

Shishak and Shoshenq: A Disambiguation

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The conventional history of the ancient Near East at large, including Egypt and the eastern Mediterranean basin, contains several “Dark Ages,” poorly documented transitional periods of uncertain length. James et al. 1991 have argued that the most significant of these Dark Ages—the transition from the Late Bronze to the Iron Age during the last two centuries of the second millennium BCE—is largely an artifact of an overly long reconstruction of the Egyptian Third Intermediate Period, and that this Dark Age presents itself in every chronology linked to the Egyptian. A wide variety of often seemingly contradictory scientific, archaeological, art-historical, and philological evidence has been adduced to argue for the status quo, or to lengthen or to shorten by up to several centuries the relative chronology for this period. This review article comments on the papers presented at the Third BICANE Colloquium held at Sidney Sussex College, Cambridge (2011), summarizing a variety of recent positions taken in the ongoing evaluation of James et al. 1991, testing the specific proposition that the tenth century BCE biblical Egyptian King Shishak is to be distinguished from the historical Egyptian King Shoshenq I, who is now to be situated about a century later in the latter half of the ninth century BCE. The wider historical implications of this proposed distinction are examined in new detail.

The absolute chronology of the ancient Near East during the first millennium BCE is principally determined by Ptolemy’s (Royal) Canon (see Depuydt 1995a), whose Babylonian segment establishes the onset of the first regnal year of king Nabû-naṣir on 26 February 747 as the earliest secure historical date directly linked to the Julian calendar. The absolute chronology of Assyria may be extended back to the first regnal year of king Adad-nērārī II in 911/0, as determined by the Assyrian Eponym (*limu*) Lists (Millard 1994), which may be cross-linked with the Canon by equating the solar eclipse noted during the eponymate of Būr-Saggilê with that calculated to have been observable in 763, as first noted by H. C. Rawlinson (1867). On the basis of this Assyro-Babylonian chronological backbone, the earliest secure Julian-year dates in neighboring states may be determined: for example, the reign of King Ahab of Israel ended in 853 (Thiele 1983: 94–95); similarly, year 1 of Egyptian King Psammetichus I, founder of Dynasty XXVI, is 664 (Kitchen 2002: 5–6). All further proposed historical dates for these states, and those of neighboring regions whose chronologies are built upon them (e.g., the Aegean, Cyprus, Anatolia, Elam), prior to these earliest secure historical dates must be understood to be modern conventions for which at present no further direct confirmation, scientific or otherwise, exists.

The conventional Egyptian chronology (whether high or low) rests on the so-called Sothic hypothesis, wherein it is assumed that throughout the history of Dynastic Egypt the civil year was taken to be exactly 365 days long, and that as a result it slipped forward continuously

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across the seasons in a 1460-(Julian) year-long cycle (the period between successive heliacal risings of Sirius on the first day of the civil calendar) without a single adjustment of any sort ever. Leo Depuydt (1995b: 45 n. 1), writing in support of this postulate, acknowledges that the conventional Egyptian Bronze Age chronology can only be correct in the complete absence of any calendrical tampering. Peter James, in collaboration with I. J. Thorpe, Nikos Kokkinos, Robert Morkot, and John Frankish, in a volume entitled *Centuries of Darkness: A Challenge to the Conventional Chronology of Old World Archaeology* (London: Jonathan Cape, 1991), argued that the Dark Age at the transition from the Late Bronze to the Iron Age during the last two centuries of the second millennium BCE is largely an artifact of the overly long conventional reconstruction of the Egyptian Third Intermediate Period (Dynasties XXI–XXV)—the nearly five centuries prior to 664 in the conventional chronology—and that this Dark Age presents itself in any and every chronology from western Europe to Iran that is directly or indirectly linked to the Egyptian.

The central hypothesis of the *Centuries of Darkness (CoD)* model proposes specifically that there are significant misalignments and distortions present in the conventional chronology of the ancient Near East at large specifically caused by the currently accepted model for the reconstruction of the Egyptian Third Intermediate Period (TIP) and its relationship with the end of the New Kingdom's Dynasty XX. Its date, in turn, is ultimately dependent upon the Sothic hypothesis (James et al. 1991: 227–28). The standard model for this period is epitomized by Kenneth A. Kitchen's *The Third Intermediate Period in Egypt (1100–650 BC)*, first published in 1973, with two revised editions following in 1986 and 1996. *Centuries of Darkness* presents a radical point-by-point summary and reappraisal of the transition from the Late Bronze Age (LBA) to the Iron Age (IA) across the eastern Mediterranean basin, northeast Africa, and west Asia. The *CoD* model has generated scores of widely divergent critical scholarly reviews, for a listing of which see <http://www.centuries.co.uk/reviews.htm>. This is hardly unexplored country, but the *CoD*'s judicious removal of just over two centuries in total from the TIP—archaeologically, the equivalent of the LBA to IA transition in the Levant—achieved by overlapping portions of the relevant Egyptian Dynasties traditionally seen as successive (cf. Manetho, *Aegyptiaca*), the removal of spurious pharaohs, the shortening of others' ascribed reigns, and the necessarily concomitant shortening of the Iron IA–IIB Levantine archaeological periods, does provide an alternative footing upon which a testable framework might be rebuilt, one where proposed new insights might be subject to rigorous testing, without resorting to faith or authority for solutions.

To quote the editors of the volume under review, Peter James (also the principal author of *Centuries of Darkness*) and Peter G. van der Veen,

BICANE is the acronym for the study group formed to make a fundamental review of “Bronze to Iron Age Chronology of the Ancient Near East.” While not a formally constituted body, it is an umbrella for a collaboration between an increasing number of scholars working together on the chronological interrelations between the archaeology and history of the Aegean, north-east Africa (Libya, Egypt and Nubia) and Western Asia . . . during the Late Bronze and Iron Ages. (p. ix)

The papers presented in *Solomon and Shishak* center around a single but central postulate of the *CoD* model: that the biblical Egyptian King Shishak (1 Kings 14:25–26; 2 Chron. 12:2–9), who raided Judah, seizing its fortified cities and besieging Jerusalem in Rehoboam's fifth regnal year, circa 925, is—despite the obvious similarities in the names and the fact that both kings campaigned in the Levant—to be clearly distinguished from Hedjkheperre Shoshenq I, the Libyan founder of the Egyptian Dynasty XXII, whose triumphal reliefs next to

the Bubastite Gate at Karnak describe his campaigns in Syria, Canaan, the Negev, and Trans-jordan. Based on the archaeological, art-historical, and philological evidence first presented in *CoD*, significantly updated and expanded here, the historical Shoshenq I is rather to be sought in the later ninth century, in an “Omride”—rather than “Solomonic”—Iron Age IIA period, as argued for in Israel Finkelstein’s (1995, 1996) “Low Chronology.” (Finkelstein’s Low Chronology has also generated dozens of widely divergent critical reviews, for a listing of which see <http://www.cjconroy.net/bib/chron-low.htm>.) Further evidence is presented in support of the identification of the biblical Shishak with Egyptian king Ramesses III, ruling at the onset of the Iron IA period in the later tenth century. Finally, Davidic and Solomonic Israel is identified in the terminal period of Late Bronze IIB, here re-dated to the eleventh and tenth centuries.

The colloquium’s three sessions were entitled: 1) Is the Biblical Shishak the Same as the Egyptian Pharaoh Shoshenq I? (ten papers); 2) The Glorious Reign of Solomon, Fact or Fiction? Archaeological and Historical Reflections (six papers); and 3) The Egyptians and Jerusalem (four papers). Some of these papers are critical of the original model, while others offer refinements, additional support, as well as improvements to it.

In the opening essay “Shishak and Shoshenq: A Chronological Cornerstone or Stumbling-block?” John J. Bimson provides a historical overview of the central arguments for and against this identification, which was first proposed by Jean-François Champollion in 1828, at the dawn of the decipherment of Egyptian hieroglyphics (Champollion 1868: 81). The identification is held as confirmed by Kenneth A. Kitchen (1996: 72–76) through his method of “dead-reckoning” back from a claimed fixed point in 716 BCE, despite the significant incongruities in the two kings’ itineraries and the absence of any mention of Jerusalem and of all but one of Rehoboam’s fifteen fortified Judean towns, Aijalon (2 Chron. 11:5–12), in the admittedly damaged Egyptian record. Bimson stresses (pp. 4–5) that, given the hostility of the Deuteronomist towards Jeroboam I and his perceived failings, the Bible’s silence on the supposed need of the Israelite king for Shishak’s Egyptian army to deliver to him his claimed kingdom is not easily accounted for. This is not *argumentum ex silentio*: the picture outlined by the events of 1 Kings 12:1–32, where Jeroboam is summoned from Egypt to join the assembly at Shechem to voice objection to Rehoboam’s continuation of Solomon’s obnoxious policies, followed by Jeroboam’s divinely sanctioned and militarily unopposed secession, is substantially different from and, in fact, irreconcilable with the conventional view. The fragment of a monumental commemorative stele of Shoshenq I found in 1925 at Megiddo among unstratified debris in a spoil heap left by German archaeologists excavating there between 1903 and 1905 (cf. Chapman 2009) is in conformity with that king’s inscription at Karnak, but is of no chronological value.

Aidan Dodson responds with “Shoshenq I: A Conventional(ish) View.” Accepting the equation of Shishak and Shoshenq I, Dodson acknowledges the absence of Jerusalem on the legible parts of the unfinished Bubastite Portal at Karnak. Reinterpreting the evidence provided by Stele 100 at the Gebel el-Silsila quarries, dated year 21 Shoshenq I, Dodson proposes that Shoshenq may have campaigned more than once in the Levant, speculating that the campaign to Jerusalem occurred too close to the king’s death to have been added to the soon to be abandoned construction. Dodson’s appended chronological table (pp. 12–16) represents only a slight modification of Kitchen’s conventional chronology. The addition here of the regnal dates of the kings of Assyria offers an unwarranted air of accuracy; the assignment of the year 824, the last year of the reign of Shalmaneser III, to the usurper “Ashurdaninapal” (cf. RIMA 3 [BM 118892] i 39, 52: ^{md}*aš-šur-KAL-in-A*) is an unnecessary interpolation of the Assyrian evidence.

Shirly Ben-Dor Evian, in “Shoshenq I and the Levant: Synchronizing Chronologies,” argues, on the basis of several aspects of what she terms “Early Iron Age IIA” material culture, for a “lingering Egyptian influence” (p. 17) on ancient Israel, specifically in the wake of Shoshenq I’s campaign, noting in particular that the spatial distribution of Egyptian pottery and seals coincides with the areas claimed as subdued by Shoshenq. However, in the *CoD* model, where Iron Age I is argued to be an approximately four-decade-long period spanning the last quarter-century of the tenth and first decade and a half or so of the ninth centuries, preceding a largely “Omride” ninth century Iron Age IIA, and where Shoshenq I’s reign is argued to be in the latter half of the ninth century, Ben-Dor Evian’s observations can now be attributed instead to the period’s proximity to the declining years of Dynasties XX and XXI. The noted use by Israelites of Egyptian hieratic numerals, in evidence through late pre-Exilic times, likely reflects rather the derivation of the Canaanite alphabetic script (see conveniently Hamilton 2006), probably including numerals, from Egyptian hieroglyphic and hieratic scripts during the Middle Kingdom/Second Intermediate Period.

Robert Morkot, one of the original contributors to *Centuries of Darkness*, and Peter James, in “Dead-reckoning the Start of the 22nd Dynasty: From Shoshenq V back to Shoshenq I,” re-examine Kitchen’s method of “dead-reckoning” backwards through the Third Intermediate Period by reconsidering both its verifiable duration and its historical anchor point. In at least five cases (pp. 22–24) the authors demonstrate how the extant regnal data have been massaged upward, resulting in a potential fifty-year range of error in the conventional chronology. Then, supported by “every available genealogy” for the TIP, they argue for the identification of Osorkon III with Crown Prince/HPA Osorkon, son of Takeloth II (accepted by Kitchen), and the removal of three generations of royal figures of uncertain affinity from the later years of the Libyan genealogy. Further, Osorkon III is identified with the king of the same name mentioned by Piye (ca. 730–ca. 715), with Shilkanni in the annals of Sargon II (716), and with So, to whom Hosea of Israel sent messengers (ca. 725), thereby obviating the need for an “Osorkon IV,” the putative son of Shoshenq V, for whom the authors argue there is no credible archaeological evidence (pp. 33–37). With Shoshenq V now postulated as a contemporary of Taharqo, Morkot and James, dead-reckoning backwards using minimal regal lengths and Apis bull data, suggest the onset of Shoshenq I’s reign in 839 or 829 (cf. ca. 810 as first offered in James et al. 1991: 257), implying that an earlier Egyptian king must be identified with the tenth-century Shishak.

Ad Thijs’ contribution, “From the Lunar Eclipse of Takeloth II back to Shoshenq I and Shishak,” is the development of an earlier publication (Thijs 2010). After providing a possible astronomical explanation for an enigmatic reference to the moon in the dated (25/XII/15 Takeloth II) Chronicle of Crown Prince/HPA Osorkon—argued to most closely correspond with a penumbral lunar eclipse (a phenomenon typically near the limit of naked-eye visibility) calculated to have occurred two days earlier on 15 February 756—Thijs considers the wider implications of the re-identification and re-dating of King Osorkon III to the later eighth century. He reexamines the distribution and assignment of the Apis bull burials and the accompanying dedicatory stelae in the Lesser Vaults of the Memphis Serapeum. Thijs finds that his model with seven original burials, rather than Kitchen’s proposed nine, fits much better with his new astronomically based dates for Takeloth II (ca. 770–ca. 745). Utilizing additional astronomical as well as historical data, Thijs proposes a sequence of absolute dates (p. 56, fig. 11) for the main line of Libyan kings, beginning with year 1 Shoshenq I in ca. 843, somewhat earlier than the dates proposed in the previous paper. Far less compelling and more speculative is his further brief discussion of the identity of the biblical Shishak, likely a Ramesside, and of Zerah the Kushite (2 Chron. 14:8–14).

In the next two papers, the authors, Troy Leiland Sagrillo, “Shoshenq I and Biblical Šīšaq: A Philological Defense of Their Traditional Equation,” and Peter van der Veen, “The Name Shishaq: *Šošēnq* or *Šyšulq*? Responding to the Critics and Assessing the Evidence,” argue respectively against and for the etymology of the biblical name Shishak (*ššq*) among the hypochoristica evidenced for Ramesses II and III (*ssysw*, *ssw*, *ss*). Points of contention include issues of comparative phonology, especially the realization of sibilants in Hebrew, Egyptian, and cuneiform scripts; the possible paleographic confusion of the early Iron Age Hebrew letters *waw* and *qop*; and/or the employment of a Hebrew pun or other word play, the latter certainly in evidence elsewhere in the biblical text. Given the arguments and counter-arguments offered here, it seems clear that, although some of the points made favoring the equation Eg. *ssw* = Heb. *šyšq* are suggestive, the philological arguments adduced to date do not appear in themselves to be decisive on this issue.

In “Ramesses III as Biblical Shishak? Some Notes on the Archaeological Evidence,” John Bimson seeks to test this identification by examining whether the archaeological and inscriptional evidence can be harmonized at the LB IIB–Iron I transition during the reign of Ramesses III, when “the number of LBA cities dwindled and Iron I villages proliferated in the central highlands” and in the Galilee (p. 98). The *CoD* model rejects Albright’s identification of this transition as the mark of early Israelite settlement, identifying it rather with the breakup of the United Monarchy during an era of marked economic and political decline, in part the result of Solomon’s unsustainable and unpopular policies. These Iron I settlements are now seen as the visible manifestation of Jeroboam I’s secession and the flight of refugees from further oppression by Solomon’s son Rehoboam from such cities as Megiddo and Hazor (cf. Zuckerman 2007), perhaps weakened by internal revolts, while sites such as Shechem and Tirzah, early Israelite capitals, flourished. The final abandonment or destruction of those Iron I settlements, after several decades of continued political instability in Israel, is now located early in the “Omride” Iron IIA period, its onset re-dated to the early ninth century. It might be added here that the apparent absence of distinctive Iron I burials may now be explained by the fact that the period spans less than half a century rather than the conventional two centuries (or more than three in the Low Chronology) and that during this brief period the Iron I population in the central highlands continued to employ LBA burial practices (cf. Kletter 2002: 39).

Bimson moves on to reassess the evidence for Ramesses III’s campaign(s) in western Asia, including Papyrus Harris I and the Medinet Habu reliefs and inscriptions, in the light of the recent acceptance by Kitchen of an Egyptian attack on Amurru (p. 105) and the reinterpretation of the evidence by Robert Drews (2000), identifying Ramesses’ attack on Djahi not as a defensive measure within or close to Egypt’s borders, but rather as a retaliatory raid somewhere in the northern Levant (pp. 104–8). Finally, Bimson examines the Chronicler’s account of Shishak’s invasion, finding that references to Libyans, Sukkim (= Tjukten/Tjeekten), and Kushites, difficult in the conventional chronology, would not be out of place in Dynasty XX (pp. 108–9). He adds that of Rehoboam’s fifteen fortified towns taken by Shishak, according to the Chronicler, only one (Marashah) of the eight sites for which there is archaeological evidence has failed to produce evidence of LB II/Iron I occupation and abandonment, likely due to limitations of the excavation (pp. 109, 111–12).

Peter van der Veen and Peter James, in “Zerah the Kushite: A New Proposal Regarding His Identity,” seek to identify this invader of southern Judah during the reign of King Asa in the earliest years of the ninth century (2 Chron. 14:9–15). Rejecting the conventional identification with Osorkon I, the authors propose that Zerah is more plausibly to be identified, both historically and etymologically, with Userḥau, “overseer of the (northern) foreign lands,” the

highest-ranking Egyptian official in western Asia, who campaigned in the Levant in year 5 of Ramesses IV. (For the restoration of Sinai 297+300, see Dijkstra 2009.)

In the following paper, “When *Did* Shoshenq I Campaign in Palestine?” James and van der Veen locate Shoshenq I’s invasion of Canaan in or shortly before his twenty-first regnal year and seek to equate it with the movements of the anonymous “savior” who rescued Samaria following the attacks by Hazael and his son Ben-Hadad “III” of Damascus throughout the reign of Jehoahaz (2 Kings 13:1–7) (r. 819–804/3 [Galil 1996]; 814/3–798 [Thiele 1983]), and continuing into the reign of his son Joash. However, if these events are to be squared with 2 Kings 7:6–7, where an unnamed king of Samaria besieged by a Ben-Hadad is saved by the Israelite king’s hiring of the kings of the “Hittites” (Assyrians?) and the Egyptians, whose approach causes the besieging Aramaeans to withdraw, then Shoshenq I’s first regnal year must have been closer to ca. 817, rather than the dates proposed above (Thijs: 843; James and van der Veen: 839/29; cf. James et al. 1991: ca. 810). Note further that Assyrian King Adad-nērārī III, whose first regnal year was 810, is not thought to have campaigned in the West prior to 796. Additionally, Jehoahaz’s son Joash (r. 805–790 [Galil 1996]; 798–782/1 [Thiele 1983]), who in fact did pay tribute to Adad-nērārī III sometime between 805 and 796 (Galil 1996: 53), is said only to have “recovered” (*√lqh*) the towns previously conquered by Hazael during the reign of the latter’s son, Ben-Hadad “III” (2 Kings 13:22–25).

In short, if in fact 2 Kings 7:6–7 in any way reflects the events of 2 Kings 13:1–7, then contra James and van der Veen, Ben-Hadad “III” must have abandoned the siege of Samaria circa 796 in order to defend Damascus from the impending attack by Adad-nērārī III and the threat posed by Shoshenq I’s campaign to recover (on behalf of Joash?) the territory previously seized by Hazael. On the other hand, the authors’ argument that Shoshenq I’s reference to the land of “Mitanni” is merely an anachronistic reference to Assyrian-dominated northern Syria may be compared favorably with the contemporary use by Adad-nērārī III of the equally anachronistic term “Ḫatti” referring to the same general region (e.g., RIMA 3 207 [BM 131124]: 3).

The papers from Session 2 begin with Rupert Chapman’s “Samaria and Megiddo: Shishak and Solomon,” a reexamination of the dating of Iron IIA pottery in the light of Ron Tappy’s reanalysis of Kathleen Kenyon’s excavation archives for Samaria. Chapman finds that, indeed, the Iron IIA assemblages at Samaria, and by extension, at Megiddo, belong to the ninth century, and thus are “Omride” rather than “Solomonic,” a position in line with Finkelstein’s Low Chronology, and as far as it goes, not incongruent with the *CoD* model. However, unlike the latter, Chapman does not propose any significant compensatory lowering of the dates of the preceding periods, presuming that Megiddo Stratum VIIA (Iron IA) is still correctly dated to the end of the twelfth century (ca. 1125) (p. 146), forcing one to seek evidence for King David, whose existence he at least accepts on the basis of the Tel Dan Stele, and for King Solomon, for whom he finds no verifiable extra-biblical evidence, in the “uniformly unimpressive” Iron IA–IB strata (p. 143), now presumably representing a more than three-century-long period.

Wolfgang Zwickel, “Solomon’s Temple, Its Cultic Implements and the Historicity of Solomon’s Kingdom,” examines the LBA and IA archaeological parallels for Solomon’s Temple and its implements in an effort to date the relevant biblical text, 1 Kings 6–7, which he retranslates in an appendix (pp. 152–54). Zwickel finds that there is “enough circumstantial evidence” (p. 151) to conclude that the Temple and its implements were the products of a large single polity such as Solomon’s rather than of the later Divided Monarchy.

The next paper, “Josephus and Greek Chronography: Troy, Solomon, Shishak and Rameses III,” is by Nikos Kokkinos, another of the original contributors to *Centuries of Darkness*.

Kokkinos reconsiders Josephus as an ancient chronographer in the light of the for the most part Hellenistic historians, foremost Manetho, upon whom Josephus drew in order to argue for the greater antiquity of the Jews than of the Greeks. Kokkinos dissects in great detail in an appendix (pp. 168–77) and series of tables (pp. 178–89) each of the sources in order to demonstrate how Josephus came, on the one hand, to a “high” chronology, placing Joseph’s arrival in Egypt nearly a thousand years before the Trojan War (ca. 1120) and the commencement of Solomon’s reign (ca. 1129), but, on the other hand, to a “low” chronology derived from the *Tyrian Annals*, placing the building of the Temple about 957/6. Nonetheless, Kokkinos argues that at least implicit in Josephus’ relative chronology (although unbeknownst to him) is the identification of the Queen of Sheba with the female Egyptian King Tausret (“Thuoris”) and of Shishak with Ramesses III (“Rhampsinitos”).

Peter van der Veen, “Early Iron Age Epigraphy and Chronological Revision: A Summary Article,” offers an anecdotal summary of epigraphic finds whose general provenience, if not specific stratigraphic context, is known, and that appear to support a significantly lower chronology. These include the Shema’ and ’Asaph seals from Megiddo, the re-inscribed royal statues of Shoshenq I and Osorkon I from Byblos, post-Ramesside mass-produced stamp-seal amulets naming Siamun excavated in Dor, the Shoshenq I stele fragment from Megiddo, Iron Age I–IIA body sherds inscribed *nmš*, the Kefar Veradim fluted bowl, the Tell Fakhariyeh statue with bilingual inscription, and the alphabetic inscribed bronze arrowheads. Most notably absent from this list is Ahiram’s sarcophagus inscription from Byblos, despite the presence on the volume’s front cover of a facsimile of a detail of the sarcophagus relief showing the enthroned dead king (see appendix below).

Uwe Zerbst and Peter van der Veen’s “Does Radiocarbon Provide the Answer?” demonstrates that there remain significant conflicts between archaeological- and radiocarbon-based time scales not ameliorated by the use of elaborate Bayesian statistical methods. Following a detailed summary of the Bayesian approach for improving the precision, but not necessarily the accuracy, of the calibrated data, and a brief discussion of the “outliers” problem, the authors, after examining fifteen distinct archaeological periods, find there are still significant conflicts between calibrated radiocarbon and (conventional) historically determined dates, the latter typically on the order of from one to four centuries younger. Although briefly noted (e.g., p. 208, fig. 10), the lower yet astronomically determined dates for the construction of several Old Kingdom pyramids as established so elegantly by Kate Spence (2000, 2001) deserve far more attention: Her date for the start of construction of Khufu’s pyramid in his second regnal year is 2480 ± 5 , providing a seemingly secure anchor point for Dynasty IV. Perhaps most telling is the two-century error introduced by calibration into the radiocarbon dating of the bones of those killed in the collapse of a city gate during the destruction of Nineveh, historically determined to be in 612. First noted by Taylor et al. 2010, the fact that the majority of the offsets in the radiocarbon dates could not be adequately accounted for suggests the possibility of a more deeply rooted systematic error at play (cf. Porter and Dee 2013: 1374).

Robert M. Porter, “Recent Problems with Dendrochronology,” begins his review by focusing on the work of P. I. Kuniholm and the attendant dating of in particular the Late Bronze and early Iron Ages in Anatolia. Porter clearly enunciates the inherent weaknesses and unwarranted assumptions behind Kuniholm’s data and his questionable methodology, concluding that the resulting solutions cannot always be trusted. Perhaps most significant is Porter’s inference that as a consequence the International Calibration Curve for radiocarbon dating, which is directly informed by dendrochronology—much of whose primary data remains unpublished—may be similarly suspect, perhaps contributing to the possible

systematic error in radiocarbon dates noted above. Note further Steven W. Cole (2014: 5), who has similarly remarked on the difficulties resulting from the “unquestioned acceptance of the dendrochronological dating” of the Anatolian timbers used to determine the absolute dates of the earlier Old Assyrian chronology.

The papers from Session 3 begin with Peter James, “Kings of Jerusalem at the Late Bronze to Iron Age Transition—Forerunners or Doubles of David and Solomon?” After examining the nature of the Egyptian presence in the Levant during the Late Bronze Age, in which “control” of the region was largely mediated through ostensibly loyal local princes, James seeks to identify the activities of some of these rulers—rarely identified by name in the Egyptian sources, apart from the Amarna letters—in the archaeological record of, in particular, Jerusalem, Megiddo, and Lachish at the LBA/IA transition. Following a brief discussion of the likelihood of the continued use of the massive Middle Bronze Age walls at Jerusalem until the Iron Age II period, which would account for the apparent absence of specifically LBA walls, James reconsiders when and by whom the so-called Stepped Stone Structure, previously identified by Kenyon as the biblical “Millo,” and the adjoining “Large Stone Structure,” excavated more recently by Eilat Mazar (cf. Finkelstein 2011), were built in the City of David between what is conventionally the twelfth and ninth centuries. Given the resources, manpower, and organization required for such massive undertakings in Jerusalem, and in the light of the Ramesside finds in the vicinity of the St. Étienne monastery, James postulates the existence of a regional polity, “Dynasty J,” with which he also associates the Megiddo Stratum VIIA palace, especially its treasury (which “may have replaced a similar unit in the modified part of the western wing” of the level VIIB palace, according to Ussishkin 1995: 240–46), including hundreds of imported and locally made ivory carvings, the hieratic bowls from Lachish Level VI mentioning a *wr* “foreign ruler,” the *wr* “(Ir)su the Kharu” of Papyrus Harris I, and evidence for large-scale bronze casting in the Jordan Valley.

James concludes that, regardless of chronology, “Dynasty J” must have been at its pinnacle of power between year 5 Merneptah and year 8 Ramesses III, a span of no less than thirty-two years (in the low Egyptian chronology), during which time there was no known significant Egyptian military activity in the Levant. This remarkable dynasty, in the conventional chronology, might be successors of Abdi-Ḥeba, who himself appears to have enjoyed a special relationship with Egypt (Moran 1975), or rulers of a new dynasty, perhaps “Jebusites.” In James’ *CoD* model this would be expected to be the period of Solomon, who, he suggests, perhaps functioned as the Egyptian viceroy at Jerusalem. Note, however, that when Hans Goedicke’s (1979: 13–14) singular translation and interpretation of the “Historical Section” of Papyrus Harris I (lxxv 1–9) is transposed onto the lower *CoD* timeframe, then *Sw*, instead of being the possible “historical prototype of Saul,” might have been the historical (prototype of) Solomon.

Simone Burger Robin, “Analysis, Interpretation and Dating of a Problematic Egyptian Statuary Fragment Discovered in Jerusalem,” discusses the fragment of a red granite Egyptian statue of a royal woman, without inscription or controlled archaeological provenience, which is said to have been discovered by workmen in Jerusalem in the 1920s. Rejecting a Middle Kingdom origin for the statue, Burger Robin dates it rather on the basis of specific iconographic parallels to the reign of either Ramesses II or Merneptah.

In Peter van der Veen and David Ellis’ “‘He Placed His Name in Jerusalem’: Ramesside Finds from Judah’s Capital,” the authors survey a collection of “high status” Ramesside finds, including the previously noted statue of a royal woman, all of which appear to have originated at points to the north and west of Jerusalem’s Old City (and the City of David). These objects, many with funerary associations, according to van der Veen and Ellis, together sug-

gest the presence of Egyptian officials stationed, and buried, near Jerusalem during Dynasty XIX. When seen within the context of the *CoD* chronology, many of these objects, especially the queen's statue, would not be inconsistent with the presence and perhaps residence of Solomon's royal Egyptian bride, Merneptah's daughter, who gave Gezer, previously sacked by her father the pharaoh, as dowry. If true, this would be a most noteworthy turn of events in light of Dynasty XVIII's expressed distaste for such marriages (e.g., VAB 2 4).

Dan'el Kahn concludes the volume with "The Campaign of Ramesses III against Philistia," an abbreviated and amended version of a previously published article (Kahn 2011) that argues that the assumed connection between the sea and land battles fought by Ramesses III against the so-called Sea Peoples should be rejected, a position previously argued by Drews 2000, but ignored here. Kahn locates Ramesses III's campaign during his eighth regnal year against the Philistines (*plst*) at Djahi not in southern Canaan or Egypt itself, but rather well to the north in the Amuq plain (see now Kahn 2016). Kahn bases his proposal on references in Iron Age Hieroglyphic Luwian inscriptions to the land of "Palastin" or the like, dated conventionally to circa the eleventh century (cf. Emanuel 2015). (It would indeed be ironic if any of the destructions of coastal sites commonly attributed to the supposed movement southward of the "Sea Peoples," including the Philistines, were in fact the handiwork of the forces of Ramesses III on their way north to confront the Philistines at Djahi.) But whether or not Djahi is to be located in the Amuq plain is not a chronology-dependent question and its confirmation must come from elsewhere. But the identification of the biblical Philistines with their archaeological counterparts is chronology-dependent: In the *CoD* model, with the period of the United Monarchy located in the latter half of LB IIB, so must be the Israelites' Philistine adversaries. Hence archaeologically these are not to be sought in the Iron I period—where in the conventional chronology they are invariably tied to the distribution of Palestinian Mycenaean IIIC:1b ("Philistine Monochrome") and Palestinian Sub-Mycenaean wares ("Philistine Bichrome")—but rather within the Late Bronze Age strata of those sites within the sway of the rulers of the Pentapolis.

Each of the contributions is appropriately well illustrated with clear black-and-white photographs, plans, maps, and tables, and accompanied with full bibliography. However, in the absence of much-needed indices, internal cross-references of the sort, "see . . . elsewhere in this volume," are not particularly reader-friendly. The occasional typographical errors are generally not significant. The volume's thin plastic-coated paper covers are flimsy and the binding weak; several pages have come loose in the well-thumbed review copy.

This reviewer finds *Solomon and Shishak*, and the original *Centuries of Darkness* model upon which it builds, to be potentially useful tools for any well-informed reader with a serious interest in the contentious field of ancient Near Eastern chronology in general and biblical chronology in particular. These volumes provide a useful, if imperfect, alternative roadmap that may well yet lead to a more coherent and comprehensive revised relative chronology of the greater ancient Near East. Unfortunately the presently proposed model still lacks the necessary fine-scale structure required by historiographers for an absolute chronology of the period under review; this is best exemplified by the generally lower, more accurate, but still imprecise regnal dates proposed variously above for Shoshenq I. To be welcomed would be future colloquia like *Solomon and Shishak*, where interdisciplinary groups of scholars might evaluate the suitability for and impact of this proposed revised chronology on, in particular, the order and arrangement of the Middle Assyrian and contemporary Kassite, Isin II, and Middle Elamite dynasties, and on the late Imperial Hittite and early "Neo-Hittite" states—all cultures for which there exist extensive literary as well as archaeological sources, and, most importantly, well-noted Egyptian and Levantine synchronisms. Central

to any such discussions must be the apparent conflicts between the generally higher dates proposed by the scientific methods, principally radiocarbon and dendrochronology, and the significantly lower archaeologically and historically determined dates; these discrepancies need to be addressed seriously and openly and resolved by reason alone.

In conclusion, it seems more than likely to this reviewer that the Dark Age at the transition from the Late Bronze Age to the Iron Age in the ancient Near East is largely, but certainly not entirely, an artifact of the conventionally reconstructed chronology, and, in the face of the supporting evidence presented in part by the contributors above, should be given further serious scholarly (re)consideration. Two additional studies bringing further insights to the discussion might be noted: Amos Nur and Eric H. Cline (2000) have amassed evidence to suggest that an “earthquake storm,” a series of related earthquakes that spread across southern Greece, western Anatolia, and coastal northern Syria and the southern Levant over a fifty-year period or so at the LB/IA interface, may have been a significant contributor to the site destructions observed (cf. Drews 1993: 33–47); and Dafna Langgut et al. (2013) have presented evidence for a drought phase at the LBA/IA transition associated with a 3.2 kyr BP (ca. 1050±150) aridification event that also appears to have had a brief but significant impact on the region. But while the exact shape of a revised chronology of the period remains to be determined, the outlines of a potentially viable alternative shorter chronology would appear now to be available (see Figure 1).

APPENDIX

The sarcophagus of King Ahiiram of Byblos, with its relief carvings and incised linear alphabetic inscription, is conventionally dated no later than circa 1000 BCE and as early as the thirteenth century, but has also been dated to the ninth through early eighth century by an entire suite of art-historical and archaeological considerations enunciated quite clearly some forty years ago by Edith Porada (1973) and, a decade later, with additional philological and paleographical evidence by this reviewer (1983) while still a student of the late professor. This lower date is now substantially reinforced by the relief-carved ivory pyxis IM 79513 (height 6.4 cm) excavated in Well AJ of the North West Palace at Nimrud. Of one of the narrative reliefs on the pyxis, depicting a royal banquet, Georgina Herrmann noted:

It is remarkable just how closely the arrangements of figures and the form of the furniture, both the sphinx chair and the table, are paralleled on the scene illustrated on the famous sarcophagus of King Ahiiram. (1989: 90)

Herrmann ended the above sentence with a footnote citing Porada 1973, who in her turn had concluded that:

Ahiiram’s reliefs continue the iconographic traditions of Syria and Palestine as well as of New Kingdom Egypt, *but they have assumed the simplified, heavy forms found on the reliefs of Carchemish and of Ashurnasirpal II of the ninth century B.C.* (Porada 1973: 364, emphasis added)

Despite the continued misuse of a statement Porada made there (p. 364) regarding the possible dating of the inscription on the tomb to about 1000, a necessary acknowledgement of Albright’s then-dominant position—in which case the reliefs would be the only example of the plastic arts in the region for this period—Porada firmly believed that the Ahiiram tomb reliefs were perfectly good examples of ninth- and early eighth-century North Syrian art. It is difficult to imagine otherwise how two local artisans supposedly working several centuries apart, in media (limestone and ivory) of different hardness and on scales differing by a full order of magnitude, could have produced essentially identical representations of what is self-

JULIAN CALENDAR DATE (BCE)	EGYPT	SOUTHERN LEVANT
	↑	↑
ca. 1060±	Dyn. 18	LB IIA
	-----	-----
	Seti I	LB IIB
	Ramesses II	
		Saul
		David (ca. 1010–970)
ca. 950±	Merneptah	Solomon (ca. 970–930)
	<u>Tausret</u>	
	Ramesses III	
ca. 925 (5 Rehoboam)		-----
		IA IA (Jereboam I)

ca. 884±	(Userḥau)	IA IB

		IA IIA (Omri)
		Ahab (d. 853)
ca. 830±	-----	Hazael
ca. 800±	Shoshenq I	-----
		Ben-Hadad “III”
	↑	IA IIB
	TIP	
	↓	
732/722/701		-----
	-----	IA IIC
	—664—	-----
		—587—

Fig. 1. A Revised Chronology

evidently the same type of a tripod table with zoomorphic legs and a distinctive vertical prop between the stretcher and the underside of the table’s blade, otherwise firmly dated to the ninth through eighth centuries (see Gubel 1987: 251–61, Type VIII-d).

Compare further the markedly similar tables depicted on a fragmentary ivory plaque from late eighth-century Nimrud (Room SW 37), as noted by Herrmann et al. 2004: 138, S1890 (ND 9094), and on a Phoenician bronze bowl from Idalion, Cyprus, datable between the last

half of the ninth and the first half of the eighth centuries, as noted by Glenn Markoe (1990: 21–22, fig. 12). Within the same context, Markoe adds (p. 24, n. 15) that “the shape of the eye, nose, and short, rounded ear” of the lions on the Ahiram sarcophagus “find close stylistic parallels in the felines depicted on the ninth-century bronze bowl from the Athenian Kerameikos.” The most parsimonious explanation for the noted similarities among these offering tables (as well as numerous other previously noted iconographic details) is that the artisans producing them were more or less contemporaries rendering in their respective media and scales virtually identical pieces of elite furniture of self-evidently great prestige. Since the ivory carving IM 79513, stylistically an archetypal example of the “Flame and Frond” school (Hermann 1989; cf. Feldman 2012), is securely dated to the ninth through early eighth centuries, as is the Cypriot bowl, then the reliefs, and hence the inscription, carved on Ahiram’s sarcophagus must also be dated to the latter half of the ninth or first half of the eighth centuries.

Finally, it may be noted that Benjamin Sass and Israel Finkelstein’s most recent treatment (2016) of the replacement of the LBA-early IA Old (“Proto-”)Canaanite script during the ninth century omits any discussion of the place of the so-called Old Byblian royal inscriptions, including the Ahiram sarcophagus inscription. These inscriptions (KAI 1–2, 4–7), conventionally dated to the tenth century but re-dated on internal paleographic grounds by this reviewer (1983) to no earlier than the mid-ninth through eighth centuries, a position previously accepted by Sass (2005: 16), are, in fact, fully complementary with the picture presented now by Sass and Finkelstein 2016: The Old Byblian royal inscriptions represent, again as previously argued by this reviewer (1983), yet another “national” script typical of the region during the ninth and eighth centuries. (See now B. Sass, “The Emergence of Monumental West Semitic Alphabetic Writing, with an Emphasis on Byblos,” *Semitica* 59 [2017]: 109–41.)

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