

NAUKRATIS REVISITED

The last three decades have seen the publication of a number of key studies on Naukratis (Kom Ge'if), the earliest known Greek colony in Egypt. Foremost are the reports of the surveys and excavations carried out at Naukratis during 1977–1978 and in three seasons between 1980 and 1982.¹ In a special volume of the series Venit has brought together all the painted Greek pottery from the site in Egyptian museums.² No less important are the recent papers by Bowden on the shrines and pottery dating,³ the new discussion by Gorton of the scarabs,⁴ the volume by Möller on Naukratis as a trading centre⁵ and the proceedings of a conference held in 1999.⁶

Inevitably, though to differing degrees, these studies touch on the long-standing and tangled question of chronology – one that arose almost as soon as Petrie discovered the site in 1884. The main difficulty has always been to reconcile the literary evidence for the early history of Naukratis (principally Herodotus) with the results of excavation. While most archaeologists since Petrie have tended to date the earliest Greek pottery at the site to the mid or late 7th century BC, Herodotus stated that Naukratis was given to the Greeks as a trading colony by Pharaoh Amasis, whose reign began in 570 BC. This raises a clear philosophical dilemma, neatly characterised by Bowden: should the pottery dating correct Herodotus, or Herodotus correct the pottery dating? The present article will review the problem and place it in the wider context of an ongoing debate over Archaic Greek chronology.

¹ A. Leonard, *Ancient Naukratis: Excavations at a Greek Emporium in Egypt. Part I: The Excavations at Kom Ge'if* (Atlanta, GA 1997); W. D. E. Coulson, *Ancient Naukratis II: The Survey at Naukratis and Environs. Part I: The Survey at Naukratis* (Oxford 1996); A. Leonard, *Ancient Naukratis: Excavations at a Greek Emporium in Egypt. Part II: The Excavations at Kom Hadid* (Boston, MA 2001).

² M. S. Venit, *Greek Painted Pottery from Naukratis in Egyptian Museums* (Winona Lake 1988).

³ H. Bowden, "The Chronology of Greek Painted Pottery: Some Observations", *Hephaistos* 10 (1991) 49–59; idem, "The Greek Settlement and Sanctuaries at Naukratis: Herodotus and Archaeology", in: M. H. Hansen, K. Raaflaub (eds.), *More Studies in the Ancient Greek Polis* (Stuttgart 1996) 17–38.

⁴ A. F. Gorton, *Egyptian and Egyptianising Scarabs* (Oxford 1996) esp. 91–131, 177–180.

⁵ A. Möller, *Naukratis: Trade in Archaic Greece* (Oxford 2000).

⁶ U. Höckmann, D. Kreikenbom (eds.), *Naukratis. Die Beziehungen zu Ostgriechenland, Ägypten und Zypern in archaischer Zeit: Akten der Table Ronde in Mainz 2.–27. November 1999* (Möhnesee 2001).

HISTORY OF THE PROBLEM

After his excavations of 1884–1885, Petrie offered a date of c. 650 BC for the origin of the Greek settlement at the site.⁷ It was not long before Hirschfeld raised the obvious objection that this conflicted with the testimony of Herodotus (2. 178):

Φιλέλλην δὲ γενόμενος ὁ Ἄμασις ἄλλα τε ἐς Ἑλλήνων μετεξετέρουσ ἀπεδέξατο καὶ δὴ καὶ τοῖσι ἀπικνεομένοισι ἐς Αἴγυπτον ἔδωκε Ναύκρατιν πόλιν ἐνοικῆσαι· τοῖσι δὲ μὴ βουλομένοισι αὐτῶν ἐνοικέειν, αὐτόσε δὲ ναυτιλλομένοισι ἔδωκε χώρους ἐνιδρύσασθαι βωμοὺς καὶ τεμένεα θεοῖσι. Τὸ μὲν νυν μέγιστον αὐτῶν τέμενος καὶ ὀνομαστότατον ἐὸν καὶ χρησιμώτατον, καλεόμενον δὲ Ἑλληνιον, αἶδε πόλιές εἰσι αἱ ἰδρυμέναι κοινῇ· Ἴώνων μὲν Χίος καὶ Τέως καὶ Φώκαια καὶ Κλαζομεναί, Δωριέων δὲ Ῥόδος καὶ Κνίδος καὶ Ἀλικαρνησσὸς καὶ Φάσηλις, Αἰολέων δὲ ἡ Μυτιληναίων μούνη. Τουτέων μὲν ἐστὶ τοῦτο τὸ τέμενος, καὶ προστάτας τοῦ ἐμπορίου αὐταὶ αἱ πόλιές εἰσι αἱ παρέχουσαι· ὅσαι δὲ ἄλλαι πόλιες μεταποιεῦνται, οὐδέν σφι μετεὸν μεταποιεῦνται. Χωρὶς δὲ Αἰγινῆται ἐπὶ ἐωυτῶν ἰδρύσαντο τέμενος Διὸς, καὶ ἄλλο Σάμιοι Ἴηρης, καὶ Μιλήσιοι Ἀπόλλωνος.

Amasis became a lover of the Greeks, and besides other services which he did to some of them he gave those who came to Egypt the city of Naukratis to dwell in, and to those who voyaged to the country without desire to settle there he gave lands where they might set altars and make holy places for their gods. Of these the greatest and most famous and most visited precinct is that which is called the Hellenion, founded jointly by the Ionian cities of Chios, Teos, Phocaea, and Clazomenae, the Dorian cities of Rhodes, Cnidus, Halicarnassus, and Phaselis, and one Aeolian city, Mytilene. It is to these that the precinct belongs, and these are they that appoint wardens of the port; if any other claim rights therein they have no part or lot. The Aeginetans made a precinct of their own, sacred to Zeus; and so did the Samians for Here and the Milesians for Apollo.

Hirschfeld argued that this passage gives us a firm *terminus post quem* of 570 BC for the Greek settlement at Naukratis.⁸ Petrie left the job of replying to his successor Gardner, who directed work at the site during 1885–1886. Gardner’s reply largely concentrated on a stratigraphical question concerning the “Scarab Factory” identified by Petrie, to which we will return later. In Gardner’s opinion it provided “indisputable evidence” that the Greek colony was founded before the reign of

⁷ W. M. F. Petrie, *Naukratis I* (London 1886).

⁸ G. Hirschfeld, “Die Gründung von Naukratis”, *RhM* 42 (1887) 209–225.

Amasis.⁹ Yet the subsequent excavations of Hogarth and colleagues¹⁰ were to raise serious questions about the “indisputable” evidence from the Scarab Factory and Petrie’s wider understanding of the site. Indeed, Hogarth preferred a Herodotean date (post 570 BC) for the founding of the colony.

After Hogarth, excavation ceased for over seventy years, during which research “moved into the library”.¹¹ From a study of the Greek pottery Prinz concluded that the colony did indeed date to the 7th century BC.¹² Later Price studied the individual ceramic types, classifying them by provenance, and concluded that “the general consensus of archaeological opinion... had completely veered round in favour of a seventh century dating, that one authority now differs from another only in putting the date in the third or fourth quarter of the century”.¹³

However, the next major study, that of Swedish archaeologist Gjerstad, took a very different approach, eschewing the alleged consensus as both premature and dependent on circular reasoning.¹⁴ Gjerstad proceeded with a detailed analysis of the *only* part of the site where Petrie had left a detailed record, including a section diagram (rare in the 19th century). This was a pit (actually *favissa*) discovered by Petrie in the *temenos* of the Apollo temple in the northern (Greek) part. It was filled with broken pottery and other objects which had been periodically cleared out of the temple; as votive gifts they required burial on hallowed ground to avoid desecration. The finds formed strata separated by layers of sand (either deliberate burial or natural accumulation) or building debris from the temple. Gjerstad painstakingly collated every scrap of information given by Petrie in order to interpret the stratigraphy within the pit. This done, he translated Petrie’s pottery descriptions into contemporary terminology and came up with a significant result. The ceramic sequence matched that known from elsewhere in the Greek world, reassuring Gjerstad that he had correctly interpreted the stratigraphy. The finds from the pit could also be correlated with the remains of the temple itself, as described by Petrie.

⁹ E. A. Gardner, *Naukratis II* (London 1888) 70–73.

¹⁰ D. G. Hogarth, “Excavations at Naukratis”, *BSA* 5 (1898–99) 26–97; D. G. Hogarth, H. L. Lorimer, C. C. Edgar, “Naukratis 1903”, *JHS* 25 (1905) 105–136.

¹¹ See the lucid summary of earlier work in Leonard (n. 1, 1997) 17–19.

¹² H. Prinz, *Funde aus Naukratis: Beiträge zur Archäologie und Wirtschafts-geschichte des 7. und 6. Jahrhunderts v. Chr. Geb.* (Leipzig 1908) 37 f.

¹³ E. Price, “Pottery of Naucratis”, *JHS* 44 (1924) 180–222, esp. 181; cf. M. Kerschner, “Perspektiven der Keramikforschung in Naukratis 75 Jahre nach Elinor Price”, in: Höckmann, Kreikenbom (n. 6) 69–94.

¹⁴ E. Gjerstad, “Studies in Archaic Greek Chronology. I. Naukratis”, *Annals of Archaeology and Anthropology* 21 (1934) 67–84, esp. 67.

Gjerstad's interpretation – involving a sequence of four temple buildings instead of two – was different from that of Petrie, and appeared to resolve a number of anomalies in his model. Finally, Gjerstad assigned dates to the sequence by using the sculptural and architectural remains from the pit and temple. Thus, for example it seemed that the fragments of Apollo temple IV were no earlier than about 520 BC, postdating the Persian invasion of Egypt in 525 BC. The beginning of Apollo temple I could be dated by a Cypriot head found at the very bottom of the pit. Gjerstad dated it no earlier than 570 BC, basing this on his excavation of the stratified layers of sculptures found in the temple of Ajia Irini, Cyprus. The dating was supported by a Greek capital from Apollo temple I which belonged stylistically to “about the middle of the sixth century BC”.¹⁵ Gjerstad concluded that while a date of 600 BC was possible, the Apollo I temple had most likely been constructed c. 570 BC. As it was built on clean mud and associated with the earliest stratified pottery styles from the site, he saw nothing to conflict with the Herodotean date for the founding of the colony. By implication some of the widely accepted dates for Archaic pottery were too high – a conclusion which did not worry Gjerstad as his own researches on Cyprus and elsewhere were leading him in a similar direction.¹⁶

Despite their elegance, Gjerstad's arguments fell largely on deaf ears. Cook briefly dismissed Petrie's data from the Apollo *temenos* as “unsatisfactory”, and Gjerstad's conclusions as therefore “suspect”.¹⁷ On the grounds that some sherds occurred “too late” in the stratification (by his own reckoning), Cook concluded: “If Gjerstad is right in his interpretation of Petrie's classes – and he would seem to be right – the strata of the Apollo site must have been seriously disturbed”. Cook's own view, based on his dating of the Greek pottery from other sites, was that the Greeks settled Naukratis in the late 7th century and that the Herodotean date was thus wrong.

In 1951 Egyptologist von Bissing made his own survey of the relevant authorities regarding the architectural remains from Greek Naukratis. His conclusion (in broad agreement with Gjerstad) was that none need be earlier than the 6th century BC. He also added a new dimension to the controversy – against Petrie, von Bissing could find no clear scarab evidence at the site for any Pharaoh earlier than Psammetichus II (595–

¹⁵ Gjerstad, *op. cit.*, 82, 77.

¹⁶ See e. g. idem, “Studies in Archaic Greek Chronology. II. Ephesus”, *Annals of Archaeology and Anthropology* 24 (1937) 15–34.

¹⁷ R. M. Cook, “Fikellura Pottery”, *BSA* 34 (1933/34) 1–98, esp. 86 n. 2.

589 BC).¹⁸ He concluded that Naukratis had been founded in the reign of this Pharaoh, a date midway between the positions of Gjerstad (c. 570 BC) and Cook (late 7th century BC).

Gjerstad returned to the fray in 1959, augmenting his original arguments. He also provided a detailed rebuttal of Cook's position regarding the stratigraphic evidence from the Apollo temple, fully answering his claim that "no stratification is possible".¹⁹ And there remained an overriding point unexplained by Cook. If the site had been "seriously disturbed" it would have been impossible for Gjerstad to produce a scheme matching the known succession of Greek pottery styles. Cook himself admitted that Gjerstad's analysis of Petrie's pottery types was correct, even accepting some of the dates arrived at.²⁰ Leonard acknowledges the brilliance of Gjerstad's synthesis,²¹ yet unfortunately he – and most other scholars since – have overlooked the importance of Gjerstad's detailed response to Cook.²²

At this point we need to ask why there was such reluctance to accept the apparently straightforward case offered by Hirschfeld, Hogarth and Gjerstad, and why there was such eagerness by others (Gardner, Prinz, Price, Cook) to prefer a 7th-century date? Setting aside for the moment Petrie's alleged evidence from the Scarab Factory, some brief observations on the early development of Archaic chronology need to be made.

First, around the turn of the 19th-20th centuries the dating of Greek pottery was very much in its infancy. Understandably, this period also saw the halcyon days of a tendency to which archaeologists are often prone – to exaggerate the antiquity of their discoveries. It is conspicuous in Petrie's writings on Naukratis, which are replete with phrases such as "the oldest", the "earliest Greek", etc. The Greek settlements at Naukratis (650 BC) and Daphnae (which he set even earlier, at 665 BC) were heralded by him as the first of a series of "steps" (provided by Egyptian evidence) which ultimately demonstrated the great age of Mycenaean civilization.²³ Similarly, the

¹⁸ F. W. von Bissing, "Naukratis", *Bulletin de la Société Royale d'Archéologie d'Alexandrie* 39 (1951) 32–82, esp. 65–66.

¹⁹ E. Gjerstad, "Naukratis Again", *Acta Archaeologica* 30 (1959) 147–165, esp. 156–157.

²⁰ Cook (n. 17): "The Fikellura he dates after 550 BC: this may well be true".

²¹ Leonard (n. 1, 1997) 33 n. 42.

²² See the rather negative view of the Apollo temple stratigraphy *ibid.* 32–33 n. 40 and Möller (n. 5) 90–92.

²³ W. M. F. Petrie, "The Egyptian Bases of Greek History", *JHS* 11 (1890) 271–277, esp. 271–273; for the 19th-century debate on the dating the Mycenaeans see

chronology then available for the East Greek sherds from Naukratis (as discussed by Prinz and Price) was largely a matter of guesswork, also with a tendency towards high dates.²⁴

A new stage came with the advent of a more methodical chronology for the Archaic. Its erstwhile chaotic state was brought into order, principally, by two classic studies – Beazley’s work on Black-figure and Red-figure styles and more importantly here, Payne’s definitive study in 1931 of the classification of Protocorinthian and Corinthian. To provide fixed points for his relative chronology Payne drew on the evidence from the Greek colonies on Sicily. Their foundation dates could be calculated from information given by Thucydides, and their earliest Greek pottery dated accordingly. Thus Payne set the beginnings of Protocorinthian in the last decades of the 8th century, mainly from the evidence of Syracuse and Megara Hyblaea. The chronology of the Protocorinthian/Corinthian transition, however, was a much disputed point, with some scholars arguing a date c. 580 BC. Payne refuted this by reference to Selinus, whose foundation Thucydides gave as 628 BC.²⁵ As Protocorinthian was absent from the site, but Early Corinthian well represented, Payne set the transition between these styles c. 625 BC, where it has effectively remained ever since.

Payne used Naukratis to control the conclusions derived from Selinus. Though he felt that the “exact date of the foundation of Naukratis is doubtful”, he assumed (as “is now generally recognised”) that the earliest Rhodian vessels from the site date to the late 7th century BC. Noting the absence of Protocorinthian, and the occurrence of a few Early Corinthian pieces, he took this as confirmation of his chronology, with the transition from Protocorinthian to Corinthian complete by c. 625 BC.²⁶ This led to a major reduction to Petrie’s date for the colony. Largely through the agency of Cook, Payne’s chronology came to provide the modern, conventional dating for the arrival of the Greeks at Naukratis, c. 615/610 BC.²⁷ It was still too high, of course, for the Herodotean date – as stressed by Gjerstad. He was confident that if other means than the Greek pottery could establish its foundation

conveniently P. James, I. J. Thorpe, N. Kokkinos, R. Morkot, J. Frankish, *Centuries of Darkness* (London 1991) 15–17, 93–94.

²⁴ R. M. Cook, *Greek Painted Pottery* (London³1997) 296: “...by a series of prejudiced errors the chronology of East Greek was set some thirty to forty years too high...”

²⁵ H. Payne, *Necrocorinthia* (Oxford 1931) 4, 22–23.

²⁶ *Ibid.*, 25, 32, 56.

²⁷ R. M. Cook, “Amasis and the Greeks in Egypt”, *JHS* 57 (1937) 227–237; cf. J. Boardman, *The Greeks Overseas* (Harmondsworth¹1964) 138.

date, then Naukratis would provide “a fixed point for dating that pottery. There are such means: literary sources, the date of the Cypriote sculptures found in Naukratis and epigraphical evidence”.²⁸ Since Naukratis was founded, in his opinion, no earlier than c. 570 BC, he concluded that the conventional dates for Early Corinthian (c. 625–600 BC) were “somewhat too high”, drawing attention to Payne’s own admission that the lower limit might be reduced to c. 590 BC.²⁹ He ended with the promise of a further study on the dating of Corinthian pottery – unfortunately unrealised. Gjerstad was already preoccupied with the chronological problems regarding the foundation of another city – Rome.³⁰ The main champion of a low dating for Naukratis had moved on, and by default the Payne/Cook model became the norm.

Nevertheless, the problem of Naukratis has stubbornly refused to “go away”. Recently it has received increasing mention in the literature.³¹ Its return to the limelight is largely due to the challenge to the conventional Archaic chronology initiated by Francis and Vickers.³² In a series of articles they challenged the prevailing archaeological chronology of the 8th to 5th centuries BC, offering reductions (at points) as great as 80 years. It is fair to say that their overall proposals are generally considered too extreme. Nevertheless, their critical forays have played a key role in prompting a wider review of the foundations of Greek Iron Age chronology.

For most of the 20th century it was thought that the chronology of the western colonies derived from Thucydides was supported by the Near Eastern contexts of Protogeometric and Geometric pottery. However, at each site in question the evi-

²⁸ It is beyond my competence to assess the significance of the earliest inscriptions from Naukratis. Needless to say they have been a subject of dispute – see e. g. Gardner, *op. cit.* (n. 9) 72–74; Edgar in: Hogarth (n. 10) 51–52; Hogarth et al., *op. cit.* (n. 10) 108; Gjerstad (n. 19) 161 and n. 42; M. M. Austin, *Greece and Egypt in the Archaic Age* (Cambridge 1970) 24; Möller (n. 5)

²⁹ Payne 57.

³⁰ For various bibliographies of Gjerstad see P. Åström, “Einar Gjerstad’s Cypriote Publications”, *Archaeologia Cypria* 1 (1985) 9–14, esp. 9.

³¹ E. g. M. Vickers, “Early Greek Coinage: A Reassessment”, *Numismatic Chronicle* 145 (1985) 1–44, esp. 18; A. M. Snodgrass, “Greek Archaeology and Greek History”, *Classical Antiquity* 4 (1985): 2, 193–207, esp. 200; D. W. J. Gill, “The Temple of Aphaia on Aegina: The Date of the Reconstruction”, *BSA* 83 (1988) 169–177, esp. 174 n. 28; R. M. Cook, “The Francis-Vickers Chronology”, *JHS* 109 (1989) 164–170, esp. 165; Bowden (n. 3, 1991); idem (n. 3, 1996) 24–28; J. Whitley, *The Archaeology of Ancient Greece* (Cambridge 2001) 67, 74.

³² Vickers, *op. cit.*; E. D. Francis & M. Vickers, “Greek Geometric Pottery at Hama and Its Implications for Near Eastern Chronology”, *Levant* 17 (1985) 131–138; for further bibliography and discussion see Cook (n. 31); W. R. Biers, *Art, Artefacts, and Chronology in Classical Archaeology* (London 1992) 82–85, 99–101.

dence/context has proved to be unclear or problematic, while the value of the Thucydides' calculations has also been challenged.³³ Even if Thucydides' dates are accurate, it is clear that their consequences have not been logically or systematically followed through. This is conspicuous in the case of Selinus. The problem was frankly admitted by Cook in 1969³⁴ and restated by him in 1972: "It now appears that Transitional or even Late Protocorinthian too was excavated there".³⁵ Cook rejected the logical upshot – "simply to lower the dates of the phases by fifteen or twenty-five years" – as "impracticable", and suggested that Eusebius' date for the colony (650 BC) might be preferable to Thucydides' (628 BC). In 1997, while repeating his cautions about Thucydides, Cook changed his answer to the problem, noting that the graves with Transitional and Protocorinthian also contained indigenous pottery: "So the cemetery may have been a native pre-colonial one – natives elsewhere imported Greek pots – and the accepted chronology can be justified without the shifty device [*sic*] of preferring Eusebius here to Thucydides".³⁶ Yet the presence of (an unquantified amount of) Sicilian pottery in the cemetery does not necessarily prove that the cemetery was a native one. Further, Selinus cannot be treated in isolation on an *ad hoc* basis; it is far from being the only problem site among the western colonies.³⁷

Further discussion of the dating of Protocorinthian and the related question of when Late Geometric ended is beyond the scope of the present article. Suffice to say that debate continues.³⁸ The present writer and colleagues have suggested that a provisional lowering of the end of the Late Geometric from

³³ Francis, Vickers, *op. cit.*; M. D. Herrera, J. Balensi, "More about the Greek Geometric Pottery at Tell Abu Hawam", *Levant* 18 (1986) 169–171; P. James, I. J. Thorpe, N. Kokkinos, J. Frankish, "Bronze to Iron Age Chronology in the Old World: Time for a Reassessment?", *Studies in Ancient Chronology* 1 (1987) 1–147, esp. 34–39; V. Hankey, P. Warren, *Aegean Bronze Age Chronology* (Bristol 1989) 167; James et al. (n. 23) 99–110; Bowden (n. 3, 1991) 49–50; I. Morris, "Geometric Greece", *Colloquenda Mediterranea A/2* (1993) 29–38, esp. 31; cf. A. Fantalkin, "Low Chronology and Greek Protogeometric and Geometric Pottery in the Southern Levant", *Levant* 33 (2001) 117–125.

³⁴ R. M. Cook, "A Note on the Absolute Chronology of the Eighth and Seventh Centuries", *BSA* 64 (1969) 13–15, esp. 14. Then his conclusion was that "...we are left with the unedifying reflection that a wrong use of the wrong date somehow gave the right result".

³⁵ R. M. Cook, *Greek Painted Pottery* (London ²1972) 263.

³⁶ R. M. Cook, *Greek Painted Pottery* (London ³1997) 253.

³⁷ See James et al. (n. 23) 102–103, 360 n. 24.

³⁸ See J. C. Waldbaum, "Early Greek Contacts with the Southern Levant, ca. 1000–600 B.C.: the Eastern Perspective", *BASOR* 293 (1994) 53–66; J. C. Waldbaum, J. Magness, "The Chronology of Early Greek Pottery: New Evidence from Seventh-Century B.C. Destruction Levels in Israel", *AJA* 101 (1997) 23–40; Fantalkin (n. 33). Some of the recent debate has focussed on the significance of the Bocchoris scarab from Pithekoussai, found with EPC vessels – see D. W. J. Gill, M. Vickers, "Bocchoris the Wise and Absolute Chronology", *MDAI(R)* 103 (1996) 1–9; D. Ridgway, "The Rehabilitation of Bocchoris: Notes and Queries from Italy", *JEA* 85 (1999) 143–152.

c. 700 BC to c. 675 BC can make better sense of both the Western and Near Eastern evidence.³⁹ The reduction was accepted as “plausible” by Ian Morris, who adduced further supporting arguments.⁴⁰ And Sarah Morris has since argued that the “Geometric period lasted well into the seventh century”.⁴¹ A lowering of terminal Late Geometric by a quarter of a century would not necessarily have a knock-on effect on Corinthian in the “relay chronology” (as Gjerstad called it); nor need the Protocorinthian from Selinus, which Cook feared could “lower the dates of the phases by fifteen or twenty-five years”, have precisely that effect. But *they may well have* – and the possibility needs to be explored with due rigour. At the very least, both the fragility and fluidity of 8th–7th ceramic dating are now clear. It is surely unrealistic to insist any longer that Payne’s dating of the Corinthian sequence, based on the shaky archaeological chronology derived from Thucydides, can be safely used to control the dating of Archaic sites elsewhere in the Greek world, especially those which may have their own ‘voice’ regarding chronology. Pending a full reappraisal of all the fixed points for dating Corinthian pottery (including correlations with Attic), Gjerstad’s assertion that Naukratis could itself provide a fixed point in Archaic chronology is surely worthy of re-examination.

THE NEW SITE REPORTS

As Leonard remarked, a final answer to the longstanding controversy can only come about through renewed excavation.⁴² Unfortunately, the desire for a “true stratigraphic sequence” for Archaic Greek Naukratis is unlikely to be realised in the foreseeable future. In 1899 Hogarth had already encountered problems as parts of the site were “sodden with the infiltration of water”, and the northern area is now completely submerged under a lake.⁴³ The new excavations were thus limited to the southern area, where no Archaic deposits were found.⁴⁴

Yet the excavations have still been able to clarify a number of relevant matters. Notably, Petrie claimed to have excavated an enormous, square,

³⁹ James et al. (n. 23) 111.

⁴⁰ Morris (n. 33) 30–31.

⁴¹ S. P. Morris, “Bearing Gifts: Euboean Pottery on Sardinia”, in: M. S. Balmuth, R. H. Tusk (eds.), *Sardinian and Aegean Chronology* (Oxford 1998) 361–362.

⁴² Leonard (n. 1, 1997) 19.

⁴³ Hogarth et al. (n. 10) 105; Leonard (n. 1, 1997) 20.

⁴⁴ *Ibid.*, 34 n. 69. Survey work produced surface finds of two sherds of Archaic Chian and numerous examples of Black Glazed Ware (5th–2nd centuries BC) – see Coulson (n. 1) 19 f.

brick-built structure at the southern end of the site, which he called the “Great Temenos”, identifying it with the great Hellenion described by Herodotus. However, his successor Hogarth was baffled by this claim. He found no early Greek remains, but only Egyptian, in the vicinity of the “Great Temenos”. Discovery of a stela of Pharaoh Nectanebo I (380–362 BC) referring to the town of *Pi-emro*⁴⁵ seemed to suggest that the southern part of the site was an Egyptian, rather than a Greek settlement. Searching for foundation deposits underneath the “Great Temenos” Hogarth was at a loss to “find any clear evidence of the existence of a Great Wall of any kind”. Instead he believed he had found “an aggregate of house remains, piled up round a lower area, wherein lay the Egyptian temple and public buildings, one of which contained the Nectanebo Stela...”.⁴⁶ Hogarth’s suspicions have been vindicated by the modern work. Only Egyptian pottery of the Ptolemaic period was found in association with the remains of the “Temenos” while, with the possible exception of one wall, only domestic architecture was found. “Such artefactual evidence,” Leonard concluded, “greatly supports the views of Hogarth (against those of Petrie and Gardner) concerning the nature and date of the architecture in the southern end of the ancient city of Naukratis”.⁴⁷

It appears that Petrie’s Archaic Greek “Temenos” was neither Archaic nor Greek, and possibly not even a Temenos.⁴⁸ Though a negative result, it greatly clarifies our picture of the site as a whole. Petrie was wrong in seeing early Greek activity throughout the site, and the new excavations have confirmed Hogarth’s understanding that the town effectively comprised two parts – the northern Greek (Naukratis) and the southern Egyptian (*Pi-emro*). The new excavations thus place a large question mark against Petrie’s wider understanding of the site. Conversely, Hogarth’s stock as an excavator/interpreter rises against that of Petrie.⁴⁹

Given this, one might have expected the new site reports to be sympathetic to Hogarth’s arguments regarding the foundation of the Greek colony. This is not the case, however. The task of assessing the historical and archaeological evidence was given to an Egyptologist, Sullivan, whose account is strongly partisan to the high Petrie dating. Indeed, he attempts to

⁴⁵ Hogarth et al. (n. 10) 106; for a modern translation and discussion see M. Lichtheim, “The Naukratis Stela Once Again”, in: J. H. Johnson, E. F. Wente (eds.), *Studies in Honor of George R. Hughes* (Chicago 1976) 139–146.

⁴⁶ Hogarth et al. (n. 10) 111.

⁴⁷ Leonard (n. 1, 1997) 30.

⁴⁸ See *ibid.*, 34–35 n. 69.

⁴⁹ Leonard (n. 1, 1997) 14 praises Hogarth’s general model of the site.

raise the now conventional date for the beginnings of the Greek colony from c. 615/610 BC to an earlier point in the reign of Psammetichus I, about 650 BC. To sustain this, Sullivan extends Petrie's unwarranted identification of the mercenary camps (στρατόπεδα) founded by Psammetichus with Daphnae (Tell Defenneh) to include Naukratis as well.⁵⁰ The only serious literary evidence⁵¹ he invokes to support an early date is a passage from Strabo (17.1.18). This relates how Milesians, "in the time of Psammitichus" fortified a settlement on the Bolbitine mouth of the Nile; "but in time (χρόνον δέ)" they sailed to the Saitic nome, fought a battle and founded Naukratis. Unrealistically, Sullivan claims that this "fixes" the origins of Greek Naukratis to the reign of Psammetichus I (664–610 BC).⁵² As Gjerstad noted, the chronology of this Milesian story (with two episodes separated by an unknown period of time) is by no means incompatible with that of Herodotus.⁵³ In any case Strabo's account seems to be garbled with later (5th-century BC) events⁵⁴ and cannot be used to contradict Herodotus' clear statements.

HERODOTUS AND AMASIS

With respect to our primary source, Sullivan denies that Herodotus characterised Naukratis as a new town founded in the reign of Amasis: "Since he speaks of the place as a 'city' (*polis*) rather than as a 'site for settlement' or a similar expression, Herodotus is not clearly implying in this passage that Naukratis was founded at this time".⁵⁵ While Sullivan claims here to be dispensing with an "old shibboleth regarding the foundation of the city", he is merely attacking a straw man of his own making. That Amasis may have

⁵⁰ Petrie (n. 23) 272; R. D. Sullivan, "Psammetichus I and the Foundation of Naukratis", in: Coulson, *op. cit.* (n. 1) 177–202, esp. 187–188. There is no suggestion in Hdt. 2. 154 that he identified the στρατόπεδα with either Daphnae or Naukratis. The impossibility of confusing them was forcefully explained by Cook, *op. cit.* (n. 27) 234–236).

⁵¹ While mentioning statements by late chronographers to the effect that Naukratis was in existence by the 23rd Olympiad (688–685 BC), or even the fourth year of the 7th Olympiad (749 BC), Sullivan (*op. cit.*, 177) fortunately agrees that such references "sustain little reliance". On the absurdity of such dates see A. R. Burn, "Dates in Early Greek History", *JHS* 55 (1935) 130–146 and James et al. (n. 23) 328.

⁵² Sullivan, *op. cit.*, 178; cf. 186.

⁵³ Gjerstad (n. 14) 69.

⁵⁴ Petrie, *op. cit.* (n. 7) 4; Gjerstad, *op. cit.* (n. 14) 68; Bowden, *op. cit.* (n. 3, 1996) 25.

⁵⁵ Sullivan, *op. cit.*, 178.

given a pre-existing town to the Greeks has been generally accepted by both ‘high’ and ‘low’ chronologists from Petrie to Gjerstad.⁵⁶ The important question is whether Herodotus was mistaken in stating that it was Amasis who gave the town (or part of it) to the Greeks. *Prima facie*, this seems unlikely. The origin of Naukratis forms an integral part of his Amasis narrative; and had he misidentified the city’s patron, there would doubtless have been Naukratites able to correct him by reference to local memories. (Amasis’ reign ended in 526 BC, some three generations before Herodotus’ visit to Egypt.)

Here we need to remember that where Herodotus’ other statements about Naukratis can be compared with archaeological evidence, they have always proved accurate. At a general level, “Herodotus... relates that Naukratis was founded almost exclusively by East Greek city states, so it is not surprising that most of the pottery excavated at Naukratis was made in the Greek centers of western Anatolia and the islands that lie off its coast”.⁵⁷ In more detail, his lists of Ionian and Dorian *poleis* involved in the colony are headed respectively by Chios and Rhodes – matching the large quantities of pottery from these islands among the earliest finds. Other *poleis*, such as Clazomenae and Mytilene, have been identified ceramically.⁵⁸ Again, as Bowden notes, the earliest stratified pottery at the site comes from several different contexts, including the temples of Apollo and Aphrodite⁵⁹ – supporting Herodotus’ account of how the sanctuaries were founded together with the settlement.

Because of his dating of the Greek pottery, Cook had to assume that Herodotus was mistaken regarding the Pharaoh who gave Naukratis to the Greeks. Yet he admitted that this was “surprising”,⁶⁰ and tried to find an explanation in an idea mooted by Petrie – that Amasis had ‘reorganised’ an already existing Greek settlement. To provide a rationale for such a reorganisation, Petrie recast Amasis as an anti-hellenic pharaoh; thus the grant of Naukratis was not a generous gift to his commercial and political allies, but a way of implementing “strong measures against Greek trading” by restricting the Greeks to one site.⁶¹ The reorganisation model re-

⁵⁶ Petrie (n. 7) 4; Gjerstad (n. 14) 68.

⁵⁷ Venit (n. 2) 1.

⁵⁸ See Austin (n. 28) 24–27 and J. Boardman, *The Greeks Overseas* (London 1999) 122–125; for more detail see von Bissing (n. 18) 41–48; Venit (n. 2); Kerschner (n. 13).

⁵⁹ Bowden (n. 3, 1996) 27.

⁶⁰ Cook (n. 27) 233.

⁶¹ See Petrie (n. 7) 7–8.

mains the preferred solution to the apparent conflict between Herodotus and the archaeological dating,⁶² while its corollary that Amasis was initially anti-hellenic has taken on a life of its own.⁶³ Thus a modern myth has grown that Amasis' rebellion against his predecessor Apries was part of a nationalist uprising against increasing Greek influence.⁶⁴ The idea is strange, given Herodotus' description of Amasis as a φιλέλληνην. So it has to be assumed that that his pro-hellenic stance was the result of a change of policy late in his reign.

Two arguments have been offered to show that Amasis was originally antagonistic toward the Greeks. First, Hdt. 2. 162 – 164 is cited to the effect that his rival Apries defended himself with a large army of Greek mercenaries. Second, a reading of Amasis Elephantine Stela appeared to show that, as late as his Year 3, Apries was attempting a comeback at the head of a large Greek force. Both points can be evaluated in the light of the corrected reading of the Stela.⁶⁵ It confirms Apries' reliance on mercenaries in the Year 1 (Amasis) as it talks of him manoeuvring with "boats filled with Greeks (*ḥ3w-nbw*)". Yet the idea of Apries returning with Greeks in the Year 3 has now been scotched. The year involved is actually 4, and the invading foreigners (*Sttyw* or "Asiatics") are now agreed to be the army of the Babylonian king Nebuchadrezzar.⁶⁶ This changes the picture considerably. Apries' reliance on Greek troops at the beginning of the civil war may simply have been by default – as (according to Herodotus) the native Egyptian armies had united under Amasis to oust him. This is very different matter from the simplistic notion that Apries (who had attacked Cyrene!) was pro-hellenic and Amasis anti-hellenic. The importance of East Greek

⁶² Or "usual way out" to use the expression of Bowden (n. 3, 1996) 24. For examples see Boardman (n. 58) 117 and the following note.

⁶³ H. R. Hall (in *CAH* III [1954] 302) described Amasis as head of "a nationalist and anti-foreign revolution". Cook (n. 27) 235; cf. 232 stated that "The Amasis of the Egyptian records rose to power as the head of an anti-Greek movement". A. B. Lloyd (*Herodotus Book II*, vol. 3 [Leiden 1988] 178) called him "the champion of national interests", while N. Grimal (*A History of Ancient Egypt* [Oxford 1992] 363) portrayed Amasis as "confronting the problem of the Greeks... by concentrating the foreigners in the city of Naukratis". Sullivan (n. 50) 187 described Naukratis as "an appropriate location for restricting Greek settlement".

⁶⁴ As a further development, Grimal (*op. cit.*, 363) casts the civil war between Apries and Amasis as *originating* in a conflict between the regular Egyptian army and the Greek mercenaries.

⁶⁵ E. Edel, "Amasis und Nebukadrezzar II", *Göttinger Miszellen* 29 (1978) 13–20; A. Leahy, "The Earliest Dated Monuments of Amasis", *JEA* 74 (1988) 183–199.

⁶⁶ Edel, *op. cit.*; Leahy, *op. cit.*, 191.

mercenaries to the stability of the dynasty does not allow us to think in such terms. On the defeat of Apries, Amasis must have immediately come to terms with the mercenaries, 30 000 in number according to Herodotus (though allowing for casualties). To imagine that they were left in a political vacuum, while Amasis faced the aggressive Neo-Babylonian Empire on his borders, is absurd.

Fortunately we do not have to speculate. The Year 4 entry on the Elephantine Stela is matched by a cuneiform text describing Nebuchadrezzar's attack on Egypt in his 37th Year (= 567 BC = Year 4 Amasis). Though the Babylonian record is fragmentary, enough survives to show that Amasis "called on" troops not only from Egypt, but from "the town Putu-Iaman" (agreed to be Cyrene) and "distant regions amidst the sea" (manifestly the Aegean in this context).⁶⁷ As Putu-Iaman (literally "Libya of the Ionians/Greeks") was an ally in 567 BC, Leahy argues that Amasis' marriage-alliance with a Cyrenian princess belongs early in his reign.⁶⁸ He dates to the same period Amasis' removal of the Greek mercenaries from the "Camps" (στρατόπεδα) founded near Bubastis by Psammetichus I, to new barracks at Memphis (Hdt. 2. 154). Remarkably this too has often been cited as evidence of Amasis' 'anti-hellenic' reorganisations. Herodotus wrote that Amasis transferred his Aegean mercenaries from Bubastis to Memphis to protect him from his *Egyptian* subjects! (Petrie omitted the last words.)

So much for Amasis the arch-nationalist whose only role at Naukratis was to "restrict" the Greeks. From a historian's perspective it is fair to say that the efforts to gainsay the plaintext of Herodotus seem like gratuitous complications. Moreover, they only seem to have been attempted because Greek pottery dating has 'confidently' placed the beginnings of Naukratis some 30 years before the reign of Amasis.

CYPRIOT AND EGYPTIAN EVIDENCE

Other controls than the accepted Greek pottery chronology can be brought to bear on the Herodotean date for the foundation of Greek Naukratis. These are provided by the non-Aegean evidence, namely Cypriot, Egyptian and Phoenician.

⁶⁷ J. B. Pritchard (ed.), *Ancient Near Eastern Texts Relating to the Old Testament* (Princeton 1969) 308; for the identifications see Edel, *op. cit.*, 15–16, Leahy, *op. cit.*, 191–192.

⁶⁸ *Ibid.*, 192–193.

The earliest Cypriot finds from the site are the sculptures from the Apollo temple. As noted earlier, Gjerstad identified a head from the bottom of the *temenos* pit as early Neo-Cypriot in style, hence dating to c. 560 BC. Möller objects to this: “The oldest Cypriot terracotta head cited by Gjerstad may be compared with Proto-Cypriot examples from the Heraion on Samos, enabling a dating, in accordance with Samian chronology, to the end of the seventh century”. From this she flatly concludes: “In the light of Schmidt’s better-established chronology for Samos, Gjerstad’s chronology for the Cypriot sculptures would appear to be set about 40 years too late”.⁶⁹ Matters, however, are not so simple.

First, the head is classified by Gjerstad as belonging not to the Proto-Cypriot, but to the Neo-Cypriote Style (his dating: 560–520 BC). It is one of four such sculptures associated with the Naukratite Apollo I temple, for each of which Gjerstad found almost exact parallels in stratified examples from Cyprus. Nevertheless, Gjerstad did identify, from less certain contexts at the temple, some pieces of the 2nd Proto-Cypriot style, which he dated to c. 580–560 BC.⁷⁰ Schmidt’s Samian chronology would date the Neo-Cypriot examples to 610/600–560/550 BC and the 2nd Proto-Cypriot to 670/660–610/600 BC.⁷¹ This would indeed raise considerably Gjerstad’s *terminus post quem* for the Apollo temple finds. Yet it should be remembered that Schmidt’s “better-established chronology” is of course based on the conventional dates for the Greek pottery found at the Heraion.⁷² To cite it without qualification, as Möller does, merely reverts us to the circular argument Gjerstad identified in 1934 – use of the accepted chronology for Archaic pottery, through cross-dating, to reinforce itself. Further, the Samian chronology is far from being accepted by the majority of experts on Cypriot sculpture. Refinements and adjustments have been made to Gjerstad’s chronology, the net result of which would be to *lower* rather than raise his dates. Using careful comparison with Greek, Egyptian and Phoenician sculpture, Vermeule and Markoe have lowered the beginning of the 2nd Proto-Cypriot Style to c. 560 BC (from Gjerstad’s suggested date of c. 600), and the beginning of Neo-Cypriot by ten years, from 560 to 550 BC.⁷³ Through her extensive

⁶⁹ Möller (n. 5) 91.

⁷⁰ Gjerstad (n. 18) 161.

⁷¹ G. Schmidt, *Samos VII: Kyprische Bildwerke aus dem Heraion von Samos* (Bonn 1968) 94. For the differing views on the chronology of Archaic Cypriot sculpture see Table 3 of A. T. Reyes, *Archaic Cyprus: A Study of the Textual and Archaeological Evidence* (Oxford 1994) 161.

⁷² Further, P. Gaber-Saletan (*Regional Styles in Cypriote Sculpture: The Sculpture from Idalion* [New York 1986] 70 n. 14) has raised a number of questions about the stratigraphic evidence from Samos, criticising the lack of detail in publication of the find spots and sections, which make it “difficult to assess these finds in context”. Many of the sculptural finds come from a pit cut through a floor dated by the excavators to c. 600 BC. The context cannot be earlier than the floor, and the contents are “most probably later” (Pers. comm. Pamela Gaber-Saletan).

⁷³ C. Vermeule, “Cypriote Sculpture, the Late Archaic and Early Classical Periods: Towards a More Precise Understanding”, *AJA* 78 (1974) 287–290; G. E. Markoe, “Egyptianising Male Votive Statuary from Cyprus: A Reexamination”, *Levant* 22 (1990) 111–122. They have also simplified Gjerstad’s complicated sequence of overlapping styles, by scrapping his “Cypro-Egyptian” Style (c. 570–545 BC). Cf. A. Hermay, “Naukratis et la sculpture égyptisante à Chypre”, in: Höckmann, Kreikenbom (n. 6) 27–38.

studies of regional styles in Cypriot sculpture, Gaber-Saletan has arrived at the following dates for the relevant periods: 1st Proto-Cypriot (600–560); 2nd Proto-Cypriot (560–540 BC); Neo-Cypriot (550–520 BC).⁷⁴ Not forgetting regional variations, it is dates of this order, rather than the Samian, which are currently preferred. Thus, depending on whether it belongs to the 2nd Proto-Cypriot or Neo-Cypriot styles, the head from the bottom of the *temenos* pit should date no earlier than about 560–550 BC.

So, far from contradicting Gjerstad, recent studies of Cypriot sculpture have tended to reinforce his case that the Apollo temple was built not earlier than the second quarter of the 6th century BC. The conflict with the Samian dates remains unresolved, but this only serves to illustrate the dichotomy between Greek and Cypriot Archaic chronologies.⁷⁵ It is particularly conspicuous at Naukratis. For example, as Möller points out, the Cypriot head from the bottom of the *temenos* pit was found beneath an East Greek vessel.⁷⁶ It belongs to the ‘Wild Goat Middle II’ style, usually thought to have ended c. 600 BC,⁷⁷ some 40 years before the head was sculpted according to the preferred Cypriot chronology.

Unfortunately, uncertainty about the Cypriot dates continues.⁷⁸ Less equivocal evidence should be provided, surely, by the dateable Egyptian finds from early Naukratis. The chronology of Egypt in the pre-Persian period has never been influenced by the Greek – only the other way around.⁷⁹ How, then,

⁷⁴ Gaber-Saletan, *op. cit.*; eadem, “Regional Styles and the Chronology of Sculpture”, in: P. Åström (ed.), *Acta Cypria* (Jonsered 1992) 224–227.

⁷⁵ The tensions between the two chronologies are well set out by L. W. Sørensen, “Early Archaic Limestone Statuettes in Cypriote Style”, *Report of the Department of Antiquities Cyprus* 1978, 111–121.

⁷⁶ Petrie (n. 7) 14, 18, 20; Möller (n. 5) 91.

⁷⁷ R. M. Cook, P. Dupont, *East Greek Pottery* (London 1998) 10.

⁷⁸ Möller, contradicting her own definitive pronouncement that Gjerstad was “wrong”, also states: “As yet, no absolutely certain dating is available for Cypriot sculpture” ([n. 5] 156). Karageorghis has been cautious, citing both the Gjerstad and Schmidt chronologies in his series on Cypriot sculpture (e. g. *The Coroplastic Art of Ancient Cyprus: III. The Cypro-Archaic Period Large and Medium Size Sculpture* [Nicosia 1993] xi). A recent work (idem, *Early Cyprus* [Los Angeles 2002] 183) follows Schmidt while pointing out that Lewe, who prefers a lower chronology, “does not accept the Samian dating as absolutely valid”.

⁷⁹ While there may be grounds for dispute over the dating of earlier Egyptian dynasties (see James et al., *op. cit.* [n. 23]; P. James, N. Kokkinos, I. J. Thorpe, “Mediterranean Chronology in Crisis”, in: M. S. Balmuth, R. H. Tykot [eds.], *Sardinian and Aegean Chronology* [Oxford 1998] 29–43), there is none at all concerning the 26th (Saïte) Dynasty (cf. L. Depuydt, “On the Consistency of the Wandering Year as Backbone of Egyptian Chronology”, *JARCE* 32 [1995] 43–58). Egyptian dates for this period can be safely back-calculated by collation of numerous documents, back from the Persian invasion of Egypt in 525 BC, and cross-checked

does the evidence of pharaonic dating stand with regard to the history of the Greek colony at Naukratis? Little Egyptian pottery from the northern, Greek area of the site was published by the early excavators and does not lend itself to precise dating. Fortunately the site offers a more diagnostic dating tool: numerous scarabs, many bearing royal names. They were mainly locally made, and Petrie identified a deposit (in the middle, “town” area of the site) as the remains of a “Scarab Factory”. It contained not only hundreds of (largely faience) scarabs, but moulds for their manufacture.

Petrie gave the impression that a great number of scarabs were found bearing the cartouche of Psammetichus I (664–610 BC).⁸⁰ However, von Bissing insisted that there was only one scarab of this pharaoh from Naukratis, bearing his prenomen Wahibre (*uah-ib-Re*).⁸¹ Gorton is only slightly more generous. She notes “a very few scarabs” from the site which might belong to this Pharaoh and specifies two (both with Wahibre).⁸² Even these are problematic. While the prenomen of Psammetichus I, Wahibre was also the nomen of Apries (589–570 BC). Petrie himself was aware of this difficulty and admitted that the Wahibre scarabs from Naukratis “probably [belong] to the latter”.⁸³

By contrast Psammetichus II (595–589 BC) is well attested at Naukratis, indeed the best attested there of any 26th-dynasty pharaoh. There are many with his distinctive Horus name *men-ib-Re*.⁸⁴ The nomenclature on the scarabs is not always so diagnostic, however, and those bearing the nomen “Psamtek” might belong to any of the three 26th-dynasty pharaohs of this name. So to be fair to Psammetichus I, it is unfortunate that his nomen and prenomen were used by other 26th-dynasty pharaohs (Psammetichus II and III, and Apries respectively). Thus a simple count of the scarabs which can *definitely* be attributed may be biased against him (compared, say, to Psammetichus II). In the absence of clear nomenclature the attribution of scarabs to individual pharaohs is not an exact science and depends on other clues such as iconography, style and fabric. It also becomes particularly difficult during the Saite period, when scarabs were increasingly manufactured by non-Egyptians.

Given this, all one can do is review specialist opinion. Petrie offered little in support of a major presence of Psammetichus I at Naukratis. Its only

with the astronomically-fixed chronology of 7th–6th century Assyria and Babylonia.

⁸⁰ “Now many [scarabs] of Psamtik I are found, and some of Psamtik II...” (Petrie [n. 7] 5).

⁸¹ Von Bissing (n. 18) 41, 65–66.

⁸² Gorton (n. 4) 178.

⁸³ Petrie (n. 7) 5; idem, *Scarabs and Cylinders with Names* (London 1917) 32. To arrive at his “many [scarabs] of Psamtik I”, it seems Petrie relied heavily on those decorated with a lion and a sun disk, on the assumption that this combination was adopted as a “badge” by Psammetichus I. But his badge theory is by no means universally accepted – for alternative interpretations see Gorton (n. 4) 106.

⁸⁴ *Ibid.*, 101, 102, 130; cf. Petrie (n. 7) 32.

recent adherent, Sullivan, though an Egyptologist, shied away from analysing the scarab finds in detail, remarking that they have “not commanded a consensus”.⁸⁵ Gorton’s cautious summary seems fair: “Since the only Egyptian rulers reliably named on the scarabs from the factory are Psamtek II and Apries it seems possible that its main period of operation, that is to say of mass production... was in the years of these Pharaohs (595–570)”. Her conclusion – “it seems unlikely that the factory could have been in production before the beginning of the 6th cent.”⁸⁶ – agrees with that of von Bissing, with no recent studies to the contrary. Their assessment is also supported by a negative argument. The idea that the Scarab Factory may have begun in the reign of Psammetichus I faces a problem, raised long ago: there are no scarabs of the powerful (Greek and Phoenician-friendly) pharaoh Necho II (610–595 BC) from the site.⁸⁷ Unless we develop an *ad hoc* model – involving the vicissitudes of taphonomy or excavation – to explain their absence, it is safest to accept von Bissing and Gorton’s assessment that the output of the Factory essentially dates to the reigns of Psammetichus II and Apries (595–570 BC).

RECONSIDERING THE SCARAB FACTORY

Prima facie the absolute dates provided by the scarab evidence might suggest a 6th-century date for the Greek foundation of Naukratis, though one earlier than Amasis. But this presupposes certain knowledge about the chronological relationship between the Greek *polis* and the Scarab Factory. Actually, their relative dating has never been properly agreed. Four models have been suggested:

1. The Greek colony and the scarab factory were both founded in the reign of Psammetichus I. The factory was closed by Amasis [Petrie].
2. The Greek colony and the scarab factory were founded simultaneously in the reign of Psammetichus II [von Bissing].
3. The Greek colony was founded in the late 7th century BC. The scarab factory may have been founded later under Psammetichus II [Boardman].⁸⁸

⁸⁵ Sullivan (n. 50) 194 n. 72 merely contrasts the views of Petrie and von Bissing and acknowledges that the latter “dismissed all but one of the scarabs for Psammetichus I”.

⁸⁶ Gorton (n. 4) 178, cf. 91.

⁸⁷ Von Bissing (n. 18) 41, 66.

⁸⁸ Boardman (*op. cit.* [n. 27] 138–143) was sympathetic to von Bissing’s argument re the scarabs, and though he dated the arrival of the Greeks at Naukratis to c. 620, he placed the Scarab Factory in the 6th century BC. See Möller, *op. cit.* (n. 5) 153 n. 528.

4. The scarab factory was founded (by Phoenicians) in the reign of Psammetichus II and closed when the Greek colony was founded in the reign of Amasis [Hogarth, Edgar].

Using Gorton's understanding of the scarab evidence, von Bissing's and Boardman's models (2 and 3) are an improvement on Petrie's (1). Yet Hogarth's model (4) accounts better for the overall scarab distribution, including the lack of evidence for both Necho II and Amasis. Further, it should be clear that while von Bissing's interpretation (3) comes close, it is only Hogarth's (4) that accommodates the testimony of Herodotus as well as the scarab evidence.

The key question is of course the archaeology of the Scarab Factory, subject of the major disagreement between Petrie and Hogarth noted at the beginning of this article. Petrie's assumption that the Factory was an integral part of the Greek colony was based on the following arguments:⁸⁹

A. Numerous sherds of Greek pottery were found with the remains of the Factory. Yet the context, as discovered by Petrie, was highly disturbed and almost destroyed by the diggings of Arab treasure-hunters.⁹⁰ For the Greek sherds found there, the observations of Hogarth's pottery expert Edgar should need no further comment:

The fact that a good deal of Naukratite pottery was found along with the scarabs is far from being a proof that the two were contemporary. It is clear that the pottery was part of the refuse from the neighbouring temple of Aphrodite discovered in the following season. One of the fragments in fact bore a dedication to Aphrodite...; and this year again, near the same spot, we found among a great number of scarab moulds several fragments of the same ware dedicated to the goddess... It is not difficult to see how broken pottery thrown out of the temple could become intermixed with the earlier *débris* round about... *It is unnecessary to attach the slightest weight to this particular item of evidence.*⁹¹

B. Petrie claimed that a burnt stratum was found through a large area of the southern part of the site, that it lay below the Factory and that it contained the earliest Greek pottery. Taken at face value this would indeed seem to be "indisputable" evidence, as Gardner saw it, that the arrival of the Greeks preceded the Factory (with its scarabs of Psammetichus II and Apries). Yet in open contradiction to Petrie, Hogarth and Edgar asserted that

⁸⁹ Conveniently summarised in Gardner (n. 9) 71; cf. Leonard (n. 1, 1997) 10.

⁹⁰ Von Bissing (n. 18) 66; Petrie (n. 7) 22; cf. 36.

⁹¹ Edgar in: Hogarth (n. 10) 50 – my emphasis. Many modern scholars (e.g. Gorton [n. 4] 178) seem to have overlooked Edgar's statement and taken the Greek pottery finds at face value.

they did not find any Greek material in the burnt stratum, only rough kitchen ware not necessarily Greek in character.⁹² We seem to be left with Petrie's word against that of Edgar and Hogarth, but there are other ways of seeing the matter.

Petrie was one of the great pioneers of scientific archaeology, and his preoccupation with measurement was exemplary when recording weights, dimensions, etc. However, he also placed an undue stress on the spot-heights of his finds. This is his report on the burnt layer:

Below the bottom of the stratum in which the scarabs were found, there lies two feet lower a black burnt stratum full of charcoal and ashes, which forms almost the earliest stratum of the whole southern half of the town. According to the average rate of accumulation of earth during Greek times this bed of two feet would represent about half a century. And about half a century before the beginning of the scarab factory would lead us to about the middle of the seventh century B.C.⁹³

Modern archaeologists might see Petrie's calculations regarding "average rate of accumulation during Greek times" as rather quaint. In the same way that he was misled by his belief that brick dimensions could be used as a means of absolute dating,⁹⁴ his trust in absolute height may have deceived him. He himself admitted that spot-heights were "more or less doubtful" when it came to deciding the "precedence in time" of various levels across the site,⁹⁵ and rightly ignored them when they were irrelevant (e. g. in the case of the Apollo *temenos* pit). With respect to the burnt stratum he noted a two-foot difference between the heights for points reported as "at Scarabs" (lowest) and "on S[outh]" near the alleged Great Temenos (highest).⁹⁶ The problem is that Petrie gave no evidence for the topographical relationship between this burnt layer and the Factory.⁹⁷ When he stated that the burnt layer was "below" that

⁹² Hogarth et al. (n. 10) 107.

⁹³ Petrie (n. 7) 5. Apart from a brief mention of three different spot-heights for the burnt stratum (*ibid.* 88, 89), this is his sole account of its stratigraphy. Cf. the 12 pages, numerous plates and even a drawing of the stratigraphic section, given for the pit in the Apollo *temenos*.

⁹⁴ Petrie (n. 7) 6 expressed his belief that Egyptian bricks decreased by "about an inch in length, per century". He used these brick measurements to support his dating of the "Great Temenos" to the early 26th Dynasty. In fact the bricks are almost certainly Hellenistic in date (see Leonard [n. 1, 1997] 8).

⁹⁵ Petrie (n. 7) 21.

⁹⁶ *Ibid.*, 88.

⁹⁷ Matters are further complicated by Petrie's mention of *another* level with "burnt ash and bone" (only vaguely reported) below the Apollo temple, which he dated ("by

of the Factory he only seems to have meant lower in terms of absolute height – a very different matter from “underlying”. His reports give no hint that he actually dug *through* the Factory bed to see what was underneath. (Nor for that matter do those of Gardner or Hogarth.) Conversely, it is implied in Hogarth’s account that the burnt stratum lay *strictly between* the Scarab Factory and the Great Temenos and no further: “the thick burnt bottom stratum, which Mr Petrie dated before all other human remains on the site, was found wherever we sank pits between Mr Petrie’s ‘Scarab Factory’ and his ‘Great Temenos’, but nowhere either north or south of this area”.⁹⁸

It seems that Petrie may have merely *deduced* that the burnt layer underlay the Factory. With regard to the ceramic finds, von Bissing noted that Petrie published only *one* vase from the burnt level. It was Greek (a Chian amphora).⁹⁹ He also made a very important observation with respect to the conflict between Petrie and Hogarth over the nature of the pottery from the burnt stratum. This concerns a wider problem in Petrie’s excavation management. Much of his data collection relied on local farmers, who simply reported their findspots to him. As he paid them for Greek ceramics, but not for Egyptian, it is understandable that they ‘found’ much early Greek ware in the southern part, where Hogarth and Edgar could discover none.¹⁰⁰ As it happens, even the one vessel, the Chian amphora, that he published “from” the burnt layer spoils his argument. He found two very similar vessels at Tel Defenneh, adding the “strange fact” that they were sealed with the cartouche of Amasis.¹⁰¹ A “strange fact” indeed: the Tel Defenneh amphorae are presently dated to the third quarter, and the Naukratis example to the first half of the 6th century.¹⁰² Such a vessel can-

the ordinary rate of accumulation”) to c. 800 BC, or even earlier (*ibid.*). Despite Hogarth’s insistence that the burnt layer did not extend north of the Scarab Factory, there remains a nagging doubt as to which of these *two* burnt levels Petrie identified “at” the Scarab Factory.

⁹⁸ Hogarth et al. (n. 10) 107.

⁹⁹ Petrie (n. 7) 21 & Pl. XVI, 4; von Bissing (n. 18) 36.

¹⁰⁰ Von Bissing (n. 18) 49. Petrie (n. 7) 35 was commendably frank about the limitations of his research in this area of the site: “Comparatively little was done in excavating the town, the three places which took up nearly all our work being the gateway building in the Great Temenos, the large block of chambers in the same, and the temenos of Apollo. Most of the objects from the town were therefore obtained from Arabs digging there for earth... Hence I seldom knew the details of a find, and even the site of it was often not known...”.

¹⁰¹ W. M. F. Petrie, *Tanis II* (London 1888) 64.

¹⁰² Dupont’s Chian amphora types 1g, and 1e-f, respectively. See Cook, Dupont (n. 77) 148–150, 210 nn. 44, 46.

not have underlain (with half a century's accumulation of debris) the Factory with its scarabs of Psammetichus II.

In short, the evidence for the "burnt stratum" presented by Petrie is highly equivocal. Poorly published and self-contradictory, it would seem that Hogarth was justified in rejecting it as a means of relative dating between the Scarab Factory and the earliest Greek settlement.

C. Petrie's final argument concerned the ethnicity of the scarab manufacturers. First, he argued, the Factory was engaged in commercial relations with Rhodes. Second, the errors sometimes committed by the scarab-makers in attempting to render Egyptian hieroglyphics show that they were not Egyptian. The combination of these points shows that the scarab-makers were Greek.

That the Factory was producing for a Greek market seems likely, as apparently Naukratite scarabs have been found on Rhodes and elsewhere in the Aegean.¹⁰³ But that does not tell us that the scarab-manufacturers were Greeks. Hogarth argued that they were Phoenicians, actually a more serious candidate. The Phoenicians – rather than the Greeks – were experts at producing egyptianising artworks, replete with imperfect hieroglyphics. Gorton has identified a number of Phoenician scarab workshops (e. g. in the Levant, Carthage, Sardinia) producing similar products to those of Naukratis.¹⁰⁴ Indeed, a Phoenician identity for the Naukratite craftsmen is accepted by Sullivan. In lieu of firm evidence for Greek colonists as early as c. 650 BC, he argues that they used the material culture of an already present Phoenician settlement. Following Hogarth, Sullivan sees a Phoenician colony at Naukratis as responsible for the scarabs, the finds of decorated tridacna shells and the glazed sandy ware Hogarth found in layers below those with painted Greek pottery of local manufacture.¹⁰⁵ Similar glazed ware found at Kameiros (Rhodes) has long been recognised as Phoenician.¹⁰⁶

Regarding the carved tridacna shells, Möller notes that "current trend would seem to favour their Syro-Phoenician origin".¹⁰⁷ Yet while the style is (Assyrianising) Phoenician, their place of manufacture was likely to have been Naukratis itself, shown by the occurrence of undecorated examples.¹⁰⁸

¹⁰³ Boardman (n. 58) 127; Gorton (n. 4) 92.

¹⁰⁴ Gorton (n. 4) 132–137, 183–184.

¹⁰⁵ Sullivan (n. 50) 187; Hogarth et al. (n. 10) 107.

¹⁰⁶ Edgar in: Hogarth (n. 10) 49.

¹⁰⁷ Möller (n. 5) 165.

¹⁰⁸ Petrie (n. 7) 35; Edgar in: Hogarth (n. 10) 49.

As to the dating, Möller cites Stucky as placing their production between c. 675 and 600 BC: “In this case, the shells would be among the oldest finds in Naukratis”.¹⁰⁹ More specifically Stucky argued that “the end of the third quarter of the seventh century B.C. may be considered the earliest time for the import of *Tricadna* shells to Naukratis”.¹¹⁰ Brandl’s more recent study sets out in detail a strong case for placing the entire carved *Tricadna* industry in a limited period between 630 and 580 BC.¹¹¹ Thus they would still be “among the oldest finds in Naukratis”. The date range allowed by Brandl overlaps with that of the scarabs of Psammetichus II and Apries (595–570 BC) – arguably another product of the Phoenician residents at Naukratis.

It is significant that the earliest firmly datable evidence from the site (scarabs and *tricadna* shells) is better interpreted as evidence of a Phoenician than a Greek presence. By helping to document this, Sullivan unwittingly assisted in removing the *only* absolutely-dated objection to the Herodotean dating – namely the scarabs of Psammetichus II and Apries. In the absence of sound stratigraphic evidence to the contrary, there is no good reason to reject Hogarth and Edgar’s model for the earliest activity at the site. In their view a small Phoenician factory preceded the Greek colony; when the town was given over to the Greeks by Amasis, the Factory was closed: “It appears... that shortly after the death of Apries the Phoenicians for some reason either gave up their Egyptian business or removed it elsewhere”.¹¹² This remains the most economic way to explain the pattern of scarab evidence – including the otherwise puzzling absence of any from the reign of Amasis, when all parties agree that the site was occupied by the Greeks.

It also makes good sense in terms of mid-26th-dynasty international affairs. To counter the nascent Babylonian empire, Egypt became increasingly reliant on professional troops from the Aegean (the well-known Carians and Ionians). At the same time it was a longstanding Egyptian policy to maintain commercial ties with the Lebanon, principally to secure supplies of timber needed for temple construction. This Phoenician

¹⁰⁹ Möller (n. 5) 165.

¹¹⁰ R. A. Stucky, *The Engraved Tricadna Shells* (São Paulo 1974) 92.

¹¹¹ B. Brandl, “Two Engraved *Tricadna* Shells from Tel Miqne-Ekron”, *BASOR* 323 (2001) 49–62, esp. 58–60. Excluding the Greek findspots (Cyrene, Samos and Naukratis), to avoid circularity of argument, there are four Mesopotamian contexts (Nimrud, Assur, Babylon, Warka) to confirm his date range. As Brandl remarks, the dating of the last three disproves Stucky’s contention (*op. cit.*, 95) that “in Mesopotamia no fragments occur in clearly Post-Assyrian levels...”. Actually, they show that a slightly later end for the range, a decade or so after 580 BC, is probable.

¹¹² Hogarth (n. 10) 50.

policy continued through the mid-26th Dynasty, when we find Necho II using Phoenician sailors to explore the Red Sea. His successor Psammetichus II visited Byblos personally in 592/1 BC.¹¹³ According to Hdt. 2. 161, Apries fought naval battles with Tyre and Sidon, though this is usually interpreted as a garbled report of conflicts with Nebuchadrezzar's forces, during their long siege of Tyre between 586–574 BC.¹¹⁴ The Egyptians are thought to have supported, or even instigated, the rebellion which led to the siege. It is against this background that we can envisage the establishment of Phoenician trading/manufacturing concern at Naukratis about 600 BC, or slightly earlier. However, the political map was about to be sharply redrawn. In the early years of the Babylonian empire Phoenicia, under the kings of Tyre, had retained a measure of independence. But, as Freedy and Redford put it: "at the conclusion of the thirteen-year siege in 574 or 573 B.C. Tyre was definitely in the Chaldaean camp... When next an Egyptian king would consider foreign alignments to counter Asiatic initiatives, it would be to Greek freebooters and to Greek tyrants that he would turn".¹¹⁵ Indeed, when Nebuchadrezzar attacked Egypt in 567 BC (by sea as well as land), Phoenicians would have been pressganged into service in the same way they were a century earlier by the Assyrians in their successful attack on Egypt. Thus the suggested closure of a Phoenician commercial foundation at the site in favour of the Greeks can be seen as a reflex to the dramatic changes in international relations at the very time of Amasis' succession.

In conclusion, the evidence from the Scarab Factory provides no support for the foundation of Greek Naukratis as early as the 7th century BC. The evidence for Psammetichus I is uncertain, while scarabs of Necho II are absent. The only pharaohs seriously represented are Psammetichus II and Apries (595–570 BC). The Factory belongs either to the earliest Greek settlement (following von Bissing) or, more likely (following Hogarth), to a short-lived Phoenician venture that was closed when Amasis gave Naukratis to the Greeks (c. 570–565 BC). Such a model might clarify the longstanding question of the *polis* that Amasis gave over to the Greeks. Herodotus implies the existence of a non-Greek town before Amasis (see above). Firm evidence has always been elusive; certainly no substantial Egyptian remains have been found which would fit the bill. If we envisage the pre-Greek town as a Phoenician factory and harbour with an attached Egyptian village then we may – at long last – have an answer.

NAUKRATIS AND OLD SMYRNA

Finally the question of the earliest Greek pottery at Naukratis needs to be addressed. Bowden notes that it comes from many parts of the site, "but

¹¹³ K. S. Freedy, D. B. Redford, "The Dates in Ezekiel in Relation to the Biblical, Babylonian and Egyptian Sources", *JAOS* 90 (1970) 462–485, esp. 478–480.

¹¹⁴ Freedy, Redford, *op. cit.*, 481–484; T. G. H. James, *CAH* III (31991): 2, 725; Grimal (n. 70) 362–363.

¹¹⁵ Freedy, Redford, *op. cit.*, 484.

where the original location is known it is mostly from the various sanctuaries. Most of the material therefore is from dedications, and ought thus to reflect the fortunes of the city reasonably well".¹¹⁶ So he proposed a simple experiment to compare the conventional and Herodotean chronologies against the pottery finds. He quantified the pottery as catalogued by Venit, according to conventional dates (within five years), plotting it into a simple graph of numbers of sherds against time. Using the conventional dates, there is a massive increase in the amount of pottery at the end of the 7th century BC. Then there is a sharp downturn c. 525 BC at the time of the Persian invasion of Egypt. Both swings on the graph conflict with the clear statements of Herodotus, not only regarding the date of the settlement, but also with respect to 525 BC, which he depicts (3. 139) as a 'boom time' for the Greeks in Egypt: "when Cambyses, son of Cyrus, invaded Egypt, many Greeks came with the army, some to trade and some to see the country itself".

Bowden argued that if the pottery chronology is reduced by a hypothetical 40 years, the ceramic pattern then corresponds to Herodotus' narrative at three points: (1) the sharp upswing is now at c. 565 BC, matching the settlement under Amasis; (2) there is no decline at Cambyses' invasion, c. 525 BC; (3) and instead the downturn reflects the effects of Xerxes' punitive repression of the Egyptian revolt in 485 BC (Hdt. 7. 7). There is much to be said for Bowden's quantitative approach. He hints at a methodology which, though it may be uniquely applicable to Naukratis, may circumvent the circular arguments concerning 'pre-colonial' Greek pottery seen at Selinus and other western sites.¹¹⁷

But is such a model possible or likely? Taking Bowden's correspondences in reverse order, the last (3) would involve a shift in dating from 525 to 485 BC. This is problematic. Francis and Vickers argued that Greek pottery chronology can be revised as late as the mid-5th century BC, but despite growing appreciation of the fragility of the conventional Archaic chronology, their proposals (especially for such late dates) have found no

¹¹⁶ Bowden (n. 3, 1991) 53.

¹¹⁷ NB. The limited number of sherds dated earlier than the end of the 7th century are not from known contexts and allow for the limited importation of Greek ware into the site before the reign of Amasis, during the time of the Scarab Factory. It is also possible that some Greek (and Cypriot) traders were present in an essentially Phoenician settlement; but this would not affect the argument here, which concerns the major pottery trends associated with the buildings. It would be inconceivable that Greeks could have built sanctuaries at Naukratis without the pharaonic permission that Herodotus describes.

support. A major stumbling block is provided by the Attic pottery from the Mound built for the warriors who fell at Marathon in 490 BC.¹¹⁸ The fixed point provided by these finds has never been dealt with adequately by Francis and Vickers.¹¹⁹ As it would cross this threshold, Bowden's (3) proposal (4) seems unlikely. Any possible compression in the dating must reach near minimum (say ten years or so) by 490 BC. Nevertheless, Bowden's experimental model could be improved by considering a smaller reduction (25 years) at this point. Rather than the suppression of Xerxes in 485 BC, the sharp drop in Greek imports at Naukratis might be better associated with the Ionian Revolt against Darius I (499–494 BC). The Revolt extended to embroil Cyprus, involved naval warfare of Greek versus Phoenician and Egyptian fleets, and ended with the enslavement of Miletus and other calamitous developments – these events would surely have had severe repercussions for trade between East Greece and Naukratis.

However, Bowden's earlier correspondences are extremely plausible. Without specifying a particular reduction, the invasion of Cambyses (2) would no longer correspond with a 'crash' at Naukratis but to one of the prosperous decades of the mid-6th century BC. With respect to the massive (1) upswing in Greek pottery (3), associated with the first temples, this naturally fits Herodotus' account of the settlement under Amasis. Here a large reduction (of up to 40 years) is more reasonable. It is far less drastic than the Francis & Vickers model, which would involve a reduction of some 60–80 years at this point. It is also close to the conclusion arrived at independently by the present author and colleagues on other (partly Near Eastern grounds), recommending a chronology approximately halfway between the Francis and Vickers and conventional models¹²⁰ – a notional revision of some 35 years.

A lowering of Archaic chronology (at c. 600 BC) by three decades or so is in step with the 25-year reduction for Late Geometric discussed above. And while speculative, it should be noted that similar low datings for the Corinthian (and related) sequences have long formed a respectable "undercurrent" in the literature. In establishing his dates for Early Corinthian (625–600 BC), Payne had to dismiss the opinions of Pottier, Rumpf and others who saw the preceding Protocorinthian style lasting as late as 580 BC,¹²¹ a difference of some 45 years. Maintaining his earlier position, Langlotz proposed that Payne's start date for Middle Corinthian should be lowered from 600 BC

¹¹⁸ As stressed in James et al. (n. 23) 97.

¹¹⁹ See the criticism of Biers (n. 32) 101.

¹²⁰ James et al. (n. 23) 359 n. 11; 372 n. 65.

¹²¹ Payne (n. 25) 22.

by 20 to 30 thirty years, i. e. 580 or 570 BC.¹²² Gjerstad suggested lowering Middle Corinthian by 25 and Ducat by 15 to 20 years.¹²³

The Early to Middle Corinthian transition is particularly important at Naukratis. There are a few pieces of Early Corinthian without firm context, but the succeeding Middle Corinthian and contemporary East Greek styles are well attested – hence the conventional dating of the settlement close to the EC/MC transition. Lowering the “600 BC” of the Payne/Cook chronology by 35 years would bring it to c. 565 BC, early in reign of Amasis. Such a reduction would not apply evenly to every site, pottery style or even dating-scheme and indeed only applies to the Payne/Cook chronology. In ~~her~~ his monumental study of Corinthian pottery Amyx has already lowered the EC/MC transition to 595/590 BC.¹²⁴ Taking this as the “norm”, as most scholars now do, the notional reduction from the conventional chronology need only involve 25 years to bring us to 570/565 BC. A modest revision of this length would restore harmony between the Greek pottery and the account of Herodotus, as well as the dating of the Cypriot sculptures.

Another focus of dispute between Herodotean and archaeological chronologies is Old Smyrna, the key site for the dating of Corinthian pottery after Selinus. Hdt. 1. 16 states that Alyattes the Lydian conquered Smyrna, and a destruction level generally thought to reflect this event has been identified at the site. It was evidently destroyed during EC, “and well before the end of that period”.¹²⁵ Herodotus should thus provide us with a fixed point within EC. The problem is, at which point in the long reign of Alyattes (c. 618–560 BC) did Smyrna fall? Cook and Dupont state that “it should have been early, though after his five seasons of campaigning at Miletus” and dated the sack to c. 600 BC.¹²⁶ Yet neither they, nor anyone, has produced historical evidence that the siege occurred soon after the Milesian campaign or that it was “early”. Against this is an observation (of

¹²² E. Langlotz, Review of H. Payne, *Necrocorinthia*, *Gnomon* 10 (1934) 418–427.

¹²³ E. Gjerstad, *The Swedish Expedition to Cyprus IV:2* (Stockholm 1948) 208 n. 1; J. Ducat, “L’archaïsme à la recherche de points de repère chronologiques”, *BCH* 86 (1962) 165–184, esp. 181. For these and other scholars who have argued for “extremely low” Corinthian dates see R. J. Hopper, “Addenda to *Necrocorinthia*”, *BSA* 44 (1949) 162–257 and D. A. Amyx, *Corinthian Vase-Painting of the Archaic Period: Vol II, Commentary: The Study of Corinthian Vases* (California 1988) 403–413.

¹²⁴ Amyx, *op. cit.*, 428.

¹²⁵ J. K. Anderson, “Old Smyrna: The Corinthian Pottery”, *BSA* 53–54 (1958–1959) 138–151, esp. 148.

¹²⁶ Cook, Dupont (n. 77) 9.

Langlotz and others) recently revived by Bowden. In Herodotus' digest (1. 16) of Alyattes' four great campaigns (other than Miletus), the conquest of Smyrna is listed after the war against Cyaxares the Mede. If we understand his list to mean consecutive events, then Herodotus thought Smyrna was captured after the Median war (Hdt. 1. 73–74). This culminated famously – partly due to the occurrence of the eclipse allegedly predicted by Thales – in the treaty of 585 BC.¹²⁷ Consequently Langlotz dated the sack of Old Smyrna to c. 580 BC.

John Cook, the excavator of Old Smyrna, rejected the idea “as it conflicts with the archaeological evidence and would entail a drastic revision of Corinthian pottery”. Appealing to Payne's date from Selinus, he added: “It would be hazardous to interpret the literary evidence in a way which would make it necessary to bring down the lower limit of the Early Corinthian style to a date considerably after 585 B.C.”.¹²⁸ It would seem, once again, that the conventional Archaic Greek chronology has been used to influence our understanding of Herodotus. Of course the argument from Herodotus' clipped account is far from conclusive. But the point remains that there is nothing in his account to prevent a date after 580 BC and nothing to support a date early in Alyattes' reign – only a clue suggesting it occurred late. Thus apart from the ‘known’ pottery dating, there is no objection to Langlotz's date of c. 580 BC – or even 575 BC. Ironically this was the range originally preferred by Robert Cook; in the very paper where he dismissed Gjerstad's Naukratis chronology he stated that Old Smyrna was “destroyed late in the first quarter of the century”!¹²⁹

Old Smyrna also provides a control on the Francis and Vickers model. They argued that its destruction date could be lowered from c. 600 BC to c. 540 BC, by linking it with the campaign of Harpagus the Mede who subdued the Ionian cities for the Persian conqueror Cyrus (Hdt. 1. 162). In support, Vickers argued for greater respect for the Herodotean tradition concerning Naukratis and noted an apparent anomaly.¹³⁰ The Chian amphora which Petrie published from his notorious burnt stratum is similar both to examples from Old Smyrna (conventionally before 600 BC) and those from Tel Defenneh sealed with the cartouche of Amasis already mentioned. Cook

¹²⁷ See D. Panchenko, “Democritus' Trojan Era and the Foundations of Early Greek Chronology”, *Hyperboreus* 6 (2000): 1, 31–78, esp. 67 n. 85.

¹²⁸ J. M. Cook, “Old Smyrna, 1948–1951”, *BSA* 53–54 (1958–1959) 1–34, esp. 26.

¹²⁹ Cook (n. 17) 89.

¹³⁰ Vickers (n. 31) 18–19.

acknowledged this as a “positive argument” for a revision, but he had to point out that their dating of the sack of Smyrna as late as 540 BC would make the observation “backfire” – as the amphorae from the Old Smyrna destruction are “early in the [Chian] series, the Amasis ones late”.¹³¹ But, as we have seen, we are not restricted to the choices of 600 BC and 540 BC for the fall of Smyrna – a date around 575 BC is also plausible. The chronology argued here would bring the Smyrna and Naukratis amphorae much closer in time, but also keep them in their correct relative order: the former deposited before c. 575 BC, the latter after 570 BC.

CONCLUDING REMARKS

An Archaic chronology can be developed which harmonises the fixed points offered by Herodotus (Alyattes and Smyrna; Amasis and Naukratis; Cambyses and Greeks in Egypt) with the relative ceramic dating.¹³² If we allow that the Scarab Factory at Naukratis was a Phoenician concern, closed when the site was granted to the Greeks near the beginning of the reign of Amasis, the same chronology does justice not only to the testimony of Herodotus but to the Egyptian (scarab) evidence, the stratigraphic observations of Gjerstad and the generally accepted dating of the Cypriote sculptures.¹³³

Such a model is attractive, especially as it allows Herodotus’ account and the Egyptian evidence to intertwine at different levels, both historical and archaeological. It would not seem to face any concrete obstacles in the Aegean world or in Egypt.¹³⁴ The only apparent conflict with such a Herodotean/pharaonic chronology concerns the Palestinian sites with finds of Corinthian and East Greek pottery similar to that of Old Smyrna and Naukratis. However, the dating of these Palestinian sites remains *sub judice*. There are major uncertainties about the local pottery chronology, compli-

¹³¹ Cook (n. 31) 165, citing Dupont. See n. 102 above.

¹³² Bowden (n. 3, 1991) 51; (n. 3, 1996) 28 n. 61 touches on a further case, Tocra in Cyrenaica, where he feels the conventional dating of the earliest settlement (with EC pottery) can only be maintained at the expense of distorting Hdt. 4. 1. 59.

¹³³ It is significant that Gjerstad’s low chronological scheme for Cyprus was itself based on scarab dating – for comment and references see Sørensen (n. 75); James et al. (n. 23) 153–154, 367 n. 37.

¹³⁴ There is not space here for a full discussion of all the proposed ‘fixed’ points for late 7th–6th century Greek pottery. But see e. g. J. Boardman, (“Dates and Doubts”, *Archäologischer Anzeiger* [1988] 423–425) who allows that a compression of 25 years in the internal chronology of 6th-century Attic vase painting would be biologically feasible in terms of the life-spans of the known painters.

cated by circularity of argument with borrowing from the Greek dates. I hope to address this question in detail elsewhere.

In his incisive review of the Francis and Vickers chronology Cook stressed the value of their critical work and concluded: “The conventional absolute chronology is much less sure than is often supposed... There is continuing need for minor modifications of the relative chronology, for example that of much East Greek pottery; and stylistically determined sequences are always liable to be too rigid”.¹³⁵ As it has now done for well over a century, a combination of literary and archaeological evidence from Naukratis strongly argues for such a modification. Its promise as a fixed point in Archaic chronology – perceived long ago by Hogarth and Gjerstad – has never been realised. The time may now be ripe for the evidence from Naukratis to come into its own.¹³⁶

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Геродот относит возникновение греческой колонии в Навкратисе ко времени фараона Амасиса (570–526 гг. до н. э.). Обычно считается, что археология опровергла Геродота, поскольку из принятой системы датировки архаической керамики следует, что колония была основана в конце VII в. до н. э. Однако рассмотрение совокупности археологических и исторических данных подводит к выводу, что Геродот прав, а в системе датировки по керамике кроется ошибка. Материалы Навкратиса (а также Селинунта и Смирны) побуждают заключить, что основанная на керамике хронологическая система должна быть сдвинута вниз от рубежа VII–VI вв. на 25–35 лет.

¹³⁵ Cook (n. 31) 170.

¹³⁶ For help and encouragement I would like to thank Alan Griffiths and Dmitri Panchenko, and for reading various sections and providing specialist comments Robert Morkot (Egyptology), Pamela Gaber (Cypriot sculpture), Nick Thorpe (archaeological methodology) but especially Nikos Kokkinos (for many years of joint research and discussion of Greek chronology). All errors of judgement are naturally my own.