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Review Feature

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Review Feature

Centuries of Darkness

by Peter James

in collaboration with I.J.Thorpe, Nikos Kokkinos,
Robert Morkot & John Frankish. Foreword by Colin Renfrew.

London: Jonathan Cape, 1991, 434pp.

Some years ago, Mortimer Wheeler bemoaned archaeologists' obsession with chronology, saying 'we have been preparing time-tables; let us now have some trains' (Wheeler 1954, 245). Today, almost forty years later, many basic problems of chronology are still to be resolved, and despite Wheeler's justifiable frustration it is clear that without a reliable chronology it is difficult to develop confident models to explain past processes and events. True, significant advances have been made, particularly in methodology. Scientific techniques now provide ever more reliable and accurate timescales, and the development and extension of dendrochronology allows precise dating for most of the historic and prehistoric periods as far back as the neolithic. For the early history of the western Old World, however, considerable reliance must still be placed on the historical chronologies of Egypt and Mesopotamia. These are the result of over 150 years' work by philologists and archaeologists. Though the basic framework which they provide is constantly being refined in the light of new discoveries and reassessments, the outline established some 50 years ago has come to be broadly accepted, both by Near Eastern historians, and by prehistorians working in areas such as Greece and Asia Minor who have taken their chronological fixes from direct ties with the better-documented regions. Early this year, a dramatic challenge to this framework appeared in Centuries of Darkness, a book written by a group of younger scholars who claim that the accepted chronology of the Late Bronze Age is as much as 250 years too high. They would move the end of the Egyptian New Kingdom from 1070 BC to around 825 BC, and make Ramesses II, the creator of the great rock-cut temple at Abu Simbel, a pharaoh of the eleventh-tenth centuries rather than the thirteenth, as is generally thought. The authors of this new proposal maintain that down-dating the end of the Late Bronze Age by this amount would solve many of the problems associated with the so-called Dark Age of Greece and parts of the Near East which followed the collapse of the Hittite empire and the Mycenaean palace civilization, conventionally placed at around 1200 BC. Is such a dramatic revision possible in such a relatively well-documented part of the ancient world? To assess the implications of this theory, we have here invited a number of regional experts to comment on the proposed chronological revolution. First, however, the authors of Centuries of Darkness summarize their radical proposal.

Centuries of Darkness: Context, Methodology and Implications

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It is nearly 100 years since the Cambridge University Press invited Classical scholar Cecil Torr to present his case for rejecting the high chronology then being imposed on Mycenaean civilization by the discoveries of Flinders Petrie in Egypt. Again today, as in the 1890s, the chronology of the Late Bronze to Iron Ages of the ancient world is at the cross-roads. We see our work in the context of many recent trends in chronological research¹, which have intensified since our preliminary discussion (James *et al.* 1987).

In the Mediterranean these include: the un-hinging of the earliest Late Minoan from its traditional link with the beginning of New Kingdom Egypt (first raised in Warren 1979, 106-107), now embroiled in the argument over the dating of the destruction of Thera (Betancourt 1987; Warren & Hankey 1989, 138-41); the abandonment of the fixed point used to date the Middle and Early Geometric (and hence the Protogeometric) once thought to be provided by Tell Abu Hawam III (Warren & Hankey 1989, 167); the re-examination of the Greek pottery at Veii which supports a reversion to the low chronology for the later phases of Early Rome; and Francis' and Vickers' sustained re-examination of the foundations of Late Geometric and Archaic chronology.

In the Near East, recent developments include: increasing doubt about the value of the Egyptian-based date for the end of the MBA in Palestine (Hoffmeier 1989; 1990; 1991); Bietak's promised restructuring of the relative chronology between Egypt and Palestine during the early LBA (1984; 1989); the discovery of Midianite ware (usually dated by Egyptian chronology to the thirteenth-twelfth centuries BC) at Teyma in northwestern Arabia in a context no earlier than the eighth century BC (Muhly 1984); the backdating of the Lion Gate inscriptions at Hittite Malatya by Hawkins from the tenth century to the late twelfth/eleventh; the crisis in the palaeographic dating of the 'Proto-Canaanite' / early Phoenician alphabet (placing Egyptian and Assyrian chronologies in direct conflict) caused by the discovery of the bilingual Tell Fakhariyah statue; Wightman's reopening of the controversy

surrounding the identification of Solomonic strata in Palestine (1990); and Barkay's redating of the end of Palestinian Iron Age II to 520 BC, rather than 587 BC as hitherto universally accepted (Mitchell 1990).

Most important of all, Sothic dating, once the key to a chronological system stretching as far afield as Wessex and Iran, is now being quietly abandoned by Egyptologists. Based on the theory that the Egyptian calendar shifted gradually with respect to the seasonal year and the heliacal rising of Sirius, Sothic dating has never been adequately demonstrated. At the first *High, Middle or Low?* conference at Gothenburg in 1987, Helck drew attention to a long-neglected and fatal flaw in the interpretation of the supposed Sothic date given by the Ebers Papyrus, hitherto the linchpin of New Kingdom and later chronology. At the second *High, Middle or Low?* conference held in Austria last year, Helck was joined in his critical stance by several other Egyptologists.

These are just a few of the chronological cans of worms which have recently been opened by scholars working broadly within the orthodox dating framework. Our understanding is that treatment of these problems on an *ad hoc* basis, concentrating on individual areas, is inadequate. So too is an Egyptocentric approach to chronology. As we hope to have shown in *Centuries of Darkness*, the long-standing difficulties of Late Bronze to Early Iron Age chronology must be seen as interconnected. Their focus is, of course, the prolonged 'Dark Ages' which supposedly afflicted the ancient Mediterranean and Near East roughly between 1200 and 800 BC.

The Dark Ages in Perspective

The current understanding of the nature of these Dark Ages presents some seemingly intractable problems. The puzzling 'lacunae' in the archaeological record are not simply gaps in our knowledge, as they concern some of the most heavily excavated regions in the world. For example, after the withdrawal of the Egyptian administration in the early 11th century BC, Nubia was supposedly depopulated. The reappearance of Egyptian culture around 800 BC, with the same pottery, the same gods and temples and the same script which they had used 300 years previously, has always been seen as a new beginning. But where did the Nubians go in the meantime?

Not a shred of evidence has been discovered of their whereabouts during this 'absence'. Moreover, there is no break in the archaeological record, in which 'eleventh' century remains blend imperceptibly into those of the early eighth: indeed, it is virtually

impossible to distinguish between Nubian pottery of the 20th Dynasty and that of the early 25th Dynasty. Troy presents a strikingly similar picture over the same period of time. The site was supposedly abandoned during the tenth century, and then reoccupied c. 700 BC by people still using Grey Minyan Ware. In Blegen's opinion they had simply moved away to an unknown location, then returned using the same material culture. Even more confusing, Greek Geometric sherds of eighth-seventh century type are also found in buildings which date to before the abandonment. In central Turkey we find a familiar story, with the Hittites disappearing in the early twelfth century, to be followed, after a long period of utter darkness, by new Phrygian settlers in the late ninth century. Yet at Gordion, excavations have produced an apparently impossible sequence, showing that the latest Hittite pottery was only gradually replaced by Phrygian wares.

In the Western Mediterranean the coast of Sicily was supposedly abandoned, then reoccupied, then abandoned again and finally occupied once more by Greek colonists. Despite the clear statement of Thucydides that the Greek colonists expelled the local inhabitants of Syracuse, the present archaeological dating has them settling on a site which was violently destroyed, but a century too early to match the historian's account.

During the Post-Kassite Period in Babylonia (1050-750 BC), securely datable remains are almost completely absent, even at the major cities. Despite intensive field surveys it has been hard to identify pottery from this period. The number of cuneiform texts dating from 1000-750 BC dwindles to a mere sixty, posing the problem of how literacy itself could have been preserved over such a long time. In Iran there is a similar lacuna in the documentary record between the break-up of the Middle Elamite Empire c. 1100 BC and the resurgence of Elam in the eighth century BC. Again, even the identification of a distinct material culture for the intervening years has proved difficult.

These are merely some of the worst scenarios. Many seem to defy common sense, and at times fly in the face of literary evidence which there is no other reason to doubt. In other areas, continuous sequences of material culture have been created, but their dating and interpretation is extremely problematic. Cypriot archaeologists still adhere to the low dating of Gjerstad, which minimizes the Dark Age of the tenth-ninth centuries (see Table 1). But their chronology is at variance with the dates provided by well-stratified finds of the same material found in Palestine. Despite the evolution of the Archaic Cypriot script from the

Cypro-Minoan used during the Bronze Age, there is only a single inscription to fill the void in literacy between the eleventh and eighth centuries. In Greece, ceramic specialists have been forced to stretch out the meagre remains of the Submycenaean, Protogeometric and Geometric periods to fill the time from 1075 BC to 700 BC imposed by the accepted chronological framework. A wide range of skills including pottery painting, literacy, monumental architecture, and bronze and ivory-working disappear with the Mycenaean palaces and then begin to reappear in the ninth century or later, with no intervening examples to explain the continuity.

The LBA traditions reflected in Geometric ivory-working, as well as the skills themselves, are supposed to have been preserved in the Levant; but here also there is a mysterious gap in the archaeological record over the same period, with no examples known between the early twelfth century and c. 900 BC. Yet reviewing the stylistic evidence for continuity between the LBA and the earliest IA examples, Herrmann (1989, 105) insisted that from an art-historical perspective the twelfth to tenth centuries were 'almost certainly no Dark Age in the Levant'.

While plausible models for a recession descending at the end of the Bronze Age are available, explanations in terms of revivals, heirlooms or the use of perishable materials fail to explain the underlying continuities in material culture over such an extended period. In one sense the present authors have returned to the position taken by Classical archaeologists Murray and Torr, and Egyptologist Lieblein, who around the turn of the century were bold enough to challenge the chronology itself, and fiercely resisted the trend to introduce ever-growing 'Dark Ages', required by the popular high Egyptian chronology, into the histories of Greece and Anatolia. There is little point enlarging on the nature of the Dark Ages, a process which inevitably involves qualitative judgments, unless we are first sure of their length. Expressed simply, are the late twelfth to early ninth centuries BC a *real* period of time or a creation of nineteenth century scholarship?

Chronology of the Third Intermediate Period

Strangely, the imminent collapse of Sothic dating does not appear unduly to worry most Egyptologists, who believe that the historical Egyptian chronology is now so well-refined that it can stand without reliance on astronomy. The clothes have no Emperor inside them, but apparently the outfit once worn by the imaginary Emperor is good enough by itself to size him up. Proponents of the traditional Egyptian chronology

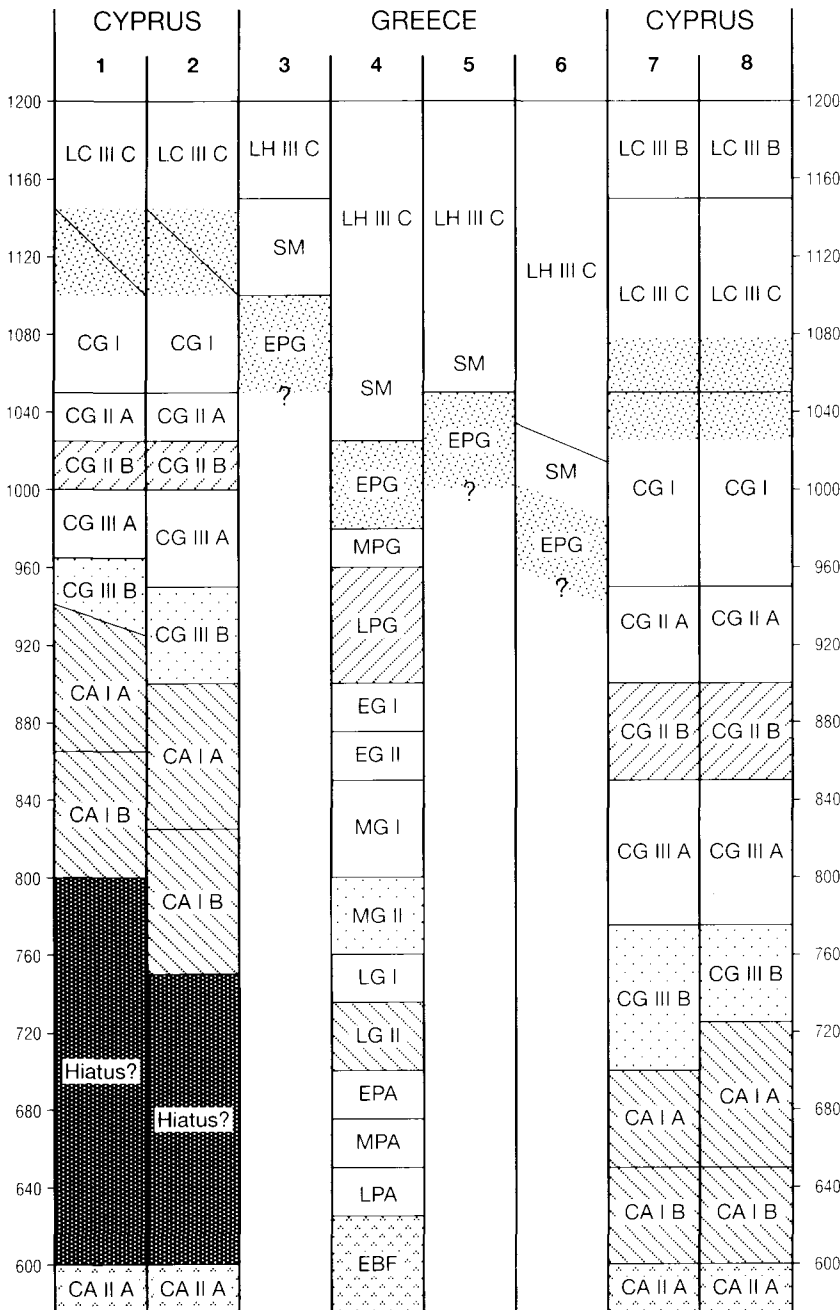


Table 1. The 'high' and 'low' chronologies for Iron Age Cyprus. The first two columns show the development of Van Beek's scheme, based on links with the 'high' Palestinian chronology. The last two show Gjerstad's scheme, a local relative sequence tied to a 'low' Palestinian chronology at its beginning, and a combination of Greek and Egyptian synchronisms at the later end. The middle columns show the varying dates offered by Aegean archaeologists for the Greek Iron Age, which fall uncomfortably between the two Cypriot extremes. Shaded areas indicate those pottery phases which should be partly contemporaneous. Van Beek's dates, while consistent with those currently preferred by the majority of archaeologists in Israel, would have drastic consequences if applied to Greece, as well as introducing a 'dark age' in the Cypriot sequence during the eighth and seventh centuries. A solution to this quandary can be found by a reduction in the Iron Age dates for Palestine. 1, Van Beek 1951; 2, Van Beek 1955; 3, Furumark 1941; 4, Furumark 1944, Desborough 1952, Coldstream 1968, Cook 1972; 5, Iakovidis 1970; 6, Mountjoy 1988, Hankey 1988; 7, Gjerstad 1948; 8, Gjerstad 1974.

now maintain that dead reckoning and synchronisms with Western Asia provide the basis for their chronology, but a brief look at the end of the Third Intermediate Period reveals a disturbingly uncritical approach to the data.

Firmly dated Egyptian history begins with Taharqa (690-664 BC) of the Nubian 25th Dynasty, who can be securely placed in time by links with Assyrian history. Immediately before this date, however, the conventional chronology, canonized in Kitchen's

monumental *The Third Intermediate Period in Egypt* (1986), begins to drift away from the evidence. Before Taharqa, Kitchen found a 'fixed point' in an Assyrian text which he understood as meaning that an anonymous Nubian ruler had conquered Egypt by 712 BC. From this he guesstimated the beginning of the reign of Shabaqo (who is known to have conquered Egypt) as 716 BC². It is now universally accepted that the text was mistranslated (Redford 1985), yet Kitchen (1986, 583) still adheres to his dates for this king.

Before the reign of Shabaqo the conventional model relies on a series of tangled (and often circular) calculations to date the last rulers of the Libyan 22nd and 23rd Dynasties. Synchronisms with Western Asia are again invoked, but are unconvincing. An Assyrian text of 716 BC refers to a king Osorkon ('Shilkanni'), while *c.* 725 BC the Bible records that the king of Israel solicited help from a Pharaoh called So, a name which Kitchen identifies as (O)so(rkon). To suit these references (and another to a king Osorkon on the Victory Stela of the Nubian ruler Piye) Kitchen has inserted into his chronology an 'Osorkon IV' (with a reign of some 14 years), whose existence, despite these far-reaching international connections, is supported only by the evidence of a single ring. The well-attested Osorkon III is ruled out by Kitchen on chronological grounds, since he has dated him much earlier (787-759 BC). There are, however, good reasons to believe that Osorkon III (whose daughter was adopted by Shabaqo's sister) was still reigning in 716 BC, reducing the length of the Third Intermediate Period by at least 43 years.

Kitchen's next fixed point is borrowed from biblical chronology via the generally accepted equation of the Egyptian 'King Shishak', who invaded Judah *c.* 925 BC, with Shoshenq I, founder of the 22nd Dynasty, thereby placing Shoshenq's last year in 924 BC. This date is supported, in Kitchen's view, by 'the series of known regnal years of his successors, which fill up the interval 924-716/712 BC almost completely, leaving just 14/18 years for the one king (Osorkon IV) whose reign is poorly documented in terms of monumental year-dates' (1987, 38). Here the supposed use of 'dead reckoning' backwards is nothing more than the filling up of an already preconceived time-frame. Osorkon IV, who has no year dates at all, is by no means the only poorly attested king from the Third Intermediate Period. For the 21st Dynasty there are no regnal years for its founder Smendes, while Dodson (1987) has argued persuasively for the abandonment of an independent reign of 14 years for Psusennes II. As our book was in press, a paper appeared by Aston (1989) coincidentally agreeing that there was an overlap of some 20 years between the reigns of Takeloth II and Shoshenq III. Rather than subtracting this figure from the overall total for the Third Intermediate Period, Aston at that time elected to give the surplus years to Osorkon II. Much of current research on this period amounts to number-juggling within a pre-set framework, based ultimately on the Sothic dating of the New Kingdom and on the link between Shoshenq I and Shishak, an identification questioned by Wallenfels (1983) on the basis of Phoenician

palaeography and by us on geopolitical grounds.

The minutiae of Third Intermediate Period chronology may appear to be of little interest to archaeologists outside Egypt, but they now constitute the bread-and-butter of Egyptian dating. As we have stressed, it has not been fully appreciated that for every doubtful year of Egyptian history granted to the this period, another year is added to the Dark Ages of the Mediterranean and Near East. With the apparent demise (or at least extreme ill-health) of Sothic dating, all would now seem to be 'up for grabs'. Egyptologists make increasing reference to Mesopotamian chronology to support the dates for the New Kingdom originally derived from Sothic dating and fine-tuned by lunar dating. But most of the standard synchronisms actually rely on circular arguments, or dubious restorations of texts, and only two links seem to be reasonably clear. In any case, the value of such synchronisms is of course dependent on another question of overriding importance, the accuracy of the chronology derived from the Assyrian King List. Rowton (1970, 195-6) cited Egyptian and Hittite evidence to verify the dates of Assurballit I, underscoring the perfect circularity of the reasoning behind much of the cross-dating between Mesopotamia and the Near East.

A New Approach

In an attempt to restore some order to the chaos in which the chronology of the ancient Near East and Mediterranean presently stands, we have presented an alternative view. As well as ruthlessly jettisoning many long-standing assumptions, we have attempted a new methodological approach to the problems based on the following points:

1. None of the previously accepted 'astronomical' methods of dating the Middle and Late Bronze Ages of the Near East is acceptable by today's critical standards. Belief in Sothic dating, and in any of the various Babylonian chronologies derived from the 'Ammizaduga' tablets, must be suspended. At present they provide no independent basis for chronology.
2. A review of the existing radiocarbon evidence shows it to be inadequate as a foundation for a chronology for the period and area under discussion, and certainly does not confirm the conventional scheme. The number of sites with proper sequences of C14 dates needs to be dramatically increased. At present there are less than half a dozen sites for which sufficient

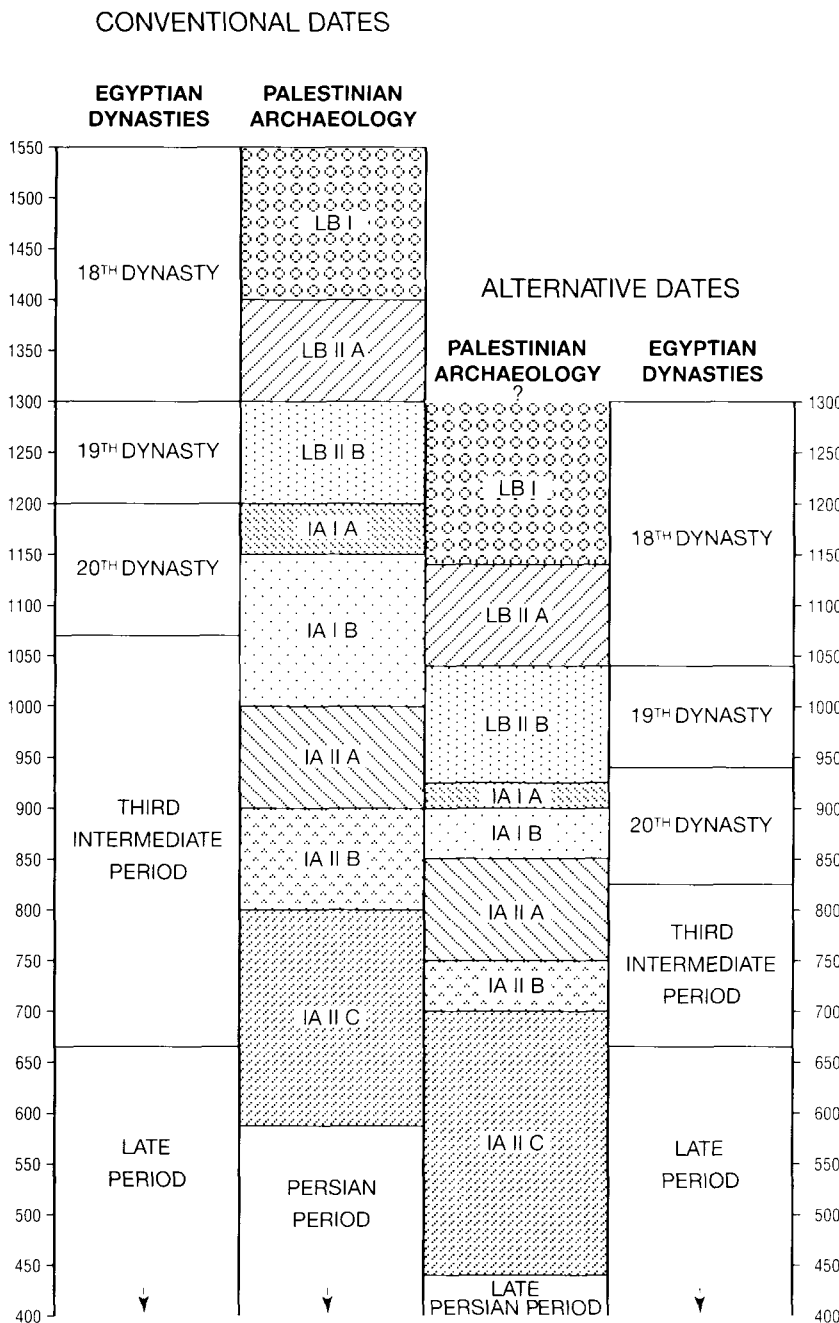


Table 2. A comparison of the conventional and alternative dates for the archaeological phases of Late Bronze Age to Iron Age Palestine. The conventional dates are based on synchronisms with the generally accepted historical chronology of Egypt. The alternative dates are consistent with the biblical record and other non-Egyptian evidence and are in step with the compression of Egyptian historical chronology set out in Table 3.

samples of short-lived materials have been collected to make discussion worthwhile. The most important of these is Thera, where a mass of dates suggests that the site was destroyed a century earlier than previously thought. One might therefore imagine (leaving aside the ongoing arguments concerning the emission of old carbon into the atmosphere by volcanoes) that the conventional chronology is really too

low. It is possible, however, that the explosion of Thera took place before the New Kingdom, during the Second Intermediate Period whose length remains uncertain. As it has been argued on independent grounds that the length of the LBA should be increased, the date of the Thera explosion has little bearing on the chronology of the end of the LBA. A new chronology may eventually be founded on tree-ring dating, which

will provide more precise dates than C14 is ever likely to achieve. So far, however, the continuous Greek/ Anatolian sequence extends back only to AD 1073, to which earlier floating sequences have yet to be joined.

3. The historical chronologies of Egypt and Babylonia are sound back to *c.* 705 and 747 BC respectively (and that of Greece in broad terms to *c.* 700 BC). Assyrian chronology, based on the eponym list of annual officials and a solar eclipse reference of 763 BC, can be authenticated, within a year or two, back to 911 BC. Old Testament chronology can be cross-checked by the Assyrian back to 853 BC and is agreed to be generally reliable as far back as the United Monarchy of the mid-tenth century. In Mesopotamia before 911 BC there are few, if any, independent controls available on the king-list based chronologies. We have pointed the way to a new approach to the Assyrian King List (which forms the backbone of Mesopotamian chronology) by proposing that parallel lines of rulers may have been masked in the List and shown as successive. Yuhong and Dalley (1990) have now argued this convincingly for the earliest portion of the Assyrian King List.
4. The only way forward is to begin by rebuilding historical chronology from the known to the unknown. In Egypt this means working backwards through the Third Intermediate Period from the 25th Dynasty which was certainly in power in Egypt by *c.* 705 BC. Preliminary work undertaken for Mesopotamia needs to be further developed.
5. We have worked back through the pottery sequences from all the areas affected. For example, combining guesstimates already offered (on the basis of local evidence) for the duration of the various ceramic phases of Dark Age Greece can produce a considerably lower date for the beginning of the Iron Age than that allowed by the conventional Egyptian chronology. Comparison of such local sequences enabled a consistent pattern of relative archaeological chronologies to be drawn.
6. Fixed points for the framework can be found by careful collation of archaeological materials with written records datable by sound historical chronologies. For example, from the tenth century onwards the Old Testament narrative can be used as a control on the archaeological dating of Palestine (see Table 2). The 'eleventh-tenth' century pottery found in the palace built by Kings Omri and Ahab at Samaria around 880

BC should, as Kenyon insisted, be dated to the early ninth century and not attributed to an elusive earlier settlement. For West Semitic palaeography, the fixed Assyrian date of 866 BC for the father of the official who dedicated the Tell Fakhariyah bilingual inscription is to be preferred to the eleventh century date imposed by conventional Egyptian dating. A downward shift of this order for the Levantine alphabet would make the Shipitbaal known from an 'early ninth' century Byblite text contemporary with an Assyrian reference to a king of this name at Byblos *c.* 740 BC.

Implications

Our main conclusion is that a vast array of evidence from stratigraphy, typology, art history, genealogies and local historical data shows that the Sothic-based chronology for New Kingdom Egypt (and hence for the Late Bronze Age of the Aegean and Near East) is too high. The evidence from each area (Italy, Sardinia, Sicily, the Balkans, Greece, Anatolia, Cyprus, Syria, Palestine, Nubia, Mesopotamia, Iran and the Arabian Gulf) suggests with an almost remarkable consistency that the error involves up to 250 years. Moreover, internal evidence from Egypt agrees with this pattern and allows a telescoping of the Third Intermediate Period by the same amount of time (Table 3). The dates for 22nd and 23rd Dynasty objects found outside Egypt now become consistent with the chronological pattern provided by their local contexts.

The implications of our proposed revision are, of course, profound, and we can give here no more than a brief overview of the new picture which emerges. In the Western Mediterranean the myth of early Phoenician colonization can be abandoned, the Sub-Apennine and Proto-Villanovan cultures of LBA Italy can be overlapped so that settlements and burials go together, and the early remains from Rome, presently dated to the tenth century, would come down to the eighth century, where Roman traditions placed the foundation of their city.

In the Aegean, the final expiry of Mycenaean civilization is set in the early ninth century, with the rise of Greek city states in the eighth century seen as a renaissance after a much shortened period of recession. We can now begin to understand the underlying continuity in ceramics, ivory-working, weaponry, bronze working, architecture and religion. The 'Homeric problem', the strange mingling of elements from the LBA and Homer's own time, becomes far less baffling. The lapse of Greece and Cyprus into illiteracy

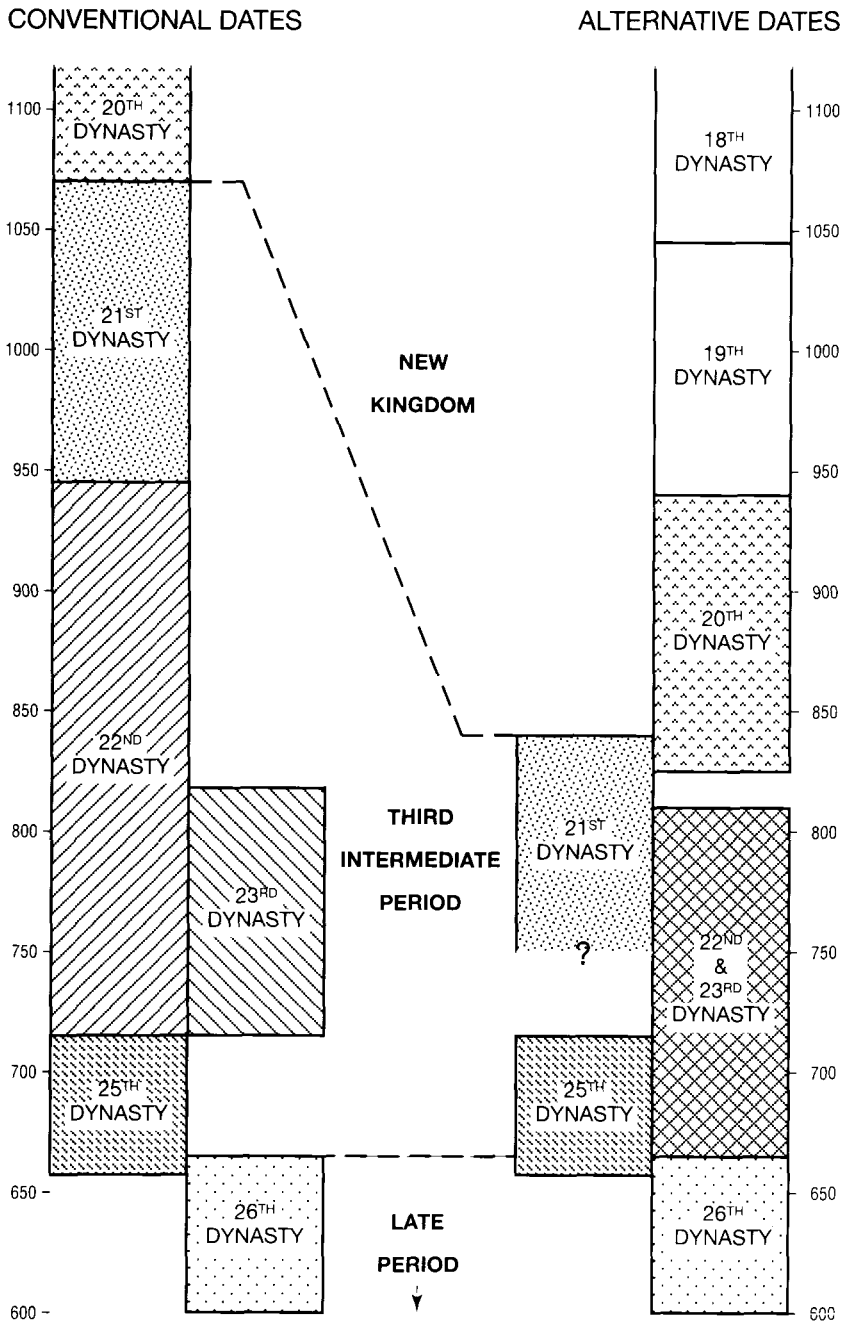


Table 3. A comparison of the conventional dates for the Third Intermediate Period with the provisional alternative scheme. A substantial compression in the overall chronology can be achieved by allowing greater overlaps between the Third Intermediate Period dynasties (particularly the 21st and 22nd), and by strict adherence to the reign-lengths actually given by the contemporary Egyptian monuments, rather than those provided by Manetho. The result is a substantial lowering in time of the New Kingdom and with it the Late Bronze Age of the Eastern Mediterranean (Table 2). The 24th Dynasty, which lasted thirteen years at most, is not shown here; on the conventional dating, it ruled in the western delta concurrently with the last years of the 22nd and 23rd dynasties, and is of little chronological significance in either scheme.

during the Dark Ages is almost eliminated, along with the problem of the adoption by the Greeks of an alphabet which was already 300 years old by the eighth century. Cypriot and Levantine chronologies can be harmonized and the long-standing Black-on-Red Ware problem, which prevents any meaningful discussion of cultural relationships between the two, dissolves.

In Anatolia, the centuries-long abandonment of Troy can be largely closed. Similarly, in our model the

Hittite Empire gradually disintegrated in the mid-tenth century to be replaced by smaller kingdoms, with no break in continuity at the key Neo-Hittite sites of Carchemish and Malatya. The Central Anatolian Dark Age before the arrival of the Phrygians can be drastically shortened.

The riches, building traditions and temple furniture of Solomon's golden age as recorded in the Bible can be matched with a series of rich sites and individual finds from the end of the LBA. Those

Psalms traditionally composed under King David, which closely resemble LBA hymns from Ugarit and Egypt, can now be seen as belonging to a contemporary poetic tradition. In Mesopotamia, Elam and the Arabian Gulf the Dark Ages in archaeology, art and literacy can be considerably reduced by allowing for more than one dynasty ruling in parallel, in both Assyria and Babylonia.

Notes

1. To avoid complicating this brief summary, we have given here only a few key references, together with some which appeared too late for inclusion in *Centuries of Darkness*, where the reader will find fuller documentation.
2. For convenience, we used Kitchen's date of 716 BC as a base point for the notional Egyptian chronology developed in our thesis. The actual date of Shabaqo's accession may be as late as 708/707 BC.

Egyptian Chronology: Problem or Solution?

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Modern study of the chronology of the preclassical ancient Near East was first made possible by that series of discoveries which has opened up to us the history and cultures of that richly complex world - the decipherment of ancient Egyptian; then of the languages inscribed in cuneiform; then the establishment of basic principles in 'reading' the non-literary record: archaeological stratigraphy, typology and the rest (joined nowadays by aids from the natural sciences). Because of the often fragmentary state both of the original data and of our interpretation of that data, recovering a reasonably accurate chronology has been a very cumbrous affair and remains so.

In terms of cultures known from the first written records, the general position today is that we have (in Egypt) up to 200 years' margin of error at most at the start of the 1st Dynasty (3100/2900 BC), up to 30 years' margin of error in the early second millennium (12th Dynasty ending in 1786 or 1759 BC), within 10/20 years in the later second millennium BC (18th Dynasty beginning within extreme limits of 1550/1530 BC; kings Thutmosis III, Ramesses II, acceding in 1490/79 and

1290/79 BC respectively), and so into the first millennium to zero error by 664 BC (26th Dynasty). And similar if different sums apply in Mesopotamia, where zero error (at least in Assyria) obtains from 911 BC. Other regions and cultures fit within this framework: be it Hittites, Levantine peoples, Elam or Iran.

From the 1950s a rank amateur, Immanuel Velikovsky, thought that he could do better than the toiling scholars, and proposed to remove 500 years from ancient Near Eastern history. By this means, an identity could be found for the elusive Queen of Sheba who visited Solomon in the tenth century BC - drop the fifteenth-century Egyptian queen Hatshepsut by half a millennium and the problem was 'solved'. But this led to a vast multitude of sheer impossibilities. Ramesses II (thirteenth century BC) had to become Necho II (600 BC). Yet Shoshenq III (c. 800 BC) would then have cut up a colossus of Ramesses II (still 200 years into the future!) to build his gateway at Tanis. Velikovsky's schemes led to such absurdities wholesale. As he had no personal control of the language and other skills for such study, 95% of the data were beyond his reach or understanding.

The authors of *Centuries of Darkness* still share some of Velikovsky's errors in presuppositions and approach, while recognizing that he went much too far. They would drop the dates of the ancient world by only 250 years - a quarter-millennium instead of half.

In Egypt, this is to be achieved by making the 21st and 25th Dynasties almost entirely contemporary with the sequence of 20th-22nd/23rd-26th Dynasties, as the summary chart (James *et al.* 1991, 258; reproduced here as Table 3) makes admirably clear. However, the reasons given for reducing Egypt's timespan in this period, and by these methods, are wholly fallacious. First, they claim that the period of the 21st to 25th Dynasties (the 'Third Intermediate Period') has been determined in length by the prior fixing of the earlier 18th-20th Dynasties (using Manetho and astronomic datings by the star Sothis): 'It is true to say that the monumental and inscriptional evidence . . . has been arranged within an *already predetermined time-span*' [italics ours] (pp.231-32). This assertion is, quite simply, not true. The basic method used (as for example by this writer: Kitchen 1986) was to collect all the items of evidence, scrutinize each one in its own right, then fit together the smaller, tight groups of data that belonged closely together, and finally observe the clear links that bound the groups into larger wholes. By this means, clear successions of kings, officials, etc., emerged without relying on any alien evidence. The one assumption that was made in terms of dates was

that 664 BC for the beginning of the 26th Dynasty was a firm base-date - an assumption made by James and his colleagues also. The monuments supplied a dead-reckoning back in time, which in turn could be refined by correlations with Assyrian and Hebrew chronology *and not the other way around* as James and his colleagues wrongly allege. When they state flatly (p.222) that 'The length of the T[hird] I[ntermediate] P[eriod] has not been determined by careful reconstruction working back from the firm dates of 26th-Dynasty Egypt' but that 'a length of time has first been created, and the TIP used to fill it', they are in fact stating the exact opposite of the truth. The date 1070 BC was not the antecedent date for the end of the 20th Dynasty; it was 1085 BC, and some today (e.g. von Beckerath) still hanker for dates higher than 1070 BC.

On this basis, the absolutely exact correlation between Shoshenq I of the 22nd Dynasty and the Shishak (better with *kethiv*, Shushaq) who troubled Solomon's successor Rehoboam fits perfectly. Hebrew Shusha(n)q is phonetically a close equivalent of the Egyptian name, recorded also in cuneiform as Shushinqu. Just as the Hebrew form omits the *n*, so that *n* is also often omitted in Egyptian examples of the name (for examples, see Kitchen 1986, 73, n.356). By contrast, the Sessi advocated by James and his colleagues (pp.257, 385, n.135) has not one single consonant in common with Shishak - *s* and *sh* must not be confused in these languages (especially in the first millennium BC), and there is no *q* in Sessi. Moreover, Sessi is a nickname applied exclusively (it would seem) to Ramesses II - not to Ramesses III (the new 'Shishak') as these writers would like to assume. For purely linguistic, not chronological, reasons, the equation of Shishak (Shushaq) and Shoshenq is, frankly, unassailable. Their other objections to this key synchronism are frivolous and exaggerated. The Shoshenq list of Palestinian names is not complete, nor was Jerusalem stormed; hence, it may be lost in a lacuna, but never need have been mentioned at all. Shoshenq had to reduce to obedience *both* Hebrew kingdoms; the Judean chroniclers (naturally) concentrate on Jerusalem's losses, whereas more places from the northern kingdom appear in the list, as more places were taken or passed through there. Wallenfel's subjective arguments on the supposed development of the Northwest Semitic script on the Byblos monuments are without merit - he has received no endorsement from the real experts in this field; it is precise historical dating that determines the profile of epigraphic change (its speed or slowness), not *vice versa*.

These authors have a neurosis about supposed

'gaps' in history. They seem not to understand that a 'gap' in our modern knowledge does not always signify such a gap in antiquity - our knowledge today of the past is full of lacunae - either because the data have not yet been exhumed from the Near East's numberless mounds, or because they have been irretrievably destroyed (like most Egyptian papyri). They make no allowance whatsoever for changes in ancient tastes. Thus, they point to the lack of Apis-bull burials for the entire 21st and early 22nd Dynasties as a 'gap' (p.238). In our knowledge, yes; in history, almost certainly not. They admit to the existence of the Apis-embalming-table of Shoshenq I - which would have been used for embalming at least one Apis in his reign, a burial they are forced to admit has never been found. So, why not others? We do not know what the burial-customs for Apis were between Ramesses XI and Osorkon II; they might have been altogether simpler, and merely not yet discovered. Who - before they were found - could ever have guessed at the incredible runs of Mother-of-Apis and other galleries unearthed at Saqqara in recent decades? These authors simply do not appear to understand how uneven our knowledge is. Until the Ebla archive was found in Syria, written history there in the third millennium BC was a total 'gap' - likewise in Syria in the second millennium BC before Mari and Ugarit were found. We cannot forbid the future to provide further data, sometimes in unexpected quantity.

Apparent lack of architecture in the 21st-22nd Dynasties is alleged as another 'gap'; but not every age built like Ramesses II - they could not afford to, or did not care to. There were new buildings, both at Memphis (Siamun built there), and on a grandiose scale at Tanis: Psusennes I (the main temple), Siamun (a pillared court), Osorkon II (another court and other pillared structures), Shoshenq III (a great gateway), Shoshenq V (a jubilee-chapel), and so on. Most of these are not standing now - but they were, then. The same is true of the once noble structures of Osorkon I and II at Bubastis (now just rubble), and the great court and pylon of Shoshenq I at Memphis (now known solely from the king's Theban texts, and Herodotus). So, the gap is a baseless illusion, as are other allegations of this kind.

The arguments about genealogies (pp.238 ff.) are no sounder. These authors ignore the hard fact that we have an *unbroken series* of High Priests of Amun in Thebes from the 20th Dynasty right through, continuously, to the late 22nd/23rd Dynasty. Piankh is securely dated to Ramesses XI, and his successors go from father to son right through to Psusennes II/III at the end of the 21st Dynasty, with neither gaps nor

rivals. The new king Shoshenq I then installed his own son, Iuput, and we can, again, follow through an entire series almost to the end of the Libyan period. Until then, there are no gaps, and only one serious rivalry - between Prince Osorkon son of the northern king Takeloth II (not Theban, as recently wrongly alleged), and Theban pretenders. At the end of the Libyan period, the authors claim to be astonished that an absence of High Priests of Amun for 30 to 50 years should occur (p.246). Here, they have erred thrice over. First, they have failed to notice the discovery of a 'new' High Priest, Osorkon F, c. 754-734 BC (Kitchen 1986, 564-5, 594 end). Second, they omit to notice that no High Priest of Amun was on hand to greet King Piye (Piankhi) in Thebes in about 728 BC - probably because there was none. And in the following 25th-26th Dynasties, the High-Priesthood of Amun has become all but totally invisible, its power gone forever. There is not the slightest possibility of making the semi-royal High Priests of Amun of the 21st Dynasty the contemporaries-in-office of the Libyan series of the 22nd/23rd Dynasty, nor of any of the latter battling it out with the God's Wives of Amun in the 25th Dynasty. The gaps in other lines of offices are also (despite these authors' claims: pp.245-7) very limited. Given that we are so dependent on coffins and suchlike funerary effects and occasional statuary (per family), what is amazing is how much is known, not the contrary.

Much is made of the fact that the tomb of Psusennes I (Akheperre) appears to have been built later than the tomb of the later king Osorkon II, which suggests to our authors that their historical order should be reversed (pp.243-5, 256, n.133). However, as Dodson (1988) pointed out, there is not the slightest guarantee that Osorkon II was the first owner of the tomb in which his burial was made. It may well have originally been that of either Smendes I or Amenemnisu, before Psusennes I. The architectural sequence has no bearing whatever on the history of these dynasties, as it is a known fact that various burials were moved around in this group of tombs. Amenemope, for example, was installed in Psusennes I's wife's chamber, eliminating every trace of her effects. Of the sequence of Akheperre as Psusennes I and Tyetkheperure as (Har)-Psusennes II, there can be no real doubt. The former was clearly a precursor of Amenemope whose burial replaced that of Psusennes I's queen, while the latter's place is clearly fixed by the epigraphic links between the Theban High Priests and Amenemope and his successors down to Tyetkheperure Har-Psusennes, with the latter's daughter Maatkare marrying the son of Shoshenq I, founder of the 22nd Dynasty.

The dislike of James and his colleagues for the work of Manetho and for the use of Sothic astronomical dating is very clear. Far from being any kind of 'shackle', the dynastic divisions of Manetho have time and again turned out to correspond to reality. His lists are similar in nature to other Egyptian lists (e.g. the Turin Canon of kings); they suffer from having been in part badly miscopied across several centuries. Exactly like other lists, his data have to be patiently compared with first-hand evidence, and evaluated accordingly - not dismissed out-of-hand. Neither should the Sothic or Sirius mode of dating be abused. It is *not* the basis for Egyptian chronology for the New Kingdom or Late Period. But it is a tool in trying to refine dates obtained by dead-reckoning of regnal years, in conjunction with other aids such as synchronisms with Western Asia; and it is open to varying estimates, according to where the Sothic observations might have been made (Memphis? Thebes? Elephantine?). Contrary to the authors (pp.227-8), there is not the slightest reason to assume any calendrical adjustment in Egypt before the Ptolemies. It was precisely the new Hellenistic regime that tried to change things - and ultimately failed. Evidence is what is required here. Lunar dates are too limited within their 19-year cycles to be of any use for wider dating.

The continuity of art-forms (pp.234ff.) is of no evidential value whatever for absolute dating. One has only to recall the continuity of art from the Old Kingdom into the early Middle Kingdom, making it difficult to distinguish 4th Dynasty from 12th Dynasty blocks (as at Lisht), or from the Middle Kingdom into the early 18th Dynasty, where it is impossible to lop centuries of kings from the historical record. Art in the Ramesside tradition is not Ramesside, but evolves in that tradition with its own variations in detail. And in the 21st Dynasty, new things happen, such as the start of the series of fine gold-inlaid bronze statuettes that reached their peak in the 22nd Dynasty and then declined (Fig. 1). The 'new chronology' would make a nonsense of this sequence.

Time and again, presentation of data is deliberately 'slanted'. In treating of Libyan-period kings, some are unjustly dismissed as 'ciphers' (p.232). The length of Osorkon I's reign is suggested not only by otherwise unattributable bandage-epigraphs of Year 33 (p.380, n.36), but also from the number of successive officials that belong to this reign, and from this king's use of a jubilee-formula used only of kings who celebrate two *sed-heb* jubilees (Years 30, 33). Datelines assigned to Takeloth I are not arbitrary, but come inside a definite sequence that leads to this result. Much is made of ninth/eighth century Egyptian vases



Figure 1. *Queen Karomama, wife of King Takeloth II, 22nd Dynasty. Bronze, originally inlaid with gold, silver and electrum (from Capart 1931)*

with royal names turning up in Spanish or Mesopotamian contexts of eighth/seventh century date (pp.252-3); in antiquity, there was no air-freight service, and the leisurely spread of such artifacts is of no chronological value, especially when some are actually local imitations of Egyptian originals that must have arrived earlier. And again, Bennett's date for the Edomite citadel at Buseirah in the eighth century BC is cited, and then Millward's wonder that a tenth-century Egyptian faience chalice should arrive there, then or later (p.251). So, a 200-year discrepancy, QED! However, our authors omit the fact that such faience-work was made right down to the eighth century BC itself (Bocchoris vase; Kurru vessels), as Millward herself observed. Indeed, the Buseirah example's degraded hieroglyphs suggest a later-than-tenth-century date; hence no gap need have existed at all.

The consequences that follow, if the 'new chronology' were to be embraced, are incredible. We are seriously expected to believe that Merenptah was Solomon's father-in-law, giving him a daughter to wife - yet, on the 'Israel stela', Merenptah boasts of

victory over *foes*, including 'Israel, its seed is not', that is, he has destroyed Israel's offspring (or crops) in battle; a strange father-in-law! The idea that Ramesses III could be Shishak is contradicted by our data on Ramesses III. There is no evidence that he invaded Palestine in Year 12 (a rhetorical text of that date by itself proves nothing). The Medinet Habu Syrian war-reliefs are most likely merely copies from those of Ramesses II, as they include entities no longer extant for Ramesses III to battle against. Ramesses III attacked not Israel, but Edom in south Transjordan, as the factual descriptions in Papyrus Harris I make clear. This has nothing to do with Rehoboam's time.

But the most astonishing result is one not touched on by these authors. On their dates, King David would have carved out his empire in Syria from the Euphrates to SW Palestine right in the middle of the reigns of Ramesses II of Egypt and the Hittite king Hattusil III, after their peace-treaty ending two decades of war over who should have how much of Syria. Is it even remotely conceivable that these two formidable rulers should just sit idly by, cowering with armies in

mothballs, while some upstart prince from Jerusalem's hills calmly carved out three-quarters of their hotly-disputed territories (and revenues) for himself? This is sheer fantasy, given the known nature of ancient attitudes and politics. Moreover, it leads to all sorts of other errors; Hamath on the Orontes had no role whatever in Ramesses II's time, but Qadesh and Tunip did, contrary to David's time, when Hamath and Damascus were important and Qadesh and Tunip in total eclipse. In short, this volume is simply a sustained piece of special pleading revealing only a shallow knowledge of the mass of relevant data involved. Egyptian chronology in the late second and early first millennium BC is by no means 'ramshackle' (p.xv) nor is a 250-year chronological 'revolution' even remotely possible.

In their summary above, the authors merely persist with the same errors in fact and method. Despite their denials (and appeal to the palaeographic guesswork of Wallenstein), Hebrew Shisha(n)q/Shusha(n)q remains absolutely Shoshe(n)q, the *n* often being omitted in Egyptian. Otherwise we may as well deny that Hebrew *Tirhakah* is Egyptian Taharqa (requiring assumption of metathesis), or that Egyptian *Ptawmysis* is Ptolemaios (Ptolemy), *Qliwpr(t)* is Cleopatra, *Ntrywš* is Darius, or *Ilksndrs* is Alexandros (Alexander). We either accept straight and strict philological facts, or give up all equations entirely; and that is the end of the matter. Again, the persistent attempts to evade the evidence for Osorkon IV simply will not wash. His prenomen, Akheperre Setepenamun, is wholly different from those of Osorkon I (Sekhemkheperre Setepenre) and of Osorkon II and III (Usimare Setepenamun). Worse still, all four Osorkons have different mothers: Osorkon I was son of Karamat, Osorkon II was son of Kapes, Osorkon III was son of Kamama; Osorkon son of the God's Mother Tadibast can be none of these, and hardly other than Osorkon IV Akheperre. Whether this king be known from two or two hundred monuments is entirely beside the point; an almost powerless dynast in the marshy Delta does not leave vast monuments of imperial scale, nor have Delta temples ever survived like Upper-Egyptian ones. Evidence must be weighed by quality, not just quantity. The prenomen Akheperre links Osorkon IV with his predecessor Shoshenq V, whose prenomen he simply imitated, in the tiresomely unoriginal habit of that epoch. That Piye (Piankhy) should find an Osorkon in Tanis/Bubastis in c.728 BC, Hoshea send to (O)so(rkon) - abbreviated forms are known - in 725 BC, and the Assyrians know a Shilkanni in 716 BC is too much of a coincidence to be glossed over. By contrast, there is not the remotest possibility of Osorkon III

being any of these. First, he did *not* reign in the fief of Bubastis/Tanis as did Shoshenq V and Osorkon IV. Second, he was followed by Takeloth III, Rudamun, and at least 21 years of Iuput II *before* Piye's invasion of Egypt in c. 728 BC.

A huge amount more might be said to expose the shallowness of the authors' case. Typical is their allegation that the Tell el-Fakhariyah statue's bilingual text (Assyrian and Aramaic) sets Egyptian and Assyrian chronology in contradiction. This is a nonsense, insofar as the monument concerned has itself no fixed date! That of c. 860 BC is highly probable, but not certain (as its editors' caution makes very clear) - and other estimates of its date range from the tenth to the eighth centuries BC. There is no basis for chronology here, and it is totally irrelevant to Egypt and its chronology.

The attempt to discount the link between Midianite (better, Qurayyah) pottery in North West Arabia and Ramesside Egyptian works at Timna in the thirteenth/twelfth centuries BC in favour of an eighth-century date, with totally uncritical reliance on Muhly, is another misconception. Muhly's arguments were rigorously disproved and the unsatisfactory nature of findings at Tayma made very clear by Parr (1988; 1989) in two fundamental studies apparently unknown to James and his colleagues. And so the tale of accumulated error might run and run and run . . . but must here stop.

Explaining Ancient Crises

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Although *Centuries of Darkness* is ostensibly about chronology, and relentlessly so, it has an important subtext: that civilization, once started, ran progressively and also at an even pace. When it appears to be interrupted, or when 'periods' (usually synonymous with styles of pottery decoration) seem to deviate from what we should expect to be a normal span of time, then we should start to think that we are being misled, either by ambiguous evidence or by scholars unwilling to think boldly. Cultural development is, in the eyes of the authors, essentially predictable. *Centuries of Darkness* thus throws out two challenges: how can we justify the established chronology, and also, supposing that Dark Ages should remain, how should we fit them within a general explanatory model of the

dynamics of whatever it is that we characterize as civilization?

One area that the book deals with, Nubia, does, when looked at over a longer timespan, illustrate well the unpredictability of cultural change, its erratic timescale, and the huge impact of crises on relatively small populations. Consider first the case of Lower Nubia, a marginal area in all senses, caught between Egypt to the north and Upper Nubia (the northern Sudan) to the south. For a while, the Neolithic cultures in Lower Nubia and Egypt seem to have developed in parallel, producing in Lower Nubia the A-Group culture. The emergence of the early Egyptian state, however, brought about a marked imbalance in organization and resources between the two countries, the Egyptians invaded, and by the middle of the 1st Dynasty A-Group culture had disappeared. There then follows a hiatus of (on conventional chronology) some four centuries at least in the archaeological record of indigenous culture, ended by the appearance of C-Group culture. Adams (1977, 143) has written: 'Notwithstanding their minor divergences the graves and the pottery of the A and C Horizons are remarkably like each other; so much so that a close cultural connection between them can hardly be questioned. Given the fact that there may be a lapse of several centuries between our latest known remains of the A Horizon and our earliest remains of the C Horizon, the wonder is not that they are different but that they are so little different.' This is just the kind of quotation which the authors of *Centuries of Darkness* seize upon in other contexts to justify their disbelief that a cultural tradition can come and go with long gaps in between. Should we be suspicious of this particular Nubian 'Dark Age'? The answer has to be no, because in Egypt this was the great age of pyramid building, and whilst there could be scope in the extremely limited historical documentation to overlap dynasties, it is not feasible to overlap the colossal mobilization of resources that lay behind pyramid building. The hiatus between A-Group and C-Group seems unavoidable, and one is obliged to seek a cultural explanation, such as a reversion to a semi-nomadic way of life. Once established, C-Group culture persisted for no less than seven or eight centuries, showing some but not very marked stylistic changes. For part of this time (the Egyptian Middle Kingdom) its communities shared the Nile Valley with Egyptians based in huge fortified towns, yet seem to have remained remarkably free of Egyptian influence, almost as if the two groups belonged to different times. Yet they cannot have done.

With the following period, the New Kingdom,

we enter the subject of one of the chapters of *Centuries of Darkness*. The Egyptians have taken over Nubia yet again, and, through a series of temple towns, have converted it into an extension of the Egyptian state, in the process of which Nubians evidently became thoroughly acculturated. But then, once again, the historical and archaeological records begin to diverge. We know from textual sources that the system of peasant farming which the Egyptians must have introduced was in operation towards the end of the period, yet the number of datable tombs had sunk to the negligible well before. The archaeological Dark Age of Lower Nubia was already beginning, before the formal end of the New Kingdom. In terms of evidence pointing to regular settlement it was (with the likely exception of Qasr Ibrim) to remain like this until Roman times, thus for virtually one thousand years. Why this should have been so cannot easily be explained. The New Kingdom ended with a Nubian army under the viceroy moving north into Egypt and fighting as far north as Middle Egypt, and subsequently we hear of an Egyptian army on a campaign into Nubia. But the scale and reasons for these events escape us.

The authors of *Centuries of Darkness* raise an important issue at this point. 'To dismantle a 500-year-old bureaucracy is no overnight matter: records of land tenure, endowments, taxation assessments, viceregal correspondence . . . all have to be disposed of, or transferred elsewhere. Were the temples closed down? If so, what happened to their administrative records, furniture and fittings, libraries, and, most significantly, cult images?' (p.207-8) These are apt questions, but for Lower Nubia the authors' solution is irrelevant, for their compression of chronology makes no difference. The temple towns and the rock temples of Ramesses II remained, by and large, deserted. There is no Dark Age followed by a resumption of normal activity. The dismantling of the administration and closure of the temples (including Abu Simbel) must have happened anyway. At three places (Semna, Buhen, and Qasr Ibrim) the Napatan king Taharka built a temple (or, at Buhen, added a little pronaos) but the extensive archaeological investigations in and around the first two have shown that the temples must have been primarily territorial markers served by a tiny community living in a land which was basically devoid of settled life.

The authors are drawn irresistably to the sudden appearance of the Napatan kingdom in Upper Nubia (which became, following the takeover of Egypt, the 25th Egyptian Dynasty). It is at this point in the Nubian section that their revised chronology makes its principal

impact, for instead of 350 years of virtual nothingness in Upper Nubia between this event and the end of the viceregal administration of the New Kingdom, the authors' scheme substitutes a single century. Into this century one can fit the ancestral phase of the Napatan royal cemetery at el-Kurru, and on the limited evidence available their solution is neat. A few burials, however, do not of themselves fill a 'Dark Age'. The absence of settlement sites for the pre-Napatan period, and to some extent also for the Napatan Period (as is illustrated in respect of the large cemetery of Missiminia in the well surveyed Dal Cataract area) raises the possibility that the lives of at least a part of the population in question were not centred on discrete settlements along the banks of the river. And our impression of a full historical picture of Upper Nubia in New Kingdom (viceregal) times may also be an illusion. Although the authors complain that 'all the writing on this period has been Egyptocentric, obsessed by those problems directly related to Egypt, and using only Egyptian-type evidence' (p.206), their approach is no different. They appear to assume that indigenous Nubian life of the New Kingdom was lived out in the Egyptian towns. Yet the New Kingdom historical sources document the existence of Nubian entities (tribes, states, places, it is hard to tell) that the Egyptians found threatening, and against whom an army sometimes had to be dispatched. It is hard to believe that these enemies lived in or beside the Egyptian towns. It is more sensible to consider some of them at least as living either in the desert hinterlands or in areas south of the regions under Egyptian control. But where is their archaeological record? Outside the Egyptian enclaves there is already a 'Dark Age'-like absence of evidence.

The rise of the Napatan kingdom is a clear case of state formation. As soon as one invokes this phrase the familiar range of explanatory models becomes relevant, which should lead to discussions of changes in the economic base, in the degree of social organization, and in contacts with external societies during the formative phase. But the continuing insufficiency of archaeological evidence for indigenous peoples outside the urban network planted by the Egyptians provides no secure basis for discussion. In these circumstances the authors' evidence for chronology and state origins, which consists of a few royal names and a handful of Egyptian or Egyptian-like artefacts for which a terminal date is highly uncertain, is trivial and, for the question of state origins, likely to be very simplistic.

Under the Napatan kings, substantial cultural output resumed both in Egypt and in a few centres in Upper Nubia. It did not, however, simply pick up

from where the New Kingdom had left off. The 25th Dynasty marked the beginning of an engaging interest in the distant past. Reliefs from temples and tombs that were over a millennium and a half old were copied (Fig. 2), and a few scribes tried to learn how to write official documents in the language and orthography of that distant age. The result was an eclectic style of the kind that the authors find so baffling when it surfaces elsewhere in less well documented cultures.

Nubia provides a case history of an area which, for a long time, remained close to the limits of cultural stability, and was, from time to time, pushed across it by either external or natural forces. 'Dark Ages' are an important and regular part of the record that we so far have. It was also the scene of state formation which started from a basis which may not have been one of settled, farming life, and where the new rulers looked outside for a model of the ideal court, and borrowed it from a neighbour (Egypt), adding their own modifications. The point can be made that the lessons of Nubia and the northern Sudan should not be applied too readily to the Aegean and Southern Europe which, until the period in question, had shown a greater degree of continuity in way of life. But we know from independent sources that powerful agents of disturbance were abroad as the Late Bronze Age ended. The migrations of North Africa and the Eastern Mediterranean, and the ensuing period of state formation which gave rise to, amongst others, the Libyan, Philistine, and Israelite kingdoms, point to profound and widespread changes in the way of life of whole peoples, the background to which we as yet do not understand, and also to variety in the ways by which secondary states have come into existence. In these circumstances we are no longer entitled to assume that established patterns would continue along a predictable course.

If we adopt a more detached and behavioural stance we can say, from a broad spectrum of evidence, that human society in general retained the potential to exist in more than one mode: amongst others, the settled, hierarchical, materialistic mode, and its opposite, well studied and defined by anthropologists and exemplified by tribal societies. As this book shows, artefact-oriented archaeology which is based on a simplistic philosophy is easily disconcerted by a change of mode of this kind and tries to explain it away, whereas, if one takes for granted that an alternative mode of life, fragmented and materially unproductive, was potentially always there, waiting to be resumed when the circumstances demanded, the 'Dark Ages' of this book cease to be mysterious. Knowing that

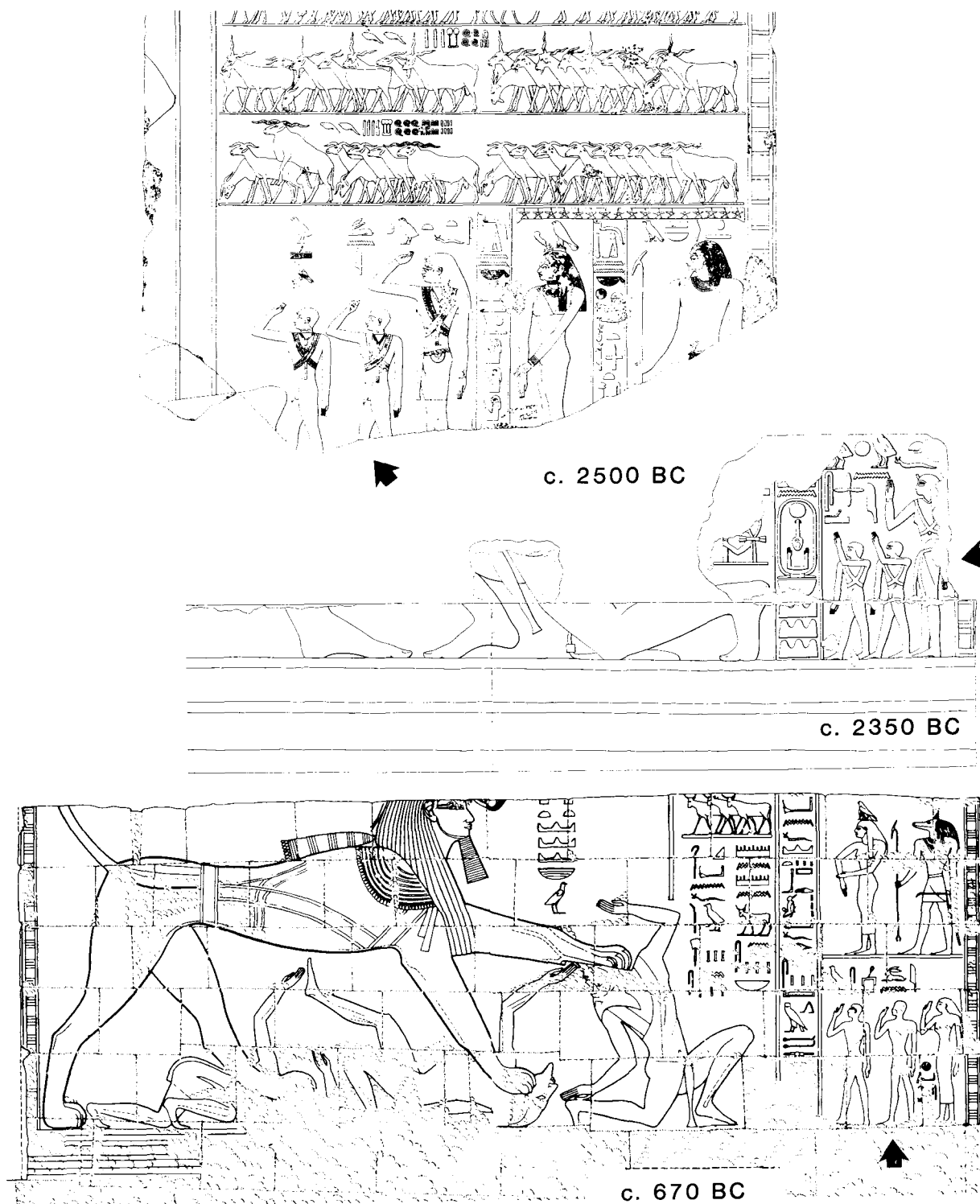


Figure 2. *Jumping the centuries: Three Libyans (two boys and a woman) salute as Pharaoh vanquishes representatives of their tribe. Artists from two widely separated periods and working in two countries (Egypt and Nubia) have used the same source, though without copying it exactly. They have, however, retained the same personal names of the three Libyans. Top: pyramid temple of king Sahura (after Borchart 1913, Blatt 1); middle: pyramid temple of King Pepi II, Saqqara (after Jéquier 1938, pl. 9); bottom: temple of King Taharqa, Kawa (after Macadam 1955, pl. IX).*

something of great magnitude was taking place but being unable properly to account for it leaves us poorly equipped to pass judgements on the meaning and implications of particular archaeological situations, including those which pertain to the 'Dark Ages'. The authors' view that the progress of civilization, once started, was inevitable and its inner structures self-correcting leads them all too easily to a kind of fundamentalist belief in the superiority of common-sense knowledge. When presented with a description (by Snodgrass) of a reverted life-style in post-palatial Greece (p.88) they 'find it difficult to see why it took so long for the economy to recover'. But it is idle to pretend that we know enough about early economies (or, indeed, any economies) to predict what should happen. One can draw from these periods a very different lesson: that, beyond a certain point of collapse, the mode adapted to reduced circumstances becomes a system which tends to be self-perpetuating, until circumstances change and the processes of state formation may begin all over again.

Perhaps it is the necessarily abbreviated presentation of evidence for such a large area that is responsible for the way in which Bronze Age societies come across as being almost identical with their artefacts - the sherds-as-people syndrome. With their makers reduced to automatons it then becomes strange that styles from an earlier phase can 'jump' a couple of centuries to reappear when the same mode of existence came to prevail once more. Yet we know enough of humanity to make the assumption that the successor societies possessed an intellectual life, and that an important component would have been myths about their own origins, fortified by traditions, part genuine and - often misunderstood by ourselves - part invented. The settings for the revitalized and resettled societies were landscapes which must have been eloquent with the remains of past civilizations, and which, through standing monuments and things dug up from graves and temple repositories, provided the necessary cues and source material. In being told that the process is odd, or even inexplicable, we are encouraged to turn our backs on something which is of great fascination and might actually be well illustrated by the periods in question, namely, the conservation and transmission of knowledge and cultural forms across periods when the means of living and values were different.

Having persuaded themselves that cultural-political explanations for the circum-Mediterranean 'Dark Ages' are unpalatable, the authors find chronology a softer target, and especially the chronology of Egypt. They attack the Egyptian

astronomical data (the Sothic cycles) which seem to provide two fixed points, one in the Middle Kingdom, and one in the early New Kingdom. The first is irrelevant to the periods in question, whilst the uncertainties surrounding the latter have been well aired. New Kingdom chronology is based on a complex web of arguments, including, it has to be admitted, reconstruction of the chronology of the Third Intermediate Period (discussed above by Kitchen). During the latter, the internal political situation was indeed sufficiently complex to make it seem not out of the question that alternative reconstructions are historically feasible. It is easy to imagine that, on the documentary evidence, arguments over which is the best reconstruction could run indefinitely. Their book does, therefore, have a justifiable point to make. Those who work in areas with a chronology derived from ancient written sources have been slow to take advantage of the independent means of checking their work: radiocarbon dating and dendrochronology.

Here the authors become ambiguous, and not without reason. They quote the set of five radiocarbon dates from a single deeply buried context at the Egyptian site of Amarna. The dates form a reasonably consistent group which points to a real date which is either around that derived from the conventional historical chronology, or a little higher. The authors discount this group, in the first place by reducing the number that they can accept from five to three because one sample was charcoal and another was wood. They then quote two radiocarbon dates from two walls in the tomb of Horemheb, which gave widely different readings, and use this in a way which appears to discredit the radiocarbon method (although they themselves use single radiocarbon dates when it suits their argument). Apart from the fact that the chopped straw which provided the Horemheb samples is itself prone to unreliable results (as they point out on p.323), the divergent dates simply illustrate the repeated warnings from experts that real chronology should be deduced from the careful assessment of multiple dates, and that it is in the nature of the radiocarbon measurement process that variance is bound to be present.

If we had to construct an Egyptian chronology solely on the basis of current radiocarbon dates the results would be very imprecise. But this is not the issue here. We are faced with a choice between two significantly different chronologies. The radiocarbon evidence favours the traditional chronology; it does not favour the new one. There is, too, just sufficient archaeological interrelationship between Egypt and the Aegean to provide a common chronological

platform. Although it would be idle to claim that the growing number of radiocarbon age determinations from Aegean sites is yet sufficient to create an authoritative basis for an independent and closely defined chronology for the Late Minoan and Late Helladic periods, yet again, at the level of the very crude judgement that we are asked to make, the general picture is clear enough (considered in some detail by Manning 1988). This is that conventional chronology is not too wide of the mark, might perhaps be a little low (in view of the Thera evidence), with the Amarna multiple dates providing (from the large amounts of Mycenaean pottery from the site) a consistent placement for Late Helladic IIIA. With the 18th Dynasty within this general bracket the rest of the New Kingdom follows.

The 'Dark Ages' and other cultural and archaeological oddities which the authors of *Centuries of Darkness* point to are really neutral as evidence for chronology. Whether we collapse or expand the chronology of a particular culture we can always make it mean something. There is no real *a priori* case for a major revision. What we have in *Centuries of Darkness* is an alternative history and chronology produced from the traditional sources by the same scholarly processes that have been responsible for the traditional scheme. The authors have professional training in their areas, and know their sources well enough. In highlighting (and perhaps overdramatizing) a variety of other scholarly debates they clearly see themselves as participants in the normal processes by which historical knowledge advances. But with the one independent means of verification - radiocarbon dating - supporting the old, why should we accept the new?

We need more radiocarbon dates, but this book is not altogether about chronology. We also need more imaginative, flexible, and informed models of how societies and cultures behave under changing circumstances.

The Chronology of Assyria - an insurmountable obstacle

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The Assyrian King List presents a major stumbling-block to the attempt by James and his colleagues to compress the established chronology of the ancient

Near East. Indeed, having considered and dismissed the Egyptian evidence, they are obliged to admit that 'At this stage, the one remaining obstacle to constructing a rational chronology for the ancient world would appear to be the monolithic King List recorded by the ancient Assyrians, backbone of the accepted Mesopotamian dating system' (p.290). Their solution to this is to assert that some of the reigns recorded in that list must have been simultaneous. A brief consideration of this idea quickly makes it apparent that the arguments put forward to support it do not hold water, and indeed that the authors' position is internally inconsistent.

The reign-lengths of the Assyrian King List are generally held to give a solid chronology back to the fifteenth century BC, with a margin of error of about ten years. The only ways round this are either to discredit the evidence entirely, or to assume that some of the reigns listed overlapped. The latter is the solution proposed by James and his colleagues. No details of this overlap are suggested: presumably it needs to dispose of some 250 years. The last well-known king of the Middle Assyrian era is Tiglath-pileser I: if his dates are dropped by 250 years his reign runs from 865-827 BC, which is patently absurd. Even with a smaller overlap it is quite unclear how the solution is envisaged. Is the idea that the list alternates the kings of one dynasty with the other? Or is there an entire block to be shifted down in time? Since these details are not spelled out it is unnecessary to refute them, but I will turn to the specific points which are held to promote the idea of parallel dynasties. These are:

1. King Lists present overlapping events as consecutive
2. Dual monarchies or parallel dynasties - it is not clear which is really intended - are well attested elsewhere
3. Ili-hadda is mentioned as ruling jointly with Assur-nirari in a contemporary text
4. Poebel proposed that Assyria 'fragmented into four separate kingdoms under Assur-nadin-apli, Assurnasirpal and Enlil-kudur-usur . . . and Ili-hadda'
5. Genealogical statements are often erroneous.

We may take these in turn:

1. What happens in the early sections of King Lists, where reigns are mythical and scribes may have attempted to harmonize competing dynasties, cannot be used to discredit later sections where reigns are exact (and can often be independently

confirmed). In the case of the Assyrian King List, the evidence of the Eponym List makes an overlap of this kind improbable (see below).

2. This is only relevant if the same can be proved for Assyria; (3) and (4) below do not constitute adequate proof.
3. It is true that Ili-hadda and Assur-nirari are mentioned as contemporary kings, but this is embarrassing for the authors because Ila-hadda does not feature in the King List, and their theory would require such dual reigns to be presented consecutively. This therefore undermines the idea that the King List might give an unduly expanded view of Assyrian chronology.
4. Poebel's theories of four separate Assyrian kingdoms have indeed been ignored by later scholars, because there is not a shred of evidence to support them.
5. There are indeed discrepancies in genealogical information, but there is no reason to suppose these affect the basic year count.

Let us freely admit that there are discrepancies and inconsistencies in the King List, but there is nothing to suggest the drastic measures proposed. On the contrary, the evidence of the canonical list of Assyria's annual eponyms (high officials after whom years were named, like the Roman consuls) makes them highly improbable.

The authors are uncharacteristically (though justifiably) respectful of the Eponym List: 'Assyrian history is firmly datable, with a margin of error no greater than a year, as far back as 911 BC, when the continuous Eponym List began' (p.268). The first half of this statement is true, but the second half conceals a crucial misapprehension. The 'continuous Eponym List' only begins today at 911 BC because a cuneiform tablet is broken at that point, so that some of the names before 911 BC are not known to us. The list of eponyms undoubtedly went much further back in time, and one tablet from Assur (KAV 21+22) in all probability started with the reign of Tukulti-Ninurta I in the late thirteenth century (Fig. 3). Parts of this list which are preserved include the names of kings known from the King List (e.g. Shalmaneser II, Assur-nirari IV and Assur-rabi II, traditionally dated 1030-1019 BC, 1018-1013 BC and 1012-992 BC respectively) and give the number of years (i.e. eponyms' names) known for each reign, which coincide with the data of the King List. Where there are breaks, the number of lines lost (which can be pretty accurately estimated since the later columns of the tablet cover the later centuries where the numbers of



Figure 3. Part of the Eponym List tablet KAV 21 (columns iii-v, including 'traditional' years 1086-1074, 1033-1005 and 966-963 BC). (From Schroeder 1920)

years are quite certain) also agree with the data of the King List. Therefore even if we can telescope the King List (which I completely disbelieve), we must also find some way of discounting the evidence of the first part of the Eponym List. If one accepts the later sections of the list (after 911 BC) as reliable, as the authors do, the only grounds for discounting the earlier half are that it does not agree with one's own theories.

Thus the Assyrian evidence remains an insurmountable obstacle composed of precise data, one which cannot be removed by vague assertions such as 'A strong whiff of unreality pervades the accepted scheme of Mesopotamian history during the Dark Age. A shorter time-scale . . . would seem to be the only solution capable of restoring a realistic focus on the evidence' (p.290).

There is indeed no need for the shorter time scale which James and his colleagues advocate. Periods

when political authority is weakened coincide with a reduction in written documentation and in archaeological remains. This is not surprising, since the bulk of the historical information available to us is the propaganda of political elites, or the product of stable commercial and administrative organizations. Similarly, the archaeological remains will naturally be less conspicuous in times of political weakness since major public buildings will not have been constructed. Arguments from silence are notoriously dangerous. Somewhere in the soil of Iraq there is no doubt a complete Eponym List. We have recently learnt, through the chance discovery of two stelae in Turkey, that Assyrian political influence was sustained west of the Euphrates at a time when we have no royal inscriptions and scholars had been in the habit of writing about a period of Assyrian 'weakness' or 'collapse'. Similarly, the annals of Assur-dan and Adad-nirari (934-891 BC) were unknown before the First World War, and this silence would have increased still further the period of documentary darkness which the authors feel uneasy about.

This uneasiness is not unreasonable. We still do not understand what was going on during these periods of recession, or how literary and cultural continuity was maintained. It is a problem well worth studying, but it will not be resolved by trying to abolish it.

The Aegean Angle

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The conclusions reached by James and his colleagues, right or wrong, apply to the later Bronze Age and the earlier Iron Age and, above all, to the relationship between the two (I speak in Mediterranean terms, although I am aware that in regions further north we are talking about the earlier and later Bronze Age). Counter-arguments must therefore be based on a comparatively broad spectrum: the book cannot be answered in terms of just one period, however unsatisfactory its conclusions appear to be in that closer context. What we should do is to go to the heart of the question, the astronomical fixes for Middle and New Kingdom Egypt which have been assumed in every current or recent construction of the historical chronology. We should ask ourselves, as the authors have done, whether these constructions would still stand up, from their own internal strength, if these supposedly fixed points were loosened.

Their own answer is of course 'No'. Every chronological sequence, however apparently remote in time or in space, has been constructed out of *less* reliable data, in the knowledge that the results must ultimately be compatible with these more reliable, if not unquestioned, Egyptian fixes. There was thus a degree of built-in dependence, conscious or otherwise: nobody was building structures designed to withstand such a shock as the removal of the fixes.

Thus far, I think that the authors are probably right, though because the dependence was not always a conscious one, many will want to contradict them. The next step is to ask what difference it would make to us if they were right, not only about this, but about the whole alternative construction that they propose. If I were a prehistorian of the Aegean (or other European) Bronze Age, I do not think that I should mind very much. I should be interested - after all, even those who study human origins are interested to know *roughly* what period they are talking about. But I should also feel fairly impartial, because there would not be any very profound impact on the kinds of question that (in the 1990s) I should mainly be engaged with.

For an archaeologist of proto-historical periods, the impact is different. First, there will be a violent change in the relationship between the archaeologically-dated episodes - such as they are - which remain clearly dated from documentary sources. That even the authors of *Centuries of Darkness* accept the retention of some documented dates in this period is shown by the striking case of the kingdom of David and Solomon, which finds itself translated to the Canaanite Late Bronze Age (pp.197-200), since it cannot move forward in time along with its archaeological background. Secondly, the proposed compression of the historical chronology is almost entirely concentrated in the Early Iron Age: in Aegean terms, the fall of Mycenae at about 950 BC is only at about half the distance in time from the onset of Greek colonization that it used to be under the traditional scheme, which would put them at about 1200 BC and 750 BC respectively.

Even so, if challenged to *prove* the authors wrong for this period, without appealing to the authority of the earlier Egyptian dates, I should feel hard put to it. True enough, from beginning to end we have a series of twelve successive and objectively-definable Aegean pottery-styles to accommodate: earlier and later Late Helladic IIIC, Submycenaean, Early, Middle and Late Protogeometric, Early Geometric I and II, Middle Geometric I and II, and (covering the early stages of colonization) Late Geometric I and II. Under the traditional chronology, 500 years are apportioned

between these twelve styles, with intermediate fixes that are few, shaky and indirect. If the authors now tell us (pp.111, 319) that there are only at most 275 years to go round for the whole sequence, how can we *show* that our 50- and 60-year periods are impossible to compress into their 20- and 30-year ones (except possibly in the case of the last two periods where the issue of the working careers of identifiable vase-painters is involved)? We cannot.

There would even be attractions in such a course. To take a case which the authors have raised elsewhere (James *et al.* 1987, 26-7) but which is only fleetingly mentioned in this recent book (pp.85-6): the controversial building on top of the hall of the palace at Tiryns. Was this, as the original excavators and many subsequent authorities argued, a (probably Late Geometric) Temple of Hera? Or was it, as Carl Blegen and the current excavators of Tiryns have maintained, a much earlier (Late Helladic IIIC) and much impoverished re-building of the palace hall which had up to then remained standing? On the conventional chronology, these two datings differ by at least 450 years; on the *Centuries of Darkness* scheme, they are less than half that distance apart, with the faint but intriguing possibility that *both* interpretations could be substantially right.

For all these reasons, I should probably be more favourably disposed than most to the arguments advanced by James and his colleagues, were it not for the completely independent factor of scientific dating - primarily by dendrochronology, but also by radiocarbon. Three features of dendrochronological dating strike me as particularly relevant here:

1. It can lead to direct links with *historical* chronologies, as well as indirectly-based archaeological ones;
2. It can span the whole period of Bronze Age/Iron Age transition, as *Centuries of Darkness* itself does, and in so doing largely by-pass the '1st-millennium radiocarbon disaster' (p.325);
3. It can also leap over Colin Renfrew's radiocarbon 'fault-line' (p.22) in south-eastern Europe: indeed, with the present state of the data it has to do so, since the best available links are between the Anatolian series and the northern European one.

This makes it a minor tragedy that James and his colleagues have not made greater efforts to accommodate the latest scientific data. After all, it is now eighteen years since Clark and Renfrew concluded that the calibrated radiocarbon chronology and the Egyptian historical chronology were broadly compatible and therefore presumably both broadly

correct (Clark & Renfrew 1973). It is sixteen years since I argued that the Aegean 'historical' chronology for the Bronze Age should be abandoned for any purpose of comparative dating within prehistoric Europe, in favour of the Aegean radiocarbon series (Snodgrass 1975); and six years since, with faith in the 'historical' chronology further shaken, I suggested that it might prove less reliable than the radiocarbon series for any purpose (Snodgrass 1985). I choose these three expressions of opinion at random from a much wider range of statements, as representing the varied reaction of archaeologists to a prospect that was looming ever closer through the 1970s and 1980s: the prospect of the direct confrontation between scientific and historical dating methods. I now find that none of the three publications is included in the bibliography of *Centuries of Darkness*. As the authors show by several observations (notably in the Preface, xix-xx), they are not, in 1991, awfully worried about a crisis that was already beginning to keep some of us awake at night in the earlier 1970s. But the crisis is on us. Peter Kuniholm has already announced that his Anatolian tree-ring series can be matched up at one end with timber from a Hittite palace and at the other end with the construction of the Phrygian tomb-chamber at Gordion 654 years later; and that, through calibration wiggles, it can be fitted on to the continuous northern European oak sequence - thus neatly exemplifying points (2) and (3) above.

Against such a background, the arguments of James and his colleagues wear an unreal look. They are a bit like a detailed scheme for re-organizing the East German economy, produced in 1989 or early 1990. But even such a scheme could have considerable value for those planning in today's circumstances, and here too the parallel with *Centuries of Darkness* holds. The book succeeds in showing up what was wrong with the old system, at a time when the old system will have to show considerable flexibility anyway. And that is worth having.

Urnfield Reflections

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Reviewing the volume *Studies in Ancient Chronology* (James *et al.* 1987) which provided the initial critique from which this more radical vision grew, we ended by quoting Bertrand Russell's aphorism on the role of philosophy: 'The net result is to substitute articulate

hesitation for inarticulate certainty.' While the aim of that collection of papers was to cast critical doubt on conventional chronological assumptions, the present book goes much further and argues for a new solution to the ills of the Late Bronze Age and the beginning of the Iron Age. Like Colin Renfrew's *Archaeology and Language* (1987) from the same publishing house (which it much resembles in style and appearance), the critique is more convincing than the proposed answer - which itself creates far more problems than it solves.

Its merit, as with Renfrew's, is to draw attention to the architecture of the problem: in this case, the interlocking and often circular chains of reasoning which link sequences in different areas - constructions which are often houses of cards rather than solid frameworks. Such designs must be thoroughly scrutinized, and real problems in chronology are complex logical structures in which techniques of direct time-measurement are often only a small part; indeed there is a pressing need for the kinds of algorithm that Jim Doran began to explore some two decades ago, to simulate the various kinds of argument which have to be articulated in reaching chronological conclusions from diverse sources (Doran 1970, 60-3; 1977). The centuries around the beginning of the first millennium BC exemplify almost all the difficulties involved in defining archaeological spatio-temporal packages, reconstructing historical situations, and combining the fragmentary record of both into a coherent account. Not unusually the equations have been sought at far too detailed a level: particular destructions, specific invasions, or mythical migrations of refugees. It is the overall design that needs attention, and requires its own models.

It also requires an understanding of the nature of the archaeological record. The amount of material recovered is *always* sparse in relation to the spans of time involved - we should be more surprised at how relatively full it seems at certain periods than at how empty it seems at others. 'We must move from the traditional model of archaeological knowledge as a Gruyère cheese with holes in it to that of a sparse suspension of information particles of varying size, not even randomly distributed in archaeological space and time', as David Clarke pithily put it (1973, 10). The lack of settlements at periods when only graves are known is the normal condition of much of prehistory, and *horror vacui* is a poor principle to employ in creating chronologies. The idea that 'estimates for the duration of the various ceramic phases of Dark Age Greece' might be simply summed to give an accurate estimate for their total duration is quite hair-raising. Nor are the general grounds for compressing the

'Dark Age' which followed the partial collapse of Bronze Age urbanism in the East Mediterranean immediately convincing, given the expectations to be derived from that model: the disappearance of major centres and élite elements such as literacy. Prehistory is a slow process, and only the event-filled years of the (conventionally dated) later second millennium in this region might lead us to want a faster pace of change before the emergence of the Greek city-states. It is, in any case, hardly accurate to talk of an eighth century 'renaissance' of the (palatial) cultures of the Late Bronze Age, when the two are so radically different that the very idea of a Greek Bronze Age civilization was resisted by classical scholars for a whole generation after Schliemann's discoveries.

This is not an attitude of complacency, for there is much that is wrong with conventional archaeological chronologies for this period. Sub-division of pottery styles is often a substitute for thought about what was actually going on. Lewis Binford's pertinent question 'what are our typologies measuring?' is particularly relevant to pots, whose study is further bedevilled by the 'pots = peoples' equation that is rife in accepted accounts of this time - and often built into chronological arguments, such as those concerning material supposedly associated with the Philistines. Generally, however, such problems require intelligent conceptualization and explanation, not chronological compression. It is a feature of the period that a less impressive material record was being generated in the Aegean than in the years of palatial centralization, while in surrounding regions of Urnfield Europe, the steppes in the Sabatinovka period, the Koban phase Caucasus and especially in Luristan, bronze-using economies reached a new scale of output and complexity. Paradoxically, therefore, it is in these regions - and especially in the well worked area of the European Late Bronze Age - that these new chronological assertions can most easily be tested.

It is thus rather ironic that conventional datings of the early Urnfield period have recently received striking confirmation from dendrochronology, and that the latest revisions of those parts of the Hallstatt periodization which lie within the first millennium have made them somewhat older rather than younger. The Central European oak chronology (based on the Hohenheim South German master sequence, tied both to the North German and the Irish standard oak chronologies) now reaches back some seven millennia, and has revolutionized the prehistoric European timescale. It gives direct and unambiguous (and therefore more precise) dates than is possible with dendro-calibrated radiocarbon measurements, and

provides a firm backbone for associated cultural sequences. Neolithic and Early Bronze Age lakeside settlements now have astonishingly accurate measures of age (Becker *et al.* 1985; Bebbler *et al.* 1983; Billamboz *et al.* 1989; Gross 1987); and though such settlements largely disappear in the Middle Bronze Age (and dry-land sites have only been dated by calibrated radiocarbon: e.g. Rageth in Osterwalder & Schwarz 1986, 83-9), rich lakeside habitations resume in the Late Bronze Age. These give the opportunity to provide absolute measures of age for objects, such as bronzes, which are associated with them, and also occur in hoards and in graves - including cemeteries with horizontal stratigraphy.

The phases of the Late Bronze Age Urnfield culture, spanning the period from the fourteenth to the eighth centuries, can thus be defined and dated with some precision. Lothar Sperber's recent monograph *Untersuchungen zur Chronologie der Urnenfelderkultur im nördlichen Alpenvorland* (1987) sets out the current state of the art: 'Die auf traditionellem archäologischen Weg erzielten Zeitansätze, die Dendrochronologie und die ¹⁴C-Daten lassen sich widerspruchsfrei in Einklang bringen' (Sperber 1987, 144). These results set Reinecke Br D from c. 1350 BC to 1225 BC, Ha A down to c. 1100 BC, and the various divisions of Ha B down to c. 750 BC. (We have rounded out the figures, to avoid a spurious impression of accuracy for the characterization of typological constructs: it is the overall span with which we are here concerned. However for a genuine event such as the felling of an oak for the Egtved tree-trunk coffin burial we can be more precise: summer 1370 BC - information from Klavs Randsborg.) These dates give no impression of a gap, and by association-linkage to Mycenaean Greece they confirm estimates (based, remember, ultimately on Egypt) of LH III B as thirteenth century (see Table 4). Not much scope here for a down-dating of two and a half centuries: but every hope that Peter Kuniholm's dendro-sequence for the eastern Mediterranean (continuous from the twenty-third to eighth centuries, and now fixed by 'wobble-matching' in Heidelberg) will provide an equally secure basis for the eastern Mediterranean (Kuniholm & Striker 1987; 1988; Kuniholm & Newton 1989; Kuniholm 1990; 1991; cf. Liphshitz 1988; and see also *Proceedings of the First International Cedar Symposium (Antalya, Turkey, October 1990)*, in press). Meanwhile, we must call on prehistoric Europe to redress the balance of the Mediterranean world.

Both the authors and the writer of the Foreword draw a parallel for their dating reformation with the radiocarbon revolution of prehistoric Europe and the

current controversies over the date of the Thera eruption and its implications for the chronology of the Late Bronze Age Aegean. Neither provides an exact parallel for what is proposed here. The 'links' between temperate Europe and the East Mediterranean that were severed by radiocarbon were vague analogies, not the imports and associations (albeit with their inherent \pm error terms) that bind Urnfield Europe to the Aegean. Nor is the volcanic environment of Thera, with its vents of dead carbon (Olsson 1987, 18-23; Bruns *et al.* 1980), the most propitious testing-ground for radical radiocarbon chronologies. Both suggest that source criticism of archaeological materials (and their radiochemical environments) is a necessary preliminary to reliable revisions. There is, indeed, a new chronological revolution in the offing, created by the span of time opened up in Bronze Age Europe between the stable late second millennium dates for the onset of the Urnfield period and the dendro-dates for Reinecke A1(b) around 2000 BC at Leubingen and Helmsdorf (Becker, Jäger, Kaufmann & Litt 1989; Becker, Krause & Kromer 1989). This is not a re-alignment of correlations so much as a change in scale, by which certain phases are inflated to twice the span conventionally allotted - a salutary warning against taking such intuitive estimates of duration too seriously. These earlier Bronze Age revisions, however, have nothing to do with the problems addressed here; and their effect is, in any case, to expand the time-frame and not compress it. Some inflation and readjustment of the comparable periods of East Mediterranean chronology may still be necessary (even though the basic parameters of the historical system seem remarkably sound: see Hassan & Robinson 1987); but major compression of any part of the historical sequence would cause real headaches - notably for Middle Kingdom and Second Intermediate Period Egypt, if the 18th Dynasty were to fall to the centuries around 1100 BC! It is a fundamental criticism of the book that such implications are not explored, and their consequences in creating 'dark ages' at other, less plausible, periods are not faced.

Inevitably, in gathering evidence of chronological discontents, some old chestnuts are picked up along with more genuine grounds for concern. For instance, the disappearance of an iconic record of chariotry in Greece between 1100 and 700 BC has more to do with a lack of pictures than an absence of vehicles, so that their reappearance in a distinctive Greek form should occasion no surprise, and is no argument for abolishing the interval as a span of time. The clearly Bronze Age elements in the Homeric epics are not a 'strange mingling', with 'baffling' consequences, but a logical

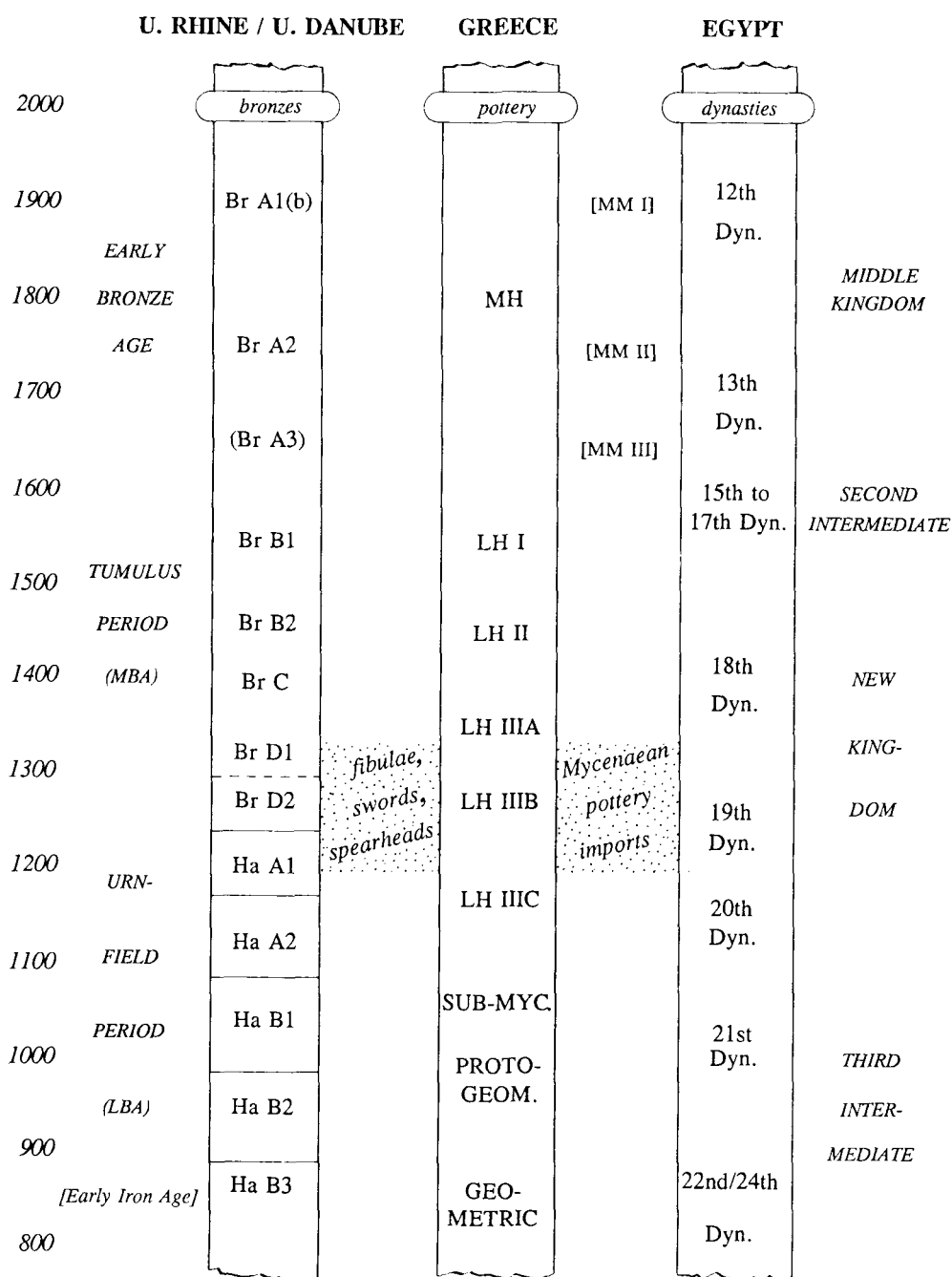


Table 4. Comparison of the dendro-dated sequence in west-central Europe with conventional chronologies of Greece and Egypt, showing the zone of cross-dating defined by the coincidence of central European types in Mycenaean Greece and LH IIIB pottery exports to Egypt. (European sequence post-1400 after Sperber (1987), using his phase definitions and dated boundaries (horizontal divisions). At this level of resolution, the typological definitions of the phases are as important as the absolute dates. Note that Peter Kuniholm's east Mediterranean dendro-sequence now covers the whole of this period, and will soon allow comparable precision for the two right-hand columns.

feature of the growth and re-interpretation of the epic tradition (Sherratt 1990). Nor need the production of artefacts such as the Cypriot *ajouré* bronze stands have ceased abruptly in the twelfth century (to leave a gap of 200 years before the description of the metalwork of Solomon's temple in I Kings 7). In fact, the increasing evidence for Cypriot maritime enterprise in the western Mediterranean in the closing centuries of the second millennium goes a long way towards resolving the 'myth of early Phoenician colonization', with direct

Levantine involvement as a relatively late stage in the process (Sherratt & Sherratt, in press). Equally, the lack of archaeological evidence for great wealth in the Iron IIA period (within which the reign of Solomon is believed to fall) is hardly grounds for assigning him to the richer Late Bronze Age, since (as Michael Vickers has shown) there is no simple correlation between textual descriptions and archaeologically deposited wealth. These examples betray a degree of naïveté about the nature of archaeological evidence, or more

disquietingly a propensity to collect uncritically any superficial anomaly that adds grist to the mill. Quite soluble problems are exaggerated, or even created, by mixing old guesses (Blegen's date for the end of Troy VIIb) and unresolved lacunae (coastal settlement in Sicily) with face-value Thucydides! All of these sorts of evidence need their own source-criticism before they can even be put together into a problem. The image of the 'detective' finding 'clues' to the 'solution' (see blurb) is a substitute for analytical thinking, and begins to shade into the style of Velikovsky (or even von Däniken), with unrelated episodes linked by relentlessly aphoristic sub-headings.

It would be ungracious - having praised their earlier critical efforts to draw attention to the tenuous threads that hold together widely believed ceramic sequences and destruction levels - to be too hard on this piece of rhetorical advocacy and its attempt at a constructive solution, and to register complete disagreement with what is proposed. Yet this is essentially what Colin Renfrew does in his Foreword, and he is right. Like Renfrew's own *Archaeology and Language*, or Martin Bernal's *Black Athena*, this book bravely takes on a whole area of scholarship and exposes its weaknesses - before they, in turn, expose their own. Each one contains some assertions that are gloriously right, but their solutions cannot be recommended as convincing answers. It is nevertheless a valuable (if unpopular) role to play in the ecology of academe, by forcing us to be explicit about the nature of the archaeological record and by bringing specialist debates into a wider framework of discussion. After all, the collapse of Bronze Age civilization is a theme which unites practitioners of archaeology throughout the western Old World.

One of the most attractive elements of this book is its dust-jacket, itself designed by Peter James. It features a jigsaw-puzzle of the hunting scene from the painted box of Tutankhamun, with upper and lower sections separated by a yawning black gap. The observer is subconsciously invited to join the two sections by fitting them together: a visual metaphor for the James Solution to Dark Ages. An alternative approach, not allowed by this design, is to dispel the darkness by turning up the light - or even taking off the tinted spectacles.

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