s* is difficult to explain in such case. If *nʿy ir-i-s* is to be read, it must be intended as a summing-up of the previous achievements. The following line may well be an afterthought, something which the narrator did not do himself, but caused to be completed.

(q) In this case *mnq* is written without the final *-e*, and is probably the *sdm:f* after *di-i*. The writings in line 6 are probably infinitives with a final vowel, as often in Akhmimic (cf. forms like *NOYQME*). For similar final vowels see *stne* in line 1, *smne(?)* in line 3, *hdwe* in line 7, *Ybʿ* (note (b) above) and *Spe* (note (c) above).

(r) The element *ph* is clear, but the final signs are difficult owing to surface damage. The sign towards the end may have been a wood-determinative. There is no word *ph:t* attested, but it may be a variant of *ph-ḥrt* ‘Riegel’ (*DG*, 138), if this is the true reading (cf. Spiegelberg, *Mythus*, Glossar, 275).

Translation of demotic text

1. [Osiris Petiesi son of Pakhnum], his mother being Takhnum, *kalasiris* of Elephantine and standard-bearer^a before (the) Nome^b at the head of the five companies,^c [servant(?) of]^d the gods of
2. Nubia, who gave prosperity to him within their temples, Pharaoh being the one who commanded this^e and who did good by(?)
3. Petiesi. He says: ‘Every man shall prosper^f who shall come to enrich^g the festivals and the acts of praise, which(?) Isis has made great. Observe the affairs of the god(s);

4. give your one-tenth^h to your gods. Do not neglect the interests of the gods. [I made] sanctuary(?) inⁱ Nubia, I served
5. four Pharaohs. The gods rescued me from (the wrath of) their lightning, and brought me to my home-town because of the works of piety which I performed.
6. (I) completed^j a harp of gold for^k Elephantine, and another for Philae [with a view to its revelation.^l I] completed a bed of
7. ebony for the house-of-appeal^m of Hathor. I gilded a [doorway]ⁿ of gold for the Nome.^b These things I did,^o
8. and I caused to be completed a doorbolt(?)^p for the temple of Philae'.

Notes to the translation

(a) See note (g) to the translation of the hieroglyphic text. It is interesting to note how the military titles of Petiesi predominate in the demotic version, while the hieroglyphic text lays emphasis on his priestly duties.

(b) See note (l) to the hieroglyphic text.

(c) The hieroglyphic text has merely 'standard-bearer before the Nome', again less specifically. But if we combine this information, it appears that Petiesi was standard-bearer to the five regiments of his province, an office which he may have combined with 'acquaintance of the king's house in Nubia' (hieroglyphic text, lines 5-6). See commentary below.

(d) Restored from the hieroglyphic equivalent.

(e) There is no copula to link *Pr-ꜥ* and *pꜣ iꜣr ḥn-s*, but the translation is unavoidable.

(f) The problem lies in the meaning of *gm mdt*. The natural equivalent would be 'find out, discover', but the context demands more. Perhaps the phrase means 'find wealth', or perhaps 'acquire knowledge', but I cannot quote a parallel.

(g) See textual note (h) on p. 174 above. The possible readings point either to verbs meaning 'celebrate' (*šbn*) or 'uphold, make prosper, equip' (*smne* or *sbte*). The latter suit the context well; see commentary below.

(h) *r-IO*, cf. *ꜣMHT* (Crum, *CD*, 188a), here either as a tax or as a voluntary contribution. See commentary below.

(i) See note (j) to the text on p. 174 above. The context demands a reference to religious interests (hence not 'cargo-place'), but the reading is doubtful. The force of the *n* is therefore hard to determine; perhaps 'in Nubia'?

(j) See textual note (l) on pp. 174-5 above.

(k) The rare use of *r* as dative before a place name, also in the next line and probably to be restored in line 8; cf. the uses given in Spiegelberg, *Grammatik* (Heidelberg, 1925), §279a). Compare the use of *n* before *Pꜣ-tꜣ-nḥs* in line 4 above.

(l) See textual note (m) on p. 175 above. The phrase probably means that the harps were shown to priests or the public on festival occasions.

(m) For this chapel see Junker, *Götterdekret*, 4 where it is said that Sekhmet had a sanctuary on Philae, *ꜣt n ꜣš*, 'wo sie als Hathor wohnte'. The force of the *ꜣš* is obscure;

Junker translates 'Rufhaus'. On Aphrodite (= Hathor) at Philae see A. Bernand, *op. cit.* I, 153-7; she is so well attested that one wonders if the reference to Sekhmet is necessary. For representations of harps in the reliefs of the Hathor chapel see Weigall, *Antiquities of Lower Nubia* (Oxford, 1907), 54-5.

(n) See textual note (o) on p. 175 above. For the translation 'doorway, pylon' rather than 'tower' see Spiegelberg, *Demotica* II, 32.

(o) See textual note (p) on p. 175 above.

(p) See textual note (r) on p. 175 above.

Commentary

Stele Aswan 1057 was found in 1907-8 during the first survey of Nubia. It is associated with graves 98 and 100 in cemetery 3 on the island of El-Hesa, the burial-place of, among others, the priests of Isis in Philae; this alone suggests that its owner, Petiesi son of Pakhnum, was an important man. As such he earns a brief mention in the *Prosopographia Ptolemaica* (III, 5740). A date in the Ptolemaic period is likely enough from the writings in the hieroglyphic text.³ The demotic bears out this impression, but several words appear closer to the forms used in the Roman period (compare the writings of *gm*, *hḏwe* and perhaps *dmi* as well as the first future *nṣ-gm* in D3). It is possible, then, that Petiesi was active towards the end of Ptolemaic rule over Egypt and Nubia, although the inscripational remains in Greek from El-Hesa are concentrated on the period of Ptolemies VI-VIII.⁴ Some, if not all, of this, however, was imported from Philae, and, on balance, it appears that we should assign to Petiesi a later date. It is unfortunate that no other trace of him seems to survive.⁵

The stele of Petiesi, son of Pakhnum, gives useful sidelights on the religion of Late Period Philae: there is the interesting list of the gods of Philae, including Amun, in H2, and in the same line unusual titles of the principal goddess, Isis. The hieroglyphic text also gives us an unusual and full variant of the standard-offering formula, introduced by the irregular writing *hḏpt* for *htpt* (H1), and we also find the four 'sons of Horus' in female forms, both in the text (H3) and in what remains of the vignette. This may reflect a local tradition of some sort.⁶ But it must be admitted that the interest of our stele is not primarily as a source for later Egyptian religion; it is the historical and the social information that it contains which make it worth our attention.

In the hieroglyphic version Petiesi appears firstly as a priest, prophet of the temples of Khnum and Isis (H5). Isis here is clearly Isis of Philae (compare the opening lines of the hieroglyphic text) rather than the smaller cult of Isis in the town

³ In this commentary I have used H1-7 for lines from the hieroglyphic part of the stele, and D1-8 for the demotic.

⁴ A. Bernand, *op. cit.* 122-4.

⁵ There is a prophet of Isis named *Pṣ-di-3st* in a Ptolemaic sealing published by M. A. Murray (*ZÄS* 44 (1907-8), 66 no. 15). This probably comes from Philae, not Edfu, as the author suggests (*idem.*, 63), but the father's name is not reconcilable with *Pa-Hnmw*.

⁶ See note (j) to the hieroglyphic text on p. 171 above.

of Syene.⁷ The cult of Khnum is therefore probably that of Philae, rather than the main centre on the island of Elephantine, but the latter cannot be ruled out in view of the title *gl-šr Ybꜣ* (D1) and the donation which is made to the island, presumably to one or more of its temples, in D6. Petiesi also claims a general concern for the gods of his nome (H5), and perhaps for the gods of Nubia as well (D1-2); but in spite of this far-ranging piety, he is clearly not one of the high-priests of Philae. These priestly duties seem to be ones which have been acquired in the course of another career, and the demotic text on our stele makes it clear that Petiesi was first and foremost a soldier. In D1 he is a *gl-šr Ybꜣ* 'warrior of Elephantine' (Elephantine is well known as a garrison-post from the Twenty-sixth Dynasty onwards).⁸ He is also described as a 'standard-bearer before the five regiments' (D1), and the whereabouts of these regiments is made clear in H5, where he is said to be the standard-bearer 'before the Nome'. Essentially, therefore, he occupies a high position in the local armed forces, and these armed forces are in one of the strategic defence zones of Egypt. It was probably this key rank which brought him to the attention of one of the Ptolemies, since he describes himself in H5-6 as *irḥ-pr-nsw n Tꜣ-sty* 'acquaintance of the king's house in Nubia'. This title, which seems to be unparalleled, suggests an exalted rank, perhaps corresponding to the *συγγενεῖς* of the Ptolemaic court, but this is probably an exaggerated view of the matter.⁹ The principal governors of Nubia and the Decaschoenus are known for our period,¹⁰ and while it is always possible to find a suitable gap for Petiesi and to assign him to it, it is more likely that a native Egyptian officer would have held a trusted, but inferior, rank in the Ptolemaic hierarchy. Indeed, it is quite possible that during the troubles which are implied in D5, it was this subordinate rank which ensured his survival, and the title *irḥ-pr-nsw n Tꜣ-sty* may have been essentially honorific, bestowed by one of the four kings that Petiesi served, and perhaps by the last in the sequence as a reward for his long service. It is interesting that no demotic equivalent is given for the title. There is a passage in D4, *ir-i iwne(?) n Pꜣ-tꜣ-nḥs* which is clearly a comment on Petiesi's activities in Nubia, but the key word is unfortunately obscure.¹¹ In general, however, Petiesi son of Pakhnum emerges as a local army officer, who was entrusted by the king, or by his governor, with some control of the affairs of Nubia. This control, as so frequently in Nubia, was exercised through the local temples, and Petiesi would have acquired much of his authority through the priesthoods that he held in Philae. The combination of military and priestly rank was not merely a social convention; in Nubia it made political sense as well.

It is clear from this that Petiesi was in a position to enrich himself, and he states this expressly in D2, where he ascribes his wealth to the gods (the phrase 'in their

⁷ For which see E. Bresciani, *Tempio tolemaico di Isi ad Assuan* (Pisa, 1978). It is interesting that Petiesi makes no mention of Syene in our text.

⁸ *PP* I, 3049 is one Pachnoumis, a soldier of the garrison at Elephantine in 121-19 (P. dem Berlin 13597). This could be the father or our Petiesi, but unfortunately the name is common.

⁹ On the *συγγενεῖς* see, for example, A. Bernand, *op. cit.* I, 179 (n. 5)-80, but note the reservation on p. 238.

¹⁰ L. Mooren, *La hiérarchie de cour ptolémaïque* (Louvain, 1977), 127-30.

¹¹ See notes (i) to the translation on p. 176, and (j) to the text on p. 174, of the demotic version.

shrines', *hn nꜣy·w irpyw*, may be cosmetic, but could imply that much of his prosperity came from the priesthoods that he occupied). However, he is at pains to add that Pharaoh himself approved of this and did good by him (D2). This may refer to wealth obtained during his military career, although it would be wrong to make a sharp distinction between the secular and the religious sides of his activity. Like many men to whom success has come, he succumbs to the temptation to give the recipe: piety must come first, and no occasion neglected to further the interests of the gods (D3–4). Particularly interesting is his instruction to give a tenth of one's property to one's gods (*my pꜣy·tn r-IO n nꜣy·tn ntrw*, D4). The figure of one-tenth is known as an equivalent for the ἀπόμοιρα and related taxes,¹² and it also occurs frequently enough in the inscriptions from Philae,¹³ but, although there was a multiplicity of taxes in Graeco-Roman Egypt, it is unlikely that Petiesi is merely reminding his readers to pay one of them. There were policemen who could do that more effectively, although the state might not have enforced the payment of a temple tithe. It looks more as if the reference is to a voluntary contribution of some kind, an ideal expression of a person's religious loyalty. As such, it is worth noting that the address in lines D3–5 is directed at prosperous pilgrims to the rites of Philae, 'which Isis has caused to be great' (D3).¹⁴ It is this class of people who would be most likely to return home full of pious feelings, and who would be in a financial position to express those feelings tangibly. At Philae itself the tithe is frequently mentioned, and may have been compulsory to visitors; the present text seems to extend this to a universal principle.

Whatever the exact source of Petiesi's wealth, it is clear that at some point in his career he encountered trouble. In D5 he describes how the gods rescued him 'from the wrath of their lightning', and brought him safely home because of the works of piety which he had performed. This is clearly a veiled reference to an event, or events, which could have appeared as some sort of divine retribution. The reference may be to a natural disaster, but it is more likely, in view of Petiesi's military career, to apply to warfare, and this leads us to examine the preceding sentence, where he says that he served four Pharaohs (D4–5). If we assume that Petiesi's active career lasted forty years at the most, it is clear that we are dealing with short reigns or with a period of some turmoil. On the dating scheme proposed above, candidates for these four kings could be Ptolemy VI, his brother Ptolemy VIII and the ephemeral Eupator and Neos Philopator; or the various combinations which characterized the war between Soter II and Alexander II; or Aulettes, Berenice IV, Cleopatra VII (with or without Caesarion), and possibly Augustus.¹⁵ A later date—such as one involving the year of the four emperors (AD 69)—is probably to be excluded on

¹² G. Mattha, *Demotic Ostraka* (Cairo, 1945), 52–3.

¹³ Griffith, *Dodecaschoenus* Glossary, p. 196.

¹⁴ It is probable that the words *i-di 3st ꜣy·w* refer to the rites and festivals mentioned immediately beforehand, even though the phrase constitutes a truism. Nevertheless, the address is clearly aimed at the influential, and it is just possible that the words refer back to the people themselves, 'whom Isis has caused to be great'.

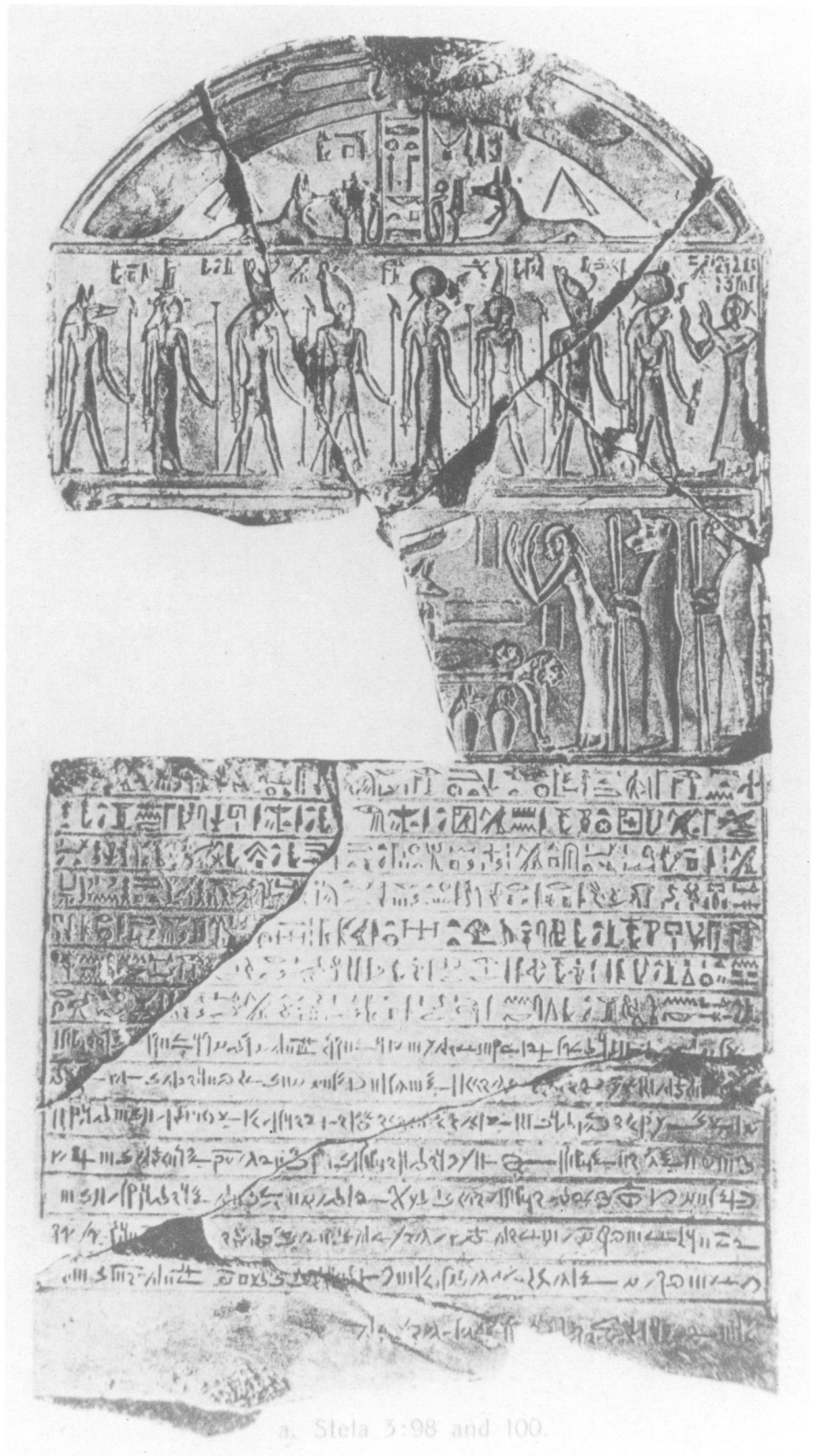
¹⁵ The latter possibility would make Petiesi a contemporary of the events in the trilingual inscription of Callimachus (Turin, 1764); on which see the forthcoming bibliography by Adel Farid mentioned in the introduction to the present article.

palaeographic grounds. Any of the Ptolemaic options listed above would have given Petiesi an excellent opportunity of finding himself on the losing side, or of a narrow escape in battle, either against the troops of a rival Pharaoh or, less likely, against the Nubians. (The political history of Nubia in the late second and first centuries BC was not eventful.) Nevertheless, he clearly survived, and it is always possible that he took advantage of the troubled times to enrich himself, legitimately or otherwise. It is perhaps in this light that we should see the pious donations which he lists for us in D5-8. In each case he chose the most costly materials: a golden harp for a temple in Elephantine and a similar one for the temple of Philae, which were apparently intended to be seen on public occasions (D6); an ebony couch or bed for the chapel of Hathor on Philae (D6-7)—ebony being a product of the south, although at this period the same cannot necessarily be said for gold—and the gilding of an entire gateway, or pylon, for the nome; in other words, in some public place. The last is no mean gesture, and must have entailed considerable expense, but Petiesi also states, rather as an afterthought, that he paid for the completion of a doorbolt(?) for the temple of Philae (D8). This may sound unimpressive, but one of the double bolts for a major door in a temple such as that of Isis on Philae would have been a considerable object, especially if it were made of some costly material. If Petiesi were following his own precept here, and giving a tenth of his income to his gods (D4), then he must have been a rich man indeed. He may, however, have gone beyond his own limit in his enthusiasm. Some idea of the wealth of Philae, even in the middle of the third century AD, can be obtained from Philae 416, the account of a Meroitic envoy who donated ten talents of silver to the priesthood of Isis, followed this with a second similar gift, both from his royal master, and in his own right gave the tenth part of the gold coins which had been entrusted to him for the goddess, the whole being melted into a golden *qbh*-vase weighing $4\frac{1}{2}$ Roman pounds.¹⁶ This was by no means the limit to their donations, as may be read in Griffith's account.¹⁷ By this period the idea of a tithe on visitors was standard at Philae, and it may be that the inscription of Petiesi marks the beginning of this tendency. For this and for other reasons, it is a pity that we cannot date our stele precisely, but it is possible that more information will come to light.¹⁸ Nevertheless Petiesi son of Pakhnum, soldier of Elephantine, deserves to be better known as part of the history of Nubia, and as a reminder of what still awaits our attention even in the most obvious places.

¹⁶ Griffith read *lytret* (λίτρον or λίτρα); *lybret* (*librum* or *libra*) is also possible.

¹⁷ *Dodecaschoenus*, 114-19. A useful account of the history of Philae and the area between the first two cataracts during our period is contained in W. Y. Adams, *Nubia: Corridor to Africa* (London, 1977), ch. 12.

¹⁸ The other stelae from cemetery 3 published in *Arch. Survey of Nubia 1907-8*, pl. 15 are worth considering, and it may be that other material from the same site exists in museums, whether in Egypt or abroad.



a. Stela 3-98 and 100.

Aswan 1057, rephotographed by Nigel Strudwick from Reisner, *Archaeological Survey of Nubia 1907-9*, pl. 15 (a)

A PIOUS SOLDIER (pp. 169-80)

APPENDIX TO NAQADA EXCAVATIONS SUPPLEMENT

By JOAN CROWFOOT PAYNE

This is a list of corrections and additions to Petrie's *Naqada Excavations: a Supplement*, by E. Baumgartel, based principally on records of Petrie's 1895 excavations recently rediscovered in University College, London.

IN 1895 Petrie excavated the vast Predynastic cemetery at Naqada, still the largest cemetery of that date known to us, and in 1898/9 he dug three smaller cemeteries of the same date near Hu. On the results of this work he based his famous system of Sequence Dating. Both sites were published rapidly, *Naqada & Ballas* in 1896, and *Diospolis Parva* in 1901, but, although excellent for the period, neither volume provided the details of grave groups essential for subsequent research on the internal chronology of the period. For this reason, Elise Baumgartel spent many years travelling to museums all over the world, recording identifiable objects from Naqada, in order to reconstruct as far as possible the grave groups on which Petrie's work was based. She published the results of this work in 1970 in *Petrie's Naqada Excavations: a Supplement*. As soon as this was published, she started to compile an appendix, a work in which I helped her, and continued after her death in 1975. For some years the appendix stayed short; it included small groups which passed through the sale rooms, generally in private hands, and unexpected little collections found in smaller museums, as well as corrections to the objects already published.

In 1982 University College, London, made a happy discovery; original records of Petrie's excavations in 1895 were found to have miraculously survived.¹ They include most of the records for both the Naqada and Ballas cemeteries, and also lists of special objects from these cemeteries, such as beads, stone vases, games, palettes, and maceheads. From these sources it has been possible to make a major review of part of the *Naqada Supplement*. The two cemeteries of Naqada and Ballas were excavated simultaneously; the former was numbered from 1 to 1953, the latter from 1 to 875, with the result that there has been considerable confusion between the first 875 graves of the two cemeteries.² The University College records have now made it possible to clear up nearly all of this confusion. As will be seen, most of the entries in the following appendix are in the first part of the cemetery. Two entries in the appendix are of particular interest. The provenance of the Berlin cylinder seal (Berlin 12848), discussed in detail in *Naqada Supplement* (p. 8), has now been established. There is no mention of it in the records of grave T29, the

¹ J. Bourriau, *JEA* 70 (1984), 130.

² E. J. Baumgartel, *Petrie's Naqada Excavations: a Supplement* (London, 1970), 6.

provenance given by Scharff.³ It is, however, clearly described under this grave on Petrie's list of beads. Further, the model of a hawk (Ashmolean Museum 1895.137) from an early grave 721, is made of lead, not of silver as published.⁴ This error was presumably caused by the confusion of fragments sent to the laboratory for analysis.

I am exceedingly grateful to all the museum officials who have helped me, particularly P. R. S. Moorey and Helen Whitehouse, of the Ashmolean Museum, and Barbara Adams of University College. Mrs Adams has recently worked through both these museum collections identifying hippopotamus and elephant ivory. I have not added this information to the appendix, as it does not cover other collections, but the information is available to anyone working on objects of ivory or bone. University College can also supply microfiches of the Petrie records.

Explanation of Additional Abbreviations

Tomb Catalogue

Add to Pottery types	Quibell & Green, <i>Hierakonpolis</i> II, pl. lxix
Add Palettes	Petrie, <i>Prehistoric Egypt Corpus</i> , pl. lii–lix
	Petrie, <i>Prehistoric Egypt</i> , pl. xliii–xliv

Names of Museums

Birmingham City & Art Gallery	for BMC read BC
Add Maidstone Museum	MaM
Add Science Museum, London	SM

Tomb

- | | |
|---|--|
| 1 Add palette 89 (UC.4705) | 30 Add stone vase 133, steatite (UC.4656) |
| 2 Delete pot W37 (AM.95.559) | Add palette, shapeless (UC.4657) |
| Add palette, gazelle head fr. (AM.95.870) | 31 Add beads, faience (UC.5010) |
| Add pebble (UC.35982) | 34 Add spindle whorl, limestone (UC.5130) |
| 3 Add ivory hairpin (AM.95.957) | 35 Add pot F58D (AM.95.689) |
| 6 Add potsherd B, M432 (UC.36318) | 39 Add resin (UC.5104) |
| Delete pot L14B (AM.95.646) | 49 Add potsherd B, M48 (UC.36325) |
| 8 Add stone vase 59, limestone (UC.4413) | 50 Add stone vase 32, red/buff limestone (UC.4982) |
| Add palette, shapeless (UC.4415) | 52 Add beads, clay (UC.5016) |
| Add slate tag 101T (UC.4414) | 63 Add silver needle (UC.36151) |
| 10 Add slate tag 101R (UC.4518) | Add copper needles (UC.36152–4) |
| 11 Add pot L43P (FW.E46.1898) | Add copper chisel (UC.36150) |
| 22 For pot R24M (UC.5087) read R24M (UC.5807) | 68 Add pebble (UC.35983) |
| 29 Add pot L19P (Bo.25.95.36) | 74 Add obsidian blade (AM.95.1147) |

³ A. Scharff, *Die Altertümer der Vor- und Frühzeit Agyptens*, II (Berlin, 1931), 98, no. 133.

⁴ For analysis and discussion see Z. Stös-Fertner and N. H. Gale, *Archaeo-Physika* 10 (1979), 299–314; N. H. Gale and Z. A. Stös-Gale, *JEA* 67 (1981), 115; Z. A. Stös-Gale and N. H. Gale, Actes du XX^{ème} Symposium International d'Archéometrie, *Revue d'Archéometrie* no. 5 (1981), 285–95.

- 89 For stone vase 81, serpentine (AM.95.217) read S81, serpentine (AM.95.217)
- 93 For pot L50 (AM.95.652) read L56 (AM.95.652)
- 95 For palette 4U (UC.4704) read 4V (UC.4704)
- 100 Add pot D31B (AM.95.588)
Add stone vase 37, basalt (UC.5408A)
Add stone vase 161, marble (UC.5407)
Add flint fishtail I, 12 (UC.5405)
Add flint fishtail II, 7 (MM.2424)
Add flint fishtail fr. (UC.5404)
- 101 Add pot W55 (CB.E901)
- 102 Add copper chisel (UC.5061)
- 106 Delete pot L36A (BFA.97.89)
- 107 Delete pot L33A, M (UC.6242)
Add limestone weight (SM.35.405)
- 113 Add pot W71A (AM.95.555)
- 118 Add palette, rectangular (UC.4714)
- 119 Delete pot R24A (OIC.816)
- 124 Add pot D14G (AM.95.599)
- 128 For pot P23C (FW.E.P239) read P23C (FW.E79.1896)
- 138 Add pot B74B (Private)
Add pot D66B-P (FW.E.P109)
- 140 Delete pot P13F (OIC.1783)
Add pot W62 (FW.E74.1896)
- 144 Add bead, banded limestone (UC.4641)
- 145 For pot F58B (AM.95.397) read F58R (AM.95.397)
- 146 Delete bird amulet, slate (UC.5667)
- 147 Add flint blades, II, 12 (UC.4833-4)
Add Spatha shell (UC.4461)
- 149 Delete pot P11M (UC.5844)
- 150 For stone vase 54, blackened (NC.4984) read 54, blackened (UC.4984)
- 158 Add pebble (UC.35986)
- 159 Add pebble (UC.35985)
- 160 Add pot P58A (AM.95.419)
- 161 Add pot P93A (Private)
Add flint blade, II, 12 (UC.4835)
- 162 Add ivory combs (UC.4545-50)
For flint blades II, 12 (UC.4534-43) read flint blades, II, 12 (UC.4534, 4539-43)
Add pink limestone lumps (UC.4544)
- 165 Add pebble (UC.35987)
- 168 Add pot D as 27G (MuM.T1167)
- Delete pot L17N (UC.5990)
Add pebble (UC.35988)
- 169 Add ivory rods & blocks (UC.6016-17)
Add flint blades, II, 12 (UC.4862-3)
Add pebble (UC.5946)
- 174 Delete pot P23C (PhU.E14780)
- 176 Delete pot P93B (PhU.E1794)
Delete pot R24B (KN.4376)
- 177 Add pot W4 (AM.95.535)
Add stone vase 22, white veined red (Be.12925)
Add ivory comb frs. (UC.4377)
Add ivory rod fr. (UC.4378)
Add flint blade II, 12 (UC.4376)
Add spindle whorl, limestone (UC.4375)
Add pebble (UC.35989)
- 178 For pot N15 (AM.95.501) read N10 (AM.95.501)
- 179 Add alabaster rod (UC.5185)
Add flint blade II, 12 (UC.4864)
Add pebbles (AM.95.1157), (UC.35990)
- 181 Add beads, cornelian (UC.35978)
Add flint blade II, 12 (UC.4865)
- 182 Add palette, fr. bird's head (UC.4716)
- 185 Add pot P40E (UC.5875)
Delete beads, cornelian, gl. steatite (AM.95.879)
Add pebble (Be.12918)
- 186 Add pebble (UC.35991)
- 191 Add pot B63A (CNH.31490)
Add pot D93B (AM.95.625)
- 192 For pot P37 (FW.E.P50) read P37 (FW.E75.1896)
- 193 Add pot D78A (AM.95.579)
- 197 Add flint, atypical (UC.4866)
- 206 Add pot B26A (FW.E74.1896)
Add potsherd P, M (UC.36052)
- 208 Add ivory knobs (UC.5673-7)
- 210 For pot P95G (FW.E.P238) read P95G (FW.E78.1896)
Add palette, shapeless (UC.4707)
Add flint sickle blade II, 4 (UC.6238)
Add flint blade II, 12 (UC.4836)
- 211 Add tag, sandstone (UC.4529)
Add calcite pebble (UC.4530A)
- 215 Add flint sickle blade II, 4 (UC.4868)
Add flint blade II, 12 (UC.4869)
- 217 Add pot P37 (OIC.1805)
Add palette, oblong (MM.8368A)

- 221 Add pot F85c (AM.95.656)
 223 Add pot C30H (AM.95.480)
 Add flint knife fr. (MM.5580)
 Add flint blade II, 12 (UC.4777)
 224 Delete pot P53 (Bo.25.95.28)
 225 Add potsherd R, M378 (UC.36067)
 228 Add pot P46B (Be.12966)
 Delete pot L56-56B (Be.12966)
 236 Add pot F62B (AM.95.451)
 241 Add pot B57B (Private)
 242 Add potsherd N (UC.4682)
 Delete pebble (AM.95.1154)
 245 Add pebble (UC.35993)
 252 Add palette 45H (UC.4718)
 257 Add palette 87L (UC.4717)
 Add flint blade II, 12 (UC.4872)
 259 Delete pot L36A (Bo.25.95.68)
 264 Add copper cone (UC.5062)
 265 Delete pot L2A (AM.95.717)
 Delete pot L2A (OIC.1748)
 Delete pot L12B (OIC.1733)
 Delete pot L19C (UC.5794)
 Delete pot L25A (OIC.1744)
 Delete pot L29A (BFA.97.147)
 Delete pot L78A (OIC.1634)
 Delete pot L78A (PhU.E14778)
 Delete pot L78A (OIC.1645)
 266 Add pot F90 (AM.95.797)
 271 Add clay figure (AM.95.128)
 Add malachite (UC.4250)
 272 Add flint knife II, 8c (KN.4251)
 273 Add fruit & wood (UC.36097)
 276 Add pot B62B (CNH.31468)
 278 Delete pot L64 (Bo.25.95.80)
 279 For pot P24N (UC.4587) read P24N,
 M598 (UC.4587)
 281 Delete pot L53J (OIC.755)
 284 Delete pot W80 (PhU.E1653)
 293 Add potsherd B, M282 (UC.36068)
 304 Delete pot P94C (PhU.E1804)
 Delete pot R91C (PhU.E1718)
 324 Delete palette 91D (Bo.44.96.10)
 327 Delete pot B24C (Be.13038)
 329 Add pot D5H (AM.95.565)
 330 Add pebble (UC.35996)
 333 Add pot P75G (FW.E72.1896)
 341 Delete pot B47 (PhU.E1527)
 Delete pot B75B (OIC.1825)
 344 Delete pot B58B (PhU.E1508)
 345 Delete pot P40C (Bo.25.95.25)
 Delete palette, two fishes (AM.95.845)
 355 For stone vase 7c (PhU.E14417) read
 pot L7c (PhU.E14417)
 359 Delete pot D10K (PhU.E1391)
 362 Delete pot D67c (BMC.P5)
 372 For pot P82B (FW.E.P242) read pot
 P82B (FW.E80.1896)
 374 Add pot W61 (Be.13377)
 376 Delete pot B35D (UC.4260)
 Delete pot B74A (PhU.E1454)
 Delete pot W61 (Be.13377)
 382 Delete pot P40C (Bo.25.95.26)
 385 Delete palette 45B (PhU.E1236)
 386 Delete pot W55 (AM.95.547)
 Delete pot L19A (PhU.E1819)
 387 Delete pot B58A (RM.18.5)
 Delete pot R84D/85P, M117 & 122
 (Be.13059)
 388 For beads, cornelian, glass (UC.4503)
 read beads, cornelian, obsidian or
 pitchstone (UC.4503)
 For vitreous lumps (UC.4504) read fr.
 pitchstone? (UC.4504)
 391 Add pebble (UC.35998)
 393 Add pebble (UC.35999)
 397 Delete pot B35A (BFA.97.84)
 398 Delete pot R23c (PhU.E1679)
 401 Add pot D66c (OIC.734)
 404 Delete palette 92P (UC.4721)
 406 Delete pot P40B (PhU.E1812)
 Delete pot D61B (PhU.E1662)
 409 For pot D5H (AM.95.585) read D36D
 (AM.95.585)
 419 Delete shell armllet (PhU.E1159)
 421 For stone vase 57, red breccia (MM.
 4521) read stone vase 51, red breccia
 (MM.4521)
 424 Delete pot B71B (Bo.25.95.21)
 Delete pot P56B (PhU.E.1771)
 426 For pot P13 (AM.95.675) read L19H
 (AM.95.675)
 430 Delete pot B35B (PhU.E1497)
 Delete pot L59c (OIC.845)
 431 Delete flint fishtail I, 12 (PhU.E1109)
 435 Delete pot B27D (AM.95.254)
 Add pot P40E (CNH.31461)
 438 Delete pot B18c (BMC.P2)
 439 Delete pot F11A (BFA.97.129)

- 440 Delete pot P22A (PhU.E14779)
Delete pot D9c (FW.E60.1896)
Delete pot as R1B (KN.4399)
- 441 Delete pot P80s (UC.5885)
Delete palette 46 (IAL)
- 447 Delete pot B41 (PhU.E1515)
Delete pot B42A (Bo.25.29.15)
Delete pot P40H (OIC.1838)
Delete palette 45A (UC.4724)
- 450 Delete pot P23 (UC.6006)
Delete pot P40E (PhU.L55-302)
Delete palette 46R (CB.E424)
Delete ivory rod (UC.6019)
For flint blades 11, 12 (UC.488-91) read
flint blades, 11, 12 (UC.4888-91)
- 453 Add pebble (UC.36000)
- 454 Add potsherd R, M324 (UC.36316)
Add pebble (UC.36009)
- 456 For pot R81 (AM.95.688) read R81,
M439 (AM.95.688)
For flint knife 1, 14A (AM.95.1005) read
flint knife 1, 14B (AM.95.1005)
Delete bead grinders (AM.95.991)
(UC.5662) (UC.4796A)
- 457 Add stone vase *N&B* XLII 26, limestone
(UC.5480)
- 460 Add pot W62 (FW.E.P60)
- 461 Delete pot as P56B (BFA.97.111)
Add limestone weight (SM.35.377)
- 464 Delete palette 96M (CNH.31445)
- 469 Delete pot B38A (BFA.97.83)
- 470 Delete pot B12A (UC.6007)
Add pebble (UC.36010)
- 471 Delete bead grinder (AM.95.992)
- 473 Delete pot L26H (Bo.25.95.60)
- 474 Delete pot B58C, M227 (AM.95.255)
- 476 Delete pot P14 (PhU.E1704)
- 479 Delete pot B25A (OIC.1858)
- 483 Delete pot P24K (OIC.1711)
- 485 Delete pot P16D (UC.5854)
- 488 Add pot B57B (Steele, 9 Ashley Gdns.,
SW1)
Delete palette, turtle (PhU.E1229)
- 491 Delete palette 98N (Bo.44.96.12)
- 492 Delete pot R17C (PhU.E1738)
Delete pot L7C (PhU.E14391)
- 494 Delete palette 46U (CNH.31448)
- 499 Delete pot R36, M as 57 (AM.95.744)
- 501 Delete pot F14 (PhU.E14399)
- 506 Delete pot B38CD-59AB (Be.12996)
Delete pot B47 (CNH.31474)
- 507 Add flint scraper 11, 6B (UC.4944)
- 512 Add frs. wood (UC.36098)
- 513 Delete palette as 60 (Bo.44.96.6)
- 514 Add pot P40E (AM.95.1259)
- 519 Delete pot R24A (UC.5768)
- 519b Add pot L12D (AM.95.748)
- 521 Add potsherd R, M536 (UC.36069)
- 524 For pot P22A (UC.5855) read L12C
(UC.5855)
- 526 Add flint blades 11, 12 (UC.4906-10)
- 532 Delete pot B58A (AM.95.267)
- 535 For pot R38 (UC.5802) read L16N
(UC.5802)
- 546 Delete pot P24N (BFA.97.135)
Add pot D68A-M (Be.13049)
- 548 Delete pot B13A (OIC.1693)
- 550 Add palette 35H (Bo.44.96.3)
- 564 For pot F31A (AM.95.370) read F31D
(AM.95.370)
Delete pot L45 (PhU.E14410)
- 565 For pot B37A (UC.5694) read B39A
(UC.5694)
Delete pot R38 (Be.13067)
- 571 Add pot P22A (UC.5848)
- 572 For pot L7C (OIC.1760) read R33B
(OIC.1760)
- 575 Delete flint knife 11, 8B (UC.4913)
- 576 Delete beads & pendants, serpentine
(UC.5389-91)
- 577 Delete pot B57G (OIC.1857)
Delete pot P40B (BFA.97.97)
- 578 Delete pot R69B (PhU.14439)
Delete pot L12K (Be.12981)
- 581 For pot P23D (BFA.97.104) read P93D
(BFA.97.104)
- 582 Delete pot B61A (AM.68.1275)
Delete pot P40E (PhU.E1815)
Delete palette 46M (UC.4725)
- 583 Delete pot D27 (FW.E.P43)
- 585 For pebble (UC.5963) read pebble
(UC.5965)
- 588 Delete pot W19 (AM.95.533)
- 590 Delete pot B58C (OIC.1679)
- 594 Delete pot B58A (Bo.25.95.1)
- 595 Delete palette 61H (PhU.E1226)
- 597 Delete pot R81, M444 (Bo.25.95.52A)
- 598 Delete pot W19, M221 (UC.4341)
Delete palette 45S (UC.4344)

- 602 Delete beads, cornelian (UC.4663)
Delete pendants, cornelian (UC.4661-2)
- 604 Delete pot P23B (PhU.E15577)
Delete pot P40C (PhU.E14915)
- 607 Delete palette 96U (UC.4756)
- 608 Delete palette 96A (UC.4739A)
- 609 Delete pot R28 (Be.13063)
- 610 For pot L16F (OIC.1764) read R34 (OIC.1764)
- 614 Delete pot P23A (PhU.E1693)
- 624 Delete pot W62 (PhU.E1660)
Delete ivory spoon (PhU.E1177)
- 625 For pot D68 (UC.4327) read D61 (UC.4327)
For pot R36 (UC.5993) read L12B (UC.5993)
For pot L7A (MuM.1129) read R33B (MuM.1129)
- 631 For pot R65C (AM.95.76A) read R65C (AM.95.76A)
- 638 Delete bird's egg (UC.5932)
- 639 Add pot D1A (AM.95.567)
- 643 Add pot D59B (AM.95.612)
- 644 Delete palette 61R (Bo.44.86.5)
- 646 Delete stone vase, squat, model, limestone (UC.4991)
- 648 Delete palette 24P (Bo.44.96.1)
- 649 Delete stone vase 126, pink limestone (PhU.E1308)
- 650 Delete pot B11N (UC.5716)
Delete pot B57C (PhU.E1522)
- 656 Delete stone vase 4, granite (UC.4987)
- 657 Add pot P95A (Private)
- 658 Add pebble (UC.36013)
- 663 Delete flint fishtail 11, 7 (UC.4919)
- 664 Delete pot D67C (FW.E57.1896)
- 666 Add pebble (UC.36014)
- 667 Add flint blade 11, 12 (UC.35980)
- 669 For pot P16 (UC.5845) read L12B (UC.5845)
- 673 Delete pot R1B (Be.13067)
- 675 Add pebble (UC.36015)
- 676 Delete pot B75B (FW.E22.1896)
Delete pot R69C (BFA.97.105)
- 677 Delete pot L29A (BFA.97.146)
Delete pot L50A (UC.6062)
- 679 Add pot P11 (MaM.EA10)
Add pot R22A (UC.6065)
- 680 Add bone blocks (AM.95.144b)
Add pebble (UC.36016)
- 683 Delete flint blade 11, 12 (UC.4927)
Delete flint borer 11, 13 (UC.4928)
- 692 Add branch frs. (UC.36100)
- 694 Add pot P93C (Private)
- 698 Add shells (UC.36078)
- 705 Add pebble (UC.36017)
- 712 For pot R65B (UC.5786) read R as 44M, M519 (UC.5786)
- 721 For hawk, silver, frg. (AM.95.137) read hawk, lead, frg. (AM.95.137)
Add copper wire (AM.95.974)
- 723 Add ivory hairpin (UC.5215)
Add shells (UC.4997)
- 743 Add resin (UC.36090)
- 752 Delete flint knife 11, 8B (PhU.E1104)
- 755 Add pebble (UC.5151)
- 759 Delete stone vase 148, marble (PhU.E1363)
- 767 Delete pot P40E (UC.5874)
Add shell (UC.36082)
- 770 Delete pot L58D (BFA.97.102)
- 771 Delete pebble (UC.5157)
- 775 Delete pot R23A (UC.5795)
- 777 Delete palette as 61H (CNH.31449)
Delete flint knife 1, 14A (PhU.E1103)
- 779 Delete pot P22B (PhU.E14808)
Delete pot P82C (BFA.97.98)
Delete pot P93C (PhU.E1753)
Delete pot P95B (PhU.E1803)
Delete pot R69B (PhU.E14467)
Delete flint blades 11, 12 (UC.4946-7)
- 780 Delete pot L2A (OIC.1619)
- 791 Add pebble (UC.36019)
- 792 Delete pot B74 (UC.6067)
- 793 Delete pot R23A (OIC.937)
Delete pot R23A (UC.5994)
- 799 Delete palette 570 (UC.4728)
- 801 Delete pot B35B (OIC.1811)
- 804 Add pot D41S (UC.6069)
Delete beads, faience (UC.5084A)
- 809 Delete pebble (UC.5968)
- 810 Delete palette as 98F (PhU.E1218)
- 812 Add rubber, stone (UC.6223)
- 814 Delete palette, fish (PhU.E1224)
- 824 Add pebble (UC.36021)
- 825 Add pot P95A (Private)
- 828 Add pot D67D (Private)

- 837 Delete pot B56 (PhU.E17541)
Delete armlet, shell (UC.5672)
- 839 Delete pot D67J (PhU.E1400)
- 842 Add pot as D61 (FW.E.P254)
- 844 Delete palette 15M (PhU.ES1225)
Delete palette as 88M (Bo.44.96.7)
- 849 Add pebble (UC.36022)
- 855 Delete copper fish hook (AM.95.984)
- 867 Delete stone vase S17B; alabaster
(AM.95.197)
Add pebble (UC.36023)
- 870 Delete palette as 67H (Bo.44.96.8)
- 871 Delete armlet fr., ivory (UC.5631)
Delete armlet, shell (UC.5630)
- 874 Delete pot P46K (PhU.E.1805)
Delete palette as 87F (PhU.ES1242)
- 875 Add pot B6E (AM.95.648)
- 879 For pot B53A M12 (UC.5899) read
B25N, M12 (UC.5899)
- 906 For pot P8A (AM.95.429) read B44W
(AM.95.429)
- 964 Delete flint sickle blade II, 4 (UC.4914)
Add pebble (UC.36024)
- 1019 Add pot F58B (UC.5742)
- 1021 For stone vase S64 (AM.95.215) read
S64, limestone (AM.95.215)
- 1039 Add flint blade II, 12 (UC.4929)
- 1041 For pot P24 (AM.95.275) read P24
(AM.95.275)
- 1069 Delete pot F58B (UC.5742)
- 1108 Add pot D as *Matmar* XIII, 3 (AM.95.
685)
Add pot R55E (MaM.EA315)
- 1135 Add pebble (UC.36025)
- 1208 For flint blades II, 12 (UC.4278-9)
read flint blades II, 12 (UC.4278-80)
- 1224 Add pebble (UC.36026)
- 1229 Add flint blade frag. (UC.4843A)
- 1234 Add beads, clay, cornelian, garnet,
olivine, faience, steatite, breccia, &
shells (UC.36084)
- 1236 Add pot D61A (AM.95.613)
- 1238 Add pot B24B (MaM.EA6)
Add potsherd P, M292 (UC.36070)
- 1257 For pot P63 (FW.E.P248) read P63
(FW.E81.1896)
- 1263 For flint knife II, 8c (LC.56.2028) read
flint knife II, 8c (LC.56.20.28)
Add pebble (UC.36027)
- 1296 Add pebble (UC.36028)
- 1314 Add pebble (UC.36029)
- 1325 For pot L28A (AM.95.755) read R23A
(AM.95.755)
- 1335 For pot B25A (UC.5706) read B74A
(UC.5706)
- 1353 Add palette, turtle (UC.6035)
- 1372 Add pebble (UC.36030)
- 1373 Add pot as B11E (FW.E38.1896)
- 1377 Add pebble (UC.36081)
- 1379 Delete pot B62B, M381 (AM.95.1256)
- 1387 Add pebble (UC.36032)
- 1388 Add branch frs. (UC.36099)
- 1410 Add nuts (UC.36094)
- 1411 Add reeds (UC.36095-6)
- 1413 Add pebble (UC.36033)
- 1465 Add reed mat frs. (UC.36101)
- 1468 For pot B11P (RM.18.2) read B11F
(RM.18.2)
- 1484 For pot B75C (Be.13042) read B75B
(Be.13042)
- 1487 For pot F96A (AM.95.815A) read
F96A, M *N^cB* xxvii 96A (AM.95.
815A)
Delete pot F64 (UC.6013)
- 1489 Add pebble (UC.5982)
- 1490 For F sherd (AM.95.389) read F7
(AM.95.389)
- 1503 For pot B27B (FW.2088.P3) read
B27B (FW.E71.1896)
Add female fig., veg. paste (UC.19629)
Add wood, leather, reeds (UC.36102
A-D)
Add shell (UC.36074)
- 1505 For pot fr. (9D.OIC.NN) read fr. C9D
(OIC.NN)
- 1520 For pot D24N (Be.13054) read D24A
(Be.13054)
- 1534 Add beads, cornelian (UC.36083)
- 1536 Add potsherd, B, M397 (UC.36071)
- 1539 Add pot B74A (CNH.31488)
- 1546 For pot B57C (FW.P2) read B57C
(FW.E70.1896)
- 1547 Add frs. wood, some worked (UC.
36093)
- 1552 Add potsherd B, M522 (UC.36326)
- 1556 Add pebble (UC.36038)
- 1560 Delete pot P14 (UC.6058)
- 1562 Add pebble (UC.36034)
- 1578 For flint frg., atypical (UC.4953) read
flint adze, 1, 9 (UC.4953)

- 1586 Add pot B22A (MaM.EA11)
 1589 Add wood (UC.36091)
 1599 For pot L20 (Be.12982) read P1A
 (Be.12982)
 1625 Add shell (UC.36080)
 1636 Add nuts (UC.36092)
 1648 Add pebble (UC.36036)
 1655 Add potsherd B, M35 (UC. 36317)
 1665 Add pebble (UC.36037)
 1669 Add pot B53F (CNH.31471)
 1679 Delete pot B11D (CB.E835)
 1680 Add pot B11D (CB.E835)
 1683 Add flint fishtail fr. (UC.36075)
 1684 Add shell (UC.36079)
 1689 For stone vase 11, basalt (AM.95.187)
 read stone vase 111, basalt (AM.95.
 187)
 1690 For pot B25C (AM.95.338) read B25C,
 M as 86 & 94 (AM.95.338)
 1691 For pot B22F (AM.95.307) read B22F,
 M as 558 (AM.95.307)
 1693 Add pot B62B (MaM.EA7)
 1694 Add pebble (UC.36039)
 1698 For palette 2 (UC.4681) read palette,
 fish (UC.4681)
 1707 Add pebble (UC.36040)
 1723 For pot B22C (UC.5997) read P22C
 (UC.5997)
 1728 For ivory pin-comb (UC.5213) read
 bone pin-comb (UC.5213)
 1729 Add pot D31L (Private)
 1749 Add pebble (UC.36018)
 1757 Add pebble (UC.36041)
 1758 Add pebble (UC.36042)
 1766 For pot B11M (FW.E30.1896) read
 B11M (FW.E36.1896)
 1775 Add pebble (UC.35981)
 1783 Delete pot B27F, M37 (AM.95.318)
 For beads, glazed steatite (AM.95.
 880) read beads, faience (AM.95.
 880)
 1785 Add pot B27F, M37 (AM.95.318)
 1787 For pot W87 (AM.95.624) read D87
 (AM.95.624)
 1792 Add potsherd B, M535 (UC.56315)
 1796 Add pot D61A (Be.13048)
 1805 Add pot as R69H (AM.95.762)
 1812 For pot W91A (AM.95.631) read
 D91A (AM.95.631)
 1832 For pot B62A (AM.95.1885) read B62A
 (AM.95.1185)
 1834 Add potsherd R, M (UC.36072)
 1848 Add pebble (UC.36043)
 1850 Add potsherd R, M (UC.36073)
 1853 Add pot P40E (MaM.EA9)
 1854 Add pebble (UC.36044)
 1856 For pot L12B (OIC.931) read R34C
 (OIC.931)
 Add pebble (UC.36045)
 1859 Add pebble (UC.36046)
 1869 For pot D61D (OIC.768) read D67D
 (OIC.768)
 1875 Add pebble (UC.36047)
 1883 For palette 96 (Be.12879) read 92M
 (Be.12879)
 1886 Add pebble (UC.36048)
 1890 Add pebble (UC.36049)
 1894 Add pot P24N (MaM.EA8)
 1904 Delete pot L38A (UC.5998)
 1907 Delete pot L36B (MuM.1796)
 1917 Add pebble (UC.36050)
 B15 Add beads, faience (UC.34346)
 B18 For pot L12C (UC.6063) read P22A
 (UC.6063)
 B19 Add potsherds N (UC.4391)
 Add potsherd D41 (IAL)
 Add flint flake (UC.4390)
 B25 Add pot P95A (Private)
 Add stone vase S2, cord dec., alabaster
 (UC.6213)
 B64 Add potsherd W (UC.6207)
 B101 For pot W19 (FW.E.P11) read W19
 (FW.E73.1896)
 Delete pot D1A (AM.95.567)
 B117 Add ivory comb (UC.4567)
 B121 For palette 99 (Be.12884) read 92D
 (Be.12884)
 B137 Add pot B23D (UC.5689)
 T3 Add potsherd as D43C (IAL)
 T4 For potsherd as B11F (AM.95.1194)
 read as B11F (AM.95.1194B,C)
 For pot B46 (AM.95.1194) read B46
 (AM.95.1194A)
 For potsherd as B53B, M (AM.95.
 1193) read as B53B, M (AM.95.
 1193 + 1194F)
 Add potsherds B (AM.95.1194D,E,G)
 Add pot P22 (AM.95.1194I)
 Add pot P22 (AM.95.1194K)

- For pot P as 24K, sherd (AM.95.1194) read P as 24K, sherd (AM.95.1194J)
 Add potsherd P (AM.95.1194H)
 Add pot F14 (AM.95.1202)
- T11 For pot P11F (FW.E11.1896) read P11F (FW.E11.1895)
- T17 Add potsherd P (UC.36087)
- T18 Add palette 78D (UC.6037)
- T29 Add cylinder seal, limestone (Be.12848)
 Add pebble (UC.5987)
- T33 Delete pot W71A (AM.95.555)
- T36 For pot F17B (AM.95.374) read F17A (AM.95.374)
 Add beads, gold (UC.5411)
- T39 For pot L7G (FW.P120) read L7G (FW.E77.1896)
- South Town For pot F85B (AM.95.815) read as D40 (AM.95.815)
 Add potsherd R84/86, M (AM.95.693)
 Add ivory harpoon (UC.5289)

MUSEUM ACQUISITIONS, 1985

EGYPTIAN ANTIQUITIES ACQUIRED IN 1985 BY MUSEUMS IN THE UNITED KINGDOM

Edited by JANINE BOURRIAU

THE list includes some 1984 acquisitions by the Birmingham City Museum and Art Gallery.

Predynastic

- 1-2. Burnished red ware bowl and black topped vase, University College 31606-7. From Diospolis Parva, Cemetery H.
- 3-4. Pottery dish and vase, University College 36350-1. From Harageh(?).
5. Marl pottery jar. University College. From Ballas, grave 430. Petrie MSS notebook no. 145.
- 6-7. String of beads and four ivory fragments, University College 31493-4. From Diospolis Parva, R.129.
8. Twelve ivory fragments, University College 31507 A-L. From Diospolis Parva.
- 9-10. String of beads and an agate ball bead, University College 31489-90. From Diospolis Parva.
11. String of beads, University College 31492. From Diospolis Parva, H132.
12. String of beads, University College 36346. From Naqada, B15. Petrie MSS notebook no. 71.
- 13-72. Flints, University College 31521-70. From Diospolis Parva or Hierakonpolis.
73. Beetles, University College 31495. From Diospolis Parva, B17. Petrie, *Diospolis Parva*, 33.

Early Dynastic

74. Potsherd with part of the name of Semerkhet, University College 36327. From Saqqara, Archaic Cemetery. First Dynasty.
75. Limestone vase, University College 31497. From Diospolis Parva, (?) N6.
- 76-8. Fragments of faience tiles, University College 35552-4. From Abydos, Osiris Temple, Chamber M64.
- 79-81. Fragments of faience tiles, University College 35556, 35558, 35581. From Abydos, Osiris Temple, Chamber M65.
- 82-3. Fragments of faience tiles, University College 35560-1. From Abydos, Osiris Temple, Chamber M69.
- 84-94. Fragments of faience including cylinder and barrel beads, and base of an alabaster vase, University College 35562-4, 35567-74. From Abydos, Osiris Temple, Chamber M69.
- 95-102. Fragments of faience, calcite bowl and cylinder vases and chert flakes, University College 35582, 35596, 35600-1, 35623-5, 35628. From Abydos, Osiris Temple, Levels 240'', 182'', 175'', 198-200'', 183'', 160'', 166'', and 198''.

103-18. Fragments of faience tiles, University College 35575-7, 35583-5, 35587-9, 35592-5, 35597-9. From Abydos, Osiris Temple.

119-32. Fragments of faience, including model pottery, beads, rings, and bracelets, University College 35602-5, 35609-12, 35614-17, 35619-20. From Abydos, Osiris Temple.

133-5. Fragments of faience figurines, University College 35606-8. From Abydos, Osiris Temple.

136-42. Fragments stone vessels and bracelets, University College 35621-2, 35626-7, 35637-9. From Abydos, Osiris Temple.

143-8. Chert blade and fragments of ivory, University College 35629-31, 35633-5. From Abydos, Osiris Temple.

149-50. Fragment of bracelet, greywacke, and fragment of carved bone, University College 35632, 35636. From Abydos, Osiris Temple, levels 190'' and 205''.

151. Fragment of an alabaster vase, University College 35557. From Abydos, Temenos tomb M12 (?).

152-9. Pottery head, modelled in relief, fragments from stone vases and limestone macehead, University College 35537-44. From Abydos(?), temenos (town) of Osiris.

160-1. Strings of beads, University College 35613, 35618. From Abydos Osiris Temple or Hierakonpolis, Main Deposit.

Old Kingdom

162. Fragment of limestone sunk relief from a tomb, showing the tomb owner Ka-irr holding a staff, British Museum EA 69573. Gift of Major Gordon Maclean. (pl. XI, 1)

163. Fragment of limestone relief, University College 35647. From Abydos, (?)Osiris Temple.

164. Fragment of faience tablet, University College 35646. From Abydos, Osiris Temple, found in front of door.

165-8. Strings of beads, University College 31510, 31571-3. From Diospolis Parva, D14, W83, W108, D7.

First Intermediate Period

169. Calcite cylinder jar, University College 31585. From Diospolis Parva, Y152.

170-1. Pottery jar and string of beads, University College 31496, 31575. From Diospolis Parva, Cemetery W.

172-3. String of faience beads and twenty ivory inlay fragments, University College 31488, 31506. From Diospolis Parva.

Middle Kingdom

174. Steatite statuette of a standing woman, named on the base as Heti, daughter of Neferu and mother of Renefseneb, Ashmolean Museum 1985.152. Ashmolean Museum, *Annual Report* (1984-5), pl. 1. (pl. XI, 2)

175. Pottery model dish, University College 31602-3. From Diospolis Parva, Y6. Mace notebook, 40.

176-85. Faience, shell and carnelian beads, University College 31487, 31576-8, 31581, 31614, 31482, 31582-3. From Diospolis Parva, Y100, W74, W14, W114, Y82, Y16.

186. Faience crumb beads, University College 31484. From Diospolis Parva, Cemetery Y.

187-8. String of beads and shell pendant, University College 31579-80. From Diospolis Parva, Cemetery W.

189. Carnelian spacer bead, University College 31584. From Diospolis Parva(?).

Second Intermediate Period

190. Limestone stela of Prince Ahmose shown seated with his pet dog under his chair, receiving offerings, British Museum EA 69673. Seventeenth Dynasty. (pl. XI, 3)

191-3. String of faience beads and flints, University College 31481, 31485-6. From Diospolis Parva, X85, X28. Pan-grave culture.

194-203. Strings of shell and faience beads, University College 31588-97. From Diospolis Parva, X8. Pan-grave culture.

204. String of disc beads, University College 31598. From Diospolis Parva, Cemetery X.

205-7. Potsherds and string of beads, University College 31504-5, 31599. From Diospolis Parva, Cemetery YS.

208. Small redware pottery bowl, University College 31615. From Diospolis Parva, Cemetery Y.

209. Pottery door used as lamp, University College 31611. From Diospolis Parva, (?)Cemetery W.

210. Two pottery objects, University College 31512 A, B. From Diospolis Parva.

New Kingdom

211. Fragment of limestone relief from a tomb chapel showing the owner seated in front of an offering table. Probably Memphite. British Museum EA 69667. (pl. XI, 4)

212. Limestone figure of a crocodile, inscribed on the base. British Museum EA 69671.

213. Limestone sarcophagus fragment of Menna, British Museum EA 69674. Nineteenth Dynasty. From Sedment, Tomb 1955. Gift of Dr C. N. Reeves. Petrie and Brunton, *Sedment* II, 31.

214. Wooden shabti of King Sethos I, Royal Museums of Scotland 1985.585. Gift of Mr K. Ingles.

215. Two handled pottery juglet, University College 31491. From Diospolis Parva, Mastaba D5. Petrie, *Diospolis Parva*, 37.

216-17. Syrian(?) pottery, University College 31500-1. From Diospolis Parva, Y20. Mace Notebook 40.

218-19. Potsherd from stirrup jar and human hair threaded with beads, University College 31502-3. From Diospolis Parva, Y32.

220. Ivory clapper in the form of a right hand, University College 36314. From Rifeh. Petrie, *Gizeh and Rifeh*, 23.

221. Calcite hair-rings, University College 31617. From Meydum Tomb 50. Petrie, *Objects of Daily Use*, 22.

222-422. Faience beads, amulets, and rings; calcite, wooden, shell, and bone fragments; pottery and copper fragments; mud figurines; basketry; potsherds; pottery jar; stone fragments; pieces of faience tile and glass; leather and bone fragments; and mud sealings, Bolton Museum 162. 1985/1-200. From the Workmen's Village at Amarna, excavated between 1979-82. Gift of the Egypt Exploration Society.

423. String of glass beads, University College 31601. From Diospolis Parva, tomb W38.
- 424-7. Alabaster dishes, Birmingham City Museum A.59-1984, A.60-1982, A.61-1984, A.62-1984. From Abydos, Garstang's 1909 excavations, tombs 3, 954, 990, 1142. (Middle Kingdom or New Kingdom ed.) Transferred from Tamworth Museum.
428. Alabaster dish, Birmingham City Museum A.63-1984. Possibly from Abydos. Transferred from Tamworth Museum.
- 429-30. Pottery bowls, Birmingham City Museum A.64/5-1984. Transferred from Tamworth Museum.
431. Fragment of linen cut from a long length of un-dyed plain weave linen with a looped fringe, Durham Oriental Museum, acquired in 1985. Probably from Thebes.

Third Intermediate Period

432. Bronze figurine of a priest, Royal Museums of Scotland 1985.210. Twenty-first to Twenty-second Dynasty. Found near Cupar, Fife.
433. Pottery Bes jar, University College 36313. Twenty-second to Twenty-sixth Dynasty.
434. Fragment of painted linen cartonnage showing winged goddess, University College 38039. Probably from Thebes, area around the Ramesseum. Twenty-second Dynasty or later.

Late Period

435. Faience shabti of *P3-di-Hr*, Fitzwilliam Museum E.1.1985. Thirteenth Dynasty. Gift of Miss V. Wickens.
- 436-75. Shabtis, figurines, amulets, and beads, including items from Amherst and Bethell collections, Royal Museums of Scotland 1985.586-625. Gift of Mr K. Ingles.
- 476 A-D. Fragments of unpainted linen cartonnage, University College 38038 A-D. Probably from Thebes.
477. Amulet of Tauret, University College 36328. From Saqqara, Sacred Animal Necropolis.

Ptolemaic Period

- 478-81. Plaster moulds and cast of woman's head, female figures, and beggar, University College 33577, 33585, 33588, 33590. From Memphis(?).

Roman Period

482. Sandstone uraeus lintel fragment, Bolton Museum 161.1985/1. From Qasr Ibrim, Room 241, excavation no. 63/14. Probably Meroitic, re-used in Bosnian period. Gift of Egypt Exploration Society.
483. Sandstone relief fragment inscribed *ꜥnh dt*, Bolton Museum 161.1985/2. From Qasr Ibrim, House LC2-2, Room 8, excavation no. 63/7. Re-used in Late-Christian context. Gift of Egypt Exploration Society.
484. Fragment of carved stone, Birmingham City Museum A.238-1982. From Qasr Ibrim, excavation no. 64/102. Re-used in X-group context. Gift of Egypt Exploration Society.
485. Fragment of stone stela with Greek inscription, Birmingham City Museum

A.239-1982. From Qasr Ibrim, excavation no. 66A/7. Found re-used in Bosnian context. Gift of Egypt Exploration Society.

486. Fragment of limestone lintel, Birmingham City Museum A.240-1982. From Qasr Ibrim, excavation no. 78.3 16/14. Probably Meroitic. Gift of Egypt Exploration Society.

487. Wax encaustic portrait, University College 36347. From Hawara Cemetery, possibly Body n. Petrie, *Hawara, Biahmu and Arsinoe*, 19. Second century AD.

488. Wax encaustic portrait, University College 36348. From Hawara Cemetery, body BA. Second century AD.

489. Bronze statuette of an Apis bull, Fitzwilliam Museum E.3.1985. Gift of Miss V. Wickens.

490. Fragment of carved serpentine, University College 35529. From Koptos.

491-2. Terracotta mask of Bes or a satyr and a plaque of Harpocrates, University College 33594, 33602. From Memphis.

493. Terracotta model of a procession, University College 33595-601. From Memphis.

494-507. Plaster moulds of patera handle, head of a horse, Heracles, Isis, column capital, utensil stand, women, Serapis, arm, man, man's head, ornamental spray, University College 33575-6, 33578-83, 33586-7, 33589, 33591-2.

508. Pottery lamp, British Museum EA 69669. Gift of Mr W. J. Hillier.

509. Mummy elaborately wrapped in lattice style with pink squares and gilt studs. The portrait panel in front of the face is missing. X-ray shows the intact skeleton of a 14-year-old male with a metal plate *in-situ* over his tongue. Durham Oriental Museum 1985-61. First Century AD.

510-11. Limestone and pottery objects, University College 31498-9. From Diospolis Parva.

512. Top of surveyor's mark, University College 31480. From Diospolis Parva, near Roman Temple. Petrie, *Diospolis Parva*, 56.

Coptic Period

513. Sherds of a painted jar, Birmingham City Museum A.236-1984. From Qasr Ibrim, excavation no. 78.3.11/6. Classic Christian. Gift of the Egypt Exploration Society.

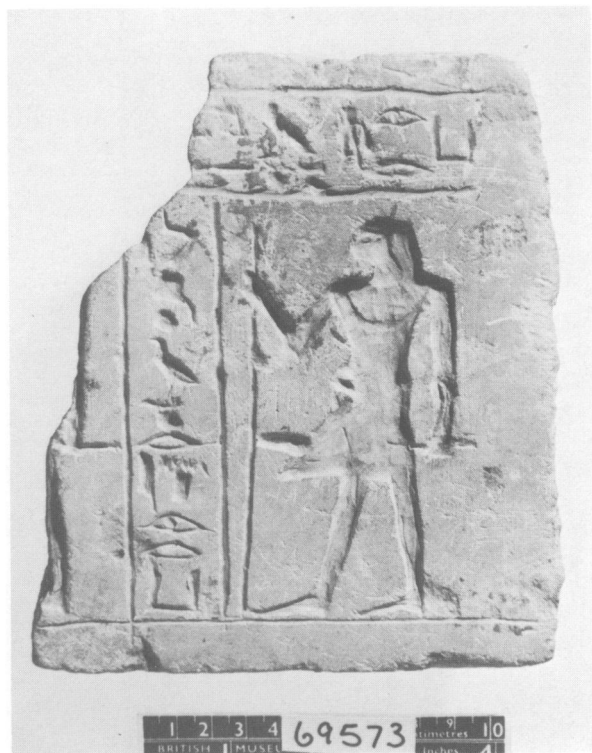
514-15. Painted pot and sherd, Birmingham City Museum A.235-1984, A.237-1984. From Qasr Ibrim, excavation nos. 66/35, 66A/385. Gift of Egypt Exploration Society.

516. Globular pottery storage jar with black painted decoration, Bolton Museum 161.1985/3. From Qasr Ibrim LC1-4, 66A/71. Late Christian. Gift of Egypt Exploration Society.

Date Uncertain

517-19. Pieces of green chrysophase or fluorite, green felspar and turquoise, and talc set in sandstone, University College 35524-6. From Serabit el-Khadim or Wadi Maghara mines. Petrie. *Researches in Sinai*, 57 p. 61.

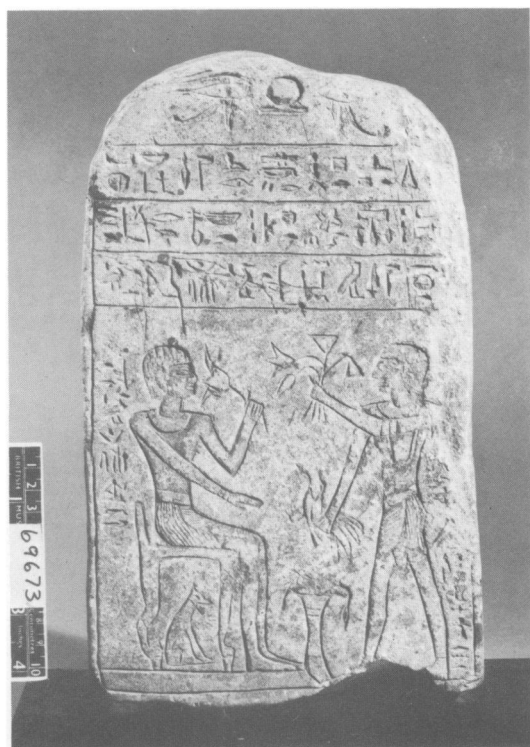
520-1. Fragments of animal bone and flattened roll of baked clay, University College 35545-6. From Abydos, temenos (town) of Osiris.



1. British Museum EA 69573 (no. 162)



2. Ashmolean Museum 1985.152 (no. 174)



3. British Museum EA 69673 (no. 190)



4. British Museum EA 69667 (no. 211)

BRIEF COMMUNICATIONS

Further reliefs from the chapel of *Rc-htp* at Meydum

A substantial number of decorated Old Kingdom tomb-chapels are published as line drawings, with few, if any, plates included to illustrate aspects of sculpturing technique. In order to establish the identity of fragments that might originate from such tombs, it is therefore necessary to place greater emphasis on subject matter as a criterion for identification, and less on details of style. This point is demonstrated in the study of two reliefs which can be attributed to the chapel of *Rc-htp* at Meydum. Their identification increases the number of fragments known from this chapel, and raises hopes that small reliefs from the same source have found their way into museum storerooms or private collections.

IN *JEA* 72 (1986), I presented a reconstruction of the decorated walls in the Fourth Dynasty chapels of *Rc-htp* and his wife, *Nfrt*, at Meydum.¹ The present discussion is a brief addition to that paper, written to describe various techniques of identification and to include two small reliefs to the damaged scenes in the chapel of *Rc-htp*.

1. Fitzwilliam Museum, Cambridge, EGA.3077.1943 (formerly in the Gayer-Anderson Collection)
Provenance unknown

This fragment shows the upper half of a minor male figure facing right (pl. XII, 1).² The left arm (now cut off at the shoulder) was raised, while the right arm (now cut off above the elbow) hung loose. There are indications that the relief was 'tidied up' to make it a more attractive salepiece. All four sides are neatly trimmed, and a small section of the figure's head is patched with a pale-coloured substance. Above the head there is a diagonal cut resembling the left side of a bucket-shaped basket, like those carried by many estate figures in early reliefs. However, the edge of this basket has been ground away, leaving a background mark too deep to remove. No paint remains on the body of the figure but there are traces of black paint on its head, probably representing real hair as opposed to a wig. Both the collarbone and bicep muscle are suggested by modelling, while the ear, eye, septum, and lips are defined by precise carving. The best indications as to the identity of the Fitzwilliam relief are the size and posture of the figure and the quality and style of its execution. In every respect these details link it with male estates depicted in the tomb of *Rc-htp* and *Nfrt* at Meydum.

Of the eight right-facing estates still preserved when Petrie made line drawings of the walls, only three are missing from my reconstruction in 1986:

- (1) A male estate in the upper register of the left sidepiece of the recess of *Nfrt*
- (2) A male estate in the upper register of the south wall of the chapel of *Rc-htp*
- (3) The upper half of a male estate in the upper register of the east entrance wall of the chapel of *Rc-htp*.

The first two could not possibly be identified as the Fitzwilliam relief. The right arm of estate (1) was already damaged at the shoulder when Petrie made his drawings, while the same arm on estate (2) must join directly to a large fragment of the south wall in the Egyptian Museum in Cairo.³ By comparison, the hanging arm of the Fitzwilliam figure is well

¹ Y. M. Harpur, *JEA* 72 (1986), 23-40.

² The relief is on display in the Fitzwilliam Museum. I should like to thank Miss Janine Bourriau for allowing me to publish it.

³ Harpur, 47, 54, figs. 3, 9.

preserved, and, since there is a space between this arm and the edge of the block, the fragment could only be the upper body of the male estate on the south side of the east wall, also in the Egyptian Museum (fig. 1).¹ As a final check, a facsimile copy of the Fitzwilliam fragment was matched to the half-preserved estate in the Cairo relief, then the size of the reconstructed figure was compared with that of other male figures in the composition. The differences were so minimal that the identity and position of the fragment were confirmed.

It is almost certain that the Fitzwilliam relief was a fragment stolen from the chapel of *Rc-htp* between 1891 (when Petrie copied the decoration) and 1910 (when he was authorized to transfer the vandalized walls to the Egyptian Museum). No record survives as to its date of purchase by Gayer-Anderson, but it could well have been 1901–2 when other fragments from the same wall turned up in Cairo, in the possession of two dealers called 'Ali' and 'Nahman'.²

2. Pushkin Museum, Moscow, I.1.a.5569(4034) (formerly in the Golenischev Collection)
Provenance: Saqqara (Hodjash and Berlev, see n. 6)

This is a relief fragment of a right-facing male figure, similar in proportions to the figure in the relief just described (pl. XII, 2).³ The right arm of this man curves slightly outwards and is broken at the wrist, while the left arm is bent more noticeably at the elbow so that the forearm is held away from the body. In this hand the man clutches the neck of a wild duck. Its raised wing fans across the simple outlines of the man's kilt and knotted waistband, creating a distinctive pattern of flared lines and curves in the lower foreground of the relief. The fragment was acquired by Vladimir Golenischev in the early years of this century and entered the Pushkin Museum with the rest of his collection after his death. In the recent publication of the Golenischev Collection, Hodjash and Berlev suggest that it comes from Saqqara and can be dated on stylistic grounds to the Sixth Dynasty. The figure has a roundness of form that is often synonymous with Saqqara workmanship, but its high relief and the smooth, flat background are characteristic of Third and early Fourth Dynasty sculpture, particularly in private tombs at Meydum and Saqqara and in the temple reliefs of Snefru at Dahshur.⁴ The posture of the figure and position of the bird are the clearest indications of the relief's likely provenance and identity. Every line seems to correspond with Petrie's drawing of the male bearer in register (3) of the south-east wall of the chapel of *Rc-htp* (fig. 1). To demonstrate this, eleven similarities are noted below:

1. The ear is in an unusually high position in relation to the eye.
2. The figure's right arm is curved outwards slightly, because something was originally depicted in its right hand.
3. There is a break in the stone below the figure's right wrist.
4. The left forearm bends outwards away from the body.
5. The left hand is shown from the outside as a fist rather than from the inside with fingers visible.
6. A live bird is held by the neck rather than by the wings or feet.
7. The bird's upper wing and foot are in exactly the same position in both cases.
8. The number and angle of the feathers spread over the kilt are the same in both cases.

¹ Harpur, 53, fig. 8 [7].

² Ibid. 54–5. A dealer called Nahman is mentioned in the *Journal d'Entrée* next to entries recorded in the 1930s. If this is the same man, he must have been active in the Cairo antiquities trade for many years.

³ S. Hodjash and O. Berlev, *The Egyptian Reliefs and Stelae in the Pushkin Museum of Fine Arts, Moscow* (Leningrad, 1982), 9 [introduction], 47 [14], 54 [14]. Photographic reproduction courtesy Pushkin Museum.

⁴ Good examples are: W. S. Smith, *A History of Egyptian Sculpture and Painting in the Old Kingdom* (London, 1946), pls. 35 (*htj-rs*), 36b (*Jj-nfr*), and 36c (*Hc-brw-Skr*). Cf. also A. Fakhry, *The Monuments of Snefru at Dahshur*, II. *The Valley Temple, Part 1, The Temple Reliefs* (Cairo, 1961), pls. xiii–xxxii.

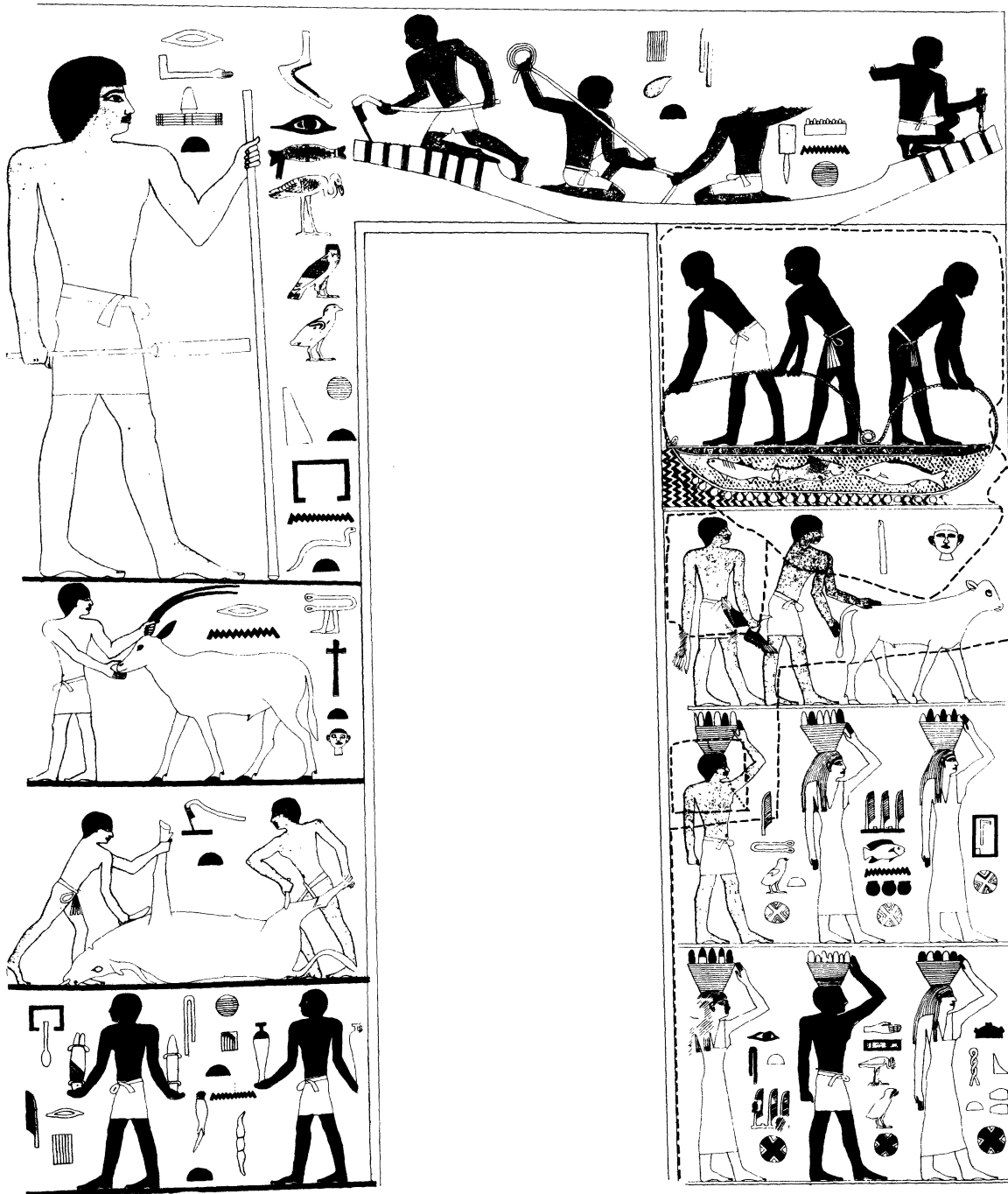
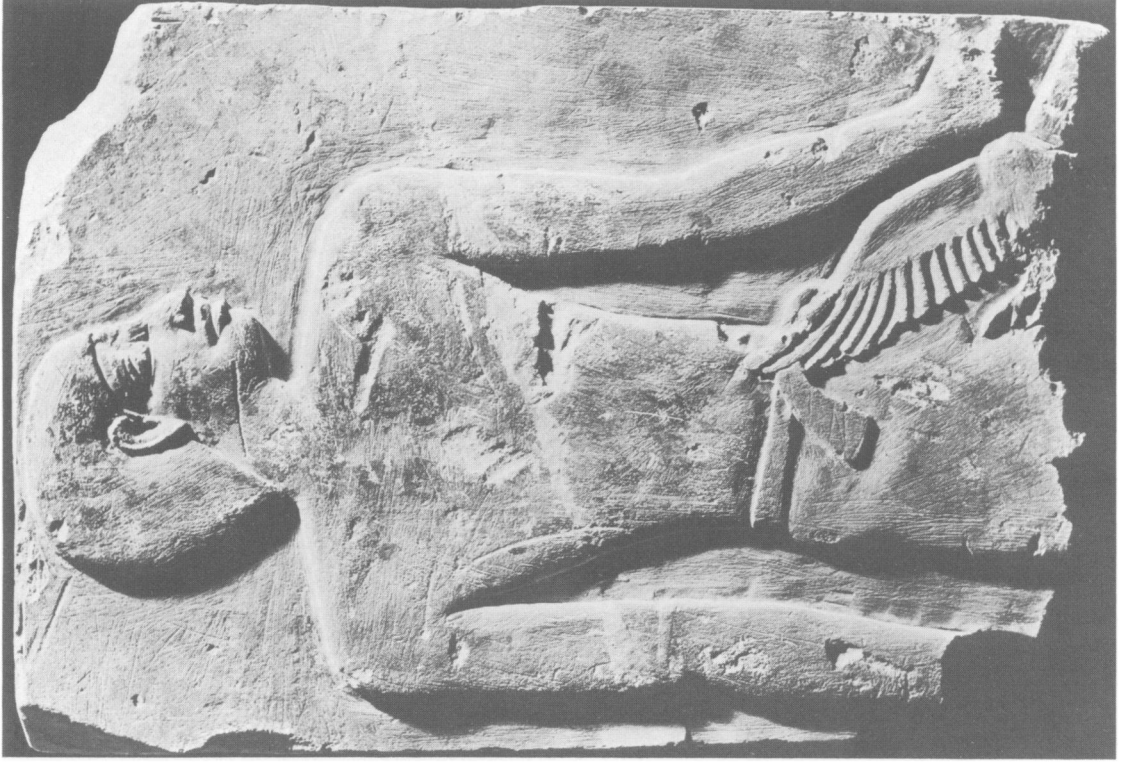


FIG. 1



1. Fitzwilliam Museum, Cambridge, EGA. 3077.1043



2. Pushkin Museum, Moscow, I.1.a.5569

9. The number and angle of the feathers spread across the knotted waistband, and the amount of knotted cloth still visible, are the same in both cases.
10. The sloping edge of the stone near the crown and forehead of the Pushkin figure corresponds with the edge of the dragnet scene fragment (i.e. East Berlin No. 15756).¹
11. Although numerous Old Kingdom tomb reliefs include a male bearer carrying a wild duck, the angles of the bearers' limbs and the spread of the duck's wings and feet do not correspond in every detail with the same features on the Pushkin relief or in Petrie's line drawing.

As confirmation, an enlarged version of Petrie's line drawing was superimposed on a photograph of the Pushkin fragment, enlarged to its true size, then an acetate drawing of the latter relief was matched to the east wall registers in Cairo. Allowing for slight distortions in each process, there is no doubt that the Pushkin piece is the upper body of the bearer in the third register.²

The Fitzwilliam and Pushkin reliefs are relatively insignificant parts of *Rc-htp*'s chapel reconstruction, yet they effectively illustrate the problems in matching fragments to line drawings in publications without plates. Additionally, their identification proves that quite small fragments of the chapel were removed for sale, hence the possibility that similar pieces exist in private collections like those of Gayer-Anderson and Golenishev.

YVONNE HARPUR

A slab of *'Int-kʿs* in the Fitzwilliam Museum

Publication of a slab showing *'Int-kʿs* and her daughters, which has been in Cambridge since 1909, although nothing is known of its origins. It is suggested that it was originally part of a tomb at Giza. Stylistically, it resembles pieces of both the early and late Old Kingdom. A date in the mid-Fifth or late Sixth Dynasty is likely.

Description

The subject of this communication (pl. XIII, 1) is Fitzwilliam Museum E.7.1909, a rectangular Old Kingdom slab showing *'Int-kʿs* and her children.³ It was bought by the Fitzwilliam in 1909, but its provenance is unknown. It is of limestone, and measures 76 cm in height, 48 cm in width, with a maximum thickness of 12 cm. The decoration is carved in raised relief with the exception of the texts at the top, which are sunk. A border is indicated on all but the top side, although the border line on the right stops at the head of the principal figure.

The owner of the monument stands at the right-hand side of the block facing left. Her right arm is clasped to her breast while her left hangs beside her. She wears a fully detailed tripartite wig, a plain close-fitting dress with shoulder straps, a collar, bracelets, and anklets. The right-hand part of the text at the top of the slab, in two horizontal lines with the same orientation, consists of: *ḥm(t)-ntr Hwthr ḥm(t)-ntr Nt 'Int-kʿs*, 'The priestess of

¹ H. Fechheimer, *Die Plastik de Aegypten* (Berlin, 1914), pl. 104 [No. 15756, including an artist's drawing of the register below the dragnet scene]. Unfortunately, this relief was destroyed during the Second World War.

² In their introduction Hodjash and Berlev observe that Golenishev acquired several reliefs in the early years of this century from a Cairo antiquities dealer called 'Ali'. It is conceivable that the Pushkin fragment was purchased from the same 'Ali' as the one from whom Borchardt acquired the dragnet scene and two other reliefs from the east wall (see nn. 2-3, p. 198).

³ I wish to thank Miss Janine Bourriau for permission to publish this piece, and also to Dr C. Simon for help and information. I am also indebted to Dr Yvonne Harpur for her comments on stylistic matters. The slab will be illustrated in Miss Bourriau's forthcoming brief introduction to the Egyptian Collection, *Ancient Egypt and the World*.

Hathor and priestess of Neith, *'Int-kʿs'*. † in the deceased's name is reversed, as in that of her daughter at her feet. As is not uncommon in the Old Kingdom, all examples of this sign were carved the same, regardless of the orientation of the text.

In the bottom right-hand corner is a small figure of a girl whose name is perhaps to be read *Nfr-pds*. Her right arm is placed around the left leg of *'Int-kʿs* who may be her mother. *Nfr-pds* is naked; her hair is arranged in a sidelock. This reading of her name does not take into account the small round sign above her head. It seems too small for *h*; perhaps the signs are transposed, and the second word is *psd*, 'light up' (*Wb.* 1, 556–8), which can have a circular (solar) determinative. The principal objection to this is that this word seems otherwise unattested before the New Kingdom. No similar names can be found in Ranke, *Personennamen*.

In front of *'Int-kʿs*, to the left of the stone and facing right, are two registers each depicting two women. Each figure is approximately half the size of *'Int-kʿs*. They are identically dressed, with undetailed tripartite wigs, close-fitting dresses, collars, and bracelets. The only exception to this is that the leftmost woman in the upper register has no bracelet on her right arm. They all adopt the pose (in reverse) of *'Int-kʿs*. Above the two upper women is the following text, in two lines facing right: *zʿ(w)tʿs Hnwtsn* (right), *Ny-ꜥnh-Bʿstt* (left), 'Her daughters, *Hnwtsn* and *Ny-ꜥnh-Bʿstt*'. Before the face of the right-hand woman in the lower register is *zʿ(w)tʿs*, 'her daughters', referring to both women but so located due to the lack of space above them. Their names are written in front of their legs: the woman on the right is named *Ny-kʿw-Hwthr* and that on the left *Ny-ꜥnh-Hwthr*.

The preservation of the piece is generally excellent. The hieroglyphs in the top right-hand corner have been damaged, presumably as a result of the removal of the block. Otherwise, slight damage has been suffered by the facial features, particularly of *Ny-kʿw-Hwthr* and, to a lesser extent, *'Int-kʿs* and *Nfr-pds*. An attempt has been made to erase the *zʿ*-goose before the lower pair of women. It is possible that such localized damage was in fact deliberately carried out. The quality of the raised relief is good, but that of the sunk is of a lower standard. No traces of colour are preserved.

Comments

Parallels for this piece have proved extremely difficult to uncover. There is a strong similarity to a pillar in the rock-cut chapel of the king's daughter (probably of Khafre) *Hmt-Rc* at Giza.¹ The deceased is shown facing three registers of her children of both sexes, with a small daughter behind her grasping her leg in the manner of *Nfr-pds*. There the similarity ends. It is surely impossible that a stone of the shape of Fitzwilliam E.7.1909 could have originated in a pillar, although how it would have fitted into a mastaba wall is unclear. The likely date of the tomb of *Hmt-rc* is the beginning of the Fifth Dynasty. A less similar example shows *Snt-itis*, the wife of the dwarf *Snb* (probably Sixth Dynasty), seated with two children before her and one behind.²

Both these tombs are at Giza. A persuasive argument in favour of a Giza origin for our block is the choice of names: *Hnwt-sn*, because of its associations with the similarly-named wife of Khufu, is very common at that site; *'Int-kʿs* is also found almost exclusively at Giza.³ Of the numerous women named *'Int-kʿs* known from that site, none is clearly the same as our subject; the owner of the offering basin BM 1175⁴ was also 'priestess of Hathor', but her

¹ S. Hassan, *Giza* VI [3] (Cairo, 1946), fig. 41.

² H. Junker, *Giza* V (Vienna/Leipzig, 1941), 91 Abb. 23. I prefer Junker's dating of this tomb (*ibid.* 3–6) to N. Cherpion's assignment to the Fourth Dynasty (*BIFAO* 84 (1984), 35–54).

³ Compare the number of references in the indices of PM III², 370 (Giza) with 956–7 (Saqqara); *Hnwt-sn*: 14 (Giza), 4 (Saqqara); *'Int-kʿs*: 8 (Giza), 2 (Saqqara).

⁴ *BM Stelae* 1², pl. xxxiii (4).



1. Fitzwilliam Museum, Cambridge, E.7.1909

A SLAB OF 'INT-K₃S (pp. 200-2)



2. Block from north wing of Second Pylon, Karnak

EIN KÄFIG FÜR EINEN GEFANGENEN (p. 206)

name is written with \sqcup rather than \sqsubset . This title is so common in the Old Kingdom that we should place no great emphasis on its presence or absence without other evidence.

The date is far more problematic than the provenance. Certain aspects of the way the deceased is represented recall the style of the Fourth and early Fifth Dynasties: the very broad shoulders are reminiscent of the reliefs of *Ḳbtt* and *Wnšt*¹ as well as that of *Hmt-r* (above) and the relief possibly showing *Htp-hrs* and *Mrs-ḥḥ*.² These figures are also drawn so as to leave a gap between the body and arm of the woman through which the wig is sometimes visible, a feature which disappears later in the Fifth Dynasty. Against this, the use of sunk relief is extremely rare at Giza before the middle of the Fifth Dynasty, while sidelocks appear first in the Old Kingdom on figures of male children, and only later are they seen on girls,³ favouring a later rather than earlier date in the Fifth Dynasty.

The jewellery points towards a later date. In the depictions of women of the Fourth to early Fifth Dynasty noted above, there appears to be at most one bracelet per arm, and the anklets are plain or almost non-existent, while the bracelets worn by the wife of *Snb* (above) provide one of the closest parallels to those of *ʿInt-kʿs* and her family. It is possible that the wearing of bracelets and anklets in this fashion developed during the Fifth Dynasty. It is, of course, just possible that this piece is an archaizing example from the later Old Kingdom, when some of the earlier Old Kingdom stylistic devices reappear,⁴ although the cutting of the slab of *ʿInt-kʿs* is of rather a higher standard than most of these late pieces. I feel that the date must remain rather vague, with a preference for either the middle Fifth Dynasty or the later Sixth Dynasty.

ʿInt-kʿs held two titles ‘priestess of Hathor’ and ‘priestess of Neith’, both extremely common titles of women in the Old Kingdom. Although such names are common, the attachment of *ʿInt-kʿs* to the cult of Hathor was surely responsible in part for the naming of two of her children after the goddess.⁵

NIGEL STRUDWICK

Ein Käfig für einen Gefangenen in einem Ritual zur Vernichtung von Feinden

In *JEA* 71 (1985), M. Lehner and P. Lacovara sought to explain the ‘enigmatic wooden object’ found on the south side of Cheops’ pyramid in 1960 as a frame for the transport of a tomb statue. It is here suggested that the object is actually a cage for a prisoner, which was used in a ritual for the destruction of the enemy. In support of this are cited a relief block from Karnak depicting just such a cage, and the Ritual for the Destruction of the Enemy (P. BM 10081), as well as the so-called Execration Texts and captive statues of the Old Kingdom.

Im *JEA* 71 (1985) wurde versucht, eine Erklärung für das ‘enigmatic wooden object’ zu geben,⁶ das 1960 an der Südseite der Cheopspyramide in einer mit dünnen Leinenschnüren versiegelten Holzkiste entdeckt worden war, die sich in einer aus dem Felsen herausgehauenen Nische am Boden eines tief in den felsigen Untergrund getriebenen und mit drei großen Kalksteinblöcken verschlossenen Schachtes befand.⁷

Danach hätte es sich bei diesem rätselhaften Objekt (fig. 1)⁸ um ein Gestell zum Transport der Grabstatue, resp. der Ka-Statue, gehandelt, das ursprünglich auf einem der typischen

¹ Junker, *Giza* 1, Abb. 51, 63.

² Smith, *Sculpture*, pl. 45.

³ Müller, *LÄ* III, 273-4.

⁴ Such as the figure of *Ny-ḥḥ-Hwthr*, Junker, *Giza* IX, Abb. 39.

⁵ The following references to the names come from Ranke, *PN* 1: *ʿInt-kʿs* 36 (7), *Hwt-sn* 244 (1), *Ny-ḥḥ-Bʿstt* 171 (8), *Ny-kʿw-Hwthr* 180 (24), *Ny-ḥḥ-Hwthr* 171 (18). The name *Ny-ḥḥ-Bʿstt* is normally male.

⁶ M. Lehner und P. Lacovara, *JEA* 71 (1985), 169-74.

⁷ A. H. ‘Abd el ‘Al und A. Youssef, *ASAE* 62 (1977), 103-20, Taf. 1-15; A. H. ‘Abd el ‘Al, *ASAE* 63 (1979), Taf. 1-2.

⁸ Nach Lehner und Lacovara, op. cit. 169 fig. 1.

Transportschlitten befestigt gewesen wäre.¹ Die beiden Ösen an der hohlkehlenartigen Bekrönung hätten demnach zur komplizierten Befestigung und Stabilisierung dieses Gestells auf dem Transportschlitten mittels des Zugseiles gedient.²

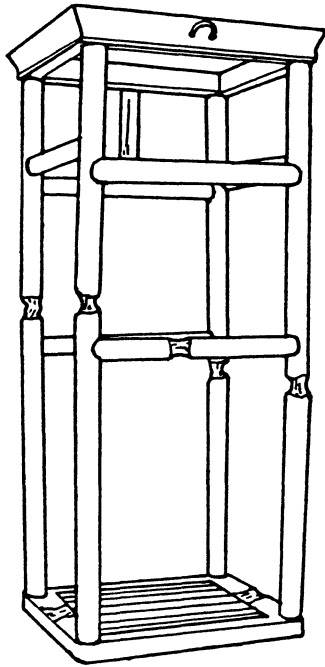


FIG. 1



FIG. 2

Als Beleg dafür wird die auf Grabreliefs—vor allem des Alten Reiches—häufige Darstellung des Statuentransportes zum Grabe herangezogen,³ jedoch zeigt keine einzige dieser Szenen solch ein Transportgestell, sondern es werden stets mit Türen verschließbare Statuenschreine abgebildet, die keinerlei Ähnlichkeit mit dem zur Diskussion stehenden Objekt aufweisen.⁴ Beim einfachen Statuentransport, also ohne Statuenschrein, wird außer dem gebräuchlichen Transportschlitten kein weiteres Transportgestell verwendet.⁵

Es existiert nun aber in der Tat eine direkte Parallele, und zwar auf einem Reliefblock, der von Legrain in Karnak gefunden worden ist, möglicherweise vom 2. Pylon stammt (leider fehlt eine genaue Ortsangabe) und wohl in die Zeit Tutanchamuns datiert (pl. XIV, 1).⁶ Dieses Relieffragment zeigt ein—nach den zahlreichen Ruderern zu urteilen—außergewöhnlich großes Schiff mit prunkvollem, urängeschmücktem Aufbau, an dessen Vorderseite

¹ Ibid. 170-1, 174.

² Ibid. 174, 175 fig. 4.

³ Ibid. 170-1.

⁴ Cf. M. Eaton-Krauss, *The Representations of Statuary in Private Tombs of the Old Kingdom* (Wiesbaden, 1984), Taf. 1 (58), 10 (78), 13 (85, 86), 16 (98, 99), 17 (100, 107), 19 (110, 111, 112), 20 (115), 21 (126), 22 (127), 24 (138, 139); Fr. Abitz, *Statuetten in Schreinen als Grabbeigaben in den ägyptischen Königsgräbern der 18. und 19. Dynastie* (Wiesbaden, 1979), Abb. 16.

⁵ Cf. Eaton-Krauss, op. cit. Taf. 10 (75, 76), 11 (81, 82), 12 (83, 84), 14 (94, 95), 15 (96, 97), 18 (108, 109), 20 (113-14), 21 (125), 22 (128, 129).

⁶ H. Chevrier, *ASAE* 53 (1956), 11, Taf. 7.

einem undatierten Papyrus der Nektaneboszeit, in drei Sprüchen gegen Feinde überliefert.¹ Es trägt dort den Titel:

pꜣ ḥtm rꜣ nj ḥftjw
ḥtm ḥbt

‘Das Versiegeln des Mundes von Feinden.
Das Versiegeln der Richtstätte.’²

Die ersten sechs Sätze des ersten Spruches lauten:

ḥtm pt ḥr jmjꜣs
ḥtm tꜣ ḥr jmjꜣf
ḥtm ḥrj-n(t)jw-ꜣ m ꜥwnw
ḥtm bꜣj m Rꜣ-stꜣw
ḥtm ḥtm m ꜣbdw
ḥtm ꜣf(d)t m ꜥꜣt-psꜣ

‘Versiegelt ist der Himmel über dem, was in ihm ist.
Versiegelt ist die Erde über dem, was in ihr ist.
Versiegelt ist ‘Der mit Ritualvorschriften gefüllte’ in Heliopolis.
Versiegelt ist das Erdloch in *Rꜣ-stꜣw*.
Versiegelt ist der Kasten in Abydos.
Versiegelt ist der Kasten in *ꜥꜣt-psꜣ*.’³

Dabei scheint der vierte Satz, der vom Versiegeln des Erdloches in *Rꜣ-stꜣw* (= Gîza!)⁴ handelt, unmittelbar auf das in Gîza vollzogene Ritual zur Vernichtung von Feinden Bezug zu nehmen. Durch die rituelle ‘Versiegelung’ des Käfigs tief in der Erde sollten symbolisch alle Feinde unschädlich gemacht werden.

Für eine Datierung des Objektes in die 4. Dynastie⁵ spricht nicht nur die Fundlage zwischen der Chephrenpyramide und der Nebenpyramide G II-a, sondern auch die Technik und der enorme Aufwand mit dem es im Felsboden ‘versiegelt’ worden ist. Die Aktivitäten in Gîza beschränken sich jedoch nicht allein auf das Alte Reich, sondern werden vor allem im Neuen Reich in großem Umfang wiederaufgenommen⁶ und dauern bis in die Spätzeit an.⁷ So galt Gîza—wie aus dem Text aus der 30. Dynastie(?) hervorgeht—bis in die Spätzeit als einer der Orte, an dem die rituelle Feindvernichtung stattfindet.

Die rituelle Feindvernichtung läßt sich vom Alten Reich bis in die ptolemäisch-römische Epoche in zahlreichen Varianten belegen. Für die Zeit des Alten Reiches sei dazu auf die mit den sog. Ächtungstexten und Personennamen beschrifteten Feindfiguren aus Gîza verwiesen,⁸ sowie auf die Gefangenenstatuen aus dem Pyramidenbezirk des Königs Djoser,⁹ aus dem Totentempelkomplex des Königs Niuserre und aus den Pyramidentempeln der Könige Djedkare-Asosi, Unas, Teti, Pepi I., und Pepi II..¹⁰

Nachtrag

Frau Hourig Sourouzian hat mich freundlicherweise in einem Gespräch darauf aufmerksam gemacht, daß der von Legrain gefundene Block zu einem Ensemble von Reliefblöcken

¹ S. Schott, *ZÄS* 65 (1930), 35–42.

² *Ibid.* 35–6.

³ *Ibid.* 36–7.

⁴ Cf. Chr. M. Zivie in *LÄ* II, 607–8; *idem.*, *JEA* 70 (1984), 145.

⁵ So Lehner und Lacovara, *op. cit.* (ohne Diskussion der Datierungsfrage).

⁶ Chr. M. Zivie, *Giza au deuxième millénaire* (Kairo, 1976); *idem.* in *LÄ* II, 606–7.

⁷ *Idem.* in *LÄ* II, 608.

⁸ G. Posener in *LÄ* I, 67–8; s. zuletzt A. M. Abu Bakr und J. Osing, *MDAIK* 29 (1973), 97–133; J. Osing, *MDAIK* 32 (1976), 133–85. Aus Gîza auch aus der 18. Dynastie (!) belegt: G. Posener, *op. cit.* 68.

⁹ J. Ph. Lauer und J. Leclant, *RdE* 21 (1969), 61 mit nn. 4–5.

¹⁰ *Ibid.* 55–62, besonders 60 mit nn. 1–5.



1. Fitzwilliam Museum, Cambridge, E.7.1909

A SLAB OF 'INT-K₃S (pp. 200-2)

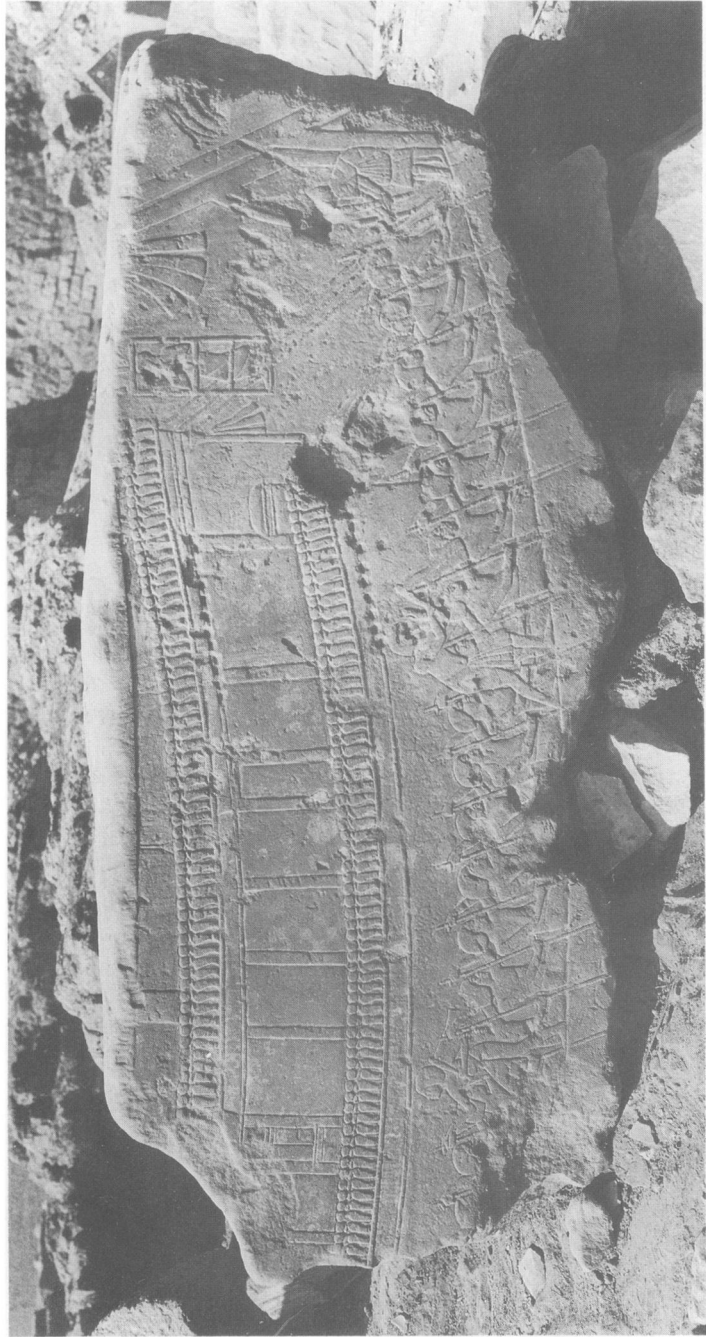


2. Block from north wing of Second Pylon, Karnak

EIN KÄFIG FÜR EINEN GEFANGENEN (p. 206)



2.



1.

Block probably from north wing of Second Pylon, Karnak
EIN KÄFIG FÜR EINEN GEFANGENEN (pp. 203-4)



1. Block from north wing of Second Pylon, Karnak

EIN KÄFIG FÜR EINEN GEFANGENEN (p. 206)



2. Vienna AS 5814

Courtesy Kunsthistorisches Museum, Vienna

THE VIENNA STELA OF MERYRE (pp. 213-16)



3. The walking-stick of Meh. N.1440. *Courtesy the Oriental Museum, University of Durham*

TWO WALKING-STICKS (p. 218)

gehört, auf denen Szenen aus einem großen Triumphzug des Tutanchamun dargestellt sind.¹

Einige der mir bekannten Reliefblöcke aus diesem Ensemble lassen sich dabei, wie ein Vergleich der Darstellungen und Inschriften zeigt, direkt hintereinander anordnen und zu einer Szenenfolge zusammenstellen:

1. Zwei Reliefblöcke, die im Jahre 1953 von Chevrier im Nordflügel des 2. Pylons des Amun-Tempels von Karnak gefunden worden sind.² Der eine Block (pl. XIII, 2)³ zeigt als zentrales, durch die Größe der Einzelfiguren deutlich hervorgehobenes Motiv das Vorführen an den Handgelenken, bzw. an den Ellbogen gefesselter und durch einen um die Hälse geschlungenen Strick aneinandergebundener nubischer Gefangener.⁴ Dahinter ist unterhalb einer nur sehr fragmentarisch erhaltenen Schiffsprozession eine Gruppe von Soldaten zu sehen, die, angeführt von einem Trompeter, aus mit Sichelschwertern bewaffneten und Trompeten tragenden ägyptischen sowie einem mit einem Speer bewaffneten asiatischen Soldaten besteht. Auf dem anderen, bisher unveröffentlichten Block (pl. XV, 1) ist der Aufmarsch mit Bogen bewaffneter nubischer und mit Speeren bewaffneter asiatischer Soldaten dargestellt.⁵

2. Ein Block im Kairener Museum, der ebenfalls aus dem 2. Pylon des Amun-Tempels von Karnak stammt, auf dem eine Truppenparade ägyptischer Soldaten und darüber eine Schiffsprozession, von der nur mehr der untere Teil erhalten ist, dargestellt ist.⁶

Es ergibt sich somit die Szenenfolge: Vorführen der gefangenen Nubier (pl. XIII, 2)—Aufmarsch der Soldaten und Schiffsprozession (pl. XIII, 2; XIV, 1; Kairener Block), wobei der von Legrain gefundene Block, auf dem das Prunkschiff mit dem Gefangenenkäfig zu sehen ist, in die über der Truppenparade dargestellte und aus mehreren Schiffen bestehende Schiffsprozession eingefügt werden kann.

ALFRED GRIMM

Remarks on the beings called *mrwty* or *mrwryt* in the Coffin Texts

The beings called *mrwty* or *mrwryt* mentioned in the Coffin Texts, seem to represent a divine complex which includes both the double uraeus and the pair of zoomorphic goddesses Isis and Nephthys. In spite of similarity of name, the two songstress goddesses *Mrtj* should not be included here.

In some chapters of the Coffin Texts certain beings called *mrwty* or *mrwryt* are mentioned,⁷ sometimes with words of praise and elsewhere as dangerous enemies of the deceased.⁸

¹ Dies bestätigt die von mir vorgeschlagene Datierung dieses Blockes in die Zeit Tutanchamuns. Dagegen datiert Abdul-Kader Mohammad in *ASAE* 56 (1959), 132 (mit Taf. 1) diesen Block in die Zeit Amenophis' II.; PM 11², 40 ('Various dates') wird er ebenfalls Amenophis II. zugeschrieben(!).

² Für die Überlassung der im Jahre 1955 im Hof des Chons-Tempels von Karnak aufgenommenen Photographien aus dem Nachlass von Siegfried Schott möchte ich Frau Erika Schott ganz herzlich danken.

³ PM 11², 40, dort jedoch in die Zeit Amenophis' IV. datiert; s. Leclant in *Or.* 23 (1954), 65 (b); Chevrier in *ASAE* 53 (1956), 8 fig. 1, 11, Taf. 1; Leclant in *Or.* 24 (1955), 299 mit n. 3, Taf. 19 (fig. 4), dort Tutanchamun zugeschrieben; Text: Urk. IV, 2047. 19-20 (779A), dort ebenfalls in die Zeit Tutanchamuns datiert.

⁴ S. Chevrier in *ASAE* 53 (1956), 8 fig. 1, 11; cf. Leclant in *Or.* 23 (1954), 65(b).

⁵ Cf. dazu einen weiteren, von Leclant in *Or.* 23 (1954), 65(b) angezeigten Block: 'Un autre bloc montre des soldats qui ont enfilé chacun trois mains humaines coupées à la pointe de leur lance.'

⁶ Kairo T. 8/6/24/4: PM 11², 40 ('Temp. Tutankhamūn'); W. und B. Forman-M. Vilímková, *Ägyptische Kunst aus den Sammlungen des Museums in Kairo* (Hanau/Main, 1962), Taf. 74.

⁷ D. Meeks, *Année lexicographique* 11, 78.1785, 78.1790.

⁸ R. O. Faulkner, *Ancient Egyptian Coffin Texts* (Warminster, 1973), Spells 439, 441-8, 450.

They are 'the two *mrwty*', their nature is feminine and dual, and their names are rendered thus:



The goddesses Isis and Nephthys also occur as two *drty*-birds (kites)² or as two female falcons.³ The *mrwty* are described in the Coffin Texts as 'the Companions of Re' (and sometimes of Osiris).⁴ In the Book of the Dead a pair of *mrty*-snakes of obscure meaning are mentioned. These beings are probably related to the royal uraeus cobras which accompany the sun disc (Companions of Re?), a very frequent image in ancient Egyptian religious iconography.⁵ The royal cobras share with the two female falcons, Isis and Nephthys, the role of appearing over the head of Atum according to the above mentioned religious texts. In some versions of the Book of the Dead, the *mrty*-snakes are replaced by 'the Two Sisters', Isis and Nephthys.⁶ We seem, therefore, to be in the presence of a divine complex known as *mrwty* which includes both the double uraeus and the pair of zoomorphic goddesses Isis and Nephthys. The latter, in spite of their characteristic benign nature, would become, as part of the *mrwty* group, a force usually hostile towards the deceased, in a manner similar to that of other not well-defined entities known as 'the Messengers of Osiris'.⁷

In spite of similarity of name, the two songstress goddesses *mrty* should not be included in this divine complex because of their different nature and cultic characteristics.⁸

ALBERTO BIANCHI

Amenophis III's vizier Amenhotep at Silsilah East

Work of the Archaeological Survey of Egypt at Gebel es-Silsilah has confirmed Legrain's suggestion that the Amenhotep shown adoring the cartouches of Amenophis III in a shrine on the east bank was 'the overseer of the City and vizier' of that king. The same officer must be the *'Imn-htp* also shown adoring Amenophis III's cartouches on Legrain's Stela B, where the man's name is legible but his titles are not. It is possible that he was

¹ A. De Buck, *The Egyptian Coffin Texts* (Chicago, 1935-47), V297a,e,g; 301d; 311g; 315c,h. The last determinative, which appears only once, may be due to the word for love (*mrwt*) which it usually follows. G38 and G39 both occur as generic determinatives for birds, cf. A. Gardiner, *Egyptian Grammar*³ (Oxford, 1957), 471.

² Faulkner, *Concise Dictionary of Middle Egyptian* (Oxford, 1962). The two *drty*-birds were Isis and Nephthys as mourners of Osiris; usually two women played this role while the deceased was carried to his grave.

³ T. G. Allen, *The Book of the Dead and Coming Forth by Day* (Oxford, 1974), Ch. 17a § 6. It should be noted that there is also a bird with a name very similar to those of the *mrwty* and the *mrty*, the *mrwryt*, which is represented as a black stork. See N. Davies, *JEA* 35 (1949), 16, pl. 2 and Faulkner, *AECT* II, 77 n. 2. Faulkner pointed out as well that an epithet applied to the goddess Sekhmet is *mrwytt*, out from which *mrwt* with its terrible connotations may have originated. I agree with Faulkner that *mrwty* need not be translated as 'well-beloved' since the sign 𓆎 also occurs in numerous words of different meaning, e.g., *mrty* 'banks', *mr* 'bind', *mrt* 'street', etc. Semantic studies do not seem, therefore, to be of much help by themselves in finding the true nature of the *mrwty*.

⁴ Faulkner, *AECT* II, 78.

⁵ Allen, *ibid.* Ch. 37, 41b, 58 § S1, 122a § S1. These two snakes appear alone in the Papyrus of Hunefer, without the company of the sun-god, facing the deceased, standing over papyrus stems. See Rossiter, *The Book of the Dead, Papyri of Ani, Hunefer, Anhai* (Fribourg-Geneva, 1979), 87.

⁶ Alvarez Flores, *Libro de los Muertos* (Barcelona, 1973), Ch. 37, 67 (Papyrus of Nu).

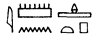
⁷ Isis, Nephthys and the Baboon are sometimes 'The Examiners' (*sipw*) who frightened the deceased in the Place of Sacrifice and whose vigilance it was impossible to escape. Cf. Allen, *op. cit.* Ch. 17 § 5. It is also possible that there is some relationship between the meaning of the *mrwty* and that of the Messengers of Osiris whose dangerous presence is explicit. These beings already appear in the Pyramid Texts § 125 and § 136 and in the CT Sp. 236 and 458. In the Book of the Dead they are mentioned in Ch. 39, 31a, 125 § 55 and 163 § 4. They are the *wprwtyw*, aggressive and much feared entities which are attached to benign gods. See Koenig, *RdE* 33 (1981), 33.

⁸ See my forthcoming study *The goddesses Mrty in ancient Egyptian religion*, written in collaboration with J. R. Ogdon, and which will offer an alternative to W. Guglielmi's views as expressed in his paper *Die Mrty, Ägyptische Vorläufer der Sirenen?*, 1st International Congress of Egyptology (Cairo, 1976), 34-6.

also represented in two more shrines at Silsilah East. In all these monuments his figure and identity docket were hacked out in antiquity for unknown reasons.

AMONG the scanty monuments on the east bank of the Nile at Gebel es-Silsilah are the wrecks of three small shrines of the naos type and a cliff-stela.¹ Dating back to the reign of Amenophis III, or c. 1400 BC, they were originally embellished with reliefs and hieroglyphic inscriptions of excellent workmanship. All were damaged with a vengeance in antiquity: the name of Amun was excised from them by the Atenists, the king's nomen *'Imn-htp* was erased, leaving only the empty frames of the cartouches. Even more extensive injury was inflicted when the figure, name, and principal titles of a high officer of the realm who appears to have been conspicuously represented in all four monuments were defaced by anonymous hands, doubtless bent on removing all traces of his presence and activities at Khenu and perhaps also elsewhere.

The shrines and the stela were already in sorry condition when Georges Legrain made a careful record of them in 1902.² Sometime between 1976 and 1978 they were again vandalized, texts and representations being badly disfigured, large portions of inscribed and sculptured surfaces broken off and shattered. As a result, a good deal of what Legrain had seen and copied is no more.

On the stela (Legrain's B) and on one of the shrines (F) the name of the dishonoured officer had survived, certainly not scatheless yet legible, and was correctly read  by Legrain, who suggested that this Amenhotep was the overseer of the City and vizier known to have been in the service of Amenophis III, although he could detect no trace of the titles *imy-r niwt* and *t'ty* in the gaps where he expected them to have stood. Most scholars who have since been concerned with vizier Amenhotep in one way or another have accepted, though not unreservedly, his identification, which has remained hypothetical for lack of conclusive evidence.³

Textual evidence confirming Legrain's view has come to light in the last few years through the work of the Archaeological Survey of Egypt at Silsilah East.⁴ Fig. 1 reproduces my own free-hand copy of the hieroglyphic identity docket of the proscribed officer, who is shown within a frame affecting the form of a round-topped stela cut on the exterior left-hand side wall of one of the shrines (Legrain's shrine F). Made in early February 1980 after close examination of the text under different light conditions, my copy was collated by Professor Jürgen Osing as a preliminary to the final life-size facsimile drawing of the shrine wall, which was completed *in situ*, with constant reference to the original; both hand copy and facsimile were once more checked against the original in January 1982. For some reason, Amenhotep's foes attacked this corner of the monument with less vigour than elsewhere and only scraped the surface of the text, which yields: *Giving praise to the good god and kissing the ground before the lord of the Two Lands by the hereditary noble and count, sole friend, overseer of the City, and vizier Amenhotep*. The vizier was depicted directly underneath, turned to the left, with both arms upraised in adoration, paying homage to his sovereign,

¹ PM v, 220: Shrines A, E, and F; Stela B.

² *ASAE* 4 (1904), 198 ff. The letters A, B, E, and F used by Legrain to designate the four monuments that concern us here are retained in this paper to facilitate reference.

³ See, e.g., A. Weil, *Die Veziere des Pharaonenreiches* (Strasbourg, 1908), 85 f. (§ 12); W. Helck, *Zur Verwaltung des Mittleren und Neuen Reichs* (Leiden/Cologne, 1958), 443 f. (17); idem. in *LÄ* 1, 22 s.v. Amenophis; Gordon, *MDAIK* 39 (1983), 77 f. with n. 10. To my knowledge only D. Redford, *History and Chronology of the Eighteenth Dynasty of Egypt* (Toronto, 1967), 151 with n. 266, does not concur with Legrain's opinion, and proposes to identify the Amenhotep of Silsilah East with the Memphite high steward Amenhotep son of Heby; he gives no reason for his identification nor does he substantiate it.

⁴ Cf. A. B. Lloyd, *JEA* 66 (1980), 3; Egypt Exploration Society, *Report for the Year 1979/80*, 4 f.; J. Leclant, *Orientalia*, NS 51 (1982), 87, no. 55 with nn. 199–201. I am obliged to the Egypt Exploration Society's Committee for permission to make use in this article of unpublished material from the files of the Archaeological Survey.

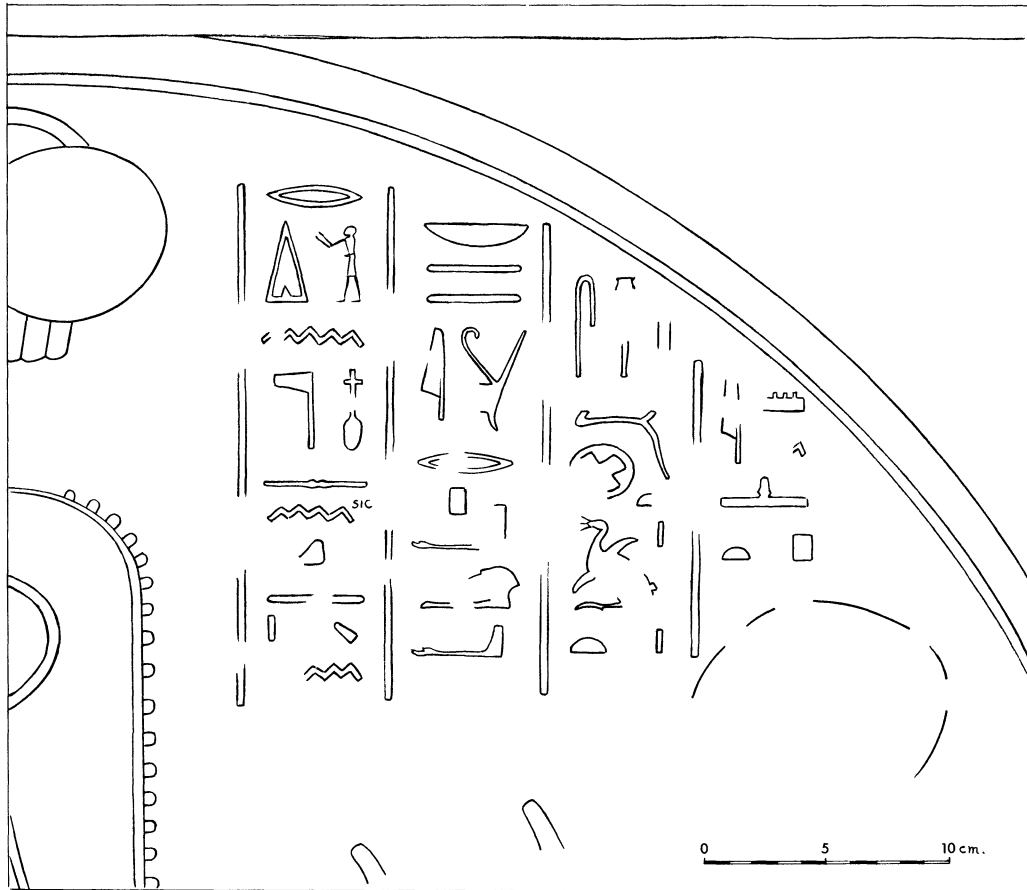


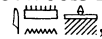
FIG. 1

who was represented by his cartouches.¹ His figure was thoroughly chiselled out; only fragments of the outline of his bald head and the finger-tips of the uplifted open hands still remain on the wall, as shown in our copy.

Who but this vizier Amenhotep could have been the officer similarly represented adoring Amenophis III's cartouches within a round-topped stela carved on the exterior right-side wall of the same Shrine F and equally hacked out? Unfortunately, on the right-hand side stela the obliteration of the male figure and accompanying legend was far more effective, leaving only the broken contour of his shoulders and shaven head and, above him, a dense tangle of criss-cross scratches made by the tools used to remove the inscription which must originally have been there and of which not a trace can now be detected.

The cliff-stela referred to above (Legrain's B) was the object of the same ill-treatment as shrine F both in ancient times and in the last decade. The name of Amun was removed from it by Akhenaten's agents; Pharaoh's nomen, *'Imn-htp*, was expunged, while his prenomen was allowed to remain undisturbed; also effaced were the two figures of an officer wearing an ankle-long garment wide below the waist and shown standing, with arms uplifted in a gesture of worship to the cartouches of his king; his main titles and name were also rubbed out from the two identity docketts in which they occurred. Like Shrine F, the stela received

¹ Cf. H. Grapow, *Wie die alten Ägypter sich anredeten, wie sie sich grüssten und wie sie miteinander sprachen* II (Berlin, 1960), 61 with n. 7, quoting *Urk.* IV, 330, 11-17.

severe injury from criminal hands between 1976 and 1978. Early this century Legrain found good traces of the officer's name, , in each of the label texts above the effaced figures, but could discern no vestige of the titles *imy-r niwt* and *ḥty* in the lacunae preceding the name, where the erasing had been done with great determination. Legrain's reading *ʿImn-ḥtp* was confirmed by Professor Osing and myself when we copied the stela in February 1980. It is therefore certain that the persecuted high officer depicted on Stela E was called Amenhotep, and, though the title of vizier cannot now be read, only the hypersceptical will doubt that he was the same Amenhotep shown in Shrine F.

The vizier Amenhotep might just possibly have also been represented in two other shrines at Silsilah East. In Legrain's 'Socle A', which bears an inscription of Amenophis III's year 35,¹ the figure of a man, with hands raised chin-high in sign of adoration and dressed in a garment full and swelling below the waist and reaching his ankles, was carved twice on the front and twice on the south side of the monument. The four representations were chiselled out in antiquity along with his occupational titles and name. The outline of the expunged figures was seen by Legrain and could still be readily traced in 1980 notwithstanding extensive additional damage done to the shrine two or three years earlier, but the erasure of the man's name and that of his actual office had been thorough.



The shrine designated 'Naos E' by Legrain, also *temp.* Amenophis III, was mutilated in ancient times, again grievously damaged in the 1970s, and is now an utter ruin. The interior north wall has preserved the lower end of a sculptured standing male figure attired in an ankle-length, wide robe and exhibiting clear signs that a serious attempt was made to obliterate it in antiquity; a loose fragment of the same relief indicates that the man was represented with uplifted arms. On the opposite side of the interior, and on a small fragment fallen from it, are again traces of a man on his feet and dressed in a long wide garment, apparently the counterpart of the representation on the north wall and also hacked out in ancient times.

That the wilfully mutilated figures in Shrines A and E were representations of the vizier Amenhotep is only a possibility. On the other hand, it is beyond question that the Amenhotep depicted in Legrain's Shrine F and Stela B was, as he suggested, Amenophis III's vizier.

Only a few other monuments of vizier Amenhotep are known to us from other sites;² they show no sign of violence. No Egyptian text has yet been found that offers any hint as to why such drastic action was taken to eradicate all vestiges of him at Khenu; neither is it possible to determine who did it or ordered it to be done, nor whether it was done during his lifetime or posthumously. It may be doubted that the why, the who and the when will ever be known.

RICARDO A. CAMINOS

¹ It seems appropriate to confirm here that the date *ḥt-sp 35 tpy (n) šmw sw 1*, 'Regnal Year 35, first month of Shomu, day 1', as copied by *LD* III, 81, c, and Legrain, *ASAE* 4 (1904), 198 (at end), and followed by *Urk.* IV, 1920, 3 (no. 713), was neatly carved in the first place and found intact and perfectly legible by the Archaeological Survey in 1980. The reiterated statement that the inscription bears 'la data dell'anno xxvii del regno di Amenôf III' is doubtless the result of a misreading: I. Rosellini, *Monumenti Storici*, Text, III, pt. 1, 215, and *Monumenti del Culto*, Text, 235. The alternative date '(or 27)' in *PM* v, 220, should be either qualified or cancelled; see also Gauthier, *LR*, II, 311, xviii, n. 3.

² They are listed in the Weil, Helck, and Gordon references in n. 3. Helck's and Gordon's attribution of a West Silsilah niche to the overseer of the City and vizier Amenhotep (under the *alias* Huy) is a mistake arising from a faulty copy of the name on a niche in the speos of Haremhab at Silsilah West. Vizier Amenhotep has no monument on the west bank at Silsilah. The niche mentioned by Helck and Gordon commemorates Ramesses II's vizier Khay, whose name occurs twice in it and is incorrectly given as  in J. Champollion, *Notices descriptives*, I, 261 (j); the correct reading is : I quote from my own 1975 copy which confirms the reading 'Chāi' in *LD*, Text, IV, 86, e, with n. 3; see also *KRI* III, 46, no. 20, 14 with n. a. Entry (37) in *PM* v, 211, should be corrected accordingly.

Pillow stuffings from Amarna?

Report on an analysis of nine samples of masses of fibres excavated at Amarna in the 1920s, which were examined by optical and scanning electron microscopy. Wool, flax, and cotton fibres were present and also fibres from a fur-bearing animal, feathers, and small slivers of wood. These masses were previously identified as the remains of pillow-stuffings. The presence of cotton in some samples indicates that they date at the earliest to the Roman period and may even be relatively modern. Recent gerbil activity at Amarna has been noted and these specimens therefore seem very likely to be the remains of gerbil nesting material, rather than connected with any human occupation at the site.

NINE distinct masses of fibres from excavations at Amarna in the 1920s were presented to Bolton Museum by the Egypt Exploration Society in 1929. They were described as 'samples of wool etc. found in the houses' and identified as remains of pillow stuffings or cushion fillings. They bear the museum registration numbers 28.29.10/1-9. The fibres had not been subjected to a detailed examination and it was felt that an analysis of samples taken from them would be both interesting and valuable. This was carried out by Elizabeth Lennox at the Postgraduate School of Studies in Textile Technology at the University of Bradford.

The masses of fibres were found in Eighteenth Dynasty houses at the city of Akhetaten and it is thus not surprising that they were assumed to be of that date. Not all have a specific provenance within the site. Samples 28.29.10/5, 6, and 7 have been displayed in the museum at some time and details relating to their discovery lost. It seems probable that they derive from the same area as the others, but this cannot be verified. 28.29.10/1-4, 8, and 9, stored in sealed boxes with their accompanying data,¹ came from three houses in the North Suburb. 28.29.10/1 and 2 were discovered in House U.36.53, given the excavation number 29/188, and noted as 'feathers and wool (cushion-filling?) and skeins of spun wool'.² U.36.53 was in the south-central quarter of the North Suburb, an area of very mixed housing and had been the central dwelling of an estate. Although it was apparently badly ransacked, a number of other items were found, including a faience ring bezel of Akhenaten, a fragment of faience tile, a gaming piece, rings, pendants, inlay, beads, and pottery.³ Samples 28.29.10/3, 8, and 9 came from U.35.23 in the Northern quarter of the North Suburb, an area probably inhabited by craftsmen. No details of the building are given and the only finds recorded are a ring, pendant, beads, and pottery.⁴ Sample 28.29.10/4 derives from T.35.8 in the north-western quarter of the North Suburb. This building comprised a few rooms which had probably been storage magazines. As with U.35.23, the finding of masses of fibres is not mentioned, and the objects recorded from T.35.8 are listed as fragments of a faience ringstand, fragments of a clay sealing, a painted clay spiral, rings, a wedjet eye amulet, pendants, beads, and pottery.⁵ The excavators noted that the north-western quarter, which was near the quay and basically a mercantile area, yielded a number of ring bezels bearing the name of Tutankhamun and was therefore possibly inhabited for slightly longer than some other parts of the site. Evidence indicated that this quarter was also occupied to some extent in Roman times.⁶

The nine samples were examined by both optical and scanning electron microscopy for the purpose of identifying the constituent fibres. The scanning electron microscope is the most suitable instrument for examining surface structure since it has a higher depth of field than other types of microscope. It also has a resolving power of the order of 10 μm . However,

¹ Details of provenance obviously came with the specimens and were duly noted by Thomas Midgley (curator at Bolton from 1906-34). Midgley's interest in Egyptian textiles was well known to Egyptologists at that time and he provided a number of reports on textiles for publication.

² H. Frankfort and J. D. S. Pendlebury, *The City of Akhenaten* II (London, 1933), 25.

³ *Ibid.* 95.

⁴ *Ibid.* 92.

⁵ *Ibid.* 40, 81.

⁶ *Ibid.* 37, 44.

it does not give information about the internal structure of intact fibres, which is best obtained from an optical microscope. Both animal and vegetable fibres were present and the results of the analysis are summarized in the following table:

Table of fibres found in the nine samples

Sample no.	Cotton	Flax	Wool	Animal	Feathers
28.29.10/1	—	×	×	—	—
28.29.10/2	×	×	×	—	×
28.29.10/3	—	—	×	—	—
28.29.10/4	—	—	×	—	—
28.29.10/5	×	—	×	—	×
28.29.10/6	×	—	×	×	×
28.29.10/7	—	×	×	×	—
28.29.10/8	×	×	×	×	—
28.29.10/9	—	×	×	—	—

Cotton: In four samples, cotton was clearly identified in the form of both yarns and also loose fibres. In some cases it had been dyed blue.

Flax: In five cases, fibres were found showing the transverse dislocations or nodes, which are characteristic of a bast fibre like flax.

Wool: In all the samples, fibres occurred which were animal in origin. Many had very poor surface characteristics but some showed scale patterns similar to wool.¹ These fibres varied from cream to brown with some possessing a central medulla. An animal fibre is made up of three regions, namely the cuticle or outer layer of overlapping scales, the cortical area of spindle-shaped cells and the medulla in the centre (not always present). The medulla is a cavity formed by cells which collapse during the growth process leaving pockets of gas.² It may comprise a continuous cavity running longitudinally along the axis of the fibre or small cavities separated from each other by areas of cortex. Most of the fibres in these samples did not have a medulla and those which did possessed continuous cavities. This is characteristic of a normal wool sample.³ Fibre diameter measurements were carried out on fifty specimens from 28.29.10/3 and from 28.29.10/4. Those from 28.29.10/3 were in the form of loose fibres and gave a mean diameter of 21.98 μm . These were mainly cream with a few brown, and the fibre distribution was skewed towards the fine diameter which one would expect of a wool sample of this date.⁴ The fibres from 28.29.10/4 were in the form of a yarn and the apparently much larger number of thicker fibres may be due to damage of the fine fibres during untwisting and dissection of the yarn. This sample gave a mean diameter of 27.37 μm .

Animal: In three of the samples a second animal fibre was detected. It had the outer layer of scales similar to all animal fibres, but the central medulla was laddered. This appeared as a series of evenly spaced dark patches, either longitudinal or multiserial, and is usually seen on fur-bearing animals.⁵ Under the electron microscope the scale structure was visible and when compared to a variety of scale structures was difficult to identify. All of these fibres were white, were few in number, and even in sample no. 6 (where they occurred most frequently) probably totalled less than one hundred fibres. They appeared to be intermittently mixed with the rest of the sample.

¹ H. M. Appleyard, *A Guide to the Identification of Animal Fibres*, 104-14, figs. 72-82.

² A. B. Wildman, *The Microscopy of Animal Textile Fibres*, 50.

³ J. G. Cook, *Handbook of Textile Fibres*, 1 (Watford, 1968), 98-9.

⁴ M. L. Ryder, 'Wools from Antiquity', *Textile History* 5 (1974), 100-10.

⁵ Wildman, op. cit. 51.

Feathers: In three of the samples there were feathers and tiny pieces of wood, which seemed to be waste material.

The results of the analysis¹ as shown in the table above proved interesting. It is a moot point whether these remains were indeed fillings for pillows or cushions. The presence of wool in all the samples was hardly surprising as it confirmed the excavators' original assessment of the fibres and was the material which one might expect to find. Flax, animal fibres, and feathers would also have been available for use in the late Eighteenth Dynasty. However, the discovery of cotton fibres in four of the samples was more problematical. Cotton was first used in Egypt on any significant scale in the Roman period and even then was not particularly common. In two of the four samples both cotton and flax were present. In its early use, and perhaps until the seventh century AD, cotton was no doubt often employed as a weft thread with linen warps, a fabric later called fustian, which continued to be widely woven in Europe until at least the eighteenth century AD. One might reasonably conclude that these samples could date to the Roman period, when there was some occupation at Amarna,² or possibly later.

However, a distinct note of caution must be sounded as this story could have a veritable sting in the tail!³ The site of Amarna in the 1980s is infested with gerbils, who chew substances like wool, flax, feathers and cotton in order to make their nests. The end result bears a very close resemblance to our samples and might explain the fine wool diameter associated with ancient wool samples, the presence of fibres from a fur-bearing animal and the tiny pieces of waste like wood. It therefore seems that these so-called 'pillow stuffings' may not be connected with any human occupation at Amarna, but are likely to represent the remains of gerbil activity at the site, either as early as the Roman Period or as late as the 1920s.

ANGELA P. THOMAS

The Vienna stela of Meryre

The fragmentary stela of Meryre, presently in Vienna (no. 5814), is one of very few objects known from this Eighteenth Dynasty individual. This article presents two partial sketches of the stela found in a collection of the sketches and rubbings of the nineteenth-century traveller Paul Durand which is presently in Montreal. One allows us partially to reconstruct the missing fragment of the stela, and records of dates in the collection permit some educated guesses about the early modern history of the stela.

VIENNA stela no. 5814,⁴ probably dating to the reign of Amenhotep III, has long been of interest due to its representation of an otherwise unknown Eighteenth Dynasty prince, Sa-atum,⁵ not to be confused with an earlier prince of the same name who was possibly the father of a wife of Thutmose III.⁶ The owner of the stela is one Meryre, who is known from

¹ I gratefully acknowledge my debt to Elizabeth Lennox on whose report I have drawn for details of the examination method and the results of the analysis.

² *The City of Akhenaten* II, 66-7.

³ I express my sincere thanks to Gillian Vogelsang-Eastwood for information on the recent gerbil activity at Amarna and for her information and opinion of similar samples from the 1928/9 season which she examined at the Science Museum in London.

⁴ PM III², 706.

⁵ H. Brunner, *ZÄS* 86 (1961), 96, n. 1; H. Satzinger, *Echnaton, Nofretete, Tutanchamun. Ausstellung-Wien. 23 April-29 Juin 1975* (Vienna, 1975), 141; R. Drenkhahn, *LÄ* IV, 1106, n. 5. See, too, N. de G. Davies, *The Tombs of Menkheperasonb, Amenmose and Another* (London, 1933), 39-40, and W. Helck, *Zur Verwaltung des Mittleren und Neuen Reiches* (Leiden and Cologne, 1958), 353.

⁶ W. M. F. Petrie, *History of Egypt* (London, 1896), II, 144, fig. 90, and H. Gauthier, *LR* II, 273-4. Note that Sa-atum also occurs as the Horus name of Thutmose IV, *ibid.* 295, xix, B.

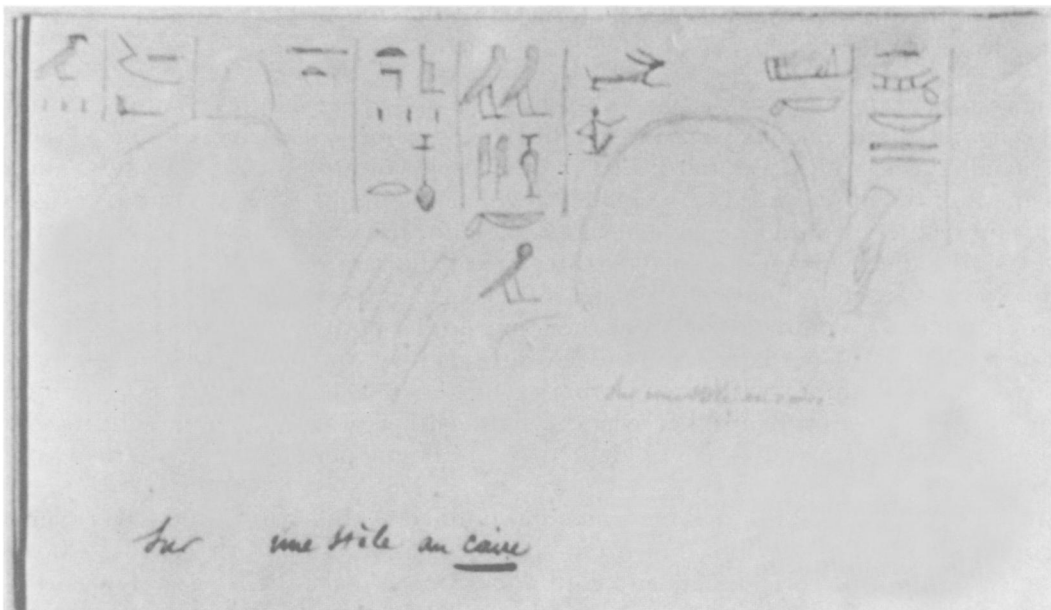


FIG. 1

Courtesy Art 45, Montreal

another block in Vienna, no. 5815.¹ Meryre has been identified with the unnamed owner of Theban Tomb 226,² but this has not been universally accepted³ and the style of the stela points to a Memphite origin.⁴ A fragmentary door-jamb of a Meryre was noted by Gunn at Saqqâra,⁵ and A.-P. Zivie's recent discovery of the actual tomb of Meryre at Saqqâra effectively puts an end to the discussion.⁶

It was recently brought to my attention by Dr N. B. Millet that notes, sketches, and rubbings of Paul Durand⁷ pertaining to Egypt had turned up at an art gallery in Montreal. The owner, Mr S. Viseman of Art 45, kindly allowed me to study the material, which I found to contain two drawings which are relevant to Vienna 5814.

The first, labelled 'Sur une stèle au Caire', merely shows the outlines of the heads and upraised hands of a man and a woman with eight short vertical columns of text overhead (fig. 1). The text and figures are identical to those of the first register of Vienna 5814 (pl. XV, 2). The second (fig. 2), with an identical label, is clearly a copy of the band of inscription running along the top and sides of Vienna 5814. However, as can be seen from pl. XV, 2 and fig. 2, this second sketch shows us that Durand saw the stela when it was still more or less intact, for Vienna 5814 is now missing the right side of the top and all of the right vertical inscription.

As the entry on Durand in *Who was Who in Egyptology* is inaccurate, confusing three people of the same name,⁸ the documents in Montreal help to determine the dates of Paul Durand's journeys in Egypt, providing us with the following grouping of dates: 1842-3;

¹ Satzinger, *op. cit.* 142-3.

² Davies, *op. cit.* 37. See too Helck, *op. cit.* 353-4, 470.

³ Satzinger, *op. cit.* 141-2 and PM III², 706.

⁴ Compare: CG 34049, 34050, 34054, and 34055.

⁵ *Gunn Notebook* 7, no. 49. I would like to thank Dr J. Malek of the Griffith Institute for a photocopy of this item.

⁶ A.-P. Zivie, *BSFE* 84 (1979), 29-30; *BSFE* 98 (1983), 50-1 and pls. i-ii; *Mélanges Adolphe Gutbub* (Montpellier, 1984), 245; *ASAE* 70 (1984-5), 228.

⁷ W. R. Dawson and E. P. Uphill, *Who was Who in Egyptology*, 2nd edn. (London, 1972), 93.

⁸ M. Dewachter, *BiOr* 37 (1980), 304.

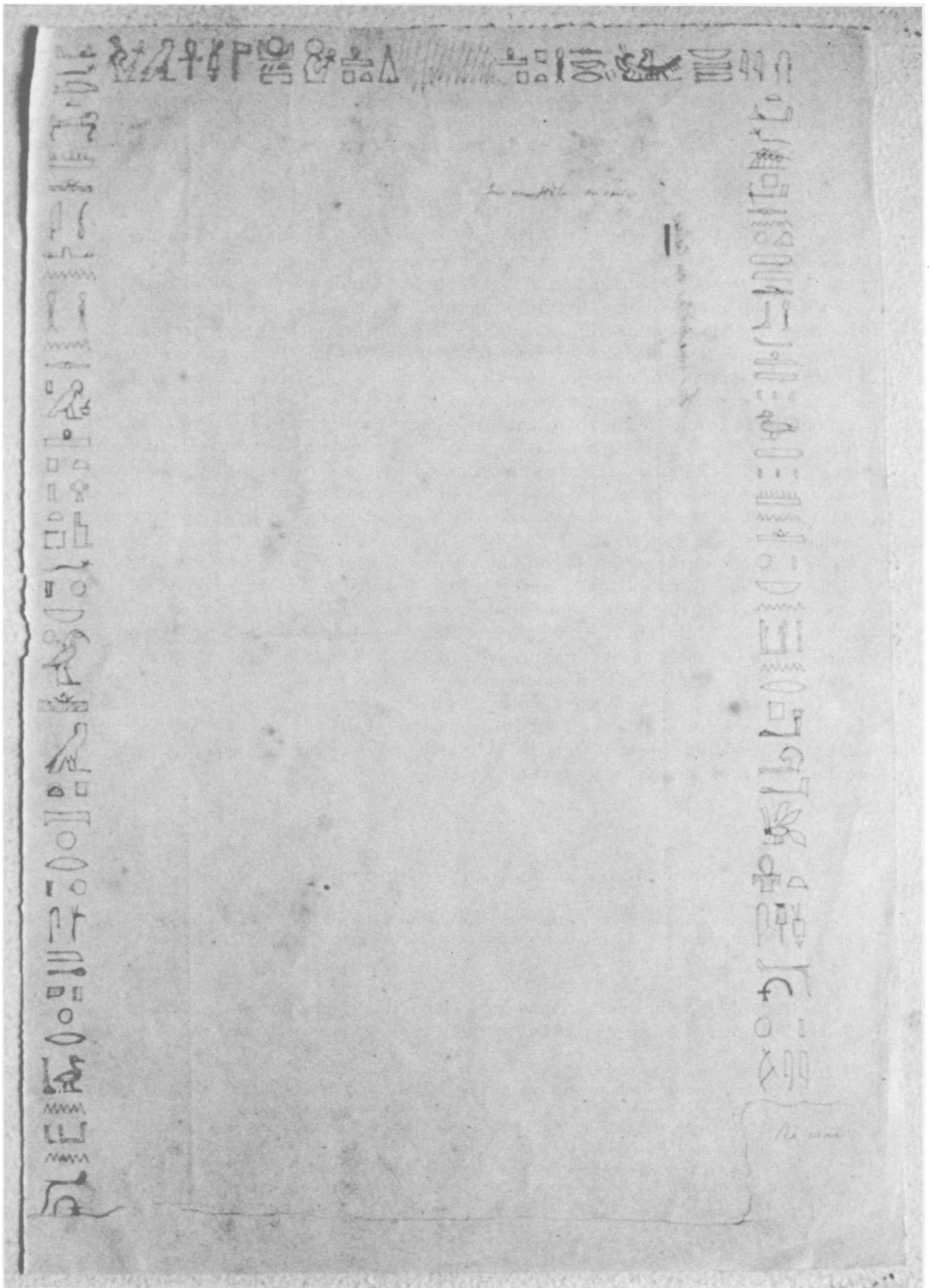


FIG. 2

Courtesy Art 45, Montreal



1. Block from north wing of Second Pylon, Karnak

EIN KÄFIG FÜR EINEN GEFANGENEN (p. 206)



2. Vienna AS 5814

Courtesy Kunsthistorisches Museum, Vienna

THE VIENNA STELA OF MERYRE (pp. 213-16)



3. The walking-stick of Meh. N.1440. *Courtesy the Oriental Museum, University of Durham*

TWO WALKING-STICKS (p. 218)

1845; 1848; 1864–6. The earliest date noted was 31 December 1842, the latest 19 March 1866. Most of the dated documents only provide the year; the majority are undated. It is known from another source that Durand was in Egypt from 12 December 1844 until late in February 1845.¹ The few documents in Montreal that indicate both date and locale are entirely consistent with this, and appear to be the only known evidence for Durand's presence in Egypt in 1848, 1864, 1865, and 1866.²

Since Durand did not date his sketches of the stela, all we can state is that the stela was probably still intact (or the fragments kept together) when Durand sketched it sometime between 31 December 1842 and 19 March 1866. Bergmann, in 1887, stated that Vienna 5814 and 5815 had been obtained by the museum in Vienna 'Aus der Sammlung Miramar'.³ However, a catalogue of the Miramar collection published in 1865 does not mention them.⁴ A large part of this collection was presented in 1855 by Said Pasha to the then Archduke Ferdinand Maximilian, who purchased other objects in Egypt as well. Ferdinand Maximilian acquired many more antiquities in 1865–6; these do not appear in the 1865 catalogue.⁵ It would seem, then, that Vienna 5814 and 5815 were purchased for the Miramar collection in 1865–6 and it is probable that Durand saw the stela in Cairo at this time, although, of course, an earlier date for his sketches is still possible as we do not know how long the stela was accessible in Cairo prior to its purchase for the Miramar collection. It is of interest to note that Durand was taken by Ampère, during their 1844–5 voyage together, to visit the private collections of Clot-Bey, Abbot, Rousset, and others.⁶

The lost part of the perimeter text of Vienna 5814, now supplied by Durand's sketch, reads: 'A boon [which the king gives] (to) Ptah-Sokar, Lord of the Shetjeyet, that he may cause the receiving of offerings consisting of funerary meals and provisions, enduring every day, for the Ka of the Hereditary Noble, Count, Seal-bearer of the King of Lower Egypt, Sole Friend, Overseer of the Treasury Meryre [. . .].' The titles Seal-bearer of the King of Lower Egypt and Sole Friend do not occur on the extant fragment of Vienna 5814 or 5815 or the fragmentary door-jamb sketched by Gunn. They are, however, attested in Meryre's tomb.⁷ The Durand sketch completes the left hand *hṯp di nsw* formula with the now missing *di*. Finally, it would appear that the title Overseer of the Treasury was then clearly apparent at the bottom of the perimeter text on the left.

DAVID BERG

Two walking-sticks with texts mentioning Memphis

Publication of two walking-sticks of the New Kingdom, probably from Saqqâra, and belonging to the lector-priest Neferhotep and the overseer of oil-makers Meh respectively.

THE 'third foot' of the 'four-footed, three-footed, and two-footed' was a walking-stick, and the riddle could have been asked by an Egyptian rather than a Boeotian sphinx, so typical of ancient Egypt were wooden sticks and staves. In addition to their many practical uses, sticks

¹ J.-J. Ampère, *Voyage en Égypte et en Nubie* (Paris, 1846–9), 2 and *passim*.

² M. Dewachter, personal communication, 16/1/86. It is possible that Durand was in Egypt in 1867 as well. G. Goyon, *Les Inscriptions et graffiti des voyageurs sur la grande pyramide* (Cairo, 1942), No. S. 83, pl. xlix records 'Durand 1867', but there is no evidence that this individual was the same as our Paul Durand.

³ E. von Bergmann, *RT* 9 (1887), 46–7.

⁴ S. L. Reinisch, *Die aegyptische Denkmaeler in Miramar* (Vienna, 1865).

⁵ Much of the information on the Miramar collection was kindly provided for me by Dr H. Satzinger of the Kunsthistorisches Museum, Vienna.

⁶ Ampère, *op. cit.* 567 ff.

⁷ A.-P. Zivie, personal communication, 23/1/86.

denoted old age, rank, and dignity, and were often shown in the hands of officials. They accompanied a man to the grave, and formed an important and ideologically well-justified part of his funerary equipment.

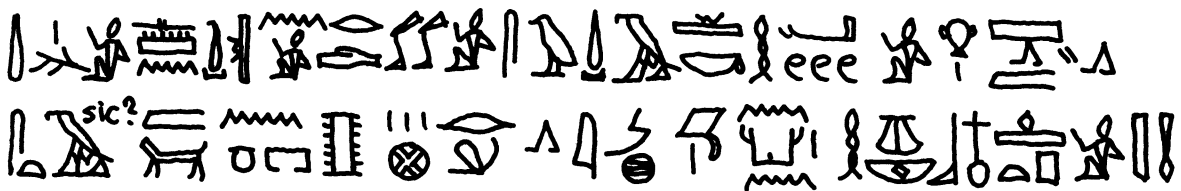
Several inscribed sticks or their parts are known to have come from Saqqâra tombs of the New Kingdom: those of Hatiay, temp. Amenophis III or IV (Leiden I.86, PM III², 775), Patenemhab, end of the Eighteenth Dynasty (Leiden AH.140C, PM III², 711), Serbykhen Iby, Ramesside (formerly Berlin 1284, PM III², 717), and an alabaster knob from a stick of Ptahmose, temp. Amenophis III (BM 54989, PM III², 713). An ornamental wooden pommel in the shape of a papyrus umbel from a stick of Ptahemhet Ty, temp. Tutankhamun or Ay, is in Leiden (I.89, PM III², 712), but the possibility that this is a knob of a box or a furniture-item should not be discounted. These objects suggest that the burial-chambers in which they were deposited have been plundered, either in ancient times or in the first half of the nineteenth century when Saqqâra witnessed intensive 'exploration' by traders in antiquities and agents acting on behalf of important collectors.

The provenance of the two sticks which are the subject of this communication is not absolutely certain, but their inscriptions mention Memphis. Both probably date to Saqqâra's peak period between the mid Eighteenth and the mid Twentieth Dynasty. Furthermore, three sticks with texts referring to Memphis, and thus perhaps also from the same area, have been listed by A. Hassan (*Stöcke und Stäbe im pharaonischen Ägypten* (Munich, 1976), 140-1 [16], 143 [21], 145-6 [25]), and yet another mentions 'One who is south of his wall', i.e. Ptah of Memphis (idem, *ibid.* 154-5 [42]).

The stick of Neferhotep

The text 'on a broken stick belonging to Mr. Leader found at Athribis' was copied by Sir John Gardner Wilkinson sometime during the first half of the nineteenth century, MSS ix.182 [top]. It is quoted here from the Gardner Wilkinson papers from Calke Abbey, Bodleian Library, Oxford, by kind permission of the National Trust. 'Mr. Leader' is almost certainly the same as Revd Johann Rudolph Theophilus Lieder, a German Protestant missionary who assembled a collection of antiquities between his arrival in Egypt in 1826 and his death in 1865. Many pieces from Lieder's collection were bought by Mr (later Lord) Amherst, but I do not know the present location of the object recorded by Wilkinson.

The text contains an address to the stick by its owner, and is presented here in my own rendering of Wilkinson's copy. The original was, no doubt, in a vertical column.



mdw-i smn n-i rdwy-i swd-k ḥꜣw-i ḥꜣ šm swt<wt> m-hnw 'Inbw r ph<i> imšh n kꜣ n hry-ḥtp Nfr-ḥtp mꜣꜣ-hrw

'(O) my stick, support my legs for me! May you keep my limbs safe on (my) walks and travels in Inbu, so that I may reach the venerated state. For the *ka* of the lector-priest Neferhotep, justified.'

It seems that Wilkinson miscopied *w* of *swtw* as *ꜣ*. The text is interesting because it apparently suggests that the stick was already used in the lifetime of its owner, and thus, was



1. Block from north wing of Second Pylon, Karnak

EIN KÄFIG FÜR EINEN GEFANGENEN (p. 206)



2. Vienna AS 5814

Courtesy Kunsthistorisches Museum, Vienna

THE VIENNA STELA OF MERYRE (pp. 213-16)



3. The walking-stick of Meh. N.1440. *Courtesy the Oriental Museum, University of Durham*

TWO WALKING-STICKS (p. 218)



not an item specially made for the tomb. 'Inbw, 'The Walls', was one of the several equivalents of *Mn-nfr*, 'Memphis', during the New Kingdom. In view of this, the alleged provenance, Athribis (Tell Atrîb in the central southern Delta), seems unlikely.

The stick of Meh (pl. XV, 3)

This object originally belonged to the collection of Algernon, Lord Prudhoe (later the fourth Duke of Northumberland) at Alnwick Castle, and is now in the Oriental Museum, University of Durham (N.1440). I am grateful to the Keeper, Mr John Ruffle, for his kindness when I studied it and for permission to publish it. S. Birch (*Catalogue of the Collection of Egyptian Antiquities at Alnwick Castle* (London, 1880), 187) and A. Hassan (op. cit. 155-6) have given the basic information concerning this stick, but my interpretation of the inscription differs significantly. The text given here is based on a photograph and my hand-copy; thus it is not a true facsimile (the stick is part of the permanent display and it was not convenient to remove it).

mdw iswt n hry ps sgnn Mh nb iswt nfrt m Mn-nfr nb qrst hr Pth-Skr

'The staff of old age of the overseer of oil-makers Meh, possessor of good old age in Memphis, (and) possessor of burial from Ptah-Sokar.'

The text contains an error committed by the engraver: Δ was carved instead of \triangle . Some of the signs are palaeographically interesting:

N1	O24	Q6	S43
V30	W11	Y1	Aa1

The basic meaning of *sgnn* is 'oil' (W. Helck, *Materialien*, III, 504-5, and J. J. Janssen, *Commodity Prices* (Leiden, 1975), 336-7), but it can be widely applied, from lamp-oil to cosmetic ointments. The title *ps sgnn* quite literally means 'cook of oil' (discussion, U. Verhoeven, *Grillen, Kochen, Backen* (Brussels, 1984), 99), and Meh seems to have been an overseer in charge of a Memphis workshop where oils were prepared.

JAROMIR MALEK

A West Semitic title on an Egyptian stela in Rio de Janeiro

A preliminary publication is given of the stela Cat. 2442 of the National Museum at Rio de Janeiro, which features a couple, Raia and Maia, of about the Nineteenth Dynasty, worshipping Osiris with a short hymn. Raia's only title is *q-d-n* (in group-writing), otherwise unknown in Egyptian (and different from *kdn/ktn*, 'charioteer'). This word is probably a loan from West Semitic *qsn*, 'commander/leader/boss'; perhaps Raia had been so termed by foreigners in his charge.

AMONG the Egyptian stelae in the National Museum at Rio de Janeiro is a small grey limestone votive stela of typical Nineteenth Dynasty type (cf. fig. 1).¹ It has the catalogue number 2442 in the summary guidebook by A. Childe.² In the upper of two registers, the twin Horus-eyes flank the *shen*-sign over 'water' and a pot. A conventional offering-stand at the right is for the benefit of 'Osiris, Lord of the Sacred Territory', enthroned at the left, facing right.

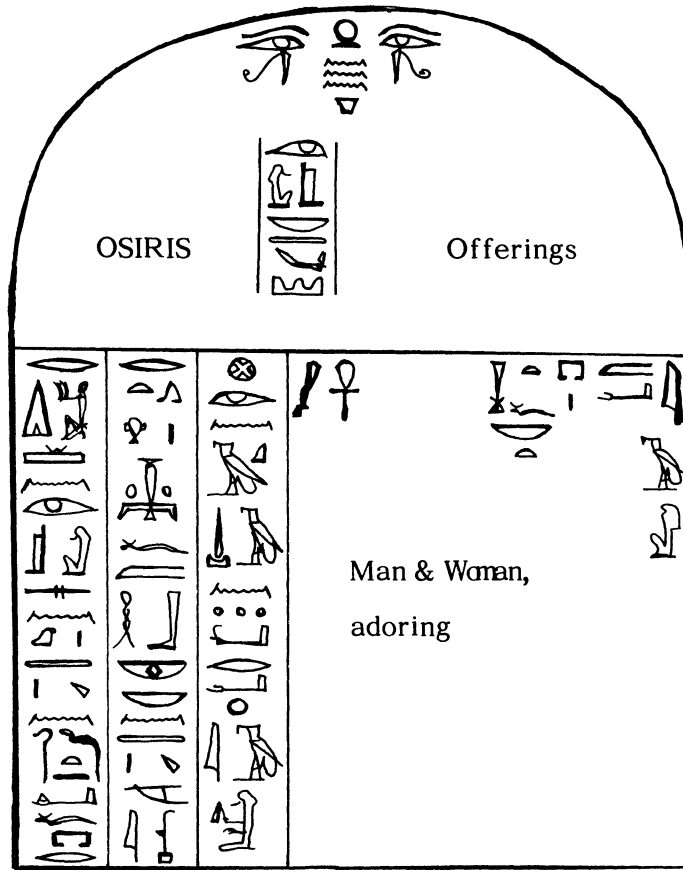


FIG. 1

In the lower register, a man and woman in ordinary dress stand and adore the deity above. The texts flanking the couple read: 'Giving praise to Osiris, paying homage to the Ruler of Everlasting, that he may give what appears on his altar at every festival in Egypt (*Tameri*);

¹ It is with the greatest pleasure that I thank the Director of the Museu Nacional do Rio de Janeiro for the initial permission to study the Egyptian objects in that Museum; most especially, I thank Professor Maria de C. Beltrão (Setor de Arqueologia) and her colleagues for their most kind assistance in every way; and my thanks also to Messrs Ricardo and Roberto Costa Souza and Gilberto dos Santos Valladão, for their friendly and practical help. Fig. 1 is based on my hand-copy of September 1985.

² Alberto Childe, *Guia das Collecções de Archeologia Classica* (Rio de Janeiro, 1919), 43; it is hoped to provide a fuller publication of this stela along with the rest of the Egyptian collection in Beltrão & Kitchen, *Catalogo dos Monumentos do Egito Antigo, Museu Nacional do Rio de Janeiro* [forthcoming].

made by the leader/commander Raia,¹ renewing life, (and) his sister, the lady of the house, Maia'.²

Leaving aside the picturesque phrase about 'every festival in Egypt', the one remarkable feature is Raia's title, *q-d-n*, which is written in the so-called syllabic orthography and is certainly not an Egyptian word. It is absent from the *Wörterbuch*,³ but its origin is not far to seek. The Egyptian consonants correspond most closely to the Semitic root *qšn*.⁴ This is first attested in the language of the ancient North Syrian seaport of Ugarit in the fourteenth–thirteenth centuries BC, in the proper names *Qšn* and *Bn-qšn*, belonging to personnel in administrative lists.⁵ These are of the same general date as our Egyptian example, but add nothing to our understanding of the word. However, the term recurs in Biblical Hebrew (in the form *qāšîn*) as a common noun, in two related acceptations⁶—that of commander of soldiery (Joshua 10: 24; Judges 11: 6, 11; cf. Daniel 11: 18), and that of 'leader' in general, including the political leaders of a community or nation (Isaiah 1: 10; 3: 6, 7; 22: 3; Micah 3: 1, 9; cf. Proverbs 6: 7; 25: 15). Therefore, this Egyptian example may also be rendered as 'leader/commander'; it has the same striking-arm determinative as Egyptian *ṯsw*, 'commander', and similar words.

The role of Raia remains obscure. He and his wife have very common, unremarkable Egyptian names, and wear ordinary dress (no military kilt, for example). The rare term *qšn/qđn* plays no part in the Egyptian military or civil hierarchies, and it is Raia's only title. Hence, it seems unlikely that Raia was a commander in the Egyptian armed forces, and improbable that his title implied any very high standing. He is certainly no 'national' leader in Egypt.

Perhaps Raia was an overseer or foreman of a group or gangs of foreign workmen of West Semitic origin serving in Egypt, who, having no pretentious Egyptian title by which he might dignify himself, adopted their appellation for him, *qašînu*, 'leader', 'the boss', as at least being a distinctive title to place upon his humble votive stela. This would suit well the cosmopolitan atmosphere of the Egypt of the Nineteenth Dynasty.

K. A. KITCHEN

A statue of Prehirwenmef, son of Ramesses II

Publication of an incomplete, unprovenanced, quartzite standing statue of Prehirwenmef, third son of Ramesses II, in the Burrell Collection, which supplements the numerous reliefs in which the prince is depicted, on temple walls at Thebes and in Nubia. The texts include two titles not otherwise recorded for him: *ḥꜣt mšꜣ n nsw* (possibly an early version of *ḥꜣwtꜣ n mšꜣ*) and *tp(y) n qnyw*.

THERE is a statue of Prehirwenmef, the third son of Ramesses II, in the Burrell Collection, Glasgow Museums and Art Galleries, register number 13.83. It was acquired by Sir William

¹ This name is very common, cf. Ranke, *PN* 1, 220: 7.

² This name is equally common, cf. *ibid.* 1, 146: 1.

³ It should not be confused with *kṯn/kđn*, 'charioteer', with *k* instead of *q*, and entirely different determinative(s). The supposed spelling of *kṯn/kđn* with a *q* (*Wb.* v, 148) finds no support in the published *Belegstellen* or in any example known to me.

⁴ On *q/q* and *d/š*, see M. Burchardt, *Die altkanaanäischen Fremdworte und Eigennamen im Aegyptischen*, 1 (Leipzig, 1909), 39, § 117, and 49, § 153. The only alternative would be *q/g*, *d/z*, and *n/l*, giving *gozel*, 'thief'—hardly a title that anyone would boast of, on even the humblest stela!

⁵ References, see C. H. Gordon, *Ugaritic Textbook* (Rome, 1965), Part III, 479, No. 2257; R. E. Whitaker, *A Concordance of the Ugaritic Literature* (Cambridge, Mass., 1972), 553: *qšn*; M. Dietrich, O. Loretz, J. Sanmartin, *Die keilalphabetischen Texte aus Ugarit* (Neukirchen-Vluyn, 1976), 190, no. 4.63: III, 6; 218, no. 4.122: 4; 243, no. 4.204: 10; 270, no. 4.285: 10; 360, no. 4.617: 21.

⁶ References in F. Brown, S. R. Driver, C. A. Briggs, *A Hebrew and English Lexicon of the Old Testament* (Oxford, 1952), 892b under 11 *qšh*.

Burrell on 2 March 1948 from Spink and Son. Previously it had probably been in the collection of the Vicomte de Harcourt.¹ Its provenance is not known. The statue (pl. XV, 1, 2) is of brown quartzite and its surviving dimensions are: height 68.8 cm, width 22.7 cm, and depth 33 cm. It represents a standing figure with arms at the sides and left leg advanced, dressed in a shirt and long kilt, both of which show pleating, with a flared apron down the front of the dress. The head, left shoulder and arm, feet, and base are missing. A rectangular back support, the top and bottom of which are broken off, is set at an angle of 15° to the front plane of the figure. Details of the costume such as the retaining tab of the kilt, the looped edging, regular pleating, and one preserved tie of the draw string at the shirt neck, are rendered carefully. The right hand grasps a piece of cloth(?); of the left only a ridge remains on the side of the body to show its position.

Incised inscriptions appear on the centre panel of the apron, on the right side of the skirt of the kilt, on the left side behind the leg, and on the side and back of the dorsal support (pls. XVI-XVII and fig. 1).

Front: *sꜣ nsw kꜣn tp(y) n hm:f Pꜣ-Rꜥ-hr-wnm:f*, King's son, chief charioteer of his majesty, Prehirwenmef.

Right Side: *nb tꜣwy Wsr-mꜣꜥt-Rꜥ stꜣ-n-Rꜥ*, Lord of the Two Lands, 'Usimare-setepenre'; *nb hꜥw Rꜥ-mss(w) mry 'Imn*, Lord of the Crowns, 'Ramesses Meryamun'.

Left Side: (1) *r-pꜣt hꜣt mꜣꜥ n nsw tp(y) n qnyw sꜣ nsw n ht:f mr[f]* (2) *kꜣn tp(y) n hm:f Pꜣ-Rꜥ-hr-wnm[f]* . . ., (1) The prince, leader of the army of the king,^a chief of the 'Braves', the king's son of his body, whom [he] loves, (2) chief charioteer of his majesty, Prehirwenm[ef] . . .

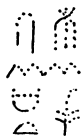
Rear: [*sꜣ nsw n ht:f mr:f*] [*mꜣs n [hm]t nsw wr[t] sꜣ nsw imy-r ssmꜣ Pꜣ-Rꜥ-hr-wnm[f]*] . . ., [King's son of his body whom he loves],^b born of the king's great [wife],^c royal scribe, master of the horse, Prehirwenm[ef].

Commentary

(a) *hꜣt mꜣꜥ n nsw*, 'Leader of the Army of the King'. In a personal communication Dr K. A. Kitchen suggests *hꜣt* may be an early spelling of *hꜣ(w)t(y)*, 'Leader', or, alternatively, might be understood as (*r*)-*hꜣt* 'in front of/at head of' with omission of vocalic *r* (reduced to 'e' as in later Coptic). For this and other valuable comments, I am much indebted to Dr Kitchen.

(b) restore: [*sꜣ nsw n ht:f mr:f*], as *left side*, line (1).

(c) the traces in fig. 1 suggest:



Some of the titles borne by Prehirwenmef on the statue also occur on other monuments where he is represented. A list of such titles drawn from the available sources emphasizes the military nature of Prehirwenmef's career.

¹ A 'Vicomte d'Hardecourt' had been credited in Sir William Burrell's Purchase Book for 1948 as the one-time owner of the statue. Search by Dr M. L. Bierbrier, to whom grateful thanks are due, failed to locate a reference to such a person. Dr Bierbrier was, however, able to supply a likely candidate, namely Emmanuel Vicomte de Harcourt. To quote from his communication of 30 May 1984: '... he was born on 23rd June 1844, was still alive in 1923, but apparently dead by 1933. He was the son of Georges Marquis de Harcourt. He married on 15th October 1887 Baroness Iphigénie Sina (died 1914) widow of the Duc de Castries. She was an heiress, the daughter of an extremely wealthy Greek banker and if her husband is the collector, he certainly had the means to do so.'

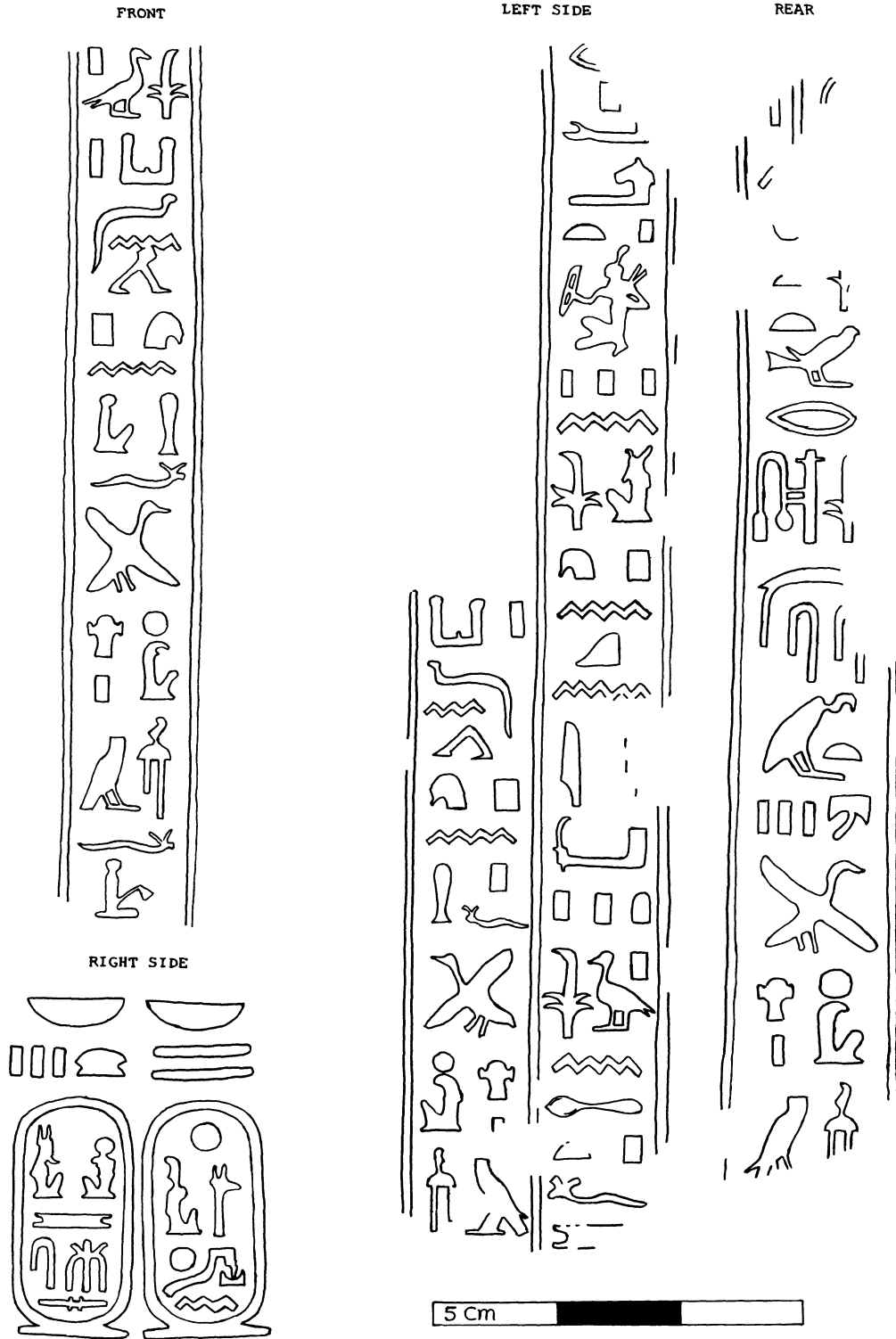


FIG. 1

<i>Title</i>	<i>Number of monuments on which attested</i>
1. <i>sꜣ nsw</i> , King's son	13
2. <i>kḏn</i> (var. <i>kṯn</i>) <i>tp(y) n ḥm:f</i> , Chief charioteer of his majesty	11
3. <i>ḥꜣt mšꜥ n nsw</i> , Leader of the army of the king	1
4. <i>tp(y) n qnyw</i> , Chief of the 'Braves'	1
5. <i>r-pꜣt</i> , Prince	2
6. <i>sš nsw</i> , Royal scribe (var. <i>sš nsw mšꜥ</i> , Real royal scribe)	6
7. <i>imy-r ssmt</i> , Master of horse (var. <i>imy-r ssmt n nb tꜣwy</i> , Master of horse of the Lord of the Two Lands)	11
8. <i>ḥry pḏt</i> , Commander of troops	3
9. <i>tꜣy ḥw ḥr wnmy n nsw</i> , Fan bearer on the right side of the king	4
10. <i>tꜣ pḏt</i> , Troop commander	1
11. <i>idnw tpy n pꜣ mšꜥ</i> , Chief Lieutenant Commander of the army (var. <i>idnw n pꜣ mšꜥ</i> , Lieutenant Commander of the army)	5
12. <i>imy-r mšꜥ wr</i> , Great overseer of the army	1

Monuments

Statue (Burrell Collection 13.83) (titles 1-7)	
Ramesseum, Hypostyle, W. wall, N. half (1, 2, 6, 7, 9, 11)	KRI II, 862.1.8
Ramesseum, 2nd Court, W. wall, S. half (1, 2, 6, 7, 9, 11)	KRI II, 862.2.9
Ramesseum, Hypostyle, W. wall, S. half (2, 6, 9, 12)	KRI II, 130.15
Ramesseum, 1st Court, palace façade (1, 2, 7)	KRI II, 871.4
Abu Simbel, 1st Hall, E. wall, S. of door (1, 2, 6, 7, 11)	KRI II, 862.4.11
Abu Simbel, 1st Hall, S. wall, extreme E. side (1)	KRI II, 206.11
Abu Simbel, lesser, Speos Hathor, façade (1)	KRI II, 766.4.11
Derr, 1st Hall, rear wall, left side (1, 2, 7, 11)	KRI II, 862.5.12
Wadi es Sebua, inner court, N. side (1, 2, 7, 9, 11)	KRI II, 862.6.13
Luxor, forecourt, interior, S. wall, W. half (1, 7, 8)	KRI II, 609.4.5
Luxor, forecourt, exterior, E. wall (1, 2, 6, 7)	KRI II, 183.1
Luxor, forecourt, interior, W. wall, N. half (2, 7)	KRI II, 862.3
Luxor, representation of family of Ramesses II (2)	Daressy, RT 14 (1893), 32
Scarab-topped plaque (MacGregor Collection) (1, 8)	KRI II, 871.8
Scarab (Fraser, <i>Catalogue, Scarabs, Fraser No. 338</i>) (1, 5, 7)	KRI II, 871.9
Karnak, statue base (8, 10)	KRI II, 871.6

The titles in the inscriptions relate to the prince on his own or in company with some of his brothers. The most frequently occurring, except for *sꜣ nsw*, are *kḏn* (var. *kṯn*) *tp(y) n ḥm:f* and *imy-r ssmt*. Two titles present on the Burrell statue are not in the other representations. The first of these is *ḥꜣt mšꜥ n nsw*.¹ The other is *tp(y) n qnyw*.² It is apparent that the prince held a command in the army, *imy-r ssmt*, referring more specifically to the chariotry. In contrast, *kḏn tp(y) n ḥm:f* was honorific. There is no hint of Prehirwenmef's military rank other than that which would be accorded to a royal prince with an obvious inclination to pursue the career of a soldier—a true son of his father Ramesses II.

¹ For *ḥꜣt mšꜥ n nsw* in the New Kingdom, see A. R. Schulman, *Military Rank, Title and Organisation in the Egyptian New Kingdom* (Berlin, 1964), 49, who gives examples and suggests that it was not a rank but an epithet, since it appears neither in any list of ranks nor in protocols of individuals.

² Schulman, op. cit. 67, contra R. O. Faulkner, *JEA* 39 (1953), 44 n. 10, considers there is insufficient evidence for the assumption that there existed a separate corps of the army—the 'Braves'. Dr Kitchen, letter of 23 June 1986, suggests that the title may reflect the prince's role at Kadesh.



1. Front



2. Back

Burrell Collection 13.83

A STATUE OF PREHIRWENMEF (pp. 220-4)



1. Right side



2. Left side

Burrell Collection 13.83

A STATUE OF PREHIRWENMEF (pp. 220-4)

That Prehirwenmef did experience active service in the field is demonstrated by his presence in scenes on the walls of a number of temples:

(a) Abu Simbel, 1st Hall, south wall, east side—attack on an unnamed Syrian fort.¹

(b) Abu Simbel, 1st Hall, east wall, south of door—an unspecified Nubian war.² Spalinger,³ commenting on these depictions, sees no reason to consider the Asiatic, Libyan, or Nubian wars as being anything other than records of actual campaigns which occurred within the first five years of Ramesses II as sole king. At the same time, he does not connect the Asiatic scene on the south wall with the Hittite war on the north wall as Wreszinski⁴ had proposed.

(c) Beit el Wali, south wall—reception of Nubian tribute.⁵

(d) Ramesseum, Hypostyle, west wall, south half—the ‘fan bearer’ Prehirwenmef, probably the prince, chases off in a chariot at Kadesh.⁶

(e) Karnak, Great Hall, exterior, south-east angle—presentation of prisoners after Kadesh.⁷

(f) Ramesseum, Hypostyle, east wall, south half—year 8, northern campaign, attacks on Dapur and Tunip.⁸

(g) Luxor, forecourt, exterior, east wall—probably after year 10, expedition in Transjordan.⁹

Perhaps the prince also participated in the actions in Edom and Moab in the period covered by years 11–20. However, by the latter year, 1260 BC, the prince had died aged about 26 or so, having been born to queen Nefertari¹⁰ as her second(?) child about 1286 BC. The spelling of the king’s nomen *Rc-ms-s(w)* may indicate that the statue was carved prior to year 21, if it was produced in some workshop in Thebes or further south.¹¹

JAMES K. THOMSON

The Takhats and some other royal ladies of the Ramesside period

This paper discusses the relationships of the royal ladies Takhat who appear on the monuments. It distinguishes two, A, wife of Sethos II and mother of Amenmesse, and B, wife of Prince Montjuhirkopshef B, son of Ramesses III, and mother of Ramesses IX, buried in KV 10, the old tomb of Amenmesse. The other lady depicted in that tomb is identified as the wife of Ramesses IX. In addition, Grist’s identification of Tyti (QV 52) as a daughter and wife of Ramesses III (JEA 71 (1985), 71–81) is opposed and Amenmesse’s paternity of Siptah supported.

A CONSIDERATION of the number and relationships of the royal ladies bearing the name Takhat has considerable bearing on the relationships of certain later Ramesside rulers, but most discussions seem to incorporate assumptions which are not sustainable. The monuments naming a royal Takhat are as follows:

I. Paintings in the well-room of the tomb of King Amenmesse (KV 10)¹² depict the *mwt-ntr mwt-nsu wrt (Tj-hrt)* offering to, and in the presence of, various deities.

¹ KRI II, 206.6–11 = PM VII, 102–3 (39)–(40) = W. Wreszinski, *Atlas* II, pl. 183 (F53 Centre de Documentation).

² A. J. Spalinger, JEA 66 (1980), 86 = Wreszinski, *Atlas* II, pl. 184 (F1–6 Centre de Documentation).

³ Spalinger, op. cit. 86–7.

⁴ Wreszinski, op. cit. II, pl. 183.

⁵ Spalinger, op. cit. 89.

⁶ KRI II, 130.15.

⁷ KRI II, 144–5.

⁸ Spalinger, op. cit. 93; F. Gomaa, *Chaemwese, Sohn Ramses’ II und Hoherpriester von Memphis* (Wiesbaden, 1973), 5 and n. 5.

⁹ Kitchen, JEA 50 (1964), 61, fig. 5 Fv1 = KRI II, 183.1.

¹⁰ Kitchen, *Pharaoh Triumphant: the life and times of Ramesses II* (Warminster, 1982), 40, 102.

¹¹ Kitchen, in *Ägypten und Kusch*, eds. E. Endesfelder et al. (Berlin, 1977), 220–1; Spalinger, op. cit. 95–8.

¹² PM I², 517–18; E. Thomas, *Royal Necropoleis of Thebes* (Princeton, 1966), 110–11.

2. A relief on a statue usurped by Sethos II from Amenmesse, at Karnak,¹ is labelled as depicting the *hmt-* (written over erased *mwt-*) *nsw* (*T3-hct*).
3. A small figure carved at the side of a statue ultimately usurped by Sethos II² has an apparently original label *s3t-nsw hmt-nsw wrt hnm Hr-s* (*T3-hct*).³ Cairo Museum, CG 1198.
4. A fragment of vase recovered from the Biban el-Moluk, near KV 6,⁴ is inscribed [. . . .] (*T3-hct*) [.].
5. Ostrakon Louvre 666 names a number of royal daughters, dated to year 53 of Ramesses II.⁵ Among them is the *s3t-nsw T3-hct*.

Most writers have assumed that all the references are to a single lady, a daughter of Ramesses II who bore the usurping king Amenmesse.⁶ This hypothesis rests largely upon the assumption that **1** provides conclusive proof that Amenmesse's mother was named Takhat. However, as Thomas⁷ and I⁸ have already noted, this is by no means the case. Indeed, the evidence of the tomb points strongly to the conclusion that *this* Takhat was *not* his mother.

In addition to the paintings of Takhat in the well-room, the following mid-length pillared hall has similar ones of the *hmt-nsw wrt nbt-t3wy* Baktwernel, assumed to be Amenmesse's spouse. Unfortunately, these chambers are today inaccessible, but James Burton recorded his opinion that the chamber had been 'appropriated by the queen', and 'plastered over in a more inferior style'.⁹ Since the paintings' subject-matter is appropriate to an independent tomb,¹⁰ and unparalleled in a king's,¹¹ one is driven toward the conclusion that the decoration of the two chambers for the two ladies must post-date Amenmesse's demise and/or overthrow, and marks the take-over of the tomb for their burials. In this case, their titles make it difficult to link them with Amenmesse. If Sethos II, on his restoration, had gone so far as to erase the decoration of the usurper's tomb, he would hardly have allowed his women-folk burial in the same monument bearing titles which referred to Amenmesse as *ntr* and *nsw*.

Thus, we must look elsewhere for the ladies' royal son and husband, presumably the same person. A clue may be that the only other royal Baktwernel so far known seems to have been the wife of Ramesses XI, buried in the Biban el-Harim.¹² In the light of Kitchen's point about papyponymy in the Ramesside royal family,¹³ if this Baktwernel were a sister-wife, then the similarly named lady of KV 10 could conceivably have been her grandmother, and hence wife of Ramesses IX. The latter's mother could then have been Takhat. Support is lent to this hypothesis if the titles in KV 10 are complete, rather than contractions.¹⁴ If they are complete, Takhat's husband and Baktwernel's father will not have been kings. This fits in

¹ F. J. Yurco, *MMJ* 14 (1979), 18-20, figs. 4, 6.

² L. Borchardt, *Statuen und Statuetten von Königen und Privatleuten* (Berlin, 1934), IV, 97-9.

³ Yurco, op. cit. 28, nn. 34, 36, *contra* J. Vandier, *RdE* 23 (1971), 182.

⁴ H. Carter, MSS I.J.386, 324. I would like to thank Mrs Diana Magee for access to these papers in the Griffith Institute.

⁵ *KRI* II, 922-3.

⁶ e.g., Yurco, op. cit. 15-31; K. A. Kitchen, *Pharaoh Triumphant* (Warminster, 1982), 216, distinguishes a Takhat I, mother of Amenmesse, and II, wife of Sethos II.

⁷ Op. cit. 111.

⁸ A. M. Dodson, *DE* 2 (1985), 9-11.

⁹ BM MSS 25642, 13.

¹⁰ As noted by Thomas, loc. cit.

¹¹ The depiction of family in kings' tombs appears restricted to three wives and a daughter on a pillar in the burial chamber of KV 34 (Tuthmosis III), and a possible wife in a chamber of KV 14, PM I², 553, 530. A text of a queen also occurs in KV 17, PM I², 540. In none of these are the ladies a prominent feature of the decoration, let alone having main-axis chambers wholly dedicated to them.

¹² 'Wife of King Menmare.' The tomb is recorded as robbed in Year 1 of the *whm-mswt* in Papyri BM 10052 and Mayer A, *KRI* VI, 767-828. See Thomas, op. cit. 269, 271.

¹³ *SAK* 11 (1984), 127-34, esp. 133-4.

¹⁴ That the latter possibility must be borne in mind is demonstrated by the titles of Iset B in QV 51, which omit reference to her being the wife of Ramesses III, leading to her identification as the wife of the mythical 'Prince Ramesses' of Seele *et al.*; cf. Kitchen, *JEA* 58 (1972), 182-94 and *JEA* 68 (1982), 123.

well with Kitchen's proposal that Ramesses IX was the son of Prince Montjuhirkopshef B, son of Ramesses III.¹ Since this prince, and his son Khaemwaset D (= Ramesses IX), were not in the direct line of succession, their marriages to commoners would be quite natural.

A further point is that one of the very few other tombs in the Kings' Valley to be *decorated* for someone other than a king² is KV 19, that of Prince Montjuhirkopshef C, son of Ramesses IX.³ This might suggest that the latter wished to employ the Valley as a family cemetery, rather than use the Biban el-Harim. One might now attribute **4** either to KV 10, as part of Takhat's burial, or to KV 6 as an heirloom, buried with her son.

We are now left with the ladies of **2**, **3**, and **5**. The question arises with **2** of whether her figure is contemporary with the carving of the statue, or was added at the time of its usurpation. That it is original is suggested by the alteration of the title, and the erasure of the name of a king's wife on a companion statue.⁴ Another statue of the same group also includes a female figure carved in relief, but the part formerly containing her name and titles is entirely broken away.⁵ Since the original ownership of the whole group of statues by Amenmesse has been demonstrated by Yurco,⁶ one appears inevitably led to the conclusion that, unless the change in title was the correction of an error under Amenmesse, the latter's mother *was* named Takhat. Thus we have the extremely odd situation of a king with a mother whose later homonym usurped his tomb! However, this seems the most likely explanation of the evidence.

The next question is whether **3** is the same lady or a third Takhat. The inscription accompanying the queen's figure seems to be original, unlike that naming the king,⁷ in which case this Takhat must have been a royal wife of, at the latest, Amenmesse. However, its retention after the usurpation of the statue would suggest that she was also a spouse of its ultimate owner, Sethos II.⁸ The substitution of *hmt* for *mwt* on **2** would surely indicate that the mother of Amenmesse also became Sethos' consort. Since it seems unlikely that he had two wives named Takhat, one would tend to equate the two. Thus, Amenmesse ceases to be a possible owner for CG 1198, which must then have belonged to an earlier king. The problem is to identify the latter; Yurco suggests either Ramesses II or Merenptah, on balance favouring the latter.⁹ However, there exists a third possibility—Sethos II. It has been argued that the reign of Amenmesse represents the rule of a counter-king, mainly in the south, during a considerable part of Sethos' reign;¹⁰ in this case, the statue could have been made for Sethos II, usurped by Amenmesse, and then reclaimed by Sethos.¹¹ This would make Amenmesse Sethos II's son, though leaving the reasons for his revolt entirely obscure. As to Takhat's origins, **3** shows her to be the daughter of a king. As the only other *syt-nsw* of the name known, the lady of **5** would seem likely to be the same woman in her younger days. As (probably) a child of Ramesses II's old age,¹² she could have been younger than her great-nephew and husband, if the latter was one of Merenptah's first-born.

We may now seek the identity of the wife of Amenmesse, whose name has been erased from the Karnak statue. Yurco assumes her to have been Baktwernel,¹³ but with the latter

¹ Kitchen, *SAK* 11, 127–34.

² KV 3 (son of Ramesses III) and KV 13 (Chancellor Bay(?)) appear to have been at least intended to be decorated, in contrast to such tombs as KV 46 (Yuya and Tjuiu).

³ PM 1², 546.

⁵ Op. cit. 24, fig. 12.

⁶ Op. cit. *passim*.

⁴ Yurco, op. cit. 22–3, figs. 10, 11.

⁷ Op. cit. 28.

⁸ *Contra* Yurco. It would seem odd for a king to be accompanied on a statue by another king's wife, unless she were his mother. The case of Hatshepsut as *hmt-nsw* (sc. of Tuthmosis II) on early monuments of Tuthmosis III is rather exceptional, as perhaps is that of Tawosret and Siptah.

⁹ Op. cit. 28 n. 36.

¹⁰ R. Krauss, *SAK* 4 (1976), 161–99 and 5 (1978), 131–74; Dodson, op. cit. 8–9.

¹¹ Double-usurpation is not ruled out by Yurco, loc. cit.

¹² She is not mentioned in the earlier, monumental, lists, *KRI* 11, 916 ff. Various other explanations have been suggested, but a straight list of daughters of Ramesses II seems the most likely.

¹³ Op. cit. 23.

now eliminated from this role, another candidate must be sought. The most probable is Tia, mother of Siptah, if one accepts Aldred's arguments for the latter being a son of Amenmesse.¹ Siptah being thus, on the present construction, grandson of Sethos II, his accession would become more explicable, in spite of the identity of his father. He may have been Sethos' nearest relative at the latter's death, though one cannot discount the activity of the Chancellor Bay, explicit in his boast of *smn nsw r st it:f*, 'placing the king on the seat of his father'.²

* * *

In his *SAK* paper (p. 225 n. 13 above), Kitchen sought to identify the *nbt-twy hmt-nsw sst-nsw n ht:f* Tyti as the wife of Ramesses X, daughter of Ramesses IX, and mother of Ramesses XI. Grist has since tried to identify her as a daughter-wife of Ramesses III and mother of Ramesses IV.³ His dating of her tomb (QV 52) is based on similarities between decorative motifs found in it and those of Ramesses III himself (KV 11), Prince Parehirwenemef B (QV 42), Prince Sethhirkopshef B (QV 43), and, to a lesser extent, Princes Khaemwaset C (QV 44) and Ramesse C (QV 53), as well as on differences between them and that of Iset B (QV 51). While recognizing the similarities,⁴ one must bear in mind the paucity of post-Ramesses III Biban el-Harim and *completed-as-designed* Biban el-Moluk tombs,⁵ which makes the conclusion less secure than might at first sight appear. Additionally, on Grist's genealogical reconstruction, QV 52 *cannot* be contemporary with the compared princes' tombs: as *mwt-nsw*, Tyti's tomb would have been decorated under Ramesses IV, while QV 42 and 43 date to the earlier years of Ramesses III.⁶

In his identification of Tyti, Grist follows van Siclen's suggestion that her unusual plumed head-dress denotes a daughter-wife. Iset B, mother of Ramesses VI, is also so identified. This is based on the wearing of the head-dress or a 'similar' one by two definite daughter-wives, Nebettawy (Ramesses II) and Sitamun (Amenophis III).⁷ I find it difficult to allow this *possible* interpretation as good evidence for two other ladies without any corroborative evidence. Indeed, outside the royal family such head-dresses are worn by commoners⁸ as well as the wife of the King of Ugarit!⁹ Additionally, the age factor would seem to be against Ramesses IV being the son of a father-daughter marriage. It seems inconceivable that Ramesses III could have married a daughter while still a commoner, or even Crown-Prince,¹⁰ so that if he was the son of such a union, Ramesses IV could not have been much over thirty at accession. This would leave his son, Ramesses V, still in his teens at his father's death, and not much over twenty at his own demise. Now, if the mummy is correctly identified, the latter's X-ray age at death is thirty, which cannot be regarded as other than an absolute minimum, given the consistent under-estimation of age in recent radiographic analyses.¹¹ Thus, it seems unlikely that Ramesses IV could have been the offspring of a father-daughter union. The age factor is not against Ramesses VI being so

¹ *JEA* 49 (1963), 41-8; cf. also E. F. Wentz in J. E. Harris and E. F. Wentz (eds.), *X-Ray Atlas of the Pharaohs* (Chicago, 1980), 147.

² *KRI* IV, 364, 371.

³ *JEA* 71 (1985), 71-81.

⁴ However, one must correct his statement regarding the non-occurrence under Ramesses III of the *hkr/jackals* motif, found in QV 51. It is found under that king, on the interior of his sarcophagus (Dodson, *DE* 5 (1986), 35), as well as on the exteriors of those of his predecessors Siptah and Sethnakhte (cf. Dodson, *JEA* 72 (1986), 196-8).

⁵ Cf. Thomas, *op. cit.* 127-36.

⁶ Kitchen, *JEA* 58, 186-8.

⁷ C. C. van Siclen III, *JNES* 33 (1974), 150-3.

⁸ e.g. a daughter of Menna (TT 69); A. M. Davies, *Ancient Egyptian Paintings* (Chicago, 1936), II, pl. liii.

⁹ C. F. A. Schaeffer, *Ugaritica (Mission de Ras Shamra)*, III (Paris, 1939), figs. 118, 126. I thank Dr K. A. Kitchen for bringing these two examples to my attention.

¹⁰ Given that only reigning kings are known to have practised such marriages, and then rarely.

¹¹ See Kitchen, *JNES* 44 (1985), 235-7, reviewing Harris and Wentz (eds.), *op. cit.* For example, Ramesses III has an X-ray age of thirty after a reign of 32 years!

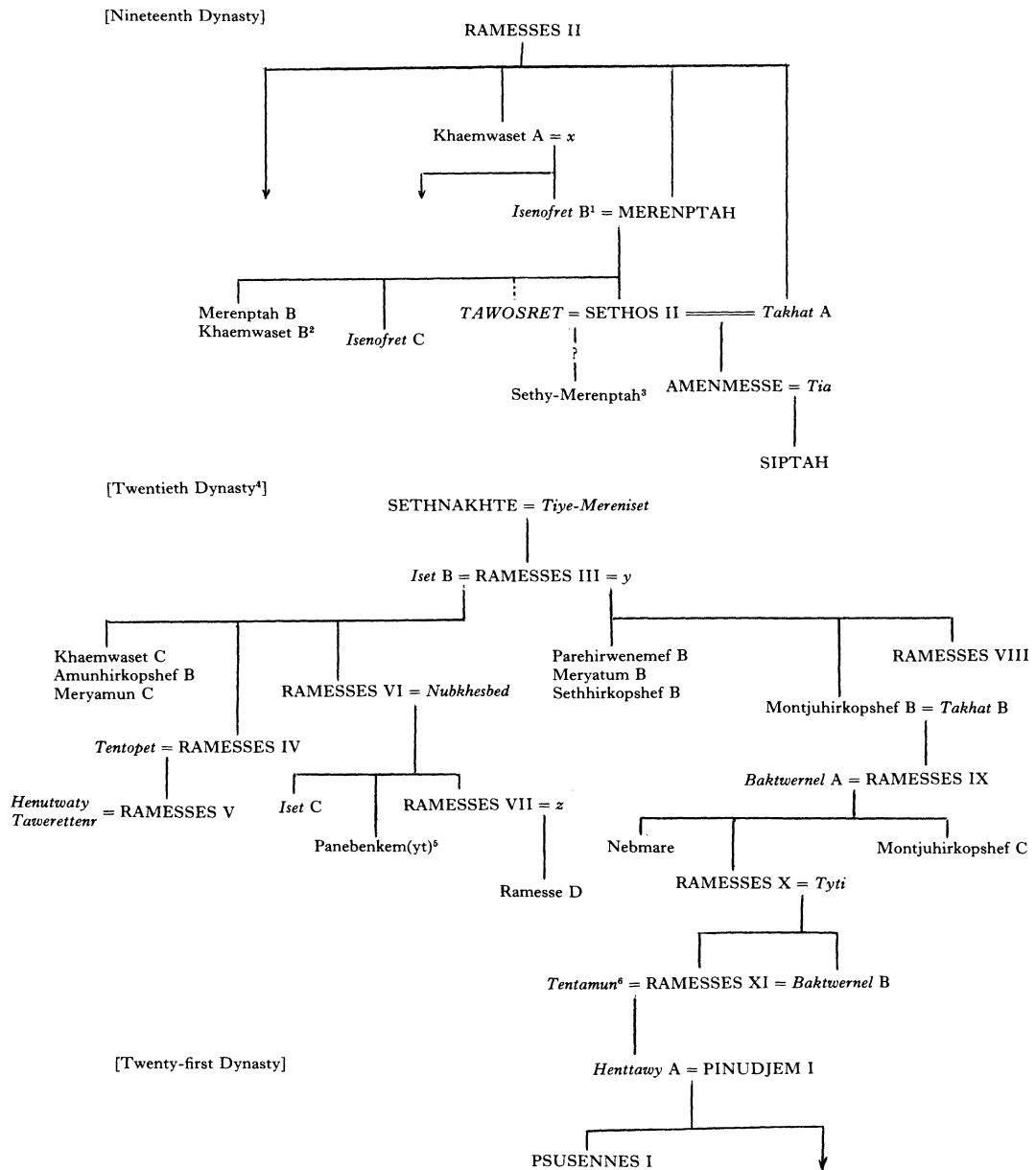


FIG. 1. Tentative Genealogical Table of the Ramesside Period. Females italicized

¹ E. Cruz-Uribe, *GM* 24 (1977), 23-9.

² Yurco, *JSSSEA* 8 (1978), 70.

³ For doubts as to his existence, see Wente, *op. cit.* 146-7.

⁵ Kitchen, *JEA* 68 (1982), 124.

⁴ Upper segment after Kitchen, *op. cit.* 194.

⁶ See A. Niwiński, *JARCE* 16 (1979), 50-1, and K. A. Kitchen, *The Third Intermediate Period in Egypt*, 2nd edn. (Warminster, 1986), 536-40. Whichever reconstruction is accepted, it seems difficult to doubt that a Tentamun was a wife of Rameses XI. She will then have been a second wife, perhaps replacing Baktwernel B after her death before the end of the second decade of her husband's reign, cf. p. 225 n. 12, above.

conceived, since he would seem to have been born after the death of Prince Amunhirkopshef B early in his father's reign;¹ however, if Ramesses IV was not the son of Tyti, her title *mwt-nsu* is incompatible with her being wife of Ramesses III² and so a blow against van Siclen's interpretation of the 'daughter-wife head-dress', and against Iset being a daughter-wife.

I therefore revert to Kitchen's suggestion and propose the genealogical trees shown opposite.

AIDAN DODSON

A note on the family of Montemhat

The elimination of two non-existent members of Montemhat's family (Hetepamun ii and Titenese ii) follows a corrected reading of a text in the Vassalli MSS.

IN 1979 M. L. Bierbrier published some observations on the family of the Fourth Prophet of Amun and Mayor of Thebes Montemhat.³ His study was based on unpublished drawings made in 1862 by Luigi Vassalli, assistant to Mariette. These manuscripts are now kept in Milan and copies of them are held at the Griffith Institute in Oxford. Among the drawings are copies of the texts and decoration of several coffins belonging to relatives of Montemhat, which were discovered in Mariette's excavations at Deir el-Bahri. Most of this material remained unpublished and several of the most important pieces seem now to be totally lost.

One of the items illustrated is a cartonnage mummy-case which belonged to the Lady of the House Titenese. According to Bierbrier,⁴ this lady's ancestry is recorded as: *nbt pr šps Tit-n-3st sst (n) wrb Htp-'Imn s3 (n) imy-r šn hm ntr n 'Imn imy-r niwt t3ty Ns-Min [(sic)]*. It is already well known that the Vizier Nesmin B was married to a lady Titenese, daughter of Hetepamun.⁵ If the above genealogy cited by Bierbrier were correct, the Hetepamun mentioned there must have been the son either of Vizier Nesmin A or Vizier Nesmin B. The former hypothesis would demand that Nesmin B married the granddaughter of Nesmin A, and thus Nesmin B's wife would have been one generation younger than himself. The alternative, favoured by Bierbrier, is to identify the Hetepamun of the cartonnage as the son of Nesmin B and his wife Titenese. This 'Hetepamun ii' would thus have been named after his maternal grandfather, and 'Titenese ii', his daughter, would have been named after her grandmother.⁶

However, through the kindness of Dr Jaromir Malek, I have been able to examine the copies of the Vassalli MSS kept at the Griffith Institute, and a fresh study of them has shown that the genealogy cited by Bierbrier is erroneous. On the sketch of the cartonnage (MS 54, right) Titenese is described as: *ss3 n Htp-'Imn m3r-hrw t3 h3sit n hm-ntr 'Imn imy-r niwt t3ty Ns-Mnw m3r-hrw*. This description occurs three times, with slight variations in the titles of Hetepamun and Nesmin. There is, however, no variation in the terms of relationship

¹ Kitchen, *JEA* 58, loc. cit. It is also conceivable that he had been born earlier with another name, but renamed to keep up his father's tally of sons named after Ramesses II's. For a possible renaming under Ramesses II, see J. Yoyotte, *BiOr* 26 (1969), 14, 15.

² One could possibly save the hypothesis by making Tyti the mother of Ramesses VIII. However, as *mwt-nsu* she could not then have decorated her tomb until his reign, so removing it even further from the time of the tombs with which Grist compares it.

³ In J. Ruffle, G. A. Gaballa, K. A. Kitchen (eds.), *Glimpses of Ancient Egypt* (Warminster, 1979), 116-18.

⁴ Op. cit. 117.

⁵ Cf. the genealogical data on coffins Cairo CG 41020-1; A. Moret, *Sarcophages de l'Époque Bubastite à l'Époque Saïte* (Cairo, 1913), 199-219, pls. XX-XXIV.

⁶ Bierbrier, op. cit. 117 and chart.

used. This Titenese is clearly described as the daughter of Hetepamun and as the wife of the Vizier Nesmin,¹ and she can therefore be recognized as the already attested wife of Nesmin B.² Hetepamun is not called the son of a Vizier Nesmin, so neither 'Hetepamun ii' nor 'Titenese ii' ever existed, and both can be deleted from the family of Montemhat.

JOHN H. TAYLOR

A seated man wearing a cloak (Naples no. 237)

Publication of Naples 237, an Eighteenth Dynasty statuette reworked in about the Twenty-fifth Dynasty, when it was inscribed for the northern Vizier Horkheb.

IN the Museo Nazionale, Naples, no. 237, is a basalt statuette of a seated figure, formerly in the Borgia collection (pl. XVIII, 1-4).³ It was given, and still bears, the number 203 by Georg Zoega when preparing his manuscript catalogue.⁴ A copy of the relevant entry is reproduced in fig. 1. The statuette shows a cloaked man sitting on a cubic seat with a slightly raised back; the base of the seat is prolonged to serve as a footrest. The total height is 25 cm. The dorsal pillar is uninscribed and connects the back of the seat with the lower edge of the wig. On the top of the seat, down its front and continuing on to the base is a single column of text on each side of the man's body. The hieroglyphs are crudely made and the text unframed. The man's body is entirely enveloped in a cloak which even covers his feet. This is unusual in seated sculptures of private persons, whereas it is common in block statues and occasionally found on seated ushabti figures.⁵ That the Naples statuette was not intended as a ushabti is evident. The hands are free of the cloth; the left is placed open on the right side of the chest. The arm forms a straight line from elbow to fingertips, pointing to the right shoulder. The right hand clasps a kerchief below the left wrist. The cloak has no visible edges except at the wrists. The man wears a plain double wig, the upper part of which covers most of the ears; the wig is fairly flat on the top and falls evenly towards the shoulders, spanning their entire width, and it is slightly rounded at the edge, where it meets the shoulders.

Wigs of this type made their appearance in the New Kingdom before the reign of Amenophis II. There is no indication of the strands of hair and Vandier has mentioned that these wigs have the appearance of a cloth covering the wig.⁶ Rare examples are also found during the Ramesside period⁷ and in the Twenty-third Dynasty.⁸ During the Late Period

¹ As on Cairo CG 41020; Moret, *op. cit.* 204, 206, pl. XXII—except that here the two relationships are written in reverse order: Titenese, wife (here written $\overline{\text{T}}$) of Nesmin, and daughter of the *wcb*-priest of Amun Hetepamun. Titenese's wooden stela in the Cairo Museum (T. 28.1.25.6) also describes her as the wife ($\overline{\text{T}}$) of the Vizier Nesmin, without any reference to her parentage; C. Aldred, F. Dumas, C. Desroches-Noblecourt, J. Leclant, *L'Égypte du crépuscule* (Paris, 1980), 117, fig. 103.

² The cartonnage copied by Vassalli is doubtless the one mentioned by Mariette as belonging to the same Titenese who owned the rectangular coffin Cairo CG 41020; A. Mariette, *Notice des principaux monuments exposés dans les galeries provisoires du Musée d'Antiquités Égyptiennes de S.A. le Vice-Roi à Boulaq*, 1st edn. (Alexandria, 1864), 196, 241; 5th edn. (Cairo, 1874), 230, 243 (called 'Anes').

³ A. Ruesch, *Guida illustrata del Museo Nazionale di Napoli* (1908), 127, no. 366; cf. G. Wad, *Fossilia Aegyptiaca Musei Borgiani Velitris* (1794), 10, no. 203; *Basaltes canescenti-niger, praecedenti similis, inspersus hornblenda nigra, partim informi, partim crystallisata crystallis, ut videtur, silicis olivaceo viridis et mica atro-nigra rara, quae superficiem politam reddidit nigrius maculosam. Statua sedens in throno hieroglyphicis decoro, alta palm. 1, unc. 1½.*

⁴ The statue has been given other numbers (see pl. XVII, 1-4), for which the present museum authorities are unable to trace references.

⁵ P. A. A. Boeser, *Statuetten, Rijksmuseum van Oudheden, Leiden* (Leiden, 1925), pl. vii, no. 27.

⁶ J. Vandier, *Manuel d'archéologie égyptienne*, III (Paris, 1958), 483-4.

⁷ Cairo CG 548: L. Borchardt, *Statuen und Statuetten*, II (Berlin, 1925).

⁸ Cairo CG 42229: G. Legrain, *Statues et statuettes*, III (Cairo, 1914).

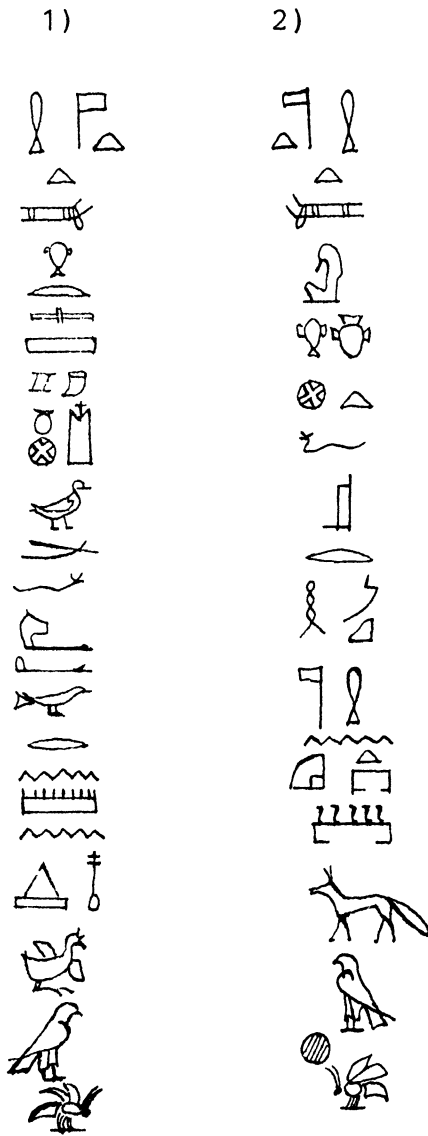


FIG. 2

the type is occasionally found on sculptures from the Twenty-fifth Dynasty, imitating those of earlier periods. It also occurs during the reign of Psamtik I and briefly in the Ptolemaic Period.¹ In all instances where this wig is used, the ears are half-covered. During the Kushite Period and immediately afterwards somewhat similar wig types—also of New Kingdom inspiration—are found with indications of the strands of hair, either striated or with elaborate meshes ending in curls below. They all cover half the ears, whereas the more common bag-wig leaves the ears free.

The long cloak covering both shoulders was not used by private males during the Old Kingdom² but became quite common during the Middle and New Kingdoms. It went out of fashion at the end of the Eighteenth Dynasty, not to be revived until the Twenty-fifth Dynasty.³ The cloak usually terminates fairly low on the legs or at the ankles. When the legs are close together, the hem of the garment may be almost invisible, as on a statuette from the Eighteenth Dynasty in Cairo,⁴ where the details of the damaged feet are barely recognizable. Hornemann's meticulous drawing of the sculpture shows that the feet were not covered.⁵ Of special interest in connection with the Naples statuette is the seated statue of Ahmose, called Ruru, in the Brooklyn Museum,⁶ and dated to the joint reign of Hatshepsut and Tuthmosis III. It shows a man wearing a tight-fitting cloak, which apparently covered both legs and feet completely. Although the feet are broken off at the beginning of the instep, there is no visible indication of a hem, and the single column of framed text which runs down his lap, knees, and shins ends at the break with the beginning of the owner's name.⁷ This clearly indicates that the feet were covered as on the Naples statuette, since it is hardly feasible that the text would have been inscribed on his bare feet.

The Naples statuette was formerly attributed to the Late Period, presumably because of the inscriptions (fig. 2), which certainly are late—probably of the Twenty-fifth Dynasty or immediately after. These have been added on the top, front, and base of the cubic seat, in rough, badly executed hieroglyphs. The secondary character of these texts is emphasized by the lack of the customary framing lines.

¹ B. V. Bothmer, *ESLP*, 3.

² E. Staehelin, *Untersuchungen zur ägyptischen Tracht im Alten Reich* (Berlin, 1966).

³ J. Leclant, *Enquêtes sur les sacerdoces égyptiens* (Cairo, 1954), 4.

⁴ Cairo CG 42112: Legrain, *Statues et statuettes*, I (Cairo, 1906).

⁵ B. Hornemann, *Types of Ancient Egyptian Statuary*, III (Copenhagen, 1957), pl. 763.

⁶ Bothmer, *Brooklyn Museum Annual* 8 (1966-7), 55-89, figs. 1-4.

⁷ S. Sauneron, *Kémi* 18 (1968), 45-50.

Text 1: 'God's father and prophet of Atum, master of secrets in Heliopolis, *sꜣ mrꜣ* priest,^a great count of Memphis,^b vizier Horkheb'.

Text 2: 'God's father and prophet of Atum who is in his town (and of) Osiris Hemag,^c prophet of Ipet,^d judge Horkheb'.

Notes

(a) See G. Posener, *La Première Domination perse en Égypte* (Cairo, 1936), docs. 13-14; J.-C. Goyon, *Rituels funéraires de l'ancienne Égypte* (Paris, 1972), 342.

(b) See E. Graefe, in M. Bietak and E. Reiser-Haslauer, *Das Grab des 'Anch-hor*, 1 (Vienna, 1978), 41, 47.

(c) J. Yoyotte, *BIFAO* 54 (1954), 83-115; R. El-Sayed, *Documents relatifs à Saïs et ses divinités* (Cairo, 1975), 208-13.

(d) See B. Letellier, *BIFAO* 70 (1971), 127.

The vizier Horkheb does not seem to be attested elsewhere, unless he is identical with the vizier of the same name known from a block statuette of approximately the same date, New York MMA 35.9.1.¹ The latter seems to be Theban, however, whereas the titles of this Horkheb clearly place him among the northern viziers of the period.² During the Kushite period, when a revival in the plastic arts took place, earlier sculptures were often taken as models. In this case, an original New Kingdom statuette had the doubtful honour of being reworked as a memorial for Horkheb.

Sculptures of private persons sitting on cubic seats are often abundantly inscribed on the sides of the seat, on the front, and on the garment, as well as on the back pillar, which normally protrudes considerably from the neck or shoulders in the New Kingdom. During the Twenty-fifth and early Twenty-sixth Dynasties, the back pillar often runs flush into the wig border. The Naples statuette has been considerably reworked in the Late Period. The back pillar has been cut in to the level of the wig, obliterating all signs of an earlier text. The reworking may also be responsible for the lack of any V-neck opening of the cloak, and the fingers of the left hand have been rubbed down. The alteration is particularly noticeable on the front of the body—at the abdomen level there is a distinct cavity instead of the usual sloping plane from underarm to knees. This has created a fairly level lap with uncommonly thin thighs and no depression between the knees. The reworking is easily felt with the fingertips. These details are the tell-tale indications that a text column once ran down the front of the garment over lap and shins.

The proof of the statuette's past history as a New Kingdom sculpture is found in the face, which has fortunately escaped later reworking and mutilation. The eyebrows are plastic and curve gently from the root of the nose toward the temples, but they are not extended beyond the corners of the eyes, which are deeply set with rounded eyeballs and rather fleshy eyelids bordered by a rim; there are no cosmetic lines and the treatment of the eye section has a remarkable similarity to a head of Amenophis II in Munich.³ The nose is fortunately undamaged and shows no sign of having been reworked. It is small and straight with finely modelled nose wings and nostrils. The philtrum is delicately indicated above the faintly smiling mouth, a detail which is noticeable on many sculptures from the time of Amenophis II and Tuthmosis IV. The gentle idealizing expression is particularly striking in profile.

¹ L. Bull, *BMAA* 30 (1935), 142-4, fig. 3; cf. H. De Meulenaere, *JEA* 68 (1982), 141 n. 6.

² For Lower Egyptian viziers of this period, see most recently K. A. Kitchen, *The Third Intermediate Period in Egypt*, 2nd edn. (Warminster, 1986), 567-8, 598.

³ Munich ÄS 500: H.-W. Müller, *Münchner Jahrbuch der bildenden Kunst* 3-4 (1952-3), 67-84.



1. Front



2. Back



3. Right side



4. Left side

Naples 237

A SEATED MAN WEARING A CLOAK (pp. 230-4)

Usurpation of earlier sculptures is known from almost any period. A statue in Bologna,¹ showing a seated man wrapped in a cloak, is similar to the Naples statuette in several ways and appears to have suffered the same fate. The head is missing and only a fraction of a wig of New Kingdom type is preserved. The hem of the cloak is indicated by an incised line; the hands are placed in the same position but there is no kerchief in the right hand. The feet are covered in the same manner and one framed line of text with deeply carved hieroglyphs runs down his lap, continuing down the front of his shins to terminate at the instep. The inscriptions are partly hacked away but the owner's name, Hapu-seneb, is clearly seen in fine, well-spaced hieroglyphs. The square-topped back pillar runs from the lower border of the wig to the upper edge of the seat; the back pillar may have been cut back, since there is no text on it, whereas the sides and back of the square seat are covered with texts. Although one may deplore the fact that this fine statuette in Naples of a now unknown man from the New Kingdom has been treated with such scant ceremony in the Twenty-fifth Dynasty, it still presents an excellent example of usurped sculpture reworked in the Late Period. The reason for selecting this statuette for reuse during the Kushite Period was probably that it was a good example of New Kingdom sculpture and satisfied the taste in costume, wig, and position, which at that particular time was being revived as shown by, for example, the statue of Mentuemhat in Berlin.²

E. KERN LILLESØ

Chatby Reconsidered³

This paper re-examines the evidence for the dating of the Chatby necropolis in Alexandria. The finds from the cemetery were originally dated by Breccia to the late fourth century BC, a dating which has long been maintained because of the similarity between the Chatby finds and the latest material from Olynthus. Re-examination of the evidence for Philip's 348 BC destruction of Olynthus now seems to indicate that the city was not finally abandoned until 316 BC, thus lowering the dates for the use of the necropolis. Re-examination of the hydriai and terracottas from the cemetery and comparison with the finds from Koroni further indicate that use of the cemetery extended at least until the middle of the third century BC.

THE dating of the Chatby cemetery in Alexandria has been controversial since Breccia first published its contents in 1912.⁴ Breccia originally dated the necropolis to the late fourth century BC.⁵ Fraser agreed with this dating, although inclined to feel that it probably remained in use until the middle of the third century BC.⁶ Adriani, however, attempted to date the cemetery to the middle of the third century because he saw affinities with the Moustafa Pasha necropolis⁷ whose contents cover a chronological span from approximately 220 to 150 BC.⁸ Adriani, in effect, wanted to minimize the gap between Chatby and Moustafa Pasha. At the same time, a fourth-century date was re-emphasized by Thompson who, in comparing the pottery and lamps from Chatby with the latest from Olynthus, which was destroyed in 348 BC, wondered whether the Chatby finds descend at all into the third century

¹ S. Pernigotti, *La statuaria egiziana nel museo civico archeologico di Bologna* (Bologna, 1980), 33-6, pls. xxxiv-xxxviii.

² Berlin 17271: Leclant, *Montouemhat* (Cairo, 1961), pls. xii-xv.

³ The author would like to thank Professors James Dengate, Thomas Kelly, and Susan Rotroff, all of whom took the time to read this manuscript and offer suggestions for its improvement.

⁴ E. Breccia, *La necropoli di Sciatbi* (Cairo, 1912).

⁵ *Ibid.*, xlvi, 27.

⁶ P. M. Fraser, *Ptolemaic Alexandria*, 1 (Oxford, 1972), 32 and n. 240; see also T. Rönne and P. M. Fraser, *JEA* 39 (1953), 89 n. 1.

⁷ A. Adriani, *Annuaire du musée gréco-romain* (Alexandria) 1 (1933-5), 135 ff. Also, Adriani, *Annuaire du musée gréco-romain* 3 (1940-50), 115.

⁸ For a good review of the evidence for this dating, see Fraser, *Ptolemaic Alexandria*, II, 106 n. 255.

BC.¹ Thompson's arguments were based upon the similarity of the Chatby material with the latest finds from Olynthus and the fact that the necropolis yielded a considerable amount of red-figure pottery which could not last into the third century.

Yet the nature of Philip's destruction of Olynthus in 348 BC is not at all clear. Dengate,² who has recently reviewed the evidence, concluded that the city was reconstituted after 348 BC, and not finally abandoned until 316 BC when Cassander moved the population to his new city of Cassandreia. That Olynthus was destroyed completely in 348 BC was always maintained by Robinson who viewed this as a fixed point in fourth-century chronology.³ Ferguson, on the other hand, pointed out that the city must have continued in some form after 348 BC, since between a quarter and a third of the datable Olynthians known by name lived after that date.⁴ If Ferguson and Dengate are correct, then the latest finds from Olynthus are to be dated to 316 BC and not to 348 BC.

If one accepts this and also considers the comparison of the Chatby and Olynthus finds to be valid, then one must conclude that Chatby did not begin until 316 BC and that use of the necropolis probably carried on into the third century. The late fourth-century dating of the Chatby finds was an important element in Thompson's consideration of the chronological span for his Group B, from the Athenian Agora, which he dated to between 320 and 275 BC.⁵ Grace and Rotroff, however, have recently lowered its terminal date to c.240 BC, some thirty-five years later.⁶ Such a redating is important for Chatby, for it allows a greater chronological range for the material.

The Hadra hydriai from Chatby should also be considered in any attempt to lower the dates of the cemetery. Breccia originally noted the absence of figured Hadrads at Chatby; he therefore proposed that the hydriai decorated with floral or linear motifs, which are characteristic of the Chatby graves, should antedate the figured vases.⁷ The latter he assigned to the first half of the third century BC and used them as evidence for his late fourth-century dating of the Chatby necropolis. More recently, however, Callaghan, using newly excavated material from Knossos and Lyttos, has proposed a lowering of the figured style to the final quarter of the third century BC with a probable extension into the early second.⁸ Since it is now clear that the vessels decorated in the floral or linear style precede the figured vases, it follows, then, that the floral/linear style should continue at least to the third quarter of the third century BC.

Indeed, many of the Chatby Hadrads have the globular shapes and rather bulky proportions that are characteristic of an early phase of the hydriai. Such an early phase has recently been isolated by Enklaar, who dates it to c.260–240 BC.⁹ This again allows use

¹ H. A. Thompson, *Hesperia* 3 (1934), 315 n. 1.

² J. Dengate, 'A Solution to the Problem of Early Hellenistic Chronology, 250–260 B.C.' (unpublished paper). Rose, on the basis of the numismatic evidence, goes one step further than Dengate and concludes that Olynthus continued to be inhabited, perhaps extensively, into the third century BC and possibly longer; cf. M. Rose, *AJA* 88 (1984), 258.

³ See especially, D. M. Robinson, *Excavations at Olynthus*, VIII (Baltimore, 1938), 1–17.

⁴ W. S. Ferguson, *AJA* 39 (1935), 154–5.

⁵ Thompson, op. cit. 332.

⁶ V. R. Grace, *AthMitt* 89 (1974), 193–200, esp. 198 n. 19; S. I. Rotroff, *The Athenian Agora*, xxii, *Hellenistic Pottery* (Princeton, 1982), 108–9. Kroll's numismatic appendix to Grace's article supports Grace's date on the basis of wear on the coins; cf. J. H. Kroll, in Grace, op. cit. 203.

⁷ Breccia, op. cit. 27. The attempt by Guerrini to reverse this order and put the figured Hadrads at the head of the development has been criticized by Cook who has pointed out weaknesses in her groupings. See L. Guerrini, *Vasi di Hadra. Tentativo di sistemazione cronologica di una classe ceramica*, Seminario di archeologia e storia dell'arte Greca e Romana dell'Università di Roma, *Studi Miscellanei* 8 (Rome, 1964), Groups A and B; B. F. Cook, *AJA* 70 (1966), 329–30.

⁸ P. Callaghan, *BSA* 75 (1980), 33–7; *BICS* 30 (1983), 124.

⁹ A. H. Enklaar's study of the Hadra hydriai is not yet published, but he has very kindly made his views known to the author.

of the cemetery to extend into the mid third century, at least as late as 240 BC, according well with the Grace/Rotroff terminal date for Thompson's Group B. Further, for the Chatby terracottas, Kleiner proposes a general range from 300 to 200 BC.¹

Until further work is done on the pottery from Olynthus, however, and distinctions are made between the pre- and post-348 BC destruction material, the finds from Koroni must remain the best parallel for those from Chatby. The excavators at Koroni put the fort in the time of the Chremonidean War (265–261 BC),² a date which has recently been supported numismatically by Kroll.³ The similarity of the Koroni and Chatby finds again indicates that the bulk of material from the Chatby necropolis belongs to the third century BC and that use of the cemetery lasted at least until the middle of that century and perhaps later. This narrows considerably the gap between Chatby and Moustafa Pasha, to a point where it is entirely possible that when one cemetery ceased to be used, the other began its existence almost immediately afterwards.

WILLIAM D. E. COULSON

Notes on a Coptic Letter from Antinoe

This study offers alternative translations of several passages in a letter previously published in *JEA* 54 (1968), 239–42, based on their grammatical and lexical content. It is tentatively suggested that this document may have served as a *Vorlage* for someone wishing to recover a debt from one convinced that he is under no obligation to repay it.

IN *JEA* 54 (1968), 239–42, Sergio Donadoni published two Coptic letters from Antinoe, regrettably unaccompanied by photographs. The following are a few observations on the second letter.

l. 2 ΟΥΔΑΚ: the alternative 'yourself' is perhaps preferable to 'da solo'. ροϛ[ε τωνε is translated as '(la circostanza) difficile è sorta'. This is impossible for two reasons: ροϛε cannot be understood as an epithet, but must be part of the predicate, while τωνε is the adverb 'very' and not the verb τωογν 'arise'. Translate: '(times) are very hard'.

l. 3 μαρε λααγ . . .: this is not an interrogative with an optative. It is the aorist negative: 'No one helps man but his (own) family (lit. house)'.

l. 7 μαγ μπυα . . .: Donadoni has translated 'rendilo degno . . .'. I am not sure how he understands the verbal prefix, perhaps as an optative. This is not possible—it too must be the aorist negative. The verb μπυα has several possible meanings, one of which is 'be obliged' (*CD* 179a meaning (d)). One might translate 'And the man like you is not obliged to accept (my) word'.

l. 9 εϕριχω: does Donadoni really think that the verbal prefix represents a second present? It is surely a circumstantial. His understanding of the preposition 'a pro' di' is not attested (as far as I am aware): the probable meaning here is 'in the care of' (*CD* 758a in the middle of the column). The entire sentence may be rendered as follows: 'The man like you is not obliged to accept (my) word, for he knows the justice which is in the care of his (own) soul' i.e. he is not only mean (he clearly has no intention of paying back the money), but he is conceited enough to disregard the legitimate request of the writer and rely entirely on his own 'sense of justice'. †ϛοογν εμοκ: there is nothing syntactically unusual in the position of the pronoun between this verb and the subsequent noun clause (*CD* 370a).

l. 12 ενε κενος . . .: the syntax of this passage is interesting. ενε is probably

¹ G. Kleiner, *Tanagrafiguren*, *JdI*, Ergänzungsheft 15 (Berlin, 1942), 31 and 276 n. 7.

² E. Vanderpool, J. R. McCredie, and A. Steinberg, *Hesperia* 31 (1962), 26–61, esp. 59–60; *Hesperia* 33 (1964), 69–75. Also, J. R. McCredie, *Fortified Military Camps in Attica*, *Hesperia*, Supplement 11 (Princeton, 1966), 1–16, esp. 15–16.

³ Kroll, op. cit. 201.

iconography have often led me to ponder on the possibility of the art and religious ideas of ancient Egypt lingering on in early Christian art, bearing in mind the rise of monasticism in Egypt, and the strong defence of the use of sacred images by the monks there during the Iconoclastic period of Byzantine culture. It seems to me that Ritner's thesis, which I regard as both plausible and well argued, throws light on a problem of Byzantine iconography, and this, reciprocally, tends to confirm his argument. On p. 154 of his article he identifies certain representations of Anubis found beneath the feet of mummies of the Roman period with a resurrection symbol—Anubis elevates the lunar Osiris, newly reborn, into the heavens, and so also the deceased, by his own identification with Osiris.

Now, in Byzantine iconography St Christopher is often depicted as dog-headed, e.g. John Taylor, *Icon Painting* (Oxford, 1979), 65. Examples exist from many periods throughout the sphere of Byzantine influence. Although none are known before the Iconoclastic period (AD 765+), so many early paintings were destroyed in that period that it is perhaps not surprising that this should be so. The notable exception is, of course, at St Catherine's monastery, Mount Sinai. I have often wondered why the early artists chose to show St Christopher as dog-headed, and whether there could be any connection with Anubis. Now I feel that Ritner has provided proof. He equates the lunar disc with Osiris. The moon waxes and wanes, hence his reference to the crescent moon and the full.

The central story of the legend of St Christopher is that he carried an unknown and stranded child across a flooding river on his strong shoulders; as he struggled across, the weight increased and became almost intolerable as he reached the further shore. To his surprise, he found the child had grown into an adult, who revealed himself to be the Christ. It seems to me that the parallels between this story and Ritner's thesis—the young moon becoming full (just as the child becomes a man), and borne up, by Anubis in one case and St Christopher with his long muzzle in the other; and the moon/Osiris and child/Christ, both gods of the Resurrection—are too close to be mere chance. Christopher (whose name means 'Christ carrier') never existed as a person and his legend is a hotchpotch of disparate elements culled from various sources. It seems to me not improbable that ideas such as those identified by Ritner were incorporated into Christianity in the Nile valley and expressed there iconographically and by word in the strange imagery and story of St Christopher.

DOM BEDE MILLARD, OSB

Some Remarks on Deer (Cervidae) in Ancient Egypt

The evidence for deer (Cervidae) in ancient Egypt is reviewed briefly. The question of whether deer ever existed in the wild as an element of the Egyptian fauna, or were only known from rare exotic imports, cannot be conclusively answered yet. It is quite likely, however, based upon the records of the Egyptians themselves, that deer were native, whether naturally occurring or introduced. While the identification of the species depicted as the Persian Fallow Deer (*Dama dama mesopotamica*) is probably correct, the Barbary Red Deer (*Cervus elaphus*) remains a possibility.

It has long been recognized that deer, easily identifiable from the characteristic large antlers of the male, are represented in Egyptian art. There is no need to enumerate all the known examples, as these have been gathered together and commented upon elsewhere.¹ What concerns us here is the identification of the species depicted and the status of the deer in ancient Egypt.

¹ See L. Keimer, in *Mélanges Maspero*, 1 (Cairo, 1934), 273–308; L. Joleaud, in *Mémoires présentés à l'Institut d'Égypte* 27 (1935), 21–44. For Keimer's reply to Joleaud, see his *Études d'égyptologie* Fasc. 1 (Cairo, 1940), 16–18.

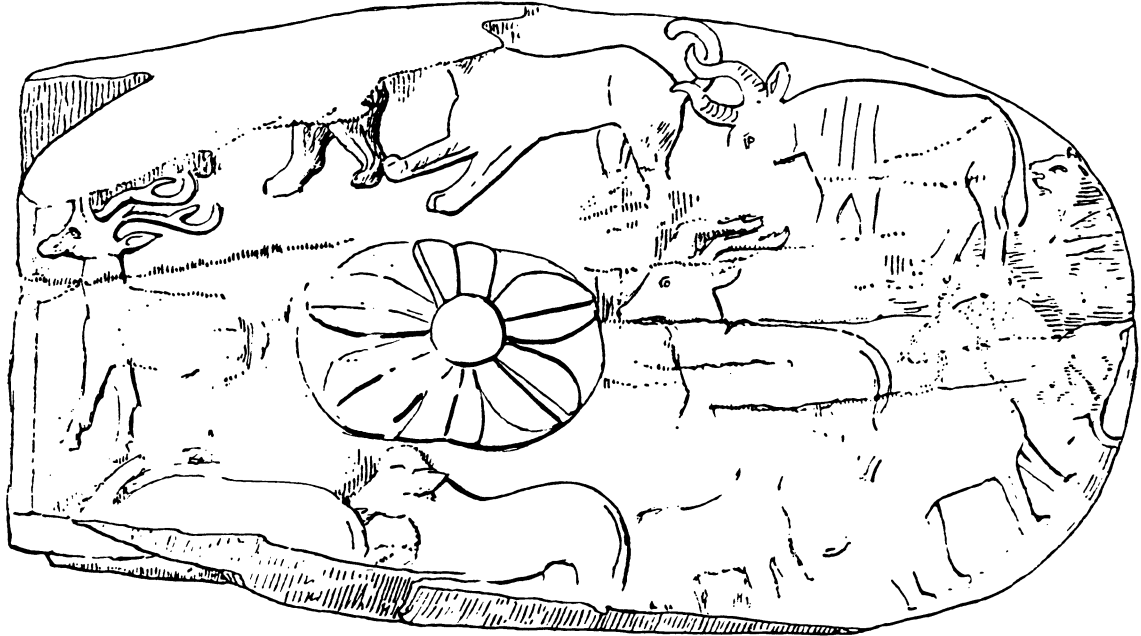


FIG. 1. Deer depicted on the Carnarvon ivory knife handle (after Bénédite)

To judge from the surviving pictorial record, deer were quite rare in ancient Egypt. In all of Egyptian art, Keimer was only able to list 25 occurrences,¹ while Butzer counted a total of 33 from the over 1,000 representations of wild animals he has viewed.² Remains of Cervidae have not been documented from the Late Pleistocene fauna of Egypt, the Sudan, or from Cyrenaica.³ Deer also seem to be absent from rock drawings in Egypt,⁴ and they do not appear in the decoration on predynastic pottery.⁵ The earliest undisputable example in Egypt comes from the Late Predynastic Period, when they can be recognized amongst the animal life pictured on knife handles (fig. 1),⁶ in a hunting scene on the Hunters' Palette,⁷ and on the Late Predynastic/First Dynasty gold mace handle from Nubia.⁸ Beginning early in the Fourth Dynasty,⁹ deer are occasionally portrayed in Old Kingdom tombs as members of the line of desert animals (fig. 2) which is brought by attendants to the deceased.¹⁰ The way in which they are handled and taken into captivity has suggested to some that the Egyptians may have 'semi-domesticated' the deer,¹¹ but this is unproven. The head of a large buck appears on a gold diadem thought to be an Asiatic import from the Hyksos Period,¹² but may

¹ Keimer, *Mélanges Maspero*, 1, 273-308.

² K. W. Butzer, *LÄ* II, 1220.

³ C. S. Churcher, *Annals of the Geological Survey of Egypt* 4 (1974), 363-84; Butzer, op. cit. 1220 n. 11.

⁴ Butzer, loc. cit. Joleaud, op. cit. 29 fig. 12 wishes to see a deer from a rock drawing illustrated in W. M. F. Petrie, *Ten Years' Digging in Egypt* (London, 1891), 75 fig. 57. However, this is quite insufficient proof.

⁵ J. Capart, *Primitive Art in Egypt* (Philadelphia, 1905), 111, and J. Vandier, *Manuel d'archéologie égyptienne*, I (Paris, 1952), 336 correctly question earlier attempts to identify deer on predynastic pottery.

⁶ Also the Gebel el-Tarif knife handle, for which see Vandier, op. cit. 547 fig. 366; H. Asselberghs, *Chaos en Beheersing* (Leiden, 1961), pl. xxxiii.

⁷ See Asselberghs, op. cit. pl. lxxv; A. J. Spencer, *Early Dynastic Objects* (London, 1980), pl. 63.

⁸ See C. M. Firth, *The Archaeological Survey of Nubia. Report for 1910-1911* (Cairo, 1927), pl. xviii; B. G. Trigger, *Nubia under the Pharaohs* (Boulder, 1976), 43 fig. 10.

⁹ See Petrie, *Medum* (London, 1892), pl. xxvii.

¹⁰ Vandier, *Manuel*, v, 8 and 48.

¹¹ e.g., Joleaud, op. cit. 37-40.

¹² See H. G. Fischer, *BMTA* 28 (1969-70), 69-70; C. Aldred, *Jewels of the Pharaohs* (London, 1971), pl. 59.

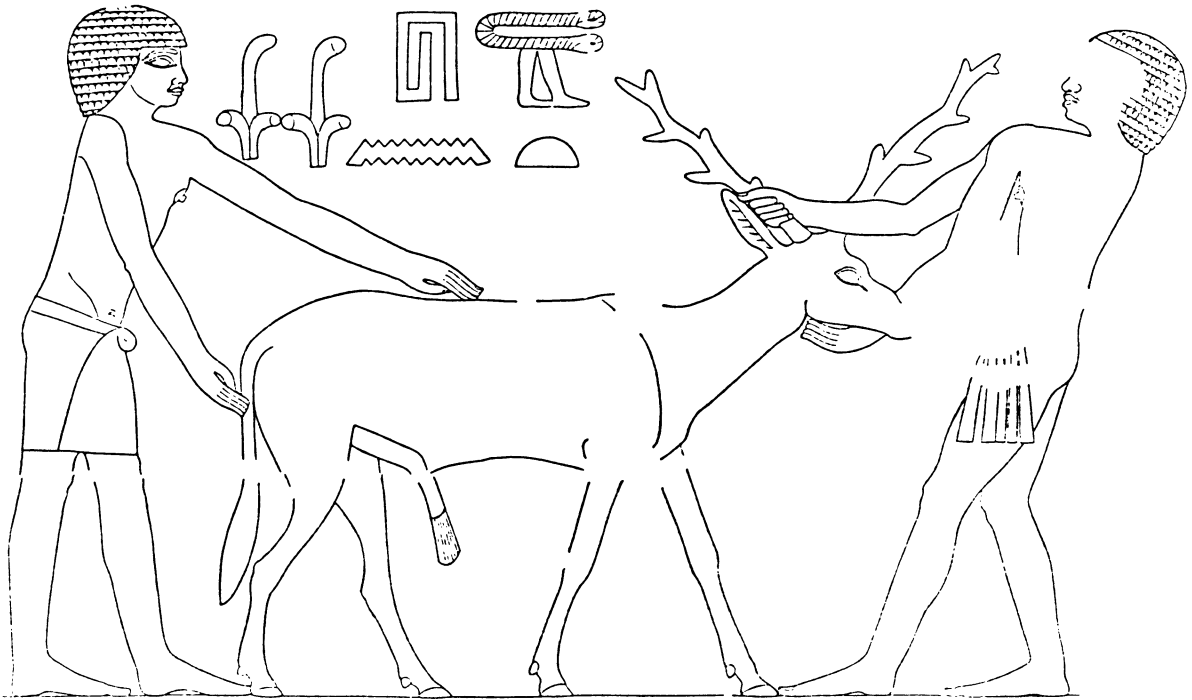


FIG. 2. Deer from the file of desert animals in the mastaba of Ti (after Wild)

be of Middle Kingdom date and only showing foreign influence. It is, however, in hunting scenes that they most frequently occur (fig. 3). From the Late Predynastic until about the mid-Eighteenth Dynasty deer sometimes appear amongst the desert fauna hunted with bow and arrow.¹ They are nearly always depicted in this type of composition in large fenced-off enclosures or 'reserves', rather than in the wild.² The Egyptian monuments then maintain a complete silence until the last quarter of the fourth century BC, when two deer are shown being brought as offerings in the tomb of Petosiris at Tuna el-Gebel.³ Attempts to identify them in later artistic works are not very convincing.⁴ Fragments of deer antler have been recovered from excavations at Deir el-Medina⁵ and Qaw el-Kebir,⁶ and are dated to the New Kingdom, but their specific identification remains uncertain. From the very stylized treatment of the antlers in these representations, it is clear that they were often done by artists who had never actually seen the living animal,⁷ and, as a result, the detail is not nearly sufficient to indicate which species of deer served as the model. The Egyptian name for the deer, *hnn*, is very rare.⁸ For our purposes, the most significant instance of it is in the 'Annals' of Tuthmosis III, where a deer is listed among the animals imported from Syria in year 38.⁹ The question is whether these pictures are adequate proof that deer were

¹ The examples are listed in Keimer, *Mélanges Maspero*, I; Joleaud, *op. cit.*

² Joleaud, *op. cit.* 43.

³ See G. Lefebvre, *Le tombeau de Petosiris*, III (Cairo, 1924), pls. xxxv, xlvi, xlviII, and xlix.

⁴ Keimer, *Mélanges Maspero*, I, 288.

⁵ *Ibid.* 274; Joleaud, *op. cit.* 34 n. 4.

⁶ W. R. Dawson, *Journal of the Linnean Society of London* 39 (1934), 143, note at bottom.

⁷ e.g., Dawson, *op. cit.* 145; Joleaud, *op. cit.* 35; Aldred, *op. cit.* 204-5.

⁸ Mostly known from labels to scenes, see G. Bénédite, *JEA* 5 (1918), 12-13; *Wb.* II, 495, 19-20.

⁹ K. Sethe, *Urk.* IV, 718; N. de Garis Davies, *The Tomb of Puyemrê at Thebes*, I (New York, 1922), 46 n. 2.

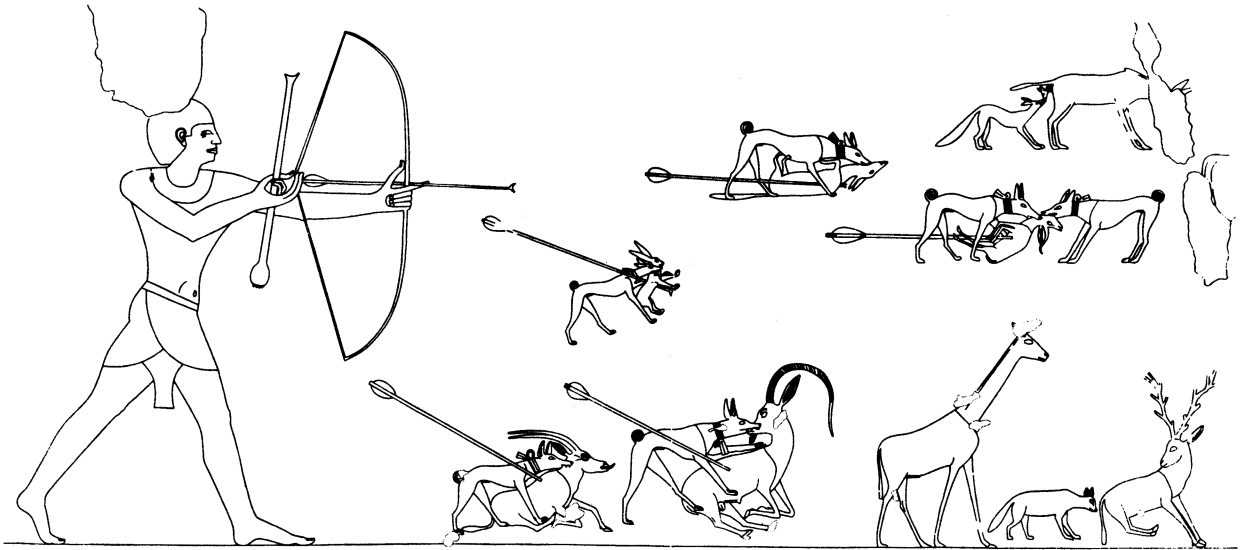


FIG. 3. Ukhhotep I hunting desert game (after Blackman)

indigenous to Egypt, or were introduced by man to the Egyptian countryside at an early age, or were always exotic imports.

With the exception of the Barbary Red Deer (*Cervus elaphus*) which occurs in Tunisia, and possibly the European Fallow Deer (*Dama dama dama*) which inhabited parts of Libya, Tunisia, and an area near the Algerian/Tunisian border until late in the last century, deer are not regarded as being indigenous to the African continent.¹ The origin of these Fallow Deer is unknown.² Joleaud has suggested that they were introduced by the Phoenicians or Romans,³ and this has tentatively been accepted.⁴ There is also some evidence that the Fallow Deer was known in Egypt (introduced?) during the sixteenth century AD,⁵ and it is very possible that they continued to live in the wild in the Wadi Natrun until the nineteenth century.⁶ There are no wild deer currently living in Egypt.⁷

The first scientific inquiry into the identity of the deer on the Egyptian monuments was made by Hilzheimer,⁸ who concluded that the portrayals must surely refer to the Persian Fallow Deer (*Dama dama mesopotamica*). He advanced the idea that they had migrated to Egypt from Western Asia during Pleistocene times. This argument was strengthened by Bate,⁹ who, while admitting that there was no definite proof, remarked that in view of the very abundant evidence for the Persian Fallow Deer in Palestine in the Pleistocene, it would not be difficult to suppose that some wanderers might have penetrated into Egypt. It is known that during the Pleistocene faunal exchanges were taking place between Africa and

¹ N. G. Chapman and D. I. Chapman, *Mammal Review* 10 (1980), 64.

² *Ibid.*

³ Joleaud, *op. cit.* 2 and 17.

⁴ Chapman and Chapman, *op. cit.* 64; C. Lever, *Naturalized Mammals of the World* (London, 1985), 173.

⁵ Joleaud, *op. cit.* 17. There is also some pictorial evidence to back up this claim—see W. J. Darby, P. Ghalioungui, and L. Grivetti, *Food: The Gift of Osiris*, 1 (London, 1977), 244 fig. 5.17 and 262 n. 7.

⁶ Joleaud, *op. cit.* 17-18; D. I. Chapman and N. G. Chapman, *Fallow Deer. Their History, Distribution and Biology* (Lavenham, 1975), 221.

⁷ D. J. Osborn and I. Helmy, *The Contemporary Land Mammals of Egypt (Including Sinai)* (Chicago, 1980).

⁸ M. Hilzheimer, in L. Borchardt, *Das Grabdenkmal des Königs Sâhu-Re'*, 11 (Leipzig, 1913), 168-72.

⁹ D. A. E. Garrod and D. M. A. Bate, *The Stone Age of Mount Carmel*, 1 (Oxford, 1937), 214.

Asia over the Suez Isthmus.¹ This hypothesis has, in general, been accepted ever since. Joleaud took it a step further, postulating the infiltration of the Persian Fallow Deer into Egypt from Asia and then up the Nile Valley into the Sudan and Ethiopia.² On the other hand, Bénédite believed that he could distinguish three different species of deer in Egyptian art: the Barbary Red, Persian Fallow, and Axis (*Axis axis*) Deer.³ In 1926 Hiltzheimer changed his previously expressed view, when he described a 'new species' of Fallow Deer from north-eastern Africa to science, which he named *Dama schaeferi*, after Heinrich Schäfer, the then Director of the Ägyptisches Museum in Berlin.⁴ He now thought that this was the species of deer known to the ancient Egyptians, and made direct comparisons between his specimen and those deer illustrated in art. This deer is today no longer recognized as a valid species, and a recent review of the Fallow Deer has concluded that *Dama schaeferi* was almost certainly an aberrant European Fallow buck.⁵ Although it lives still in the Egyptological literature,⁶ it cannot seriously be regarded as the deer of the Egyptians.

While some zoologists believe that the original distribution of the Fallow Deer may have included Egypt,⁷ this has yet to be substantiated by fossil, archaeological, or textual documentation, and other zoologists remain doubtful.⁸ There is a great body of evidence for the knowledge and exploitation of the Persian Fallow Deer during historical times in Palestine⁹ and Mesopotamia,¹⁰ and in comparison the Egyptian material is meagre. If the species' range actually extended into north-eastern Africa we should expect to see conclusive proof. Many previous investigators have maintained that deer were, though always uncommon, part of the wild Egyptian fauna,¹¹ be they naturally occurring or introduced by man. It has been suggested that the animal became extinct in Egypt after the New Kingdom, because of its absence (except for Petosiris) in art following the Eighteenth Dynasty and the arrival of a foreign deer during the reign of Tuthmosis III.¹² Also significant is the disappearance of the word *hnn* in Egyptian, to be replaced by a Semitic word for deer in demotic.¹³ However, Dawson sought to explain the deer in another way. He felt that they were known to the Egyptians from a few exotic specimens imported from other countries,¹⁴ which would account for the lack of fidelity in depiction, and that it was not possible to specify the species, but also that this had to be either the Barbary Red or Persian Fallow Deer.¹⁵ They could have been imported exclusively as game for the hunt. If the desert hunting scene in the Twelfth Dynasty tomb of Ukhhotep I (B.2) at Meir (fig. 3) is to be

¹ Churcher, op. cit. 379-80.

² Joleaud, op. cit. 13-14 and 44-6. Also see B. Brentjes, *Säugetierkundliche Mitteilungen* 17 (1969), 203-5. The animals that Brentjes calls deer on Meroitic Period pottery are usually called antelope—see S. Wenig, *Africa in Antiquity*, II (Brooklyn, 1978), 279-80.

³ Bénédite, op. cit. 14 with nn. 3 and 15.

⁴ M. Hiltzheimer, *Zeitschrift für Säugetierkunde* 1 (1926), 155; 2 (1927), 68-73.

⁵ Chapman and Chapman, *Fallow Deer*, 221.

⁶ e.g., Butzer, op. cit. 1220.

⁷ e.g., J. Clutton-Brock, *Domesticated Animals from Early Times* (Austin, 1981), 182; R. M. Nowak and J. L. Paradiso, *Walker's Mammals of the World*, II, 4th edn. (Baltimore, 1983), 1204-5.

⁸ Chapman and Chapman, *Fallow Deer*, 221.

⁹ F. S. Bodenheimer, *Animal and Man in Bible Lands*, I (Leiden, 1960), 18-28; D. Hakker-Orion, in A. T. Clason (ed.), *Archaeozoological Studies* (Amsterdam, 1975), 295-301; W. W. Ferguson, Y. Porath, and S. Paley, *Mammalia* 49 (1985), 209-14.

¹⁰ E. D. van Buren, *The Fauna of Ancient Mesopotamia as Represented in Art* (Rome, 1939); W. Heimpel, in *Reallexikon der Assyriologie und vorderasiatischen Archäologie*, IV (Berlin, 1972-5), 418-21.

¹¹ Keimer, *Mélanges Maspero*, I, 288; Joleaud, op. cit. 43-4.

¹² Bénédite, op. cit. 12; Hiltzheimer (1926), op. cit. 152-3.

¹³ Bénédite, op. cit. 12; Butzer, op. cit. 1220 and nn. 13-14. The only pre-demotic example quoted in J. Černý, *Coptic Etymological Dictionary* (Cambridge, 1976), 46, is usually translated as 'ram' or 'sheep'.

¹⁴ Dawson, op. cit. 140-2.

¹⁵ Ibid. 145.

believed, the giraffe (*Giraffa camelopardalis*) was also imported for sport, as by this time it had undoubtedly been long since eradicated from its former range in Egypt.¹

In sum, it must be acknowledged that there is, in the present state of our knowledge, little concrete evidence to help us decide whether deer were ever really part of the wild Egyptian fauna or only an exotic. It seems likely, in any case, that since the Egyptians clearly wished to create the impression that deer were native, it would not be too presumptuous simply to accept their records at face value. Its scarcity in art, the unfamiliarity the artists displayed, and the infrequent mention in texts, would seem to speak of its extreme rarity. It would, however, be a mistake to place a great deal of weight upon the artistic record as proof of this. The various species of wild animals traditionally depicted in hunting and other related compositions cannot be taken as a true reflection of their actual numbers in the country. Their occurrences in these scenes are probably less a reflection of reality and more an expression of an ideal. The disappearance of the deer in art after the Eighteenth Dynasty may well only be the result of the change from 'secular' tomb decoration, and does not necessarily mean extinction. Deer could have been more common than is generally supposed. Whichever interpretation is ultimately correct, it is still difficult to establish beyond all doubt which species is pictured. The Persian Fallow Deer remains a strong candidate.² However, the diagnostic flattened and palmate antlers of the Fallow Deer are rarely, if ever, suggested in any of the known portraits,³ and for this reason I do not think that the Barbary Red Deer should be completely ruled out. Perhaps it once had a greater distribution across North Africa than it does at the present time. Both of these species can adapt to a variety of habitats,⁴ although generally some forest is required for the Fallow Deer. Owing to the uncertainty in the status of the deer and its identification, it would, in my opinion, be rather hasty to attempt to draw conclusions regarding the landscape or environmental conditions of the country in antiquity on the basis of the presence of deer.⁵ Only further archaeological and zooarchaeological findings can hope to answer more firmly our questions about the deer.

PATRICK F. HOULIHAN

Corrections in the Identifications of the Alabaster Ibexes in Tutankhamun's Treasures

Observations made in European zoos lead to the conclusion that the facial markings on the 'bleating ibex' and the ibexes on the alabaster boat from Tutankhamun's tomb are representative of the wild bezoar goat or Syrian ibex, *Capra aegagrus*. Previous arguments that the markings represented those of the dorcas gazelle or an artist's confusion between facial patterns of domestic goats and gazelles are abandoned.

THE facial markings on the 'bleating ibex' and the ibexes on the alabaster boat from Tutankhamun's tomb (pl. XIX, 1, 4) are representative of the wild bezoar goat or Syrian ibex, *Capra aegagrus*, and *not*, as Reed and Osborn proposed, 'of a dorcas gazelle'.⁶ The erroneous statement that 'ibex do not have facial markings' was due to our having examined specimens of *Capra ibex* only⁷ and disregarded the literature on ibex. When I wrote the rebuttal to Edwards's criticism of our identifications,⁸ I persisted with an argument that 'The sharply defined facial markings . . . can be attributed to an artist's confusion

¹ Churcher, op. cit. 376; K. W. Butzer, *Early Hydraulic Civilization in Egypt* (Chicago, 1976), 26-7.

² J. Boessneck, *Veröffentlichungen der Zoologischen Staatssammlung München* 3 (1953), 30.

³ Bénédite, op. cit. 14; Davies, op. cit. 46.

⁴ Nowak and Paradiso, op. cit. 1204-5, 1209.

⁵ A. Nibbi, *GM* 41 (1980), 61-6.

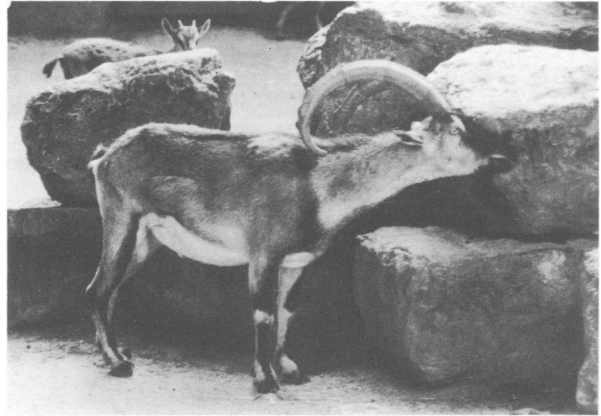
⁶ C. A. Reed and D. J. Osborn, *AJA* 82 (1978), 277.

⁷ Op. cit. 277, figs. 7, 8.

⁸ I. E. S. Edwards, *AJA* 83 (1979), 205.



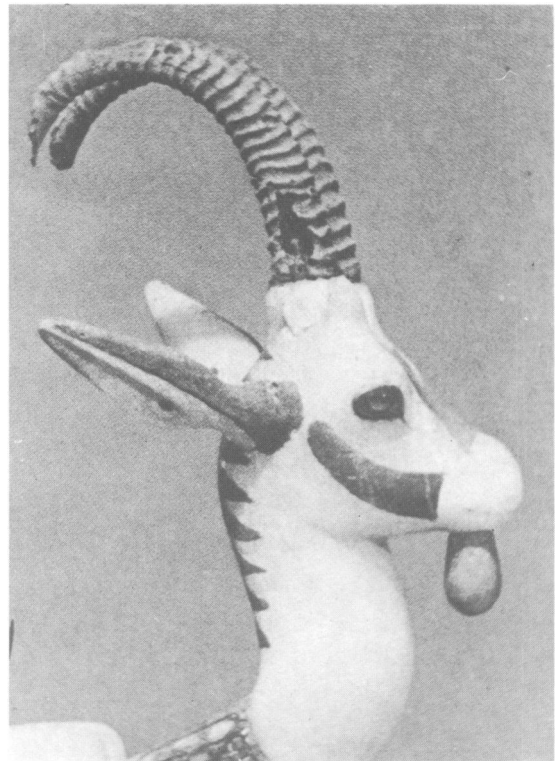
1. Tomb of Tutankhamun, the 'bleating ibex'
(*photograph by Lee Boltin, courtesy M.M.A.*)



2. Wilhelma Zoo, Stuttgart, adult male *Capra aegragus cretica*



3. Schönbrunn Zoo, Vienna, pair of wild bezoar goats (ibex), *Capra aegragus cretica*, about 1-2 years old. Developing frontal ridges can be seen at the bases of the horns of the animal in profile



4. Tomb of Tutankhamun, ibex head from prow of alabaster model boat (after H. Carter, *The Tomb of Tut-Ankh-Amen*, III, pl. 69)

between the facial patterns of domestic goats and gazelles'.¹ The example of a wild goat that I included was an adult male,² and it and the evidence of facial markings in the literature that I perused³ were insufficient to change my mind.

When my manuscript was in press, however, I discovered, to my astonishment and embarrassment, two young *Capra aegagrus* in Schönbrunn Zoo, Vienna, with strikingly gazelle-like facial markings (pl. XIX, 3).⁴ I then consulted authorities to whom Reed and I should have referred originally. In Brehms Tierleben,⁵ the young and the female in a family of bezoar goat are pictured with facial stripes, which are lacking on the adult male; the colour pattern of the male only is described. Couturier wrote that, despite some variations, the coloration is relatively constant in all forms of *C. aegagrus*. Regarding young animals of island forms, he stated that in kids of a few months of age, the face is dark brown and the black line of the cheeks is obvious.⁶ The pale, oblong mark anterior to the eye on adult *C. aegagrus* was not mentioned by these authors. It appears to be a remnant of the whitish facial stripe⁷ (pl. XIX, 2-3). Neither this feature, nor the facial stripes, is present on the Egyptian ibex, *C. ibex nubiana*,⁸ but a faint preocular mark is visible on young *C. i. siberica*.⁹ I have yet to discover at what age the facial markings disappear in the wild goat. From my observations at Hellabrunn Zoo, Munich, and Wilhelma Zoo, Stuttgart, they remain much longer on females than on males. Incidentally, in some breeds of domestic goats facial stripes are retained on adult males as well as females.

In comparison with the photographs of living specimens (pl. XIX, 2-3), stylization is, of course, apparent in the alabaster figures (pl. XIX, 1, 4). The dark cheek stripes are below the eyes rather than at the level of the eyes. The horns are not natural but carved from wood and lack the shape and ornamentation of those of living animals.¹⁰ Horns of such length would represent adult animals, as would also the beards (the beard of the bleating ibex was broken off). The dark markings on the inner anterior margins of the ears are comparable as is the black stripe on the back of the neck. The black 'knee' spots represent the whitish calluses of the living animals.

Only one other reproduction of an ibex with facial markings is known in ancient Egyptian art.¹¹ It has a horn long enough to represent an old male. The dark cheek stripe passes below the eye and the tail is twice the normal length for an ibex. Notwithstanding, this figure represents a Syrian ibex or wild goat. In conclusion, I think that we can assume with confidence that the alabaster ibexes represent the Syrian or Cretan ibex and therefore support Edwards's suggestion of influence from Syria, Crete, or Mycenaean Greece in the art of Tutankhamun's treasures.¹²

DALE J. OSBORN

¹ Osborn, *AJA* 89 (1985), 158-9.

² Op. cit. pl. 32, fig. 5.

³ D. Lay, *Fieldiana: Zoology* 54 (1967), 228; T. J. Roberts, *The Mammals of Pakistan* (London, 1977), 189; T. Schultze-Westrum, *Säugetierkundliche Mitteilungen* 11 (1963), figs. 8, 15, 22.

⁴ These two ibexes came from the herd at Wilhelma Zoo, Stuttgart, whose original stock came from an isolated population on Theodoru Island, Crete, and were described as *Capra aegagrus cretica*.

⁵ Brehms Tierleben, *Saugetiere*, 2nd edn., vol. 3 (Leipzig, 1877), 315, 314.

⁶ M. A. M. Couturier, *Le Bouquetin des Alpes* (Grenoble, 1962), 627.

⁷ Osborn, op. cit. pl. 32, fig. 5.

⁸ Op. cit. pl. 31, fig. 4.

⁹ Reed and Osborn, op. cit. fig. 6.

¹⁰ Op. cit. 278 ff., and Osborn, op. cit. 158, *contra* Cairo museum labels which suggest that they are the natural horns of young animals.

¹¹ P. E. Newberry and G. W. Fraser, *Beni Hassan*, 1 (London, 1893-4), pl. 28.

¹² I. E. S. Edwards, 'Catalogue', in K. S. Gilbert *et al.*, eds., *Treasures of Tutankhamun* (New York, 1976), 130.

Ancient Egyptian Plant-Remains in the Manchester Museum

This article provides a complete, analytical list, with provenance and date, of the plant-remains from ancient Egypt in the Manchester Museum.

THE importance of palaeoethnobotany has increasingly been recognized in recent years, and all material from excavation is now given to experts for identification. However, the botanist often has the problem that, for ancient Egyptian plants, little comparative material is available. It is not widely known that nearly all Egyptian museum collections contain botanical material, mostly in small boxes in the most distant corner of the magazine. Consequently, publication of ancient Egyptian plant-material in museums is desirable for both Egyptologists and palaeoethnobotanists.

In older collections, the provenance and date of such remains are often not known. In the Manchester Museum, however, nearly all the plant-remains are of known origin. The oldest finds are cereals of the fifth millennium BC from the Fayum, which are of great interest to palaeoethnobotany. The collection of Roman garland plants in this museum is particularly extensive. The purpose of the present catalogue is to make all the ancient Egyptian plant-material in the Manchester Museum known to scholars. The statements on provenance and date are taken from the museum inventory. Plant identification is the work of the author.

1. **Predynastic period**

- 1.1. *Hordeum vulgare* L. (Barley) mixed with *Triticum dicoccum* Schübl. (Emmer). Carbonized grains. Fayum silo 33 and 44. Inventory no. 7972. G. Caton-Thompson and E. W. Gardner, *The Desert Fayum* (London, 1934), 47.

2. **Old Kingdom**

- 2.1. *Ceruana pratensis* Forsk. and *Phragmites australis* (Cav.) Trin. ex Steud. Twigs with flowerheads and culms twisted to form a basket-coffin. Probably Third Dynasty, Tarkhan. W. M. Flinders Petrie and Ernest Mackay, *Heliopolis, Kafr Ammar and Shurafa* (London, 1915), 13.

3. **Middle Kingdom**

- 3.1. *Triticum dicoccum* Schübl. (Emmer). Chaff of emmer. Found in MK graves dug under the foundation of the mortuary temple of Ne-User-Re, Abusir. Inventory no. 1455. H. Schäfer, *Priestergräber* (Leipzig, 1908), 99 ff. and 152 ff.
- 3.2. *Hordeum vulgare* L. (Barley). Grains. Twelfth Dynasty, Kahun. Inventory no. 100. Petrie, *Kahun, Gurob and Hawara* (London, 1890), 50.
- 3.3. *Cyperus articulatus* L. Rhizomes. Twelfth Dynasty. Kahun, found in a leather bag. Inventory no. 199. Petrie, *Illahun, Kahun and Gurob* (London, 1891), 13 as roots and nuts. (pl. XX, 1)
- 3.4. *Juniperus oxycedrus* L. (Prickly Juniper). Berries. Twelfth Dynasty. Kahun, found in a toilet-box. Inventory no. 75. Unpublished.
- 3.5. *Vicia sativa* L. (Common Vetch). Seeds. Found in the skull of Khnum-Nakht, without any notes, whether the seeds were originally found in the skull or placed there later. Unpublished.
- 3.6. *Mimusops schimperi* Hochst. (Persea). Twigs and leaves. Der Rifeh. M. A. Murray, *The Tomb of Two Brothers* (Manchester, 1910), 18 and pl. v, 1.

4. **Second Intermediate Period**

- 4.1. *Cyperus esculentus* L. (Tiger nut). Rhizomes. 'Sixteenth Dynasty'. Sedment, Mayana Cemeteries, tomb 1292. Inventory no. 6636. Petrie, *Sedment*, 1 (London, 1924), 18.

- 4.2. *Hordeum vulgare* L. (Barley). Grains. 'Sixteenth Dynasty'. Sedment, Mayana Cemeteries, tomb 1292. Inventory no. 6635. Petrie, *Sedment*, 1, 18.

5. **New Kingdom**

- 5.1. *Zizyphus spina christi* (L.) Willd. (Sidder, Christ's-thorn). Seeds. Eighteenth Dynasty. Abydos. Inventory no. 3710. Unpublished.
- 5.2. *Mimusops schimperi* Hochst. (Persea). Seeds. Eighteenth Dynasty. Abydos. Inventory no. 3710. Unpublished.
- 5.3. *Mimusops schimperi* Hochst. (Persea). Twigs and leaves. Nineteenth Dynasty, found in the foundation deposit of the mortuary temple of Queen Tawosret, Thebes. Inventory no. 5938. Petrie, *Six Temples at Thebes* (London, 1897), 15.
- 5.4. *Nymphaea coerulea* Sav. (Blue-Lotus). Flower. Twenty-fifth Dynasty. Qurneh. Petrie, *Qurneh* (London, 1909), pl. liii.

6. **Roman period**

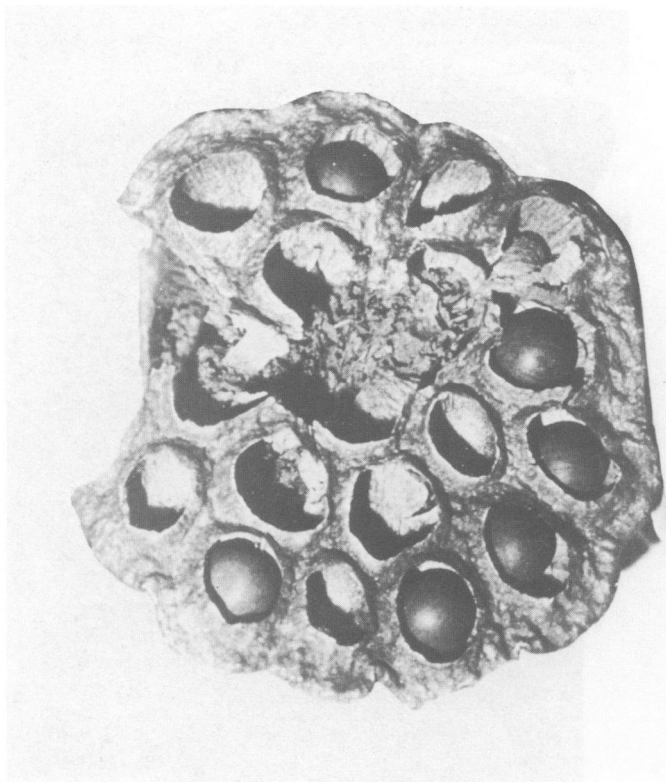
All these garlands probably come from Petrie's excavations in the Fayum. Some have no inventory number and all are unpublished.

- 6.1. *Celosia argentea* L. Inflorescence, part of a garland.
- 6.2. *Nymphaea lotus* L. Flowers, part of a garland.
- 6.3. *Nelumbo nucifera* Gaertn. (East-Indian Lotus). Receptacle with seeds. Inventory no. 6333. (pl. XX, 2)
- 6.4. *Punica granatum* L. (Pomegranate). Petals, part of a garland. Inventory no. 6332.
- 6.5. *Rosa richardii* Rehd. (= *R. sancta* Rich.). Flowers and petals, part of a garland.
- 6.6. *Acacia nilotica* (L.) Willd. (Egyptian Acacia). Flowerheads, part of a garland. Inventory no. 6332.
- 6.7. *Cordia myxa* L. Leaves.
- 6.8. *Majorana hortensis* Moench. (Marjoram). Twigs with leaves, part of a garland.
- 6.9. *Withania somnifera* (L.) Dun. (Rennet). Berries, part of a garland.
- 6.10. *Chrysanthemum coronarium* L. (Garland-Chrysanthemum). Flowerheads, part of a garland.
- 6.11. *Helichrysum stoechas* L. Flowerheads, bound to a garland.
- 6.12. *Narcissus tazetta* L. (French Daffodil). Flowers, part of a garland. (pl. XX, 3)
- 6.13. *Scirpus inclinatus* (Del.) Asch. et Schweinf. ex Boiss. Split culms made into a garland. (pl. XX, 4)
- 6.14. *Phoenix dactylifera* L. (Date-palm). Young, unripe fruits, pierced and threaded.

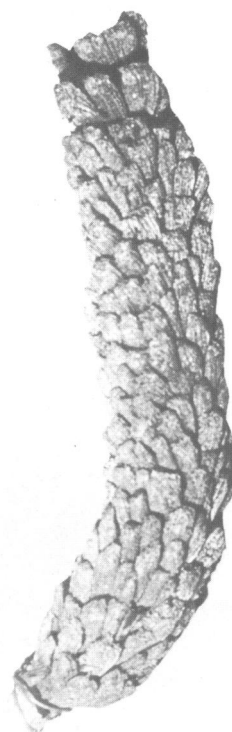
There are also three different kinds of resin in the Manchester Museum, which will perhaps one day be analysed. These resins may provide very interesting information about trade in ancient Egypt.

1. Predynastic. Broken resin-beads, El Mahasna. Inventory nos. 5090 and 5091. Published: Edward R. Ayrton, *Pre-Dynastic Cemetery at el Mahasna* (London, 1911), 27.
2. Eighteenth/Nineteenth Dynasty. Small lumps of resin, Gurob. Inventory no. 562. Unpublished.
3. Roman. Large pieces of resin, Hawara. Inventory no. 3024. Unpublished.

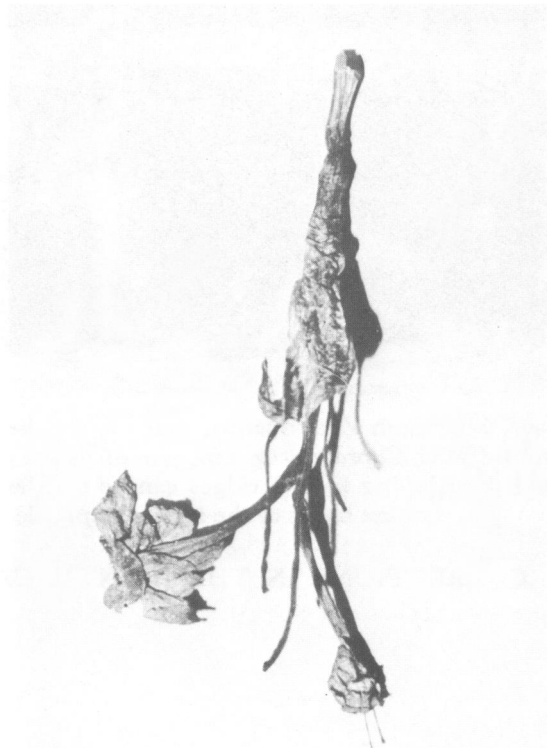
The botanical material in the Manchester Museum shows a wide range of plants, which were used in Egypt either for consumption, cosmetics, or decoration. Cultivated plants as well as collected plants were part of the ancient Egyptian culture and can give us interesting information about life in those days. It is really worth while, therefore, to conserve, study, and publish ancient Egyptian plant-material.



2. *Nelumbo nucifera* Gaertn. (East Indian lotus)



1. *Cyperus articulatus* L.



REVIEWS

Studies in Ancient Egypt, the Aegean, and the Sudan. Essays in Honor of Dows Dunham on the occasion of his 90th Birthday, June 1, 1980. Edited by WILLIAM KELLY SIMPSON and WHITNEY M. DAVIS. 286 × 223 mm. Pp. viii + 218, figs. 143. Boston, Museum of Fine Arts, 1981. ISBN 0 87846 197 3. Price not stated.

This collection of essays is a fitting tribute to the late Dows Dunham. It was presented to him on his ninetieth birthday, some three and a half years prior to his death on 10 January 1984. The volume contains a series of articles on topics related to major areas of Dunham's interests and activities on behalf of the Museum of Fine Arts, Boston. The work is introduced by a short preface by William Kelly Simpson, and contains a bibliography (pp. iv–viii) of Dunham's publications through 1981. At the end of the volume (pp. 214–18) are four appreciations of Dunham by long-time friends and colleagues.

As the title generally indicates, the volume is comprised of thirty articles dealing with ancient Egypt and its antiquities (22 articles), the ancient civilizations of Nubia and the south (5 articles), and the Mediterranean world (3 articles), although certain of the articles pertain to more than one area. The articles are arranged alphabetically.

Only three articles belong to the Aegean section of the title, and then only loosely. George M. A. Hanfmann and K. Patricia Erhart ('Pedimental Reliefs From a Mausoleum of the Persian Era at Sardis: A Funerary Meal', pp. 82–90) discuss two blocks decorated in high raised relief which formed the external pediment of a tomb of the late fifth century BC at Sardis. Depicted on the pediment is a funeral meal, the earliest use of the scene in this architectural context. The Hellenistic funeral meal is well known from Egypt in the Ptolemaic Period (see, for example, Klaus Parlasca, 'Hellenistische Grabreliefs aus Aegypten', *MDAIK* 31 (1975), 303–14, pls. 93–101) and is compatible with native Egyptian funerary tradition, if not directly related to it.

Cornelius Vermeule ('Bench and Table Supports: Roman Egypt and Beyond', pp. 180–92) presents a discussion of the decoration of stone (usually marble) support elements from furnishings of the Graeco-Roman world. In addition to describing the types of decoration which occur on such objects, he also provides an overview of ancient references to this art form. At the end of the article is a catalogue of supports, arranged typologically. Although not noted, the material of each entry in the catalogue is a variety of stone, unless otherwise specified. A wooden table (catalogue no. 23B)—from Egypt—gives a hint of lost parallels in less durable materials. While not used for tables, decorated supports are well known from other types of Egyptian furniture (e.g. chairs, beds, game boards).

The final article, the one most closely tied to the Aegean, is by Emily Vermeule ('Mycenaean Drawing, Amarna, and Egyptian Ostraka', pp. 193–9). Acknowledging many past hands in the study of Aegean–Egyptian artistic relations and the existence of Aegean influences in Egyptian art, the author goes on to explore the existence of some Egyptian influences in Aegean art. Primary Egyptian source materials are the art of the Amarna Period in general, and figured ostraca and glazed tiles of the New Kingdom, all aspects of Egyptian art which have a certain stylistic freedom. Certain of the motifs from Egypt become new—occasionally one time—elements in the vase painting of the eastern Mediterranean, but the exact manner by which these motifs were transmitted is a question not yet fully resolved.

Four of the five articles which pertain to the area south of Egypt proper—broadly the Sudan—are primarily related to the civilization of Meroe and its direct successors. The fifth article, by William Y. Adams ('Medieval Nubian Design Elements', pp. 1–10), is concerned with the stylistic innovations in design motifs of Nubian pottery of the Classic Christian Period beginning around AD 850. The pottery of this period was adorned with a wide variety of distinctive decorative bands, drawings of birds, fish, and animals, and medallion and centre stamps. The author surmises that this flowering of

Nubian pottery in the last two centuries of the first millenium AD was both part of a general artistic revival in the Old World, and a direct influence of refugee Christian Egyptian monks fleeing south from Abbasid rule.

The first of the articles on Meroe is by Fritz Hintze ('Die Groessen der Meroitischen Pyramiden', pp. 91-8). In this article, the author has applied various statistical analyses to the size of the numerous pyramids raised by the Kushite and Meroitic civilizations, using data arranged by social class of owner, date, and size from the cemeteries of Kurru, Nuri, Gebel Barkal, and Meroe. It is the author's hope that the tables thus generated be investigated in various regards. Only a brief comment about the tables is provided.

In an essay by L. P. Kirwan ('Aksum, Meroe, and the Ballana Civilization', pp. 115-19), the complex relationship between the three cultures mentioned in his title are examined. The information about the military campaigns against Meroe by the rulers of Aksum helps to establish the date towards the second half of the fourth century AD of the fall of the Meroitic kingdoms and the subsequent rise of the related Ballana Civilization.

P. L. Shinnie and R. J. Bradley ('The Murals from the Augustus Temple, Meroe', pp. 167-72) publish a group of four drawings presumably made by a Mr Schliephack and now in the Museum of Fine Arts, Boston. These drawings depict details of the remarkable, but now destroyed, murals from building M 292, the temple from which the famous bronze head of Augustus in the British Museum was recovered. This building with its murals was discovered by John Garstang in the winter of 1910-11, and Schliephack was the expedition photographer. According to the authors, the murals are only known from five unpublished photographs in Liverpool, and it is unfortunate that they did not choose to include either prints of those photographs or drawings of the murals based upon them in their article.

In the last article relating to the Sudan, Steffen Wenig ('Das Verhältnis von Wandrelief und Raumfunktion am Meroitischen Amuntempel von Naqa', pp. 201-10), following in the path established by Dieter Arnold with his study of decoration and room function in Egyptian temples, examines the Meroitic Temple of Amun at Naqa. He concludes that in this temple form descended from Pharaonic prototypes, there is a close relationship between the decoration of the portals and the original function of the rooms into which they lead. He also notes that the temple was not dedicated to Amun *per se* but to specific forms of Amun, one ram-headed and the other anthropoid. (In this article, fig. 1 is cited as coming from a publication by Caillard, but it clearly reproduces a photograph, probably from the Butana Expedition as are many of the other photographs in the article.)

The remainder of the essays in the volume pertain to ancient Egypt and to its monuments, including articles on history, architecture and archaeology, texts and lexicography, and inscribed or uninscribed objects or classes of objects.

Three articles pertain to Egyptian history. In a significant two-part article, Edward Brovarski ('Ahanakht of Bersheh and the Hare Nome in the First Intermediate Period', pp. 14-30) first sheds new light on the tomb (no. 5) of the nomarch Ahanakht at Bersheh, based upon records and photographs of the Boston-Harvard expedition which briefly worked at the site in 1915. Of particular note is a new translation of the biography of Ahanakht. In the second part of the article, Brovarski goes on to set in proper sequence and redate the nomarchs of the Hare Nome, who were buried at Bersheh and who flourished from the end of the Sixth Dynasty to the close of the Twelfth. Concomitant with this, Brovarski discusses the stylistic development of the Bersheh coffins. In particular, the nomarch Ahanakht is now dated to somewhere between the early Tenth and Eleventh Dynasties, and Nehri II would seem to begin the sequence of Twelfth Dynasty nomarchs.

In the second article, the late Labib Habachi ('New Light on the Neferhotep I Family', pp. 77-81) discusses the family relationships of the Thirteenth Dynasty king Neferhotep and his two brothers who succeeded him on the throne: the ephemeral Sihathor and Sobekhotep IV. Citing a statue from the sanctuary of Hekaib at Elephantine, the author resolves the parentage of Sihathor, but he misunderstands the title 'prince' (literally 'king's son') which is applied to the *Brothers of King Neferhotep I* and who are not *Sons of a king*. The genealogical table which Habachi provides (p. 81) introduces a second Sihathor as son of Sobekhotep IV. This prince does not exist. Habachi also suggests that the stela from the Wadi Hammamat of Sobekhotep IV supports a co-regency between Neferhotep I and that king (despite the fact that Sihathor reigned between the two, according to the Turin Canon). If there is any validity in the term *msc hrw*, 'justified', as an indication of being

deceased, on the Wadi Hammamat stela both Sihathor and Neferhetep I (as well as the three brothers' parents) are dead by year 9 of Sobekhotep IV.

Finally, Richard A Parker ('Some Reflections on the Lunar Dates of Thutmose III and Ramesses II', pp. 146-8) discusses the implications of certain lunar dates to New Kingdom chronology. For the first lunar date of Tuthmosis III, Parker continues to support the emendation of the date of the battle of Megiddo and the concomitant new moon on day 20, but he notes Kitchen's belief that either date is quite optional. As pointed out by Wente (*JNES* 34 (1975), 265) neither date is crucial in deciding the start of the reign of Tuthmosis III (although day 20 better fits a beginning in 1490 BC, while day 21 better fits 1504 or 1479 BC). For the second lunar date of Tuthmosis III, Parker still rejects the syntactic interpretation which would put the occurrence of the new moon in the foundation ceremony of the Akhmenu on III prt 1, as suggested by Wente, arguing that the ancient Egyptian would not have omitted the exact date. However, in a roughly contemporary text, the biography of Amenemheb (*Urk.* IV, 895-6), exactly what Parker objects to does occur: the death date of Tuthmosis III is noted, but the date of Amenhotep II's becoming sole ruler—on the next day—is merely implied. Presumably Parker still supports 1490 BC for the accession of Tuthmosis III. For the accession date of Ramesses II, Parker still leans towards 1290 BC but admits 1279 BC as a possible solution. Using Morris Bierbrier's comments on the destruction of Emar (in *JEA* 64 (1978), 136-7), Parker abandons 1304 BC. Parker goes on to suggest that this data may support better 1290 BC although further evidence is wanted. Since the thirteen-year range for the destruction of Emar by the Sea Peoples (as cited by Bierbrier following John A. Brinkman) is larger than the range between the two dates, 1290 and 1279 BC, this data cannot be used to decide between them.

Architecture and archaeology are represented by three essays. An article by I. E. S. Edwards ('The Air-channels of Chephren's Pyramids', pp. 55-7) discusses the unfinished air-channels of the Second Pyramid at Giza. He concludes that there were not technical difficulties which would have caused their abandonment and that they were abandoned as merely superfluous. In Edwards's view, the channels were intended as model corridors by which the deceased king might approach the northern and southern stars. Timothy Kendall ('An Unusual Rock-cut Tomb at Giza', pp. 104-14) publishes the Sixth Dynasty tomb (G 7721) of Kahertah at Giza. The tomb is noted for its twenty-eight statues (including one pair statue) in high relief carved upon its walls. The remainder of the decoration of the tomb is minimal, and the texts preserved are few. Kendall suggests that the exceptional plan of the tomb may be derived from that of the Fourth Dynasty tomb (LG 90) of Debehen, also at Giza. Finally, Peter Lacovara ('The Hearst Excavations at Deir el-Ballas: the Eighteenth Dynasty Town', pp. 120-4) gives some further information about the unpublished excavations conducted by Reisner at that site in 1900. The site dates to the end of the Seventeenth and the start of the Eighteenth Dynasties, and seems to be an artificial foundation, having either a military or residential purpose. Since publication of the article, Lacovara has undertaken new work at the site (see his 'Preliminary Report on the Deir el-Ballas Expedition, 1983', *ARCE Newsletter*, autumn 1983, 5-23).

As is not unsurprising given Dunham's interests, articles dealing with Egyptian texts and lexicography *per se* are few. Susan K. Doll ('The Day Hour Texts on the Sarcophagi of Anlamani and Aspelta', pp. 43-54) provides parallel texts in hand-copy, translations, and commentary for a group of texts which appear on the lids of two royal sarcophagi discovered at Nuri. The texts pertain to the twelve hours of the day, and the female deities associated with each hour are named. According to the author, these texts may derive from a parent document which is also the source for the Book of the Day. Del Nord ('The Term *hnr*: "Harem" or "Musical Performer"?', pp. 137-45) concludes that in at least the Old and Middle Kingdoms, there is no sexual connotation to the term *hnr*, and it is better understood as a 'group of musical performers'. (One wonders if the English term *chantry* might not be appropriate.) Finally, Louis V. Žabkar ('A New Administrative Title on the Sealings from Semna South', pp. 211-13) notes the existence during the Middle Kingdom of a *hry-tn(w)*, a 'master of counting', at the fortress of Semneh South, called Dair-Sety, 'Controlling the Nubians', and suggests that the office which the title defines seems also to have functioned at Semneh West and perhaps at Buhen. In a subsequent article in *GM* 54 (1982), 77-80, Žabkar reverses his position taken in the article and adopts a reading which he had earlier rejected. In this new article, he takes the seal to read *hrt n*, 'the upper part of' (the fortress) Dair-Sety.

Nearly half of the essays in the volume deal with miscellaneous objects or classes of objects from

ancient Egypt. Of the objects discussed, a number are of interest primarily because of the texts which they bear while others are anepigraphic or their texts of secondary importance.

Cyril Aldred ('An Unconsidered Trifle', pp. 11–13) publishes a fragmentary quartz diorite bust of a queen now in Boston (MFA 52.347). It is part of a pair statue in which the queen (originally on the viewer's left) is shown on the same scale as her partner, and the author plausibly suggests that the statue originally depicted Hatshepsut as queen with her husband Tuthmosis II. Whitney Davis ('An Early Dynastic Lion in the Museum of Fine Arts', pp. 34–42) discusses a recent addition to the Boston collection (MFA 1980.73): the front part of a porphyritic stone lion of early date. The object, thought to come from Gebelin, is one of a number of rare early sculptures not intended for a funerary purpose. The piece in question is one of the earliest of its type to encompass both monumentality and the block form. Rita E. Freed ('A Private Stela from Naga ed-Der and Relief Style of the Reign of Amenemhet I', pp. 68–76) uses the stela of Sa-Inheret, also in Boston (MFA 25.659), to develop the thesis that there is a stylistic consistency to objects dated to the reign of Amenemhet I and that these were widespread through Egypt. The style changed with the accession to sole rule of his son Sesostri I.

Lynn Holden ('An Anubis Figure in the Museum of Fine Arts', pp. 99–103) discusses two fragments of a recumbent jackal, now too in Boston (MFA 11.721). The piece originally comes from George A. Reisner's excavations in the Valley Temple of Mycerinus at Giza and is said to be the oldest surviving, fully three-dimensional representation of a jackal. Its most curious feature is that its eyes are not those of a jackal, but those of a human. The forward portion of the animal, with the exception of part of the head, is lost. The base and the lower part of the tail are also missing and were probably carved as a single, separate piece. An examination of the unfinished sides of the present base would seem to indicate that the entire statue was intended to fit into a socket upon the upper surface of the base. While the author does not cite for comparison the numerous wooden representations of the recumbent Anubis of later periods, the great Anubis from the tomb of Tutankhamun would probably serve as a suitable mode for a full restoration.

Winifred Needler ('A Wooden Statuette of the Late Middle Kingdom', pp. 132–6) publishes a small statuette in Toronto (ROM 958.221) inscribed for the District-Councillor Ibyref. On stylistic and textual grounds, she dates the piece to the close of the Twelfth Dynasty. Edna R. Russmann ('An Egyptian Royal Statuette of the Eighth Century BC', pp. 149–54) discusses a small bronze figurine from Boston (MFA 1977.16), once a part of a larger grouping, inscribed with the royal name Neferkare. She is able on stylistic ground to establish its owner as Neferkare Pefthawybast, a ruler of Herakleopolis just before and at the start of the Twenty-fifth Dynasty. In a separate appendix Lambertus Van Zelst ('Egyptian Bronze Kneeling Figure', pp. 155–6) provides a technical study of the piece and analyses its metallic composition.

Three classes of objects—all anepigraphic—are also discussed. John D. Cooney ('Notes on Egyptian Glass', pp. 31–3) discuss a number of small glass artefacts usually called ingots (Freer 09.856, MMA 18.2.12 and 26.7.1162, and Victoria and Albert C10-1946) and concludes that they are really colonettes from a frieze in the Mosque of Sultan el Muaiyad in Cairo (with V & A 364-1900 and BM 67068-70) or else pieces made to repair that frieze in the late nineteenth century. In the article, Cooney also adds to his catalogue of glass in the British Museum, no. 37496, a shrine of wood inlaid with glass in the name of Darius I; and to his list of glass sculpture, a Nineteenth Dynasty representation of a serpent on a stand. Peter Der Manuelian ('Notes on the So-called Turned Stools of the New Kingdom', pp. 125–8) makes the case that the 'turned' legs of the familiar stools which they designate do not show any evidence of being fashioned upon a lathe (and observes that lathe turning was not introduced into Egypt until the Late Period). For this reason, the author suggests that this stool type which flourished during the Eighteenth and Nineteenth Dynasties should be renamed 'flared-leg' stools after the distinctive profile of the legs. Finally, N. B. Millet ('The Reserved Heads of the Old Kingdom', pp. 129–31) discusses the various features common to all reserve heads and suggests that they could all be accounted for if the reserve heads were in fact sculptors' models. This essay seems to be the result of a seminar conducted by the author some years ago.

Three articles publish objects in which the texts upon the pieces are of primary interest. Henry G. Fischer ('Three Stelae from Naga ed-Deir', pp. 58–67) publishes for the first time two stela: that of the overseer of the herd in Shayt, Shemay and his wife Sat-net-Inheret (presently owned by Paul

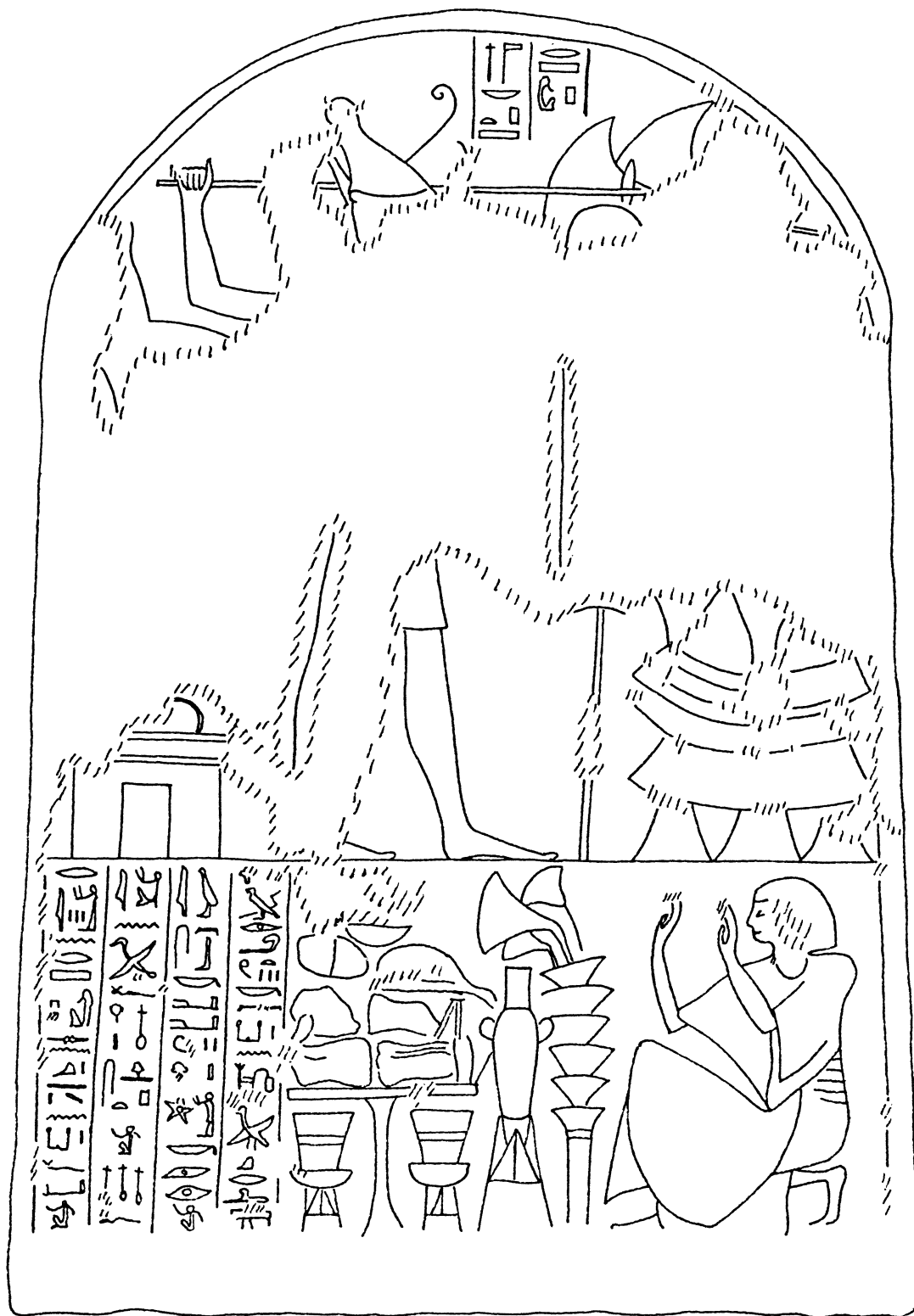


FIG. 1. Stela showing the scribe Pakeri adoring Reshep (Memphis Find Nr. M-2792).

Steiner), and that of the Count Iti and his wife Shepset (now in the National Gallery of Victoria, Melbourne, Australia). In addition, the author republishes a third stela long since known, that of the Count Idu and his wife Meret-yotef (BM 1059). All three stelae date to the First Intermediate Period, but the author declines to be more precise about them.

In the second article, William Kelly Simpson ('The Memphis Epistolary Formula on a Jar Stand of the First Intermediate Period from Naga ed-Deir', pp. 173–80) publishes an object from Sheik Farag Tomb 200 which is now in Boston (MFA 13.3791). According to Simpson, it is possible that this piece belongs to the class of texts known as Letters to the Dead, but the crucial text to so assign it is damaged or missing. The part of the text which is well preserved is the standardized opening to a letter. The object as preserved is restored in two parts, physically unconnected, although they may be aligned with reasonable certainty. It would seem that rather more space should be allowed between the two fragments than appears in the transcription and facsimile. Such additional space would allow the full text of columns 1 and 2 to be inserted as appropriate in columns 6 and 7 in the break between the two parts. Similarly, the text at the top of line 7 would complete column 1. Judging from the photograph, there is just room for the expected *k* after the *nh* and before *r* at the end of column 2. In the transcription, the *r* of *hr* in column 7 should not be in brackets. The texts of columns 1 to 4 and of columns 6 (from the broad stroke at the base of the upper part) to 9 would seem to be identical, allowing for variant columnar distribution.

Finally, Alan R. Schulman ('Reshep Times Two', pp. 157–66) publishes, along with a discussion of the various aspects of cult images of Reshep, a pair of stelae excavated at Memphis by the University Museum. This article is the third part of the author's ongoing study of the god Reshep. The first stela (field no. M-2792) is said to belong to an unidentified man with the title *ss qwr*, 'scribe of goldminers'. According to Schulman, the text in the bottom register is arranged in four tall columns and one short column. The title which he reads (omitting the *p*-bird) is at the bottom of column 4 and the name, lost except for the determinative, is supposedly in column 5. Based on an examination of the photograph published in the article, there is no fifth column; the shape which he takes to be the determinative is too far to the right to be a sign in the supposed column. What is preserved at the bottom of column 4 after the word scribe is the name Pakeri (*p* *qri*), 'the storm-cloud', as noted by Schulman, a name essentially identified by Lesko and Černý. Seemingly, this name is a variant of the attested name *qri* (see Ranke, *PN* 1, 336, 1). The problematic determinatives at the end of column 4 (PM III², 861, also had difficulty with the signs) are the recumbent Seth animal (Gardiner, Sign List E 21) and the raining *pt*-sign (Sign List N 4); both signs are associated with the word *qri*, storm-cloud. The photograph of the stela is quite good, but because of the damage to the stela, much is not clear. Figure 1 is a line-drawing of the traces which I distinguish. The object immediately below the right arm of Reshep does not seem to be the simple flower identified by Schulman. There are some errors in the transliteration of the text on page 159. Column 3 begins *iw snb·k*; col. 4 ends *p* *qwr*—if one follows Schulman. As argued by Schulman, the opening word in col. 1 may be *ir*, but paleographically the sign is *r*. Perhaps this is a defective writing of *rdi*, the verb often expected here.

The second stela (Philadelphia E 13620), is attributed by the author to another unnamed individual, supposedly with the simple title *qwr*, 'gold-worker'. Only the end of this title is preserved in the column immediately to the left of the head of the owner of the stela, but given similar traces in the lower register of the same stela, the broken hieroglyphs might as easily fit a name such as Pentaweret. It would be highly surprising to find a *qwr* offering a stela at Memphis. While it is clear that gold-workers were associated with the Temple of Ptah at Memphis (e.g. the overseers of gold-workers Pareemheb and Pahesy, cf. Janine Bourriau, *JEA* 68 (1982), 56–9, with further references), the *qwr*, according to Jean Vercoutter (*Kush* 7 (1959), 143–4), were rather low-level workmen associated with the production of raw gold in the desert. It seems doubtful that such men would have left stelae at Memphis.

The contribution which Dows Dunham made to Egyptology is marked by his diligence in rescuing from neglect the work of George A. Reisner in Egypt and in Nubia, and in his own work on materials from Naga ed-Deir and Giza; an interest which spanned the temporal and spatial breadths of Egyptian civilization. The articles of this volume emphasize the scope of Dunham's broad interests and continue the lines of research in which he had an interest. The volume is a significant collection of essays worthy of the man for whom it was intended.

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Financial and Administrative Documents from Roman Egypt. By C. A. NELSON. Ägyptische Urkunden aus den Staatlichen Museen Berlin. Griechische Urkunden, XV Band. 250 × 175 mm. Pp. xiv + 230, 2 fiches. Berlin, Staatliche Museen Preussischer Kulturbesitz, 1983. ISBN 3 88609 301 8. No price stated.

This volume extends the famous series of editions of Greek papyrus documents from Berlin. As both series- and volume-title imply, its contents are entirely in Greek and documentary. The volume opens with some more substantial 'official' documents, but the bulk of it (nos. 2497–556) is occupied with various kinds of tax receipts, mostly from the Arsinoite nome and the second century AD. There are no Ptolemaic texts, and almost nothing later than the third century. The texts are arranged by subject, not chronologically.

A number of published texts receive re-editions here: P. Turner 42 = 2459, P. Yout. 1, 23 = 2487 and 53 = 2492. *BASP* 12 (1975), 79–81 = 2463; *ibid.* 81–4 = 2474. *MPL* 2 (1977), 233–43 = 2468; *ibid.* 244–50 = 2547. *CE* 50 (1976), 121–9 = 2500, and *Hellenika* 32 (1980), 351 = 2555. 2458 is a duplicate of BGU 1, 4. 2472 extends BGU 1, 239. BGU 11, 432 is partly re-edited together with 2467; BGU 11, 434 is re-edited in the introduction to 2530. 2545 extends BGU XIII, 2285. There are suggested readings for P. Grenf. 11, 58 (p. 181 n. 1); P. Yout. 1, 38 (2525 int.).

Among officials gleaned from this volume are several strategi of the Heracleides division of the Arsinoite nome: Hierax in 159/60 (2472: = BGU 1, 239 extended); possibly Fl. Potamon (known 171–5) (2461, undated); Dioscorus in 190 (2467), and Hierax *alias* Nemesion (known c.195–7) from the undated 2463 (already published in *BASP* 12 (1975), 80). For the Theban Diopolite nome 2465 (1 June 108) gives us two new names: Hermaeus, strategus, and Chaeremon, ex-basilicogrammateus. Also new is Paniscus, ex-basilicogrammateus of Perithebas (2466: 11 November 49). Prefects mentioned are Sulpicus Similis (2465: 1 June 108) and Tineius Demetrius (Mesore 190, in 2467). Ulpus Serenianus, *archiereus*, occurs in 2470 (192/3). There are two *archidicastae*: Callinicus (2473, from Trajan's reign not earlier than 103, as *Dacicus* indicates) and (mostly restored) Achilles *alias* Herodianus from 2472 (159/60). 2550 gives us Apion, nomarch of the Arsinoite (10 March 195).

2458 is a petition to a centurion about a deposit. This text is a duplicate of BGU 1, 4 and the old and the new supplement each other usefully.

2459 re-edits P. Turner 42, a petition to officials ἐπὶ τῆς εἰρήνης.

2460 is the end of a second-century petition to an unknown prefect. The construction in 4 ff. may admit analysis as the transmission of orders along a series of officials, with γράψαι (restored) . . . συντάξαι comparing ἀξιῶ ὑμᾶς συντάξαι τοῖς κτλ. εἰπεῖν οἷς καθήκει, *JJP* 18 (1974), 178. If this is right, a passive infinitive will be more likely in 6 than ἀποδοῦναι. In 1 n. the lemma does not match the text.

2461 is a fragment of a petition about a burglary.

2462 is another damaged petition. The last line of the transcript is strikingly printed upside down. The quality of the photograph of this item (the plates for the volume are supplied in the form of two microfiches) is not good enough to tell whether this is really so. Line 8 has the extraordinary phrase ἀποκάρπωσιν νεκρῶν. αὐτῆς τ. [does not look to me a likely reading of what follows.

2463–4 are two more petition fragments.

In 2465 the prefect instructs a strategus to pay the salary of an ex-basilicogrammateus, less deductions. This is welcome confirmation of the salaried nature of the post of basilicogrammateus.

2466 is a fragment of an official letter referring to the district Περὶ Θήβας.

2467 has correspondence between the prefect and a strategus about the embezzlement of government grain.

2468 is from a fishing report, already published elsewhere.

2469 relates to priests and their activities, with several interesting titles and other words (partly listed at the end of this review). 2470 also relates to priests.

2471 is a notification to the γραμματεῖς μητροπόλεως by a weaver claiming a certain person as his *συνεργός*. The weaver is given no less than four occupation titles, λινόϋφος, ὀρθόϋφος, ἰστανάρης, and λινειφός. In line 2a λινόϋθος is a misprint. For ὀρθόϋφος—a weaver who worked standing at a vertical loom—the editor should have referred to H. C. Youtie, *ZPE* 35 (1979), 112.

2472 and 2473 both relate to security in contracts of loan or the like. The editor supposes that the two may derive from the same τόμος, yet they are sixty years apart. In 2472, regarding permission to

execute a pledge, the procedure is outlined in the text (especially 12–14) much more crisply than in the introduction (p. 33). 2473 is dated ‘about 100 A.D.’ but it cannot be earlier than 103 because of the use of *Dacicus* (line 8). The transcript gives the article in a number of places (e.g. 5 *παρὰ τοῦ NN*) where it would be contrary to normal usage. In 7 (and 8) the editor prints *Φαρμ<οῦ>θι* but this is a wrong approach: the word exhibits the phenomenon known as *Verschleifung* and the letters are not to be considered as lost. *ϰ* look puzzling after the day of the month in 8; I can discern nothing from the photograph. The note on the archidicastes here (5) is primitive compared with the more recent references in 2472 8 n. A Callinicus already appears in the lists, and has indeed long been known, namely Valerius Callinicus from P. Oxy. III, 471, but the lists place him later and dated ‘first half of the second century’. However, the Maximus in that text is now considered to be the well-known prefect C. Vibius Maximus (see G. Bastianini, *ZPE* 17 (1975), 280) who was in office precisely from 103 which (see above) is the earliest possible year to which 2473 can be assigned.

2474, perhaps an athlete’s appeal against nomination to liturgical irrigation work, was already published in 1975. The bibliography giving N. Lewis’s work on exemption from liturgies needs updating.

2475 seems to me to be misunderstood by its editor. If *τοὺς προγεγραμμένους* is right in 4, then this is surely a sworn guarantee by persons not named here that *other* persons will attend to some cultivation. The editor’s translation accords with this interpretation, but not his introduction. The format is odd: the upper margin is substantial (and obvious on the photograph), and all I can suppose is that the document stretched on to this second column which we have here. The abbreviation printed at the end of 4 is far from clear and is a hindrance to seeing the sense between 4 and 5, and is better discarded (the editor’s 5 n. ignores it).

2476 is a contract relating to an inheritance; the eldest son (aged 22?) accepts cash now in his father’s lifetime, and gives up his rights to a share in the family house which will now pass to his brothers only.

2477 is a fragment from a sale of a millstone with its accoutrements. There is a useful introduction and bibliography to which P. Oxy. LI, 3639, 10–11 n. should be added. In 3 I wonder whether *Παύσει* lurks behind the unpromising-looking *πατρ()*. In 7 ff. the second hand is much bigger which explains some of the differences in line length, but the irregularities within the second hand are still too great (contrast 9 and 10) and the text will not do as it stands. App. crit. 12, read *ἀπέχω* of course.

2478 is a sale of part of a house, with addenda to the lists of such documents.

2479–80 are donkey sales, with good bibliography, but the second-century prices listed under 2480 include two first-century items. In 2479 11 (*ἔτους*) looks very unconventional. Apart from my wondering why the omicron escapes being dotted, the abbreviation is clear and no indications of doubt are needed. On the photograph I cannot see the tops of two letters, from the year number, to which the editor’s note refers. If there are truly two letters, this restricts the date to 107/8 or later; in any case *Δακικοῦ* (13) means the text cannot be earlier than the end of 102. The editor (11 n.) suggests ‘epsilon joined to another letter’: in the context of Trajan, that could only be year 15 = 111/12. The opening layout of 2480 looks odd, as printed, but only requires closing up of the final bracket in 1; initial *ε* is enlarged and 2 is written in eisthesis, and the line lengths will do as printed. The day (4) converts as 28, not 27 February. More unconventional dots-within-brackets make their appearance in 11. A number of features in the transcript from 20 on make me uneasy. ‘*Αρπᾶς* (20) looks ill fitted to the space. ‘2H’ in 22 should rather precede the name of Pasion’s son. *πέπ[(20)* may be *ε. .[*. In 23 one expects a name to begin a *ὑπογραφεύς*, declaration, so that *ἔγ[ραψα]* (a very short supplement) is unlikely to be right. Read *E.[? 25 init.*, is *λω* possibly *των*, from *ὑπὲρ αὐ-]* *τῶν μὴ εἰδῶ(των) γράμματα*? In 28 if *Νικ()* is the person written for, *ὑπέρ* is wanted before his name.

2481 is a sale of various movables: there are some problems with the objects of the transfer. There is a red *γραφεῖον*-stamp on the back. I think the editor takes *εἰς λόγ[ον, 20*, too physically: simply a notional credit is more likely. I cannot believe in *Τω[(22)* as the name of the *ὑπογραφεύς*. Initial *θ* in 23 is perhaps just a decorated paragraphus.

2482 is a sale of unspecified type. With traces above 1, the lines should have been differently numbered. Punctuate before (*ἔτους*) in 9.

2483 is an agreed offer to harvest an olive crop in return for supplying the property owner with a proportion of the produce. The cultivation of olives in Egypt was less widespread than is sometimes

believed: *ἐλαιουργοί* used vegetable seed as their raw material. 2483, however, refers unequivocally to olives.

2484 is an application to lease a palm grove, in return for paying the taxes on the land. In 14 the papyrus has *ἐπί*, not *ἐκί*. 2485 is a lease of a garden (for growing vegetables?).

2486 is a payment through a bank to two carpenters for a wooden component of an oil mill. Translate *τοῖς δυοῖ* (8) 'both of them', not 'the two'. In 10 *.ηχ[* may be *πηχ[* (αἰοῦ), the object being of acacia wood and a cubit in length (or other measurement). *επανωτρίδου* (-δος?) in 9 looks as if it might rather be all one technical term, but its identity if so remains elusive.

2487 re-edits P. Yout. 1, 23. Expand *κατακεχώ(ρικα)* in 12, not -*ρηκα*.

2488 is an *ἀβροχία*-declaration, with an updated bibliography.

Both 2489-90 refer to declarations of livestock (sheep and goats); there is a good bibliography attached to 2489. 2490 14 appears to attest a new word, *διαμικθωτής*, but re-articulating (-]των *διὰ μικθωτῶν Φλαυίου*) allows us to avoid this. I am not sure that the notations of sheep/goats and their numbers have not been added in to the register by another hand.

In the account 2491 the name in 19 (not 20 as the introduction) should be Valeria Thaisarion, not Valeria daughter of Thaisarion, as the index to P. Mich. iv makes clear.

2492, a soldier's letter, republishes P. Yout. 1, 53 and adds subsequent bibliography. Delete the stop at the end of 16. The translation prints *cohors . . . Afrorum* (16-17), without explanation.

2493-6 are private letters. In 2493 23 I think the papyrus actually has *ταχύτατα* (*ταρότατα* ed.), the upper left arm of *χ* being lost. The Alexandrian provenance of letters with *Sarapis-προσκύνημα* (2494 introduction and 3-4 n.) has not gone unchallenged: F. Farid, *Actes XV^e Congr. int. de Pap.* iv, 141-7. In 10 perhaps *ἐξοδ]ιάσωσιν*. Something is amiss with the brackets on the back, but there is no photograph on which to check; *ἀδεδφῶ* is presumably a printer's error.

From 2497 the volume is almost entirely composed of tax-receipts. 2497-502 are receipts for beer tax, *ζυτηρά*. Supplement-lengths in 2497 are suspiciously uneven. Delete the bracket at the end of 12. In 2502 1 the final lacuna has to be wider. The line numbering is erroneous; 10 has been omitted, and 15 is misplaced.

2503-9 are receipts for crown tax, *στεφανικόν*. 2507 i 13 has more unconventional method in (*δραχμάς*). In column ii 3 transcript and note are inconsistent. In 2508 2 restore *Ἀλεξάνδρου*, not *Ἀντωνίνου* (cf. 1-2 n., inconsistent with a restored text); the translation too is inconsistent. There is no subject for *διέγρ(αψεν)*, 3. The 'recto' of 2509 is in a good 'official' cursive. The scanty remains suggest a circular letter of the procurator *πρὸς ταῖς ἐπισκέψεις* to the *strategi* of the Heptanomia and Arsinoite. Perhaps *δη]μοσία* in 7, and part of *προτίθημι*, referring to public posting.

2510-15 are receipts for *χωματικόν*, dike tax. In 2510 ii 5 the papyrus perhaps has *ἄλλος* rather than *αὐτός*. *μη(τρός)* ends the line, I think; move *τῆς αὐτῆς* to the beginning of 6? The first visible trace looks like *δ*, certainly, but the area is much damaged. In 2511 the possible years are 177-8, since Thoth in 176 would precede the joint rule of Marcus Aurelius and Commodus. But can]*ωθ* in 2 only be Thoth? Line 8 and its lemma in 1 n. are not consistent. Line 6 has yet more unconventional dots. The translation of *ῥυπαραὶ δραχμαί* as 'debased silver drachmas' is not currently accepted doctrine. The editor returns to this topic under no. 2554 with an inconsistent and confusing note (on line 6). 2512 12, the second (*ὀβολόν*) should be (*δβολός*). The date of 2514 or rather its dates pose problems. Year 16 of *Αὐτοκρατόρων* (line 7) should not refer to Marcus Aurelius and Commodus (despite the editor's note) whose joint reign began in year 17. The editor does not consider the possibility of 1 Avidius Cassius (= 175) for the date in 1-2. The year after that was the 16th, but of Marcus Aurelius only.

2516-19 are penthemeros certificates. The layout of 2519 has gone wrong. Correct is

7 Πατρ(ώντεως) Καρανίδος
8 Φανομ(γέυς) Πασοκν(οπαίου) τοῦ
9 Φανομ(γέως)
10 Τασώκ(εως).

2520 has accumulated receipts for payments of the *δίπλωμα*-tax, elsewhere associated with donkeys. Page 126, paragraph 3, for 'line 5' read 'line 3'.

2521-4 are receipts for various garden taxes: *ἐκτη παραδείσου*, *ἐλαίας παραγωγή*, *ναύβιον ἐναφεσίων*, and *ἐπαρούριον* occur, and also *γεωμετρία* and *ναύβιον κατοίκων*. The note on *προδιαγραφόμενα* (2521 6)

refers to odd amounts of chalkoi being rounded off to the nearest amount divisible by two, and cites three examples where the rounding off is to the further away of the two nearest amounts divisible by two! In 2521 7 if 'Α[ρ]ποκρ() is satisfactory, there are more attractive ways to expand it than 'Αρποκρατίω. 2523 verso 6 offers [C]τατιανός or [T]ατιανός, but these readings are inconsistent. In 2524 1 the imperial formula proposed is unrecorded. Given what little is left, a date by Commodus alone is possible. The twentieth year is the latest to survive in the document, according to line 6, but may not actually be the date of the text; the editor, it may be noted, puts a twenty-first year in line 7. Initial supplements are uneven.

2525-9 are receipts for ναύβιον κατοίκων, ἀριθμητικὸν κατοίκων, and other taxes. 2525 has four columns, each beginning with a date under Domitian (year 3 or 4). The transcript shows several elements as abbreviated. I should prefer to explain them as examples of *Verschleifung* and print them in full. The month Γερμ() in columns iii-iv has given the editor trouble. He expands it as *Germaniceus* = Thoth. However, *Germanicus* = Thoth; *Germaniceus* = Pachon. Although *Germanicus* was a creation of Domitian, the emperor here, when K. Scott wrote on Roman honorific months (= YCS 2) no example of it was known earlier than AD 89, much later than the present text. Cf. P. Turner 18 introduction. Columns i-ii are dated in Pharmouthi (in the third year) which suggests to me that the next month Pachon (therefore *Germaniceus*) may be a more appropriate moment for payment the following year. 2526 3-4 n., read 305-600, not 305-500; in 8 n., end of first paragraph, the reference should be 3-4 n., not 4 n. 2528 7, the imperial formula should include 'Αδριανού and the translation be adjusted accordingly. C. Mevius (line 4) surely ought to have a cognomen. 2529 7-8 n., last sentence, read 'Antoninus' 24th', not 'Antoninus' 4th'.

2530-1 are receipts for poll-tax. 2530 refers to λαογρ() χρυσοχ(), to be precise, extensively commented on by the editor. The transcribed διαγεγράφ(ηκε) (1) is not of course the normal perfect. There is a discussion of this form in O. Leid. 178. 1 n., from which one may conclude that the normal form of the perfect will have been intended in 2530. Despite tempting physical similarities, I wonder whether the two halves of 2531 do belong as closely as the transcript would have it. There are striking differences: payments in the upper chunk are on the thirtieth of each month (except in Thoth), with frequent hand changes. The pattern changes in the lower chunk (which does not include any of the date in 12-13, it may be noted), with more random payments, more than once in some months, *ὁμοίως* used frequently, and no hand changes. Θῶθ in 22 takes us into the following year, unless this is a second payment in Mesore. The app. crit. contains deceptive renderings of abbreviations for (δραχμαί). . .]υς in 14 looks difficult as a genitive singular personal name.

2532 is a receipt for taxes under the supervision of a nomarch, paid to the πράκτορες νομαρχικῶν. The year can be given in the heading as AD 222/3. There is a payment in nearly every month, probably totalling over 100 drachmas.

2533-40 are receipts for *syntaximon* and other charges, that is to say a bumper dues bundle covering poll tax and a whole assortment of minor imposts. For the *syntaximon*, see also 2531 introduction. The other taxes attested here are ὑϊκή, ὑπὲρ φυλάκων, ὑπὲρ μαγδωλοφυλάκων, ὑπὲρ δεσμοφυλάκων, ὑπὲρ ποταμοφυλακίδος, φύλ(ακτρον?) ἀπόρων, ὑπὲρ διπλῶν, and μερισμός οἱ ἐπιμερισμός ἀπόρων. In 2536 the commentary leaves the date and form of the imperial formula open, but the transcript is not so modest. In line 2 the tax collector's name will surely be Ὁρω, by the laws of syllabification. In 2537 app. crit. 4 the representation is not accurate; note to line 1, Antoninus' sixth year was 142/3; note to line 17, the reference at the end should be to lines 7-8. In 2539 app. crit. again some of the representations are deceptive. The line numbering of the text has gone wrong here (notes and apparatus are correct): 5 should appear opposite the line with ἐπ]ι(μερισμοῦ), 10 opposite the line beginning [πράκ(τορω). In 2540 the commentary (lines 10 and 12) could confuse the unwary, implying both AD 155 and 156 were leap years. In truth, of course, the Egyptians added an intercalary day in August 155 which throws the reckoning out by a day (when converting to Julian dates) until the inclusion of 29 February 156.

2541 is a tax receipt, not very informative. There is no justification for giving the date in the heading as AD 205, even with the addition of (?). From the photograph it does not look as if the app. crit. shows the true ductus of (γίνονται) (δραχμαί).

More tax receipts follow: 2542 for τέλεσμα καμήλων, 2543 for κύμβολα καμήλου (the discussion extends to ἐρημοφυλακία and παρόδιον; the bibliography in 2 n. should include Calderini's *Diz. geogr.*), 2544 for μερισμός τῶν θηρίων. (Καί)[σαρο]ς in 5 looks very odd: I do not think that either here or in 3 the

double curve should be understood as an abbreviation. 2545 attests *μονοδεσμία χόρτου*, 2546 *παραγραφή έλαιουργίου(?)* (the abbreviation for *(δραχμαί)*, 10 app. crit., looks suspiciously deceptive).

2547 attests a payment for *χειρωνάξιον* to the *μισθωται κοπήσ τριχός και χειρωναξιόν*; 2548 a payment by a slave to the *μισθωται γερδιών. φόρος προβάτων, φόρος νομών*, and *φόρετρον* feature in 2549–50, and 2551 also concerns grazing rights. In 2549 the abbreviations shown for *(δραχμαί)* are not accurate enough and are therefore deceptive. In 3–4 n. the bibliography on *ούςία* should include G. M. Parássoglou, *Imperial Estates in Roman Egypt* (= *ASP* xviii).

2552–4 are receipts for rent: *έκφόρια* in kind (2552: again an implausible-looking abbreviation for *δραχμαί*), *φόρος* in money (2553), a money rental for uncertainly identified property (2554). The last line of the docket on the back of 2554 is baffling; I can offer no solution. 2555 is a receipt for a quantity of grain for an unspecified purpose. The grain was paid in October from the crop of the immediately preceding summer; this of course was the freshest grain available.

2556 is a sitologus receipt. The imperial date formula has two oddities: the form *Λουγίου* (unless this can be read *Λουκίου* as seems just a possibility from the photograph) and the omission of *Σεπτιμίον* from the titles for Geta, for which no parallels are signalled in Bureth, *Les Titulatures impériales*. Lines 1–4 are transcribed with ecthesis but this is a mistake.

Finally, 2557 preserves just part of the dating formula (Trajan) from the top of a private contract. The editor restores *Δακικου*; if it were there, the document ('98–117 A.D.') cannot antedate June 103.

There are useful notes on the family of C. Julius Apolinarius (2461 3 n.); the *damnatio memoriae* of Geta (2520 2–3 n.); Pescennius Niger (2545 1–2 n.); *ούςία* (Germanicus, 2549 3–4 n.; Antonia Drusi, 2554 6 n.); Karanis kleruchies (2554 5 n.); *άγελικός* (2460 5 n.); *άνύπαρκτος* (2490 6 n.); *γενηματογραφείν* (2488 1 n.); *διεκβολή* (2486 1 n.); *κνήκος* (2484 4 n.); *πρόσθεσις/πρόστασις* (2467 24b n.); *προφήτης* (2469 i 1 n.; add P. Oxy. LI 3646, 19–22 n. to the references); *πρωτοιερέυς* (2469 ii 8 n.); *ραντής* (2469 i 10 n.); *καλάριον* (2465 3 n.); *στολισταιί* (2469 i 7 n.); *ωτήρ* used of the prefect of Egypt (2460 3 n.); *ύποδοχείον* (2485 19–20 n.); *φασήλια* and prices (2496 introduction).

The usual indexes close the volume. There is an extra one at the end, of symbols; the attentive reader of this review will realize that I advise taking many of these with a generous pinch of salt. Text and index disagree over the form of the abbreviation in 2541 3.

The editor has tried hard with a lot of very difficult cursive texts. A little more diligent checking would have removed most of my foregoing criticisms. But this is carping, set against the solid accessions to our knowledge. We look forward to more.

REVEL COLES

Re und Amun. Die Krise des polytheistischen Weltbilds im Ägypten der 18.—20. Dynastie. By JAN ASSMANN. Orbis Biblicus et Orientalis, 51. 235 × 155 mm. Pp. xi + 309. Freiburg, Universitätsverlag, and Göttingen, Vandenhoeck & Ruprecht, 1983. DM 89.

The author's *Sonnenhymnen in thebanischen Gräbern* (Mainz, 1983) constitutes the basic material for this work of interpretation, which is devoted, accordingly, to writings composed in the second half of the second millennium BC. It is assumed that the textual corpus will be in the reader's hand—not an easy concomitant, in the literal sense, for the corpus is an unusually hefty volume. If the relationship is close, it leads to very little repetition since Professor Assmann is here concerned with the general theological approach rather than with the detailed commentary of the other work.

During the three centuries between 1500 and 1200 BC many hundreds of sun-hymns were written and their great abundance might in itself suggest a routine-like activity with more than a hint of mass production arising from funerary demands. Assmann has dealt with facets of these writings in many previous studies over twenty years and with rare modesty he has arranged that his references to his own publications usually omit the author's name. It is his conviction that, apart from the much reproduced 'Standard Texts', there are numerous compositions which avoid the impression of a mechanistic approach and give expression rather to an individual viewpoint not unlike that of the writings usually grouped under the rubric of 'Personal Piety'. He finds the central concern of these sun-hymns to be with the struggle for the conceptual articulation of the unity of the divine, that is, of the One God. He reminds us that the Amarna 'revolution from above' made a drastic attempt to establish the One God in defiance of the traditional polytheism. If the attempt ended in failure, the

sequel did not mean an end of the problem. It is shown, on the contrary, that the debate continued vigorously; and the importance of the hymns deriving from tombs is that they are datable and can be related to defined localities and social strata. At the same time other sources are taken cognizance of: stelae, statues, the Book of the Dead, liturgical and literary manuscripts, and temple inscriptions. A convenient selection, in translation, is found in Assmann's admirable *Ägyptische Hymnen und Gebete*, also assumed to be in the reader's hand—in this case an easy physical possibility. 'Productive' and 'reproductive' phases are distinguished in the literary tradition of such texts. Sacred texts tend to belong inevitably to the second category; yet the process of establishing a canon leads to the call for another process, that of philological and theological hermeneutic which is the task of a specialist elite; and here the Hindu Brahmans and the Rabbinate of Judaism are compared with the scribes and lector-priests of the House of Life in Egypt. In Egypt a contrast is seen *vis-à-vis* the other traditions, in that commentary on the sacred texts becomes a necessary accompaniment of canonization. A similar process, and a more elaborate one, if phased rather later, can surely be traced in Judaism, with Midrash and Mishnah commenting on both the written and oral Torah.

The sun-hymns are divided into three groups: (1) texts of an esoteric and specialist source; (2) standard texts; (3) individual hymns which stand apart from the traditional character of the first two groups in that they are positively formulated and seek to present new theological ideas. Assmann is here, as throughout, eager to define his categories, whether literary or linguistic. He describes 'standard texts' as those which stemmed from the exoteric (as opposed to esoteric) aspect of the cult and which thus enabled some participation by the laity. An oral tradition, he thinks, must have played a part and this emphasis differs from that of H. M. Stewart's 'traditional sun hymns', where the criterion is simply frequent occurrence—a criterion which allows the term 'traditional' to be applied even to such a markedly innovatory text as the Hymn of Suty and Hor. Assmann would prefer to apply the term to hymns which occur in several forms, including vignettes of the Book of the Dead and representations of the sun's course in tombs. His application of 'traditional' is certainly acceptable. In general usage, however, the term 'standard text' does imply a text frequently copied. Again, the span of time, provenance, and social origin are covered respectively by the terms 'dia-chronisch', 'dia-topisch', and 'dia-stratisch'. The reference of 'dia-topisch' is wisely extended, as far as the reproductive tradition is concerned, to the dominance of Heliopolis. Sociologically Assmann has every right to point the difference between his exegeses and those of Piankoff and Hornung, since he bases his work on an abundance of private sources rather than on the royal Books of the Underworld which mirror the specialist knowledge of a small group of *cognoscenti*. We have seen, however, that Assmann applies a similar description to one stratum of his own material.

The discussion of the material is grouped under two main headings, 'Re-Harachte' and 'Amun-Re', and the first chapter of the first section is devoted to the 'Mysteries of the Sun-cult'. For some time now Assmann has been using the term 'Mysteries' in this connection. Another term which he has recently been using is 'Konstellation', of a group of related deities; as I have pointed out in a forthcoming review (in *CdE*) of his admirable contribution to *Funktionen und Leistungen des Mythos*, ed. F. Stolz (Basel, 1982), the term is not fully acceptable since it introduces an astral metaphor into a religion which already has a real astral element of considerable significance. In conjunction with the term 'Konstellation' Assmann uses the word 'Ikon' in a specialized technical sense (following H. Blumenberg, see p. 54) to refer to non-narrative elements such as the cycle of the solar course. In this case again we confront a term which has a traditional sense in religious discussion, with its abstract concepts 'iconography' and 'iconology', and the new technical sense demands an unfortunate process of decoding. It is the closing summation in these texts that often conveys the gist of the ritual act in a performative speech-form which is described (p. 40) as a 'metakommunikativer Hypersatz'. These terms are used in modern linguistic theory and cannot be objected to on the lines of the above remarks on the decoding of more private terminology. Nor should the term 'Mysteries', as applied to the sun cult, be wholly condemned. At least a precise attempt is made (p. 24) to explain what it denotes: it includes the secret aspect and the restriction of access, as well as the importance of 'knowing', 'speaking', and 'acting'—elements known to occur in the Greek Mysteries. Initiation is expressly not included, but on page 32 this claim seems to be modified in the statement that the king, in order to unite himself with the sun-god, becomes 'one who knows the initiation (*bzw*) into the Mysteries of the underworld, having penetrated into the holiness of the Mysteries'. Admittedly this comes from the partly contrasted literature of the underworld.

On page 67 respectful reference is made to the concept of the 'thought couplet' which J. L. Foster has propagated. The importance of the couplet has been well highlighted by Foster, and in literary studies several types have been recognized. A strange superfluity, however, attaches to the qualification 'thought'. When one reflects that everything ever written by man is the result of thought, the prefix loses any classificatory value. Other categories have been elaborated by Kitchen in *Studien zu altägyptischen Lebenslehren*, ed. E. Hornung and O. Keel (Freiburg, 1979), 252-7.

These and other matters of terminology scarcely affect the substance of the interpretations offered. It is shown that in the theology of the solar course light and movement are leading ideas and that the emphasis on the sun-god's loneliness is easily linked with the idea of his uniqueness. His being far away in the heaven makes him manifest to all, while his radiance, which conceals him, is the condition of his parousia ('Parusie', p. 107). The last word puts us on our guard. Is it the simple sense of the Greek ('presence') or the Platonic sense (the 'presence' of a Form in a particular) or one of the two Christian senses (the Advent or the Second Coming)? Happily we are instantly informed that the first meaning is intended: it is the presence of the divinity in person, a concept that is then connected with the Egyptian *nfrw*, for which the meaning 'beauty' is favoured. *Mrwt*, 'love', is shown to have affinity with *nfrw*, the quality which Rudolf Otto called the *mysterium fascinans* of the holy. In the Amarna texts and in the new solar theology the words *nfrw*, *mrwt*, and *stwt*, 'rays', are treated as synonyms. A part of the Cairo Hymn to Amen-Re is quoted where the beauty of the sun-god at midday arouses a love which has an enervating effect—an instance, perhaps, of 'Liebe als Krankheit', but more probably the idea is purely objective in that Egypt's midday sun *does* have such an effect. After presenting a version (p. 108) of parts of various hymns, Assmann sums up their doctrine in a paragraph which is noticeably more poetical than the bare bones of the original texts. One can hardly demur to this tendency; it is preferable to the late Rundle Clark's endearing habit of embellishing the versions themselves with an imposed lustre. There are instructive remarks on the idea of continuous creation, an activity furthered by every sunrise, and on the attributes of 'mother and father' and 'shepherd'.

The book's second part is devoted to 'Amun-Re' and begins with a discussion of the theology of Amūn from the time of Hatshepsut, attention being focused on the eulogistic expansions of the divine name. These shorter expressions are subjected to the same careful analysis as the longer and more imposing compositions such as the Cairo Hymn to Amen-Re, where the god is the creator and sustainer and the only source of life. He is unique not only in character but also in being; yet other gods are referred to. A likely source of this view of nature is seen in the sun-temples of the Fifth Dynasty with their depictions of the processes of life and birth. An ethical authority is also recognized in this hymn and concurrently denied—rather unconvincingly—to the Amarna hymns; a creator-god who shows loving care for all beings is an object of worship, surely, in the ethical sense since all worship implies ultimately an *imitatio Dei*.

In discussing the theme of the unity of the divine Assmann is naturally concerned with the formulations of Eberhard Otto and Erik Hornung. He does not relish the 'monotheistic tendencies' seen by the former, nor the reductionism of the latter in seeing 'uniqueness' of character as the guiding principle and no real move (apart from Akhenaten) to a belief in an exclusive unity of God. Assmann avers that such a belief was indeed the goal in relation to the concepts of an original creator-god who sustains all life and has ethical import, and is yet a hidden God whose symbols, representations, and names are those of the many gods. He presents his case with great skill, subtlety, and perception, as well as with a formidable battery of contributory techniques. The material which he so abundantly deploys exposes the strength and weakness of his case. It is not always equal to the high claims made for it, and the debate is likely to continue.

J. GWYN GRIFFITHS

Excavations between Abu Simbel and the Sudan Frontier. Part 5: C-Group, Pan Grave, and Kerma Remains at Adindan Cemeteries T, K, U, and J. By BRUCE WILLIAMS. Oriental Institute Nubian Expedition, Vol. V. 305 × 235 mm. Pp. xxvi + 235, pls. 131, text-figs. 48. Chicago, The Oriental Institute of the University of Chicago, 1983. ISBN 0 918986 33 8. Price \$50.

Like so many other great Egyptological institutions, the Oriental Institute of the University of Chicago responded quickly and positively to the UNESCO Nubian Salvage appeal. Their acceptance

of the responsibility for the excavation and recording of three separate and different concessions deserves great praise. The northernmost was the Ramesside temple at Beit el Wali; the southernmost was the Middle Kingdom fortress at Serra East, in the Sudan. The third concession, that immediately north of the Sudan-Egypt frontier, encompassed the villages of Ballaña, Qustul, and Adindan. This latter concession turned out to be full of the remains of all the various Lower Nubian cultural phases, principally contained in a series of cemeteries. The excavation of the sites of the concession was done under the direction of the late Professor K. C. Seele during the winter seasons of 1962/3 and 1963/4. The volume under consideration here is the third in a proposed series of chronologically separated parts which will constitute the final report on these excavations and is the fifth part of the Oriental Institute's Nubian Expedition full publication.

As the title indicates, this volume is the presentation of the C-Group, Kerma, and Pan Grave remains from the concession. These were found in four sites, designated cemeteries T, K, U, and J, all of them within a kilometre of each other in Adindan village. The reported remains are from two Kerma tombs, eighteen Pan Grave burials and about 375 C-Group graves. The text is equally divided into two sections—the first is a discussion and analysis of the material presented in the second, the register of the tombs and their contents.

The second section, the tomb register, is a presentation of facts. This is a straightforward matter, although one or two minor problems are created by the presentation. The encoding of repetitive information can be a useful technique. For instance, it saves considerable space in a publication and allows the writer to introduce easily a variety of sorting and analytical programmes. Dr Williams has seen fit to encode much information about grave types, burial positions, and ceramics; but the system breaks down. The reader's patience is taxed by having to re-encode for himself the position of the hands and arms of a burial from the 1, 2, 3, etc., proposed in the explanation of the system (p. 124) to the a, b, c, etc., which is actually used in the text (e.g. p. 210, K39). A more serious fault is the unexplained series of abbreviations and numbers following object descriptions in the text. One might guess at the significance of '23292' following the entry 'Beads' (p. 149, T101), but it would have been more generous of the author to have shared these secrets. The pages of this reviewer's copy were consecutively numbered and it is assumed that anything not included in the book is the result of the author's design or accident. The absence of an explanation of the abbreviations will make the report much more difficult to use for colleagues whose mother tongue is not English. The single list of abbreviations (p. 126) is of the materials of which beads were made. It seems out of place, however, as there is a fulsome discussion of the bead finds, including a seven-page table containing the Register of Beads in which the abbreviations are abundant, on pages 83-95.

The illustrations of the material are generally clear and of a good scale. If anything, it has been somewhat lavish of the publishers to produce both photographs and line-drawings of so many of the finds. The scale of the plans of cemeteries T and K (Pls. 2 and 3, respectively) is adequate, although it is regrettable that there is not a real instead of a 'stylized presentation . . . of the evidence', particularly in view of the arguments concerning stratigraphy and cemetery growth presented in Chapter 1.

The volume begins with an interesting Foreword by Professor Seele's widow on the history of the expedition. This is followed by discursive chapters on chronology, history, and burial customs, on ceramics, on objects, on art and epigraphy, and a conclusion. It is with this section of the work that greater difficulties arise. For the sake of brevity, only a few will be mentioned here and those without the expansion they engender.

It is unfortunate that this is only the first of a series of volumes to appear, for Williams is obliged constantly to refer to his own analyses and interpretations in other parts of the series. Often it is not possible to assess his interpretive statements because they are based on his own manuscripts and unpublished data. It is difficult even to know when to expect to be able to conclude one's understanding of the present volume because those others are indiscriminately referred to both as 'forthcoming' and as 'in preparation'.

Chapter 1 contains a lengthy précis of Bietak's work on the C-Group¹ and a great adherence by Williams to Bietak's analysis of C-Group material remains in Lower Nubia is postulated. It seems, however, that the exercise of relating the Chicago sites at Adindan to Bietak's work is a largely

¹ M. Bietak, *Studien zur Chronologie der Nubischen C-Gruppe*, Österreichische Akademie der Wissenschaften, Philosophisch-Historische Klasse, Denkschriften, 97 Bd. (Wien, 1968).

academic one as, for example, many of the burial customs do not conform all that well with the categories of Bietak. Williams (p. 4) eventually states that 'grave goods were the best evidence for dating the individual tombs'. In addition, to conclude that 'the Bietak chronology for IB, IIA and III is confirmed at Adindan' (p. 116), after such extensive reliance has been placed on Bietak's work, seems a somewhat circular application. Methodologically, it would have been better to have excavated at Adindan after Bietak's publication (1968) if one were seeking to confirm it.

Williams's approach to the ceramics of the Adindan sites (chapter 2) is unusual. It is not the purpose of this review to defend the work of Nordström, Shepard, and the many others interested in ancient ceramics, but it is unfortunate that Williams finds he must dismiss the results of their efforts because they employ a 'taxothetic' (p. 25) approach. He feels that a phenomenological understanding is more appropriate to ceramics studies. The former, he believes, imposes categories on the ancient material, while the latter employs 'actual conceptual categories to which these objects were intended to belong and some of the actual cognitive content which they were intended to project' (p. 25). Williams is wrong and has failed to appreciate that one of the main directions of modern studies of ancient ceramics is to come to an understanding of the ancient potters' intentions.¹ Incidentally, by employing his 'taxothetic' approach, the modern analyst is able to discern features, such as an ancient economic circumstance, which were never intentionally introduced by the potter into his manufacturing. Williams's descriptive logic finds itself occasionally in extension, as, for example, when he tells us, 'the black-topped bowl is distinguished primarily by one feature—its black top' (p. 40) in order to demonstrate that the potter knew he was going to make a black-topped bowl, and so it becomes a valid category for us to consider. Our 'taxothetic' approaches have led us to consider the black-topped bowl as a valid category for decades.

Williams's presentation of stratigraphy is weak. It is stated (p. 111) that 'only one piece of evidence helps us place this Kerma–Pan Grave occurrence chronologically—the C-Group III grave in cemetery K that actually cut into a round Pan Grave'. The only illustration of this 'cut' is on plate 3, where there is a 'stylized presentation . . . of the evidence'. It is published at a scale of 1 : 500 and the actual drawing of the two graves together is 5 mm long. For such an important argument it would be preferable to see a more graphic representation of the evidence—either a photograph or a drawing at a larger scale—to help one assess the interpretation. From the tomb register (pp. 226–7) it can be seen that while the depth of shaft B is 1.05 m, that of shaft A, which it is reported to intersect, is not recorded. The fact that there is a body, together with some pots, preserved in B cannot be given significance for stratigraphic interpretations under such circumstances.

The question of cattle raising by the C-Group in Lower Nubia is discussed at length. It is true that the presence of graffiti of cattle, of cattle dung in C-Group ceramics, and of horns and skulls in funerary contexts all serve to demonstrate the fact that cattle were a major fact of life in C-Group existence. But, without the presentation of additional evidence, it is improper of Williams to state (p. 117) that the C-Group were pastoralists. Economic dependence on cattle pastoralism begs many questions concerning, for example, the palaeoenvironment and the social complexity of the pastoral group, which remain untreated. The evidence of cattle dung, so emphatically used by Williams, is treated with considerable caution by Nordström when interpreting the common occurrence of dung as a ceramic temper: 'Whether these cattle belonged exclusively or partially . . . or were the property of cattle pastoralists in the contiguous regions must remain an open question.'² This approach is reinforced by the data and arguments of Adams,³ who asserts that cattle were a 'main focus of cultural interest' (p. 152) of the C-Group without having been 'the basis of the Nubian subsistence economy' (p. 153).

'The body feathers and the eggs were brought to Lower Nubia by trade; because the representations of ostriches are rare these birds may have been sighted only occasionally and therefore probably existed in insufficient numbers to provide the rather large number of eggs and feathers that were used' (p. 77) is a most unlikely statement. Ostriches have been sighted well north of Abu Hamid—less than 200 km from the southern limit of Lower Nubia—in the present century,⁴ and

¹ e.g. F. R. Matson, 'Ceramic Queries', in *Ceramics and Man*, ed. F. R. Matson (Chicago, 1965), 277–87; and H.-Å. Nordström, *Neolithic and A-Group Sites*, vol. 3 (Stockholm, 1972), 34. Scandinavian Joint Expedition to Sudanese Nubia.

² Nordström, *op. cit.* 24.

³ W. Y. Adams, *Nubia Corridor to Africa* (Princeton, 1977).

⁴ H. C. Jackson, 'A Trek in Abu Hamid District', *SNR* IX, 11.

it is improbable that the species was so scarce in Lower Nubia 4,000 years ago, given the general environmental conditions and its distribution today. A paucity of the bird in C-Group art may only signify a lack of its appeal to C-Group graphic taste or attest to its general lack of importance to the C-Group mind or way of life. As one female will sit on a nest of up to thirty-five eggs, it is not difficult to imagine a relatively small breeding population of the birds being able to supply all the needs of the C-Group for beads and feathers.

Williams tends to be assertive where data are not available to confirm an interpretation. For example, the use of galena as an eye cosmetic is well attested in ancient Egypt,¹ but there is no Nubian evidence to justify 'The most common (class of toilet object) is the shell used to hold galena eye paint' (p. 75). It is equally possible that the galena of the C-Group, as much as the malachite of the A-Group, could have been used as a body paint, hair colourer, or could have had some amuletic significance. All such assumptions must always be so identified.

In the discussion of art and epigraphy (chapter 4) there are errors of interpretation and identification. As a giraffe, the animal scratched after firing on pot T227: 1 (Pls. 78D and 83D) is improbable. It more closely resembles a gerenuk, although it could equally be a goat stretched for browsing. The identification of part of a crude, incised figure (p. 104, pl. 101) as 'the lower part of the calves . . . shown as contiguous inverted triangles that protrude below the skirt' would also seem incorrect. The parallels cited by Williams bear little resemblance to this figure. The legs in most of the figures are shown as separated limbs and in Williams's 'closest parallel'² the author describes the figures on the bowl as '(females?)'. The present graffito is unquestionably of a female. A more important piece, because of the prominence given it in the volume, is the 'male head' which is placed 'at the forefront of C-Group representation and in a select company of masterpieces of African art' (p. 109). This worn head (T217: 2) is the subject of ten separate views in line-drawing and photograph on plates 104 and 105. Apart from the horizontal lines beneath the nose, identified by Williams as a beard, the head is not dissimilar to that identified as a female on plate 102A (T205: 2), with essentially the same size, three-lobed hair style, and rendering of the eyes. The lower part of this latter face is missing. This reviewer fails to see any beard on the 'unique' head. Depictions of beards, in general, are with vertical lines and the horizontal lines of the present object are in precisely the right place for a mouth.

Dr Williams seems, sometimes, to be led astray by his own logic and interpretations of the rather ordinary material from these plundered sites. If one is looking for the Ark, one is very likely to find it and researchers who enter an investigation with an a priori, logical explanation may be misled. It is hoped that Dr Williams in future excavation reports of this series will confine his pen to the data and reserve his own philosophy and interpretations for other covers. The interpretations within a field report should only be those of the people who have actually worked at the site.

A. J. MILLS

Glossaria Bilingua in Papyris et Membranis Reperta. By JOHANNES KRAMER. Papyrologische Texte und Abhandlungen 30. 275 × 188 mm. Pp. 183. Bonn, Dr Rudolf Habelt, 1983. Price DM 68.

This book consists of an edition with commentary of fifteen bilingual texts (Greek and Latin) and one trilingual (Greek, Latin, and Coptic), preceded by a General Introduction. Included are all examples known to the editor of Greek-Latin or Latin-Greek 'Wortliste, Gesprächsbücher und Musterbriefe, die uns auf Papyrus oder auf antikem Pergament erhalten sind' (p. 17); omitted are glossaries to literary works, texts of a purely grammatical purpose, and mere 'Buchstabenlisten'. Also omitted are works transmitted to us by medieval manuscripts, which are already well known through their inclusion by Goetz in *Corpus Glossariorum Latinorum (CGL)*. In fact no text is included which was written after AD 600. Of the two broad categories into which such texts fall, as distinguished by Goetz, *Idiomata* and *Hermeneumata*, nos. 1-4 belong to the former category and nos. 5-16 to the latter, though no text here published corresponds exactly to those known from medieval sources.

¹ A. Lucas and J. R. Harris, *Ancient Egyptian Materials and Industries* (London, 1962), 80-4. On page 80 the authors are careful to point out that 'shells were also employed as receptacles for pigments other than eye-paint'.

² The figures are on a decorated bowl (from Tumas, not Aksha as Williams states) reported by B. Lal, 'Indian Archaeological Expedition to Nubia, 1962: A Preliminary Report', *Fouilles en Nubie, 1961-1963* (Le Caire, 1967), 114 and pl. xxxvii.

All the texts have been re-examined, either in the original or from a photograph. Note especially no. 7, half of which is in effect an *editio princeps*, no. 11, which gives us the first publication of P. Fayum 135 verso, and no. 15, which had not been restudied since Schubart's original publication in 1913 and which is here republished together with a few newly discovered fragments. The other texts show little change from earlier publications, which is not surprising since they were for the most part well edited already, but it is none the less useful to have this group of comparable material published all in one volume. The main value of the book, however, lies in the General Introduction and in the editor's commentaries to the individual texts, which naturally are concerned almost exclusively with the value of these texts for our understanding of the Greek and Latin languages and especially of the way they were spoken. Kramer is well known for his work in this field and it is good to have his views collected together in this way. Note in particular his sensible comment that we must beware of over-hasty generalization from this evidence, since the texts cover a very large time-span and are all, with the exception of no. 4, from Egypt. He is also suitably cautious in considering the purpose for which the various texts were written, concluding that there are no general rules but that each text must be considered separately.

In 3.14 n. *lab[orans* twice appears with subscript dots under the three letters preserved; presumably therefore the omission of these dots in the text is a mistake. Some of the errors in 4 (e.g. confusion of *b* and *d* and of *i* and *l*) may be palaeographical rather than linguistic; and note the misprints at the end of p. 58 n. 31. In 10.8 n. one misses a reference to E. G. Turner, *GMAW*, 13 n. 3, for the use of apostrophe between gutturals, while in 14.26 the plate in Seider favours the reading *cabicla* rather than *cubicla*.

This book will prove very useful to the student of the comparative philology of Greek and Latin, and of their development into Modern Greek and the Romance languages. It is a pity that the opportunity was not taken to make it equally valuable to the palaeographer by including plates of the texts here edited, especially as in some cases there are no published photographs or only part of the texts has been reproduced (but there *are* plates available of nos. 6 and 8, in P. Oxy. xxxiii and xlvi respectively, though this is not indicated in Kramer's bibliographical notes). In this type of text the Greek is often written in Latin characters or vice versa, which makes the letter-forms of particular interest, and a collection of plates illustrating such material would have been of special value.

J. DAVID THOMAS

Ancient Egyptian Faience. By A. KACZMARCZYK and R. E. M. HEDGES with an Appendix by P. VANDIVER. 240 × 170 mm. Pp. 587, figs. 46. Warminster; Aris and Phillips Ltd., 1983. Price £30.

In this book, Kaczmarczyk and Hedges present the results of the chemical analysis of more than 1,000 Egyptian faience objects which were selected from the collections at the Ashmolean Museum, Oxford, and from the Petrie Collection in University College London in order to span systematically the period from Predynastic to Roman times. This main part of the book is followed by a lengthy appendix by Vandiver in which she surveys the techniques used in the manufacture of Egyptian faience.

The project has increased the available analytical data for Egyptian faience by at least an order of magnitude. Therefore, in reviewing this work, the two primary questions that must be asked are: (1) how reliable are the analytical data, and (2) how valid are the interpretations of the data?

The bulk of the analyses were undertaken, using an energy-dispersive X-ray fluorescence spectrometer operating in air, on the as-received surface of the glaze as well as, for a limited number of objects, the exposed body (i.e. core). This method of analysis creates two main problems. First, because the spectrometer was operated in air, low atomic number elements such as sodium, magnesium, and aluminium are not detected. Secondly, because of the effect of weathering, elements such as sodium and potassium could have been leached from the surface of the glaze and therefore an analysis of the glaze surface may not be truly representative of its original composition. In her Appendix (pp. A42-50), Vandiver demonstrates convincingly that a considerable proportion of the original alkali (i.e. sodium and potassium oxides) present in the glaze has been lost as a result of weathering and therefore the quoted alkali contents should either be disregarded or treated with extreme caution. However, the metal elements responsible for colouring the glazes (e.g. copper,

cobalt, manganese, etc.) are far less susceptible to leaching by weathering. Therefore the data for the colorant elements, on which the majority of the discussion is concentrated, can be regarded as essentially valid. Furthermore, in criticizing the method of analysis used, it should be emphasized that, in reality, there is no satisfactory alternative to X-ray fluorescence spectrometry for the rapid, non-destructive analysis of glazes. To have taken a section through the glaze for electron microprobe analysis would clearly have resulted in an unacceptable amount of damage to the majority of the objects analysed.

In their interpretation of the analytical data for the glazes, the authors first discuss each chemical element in turn suggesting with which of the raw materials used in the production of the glaze (i.e. quartz sand, alkali, colourant), each element might have been introduced (chapter 2). Secondly, they discuss the glaze colours indicating in each case the metal oxides used as colourants (chapter 3). Thirdly, they survey, both chronologically and geographically, the changes in the metal oxides used as colourants and at the same time discuss the possible geographical location of the sources of the different raw materials used to make the glaze (chapter 5). These three chapters are very valuable in highlighting the developments that occur in the use of different colourants. However, because of the great mass of data that is presented, it is often very difficult to follow the line of argument. Furthermore, I believe that undue emphasis is placed on attempting to identify the sources of the raw materials used. Because the glazes are multicomponent materials (i.e. quartz sand, alkali, colourant) and because there is practically no reliable analytical data for raw material from the various possible sources, the bulk of the source identifications are inevitably highly tentative.

Kaczmarczyk and Hedges also devote a chapter to the composition of the faience bodies and attempt to assign the objects that they have analysed to the different faience variants proposed by Lucas in his classic work on ancient Egyptian materials and industries. However, in her Appendix, Vandiver questions the validity of categorizing faience objects on the basis of the variants proposed by Lucas since these are not based on technological criteria. Indeed, as she points out, separation into variants such as D (faience with hard blue or green body) and E (glassy faience) can conceal a technological continuum associated with a range of alkali contents and firing temperatures.

In her Appendix to the book, Vandiver presents the results of a microscopic examination of the faience which, in combination with laboratory replication, provide the basis for determining the different techniques used in the production of faience. First, she describes the methods (e.g. modelling, moulding, throwing) used to make the faience bodies. Secondly, she discusses both the external macroscopic and the internal microstructural characteristics of glazed faience which can be used as criteria for distinguishing between the three generally accepted methods of glazing faience (i.e. direct application, efflorescence, and cementation). Thirdly, she provides a survey of the different methods that have been used both to make the body and for glazing at the different periods covered by the project. The appendix thus clearly demonstrates the very considerable value to technological investigations of careful low-power microscopic examination, especially when used by someone with direct practical experience of making the material under investigation.

In summary, therefore, this book provides a most valuable data bank on the colourants and the production methods used to make faience in Egypt from Predynastic to Roman times. The book has thus already become the essential starting-point for any further work on faience. However, it must also be stated that it is sometimes extremely difficult to extract information from a somewhat overlengthy text and, in particular, to distinguish factual data from hypothesis.

With regard to future work on faience, it would be preferable if chemical analysis of the glaze could be combined with microscopic examination from the beginning. The discussion of the glaze chemistry and the methods of manufacture could thus be combined into a coherent story, rather than the latter topic being relegated to an appendix. Furthermore, it is hoped that in future work, it might be possible slightly to modify and extend the range of scientific techniques employed. First, the use of X-ray fluorescence spectrometry operated in vacuum would enable the analyses to be extended to the low atomic number elements. Secondly, X-ray diffraction analysis, which requires only a minute sample, would provide positive information on whether or not crystalline opacifiers such as lead antimonate and calcium antimonate are present in the glaze. Finally, it would be highly desirable to examine a few already damaged objects in cross-section with the electron microprobe and scanning electron microscope in order to obtain fully quantitative analyses of the glaze and information on the microstructure of glaze and body.

M. S. TITE

Aramaic Texts from North Saqqâra with some Fragments in Phoenician. By J. B. SEGAL with contributions by H. S. SMITH. Texts from Excavations, Sixth Memoir. 320 × 255 mm. Pp. xx + 217, pls. 38. London, Egypt Exploration Society, 1983. ISBN 0 85698 083 8. Price £120.

The fragmentary texts published here for the first time were discovered during the course of the Egypt Exploration Society's excavations at North Saqqâra. Most were found in 1966–7, the remainder during 1971–3. All came from piles of debris dumped in ancient times, so that their archaeological context (briefly summarized in this volume by H. S. Smith) gives no clue as to their date or original setting. To add to the editor's problems, all the fragments are severely damaged: 'As many as 104 contain only one or two or three complete words, and only 35 have more than three lines of some length. A few papyri, including those with a longer text, are in such poor condition that the writing is virtually indecipherable' (p. 3). In view of these enormous difficulties and what must frequently have become an unrewarding task of editing unintelligible scraps, Professor Segal is to be thanked and congratulated on having brought his task to completion and derived so much of interest from such an apparently unpromising collection. Although with characteristic modesty he says only that the principal contribution which this collection will make to the study of the ancient Near East lies in the field of onomastics, we should not be blinded to the other tatters of knowledge which will be dredged from this material in the future.

A brief Introduction surveys such standard topics as archaeological background, description and date of the papyri and ostraca (probably late Achaemenid, although they need not all come from the same period), summary of their contents, social background, and language.

In chapter 2, the texts themselves are presented. The papyri are grouped by subject where possible (legal and judicial, taxation, historical, and commercial), but by far the majority (150 out of 202) have to be lumped together as 'miscellaneous'. These are then followed by the twenty-six ostraca, of which twenty-one are thought to be in Phoenician, rather than Aramaic, script. The third and final chapter comprises extremely detailed palaeographic tables, with examples of every available letter from every text. A random check revealed a few minor inaccuracies, however, which may detract from the potential value of these tables. At all events, no attempt is made to propose a typological sequence, and Segal is evidently sceptical about the use of palaeography for absolute chronology (pp. 3–4). A Bibliography, a Concordance of the Various Numbers already used in recording and cataloguing these texts, Indexes of Names and Words, and a General Index complete the text of the volume, to which plates of all the papyri are appended.

A few matters in the presentation call for comment. First, and most important, the plates are of little assistance to the serious reader. They are not presented on any consistent scale, some being enlarged and others reduced without any reason that I could see or explanation that I could find. They thus give a quite misleading impression at first sight. More predictably, of course, they give virtually no help in checking or re-analysing disputed readings (though they will assist in confirming the occasional misprint, such as *h* for *t* in line 2 of no. 112). Where the plates are clear, so is Segal's reading; where he himself had difficulty, they generally show nothing at all. This should not surprise us, but it does mean that at the end of the day the provision of plates will be of little practical value in furthering the study of this collection.

Next, there are three slightly annoying features in Segal's presentation of the texts themselves. One is his refusal to use final forms of any letter. Of course, he will reply that only some of the papyri use them for only one or two of the letters, and that therefore he is representing the text as accurately as he can. However, except in the case of the palimpsests, he uses printed Hebrew characters for his transcriptions, and here the convention has always been to use final forms (see, most analogously, the other collections of Egyptian Aramaic texts), so that the failure to use them here creates a distinctly odd impression. Secondly, we should observe that because *d* and *r* are virtually indistinguishable, Segal always presents them as alternatives both in the transcriptions and (in the case of proper names) in his translation. This has the unfortunate effect of diminishing the reader's sensitivity to potential ambiguity. In the majority of cases the context leaves no doubt as to the correct reading, and the practice therefore becomes a matter of sterile routine. The provision of alternatives would thus have had a much greater impact if it had been reserved for cases of genuine ambiguity. Finally, in two out of three cases, a palimpsest is transcribed into a hand-written, rather than a printed, text (nos. 10 and

44; contrast no. 36). This seems quite unnecessary; though the handwriting is clear, it is inevitably less familiar at first than the printed form.

I turn next to Segal's work in interpreting these fragments, and here at once it must be said that he will be found a generally cautious and reliable guide. On the whole, he eschews all but the most obvious restorations, and is ever quick to point to problems in his own interpretations and to list alternative possibilities. In view of this, and because most of the texts are so very fragmentary, I shall not here attempt to supply a long list of yet further alternatives. Many will doubtless occur to any informed reader, as they must have done to Segal himself, but when all is said and done our knowledge is not advanced in even the slightest degree by reading isolated groups of two or three consonants according to one possibility that our present knowledge allows or according to another.

In one respect, however, Segal's caution leads to a measure of disappointment: he does not generally attempt a broader interpretation of even the better preserved texts, but restricts himself rather severely to textual and philological comment alone. It is true that the general groupings listed above give some elementary guidance, especially when taken together with the comments on pages 5-6 of the Introduction. Unfortunately, however, these are not always enough even to be sure why Segal has allotted a text to a particular category (for instance, why is no. 28 classed as 'historical' rather than 'legal?'). Since few, if any, are likely to ponder these texts as long or as profoundly as Segal, we would have welcomed his further guidance in this respect.

Instances of serious disagreement with or reservation about what Segal has presented will be few. It is surprising, for instance, that he does not relate his reading of Ostrakon III to the lively debate about the so-called ζ -symbol on a number of jar inscriptions and stamps from the Persian and Hellenistic periods. In *IEJ* 26 (1976), 15-19, O. Goldwasser and J. Naveh listed three possibilities and added a fourth, while A. Raban makes yet another proposal in his Hebrew University doctoral thesis on the commercial jar in the ancient Near East (1980). Elsewhere in Egypt, the symbol is sometimes found after *lmlk*, 'for or belonging to the king', and Goldwasser and Naveh propose that 'the symbol on the Elephantine jars still represents the demotic *sp*, which was understood by the Phoenicians as a determinative meaning "royal"'. The fact that in our text the ζ is followed by *lgh*, taken by Segal as *l* + n. pr., might tell against this theory. Indeed, Segal takes the ζ as an abbreviation for *tb*, 'good, valid', apparently without knowledge of B. Delavault and A. Lemaire, *Semitica* 25 (1975), 31-41. On the other hand, Segal is unable, as he admits, to explain *ght* as a proper name, and wonders whether the word as a whole is not an unusual plural form of the liquid measure 'log'. Whilst the ostrakon does not, therefore, shed decisive light on the issue, it is of relevance and deserves to be drawn into the discussion.

The temptation to make at least some sort of sense out of even a few words may sometimes have led Segal to gloss over difficulties on which he might at least have been expected to comment. In brief form, I note a number of such passages by way of illustration. 1: 2 as printed has an unprecedented example of an isolated *l*, 'to'; in 3: 7 the haph. of *hlp* governs the preposition '*m*', elsewhere always *l*; the translation of 6: 5 is probably misleading, since the haph. of *hwy* is not usually followed by '*l*'. '*lyn*' is therefore probably the end of the previous sentence; the same may well be true for '*lht*' in line 7; 14: 3 has *k'ny* as an incomplete word meaning 'now'. However, of the various attested spellings of this word none ever ends in *-y* or *-y-*; at 24: 4 *nhytyh* is translated in such a way as to imply that the verbal suffix represents the indirect object—'we shall bring him men 200'. This is not very likely, and certainly deserves comment; it would be best not to rely on 34a: 3 as indicating anything about the date of these papyri (cf. p. 4). *bywmt kbwz*, rendered 'in the days of KBWZ[(Cambyses)', occurs without a context and involves both an unattested form of the pl. construct of *ywm* and an unusual spelling of the pr. n.; at 35: 2 Segal restores [*z*]pt *lksp* = 'loan in money' without commenting on the fact that this would represent a broken construct chain.

Turning now from details of this nature, what of positive value may we derive from these texts? Segal's modesty prevents him from drawing attention to more than one of seven particular points which I have noted and which should not be overlooked in future research.

(i) Onomastics. As already stated, Segal deems this to be the chief value of these texts. There are, in fact, several hundred names in all, many not previously attested in Aramaic. Though this subject is not a special interest of the reviewer, it is evident that Segal has gone to heroic lengths in elucidating these names and drawing comparisons wherever possible, with full references to the standard

secondary studies. Here, then, is a major source of new knowledge for an already comparatively well-exploited field.

(ii) Palaeography. It is not possible to assign firm dates to any of these texts, although in one or two cases an intelligent guess may be permitted on the basis of an occasionally preserved regnal year (see the discussion on p. 4). Despite this, however, the sheer bulk of written material here published will surely be used in time to amplify and refine the standard basic analysis by J. Naveh, *The Development of the Aramaic Script* (The Israel Academy of Sciences and Humanities Proceedings, Volume V, No. 1, Preprint, Jerusalem, 1970).

(iii) The study of dialects. During the first half of this century, our knowledge of Imperial Aramaic was dominated by the relatively uniform language of the Elephantine Papyri. Recent decades, however, have witnessed the publication of more diverse material, most notably the Brooklyn Museum, the Arsames, and the Hermopolis papyri. Though all were found in Egypt, their geographical origin and cultural background are evidently quite varied. Thus, whereas the label 'Imperial/Official Aramaic' retains its usefulness (cf. J. A. Fitzmyer, 'The Phases of the Aramaic Language', in *A Wandering Aramean: Collected Aramaic Essays* (Missoula, 1979), 57–84), it is now generally agreed that alongside the relatively static form of the diplomatic language, living dialects continued to develop until eventually the language broke down altogether into the varied and increasingly diverse dialects of later times; see, for instance, J. C. L. Gibson, *Textbook of Syrian Semitic Inscriptions, Volume 2: Aramaic Inscriptions* (Oxford, 1975), 127–8; E. Y. Kutscher, 'The Hermopolis Papyri', in *Hebrew and Aramaic Studies* (Jerusalem, 1977), 53–69; and J. D. Whitehead, *JNES* 37 (1978), 119–40. Unfortunately, the present group of texts is inadequate for a full grammatical profile of their language, but a number of features will serve to increase our awareness of dialectal variation at this period. Segal has gathered the most striking phonological characteristics on pages 11–12 and some points relating to morphology. The latter, of course, is more prone to the danger that we are misinterpreting a fragmentary text. None the less, we may add to Segal's points the apparent retention of 'aleph in the perf. pe'al form b''t at 69b: 3 and qr'n at 94: 2 (or is it a masc. pl. partic.?). More securely, there seems to be a clear case at 19: 6 of an imperf. pl. form without final n, whereas it has become standard practice to regard this as a firm morphological distinction in Imperial Aramaic between the imperfect and the jussive. Finally, although it is even more hazardous to speak of the syntax of these texts, there are some surprising usages if Segal's reading and analysis are correct, particularly in the matter of word order (e.g. at 46: 3 and 4, 67a: 2, and 69: 1). It is not impossible that this too reflects dialectal idiosyncrasy. As a concluding comment under this heading, I was struck by the number of construct chains written in such a way as to imply that they were regarded as compound nouns, e.g. 28b: 4, kpyd, 'palm of hand', and kprgl, 'sole of foot'; 41: 8, ywmyld, 'birthday'; and 50: 9, byt'srn, 'prison'.

(iv) Vocabulary of Imperial Aramaic. Many words known in other forms of Aramaic are attested here for the first time in Imperial Aramaic. Not all are of equal certainty: some are clear in the present context and could in any case have been supposed to exist in Imperial Aramaic; others are more conjectural and alternative explanations should not be ruled out. As a starting-point for further work, I append a list of those which I have noted (references can mostly be found in Segal's 'Word List', though there are occasional omissions, such as štt): 'gm, 'marsh'; 'hr'y and 'hry, 'guarantor'; l'npy = 'at the cost of'; 'sprng, 'quince'; bgr, 'to reach maturity' and (adj.) 'of mature age'; byt škn, 'dwelling place'; blw, 'tax' (though cf. Ezra 4: 13, 20; 7: 24); bn'y, 'builder'; gbh, 'collect'; bgw, 'inclusive' (Segal, p. 16, seems to be certain about this unusual meaning, though failing to refer to 19: 3, which is arguably the best example. Doubts remain, however, in view of contextual problems and its quasi-technical usage in legal documents elsewhere); ddaq, 'little child'; dtbr, 'law official' (but cf. Dan. 3: 2 f.); hbl, 'woe'; hkr, 'rent'; ṭnhh, 'mill'; yld = 'children' (collective); ytb = 'pay'; ytrn, 'surplus, balance'; k'h, 'rebuke'; kbr, 'increase'; kšr, 'be suitable' (and kšyr, 'suitable'); kšys, 'elder'; m'zl, 'departure'; mkns, 'trousers'; m'sr, 'tithe'; mqwyt, 'reservoir'; mš, 'trowel'; ngrbt, 'fetter, chain'; nhl (haph.), 'give as inheritance'; nhš, pl. = 'chains'; nšb, a type of silver; sb, 'grandfather'; sk, 'total'; 'bdny, 'of a tanner'; 'll, 'produce'; psd (aph.), 'cause loss, spoil'; qrtys, 'document'; rqh, 'perfume'; š'l, 'handful'; špr, 'be pleasing' (not previously attested as a verb, but cf. Dan. 3: 32; 4: 24; 6: 2); šqp, 'strike'; štt, 'flow'; trsy, 'provisions'.

(v) In similar vein, Segal finds a number of words and forms previously unattested in any form of Aramaic. Again, I list those which I have noted without prejudice to an evaluation of the strength of

the arguments in each individual case: 'wpst, 'help, support' (should this be linked with the obscure 'wpšr in AP26?); 'ytšry, 'red barley'; 'prytr, 'most worthy'; gzr, 'carrot'; hmyt, 'complement'; hmkygrb, 'pledge'; hmr, 'reckoning'; zrk, 'measure' (?); ittaph. of ysp, 'add'; ntr (pa. and haph.), 'release'; syd, 'lordship'; s'nh, 'endowed'; 'pr, 'stag'; 'šk, 'estate' (uncertain in view both of the damaged context and of the need to postulate an unusual, though attested, interchange of consonants); šdn (haph.), 'manumit'.

(vi) Legal formulae. These have been the subject of particularly detailed studies in previous research on the Egyptian Aramaic Papyri; cf. R. Yaron, *Introduction to the Law of the Aramaic Papyri* (Oxford, 1961), and Y. Muffs, *Studies in the Aramaic Legal Papyri from Elephantine* (New York, 1968). There are two possible new examples in the present texts which should be noted, even if there are insufficient data for a full analysis: lk hmw, 'they are yours' (8: 5, 7, 8) as a statement of conveyance when followed by 'm (kl) dyn + suffix, 'with (all) his (etc.) judgement', parts of which phrase are found in each of lines 5, 7, 8, and 13; and thwh lh [l?] br', 'you will be a son to him' (11: 2) as (part of) an adoption formula, to be compared not only with the bry yhwh, 'he will be my son' of BMAP 8: 5, 9, but also with the Hebrew Ps. 2: 7. We may note as well that the method by which the signatories have added their names to this text conforms closer to Egyptian practice than do the Elephantine papyri.

(vii) Illumination of other texts. It will undoubtedly take some considerable time for these papyri to be fully exploited in the explanation of other disputed texts. We have already noted at (iv) above, for instance, some words in biblical Aramaic attested here in Imperial Aramaic for the first time; these will no doubt take their place in the continuing debate about the correct classification of that material, along with further clear examples of šmh (= effectively 'by name') at 5: 1; 9: 3, etc., in comparison with the disputed Ezra 5: 14, and the contribution of 26: 8 and 10 to the elucidation of the obscure 'ššrn' at Ezra 5: 3 and 9. Further afield, Segal does well to compare his nšb' (38: 1 and 5) with the Azarbaal Spatula (cf. J. C. L. Gibson, *Textbook of Syrian Semitic Inscriptions, Volume III: Phoenician Inscriptions* (Oxford, 1982), 9-11). That the word signifies a type of silver makes admirable and previously unsuspected sense of the much debated ksp nšbt there. Next, 40: 3 seems clearly to settle the dispute about ršwt' at AP 26: 17 (it is a type of tree), while 41: 4 leaves little doubt that m in these texts and elsewhere in Egyptian Aramaic is an abbreviation for the coin m'h.

Finally, Segal proposes a new rendering of AP 14: 3 on the basis of the use of nprt at 2: 5. The text deals with the settlement of an estate following the divorce of Mibṭaḥiah and Pi'. Cowley construed the word in question as a 1st pl. imperf. pe'al of prt: 'let us make a division concerning the money . . .'. This is unsuitable, however, because (i) there is no division in what follows. Pi' has renounced his claim to these goods completely; and (ii) it does not fit well with the inclusion of the marriage deed (spr'ntw) in the list of items which follows. Because of these difficulties, Kraeling, BMAP, p. 54 n. 13, revived an older suggestion of Halévy, who compared Arabic nafara, 'withdraw', and so rendered as a 1st sg. pf., 'I surrendered'. On this view, the items listed were surrendered to the court before the case. Then, when the wife made her oath, she was able to take them while her former husband was obliged to draw up this deed of 'removal'. An unexplained difficulty on this view, however, is the use of the preposition 'l following the verb. P. Grelot, *Documents Araméens d'Égypte* (Paris, 1972), 190, found a further difficulty in the implication that Mibṭaḥiah was the prosecuting party, whereas Grelot believes that Pi' was. He therefore notes that the same Arabic root (in the form nāfara) can mean 'disputer à quelqu'un devant un juge', and so renders 'j'avais fait opposition au sujet d'argent . . .'.

Segal now proposes a quite new solution to the problems of this passage. At his 2: 5, he compares nprt with Iranian *ni-frīti, 'curse', and so renders the line (kn 'mr 'nh mwm' nprt yhbt lh kys) 'thus he said, An oath with a curse have I given him; a purse'. This then enables him to render AP 14: 3 as 'in accordance with the law-case which we made at Syene, with a curse, concerning the money . . .', an adverbial use of the noun nprt, exactly as in the new papyrus.

The use of Iranian loan-words in Imperial Aramaic is, of course, extremely widespread, so that Segal's suggestion should be carefully considered. It advances a less 'remote' explanation than Arabic. On the other hand, Grelot's translation is extremely attractive in the context, and is not subject to the difficulties which confront Cowley and Kraeling. It would have strengthened Segal's case if he had considered the possibility of rendering his own text on this basis: 'Thus he said: I have entered into a legal dispute on oath; I gave him a purse . . .'. Unfortunately, the text is again too

damaged for a decisive evaluation, but that Segal has entered a promising suggestion should at least be clear.

In the light of these seven points and others, it is apparent that although these texts are so tantalizingly fragmentary they will yield much valuable material in the continuing study of the modest corpus of Imperial Aramaic sources. As expected, the standard of production is high (though not without blemishes; e.g. 'were put' is inverted on p. 11, line 18; a \circ has misleadingly been omitted before the final t in 4: 4; 'I' should surely be 'and' in the translation of 5: 3; the comma after '4' in note 1 to no. 7 should be a semicolon; note 5 to no. 69a is completely missing, and a line seems to have been lost from note 4 to no. 92; read t for h in 112: 2). Nevertheless, being unsuitable for purposes other than advanced and detailed research, the book will inevitably have only a very limited circulation. If it is not churlish for an outsider to comment thus on a production of the EES in a review in the Society's own journal, Aramaists will be dismayed that this material has been published in a format which will put it beyond the reach of individual scholars and of all but the best and most specialized libraries. It is much to be hoped that, in the wider interests of scholarship, the Society will investigate some of the more modern techniques of reproduction now available so that treasures brought to light with such care and dedication will not be lost again through unnecessary inaccessibility.

H. G. M. WILLIAMSON

Die Gartenpflanzen im alten Ägypten, Band II. By L. KEIMER, edited by RENATE GERMER. Deutsches Archäologisches Institut, Abteilung Kairo, Sonderschrift 13. 305 × 215 mm. Pp. ix + 86. Mainz am Rhein, 1984. ISBN 3 8053 0619. Price DM 78.

In 1924 Ludwig (Louis) Keimer, then on the threshold of his long and distinguished career, published volume i of *Die Gartenpflanzen im alten Ägypten* (reprinted Olms, Hildesheim, 1967). As the author indicated, this volume was the first of a projected three-volume work on the flora of ancient Egypt. In an article which appeared a year later, volume ii was referred to as being 'in the press'. This, however, was, as it turned out, a misplaced expression of optimism; all too familiar financial difficulties prevented publication. In later years, Keimer utilized some of the material he had gathered for volume ii in a number of articles devoted to individual species (e.g. *BIFAO* 28 (1929), 50 ff. on the sycamore fig; *BIFAO* 31 (1931), 177 ff. on the Egyptian willow).

After Keimer's death in 1956, the German Institute in Cairo acquired his papers, including the manuscript for volume ii of *Die Gartenpflanzen*. It is this manuscript which Mrs Germer has now made available. Through his close association with the great German botanist, Georg Schweinfurth (1836–1925), Keimer had acquired detailed knowledge of numerous finds of ancient Egyptian plant material, the botanical identity of which had been determined by Schweinfurth himself. On the basis of this material, Keimer was able to rectify many botanical misstatements that had appeared in earlier Egyptological literature and been accepted without question.

In addition to the purely botanical evidence, Keimer turned his attention to the textual material. The standard work on the ancient flora at the time he wrote was still Victor Loret's *La Flore Pharaonique* (2nd edn., Paris, 1892) which was based largely on linguistic evidence. This Keimer subjected to a critical assessment.

Naturally, excavations undertaken during the sixty years that have elapsed since Keimer wrote his manuscript have produced additional material on the flora of ancient Egypt in general and on the species dealt with in volume ii of *Die Gartenpflanzen*; yet surprisingly little of what was written there has been superseded by these later discoveries. On the botanical side, virtually the only workers to have concerned themselves with the ancient flora have been the late Vivi Täckholm and her colleagues.

In this second volume fifty-four species are discussed, belonging to twenty-four Families ranging from the Rutaceae to Typhaceae.

Mrs Germer has wisely confined her editorial work on Keimer's manuscript to the minimum; the later marginal additions and annotations made by the author have been incorporated into the text, but his wording has been altered as little as possible. The few additions by the editor are enclosed in square brackets. The references to sources have been checked as far as possible, and the abbreviations employed by Keimer have been altered to conform with those used in the *Lexikon der Ägyptologie*. The editor has also supplied indexes to volumes i and ii: botanical and popular names.

Two minor complaints: the autographed hieroglyphs are on the small side, at least for this reader's eyes. It would have been more convenient for shelving if the second volume had been issued in the same format as the first.

D. M. DIXON

Das Totenbuch in den Thebanischen Beamtengräbern des Neuen Reiches. By MOHAMED SALEH. Deutsches Archäologisches Institut, Abteilung Kairo, Archäologische Veröffentlichungen, 46. Mainz, Philipp von Zabern, 1984. 355 × 255 mm. Pp. 102, figs. 126. ISBN 3 8053 0587 7. Price DM 120.

Formerly it could be said that the Egyptian *Book of the Dead* was the New Kingdom (and later) papyrus edition of the funerary literature that had earlier been copied on the inside walls of Old Kingdom pyramids and Middle Kingdom coffins. The considerable overlapping of these three groups of texts, however, and their occurrence on different types of sources have certainly tended to blur the distinctions between them, and now the fact that about eighty-five spells of the *Book of the Dead* are represented to some extent or other on the walls of 130 Theban tombs is another indication of how tenuous these modern labels are.

Dr Mohamed Saleh has provided a very fine publication of the *Book of the Dead* texts and vignettes from the New Kingdom nobles' tombs. Many of the texts and scenes are published here for the first time, so the beautiful hieroglyphic transcriptions, clear photographs, line-drawings, and plans showing the location of texts are all greatly appreciated. The publication follows the order of the spells in Naville's and Budge's editions with each spell consisting of: first, the hieroglyphic heading and descriptions of the vignettes from papyri together with notes identifying the manuscripts containing each spell, and, secondly, the presentation of the material occurring in the tombs, with texts, descriptions, and as much information as was deemed necessary.

Dr Saleh concludes the work with a number of interesting observations on the distribution of texts by frequency of occurrence, by tomb location, and by date (dynasty). Also at the end of the book are separate indexes (by *BD* spell number with reference to the identifying number of the tomb in which the spell occurs) for Eighteenth and Nineteenth/Twentieth Dynasties. There is also a list of the tomb sources by TT (Theban tomb) number giving the owner's name, title, date, and location of tomb. No list of the spells found in each tomb is provided, though this can easily be worked out. A list of abbreviations to supplement those used in the *Lexikon der Ägyptologie* completes the book.

The transcription of texts throughout are in horizontal lines from left to right, regardless of the fact that the original texts on both tomb walls and papyri were generally in vertical columns with the signs facing right. Line numbers of earlier publications are not cited, and presumably this is because such numbering, if consistent throughout the tomb, would have had little relation to the lines containing only *BD* texts, and, if by scene, might have appeared confusing. It would still have been useful to have had a stroke as indication of the length of columns, their beginnings and endings, and it would also have been helpful to have had some numbering (e.g. on pp. 12-13 or 41-4) to aid in locating words, phrases, or whatever in Saleh's edition. Even Naville had retained the line numbers of his principal text for each spell. The presentation of parallels on page 12 and frequently thereafter follows Naville's economical method of transcription, with exact and complete parallels presumed until variations are given or omissions are indicated by broken lines. Checking some of these renditions gives somewhat more confidence in the consistency and accuracy of Saleh's work than Naville's. I note, however, that for the parallels on papyri, it is usually Naville's or Budge's text that is cited, and users must be warned that both should be used with great caution.

For the texts of *Book of the Dead* spell 17 in Senedjem's tomb (p. 20) a different format is encountered. The texts in vertical columns facing left give the appearance of facsimiles, but actually are based on Bruyère's edition of the texts in *MIFAO* 88 which is cited by Saleh. The first group (from *MIFAO* 88, p. 53) can be compared to the photograph of the original in Saleh's figure 8 on page 14 (lines 7-10). The distribution of the lines obviously differs from the original and from Bruyère's copy, but on the other hand at least one significant improvement over Bruyère's copy was made (i.e. the phonetic determinative of *sn*). The second group on page 20 is precisely as copied in *MIFAO* 88, p. 55 with the unexpected omission of the first line of Bruyère's text, which is also clearly part of spell 17. The line numbers cited (Zeile 7-13) are neither Bruyère's nor Saleh's, but, as

explained on the top of page 15, refer to those of Nebseni's *BD* papyrus (Neville's Aa document). The third group is from *MIFAO* 88, p. 54, rather than 55 as labelled, and like the others was originally written from right to left, though the number of signs on this and the previous text does conform with those of the original.

The long text of G265 (TT 265) on pages 21–2 presents a related but somewhat more complicated problem. It has none of the *original* line markers or numbers, but it does have six line numbers (59 . . . 68, 88 . . . 104, 62Aa . . . 63Aa) used to account for gaps in the text. Again, it was stated on the top of page 15 that this text has lines 1–81 of the Aa document. The last two numbers are indeed from Nebseni's papyrus, but the other four can only be from Papyrus Ani (Eb). Apparently instead of following Neville here, Saleh used Budge's text of spell 17 which alternated selections from Eb and Aa. It is just this problem of inconsistency in line numbering that makes it necessary to cite Budge's 1898 text edition by its page and line in all our grammars and dictionaries. Even if this comparatively scarce edition were perfectly accurate, it would still be useful to have a new edition of the *Book of the Dead* with accurate and consistent line numbering. Dr Saleh chose to omit line numbers or markers to avoid confusing the matter further, but now it will become more difficult to incorporate this significant body of parallels into a 'definitive' edition of the *Book of the Dead*.

As stated in the Foreword, Dr Saleh completed most of the work on this book in 1974–5, and the notes and list of abbreviations would indicate that the major contribution to the work since that time was Hornung's *Das Totenbuch* (1979). Although several references to Allen's *Egyptian Book of the Dead. Documents in the OIM* (1960) are found (a text based primarily on two Late Period papyri), Allen's *Book of the Dead or Going Forth by Day* (1974) is not mentioned, yet this was based on all spells from all periods, and would thus be more suitable for the contemporaneous New Kingdom parallels.

Inevitably a large number of abbreviations are encountered in Saleh's work, and the list at the back was intended to cover only those books not cited or cited differently from *LÄ* usage. The list has a few omissions including the date of Davies, *Nakht*, and the entry *Wb* (pp. 10, 14, 25, 49) which appears in the text alternating with *WB* (pp. 52, 57). Campbell, *Miral. Birth* should probably be Campbell, *Mirac. Birth* though miraculous was spelt out on page 30. References to Champ. *Mon.* (p. 29) and Champ. *Not. Descr.* (p. 59) would have been better with Champollion spelt out in order to conform to *LÄ* usage. Instead of Neville, *Pap. Funéraire*, Neville, *Pap. Fun.* occurs on pages 72, 73. Finally, with regard to the bibliographic abbreviations, Gauthier, *DC* on page 93 should be Gauthier, *DG*.

For the designations of *BD* papyri manuscripts Saleh advises on page 9 that Neville's and Allen's 'Ziffern' are used, but the notes immediately below also mention 'Maherpri' and 'Chonsu' which are not explained anywhere in Saleh's, Neville's, or Allen's works. References to these two documents will be found in Hornung's *Totenbuch*. Concerning these 'Ziffern', it should be noted that they are earlier cited first with a following note to 'see Neville', but later Neville is cited and the 'Ziffern' are added in parentheses, with the result that some readers might be misled. For example, on p. 40 n. 310, the citation 'Neville, *Tb* II. 159 (Ae, Aa, Ea)' might appear to indicate that the Ea manuscript is in Neville, though this is clearly impossible.

The variants in the tomb texts (particularly the headings) and the wonderful detail of the scenes (vignettes) make these *BD* copies very interesting and deserving of our attention. The precise translations provided are also useful in indicating subtle yet significant differences in the variants. The frequency, location, and distribution of the texts with their variations over time, are surely important for our understanding of the *Book of the Dead*, and should be considered also with regard to the papyri manuscripts. The Index of the *Book of the Dead* manuscripts now being prepared by Malcolm Mosher should be a useful tool for further study of the logic in the arrangement of spells on these documents.

LEONARD H. LESKO

Aboudi Man of Luxor. By L. J. CRAVEN. 220 × 150 mm. Pp. (vi) + 107, frontispiece. Mere, The Ladymead Press, 1984. ISBN 0 9509549 0 X. Price £7.95.

Mohammed Aboudi is perhaps best known to Egyptologists for his entertaining (if less than wholly reliable) *Guide Book to the Antiquities of Upper Egypt and Nubia*.¹ Thanks to the efforts of

¹ The reviewer's own copy is dated 1944. Some later editions are entitled *Aboudi's Guide Book to the Antiquities of Egypt*.



Lydia J. Craven, we now have an account of the man's life, told for the most part in his own words. A well-known dragoman of the inter-war years, the son of a dragoman and grandson of a *reis* once employed by Mariette, Aboudi had travelled widely in Europe and for a time served as secretary to Cole Porter. At home, he lived through, or otherwise had close contact with, many of the greatest archaeological discoveries made in the Luxor area in modern times, several of which receive a passing mention in the book. The criticism might be made that interesting sidelights on the events and personalities of these years are so few, and, where they can be checked, somewhat unreliable. It would perhaps be churlish, however, to expect scientific accuracy and exhaustive detail in what is, after all, no more than a collection of reminiscences; to adapt Aboudi's own words, one cannot be disappointed with this story 'for not having been what it was not and still is not'. The book is charmingly written, and stands as a fitting memorial to a not-unadventurous life.

C. N. REEVES

Les Divines Épouses de la 18^e dynastie. By MICHEL GITTON. Annales littéraires de l'Université de Besançon, 306, Centre de recherches d'histoire ancienne, Vol. 61. 240 × 155 mm. Pp. 139, pls. 9. Besançon-Paris 1984. ISBN 2 25160 306 9. No price stated.

Few ancient Egyptian titles have been so misinterpreted as that of *hmt ntr*. It has been received view, supported by Sander-Hansen's study of the title, that it indicated the princess who was the 'heiress', whom the king had to marry in order to legitimize his rule. As a result the title has been attributed to queens who are never recorded with it, while kings or potential kings have had to be found as husbands for princesses bearing the title although no other evidence suggests such a marriage. In fact, the contortions practised by otherwise serious scholars to fit the theory to the facts have shown not only great ingenuity but sadly also a suspension of critical faculties. It is with relief therefore that one turns to the work of the French scholars, Yoyotte (e.g. *CRAIBL* 1961, 43-52; *BSFE* 64 (1972), 36-7) and Gitton (*Ahmes Néfertary, sa vie et son culte posthume* (2nd edn., Besançon 1981); *BSFE* 75 (1976), 31-46; *L'Á* 11, 792-812, with J. Leclant), who have re-examined the whole question of the dynastic connection. The work, especially Gitton's important study on Ahmose Nofretary, the first prominent royal *hmt ntr*, leads to the conclusion that the title was a priestly one concerned with the cult of Amun and that it had no dynastic significance. Gitton's latest book on the god's wives of the Eighteenth Dynasty is therefore extremely welcome in that it extends the detailed examination of the title to the whole of the Eighteenth Dynasty. In addition, since the title was held by royal women, the work is also to some extent a study of the Eighteenth Dynasty royal family.

The book is extremely clearly written and set out. After the Introduction, it is divided into four parts, each of which is separated into two or three chapters, which in some cases are subdivided into paragraphs. Every division is given a relevant title so the reader knows exactly where he is. He is not, however, so well served by the hieroglyphs in the book which, at least in my copy, often approach the illegible, the worst case being the texts on pages 88-9, unfortunately unpublished elsewhere; it matters less that plate viii has been printed upside down. A slight discrepancy has crept in between the page numbers given in the List of Contents for parts 2 and 3 and their actual location in the book. The following mistakes also need correction:

- p. 17 n. 38(b) *Epouse du Roi* is written in error for *Epouse du Dieu*, as correctly copied on p. 25 3(a).
- p. 19 n. 44(c) T. Théb. 23 should read T. Théb. 53.
- p. 36 n. 29 CGC 601010 should read CGC 61010.
- p. 68 n. 112  has been written in error for .
- p. 74. The statue of Sitioh (Cairo JE 37638) was found in the temple of Montu at Tod, not at Karnak.
- p. 95. The title of Senseneb is *Adoratrice du Dieu* and not *Epouse du Dieu*.

In the first part of the book, the author examines the title *hmt ntr* before the time of Ahmose Nofretary. In chapter 1, he draws together evidence for 'private' *hmt ntr*, who existed at least from the Middle Kingdom. In chapter 2, he deals with the questions of the identity of Ahhotpe, mother of

Ahmoose, and whether she used the title in her lifetime. He follows the now accepted view that Ahhotpe of the royal cache is the mother of Ahmoose, and hence *mwt nsw*, and not a wife of Amenhotpe I, who had no son to succeed him; Gitton rightly rejects the child Amenemhat as co-regent and son of Amenhotpe I (see also Robins, *GM* 30 (1978), 71–5). The Dra Abu el-Naga Ahhotpe, who is not *mwt nsw*, is to be dated earlier, before year 22 of Ahmoose, because of the form of the moon hieroglyph on the coffin and associated items. The evidence shows that Ahmoose's mother was never *hmt ntr* in her own lifetime; the title is first found on the stela of Iuf, which post-dates the death of Thutmose I and is probably placed in the reign of Thutmose III by the form of the moon hieroglyph. Its appearance may be explained by the prestige acquired by the office under Ahmoose Nofretary, Meritamun, and Hatshepsut, causing it to be given retrospectively to Ahhotpe.

Gitton's statement (p. 12 n. 16) that 'we do not know a single example in the New Kingdom where a *hmt nsw* was not also *hmt nsw wrt*' is misleading. It is true that women who are *hmt nsw wrt* are also attested with the simple title *hmt nsw*. However, Sitdjhuty, *hmt nsw* of Seqenenre Tao (Schiaparelli, *Relazione*, 1, 13–21, pls. 6–7), and Merti, Menwi, Menhet (Winlock, *Treasures of Three Egyptian Princesses*), and Nebtu (*Urk.* IV, 147.12, 150.16, 152.14–15, 602.9), all *hmt nsw* of Thutmose III, are not recorded as *hmt nsw wrt*. Further, there are those *mwt nsw* who do not appear as *hmt nsw wrt* in the reigns of their 'husbands' for whom other *hmt nsw wrt* are known. Examples are Mutnofret and Thutmose I, whose *hmt nsw wrt* was Ahmoose, Ese and Thutmose II, whose *hmt nsw wrt* was Hatshepsut, and Mutemwiya and Thutmose IV, for whom two *hmt nsw wrt* are recorded, Iaret and Nofretary. Perhaps these women were also simply *hmt nsw* at this time. This raises the question of what we mean by 'marriage' in the case of Egyptian kings who are polygynous. This uncertainty is recognized by Gitton (p. 13) when he questions Tetisheri's marriage with Tao, because on contemporary documents she never bears any title other than *mwt nsw*, but suggests it is very possible that she was *hmt nsw*. One must ask whether all women who had sexual relations with the king were entitled to be called *hmt nsw* or whether the title has a narrower definition; certainly, Kiya, in the reign of Akhenaton, is not given it. (One should also note in passing Vandersleyen's arguments in *GM* 63 (1983), 67–70 questioning the assumption that Senakhtenre, as well as Seqenenre, was called Tao.)

The suggestion (pp. 25–6) that the . . . *snt nsw hmt ntr Jchms* who owns a statue or stela fragment from Dra Abu el-Naga should be identified with Ahmoose Nofretary seems to me to present difficulties. The provenance and the funerary nature of the text . . . *tw ndm n mhtj n k; n . . . snt nsw hmt ntr Jchms nbt jm:h* suggest that it comes from Ahmoose's tomb and that she is dead. The form of the moon hieroglyph (omitted in Gitton's copy) dates it to before year 22 of Ahmoose but Ahmoose Nofretary was still alive in the reign of Thutmose I.

The second part of the book deals entirely with the office under Ahmoose Nofretary, overlapping therefore with the author's previous monograph. In chapter 1, he again discusses the Donation Stela (which is now in the newly opened room in the Luxor Museum), upholding his interpretation against that of B. Menu. Ahmoose Nofretary gives up the office of second prophet of Amun and is credited with property which from then on is to be attached to the office of *hmt ntr* and transferred by heredity. There is no explanation as to how the queen got the office of second prophet, but Gitton puts forward the hypothesis that Ahmoose, anxious to restore the cult of Amun but too occupied with fighting, gave exceptional powers to Ahmoose Nofretary in the Theban priesthood and temples with the title *hmt ntr*, adding to it the more recent title of second prophet; the title *hmt ntr* brought no fixed revenues with it and the aim of the transaction recorded on the Donation Stela was to remedy this situation.

In chapter 2, the author discusses the *pr* which according to the stela the king built for the *hmt ntr*. Its whereabouts are not stated but the temple *Mn-St* springs to mind and this is confirmed to some extent by later texts which locate a *pr hmt/dw:t ntr* in this locality. Senu, known from the royal cache at Deir el-Bahri (CG 61010), has the title *jmj-r; pr (wr) n hmt ntr* and was probably steward of the foundation. The large number of items of apparel listed on the Donation Stela may have been intended for priestesses attached to the foundation.

In chapter 3, Gitton outlines the religious and ritual activity of Ahmoose Nofretary and repeats his hypothesis stated in his earlier book that the responsibility of re-establishing the cult after the vicissitudes of the Second Intermediate Period was in part the work of Ahmoose Nofretary.

In part 3, the author looks at the holders of the title after Ahmoose Nofretary beginning in chapter 1 with the immediate successors. Sander-Hansen, followed by the majority of authors, lists Meritamun, Sitamun, Sitkamose and Ahhotpe II, of whom only one, Ahhotpe, supposedly had a

daughter, Ahmose Nebetta, who married Thutmose I. It is now known that Ahhotpe does not exist and that Ahmose Nebetta must be placed earlier in the dynasty. Gitton argues that Sitkamose is shown by her name to be the daughter of Kamose and that she may have married Ahmose, who would then have had two principal wives. She had the title *hmt ntr* only on posthumous documents and it may have been given to her after death, as with Ahhotpe. It should be pointed out that if she is the daughter of Kamose, who it is generally accepted was not succeeded by a son, she should not be *snt nsw*. This title is found only in one of the titularies written by the restorers on her mummy wrappings and once more the question must be raised as to how accurate these texts are. In addition, it is only from this late source that she is attested as *hmt nsw wrt*.

The problems of the Meritamuns of the Eighteenth Dynasty have been much discussed. In the early Eighteenth Dynasty, Gitton opts for Meritamun owner of TT 358, who is *hmt ntr*, as the daughter of Ahmose and Ahmose Nofretary and the wife of Amenhotpe I (probably shown on the Qasr Ibrim stela (p. 55 n. 52); see also Robins, *GM* 56 (1982), 79–80) and an earlier Meritamun who is represented by the mummy in the royal cache (CG 61052), who is *snt nsw snt nsw* but not *hmt ntr* and about whom nothing certain is known. He mentions (p. 51 (5)) the text on Wilkinson's fragment which has been the subject of so much discussion and misunderstanding, but makes no further comment on it. Its form suggests that it comes from a coffin, sarcophagus, or canopic chest. It seems to me possible that this could be the same item as the one to which Winlock's fragments of a sarcophagus or canopic chest associated with TT 358 belong. The inscribed piece with the name Ahmose [Meritamun?] was found by Winlock outside the tomb but fitted together with pieces inside. Since one piece had obviously left the tomb at some time, others, including Wilkinson's fragment, may have done so also.

Gitton disagrees with Vandersleyen that Sitamun was the daughter of Kamose. She is connected with Meritamun in the Ritual of Amenhotpe I and on a statue at Karnak, while on another statue, which probably shows Mut holding Amenhotpe I on her lap, Ahmose Nofretary is depicted on one side of the seat and Sitamun on the other. She is called *snt nsw snt nsw hmt ntr*, which seems to show that she is the daughter of Ahmose Nofretary and Ahmose and sister of Amenhotpe I. The importance of this statue has usually been overlooked in discussions about Sitamun.

Chapter 2 deals with the holders from Meritamun to Nefrure. Gitton correctly dismisses Ahmose wife of Thutmose I from this group, leaving Hatshepsut and Nefrure. It is implied (pp. 60–1) that *snt nsw* is a title reinforcing *hmt ntr* and may not necessarily indicate a king's sister. In fact, one can be *hmt nsw* and not *snt nsw* and vice versa. Ahmose is the only queen who is *snt nsw* and not *snt nsw*. All other Eighteenth Dynasty queens who are *snt nsw* are also *snt nsw* and those who are not *snt nsw* are not *snt nsw*. Therefore, *snt nsw* is best taken as meaning 'king's (half-)sister' (see also Robins, *GM* 62 (1983), 68–9). Since *snt nsw* can mean half-sister (e.g. Hatshepsut is *snt nsw* in relation to Thutmose II and Nefrure in relation to Thutmose III), Ahmose's mother is not necessarily Senisonb.

Gitton (p. 61) makes the suggestion that Ahmose is also the mother of Amenmose, Wadjmose, and Nefrubity. Since the only appearances of Nefrubity are with Ahmose and Thutmose I, this is fair enough, but Wadjmose was more probably the son of Mutnofret, since statues of her were dedicated by Thutmose II in the mortuary temple of Wadjmose (G. Maspero, *Guide du visiteur au Musée du Caire* (3rd edn., Cairo, 1914), 124, no. 444; CG 572). It seems likely that Thutmose I married Mutnofret when he was still a private person and that he only married his (half-)sister when he was made heir to the throne, since this type of marriage was not usual among private people but typical of kings (Robins, *GM* 62 (1983), 71). If so, Wadjmose's elder brother Amenmose is likely to have been Mutnofret's son also, as we know Thutmose II was. Perhaps one of the reasons why Amenhotpe chose Thutmose I to succeed him was that he was already supplied with three male heirs.

Gitton (p. 69) suggests that Nefrure wears the nemes in the small temple of Batn el-Baqqera and on the stela from Serabit el-Khadim. The first is very damaged, but from the drawing (*ASAE* 39 (1939), 714, fig. 71) she could possibly be wearing a fillet and streamers (costume of the god's wife?) and in the second, surely she wears some form of long wig with a vulture headdress. It would be very strange to find a princess wearing the nemes.

There are problems in identifying the *hmt ntr* on CG 34013, even if we ignore the traces of Nefrure's name which Legrain thought he saw in the cartouche. Gitton (p. 70) suggests Meritre Hatshepsut, second *hmt nsw wrt* of Thutmose III, and Meritamun, daughter of Thutmose III, as

candidates, but both seem to date to the latter part of his reign and are unlikely to have been present on a stela dating to the beginning of his sole rule. Thutmose's mother Ese is, in fact, once attested as *hmt ntr* in a long titulary, but the use of *hmt ntr* as sole title, as on CG 34013, is typical of Nefrure and not of later *hmt ntr*. On the other hand, support for Nefrure's death in her mother's reign would seem to be found in erasures of her figure from scenes in Deir el-Bahri and probably at Buhen (Karkowski, *Études et travaux*, x (1978), 73).

Chapter 3 deals with the title after Nefrure. At her death, there seems to have been no queen or princess exercising the office. Gitton shows that it was not held by Thutmose III's *hmt nsw wrt* Sitioh, but that it is attested for Meritre Hatshepsut, mother of Amenhotpe II, Meritamun, daughter of Thutmose III, and Tiaa, mother of Thutmose IV. Although Mutemwiya, mother of Amenhotpe III, is often said to be *hmt ntr*, she is in fact not attested with the title, which does not appear again associated with a woman of the royal family until the Nineteenth Dynasty.

Gitton should also have considered Ese, mother of Thutmose III, for inclusion among his *hmt ntr* of the reign of Thutmose III. The title occurs on a fragmentary block from the mortuary temple of Thutmose III (Ricke, *Totentempel*, 30 n. 5). The full titulary runs *jrjt-pct wrt hswt hmt ntr hmt nsw wrt mwt nsw (3st) nh-tj*. The queen is *nh-tj*, but the provenance raises the possibility that the scene is posthumous; the use of the title may be so as well. Since Ese is also called *hmt nsw wrt*, this effects Gitton's statement that she has the title only once, on a stela where her name is not original (p. 73 with n. 135).

Gitton notes (p. 76) that on the staff from the tomb of Amenhotpe II bearing the name of Meritre Hatshepsut, the title *hmt ntr* is placed in initial position before *hmt nsw wrt* and *mwt nsw*. There is nothing unusual in this, since *hmt ntr* normally precedes *hmt nsw wrt* and *mwt nsw* and follows *srt nsw* and *snt nsw* in queens' titularies. As Meritre Hatshepsut lacks the latter titles, *hmt ntr* is naturally exposed at the beginning of the titulary.

On the statue of Neferpert (pp. 75–6), *sn* must be used to mean 'husband', a sense it can have as a free kinship term (Robins, *CdÉ* 54 (1979), 202–3). It is also used by Teye, daughter of Yuya and Tjuyu, to refer to Amenhotpe III, who is clearly not her brother (Petrie, *Illahun, Kahun and Gurob*, pl. 24, no. 7).

With reference to the statue of Huy in the British Museum (EA 1280), the identification of Meritre Hatshepsut as the *hmt ntr* born to Huy is surely correct (see also Robins, *GM* 56 (1982), 82–3), but it is interesting to speculate why an obscure prince, Menkheperre, is shown rather than the future Amenhotpe II, known to be the son of Meritre.

In part 4, we leave the *hmt ntr* and consider the divine adoratrices and the superiors of the recluses of Amun in the Eighteenth Dynasty. Apart from the use of *dwst (ntr)* in hieratic texts as an equivalent of *hmt ntr*, Senseneb, daughter of Hapusonb and wife of Puyemre, and Huy known from the British Museum statue (EA 1280) are called *dwst ntr*. Gitton speculates that they could have filled the function of *hmt ntr* during the extreme youth of Nefrure or at her death. He next discusses the recluses of Amun. The translation of *hnr* as 'harim' or 'concubines' presents problems, although *pr-hnr* refers to the harim in the Ramesses III conspiracy. Deriving from the verb *hnr* 'to imprison' it means all those who are to some extent secluded either in a palace or temple. The recluses of the temple, headed by a superior of the recluses, were connected with the musical activities so beloved by the gods. However, the majority of the *šm'jtt nt jmn* were not recluses as they include almost every woman who was anybody in Theban society. The recluses may have formed a permanent choir, and at times they were numerous enough to be divided into phyles. The superior of the recluses may well have carried out the actual function of the *hmt ntr* in temple ritual, and this would account for the appearance of a *hmt ntr* in scenes in the temple of Amenhotpe III at Luxor at a time when no royal woman held the title.

In conclusion, the god's wife was responsible for an important foundation, the *pr hmt ntr* and its personnel, the recluses. The office was to be transmitted by heredity, separately from the succession to the throne; in fact, it was passed on from mother to daughter only three times, from Ahmose Nofretary to Meritamun, from Hatshepsut to Nefrure and Meritre Hatshepsut to Meritamun. The later holders probably no longer carried out the functions of the office, which were taken over by the divine adoratrice or the superior of the recluses of Amun. It seems to me that this conclusion is strengthened by the pattern of use of the title *hmt ntr*. Ahmose Nofretary, her daughter Meritamun, Hatshepsut, and Nefrure all use *hmt ntr* as a sole title, while god's wives later in the dynasty use it in a

string of titles only and not alone. If the latter no longer carried out the functions of the office, leaving it to a priestess, this may account for the less prominent use of the title.

The length of this review reflects the importance of the book. It is vital that scholars cease to give any dynastic significance to the title *hmt ntr*, as it has been plainly demonstrated that it will not bear such an interpretation. This has consequently removed one of the main props of the 'heiress' theory, which should now be discarded (Robins, *GM* 62 (1983), 67–77). The book also contributes to studies of the royal family of the Eighteenth Dynasty. In the last decade there has been an upsurge of interest in the problems of the early Eighteenth Dynasty, generating heated arguments on the exact placing of this or that member of the royal family. Very often, surviving evidence is too sparse to allow definite solutions, leaving the field open for a variety of hypotheses. Gitton's reconstruction of the early Eighteenth Dynasty royal family will therefore almost certainly be disputed at different points by different scholars; this will not detract from the contribution made by the work to the problems involved.

GAY ROBINS

Excavations at Saqqara north-west of Teti's Pyramid. Vol. I. By N. KANAWATI, A. EL-KHOULI, A. MCFARLANE, and N. V. MAKSOUH. 290 × 217 mm. Pp. 80, pls. 47. Sydney, Macquarie University, The Ancient History Documentary Research Centre, 1984. ISBN 0 85837 547 8. Price £25.

Despite the vast amount of excavation carried out at Saqqara over the past 150 years, new discoveries, both great and small, are still being made, emphasizing the richness and importance of the necropolis with perhaps the widest chronological range of use of any in Egypt. In addition to his work at Akhmim, Dr Kanawati has found the time to carry out excavations in the area of Saqqara known as the Teti Pyramid Cemetery, this time in conjunction with Dr Aly el-Khouli of the Egyptian Antiquities Organization.

The nature of the work is somewhat different from that at el-Hawawish. The latter could be termed 'rescue epigraphy' in view of the urgent need to record unpublished standing monuments before they suffer further damage; Kanawati's work at Saqqara involves the uncovering of new monuments which, it has to be admitted, would probably be reasonably safe while still buried. The monuments uncovered comprise seven small inscribed mastabas of the later Old Kingdom (Smedenti, Ihyemzaf, Memi, Tjetetu, Tjetji, Irenakhet, and Iries), two uninscribed mastabas, and a number of later burials and finds. The chapels of Smedenti and Tjetetu were originally copied by the Society's Saqqara Epigraphic Survey. The actual area examined is to the north-west of the large mastaba of Mereruka; other excavations in the area by Dr Mahmud Abd el-Razik have indicated the presence of further tombs east of those excavated by Kanawati. The Teti Pyramid Cemetery, already rich in finds, clearly has many further surprises for us.

The offering chapels published in this volume contain principally false doors, with a few simple scenes in relief, and one still in its painted outline. There is very little in the way of outstanding artistic achievement in these monuments, nor do we gain any obvious historical, biographical, or administrative insights into the society that built them. Their great importance seems to lie in this simplicity, as they are among the first tombs of relatively minor officials of the Old Kingdom to have been found at Saqqara. The only comparable material also comes from the Teti Pyramid Cemetery, among the partly unpublished and certainly less well-recorded work of Firth and Gunn (*Teti Pyramid Cemeteries*, and archives in the Griffith Institute, Oxford). Otherwise, tombs of this type are known to us principally from Giza, among the small mastabas fitted around the larger ones and investigated by Reisner and Junker. The distribution of tombs of different classes of officials around the Memphite cemeteries has long been of interest to me, and the appearance of such relatively minor officials in the cemeteries of Saqqara may in fact help us to resolve this particular problem.


The appearance of monographs from the same author on Hawawish and Saqqara in the same year invites comparison of methods. The descriptions of the chapels at Saqqara follow the pattern established in the Akhmim volumes—owner, dating, architecture, burial apartments, scenes, and finds—and we are promised further studies on specific subjects. I find the standard of documentation more variable than in the earlier volumes. Kanawati states that his general rule has been to use


photographs as his basis with line-drawings of the less clear areas; it must be stated that the photographs are generally clear and very well reproduced. I do believe that drawings should be the essential basis with photographs where possible, and there are some plates where one cannot clearly see all the decoration (pl. 24, 35 right); tracings are not subject to the problems of using a wide-angle lens. There are also several problems with the drawings included. There is the occasional error, such as the omitted owl in the title *imy-r st hnty-š pr-ꜥ* on the right outer jamb of plate 6 (cf. photograph, pl. 5), while certain lines appear to have been straightened. It appears that the thickness of the lines is often too great for the degree of reduction used, so that they occasionally merge into one mass, such as plates 6, 10, and 18. Part of this is due to the adoption of the so-called 'sun and shadow' convention for indicating relief. Although the use of this is well-established practice, I think it has many disadvantages. It can be misleading about the thickness of a line, and most important of all, it forces the epigrapher into more interpretation than he should be making. Its appeal is primarily aesthetic, but we should be dealing in objectivity; so long as the drawings are accompanied by a reliable text—as they are here—the relief type need not be indicated as such. The two drawings on plate 15 (the second made by the Society) illustrate the difference.

The major interpretative section in the volume is the discussion of the relative dating of the Teti Pyramid Cemetery; to this should be added Kanawati's further remarks in *GM* 83 (1984), 31–8. This chapter reveals the degree of complexity in this cemetery, and the inclusion of a map of the area with all tombs marked would have been a useful adjunct to the discussion. The author concludes that the mud-brick mastabas at the west end of the cemetery are probably earlier than the reign of Teti and are part of the larger North Saqqara Fifth Dynasty necropolis, while the area to the north of the tomb of Mereruka dates from the late reign of Teti to the middle of the Sixth Dynasty. I think that there can be no doubt that most of the larger mastabas in this latter area are no later than the reign of Pepy I (Nikauizezi, Seshemnefer, Meru, Mereri, Shepsipuptah), but I would be more cautious about the smaller tombs, such as those published in this volume. As observed above, they have affinities to the First Intermediate Period material excavated by Firth and Gunn; a number of the false doors have panels of a 'T' shape, typical of the reign of Pepy II and later. It seems possible that the further north one looks from the pyramid, the later the tombs become: the large mastabas of the most important individuals (Kagemni, Mereruka, etc.) belonging to the reign of Teti to early Pepy I, followed by the not quite so elevated officials such as Meru, Mereri, and Shepsipuptah who are a little later; the northern reaches of the cemetery would be filled somewhat later, either when the area had lost its original prominence and the tombs of the most important men are elsewhere (reign of Pepy II), or when the Teti Pyramid Cemetery again became popular at the end of the Old Kingdom. Certain details also suggest that some monuments may be late Sixth Dynasty or even later, in particular the appearance of the formula *hꜥp di in nzwt hꜥp di in Wsir* on plates 22 and 29, and the *wꜥꜥt* eyes on the false door in plate 12.

It is good to see that care has been taken with the clearing and documentation of the secondary burials and finds discovered in the concession, since very little else is presently available in the way of detailed reports on such material, which has all too often been regarded as a nuisance to be ignored on the way to more promising finds. These burials are principally of the Eighteenth Dynasty and may be precursors to the greater redevelopment of this cemetery in the Nineteenth Dynasty. Although information about the position and depth of the finds is given, a simple cross-section giving visual expression to this might be a worthwhile idea.

The following more detailed comments may also be of interest:

- p. 4 The preface seems an inappropriate place for a general description of the architectural and decorative techniques, in so far as it is easily overlooked.
- p. 15 The signs  on plate 6 read as *sdꜥwt nꜥr* may be better understood as *sdꜥwtꜥ mꜥꜥt nꜥr*.
- p. 29 The titles with the element *r-ꜥ* are also known from the Memphite region in the late Old Kingdom, in the tombs of Khabauchnum at Saqqara and Meru and Sebeky at Heliopolis. In the case of the latter, we may imagine them being concerned with routes towards the Gulf of Suez.
- p. 35 The non-recording of the guide-lines and corrections is regrettable, since they might give valuable information about the processes of drawing.

- p. 41 Plate 23: the title *wr zwnw* is sometimes followed by , and so should probably be read as a separate title, *wr zwnw smsw*, 'senior physician'. Does the writing of \mathcal{Q} in the name *nb-m-dr·i* indicate a late date?
- p. 50 Plate 29: note the unusual continuation of the text from the right inner jamb to the left.
- p. 51 The location chart in plate 28 is a welcome addition, but it does not tell the reader which of plates 31-8 are best consulted for a given scene.

This book is the first real record of the excavation of an Old Kingdom cemetery of less significant individuals at Saqqara. The overall standard of the documentation is a little less pleasing than in his other work, and I cannot help but feel that in this case Kanawati has cut the occasional corner in his desire to publish his results quickly. The need to make the information available after a reasonable time is too frequently ignored with regard to field-work, but neither should it assume too great an importance. That said, the author has produced an acceptable report on his material and is to be congratulated on adding to our knowledge of one of the great sites of Egypt.

NIGEL STRUDWICK

Amarna Reports I. By BARRY J. KEMP. Egypt Exploration Society, Occasional Publications 1. 295 × 207 mm. Pp. viii + 211, figs. 92. London Egypt Exploration Society, 1984. ISBN 0 85698 094 3. Price £14.

Amarna Reports II. By BARRY J. KEMP. Egypt Exploration Society, Occasional Publications 2. 295 × 207 mm. Pp. ix + 204, figs. 109. London, Egypt Exploration Society, 1985. ISBN 0 85698 098 6. Price £16.

These two volumes of *Amarna Reports (AR)* signal a departure in the publishing traditions of the Egypt Exploration Society. In an attempt to provide site reports directly to the public as quickly and as cheaply as possible, Barry Kemp has produced these volumes through the text-formatting facility of the Cambridge University Computer Services. The result is a justified text with letter-quality characters (Latin and italic) printed on a dot-matrix printer, interspersed with line and halftone text-figures, with conventional page numbering and chapter headings and bibliographical (Harvard) references. *AR II* also contains customized computer hieroglyphs similar to the Gardiner fount.

AR I comprises a preface and synopsis by Kemp of the results of five seasons' work at the walled 'Workmen's Village', partially excavated by the EES in the 1920s (see *JEA* 66-9); six sections on the 1983 excavations to the south and south-west of the Village, with acknowledgement of the field staff involved with the specific areas; and a further eight sections of technical reports and thematic discussions. Thus the material is gathered together and described without the need to hold back publication for a 'monolithic' volume. The site is well suited to this kind of publication, being a series of well-defined units of comparatively little stratigraphic depth. *AR II* follows this pattern, with the 1984 season being described in detail, and further technical reports.

Kemp's summary of previous work at the Village (*AR I*, chapter 1) considers the evolution of ideas concerning its nature and function within the context of the Amarna site as a whole, and notes the possibility that the original artisans' quarters during the reign of Akhenaten were taken over or reoccupied after his death by guard patrols stationed near the royal tombs. The function of the 'tomb' chapels adjoining the cemetery on the east side of the Village is also reconsidered: the largest (the 'Main Chapel') is shown to have been in fact a miniature temple approached from the main thoroughfare leading to the Village gate.

The Main Chapel and smaller outlying chapels are dealt with in detail in *AR I*, chapters 2 and 3, and in *AR II*, chapters 1-4. The descriptions in *AR II* are necessarily more complete, and the careful and detailed ground plans show the respective excavations near to completion. The second volume also includes a description of the painted decoration from the walls of the Main Chapel, which should be read in conjunction with the technical report (by Gay Robins, chapter 7) on the hieroglyphic signs found among the fallen plaster fragments. The result is a partial reconstruction of the decoration of one wall of the Chapel, showing a frieze of inverted lotus flowers surmounting a winged sun-disk over the doorway.

AR II, chapter 4 concerns the evidence for the quartering of livestock at the Village, and concludes from the faunal evidence, including the study of coprolites from the area, that the chief domesticated animal was pig.

AR I, chapter 5 and *AR II*, chapter 7 are an account of the concurrent project to map the main town site, from the North City (Map Sheet 1) to the southern outskirts and 'river temple' at the modern village of Hagg Qandil (Map Sheet 8; see *JEA* 69, 22, fig. 7) at a basic scale of 1 : 1000, with contour lines at 50-cm vertical intervals. This work incorporates all the available data from previous work, including air photographs taken by the RAF in the 1930s, nineteenth-century reconnaissance maps published by investigators such as J. G. Wilkinson and K. R. Lepsius, and unpublished survey sheets from the work of Peet and Woolley for the EES.

The technical papers are in part accounts of specialist work done in the field, and in part analytical reports on collections recovered for study in this country. *AR I*, chapter 8 is an account by Ian Mathieson of resistivity-survey methods used at Amarna, which has proved particularly promising in the detection of large-scale features (e.g. quarry edges) buried beneath the desert crust, and indicates the cost-effectiveness of remote-sensing on large sites such as Amarna. The next stage is to see whether the method can be used with the same precision and reliability on silt-laden sites of the Nile valley and delta.

Chapter 9, by Ian Shaw, attempts to draw conclusions from a statistical consideration of the name designs on ring bezels, including those found in earlier seasons. Two sets of comparisons are drawn: first, between the bezels from inside, and those from outside, the Village, recovered during the 1921-2 and the 1979-83 excavations respectively; secondly, between the total assemblage of bezels from the Village and that from the main city. The chief conclusions drawn are that (a) the water-storage area outside the Village might be of early date (possibly a watering station *en route* to the royal and noble tombs, at which the Village was later sited?); (b) the high proportion of later cartouches from the Village compared with the city, despite the shorter reigns of the kings concerned, argues that it was occupied after the city had begun to decline. Although careful attention is paid to the find context of the bezels, the evidence for (a) is said to be a fragment of Amenophis III (?) found in a later floor of the water-storage area. Might this, however, be due to redeposition from an earlier context elsewhere on the site?

AR I, chapter 10, by Pamela Rose, and *AR II*, chapter 8, by Rose and Paul Nicholson, should be read together. The first is a description of the methods used to manipulate the ceramic record (an average of nearly 200,000 sherds per season) by comparing dimensions, wares, and treatment against find locations. The groupings show clear connections between form and location, and a possible correlation between social groups and the pottery they used. The second report describes an attempt to define a relatively small assemblage of forms according to seventeen native and three foreign wares. Nicholson gives an introduction to thin-sectioning technique and an analysis of the different ware groups by point-counting. The chief practical result is that point-counting did not suggest that any additional ware groups need be added to the basic repertoire.

AR I, chapter 11, by Howard Hecker, contains a breakdown of species identified from a bone assemblage collected from spoil dumps of previous work as well as from current set-piece excavations. Hecker comments on the extremely good preservation of most bones. Several wild (game?) species were identified (oryx, fox, hyaena), as well as the commoner domestic animals. The most interesting result is the high proportion of foraging livestock (goat and pig) and the likelihood (cf. chapter 4) that pigs were reared and slaughtered at the Village. It seems, however, that pig fat was imported to Amarna from the delta (see *AR II*, chapter 6). The reason for the high proportion of foragers is likely to have been the rough terrain and sparse vegetation, with a plentiful supply of organic refuse. Cattle bones were also common, but these are likely to have been brought to the Village as cuts of beef.

Chapter 12, by Linda Hulin, is a discussion of the tall 'cultic' jar stands from both chapels and houses. Only some of these showed signs of scorching within the bowls (see *AR II*, chapter 9 for the possible use of the *dôm*-palm nut as fuel). The use of gypsum plaster in cultic contexts is noted, and the possible domestic use (or cult use within the home) is considered.

Chapter 13 (V. Switsur) begins with a useful summary of the way radio-carbon dating has been applied to Egyptian samples, and proceeds to discuss the dating of organic samples from Amarna, where climatic and stratigraphic conditions have combined to give an excellent series. The results

obtained from five samples matched the established chronology, and also vindicated a new calibration curve based on measurement of Irish oaks.

Chapter 14 (Charles French) is an analysis of sediments deriving from (a) silt and marl bricks and (b) natural features (wâdy, alluvial, and desert deposits) based on grain size. The chief conclusion from an archaeological viewpoint was that there seemed to be no connection between brick composition and constructional use. Another fact of geoarchaeological interest was that silt bricks at Amarna were less uniform in composition than samples taken from Karnak for comparison, due to local characteristics of the alluvium. Chapter 15, also by French, introduces the regional geomorphology of Amarna and assesses the evidence of prehistoric occupation. The high desert plateau within half a kilometre of the escarpment showed no trace of human presence, but the lower desert (within the 'bay' of cliffs) was a promising area for Middle/Upper Palaeolithic sites, particularly north and east of the Village.

AR II, chapter 6 (M. A. Leahy) presents the hieratic jar labels found up to 1982. The spread of year dates from over one hundred examples are of Amenophis III (1), Akhenaten (23), and a successor (9). The distribution of labels should be considered in the light of remarks on that of the ring bezels, *AR I*, chapter 9.

The last two chapters (9 and 10, by Jane Renfrew and Gillian Eastwood) are preliminary reports on the palaeobotanical record and the textiles respectively. As with all other organic remains, the plant finds at the Village were well preserved, though none had been deliberately stored (the garden beds in Chapel 531 apparently do not form part of the study sample). Seeds were found in all bulk soil samples examined, and in animal dung collected from the pens south-east of the Village. Emmer and barley were human staples, and emmer was also used as fodder. Pulses (lentils, peas) and fruits (water-melon, dates) were found, and grapes were probably being consumed on site rather than pressed. It is assumed that vegetable foodstuffs will have been grown in the valley and carried to the Village, although again the presence of garden plots in the chapels does raise the possibility of market gardening on a small scale using imported alluvium. Flax seeds were also recovered below the animal pens south-east of the Village, and linen was the commonest textile found. A little wool and spun goat hair also occurred. Vegetable dyes were woad (probably) for blue, madder for red, and cloth was double-dyed with both to produce purple.

I read both these volumes with great interest. As space in periodicals becomes more restricted, and the waiting period grows longer, the attractiveness of publications which enable more detailed discussion, and which can be produced within a year of the field-work, becomes obvious. A few suggestions may be made: the site recording is based on the Harris Matrix system, now widely used on sites in this country and gradually being adopted on Mediterranean/Near Eastern sites also. Thus on site plans the contexts (deposits, units) are shown with their identifier, in this case a numerical value of one to four digits. In many cases, however, it is not clear without reference to the text whether one unit lies over or under an adjacent one, particularly since the numbering is deliberately non-sequential, i.e. 32 does not necessarily overlie 33 (see *AR II*, figs. 1.12, 2.1). This uncertainty is compounded by the practice of showing squares in several different stages of excavation on the same plan (*AR I*, fig. 4.2; *AR II*, fig. 1.2). One solution might be to include the relevant part of the matrix itself on the plan drawing. In *AR I*, chapter 7 and *AR II*, chapter 5 ('The city survey'), it would have been helpful if scales had been included throughout (although grid lines are promised for the final publication). This applies also to some of the ground plans in *AR II* (figs. 4.1, 4.5, 4.6). The inclusion of spot heights on all ground plans might have been a good idea.

This series is to be heartily welcomed. The format, despite limitations on print size and variety, is unfussy and pleasing, and the medium paper quality allows half-tone illustrations in the text with reasonable definition. Finally, the fact that the writer corrects or modifies his own text directly, and arranges his own format, means that more control is exercised over the final production, with correspondingly fewer printing errors. The value of communicating information rapidly in this form, from sites of proven importance such as Amarna, is clear at a time when there is so much renewed interest in the urban archaeology of the Nile valley.

DAVID JEFFREYS

Other books received

1. *The British Museum Book of the Rosetta Stone*. By Carol Andrews. 215 × 145 mm. Pp. 64, illus. New York, Peter Bedrick Books, 1985. ISBN 0 87226 033 X. \$10.95.
2. *Hieroglyphs*. By Norma Jean Katan. 215 × 138 mm. Pp. 80, 35 figs. British Museum Publications, 1985. ISBN 0 7141 8060 2. £3.50.
3. *Ancient Byblos Reconsidered*. By Alessandra Nibbi. 230 × 155 mm. Pp. 125, pl. figs. 10, figs. 15. Oxford, DE Publications, 1985. ISBN 0 9510704 0 1. Price £9.50.
4. *Wenamun and Alashiya Reconsidered*. By Alessandra Nibbi. 230 × 155 mm. Pp. 240, figs. 18. Oxford, DE Publications, 1985. ISBN 0 9510704 1 X. Price £12.50.
5. *Hieroglyphenschrift und Totenbuch. Die Papyri der Ägyptischen Sammlung der Universität Tübingen*. By K. Buroh, P. Jürgens, R. Müller-Wollermann, S. Schloz, and J. Zeidler. 210 × 148 mm. Pp. 104, illus. pls., 22 figs. Tübingen, Attempto, 1985. ISBN 3 92 1552 68 0. No price stated.
6. *Museo Barracco di Scultura Antica. La Collezione Egizia*. By G. Careddu. 265 × 185 mm. Pp. xviii + 63, pls. 51 (several in colour). Rome, Istituto Poligrafico e Zecca dello stato, 1985. No price stated.
7. *Textes des sarcophages égyptiens du Moyen Empire*. By P. Barguet. 195 × 125 mm. Pp. 704. Les éditions du Cerf, 1986. ISBN 2 204 02332 9. Price 270 f.
8. *Pharaoh's People*. By T. G. H. James. 195 × 128 mm. Pp. 282, pls. 16, figs. 27. Oxford University Press, 1985. ISBN 0 19 281883 X. Price £5.95 (paperback).
9. *A Dictionary of Egyptian Gods and Goddesses*. By George Hart. 198 × 128 mm. Pp. 229, many text figs. London, Routledge and Kegan Paul, 1986. ISBN 0 7102 0167 2. Price £5.95 (paperback).
10. *The Pyramids of Egypt*. By I. E. S. Edwards. 192 × 128 mm. Pp. xx + 311, pls. 63, figs. 60. London, Penguin Books Ltd., 1985. £4.95 (paperback).
11. *Cahier de recherches sur l'Égypte et le Soudain anciens*. No. 7. 270 × 220 mm. Pp. 135, pls. 16. Presses Universitaires de Lille, 1985. ISBN 2 85939 261 0. Price 180 f.
12. *Bulletin, Société d'Égyptologie Genève*. Nos. 9-10. 225 × 165 mm. Pp. 355, several figs. Geneva, 1985. Price 60 Sw. f.
13. *Ancient Egyptian Designs*. By E. Wilson. 275 × 220 mm. Pp. 26 + 100 fig. pp. British Museum publications, 1986. ISBN 0 7141 8061 0. Price £4.95.
14. *The Mendes Maze*. By V. A. Hibbs. 235 × 155 mm. Pp. 241, pls. 170. New York, Garland Publishing Inc., 1986. ISBN 0 8240 6858 0. Price \$40.
15. *Image of the World and Symbol of the Creator*. By R. B. Finnestad. Studies in Oriental Religions, vol. 10. 240 × 170 mm. Pp. x + 174, pls. 9. Wiesbaden, Otto Harrassowitz, 1985. ISBN 3 447 02504 2. Price DM 44.
16. *Studien zum Ägyptischen Königsdogma im Neuen Reich*. By R. Moftah. Deutsches Archäologisches Institut, Abteilung Kairo, Sonderschrift 20. 230 × 160 mm. Pp. xiv + 370. Mainz am Rhein, Verlag Philipp von Zabern, 1985. ISBN 3 8053 0833 7. Price DM 88.
17. *Studien zu dem Stempelsiegeln aus Palastina/Israel*. Band I. By O. Keel and S. Schroder. Orbis Biblicus et Orientalis, 67. 235 × 160 mm. Pp. 120, figs. 80. Universitätsverlag Freiburg, Schweiz, 1985. ISBN 3 7278 0336 3. Price 29 Sw. f.
18. *Egyptian Life*. By Miriam Stead. 215 × 215 mm. Pp. 72, figs. 93 (many in colour). British Museum Publications, 1986. ISBN 0 7141 2040 5. Price £4.95.
19. *Egyptian Magic*. By C. Jacq. 217 × 155 mm. Pp. 162, figs. 46. Aris and Phillips Ltd., 1985. ISBN 0 85668 299 3. Price £5.95.
20. *Ausgewählte Texte der Isis- und Sarapis-Religion*. By Maria Totti. Subsidia Epigraphica, XII. 208 × 146 mm. Pp. 231. Hildesheim, Georg Olms, 1985. ISBN 3 487 07678 0. Price DM 34.80.
21. *Le Voyage de la Déesse libyque*. By U. Verhoeven and P. Derchain. Rites Égyptiens, V. 232 × 165 mm. Pp. 107, pls. 8. Brussels, Fondation Egyptologique Reine Elisabeth, 1985. Price 450 B. f.

22. *Greek and Roman Mechanical Water-Lifting Devices: The History of a Technology*. By J. P. Oleson. Phoenix Supplementary Vol. XVI. 235 × 156 mm. Pp. 459, figs. 170, pl. figs. 3. Dordrecht, Holland, D. Reidel Publishing Company, 1984. ISBN 90 277 1693 5. Price £71.75.
23. *Portfolio of Texts and Poems on Egyptian Themes*. Produced by Carol Kent. 430 × 350 mm. Pp. 8 including one engraving. Erespin Press, 929 East 50th, Austin, Texas 78751. Price not stated. Copy available for inspection at the Society.

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