

DEFENCE OF PLANETARY CONJUNCTIONS FOR EARLY CHINESE CHRONOLOGY IS UNMERITED

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Abstract: Pankenier (2007) has responded to the critique by Keenan (2002) of astro-historiographic chronologies of early China. The arguments of Pankenier are assessed herein; the conclusion is that the arguments have negligible scholarly merit. Some problems with radiocarbon dates are also considered.

Keywords: astro-historiography, carbon-14, eclipse, planets, Xia-Shang-Zhou

1 INTRODUCTION

Pankenier (2007) (hereinafter ‘P07’) presents a response to my critique of astro-historiographic dating of early China (Keenan, 2002). The present work considers the points raised in P07. It uses the same section names and numbers as P07.

P07 begins by quoting my critique: the Xia-Shang-Zhou Chronology Project produced a chronology “... relying on a record of a solar eclipse.” (Xia, Shang, and Zhou are the first three Dynasties recorded in Chinese history.) P07 seems to suggest that the quote is inaccurate. In fact, as even a cursory reading of the Chronology Project report shows, the eclipse is relied upon. Additionally, the Director of the Chronology Project described the eclipse as being “... a key point.” (Li, 2002: 328). Similarly, Liu (2002a: Section 1), cited by P07, describes the eclipse as “... one of the fulcrums ...” of the Chronology Project. All scholars of early Chinese chronology should know this.

P07 then makes a similar suggestion with regard to the *Cambridge History of Ancient China* (Loewe and Shaughnessy, 1999). Following is a quote from this volume:

The attempt to establish the first year of the Shang has benefited from new initiatives in archaeo-astronomy. Entries in Zhou texts have been taken as mythologized memories of the five-planet conjunction that occurred in Sagittarius in 1576. Other records of what appear to be similar conjunctions, understood as symbolizing Heaven’s transferral of the Mandate, at the time of the founding of the Xia and the overthrow of the Shang, support this view. (Keightley, 1999: 248).

Elsewhere the *Cambridge History* ... cites five-planet conjunctions as one piece of evidence for chronology, whilst adding that all dates “... should be regarded as provisional.” (Shaughnessy, 1990: 23). Compare those two quotes with this statement from my critique: “[the *Cambridge History* ...] ... has provisionally adopted a chronology based on five-planet conjunctions.” (Keenan, 2002: 67). The statement in my critique is fair.

P07’s second paragraph begins as follows:

Despite the “historiographical” in his title, neither here, nor anywhere else in Keenan’s article is there any mention of the historical evidence from a variety of disciplines that has been brought to bear on the problem of the early chronology. This includes archaeological evidence (stratigraphy, ¹⁴C dating, ceramic and bronze vessel typologies, paleography, etc.)...

The assumption here is that ‘historiographical’ is defined to include ¹⁴C dating, vessel typologies, etc. That is obviously untrue, as Pankenier, who has been working in the field for decades, surely knows. For a discussion of the differences between historiography and archaeology in the context of ancient Chinese chronology, see Lee (2002b).

P07’s second paragraph continues by stating that archaeological and other evidence “... serves to narrow down the chronological range of benchmark dates to within just a few decades in some cases.” This is a statement of the obvious. The chronologies proposed by the Chronology Project and the *Cambridge History* ..., though, claim to be accurate to within about a year, and those claims rely on astro-historiography. A similar point is made by Lee (2002a: 16, 19).

To summarize, my critique does not consider archaeological chronologies, let alone criticize them. P07 invents this claim, then attacks my critique for it. Having said that, I take the opportunity here to make some remarks about one aspect of archaeology: the use of ¹⁴C in the Chronology Project—see Appendix 1.

P07’s first section ends by citing Liu (2002a; 2002b) for a discussion of a solar eclipse in 899 BC, which was considered by my critique. Liu raises two valid issues.

First, my critique’s discussion of eclipse brightness reduction refers to the reductions as percentages (Keenan, 2002: 62). The brightness reductions considered in the cited studies of Liu and co-workers, though, used absolute (rather than relative) reductions; thus the use of percentages in my critique could mislead. To rectify this, the six percentages on page 62 should be changed to decimal fractions, e.g. ‘25%’ to ‘0.25’. This error in the critique is obviously tiny.

The other valid issue raised by Liu concerns the eclipse record of 776 BC from the Bamboo Annals. Liu points out that the record is only in the *jinben* version of the Annals, not the *guben* version, and so that makes the record *a priori* less reliable than the record of the double dawn (which is in both versions). (The *jinben* is also called the ‘current text’ and the *guben* is also called the ‘old text’; for a discussion of the versions, see Nivison (1993).) This oversight in the critique is obviously not crucial.

Otherwise, the points raised in my critique remain valid. In particular, Liu’s treatment of Earth’s rotation rate, eclipse magnitude, etc.—which Liu (2002a: section 5) rightly describes as “... the essential issues ...”—is incorrect; I will not discuss those issues fur-

ther here, but instead defer to Stephenson (2008), who presents a detailed rejoinder.

Finally, I take this opportunity to fix another error in my critique. On page 66, the critique refers to a record of the eclipse of 776 BC as the sole *Bamboo Annals* astronomical record from after 841 BC. The record is actually just the earliest eclipse record in the *Annals*. This error is of negligible consequence in the context.

2 FIVE-PLANET CONJUNCTIONS

2.1 First Quotation

P07 begins by quoting from my critique: “It is unclear how close planets would have to be in order for the ancient Chinese to have considered them to be in conjunction ... some researchers have suggested that the planets only had to be within an arc of 30° (i.e. spanning 30° of the sky).” (Keenan, 2002: 67). P07 next correctly notes “Keenan’s source for these arguments is Huang Yilong (Huang, 1990) ...”, and it then argues against the analysis of Huang.

I showed the argument of P07 to Huang, who replied as follows (private communication, May 2007):

1. No ancient texts so far gave an explicit definition for 五星聚合. My estimate is based on actual usages in ancient observational records.
2. If she 舍 is a synonym for su/xiu 宿, we will find some lunar mansion extends more than 30 degrees.

I am not competent at analysing ancient texts, but that would seem to largely rebut P07’s argument. At a minimum, it can be said that there is scholarly dispute, thereby justifying my critique’s statement that “... some researchers have suggested ...”

It is also worth noting that P07’s main argument against Huang’s analysis is based on a *Mawangdui* silk manuscript from the second century BC (Han period); so even if the manuscript had given an explicit definition, it would hardly be definitive for how conjunctions were perceived by pre-Han peoples many centuries earlier. Considering both that and Huang’s reply, the central point made here by my critique has obviously not been rebutted: it is unclear how close the planets would have to be in order for the ancient Chinese to have considered them to be in conjunction.

P07’s final remark in this section is as follows: “... it is unlikely those astrologer-priests could have missed the spectacular pre-dawn planetary massing of 1953 BCE...” My critique, however, never claims otherwise. That is, P07 is again accusing my critique of saying something that it does not say.

2.2 Second Quotation

My critique observes that “There was ... no five-planet conjunction in 1576 BC, only a four-planet conjunction.” (p.63). Regarding this, P07 acknowledges that there was no five-planet conjunction at that time. P07 further acknowledges that there is no record of a planetary conjunction at that time (saying “... nor does the original text record it as a conjunction.”). P07 then tries to argue that none of this matters. This certainly does matter, though.

Pankenier’s central proposal has been that planetary conjunctions induce dynastic transitions. The lack of a

five-planet conjunction for the transition from the Xia to the Shang is fatal for such a proposal, because four-planet conjunctions (i) occur quite frequently (defined via any reasonable span of arc; for some examples, see Zhang (1990: 147)) and (ii) have the opposite astrological connotations of five-planet conjunctions (Huang, 1990: 110).

P07 further claims that my critique “... misrepresents the case ...” concerning the conjunction of 1576 BC. I disagree; consider how others have presented the case. The quotation in the Introduction from D.N. Keightley explicitly refers to a five-planet conjunction, and Pankenier’s works are the sole source for that. Keightley is a leading scholar in this area, and the quote is from the article on the Shang in the *Cambridge History of Ancient China* (the standard English-language reference for ancient China), which was edited by two other leading scholars, M. Loewe and E.L. Shaughnessy. Thus Keightley, and presumably also Loewe and/or Shaughnessy, believed that Pankenier’s proposals were based on five-planet conjunctions. (Additionally, at the Second Worldwide Conference of the Society for East Asian Archaeology (in 2000), I met other respected scholars who had the same belief.) As this evidences, Pankenier’s publications have led people into believing that the chronological proposal was based on a five-planet conjunction in 1576 BC.

Given some of the statements in Pankenier’s publications, that might be expected. For instance, the following is from Pankenier (1981-1982: 19):

... the original account of the conjunction of 1576 B.C. was an extraordinarily apt characterization of this planetary event: “The five planets regressed ...”

Another example, from Pankenier (1983-1985), is detailed in the next section. And Pankenier (1995) repeatedly discusses the conjunction of 1576 BC together with the conjunctions of 1953 BC and 1059 BC in a way that would likely induce (and evidently has induced) many people to believe that the three were alike. In other words, although Pankenier might never have explicitly stated that there was a five-planet conjunction in 1576 BC, each of Pankenier’s three main papers on the topic has been written such that it could be readily interpreted as describing a five-planet conjunction then.

One other point deserves mention. Pankenier (2007: Section 2) correctly states that I contacted him in August 1998, because of technical issues with his proposals, and that he afterwards sent me a reply. P07 faults my critique for not discussing that. For my critique to discuss that, however, would mean criticizing an unpublished private communication, which seemed to me to be unfair.

The private communication that Pankenier and I had in August 1998 was about a figure in his 1995 paper (Pankenier, 1995: Figure 2). The figure displays the sky on 27 December 1576 BC, when there was a four-planet conjunction. The figure has some obvious problems, e.g. it shows the Sun high in the sky, but lists the time of day as 23:00. So I asked Pankenier, “This isn’t the sky that you would see in China at the time?” Pankenier replied, “No”. When I then tried to ascertain why he would publish such a figure, Pankenier responded, “I’m not quite sure why.”

2.3 Third Quotation

P07 begins as follows (first quoting my critique):

The above text is like the main text cited as recording a 'conjunction in 1576 BC,' and the proposals adduce this likeness as demonstrating that the two texts record like events. (Keenan, 2002: 64).

Keenan is confused. The 'text' he refers to is from *Mozi* 墨子 (4th century BCE) and relates the myth of the founding of the Xia dynasty. The likeness asserted in regard to this passage is between the accounts of the conjunctions of 1059 and 1953 BCE, *not* 1576 and 1953

The *Mozi* passage of relevance contains an account of the founding of three Dynasties: Xia, Shang, and Zhou. The passage is translated below (Watson, 1967: *Mo Tzu* 19; Keenan, 2002: 64). It begins as a question put to *Mozi* about engaging in warfare:

Now those rulers who delight in offensive warfare attempt to put a pleasing façade upon their doctrines and criticize *Mozi*, saying "Do you claim that offensive warfare is an unrighteous and unprofitable thing?—in ancient times Yu launched an expedition against the ruler of the Miao, Tang attacked Jie, and King Wu attacked Chou, and yet all three are regarded as sage kings; why is that?"

Mozi replied: You have failed to examine the terminology that I employ and do not understand the reasoning behind it. What those men did was not "attack" but "punish".

In ancient times the three Miao tribes were in great disorder and Heaven decreed their destruction. The sun came out at night and for three days it rained blood. A dragon appeared in the ancestral temple and dogs howled in the market place. Ice formed in summertime, the earth split open until springs gushed forth, the [cereal crops] grew differently, and the people were filled with a great terror. Kao Yang gave the command in the Dark Palace, and Yu [the Xia founder] ... grasped the jade staff of authority and set out to subdue the ruler of the Miao. Amidst the din of thunder and lightning, a spirit with the face of a man and the body of a bird came bearing a jade baton to wait upon Yu. The general of the Miao was felled by an arrow and the Miao army thrown into great confusion ... This is how Yu launched an expedition against the ruler of the Miao.

In the case of King Jie of Xia [the last king of the Xia], Heaven likewise sent down its direst command. Sun and moon failed to appear at the proper time, hot weather and cold mingled in confusion and [cereal crops] were seared and died. Spirits wailed throughout the land and cranes shrieked for more than ten nights. Heaven gave its command to Tang in the Biao Palace, ordering him to take over the solemn mandate from the Xia, for the Xia had fallen into grave disorder ... Only then did the Tang dare to lead forth his troops in obedience to the command ... After a while a spirit appeared and reported to Tang: "The virtue of the Xia is in great disorder; go and attack it, and I will surely cause you to win victory over it, for I have already received the command from Heaven." Then Heaven ordered Zhuyong to send down fire on the northwest corner of the city of Xia, and Tang, leading the army of Jie, conquered it. ... This is how Tang punished Jie.

In the case of King Chou of Shang [the last king of the Shang], Heaven would not sanction his power. His sacrifices were untimely; for ten days and ten nights it rained earth at Bo, and the nine cauldrons moved about. A woman turned into a man, flesh rained down from Heaven, and brambles grew on the state roads. A red bird holding in its beak a baton of jade alighted on the

altar of the Zhou state in the city of Ch'i and proclaimed, "Heaven orders King Wen of Zhou to attach Shang ..." Tai Dian journeyed to pay his respects to the Zhou ruler, the river cast up its chart, and the land brought forth the "riding-yellow" beast. King Wu ascended the throne and in a dream he saw three spirits who said to him this: "We have already drowned Chou of Shang in the power of wine; go and attack him, and we will surely cause you to win victory over him!" So King Wu went and attacked him, and replaced the state of Shang with that of Zhou, and Heaven presented King Wu with the yellow bird pennant ... This was how [King Wu] carried on the labours of Tang.

Thus, if we examine the cases of these three sage kings, we see that what they did was not to "attack" but to "punish".

(My critique quoted the third paragraph, on the founding of the Xia.) It is apparent that the three paragraphs on the foundings of the Xia, Shang, and Zhou have similarity and are intended to be considered together. Moreover, there seems to be roughly as much similarity between the paragraph on the founding of the Shang (fourth paragraph) and each of the paragraphs on the foundings of the other two Dynasties (third and fifth paragraphs) as there is between the paragraphs on the foundings of the other two Dynasties.

The conjunction of 1576 BC is the event that Pankenier's proposals associate with the passing of the Mandate of Heaven from the Xia to the Shang (i.e. the transition from one dynasty to the next). Compare the quote from P07 at the start of this section with the following from Pankenier (1983-1985: 176-178):

... the earliest mythicized versions of the Mandate conjunctions are found in *Mozi* ... I have suggested that this *Mozi* account ... derives from oral traditions ... and that couched in the mythical language in which they are written there is much valuable information bearing on archaic cosmological, astronomical, and religious conceptions of the Chinese. The most obvious example of this is of course the planetary conjunction of 1059 B.C. ...

Mozi's account in the same context of the founding of the Shang some five hundred years earlier follows a similar pattern ... Here, too, I would suggest that "Biao Palace" does not refer to a terrestrial edifice ... but to one of the constellations ... The untimely appearance of the sun and moon ... parallels the motive given for the overthrow of Shang ...

Mozi singles out three instances of the Mandate's conferral; namely, the founding of the "Three Dynasties"—Xia, Shang, and Zhou. The parallels between the latter two events have already been discussed, so let us now turn to the earliest historical precedent ... Here *Mozi* again reports seasonal dislocations ...

Thus, Pankenier compares the three *Mozi* accounts of what he proposes are descriptions of conjunctions and says that they are similar and have parallels. Moreover, the article from which these quotes are taken is entitled "Mozi and the dates of the Xia, Shang, and Zhou". This thus falsifies what P07 claims (as quoted at the start of this section).

Finally, Huang (1990), Keenan (2002), and others have argued that the interpretation of the three *Mozi* paragraphs as records of planetary conjunctions is impressionistic and not reliable enough to form the basis of a chronology. Really, I think that is clear.

2.4 Fourth Quotation

P07 begins by claiming “Keenan does not mention the series of five lunar eclipses in the Shang divination records ...” Compare that with what my critique said (Keenan, 2002: 67): “The Late Shang chronology was claimed to be verified by records of five lunar eclipses; those records, though ...” The claim of P07 is thus an invention.

P07 next faults my critique for (parenthetically) stating that the succession of the Zhou Dynasty “... is usually dated to 1200-1000 BC ...”, arguing that the dates given in the statement are inaccurate. In fact, I wrote the dates as round numbers, as should be obvious. Exactness was unneeded, and possibly distracting, because the purpose was merely to give some idea of the date for those readers without background knowledge of ancient China. P07 claims that the exact range is 1122-1027 BC, but that claim ignores some proposals. Other authors given different ranges; e.g. in 1997, an extensive review by the Sinological Institute of Beijing Normal University gave 1130-1018 BC (cited by Li, 2003: 482). It is difficult for me to state what the true exact range is, but I agree that it would have been better if my critique had stated the range as 1150-1000 BC. How important is this, given the context?

P07 next claims “... there is also the fact that even the supposedly unreliable *Bamboo Annals* chronology is only off by four years in dating the Zhou Conquest to 1050, and by only twelve years in dating the planetary conjunction to 1071 BCE.” This supposed ‘fact’ presupposes that the astro-historiographic date proposed by Pankenier is valid. The claim is thus circular. It also illustrates how Pankenier ‘amends’ the years in ancient texts in order to obtain his chronology; this point is discussed further in the next section.

P07 then discusses the texts that purportedly describe the five-planet conjunction (in 1059 BC) proposed to be linked with the transition to the Zhou Dynasty. Those texts described the conjunction as occurring in the astronomical lodge of *Fang*, whereas the conjunction actually occurred 120° away from *Fang* (Huang, 1990: 105-106; Keenan, 2002: 64). Pankenier (1995: n.17) ascribes that discrepancy to ‘portentological revisionism’. My critique points out, though, that such revisionism “... would seem to be at least as likely to affect the conjunction’s recorded historical timing as its location in the sky” (page 66). In response to that, P07 largely just repeats the arguments of Pankenier (1995). As my critique discusses, those arguments are plausible speculation, but they “... are not reliable enough to form the basis of a chronology.”

P07 further makes the following claim:

... as regards the record of the lunar eclipse, the source text comes from chapter 23, *Xiaokai* 小開, one of the ‘core’ chapters of the *Yi Zhou shu*, which date from the late 4th or early 3rd century BCE (Shaughnessy, 1993). It is impossible to come away from a reading of the discussion of this work in the authoritative bibliography *Early Chinese Texts* with the impression that the scholarly consensus is that the *Xiaokai* chapter “... is suspected of being fabricated.”

The statement being quoted from my critique (“... is suspected of being fabricated.”) has two references

(Keenan, 2002: n.44). One of those references is by E.L. Shaughnessy (whom P07 relies upon); here is what Shaughnessy (1991: 222-223) says: “... the record itself is somewhat suspect since the ‘Xiao kai’ chapter belongs to what I have elsewhere termed the ‘Jizhong’ stratum of the *Yi Zoushu*, which I have suggested may have been composed in the fourth century A.D.” The other reference is by N. Barnard (one of the most esteemed scholars of ancient Chinese texts); here is what Barnard (1993: n.17) says: “Chang Hsin-ch’eng’s survey of the accounts and critical analyses of the *Yi-Zhou-shu* results in the impression that it is, for the most part, a forgery, if not entirely so. Liang Ch’ich’ao, for instance, is of the opinion that ‘no less than eleven of the chapters are faked, while of the remainder, many have been tampered with or falsified; but it is not easy to determine which ones are authentic’”. P07 thus ignores the references in my critique and misrepresents Shaughnessy.

Additionally, even if the text were reliable, it is far from clear that the text records an eclipse on the specified day. This point was made by my critique (p. 67) and is ignored by P07.

The last claim in this section of P07 is that the *Bamboo Annals* record of a planetary conjunction in 1059 BC must be reliable because “... it would have been utterly impossible ... to retrospectively compute the location of an 11th century BCE conjunction of planets with sufficient accuracy to place it in the correct location in the sky.” As noted above, the location given in the *Bamboo Annals* is actually in error by 120°, which obviously greatly weakens the claim to be utterly impossible. Pankenier’s proposals argue that the error was due to portentological revisionism; they then give an interpretation of the bird bearing a jade baton (mentioned in the quoted ancient texts) to relocate the conjunction, but the interpretation is plainly impressionistic and less than certain.

2.5 Fifth and Sixth Quotations

P07 faults my critique for its discussion of lunar eclipses, saying the “... one crucial fact ...” that the critique does not mention is “... the relative date in the reign of King Wen of Zhou that prefaces the reference to the lunar eclipse [in the eclipse record].” The eclipse was indeed recorded as occurring during the 35th year of the king’s reign. P07 then argues: “Given year, month, and precise day, we can be a great deal more confident about the dating of this eclipse than if only the month and day had appeared.”

We are not, however, given a certain month and day; rather, there is some uncertainty in both, especially the month (Keenan, 2002: 67). Furthermore, we are not given an absolute (i.e. calendar) year, but rather a relative year; the argument of P07 appears to mix absolute and relative years.

P07 further claims “... if 1065 BCE was King Wen’s 35th year, then 1059 BCE, the year of the conjunction of the five planets, would have been King Wen’s 41st year, precisely the result referred to above which derives from completely independent historical records.” The “... result referred to above ...” is that the five-planet conjunction of 1059 BC occurred during Wen’s 41st year. The ‘result’ is unreferenced, but Pankenier (1995: n.10) claims the same result, citing Pankenier (1992: part 2). Pankenier (1992) generally argues for

dates by making numerous revisions to the ancient texts. Here are some selections to illustrate that:

... there is a sixteen-year error in the *Bamboo Annals* for the beginning of Di Xin's reign ...

... The events of [King Wu's] actual five years of rule ... have been redistributed among the seventeen years allotted to him in the *Bamboo Annals* ...

... this contradiction between *Yi Zhou shu* and reconstructed *Bamboo Annals* is the result of the same confusion ...

... these events could not really have taken place four years after ...

... reconciling the *Yi Zhou shu* record of King Wen's death in the 9th year with the contradictory account in [*Records of the Grand Historian*] which has Wen dying in the 7th year.

... allowing only for the commonest of copyist's errors (writing '23' for '13', and '3' for '1') ...

... [*Records of the Grand Historian*] states that King Wen died six years after attacking the Quan Yi barbarians ... it appears, therefore, that [the *Records of the Grand Historian* was incorrect] about the timing of that campaign ...

... the *Bamboo Annals* figures can be shown to be unreliable; for example, Di Xin is assigned fifty-two years, though we now know that he actually only reigned for forty years; Di Yi is assigned only nine years even though ... he ruled more than fifteen.

... the *Bamboo Annals* ... misplaced a reference to a Phoenix augury alluding to the planetary conjunction by entering it under the year of King Wen's ascension in Zhou many years before.

As the selections indicate, Pankenier ignores some texts and revises others. (The selections give only some examples; there are several more.) This is a game that allows matching the texts to almost any feasible chronology.

Additionally, the claim about King Wen's 41st year assumes that the transition from the Shang to the Zhou was synchronous with the five-planet conjunction of 1059 BC. That introduces some circularity into the argument. Furthermore, it requires revision of the *Bamboo Annals*. Indeed, Pankenier (1992) relies on a revision of the *Bamboo Annals* that was shown to be unsound (Barnard, 1993); this unsoundness was noted by my critique (page 65), but is ignored by P07.

The last fault claimed by P07 is the following:

In Keenan's own words in another context (Keenan, 2002: 66): "That an actual eclipse would match the record's date just by chance is very improbable."

The other context (page 66) concerns (*near-*)total solar eclipses. The discussion here concerns *partial lunar* eclipses. The error in the argument of P07 is plain.

3 CONCLUDING REMARKS

To summarise, P07 has no significant points that are valid. Moreover, Pankenier surely knows that many of the points raised by P07 are untrue. Additionally, it is noteworthy that the Chronology Project considered using planetary conjunctions for its work, but ultimately decided to reject this approach, because the records were considered too unreliable (see, for example, Liu, 2002b: 2 and Liu, 2002a: section 2).

In conclusion, the present work, together with that of Stephenson (2007), affirms my 2002 critique: astro-historiographic chronologies of early China are unfounded.

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6 APPENDIX 1: RADIOCARBON DATING

For a short review of radiocarbon dating, see <http://www.informath.org/Basic14C.pdf>. This appendix presents a few remarks on the radiocarbon dating done in the Xia-Shang-Zhou Chronology Project. These remarks are brief, and nowhere near a comprehensive review of the project's radiocarbon dating. They do, though, demonstrate that there are some problems.

There are two aspects to radiocarbon dating, for any sample. The first is to obtain an accurate measurement of the ^{14}C in the sample (this includes sample preparation). The second is to 'calibrate' that measurement to a calendar date. (For a discussion of measurement accuracy versus dating accuracy in radiocarbon, see Wiener (2007).) Regarding the measurements made for the Chronology Project, the project's radiocarbon laboratory invested much effort in trying to obtain accurate measurements (Liu et al., 2000); I make no further comment on that aspect. Regarding the calibrations of the measurements, there are potential problems.

One problem is that appropriate confidence intervals for the calibrated dates have not always been cited. The standard in radiocarbon studies, and indeed most sciences, is to give a 95% confidence interval for a quantity (in this case, a calibrated date). Not all publications of the Chronology Project have followed that standard. For example, Guo et al. (2001: Tables 1, 2) give only 68% confidence intervals for calibrated dates. Such intervals are unrealistically narrow.

Another example of the problem is in a summary of the Chronology Project that was written by the project's Director (Li, 2002). The summary claimed that a certain ^{14}C measurement from an important tomb "... gave a radiocarbon age of 2640 ± 50 BP, or 814-796 calibrated BC". The claim is incorrect: a date of 814-796 BC is obtained by calibrating 2640 BP and ignoring the measurement imprecision of ± 50 ; when the radiocarbon age is calibrated in the standard way, the 95% confidence interval for the date is much wider: about 917-756 BC. (This interval can be reduced via statistical sequence analysis—see below.)

Another potential problem is that during times when solar output was fluctuating rapidly, ^{14}C measurements taken at a latitude near 40 N cannot be accurately calibrated by the standard international calibration curves (Kromer et al., 2001). The problem is believed to become more serious for locations closer to the equator. Ancient China lay at roughly 35 N; so the problem would be expected to be at least as serious there as at 40 N, although the inaccuracies are not constant at a given latitude, but vary somewhat with location (details are not known).

The problem was only discovered after the Chronology Project was completed. The discoverers claimed that the inaccuracy resulting from using the standard calibration curves could be a few decades; later work showed that for some samples (especially short-lived samples whose carbon came primarily from winter-time growth), the inaccuracy could be as much as a century (Keenan, 2004)—at least during part of 850-750 BC, when solar output is known to have been fluctuating very rapidly.

The Chronology Project relied heavily on bone samples for its ^{14}C dating. It is unclear to what extent those samples would be affected by latitudinal effects. The main source of carbon in the bones is believed to be millet in the diet (Guo et al., 2001: 1112); so, much would depend on the planting and harvesting schedule (see Keenan (2004) for some discussion of this issue). A comparison of bone samples of known date, from 841-781 BC (Guo et al., 2001: Table 2), very strongly suggests that the problem, if it exists, is not large.

There might have been other times in the past when solar fluctuations led to inaccuracy, albeit usually not large. This is currently an area of research. Some recent work suggests a possible inaccuracy of half a century around 1600 BC (Manning et al., 2003: data; Wiener, 2007: Figure 1); this is based on only a single sample (from tree rings, at 40 N) though, and it remains to be replicated.

The existence of the latitudinal problem indicates that some radiocarbon dates from the Chronology Project should be reassessed. A related issue is that the project used sequences of ^{14}C ages, which were then statistically combined to give highly-precise calibrations (i.e. dates). That poses a difficulty for the ^{14}C -dating of samples even during times when solar output was nearly constant. As an example, the cemetery of the Marquises of Jin was the source an important sequence of samples for ^{14}C dating (Guo et al., 2001). Many of those samples' dates are known to be from 850-750 BC, and so they can probably not be calibrated as accurately as would otherwise be expected. The dates for samples from earlier in the sequence might not be directly affected by solar fluctuations; yet the statistical analysis of those earlier samples is affected by the ^{14}C ages of samples from later in the sequence. In this way, even samples from the time when China's chronology is known (after 841 BC) can affect earlier ^{14}C -derived dates.

To conclude, the dates derived from radiocarbon are unlikely to have the accuracy that they have often been portrayed as having.

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