Shorter Notes

Centuries of Darkness: A Reply to Critics

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When writing *Centuries of Darkness*, with its proposal for a lowering of Late Bronze Age chronology in the Near East and Mediterranean by some 250 years, we expected few specialists in the numerous fields touched upon to accept readily such a radical proposal. Yet the overall response to our summary paper in the *Cambridge Archaeological Journal* (James *et al.* 1991b) was disappointing. Our critics have either misinterpreted our theory or have misrepresented the nature of the evidence. Our response here is necessarily restricted to a few key issues.

With the notable exception of Snodgrass, all the respondents claim that we confuse gaps in knowledge with real gaps in the archaeological record. This entirely ignores the purpose of the book, which was to highlight the coincidence in time of 'gaps' of a strikingly similar nature across an extraordinary range of cultures, from Italy to Iran, and from the Balkans to Sudan, between the twelfth and ninth centuries BC. This timespan is rife with chronological paradoxes which are intimately related and result from a clear conflict between two dating systems: one derived from Egyptian historical chronology based on a reconstruction of the nebulous Third Intermediate Period; and the other from local historical chronologies, such as the Assyrian, which is accurate back to the late tenth century BC (James *et al.* 1991b, 231-3).

Historical considerations

Kitchen's reconstruction of the Third Intermediate Period rests on a key assumption: the identification of Shoshenq I of the 22nd Dynasty with the biblical Shishak who invaded Judah *c.* 925 BC. This link, regarded by Kitchen as 'unassailable', allows only a 20-year latitude in later Third Intermediate Period chronology and sets the terminal date for the preceding 21st Dynasty. With the practical demise of Sothic dating, the Shoshenq/Shishak identification assumes even greater importance as the cornerstone of Egyptian historical chronology, now ironically, dependent on biblical dating. Kitchen's claim to have reconstructed this period without recourse to 'alien evidence' is simply not true.

We have never denied that Shoshenq and Shishak form a plausible philological parallel - but equivalence of names does not prove identity of individuals. The geopolitical differences between the campaigns of Shoshenq I and Shishak still remain, and the fact that the original narrative of the Shishak invasion (1 Kings 14:25-26 & 2 Chronicles 12:1-9) was written no earlier than the seventh century BC (cf. Millard 1991, 19-21) has been completely neglected. Given this, can so much really be hung on the resemblance of two names? Insistence on this identification forces the rejection of an unequivocal name pair - the Shipitbaal of Byblite inscriptions (conventionally placed *c.* 900 BC) with the Shipitbaal of Byblos in the Assyrian records *c.* 740 BC (James *et al.* 1991a, 250; 1991b, 233).

Regarding our alternative candidate for 'Shishak', the abbreviation 'Sesi' *is* attested for Ramesses III of the 20th Dynasty (Epigraphic Survey 1970, pl. 636; and Kitchen's own *Ramesside Inscriptions* V, 295, 3!). Kitchen protests that Egyptian *s* cannot become *sh* in Hebrew, but the biblical text was originally unpointed and one cannot determine whether the *shin* in Shishak was meant to be read as *s* or *sh*. The *qoph* may have been added by a scribe more familiar with Libyan royal names than with the popular terminology of Ramesside times. Such a corruption is possible. On the other hand, Chronicles give details of the composition of the Egyptian army which appear to predate the 22nd Dynasty, such as the presence of the Sukkiim, universally agreed to be an obscure tribe called Tjukten (Kitchen 1986, 295). But there is no reference to the Tjukten in Egyptian texts later than the early 20th Dynasty (Gardiner 1948, 81, n. 1; Giddy 1987, 126).

The 'incredible' fallouts of our realignment of Egyptian and biblical history are imaginary. Kitchen argues that Merneptah could not have been the father-in-law of Solomon, because that pharaoh crowed about a disaster which struck Israel; but on our model the 'Israel Stela' predates the marriage alliance between Solomon and Egypt. We have not suggested that David carved out his Syrian Empire in the middle of the reign of Ramesses II. Ancient historians agree that

Solomon's reign ended *c.* 930 BC, but hardly any accept the schematic biblical figure of 40 years apiece for David and Solomon. Such a figure for both together would be more realistic (Kokkinos forthcoming), placing David's campaigns at the end of Ramesses II's reign, when the armies of Hatti and Egypt were indeed sitting idle. The rise of Israelite nationalism under David's predecessors may well have been a contributing factor to the rapprochement which Hatti and Egypt had to reach in year 21 of Ramesses II.

Other Egyptologists, with less vested interest in the status quo of Third Intermediate Period chronology, are becoming increasingly less certain about its present reconstruction. Kemp, for example, recognizes that 'the internal political situation was indeed sufficiently complex to make it seem not out of the question that alternative reconstructions are historically feasible.' Kemp, however, assumes that our book is not just about chronology and that it has 'an important subtext: that civilization, once started, ran progressively and also at an even pace.' He evidently missed the pages we devoted to the collapse of civilization at the end of the Bronze Age. Kemp seems reluctant to consider the real problems of Nubian history between the twelfth and ninth centuries BC and consistently misrepresents what we said. For example, he claims that we 'appear to assume that indigenous Nubian life the New Kingdom was lived out in the Egyptian towns'; there is nothing in the chapter which suggests this and the exact opposite has been argued elsewhere (Morkot 1987).

The brief response from Postgate is based on misunderstandings. He 'assumes' that we have argued a wholesale reduction of the Middle Assyrian period by 250 years, deduces that the dates of Tiglath-Pileser I would drop to 865-827 BC and quite rightly notes that the result is 'patently absurd'. In reality we argued for a reduction in Tiglath-Pileser I's date of only some 110 years, bringing his reign down to *c.* 1010-970 BC (James *et al.* 1991a, 303).

Postgate implies that we were unaware that fragments of the Eponym Canon survive relating to the period before 911 BC (but see James *et al.* 1991a, 302). He discusses KAV 21+22, claiming that this tablet 'in all probability started with the reign of Tukulti-Ninurta I in the later thirteenth century', for which there is no evidence. More importantly, we did not dispute the eleventh-tenth century placement of the kings given in this tablet. Its intelligible portion extends no earlier than the reign of Shalmaneser II (1031-1020 BC), and breaks off shorty before. What we suggested was that this shadowy line ruled concurrently with a more powerful line descended from Ili-hadda, which established itself in the turmoil following Tukulti-Ninurta's assassination and continued until the reign of Eriba-Adad II, grandson of Tiglath-Pileser I. This 'Ili-hadda dynasty' is presented by the Assyrian King List as having ruled before the kings covered by KAV 21+22, but is not represented in any extant portion of the Eponym List. Postgate's belief that we must somehow discount the evidence of the earlier Eponym List is wide of the mark.

Radiocarbon and dendrochronology

The response from Snodgrass was most welcome, particularly his position that he would be 'more favourably disposed . . . were it not for the completely independent factor of scientific dating'. While we share his view of the value of dendrochronology, we said little about Kuniholm's development of an Anatolian sequence simply because we felt that comment was premature. Despite the existence of earlier 'floating' sequences, Kuniholm has still to provide a *continuous* series further back than the Middle Ages. The work still has to be completed and tested. The 71-year correction of the German tree ring sequence some years after its initial publication (Pilcher *et al.* 1984) provides a salutary lesson against relying on results from preliminary publications.

Nevertheless, as Snodgrass suggests, one can assess the implications of the 1503-year long sequence already fixed in terms of absolute dates by C14 wiggle-matching, ending with a date of 757± 37 BC for the last ring in the Midas mound at Gordion. Between this and the last preserved ring of charcoal from a Hittite building at Maşat, allegedly a palace from the time of Suppiluliuma I, lie 635 dendro-years (previously estimated at 654), giving the latter a date of 1392±37 BC (Kuniholm 1990). This agrees reasonably well with the conventional *floruit* for Suppiluliuma, *c.* 1325 BC. One swallow, however, does not make a summer. The result, far from giving a date for the building's construction, merely gives a date for the death of the last preserved tree ring, not even felling of the trees involved. Note that there is no bark present on the samples.

Snodgrass must be aware of the dangers of using results (dendro or C14) from timbers to date specific archaeological contexts. Kuniholm himself provides a classic instance from Clay Cut Building 3 at Gordion. Eight samples furnished its terminal date, but three others were shown to have been felled four centuries earlier. Kuniholm (1988, 8) stressed that 'If only the latter had been collected, the result would have been an entirely erroneous notion about the date of CC3.'

The Sherratts suggest testing our chronology by reference to Central Europe. Here there are well developed continuous dendrochronologies which can be used to provide reasonably accurate dates for the Neolithic and Early Bronze Age. These do not bear on our model. For the period discussed it is inadequate simply to quote Sperber's statement that the traditional dating for the Urnfield Culture (Bronze D to Hallstatt B) has been confirmed by C14 and dendrochronology.

Discussing the C14 dates from Thera, the Sherratts wisely stress the need for 'source criticism' as a 'necessary preliminary'. This is one of the bases for our approach to chronology. But they blithely ignore their own advice by uncritically accepting Sperber's conclusions for Central Europe. He dates the end of Bronze D to 1225 BC, essentially on the basis of the C14 dates from Padnal bei Savignon, Horizon B (Sperber 1987, 138, 297, tables 47 & 47A). There are only three results for this context (Rageth 1982, 62-3), all from charcoal, with its well known propensity to produce dates too old for its context. The calibrated date-spread actually runs from 1650 to 925 BC, with 1225 being a rough average. From this 'fixed' point Sperber then calculates the beginning of Bronze D as 1370 BC, by allowing a notional length of 50-60 years for each of his settlement phases. For Hallstatt A neither C14 nor dendrochronology was used. The period Bronze D to Hallstatt A1, for which Sperber's framework is so flimsy, coincides exactly with the zone of cross-dating between Mycenae and Central Europe dated so confidently and precisely by the Sherratts in their Table 4.

Sperber is on firmer ground when we reach the transition from Hallstatt B1 to B2. Using dendro-dates of 1028-1010 BC from Zürich Grosser Hafner, Cortaillod Est and Zug im Zumpf, he produces a transition date of 1020 BC (1987, 133). The dendro-dates used, however, were taken from squared oak timbers at Zürich Grosser Hafner and Zug im Zumpf; at the latter many samples had been cut down to the heartwood. These results therefore provide no more than a *terminus post quem*, with an uncertain distance from the end of Hallstatt B1, traditionally dated around a century later. While this provides some degree of control on the terminal point of Bronze D, it cannot rule out our revision, especially since the evidence of links between Mycenae and Europe via Italy supports a less simplistic view than making Bronze D the equivalent of LHIIIB (James *et al.* 1991a, 24).

Kemp also thinks that C14 results contradict our theory, citing from Egypt only those from Amarna. We did not 'discount' them as he states. We noted that only three, rather than five, satisfied criteria rigid enough to test our model. Nor did we cite two widely different dates from Horemheb's tomb in order to 'discredit the radiocarbon method'. We cited *three* results, all with calibrated ranges (1410-1000 BC, 1255-920 BC and 1260-990 BC) consistent with a revised date from Horemheb in the early eleventh century BC; only one accommodates a late fourteenth-century date. Our point was that while the Amarna set supports the conventional chronology, it alone is not enough to confirm it. Despite abundant evidence of selectivity in publication, we have documented a significant number of 'late' C14 dates from Italy, Balkans, Nubia, Egypt, Anatolia and Palestine. As Hassan, an authority on Egyptian C14 results and no friend of our theory, has stated: 'It is essential to obtain further reliable determinations from the eighteenth to twenty sixth dynasties, as well as for the twelfth dynasty' (Hassan 1991, 713).

Kemp states that the presently available results for the Late Bronze Age Aegean are not quite sufficient, but that at 'the level of the very crude judgement . . . conventional chronology is not too wide of the mark'. Jumping on the Thera bandwagon, he suggests that the Aegean chronology may even be too low, missing the point stressed by ourselves and the Sherratts, that the results from Thera remain *sub judice* until the effect on plants growing with a volcanic environment can be quantified.

Conclusion

Our attempt to rationalize early Iron Age chronology by lowering the dates for the Late Bronze Age has, for some, failed. While we feel that future results from C14 and dendrochronology will vindicate our model, it is clear that even if we are ultimately proved wrong, the problems we have raised will still remain.

The only alternative envisaged is raising dates for the later Iron Age, a possibility for Greek Geometric chronology tentatively suggested by Renfrew in the Foreword to our book. Cross has already suggested raising the dates for the Protogeometric in Greece on the basis of the Egyptian dating for the Levantine alphabet (James *et al.* 1991a, 85). Desborough considered the effects which an acceptance of Levantine dates would have on the chronology of Greek pottery, but rejected them as preposterous (James *et al.* 1991a, 157). Indeed they are; raising the chronology would create a new Dark Age during the Archaic period, contradicting every available historical source, beginning with Thucydides. In the long run the unchallengeable dates of Assyrian history back to the late tenth century BC would have to be rejected!

It must be clearly understood that adherents of the status quo cannot have it both ways. Cypriot and Palestinian archaeologists can no longer co-exist in a never-never land in which they 'agree to differ' about the dating of Black-on-Red Ware by as much as two centuries. The situation is absurd. We have done our best to provide the kind of imaginative solution which Kemp, the Sherratts and others feel should be applied to the chronological problems of the Dark Ages. The onus is now on our critics to provide *between them* a better general strategy for resolving the abysmally muddled state of Late Bronze to Iron Age archaeology.

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