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Volume 1

The Nivison Annals

Selected Works of David S. Nivison on Early Chinese
Chronology, Astronomy, and Historiography

Edited by
Adam C. Schwartz

DE GRUYTER
MOUTON

The publication of the series has been supported by the HKBU Jao Tsung-I Academy of Sinology – Amway Development Fund.

ISBN 978-1-5015-1454-8

e-ISBN (PDF) 978-1-5015-0539-3

e-ISBN (EPUB) 978-1-5015-0535-5

ISSN 2625-0616



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Library of Congress Control Number: 2018941017.

Bibliographic information published by the Deutsche Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at <http://dnb.dnb.de>.

© 2018 Nivison/JAS, published by Walter de Gruyter Inc., Boston/Berlin

Printing and binding: CPI books GmbH, Leck

☺ Printed on acid-free paper

Printed in Germany

www.degruyter.com

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Preface

Edward L. Shaughnessy
The University of Chicago

I first met David Nivison in December 1978, toward the very end of my first term in graduate school at Stanford University. Not unlike many prospective graduate students, at least in those days, I had not been in contact with David before applying to Stanford, nor did I really know what his current interests were. I knew of his book on Zhang Xuecheng 章學誠 and the book *Confucianism in Action* he had edited together with Arthur Wright, but these had been published many years before I had entered graduate school and were, in any event, not particularly of interest to me. During my first term at Stanford, David was on leave and not teaching (I think he was touring Europe). On his return to campus, I found him one afternoon in his office in the Philosophy department. He seemed unaware of any new students in the department of Asian Languages (the department to which I had been admitted), and the conversation began rather awkwardly, with several long and uncomfortable pauses. Finally, talking about a review of translations of the *Mencius* that he was then working on,¹ he mentioned that he was particularly impressed with D.C. Lau's solution to one passage in the "Teng Wen Gong Xia" 滕文公下 chapter, purportedly a quotation of the *Classic of Documents* (a portion of the quotation was incorporated into the *guwen* 古文 ["ancient text"] "Wu cheng" 武成 or "The Completion of War" chapter):

有攸不惟臣，東征，綏厥士女，匪厥玄黃，紹我周王見休，惟臣附于大邑周。

The state of Yu did not submit. The King went east to punish it, bringing peace to men and women. They put bundles of black and yellow silk into baskets, seeking the honour of an audience with the King of Chou, and declared themselves subjects of the great state of Chou.²

Whereas all other translators, and probably all traditional commentators as well, take the second character *you* 攸 "as the early archaic equivalent of the preverbal pronoun *suo* 所, Lau renders it as the name of a state, doing so, how-

1 The review was subsequently published as "On Translating Mencius," *Philosophy East and West* 30.1 (Jan., 1980), pp. 93–122.

2 *Mencius* 3B ("Teng Wen Gong Xia)/5; D.C. Lau, tr., *Mencius* (Harmondsworth: Penguin Books, 1970), pp. 110–111.

ever, without any indication of where he got the reading.³ In his conversation with me that day in December 1978, and also in the subsequently published review article, Nivison took some relish in pointing out that the reading “may very well be right, and impressive scholarship lies behind it.”⁴

The “impressive scholarship” to which David was referring derived from the occurrence of You 攸 as a place-name in Shang-dynasty oracle-bone inscriptions, mentioned in particular in the course of the campaign against the Renfang 人方 during the reign of the final Shang king Di Xin. As David noted in his published review, You must have been located in the Huai 淮 River valley “less than a hundred miles east-south-east of the capital of the Sung [i.e., Song 宋] state of Mencius’ day.” The tenor of my conversation with David that afternoon changed dramatically when I expressed some interest in oracle-bone inscriptions. I had never previously given any thought to such ancient inscriptions, but David’s enthusiasm encouraged me to pursue the conversation. He recounted to me that his interest in oracle-bone inscriptions had begun some seven or eight years before that when David N. Keightley had joined the faculty of the History department at the University of California at Berkeley. Questions about the grammar of Mencius’s Chinese prompted David to make his way across the San Francisco Bay to learn to read these inscriptions, and the two men struck up a friendship that endured for forty-five years and that did much to develop Early China Studies in the Bay Area and throughout the United States.

When I suggested to David that perhaps he would be willing to teach me how to read oracle-bone inscriptions, he agreed immediately and we arranged to meet informally the following term. I recruited a couple of other beginning graduate students, David Pankenier, also from the Asian Languages department, and Sun Long-kee 孫龍基, from the History department, to join us, and we met once a week throughout the winter, reading through *Xiaotun, di'erben: Yinxi wenzi Bingbian* 小屯·第二本：殷墟文字丙編. We began with the first plastron in that collection, and we probably spent several weeks on that plastron alone, reading very slowly, but with great attention to detail and with wide-ranging discussions. At the end of the term, both David Pankenier and I wrote essays in order to get credit for the course. My essay was a very immature attempt to blend paleography and mythology, inspired by the work of Marcel Granet (1884–1940). I recall that David Nivison was not much impressed with it.

3 Nivison, “On Translating Mencius,” 108, points out that Chen Mengjia 陳夢家, *Yinxu buci zongshu* 殷墟卜辭綜述 (Beijing: Kexue chubanshe, 1955), p. 306, had previously published this same interpretation.

4 Nivison, “On Translating Mencius, 108.

He returned it to me with the offhand remark that what I had done “was not entirely uninteresting.” In retrospect, I am sure that he was right to be dismissive of my effort, though I am glad to say that I persevered.

In the autumn of 1979, David offered a full-fledged course at Stanford to introduce students to oracle-bone and bronze inscriptions. There must have been a dozen students and auditors, including David Keightley, who drove down from Berkeley every week. It was toward the end of that term—“one Sunday night in November 1979,”⁵ as David himself remembered—when preparing a class on the Wei 微—family bronzes that had been discovered at Zhuangbai 莊白, Fufeng 扶風, Shaanxi just four years earlier and first published only in the previous year, that David drew a connection between date notations in bronze inscriptions and the *Zhushu jinian* 竹書紀年 or *Bamboo Annals*. One of the vessels, the *Xing xu* 癸盥, from this cache bore a fully-dated inscription:

隹四年二月既生霸戊戌

It was the fourth year, second month, after the growing brightness, *wuxu* (day 35).

It also mentioned a figure named Sima Gong 司馬共, and placed the action in a palace called the Shi Lu Gong 師录宮. David noted that this vessel can be compared with three other inscribed vessels—the *Shi Yu gui* 師餘簋, *Shi Chen ding* 師晨鼎, and *Jian gui* 諫簋—all of which are also fully-dated and all of which also mention Sima Gong and also set the action in the Shi Lu Gong. As he subsequently wrote, “These common features should create a strong presumption that all four belong to the same reign, and perhaps a short one, since the year numbers are all low.”⁶ However, David also soon discovered that the four date notations could not all be accommodated by a single regnal calendar. The *Shi Yu gui*, *Shi Chen ding* and *Jian gui* all seem to date to one calendar (which he subsequently identified as based on the year 867 BC as the first year of reign), while the *Xing xu* would only fit a calendar two years later. In his article “The Dates of Western Chou,” published in 1983, David said that this conundrum prompted him to turn to an unconventional source: “There is a book one is not supposed to use that now becomes useful—the (*Chin-pen*) *Chu-shu chi-nien* (今本) 竹書紀年, or “current” *Bamboo Annals* (hereafter *BA*) Everyone knows that it is an outrageous fake, perpetrated perhaps in the Ming dynasty, long after the original text had been lost. But it does have a complete set of dates for the Western Chou Dynasty. ... What kind of text could the *BA* really be, if it has a set of

5 David S. Nivison, *The Riddle of the Bamboo Annals* (Taipei: Airiti Press, 2009), p. 8.

6 David S. Nivison, “The Dates of Western Chou,” *Harvard Journal of Asiatic Studies* 43.2 (1983): 493.

Western Chou dates that are close to being correct? And what can then be deduced about how nearly correct those dates are?”⁷

I will not try in this Preface to recount the twists and turns that this discovery prompted in David’s scholarship over the next thirty-six years. Let it suffice to say that in his *The Riddle of the Bamboo Annals*, published in 2009, David said: “Within five minutes I realized that I was staring in disbelief at my major work for the rest of my life. ... The BA thus was not a fake but a priceless historical source. The seminar the next evening was exciting, and shaped the careers of two of the graduate students, Ed Shaughnessy and David W. Pankenier.”⁸ The seminar the next evening was surely exciting, more because of the bubbling enthusiasm of David Nivison himself than from any reaction by either me or David Pankenier. From then on, chronology, and especially Western Zhou chronology, became the focus of David’s work, and—as he said—with it also my work and that of David Pankenier. The following academic year David offered yet another graduate seminar, this time focusing exclusively on Western Zhou bronze inscriptions, and particularly fully-dated bronze inscriptions. There were now only two students remaining: myself and David Pankenier, though David Keightley continued to participate, this time by way of a primitive closed-circuit television transmission usually reserved for engineering lectures with hundreds of students. This seminar has passed into the legend of American Early China Studies. All three of the main participants—David Nivison, David Pankenier, and myself—produced papers that were subsequently published and which were seen to be quite influential: David Nivison’s “The Dates of Western Chou,” David Pankenier’s “Astronomical Dates in Shang and Western Chou,”⁹ and my own “New’ Evidence on the Zhou Conquest.”¹⁰ For his part, David Keightley continued to ask hard questions, questions that drove the other three of us not only to look for more and more evidence, but—perhaps more important—to look for the flaws in our reasoning.

For the next thirty-five years, David and I continued to look for the flaws in our own reasoning, or at least the flaws in each other’s reasoning. Some of our debates back and forth over the years have been public and well known to the small circle of scholars concerned with such questions as the chronology of

7 Nivison, “The Dates of Western Chou,” 496.

8 Nivison, *The Riddle of the Bamboo Annals*, p. 8.

9 David W. Pankenier, “Astronomical Dates in Shang and Western Zhou,” *Early China* 7 (1981–82): 2–37.

10 Edward L. Shaughnessy, “New’ Evidence on the Zhou Conquest,” *Early China* 6 (1980–81): 57–79.

ancient China. Others have been confined to a voluminous exchange, first of letters and then of e-mail, between us. The last message I had from David arrived on September 10, 2014, just five weeks before he died:

Dear Ed,

Thanks for your confirmation. I have just sent most of a book to Chen Zhi in Hong Kong. I must finish my work promptly. About ten days ago, I learned that the melanoma operated on in February has metastasized, and at 91 and much work to do, I would decline therapy even if there were any. (I'm not suffering yet.) I am sending you a description of the contents, and also two items in it that analyze the argument between us. Chen Zhi told me he hoped to publish a reply from you, maybe in the book itself. What I have written can be changed, if you can persuade me it ought to be. Look carefully at my notes on your EC 11–12 article.

Best wishes,

David (Wednesday evening)

I did not even try to persuade him to change anything, responding instead the next day saying (in part): “I think that it should be your book through and through, without any reply from me.” After David passed away Chen Zhi 陳致 prevailed upon me to write the present Preface to this posthumous collection of David's essays. Professor Chen has given me *carte blanche* to write as I wish. Nevertheless, I think this is still not the venue to explore in detail the differences between David and me, especially as they pertain to chronology and the *Bamboo Annals*. Rather, I propose to provide just some background to my relationship with David and some reflection on our methodological differences, in the hopes that this will provide some inkling of David Nivison the scholar and David Nivison the man (and, I suppose, some inkling of Ed Shaughnessy the scholar and Ed Shaughnessy the man). Some of this will inevitably be tinged with criticism; it was in the nature of our relationship.

One of the items attached to David's e-mail message of September 10 was the final essay to be written and included in the present volume. It is entitled “The Nivison–Shaughnessy Debate on the *Bamboo Annals* (*Zhushu jinian*)” (dated 21 August 2014). I suspect that many readers of this volume will turn to it first. The concluding paragraph suggests that we were never able to reconcile our differences.

Ed needs to count his costs. And he won't, because the cost of counting costs is to accept the principle that everything that could be relevant must be at least consistently explainable if not actually explained, and he won't do that, nor will he suffer anyone else trying it. Is this why he bristles at my offering him a brief note providing evidence for dating reigns in early Xia? And at my publishing a book daring to work out the changes in the chronology of Xia and Shang? These are things he just knows can't be done. So he asks, “How can Nivison be so wrong?” (p. 18)

The first paragraph says that the differences between us were philosophical, and therefore presumably were destined to be irreconcilable.

The conflict between us is actually quite interesting on a philosophical level. Ed (perhaps without realizing it) has a visceral commitment to a one-problem-at-a-time Baconian historical method, and has no patience with anything else. I am guided by “inference to the best explanation” of total evidence, by Collingwood’s concept of “rethinking,” and Popper’s strategy of discovery by trying to refute far-reaching theories. Ed can’t stand it, and can only see me as “getting ahead of my sources.” (p. 2)

I’m not at all sure that I would describe my historical method as “Baconian,” but I suppose it is true that I am more drawn to a “one-problem-at-a-time” approach than to some general theory that attempts to explain everything (or even a lot of it). David was certainly indebted to Collingwood, though the notion of “inference to the best explanation” probably owes more to David himself than it does to that mid-War British philosopher-historian.¹¹

David and I debated methodology over the years, both in print and in correspondence. He began the Preface to the last book he published during his life, *The Riddle of the Bamboo Annals* (2009), with the following lengthy reflection:

Professor Edward L. Shaughnessy opens his celebrated article “On the Authenticity of the *Bamboo Annals*” (*HJAS* 46, 1986) with a discussion of methodology, with my work in focus:

Nivison’s arguments for the authenticity of the data that he has utilized in one fashion or another in his chronological reconstruction are open to suspicions of circularity. His chronology must be correct for his interpretation of a multi-stage editorial process in the making of the *Bamboo Annals* to be correct, and the same is true, to some extent, in reverse. But, it is never acceptable methodology to prove one unknown with another unknown.

In fairness to both of us I should point out that Professor Shaughnessy is talking about work I did a long time ago. We have both come a long way since then. Further, his point is the need for as much hard data as possible in doing the sort of work we do. I readily agree that I need all the help I can get. But there is an idea in what I have just quoted that needs mending.

There is not just the “suspicion” of circularity in what I do. The circularity is there, and it is unavoidable. Typically, I assemble a mass of material, some of it well established data but perhaps of debatable relevance, some of it even more debatable hypothesis.

11 In anticipation of what was destined to be our last face-to-face meeting, about which I will say a bit more later in this Preface, David and I both re-read R.G. Collingwood’s *The Idea of History* and also his *Autobiography*, and I cannot deny that David found more to recommend in Collingwood’s historical method than did I.

Then, treating all of this as “given” (that is one point where the “circularity” comes in), I try to show that it fits together in a surprising and to me convincing way. Absolutely essential to this procedure, the massing of material must be fearless; everything, both what would favor the picture I am building and what would count against it, must be accounted for. There must be no “cherry-picking” of evidence. The aim is to end up with the best possible explanation of everything. Counter-evidence must be “explained away”; and if you can’t, you are wrong.¹²

One of the unresolved disagreements between David and me over the years, both in terms of history and philosophy, concerned my 1986 article “On the Authenticity of the *Bamboo Annals*,” in which I argued that a single strip of text had been misplaced from the annals of King Cheng to those of King Wu in the course of the late third-century CE editing of the tomb text of the *Bamboo Annals*. David initially accepted my argument. However, before long he began to argue that the strip must have been moved before the text was ever put into the tomb, and eventually came to refer to the strip in question as “Shaughnessy’s Slip” (included in the present volume), relishing the double entendre. Eventually, as in his book *The Riddle of the Bamboo Annals*, David came to argue that “actually there never was such a strip”; rather, there was some sort of notion of a strip that “tinkerers” could manipulate.

We must conclude, then, that the Cheng chronicle main text was *crudely altered in order to create the appearance of a strip that could be moved*, to make Wu Wang’s reign three years longer. Actually there never was such a strip, and no physical object was moved. All that was needed was *text*, of the right length having the right words. The tinkering with the Cheng Wang text was done simply to cover the tinkerers’ tracks, to create the possibility of a gap where (reporting to their king) they could claim there had been a strip, which they had then moved to its “proper” location in the Wu Wang chronicle.¹³

For the life of me, I cannot make any sense of this argument, despite having read various versions of it over and over again for almost thirty years. From antiquity down to the present day, editors have unintentionally misplaced bamboo strips from one part of a text to another. I took care in my article to show how and why the third-century CE editors of the *Bamboo Annals* would have been led to make such a mistake. David criticizes this focused argument as breaking up my research “into separate manageable parts and solving those parts separately” as opposed to his approach, which is to “seek a possible solution to all problems and a possible way all can be fitted together.”

¹² Nivison, *The Riddle of the Bamboo Annals*, p. 3.

¹³ Nivison, *The Riddle of the Bamboo Annals*, p. 118 (italics in original).

[T]he story he imagined was that this stretch of text was sufficiently disordered so that the Jin editors had the option of putting the strip back where it had come from, or putting it in the wrong place: the Wu Wang chronicle, making Wu Wang live three years longer. They chose the wrong place, influenced by the 3rd century historian Huangfu Mi.

Ed's story pays no attention to one part of the Cheng Wang context, the impossible dates for the death and subsequent rites for Zhou Gong (which I noticed right off). When I challenged him with this and finally got him to pay attention, his reply was (and is) that the Wei king could order his experts to make the text say whatever he wanted; so it makes no sense to suppose that he put together the complicated deception I propose. So the king didn't. Not seeing this, Ed says, is the real difference between us.

Very well, but what about the misdating of Zhou Gong's death, burial and *di* rite? Ed's defense of his story leaves that problem untouched. It is not worrying about that problem that reveals the real difference between us. Ed says in effect that you must break up your research into separate manageable parts and solve those parts separately. I say that you must seek a possible solution to all problems and a possible way all can be fitted together. (In doing this you may have to include in your story that some people told some lies, but you must be explicit about it and show that your assumption is plausible.) If you can't even imagine a way of doing this, you are almost certainly wrong, and may be wrong about almost everything. (pp. 15–16)

I may well “be wrong about almost everything,” but I am still not persuaded that I am wrong about the misplacement of this one strip.

According to David, our most important disagreement occurred in 1989. As David recounted in his response to my review of his *The Riddle of the Bamboo Annals*, early in that year he wrote a research note, and invited David Pankenier and Kevin D. Pang 龐飈鈞 to sign on as co-authors; only Kevin Pang accepted his invitation.¹⁴ In this note, he correlated a five-planet conjunction in 1953 BC, identified by Pankenier, with Pang's 1876 BC date for the celebrated Zhong Kang 中康 eclipse, and to these added a complete chronology of the Xia dynasty, incorporating two-year intervals between each and every reign. Nivison and

14 David S. Nivison and Kevin D. Pang, “Astronomical Evidence for the *Bamboo Annals*’ Chronicle of Early Xia,” *The Early China Forum*, *Early China* 15 (1990): 87–95. On p. 93, there is the following explanation for the systematic introduction of two-year intervals between reigns:

These interregnal periods in the *Annals* (not including one forty-year interregnum that has a purely political explanation) are from zero to four years, and out of seventeen Xia successions seven are just two years. Since the *Annals* chronicles have suffered various distortions in ancient editing, we can ask, may it be the case that originally (and in a sufficiently early stage of the text) all of the interregnums were exactly two years?

For David's recollection of the dispute between him and me, see his “Epilogue to *The Riddle of the Bamboo Annals*,” *Journal of Chinese Studies* 53 (July 2011): 5.

Pang concluded their note with the following statement: “Perhaps not only the Xia ‘Dynasty,’ but also Yu ‘the Great,’ and even the legendary ‘Sage Emperor’ Shun, are not myths (or not just myths), but in fact belong to precisely datable history.”¹⁵ David later recounted that at the time, I was the editor of *Early China*, and that “we almost had a public fight about this.” He claimed that because I myself was engaged in the study of early Chinese chronology, I could not serve as a disinterested editor of this work. My own sense was that as editor of *Early China*, I had an obligation to ensure that what was published in the journal met reasonable scholarly standards. I did not think—and still do not think—it was possible to derive an exact chronology of the Xia dynasty, and did not find the Nivison-Pang article convincing, primarily because of the ad hoc solution of embedding two-year intervals between each and every reign. After considerable back and forth, involving also the *Early China* editorial board, we agreed to publish the research note as a feature article of The Early China Forum, inviting also other specialists in the field to contribute their own evaluations. With contributions from Huang Yi-long 黃一農, John S. Major, David W. Pankenier, and Zhang Peiyu 張培瑜, and with individual responses by both David Nivison and Kevin Pang, the original 9-page research note ballooned to 110 pages. I am still not convinced that very much light was shed on the question of Xia chronology. Huang Yi-long, David Pankenier and Zhang Peiyu essentially used their pages to ride their own hobby-horses, attacking each other as much as they attacked Nivison and Pang. John Major did take the Nivison-Pang study seriously, concluding “It now seems more reasonable to affirm than to deny that the Xia dynasty was indeed founded at or near the time of the five-planet conjunction of 1953 B.C., and that Xia officials accurately recorded a solar eclipse in the year Zhong Kang 5.”¹⁶ However, he too rejected the way that Nivison and Pang had arrived at their chronology:

Nevertheless, it seems to me that the “correction” of the interregnums to a uniform two years apiece has more the quality of an arbitrary expedient than of a soundly reasoned procedure. ... The “correction” proposed seems, in the end, uncomfortably like a mere contrivance designed to accomplish the desired end, namely the reduction of the eighty-one-year span to a seventy-seven-year span.”¹⁷

15 Nivison and Pang, “Astronomical Evidence for the *Bamboo Annals*’ Chronicle of Early Xia,” 95 (the wording here is clearly that of David Nivison).

16 John S. Major, “Forum: John S. Major,” *Early China* 15 (1990): 116.

17 Major, “Forum: John S. Major,” 114.

For his own part, David Nivison opened his response in the Forum section of *Early China* with an epigraph quoting David Hume: "... no testimony is sufficient to establish a miracle, unless the testimony be of such a kind, that its falsehood would be more miraculous than the fact which it endeavors to establish."¹⁸ I did not think—and do not think—that it was the job of *Early China* “to establish a miracle.”

There is no point revisiting each and every point that David and I debated over more than thirty years. We sometimes even agreed (or at least came close to doing so), as when we jointly authored a paper on the date of *Jin Hou Su bianzhong* 晉侯蘇編鐘 and the chronology of the early lords of the state of Jin 晉.¹⁹ But for the most part we disagreed. In January 2011, I published in the *Journal of Chinese Studies* a review of David’s *The Riddle of the Bamboo Annals*, which had been published three years before that.²⁰ Even though David had asked me to write an in-depth review of his book, and surely knew that any review I would write would be critical, still he was stung by my criticism. He returned to the same quotation from my 1986 article “On the Authenticity of the *Bamboo Annals*” regarding circularity in his methodology quoted above (p. xiv), with an extended critique of my own methodology:

In my book (i.e., *The Riddle of the Bamboo Annals*, pp. 3–5) I replied that in the arguments to which he objected I was fitting together logically various items having low initial probability, and that it was in the *coherence* of the whole structure (and the virtual impossibility of that coherence being accidental) that had proof value, provided that some elements were tied down empirically. But let me now focus attention directly on Shaughnessy’s review. He objects that irregular breaks between Xia reigns seem more reasonable to him than the regular two-year breaks that I propose. His intuitions are relevant only in revealing that he doesn’t see what is going on: my argument structure is *hypothesis* followed by *confirmation*, and the two-year interregnums are part of my *hypothesis*.

Where, then, is the circularity that Shaughnessy saw as invalidating my work, two “unknowns” proving each other, the editorial process and the claimed true dates? I do *conclude* that I have proved them; but I begin by offering them as *hypothesis*. Each *must* assume the other; otherwise my hypothesis would be inconsistent, and therefore false be-

18 David S. Nivison, “Response: David S. Nivison,” *Early China* 15 (1990): 151.

19 David S. Nivison and Edward L. Shaughnessy, “The Jin Hou Su Bells Inscription and Its Implications for the Chronology of Early China,” *Early China* 25 (2001): 29–48; Ni Dewei 倪德衛 (David S. Nivison) and Xia Hanyi 夏含夷 (Edward L. Shaughnessy), “Jin Hou de shixi ji qi dui Zhongguo gudai jinian de yiyi” 晉侯的世系及其對中國古代紀年的意義, *Zhongguo shi yanjiu* 中國史研究 2001.1: 3–10.

20 Edward L. Shaughnessy, “Of Riddles and Recoveries: The *Bamboo Annals*, Ancient Chronology, and the Work of David S. Nivison,” *Journal of Chinese Studies* 52 (January 2011): 269–90.

fore I had gone any farther. Shaughnessy has simply confused the *consistency* required in my hypothesis with a supposed *circularity* invalidating my whole argument.

At the end, Shaughnessy repeats his praise for my two-*yuan* theory, and tells everyone how good my first article on the chronology of Zhou was—actually it contains many naïve errors, though I did get some important things right. (These include the two-*yuan* idea and the four-quarters interpretation of lunar phase terms, both of which Shaughnessy accepts.) Then, having built up some credit, he allows himself to criticize me “harshly,” assuming that he has destroyed my later work with his argument about the supposed transposed half-strip and his charge of circularity. So he says, “How is it that Nivison has been able to do so much, and yet still be so wrong?” (Review, p. 289) With this he grants himself the status of historical sage: he is “quite sure” of this, “quite sure” of that, does “not believe” this, does “not believe” that, condemning my entire pre-Zhou chronology (with no criticism of a single detail of it, his only argument being that it must be wrong because I worked it out “[as] part of a complete system based on [my] reconstruction of the *Bamboo Annals*.”²¹

To the statement that I am “‘quite sure’ of this, ‘quite sure’ of that,” David adds the following note:

Shaughnessy insists that *I* am too sure of myself. I am too amused by this to be annoyed.²²

When this response to my review was published, David sent the offprint to me inscribed: “Dear Ed: It’s your move! Best of luck, David.” We continued to write back and forth, and I even dedicated my last book to David (as well as to David Keightley and Michael Loewe), and made a special trip to Stanford in early April 2014 to deliver it to him personally. We spent a long Sunday evening in his home office debating “this” and “that,” and basically agreeing to disagree. It was great fun.

I am sure that some will find the tenor of this Preface unusual, perhaps even unseemly. David can no longer continue the debate, and so it is unfair of me to have the final word. But anyone who reads the book will notice that I by no means have the final word. That belongs very much to David, with his “The Nivison-Shaughnessy Debate” coming toward the end of the book. I know full well that David took great relish in this debate, and was hoping that it might continue for at least a few more years. While David is no more, through the good offices of Professor Chen Zhi and the Hong Kong Baptist University Jao Tsung-I Academy of Sinology, the debate can continue for at least some readers. I invite the reader to read this last essay that he continued to work on until the last

²¹ David S. Nivison, “Epilogue to *The Riddle of the Bamboo Annals*,” *Journal of Chinese Studies* 53 (July 2011): 14–15.

²² Nivison, “Epilogue to *The Riddle of the Bamboo Annals*,” 15 n. 26 (italics in original).

weeks of his life. Whether one sides with me or with David, or thinks that both of us are wrong, I hope the reader will come to appreciate the great intellectual curiosity that drove David throughout. He lived a rich life, and contributed mightily to the study of ancient China. His contributions to my own development as a scholar have been immeasurable. I can't say that I didn't occasionally find his work frustrating, but I know that I will miss debating with him.

Let me give the final word of this Preface not to David, but to his son Jim Nivison. Just four days after David passed away, Jim sent me the following e-mail message.²³

Dear Ed,

My father passed away on Thursday of last week. He was 91 years old; he lived a rich life. His desire was to be able to work as long as possible and he was able to do so up until the last three weeks of his life.

I don't believe that we ever met, but I have heard your name for many years. I take it from afar that the two of you had at times a complex relationship as friends, colleagues, collaborators and to a certain degree, academic adversaries. To hear my father talk, one of the most formative experiences of his life was being on the debate team at the Gardiner, Maine high school. He loved to try to put together a convincing argument then and that never left him. I gather that the two of you had your ongoing debates and it's my observation that it is just this kind of debate that makes the juices flow that keeps an old man young and adds years to a life. I know that was the case with my father. I'm glad and envious that my father had many friends and colleagues like you that kept him in the game for so many years.

Best regards,
Jim Nivison

²³ E-mail message, James Nivison to Edward L. Shaughnessy, October 20, 2014; used with permission.



Gina Shaughnessy, David S. Nivison, Edward L. Shaughnessy, Zhu Dexi 朱德熙, and Richard Kunst; February 1981, San Francisco



Jeffrey Riegel, Edward L. Shaughnessy, David N. Keightley, David S. Nivison, and Dongfang Shao; 6 April 2006, San Francisco



David S. Nivison and Edward L. Shaughnessy; 24 May 2009, Stanford University

1 The *He zun* Inscription and the Beginning of Zhou

About six months ago, I made a discovery that pointed toward a solution to what has been the classic problem in ancient Chinese historical studies for 2000 years—the reconstruction of Western Zhou chronology prior to 841. In brief, I noticed that the chronology in the so-called “forged” *Zhushu jinian* is really a distortion of the true dates—a distortion that can be reversed with careful analysis and with the help of bronze inscriptions. (I summarize results of this theory in an attached chart with brief explanations, “Probable Derivation of the *Zhushu jinian* Chronology for Western Zhou.”) Quite recently, turning my attention specifically to the *He zun* 鬲尊, I discovered that this one inscription provides the key to straightening out the sequence of events of the crucial first decade of the Zhou Dynasty, making it possible to pinpoint with near certainty the year, month and day of the Zhou Conquest.

I

The *He zun* inscription (Figure 1) bears the date “fourth month, day *bingxu* (23)... the king’s fifth ritual cycle.” It gives an account of an address by the king to “young nobles of the royal house” (*zong xiao zi*) given “when the king first moved his residence to Cheng Zhou” (i.e., Luoyang). To evaluate all of this, we must begin by making a tentative first approximation of the absolute date.

1. Sima Qian, in the “Lu Zhou Gong Shijia” chapter of the *Shiji*, gives reign lengths for the dukes of Lu, back to but not including Bo Qin, who was the first duke and the eldest son of Zhou Gong. These reign lengths imply that Bo Qin died in 999.
2. The “Bi Ming” chapter of the *Shang shu* has King Kang giving an appointment to the aged Bi Gong—possibly calling him from retirement—to be vice-roy in the East. This is a *guwen* chapter, and so one must be cautious. It contains a date, however, which is found also in the *Han shu* “Lü-li zhi”: “12th year, 6th month, *gengwu* (7) *fei* (the 3rd, new moon day).” If we suppose Bi Gong was being given appointments Bo Qin had filled, 999 was King Kang’s

Paper presented at the annual meeting of the American Oriental Society, San Francisco, CA, April 16, 1980; revised April 26, 1980

12th year, and his first year was 1010. The very precise month and day date in the “Bi Ming” does fit 999 exactly, if one makes a simple change of 5th month long, 6th month short, instead of the reverse.

3. The “new” *Zhushu jinian* dates the deaths of Lu dukes (after Bo Qin) through Wei Gong consistently 7 years later than the *Shiji* does, probably for this reason: The *Han shu* “Lü-li zhi”—arguing for a different Zhou chronology—says Bo Qin became duke “in the 1st year of Cheng Wang,” reigned 46 years, and died “in the 16th year of Kang Wang.” But what was the first year of Cheng? In the *Zhushu* system, 1044, which would make 999 the year of death. But arguably it could be the first year of Cheng’s personal rule, supposed to be seven years later. This would give 992 as Bo Qin’s last year, and 992 is the 16th year of Kang in the “new” *Jinian*. So this was evidently the date the *Jinian* editors intended to give. But what we actually find in the *Jinian* is not “16th year” but “19th year”—though this is inconsistent with the next dated entry in the *Jinian*, as Wang Guowei shows. This appears to be unfinished editing, and we can exploit it: 992 would be the 19th year of a reign that began in 1010. When the editors shifted the date of Kang’s succession from 1010 to 1007, they must have neglected to make a compensating change from “19th year” to “16th year” for the death of Bo Qin.
4. More confirmation of the 1010 date is possible, but this will suffice for tentative use of it. Taking Liu Xin’s reign lengths, 7 years for Zhou Gong’s regency and 30 years for Cheng Wang’s reign thereafter, one would suppose the dates are 1047–1041 and 1040–1011. The date of the *He zun* should therefore be either 1043 or 1036, depending on what is meant by “the king’s 5th ritual cycle.” If the former, the king’s address is on the 3rd, if the latter it is on the 13th. Either is possible (one should expect the event to be near the first or near the middle of the month). I assume 1040 was the year Cheng became of age (20 *sui*); so in the first case he would be 17 *sui*—not too likely, but possible. In the second case he would be 24 *sui*, obviously more probable.

II

But this traditional “7 plus 30” scheme is mistaken. There are two immediate objections:

1. The “Shao gao” and “Luo gao” chapters of the *Shang shu* narrate events, with precise month and day dates, that have to fall in the last year of the regency—which would be 1041. In particular, the king performs a sacrifice in the “Luo gao” on the last day of the year, identified as day (5) in the 60-day cycle. But for 1041 the dates throughout are one day off, making the sacri-

fice fall on the first day of 1040. The fact that the dates are this close shows that this is a first approximation. But a correction is required.

2. The *He zun* says that “the king’s 5th ritual cycle” was the year a royal residence was first established in Cheng Zhou (Luo). So, the king’s 5th year, in some sense, should be the year of the construction of the royal accommodations in the new city as described in the “Luo gao” which closes with the date, “7th year of Zhou Gong’s regency.” But also, the *Shang shu Dazhuan* says that Zhou Gong in his 5th year “built Cheng Zhou.” And so, this same year should be in some sense Zhou Gong’s 5th year also. These apparently conflicting accounts require reinterpretation or revision consistent with the *He zun*.

Previous solutions to this 5th year–7th year puzzle preserve the traditional assumption that Zhou Gong properly transferred power to the king when the king became an adult. To the contrary, the right account of events I think is as follows:

- 1057 King Wen dies (after “receiving the Mandate” by divination).
- 1056 King Wu’s first year as King of Zhou.
- 1048 The meeting of King Wu with his allies at Mengjin. This was perhaps a preliminary campaign that was not pressed, or did not succeed, the reason (or cause) being, as the Zhou saw it, that the position of Jupiter (*sui xing*) was not yet favorable (i.e., Heaven’s “command” was not yet effective).
- 1045 The final campaign against Shang is under way, and achieves a decisive victory at Muye outside the Great City Shang on the day *jiazi* (1) in the 2nd month.
- 1044 King Cheng succeeds, as a minor (16 *sui*), with Zhou Gong as his regent.
- 1043 Mourning for King Wu completed.
- 1042 Post-mourning “1st year” of King Cheng and of the Regency.
- 1040 King Cheng attains majority, 20 *sui*, entitling him to take up personal rule. But a rebellion is brewing. Zhou Gong fears the young king, his nephew, cannot handle it, and retains ultimate power (informally, and illegally). He overcomes Shao Gong’s objections and obtains his collaboration; the two gradually share power with the king. Lu, Zhou Gong’s fief, is assigned to his eldest son Bo Qin. Steps are taken against the two (three?) rebelling royal uncles.

- 1039 The revived Shang power is attacked and destroyed, and the Shang ruler Wu Geng killed.
- 1038 The campaign is extended against the Yan and other peoples who had joined the revolt. King Cheng himself —now 22 *sui*—leads this campaign.
- 1037 Shang territories are reassigned, and conquered populations relocated. Zhou Gong’s younger brother Feng is enfeoffed in Wei. Ceremonies take place in Cheng Chou (Luo), where construction of a new capital is already going on.
- 1036 Construction at Cheng Zhou continues, proceeding to the “royal city” on the north bank, now under King Cheng’s direction. King Cheng gives the *He zun* address to the “young nobles” while staying in the *jing* (military *principium*) in the 4th month. Rites at the end of the year complete the transfer of power and terminate the *de facto* “regency.” Zhou Gong is retained temporarily as viceroy in the East.
- 1033 Zhou Gong retires, and is replaced as viceroy in the East by his younger son Jun Chen (Ming Gong). (Probably later Jun Chen predeceases his elder brother Bo Qin and is replaced by him in this position. This would explain the appointment of Bi Gong as viceroy in the East occurring exactly in 999, the year of Bo Qin’s death.)

If these are the facts, it is quite possible that the early Han scholars actually had these dates and found it impossible to believe them—for:

- (a) A Zhou Gong myth had by this time developed so far that people couldn’t believe Zhou Gong had done something improper. They were therefore obliged to understand the regency—which the “Luo gao” explicitly says lasted seven years—as terminating at the end of the year before King Cheng’s majority, whatever that year was.
- (b) They trusted their retrospective astronomical calculations to establish a correct chronology. But their science was in error —
 - (i) generating a three-day error in calculating lunations nine centuries back (so that, e.g., a month actually beginning on day (8) would seem to them to begin on day (5)); and
 - (ii) generating approximately a six-year error at that remove for the position of Jupiter.

The result of trying to satisfy an impossible criterion of moral-historical judgment and using a fault science was that *all* the dates came unstuck, not just the dating of the regency. One scheme after another was tried; one error required another, and another, until Zhou chronology down to 841 was a shambles.

The *He zun* points the way out, because we cannot make sense of it unless we recognize the initial mistake.

III

Once we are back on track, the going is obviously easier. Notice the seemingly unrelated problems that are now solved.

1. The 5th year–7th year puzzle: They are the same year, 1036, which is (a) the seventh year of the regency, regular and irregular, counting from 1042; (b) the fifth year of Zhou Gong’s seven-year “regency” as described in the *Shang shu Dazhuan* counting from 1040; and (c) the fifth year of King Cheng’s legitimate personal reign, counting from the year of his majority, 1040.
2. Zhou Gong’s protest of self-justification to Shao Gong, as recorded by Sima Qian in the “Lu Shijia”: If Cheng was only a boy, there obviously had to be a regent; why should Zhou Gong have had to justify himself for being one? But if Cheng had attained manhood and Zhou Gong should have stepped down but did not, there is an obvious explanation. (Probably the “Jun Shi” chapter of the *Shu*, whether or not an actual document, should be referred to the same episode and relocated before the “Da gao,” which also belongs to 1040.)
3. The puzzling paragraph at the beginning of the “Kang gao” about construction going on at Luo: Scholars have wanted to relocate it to the beginning of the “Luo gao.” But we now see that the investiture of Feng as marquis of Wei occurred in the year immediately before the “Shao gao”—“Luo gao” events. Construction must already have been under way. Probably the latter chapters deal only with the construction of the “royal city” component of Zheng Zhou.
4. The active role of King Cheng during the later part of the regency:
 - (i) The *Shu* Preface lists a lost chapter placed after the end of the regency, describing a campaign led by King Cheng, that “destroyed the Yan”; but in the *Shang shu Dazhuan* the destruction of the Yan is the activity of the third year of the Zhou Gong regency. These are probably the same event—no longer a difficulty, if King Cheng was actually 22 *sui* at the time, not 17.

- (ii) Sima Qian (“Zhou benji”) first says that Zhou Gong ruled for seven years and then returned the government to the king after he attained manhood, and *then* he says that “King Cheng, when in Feng, sent Shao Gong again to plan the city of Luo, in accord with King Wu’s intentions.” The implied sequence of events conflicts with both the *Shu* and the *Dazhuan* accounts, which assign the construction, of which Shao Gong’s activity in the 2nd and 3rd months is a part, to the 7th or 5th year of the regency. The *He zun*, in addition to confirming that building a new capital in the East was King Wu’s intention, implies that King Cheng himself was in charge of this (having him on the scene himself the month after Shao Gong). But this ceases to be a problem if Cheng was not 19 *sui* but 24 *sui* at the time.
5. The historicity of the Regency itself: On my hypothesis, Zhou Gong was legally regent from 1044 through 1041, and retained extraordinary powers, which were probably only partly formalized, from 1040 through 1036, in these last five years deliberately behaving as a minister, giving the king greater prominence. Inscriptions do register this situation, if we read them carefully. (E.g., the *Xiaochen Dan zhi* 小臣單觶, probably reflecting events of 1039: the king himself is nominally leading the campaign; but Zhou Gong has the role of giving out rewards, normally a royal power, and as important as the power to punish.)

IV

But this hypothesis does not rest solely on its explanatory power. There are dates, in literary texts and in inscriptions:

1. The *Xiao Yu ding* has the date “25th cult-year,” “8th month, 3rd quarter, lunation (*chen*) on day (21).” Contrary to established opinions, I believe it to be a Cheng Wang inscription. The *yuan* it uses is 1044 and the year is 1020—the year, according to the *Zhushu jinian*, of a great assembly of vassals and delegations of border peoples in Cheng Zhou. The ceremonies must have had to some extent a “Roman triumph” aspect, which the *Xiao Yu ding* records. There is a *liao* victory sacrifice recorded for the day, which should fall on the full moon; and the date is exactly the 16th. (Long and short months require a small alteration: instead of ... 6(6), 7(36), 8(5), 9(35), 10(4), *run* (34), 11(4)..., read 6(6), 7(36), 8(6), 9(35), 10(5), *run* (34), 11(4)..., for the first days of the months. It is mathematically possible to assign the inscription to 984, if one uses the post-mourning *yuan* 1008 for Kang Wang’s reign and assumes that Zhao Wang is still continuing Kang Wang’s calendar until the

completion of mourning at the end of 983. But the inscription appears to refer to the Conquest as a relatively recent event. See my “Probable Derivation” chart, attached.)

2. If Zhou Gong retired three years after the end of the regency, as the *Tongjian waiji* 通鑑外集 and the “new” *Jinian* say, there would need to be a new appointment as viceroy in the East in 1033. The *Shu* Preface says this went to Zhou Gong’s younger son Jun Chen, who is persuasively identified as the Ming Gong or Ming Bao of various inscriptions by Chen Mengjia, Shirakawa and others. The *Ling yi* 令彝 has no year date, but it opens with record of the appointment of “Zhou Gong’s son Ming Bao” to very broad powers over “the three ministries and the four quarters.” Such an appointment would be made at the primary court session on the first of the month, the “great audience of the dark lutation” (*mei chen ... da fu*) of the *Da Yu ding*. The date of the *Ling yi* is “8th month, syzygy on day (21).” And day (21) is exactly the first day of the eighth month of 1033, taken as a *hai* year. (It is the 2nd, if the year is *chou*.)
3. Going back three years to 1036, the last year of the *de facto* regency, if we take this too as a *hai* year we find that the very precise dates in the “Shao gao” and “Luo gao” chapters now fit exactly. (1036 is just five years later than the first approximation 1041, which was one day off. The month–day correlations are repeated, with a one-day difference, in five-year cycles.)
4. I have identified 1044 as a *yuan* year on the strength of the “Zuo Luo” in the *Yi Zhou shu*, which as I read it says King Wu died at the end of the year of the *jiazi* victory. The *Shi Dan ding* 師旦鼎 inscription survives from the Song era; the vessel does not, and there is much argument about it. I myself suspect it is a middle Zhou fake based on a line in a chronicle. It opens, “In the first year, 8th month, day *dinghai* (24), Shi Dan (i.e., Zhou Gong) received an appointment.” (In a chronicle, this laconic statement would be the way of recording that he had been made first minister.) The first day of the eighth month of 1044 (*zi* year with *run* month or *chou* year) was exactly day (24).

The *guwen* “Wu Cheng” chapter of the *Shu* as quoted in the *Han shu* “Lü-li zhi” gives dates subject to close constraints for the first, second and fourth months of the Conquest year (the year of the *jiazi* victory). Early Han scholars, with their three-day error, would have found that the “Wu Cheng” dates seemed to fit the year 1051—marvelously, just 500 years before the supposed birthdate of Confucius and just 1000 years before the Shiqu Pavilion Conference of Scholars, convened by the Emperor Xuan-di in 51 BC (so, the reigning sovereign must obviously be a 500-year sage-ruler....). And 1051 is one of the two dates given for the

Conquest in the “new” (supposedly fake) *Zhushu jinian*. But we do not need to suppose—as I did, for some months of my research—that the “Wu Cheng” dates were faked for the Conference. They were simply misinterpreted. Closely analyzed, they fit very well into the year 1045. Further, the date of the *liao* victory sacrifice in the fourth month turns out to be (as in the *Xiao Yu ding*) exactly the full moon—the 16th.

V

I turn now to astrology. I take no stock in it myself. But as a historian I am mindful that what people have believed must be carefully checked.

1. The “Zhou Yu” section of the *Guo Yu* says the Conquest occurred in the year when Jupiter, the “year star” (*sui xing*) was in the zodiac station Chun Huo, “quail-fire” (station 8).
2. Sima Qian implies the same tradition. The attack on Shang, he writes (“Zhou benji”), was not pressed on the occasion of the Mengjin assembly because King Wu insisted it was not Heaven’s command that it be done yet. Qian records an omen: fire descends, hovers over the king’s lodging and turns into a bird, red in color (the color of the Zhou), with a rising cry. Evidently the victory was to occur in the year of bird-fire.

Liu Xin, of course, a century later, chose his date 1122 partly because his faulty calculations identified that year as a Chun Huo year. But perhaps the Conquest year *was* a Chun Huo year. Perhaps, further, Wu actually waited for that year to attack. (After all, he waited for *jiazi*, day (1), to attack, as the *Li gui* 利簋 verifies.)

I am indebted to Zhou Fagao for two bits of data, and to William Hung for two more:

- (i) The *Zuo zhuan*, for 545 BC, Duke Xiang, 28th year, says that Jupiter was then in Xingji, station 1.
- (ii) The correct period of Jupiter is 11.8565 years through the twelve stations.
- (iii) The *Zuo zhuan* statement above is a Han interpolation, using Liu Xin’s *San-tongli* 三统曆 calendar.
- (iv) As Shinjō Shinzō has shown, using P. V. Neugebauer’s tables, in 545 Jupiter was actually in station 10, Shou Xing.

Therefore, Jupiter was actually in Chun Huo, station 8, two years earlier, in 547.

The factor we need is 42:

$$42 \times 011.8565 = 497.9730 \approx 498$$

$$498 + 547 = 1045$$

Jiazi in the second month of 1045 was either Jan. 15 or March 15 in our calendar; and as the *Li gui* confirms, the battle of Muye was fought in the morning.

The *He zun* Inscription and the Beginning of Zhou: Notes

I.

(23): Here and elsewhere I use () to indicate a day denoted by its number in the 60-day cycle.

II.

The *Shang shu Dazhuan* 尚書大傳 text is found in ch. 2, comments on “Luo gao” 洛誥 (p. 101 in the edition in *Congshu jicheng* 叢書集成 3569, in which ch. 2 is pp. 55–114). It actually accounts for seven years of regency, and so apparently conforms to the tradition:

周公攝政，一年救亂，二年克殷，三年踐奄，四年建侯衛，五年營成周，六年制禮作樂，七年致政成王。(Zhou Gong she zheng, yi nian jiu luan, er nian ke Yin, san nian jian Yan, si nian jian hou Wei, wu nian ying Cheng Zhou, liu nian zhi li zuo yue, qi nian zhi zheng Cheng Wang).

The activities of the sixth year above may be what Sima Qian refers to in the “Zhou benji” after explaining the provenance of the “Zhou Guan” chapter of the *Shu*: *xingzheng li yue, zhidu yu shi gai* 興正禮樂，制度於是改; but in his account this appears to be Cheng’s work. I assume the authors of the *Da zhuan* account of the regency are saving the 7-year tradition by interpreting Zhou Gong’s hold-over appointment as viceroy in the East, 1035–34, as part of the “regency.” Other treatments of this problem are found in Wang Guowei 王國維, *Guantang bieji* 觀堂別集, ch. 1, “Zhou kaiguo nianbiao” 周開國年表, and in *Wenwu* 1976.1 articles on the *He zun* by Tang Lan 唐蘭, Ma Chengyuan 馬承源, and Zhang Zhenglang 張政娘.

III. Major Events

1057, 1056, 1048: Here I am adapting what seem to me the most probable interpretation of the *Shiji* “Zhou benji” account. Others disagree, making Wu’s first year later, and taking year identifications in the *Shiji* and the *Shu* as dating from King Wen’s “receiving the Mandate.”

1045: There is a problem when the final campaign began, and whether the *jiazi* victory was in the 11th or 12th year. I am following the *Shiji* on the second matter (rather than the *Shu* Preface), and am avoiding the first problem here.

1044: See *Yi Zhou shu* 逸周書 48, “Zuo Luo” 作雒: *Wang ji gui, nai sui shi er yue beng Hao* 王既歸, 乃歲十二月崩鎬 “After the king had returned, in the 12th month of this year he died in Hao.” It follows that 1044 is Cheng Wang’s first year. But the “Luo gao” of the *Shang shu*, which must be dated to 1036, identifies this latter year as “the 7th year of Zhou Gong’s Great Upholding of the Mandate received by Wen and Wu”—so the year-count for this seven-year “regency” must start in 1042. I suspect that the institution evident from inscriptions later was already operating: The new king’s calendar was not started until after the completion of the mourning for his father—though retrospectively the king’s actual first year (1044, in this case) as well as his post-mourning *yuan* year (1042) could be used in dating. This would not, perhaps, have been understood by the writers of two very late pre-Qin *Shang shu* chapters, the “Hong Fan” and the “Jin Teng” (both of them are literary elaborations of legends); and so at the beginning of these we find the first and second years after the Conquest taken as years when Wu Wang is still living.

1040–1036: I base the order of major events on the *Shang shu Dazhuan* account.

1036: The “Royal City” (*wang cheng* 王城) appears to have been a city separate and some distance from Cheng Zhou (Luoyang) in the Chunqiu period, but some believe there was a part of Cheng Zhou so-called in the founding period. (See Gotō Shimpei 後藤均平, “Seishū to Ōjō” 成周と王城, pp. 399–410 in *Tōyōshi Ronsō* 東洋史論叢 (presented to Wada Sei 和田清 in honor of his 70th birthday), Tokyo, 1960.) Since the *He zun* clearly identifies Cheng Zhou itself as a royal residence, there must have been a “royal” component of it.

IV.

1. The *Xiao Yu ding* 小盂鼎 is always assigned to Kang or later (by those who accept it as genuine), because it appears to speak of a *di* 禘 sacrifice to

Cheng Wang. (A later study will show that this inscription is genuine. It contains a Shang grammatical construction that could have been composed only by a person thoroughly familiar with the *jiagu* materials, discovered beginning 60 years after this inscription appeared.) I suggest that the sacrifice is actually a *di* sacrifice to the royal ancestors performed in the *miao* that is to be the location of Cheng's cult and is to be used by his descent group. There is the same problem in the *Ling yi* inscription; and the “new” *Zhushu jinian* has Zhou Gong honored by *di* sacrifices in his *miao* in Lu while he is still living. While I have dated the *Xiao Yu ding* to 1020, I should note that another Cheng Wang period date is imaginable. If Cheng (retrospectively) treated 1039, the year after his capping, as a *yuan* year, the *Xiao Yu ding* date could be 1015; and it will fit this year if it is a *chou* year. (Li Wang (857–828) apparently declared 844 a *yuan* year; and this must have been the year following his capping. This could have been a standard practice.) But when we see that 1044 must have been the year of Cheng's actual succession, it becomes the more likely *yuan*.

2. See note on “Major Events,” 1044.
3. The date 1051 is found not in the main text of the “new” *Zhushu jinian* but in a statement at the end of the account of the reign of Yu Wang: *Wu Wang mie Yin zai gengyin* 武王滅殷在庚寅. It seems reasonable to suppose that when this statement was added—perhaps in 450, the 1500th anniversary in the “1051” tradition—the main text agreed with it; and that the main text got changed later, perhaps in 951, the first year of the “Great Zhou” Dynasty. For, the present main text date 1050 would be exactly 2000 year before 951.

The supposed Conquest date 1051 is not an invention to match Sima Qian's date 551 for the birth of Confucius; for the resulting 7-year reign of Wu after the Conquest is given already in the *Guanzi* 管子 “Qi Zhu qi Chen pian” 七主七臣篇 (see Wang Guowei 王國維, “Zhou kaiguo nianbiao,” in *Guantang bieji*). It probably is a calculation based both on the “Wu Cheng” dates and on the Jupiter cycle, in which for ancient astronomers there was an error of about six years. The date 551 itself is therefore a product of the scholars, probably before Sima Qian.

I am inclined to the view held by some, that King Wu, on winning his *jiazi* victory, began the year's calendar anew, naming the victory (2nd) month “*zheng yue*” 正月, subsequently adding a *run* month after this or one of the next two months. This would give a more reasonable amount of time for finishing Conquest actions before his return to Hao. Note also that if I am right in taking the *Shi Dan ding* inscription seriously, it is necessary to take 1044 either as beginning with the *chou* month or as beginning with the *zi* month with a *run* before

the 8th month (as in Tung Tso-pin); so 1045 must at least use all of its *zi*-year months. And note that such a shift might explain the confusion about the time of the beginning of the campaign—some accounts, e.g., the *Shiji*, beginning it in the 12th month of the year before the victory. This assumption makes the date January 15, 1045. (But see below.)

V.

See: Zhou Fagao, “Chronology of the Western Chou Dynasty,” in *The Journal of the Institute of Chinese Studies of the Chinese University of Hong Kong*, vol. IV no. 1 (1971), p.184; and William Hung (洪業), Prolegomena (in Chinese) to *Harvard-Yenching Institute Sinological Index Series*, Supplement no. 11, *Combined Concordances to Chun-chiu, Kung-yang, Ku-liang and Tso-chuan*, pp. lxiv–lxvi. Hung cites Shinjō Shinzō 新城新藏, *Tōyō Temmonshi Kenkyū* 東洋天文史研究, 407–412, and P. V. Neugebauer, *Abgekürzte Tafeln der Sonne und der grossen Planeten*, Berlin, 1904. While Professor Zhou Fagao 周法高 in his valuable monograph reaches a conclusion different from mine, he uses a methodology that I have for the most part gratefully adopted throughout my research.

Having gotten to *jiazi* of the second month of 1045, one would like to pin down the date exactly. But the sources conflict on how the year began. The “Shi Fu” chapter (37) of the *Yi Zhou shu*—which may actually be a version of the “Wu Cheng”—has dates apparently requiring a *zi* year with a *run* month after the first month. “*Jiazi* of the second month” in this model would be March 15. The “Shih Fu” account of ritual events in the “fourth month” is quite convincing, and its dates require that this month be identical with the fifth month in Tung Tso-pin’s *zi*-year calendar in his *Xi Zhou Nianli pu* 西周年歷譜 (so that the *liao* sacrifice would be on the 16th).

For the *Li gui* 利簋, see *Wenwu* 1977.8. The inscription begins with an event date: *Wu Wang zheng Shang wei jiazi zhao* 武王征商佳甲子朝... “Wu Wang’s attack on Shang was in the morning of the day *jiazi*...”

Explanation of “Probable Derivation”—D. S. Nivison, April 24, 1980

- I. 6-year upward adjustment in the date of the Conquest, to accord with defective ancient astronomy. The resulting 7-year reign of Wu Wang after the Conquest is found already in the *Guanzi*, so this change, at least, is pre-Han.

- II. “Gong-he” being mistaken as the name of a calendar period (as in the *Shiji*), the event leading to it has to be in the preceding year (842 rather than 841). So, *yuan* dates of Li Wang back to Yih Wang are shifted up one year. (This change could have been made after IV but is required before V.)
- III. Reinterpretation of the Zhou Gong Regency, so that it will not extend 5 years into the personal reign of Cheng Wang. Result: all *yuan* dates from Cheng to Li shifted down 5 years. (This change is assumed in the *Shiji*.)
- IV. Reinterpretation of the regency, to make it begin with Cheng Wang’s succeeding to the throne. Result: dates shifted up 2 years, from Cheng to Mu. This makes Mu Wang’s *yuan* come exactly 100 years after 1062, which by change I is Wu Wang’s *yuan* (or, in some accounts, the date of Wen Wang’s “receiving the Mandate”).
- V. 5-year extension of Mu Wang’s reign (beyond the 50 year reached in change IV), to accommodate traditions about the composition of the “Lü Xing” chapter of the *Shang shu*. Gong Wang’s reign is reduced from 20 years (this figure is preserved in Huangfu Mi’s *Di wang shi ji* 帝王世紀) to 12 years (*ershi* 二十 to *shier* 十二), to compensate for this. Yih’s reign continues to be understood as 25 years, so his and Xiao’s date move up 3 years. These changes are all given or implied in the *Shiji* or in *Taiping yulan* quotations from the original *Shiji* text.

(The other *Zhushu jinian* Conquest date “1050” may be a change made in 951, to show that the “Great Zhou” Dynasty began exactly 2000 years after the Conquest.)



Figure 1: The *He zun* Inscription

Chart 1: Probable derivation of the Chu Shu Chi Nien [Zhushu Jinian] chronology for western Chou [Zhou]

True	Dates	I	II	III	IV	V	Result
1056	Wu Wang	1062					1062
1045	Conquest	1051	7				1051
1044	Cheng Wang	—			1044		1044
1044	Zhougong Regent			1042			
1042	Post-Mourning Yuan		7				
1040	Cheng, Capping, Yuan			1036			
1036	Zhougong returns power	30		1035	1038		1038
1035	Cheng, personal rule			1005	1037		1037
1010	Kang Wang				1007		1007
1008	Post-Mourning Yuan						
984	Zhao Wang			979	981		981
965	Mu Wang			960	962		962
945	1045 Centennial Yuan					50	
917	Gong Wang			912		907	907
915	Post-Mourning Yuan						
898	Yin Wang					20	
896	Post-Mourning Yuan						
871	Xiao Wang						
	(Yih Wang's calendar continue)						
865	Yi Wang		866	861			864
864	Li Wang's birth						861

857	Li Wang (minority)	858	853	853
857	Gong He Regent			
845	Li, Capping, Bicentennial			
844	Li, Yuan			
841	Flight to Qin	842		842
841	Gong He "Regency"			841
827	Xuan Wang			827
825	Xuan, 2 nd Yuan	+6	+1	+2
			-5	

2 Supplement to the “The ‘Question’ Question”— British Museum Scapula and British Museum Library Deer Horn

My analysis of the meaning and use of the word “*zhen*” 貞 has a surprising application.

In the British Museum there is a much studied item in the Couling-Chalfant Collection of oracle bones (*Ku-Fang* 庫房 1506; Figure 1), an inscribed scapula. In the British Museum Library there is a quite different piece from the same collection, an elaborately carved deer horn, which has an almost identical inscription (*Ku-Fang* 庫房 1989; Figure 2). There is a large literature attacking and defending the genuineness of these pieces. I will not review this literature. The matter is reviewed at length in an article in *Guwenzi yanjiu* 古文字研究 4 (1980) by Hu Houxuan 胡厚宣, who believes that they are not genuine; but the opinion of scholars is very much divided.

What makes these inscriptions interesting is the quite unusual character. They are a genealogy, of a man named Ni (兒, i.e., 倪), as follows: “Ni’s first ancestor was named A. A’s son was named B. B’s son was named C. C’s son was named D. D’s son was named E. E’s younger brother was named F. E’s son was named G. G’s son was named H. H’s son was named I. I’s son was named J. J’s younger brother was named K. J’s son was named L. L’s son was named M.” (Presumably “M” was Ni’s father). On the scapula, each sentence is a column, from right to left. At the right top of the first column, beside the name “Ni,” is the word “*zhen*,” inscribed in a different hand from the rest of the inscription. Across the top of the whole inscription, including the word “*zhen*,” is a line. (Such lines are often found on shells or scapulas bearing many inscriptions, apparently so that the text of one will not be confused with that of others.)

The deer-horn inscription is the same genealogy text, preceded not by the single word “*zhen*” but by the phrase “*wang yue zhen*” 王曰貞 (“The king says, ‘Zhen...’”). The upper part of the one spike on the horn has been smoothed into five flat surfaces around its circumference, and the graphs are arranged on these surfaces in eight columns, the first six columns being in pairs on three of the surfaces. The graphs per column are 7, 7, 7, 8, 8, 8, 8, 3. Thus the prefatory words “*wang yue zhen*” are not separated from the rest of the text but are ar-

May 29, 1983

ranged as an integral part of it. I detect no evidence that these words are inscribed by a different hand.

And in other ways the graphs of the deer horn are different from those on the scapula: From illustrations available to me, the word “*zhen*” on the scapula is in a form thought to be restricted to Period I, early Period II, Period IV, and to the Dui (Royal Family) Group, which I take to be probably early Period I. On the deer horn, it is in a form found in all periods. The graph “*wang*” (“king”) on the deer horn is a form restricted to Period V. The graph for “*zi*” (“son”) on the scapula appears to be a form probably restricted to Periods I, II, early III, and the Dui Group; while on the deer horn its form belongs to Periods IV and V. All of this suggests that the two inscriptions are not contemporary, similar though they are, and that the deer horn is considerably later. But if they are genuine, since they record the same facts, and we have them both, it is likely that they were found together.

What can my analysis of the meaning and use of the word “*zhen*” have to do with these two genealogy inscriptions? I will state my theory as briefly and as provocatively as I can.

- (1) The meaning and use of the word “*zhen*” (貞):
 - (a) “*zhen*” can precede a question
 - (b) “*zhen*” can precede each of two statements, one positive and one negative.
 - (c) “*zhen*” can precede a prayer.
 - (d) “*zhen*” can precede an expression of intent.
 - (e) “*zhen*” appears to be cognate with the word “*zheng*” 正, presumably meaning “correct.”

- (2) But only a statement can be correct, and in the sense “true.” And if one statement is true, its negation is not; therefore “*zhen*,” in the sequence “D *zhen* S” (D=diviner, S=sentence) does not mean “(D) asserts that (the following S) is true.” D has official status (often being the king). This fact (and the example from *Shang shu*, “Luo gao”) suggest the meaning “verify” or “authenticate” or “certify”: the king (e.g.) both certifies that the following sentence—question, statement, or whatever—is an officially ordered divination problem (i.e., it is neither a game nor some unauthorized person’s attempt to get a result (thus closing off the possibility that someone else might insist that a different result had been obtained).

What, then, would “*zhen*” mean before a genealogy? (There is evidence of a regular divination procedure on the scapula: Professor Li Xueqin has told me that there are two burned hollows with cracks near the graph “*zhen*” on the right of the inscription.)

- (3) Here is a possible interpretation of the two pieces:
- (a) Someone (presumably a person of wealth and status: I will call him a “noble”) comes to the king, wishing to have his genealogy authenticated.
 - (b) The noble brings with him a scapula, on which he has had his genealogy inscribed (at his own expense).
 - (c) The king consents to certify it. He has hollows made—if they were not already there—and cracked.
 - (d) The king (after officially checking the oracle result) officially pronounces the genealogical record on the scapula to be valid and correct.
 - (e) As a record and demonstration of his official judgment, the king
 - (i) has his official inscriber inscribe the officializing and authenticating word “*zhen*” in front of the genealogical inscription, at the right of it (This is why this graph is in a different “hand”);
 - (ii) has his inscriber inscribe a line above the record, to show that the scope of the certifying word “*zhen*” includes all of it (this is why the line is there, even though there is no text above it to keep separate).
 - (f) The noble then takes the scapula home. It is preserved in his family as a family treasure.
 - (g) A later head of the family commissions the carving of deer horn, to preserve and display the text on the scapula. The carving is, appropriately, elaborate.
 - (h) At the beginning of the deer horn inscription are the words “*wang yue zhen*,” “The King says, ‘I certify...’ ” This formula is almost never found. (But the situation is (as far as we know) unique.) It is appropriate here: it is exactly what the noble wants said on his deer horn, to display his status.
 - (i) We may compare the deer horn, and its “*wang yue zhen*,” to the later formula in a bronze inscription made after the person commissioning it has been received and honored at court: “*wang hu shi X ce ming Y yue...ci ru...*” (王呼史 X 冊命 Y 曰...賜汝...): “The King called out to Recorder X to record a command to Y: ‘I grant you...’ ” (Where Y is the person receiving the honor and gift, and then having the vessel made for display at home).

- (j) But the scapula continues, of course, to be preserved by the family: It is the scapula that is the actual document, and it is just as precious as ever. This is why we have them both.
- (4) This interpretation fits the two interpretations so well that we cannot reasonably suppose that the inscriptions could have been faked, unless the faker thoroughly understood the meaning and use of the word “*zhen*” as reconstructed above.

But these inscribed pieces came to light around 1905, when all scholars believed that “*zhen*” meant “ask a question by cracking” (following the apparent meaning of the definition in the *Shuo wen* dictionary), and so that the subject it introduces must be a matter of genuine uncertainty. This is one of the main reasons why these two pieces have been taken to be fakes: it seemed to make no sense to put a word meaning “ask,” or even “ask about,” before a genealogy.

One can probably say more: It is only recently, with the comparative study that a concordance and many scientifically excavated texts make possible, that anyone could come to see what “*zhen*” really means; and so it is only recently that we have come to see that “*zhen*” does not have to introduce a question.

Therefore, the two inscription texts must be genuine.

- (5) But are the inscribed objects genuine? This is a separate question, to this extent: If we had no reasons for or against the texts, if microscopic or chemical analysis of the objects showed the physical inscriptions to be faked, this would be presumptive evidence that the texts too were composed by the faker. But we do have powerful reasons for respecting the texts. So, even if the objects turn out to be fakes, the most reasonable assumption would be that they are copies of unknown originals that are now lost, and that were genuine. But in this case we would have to suppose that the copying was painstakingly exact, for the graphic differences between the two pieces are precisely related to the functional difference between the two, and this the copier could not have understood. So it is possible that the objects too are genuine.

If the reader finds this argument persuasive—or even possible—he will have to agree, I think, that “the ‘question’ question” is not a trivial one.

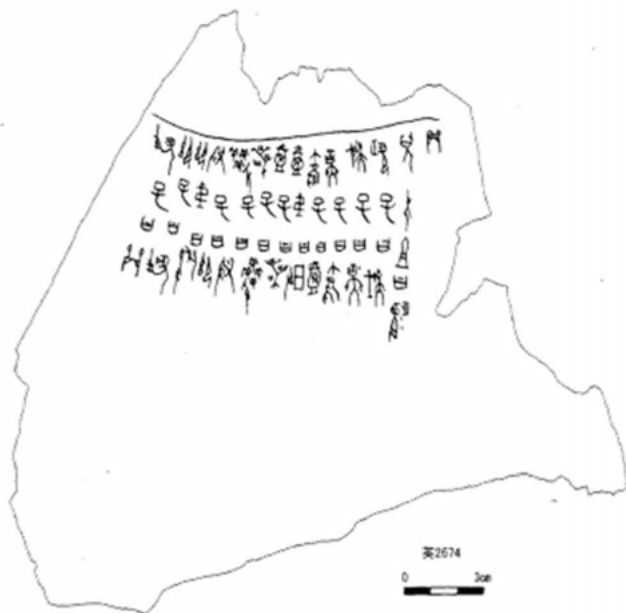


Figure 1: Ku-Fang 1506



Figure 2: Ku-Fang 1989

3 The King and the Bird: a Possible Genuine Shang Literary Text and Its Echoes in Later Philosophy and Religion

The Origin, Date and Translation of *Shang shu* 18¹

Like most students of such matters, I had always supposed that the only surviving Chinese texts that are earlier than Zhou are those we find as inscriptions on shell or one or bronze. In the literary tradition, the *Shang shu* pretends to contain pre-Zhou documents. H.G. Creel fifty years ago firmly stated his view (in *The Birth of China*) that all such texts are later inventions. Later Bernhard Karlgren, in his *Glosses on the Book of Documents*, p. 217, argued that no *Shang shu* texts are earlier than Zhou. This is what I myself have been telling my students for over thirty years—until last year, when I began to have doubts.

My doubts were caused by my work on a paper for CISHAAN #31 (Tokyo, September, 1983), “The Dates of the Late Shang Kings.” In that paper (p. 26), I examine the dates of six inscriptions that contain the expression “*Rong*” 彤 or “*Rong ri*” 彤日 (see Chen Mengjia 陳夢家, *Yinxu buci zongshu* 殷虛卜辭綜述 (Beijing, 1956), p.233). One, which I dated 24 Jan. 1100, has as part of its date the expression “*gou yu Wu Yi Rong ri*” 遘于武乙彤日, i.e., “coinciding with *rong* day for Wu Yi.” Another (my p.27, my date being 6 Feb. 1083) has the same formula, with the recipient of the *Rong* sacrifice being Bi Bing 妣丙 (consort of Da Yi 大乙).

This evidence shows, I believe, that the title of *Shang shu* 18, “Gao Zong Rong ri” 高宗彤日, which are also the first four graphs of the text, must be a standard late Shang date formula, meaning “the day of the *rong* sacrifice to Gao Zong,” i.e., to Wu Ding. But the received opinion—of most commentators, and

Paper presented at Annual Meeting of the Society for the Study of Early China, held concurrently with the 36th Annual Meeting of the Association for Asian Studies, Washington, D.C., March 24, 1984

1 Note: In October, 1983, Professor Jeffrey Riegel and I discovered accidentally that we had each been working on *Shang shu* 18, “Gao Zong Rong ri,” I on the date of the text, and Riegel on its content. The following paper is the result. This paper, early 1984, assumes the date of the Zhou Conquest to be 1040 BC. I have since that time found that two of several arguments for this date are invalid, and I now think, after closer analysis of the *Bamboo Annals*, that my original published date, 1045 BC, is probably right—DSN 30.6.89.

found in the *Shiji* (“Yin benji”), the *Shang shu Dazhuan*, and the *Shu* “Preface”—is that Wu Ding is the king *performing* the *rong* rite in the episode. (Supposedly, the recipient of the sacrifice is Tang, the Shang founder. James Legge, in his *The Chinese Classics*, vol.3, pp. 264–5, allows that a few commentators say the officiant cannot be Wu Ding; but their reason was that one cannot suppose Wu Ding to have been criticized as he is in this chapter by one of his subjects; i.e., they were unaware of the date formula as was Legge himself.)

Thus this short (82 graph) chapter must be a very old text—containing authentic Shang language, its meaning lost already by early Han. When this short text is examined closely, we find more evidence to this effect; and it shows, I think, that the “Gao Zong Rong ri” must either be itself a late Shang text or such a text but slightly edited in early Zhou. I will now offer my translation of the text line by line, with comment where needed; then I will summarize and interpret the results.

(1) 高宗彤日，越有雉雉。

On the day of the *Rong* sacrifice to Gao Zong (Wu Ding), there appeared a crowing pheasant.

(Commentators agree that the bird made its appearance in the midst of the rite, some saying that it alighted on the handle of a cauldron; its appearing is assumed to be a bad omen.)

(2) 祖己曰，惟先格王，正厥事。

Zu Ji said, “What has happened is that the [King’s] predecessor has come to the King, to correct his actions.”

(Here I follow Jeffrey Riegel: *xian* 先 is *xian wang* 先王, “the former king,” i.e., Wu Ding himself, appearing in the form of a bird. As Sarah Allan has pointed out (“Sons of Suns: “Myth and Totemism in Early China,” *BSOAS* 44.2, 1981), the *Shijing* (#303) has the Shang royal line descended from a bird (p. 304), and “the Shang rulers had a totemic relationship” with ten sons, “which were also thought to be birds” (p. 310). But who is Zu Ji? Sima Qian obviously had no idea who he was. The recovery of this information had to wait until the development of Shang oracle bone studies in the present century. The inscriptions show that Zu Ji, though not a king himself, was the eldest son of Wu Ding. He thus is almost certainly also the Xiao Ji 孝己 mentioned in late Zhou texts such as *Zhuangzi*, *Xunzi*, and *Zhu shu jinian*, as being a conspicuously filial son of Wu Ding who was rejected by his fa-

ther and died in obscurity. See D. N. Keightley, *Sources of Shang History* (University of California Press, 1978). p. 208, notes ad–ae.)

- (3) 乃訓于王曰，惟天監下民，典厥義，降年有永有不永，非夭夭民，民中絕命。

So he then lectured to the King, saying, “When Heaven inspects the people below, attending to their conduct, it sends down years of life either long or not long; it is not that Heaven causes people to die early; the people cut off their allotted lives midway.

(I take *yi* 義 as *yi* 儀, and interpret *dian* 典 as parallel to “inspect,” in effect “see to the correctness of.” For the theme of theodicy, compare *Zuozhuan*, Xiang 23/11, “Misfortune and good fortune have no gates; they are just what men invite [on themselves].”)

- (4) 民有不若德，不聽罪，天既孚命正厥德，乃曰，其如台。
[It’s just that] when people have unsatisfactory virtue, and won’t acknowledge their fault, Heaven then manifests its will [in omens], to correct their [bad] virtue; but they then say, “What concern is this to us?”

(The received interpretation takes *ruo* 若 as a verb, giving the translation “comply with virtue”; but Period I oracle inscriptions—unknown to commentators—show that *ruo* as applied to *de* 德 means “approved,” “recognized [by the spirits] as good”; “virtue” here thus means “character,” good or bad. The word *fu* 孚 has caused much trouble; the *Shiji* text has *fu* 付, showing that the word must be *fu* 符, “sign,” here of divine attitude.)

- (5) 嗚呼，王司敬民，罔非天胤，典祀無豐于昵。
Oh! Your Majesty, in having the responsibility of caring reverently for the people, is in every respect acting as the vicar of Heaven; in managing the sacrifices, do not be lavish toward those who are close to you.

(This is the line that has caused interpreters the most trouble, and none have got it right. It does not matter whether we read *si* 司, “have the responsibility for” or *si* 嗣, “inherit the responsibility for.” But *jing* 敬 is important; it means “care for, as a religious duty,” as in *jing de* 敬德, “care reverently for one’s virtue.” This shows how *yin* 胤 “continue” must be understood: continue, not temporally or spacially here, but hierarchically, playing the same part, and so exercising the authority of, acting for. The word is rare:

there are two other occurrences in the *Shang shu*, one being a name (and in a *guwen* chapter), and therefore useless for establishing the word's meaning.

But the other gives the meaning precisely. It occurs near the beginning of the “Luo Gao,” concerning the founding of Luoyang; the construction is causative:

予乃胤保大相東土，其基作民明辟。予惟乙卯朝至于洛師…

I then delegated the [grand] guardian (i.e., the Duke of Shao) to make a broad survey of the Eastern Lands, in order to lay the foundation for an illustrious royal [court] for the people. When I arrived at the Luo encampment the morning of day *yimao* (52)...

The word *yin* here means “caused... to ‘continue’ [me] in function and authority.” We can see this from two other *Shang shu* texts bearing on the matter: (1) The “Shao Gao” chapter explains what the Duke of Shao did, day by day. He arrived on the scene on day *wushen* (45) of the 3rd month, two days after *fei* (new moon day, in this case probably the 2nd of the month). The year was probably 1031 BC (in my article “The Dates of Western Chou” in *HJAS* 43 I held it to be 5 years earlier); *wushen* would be 14 February. He consulted the oracles, and laid out plans. Two days later, on *gengxu*, he organized and directed the populace in beginning the preparation of the site. Thus the day work actually started was the 6th of the month. My *HJAS* article identified the 7th of the month as *zai sheng po*, the day when ideally the waxing moon is half full; but my dating for the first half of the Zhou Dynasty was probably five years off. Taking 1031 as the date rather than 1036, and the date of the Conquest as 1040 rather than 1045, one must take the lunar phase day dates as varying in long and short months as *fei* varies, falling one day earlier when the preceding month is long, as it is in this case. Therefore the 6th was *zai sheng po*. Turning now to the introductory paragraph to the “Kang Gao,” which critics have long recognized as displaced from the “Luo Gao” and as actually describing events of the year of the founding of Luo, we read there that “On *zai sheng po* of the 3rd month, he Duke of Zhou began the foundations of a new city at Luo in the Eastern Territories.”²

But as we have seen, the Duke of Zhou did not even arrive until five days after this. So if these texts are taken to be true, the only way to make sense of this one is to understand it as meaning that the Duke of Zhou did

² Note: This argument for the meaning of the word *yin* remains valid if the Conquest date is 1045—DSN

this vicariously, through the agency of the Duke of Shao, acting for him. This is just what I take the word *yin* to mean, in the “Luo Gao” and also in the “Gao Zong Rong ri.”)

When the meaning of the chapter is clarified up to this point, the final phrase, much puzzled over, can at last be seen to mean exactly what it says, when each of its component words and phrases is given the most obvious interpretation. The last word, *ni* 昵, should not be reidentified as the cognate *ni* 禰, “father’s tablet”: that was a desperate move forced on the commentators by their supposing that (5) up to this point meant something like “when the [ancestral] kings inherited the care of the people, they were all alike continuers of Heaven,” i.e., the king being reprovved—supposedly Wu Ding—ought to favor them all equally, and not favor his own royal father especially. This is not what the king was being told. What, then? What does *ni*, “close,” taken as “those who are close to you,” refer to? It is time to review results and draw conclusions.

At one time this text must have been thought of as recording an omen warning of the fall of Shang. This is indicated by the fact that the episode is entered in the *Bamboo Annals* under the year 1246, in Wu Ding’s reign. I have argued in my *HJAS* article that the Conquest was at the very beginning of 1045, and at the end of 1046 in the Shang calendar, making 1046 the last year of Shang. I have also shown that the *Annals* plays numerological games with 100-year intervals. I think I can now show that the Conquest date must have been not 1045 but 1040; but also, that there must have been a well-reasoned belief, held for centuries before the Han, that 1045 was the correct date. It appears to me to be likely that this is why the pheasant incident is dated to 1246 in the *Annals*—by editorial license not later than 296 BC, the date the *Annals* text was interred in the royal Wei tomb.

Sometime before this—probably long before—the king being reprovved has to have been reidentified as Wu Ding. This had to have been done after the real meaning of the opening date formula had been forgotten. Furthermore, it was done early enough so that the real identity of Zu Ji was still known—that is, that he was Wu Ding’s eldest son. The reason for saying this is the belief prevailing by late Eastern Zhou that Wu Ding’s son, alias “Xiao Ji,” was rejected by his father and died in disgrace, before Wu Ding’s death. The supposed fact that Zu Ji (or Xiao Ji) had boldly reprovved his father would suggest that this might have happened, and the conjecture would in turn explain why Xiao Ji never became king.

Again we can turn to the *Bamboo Annals* for support: The death of Xiao Ji is recorded there, and is entered in the middle of Wu Ding’s reign, under the year 1250. That is not consistent with the date 1246 for the pheasant incident, to be

sure; but the *Annals* text does not have a simple linear history: as we have it, it is a reconciliation of several different conflicting chronologies; and the one that dominates in the present text dates the Zhou victory over Shang to the year 1050. It was in this chronology, I suggest, that Xiao Ji's death was first dated 1250; i.e., it too was taken to be an omen. This probably pushes the misinterpretation of the "Gao Zong Rong ri" back at least to the Chunqiu period.

How early can the composition of the text have been? As it stands, not earlier than the reign of Wu Yi, the first king for whom the generation of Wu Ding's sons were "grandfathers" (*zu*); and my tentative date for the beginning of Wu Yi's reign is 1140.

Can a later *terminus a quo* be established? Four times the word *tian* 天, "Heaven," occurs in the text, as the name of the high god. This is always said to be a Zhou name; the proper Shang name is *shang di*. But one of the main reasons why this has always been said is surely that we have so long believed that there are no remaining literary texts earlier than Zhou. It appears to be true that the expression *Tian yi Shang* 天邑商 ("Heaven's city Shang"?) occurs both in the *Shang shu* (34 "Duo shi") and in oracle inscriptions (Shima Kunio, *Inkyo Bokuji Sorui* 42.4; see also Li Xiaoding, *Jiagu wenzi jishi* #13). Perhaps this matter is ripe for reexamination.

How *late* can the texts have been composed? Not later than the time when the date formula with which it begins ceased to be understood; and so not as late as the time when the king addressed came to be thought to be Wu Ding. Let us try supposing that it was composed (that is, invented) sometime in early Zhou, before these things happened. *Why* would it have been composed then? There is an obvious reason why it should have been esteemed by Zhou readers and treated as one of the "Books of Shang" once it had come into existence and had been misinterpreted: it would have been taken as the record of an ominous incident implying that the Shang Dynasty, already in the reign of one of its great kings, was decaying and would eventually fall. But it couldn't have been *composed* with this in mind—because the person who wrote it has to have believed that the *Rong* sacrifice was *to* Wu Ting, not *by* him. It has to have been composed for another reason, and, at first anyway, preserved for another reason.

I am unable to think of a reason that any *Zhou* writer could have had. But there are obvious possible accounts that would explain its composition and preservation if it was written in late Shang. First of all, the incident might actually have *happened* as described; there is nothing in the least implausible about it. And whether it happened or not, it is not hard to see why some member of the Shang royal lineage, during one of the last four Shang reigns, might have wanted it to have happened. I call attention again to the role and position of Zu Ji—

son of the great Wu Ding, but not a reigning king, reproving a king who is one of his own junior brothers, either Zu Geng or Zu Jia. Only one of those three brothers could have one of his sons be the first king in the next generation. If two or more of those brothers were sons of Wu Ding by different consorts, then it is quite imaginable that at some time one of the two king-sons—I think we have to suppose Zu Jia—was trying to promote the candidacy of his own line in the succession, by building up the posthumous status of the consort who had been his own mother. Conceivably he would do this by giving her—a person “close to” himself—especially “lavish” sacrificial rites and offerings. The reproof then would be, as seen by a descendent of Zu Ji or Zu Geng after 1140, a plea that the status of the two unsuccessful lineages be respected. Further, the plea is set in the strongest terms: the king, as king, has a duty to Heaven to be as impartial as Heaven itself.

Why should such a text as this have been preserved across the disruption and destruction of the conquest? My interpretation offers a reason: It could have survived in one or more collateral branches of the Shang royal house that made their peace with the Zhou without a fight.

4 The Hampers of Zeng: Some Problems in Archaeoastronomy

Illustrations



貞(角), 堅(堅), 在(在), 方(方), 室(心), 亦(泥)
 兌(巽), 翌(斗), 會(率), 夔(夔), 夔(夔), 夔(夔)
 五(魂), 夔(西), 夔(東), 夔(東), 夔(東)
 夔(胃), 夔(子), 夔(維), 夔(北), 夔(參)
 夔(東), 夔(與), 夔(西), 夔(七), 夔(張)
 夔(翼), 夔(車), 在(壁)之下還有“田”

Hamper #1: Twenty-eight *xiu* (lunar mansions) on lacquer hamper



天若之維坊民
 嘗敷四興日祀
 和鉄尚歲於佳
 天若之維坊民
 嘗敷四興日祀
 和鉄尚歲於佳

Hamper #2: Text on lacquer hamper

Paper presented at the annual meeting of the Western Branch of the American Oriental Society, Boulder, CO, November 2, 1985

Transcription and Translations

Jao Tsung-I's 饒宗頤 interpretation (tentatively reconstructed by DSN), from his article “Zeng Hou Yi mu huqi qishu wenzi chushi” 曾侯乙墓匱器漆書文字初釋 (A preliminary explanation of the lacquer inscription on a hamper in the tomb of Marquis Yi of Zeng), *Guwenzi yanjiu* 古文字研究 10 (July, 1983) pp. 190–197

Line 1: It is Fang to which the people sacrifice;

Line 2: The sun-moon conjunction is in the (northern) corner;

Line 3: (a) (As for) the Four (Stars, i.e. Fang) that are auspicious for the (agricultural) year; or,
(b) (As for) the Four (Stars) that are (like?) the auspicious year (-star),

Line 4: What they preside over (?? i.e., the “many *xiu*,” p. 194, 1. 2; “*suo shang*” is not interpreted) are concordantly arranged (i.e., do not conflict with the year star, in some sense; cf 11. 2–3),

Line 5: And as (they) circle the Heaven are always in accord (= in their proper places, making auspicious celestial harmony).

David W. Pankenier's translation and interpretation (from his article, “Early Chinese Positional Astronomy: The *Guoyu* Astronomical Record,” *Archaeoastronomy* 5.3, 1982 (actually 1983), 10–20; see pp. 10–11)

Line 1: The people's sacrifice to “House” (Fang),

Line 2: the luni-solar conjunctions in the “corners”,

Line 3: the four locations in which Jupiter arises,

Line 4: Shang (asterism) like a row of troops,

Line 5: [they] order Heaven and are constantly in harmony.

Pankenier claims that the language of this –5th century inscription helps to show that the astrological text (of celestial events at the time of the Zhou Conquest) in *Guoyu*, “Zhou yu” 7, is a genuine text of a record made at the time of the Conquest, in the –11th century. This translation, which he says follows Jao (who had

not yet published; Pankenier heard him read a paper on the problem in 1981), is merely parenthetical in the context of Pankenier's article, and he does not defend it, saying only "it will not be possible to discuss this interesting catalog of seasonal observation in detail."

DSN's translation (27 October 1985)

- Line 1: *Min si wei Fang* 民祀佳坊
When the people sacrifice to Fang,
- Line 2: *Ri chen yu wei* 日辰於維
And the sun's *chen* is in the (winter-spring) corner,
- Line 3: *Xing sui zhi si* 興歲之四
You Four (Stars) that inaugurate the year,
- Line 4: *Suo shang ruo chen* 所尚若敕
May what you grant be like our plea.
- Line 5: *Tian Tian chang he* 鈇天嘗和
Thundering Heaven sounds accord.

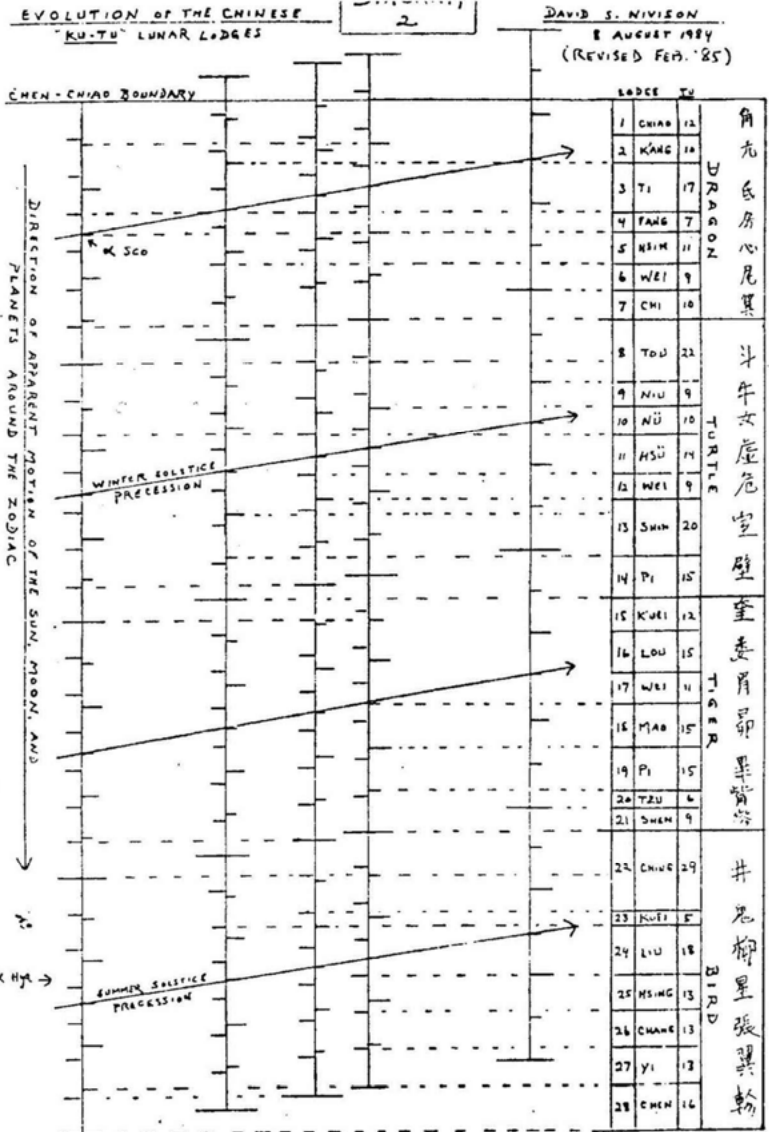
Notes:

- Line 1: The reading of the fourth graph is open to question. I follow Jao, who takes it as 坊, i.e., 房.
- Line 2: I interpret "*chen*" here as the moving zodiac: space of 30 *du* with the sun at its center, i.e., the zodiac area too close to the sun to be visible. The third graph "*yu*" 於, "in," must be a verb here, meaning "*zai yu*" 在於, "is located in."
- Line 3: "*si*" 四, as Jao argues, is "*Si*" 駟, the Team of Four Horses, i.e. Fang.
- Line 4: I interpret "*shang*" 尚 as "*shang*" 賞, "to give." "*Chen*" 敕 is a variant for 隲 or 陳.

Line 5: For the first graph, otherwise unknown, compare Mencius 1A3, “*Tian jan gu zhi*” 填然鼓之, “*Tian*-like they drum it,” i.e., “Boom boom go the drums.” I take the third graph as having the top element 𠂔 as phonetic and the bottom, 音 “sound,” as giving the meaning; perhaps it was a way of writing “*chang*” 唱, “to call out,” “applaud.”

Diagrams

P. 5



(see p. 13)	ORIGINAL (#FU HSI ⁴)	(#YAO AND SHUN ⁴)	MIDDLE HSIA	EARLY SHANG	WESTERN CHOU	"KU-TU" LODGE EX- TENSIONS (PAN HAI)
(Equatorial)	2580	2260	1872	1640	942	
(Ecliptic)	2666	2101	1748	1536	900	

The Hampers of Zeng: Some Problems in Archaeoastronomy

I shall be talking about two clothes hampers found in the tomb of Marquis Yi of Zeng, which was discovered in 1977 and excavated in 1978. The site is at Leigudun, northwest of Suixian City in Hubei. This tomb is a very rich find, already much publicized and written about; it is perhaps best known for the complete set of sixty-four musical bells, still mounted on an elaborate stand, that was found in it.

The covers of the two hampers are illustrated in *Sui Xian Zeng Hou Yi mu*, compiled by the Hubei Provincial Museum (published by Wenwu Chubanshe, Beijing, 1980). I am chiefly concerned with a short (20 graphs) lacquer inscription on hamper #2; but hamper #1 is the more spectacular. The illustration is described as a “lacquer box-cover painted with twenty-eight *xiu* (lunar mansions), length 82.8 cm, total height 44.8 cm.” The cover has red designs on a black background. In the center is a large graph of the Big Dipper. Surrounding it in a loose circle are the names of the 28 *xiu* (not always the same as the classical names), arranged in clockwise order, with a space before Jiao, indicating that it counts (as later) as the beginning of the series. If we orient the cover with Jiao at top, we find a date written under the next *xiu* name, Kang, reading “*jiayin*, 3rd day.” When this information is combined with a date on a bronze bell in the tomb, “the King’s 56th cult-year,” identifying the king as the king of Chu, the king turns out to be King Hui of Chu, and the year to be 433 BC.

For, if the lunar zodiac is conceived to be laid out on the plane of the earth with Xu (mansion #11), traditionally supposed to be the sun’s location at the winter solstice, at north, and the assumption is made that the Big Dipper’s handle points due north at an ideal 6:00 p.m. at the winter solstice, it follows that the handle would point at Kang in the 5th lunar month counting the solstice month as 1st month; and in 433 the 3rd day of this 5th month was *jiayin*. (The exact date is April 1st, 433 BC, JD 1563361.) It is conjectured that this is the date of the death of Marquis Yi; perhaps it is the date of interment, or of the end of mourning. In any case, it appears that the hamper lid was made, or at least altered, expressly for the burial.

Here I do no more than describe the results of others. And it has occurred to others as well as to me that the depiction of the Tiger at the left side of the cover, and of the Dragon on the right, indicates that in the first stages of the development of the Chinese zodiac there were only two celestial animals, the (summer) Bird and the (winter) Turtle being added later. Perhaps then the year was originally conceived to be literally a “spring-autumn” (*Chun-Qiu*); for on the Dragon’s side are arrayed the mansions through which the sun passed in autumn

and winter, and on the Tiger's side those through which the sun passed in spring and summer.

Or, using the Dipper dial method, those at which the Dipper's handle pointed in winter and spring are on the Dragon's side, and in summer and autumn on the Tiger's side. Which is right? The latter. In Han astrology, the Dragon is associated with green, which is associated with spring and east; and the Tiger with white, which is associated with autumn and west. By confirmation we find among the mansions differing in name from their classical counterparts two: instead of #13 Ying Shi (Planned House) and #14 Dong Bi (East Wall) we have respectively Xi Ying (West Plan) and Dong Ying (East Plan). The order shows that east is the direction of the progress of the sun, moon and planets around the zodiac—as indeed it is, the only way one can see the zodiac, i.e., standing in one's back yard at night looking south. If now we reckon from Jiao, top and starting point, we see that the Dragon is east and the Tiger is west.

And here, for me, emerges a puzzle: The Dipper graph is presented (I think) as we would see the dipper when we look north, and up. But the names of the mansions are arranged as the Dipper would see the corresponding constellations if it were to look down at the earth and them, assuming they are beneath the Dipper in the layout of the cosmos. Notwithstanding, the handle of the Dipper points at Fang and Xin, as is astronomically correct. Alternatively, I can imagine the bottom of the circle of mansions as being what I would see (at the right season) as I look south at night; this makes the zodiac come out right—I merely have to think of the rest of it as looped back of and the earth and out of sight; but then the Dipper, up there back of my brains, is reversed. Why? I leave the question standing, and turn back to hamper #2.

My source gives the description, “Lacquer box-cover painted with *fusang* tree and an archer. Total length 82.7 cm, total height 40.8 cm.” In the illustration I fail to find an archer; there are four trees, presumably *fusang*. There are also thirteen large hatchet-shaped designs, that seem to be used on other objects the way the so-called “thunder pattern” was used in Shang and early Zhou. Beneath one of these, on the far left at bottom in the corner is a barely visible inscription. It is reproduced in full detail at the end of an article on it by Jao Tsung-I in *Guwenzi yanjiu* 10. (See Transcription and Translation section above).

You may count, as I said, 20 graphs, and I like Jao think that they must be read in groups of four. I have tried to construct a translation that Jao might accept; he does not offer one; his procedure is to discuss each phrase that interests him, quoting similar (or apparently similar) phrases from old texts and inscriptions, without trying to reconcile incompatible citations. E.g., some of his citations imply for “*sui*,” in line 3, the meaning “(agricultural) year,” while others

he takes as making “*sui*” mean “the year (star),” i.e., Jupiter; he seems to think both are relevant; I cannot understand how. I also reproduce one other published translation (by Pankenier). Finally, you have my own attempt, which I invite you to criticize, after I offer some defense of it. I will refer to three Beijing scholars I have consulted, as “A,” “B,” and “C,” since they have not published.

Line 1: *Min si wei Fang*

“When the people sacrifice to Fang”—perhaps comparable to the familiar “event-date” formula. “A” has (perhaps alone among us) been able to examine the actual inscription closely, and doubts Jao’s transcription “Fang,” interpretable as a variant for “Fang,” name of mansion #4 or of the asterism thereof. I think the way the graph “*fang*” would have been written in Zhou scripts is close enough to what “A” describes. “B” doubts Jao’s reading of the second graph, but offers no alternative; I (and the others) see no problem in it.

Line 2: *Ri chen yu wei*

“And the sun’s *chen* is at the corner”—Jao is doubtful about the third graph in the line. Others offered me no reading; but “C” (who elsewhere accepts Jao’s readings) says he has seen a similar graph in certain silk manuscripts, used as a “connecting particle,” with apparent meaning “*zai yu*.” “B” objects that “*yu*” alone would be ungrammatical here (a full verb is needed); but I think it is impossible to give the graph any other reading. As to “*ri chen*,” Jao sometimes quotes this as “*ri yue*,” “sun and moon,” indicating that he is thinking of Liu Xin’s interpretation of the *Zuo zhuan* definition of the term “*chen*” as “*ri yue zhi hui*,” “conjunction of the sun and moon.” (The word “*chen*” ordinarily means a 12th of the zodiac, with a “*qi*-center” as its midpoint, i.e., with the first *chen*-space centered on the sun’s location at the winter solstice.) An article of mine now in press in Beijing demonstrates that the meaning of the term “*ri chen*,” However, must be “zodiac 12th centered on the sun” (as contrasted with the ordinary sense of “*chen*,” which we might call a “*qi*-center *chen*”); and so by extension “*ri chen*” means the sun itself conceived as moving through the zodiac. The term “*wei*,” as Jao notes, is technical language: there are four *wei* in the zodiac, midway between any two of the cardinal points (solstices and equinoxes). (Jao does not make clear that the *wei* are points, i.e., a *wei* is not the union of two *chen* spaces but the boundary between them.) Thus the four *wei* are the

four points through which the sun passes at the beginnings of the four seasons of the solar year. Line 3 shows what season boundary is intended; thus the meaning of line 2 has to be, in effect, “And it is the beginning of spring.”

Line 3: *Xing sui zhi si*

“May what (you), the Four (Stars) that inaugurate the year”—grammatically, lines 3 and 4 are not separable. The “sui,” or agricultural year, begins with the third month; compare *Shang shu*, “Shun Dian,” where “*sui er yue*” must be the spring equinox month, i.e., the 4th month if the winter solstice month is taken as first month. Jao points out that “si,” “Four,” must be a variant for “Si,” i.e., *Tian Si*, “Sky Quadriga,” another name for Fang. In 433 BC, Fang would have culminated shortly before dawn, at about 4:45 a.m., on the day the sun reached 315, i.e., Feb. 9th. (“B” doubts the reading “xing”; it seems obvious to me.)

Line 4: *Suo shang ruo chen*

“May what you grant (us) be like (our) plea, i.e., presentation (of our wishes)”—to “shang” add radical 154, the cowrie; and compare “shang,” “would that,” in imprecations in the *Shi*. The last graph is a well-established variant, here in a well-established meaning (cognate with “shen” (the 9th “stem”) as in “shen qing,” “request to a superior”).

Line 5: *Tian Tian chang he*

“Thundering Heaven sounds accord.”—Jao reads “jing tian,” which might mean “throughout the year,” literally “(as the sun) goes through the heavens.” But “A” points out that Jao has obviously misread the first graph; it is clear even in the reproduction (see Transcription) that it is a combination of “jin,” “metal,” on the left, and an element one would expect to be phonetic on the right that is identical with the next graph, which Jao has identified as “tian,” “heaven,” in an argument that persuades me. I take the word as onomatopoeic, and I suggest comparison with Mencius 1A3, “*Tian ran gu zhi*,” literally “*Tian*-like they drum it,” i.e., “boom boom go the drums.” Our “tian” (*d’ian) here is an almost, exact homophone. (Perhaps we should even consider the possibility that the name “*Tian*,” “Heaven,” means “the Thunderer.”) As for “chang,” taken by Jao

as “*chang*,” “constant,” I think the “*yin*” (“sound”) element in the graph has to be given its due; perhaps this is another way of rendering “*chang*,” “sing out,” out,” “applaud.”

But I could rather easily be dislodged from my interpretation of lines 4 and 5. It is the astronomy in lines 1, 2, and 3 that I would like to think I have gotten right. With this I am not quite finished. I must ask, why Fang, in line 1, referred to again in line 3 as the “Four (Stars)” or “Sky Quadriga”? What is its connection with the beginning of spring? I did observe that in 433 BC Fang would have culminated at about 4:45 a.m. on the date when the sun reached the winter-spring “corner.” But this by itself means little. For one thing, the writer is using the concepts of traditional astrology. For him, then, probably the winter-spring “corner” was not the actual midpoint between the solstice and the equinox, but the traditional boundary between lunar mansions #14 and #15, Bi and Kui. This point, as I reconstruct the ancient zodiac, was at 339.4 in 433 BC; in that year the sun reached this point on March 4, and Fang would have culminated on that night at 3:06 a.m.

To see the importance of Fang, we must go back farther. I have analyzed the “*gudu*” (old degree) system of lunar mansion extensions recorded by Liu Xiang in his (lost) *Hong fan zhuan* in the –1st century, and have reconstructed from it a series of equal-spaced calendrical zodiacs from which it must be derived. These can be dated by calculating for precession. The earliest would have been astronomically correct in the early 29th century BC. At that time the sun’s location at the autumn equinox was exactly the longitude of Antares (Alpha Scorpii, the “Fire Star”), which is the middle star of the three star Xin asterism and which was I think the boundary marker between the later lunar mansions Fang and Xin. This star therefore, at the time the Chinese first worked out their zodiac, ages ago, must have culminated at midnight at the spring equinox. We can understand, then why it was called the “Tian wang,” the “King of Heaven,” as Sima Qian says in his “Tianguan shu.” The Dipper’s handle points at the Fang-Xin asterism complex; the four stars of Fang are the four horses that pull the chariot bearing the king, represented by the three stars of Xin; and it is a chariot plus four horses that is properly called a “*si*” 駟 or quadriga. It was this celestial spectacle that marked the spring season when Chinese astrological concepts were formed. By 2500 BC, it was Fang itself that culminated at midnight at the equinox; by 500 BC, the culmination time had shifted to 1:40 a.m., but still the Quadriga dominated the springtime night sky, and would have been marked in people’s minds as the constellation of spring, the beginning of the year. (Today, of course, it is no longer so: Fang now culminates at midnight two full months

after the spring equinox. For some of the detail on what I have been saying, see the Diagram section)

The last two lines have their importance, however; for as I take them they tell me what this inscription is. It is a prayer, auspicious in tone, perhaps added by someone of importance in the ceremonies who felt an obligation to write something, and used an available blank space. If the “sun’s *chen*” has to have been exactly in the “corner,” in some sense, the date cannot be that of hamper #1, but it could be the date of a ceremonial event a month or two earlier.

5 New Study of *Xiaotun Yinxu Wenzhi Jiabian 2416*

Abstract/Introduction

Xiaotun Jiabian 2416 (=Jiaguwen heji 36511) is one of the longest *jiagu* inscription ever discovered, being an inscription of more than 60 graphs. It is a “display” inscription on a scapula, and is the only inscription on the scapula (Editor’s note: see essay, “Notes on Royal Ontario Museum, *White* Collection, #1908” in this volume for images). Apparently, it was a test of the spirits’ favor at the beginning of a military campaign against a rebellious border lord in the reign of one of the last kings of Shang. The paper has three parts:

1. Using other inscriptions and ancient texts for comparison, I will first argue for the following transcription of *Jiabian 2416*:

- 丁卯, 王卜貞:
- [1] “今卜筮九格, 余其從多甸于多伯, 征孟方伯譏。
 - [2] 東衣翌日步; 亡左。
 - [3] 自上下于厥示余受有祐; 不佻戕。
 - [4] [克誥]于茲大邑商, 亡彊在猗。”
[王占曰]: “弘吉!” 在十月, 燕大丁翌。

2. I will then translate the inscription into modern Chinese, showing more exactly what its purpose was, and showing that the text is a set of test statements, rather than questions.
3. Finally, I will attempt to date the inscription.

July 1, 1987; Paper presented to the International Conference on Shang Culture, Anyang, PRC, September 1987.

Part I: Transcription of *Jiabian* 2416

The preface (*xuci* 序辭) is not problematic. The charge (*mingci* 命辭), as I interpret it, consists of four sentences (see the numbers I have inserted in square brackets above). The opening phrase of sentence 1 of the charge, 今于九筮, I have rendered 今卜筮九格 (*jin bu shi jiu ge*). Here there are three problems: 筮, 卜, and 九.

The last of these, reading 格 (*ge*) for 九, is urged persuasively by Tang Lan (see Li Xiaoding 1965, pp. 1919–20), who notices the sentence 格人元龜岡敢知吉 (*ge ren yuan gui gang gan zhi ji*), in the “Xi Bo kan Li” chapter of the *Shang shu*, perhaps meaning “The diviners and the great tortoise do not dare to make known good fortune.” The archaic meaning of 格 is “come.” Perhaps here it is “attract,” to accept a sacrifice, said of ancestral spirits, who in “coming” show their approval of the sacrificer; in the divination rite the meaning would then be “get a favorable response,” from the spirits consulted in the rite. But it is perhaps better here to take it as meaning simply “come to” the oracle, i.e., consult the oracle, said of the diviner, as Karlgren thinks (1948–49, p. 215); context here would require “successfully consult the oracles.” This is more likely, because there is much evidence that the diviner was not consulting ancestors by cracking shells or scapulas, or by manipulating stalks, but was tapping a power immanent in these “*shen wu*” 神物 (*Zhou yi*, “Xici” A11). [I deal with this problem in an unpublished paper, “The Grammar and Theology of the Shang King’s Crack-Reading.” Presented at the annual meeting of the American Oriental Society in Toronto, Canada, 12 April 1987.]

This interpretation requires that the former two problematic words refer to divination procedures or objects. The second word, 卜, is identified as 筮 (*shi*) by Tang Lan, who calls attention to 筮, used for 筮 in bronze inscriptions (see Li Xiaoding 1965, p. 1595). If 筮 has its ordinary meaning “to divine by stalks,” then the first problem word 筮, apparently “bone,” ought to mean “to divine by cracks (produced in a scapula or shell).”

This inference calls for a surprising reading for the “bone” graph 筮. In my abstract, I have read it as if it were 筮, and gave it the reading of the interior element 卜 (*bu*) in this case, just as one reads 筮 as 占 (*zhan*). This reading accords with *Zhou yi*, “Xici” A9, and with *Shang shu*, “Hong fan” (Legge 1865, p.335), where the phrase 卜筮 (*bu shi*) is repeatedly used in the sense “divine by cracks and stalks.”

Those who think that a graph must always be read one way, or at least in phonetically closely related ways, will object. And I could well yield the point; for there is little to choose between “By divining with cracks and stalks we have (nine times=) repeatedly gotten favorable results,” and “from scapulas (*ku* 骷) and

stalks we have repeatedly gotten favorable results.” Further, the word *shi* 筮 is sometimes used as a noun, “divining stalks,” as further on in the “Hong fan” itself, where it is several times paired with *gui* “turtle” (shell). So, while my abstract has 卜筮 for 筮 卜, already I am unsure: perhaps it should be 筮 卜.

But there is no difficulty in supposing radically different phonetic readings of a graph in different oracle contexts. The graph 卍 is a good example: It appears to be a pictograph for “four sides” or “four directions,” hence to be read *fang*, geographical direction or border area; note the phrase 卍 米, in the sense 方禘 (*fang di*) “perform the *di* sacrifice to the (four) direction,” Shima (1971; hereafter Shima) 418.2 (compare Shima 158.4–159.1). But the graph 卍 is well known to be the original of 巫 *wu*, “shaman” (perhaps thought of as a *fang shi* 方士, which could originally have meant, not “expert in (magic) recipes,” but “expert (in magic) from a border country”). Yet here we have it as *shi* 筮, “divine by stalks,” or “divining stalks” (divination by stalks perhaps being thought of originally as an activity of a border-area shaman).

The graph 骨 is another such example. It can be written in different ways—骨, 骨, etc.; but the form 骨 can have any of these readings, as context may determine: *gu* (骨) “bone,” *ku* (骷) “scapula,” *huo* (禍) “misfortune (indicated by bone cracking).” And it has yet other readings, as I will show below; so why not also the reading *bu*, “to divine by making a crack (in a scapula or shell used for divination)?” So I want at least to leave this as a possibility.

The rest of sentence 1, 余其從 ... 孟方伯談, is not very difficult. The graph 于 (*yu*) has the sense 與 (according to Guo Moruo: see *Jiabian Kaoshi* 2395; but strictly speaking, this is a problem of translation rather than transcription.) The personal name of the border lord being “corrected” is obscure in the inscription, and I offer no argument for the guess (not my own) that I have adopted.

Nor does sentence 2, 惠衣翌日步, 亡左, contain any transcription problems, though the problem of translation is formidable.

Next comes the long sentence 3: 自上下...不祥哉. Here there are two graphs that are major problems: 受 and 祐. The first of these is a problem related to the much discussed phrase *shou you you* 受有祐, also occurring in this sentence. I argue in my article, “The pronominal use of the Verb *yu* (giüg: 出, 又, 有) in Early Archaic Chinese (*Early China* 3 (1977), 1–17) that this phrase means “... receive (divine) aid for this (undertaking),” the demonstrative-pronominal “for this” being the translation, in this context, of *you* following *shou* “receive.” I provide many examples of the verb *you* used attributively with pronominal effect; the simplest sort of case is “verb *yu you* noun,” meaning “verb at/to noun which ... has,” where the context supplies the logical “possessor” of the thing named by the noun; an example worth comparing with the present case is #37–

38 in my article (p. 12), *you yu you shi* 有于有示 “make offerings at their altar stands.” This example suggests that we should expect pronominal *you* where we find 𠄎 in sentence 3. As I argue further on (pp. 13–14, #74–77), the graph 𠄎 (which others have taken as 祭 (*ji*)) would have just the expected meaning if it is a Shang form of the possessive pronoun *jue* 厥; and it is easy to suppose a graphic evolution—𠄎, 𠄎, 𠄎, 𠄎, 厥—that would give this result. The hypothesis is that 𠄎 is just a “hieroglyphic” form of pronominal *you*: the full form of 𠄎 is 𠄎 (see Shima 115.1, 151.1–2, 154.1); my view is that the top left element 𠄎 connotes “precious,” and the bottom left element 示 connotes “sacred,” but the functional meaning is given in the right-hand element 𠄎, i.e., “their.” This is not to claim that the word *you* and the word *jue* are cognates or morphological variants; I do not exclude that possibility, but it is unnecessary to argue for it here. (For a different view of 𠄎/𠄎, see Takashima 1978–79.)

As for 𠄎 in the phrase 不𠄎𠄎, my transcription is the graph 祥, and it is very tentative. I have assumed that the element 𠄎, 𠄎 in this graph and in other forms of it is the “goat” pictograph; so I have searched for a modern word with the root *yang* 羊, and having the meaning that seems to be required in the frequently occurring sequence ‘*bu*-verb’ (or ‘*wu*-verb,’ when the verb is a verb of controllable action) found here: i.e., ‘negative + 𠄎’ must mean “certainly,” “without any doubt.” (For other examples, see Shima 105.3 though 106.3. For the distinction between verbs of controllable action and verbs of uncontrollable action and its grammatical importance in the Shang language, see Takashima 1973. The fact that the verb following 𠄎 determines which negative is used shows that 𠄎 itself is not the main concept in the phrase. I suggest that the phrase ‘*bu*/*wu* 𠄎’ is in effect a double negative.)

The first two graphs of sentence 4 are missing, the scapula being damaged at top left; but they can be supplied with near certainty from other inscriptions of the same form as *Jiabian* 2416 (see Shima 306.4, or 261.4–26.1, e.g., *Jiabian* 2395). The first problem is the very first word 𠄎, normally meaning “scapula” 髌, or “bone” 骨, or “misfortune” 禍. None of those meanings make sense here.

To see what is happening, compare the examples of the sequence ‘name 𠄎 月 𠄎 𠄎,’ at Shima 306.2–3, with *Nan nan* 南南 2.121 at Shima 411.3 𠄎 𠄎 𠄎 𠄎 𠄎 𠄎 𠄎 𠄎. One discovers that the graph 𠄎 is an early form of 克, “succeed in” or “be able to.” Confirmation (one of many) is found at Shima 306.4, in the group of inscription *Jiabian* 2902: One sees there again the phrase 𠄎 𠄎, in each of seven inscriptions; and notice that in each there also occurs the phrase 𠄎 𠄎 (twice) or (in the other five cases) 𠄎 𠄎, where we must read 𠄎/𠄎 as *huo*, “misfortune.” But in every case the word before 𠄎 is 𠄎, and not 𠄎. This is what should be expected, if the graphic evolution is 𠄎, 𠄎, 𠄎, 𠄎, 𠄎. The

phonetic value of the graph 骨 used as 𠄎, i.e. *ke*, archaic *k'ək*, has to be based on the reading *ku*, archaic *k'ag*, i.e. 骷, “scapula” for the original graphic form. (But see Takashima 1978–79 for a different view of the matter.)

The only remaining transcription problem is 𠄎 in sentence 4. (The king’s prognostication and the postface present no problems.) The puzzle here is that 𠄎 and 骨 appear interchangeable—compare Shima 303.2 and 217.4; and with *wang* 亡 “there will be no ...” we expect 𠄎/骨 *huo* to mean “misfortune” 禍; but “*zai* (在 “in”) + ‘misfortune’” (or “*zi* (自 “from”) + ‘misfortune’”; see again Shima 217.4) is unintelligible. The solution, I suggest, is this: the original meaning of the graph when read *huo* (archaic *g'war*) is certainly “bone” in some sense, probably “cracked scapula,” as pictured; the graph 𠄎 (𠄎) we know to mean “misfortune” (due to the displeasure of an ancestor spirit), whatever may have been its pronunciation. So, the phrase 亡骨在𠄎 means just what it says: “There is no ancestor-spirit-caused misfortune (indicated) on the cracked scapula(s).” The shorter phrase 亡骨, *wang huo*, is just an abbreviation; and from that abbreviation we get the later meaning “misfortune” for 禍 *huo*. One can now make an educated guess as to the form 𠄎: since 骨 had several readings—*g'war* (禍), *k'ag* (骷), perhaps also *puk* (卜), and was sometimes written simply 𠄎, with more readings—*kwət* (骨), and *k'ək* (克), by late Shang the ambiguity had become uncomfortable; so a phonetic was added: 犬, *k'iwən*—not exact, but close enough to pick out the reading *huo* (*g'war*) from the list of possibilities (final -n and final -r being interchanged in a number of archaic pronunciations).

Part II: Translation and Interpretation

I translate the whole inscription as follows:

[Preface:] Day *dingmao* (4), the king divining by crack:

[Charge:] [1] “Since we have now many times received favorable responses in divining by scapulas and by stalks, I will accompany my many governors and many lords to correct Tan, lord of the Yu Fang. [2] It will be on the day of the public *yi* ceremony that we start our march; there will be no mishap. [3] From the greater and lesser ancestral spirits in their altar stands I will receive aid in this undertaking; we will surely be victorious. [4] It can be announced to this great city Shang that there is no ancestor-spirit-caused misfortune indicated on the scapulas.”

[Prognostication:] The King read the crack and said, “Vast good fortune!”

[Postface:] This is in the 10th month, coincident with the *yi* sacrifice to royal ancestor Da Ding.

Some of the possible objections to this translation have been anticipated in my discussion of transcription problems. I turn now to those that remain.

Sentence 2: The graph *yi* 衣 is taken in *Jiabian Kaoshi* to be a place name; this would force me to translate “It will be Yi that we set out for on the following day.” Yi occurs in much later texts as a variant for Yin (the supposed dynasty name); and it does occur as a place name, rarely, in a few oracle inscriptions. But its supposed frequent occurrence as a place name is in many hunting inscriptions (see Shima 257.4–258.1). In these, however, I believe it is in fact not a place name, but is part of four-graph apotropaic phrases: *yi ru wang zai* 衣入亡災, “(putting on his cloak =) going out and (entering =) coming back he (the King) will have no misfortune”; or *yi zhu wang zai* 衣逐亡災, “going out and engaging in the chase he will have no misfortune.” (Compare the phrase *wang lai wang zai* 往來亡災, “going forth and coming back he will have no misfortune,” Shima 289.4–290.1.)

The following two inscriptions (at Shima 258.4) seem to prove that *yi* 衣 is a verb, denoting an action that is spoiled by rain: (*Ling shi* 22) ... [bu] *gou yu ke yi wu yue* ... [不] 遘雨克衣五月, “... [not] encounter rain; we can yi; 5th month”; and (*Jing jin* 3209) *zhen: yi ru bu gou yu* 貞衣入不遘雨, “divining: when we yi and (enter =) return, we will not encounter rain.” Therefore, I suggest that *yi* 衣 describing a rite means that the rite is conducted in public, in the open air, hence my translation for *yi yi ri* 衣翌日, “the day of the public yi ceremony.”

Sentence 3: One thing needs to be added here to clarify the meaning of the line: the *shi* 示, (moveable) altar stands, were to be taken on the campaign, so that the ancestors would be at hand if help were needed; for another example of the practice, see Nivison 1977, p. 13, #74–77; also p. 12, #45.

Sentence 4: For the importance of favorable divinations before the ruler made his formal *gao* 誥, his address to the people of his capital before starting on campaign, compare the “Da gao” chapter of the *Shang shu*. We can see now what the function of this inscription is: It is a summary test of the good will of the supernatural powers, after a previous series of divinations (referred to in the opening line of the charge); it is needed as a final validation of the king’s forthcoming *gao*. For other examples, see Shima 306.4, especially the similarly worded long inscription (*Qianbian* 4.18.1/3.27.6 and *Tongzuan* 593) initiating the campaign against the Ren Fang “in the 9th month ... 10th year.” And notice also the series of seven short inscriptions under *Jiabian* 2902, already examined.

One last point on the translation: I have been insisting on treating the charge in *Jiabian* 2416 as a set of statements. Most scholars feel that divination texts must be questions; for, after all, the diviner is at least pretending to be seeking to find out what is going to happen. Does one not use questions to find out things?

Not necessarily. The diviner is using words, but he is also making and interpreting cracks. Did the crack say, “Yes,” or “No,” in response to a question? Or did it say “True,” or “False,” in reaction to a test statement? Either would have

served the diviner just as well. In the present case, the words “*Yu qi* + verb” in Sentence 1 cannot be a question, but must be an expression of determination, “I will” do so-and-so. (Compare *Shang shu*, “*Jiu gao*,” Legge 1865, p. 412, 余其殺 *Yu qi sha*) If sentence 1 is a statement, there is nothing in the visible grammar of Sentences 2, 3, and 4 to show that they, in contrast to sentence 1, should be read as questions. We could understand the whole inscription as “S & S & S & S [-true or false?]”; but the words “true or false?” are not there. Better, then, to take the inscription as a set of test sentences, with the question “true or false?” asked not in words but in the diviner’s act of cracking the bone. (See my unpublished paper (“The ‘Question’ Question” ‘問’問) presented at the International Conference on Shang Civilization held in Honolulu on September, 1982; abstract in *Early China Supplement* 1 (1986), 30–31).

Part III: The Date of *Jiabian* 2416

The only dates in the inscription, apparently, are (a) in the heading, “(day) *ding-mao*,” and (b) in the close, “10th month.” We are not told the day of the month, nor the year of the reign, nor the name of the reigning king. How then is it possible to establish the date?

Perhaps, as follows:

- (1) Chen Mengjia (1956, pp. 309–310) groups together available inscriptions and fragments concerning the Yu Fang campaign. The last of these is *Jiabian* 3939, an inscription on an animal skull recording the catch in the hunt staged at the end of the Yu Fang campaign, to mark its success. This inscription has a date. In *Jiabian Kaoshi*, it is read “2nd month, 10th year, *Rong* 𠄎 day.” Chen, admitting that the inscription is not clear, cautiously reads the date as “9th month, 6th year.” Shima (1958, p. 414) also reads “9th month,” but decides on “10th year.” Li Xueqin (1959, pp. 92–93) reads it “9th year.” One can imagine “8th year or “7th year,” in my judgment, but there are surely no other possibilities; the text requires a single number between one and ten, and numbers 1 through 5 can be seen to be impossible. This, however, is little help; some way other than scrutinizing the bone or rubbing must be found if the year is to be identified.
- (2) I assume that the inscription is Period V, or close to it. The appropriate late Shang first-year dates to try, then, are these: Wenwu Ding, 1118; Wenwu Dig

as Di Yi, 1105; Zhou, 1086; Zhou as Di Xin, 1068. (Period V is said not to include Wenwu Ding; but Wenwu Ding and Di Yi, I believe, are the same person.) The argument for these dates, briefly, is this:

- a. The *Bamboo Annals* (*Jinben zhushu jinian*) is not a forgery, although its chronology for very early history (whether or not correct) has been distorted by editorial changes made in the original text as early as Eastern Zhou (Nivison 1983; Shaughnessy 1986). (Note: the original text is not the so-called “*guben*,” which is not a text at all.)
- b. The date of the Zhou Conquest in the original *Bamboo Annals*, before distortions, was 1045 (Nivison 1983, p. 564). This is almost certainly the correct date. In 1983, I made a serious error in interpreting the *Shiji*, “Zhou benji” account of the Conquest (see Nivison 1984), and correcting this mistake pointed to 1040 as a possible date. But for the present analysis, it suffices that the original *Annals* took 1045 to be the date.
- c. The present *Bamboo Annals* dates Di Xin from 1102, giving him 41 years through 1062. The next year, 1061, the book identifies as the first year of Wu Wang of Zhou, i.e., the first year of the first Zhou king who (later) was king of all China.
- d. These are distortions; the dates should be 41 years, 1086 through 1046, the next year being the first real year of Zhou, in the original text.
- e. The date 1086 can be confirmed as historically correct by analyzing the Ren Fang campaign inscriptions (see Chen 1956, pp. 301–304); these require that the 11th year of the current reign begin with day *yiwei* (32), *bingshen* (33), or *dingyou* (34); 1076 is almost the only possibility. Also, inscriptions with month dates in the set show a 10th year 9th month longer than one lunar cycle. If the year was 1077. And there was an intercalary 9th month, the first day of that month is the autumn equinox, which was supposed to be in the 9th month in a standard Shang lunar calendar.
- f. The Ren Fang set then reveals that the last day of the 10th year is called “Rong” 𠄎. We can use this information to interpret the *Xiaochen Yu zun* in the Brundage Collection in San Francisco, which is dated “day *dingsi* (54)... when the king was returning from his campaign against the Ren Fang, in his 15th year, *Rong* day”: This must be a different campaign, and different king, whose 15th year must end on day *dingsi* (54). The only reasonable possibility is the year 1091, and this shows that the Di Yi reign began in 1105, and lasted 19 years.
- g. The *Bamboo Annals* dates Di Yi to 1111. And gives him 9 years; thus what it has done is to move Di Xin back 10 + 6 years, by (i) taking ten years

- from Di Yi and giving them to Di Xin; and (ii) moving both Di Yi and Di Xin back 6 years.
- h. Therefore, we should try the assumption that the *Bamboo Annals'* date for Wenwu Ding, which is 1124, is also six years too early, and should be 1118.
 - i. These three dates, 1118, 1105, and 1086, can then be confirmed by a number of other inscriptions.
 - j. For 1068 (which is impossible anyway) see Nivison 1987.
- (3) The inscription that starts the Ren Fang campaign of 1077–76 ends with the words, “coincident with the *zai* 𠄎 sacrifice to Shang Jia.” The date is day *jiawu* (31), 29 September. This rite is the third in the late Shang sacrifice cycle, which must therefore have begun in 1077 on 9 September, day *jiaxu* (11). On the other hand, *Jiabian* 2416, also starting a campaign, has the dating information—let us now pay attention to all of it—“day *dingmao* (4)... 10th month, coincident with the *yi* sacrifice to Da Ding.” The mention of the sacrifice is the key to the problem. The proper day for a Da Ding sacrifice is the *ding* day in the *xun* following the *xun* of the corresponding proper day for Shang Jia, i.e., for the *yi* sacrifice, the *ding* day in the 28th *xun* of the sacrifice cycle. (See Shima 1971, pp. 556, 558.) It follows that the current cycle applying to *Jiabian* 2416 started on day *jiawu* (31), in late winter. Therefore, *Jiabian* 2416 cannot be dated by taking 1086 or 1068, both being relatively close to 1077, as first year.
- (4) Nor can it be dated by using 1105 as first year, if the following reasoning is accepted:
- a. The argument from the date of the autumn equinox (above) dates the 10th–11th year Ren Fang series as beginning 29 September 1077 (rather than 60 days later—a choice that in any case would require that both 1077 and 1076 begin with the *mao* (spring equinox) month, with an intercalation in late 1077; and this is very unlikely).
 - b. There is a routine sacrifice inscription fragment (Shima 92.1 and 417.3, *Xubian* 1.5.1) that has a cycle beginning on day *jiaxu* (11) with the next sacrifice (*ji* to Shang Jia) on day *jiashen* (21), in the 11th and 12th months, in the “3rd cult year.” The only possible 3rd year (on which there could be an 11th–12th month break between these two days) is 1084.
 - c. The normal Shang calendar would make the 12th month the winter solstice month. But in this case, consistency with the Ren Fang set requires taking

the 12th month 60 days earlier, 20 October—18 November, so that 1083 began with the *hai* month (the result of missing intercalations—which is easier to assume than the excessive intercalations that would be required for a later dating of the Ren Fang set). With this dating, we see that the ritual cycle first day was kept at *jiawu* in this reign, and was precessing at the mean rate of 21 days in 4 years.

- d. One would suppose that ritual cycle (*si*) ought to start, in each “cult-year” (*si*), in the normal Shang first month in the civil calendar, the *chou* (post-winter-solstice) month.
- e. If the same *ganzhi* day was kept as the first day of the ritual cycle, so that the beginning day of the cycle was allowed to precess each year, one can calculate that, for it to be at 9 September in 1077, it must have been precessing since 1105, the beginning of the preceding reign.
- f. In 1105, earlier inscriptions show that the ritual cycle began on day *jiazi* (1) (see below). The beginning day must have been kept at *jiazi* through the reign, because another routine sacrifice inscription, *Yi zhu* 376 (Shima 494.3), implies a cycle beginning on *jiazi* in the 11th month. The beginning day probably changed to day *jaxu* in 1086.

It is thus not possible for *Jiabian* 2416, requiring a cycle beginning on *jiawu* (31), and in late winter, to be dated in either the 1105 calendar or the 1086 calendar. (As for the 1068 calendar, even assuming a continuous use of 37-*xun* cycles after 1077, such a date would be too soon for the cycle’s first day to have returned to late winter.) The only possibility left is Wenwu Ding.

- (5) But which year of Wenwu Ding? *Jiabian* 2416 shows that the cycle was beginning at the proper time of year, in the *chou* month. To keep it beginning there, cycles of 36 and 37 *xun* must alternate (since $(36 + 37) \times 10 = 2 \times 365$). Three inscriptions with year dates can be dated to 1113, 1112 and 1111:
 - a. The *Feng yi* (text in Shima 1958, p.156): “*Yiyou* (22)... coincident with *Rong* 𠄎 day for Wu Yi, in the king’s 6th cult-year, *Rong* 𠄎 day.” Wu Yi’s proper day is *yi*-day in the *xun* after the corresponding proper day for Zu Jia, whose *Rong* is *jia*-day of the 24th *xun*. Therefore Wu Yi’s *Rong* is 241 days after the first day of the current cycle, which must be *jiashen* (21). There are two possible 6th years to consider: these are 1113, 6th year of Wenwu Ding, and 1063, 6th year of the (actual) Di Xin reign. Now 1063 would require that 1064 be a *jiashen* year, and this isn’t impossible (see Nivison 1987); however, if 1063 is chosen, the date turns out to be 10 May 1063, 7th day of the (*run*) *chen* month, and there seems to be no reason

for this day to have been a “*Rong* day” in the civil calendar sense. “*Rong* day” cannot mean “last day” of the lunar month or year here in any case; but if 1113 is chosen, the cycle began in the *chou* month on JD 131 4931, and the date of the inscription is JD 131 5172, which is 29 September. This would be the autumn equinox day in the *jieqi* calendar dividing the solar year into equal 24^{ths}; so I take 1113 to be the more probable date.

- b. Shima 244.2, *Yicun* 545: “Day *guiwei* (20) ... 5th [month, *jiashen* (21)], *zai* 𠄎 sacrifice to Zu Jia, in the king’s 7th cult-year.” The date must be 1112, 26 May, JD 131 5411, with the current cycle beginning on 5 February, JD 131 5301, on the *chou* month, on day *jiawu* (31)—the *zai* to Zu Jia being on the *jia*-day of the 12th *xun*.
- c. Shima 1958, p. 153 (*Jingzhang* 382, *Ku-Fang* 1661, *Jiabian* 297, also at Shima 92.1) “Day *guiyou* (10)... in the 3rd month, day *jiaxu* (11), *ji* 祭 to Xiao Jia and *zai* Da Jia, in the king’s 8th cult-year; day *guiwei* (20) ... in the 3rd month, day *jiashen* (21), *zai* to Xiao Jia and *xie* 𠄎 to Da Jia,” etc. The first occurrence of the word read here (and in Shima 1958) as “3rd” is unclear, on the break between two fragments, and recent study (“Oracle Bone Collections in Great Britain” I, 1, p. 6) takes it as “2nd”; it must be “3rd” (or a mistake for “3rd”), and one must assume a (delayed) intercalation at the 3rd month; or else the next “3rd” must be a mistake for “2nd.” The date is 1111, 12 March, JD 131 5701, with the current cycle beginning on JD 131 5661, in the *chou* month, on day *jiawu* (31)—the *ji* to Xiao Jia being on *jia*-day of the 5th *xun*.

Thus one year—the 6th (1113)—has the cycle beginning on *jiashen* (21), and then two years in succession—7th (1112) and the 8th (1111)—have the cycle beginning on *jiawu* (31). We should expect, therefore, that the next two years, the 9th (1110) and 10th (1109) will have the cycle beginning on *jiachen* (41). *Jiabian* 2416 requires a cycle beginning on *jiawu* (31). But ritual inscriptions in the Yu Fang set for the following spring require that the cycle ten begin on *jiachen* (41):

Shima 92.1, *Hou, shang*, 18.6: “... in the 3rd month, day *jiashen* (21), *ji* 祭 to Xiao Jia... this being when the king is returning from his campaign against X, lord of the Yu Fang”

The *ji* to Xiao Jia is *jia* day of the 5th *xun*, so the cycle must begin on *jiachen* (41), days earlier. If the date is 1110, the 9th year, the cycle began on JD 131 6031, in the *chou* month, here the first month of the civil year, and the inscription date is the 2nd of the 3rd month, 18 March, JD131 6071.

So we should read *Jiabian* 3939 as “9th year,” putting *Jiabian* 2416 in the 8th year, 1111. More confirmation is obtained by simple counting: 1114–1113, (21);

1112–1111, (31): 1110–1109, (41); 1108–1107, (51); 1106–1105, (1)—which was to be expected, from the precession argument above.

Where was Wenwu Ding leading his armies? The location of Yu is disputed, but most scholars (not including Shima, but including, e.g., Li Xueqin, 1959, pp. 92–93) would locate the Yu Fang southwest of Da Yi Shang, near modern Qingyang in Henan north of the Yellow River. The exact date of *Jiabian* 2416, if my reasoning is accepted, is 31 October, 1111 (JD 131 5934).

*

Obviously, in this paper I have ventured, with too little caution, to set forth conclusions that must remain debatable. I hope that my friends, especially our Chinese hosts, will be generous with their instruction. I thank those who have helped me already (especially Professors Edward L. Shaughnessy and David N. Keightley, and Sarah Allan); of course, I alone bear responsibility for any errors.

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6 Research Notes On Yin Li Chronology per Zheng Xuan

Research Note (22 June 1988)

Zheng Xuan says that Wen received the Cinnibar Writing portent, etc, signifying transfer of the Mandate, in the (Yin Li 殷歷 system) year “*Wuwu bu* 戊午步 29,” which is 1083 BC. For convenience, therefore, we can use *gongyuan* years in a tabular representation of Zheng’s complete concept of the chronology of Conquest-era events:

<i>Wuwu bu</i>	29		1083	Cinnibar Writing		
	30	1	Mandate	1082		
	31	2		1081		
	32	3		1080		
	33	4		1079		
	34	5		1078		
	35	6		1077		
	36	7		1076	Wen dies, age 97;	Wu = 83
	37	8		1075	Cheng born, age	1 84
	38	9		1074		2 85
	39	10		1073		3 86
	40	11	1	1072	1 st campaign	4 87
		12	2	1071		5 88
1		13	3	1070	Conquest	6 89
2		14	4	1069		7 90
3		15	5	1068	“Jin Teng”	8 91
4		16	6	1067		9 92
5		17	7	1066	Wu dies	10 93
6				1065	mourning 2	11
7				1064	mourning 3	12
8				1063	Zhou Gong flees	13
9				1062		14
10	Regency	1		1061	Zhou Gong back	15
11		2		1060		16
12		3		1059		17
13		4		1058	“Kang Gao”	18
14		5		1057	“Shao Gao”	19
15		6		1056		20
16 th	Conquest year	7		1055	“Luo Gao”	21
				1054	Cheng rules	

Most of this comes from Kong Yingda's commentary to the "Odes of Bin," introductory section, quoting Zheng Xuan's commentary to the "Jin Teng." (I have also used *Congshu jicheng* 3572, *Shang shu Zheng zhu*, pp. 60–61.) The importance of this commentary is that here Zheng double-checks his year sequences by matching one against another, concluding with the last year of the Regency being also (a) the 16th year counting from the Conquest, and (b) Cheng Wang's 21st year, counting from his birth, which is said to be in the year after Wen died. Therefore there is absolutely no question about the year Zheng takes as the year of the Conquest; it is not *Wuwu bu* 40, to which he dates a Wu Wang campaign against Shang, but two years later, i.e. 1070.

And since this whole scheme is Zheng's modification of the Yin Li scheme, modified by dating the first year of the Mandate as *Wuwu bu* 30 (= 1082) rather than as *Wuwu bu* 29 (= 1083); and since the obvious reason for the change is to get the Conquest in the "13th year of the Mandate," in agreement with Liu Xin, it follows *prima facie* that the Yin Li Conquest date was 1070. For if Zheng had not been trying to respect the Yin Li Conquest date, he could simply have let the Mandate count start with *Wuwu bu* 29, taking 1071 as the Conquest year.

As for Wu Wang's death date, Zheng says that Wen had seven years of Mandate, and that Wu Wang had seven years starting with the "white fish" incident, which occurred in the preliminary campaign in the 11th year. This puts Wu's death in the 17th year, as does the *Bamboo Annals*.

Is the dating in the *Annals* following Zheng, or is he following a previously accepted chronology? If the former, then the tomb text of the *Annals* could have had 1045 as Conquest date, the Jin Dynasty editors then moving the "15th–16th–17th" year slip from Cheng to Wu. If the latter, then these changes in the *Annals* text were made sometime in Eastern Zhou. In my 19 October '87 note, I assumed the former.

I now doubt this. It is true enough that Zheng gets "17th year" because his premises require it, and we know them as Han premises:

- (1) *Shiji*, and probably Yin Li, say Wen had 7 years, of mandate
- (2) *Li ji* says Wen died at 97, Wu at 93
- (3) *Da Dai Li ji* says Wu born when Wen was 15 *sui*

From (2) and (3), Wu was 83 when Wen died; therefore by (1) he was 83 in Mandate 7; and therefore he was 93—his age at death—in Mandate 17.

But the present text of the *Annals* rejects (1) and (2), and implies that (3), too, is false (if we assume the tradition that Wen was already alive when Dan Fu died). Jin Dynasty editors would have had no reason to accept Zheng's conclusion. It is as reasonable to suppose that the "17th year" as death date was an

Eastern Zhou error taken from an Eastern Zhou Annals text, and that the traditions in Zheng's premises were in part prompted by that error.

Did Liu Xin invent the idea of a "Mandate calendar"? It doesn't matter. I know now that the *Annals* at some stage—I think this must have been a middle or early Eastern Zhou stage—did have the Conquest dated in a calendar that began in 1056; this was a royal calendar, not a Mandate calendar, but it did begin before Wen died. Wu continued the calendar after Wen died, but we do not need the "Mandate calendar" concept to explain that. Wu could have been simply holding off from promulgating a new calendar until after he completed his father's work; this is suggested by his behavior in the first assembly in Mengjin. Or Wen could have named his son and heir alternate "*wang*" in 1056, following what I have argued was Shang practice.

Dong Zuobin had good reason to take 1070 as the Yin Li Conquest date. Further, Chen Mengjia does not say that the Yin Li year for the Conquest was 1076 (*Buci zongshu* p. 212); he says that the statement in Han apocrypha that "the reign of the Azure Emperor lasted 820 years" yields the date 1076 as the first year (256+820); in context it is probable that he understands this as Wu Wang's succession year. (For Zheng Xuan, the succession year is 1075; he could get this by adding 256 and 820 inclusively.)

Incidentally, Zheng's taking the "11th year" campaign of the *Shu* "Preface" to the "Tai shi" as being *Wuwu bu* 40 is indirect confirmation (a) of my claim that he is following Liu Xin, and (b) of my claim that Sima Qian misinterprets his sources and does put the Conquest in the 11th year. At least, that is what Liu Xin thinks Sima Qian means: Liu takes the "Tai shi" to be the "Tai shi" referred to in the "Qi Shijia" as in the 9th year rendezvous campaign, and so he "corrects" the *Shiji*; the *Shiji*'s "9th year" should be "11th year," since obviously Sima Qian did not know that Wen had nine Mandate years, not seven; so the Conquest two years later must be the 13th year. (But I have to grant that Liu understands the *Shiji* sequence of dates as in a continuous calendar, whatever Sima Qian himself thought.)

Thus Liu is understanding the *Zhou Benji*'s date for the river-crossing before Muye, the "12th month of the 11th year," as meaning the beginning of the 11th year, not the end (and so, in the *Shu* "Preface," explicitly the "first" month). "Corrected," this becomes the date of the first campaign. Zheng does exactly the same thing. We see that this was the standard Han way of understanding dates, and we should therefore assume either that Sima Qian understood his dates this way, or that two different people wrote the "Benji" and "Shijia" sections of the *Shiji*, or that someone has "corrected" the "Shijia" texts after they left Qian's hand. I took the second alternative in *HJAS* 43 (being then unaware of the first

possibility); later in *Early China* I took the first alternative. All three are possible; but if we consider just what the *Shiji* says, and what it seems to be, we have to say that Sima Qian misread his texts, twice: he read the dates wrong; and he assumed a new calendar for Wu.

Should we say that someone living as close after the Tai-chu reforms as he did ought to have seen that this way of reading old dates was wrong? Not necessarily: It is likely that Tai-chu merely officialized entrenched ways of talking, in taking what everybody was calling the “first month”—the Xia first month—as the official first month; and that this ordinary way of talking had come about from the multiplicity of different state calendars in late Warring States China: to avoid confusion, one would learn to say that state A’s year began with the 10th month, state B’s with the 12th; and so, if you were talking about state A, “nth year 11th month” would be the second month of A’s year n.

Research Note (19 October 1987)

Two weeks ago Edward Louis Shaughnessy (in a phone conversation from Chicago) was defending his contention that my doubts about 1045 as Zhou Conquest date are unjustified.

He argued that Zheng Xuan (127–200) puts the date of the “Mandate” at *Wuwu bu* 29, and the date of Wu’s attack on Yin in the 11th year (as stated in the *Shu* “Preface” to the “Tai shi”) in *Wuwu bu* 40. This should be the 12th year of the Mandate. So Zheng confirms the “Mandate Calendar” concept, and shows that he would reject the possibility I have pointed out, of taking the Conquest date (whether 11th year or 12th year) as in a Wu Wang calendar starting after the death of Wen Wang. And Zheng Xuan ought to be respected.

I have examined Zheng’s position as analyzed by Kong Yingda in Kong’s long comment at the beginning of the “Da Ya” and elsewhere. A very different picture of what Zheng thought emerges from this analysis.

1. In using the “*Wuwu bu*” (etc.) dating system, Zheng is accepting the Yin Li chronology of the Conquest, with certain modifications. *Wuwu bu* 29 is 1083, and *Wuwu bu* 40 is 1072.
2. The modifications:
 - (i) For the Yin Li, 1084 is the last year of Shang (the 496th year, counting from 1579). For Zheng, Zhou begins with the Mandate calendar which for him takes as year #1 the year following the portent of Wen’s receiving the Cinnibar Writing from the Red Bird, which occurs in *Wuwu bu* 29; in this sense, for Zheng the Shang lasts 497 years, and Zhou starts in 1082, i.e., *Wuwu bu* 30. Kong gives long and careful arguments to show

that this is in fact Zheng's chronology. We must ask why Zheng departs from the Yin Li in this way.

- (ii) Another apparent discrepancy is the date 1072 for the Conquest, if that is what is meant; for the Yin Li date is 1070. This too requires explanation.
3. The key to both problems is that Zheng Xuan is trying to reconcile the Yin Li Chronology with that of Liu Xin, while (of course) rejecting Liu's absolute dating. Liu had put the Conquest in the 13th year of a supposed Mandate calendar; and if Mandate #1 is 1082, and the Conquest year is 1070, then the Conquest was in the 13th year of the Mandate calendar. We see at once that Zheng may not have regarded *Wuwu bu* 40 (1072) as the Conquest year after all.
4. And in fact he did not. This can be proved out of Zheng's own mouth:
 - (i) At one point, Kong quotes him as saying (in Zheng's commentary to the "Luo Gao") that "when Wen Wang got the Red Bird and when Wu Wang looked down and took the White Fish, both had seven years." The White Fish incident occurred as Wu was crossing the Yellow River at the time of the preliminary meeting with Zhou allies at Mengjin two years before the final conquest expedition, according to the "Zhou Benji."
 - (ii) Kong later quotes Zheng's commentary to the "Jin Teng" as repeating the familiar (mythical) vital statistics on Wen and Wu: Wu was born when Wen was 15 (presumably *sui*); Wen died at the age of 97, Wu then being 83; Wu died at 93, according to this mythology.
 - (iii) If Mandate year #1 was the year after *Wuwu bu* 29, i.e., *Wuwu bu* 30, the Wen died in *Wuwu bu* 36 (i.e., in 1076) according to Zheng, when, as he says, Wu was 83. But if Wu's "attack" on Shang (*Shu* "Preface" to "Tai-shi") was in *Wuwu bu* 40, i.e., 1072, then Wu was 87 in *Wuwu bu* 40. If he had seven years starting with the year of the White Fish portent, and died at 93, the White Fish portent must have been when Wu was 87, i.e., in 1072.

Therefore, for Zheng, 1072 was not the year of the Conquest, but was instead the year of the preliminary campaign. Lest there be any doubt that the foregoing analysis does in fact give Zheng's chronology of the Conquest, it can be confirmed from the fuller quotation from Zheng's commentary to the "Jin Teng" found in Kong Yingda's long commentary at the beginning of the "Odes of Bin" in the *Shijing*:

Wen Wang in [his] 15th year produced Wu Wang. In [his] 97th year he died. At the time of his death Wu Wang's [years] were 83. In Wen Wang's receiving of the Mandate this was

the 7th year. 6 years later [Wu Wang] attacked Zhou [Xin]. 2 years after this he became sick. 2 years after he recovered from his illness he died. At the time of his death he was 93. In the year after Wen Wang died Cheng Wang was born....

(This long quotation from Zheng's commentary to the "Jin Teng" goes on to give Zheng's highly unusual chronology of events down to the end of the Regency.) It is clear from the above that the Yin Li date for the Conquest is accepted by Zheng. The only departure from the Yin Li is his dating the beginning of Wen's final seven Mandate years with the year after his receiving the Cinnibar Writing rather than in that same year.

But the *Shu* "Preface" dated the "Tai shi" to the "11th year, 1st month," when the Zhou forces had crossed the Yellow River on the day *wuwu* (55); and the "Zhou Benji" in the *Shiji* also dates the "Tai shi" the same way, except for making it the "12th month," and for the "Zhou Benji" this is the final Conquest campaign. So at this point Zheng departs from the *Shiji*. Why?

What he must be doing is, like Liu Xin, following the *Shiji* "Qi Shijia" in part: It quotes an address by Wu at the Mengjin meeting in the preliminary campaign, and refers to it as this "Tai shi." Liu appears to be picking this up, then using the *Shu* "Preface" to "correct" the date to "11th year," so that for him the "Tai shi" address is made at the River-crossing in the preliminary campaign, and the date of that campaign is changed to the "11th year," with the result that for Liu the Conquest is in the "13th year." This is as it must be if his "Quail Fire" theory is to be valid. That theory—which Liu apparently constructed out of the astronomical fact (available to him we don't know how) that Jupiter was in Quail Fire in the "Mandate" year, and the claim from the forged *Guoyu* text that Jupiter was in Quail Fire at the time of the launching of the final Zhou campaign—is what forces Liu both to date the Conquest to the "13th year" (after one Jupiter cycle of 12 years), and also to take this and other dates to be in a supposed "Mandate" calendar. Evidently Zheng follows Liu in both matters, the 13th year date and the Mandate calendar concept, though as far as I know he avoids any mention of Jupiter and Quail Fire.

If this is right, there is no reason to rely on Zheng Xuan as validating the Mandate calendar concept of the dating of Conquest events, and still less reason to take him as validating the "12th year" as Conquest date in such a calendar. The oldest available sources—*Lü Shi Chunqiu*, *Zhushu jinian*, *Shiji*—unequivocally put Conquest dating in a Wu Wang calendar. And the two oldest also unequivocally say that the year was Wu's 12th year, as probably did Sima Qian's sources, though he appears to have misinterpreted them. All evidence points to the conclusion that the Mandate calendar idea was Liu Xin's invention.

The only question remaining is whether the Wu Wang calendar began in 1051, which must then have been his succession year, or in 1056, which year must have another explanation. In the first case the Conquest must be dated to 1040, in the second to 1045. (For analysis of lunar phase dates in *Shang shu* chapters shows that 1040 and 1045 are the only possible Conquest dates.)

Reconstructing Zheng Xuan's Conquest chronology is not, however, an exercise with merely negative results; there is an unexpected dividend: If Wu died at the age of 93 for Zheng, and if Wu was 83 at the time of his father Wen's death, which for Zheng was year 7 in the Mandate calendar, then Wu's death was in the 17th year. That is the year of Wu's death in the present state of the *Zhushu jinian* text (although there it is Wu 17, not Mandate 17). Zheng, then, appears to be the source of the *Zhushu jinian* editors' motive for moving a bamboo slip from Cheng Wang to Wu Wang to get the 17th year date. And since we can give a coherent explanation for Zheng's arriving at the 17th year as the date, strictly in terms of beliefs he would have held, some of which—i.e., the extent to which Zheng follows Liu Xin—could not have been available to possible Late Warring States editors of the *Zhushu jinian* text, this is strong reconfirmation for ELS' view that the tomb text of the *Zhushu jinian* had Wu dying in his 14th year; and it effectively refutes the possibility I had been considering, that the misplacing of the slip that gives the 17th year as the year of Wu's death might have been done before the *Zhushu jinian* was placed in the tomb in 296 BC.

[It would make sense to fill in Zheng's thinking in a different way:

- (1) With the Yin Li school, he accepts the length of the reign of the "Azure Emperor" (i.e. the Zhou Dynasty) as 820 years, but reads this inclusively, getting 1075 for the beginning of Wu's reign rather than 1076.
- (2) Since he follows the *Shiji* and the *Shang shu Dazhuan* in giving Wen seven years of "mandate" rule, it is this that forces him to redate the end of Shang to 1083, and to begin his Mandate calendar in 1082; i.e., it is not a desire to validate Liu Xin's 13th year as Conquest date. "13th year" just happens to be the result.

But I think Zheng's thinking went the other way. For consider the extent to which he agrees with Liu: The Conquest was in the 13th year. The "Taishi" was delivered at the preliminary meeting in the "11th year." And all these dates are in a "Mandate" calendar. His readjusting of Yin Li chronology must have been the means to his end, and not the end itself, which had all these agreements with Liu as mere accidents.]

7 A Tell-tale Mistake in the *Lü shi Chunqiu*: The Earthquake Supposedly in the Eighth Year of Wen Wang of Zhou

In chapter 6 of the *Lü shi Chunqiu*, titled “Last month of Summer,” the fourth section is titled “*Zhi yue*” (制樂), meaning approximately “(good) government and (harmonious) music”—but although other sections do have something to do with music, the word “*yue*” here perhaps must have its other reading and sense, *le*, “felicity.” The section recounts three incidents, concerning Tang of Shang, Wen Wang of Zhou, and Duke Jing of Song, and in each case good words and acts of the ruler are rewarded by a reversal or negation of misfortune.

The story about Wen Wang reads as follows:


When Wen Wang of Zhou had been ruler for eight years, in the sixth month of the year (*sui liu yue*) Wen Wang went to bed sick. Five days later there was an earthquake, and to the east, west, south and north, it did not go beyond the capital suburbs. His officers were all frightened and said, “Please let us avert the curse.” Wen Wang replied, “How?” they answered, “We could mobilize the people and extend the city walls; wouldn’t that avert it?” Wen Wang said, “No. Heaven displays an evil omen in order to punish someone who is guilty. I must be the guilty person, and heaven is doing this to punish me. If I were now to mobilize the people to extend the walls, this would double my guilt. This won’t do.”

Wen Wang then carried out a reform of his conduct of rites and administration; whereupon,

In no time at all, his sickness stopped. Wen Wang had ruled for eight years when the earthquake happened. Forty-three years after the earthquake, when he had ruled in all fifty-one years, he died. This was Wen Wang’s way of cutting short calamity.

I shall neglect the quaint but familiar religious aspects of this story. The problem that draws my attention is that Wen Wang did not reign for 51 years. As I think I have proved (“The Dates of Western Chou,” *HJAS* 43, 1983), his reign in Zhou began in 1101 BC, and after two years completing mourning for his father Ji Li,

Paper presented at the annual meeting of the Western Branch of the American Oriental Society, University of Colorado at Boulder, October, 1989

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<https://doi.org/10.1515/9781501505393-007>

Wen's official calendar began in 1099; he died in 1050, thus reigning a total of 2+50, or 52 years.¹ Why does the *Lü shi Chunqiu* get the reign-length wrong?

We discover why when we examine the record of this earthquake in the *Bamboo Annals* (*Zhushu jinian*). There it is recorded as having occurred in the sixth month of the 3rd year of the next-to-last Shang king Di Yi. But as I have also shown (in the same article), the *Annals'* reign for Di Yi, said there to be nine years, is ten years short; it was actually 19 years, 1105–1087.² Therefore, the record of the earthquake should read “13th year” rather than “3rd year,” making the actual date 1093. But 1093 was not the 8th year of Wen Wang. In his official calendar it was the 7th year, while counting from his succession it was actually his 9th year. This corresponds to the mistaken reign length: not 51 years, but 50 years in his official calendar, and 52 years actually.

Now notice the way the *Lü shi Chunqiu* account reads: “When Wen Wang of Zhou had been ruler for eight years, in the sixth month of the year (*sui liu yue*) Wen Wang went to bed sick ...” Why the repetition of “Wen Wang” here?

I submit that the author of this essay must at this point have been copying from a lost source, that read something like this: “In the 8th year of Wen Wang ... *sui*, 6th month, Wen Wang went to bed sick ...” The narrative from which the copying was done must have had some event in the 8th year, either before the sixth month of that year or undated as to month, which has been omitted as irrelevant to the writer's interest in the *Lü shi Chunqiu* account. It would be natural for this original account, when proceeding to a new episode, to repeat “Wen Wang.” The writer of the *Lü shi Chunqiu* essay simply copies this. But also, as he copies, he supposes that “*sui*” in *sui liu yue*, the opening phrase in the episode that interests him, simply resumes “8th year,” thus meaning “in that year”; whereas actually it must mean “after a year.” That is to say, the earthquake actually took place in the sixth month of the 9th year, which was indeed 1093, the 13th year of Di Yi. The year of Wen's death, which we know to be 1050, is then exactly 43 years later—as the

1 For Wen's dates, see especially *HJAS* 43:517–524. The *Bamboo Annals* account gives Wen 52 years of reign, 1113–1062, his death occurring 9 years after a conjunction dated 1071. This conjunction has been back-dated 12 years (which would be one Jupiter cycle), from 1059. That Wen had a calendar beginning in 1099 is proved by lunar eclipse recorded in the “Xiao kai” chapter of the *Yi Zhou shu*, as being in Wen's 35th year; the eclipse was actually in March of 1065 (see *HJAS* 43:521).

2 For Di Yi's dates, see *HJAS* 43:558, note 87, for part of the argument. Oracle inscriptions for a campaign in a late Shang king's 10th and 11th years (Chen Mengjia, *Yinxu buci zongshu*, Beijing, 1956, pp. 301–4) can be dated to 1077–76 (see *HJAS* 43:501), so the first year of the reign, which can be shown to be that of Di Xin, was 1086.

writer's source text must have told him, without saying that the total reign was 51 years, which it was not.

This conclusion can be confirmed by examining another dated event in the *Bamboo Annals*. The present text dates the death of Wu Wang of Zhou to the 12th month of his 17th year. But Professor Edward Shaughnessy (*HJAS* 46 (1986)) has proved that the date should be the 14th year, and that the distortion in the *Annals'* chronology results from a bamboo slip having been moved from the Cheng Wang chronicle to the end of the chronicle for Wu Wang.³ This means that in the *Annals* as originally written, Wu died just two years after the Conquest of Shang, at the very end of the year.

One now can compare this with an account of Wu's death in the *Yi Zhou shu*, "Zuo Luo" (chapter 48). This says that Wu after the Conquest made the Shang prince Lu Fu ruler of the Shang to continue the Shang sacrifices; then Wu established three of his own brothers, Guan-shu and two others, as overseers of the conquered lands and of the Shang officials. The account continues, "Having done this, the king returned home, and then *sui* 12th month died in Hao (the Zhou capital). Another part of the *Yi Zhou shu*, "Da Kuang" (chapter 38) starts out, "13th year: The king was in Guan, and with Guan-shu personally functioning as overseer of Yin (i.e., in the ceremony), the lords of the eastern regions all received gifts from the king..." If Wu Wang did indeed die at the end of his 14th year, we see from this that the phrase "*sui* 12th month" must mean "a year later, in the 12th month." This is the meaning that the commentator Kong Zhao gives for this phrase, and he is now proved to be right.⁴ (And I, I must admit, was wrong in "The Dates of Western Chou," for there I interpreted *sui* as meaning "in the same year," but specifically in the Xia calendar; thus I dated Wu's death to the *chou* (post-solstitial) month at the *beginning* of 1043, rather than to the 12th month at the *end* of the year.)

Interestingly, we see that although the word *sui* does not appear in the *Bamboo Annals*, had its author seen the word (as he may well have) in the dates in the other two texts—the *Yi Zhou shu* and the source text for the *Lü shi Chunqiu* account—he probably would have interpreted it correctly.

There is one other case like these, where *sui* must have been used correctly in some early text recounting events that are also recounted in the *Annals* without

³ Edward L. Shaughnessy, "On the Authenticity of the *Bamboo Annals*," *HJAS* 46:149–180.

⁴ Kong Zhao worked in the middle of the +3rd century, probably before the recovery of the *Bamboo Annals*. Some editions give a variant text: "a year being completed" (*cheng sui*), instead of "then, in a year" (*nai sui*). My argument is in effect that *sui* alone in a date means *cheng sui*.

using the word *sui*, and again the account in the *Annals* is correct,⁵ while the source text has been copied into a text we now have, in such a way that *sui* is taken incorrectly to mean “of this year” rather than “after a year.” This case thus is like the error in the *Lü shi Chunqiu*, and it must be similarly late. The publication date of the *Lü shi Chunqiu*, give in the book itself, was 239 BC.

This text that we now have, and that is very familiar to us, is—shockingly—the “Shun dian” chapter (chapter 2) of the venerable Classic, the *Shang shu* or *Book of Documents*. The word *sui* is found in the date of the first of a series of imperial tours of inspection that must be in either the first or the second year of the Emperor Shun, after Yao’s abdication in his favor. To see the problem clearly, it will be useful to compare the accounts of the beginning of Shun’s tenure in the *Shang shu* and in the *Bamboo Annals*:

Yao, 70th year:

Shu: Yao announces his intention to resign, and tentatively accepts a recommendation that Shun be his successor, then (this year or the next) gives his two daughters to Shun in marriage.

Annals: Yao appoints Shun chief minister, effective the first month of the year.

Yao, 71st year: *Annals*: Yao gives his daughters to Shun.

Yao, 73rd year: *Shu*: Yao says he has examined Shun for three years and has found him worthy; on the first day of the first month, he abdicates in Shun’s favor. The three years thus must be years 70, 71, and 72. Shun performs various rites and sacrifices, calls in the nobles’ tokens of office and formally reissues them; then,

sui 2nd month, Shun made a tour of inspection to the East,...

and from the context this must be the same year; because father on, the *Shu* says that Yao lived 28 years more and then passed away; and hagiography would require this to be in his 100th year.⁶ Also, it is said after the description of the four inspection tours to the four sacred mountains, in the four directions and spread

5 In saying that “the account in the *Annals* is correct,” I do not wish to imply that I take that account as true. Rather, it is correct in its interpretation of the probable source or sources used. The Yao-Shun story, both in the *Annals* and in the *Shang shu*, is mythical, probably even to the names “Yao” and “Shun.” But I would argue that the myth has been superimposed on actual history, some of which can be recovered, even to exact dates, from a sufficiently careful analysis of the *Bamboo Annals*.

6 It is true that Cai Shen (1167–1230) has Yao’s reign 101 years long; but he does this by an exclusive count (73 + 28), not by recognizing *sui* as referring to year 74 and counting inclusively (see E. Chavannes, *Mémoires Historiques* 1:69, note 1; the *Shiji*, “Wu di benji”, supports Cai). Both the pseudo-Kong Auguo commentary and the (Tang Dynasty) Kong Yingda commentary to the *Shang shu* take *sui er yue* to be the very next month after the reissuing of tokens, assumed to be in the first month.

through the year in the equinoctial and solstitial months, that “in five years there was one [set of four] tours of inspection, and four receptions at court of the nobles.” We would assume (and ancient commentators agree) that the tour (in four parts) is here supposed to occur in year 2, 3, 4, and 5.⁷

Annals: Yao abdicates in favor of Shun, in the first month of the year.

There is nothing else in the *Annals* for Yao’s 73rd year. In that account, the tours of inspection are dated to Yao’s 74th year. We see, therefore, that if the composer of the “Yao dian” and the “Shun dian” and the compiler of the *Bamboo Annals* were using the same source text or related ones, the former has misunderstood *sui* as meaning “of the year,” and the later has understood it correctly as meaning “after a year,” or “in the next year.” As we might expect, in the *Annals* Yao does die in his 100th year of (honorar) reign.

I think we can conclude, therefore, that even though there are some very old texts in the *Shang shu*, the opening chapters are unimpressively late. The “Yao dian” and the “Shun dian” are not Western Zhou texts. They are not early Eastern Zhou texts. They are not even early Warring States texts. They are probably quite late Warring States texts, although they probably do use—or misuse—earlier material.⁸

Note: Once the problem of the correct interpretation of the word *sui* is resolved, another problem in the “Shun dian” text resolves itself. Much discussed is the meaning of the sentence that we find just before the account of the four inspection tours:

7 “In five years there was one (set of) tours of inspection and the many lords four times came to court”: *Shang shu* text as quoted in *Shiji* (1.18a of Taipei Yiwen shuju edition). The *Shiji jijie* commentary of Pei Yin (+5th century) at this point quotes Zheng Xuan (Eastern Han), as saying “in the (recurring) year of the tours of inspection, the feudal lords had audience at the foot of the sacred mountain of their region; in the our intervening years, the lords of the four regions came to court in turn at the capital.”

8 Earlier material: Not very much earlier, in my view: the literary articulation of the Yao-Shun myth is post-Confucius. The praises of Yao and Shun by Confucius in the *Analects* were probably added at least 50 years after his death. (They are most conspicuous at the end of Book 8, which celebrates Zengzi and must have been composed after the latter’s death in 435 (argument by Bruce Brooks).) The rewriting of the *Annals* to give Yao 100 years of reign, which is a part of the hagiographic transformation of Yao, requires redating of the solar eclipse of the 5th year of Zhong Kang of Xia from 1876 to 1948 BC, and this can be proved to be not earlier than 427 BC (see D. S. Nivison and K. D. Pang, “Astronomical Evidence for the *Bamboo Annals*’ Chronicle of Early Xia,” to appear in *Early China* 15, 1990).

輯五瑞，既月，乃日觀四岳群牧，班瑞于群后
Ji wu rui ji yue nai ri jin si yue qun mu ban rui yu qun hou

If we suppose that this too was copied verbatim from an earlier source, and that this activity does not have to be in the first month or early in the second month, but can be spread through the year, then we can take it thus:

He called in the five kinds of tokens of authority, and when the appropriate month had come (sc. for the reception of nobles from the part of the world due to be audiened in that month), he then day by day gave audience to the lords of the four peaks (i.e., the four regional overlords, in the scheduled month of each), and to the many pastors (i.e., subordinate lords), and redistributed the tokens to the many lords (i.e., both overlords and lower lords).

The emperor's activities of the first year, therefore, were first to pay respect to the spirits (first month), and then (throughout the rest of the year) to reconfirm, under his own authority, the various lords of realm in their various offices and domains. Only after that did the repeating five-year cycle of tours and audiences commence.

NIVISON: "TELL-TALE MISTAKE," W.B. ROS, 15 OCT '29

1. 呂氏春秋：制樂

周文王立國八年，歲六月，文王寢疾。五日而地動……
文王即位八年而地動；已動之後四十三年，凡文
王立國五十一年而終。……

2. 竹書紀年：帝乙

三年…夏六月，周地震。

3. 逸周書：作雒，第四十八

武王克殷，乃立王子祿父，俾守商祀。建管叔
于東…俾監殷臣。王既歸，乃歲十二月崩鎬。

孔曰：乃歲，乃後之歲也。

4. 尚書：舜典

(a) 正月上日，受終于文祖。在璿璣玉衡，以齊
七政，肆類于上帝，禋于六宗，望于山川，徧于
群神。輯五瑞，既月，乃日覲四岳群牧，班瑞
于羣后。

(b) 歲二月，東巡守，至于岱宗，柴，望秩于山川，肆
覲東后…五月南巡守…八月西巡守…
十一月朔巡守…

孔傳：諸侯爲天子守土，故稱守巡行之。既班
瑞之明月，乃順春東巡…

孔穎達疏：正義曰，舜既班瑞羣后，卽以其歲二月
東行，巡省守土之諸侯…

(c) 五載一巡守，羣后四朝…

8 The Origin of the *Chaochen* Rule

I am distributing my paper. If I declaimed it to you in the short time we have, you would not understand it, and neither would I. In the next few minutes, I will try to say enough about it so that you can decide whether to take it home and read it.

Liu Xin, d. 23 CE, was a kind of Chinese polymath—historian, astronomer, metaphysician, cosmologist, bibliographer, dismally unsuccessful politician (this did him in). He was perhaps the first historian anywhere to try to pin down dates of events in the remote past by recalculating astronomical phenomena referred to in old texts.

One of these was a paragraph in a book called the *Guoyu*, “Dialogues of the States,” an anonymous “history” consisting mostly of long conversations between kings, dukes, ministers, etc., most of it compiled in the –4th and –3rd centuries. I think that the paragraph in question was composed in the –1st century and sneaked into the text at that time; but Liu took it at face value. This paragraph contains a run-down of the zodiac positions of Jupiter, the sun and the moon, etc., on the day when King Wu of Zhou launched his victorious campaign against the Shang Dynasty. (The astronomy may have been worked out centuries earlier, and, merely used here; but that does not concern me.) The date of this event was, and has remained, controversial. Liu attempted to settle the matter scientifically, by astronomy. In conjunction with this effort, he worked out a complete mathematical system of positional astronomy, that could be used to calculate the position of any planet at any time in the past. I am concerned with his rule for Jupiter.

In my paper I have translated his rule, and I have worked out several demonstrations of it. I have done this for my own convenience, and perhaps yours; it is not new research. I will not go over it now. The rule makes a small improvement in a piece of general knowledge about Jupiter. The Chinese called Jupiter “*sui xing*,” the “year star,” because it was supposed to move on the average the equivalent of one zodiac space a year, completing a circuit of the sky in 12 years. Liu’s rule in effect said that Jupiter actually “jumped a space” (*chao chen* 超辰) every twelve such 12-year cycles, i.e., that it traversed 145 spaces in 144 years. Liu is to be commended for seeing that the popular view was wrong,

February 28, 1990; Paper presented at the annual meeting of the American Oriental Society, Berkeley, CA, March 4, 1990

but he was wrong himself, because the actual “jump” is about one in every seven cycles.

I am interested in where he got his rule, and (perhaps the same question) why this wrong rule satisfied him. All I do (or can do) is to give you a possible explanation. It is the best one I can think of. I invite you to find a better one.

Even to be aware that the popular 12-year rule was wrong, Liu, or someone before him, must have had records thought to be reliable, mentioning or implying Jupiter’s zodiac position in some determinate year many centuries earlier. For then, simple arithmetic would show that the cycle could not be an exact 12 years, and would show at once exactly what it really is. And of course, if the record were wrong, albeit believed to be right, then one’s arithmetic would give a wrong answer.

I propose that this is exactly what happened. But the record in question was one that lay buried in the tomb of king Xiang of the state of Wei from 299 BC until 281 CE. So if I am right, Liu’s rule was “discovered” (shall we say) all of three centuries before Liu’s own time. Here things get controversial, because the record is the so-called “Bamboo Annals,” *Zhushu jinian*, which many take to be a Ming Dynasty fake. It purports to be a chronicle covering about 2000 years, with exact dates of events. Recent work, some of it by Edward Shaughnessy and some of it mine, proves, I think, that it is not a fake, though most of the dates in it prior to the late –9th century are at least slightly wrong.

This chronicle records a conjunction of the planets in the so-called “lunar lodge” Fang, about at Antares, in the 32nd year of the last Shang king Di Xin. (The conjunction was actually 12 years later and 4 Jupiter stations prior, as Needham and Pankenier show.) Fang is the middle space in the Jupiter station Dahuo, “Great Fire,” #10 as I have the stations numbered on the last page of my paper (#11 if you use Liu Xin’s numbering, used in my examples). One of these planets has to be Jupiter; so the *Annals* says that Jupiter was in Dahuo in 1071.

And this must have been trusted in Wei in the late –4th century. The *Annals* also says that the Jin state, which Wei succeeded, was founded on a date converting to 1035 BC—when (since the 12-year rule works in the short run) Jupiter would also be in Dahuo; and when Duke Ying of Wei proclaimed himself King Hui-cheng, he took the year 335, just seven centuries later, as his first year as king. Further, his successor King Xiang thought so highly of the *Annals* that he had it buried with him. We read, moreover, in another part of the *Guoyu* that Jupiter was in Dahuo when Jin was founded. (But alas! Actually Jupiter was in Dahuo in late 1032 and in 1031, not 1035.)

So I infer that it was believed in high places in Wei in the late 300’s BC—on the basis of a text that probably was thought of as an official Wei state chronicle.

cle—that Jupiter was in Dahuo in 1035 BC. But anyone watching the sky in the year exactly 12×60 years after 1035, i.e., in 315, would see that Jupiter was not in Dahuo, but was five stations farther on. Simple arithmetic would show at once that Jupiter's cycle was not exactly 12 years. 12×60 is 144×5 , and in that many years, it would seem, Jupiter had traveled $5 + (144 \times 5)$ stations, i.e., in each 144 year it had traversed 145 stations. If this ratio, revealed by text and observation, were accepted, then King Xiang could sleep in peace: His state chronicle was accurate, and using it had enabled the scientists of the day to get their latest results. Or so I suppose.

For this is exactly Liu Xin's formula. And this is the best explanation I can think of, of where he might have gotten it. But, necessarily, indirectly: Liu was Han court bibliographer, and has left a famous catalog of the imperial library, which was *the* library. If there had been a copy of the *Bamboo Annals* above ground, he would have listed it. So if this is his ultimate source, the rule must have been passed on among astronomers down to Liu's time. But here there is another puzzle, because every astronomer or astrologer before Liu whose beliefs are known—including pointedly Sima Qian, historian and court astrologer to Emperor Wu Di—says that Jupiter's cycle is 12 years.

I tentatively infer from this a deeper truth about Chinese science (and surely this is nothing new). In the ancient view of things, it seems to me, there was a set of ideal truths about the world, one being "Jupiter cycle, 12 years." These did not cease to be *true* merely because the specialist had to use his little rules of thumb, like the 144:145 ratio, to adjust the ideal rule to messy empirical reality. It would follow that, usually, scientific progress lags not a little behind scientific discovery.

The point of my paper, however, is only to show you the possible explanation for the Jupiter rule that Liu Xin adopted, that I found by reflecting on the *Bamboo Annals*, with the cautious claim that it is the best explanation available. It would not be fair for me to stop without pointing out that I base my work on two controversial claims:

- (1) I am claiming that the "Present text" *Bamboo Annals* is not the post-Song fabrication or reconstitution that most leading Chinese scholars of the past three centuries have thought it to be, but is for the most part, with some minor changes, the very text that was found in the Wei-state tomb or repository in 281 CE. I invite discussion of this, but I believe that recent scholarship, especially that of Edward Shaughnessy, places this claim beyond reasonable doubt.
- (2) Further, crucial to this paper, and here even Shaughnessy would not agree, I hold that the dates of events and reigns in this "Present Text" are the same

as the dates expressed or implied in the text as recovered in 281 CE. These dates are not expressed the same way now as in the recovered text; that we can prove, but it is a trivial point. I think I have ample proof that the dates are the same, but I invite discussion of this too.

And I point out one more matter: You can read my paper inside out, so to speak, as a modest confirmation of these two claims; for from them I derive a result, that those in Wei who esteemed this text enough to give it the treatment they gave it in the late –4th century must have concluded that Jupiter really goes through 145 stations in 144 years. I then match this result with an independently known fact, that this is precisely Liu Xin's ratio. One can then reason that this correspondence between result and fact is not likely to be accidental. And if it is not an accident, then the *best explanation* of this correspondence is that my two claims about the "Present Text" *Bamboo Annals* are true.

Appendix: *Chaochen* Rule Explanations

[To Liu Xin (d. 23 CE) is due the enduring belief that the Zhou Conquest of Shang occurred in 1122 BC. His argument rests heavily on incorrect astronomy, notably his “*chaochen*” (jumping a *chen*-space) rule for calculating earlier positions of Jupiter. Here I first explain the rule, and then argue that it may have been posited three centuries before Liu Xin.]

M. Teboul, “Les Premiers Développements de l’Astronomie Chinoise des Royaumes Combattants au Début de l’Ère Chrétienne” (*BEFEO* lxxi (1982) 147–164+3pp.), p. 154, offers an argument that Shi Shen was active as an astronomer/astrologer at the court of Hui-cheng of Wei ca. –330. In *Les Premières Théories Planétaires Chinoises* (Mémoires de l’Institut de Hautes Études Chinoises, vol. XXI), pp. 118–167, he reviews astronomical theories prior to Liu Xin, and finds that none of them posits a sidereal period for Jupiter other than the 12-year period credited to Shi Shen in the *Kaiyuan Zhanjing*. Liu Xin posits a period of approximately 11.92 years (Teboul, *LPT* p. 12), Jupiter supposedly making 145 sidereal revolutions in one “great year”=1728 years, the formula often referred to as Liu’s “*chaochen* law,” i.e., 145 stations in 144 years (e.g., N. Sivin, *Cosmos and Computation in Early Chinese Mathematical Astronomy*, Leiden: E. J. Brill, 1969, p. 16).

Before going farther with a discussion of the *chaochen* rule, let me explain it precisely. My explication is adapted from my late teacher William Hung (Hong Ye 洪業), Prolegomena (in Chinese) to the *Combined Concordances to Ch’unch’iu, Kung-yang, Ku-liang and Tso-chuan* (Harvard-Yenching Institute Sinological Index Series, Supplement No. 11, vol. 1), p. 1xv. Professor Hung quotes and explains Liu’s text as included in the *Han shu* (21 B, “Lü-li zhi” chapter, p 35a–b in the (Taipei) Yiwen shuju edition of the *Han shu buzhu* of Wang Xianqian). He also illustrates the rule by working through two applications of it. I would have found the text baffling without Hung’s help; so it may be useful to others if I here go through it line by line. (I venture to deviate from Hung a bit in steps 1 and 2, in ways suggested by Sivin, pp. 18–19; my reading is mathematically equivalent to Hung’s.) After going through one of three applications of my own, I will explain what it is doing:

1. 置上元呂來 Set down [the number of years] since the First Beginning (*shang-yuan*). (*shang-yuan*=143127 years before 104 BC. So add 143127 to 104.)
2. 外所求年 Stay outside of the year sought.
(I.e., subtract the date being investigated from the result of line 1.)
3. 盈歲數除去之 Fill it up with the *sui*-number and subtract.
(The “*sui*-number” (Jupiter’s “great year”) is 1728; multiply this number by the largest number that will give a product not exceeding the number obtained in line 2, and subtract this product from that number. Call the result “A.”)
4. 不盈者呂百四十五乘之 Take what isn’t filled up, and multiply it by 145.
(Multiply A by 145.)
5. 呂百四十四為法 Use 144 as divisor.
(I.e., “fill” the number obtained in line 4 by the largest whole number of 144’s (with possible remainder).)
6. 如法得一名曰積次 The unity obtained by applying the divisor is called the “station total” (ST).
(I.e., the part of the result that is a whole number. Usually there will be a remainder, interpreted in step 7. See the explanation following Problem I below.)
7. 不盈者名曰次餘 [The part of the result of step 4] that is not filled up by [the operation in step 5] is called the “station excess” (SE).
(SE= the number of extra 1/144ths of a station travelled at the end of A. See explanation after Problem I.)
8. 積次盈十二除去之 Take the “station total” and fill it with 12’s, and subtract them.
(Subtract from ST the highest possible multiple of 12.)
9. 不盈者名曰定次 The unfilled part [of the “station total”] is called the “determined station” [number].
(I.e., the number got by the operation in line 8, =DS.)

10. 數從星紀起 [Use the “determined station” number] and begin counting from station Xingji. (I.e., take Xingji to be “1” when counting off the DS number on the sequence of 12 Jupiter stations.)
11. 算盡之外則所在次也 The station beyond [the one reached by] completing the count [in line 10] is the station where [Jupiter] was [in the year you are examining.] (I.e., DS would be the number of the station you would get if you called Xingji “0”—which was Hung’s procedure.)

As Hung explains, in a year when $SE=144$, it becomes 0, and ST (the multiplier of 144) is therefore increased by 1, and DS too must be 1 greater than it would have been; this is the year when Jupiter “jumps a *chen*” (*chaochen*), and there must therefore be one *chaochen* year (cc-year) every 144 years. The following turn out to be “*chaochen*” years in Liu Xin’s system (BC dates), with corresponding Jupiter locations (Xingji=0):

2255: 7	1535: 0	815: 5
2111: 8	1391: 1	671: 6
1967: 9	1247: 2	527: 7
1823: 10	1103: 3	383: 8
1679: 11	959: 4	239: 9

Now some demonstrations:

- I. Problem: Using Liu’s *chaochen* rule, find the station location if Jupiter in the year 135 BC.
- $143127+104=143231$ (Simplifying: $143127+104=143231$)
 - $143231-1035=142196$ $143231-(1728 \times 82)=1535$
 - $1728 \times 82=141696$ $142196-141696=500=A$ $1535-1035=500=A$
 - $A \times 145=500 \times 145=72500$
 - $144 \times 503=72432$ $72500-72432=68$
 - $ST=503$
 - $SE=68(1035+68=1103, \text{ closest earlier } \textit{chaochen} \text{ year})$
 - $12 \times 41=492$ $503-492=1$
 - $DS=11$

10. If Xingji=1, then Dahuo=11
11. Dahuo+1=Ximu

(Actually, in 1035 Jupiter probably would have been seen as in Chunshou, five stations earlier.)

Jupiter's "sui-number" is $1728=12 \times 12 \times 12$, because if Jupiter (J) is at 0 in year y , it will be at 0 in $y+12$, but at $0+1$ in $y+(12 \times 12)$; therefore the next cc-year when J will be in 0 must be year $y+(12 \times 12 \times 12)$. In other words, in the table above J is at 0 in 1535, and so the next earlier cc-year when J was at 0 must be $1535+(12 \times 12 \times 12)$, and so on. The "shang-yuan" year is itself such a year. (Why Liu Xin chooses the *shang-yuan* is not part of the present problem.)

What the rule does, therefore, is to identify the last "J at 0" cc-year before the target date; for relatively recent dates this year is 1535; then the rule operates in the interval ("A") between 1535 and the target date (1035), using the fact that just n cc-years occur in the interval (in this case $n=3$), to determine how many stations are traversed in this interval. In steps 4 and 5, assume 145 time units per year; in each unit Jupiter averages $1/144$ station (leaving a mean "Jupiter exact" of $1/144$ station per year). In 500 years Jupiter travels 500×145 units. Divide by 144 to get the number of stations: $(500 \times 145) = 500 \times (144+1) = (500 \times 144) + 500$; divided by 144 the result is $500+500/144$, $=500+3+68/144$. Jupiter's location is then determined by counting off 12's, since 1535 is a J-at-0 year.

II. Problem: Using Liu's *chaochen* rule, find the station location of Jupiter in 320 BC.

1. $143127+104=143231$
2. $143231-320=142911$
3. $1728 \times 82=141696$
 $142911-141696=1215=A$
4. $A \times 145=1215 \times 145=176175$
5. $144 \times 1223=176112$
 $176175-176112=63$
6. $ST=1223$
7. $SE=63$ ($320+63=383$, closest earlier *chaochen* year)
8. $12 \times 101=1212$
 $1223-1212=11$
9. $DS=11$

10. If Xingji=1, then Dahuo=11
11. Dahuo+1=Ximu

(Actually it should be Dahuo.)

III. Problem: Using Liu's *chaochen* rule, find the station location of Jupiter in 478 BC.

1. $143127+104=143231$
2. $143231-478=142753$
3. $1728\times 82=141696$
 $142753-141696=1057=A$
4. $A\times 145=1057\times 145=153265$
5. $144\times 1064=153216$
 $153265-153216=49$
6. $ST=1064$
7. $SE=49$ ($478+49=527$, closest earlier *chaochen* year)
8. $12\times 88=1056$
 $1064-1056=8$
9. $DS=8$
10. If Xingji=1, then Chunhuo=8
11. Chunhuo+1=Chunwei

(In 478 Jupiter ranged between 65° and 107° . In the system Liu was using, this would have made 478 a Chunshou year, Chunshou in his system being about 60° to 97° at that time. The *Zuo zhuan* implies that Jupiter was in Chunhuo in 478: see Zhao 3 *Zuo* and Ai 17 *Zuo fu* iv. Liu evidently checked his theory against the *Zuo zhuan*, and most Jupiter dates found there support him, interestingly; but he missed this one. So much for the quaint hypothesis that Liu forged the *Zuo zhuan*. See Hung p. lxvi.)

In my article "Guoyu 'Wu Wang fa Yin' tianxiang bianwei" (*Guwenzi Yanjiu* 12 (1985) pp. 445–461), I argue that the long paragraph containing an astrologer's description of celestial events at the time Wu Wang of Zhou launched his victorious campaign against Shang, found in the "Zhou yu" section of the *Guoyu* ("Zhou yu" 3.7), is actually an invention added to the text in the middle of the –1st century in the Western Han Dynasty, sometime between the time of Sima Qian and the time of Liu Xin. In the course of my argument I say (p. 455) that the fabricator of this astrological account must have been using the equivalent of Liu's *chaochen* law in his figuring. I now believe that the astronomy in the text was calculated in the early –5th century and merely used by the forger; but still

one must suppose that it made sense to him. So I must hold that Liu did not himself work out the *chaochen* law, but took it from some earlier work or source.

Where did Liu get it? There has been much discussion of the wider question, of the extent to which Liu's entire Triple Concordance system was original with Liu—see N. Sivin, *Cosmos and Computation*, pp. 11–12, n. 1. Sivin himself recommends the view that “Liu took over the Grand Inception calendrical methods and constants, but with great originality extended them into a universal system which became the pattern for his successors.” (He finds this view already in Xu Gan in late Eastern Han.)

Actually the interesting question, to me, specifically about the *chaochen* law, is a different one: How was it arrived at, by the astronomer or astrologer who did work it out, whoever that person was? One could ask this question, even if one did believe that it was Liu's own work.

Perhaps the 144:145 ratio is simply generated by the numerological features of the *Santongli* system Liu is developing. But Teboul has argued that although there is numerology in the system, it appears as *a posteriori* justification of the mathematical rules Liu presents, rather than being used to derive those rules; and further, that the numerology in Liu's treatise is confined to the first part, in which he presents his calendar system, and is absent in his subsequent discussion of the movements of the planets (*LPT* pp. x–xi).

Perhaps, then—since the ratio after all is wrong: it ought to be about 84:85—it results from analyzing only near-term observations. (Perhaps Teboul would hold this view; see his discussion on a related point on p. 88.) But this is not very likely. Liu, or whoever it was, would surely be aware that Jupiter's apparent motion is complex enough to make a deduction of a 144-year rule from present or recent observations rather dangerous. Further, the very fact that he was dissatisfied with the received 12-year rule implies that he and/or others must have noticed that application of the 12-year rule over centuries of time had led to error; and this could not have been noticed unless there were records, accepted as accurate, of Jupiter's positions at independently determinable dates centuries earlier. And one would expect that this kind of data itself would have been used in calculating a *chaochen* rule.

In the rest of this memorandum, I will argue that this is exactly what happened, and that it happened long before Liu Xin did his work. If I am right, it follows that I was quite justified in assuming that the forger of the *Guoyu* paragraph had access to an equivalent of Liu's *chaochen* law.

One of the early texts still surviving that implies positions of Jupiter at given dates is the *Bamboo Annals*. I do not mean the so-called “*guben*” text (which is not a single text at all), but the *Jinben Zhushu jinian*, long supposed to be a fake,

but now shown by the work of E. L. Shaughnessy (“On the Authenticity of the *Bamboo Annals*,” *HJAS* 46 (1986) pp. 149–180) to be a genuine Warring States document.

But in using this book I have to go beyond Shaughnessy, because he believes that the dates of reigns in it, down to 841, were edited into the text by the Jin Dynasty court scholars who worked on it after its discovery in a Wei state royal tomb in 281 CE. My own article “The Dates of Western Chou” (*HJAS* 43 (1983)) assumes that the text as discovered did have dates (I was wrong in holding that its date for the Zhou Conquest was 1027, however); I now have firm proof of this, developed in articles soon to be published, notably D. S. Nivison and K. D. Pang, “Astronomical Evidence for the *Bamboo Annals*’ Chronicle of Early Xia,” to appear in *Early China* 15. This, as well as other work of mine, implies that the tomb text of the *Annals* had exactly the same (absolute) dating scheme as the text we now have, and in fact in almost all important respects is that very text.

The *Bamboo Annals* chronicle for Di Xin of Shang dates a conjunction of the five visible planets to 1071, and locates the conjunction in lunar lodge Fang, which is the middle lodge of Jupiter station Dahuo, as defined later. The *Annals* account therefore assumes in effect that 1071 was a Dahuo year for Jupiter, which must of course have been part of the conjunction. It follows (though it is not stated) that 1050, the date of the Conquest in the chronicle text, was Chunhuo year, and 1035 was a Dahuo year, if one assumes a 12-year period for Jupiter.

This is consistent with the *Guoyu*: it says (“Zhou yu” 3) that Jupiter was in Chunhuo at the Conquest, and (“Jin yu” 4) that Jupiter was in Dahuo at the time the fief of Jin was created (i.e., granted to Tang-shu Yu, younger brother of Cheng Wang). A statement appears in the *Annals* under Cheng 10=1035, that in that year Yu was granted the recently subdued territory of Tang as a fief, the nucleus of the later Jin state.

But the date of the conjunction is 12 years early: the conjunction intended was actually the conjunction of 1059 (as J. Needham has suggested and Pankenier has argued). On my own analysis, it has been shifted back one Jupiter cycle to satisfy astrology and numerology (“The Dates of Western Chou,” *HJAS* 43 (1983) pp. 536–38 and 577). Moreover, it was not in Dahuo but in Chunshou, four stations earlier. Likewise, the date 1035 is four years early: The implication of the text is probably that Jupiter was not just in Dahuo but in Fang, a much smaller space (5 or 7 Chinese degrees, just west of Antares). Jupiter was actually in Fang for most of 1031.

Since we now know the *Bamboo Annals* to be a Warring States text entombed in 299 BC, the calculations of Jupiter positions included in it were probably originally done using a 12-year period, if one assumes that the compilers took over and used “received” information. This would produce an error of one year too early (for a given station), or one station too late (for a given year), for every 84 years back. A four-station error thus indicates a calculation done around 750–700 BC. The 12-year back-shift puts the conjunction exactly 300 years before the end of Western Zhou, when Lord Wen of Jin achieved eminence by saving the dynasty. It seems not unlikely, therefore, that the computations were part of an attempt to confirm 735 as the third centennial of Jin.

We do not, of course, need to suppose that this conjunction account was incorporated at that time into a *Bamboo Annals* text as we know it, but it is likely to have been preserved in official Jin state chronology. This seems the more likely when we notice that Ying, duke of Wei, who regarded his state as the successor-state to Jin, chose the year 335 (according to the *Annals*) as year 1 of his royal calendar, declaring Wei a kingdom and himself now “King Hui-cheng” (the “King Hui of Liang” in Mencius, Da-Liang being the capital of Wei).

If the Wei chroniclers and astrologers at the time Hui-cheng declared himself king believed that Jupiter had been in Dahuo 700 years earlier, in 1035, they would have expected Jupiter to reappear in Dahuo in 339. It did not, of course. Actually it was in Fang briefly in 332 and 331. This would have invited study, but offered ambiguous data. One cycle later, Jupiter would be observed to be in Fang in 320 (and not in either 321 or 319). The expected date for Jupiter to be in Fang, hence Dahuo, if the period were exactly 12 years, would be 315 ($720 = 60 \times 12 = 5 \times 144$ years after 1035). But instead, it would be observed in 315 to be five stations beyond Dahuo. I.e., in each of the last five 144-year spans, so it would have been believed, Jupiter had traversed 145—not 144-stations.

This is exactly Liu Xin’s formula. (Thus, as in my first two demonstrations, he would get the same station for both 1035 and 320—Ximu rather than Dahuo, because his ratio yields an average one—station error every 206 years back.) Given the evident astrological importance of Jupiter’s supposed location in Fang in 1035 and the importance of that date as just 700 years before Wei’s debut as a kingdom, it is inconceivable that this deviation from received beliefs was not noticed at the time. Remember that this was the era of Shi Shen of Wei, the most famous of the pre-Han astronomers; and also the very time when Mencius was remarking about the wonders of chronological astronomy, telling us that with its techniques one could sit at one’s desk and calculate the exact dates of winter solstices in any year past or future.

And, one must ask, why would the *Bamboo Annals* have been regarded with such esteem that King Xiang of Wei had it buried with him in 299, if it were seen as containing an astrologically significant error on a matter of great importance to the Wei state? But if, before the burial, a *chaochen* rule had already been deduced by astronomers, using this very text and matching it against current observations of Jupiter, then the *Annals* would be regarded as correct, and as having yielded the latest “results.”

I therefore am inclined to think that a 144:145 *chaochen* rule was first formulated in the late –4th century. We must ask why, if this was so, did all known specialists before Liu Xin—even including Sima Qian—say that Jupiter’s period is exactly 12 years? I suggest that this may tell us something important about ancient Chinese conceptions of what we call science. Perhaps for them the way the world worked was truly described by an ideal model, exhibiting an arithmetic beauty: 12 years for Jupiter, 12 lunar months for a year, the solstices and equinoxes dividing the year into exact 4ths, etc. Of course, observed reality was a bit messy, and specialists knew tricks for adjusting observed reality to the ideal, but it was the ideal that the specialists’ adjustments could and should be incorporated into a revised, more complicated, but really more splendid, ideal. (And still later, among the specialists, that it was the complications that were really interesting; leave the ideal to the politicians and the philosophers. See Sivin, *passim*.)

It was precisely that, it seems to me, that Liu Xin’s *Santongli* attempted; and his real achievement was to convey the idea in doing this that a new, revised ideal was possible, more exciting than the old one. But in doing this he may well have taken over and adapted a *chaochen* rule that had long been part of the astronomer–technician’s bag of tricks. I would not argue that such a rule had been generally accepted; and probably by the –3rd century the *Bamboo Annals* text was no longer available.

We have to try to explain why Liu Xin incorporated into his system a rule that was wrong. Would he not have tried to test it, on references in histories available to him, of locations of Jupiter in given years? He did do exactly this, probably, with the *Zuo zhuan* and the *Guoyu*. Almost all the dated Jupiter locations in the *Zuo zhuan* seem to confirm Liu’s rule—almost all, but not quite all. Why this should be so is not hard to see: We do not know exactly when Liu did his figuring, nor exactly when the *Zuo zhuan* was written, but let us try reasonable guesses, that Liu worked around the year one, and that the *Zuo* was compiled around 340 BC. If the *Zuo* compiler used the simple 12-year rule, and Liu used his *chaochen* rule, they would get the same results for dates over a range of almost two centuries, from the early 600’s to about 500 BC. This includes most

of the history covered in the *Zuo zhuan*. The same principle applies to the *Guoyu*.

Note: The Jupiter stations (*ci* 次) are not listed in the more popular dictionaries. Taking Xingji as 0, they are as follows:

0. Xingji	4. Daliang	8. Chunwei
1. Xuanxiao	5. Shishen	9. Shouxing
2. Juzi	6. Chunshou	10. Dahuo
3. Jianglou	7. Chunhuo	11. Ximu

Approximate locations can be estimated as follows: The east boundary of Fang (the second of the three lunar lodges of Dahuo) was Alpha Scorpii (Antares), at about 203° about 1000 BC. The west boundary of Xing (the second of the three lodges of Chunhuo) was Alpha Hydrae (the Chinese “Bird Star”), at about 104° around 1000 BC. These degree values increase (because of precession) at about one degree every 71.6° years. There were different systems of boundaries in use at various times, but I believe that the above data hold for all of them. (The 28 lunar lodges were distributed among the 12 stations two or three to a station: stations 1, 4, 7, and 10 above had three each. For more exact information on the lodges and their widths, see my article “The Origin of the Chinese Lunar Lodge System,” in A. Aveni, editor, *World Archaeoastronomy*, Cambridge University Press, 1989.) Locations of Jupiter can be obtained from Stahlman, W. D., and Gingerich, O., *Solar and Planetary longitudes for Years -2500 to +2000*, Madison: University of Wisconsin Press, 1963.

While the “*chaochen*” concept is essentially that in n years Jupiter moves through $n+1$ stations (*ci*), a *chen* (in the meaning of the word relevant to the present problem) is not, strictly speaking, a *ci*, though they stand in one-to-one correspondence. As I understand the matter, archaic Chinese calendrical astronomy projected the structure of what we call the zodiac onto the horizon, i.e., divided it into 12 ideally equal spaces, called *chen*, correlated with the positions of the sun in the 12 tropical months, the space at due north matching the sun’s position at the winter solstice. The space corresponding to a given month m then was defined as the horizon-space toward which the Dipper’s handle could be observed to point at the time of the beginning of the m th lunar month. As a result, while the sun, moon and planets move through the actual zodiac counter-clockwise (west to east), the *chen*-sequence is clockwise. The *chen* are named with the 12 “terrestrial branches” (*zi*, *chou*, *yin*, etc.: sometimes called “chronograms”). Jupiter, the “year star” (*sui xing* 歲星), is thought to move from *ci* to *ci* in successive years, so an imaginary “Counter-Jupiter,” Taisui or Sui Yin,

is posited as moving in the opposite direction from *chen* to *chen* in successive years. This way of speaking is then transferred back onto the description of the movements of Jupiter, so that it is said to be moving from *chen* to *chen* in reverse order (as in the *Shiji*, “Tian guan shu”). Thus it has “jumped a *chen*” when it is seen to be one station (*ci*) beyond where the traditional 12-year rule would lead one to expect it.

9 A New Analysis of the *Guoyu* Astrological Text

Abstract/Introduction

In this paper I refute the hypothesis that I advanced in my article (in Chinese) in *Guwenzi Yanjiu* in 1985, that *Guoyu*: “Zhou yu” 3.7 was fabricated and inserted into the *Guoyu* in the 1st century BC. The paragraph occurs at the end of the section on Jing Wang of Zhou (544–520 BC) discusses the casting of bells and gives the zodiac locations of Jupiter, the sun, moon, etc., “when Wu Wang attacked Yin.” I show that my former hypothesis was dependent on the no longer defensible theory that the Zhou Conquest of Shang took place in 1045 BC. The date I now think I have proved (1040 BC) requires another explanation of the *Guoyu* paragraph, which I continue to hold to be literally false, and to have entered the *Guoyu* in the middle –1st century; but I present here an argument from calendar astronomy that the astronomical details were calculated in the early –5th century. My argument also implies that an equivalent of the 76-year Callippic cycle was known in China a century earlier than previously supposed, and about 150 years before Callippus. At the end, I briefly discuss the implications of this discovery (if discovery it be).

This paper is not a typical historical demonstration, using testimony and documentation, that some identifiable person or persons did something at some time and place. It is, on the contrary, an “argument to the best explanation,” starting with a necessarily thin set of (probable) facts and tentative assumptions: (1) There is the thing to be explained, which is the *Guoyu* text (see frontispiece). (2) I posit that its author knew the correct date of the Zhou conquest, which I believe I have shown (in a paper before the Western Branch of AOS in October 1991) to have been in the first half of 1040 BC. (3) I also posit that the author had the month and day dates for the Conquest campaign that Sima Qian seems to be using in the *Shiji*. (4) I posit that the author was using the calendar system that came to be known as the “Yin Li.” (5) I assume that he believed the period of Jupiter to be exactly 12 years. I then *deduce* from (2) through (5) (and from the actual positions of Jupiter) that to get the first line, “Jupiter was in Quail Fire,” he would have had to be working in the early –5th century; and that if he was working at that time, he would have reasoned his way to all the other lines in the *Guoyu* text. I think that this is the best explanation of the *Guoyu* text that can be found. I conclude that if

February and October, 1991; Paper presented at the annual meeting of the American Oriental Society, Cambridge, MA, 1992

it is, then my posits (2) through (5) are very probably true, since these posits are fairly probable anyway; that is to say, my demonstration if successful tends to *confirm* the assumed premises, as long as no better explanation is in sight.

Finally I focus my attention on posit (4)—the “Yin Li” system being equivalent to the “Callippic” cycle—and on the early –5th century date when I am obliged to suppose this system was being used. I argue that the Babylonian-Greek systems and the Chinese system were probably worked out independently.

昔，武王伐殷，歲在鶉火，月在天駟，日在析木之津，辰在斗柄，星在天龍，星與日辰之位皆在北維。

——國語，周語下，七

A New Analysis of the *Guoyu* Astrological Text

My *Guoyu* article in *Guwenzi Yanjiu* (Nivison 1985, summarized in Nivison 1983 pp. 510–512) presents an argument that is undermined by my present conclusion (especially in Nivison 1990b) that the Zhou Conquest Shang (Yin) was in 1040, not 1045. Why? The *Guoyu* text (“Zhou yu” 3.7) locates the sun at the beginning of the campaign as “in the ford at Split Wood,” i.e., in the middle of the Milky Way. This means that the date has to be some time *before* the winter solstice. This is required, also, by the hypothesis that the Conquest was in 1045; so the pre-solstice date in the *Guoyu* was explained as simply reflecting what had in fact been the case. But if the Conquest was in 1040, then the launching of the campaign must be *after* the solstice, near the end of the first Zhou month. Therefore once “1040” is accepted the *Guoyu* pre-solstice date lacks any explanation.

At the same time, switching to 1040 as Conquest date opens the way for a quite different account of the *Guoyu* text. It still seems likely to me that the paragraph was lacking in the *Guoyu* as seen by Sima Qian, and was inserted in the middle of the –1st century, before Liu Xin. But it also seems to me probable that the astronomical data in it was calculated at a much earlier time, and was merely used (and reinterpreted), but not invented, by the author of the rest of the paragraph added to the *Guoyu*. One must, in short, search for a “best explanation” for the text; and it now is evident that the one I offered isn’t good enough.

The opening statement in the *Guoyu* account is that “when Wu Wang attacked Yin Jupiter was in Quail Fire” (Chun Huo). So the first thing to notice is that if the Conquest date is 1040 rather than 1045 (or 1046, which Pankenier (1984) thinks the text proves, or 1122, which Liu Xin 2000 years ago thought it proved), then we must look for a possible date of calculation when a calculation of Jupiter’s position using the supposed 12-year cycle would have picked late 1041 as a

time when Jupiter would have been in Quail Fire. For this, even the end of the reign of Jing Wang of Zhou, ca. 522–520, the time the conversation is dated in the *Guoyu* would have been quite possible. Further (if I argue rightly that a chronology revision occurred shortly after 428 BC; see Nivison 1991a, pp. 4–5), at this time the Conquest probably would still have been thought to have been in 1040.

Moreover, this is early enough so that the correct meanings of lunar phase terms used in the original “Wu Cheng” chapter of the *Shang shu* would still probably have been known. But in 522, if the *zhang-bu* system had been used in the calculation, the beginnings of months in the –11th century would have been dated two *ganzhi* days earlier than they should be. This would be enough to make it impossible to apply the “Wu Cheng” lunar phase terms in the intended months, so the calculator would have been forced to the conclusion that the entire campaign was two months earlier—since the regular alternation of 29-day and 30-day months means that as one goes back the first days of months in the *ganzhi* system become later.

Let me clarify this point with two diagrams. The first one gives the actual situation in 1041–40 BC (as demonstrated in Nivison 1990b), figures in parentheses being *ganzhi*:

Xia month	9	10	11	12	1	2	3	4
Zhou month	11	12	1	2	3	4	5	6
Day 1	(6)	(35)	(5)	(34)	(4)	(33)	(3)	(32)
2	F		F		F		F	
3		F		F		F		F

23			J					
24			(29)			J		
25			(30)					
26								
27								
28								
29	(34)		(33)		(32)	(1)	(31)	
30		(4)		(3)		(2)		(1)

Xia 11th month day 26, *Guisi* (30) (= 17 Jan 1040) is the day of March; Xia 2nd month day 29, *jiazi* (1) (18 Apr 1040) is the victory day. F=*fei* (new moon day) is the 2nd after a 30-day month and the 3rd after a 29-day month, and other lunar phase days vary similarly. J=*jisipo* is the 24th after a long month and the 25th after a short

month, and is the day immediately preceding *pangsipo*. Next, the situation as seen in 490 BC:

1	(4)	(33)	(3)	(32)	(2)	(31)	(1)	(30)

25	J			J				
26	(29)							
27	(30)							
28								
29	(32)		(31)		(30)		(29)	
30		(2)		(1)		(60)		(59)

Notice that in this *zhang-bu* conversion, the victory day *jiazi* (1) no longer falls in the Xia 2nd month. The calculator, knowing that it must be in the 4th quarter, is thus forced to assume that the dating is in the Zhou calendar, whose 2nd month is the Xia 12th month of the preceding year.

The pressure of *zhang-bu* calculation would also be a good explanation for the discrepancy between the Conquest campaign dating in the “Wu Cheng” (as retrieved and degarbled from Liu Xin’s quotations from it in *Han shu* 21 B), and the dating implied in the *Shiji*, “Zhou benji”: The former says that the campaign started in the first month and concluded with victory in the second month. The latter says that Wu got his forces across the Yellow River in the 12th month, and won at Muye in the second month—dates that fit the facts, if taken as Xia calendar dates (Nivison 1990b). It is the victory date that must be moved back two months, by a calculator who uses the *zhang-bu* system and (more or less) respects the true values of the lunar phase terms; and there are two possible solutions: He can move everything back two months, as in the *Guoyu*; or he can just move the victory date back, and make the “second month” victory occur the month after the beginning of Wu’s march in the (Xia calendar) 11th month, i.e., (Zhou calendar) first month. He would thus get the dates given in the “Wu Cheng”—almost. For there is one telltale bit of phrasing in the “Wu Cheng”: the date “2nd month” (*er yue*) is preceded by the word *lai*, which almost certainly meant “of the next year”; if so, the date “1st month” (*yi yue*) must originally have been “11th month” (*shi-yi yue*).

Next, consider the *zhang-bu* system, as compared with the actual first-month-first-days and the actual winter-solstice-first-days for the first years of each *zhang* and each *bu*: For *bu*, from *jimao bu* (1187 BC) to *yiyou bu* (123 BC), this

is the picture (19 years=1 *zhang*, 4 *zhang*=1 *bu*=27759 days; the first day of each *zhang* is a winter-solstice day):

First year	1.1 <i>ganzhi</i>		Actual first day	Actual solstice
1187	<i>jimao</i>	16	12.28=18	12.31=21
1111	<i>wuwu</i>	55	12.28=57	12.30=59
1035	<i>dingyou</i>	34	12.28=36	12.30=38
959	<i>bingzi</i>	13	12.27=14	12.29=16
883	<i>yimao</i>	52	12.27=53	12.29=55
807	<i>jiawu</i>	31	12.27=32	12.28=33
731	<i>guiyou</i>	10	12.27=11	12.28=12
655	<i>renzi</i>	49	12.26=49	12.27=50
*579	<i>xinmao</i>	28	12.26/27= 28/29	12.26=28
*503	<i>gengwu</i>	7	12.25/26=6/7	12.26= 7
*427	<i>jiyou</i>	46	12.26=46	12.26=46
*351	<i>wuzi</i>	25	12.25=25	12.25=25
275	<i>dingmao</i>	4	12.25= 3	12.24= 2
199	<i>bingwu</i>	43	12.25=42	12.24=41
123	<i>yiyou</i>	22	12.24=20	12.24=20

One sees that the system zeros out (no difference between system date and actual dates) in the *bu* beginning 579, 503, 427, and 351. The next task is to examine these *bu*, *zhang* by *zhang*:

*579	<i>xinmao</i>	28	12.26/27=28/29	12.26=28
560	<i>gengwu</i>	7	12.26= 8	12.26=8
541	<i>gengxu</i>	47	12.27=48	12.27=48
522	<i>gengyin</i>	27	12.26=27	12.26=27
*503	<i>gengwu</i>	7	12.25/26=6/7	12.26= 7
484	<i>jiyou</i>	46	12.25=46	12.25/26=46/47
465	<i>jichou</i>	26	12.26=26	12.26=26
446	<i>jisi</i>	6	12.26= 6	12.26= 6
*427	<i>jiyou</i>	46	12.26=46	12.26=46
408	<i>wuzi</i>	25	12.25=25	12.25=25
389	<i>wuchen</i>	5	12.26= 5	12.26= 5
370	<i>wushen</i>	45	12.25=44	12.25=44

*351	<i>wuzi</i>	25	12.25=25	12.25=25
332	<i>dingmao</i>	4	12.25= 4	12.24/25=3/4
313	<i>dingwei</i>	44	12.26=44	12.25=43
294	<i>dinghai</i>	24	12.26=24	12.25=23

(I use data in Shijo 1928 pp. 542–543, Dong 1960, Goldstine 1973, and Stahlman and Gingerich 1963.)

The time span that zeros out is 522 to 389, suggesting that the system was adopted in the middle of this period. (Because, since the system is slightly inaccurate, as it is applied farther and farther from the date of adoption, it will begin to deviate from actual dates, getting them too early as one calculates backward, and too late as one applies the system forward. The reason for this is that the system is based on the formula 4×19 solar years = 4×235 lunar months = 27759 days, which is slightly longer than an exact 76 solar years. This formula is the same as the reformed Metonic cycle worked out by Callippus in 330 BC in Athens, except that Callippus' system did not correlate *zhang*-first-day, first-of-the-month, and winter-solstice-day.)

But the calculation producing the *Guoyu* text probably has to have been done when a calculation of Jupiter's position in late 1041, using the supposed 12-year cycle, would put the planet in Quail Fire (Chun Huo) at that time. The conventional scope of Quail Fire is lunar lodges Liu (#24), Xing (#25) and Zhang (#26), the western boundary of Xing being marked by the "Bird Star," Alpha Hydrae. I will examine the situation between 525 and 429, at 12-year intervals (since $1041 - 525 = 12 \times 43$, and $1041 - 429 = 12 \times 51$). In 525, Alpha Hydrae was at about 110.9° , and in 429 it was at about 112.3° . In the system used in the 6th and 5th centuries BC, Liu was 18 *du* (Chinese degrees) and Xing and Zhang were each 13 *du* (Nivison 1989 p, 214). (I assume that one Chinese degree = $360/365 = 0.9863^\circ$.) Thus Quail Fire in 525 was about 93.1° to 136.5° , and in 429 it was about 94.5° to 137.9° . At 12-year intervals between these dates Jupiter ranged as follows during the Julian year (the figures are in degrees, ecliptic):

525	81	-	79	-	93 (Jan 7)	-	120	-	118
513	87	-	83	-	93 (May 25)	-	124	-	121
501	90	-	87	-	93 (May 2)	-	128		
489	97	-	92	-	93 (Mar 25)	-	133		
477	102	-	97	-	137				
465	106	-	101	-	137 (Oct 15)	-	142		
453	112	-	105	-	137 (Sep 22)	-	146		

441	118	-	110	-	137 (Sep 9)	-	150
429	121	-	114	-	137 (Aug 22)	-	155

The zodiacal location of the sun given in the text (“the Split Wood (Xi Mu) ford” and the “Northern Tie”; see below) requires a date later than Oct 15; therefore if the calculation was based on a current observation, that observation could not have been one later than 477; and we should suppose a calculation before 465. It could have been made as early as 522 (the implied date of the conversation “recorded” in the *Guoyu* in which the description of the skies at the time of the beginning of Wu Wang’s campaign is given); and indeed, as early as 525. But since it is necessary to assume that the *zhang-bu* system was used in the calculation, the time is likely to be later. (Jupiter was not in Quail Fire (astrological due “south”) at the Conquest. On the contrary, it was in the astrological due “north,” as *Xunzi* (#8 “Ru Xiao”) indirectly implies (Nivison 1990b). For the meaning of “north” and “south” see Nivison 1989 p. 209.)

The next constraint to consider is the stated position of the sun, together with the information in the “Wu Cheng,” presumably known to the calculator, that Wu Wang began marching on day *guisi* (30).

There are two statements that I take as locating the sun. The text says that the sun was in the “Split Wood ford,” which is vague enough not to be a problem. (The asterism “Split Wood” is probably the asterisms Ji and Dou; the Jupiter station “Split Wood” is classically defined as the lunar lodges Wei and Ji; the sun will turn out to be in Ji, both asterism and lodge.) But the text also says that “the *xing* and the location of the *richen* were both in the Northern Ties.” I will consider *xing* in due course. But the term *richen* requires study: It is sometimes treated as two terms, *ri* (sun) and *chen* (of uncertain meaning). One meaning of *chen* is the 30- or 31-*du* horizon spaces (named by the chronograms *zi*, *chou*,...) toward which the Big Dipper’s handle is thought to point in successive months. I have argued (Nivison 1985) that the sun’s *chen* is the moving 30-*du* zodiac space with the sun at its center (within which no star is visible at any time); and that it is at the leading edge of this space that the Dipper’s handle points. This assumption is required by the rule that when the handle points between two *chen* spaces at the beginning of a lunar month, that month must be intercalary (since to bear the next number, in the lunar series a month must contain a “*qi*-center” point at the middle of a *chen*). Here I assume that to give a point-location of the “sun’s *chen*” is to say that the *chen* is centered on that point, i.e., that that is where the sun is. Alternatively, *richen zhi wei* may be taken as “the sun-*chen* location,” i.e., the sun’s location in the chronogrammatic analysis of the year. In either case, we are here being told simply that the sun is at the Northern Tie.

Or at one of them: The Northern Ties (*bei wei*) as defined in *Huainanzi*, “Tian wen,” are zodiac *points* dividing the solar seasons autumn-winter (properly 225°) and winter-spring (properly 315°). If the sun is at the “Split Wood” ford, but is also said to be at the “Northern Tie” (*bei wei*), and if this Northern Tie point is at a “*qi*-node” i.e., at the beginning of a 15-or 16-*du* solar season space (to be expected, since the four annual seasons are each assigned exactly six such spaces), then it must be 46 days or *du* short of the point taken to be the solstice, in the standard zodiac in use at the time of the calculation. In the early Zhou zodiac (see Nivison 1989 pp. 209, 212, 214), the Dou-Niu boundary was the beginning of the winter-solstice solar month space, which I assume to have comprised a 16-plus a 15-*du* solar season space, with the solstice at the boundary between them. Therefore this “Northern Tie” point was two *du* into lunar lodge Ji (#7), i.e., 30 *du* before the beginning of the solar solstice-month space. Since Alpha Scorpii was at 203° in –1000 (Ahnert 1960, XXVI), and marked the western boundary of lunar lodge Xin (#5); and Xin was 11 *du* and the next lodge Wei (#6) was 9 *du*, the Northern Tie was 22 *du*, or 21.7°, beyond, at about 224.7°. Since the solstice is by definition 270° and 46 *du*=45.37° (1 *du*=360/365=0.9863°), the point ought to be about 224.6°; so –1000 is about the time the zodiac map was fixed. (My date –1100 in Nivison 1989 as when these boundaries would be astronomically correct is about a century early; I was using a not quite accurate location of Alpha Scorpii.)

The problem now is to reconstruct the reasoning of a possible calculator working in the late –6th or early –5th century BC. He would need to know where, in the sky and on his zodiac chart, the Northern Tie point was. It must be where it was in –1000, so its degree value will be 224.7° plus the amount of precession in the interval. This will make the point either 231°, or 232°, depending on the date.

Beyond this, the calculator will need the following:

- (a) The *ganzhi* of the day when the sun got to the Northern Tie, in some year a multiple of 19 after 1041 BC: 528, 509, 490, or 471.
- (b) The *ganzhi* for the first day of the first year of each standard *zhang* containing a date he is working with.

This will enable him to convert an observation datum to a (putative) datum for 1041, as follows: Suppose he is checking the year 490. 490 is year 14 in the *zhang* beginning with 503, just as 1041 is year 14 in the *zhang* beginning with 1054. 503 begins with day *gengwu* (7), = (67). 1054 begins with day *dingsi* (54). 67–13=54. Further, for the calculator, each *zhang* begins with the winter solstice, always in the same sidereal location (since he knows nothing of precession). And so also, any two dates within two different *zhang* but equally distant in time from the first

days of their respective *zhang* must for the calculator be dates on which the sun will have exactly the same sidereal location. So the *ganzhi* of the Tie day in 490, minus 13, must be (for him) the *ganzhi* of the Tie day in 1041.

The mid-point of the range of dates that zero out for the *zhang-bu* system as tabulated is about 450 BC. This is hardly a scientific result, because the system could well have been “frozen” at any date within the “zero-out” range in such a way that it behaves the way it does. But this consideration does suggest that we start by trying the latest possible date of calculation. It will be a calculation done between 471 and 466, using the observation of Jupiter’s positions in 477, observation of the sun’s movements in 471, and the information that the day *jiyou* (46) is the first day of the *zhang* containing 471 (i.e., the *zhang* beginning 484).

What was Tie day in 471? All the calculator has to do is look, and then check his calendar; but for me it’s more work. I first must determine the degree value of the Tie point in 471, then determine the date in *my* (Julian) calendar when the sun reached this point, and then determine what *ganzhi* it was. Then I must convert, as would he.

For me, it goes as follows: The Tie point in 1000 BC was 224.7° . $1000-471=529$, divided by the precession rate $71.6=7.39^\circ$; $224.7^\circ + 7.39^\circ=232.09^\circ$. The *ganzhi* beginning 1054 is (in the system) *dingsi* (54), and the *ganzhi* beginning 484 is *jichou* (46), so to convert I must add 8. Consulting Stahlman and Gingerich, I find that the sun was at 232° in 471 on 19 Nov, i.e. Julian Day (JD) 154 9713. I divide by 60 and subtract 10 from the remainder, to find the *ganzhi* for this day, which is *bingxu* (23). $23 + 8=31$, and *jiawu* (31) is the day after *guisi* (30), the day of March. So this did not work, although it’s close enough to show that my hypothesis is probably right. But if it is, the calculation probably was done earlier.

So let’s try 490. As above shown, the conversion is minus 13. $1000-490=510$, i.e., precession is 7.12° , and the Tie point is 231.8° , still about 232° . The sun again reached this point on 19 Nov, =JD 154 2773, =*bingwu* (43) and $43-13=30$. Success! So 490 is a possible date as basis of calculation, especially so in view of the fact that 489 was a Quail Fire year.

What about $490 + 19=509$? The precession adjustment is 6.86° ; so the Tie point should be 231.56° . Ambiguous: using Ahnert’s tables, I find that the sun reached 231° about 01:30 in China on 19 Nov, JD 153 5834 =*dingmao* (4). 522 begins with *gengyin* (27), $+27=dingsi (54); and $27 + 4=31$. So 509 probably doesn’t work.$

What, then, about 528, taking the text just as it reads—for someone talking in 522 would have been basing his figures on 528. The conversion is probably plus 7; the *zhang* begins in 541, and in the system (but perhaps not in fact) this year begins with *gengxu* (47), $+7=(54)$. In fact it may begin with day *xinhai* (48), so that the conversion would be plus 6. The precession adjustment is 6.59° , so the

Tie point is 231.3° ; and in 528 the sun reached 231° on 18 Nov=JD 152 8893= *bingxu* (23), + 7= (30).

Again, success, but uncertain. 522 itself is a *zhang* year, so this year might have supplied the calculator with the *zhang* first day that he uses. Its first day was *gengyin* (27), and it must be matched with the *zhang* year following 1041, i.e., 1035, whose first day in the system is *dingyou* (34), so the conversion here too is plus 7, giving the date *guisi* (30). One more detail, however, favors 490 over 528, as will be seen.

Thus the calculator ca. 522 BC or ca. 490 BC, *knew* (from the “Wu Cheng”) that Wu Wang began his campaign on *guisi* (30), and that the victory was in the “2nd month,” and he thought he knew (because the *zhang-bu* calculation required it) that this 2nd month was in the Zhou calendar, hence that *guisi* must be near the end of the Zhou 11th month. Knowing these things, he checked the conversion equivalent of *guisi* day in a properly selected year (or years) in his own time, and noticed that it was (always) when the sun was at (or, sometimes, nearly at) the Northern Tie point in his star chart.

He has already established that Jupiter was in Quail Fire at this time. Let us consider the other *Guoyu* statements:

- The Moon was in the Heavenly Four (Tian Si)
- The *chen* was in the Dipper’s Handle
- The *xing* was in the Heavenly Turtle (Tian Yuan)
- The *xing* (as well as the sun) was in a “Northern Tie”

The “Heavenly Four” is the asterism Fang, the four stars that appear to be as many horses pulling the chariot (Xin) containing the Heavenly King (Antares, Alpha Scorpii). Its location is 27 *du* west of the Wei-Ji boundary, hence 29 *du* west of the sun, if the asterism Fang is at the western boundary of lodge Fang (#4) (since Fang is 7 *du*, Xin is 11, and Wei is 9). The moon moves east about 13.2 degrees, or 13.4 *du* a day, and the sun moves east one *du* a day. Therefore the moon will overtake the sun during day *bingshen* (33). This is what favors 490 as basis for calculation, because the first day of the 12th month of 490 was *jiyou* (46), whose conversion equivalent is *bingshen* in 1041; whereas the 1st of the 12th month of 528 was *gengyin* (27), converting to *dingyou* (34).

On *bingshen* (33) (for the calculator) the sun will therefore be 25 *du* beyond the Fire Star Alpha Scorpii, at (lodge) Ji 5, but asterism-wise just within the tip of the “handle” of the asterism Dou, the Southern Dipper. The word *chen* is sometimes defined as the conjunction of the sun and moon (among many meanings), and we can take it, thus, or as meaning that the Big Dipper’s Handle’s pointing is

indicating that the forthcoming conjunction will be in the Southern Dipper's Handle.

The *xing* is whatever asterism culminates at dusk. I take dusk in this case (perhaps always, in such texts) to be an ideal dusk, when the sun is 90° west of the meridian. Therefore, if the sun is at the “northern Tie” at the western edge of the space in the sky that is the sun’s abode in winter, the *xing* must be at the other “Northern Tie,” at the eastern edge. It follows that “Tian Yuan,” the “Heavenly Turtle,” cannot be the Jupiter station Xuan Xiao, as the commentary of Wei Zhao said it to be, but must be the celestial animal of the whole winter “palace,” the Dark Turtle” (Xuan Wu). Notice (Nivison 1989 p. 214) that the solar winter space from tie to tie does not coincide with the astrologer’s Northern (Winter) Palace. The latter begins at the Ji-Dou (#7–8) boundary and extends to the Bi-Kui (#14–15) boundary, whereas the two “Ties” are just beyond Wei-Ji (#6–7) and just short of Shi-Bi (#13–14); so the *xing* does fall within Xuan Wu.

This interpretation (as noted above) cannot be reconciled with the “Wu Cheng,” if that text is taken to say that the launching of the campaign was in the first month and the victory was in the second month *in the same calendar*, i.e., understood as the *following* month; The *Guoyu* text does say explicitly that the marshalling of troops on *guihai*, the day before the victory, was in the second month. But if the sun is at the “Northern Tie” at the beginning of the campaign, this campaign-beginning month has to be the eleventh month (in a year in which the solstice is in the first month); and for the victory month to be the very next month we would have to suppose that the calculator is imagining a calendar in use at the time of the Conquest in which this eleventh month—pre-pre-solstice month, or last month of autumn—counts as first month. This would, indeed, be a “Fire Star” calendar, but I hesitate to suppose that the calculator had such a strange view, and I accordingly think that he does not identify the month because he assumes that the victory was not one month but three months after the launching, as I think I have proved (Nivison 1990b) was in fact the case; i.e., for him, the campaign began in the 11th month of the Zhou calendar for the year corresponding to 1041, and the victory was in the 2nd month of the Zhou calendar for the year corresponding to 1040 (assuming a Zhou calendar in which the solstice month is the first month)—just as the *Shiji*, “Zhou benji,” seems to say. (I would hold that “11th month–2nd month” is right, but that the calendar was the Xia calendar, making the Conquest date 18 April 1040 BC; see Nivison 1990b.)

If this is right, then at the time of the calculation, probably early –5th century, it was not only still known that the Conquest was in 1040, but also that it was three months, and not just one month, after Wu Wang began his march. But the astronomical interest of this study will be another point that I have mentioned: I

am able to work this out only by assuming that some Chinese with a sophisticated understanding of calendar astronomy was using the Chinese equivalent of the Callippic cycle a century earlier than previously supposed, and as much as a century and a half before Callippus is known to have been working in Greece.

*

Three incidental matters deserve comments:

1. My interpretation of this *Guoyu* text is obviously not that of Liu Xin, nor of the (San Guo) Wu commentator Wei Zhao, who largely follows Liu. Why did they go wrong (as I must be assuming)?

The basic trouble, I think, is their misreading of the line “the *xing* was in the Heavenly Turtle (*Tian Yuan*).” The following text says that this “Heavenly Turtle” is the origin of the (distaff line of) Zhou ancestry, which could be taken as the Qi state, and the *fenye* (astrological correlate) of Qi was the Jupiter station Xuan Xiao. So, it is supposed, “*Tian Yuan*” is another name for Xuan Xiao. Of course the author of the paragraph may himself have thought this, misunderstanding the astronomical “record” he was using. But he may simply have reasoned that since Xuan Xiao (*fenye* of Qi) is *within* Xuan Wu=*Tian Yuan*, therefore this line of ancestors “came from” *Tian Yuan*.

But if one supposes that *Tian Yuan*=Xuan Xiao, then it becomes impossible for *xing* to mean the asterism culminating at dusk (its meaning in the “*Yao dian*”), and necessary therefore to search (hard!) for another meaning; so Liu chooses “*chen xing*”=Mercury, which he thinks he knows how to locate. And therefore, too, it is impossible for “*bei wei*” to have its proper technical meaning. *Bei wei* has to be taken the name of an *area*, and so Wei Zhao concludes it just means “the watery location” (*shui wei*), i.e., the winter quadrant the zodiac bounded by the (true) *bei wei* points.

2. The astronomer Meton of Athens worked out the 19-year cycle no later than 432 BC, and it is believed that he had a Babylonian source, for the 19-year cycle was apparently known in Babylon as early as the early –5th century, though not regularly until 367 BC. Callippus belonged to the circle of the Academy and the Lyceum, and revised Meton by grouping four 19’s into a cycle of 76 years and subtracting one day; the first known application was in 330 BC. (See the *Dictionary of Scientific Biography*, articles on Meton and on Callippus.) All of this was considerably later than the use of the equivalent system that I seem to be finding in China in the early –5th century.

But I think it is very unlikely that the ancient West got this information from China. The discovery of these cycles is almost inevitable in any civilization that has a calendar problem of reconciling the solar and lunar years and keeps an exact count of days. (The basic purpose of these cycles is to have a rule for intercalation; seven intercalary months in 19 years were found to bring the solar and lunar calendars into almost exact alignment.) It even seems likely that the pre-Columbian Maya had discovered the 19-year count (see Aveni 1980 p. 170). If the Chinese anticipated the West in this matter, the reason, I suggest, is that the Chinese from very early times used their cycle of 60 for naming days; this made it easy to figure an exact day-count between two widely separated dates.

One can think of the procedure of discovery as follows: As a first approximation, it is noticed that there are about 12 lunar months in a four-season year. (Obviously, this will be noticed independently in many parts of the world.) But then it is noticed that there is about a third of a month left over. So, second approximation: add an extra lunar month every three years. (Observation will show that occasionally there must be an extra long (30-day) month.) But then it is noticed that there are still a few days left over, just enough so that in 19 years an additional intercalation is needed; so, third approximation, make seven intercalations spread through 19 years. Confirmation: It is then noticed that for any 19-year period in which the first day of the first month is the winter solstice, almost always the first day of the first month of year 20 will be the winter solstice. Another small adjustment (the “Callippic”) results from setting the number of days in a 19-year period ($19 \times 365 \frac{1}{4} = 6939 \frac{3}{4}$) at 6940, and “paying back” by deleting one day every 4×19 years.

The rest (in China) is Chinese elegance: There are 27759 days in a 76-year *bu*, and this leaves a remainder of 39 when divided by 60. Therefore only after 20 *bu* will the accumulated remainder be divisible by 60. So the *ganzhi* of the first day in each *bu* in a series of 20 *bu* is unique, and can be used as the name of that *bu*. One can then designate a year unambiguous (in the stretch of history one is likely to be interested in) as, e.g. “the 29th year of *wuwu bu*”, i.e., 1083 BC. This system has known explicit uses only in certain Han texts, for giving dates of historical events and portents. In the ancient West, the cycles were used primarily for absolute dating of astronomical phenomena.

(A recent study of Meton’s work—Bowen and Goldstein, 1988—shares my persuasion that long accumulation of data and a high level of scientific observational rigor are not needed for the discovery of the 19-year cycle. It could be done with almost no observations, if one merely notices that (1) the solar year slightly longer than 365 days; and (2) a lunation takes either 29 or 30 days, the 30-day

months being slightly more numerous. Reflection then shows that a cycle of p years containing $(p \times 12) + q$ months can be determined by solving the inequality

$$(29 \frac{1}{2} \times q) < (11 \times p) < (30 \times q)$$

for the least integral values of p and q , which are $p=19$, $q=7$ (19 years, 7 intercalations, i.e., $(12 \times 19) + 7=235$ months). I will not risk an opinion as to how it was actually worked out in China.)

3. There is a possibility that for a time persuaded me, that a critic may wish to pursue. As I argue in Nivison 1990b, the Zhou year at the time of the Conquest had for a considerable time not been corrected for precession (of which the Chinese were unaware, anyway). I accordingly found reason to construct a Conquest year calendar that put the winter solstice two days late. In this calendar, I proposed that the first of the 24 solar periods was 16 days. I could get the same result by making it 15 days, and making the last period of the year corresponding to 1041 BC a 16-day period; and this is exactly what I do have to assume to have been standard in the present study. (Thus, the solar distance from the autumn-winter Northern Tse point to the winter solstice is set at 46 *du*, in time order 15, 15, 16.) This, for the Conquest year, is tantamount to assuming that the Zhou calendar was three days (rather than two days) late in dating the solstice.

If so, then we should suppose that it was also three days late with Northern Tse day, i.e., in effect, that their Northern Tse point at that time was approximately at 228. And, in 1041 BC, the sun reached this point on day *guisi* (30), which was 18 Nov, and (probably) in the Chinese lunar calendar was the 25th of the 11th month.

This suggests the surprising possibility that the figuring that produced the line “Jupiter was in Quail Fire,” in the early 5th century BC, did not generate all of the other lines in the *Guoyu* account, but merely reconfirmed or corrected them—i.e., that the *Guoyu* “record” really is in part a record.

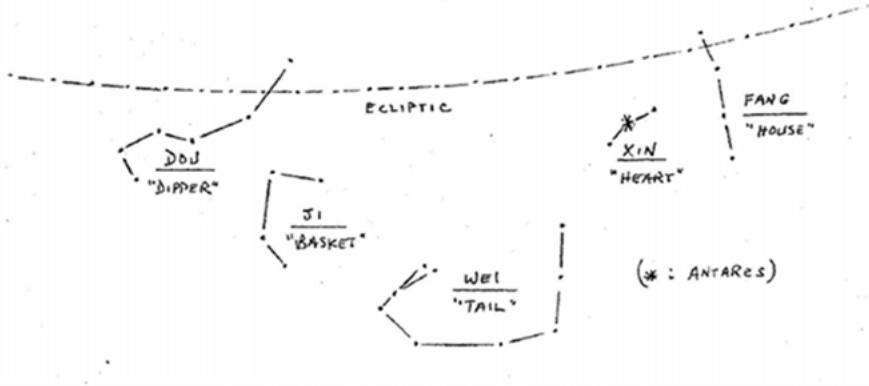
I have rejected this possibility, for the following reasons.

- (a) If one suppose that the campaign began in the 11th month (Zhou calendar) of 1041, and concluded with victory on *jiazi* at the end of the 2nd month (Xia calendar) of 1040, figuring the dates of the day *jisipo* accordingly, one finds that it falls on the 23rd after a long month, but on the 25th after a short month. I do not think that the variation can be more than one day.
- (b) If, to avoid this difficulty, one supposes that the victory was 60 days earlier, i.e., on *jiazi* of the Zhou 2nd month, which would be 17 Feb, one is concluding that it was after all not on Qing Ming day; and this leaves the last line of the “Da ming” ode unexplained (Nivison 1990b); for that line

seems to say plainly that “the attack on Shang occurred in the morning, on Qing Ming day.”

- (c) In either case, one foregoes the best explanation I have so far been able to find, of the discrepancy between the Conquest year dates in the “Wu Cheng” and those in the *Shiji*, “Zhou benji.”
- (d) One has to suppose one other correction, in addition to the location of Jupiter: On *guisi* of the 11th month of 1041, the moon was still more than two days short of being in Fang (the “Heavenly Four”).

The Tie day coincidence is, after all, not altogether remarkable. For, we know from the older lunar lodge system preserved in notes in the *Kaiyuan zhanjing* (Nivison 1989) that an ossified calendar zodiac set at the beginning of Zhou almost certainly went unchanged throughout the entire Zhou period—probably not being officially discarded until the Tai Chu reform of 104 BC (when, according to the *Shiji*, “Li shu,” “the degree values of the sun’s *chen* (*richen zhi du*) again accorded with the Xia (natural) calendar”). And this fact, together with the mathematics of the *zhang-bu* system, would guarantee that a calculator in the early 5th century BC would be only a few days off the mark.



The Chinese *Zhang-Bu* System

76-Year <i>Bu</i>		1 st Year (BC)	19-Year	Zhang	1 st Day	Cycle #
Jia-zi	(01)	1567	01	40	20	60
Gui-mao	(40)	1491	40	19	59	39
Ren-wu	(19)	1415	19	58	38	18
Xin-you	(58)	1339	58	37	17	57
Geng-zi	(37)	1263	37	16	56	36
Ji-mao	(16)	1187	16	55	35	15
Wu-wu	(55)	1111	55	34	14	54
Ding-you	(34)	1035	34	13	53	33
Bing-zi	(13)	959	13	52	32	12
Yi-mao	(52)	883	52	31	11	51
Jia-wu	(31)	807	31	10	50	30
Gui-you	(10)	731	10	49	29	09
Ren-zi	(49)	655	49	28	08	48
Xin-mao	(28)	579	28	07	47	27
Geng-wu	(07)	503	07	46	26	06
Ji-you	(46)	427	46	25	05	45
Wu-zi	(25)	351	25	04	44	24
Ding-mao	(04)	275	04	43	23	03
Bing-wu	(43)	199	43	22	02	42
Yi-you	(22)	123	22	01	41	21

The 60-Day Cycle

	Gan	1	2	3	4	5	6	7	8	9	10
Zhi		Jia	Yi	Bing	Ding	Wu	Ji	Geng	Xin	Ren	Gui
1	Zi	01		13		25		37		49	
2	Chou		02		14		26		38		50
3	Yin	51		03		15		27		39	
4	Mao		52		04		16		28		40
5	Chen	41		53		05		17		29	
6	Si		42		54		06		18		30
7	Wu	31		43		55		07		19	
8	Wei		32		44		56		08		20
9	Shen	21		33		45		57		09	
10	You		22		34		46		58		10
11	Xu	11		23		35		47		59	
12	Hai		12		24		36		48		60

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10 Qingming Day, 1040 BC

Abstract: I will briefly review the arguments over the date of the Zhou Conquest of Shang, following my discovery in 1979 that the *Zhushu jinian* (“*Bamboo Annals*”) appears to provide the key to dating Western Zhou bronze inscriptions. In a paper in the Metropolitan Museum in New York in June of 1980 I argued that the date was early 1045 BC, an argument I enlarged in an article in *HJAS* in 1983. In 1984 I published in *Early China* a tentative argument for 1040. I will now outline eight independent proofs that the date is indeed 1040. The decisive demonstration will show that the victory at Muye occurred on Qingming Day, in the spring of that year.

In my “Response” to critics in the Forum Section of *Early China* 15 (p. 156), I reviewed work on the problem of using the *Bamboo Annals* together with inscriptions and *Shang shu* text to ascertain Western Zhou dates, observing again that my date 1045 for the Zhou conquest of Shang, published in *HJAS* 43, 1983, and D. W. Pankenier’s date 1946, in *Early China* 7, are probably both wrong. As my *HJAS* article in 1983 was going to press, I discovered an error in my argument, that led me to publish a note in *Early China* 8 the next year, with an argument for 1040. But that argument too is wrong, and in subsequent work I reverted to 1045, without conviction. It is time to resolve the matter.¹

Paper presented at the Annual Meeting of the Western Branch of the American Oriental Society, University of Oregon, Eugene, Oregon, October 25–27, 1991.

I am indebted to E. L. Shaughnessy and to D. W. Pankenier for many suggestions over the twelve years of my work on these problems; and not less, for the stimulation of much productive controversy, which may well continue.

1 The essence of my argument in *HJAS* 43 was this: Wen Wang died in 1050 (see below), in what the *Shiji* says was the 7th year of an apparently continuing royal calendar; and the account in the *Shiji* goes on to say that in the 12th month of the 11th year Wu Wang’s forces crossed the Yellow River, and defeated the Shang in the following 2nd month. This seemed to be confirmed by the *Bamboo Annals* and the *Lü shi Chunqiu*, both of which say that Wu won his victory in the 12th year, albeit in his own 12th year. The error was my failure to notice that Sima Qian, and other Han scholars such as Liu Xin and Zheng Xuan, systematically misread old dates, taking month designations as names of months in the “Xia” calendar, so that for Qian “12th month” just meant “post-winter-solstice month,” no matter when the official year began; thus for him “11th year 12th month” was the second month of the 11th year in the Shang calendar, this is reflected in the “Shijia” chapters, where the victory is said to be at the beginning of the 11th year and the calendar is explicitly said to be Wu’s own. So I reasoned that Qian was misreading

1. About 40 different dates for the conquest have been proposed over the past 2500 years, and most of them still have their advocates. But the only dates reflecting new work on the *Annals* are 1046, 1045 and 1040. Once 1050 is fixed (by astronomy) for Wen Wang's death, the Conquest must be seen as following within a dozen years. One then finds that lunar phase dates in *Shang shu* chapters are satisfied only by 1045 and 1040.²

Pankenier challenges the dates in these texts, but their validity and meaning can be established by a simple argument: The “Kang Gao” opens with several sentences that are misplaced. It has long been supposed that they belong at

sources like the *Lü shi Chunqiu*; and since the date had to be either 1045 or 1040 (see below), the only possibility seemed to have Wen dying in the 50th year of his succession calendar beginning in 1101, rather than in the 50th year of his accession calendar beginning in 1099, i.e., his death date was 1052, so that 1040 was Wu's 12th year. This was my concept in my *Early China* correction: “1040 as the Date of the Chou Conquest,” *Early China* 8, 1982–83 (actually 1984), pp. 76–78. But for various reasons making Wen die in 1052 is impossible. So perhaps (as I had supposed in 1983) the Conquest was in 1045 after all, though not in a “12th year” that could be Wu's *simpliciter*; this idea I used in publications between 1985 and 1989. The alternative is that the Conquest was dated in Wu's calendar (which every source before Liu Xin that is clear about the matter says), but “12th year” is a mistake. In that case, the most likely date is 1040, i.e. Wu's 10th year, which had become for some reason “12th year” in Warring States source. It was only last year that I found (with evidence) an adequate account of how this change have come about, which will require of space that only a book affords. The present short paper merely presents some of the confirming evidence.

But there was a belief, probably widely held as early as 400 BC, that the Conquest was not in 1040 but in 1045. The issue was (as I see it) whether the Duke of Zhou's regency coincided with the first seven years of Cheng Wang's succession count (2 + 30, = 1037/1035–1006), or preceded Cheng's 30-year official calendar (1035–1006), the latter (1045) view gave greater stature to the Duke, who was probably being advanced to “sagehood” at just this time.

2 That Wen Wang had a calendar beginning in 1099 is proved in several ways, notably by the dating of a lunar eclipse, actually 1065, in the “Xiao Kai” chapter (#23) of *Yi Zhou shu* to Wen's 35th year. Was that calendar his succession calendar or his accession (post-mourning) calendar? The *Annals* account in effect dates a “mandate” given to Wen in the year after a conjunction of planets—the conjunction was actually in 1059—to his 44th year, implying that he reigned 52 years, dying in the 9th year of his mandate; and death in the 9th mandate year is also indicated by *Yi Zhou shu* “Wen Zhuan” (#25). On the other hand, the *Shang shu* “Wu Yi” and the *Shiji* “Zhou Benji” say that Wen reigned 50 years. Confirmation of 52 (=2+50) years, requiring 1050 as death date, is obtained by analyzing the story about an earthquake in the early reign of Wen in Zhou, found in *Lü shi Chunqiu* 6.1. The compiler has copied out a source giving the date, which he takes to be “8th year,” whereas it is actually the 9th year. He misinterprets “*sui liu yue*” as “in the sixth month of the (same) year”; it actually means “in a year, in the sixth month” i.e., in the next year; in consequence, when he says Wen reigned 51 years, we must correct this to 52 years. (I demonstrated this in my paper “A tell-tale Mistake in the *Lü shi Chunqiu*,” offered to this society annual meeting in Boulder Colorado, October 1989.) For 1045 and 1040, see note 4.

the beginning of the “Luo Gao,” since they have to do with things done at the beginning of the last Regency year. But they do not fit there either. On the contrary, they are obviously an alternative to the narrative that prefaces the “Shao Gao.” This narrative says that the Duke of Shao (acting for the Duke of Zhou) began work on the site of the new city that was to become Luoyang four days after “new moon day” (*fei*) of the third month, i.e., on the 6th or 7th. The misplaced “Kang Gao” text says that this action occurred on “*zaishengpo*,” and in Wang Guowei’s system for interpreting these terms (which most accept) this should be the last day of the first quarter, which should be either the 6th or the 7th. So Wang is essentially right, and the texts are valid.³

3 Why do I find this “Kang Gao” argument decisive? The text has been out of place at least as long as the “Shao Gao” has been called “Shao Gao,” i.e., at least since before the compilation of the *Shiji*, because the present opening of the “Shao Gao” makes it appear—as for 2000 years scholars supposed—that the Duke of Shao rather than the Duke of Zhou is speaking. It is inconceivable that a hypothetical faker of the *Shang shu* lunar phase dates could have made these two opening of the “Shao Gao” coincide in meaning in just the way modern analysis of the system (which has *not* made use of the “Kang Gao” evidence) would cause us to expect, unless he just knew the correct meanings of the terms. But in that case there ceases to be any reason to suppose the dates are faked. (It is primarily the “*shengpo*” (2nd quarter)—“*sipo*” (4th quarter) terms that have been in dispute; I am taking *po* to mean (enlarged =) “gibbous moon”; “*chujì*” (first auspiciousness) and “*jiwang*” (after full moon) are obviously the first and third parts of the month, if a “four quarters” interpretation is adopted.)

That the opening lines of the “Kang Gao” are in fact an alternative opening for the “Shao Gao” requires argument: The objection that the former says the Duke of Zhou began the work on the foundations of the new city, whereas the latter says the Duke of Shao did it, is answered by the account the Duke of Zhou gives the king at the beginning of the “Luo Gao”: he did it, he says, “*yin bao*,” which means “causing the *Taibao* (= the Duke of Shao) to act for me” (see the use of the word “*yin*” 胤 in *Shang shu* chapter 18, “Gao Zong Rong ri”), and he adds that we (*wo*, not *yu*) took the oracles, i.e., he does not represent that he did this in person. The objection that the opening has two valid replies commonly made: a change of subject before *yue* “said” is quite possible; and in the address we find “Dan *yue*,” which ought to mean “I, Dan, say...,” for in the only other cases in the *Shang shu*—there are six of them—where “Dan” is used as a personal name it is always the Duke of Zhou who is referred to and speaking. (The standard account is that the Duke of Shao is quoting the Duke of Zhou at this point, referring to him by his personal name—even though he has already addressed him as *Gong*.) The decisive proof is the way the “Kang Gao” account reads: The Yin multitude is brought to the Duke of Zhou, and he “encouraged” (*qin*) them all, then using the occasion to give a major address on matters of government. This is what he does, at the end of it picking up again the word *qin*: “We would not presume to encourage [you]” (*wo fei gan qin*), i.e., to suppose that you need encouragement—thus making a graceful apology for having turned what was billed as a pep-talk into a speech of a different kind.

Examination of *Shang shu* dates in the “Shao Gao” and the “Luo Gao” show that the last year of the Regency must be either 1036 or 1031, and the fragments of the “Wu Cheng” quoted in the *Han shu*, “Lü-li zhi” (21B) are then satisfied by either 1045 or 1040. But the earlier dates, 1045 and 1036, require a calendar beginning each year with the pre-solstice month, while the later dates 1040 and 1031 accept a calendar beginning with the winter solstice month; and the latter type of calendar is what the *Shiji* says the Zhou calendar was. So, *prima facie*, the Conquest year should be 1040.⁴

⁴ This argument by itself is weak as an argument for 1040 over 1045, because I think it can easily be shown, e.g. by inscription dates, that the classical Zhou calendar was often enough not used in Western Zhou; in fact, I will argue below that most lunar month dates for Conquest-year events must be interpreted as in the “Xia” calendar. But once one sees that the lunar phase constraint must be respected, it eliminates all candidate Conquest dates other than 1040 and 1045. These are the only two years that work, even if Tung Tso-pin’s tables of first days of months (*Chronological Tables of Chinese History*, Hong Kong University Press, 1960), which I use, are here or there a day off, and no matter whether the “first month” is taken to be the pre-winter-solstice month, or any one of the next three months, with intercalary months posited wherever they might possibly occur.

A table of possibilities demonstrates this. If the *jiazi* victory was in the “2nd month,” and five days counting from *jisipo* taken as the first day of the fourth quarter (see note 6), then *jiazi* cannot be later than the 30th, making the first day of the month *yiwei* (32) at the earliest. Let us suppose that *jisipo* then the first of the month would be day *wuxu* (35). Since Dong is sometimes a day off the actual syzygy in China, I will check as possible first days of the Conquest month days *jiawu* (31) through *jihai* (36); and pretending that I do not know the month, or when intercalations were done, I check every month from the pre-winter-solstice month through the next six months (0, 1, 2, 3, 4, 5, 6.). The year cannot be Wu’s succession year 1049, but let us check the next ten years (I use numbers for *ganzhi*):

Month	0	1	2	3	4	5	6
Year							
1048	51	21	50	20	49	19	49
1047	46	15	45	14	44	14	43
1046	10	39	9	38	8	37	7
1045*	*4	<u>34</u>	3	<u>33</u>	2	<u>32</u>	1*
1044	59	28	58	27	57	26	56
1043	22	52	21	51	21	50	20
1042	17	46	16	45	15	44	14
1041	11	41	11	40	10	39	9
1040*	* <u>35</u>	5	<u>34</u>	4	<u>33</u>	3	<u>32</u> *
1039	30	59	29	58	28	57	27

It will at once be seen that the possible first days (underlined here) occur only in years 1045 and 1040.

2. But this conclusion contradicts a text in the *Guoyu* (which was the basis of Pankenier's case), stating that Jupiter was in Jupiter station Quail Fire at the beginning of the Conquest campaign. If this text is to be rejected, it must first be accounted for. I have published an argument ("*Guoyu* 'Wu Wang fa Yin' tian xiang bian wei," *Guwenzi Yanjiu* 12 (1985), pp. 445–465) that it was deduced in computations in the –1st century, perhaps about 50 BC, and then inserted in the *Guoyu* (for while Liu Xin exploits the text, Sima Qian a century earlier knew nothing of it). But this analysis assumed that the Muye victory was in January of 1045, and that Wu's campaign therefore started before the winter solstice month, as the *Guoyu* requires. This is probably not true, and I here offer a better explanation:⁵

The *Guoyu* text represents its information as given in a conversation occurring around 522 BC. If the Conquest was actually in 1040, and we accept the month and day dates in the "Zhou Benji" in the *Shiji* but interpret those dates as in the "Xia" calendar (beginning the year in the pre-spring-equinox month), and assume that those month and day dates, and the year date 1040, were "received" information ca. 525–475 BC, then a calculator at that time would have deduced (a) that Jupiter must have been in Quail Fire at the beginning of the Conquest campaign, but also (b) that the month dates must be interpreted as in the Zhou calendar rather than in the Xia calendar, which would make the campaign begin before the solstice, as in the present *Guoyu* text. So it would seem that the best explanation of the "Quail Fire" tradition calls for dating the Conquest to 1040.⁶

5 The *Guoyu* astrological text is found in section 7 of part 3 of the "Zhou Yu." It requires a pre-solstice-month starting date for the campaign because it locates the sun in "*Ximu zhi jin*," "the ford of Split wood," i.e., the crossing of the Milky Way in the "Basket"—"Southern Dipper" area, at a point about 225 degrees west of the solstice point in the zodiac being used (probably a zodiac correct for about 1000 BC), and the position given for the moon shows that we must suppose the month to end just a few days later. While I now think that my account in *Guwenzi Yanjiu* is unlikely to be the way these astrological details were derived, it may well have been the way they were understood by a person adding them to the *Guoyu* in the –1st century.

6 In this reasoning I am accepting the following month day dates, and am assuming that a calculator in the early –5th century is also accepting them: start of the campaign, day *guishi* (30), =the day after *pangsipo* (as in Liu Xin's quotation from the "Wu Cheng" in *Han shu* 21B (p.60a of 76 in my text), where the month is given its Zhou calendar name "1st month"); Zhou forces crosses the Yellow River, 12th month, day *wuwu* (55) (as in *Shiji*, "Zhou Benji," p. 8b of 40); victory at Muye, 2nd month, day *jiazi* (1) = 5 days counting from *jisipo* (as in *Shiji*, "Zhou Benji" and in Liu Xin's quotation from the "Wu Cheng"). I count as a mistake Sima Qian's assumption that "12th month" was an earlier date *in the same year* as "2nd month" (for him, "11th year), but I think Qian was right (for reasons apparent in sections 3 and 8 of this paper) in

3. There is another tradition about Jupiter, buried in a commentary to the “Ru Xiao” chapter of *Xunzi*, which says that at the beginning of the Zhou campaign Jupiter was in “the north.” “Quail Fire” is in the south on an astrologer’s chart; due north would imply a location of the planet in lunar lodge Xu, in the vicinity of Aquarius. If the dates in the “Zhou Benji” are used, but taken as in the so-called Xia calendar, then the campaign must have begun in mid-January, 1040, and at that time Jupiter was in Xu. This alternative tradition thus further confirms 1040 as the Conquest date.⁷

assuming that these dates (obviously copied from some source) are Xia calendar dates, e.g., that “12th month” is the month *after* the winter solstice month.

A calculator in the early –5th century would have concluded that the Conquest campaign, if beginning in late 1041, must have begun at a time when Jupiter was in Quail Fire, for the following reason: He would have believed (mistakenly) that the Jupiter cycle is exactly 12 year. He would have observed that (e.g.) 489 was a “Quail Fire” year for Jupiter; and he would then see that 489 is just 12 x 46 years after 1041.

And he would have concluded that received month dates for Conquest-year events must be read in the Zhou calendar rather than in the Xia calendar, for a more complex reason: The classical system of counting years by 19’s, with a *gan* day-cycle designation for the first day of each 19-year *zhang*, gives days true to the actual lunar and solar calendars between 579 and 389 BC. This suggests that the system was first used in the early –5th century (the midpoint being 484). A calculator using this system at that time to determine *gan* designations of first days of lunar months in 1041–1040 would get them two cycle numbers too early, since the system has a built-in error. This would force him to conclude that a Xia-calendar interpretation of known dates of events in the Conquest year could not be right, because the error would tell him that the date *ji* (1) for the battle of Muye would not be in the last quarter of the Xia 2nd month, but would be the first day of the Xia 3rd month. Therefore he would be obliged to date the battle back two months, i.e., interpret “2nd month” in the Zhou calendar. This would make *ji* fall on the last day of the 2nd month, which would seem acceptable.

The calculator could now do either of two things. He could interpret all the month dates in the Zhou calendar, i.e., move them all back two months, making the campaign begin in the last month of autumn, as does the *Guoyu* astronomical data; or he could keep the starting month fixed, rewriting it in the Zhou calendar as “1st month,” thus making the campaign last only one month, from launching to victory. This appears to be what was done in an adjustment of the *Shang shu* text.

7 I am indebted to Prof. D. W. Pankenier for calling my attention to the “north” tradition. (See his Stanford doctoral dissertation, “Early Chinese Astronomy and Cosmology: the “Mandate of Heaven” as Epiphany” (August, 1983) p. 241. Pankenier himself dismisses this tradition (pp. 243–244) without claiming to have disproved it.) I tentatively take 17 January 1040 as the kick-off date, because I see no way that the alternative, the last month of autumn in the preceding year, could have led to a reinterpretation that made the date the “1st month.” (The last month of autumn would still have Jupiter “in the north” astrologically speaking, although not due north.)

4. The *Guoyu* says that the state of Jin began in a year when Jupiter was in station Great Fire (vicinity of Antares). This is consistent with the *Bamboo Annals* which dates the founding enfeoffment of Cheng Wang's younger brother Yu in Tang (later Jin) to 1035, and dates a conjunction of the five planets, said there to be in lunar lodge Fang—in the middle of Great Fire—to 1071, which would be three 12-year Jupiter-cycles earlier. The *Bamboo Annals'* date for the enfeoffment is three year after the end of the Duke of Zhou's regency as dated in the *Annals*. Now, the actual date when Jupiter was in Great Fire was 1031 (also late 1032). Therefore if the relative event-sequence in the *Annals* is approximately right, and if this Jupiter tradition is true, then the Regency must have ended well before 1031, and this would require that the Conquest be actually in 1045. But if the tradition about Jupiter is accepted, and there is evidence that the enfeoffment occurred before the end of the Regency, then the conquest must actually have been in 1040.⁸

And there is such evidence: The *Shiji* chapters “Zhou Benji,” “Lu Shijia,” and “Jin Shijia,” all give details of the sequence of events leading to the enfeoffment of Tang-shu Yu that date it before the end of the Regency. For this kind of material the *Shiji* is not always reliable; but there is confirmation in the *Zuo zhuan* (Xi Gong 15.14), where near the end of this long section we read, “Moreover I have heard that when Tang-shu was enfeoffed, Jizi said, “His posterity is sure to be great.” Jizi, a shy Shang prince and reluctant vassal of Zhou, almost never came to court; but he almost certainly would have been part of a convocation of the regional lords recorded in the *Annals* in the summer or autumn of the last regency year. This, then, is likely to have been the time when Tang-shu's enfeoffment was formalized; and if it was, then the Conquest must have been in 1040.⁹

Note that the “Wu Cheng” text as quoted by Liu Xin has the victory dated “*yue ruo lai er yue ... jiazi*,” which has to mean “on day *jiazi* (1), ... in the (coming 2nd month =) 2nd month of the next year,” showing that a source text must have had “*shi-yi yue*,” “11th month,” instead of “*yi yue*,” “1st month,” for the date of Wu's departure from his capital. Similarly, *Yi Zhou shu* “Shi Fu,” “*lai dingmao*,” means “on day *dingmao* (4), in the next month.” (This “*lai*” idiom, now recoverable from oracle inscriptions, has been misunderstood for over 2000 years.) See note 20.

8 See *Guoyu*, “Jin Yu” 4 (about one-fifth of the way into the long first section) for the “Great Fire” location of Jupiter at Jin's beginning. In *Annals* terms the event was in Cheng 10, and the conjunction was in Di Xin 32.

9 According to the *Annals*, in Cheng 8 (the year after the end of the Regency), the rebelling state of Tang (which became Jin) was reduced, later (Cheng 10) being given as fief to Yu, who becomes known as “Tang-shu Yu.” In Cheng 11 Tang-shu finds a grain-stalk prodigy and presents it to the king. In the *Shang shu* prefaces the grain-stalk affair occurs during the Regency, and so also in the *Shiji* “Zhou Benji” and “Lu Shijia.” Further, in *Shiji* “Jin Shijia,” it is the Duke

5. The *Shiji* appears to represent the Duke of Zhou's son Bo Qin as already lord of Lu early in the Regency, before the outbreak of the revolt of Lu Fu and the royal uncles. Therefore, determination of the date of the beginning of Bo Qin's reign will strongly confirm either 1045 or 1040 as Conquest date.¹⁰

The probable date can be got as follows: Liu Xin (*Han shu* 21 B) says that Bo Qin reigned 46 years. The *Shiji*, "Lu Shijia," gives reign lengths of Lu dukes that imply that Bo Qin died in 999. This indicates 1044 as his first year; and this is possible only if the Conquest was in 1045, and his father the Duke of Zhou gave the fief of Lu to his son almost at once, after Wu Wang granted it to the Duke. But is 999 the year of Bo Qin's death?¹¹

Almost certainly not. In the *Bamboo Annals*, too, one finds data on the chronology of Lu, in the form of entries recording the deaths of most of the Lu dukes. The data is incomplete, and distorted; but carefully analyzed it shows that the tradition reflected in the *Annals* had Bo Qin dying in 990. This would imply 1035 as his first year, which was the first year also of Cheng Wang's 30-year accession calendar (whichever date we take for the Conquest). And there is a tradition (Liu Xin has it) that Bo Qin's and Cheng's reigns began at the same time. Further analysis reveals why the *Shiji* and the *Annals* differ: in the *Shiji*, the seventh duke, Xian, has a reign of 32 years, which would have to be 887–856. The date of Xian's death has dropped out of the *Annals*, but one can deduce that his reign must have been 23 years (rather than 32), 878–856. The shorter reign is almost certainly correct, because Xian was the brother of his predecessor Duke Li, whose reign was 37 years; and their father Duke Wei reigned 50 years.¹²

of Zhou who suppressed the rebelling Tang, during the Regency; and its account of the circumstances of the enfeoffment makes it clear that Cheng was still a minor.

10 The "Lu Shijia" says that Wu Wang granted Lu to the Duke of Zhou right after the Conquest, and that the duke gave it to his son Bo Qin soon after Cheng Wang's succession. Only then does the account take up the outbreak of the eastern revolt of Lu Fu and the royal uncles.

11 Liu Xin's account in *Han shu* 21B (p. 63a of 76 in my text) is often read as saying that he merely "inferred" the figure "46." I read it instead as taking the datum "46 years" as a premise, leading to the "inference" that Bo Qin "served Kang Wang." Chavannes, a century ago (*Mémoires historiques* 1, p.cxciii), noticed that the *Shiji*'s implied death date 999 for Bo Qin is exactly 46 years (inclusive) after the *Bamboo Annals*' first year for Cheng Wang, the date converting to 1044. This suggests that 46 years was well known to be the length of Bo Qin's reign in Lu.

12 Liu Xin (p. 62a) says that Bo Qin's 46-year reign and Cheng Wang's 30-year reign began in the same year, and I agree. Liu, however, makes the 7-year Regency precede the 30 years, as does the *Annals*, though the latter simply gives Cheng 37 years, including the Regency. I am arguing that the Regency was the first 7 years of 32 (= 2 + 30) years for Cheng, i.e., that it began

The implication then is that Bo Qin's rule formally began in 1035, and that the Regency began not much earlier; and this requires that the Conquest was in 1040.¹³

6. How long did King Wu live? The *Li ji*, “Wen Wang shizi,” says he had 93 years (and that his father Wen had 97 years). We scoff; but we may take seriously the entry at the end of Wu Wang's chronicle in the *Annals*, that gives Wu Wang 54 years.¹⁴

When was Wu born? An often quoted passage from some unknown source reads (variously), “*Wen wang (nian) shiwu (er) sheng Wu Wang*,” always interpreted “Wen Wang produced Wu Wang at age fifteen.” But this is improbable (Wu actually had an elder brother, who died young; and there is an even chance that Wen's first offspring were females). I suggest that the original wording probably was that Wu was born “in year fifteen” (*shiwu nian*) of the current reign, perhaps Wen's own, but perhaps instead the reign of Di Yi, the (probable) current Shang king.¹⁵

Di Yi's dates are known: I have demonstrated (in my *HJAS* article, p. 558, and in an earlier AOS conference paper in 1983) that the Di Yi reign began in 1105. Di Yi 15 thus was 1091. 1091 would be the first year of a 54-year life that ended in 1038. Wu died two years after the Conquest, and this again puts the Conquest in 1040.¹⁶

in Cheng's succession year, and that “30 years” is simply Cheng's reign counting from his accession year (which I take to be 1035).

13 It is possible that Bo Qin's tenure in Lu was actually 2 + 46 years, i.e., that he was given his father's fief immediately. See my argument in *HJAS* 43 pp. 530–531.

14 An engaging possibility is that Wen lived 79 years rather than 97. This would put his birth in 1128. This is one year before the date of the death of Wen's grandfather Dan-fu, if his death-date given in the *Annals* chronicle for Wu Yi of Shang is reduced by 12 years, as must be done for pre-Conquest dates for Zhou in the *Annals*. Tradition says that Dan-fu noticed Wen (Prince Chang) as a baby just before he died, and that it was for this reason that Ji Li (Wen's father) became the successor.

15 Liu Xin uses the line in his analysis of Zhou chronology in *Han shu* 21B (p. 61a of 76, in the edition I happened to use). Kong Yingda (Tang Dynasty) uses it repeatedly in his sub-commentary (*shu*) to the Classics, and when he gives a source it is usually Zheng Xuan's (lost) comments at the opening of the “Odes of Bi” and to the “Decade of Wen Wang” in the *Shijing*.

16 For Wu's death two years after the Conquest, see, e.g., *Shiji*, “Feng Shan Shu” (p. 7a of 28 in my text). Shaughnessy shows that “two years” is an exclusive rather than inclusive count. (See E. L. Shaughnessy, “On the Authenticity of the *Bamboo Annals*,” *HJAS* 46 (1986), pp. 149–180.)

7. Chapter 45 in the *Yi Zhou shu* is titled “Wu jing,” i.e., “Wu (Wang) Warned.” It begins: “It was the 12th cult-year, 4th month. The King reported a dream. On day *bingchen* (53)” the dream was divined; and (the text continues) “(the King) then issued an order directing Dan, Duke of Zhou, to appoint the successor, and to give Prince Song the text, and (a copy of) the “Bao Dian” (“Treasured Document”).”¹⁷

We must assume that the dream signifies Wu’s impending death in (probably) the same year. If Wen Wang died in 1050, the year of Wu’s death here indicated is again 1038. Further, the day-date fits, if we assume that Zhou calendar was two days behind precession, for that would make the winter solstice appear to fall on 1 January 1038 (rather than on 30 December, its actual date). 1 January happened that year to be the first day of a lunar month, and if that month counted as the Zhou 1st month then the “4th” month would begin with day *yimao* (52). When a date is incomplete, it is likely that the events recorded start with the first of the month. If the dream occurred that night, it was being reported by the king the next day, i.e., *bingchen*, as stated.¹⁸

The “Bao Dian” is chapter 29 of the *Yi Zhou shu*. It consists of a homily by the King, and it opens with a more complete date: “It was the King’s 3rd cult-year, 2nd month, day *bingchen* (53), first day of the month.” In a calendar in which the 4th month begins with day *yimao* (52), the 2nd month must begin with *bingchen* (53), so it appears that the same year is meant—now called “the King’s 3rd cult-year”—which it must be, if Wu Wang died two years after the Conquest, and the Conquest was in 1040. 1038 is the only year that could be both the year of Wu’s death in his own 12th year, and also the date of document issued by him in his “royal” 3rd year.¹⁹

17 “The text”; I assume, of the king’s order. This chapter is a fragment, not always clear.

18 An example of a first-of month date not so indicated is the appointment of Mao Qian in the 9th year of Gong Wang in the *Annals*, there said to be “first month, *dinghai*.” The year was 909, whose (Zhou calendar) first month began with day *dinghai*. (In *HJAS* 43, pp. 505, 566, I incorrectly dated the dream incident to the month of the victory celebration in the Conquest year.)

19 The two chapters (#29, #45) apparently have different sources: The “Bao dian” (#29) uses the quasi-copula particle *wei* as in the *Shijing*: the “Wu Jing” fragment appears older, and use *wei* as in the *Shang shu*. For this and other reasons, I doubt that the homiletic text is actually Wu’s; but this need not invalidate the date. The “Xiao Kai” (#23) is a homily described as Wen’s; it surely isn’t, but the date is validated by astronomy. What is happening, I suggest, is that some old chronicle contained dates and events, and later invention supplied the texts of addresses or conversations referred to or implied in the chronicle. But it must be admitted that many dates in the *Yi Zhou shu* are the result of later invalid deduction; e.g., the year after the Conquest was not (in my judgment) the “13th year” (“Da Kuang,” #38) but the 11th.

8. In the foregoing step I needed to assume that the Zhou calendar was two days behind precession. The same assumption is indicated if 1031 was the last Regency year, for in the “Luo Gao” the last day in the “12th month” of that year, the date of a great rite by Cheng Wang, is said to be day *wuchen* (5). In 1031–1030, just as in 1039–1038, the Julian calendar and the lunar calendar happen to coincide: *wuchen* is the last day of a lunar month, and is also 31 December. But the solstice that year was 30 December, so in the Zhou calendar the month ought to have been the first month of the year corresponding to 1030. Thus (if this test uses a Zhou calendar) we have to assume again that the current Zhou calendar was at least two days behind precession (of which the Chinese at this time were ignorant). My guess is that Cheng thought he was sacrificing on the eve of the winter solstice.²⁰

My chief reason for returning to the date 1045 for the Conquest was my conviction, based on fair evidence, that in late Shang and early Zhou first days of the 24 solar weather periods were favored as lucky days and chosen for important state acts, such as the launching of a campaign, the fighting of a battle, or a victory celebration. 1045 located these events in the Conquest year on such days; 1040 did not. But if the Zhou calendar was two days off in dating the win-

20 But it is, I think, the Xia calendar and not the Zhou calendar that the Zhou court was still using at this time, and month numbers in the “Shao Gao” (and “*shi-yi yue*” in the “Wu Cheng”) have been rewritten so as to translate these dates from the Xia calendar to the Zhou calendar. The argument: the word *lai* in a date signifies that the following named month (or day) falls in the following larger time unit, year (or month, or, in Shang oracle inscriptions, *xun*). Therefore the sequence in (e.g.) the “Shao Gao,” “*er yue... yue ruo lai san yue...*” must have originally been “*shi-er yue ... yue ruo lai zheng yue ...*” if this is right, it is another powerful argument for 1040 as Conquest date, because 1031 will then satisfy the “Shao Gao” dates, but 1036 will not (See note 7.)

In any case, the calendar would posit a solstice day that would be one day late for every 70 years that the actual occurrence of the solstice had not been checked by observation and corrected accordingly. Note the reference to the sacrifice in the “Luo Gao”: *zheng ji sui*: *zheng* is defined as a winter sacrifice to royal ancestors; *ji sui* literally is “sacrifice [to or by] *sui*,” where *sui* can be either the name of some kind of cutting sacrifice, or (its normal meaning) “year.” If it is the latter here, the meaning seems to be “performed the winter sacrifice, thus ritually marking the turn of the year.” If the year was 1036 rather than 1031, not only is it impossible for the rite to be on the eve of mid-winter; it isn’t even in winter, but at the end of autumn, contrary to the meaning of the word *zheng*.

(The *Shang shu* text of the “Luo Gao” does not say explicitly That day *wuchen* is the last day of the month. The only possible *wuchen* days in Dong’s tables are last-of-month days, and the “*zhuan*” commentary ascribed to Kong Anguo does make it explicit, calling *wuchen*-day “*hui*.”)

ter solstice, to which the 24 periods must be keyed, the situation is reversed, and it is 1040, not 1045, that satisfies this requirement.²¹

The Conquest calendar in 1040 then is as follows, if I assume that at least up until the Conquest the Zhou used the popular “Xia” calendar beginning the year with the pre-equinox month, and only afterwards (possibly long afterwards) did they promulgate what is classically described as the Zhou calendar, beginning the year with the solstice month. (Dates are taken from Liu Xin’s citations from the “Wu Cheng” in *Han shu* 21 B, and from *Shiji*, “Zhou Benji.”)

- Zhou 1/ Xia 11, day *guisi* (30) =17 January,
 Campaign begins, first day of Xiaohan (Lesser cold)
 Zhou 2/ Xia 12, day *wuwu* (55) =11 February
 Zhou armies cross the Yellow River
 Zhou 4/ Xia 2, day *jiazi* (1) =18 April
 Victory at Muye, first day of Qingming (Clear Brightness)
 Zhou 6/ day *gengxu* (47) =3 June
 Celebration in Zhou on full moon, first day of Xiao-man (Grain Ripening)

This analysis is confirmed by the last line of the “Da Ming” ode in the “Da Ya” part of the *Shijing* (Ode 236). That ode narrates Heaven’s favor to Zhou through Wang Ji, Wen and Wu, down to Wu’s victory. The last line reads, “*si fa da shang, hui zhao qing ming*” 肆伐大商 · 會朝清明. The meaning has escaped all translators and commentators: the line says, “He (Wu) let loose [his forces] and attacked great Shang; this occurred in the morning, Qingming [Day].”²²

21 Other examples of *qi*-days as lucky days: (1) the day of “attacking the site (*gong wei*) in the “Shao Gao” which would be the first day of Lichun (“Beginning of Spring”), if the year is 1031 and again we suppose the calendar is two days behind precession; the date would be 16 February. (2) The date of the victory celebration in the *Xiao Yu ding* inscription, 25th year, 8th month, 3rd quarter, *jiashen* (21), presumably the first day of the 3rd quarter, since a *liao* sacrifice is performed. Again I assume that the Xia calendar is in use, but by now correction has been made for precession. It turns out that the date is the 15th of October, 979 (25th year on Kang Wang’s accession calendar counting from 1003), 16th of the lunar month, and the first day of Hanlu (“Cold Dew”).

22 Karlgren’s translation, for example, is “He killed and smote the great (people of) Shang; the morning of the encounter was clear and bright.” (Bernhard Karlgren, *The Book of Odes*, Stockholm, 1974, p. 1888.) Any such interpretation gives a very strange—indeed, quite pointless—final line for the poem.

Some will object that in taking “Qingming” as the name of the solar *qi*-period (here, for its first day), I am assuming without evidence that the system of twenty-four solar periods, and their names, existed already in eleventh century BC. I have at least two replies: (1) Evidence does not have to take the form of testimony or the occurrence of terms in a text. See my “The

Origin of the Chinese Lunar Lodge System” (in A. F. Aveni, editor, *World Archaeoastronomy*, Cambridge University Press, 1989), where by analyzing the earliest surviving evidence of such systems, I show that partitions of the zodiac into equal 28ths and equal 24ths must have been known in China as early as the third millennium BC; and a division into 24ths implies a system of twenty-four solar periods, however named. (2) It is true (as far as I know) that there are no other occurrences of the name of a solar period in the *Shijing* or in any text of similar antiquity. But the first day (or so) of Qingming has special importance, because it was the major religious festival in the ancestor cult; “Qingming,” therefore, is likely to be one of the oldest of the names.

Another quite proper request must be addressed, however. My identification of *qi*-days depends on more assumptions than the one stated, that the winter solstice day was two days late in the Zhou calendar. Ideally a *qi*-period was 15 days—see *Huainanzi* “Tian Wen Xun” paragraph 12—but a year is five (sometimes six) days more than 24×15 . How does one decide where to locate the (normally) five supernumerary days that create five 16-day periods?

I have assumed that the five days are the solstices and equinoxes, and the first day of summer. The winter solstice day is indicated by the fact that in the oldest form of the system of 28 lunar lodges the lodge Xu is 14 *du* wide, whereas other residues of an ancient equal-space system, Xing, Zhang and Yi, are 13 *du* ($365 = (28 \times 13) + 1$); and it was in Xu that the winter solstice was located. My choice is reconfirmed by reading of “Tian Wen” 12, which also guides me to select the other days: it says that 46 days pass from an equinox or solstice to the beginning of the next season, and also that 46 days pass from the beginning of summer to the summer solstice.

I have computed the date of the winter solstice in China in late 1041 BC: it occurred at about 19 hours on 30 December, i.e., Julian Day 134 1562; so I assume that the Zhou court thought it was on 1 January, JD 134 1564. This gives the following *qi*-calendar for the first half of 1040 BC:

<i>Qi</i> -period	Days	1 st Day	<i>Ganzhi</i>	JD 134	
Dongzhi	16	1 Jan	(14)	1564	
Xiaohan	15	17 Jan	(30)	1580	Campaign begins
Dahan	15	1 Feb	(45)	1595	
Lichun	15	16 Feb	(60)	1610	
Yushui	15	3 Mar	(15)	1625	
Jingzhi	15	18 Mar	(30)	1640	
Chunfen	16	2 Apr	(45)	1655	
Qingming	15	18 Apr	(1)	1671	Victory at Muye
Guyu	15	3 May	(16)	1686	
Lixia	16	18 May	(31)	1701	
Xiaoman	15	3 June	(47)	1717	Victory rites
Mangzhong	15	18 June	(2)	1732	

The date of the Victory rites happens also to be the first day of the 3rd quarter of the month, *jiwang*, and a *liao* burning sacrifice is made. The same thing is done in the victory celebration recorded in the *Xiao Yu ding* inscription, also on a day which is both a *qi*-day and a *jiwang*-day.

The entire poem celebrates the glorious virtue of the ancestor-kings and their consorts, to whose merit the great victory on Qingming Day, in addition to being a mark on the calendar, is the most important annual festival in the ancestor cult. Thus we can now recognize the “Da Ming” ode as a Qingming Day hymn.

Note (October 1991): In this conference paper, I have set down only those arguments that allow a reasonably brief statement. They are taken from a book-length manuscript that has been my occupation for the past twelve months, on the problem of the exact date of the Zhou Conquest, subjoining a selection of unpublished papers of mine over the past dozen years that are directly or indirectly relevant. The largest part of this task I have had here to omit entirely: working out a satisfactory explanation of the various theories found in ancient literature, most notably the chronology found in the so-called “modern text” *Bamboo Annals* (actually a Warring States text), and the very different chronologies of Han scholars such as Liu Xin and Zheng Xuan. Like much in the following paper, his explanation requires me to reject or correct important parts of my article “The Dates of Western Chou,” *Harvard Journal of Asiatic Studies* 43 (1983), pp. 481–580.

11 Kong Jia of Xia, 1577–1569 BC

Abstract: This paper ties together certain results in my continuing investigations into the exact dating of very early Chinese history in *Early China* 15 (1990), I wrote (with K. D. Pang) that the eclipse assigned by tradition to the reign of Zhong Kang, fourth king of Xia, occurred on 16 October 1876 BC, and that the reign lengths of early Xia kings in the *Bamboo Annals* appear to be valid, but that there was always an interregnum of just two years between reigns (for completion of mourning). In a paper presented in Los Angeles in May, 1990, I extended this hypothesis through to the end of the dynasty, getting as terminal date 1555 BC (previously established by Pankenier), with more confirming evidence. In the “Chinese Identities” conference in Berkeley in February, 1994, I presented a paper that attempted to give exact dates for all rulers of the ensuing Shang Dynasty, arguing that the reign lengths in the *Annals* for this dynasty too are for the most part valid, and that the resulting dates explain the final *gan* component of the name of each Shang king, the *gan* being determined by the *gan* of the first day of his reign.

In the present paper, I combine these results and test them by applying them to the one Xia king whose (commonly used) name ends in a *gan*, namely the fourteenth Xia king Kong Jia.

When I was in graduate school in Harvard University almost fifty years old ago, my teachers taught me that the so-called “modern text” of the *Bamboo Annals* (*Jinben Zhushu jinian*) is a faked text, probably done in the Ming Dynasty. The faker was said to have pretended his work to be the supposedly lost chronicle that had been buried in 299 (or 296) BC, and discovered around 280 CE. He had used a few historical facts but had invented freely, adding purely imaginary dates for all events and reigns. The precious original had disappeared centuries earlier.

Modern scholars have collected quotations from the (real) text found in historical commentaries and encyclopedias, calling them the “ancient text,” i.e. the genuine text, or what there is left of it. The greatest scholars in China in the 18th century had assured us of the fraudulence of the “modern text,” and the modern scholar Wang Guowei (and others before him) had “proved” it, by

Paper presented at the annual meeting of the Western Branch of the American Oriental Society, Portland, Oregon, October 21–23, 1994

painstakingly finding the source of every item in it (or almost: he did not account for the dates). I took all this for granted until 1979, when I accidentally found that I could use the *Annals* to help solve dating problems in the texts of bronze inscriptions of Western Zhou, mid-11th to early 8th centuries BC. E. Shaughnessy, in an article in 1986, has shown that parts of the book are, verbatim, the original text. This is probably true of almost all of it.

I have spent much time in the past 15 years pursuing this discovery, making many mistakes and soon correcting some of them. Perhaps what I think I discovered in July of this year is really another mistake. You be the judges.

Very simply, I am putting together two “results”—as I would call them. (Others have sometimes been less kind.) I will show that together they imply something that one would not expect to be able to know, that if true is very unlikely to be accidentally true. And then, by a completely independent argument, I will show that what they imply is indeed true.

The first “result”: In 1990, in *Early China* 15, I published an article together with K. D. Pang, arguing that the eclipse of the sun traditionally assigned to the reign of the fourth Xia King Zhong Kang, and in the *Annals* to his fifth year, was the eclipse of 16 October 1876 BC; and further, that this is the year implied by a simple historical calculation: One starts with the conjunction of planets in February of 1953 BC, which Pankenier has argued marked the first year of effective power of Yu, the first ruler of Xia. One then provisionally accepts the reign lengths of the first four Xia Kings in the *Annals*, and supposes that between each reign there was a two-year interregnum for completion of mourning. (The *Annals* has interregnums between Xia reigns, and about a third of them are exactly two years, which would be the expected number.) It then turns out that the fifth year of Zhong Kang is 1876, and the stated month date converts to 16 October. And the eclipse occurred when the sun was exactly in the spot in the zodiac that tradition said it was in.

Also in 1990, I presented a still unpublished paper in Los Angeles in which I extended this argument through to the end of the Xia Dynasty, taking the reign lengths given in the *Annals* and assuming two years after each death of a ruler. What I ought to get, as the last year, was 1555 BC. Pankenier had already established that, and I had more reasons of my own. What I *found* was that it was the next-to-last king, Fa, whose reign ended in 1555. I then reviewed accounts in the *Annals* and elsewhere of the supposed last king Jie, called “Di Gui” in the *Annals*, and found multiple reasons for concluding that Jie was a literary invention, probably a product of the philosophical imagination of the middle or late 400’s BC. (“Di Gui” may actually have been another name for the real last king, Fa.)

This much gives me exact dates of all the Xia kings. I have supplied you with the complete list, even though I am concerned here only with a few kings at the end.

Next, I went to work on the following dynasty, the Shang. The *Annals* account gives reign lengths, but no interregnums. The name of each Shang king ends in a syllable that is the name of one of the days in the age-old Chinese ten-day week. Much work has been done attempting to explain these “*gan*” names. I had noticed years ago that date I had derived for the first year of the next-to last Shang king Di Yi, 1105 BC, began with a lunar month beginning with an *yi* day in the sixty-day cycle. But when I had tried this out on other kings, it didn’t work. Only last year did I see that probably the chronology for the Shang in the *Annals* was like that for the Western Zhou: There, there were in effect two-year breaks between royal calendars, but sometime around 400 BC people forgot about them, so that the whole chronology shrank, towards the end and away from the middle. The result had been that the king in the middle, Mu Wang, who reigned a quite respectable 39 years anyway, had his reign bloated to 55 years, the way it is in the *Shiji* and in the *Annals*.

The Shang list too has a king in the middle with a very long reign, Da Wu, with an impossible 75 years. (We are talking about something that got established fairly early on. Because you find this 75 years already in the “Wu Yi” chapter of the *Shang shu*.) Quick experimentation showed me that two year gaps didn’t work. But three years did: The last ruler, Di Xin, had a calendar beginning with 1086 BC, as many inscriptions (combined with astronomy, and with the *Bamboo Annals*) show. 1086 does not begin with a *xin* day, but 1089 does. Inscriptions, and the date for Di Yi (and again the *Annals*) show that the reign calendar preceding Di Yi, i.e., Wenwu Ding, began in 1118 BC. 1118 did not begin with a *ding* day, but 1121 did.

Encouraged, I went back to the beginning of Shang. There was funny-business after the death of the founder, Tang (a.k.a Da Yi), which occurred (I have reasoned) in early 1542. His prime minister Yi Yin, taking over during mourning maneuvering toward the throne himself. He exiled Tang’s grandson and successor Da Jia, and put two puppets on the throne by turns, Zhong Ren and Wai Bing. But I found a plausible sequence of dates that explained them all, and I have supplied you with this too. The scheme posits three-year breaks between calendars, with the first day of the succession year giving the king’s *gan* unless that *gan* would be the predecessor, in which case the proper calendar first year is used instead. I carry the scheme down to that long-lasting king Da Wu. I think he reigned much less than 75 years. But more interesting, a 12-year

reign of a “Yong Ji” is in all the received chronologies placed before Da Wu, whereas oracle inscriptions prove that Yong Ji (or Lü Ji) belongs after him. Why?

Da Wu of Shang is like Mu Wang of Zhou not just in that theirs are the two longest reigns in the received accounts of their dynasties, but also in two other respects. Both are kings in the fifth generation starting with the founder. And both have reigns that are supposed to begin exactly 100 years after the founder proclaimed his imperium. In Da Wu’s case, the year is 1475, just 100 years after 1575, which in the *Annals* is the first year in Tang’s year count, before his conquest of Xia. This wasn’t his succession year but his proclamation year; and we know this, and know furthermore that it is right, because both dynasties, as Pankenier has shown, were heralded by signs from Heaven. For the Shang, this was a dramatic formation of the planets at the end of 1576, described in the *Annals* (where it is misdated to 1580), and identified by Pankenier—his most important discovery.

So as the Shang chronology was perfected by the historians who worked on it, 1475 must have been a fixed date. But 3-year mourning intervals for four reigns had disappeared, and something had to fill the resulting 12-year gap. So (I am assuming) Da Wu’s successor was given a 12-year reign, and was thought actually to have preceded him. Again I will not recite a list of names and figures, and will ask you to examine the work I have put in your hands.

I would be the last to insist now that you have no choice but to believe all this. I don’t even dare to put before you my working out of the rest of the Shang king list, which is much more problematic. But I do think that there is enough here to be worth pursuing; and this takes me back to late Xia.

The fourteenth Xia king Kong Jia has a short but colorful account in the *Annals*. He is said, for example, to have had an officer named Liu Lei, who was a trainer of dragons, delighting the king. A female dragon in Liu’s care died; Liu pickled it, and fed the pickle to the unsuspecting Kong Jia, who liked the flavor, but then later asked where the dragon had gone. So Liu had to flee.

But this is not why Kong Jia is important to me. Kong Jia is the only Xia king whose regularly used name ends with a *gan*. “Kong Jia” means, literally, “great *jia*,” *jia* being the first of the ten day-names. Why “great”? Just possibly because what the name picks out is not just a *jia*-day, but the first *jia*-day of the sixty day cycle, *jiazi*. But to go on, we need to think we know, independently, what the first day of Kong Jia’s reign was. This is why my first “result” is necessary. It tells me that Kong Jia’s predecessor died in 1580, so that Kong Jia himself succeeded in 1579, and had a calendar that began in 1577. He was a Xia king, and the kind of lunar calendar that is supposed to have been used in the Xia era took the lunar month preceding the month containing the spring equinox as the first

month of the year. So what I have to identify is the first day—the “dark of-the-moon” day or “syzygy” day, I will assume—of that month, in a Chinese year more or less coinciding with the year 1577 BC.

Here is one way of doing it, if you don’t have a computer program that does it for you. The relation of first days of lunar months to days in the solar year is repeated approximately every 19 years. Take forty 19’s = 760, and consult Herman H. Goldstine’s *New and Full Moons, 1000 BC to AD 1651* (Philadelphia: American Philosophical Society, 1973), for the year 1577–760, =817 BC. The new moon beginning the pre-spring-equinox month in that year occurred on 5 February. The 19-year cycle if used without correction would yield a date that would be one day too early for every 310 years back, so 15 February should be two days early. Next, consult W. D. Stahlman and Owen Gingerich, *Solar and Planetary Longitudes for years –2500 to +2000 by Ten-day intervals* (Madison: University of Wisconsin Press, 1963), for the year 1577 BC (i.e., –1576), for 17 February. This day had Julian Day number 114 5471. To obtain the 60-day cycle number for that day, divide the JD number by 60 and subtract 10 from the remainder. The answer in this case is day one in the cycle, *jiazi*.

Finally, go to Paul Ahnert’s *Astronomisch-Chronologische Tafeln für Sonne, Mond und Planeten* (Leipzig, 1960), and check out 17 February 1577 BC to see if it was in fact a first-of-month day. One finds that it was. I have given you the calculation, face-up.

What does one make of this? Certainty we do not have; but then, we never have it. There is a one-in-sixty chance that a day selected blind might turn out to be a *jiazi* day. Looking again at the *Bamboo Annals*, one finds that Kong Jia’s predecessor king Jin had another name sometimes used, “Yin Jia.” The most obvious meaning is “succession-*jia*.” So, look again at Nivison’s table of dates for Xia kings, finding that Jin’s succession year (the year following the year of death of his predecessor) was 1589 BC; and now go through the same calculation, finding that this year in the Xia calendar probably began with day *jiaxu*. There was a one-in-ten chance that this date might accidentally turn out to be a *jia* day; and so altogether a 1-in-600 chance that the two *jia* names would turn out accidentally to correspond to expected *jia* days. Not too bad.

Shall we say, then—cautiously—that the Shang institution of *gan* names for kings was being anticipated by Xia rulers? Perhaps not: First, we deal with only two kings, #13 and #14 out of a list of sixteen. Second, they are successive kings, yet they have the same *gan*; and the Shang would never do that. Third, there are, in the *Annals*, a few pre-dynastic Shang dates and names to check; and when I check them, I find the Shang convention already in use during the Xia

era. Explaining this would take time that I don't have; but I have supplied you with these data too.

So I see the matter this way: the *gan* name convention for rulers was used by the Shang royal ancestors for centuries before the Shang dynasty. Toward the end of the Xia era, it began to be used by others too, at least by Xia kings, but incompletely and only occasionally.

But, of course, the real news is that something all of us learned in school—and something some of us still vehemently insist—isn't right: Xia has been “mythicized,” but Xia isn't a myth. There really was a Xia Dynasty. And I seem to have the names and exact dates of its kings, every one of them. *What* the Xia was—a sophisticated urban civilization, with writing; or just a collection of dirty stone-age villages with common chiefs who somehow got their names and years remembered; or something in between—this is a different question entirely, for which I have no answer.

And there is one more thing to recognize from this inquiry: the “modern text” *Bamboo Annals*—called “modern” only apologetically, because it is supposed to be a fake—is turning out to be a veritable rosetta stone for figuring out the exact dating of unbelievably early Chinese history.

These several matters of fact to which my argument points are important, I think. But before I close, I want to redirect your attention to the form of my argument itself. I do this because it has been my experience that most historians dismiss this kind of argument as unacceptably speculative.

I began with two hypotheses that I thought I had established reasonably well (by argumentation similar to the present argument, actually). One gave me the exact dates of the Xia kings; the other explained the *gan* names of the Shang kings as being derived usually from the first days of their reigns. I put these two hypotheses together into a single theory, and found a test case, in the late Xia king Kong Jia, conspicuous because he, unlike other Xia kings, had a *gan* name. The first hypothesis gave me his exact dates. The second one told me that the first day of his reign, given those dates, ought to be a *jia* day, and probably a *jiazi* day. Then by a completely independent calculation, owing nothing to history, I found that the indicated day *was* a *jiazi* day. I argue now that the *best explanation* of this “predictive” success is that each of my two hypotheses, as a whole, is true.

Kong Jia of Xia: Data

First days for Shang pre-dynastic ancestor, late Xia kings, and early Shang kings, as determining *gan* names.

1 Shang high ancestor

Shang Jia Wei: Since the date in the *Annals* for what seems to have been Tang's proclamation year seems to be correct as given (1575, the year following the conjunction of 1576), one may tentatively assume that the absolute date equivalent of the *Annals* date for Shang Jia Wei (recognized as the founding ancestor in Shang sacrifices) is also correct. I.e., I assume that in pre-Han revisions of the *Annals* the dating of events in the Shang house was controlled by an independently preserved chronology; it can be shown that this was approximately true for the ducal house of Lu. In the *Annals*, Shang Jia Wei's father Zi Hai (or Wang Hai) was killed by the lord of You Yi in the 12th year of Xie of Xia, i.e., in 1719 BC (the actual Xia date must have been the 35th year of Mang of Xia). Therefore Shang Jia Wei's succession year was 1718. (In 1715, after completion of mourning, he avenged his father by attacking and killing the lord of You Yi.) The Shang year is supposed to have begun with the month after the winter solstice month. In 1718, this month began on 18 January, JD 109 3941, which was day *jiaxu* (11).

2 Xia kings

Yin Jia: This was another name for the 13th king, Jin, the predecessor of Kong Jia. Jin's dates were 1589/1587–1580. Since "Yin Jia" means "succession Jia," we examine the year 1589. The Xia calendar was supposed to begin with the pre-equinox month, which in 1589 probably began on 1 March, JD 114 1101, a *jiaxu* (11) day. (The Xia day was supposed to have begun at dawn. The syzygy occurred about an hour after midnight in China.)

Kong Jia: Dates, 1579/1577–1569. The king's official calendar began with 1577, and the first day of the pre-equinox month was 17 February in that year, JD 114 5471, a *jiazi* (1) day. See separate calculation.

Di Gui: I have argued that the *Annals* account of the last Xia king is a literary invention, and that there was no king Jie (the better-known name). But it is likely that the name "Di Gui" was actually another name for the actual last ruler of Xia, the 16th king, whose name was Fa. The dates of king Fa were 1563/1561–1555. The succession year 1563 began 12 February, JD 115 0580, which was a *guiyou* (10) day.

3 Early Shang kings

Tai Yi (Da Yi, also called Tang, the dynastic founder): His succession year in the Shang royal line is probably unknowable. The *gan* "yi" may, however, have been determined when he proclaimed himself king of the world. I assume that

he did this in 1575, after the conjunction in 1576. He could not have used 1575 to determine his *gan*, because the year began with 26 January, JD 114 6180, a *guichou* (50) day, and *gui* was the *gan* of his father Shi Gui. But it is possible that (as in Western Zhou) the purpose of the post-mourning calendar was not just to enable the king to finish mourning before calling himself king, but was also to enable subjects to clear any mourning obligations before ritually recognizing a new ruler. (See my “The Dates of Western Chou,” *HJAS* 43:530–531.) The normal interval in Shang being three calendar years (rather than two, as in Xia and Zhou), there would have to be a new official calendar in 1572. (I am supposing that at the beginning of Shang, it happened to be the equivalent of the “Mandate” calendar that was continued, in the *Annals*, after the conquest of Xia; whereas in Zhou it was the calendar beginning in 1056, two years after the “Mandate” year.) 1572 began 22 January, JD 114 7272, day *yichou* (2).

Zhong Ren: Tang (Da Yi) is said variously to have ruled 12 years or 13 years. I assume that he died early in the 13th year, 1542; and that it was the Shang rule that a king’s year of death counted as in his calendar only if he lived through most of it. (It was this principle, I believe, rather than any difference in the length of mourning, that made most of the mourning-completion intervals three years rather than two in the Shang.) Thus 1542 counted as the first year of the next reign. The prime minister Yi Yin, seeking the throne for himself, was in control. I now propose the following account for what seems (from the *Annals*) to have happened: Yi Yin at first recognized Tang’s grandson Da Jia (Tai Jia) as heir, but named an alternate “king” to perform other ritual royal functions while Tang attended to arduous mourning obligations. This was Zhong Ren; and 1542 was a *ren* year. Shortly after that, Yi Yin exiled Da Jia; his years of exile were 1541–1536. To replace Da Jia as mourner, Yi Yin named Wai Bing, 1541–1540, another puppet. 1541 was a *bing* year. After the mourning interval was over, Zhong Ren continued as nominal king, 1539–36, when Da Jia escaped from confinement, returned and killed Yi Yin. (Tradition, and the *Annals*, give Wai Bing two years, and Zhong Ren four, in that order.) But 1539 was rightfully Da Jia’s first official year, and it was a *jia* year, determining Da Jia’s *gan*, since his succession year 1542 had provided Zhong Ren’s *gan*. The first day of 1542 was 22 January, *renyin* (39).

Wai Bing (Bu Bing): Year 1541, first day 11 January, *bingshen* (33). With Tai Jia, grandson of Tang.

Tai Jia (Da Jia): Year 1539, first day 18 January *jiayin* (51). The dates: 1542/1539–1528, 3+12 years. (*Annals*: 12 years)

Wo Ding: Dates 1527/1524–1506, 3+19 years. 1527 was a *jia* year, beginning 4 February, day *jiayu* (11), and was avoided, *jia* being the predecessor’s *gan*. 1524 began 1 February, *dinghai* (24). (*Annals*: 19 years) Royal brother, not main line.

Xiao Geng (Da Geng): Dates 1505/1502–1498, 3+5 years. 1505 began 3 January, day *dingyou* (34), and *ding* had to be avoided. 1502 began 31 December (1503), *gengxu* (47). (Here I assume the solstice month; solstice date was 2 January.) (*Annals*: 5 years)

Xiao Jia: Dates 1497/1494–1478, 3+17 years. 1497 began 3 February, *gengxu* (47), avoided. 1494 began 1 January, *guisi* (30) (assuming the solstice month). *Gui* is always avoided (perhaps as being the *gan* of Tang’s father), the next day *jia* being used instead. (*Annals*: 17 years) Brother of Xiao Geng.

Da Wu (Tai Wu): Dates 1477/1474–?, 3+? years. I assume the solstice month: 1477 began 24 December (1478), *jiayin*, avoided. 1474, if an intercalation was missed, began 22 November (1475, beginning of winter), *wuxu* (35). Son of Xiao Geng.

(Chance of these results being accidental: 1, in 10 raised to a power equal to the number of successive *gan* determinations and avoidances.

4 Calculation for Kong Jia (using Paul Ahnert’s *Tafeln*):

1577=–1576, =–1600+24; 17 February = JD 114 5471 = *jiazi* (i)

	Sun		Moon		
	L	A	L	A	E
s	265.2	138	102.9	212.0	38
a	0.9	L+A	90.5	304.3	103.4
m	30.6		48.3	33.7	351
d	15.8		210.8	1.7	181
	*		*	*	*
L	312.5		L 666.3	A 350.8	E 770
			=306.3		=50
L'	+1.9		L'– 1.1	L+A	297.1
	*		Le+ 1.0	E+L+A	347.1
	314.4		Lv– 0.1		
			*		
			306.1		

Sun at zero hours world time: 314.4 (moves about 1 degree a day)
 Moon, same: 306.1 (about 13.2 degrees a day)

Approximate time of syzygy, about 16:30 world time, i.e., about midnight in the longitude of Luoyang (Xia capital area); the next Xia day began the following dawn.

5 Chronology of hypothetical pre-428 BC *Bamboo Annals*, Huang Di to beginning of Shang (earliest dates are not historical):

2287–2188	Huang Di, 100 years
2187–2181	Zuo Che interregnum, 7 years
2180–2103	Zhuan Xu, 78 years
2102–2101	(interregnum, 2 years)
2100–2038	Di Ku, 63 years
2037–2036	(interregnum, 2 years)
2035–2027	Zhi, 9 years (ousted)
2026–1969	Yao (Fang Xun), 58 years
1976	Yao 51, Shun (Chong Hua) appointed
1969	Shun banishes Zhu, Yao's heir; imprisons Yao
1968–1935	Shun, 32 (actually 34) years
1960	Yao dies
1959–1958	(interregnum, i.e., calendar break, 2 years)
1953	Shun 14: Yu of Xia appointed (conjunction)
1934–1933	(interregnum, i.e., calendar break, 2 years)
1932–1907	Yu, 26 years (47 in fact, 45 with calendar break)
1906–1905	(interregnum, 2 years)
1904–1889	Qi, 16 years
1888–1887	(interregnum, 2 years)
1886–1883	Tai Kang, 4 years
1882–1881	(interregnum, 2 years)
1880–1874	Zhong Kang, 7 years
1876	Zhong Kang 5, solar eclipse in Fang, 16 October
1873–1872	(interregnum, 2 years)
1871–1844	Xiang, 28 years
1843–1842	(interregnum, 2 years)
1841–1821	Shao Kang, 21 years
1820–1819	(interregnum, 2 years)
1818–1802	Zhu, 17 years
1801–1800	(interregnum, 2 years)
1799–1756	Fen, 44 years
1755–1754	(interregnum, 2 years)
1753–1696	Mang, 58 years
1695–1694	(interregnum, 2 years)
1693–1669	Xie, 25 years
1668–1667	(interregnum, 2 years)
1666–1608	Bu Jiang, 59 years (retires; no interregnum)
1607–1590	Qiong, 18 years

1598	Qiong 10: Bu Jiang dies; no calendar break shown
1589–1588	(interregnum, 2 years)
1587–1580	Jin, 8 years (a.k.a Yin Jia)
1579–1578	(interregnum, 2 years)
1577–1569	Kong Jia, 9 years
1576	Kong Jia 2: the planets “move in succession”
1575	First year of Shang’s claim to rule
1568–1567	(interregnum, 2 years)
1566–1564	Hao, 3 years
1563–1562	(interregnum, 2 years)
1561–1555	Fa, 7 years (perhaps a.k.a Di Gui)
1555	Xia defeated by Tang (Da Yi) of Shang
1554	First year of the Shang Dynasty

6 Evolution of early Shang chronology in the *Annals*

	Actual dates (hypothesis)	4-year shift, mourning deleted <i>(Bamboo Annals)</i>	Effect of mourning deletion
Conjunction	1576	1580	
Proclamation	1575	1575	
Shang 1	1554	1558	
Tang’s 12 th year	1553	1547	
Zhong Ren (for Tai Jia)	1542		
Wai Bing (mourns)	1541–40 (2)	1546–45 (2)	
Zhong Ren	(1542/) 39–36 (4)	1544–41 (4)	
Tai Jia, reign	1539–28 (12)	1540–29 (12)	
Wo Ding mourns	1527–25 (3)		–3
Wo Ding reign	1524–06 (19)	1528–10 (19)	
Xiao Geng mourns	1505–03 (3)		–6
Xiao Geng reign	1502–98 (5)	1509–05 (5)	
Xiao Jia mourns	1497–95 (3)		–9
Xiao Jia reign	1494–78 (17)	1504–1488 (17)	
Tai Wu mourns	1477–75 (3)		–12
[Yong Ji		1487–76 (12)	+12]
Tai Wu reign	1474–(?)	1475–01 (75)	
Yong Ji	?		

I assume that the revision at some stage was done by someone who realized that his “corrections” left a large gap before 1475, and filled it in with Yong Ji’s 12 years; i.e., perhaps Yong Ji’s reign (properly after Tai Wu) actually was 12 years. Or it may be that this editor, knowing he was judging four 3-year gaps to be in error, simply gave Yong Ji 12 years to make up for them.

It then follows (perhaps for another editorial hand) that if the date 1475 was regarded as mandatory, and Wai Bing and Zhong Ren were assumed to precede Tai Jia, there must be a four-year backshift of the earlier dates (except for 1575, held in place by 1475 and probably by an independent Shang chronology: 575 BC was the first year of Duke Ping of Song, heir of the Shang kings). His editorial hand would then in effect add: 1475, + 12, + 17, +5, + 19, + 12, + 4, + 2, +12 = 1558.

This would then force a four-year shift in the Zhou starting date: The original Zhou Chronology probably took 1058 as “Mandate” year (following the conjunction of 1059), and as the *de jure* first year of Zhou. The summary at the end of the Western Zhou chronicle takes 1062 (the year of Wen Wang’s death in the 3rd month) as the first (*de facto*) year of Wu Wang, and of Zhou (one would expect 1061). The summary for Shang says that Shang lasted 496 years. This was the actual time from 1554, the first year of Shang, to but not including the Zhou Mandate year 1058. Thus, given the date 1558, “496 years” *determines* the date 1062.

Note that the following absolute dates are validated by the *gan* hypothesis: 1542; 1541; 1539; 1524; 1505; 1502; 1497; 1494; 1477; 1474.

12 Shaughnessy's Slip

In 280 CE, what appeared to be a six centuries old royal tomb, of the Warring States era Wei kingdom, was discovered in Henan. The discoverers were thieves, who looted the contents. When the matter came to the attention of the imperial court of the then Jin Dynasty, it was found that important texts had survived.

The largest of these was a chronicle on bamboo slips, which has come to be known as the *Bamboo Annals*. It covered about two thousand years, down to 299 BC. Court scholars worked on it, transcribing it from an ancient script no longer readily readable. Over several centuries thereafter the text was quoted or paraphrased in various historical commentaries and encyclopedias, and listed in bibliographies. By the Song Dynasty, bibliographies suggest that at most a part of it survived, and after that the bibliographical record is silent.

In late Ming early Qing, in two *juan*, it was published in several different *congshu* collections. But scholars notices discrepancies between this text and quotations in earlier sources, and the consensus, by the late eighteenth century, was that this “modern text” *Bamboo Annals* was some antiquarian’s invention. There have been some important defenders of the text prior to my own entry into the issue about 15 years ago. The judgment that it is a fake has remained the academically correct one, and collections of quotations have been published, represented as the genuine “ancient text.” At the same time, exhaustive studies of the “modern text” have been made, notably by Wang Guowei (d. 1927), trying to show where the faker go the material for every line. Events in it are dated, back to 2145 BC, and extend back at least two centuries before that. It was assumed that the faker simply made up the dates.

Just ten years ago, E.L. Shaughnessy (University of Chicago) notices several things that fitted together. In the so-called “modern text” the chronicle for Cheng Wang of Western Zhou has a three-year gap. There are entries for every year up to year 14, then the next entry is for year 18. He then noticed that one of the Jin Dynasty editors had written that the books in the tomb discovery were written on 40-space slips of bamboo. He further noticed that the chronicle for Wu Wang gives that king five more years after his victory over Shang, whereas according to the *Shiji* and certain other sources he must have died three years

February 8, 1995; Paper presented at the annual meeting of the Association for Asian Studies, Washington, D.C., April 8, 1995

earlier. Doing some simple counting (great discoveries are often “simple,” in retrospect), Shaughnessy found that a 40-space stretch of the Wu Wang text could be cut out, and fitted into the Cheng Wang text, the cut being made in such a way that both text read smoothly; and with this change there would be an entry for every year in the Cheng Wang chronicle up through year 18 and beyond; and further, the Wu Wang text would have Wu dying two years after the victory, as required. The shifted text obviously made no sense in the Wu Wang narrative, but made perfect sense in the Cheng Wang narrative. Shaughnessy concluded that one whole bamboo slip had at some time been moved from the Cheng chronicle to the Wu chronicle, precisely in order to lengthen Wu Wang’s life by three years. Double confirmation was obtained by counting graphs from the beginning of the Cheng chronicle. If one deletes the *ganzhi* identifying the first year of the reign (a Jin editorial addition; the usage is not earlier than Han), and if one supposes a one-space gap after each entry, the text from the beginning of the Cheng chronicle to the proposed transposed slip text is exactly 10×40 spaces, i.e., ten slips.

Shaughnessy had found nearly conclusive proof that, with these changes, the first 18 years of the Cheng Wang text, at least, is word for word the text as it was buried shortly after 299 BC. He published this result in *HJAS* in 1986. One can see at once the burden of proof must not be borne by the skeptics of the *Annals*’ authenticity. Perhaps some parts of the *Bamboo Annals* “modern text” are faked; but the skeptic must now prove it, item by item. The standing presumption—always to be critically tested—should be that the whole of it is genuine. To put the matter more dramatically, what Shaughnessy did was as if he had discovered a new genuine Warring States historical text, that pretends to chronicle about 2000 years of the Chinese past as of 299 BC.

“Genuine” does not mean “true,” of course. Much of the *Annals* is true; much of it is not. But had this discovery been the discovery of a text in a newly excavated tomb, it would have been on the front page of every major newspaper.

Without for a moment questioning the value of Shaughnessy’s achievement, I do doubt one conclusion in his 1986 study. There he addresses the question, when was the slip moved? And he concludes that the Jin Dynasty editors did it, in order to make the text conform to a conception of Wu Wang’s reign that he argues can be traced back no farther than Liu Xin in the middle of the Han era, i.e., that Wu died not two years after his conquest of Shang, but (counting inclusively) either six or seven years after. I am going to have to argue that Shaughnessy “slipped” here, and that the slip was moved before the text was buried in the Warring States era, probably long before it was buried.

But first, why is the matter important? David Pankenier and I myself, following his lead, have done much work using the material in the “modern text” Bamboo Annals as the basis for inferences to the exact dating of the pre-Zhou events, back to the beginning of Shang and even earlier. This work often assumes—for me at least, always assumes—that the dates in the “modern text” are the dates that were stated or implied in the text that was buried in or after 299 BC. Our work is seriously undermined, even at best only accidentally on the mark, if Shaughnessy’s picture is correct, that the Jin Dynasty court editors took extraordinary liberties with what they had found. We would have to assume that dates in the “modern text” may simply have been invented by these editors; and this seems to have been Shaughnessy’s own assumption. I want now to show that we can reject the primary basis for this assumption—that the Jin editors fiddled with the chronology to the extent of moving a slip in order to make the text fit their own beliefs.

I will offer three arguments (there are more).

First argument: The present text dates the Zhou conquest to 1050 or 1051 if the date of the inception of the campaign is used.

Wu’s death is dated to 1045 which by the customary inclusive method of counting would be either six or seven years later. If the slip was moved, in the 4th century BC, in a text having the prestige of being an official royal state chronicle, it is likely that there would be other pre-Han texts saying the same thing. There appears to be one, the *Guanzi* (ca. 300 BC), which says that Wu Wang defeated the Shang king “and then died in seven years.” Shaughnessy is aware of this, but argues that it intends to say that Wu died in the seventh year of his reign, which (Shaughnessy thinks) began five years before the conquest.

I think that the complete [text unclear] will not allow this reading. Duke Huan of Qi expressed a desire to displace the Zhou Dynasty and make himself king. His ministers tactfully induce him to reflect, so as to give up the idea. He muses that the Zhou line had enjoyed a succession of able rulers, in Dan Fu, Wang Ji, Wen Wang, and Wu Wang. Then he continues:

Wu Wang attacked the Yin and conquered them, and died in seven years; whereupon Zhou Gong Dan assisting Cheng Wang governed the world, and only then was able to bring the lands within the four seas to order.

The point obviously was that even with this long sequence of able men, still success took a very long time. The natural way to understand “and died in seven years” is sequentially, seven years later. The “seven years” intended are probably (reading the *Annals*) Wu 11 through Wu 17 (1051–1045) inclusive. My

hypothesis that the slip in the *Annals* had already been moved before the *Guanzi* was written is the best explanation of what we read there.

Second argument: The foregoing argument implies that other Warring States historians were familiar with the *Annals* and made use of it. One should therefore ask, what about the *content* of the transposed slip? It assigns certain events to the reign of Wu Wang, that ought to be in Cheng Wang's reign. Are there any other 4th century BC histories that do the same, with the same events?

There are indeed. Here is what the slip says (I number the items):

- (1) 15th year: The Lord of the Sushen came to court in submission.
- (2) [The king] for the first time visited the sacred mountains of the four borders.
- (3) [The king] made an announcement to the city of Mei.
- (4) Winter: The Nine Cauldrons were moved to Luo.
- (5) 16th year: Ji Zi came to pay respects at court.
- (6) Autumn: The king's armies destroyed Bogu.
- (7) 17th year: (end of slip)

Wang Guowei's research, trying to show all this to be faked proves valuable. He covers items 1, 3, 4, 5 and 6, showing that the "faker" could have used other ancient sources, mostly pre-Han:

- (1) The submission of Sushen and their coming to court with tribute is recounted in the "Lu Yu" section of the *Guoyu*, a text of the 4th century BC:
- (2) Of old, when Wu Wang conquered Shang, he opened the routes to the Nine Yi and to the Eight Man; the Sushen offered tribute of arrows made of wood tipped with stone.
- (3) The announcement to Mei is probably what the "Jiu Gao," the "Announcement about Wine" in the *Shangshu*, is supposed to be. It is grouped in the *Shangshu* with the "Kang Gao" and the "Zi Cai." Its pretended date and speaker have been debated, some taking it to be taking it to be by Zhou Gong in Cheng Wang's name, some taking it to be by Wu Wang, as the "Kang Gao," addressed to "my younger brother," seems to require. None assign it to the post-Zhou Gong part of Cheng Wang's reign, as the slip required before it was moved. Therefore, it must have been moved so early that the pre-transposition text had no (?) impact on "Jiu Gao" criticism (?).
- (4) The cauldrons were supposed to have been cast at the beginning of Xia, taken over by Shang, and now forfeit to Zhou, emblematic of royal sovereignty; See the *Zuozhuan*, 2nd year of Duke Huan: "When Wu Wang defeated the Shang, he moved the Nine Cauldrons to the city of Luo." As Shaughnessy points out, this of course (is) ridiculous as a Wu Wang event, because Luo had not yet been founded. The more reason to marvel at the occurrence

- of this idea is another Warring States text like the *Zuozhuan*, compiled sometime in the 4th century BC.
- (5) The Shang prince Ji Zi is said to have been enfeoffed in Korea. His coming to court, as here in the year before Wu's death—resulting in the conversation that is supposed to have produced the “Hong Fan” chapter of the *Shangshu*—is found in the [unclear text]. In the “Hong Fan” itself the event is dated “13th year” which would be the year before Wu's death if the tradition that he conquered in the 12th year and died in his 14th year were right. It seems to me to that this shows that this tradition continued to exist, perhaps at the Zhou court, after the slip was moved in the Wei text; and the positioning of this event as occurring the year before Wu died was then read over into that tradition.
- (6) The destruction of Bogu; Again look at the *Zuozhuan*, this time Duke Zhao, ninth year. It reads, “When Wu Wang defeated Shang, Bogu and Shang Yan become our eastern territories.”

The best explanation of our being able to find so much of the content of the slip in texts dating from before the tomb was opened—and in at least two 4th century BC texts—would seem to be that the slip was moved sometime in the Warring States era, with copies or abstracts of the resulting Annals circulating widely enough for this series of events to have become known and generally accepted.

Third argument: I have explained how the slip fitted into the Cheng Wang text, originally. It was slip number 11. But how, exactly, did it fit into the Wu Wang text? After the slip [unclear text], ending with the words *shi qi nian* “seventeenth year,” there are exactly 19 graphs (plus one end-of-year space):

ming Wang shi zi Song yu Dong Gong; Dong shi you er yue Wang zhi, nian wu shi si. (space)

The royal heir Prince Song was appointed [successor] in the Eastern Palace in winter in the 12th month. The king died, at age 54.

As an integral slip, therefore, the transposed slip [unclear text] must have been next to last, the last one ending with a half slip blank. More counting suggests that this way of ending the chronicle for a reign was always followed in the original text. But not (?) if the Wu Wang chronicle text is copied from the beginning deleting initial *ganzhi* which must have been added by the Jin editors, the transposed text begins one space more than half-way down the next-to-last slip, and ends one space more than half-way down the last slip. This must have been the way the text read as recovered from the tomb. In other words, sometime before burial (a whole century, I suspect) the slip was moved, and then, later but still in the 4th century BC, the whole Wu Wang text was recopied, to make it

begin at the top of a slip with the words “*shi er nian...*” “12th year...,” as it does now.

This must have been done, furthermore, when the text was still being written on 40-space slips, and I think this has to have been in Warring States. Could the Jin editors have been the ones who did the recopying on the slip, as a sort of antiquarian exercise? But we would expect them to have inserted *ganzhi* to mark the first year and then the slip count would not have worked.

Further, the text as they edited it, and (I think) as they must have received it, took Wu 11 (1051 in the *Bamboo Annals* system), not Wu 12 as first year. We can see this from several details:

- (1) This is what is required in the chronology summary of the Western Zhou era at the end of the last chronicle, for You Wang.
- (2) Wu 11 now reads Di Xin 52; but it was still Wu 11 in the Tang Dynasty because it is quoted that way by the Tang astronomer-calendar [unclear text] Yixing; and it began the chronicle because as he quotes it the text for this year begins with the *ganzhi* for the year *Gengyin*.
- (3) When “*Gengyin*” is crossed out the text for the year 11 is exactly 40 spaces, i.e., it was an integral slip—which it could not have been with “*Gengyin*” in place. (“*Gengyin*” is still in place, in the text for this year though called Di Xin 52—a tell-tale remnant of the time when this is where the Zhou text began.)

The best explanation of this slip analysis is that the text as received in 280 CE began the Zhou narrative with the Conquest, treated as dating from the year the campaign was launched; and that this received text began at the top of the slip. And this means that the transposed slip must have been transposed before the book was buried; and long enough before the official recopies to have been made that caused the transposed text to no longer be an integral slip.

I have given you three different reasons for rejecting Shaughnessy's judgment that the slip he discovered was moved out of its proper place only when the Jin editors went to work. But I came here to praise Shaughnessy, not to bury him. I want to repeat that his discovery of the slip transposition is a great accomplishment. My own little criticism, if it is right, only serves to make that accomplishment even more valuable.

As I close, you will surely have thought of a question that I owe an answer to. Shaughnessy has given a reason why the Jin editors might have wanted to move the slip, thus giving Wu three more years of life. If I am right in thinking that the slip must have been moved six or more centuries earlier, why was it done?

I have discovered a most ingenious answer to this question. But unfortunately this will have to wait for another time.

13 Review of Sun, Xiaochun, and Jacob Kistemaker, *The Chinese Sky during the Han: Constellating Stars and Society*

Leiden: K. Brill, 1997. Xxiii. 241 pp. (including “Index of Star Names” and “General Index”), and six star maps

The object of the authors is to identify and locate all stars and constellations recorded by astrologers and astronomers in the Han era (206 BC–220 CE); and then to examine the way in which the sky as conceived by people of Han mirrored Han institutions, political, social and economic. A reviewer must point out at once what the authors detail on p. 95: Chinese constellations were not like Greek ones, which tended to be fanciful representations of mythical beings and animals. And there were many more of them: the Han sky held 283 (better denoted asterisms: many were single stars), whereas Ptolemy’s held only 48. The Chinese sky had its mythic beings (“the weaving Girl,” Zhi Nu), composite beasts (“the Green Dragon,” Cang Long) and abstract gods (“the Great one,” Tai Yi); but more noticeably, courts and palaces, imperial bureaucracies, markets and shops, wells and rivers. Probably the earliest surviving astrological catalog of the sky is a chapter in the *Shiji* (“Historical Record”) by the father and son Sima Tan and Sima Qian, of about 100 BC, both of them official astrologers. It is titled “Celestial Officials” (“Tian Guan”); more than anything else, that is what the stars were. Often an asterism was only one or two stars, in no sense picturing what it was called.

This aspect of the book (its 5th and 6th chapters) will make it of interest to intellectual historians who may not also be historians of science. Those who are will look closely at what comes first, and keeps coming into play. How did the authors do it, and did they get it right?

Their task wasn’t easy. There are no star maps or treatises containing precise data surviving from the Han. An astronomer named Chen Zhuo, of the “Three Kingdoms” period (220–280), wrote a book putting together the main traditions of astronomical lore from the Han, but this too is lost. The earliest copious supply of information is a long Tang period book on astrology written in the early 8th century, the *Kaiyuan zhan jing* (Treatise on prognostication of the

Reviewed by David S. Nivison, 1997

Kaiyuan (713–741) era), by Qutan Xida (the authors do not note that this name is Indian, a free transcription of “Gautama Siddhartha,” an Indian astronomer in the Chinese court (Needham pp. 203–203); nor are we told its length, actually 10 volumes, as reprinted by Taipei Commercial Press in 1973 from *Siku quanshu zhenben*). The book came to light in the late 1550’s but has been exploited only recently. It quotes liberally from “Shi shi,” or from a *Shi shi xing jing* (Star classic of the Shi [Shen] school); and these quotations are commonly referred to as if they were that book. Star maps too begin to appear in the Tang, and much more careful ones were done in the Song period and later. The authors have had to use all this material, described in much (if not full) detail, plagued always by a dilemma: the later the work, the more scientifically accurate; but also, the more contaminated by an overlay of knowledge that cannot be predicated of the Han.

Compounding the problem was the practice of ascribing from time to time to a named traditional school’s work information that actually was recently obtained. There were three such traditions, the school of Shi Shen, a Wei state astronomer probably of 4th century BC; of Gan De, of the Qi state, somewhat later in the “Warring States” (pre-empire) era; and of Wuxian, an astronomer said to have lived in the Shang Dynasty (16th–11th centuries BC). Actually none of this material, extracted from the *Kaiyuan zhan jing*, is pre-Han, the last probably partly post-Han. One analyzes the data, given in the form of longitudinal locations of stars with reference to known boundaries of zodiac spaces, and distances in Chinese degrees from the sidereal pole, to determine the date of the observations on which it was made. Much work of this kind had been done, leading to the conclusion that there had been two periods of observation, one pre-Han and another late Han. The authors (p. 44) apply a hypothesis of the Japanese historian of astronomy Maeyama in 1997, that the data are systematically skewed by a slight misalignment of the observing instrument, and obtain a deduced observation date for the relatively detailed “Shi school” material of about 70 BC. Since this coincides with results obtained independently by two others in 1937 (p. 65) and is consistent with other elements in the authors’ historical reconstruction, I believe they are correct, although I am not able to repeat their mathematical analysis.

By “philological” methods the authors deduce the relative order of the three schools: references within one to the others show that the Shi school came first, the Gan school slightly later, and the Wuxian school last. One school builds on the earlier one(s), filling in gaps in the sky. References to the Gan school’s work by a writer named Xi Meng show that it must antedate him; the authors then discover an obscure text in the early 6th century literary anthology *Wen Xuan* that puts Xi Meng at a meeting dated 74 CE. The Wuxian matter, they suspect,

may actually be the work of Chen Zhuo in the 3rd century. This part of the book seems to me to be quite competent detective work. Results are presented in six fold-out pages of reconstructed star maps, in which stars and asterisms are identified and located fairly exactly, on a sky-field computer-generated to allow for precessional shift since the Han.

The book has its defects. The “General Index” too often refers one to material one cannot find. Worse, there is no way to locate discussion in the “General Index” at all, and the “Index of Stars Names” refers you only to one of three appendices (for the three schools); and the entry in that appendix is the end of the road: there are no references back to discussion and analysis in the main text of the book. Alternative names of asterisms (e.g., Tian Si, “Sky quadriga,” for Fang) are not found anywhere; names of compounded asterisms are sometimes in the “General Index” but not in the “Index of Star Names.” It is implied erroneously (p.113) that systems of lunar lodges in other cultures always numbered 28. Actually, while Arab, Tibetan and some Indian systems had 28, Iranian, Tamil, Burmese and Khmer systems had 27 (see Stewart 1974); the sidereal month on which all are based is about 27 1/3 days. Transcriptions of Chinese names and terms have not been entirely cleansed of dialect features: *cheng*, p. 144, diagram, should be *chen*; “Xianyu Wanren,” p.58 and elsewhere, should be “Xianyu Wangren”; *zhen fei*, p. 97, should be *zheng fei*. At p. 112, note 19, “He Chentian” should be “He Chengtian.” And “see the *Song shu*” is an absurd reference; my own copy of the *Song shu* is 1193 pages, each page reproducing photographically four Chinese pages. (Other references are similarly inadequate.) On pp. 126–7, the term “Neo-Confucian” is misapplied to all Confucian thought after Dong Zhongshu in Western Han; it conventionally refers to Confucian philosophy of the Song (960–1280) and later. I am grateful for the long bibliographies; but the bibliography of Chinese sources gives only title, with date and author if known; no publication information (which might not matter), nor indication of size (shockingly, Needham 1959 does the same).

There is a major muddle, it seems to me, at pp. 110–111, where the authors try to introduce a distinction between a “lunar calendar” and a “solar calendar.” What they actually describe are two equivalent ways of mapping the zodiac band of stars: (1) by noticing the successive sidereal environments of full moons; and (2) by noting dawn and dusk culminations, and reasoning that the sun must be between them. Probably they are right that the former method is earlier; but they fail to show me that one method or the other is in some way more appropriate to certain asterisms, or that asterisms conceived earlier or later can be paired with the lunar or solar method respectively. Their initial example belies this: they try to sort out lunar lodge asterisms between lunar and

solar so-called “calendars”; yet they stress throughout that the lunar lodge asterisms are all relatively early.

I would argue (and have: Nivison 1989; the matter is controversial) that the concept of a lunar “lodge” (*xiu*) may not at first have involved identifying asterisms at all. One could simply note that the moon, full or not, was near a prominent feature, e.g., Antares; and then just count days until the moon got back there, getting 28; then, if one has noticed that the sun seems to move through the same band of stars during a round of seasons, in about 365 days, one divides 365 by 28. So a day’s “lodging” for the moon must be about a 13-day journey for the sun. The “lodges” would be approximately equal, and would only gradually get distended or shrunk by trying to match them with named constellations, or by other causes. At first, one could think of a lodge as a moon-day, and even use the 28 names as a 28-day cycle. This is exactly what the Chinese did, as a *rishu*, “book of days” used in picking lucky days, found in a Qin tomb, shows. (Jao Tsung-I and Zeng Xiantong 1982, Plates 36 and 37 at bottom; the scheme assumes a solar calendar of 30-day and 31-day months.)

But such a 28-day cycle is not what is normally meant by a “lunar calendar” either. A lunar calendar is a calendar that measures time by synodic lunar months, leading at once to the problem that there are more than 12 but fewer than 13 in a year. This is solved by dividing the year into 12 parts or “months” (what the *yue ling*, “monthly commands” in the *Li ji* and *Lü shi Chunqiu* are really talking about), each defined by where the sun is in the sequence of *xiu*. This is the true “solar calendar”; and by dividing each part in two, one gets the scheme of 24 *qi jie* (hardly discussed by the authors), which can be (and was) used as a tracking scheme for the synodic months in order to decide when to insert an extra one in the lunar calendar.

A thicket of muddles appears on p. 99, where the authors are trying to explain the three-star *sheti* 攝提 asterisms flanking the star Arcturus, “Da Jiao.” (1) The star is put in the longitude of the lunar lodge Kang in the maps at the end of the book (between 12 and 13 hours). But the “Tian Guan” (Chavannes 3, p. 345) locates Da Jiao, as it would be if the map were ecliptic-based rather than (as the authors have it) equator-based. This needs more study. (2) On p. 99, they mis-translate “*she ti ge*,” 攝提格 taking it as another name for *sheti* asterisms, and rendering it “the starting point of *sheti*”; this I fail to understand at all. The text (Chavannes, *ibid*) says that the handle of the Big Dipper points directly at the *sheti*, and so they (with the Handle) serve to fix the seasons and solar weather periods (*jie*); therefore they are (together) called “the *sheti* frames (*ge*),” i.e., the asterisms that clarify exactly what the Handle should be conceived as pointing to (the *sheti*, Appendix I, p. 147, are the “assistants” of Da Jiao, the Celestial

King). (3) in the note (4) to this they say (i) that *sheti* is a “synonym of Jupiter”; it is not, in my stable of dictionaries. (ii) That *she ti ge* is another name for *yin* in the “12 branch” division of the year (illustrated p. 144); but no: Sima Qian says that in the first year of a 12-year cycle (of Jupiter circling the zodiac), called “the year *she ti ge*,” the theoretical backward-moving correlate of Jupiter (the “shadow of the year [Star]”) is “in *yin*.” (Chavannes 3.357; this odd concept comes from extending the “12 branches,” which was a month count, to make it serve as a year count as well.) (iii) That “the sector *yin* included Da Jiao”; false: it was not the 3rd branch *yin* but the 5th, *chen*, that corresponded to the *xiu* Jiao and Kang, which included Da Jiao. And (iv), that this was why the *xiu* Jiao was the first of the 28 *xiu*; false: the reason must be something else; probably that when the full moon is in the Jiao-Kang sector, the sun must be on the opposite side of the zodiac in the sector that it must be in during the pre-spring-equinox month, the first month of the year in the Xia calendar. (It is not true that this insight required of the Chinese an understanding of the celestial mechanics of eclipses, as the authors appear to think (p. 135); one needs only to notice that it is when the moon is full that it rises as the sun is setting.)

When one turns to p. 144–145, where the authors describe the reverse ordering of the Gan school constellations following the 12-branch system, one realizes that they don’t see what is going on. That system matches the “branches”, *zi*, *chou*, *yin*..., to groups of two or three *xiu* in the reverse order of the sun’s movement through the zodiac. The system was, they think, used mainly for astrological purposes and was invented late, “probably during the Han.” But, granted that astrologers get their messy fingerprints on everything, the system had a practical purpose: the lunar zodiac was conceived as fixed in alignment with the cardinal directions, as one would observe (or infer) it to be laid out on the horizon, when facing south at midnight at winter solstice, according to the situation in high antiquity when the sun would be in the lodge Xu at this time. Keeping that picture in one’s head, one then observes (or infers) the angle of the Handle of the Big Dipper at a fixed time of day, ideal “dusk”, 6:00 PM. The sun moves counter-clockwise around the zodiac in the course of the year. But this is equivalent to saying that if one imagines the sun’s position fixed when observed (or inferred) at ideal dusk, then the zodiac, and the whole field of “fixed” stars including the Dipper and the “*sheti*,” appear to be moving clockwise in the course of the year, i.e., in the “reverse order” of the “branches.”

Now suppose it is the first month of spring in the “Xia” (traditional) calendar, which takes this “first month” to be the lunar month before the month containing the spring equinox. One wants to have named the “branch” spaces so that the Dipper Handle hen points at the “*yin*” space, because the first month of

spring in this calendar is called the *yin* (03) month (the solstice month being the *zi* (01) month). The sun, due west, is at the beginning of Wei-Mao-Bi on the diagram. But the Dipper Handle, conceived as in *sheti* alignment, i.e., pointing at the area “framed” by the two *sheti*, is actually always “pointing” at Jiao-Kang in the actual zodiac, corresponding to *chen* (05), not *yin* (03), though one is observing it to be pointing northeast, at the (fictitious) *yin* branch. To make the diagram true to the Handle’s actual “pointing” (thus confirming where the sun really is), mentally rotate the outer band (see p.144) counter-clockwise two spaces. This will cause the sun to be located at the beginning of the Shi-Bi space, which was the pre-spring-equinox space in the Han (the zodiac space of the *yin* month at that time). Thus the Handle’s pointing at the *yin* branch does tell us that the sun in the zodiac is at the beginning of the spring month space.

But the Handle doesn’t really point at Jiao-Kang. It would to most of us seem always naturally to be pointing at Scorpio, i.e., at Di-Fang-Xin corresponding to (artificial “branch”) *mao*. This works, for about 2000 years before the Han: if the Handle points at (branch) *yin*, to get it correctly aligned rotate the outer band one space counter-clockwise. The sun then will be at the beginning of the Kui-lou space, which for that time was the true *yin*-month space, for at that time the equinox was in Wei-Mao-Bi, and the winter solstice was in Nu-Xu-Wei.

This shows that the “*sheti* frame” alignment was an adaptation of an original system, accommodation that system to precessional shift (only the Chinese didn’t conceive it that way; it would be just a “reform”). The change meant that instead of “reading” the Handle “flat” across the top of the Dipper, one was to extend the curve of the Handle down to Da Jiao, Arcturus, in one’s mind’s eye. The meaning of the word “*sheti*” has nothing to do with this (they are just Da Jiao’s “assistants”); but their selection is convenient, perhaps, for the distance they mark out is about one hour-segment, i.e., one *qi jie*.

(Now try imagining the Handle pointing at the *mao* branch segment, clockwise next after *yin*. One finds that in this case the sun must be one zodiac 12th farther on in its true counter-clockwise direction of movement. The Handle is thus functioning like the dial of an annual clock. Is this why “clockwise” is clockwise? Was the ancient Chinese concept, or some form of it, a universal one?)

Obviously this book has captured my interest, and I am pleased to have it. But I have learned that I must use it with caution, and that it will demand patience.

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14 Zhang Peiyu on the *Dayuan Li yi* and the “Jinben” *Zhushu jinian*

The so-called “Modern Text” or “Jinben” *Zhushu jinian* (*Bamboo Annals*) has been a focus of controversy for more than two centuries. A majority of scholars argue that it is a fabrication, or at best a clumsy reconstitution, done later than Tang. But some still maintain that it is authentic, either extracted from a longer (now lost) original text (with some modification), or a copy of the original done in the Jin Dynasty, before court scholars had finished restoring the text recovered from the tomb or repository in Ji Xian (also with some modification). An insufficiently noticed difficulty with the fabrication thesis is the fact that the “Jinben” has every reign precisely dated, back to Yao (who is given the date 2145 BC); and it seems implausible that these dates would all have been simply unvented, even though it is possible (I think) to show that most of them, prior to 841 BC, are incorrect.

I want to examine here an article by Zhang Peiyu 張培瑜 (*Lishi yanjiu* 歷史研究 1999, no. 3, pp. 87–94), “*Dayuan Li yi* yu Jinben *Zhushu jinian*” 大衍曆議與今本竹書紀年, that begins to address this difficulty. Zhang argues that the “Jinben” author (faker, he thinks) was copying date information from the *Dayuan Li yi* in the *Xin Tang Shu*, and filling it out with invented detail. The argument is that in the “Jinben” the *ganzhi* 干支 names of years (*sui* 歲 names), expressed or implied, are for Xia, Shang and Western Zhou the same as in the *Dayuan Li yi*, though absolute dates and lengths of the dynasties differ.

He notes correctly that we must carefully decide whether (1) the “Jinben” copied the *Li yi*; or (2) the *Li yi* copied the “Jinben”; or (3) they had a common source. I would add to this that, it seems to me, the *best explanation* wins the argument. (1) Are there features of the “Jinben” that cannot be better explained than by supposing that its author(s) used the *Li yi*? Or (2) are there features of the *Li yi* that cannot be better explained than by supposing that its author (Zhang Sui 張遂, i.e., the Tang monk Yixing 一行) used the “Jinben”? Or (3) are there features of both that might be cited in this argument that actually are best explained by supposing that both of them were directly or indirectly following a common source? (And, I suppose, there is a fourth possibility, namely, that the matter is undecidable.)

Stanford University, 3 June 2000

I will examine (1) the *sui*-name problem; (2) the adding or omitting of 60-year cycles; (3) the case of Western Zhou; (4) the Zhong Kang eclipse; (5) evidence that Yixing used the “Jinben”; (6) evidence that even Yu Guang 虞廣 (Liang Dynasty) may have used the “Jinben” chronology; and (7) other problems, notably the supposed evidence that the original *Zhushu jinian* dated the Zhou conquest to 1027.

1 *Sui* Names

On pp. 88–89, Zhang gives 11 examples where the *ganzhi* year name, expressed or implied, in the “Jinben” and in the *Li yi* are the same. But in almost all cases the *ganzhi* is expressed in the *Li yi* and only mathematically implied in the “Jinben.” The 11 examples become 14 when instances within an example are counted. In 8 cases, the *ganzhi* is expressed in the *Li yi* but only implied in the “Jinben.” In two cases it is stated in both, and in both instances the year is (or was) taken as the first year of a reign. In three cases it is stated in neither. In only one case is it stated in the “Jinben” and only implied in the *Li yi*; and in that case the present “Jinben” takes the year to be a first year (of Wu Wang of Zhou) but the *Li yi* did not.

The point of paying attention to these distinctions is this: The “Jinben” for the most part uses *sui* names only for first years of reign. (One seeming exception that is discussed, “*gengyin*” for Wu 11, may be a residue of an early version that took this year as the beginning of Zhou. Another exception is the brief summary for Western Zhou at the end of the chronicle for You Wang; here I argue that the phrases containing *sui* names have been added, probably after the work of the restorers; if they are deleted, the remaining text loses nothing in meaning, and contains just 40 characters, making up one slip. A third exception is the account of the interregnum of 40 years between Xiang and Shao Kang of Xia. I would argue that this text is an interpolation made early, around 400 BC, and that the *sui* names are the guesses of someone working over the text later than the Jin court restorers.) In every case in Zhang’s lists—the majority of all examples, moreover—where there is a difference between the “Jinben” and the *Li yi*, setting aside the few first-year cases, it is the *Li yi* that uses the *sui* name. But the use of *sui* names for years is a practice that gradually gained ground over many centuries. So close examination of Zhang’s examples ought to suggest that it is the *Li yi* which is later.

A related point: Zhang simply assumes that if an instance is found in a quote, it can be called “Guben,” “old text,” i.e., a fragment of the original. Only two uses of *sui* names are found in such quotes: (a) the use of “*gengyin*” after Wu “11th year” (quoted by Yixing); and (b) the use of “*bingzi*” after Yao “1st year”

(used in the *Sui shu*). Since both of these are first year dates (the former counting as the beginning of Zhou), and both *sui* names appear in the “Jinben,” for Zhang to call them “Guben” is to beg the question. I.e., why shouldn’t I say that the appearance of the use of a *sui* name after a specified first year is found in a *Jinian* quote in the *Sui shu*, so here we have an example of the “Jinben” being quoted in an early Tang history? To say only this would be just as circular. But together with other indications that the “Jinben” is older than the *Li yi*, it is a point that has weight. Relevant to this: Not only are these the only two cases of the use of a *sui* name in so-called “Guben” material; also, it is evident from other such material that as a regular practice the completely restored text (as I would describe the lost “Guben”) does *not* use *sui* names for first years. This supports my judgment that the “*bingzi*” quotation is from a text we would now call “Jinben.” My recently published position is that the “Jinben” derives from a premature copy made while the work of restoring in the Jin court was still going on, and that at that stage *sui* names, and the Zhou calendar through the Jin and Wei chronicles, were used as research tools to assist the restorers in straightening out the discovered text, and were removed when this work was finished.

2 Adding or Deleting 60-year Cycles

Zhang seems to making the points that he thinks are most powerful on p. 89 (from line 13) to p. 90 (line 22). On p. 89, Zhang argues that although there is only a scattering of examples of corresponding *sui* names (for only nine out of 50-odd rulers), they are spread over the whole of the Three Dynasties, so probably the two texts are using the same system. The absolute dates are not the same, but this is because when you have only a few instances and are using them to try to restore a complete chronology, it is possible to omit here and there a whole cycle of 60 years. Since the *Li yi* is the text with only a few kings mentioned, it is presumably the “Jinben” that has done this, thus making the range of time over the Three Dynasties shorter. The *Li yi*’s totals for Xia and Shang are taken from the Han calendar-astronomer Liu Xin 劉歆, and are longer than the totals in the “Jinben.” So the *Li yi* can’t be derivative from the “Jinben.” Therefore the “Jinben” must be derived from the *Li yi*. Or so Zhang thinks.

The argument doesn’t work. It is possible for the “Jinben” to have left out or more cycles from the *Li yi* so as to get the time spans in the “Jinben” only if it is also possible for the reverse to have happened, i.e., for Yixing, or Liu Xin, to have started with the “Jinben,” or something having the same chronology, and inserted one or more cycles. Let’s look just at what Zhang says about Shang. True, the *Li yi*’s 628-year length for Shang is taken from the Han astronomer Liu Xin (who actually makes it 629 years). The “Jinben” says the length of Shang is

496 years. But the “Jinben” is explicit about the 496 years: it begins with 1558, and must end with 1063, even though the statement of the total (in the present text) is not entered at that point in the text. The end-of-Zhou summary explicitly starts the Zhou year count with 1062, the year of Wen Wang’s death and of Wu Wang’s becoming ruler. To get to the “Jinben”’s actual first year of Zhou power one has to add another 12 years, which would bring the count to 1051, the “first attack” year corresponding to what Yixing calls the year of the victory, the year before what he calls the “*ge-ming*” 革命 (change of Mandate) year). But $496 + 12 = 508$, which is just two cycles less than 628 ($628 - 508 = 60 + 60$).

The task now is to try to construct possible explanations and compare the results. Suppose someone is reading Yixing’s work and trying to make up something that turns out to be the “Jinben.” He leaves out two cycles, and gets as total for Shang $496 + 12$ instead of 628. Why? In part, because the *Shiji jijie* 史記集解 quotes the supposedly authentic *Zhushu Jinian* as saying that Shang lasted 496 years. But why the extra 12? Was this derived from the simple operation of subtraction, $628 - 496 = 132$, $- (2 \times 60) = 12$? This would show that the “Jinben” must be the derivative text.

The matter is not so simple. First, Liu Xin (*Han shu* 21 B, p. 50a–b) gives the length of Shang, from the defeat of Jie 桀 (Di Gui 帝癸) to the defeat of Zhou Xin 紂辛 (Di Xin 帝辛) as 629 years, not 628; but the *Li yi* cuts out Liu’s extra year, so that it has just $60 + 60$, added to the “Jinben”’s $496 + 12$. This suggests that it is the *Li yi* that is adjusting numbers to make them conform to an already existing “Jinben.” The suggestion is strengthened by the fact that the “Jinben” does not count the length of Shang from the defeat of Jie, as does Liu, but from the year after. If Yixing noticed this, he might say that if Liu had done the same his figure would be 628.

Is the *Jijie* the earliest source for “496” (disallowing the “Jinben”)? No: the number is found in Western Han, both in the *Yin Li* 殷曆 and in the *Yi wei ji lan tu* 易緯稽覽圖, and both must have been available to Liu (see Chen Mengjia 陳夢家, *Yin xu buci zongshu* 殷墟卜辭綜述, p. 211). Further, the idea that the defeat of Di Xin occurred in a 12th year—of Wu Wang (*Lü shi chunqiu*)—or 12 years after Wen Wang received the Mandate—apparently in a source used and misunderstood by Sima Qian—was also available to Liu, only he thought it was 13 years. This explains why Liu counted from the defeat of Jie, getting 629 years: $629 - (496 + 13) = (2 \times 60)$. If so, it must have been Liu who added the two cycles to the length of Shang. (The use of *gan zhi* as *sui* names began a century before Liu, and the mathematically identical *shetige* 攝提格 system was in use well before that. So counting years in 60’s was well established by Liu’s time.)

So it seems that neither the *Li yi* nor the “Jinben” is copying the other, and that both have (ultimately) a common source. Still, it is possible that the creator of a derivative “Jinben” could be simply trying to undo Liu’s work preserved in the *Li yi*.

It seems equally possible that Yixing had before him both Liu’s work, interpreting it as 628 years, and the “Jinben,” reading 496 + 12 years, and saw that since the difference is just two cycles, he could use the *sui*-name system (*ganzhi* for years) in the “Jinben,” and did so, because the “Jinben” came to him as a Warring States chronicle and he assumed that those *sui* names had their basis in fact (he would know that they must have been added later), even though he continued to think that Liu was approximately right about the absolute dates. So we must look farther. The first place to look is Western Zhou.

3 Western Zhou

Yixing is trying to defend Liu Xin’s position as far as possible, but he knows that he must make some corrections, because he knows how to determine correctly the *ganzhi* for first days of months in past years, and he knows that Liu did not. He accepts Liu’s definitions of lunar phase terms (which are wrong), so he accepts Liu’s judgment that the month of the Zhou conquest began with day *gengshen* (57), since the “Wu Cheng” 武成 says that the victory on *jiazi* (01) was five days inclusive after *jisipo* 既死霸, and Liu defines *sipo* as *shuo* (first of the month). But Yixing knows that this is untrue of 1122 BC, Liu’s conquest date. The solstice month of 1111 begins with *gengshen*, and Yixing chooses that year, apparently counting this as the second month, as is required (the preceding year had 13 month). His dates, compared with “Jinben” dates:

	Yixing	“Jinben”	difference
“First attack”		1051	
Victory	1111	1050	
Ge ming	1110		
Wu dies	1105	1045	60
Regency 1	1104	1104	60
Regency 7	1098	1038	60
Cheng, 1 of 30	1097	1037	60
Cheng dies	1068	1008	60
Kang 1	1067	1007	60

From the death of Wu on the difference is always one cycle. The identification of the “*shi fa*” 始伐 year as the victory year is Yixing’s move to reconcile his count with Liu Xin, who had Wu dying in the 7th year counting from the conquest, whereas the “Jinben” has him dying in the 6th year. The simplest explanation of this is that Yixing is adjusting his construction on the basis of the “Jinben.”

But there is another difference between the *Li yi* and the “Jinben,” as Zhang notices: Yixing’s last year of the Regency 1098 fits the month and day dates in the *Shang shu* chapter “Shao gao” 召誥, but the “Jinben” date 1038 does not. (The “Shao gao” day dates are based on the lunar terms *fei* 朏 (new moon day) and *jiwang* 既望 (full moon day), neither being problematic.) 1036 could be made to fit. (Five years later is the correct date, I think, fitting better.) Yixing must have known that *ganzhi* for days of month tend closely to repeat every 31 years. Knowing that 1036 would work, he must have counted back 2×31 from 1036, and saw that this got him back one cycle from 1038, preserving *ganzhi* for years, therefore making his “first attack” year *gengyin*, as his quote from the *Jinian*—which could be the “Jinben”—requires.

To assume the converse, that a person constructing the “Jinben” moved Yixing’s dates down one cycle, requires saying that this person didn’t know enough elementary calendar science to get the days right. Zhang says this. But which is more likely: that a person in post-Tang Confucian China, inventing a construction, would think he could get his work accepted even if he failed to attend to day dates in the *Shang shu*, the second ranking “Classic”? Or that a 4th century BC Warring States alteration of the chronology would ignore this problem simply because the persons doing the altering were not looking constantly at the *Shang shu* (which perhaps did not yet exist as a book, though many of its chapters did exist) and didn’t care?

Yixing had an obvious reason to move the dates back a cycle. A supposed forger would have had no reason to move them down one cycle, unless to restore a $496 + 12$ chronology consistent with the *Shiji*’s chronology of Lu. But if he were this careful, why wouldn’t he have been more careful? The “Jinben” Lu chronology is not exactly consistent with the *Shiji*, though both are far closer to fact than Liu Xin’s chronology of Lu. (The differences between the *Shiji* and the “Jinben” are explainable, and the explanation implies that the “Jinben” chronology is earlier: see Nivison, 1999, Appendix 1.) There is the final thing to consider: We now know that Wen Wang died in 1050. (The conjunction of 1059 in Wen’s 41st year, and the lunar eclipse of 1065 in his 35th year, put that beyond reasonable doubt.) So the “Jinben” dates are almost right; but Yixing’s, and Liu Xin’s, are way off.

4 The Zhong Kang Eclipse

The next matter to examine is the problem of the solar eclipse in the fifth year of Zhong Kang of Xia. Kevin Pang and I argued that this eclipse actually occurred on 16 October 1876 BC, and that the “Jinben” record seems authentic, because if one uses D. Pankenier’s dating of the conjunction of 1953 to the 14th year of Shun in the *Jinian* (*Early China* 9–10), counting forward using *Jinian* (“Jinben”) reign lengths and positing 2-year breaks between reigns for mourning, then the date one gets for the eclipse is correct; and it is just not reasonable to suppose that this is a mere coincidence. (Zhang, in his critique in *Early China* 15 (1990), either did not understand this argument or does not recognize such a probabilistic inference as an argument.) At the bottom of p. 89, Zhang says this (simply repeating his criticism of 1990):

As for the Zhong Kang 仲康 chronicle and the solar eclipse, in the Tang monograph (sc. *Xin Tang shu* “Li zhi” 曆志) [Yixing] clearly writes that this is obtained by using the method of the *Da Yan Li* on the basis of the “Yin Zheng” 胤征 chapter of the *Shang shu*. Further he says that the “Yin Zheng” in speaking of Zhong Kang “when he became sovereign of the world” is referring to his reviving the institutions of the Great Yu, and is not referring to the year of the eclipse. [He says,] “In his 5th year Xi 羲 and He 和 failed in their functions and so the king ordered a punitive expedition; Yu Guang 虞廣 thinks this was Zhong Kang’s 1st year, but he is wrong.” One can see that his words “In the 5th year of Zhong Kang, year *guisi*, 9th month, *shuo*-day *gengxu*, there was an eclipse of the sun in the 2nd degree of Fang 房” say what Yixing deduced, and do not come from some authentic bamboo text.

It is indeed obvious that the quoted words (from *Tang shu* 27 A p. 16b.) give us what Yixing deduced using the *Da Yan Li*, but it is not necessary to conclude that the words—except for “*er du*” 二度, “2nd degree of”—do not *also* come from earlier texts (not just the “Jinben,” which does not mention Fang). Throughout, Yixing is concerned with how his calendar agrees with received text. Furthermore, where did Yixing get “5th year”? That the year he derives can be called “5th year” is a historical datum, not a scientific deduction, and must have come from a historical text. Why not the “Jinben”? The *Li yi* context shows that the whole point of this statement of what the *Da Yan Li* implies is to show that among conflicting interpretations “Fang” names the lunar mansion and does not mean “proper location” (or something of the sort), and (a related point, as I will show) that among conflicting claims “5th year” is right and not “1st year.” The only point Zhang can make is that if Yixing were here quoting the *Jinian*, he should have said so. But why? I would have done so, but I am primarily interested in the “Jinben” *Jinian*, and he wasn’t. He accepts the (spurious) “Yin Zheng” of the *Shang shu* as authentic, and it is the *Shang shu* that is the prestige text that he

wants to find backing him up. Earlier (p. 16a) he quotes the “Yin Zheng” text verbatim: “In the last month of autumn, on the 1st of the month, the *chen* 辰 were unsettled in Fang.” This confirms his interpretation of “Fang.” But it does not account for “5th year.”

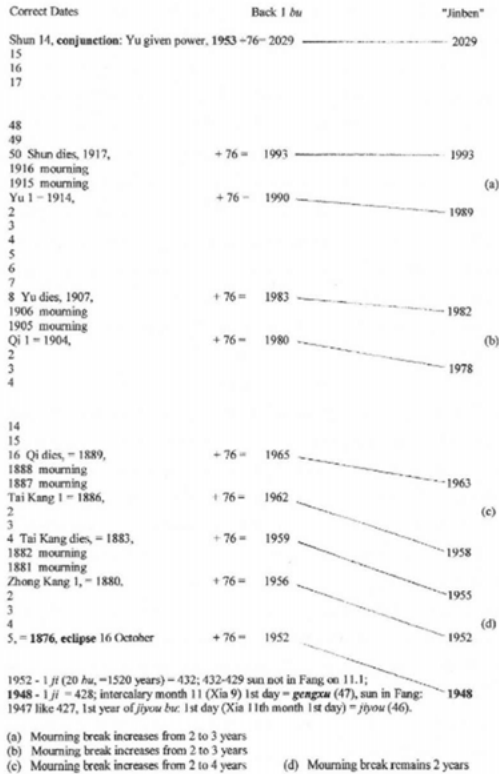
Of course Zhang’s account seems possible: Yixing could have had other sources unknown to us; and the *Da Yan Li* gave Yixing a date for the Zhong Kang eclipse that agrees with the “Jinben” *Jinian* as to implied *sui*, 9th month, and *gengyin* day. But this does not prove that the “Jinben” text can only have come from Yixing’s words now in the *Tang shu* if there is another account that is also possible. Zhang needs to demonstrate that his account is the best explanation.

The Nivison-Pang article in *Early China* 12 has another account; Zhang Peiyu was invited to criticize it in the same issue, but I am afraid we did not present it clearly enough, so let me try again: The actual eclipse was in the 1st of the (Xia) 9th month of the 5th year of Zhong Kang, which actually was 16 October 1876 BC. An editing of the chronology perhaps ca. 400 BC aimed at making Yao 1 be a numerologically significant date (2145, back 119 years—see Nivison 1999, Appendix 3—from 2026), which meant that all of these early dates had to be shifted back. The transfer of power from Shun to Yu in Shun 14 (actually, but not in the *Jinian* text, marked by the conjunction of 1953 BC) was shifted back one *bu* 部 (76 years) to 2029. But doing the same to the date of the eclipse would yield 1952 BC. The eclipse had to be in Fang (though this is not stated in the “Jinben” text); and that meant that one *ji* 紀 (1520 years, i.e., 20 *bu*) later the sun should be in Fang on the 1st of the (Xia) 9th month. The test year was therefore 432 BC (1952–1520), but in that year the sun was not in Fang on the required day, nor was it in 431, 430 or 429; but it was, in 428 (1948–1520); Fang in the late 5th century BC was about 204 to 211:

432	(Xia) 9 th month 1 st day = 12 Oct = <i>guiyou</i> (10)	sun at 193.6
431	~ 1 Oct = <i>dingmao</i> (04)	182.9
430	~ 20 Oct = <i>xinmao</i> (28)	201.8
429	~ 9 Oct = <i>bingxu</i> (23)	190.8
428	~ 28 Oct = <i>gengxu</i> (47)	209.6

Therefore 1948 was chosen. This required filling in four more years between 2029 and 1948; this was done by lengthening mourning intervals beyond two years, for a total increment of exactly four years. The gaps between reigns in the “Jinben” chronicle for Xia, we argued, were for completion of mourning, and so

ought to be exactly two years in all cases. Assuming this, and using the conjunction of 1953 as marking the first year of Xia instead of the “Jinben” date 2029, but also using the “Jinben” reign lengths, one deduces the exact date of the actual eclipse. Table 1 illustrates the explanation that I am giving for the way the “Jinben” now reads. The first of the (Xia) 9th month of 1948 was actually not a *gengxu* day; but it was, in the Yin Li calendar system being used, because 427 BC was the first year of *jiyou bu*, i.e., the 76-year period beginning with a first = solstice month (11th Xia month) that began with the solstice day, that day being *jiyou* (46) day. So 30 + 29 days earlier, which must be the 1st of the Xia 9th month of 428 (actually the intercalary Xia 9th month), must be a *gengxu* (47) day, and so also on *ji* earlier, in 1948. (Relevant here is my “note on eclipse” at the end of this article; the eclipse was not dramatic, and was north of the Xia capital, but that does not matter.)



Tab. 1: Why the “Jinben” Dates the Zhong Kang Eclipse to 1948, Day *gengxu*

The explanation may not be simple, but it is precise, in spite of its intricacy, and that is its virtue. For it would be amazing if such a precise explanation, which is just what one would expect in a construction in Warring States times, should turn out to be possible just by accident. Therefore it is almost certainly right, and therefore almost certainly better than any other explanation. This is the argument that Zhang Peiyu must defeat, if he can.

In *Early China* 15 he has made an attempt, not to engage this account directly, but to establish a claim that would make my account impossible. The claim is approximately this: The “Yin Zheng” puts the eclipse “in Fang.” In doing so, it is copying the *Zuo zhuan* for Zhao 17, 6th month, *shuo*-day. The *Chunqiu* at this date records a solar eclipse. The *Zuo* narrates an argument among officials and ritualists in the Lu court about what ritual steps to take. The chief astrologer (*taishi*) says, “in this month the sun has passed the [spring] equinox but has not yet reached the [summer] solstice” 在此月也日過分而未至。When an eclipse occurs (sc. At such a time), such and such measures must be taken. Thus the Books of Xia say, the *chen* (= sun and moon) were unsettled *yu fang* 于房, i.e., in Fang, or in their places, and such and such things were done. “It is applicable to the *shuo* of this month” 此月朔之謂也。Zhang argues (like many scholars before him) that since the *Zuo zhuan* text is about an eclipse on the first day of summer, and since the sun cannot be in Fang until autumn, the meaning of “*yu fang*” in the quoted “Books of Xia” text cannot be “in Fang,” but must be “in their places,” or something of the sort.

The conclusion does not follow. The point of quoting the (lost) Books of Xia might well be that the quoted text concerns an eclipse like the present one in that at that time, since the sun had to be in Fang, the sun had passed the (autumn) equinox but had not yet reached the (winter) solstice, and in that respect, i.e., being between an equinox and its following solstice, the Xia eclipse was like the present one. How do we decide between the possibilities? The only way is to consider plausible usage. Zhang tries to argue that it is unlikely that the lunar mansion name *fang* existed as early as these texts would imply. We can know nothing about the “Books of Xia.” Whatever this was, it could be a translation into current language of something much older. We do know a lot about the *Zuo zhuan*, and things in it show that it was compiled in the later part of the 4th century BC. But already *a century earlier*, the recently discovered “hamper of Zeng,” with its lacquer representation of the complete lunar zodiac, with names of all the lunar mansions in places, shows that the name “Fang” was well established. It is simply inconceivable that it could be used in a *Zuo zhuan* text, discussing an astronomical point, in any way other than as the name of the lunar mansion.

Now one must ask, how did Yixing construct his *Da Yan Li* date for the eclipse, if he was trying to get a date that matched what he found in the “Jinben” *Jinian*? (In view of Zhang Peiyu’s challenge, we must supplement our explanation by answering this question.) His problem was not only to find a day that was (a) a solar eclipse day; (b) a 1st-of-9th-month *gengxu* day, but also (c) a day in a year that would be a 5th year if year 1 were a year having the *sui* name found in the “Jinben” for the first year of Zhong Kang. The last requirement meant moving the date back by 60-year cycles. The *gengxu* requirement meant (1) determining the actual *ganzhi* for the 1st of the 9th month of 1948 BC; (2) counting forward to the next year having a *gengxu* day as 1st of the 9th month and noting the interval in years; and (3) allowing two years for every 60-year cycle, since shifting back 31 years would preserve *ganzhi* values for first days of month. What he got was 2128, = 3×60 before 1948.

Did the eclipse requirement—i.e., that the day identified be indeed a date of a solar eclipse in Fang—mean that he would have had to keep on jumping back, now by 30×60 year intervals, if 2128 had not just happened (amazingly) to be a 1st-of-9th-month eclipse-in-Fang year according to the *Da Yan Li*? If so, then one would think that Zhang win the argument, because if 2128 were chosen for a quite independent reason and just happened to turn out right, this would seem to be incredible indeed. But not incredible, really; for Yixing’s method (as will be seem) guaranteed that the date would be a 9th-month *shuo gengxu* day in a year having the right *sui*-name, and although the day does turn out to be an eclipse day somewhere, it apparently was not, in north China. Yixing, like any scientist, constructed his theory to fit the available data. The “Jinben”—as he chose to use it—was part of his data. The *shuo* day for the Xia 9th month in 2128 was 13 October, and it was a *gengxu* day; Yixing got that right. (In general, he gets *ganzhi* right.)

Further, it was an eclipse day. Zhang says it was, according to the *Da Yan Li* (p.91), and I will take his word for this. Zhang also says that the day was an eclipse day (“was within eclipse limits”), according to modern methods as well. I have checked, using Paul Ahnert’s tables. The *shuo* in the Xia area was during daylight, and its closure was within the limit. But apparently it was not visible to Chinese observers. (This if forgivable: tracks of eclipses far in the past are very difficult to ascertain; we still argue about them. For some of the argument on this one, see Chen Zungui, *Zhongguo tianwenxue shi*, vol. 3, pp. 853–4.) Was the sun in Fang on Yixing’s date? By my calculations using Ahnert, he had that just about right too. As a scientist Yixing is impressive. But the eclipse cannot possibly have been this early (as Zhang admits, in *Early China* 15, p. 150).

For Yixing to find an earlier year with the same *sui* name having a *gengxu* (47) day as *shuo* of the Xia 9th month was not difficult. Once he had determined that in 1948 the day was *jiayin* (51), he scanned following years until he found one with *gengxu* as *shuo* of the Xia 9th month. To do this (consult a table, find any month beginning *jiayin*, and count forward), one must go forward six years to get a year with that month beginning *gengxu*. Therefore, assume that in 1942 the 1st of the Xia 9th month was *gengxu*. This will be true at 62-year intervals back into the past, and therefore in $3 \times (60 + 2)$ years, i.e., in $180 + 6$ years back. And $180 + 6$ years back from 1942 is 180 years (3 cycles) back from 1948, i.e., 2128. Finding that he had reached an eclipse day with the sun in Fang must then have convinced Yixing that Liu Xin had been right: the three cycles are (1) the 60 years he had inserted in Western Zhou chronology to save Liu Xin; and (2) the 2×60 years that Liu Xin had inserted in the Shang chronology. Liu had inserted those 2×60 years for his own reason, which (see below) will no longer bear examination.

But couldn't a faker do this in reverse, using Yixing's 2128 and ending with the “Jinben”'s 1948? No, not getting to 1948 and thinking it had a 9th month beginning *gengxu*—unless he didn't know that *gan zhi* for first days of months tend to repeat at 31-year intervals, better at 2×31 year intervals (and *not* at 60-year intervals); and anyone doing the simplest work with Chinese dates quickly figures that out. The problem does not require a calendar scientist. And it does not even require knowing the absolute dates.

We cannot leave the matter there. A faker was not working from Yixing. But neither was Yixing starting with the “Jinben” and nothing else, because as noticed, Liu Xin seven centuries earlier also gave Shang exactly 2×60 more years than chronologies known to him. These must have given Shang $496 + 12$ years, which Liu amended to $496 + 13$ years, then adding 2×60 to get 629 years. Why? Whatever the answer (I will risk one), it seem clear that Yixing was adapting Liu's work to his own purpose.

Liu had at least the following worry (see *Han shu* 21 B, especially pp. 48b, 50a). He had the Yin Li to contend with, which took 1580 as the year of the defeat of Jie, and 1579 as the first year of Shang. (See Chen Mengjia 1956 p. 212.) It began its *zhang-bu* calendar with day *jiazi* (01), solstice/*shuo* of the (Xia) 11th month of 1568, i.e., in the Western Han and Yin Li conception, the first month of the year 1567. This was supposed to be the date of Yi Yin's sacrifice to the just deceased Shang founding king Tang. Liu wished to follow the *Shang shu* “Yi Xun” chapter (now lost and reconstituted), which made the day *yichou* (02), *shuo* of the last month of the first year of Tang's grandson and successor Tai Jia. Liu's conquest year for Shang was 1751, 629 years before his Zhou conquest year

1122. He says that Tang exercised power over 13 years, dying (presumably early) in year 13, which counted as Tai Jia 1. The month and day that Liu is assuming therefore must be the solstice month beginning 1738, first day (solstice day, in Liu’s system). The day is *gengxu* (07), which Liu would read *yichou* (02). The cumulative error in his system gave him a day three days early in 1122; it would be five days early when calculated back another six centuries. So this is why he inserted two 60-year cycles. Had he not done so, he would have gotten the year 1618, beginning with *guihai* (60), for him *wuwu* (55); 4 years later (i.e., 120 years earlier) he got *gengwu* (07), for him *yichou* (02).

But the Yin Li date 1579 for the beginning of Shang is already too early; as Pankenier has shown, it must be much later than the *cuo xing* 錯行 movement of the planets, shown by Pankenier to be correctly dated to 1576. So Liu Xin, in inserting two 60-year cycles, is really just compounding a mistake.

5 Yixing’s use of the “Jinben”

So Yixing did not calculate, find that he needed two more cycles, and then insert them. He found them already inserted (by Liu), and then calculated, knowing that he would get a 1st-of-9th-month *gengxu shuo*-day, and found that this worked. So the coincidence does not quite show that the date of the eclipse must be wholly due to Yixing’s figuring. But if he had Liu and the *Han shu* on one edge of his desk, he had the “Jinben” on the other edge, and used them both. This can be known as follows:

As Zhang Peiyu has noticed, one difference between Yixing and the “Jinben” is the reign length of Tai Kang, third king of Xia. For Yixing, it is 12 years. For the “Jinben,” it is only 4 years. Why? Zhang has shown that Yixing and the “Jinben” agree on *sui* names back to but not beyond the “end” (*mo nian* 末年) of Tai Kang. (It might be more accurate to say back to the year before the first year of Zhong Kang, because the “Jinben” has a two-year break after the death of Tai Kang, which I argue (perhaps Zhang agrees) was the mourning-completion period.) Yixing makes Shang 628 years, but to have *sui* names corresponding to *sui* names express or implied in the “Jinben” he must place those 628 years the same way the “Jinben” places its 496 + 12 years, i.e., starting with 1738, the year *after* the defeat of Jie, and ending *with* 1111, the “*shi fa*” (始伐) year, which Yixing has interpreted as the Zhou conquest year. Liu Xin gives Shang 629 years. Liu, however, makes those 629 years begin *with* the defeat of Jie, for Liu, 1751, but corresponding to 1739 in Yixing; and in counting the length of Xia, Yixing is taking this year as the first year of Shang. Therefore Yixing is in effect doing just what Liu Xin does, and the effect is to push the first year of Xia back one year beyond what it would have been. Further, both Liu and Yixing give Xia 432

years counting from Yu’s *de jure* first year, rather than 431 as in the “Jinben”; so the total relative setback of the first year of Xia for Yixing (and Liu) is two years. Yixing’s date is 2171.

The next move was to follow the “Jinben” chronology strictly. It gives Yu 8 years, so he dies (for Yixing) in 2164. Following that, there are, explicitly in the “Jinben,” three years of mourning for Yu, that for Yixing have to be 2163–61. Then comes the reign of Qi, 16 years, which have to be 2160–2145. In the “Jinben” we then find a 4-year gap, no mourning mentioned, then Tai Kang, 4 years, and then a 2-year gap, no mourning mentioned. Next comes Zhong Kang, and for Yixing Zhong Kang’s dates are fixed, by his eclipse calculation. The fifth year has to be 2128, placed (like 1948) relative to the end of Xia (at 1559) exactly as if Yixing (unlike Liu Xin) had taken the year *after* the defeat of Jie (1738) as the first year of Shang. I.e., the eclipse date is 390 years back, for both Yixing and the “Jinben.” The result is that Zhong Kang 1 must be 2132. Yixing (I assume) pays no attention to mourning unless explicitly told to do so. So for him, Tai Kang must be 2144–2133, which is 12 years. The 12 years are the sum if *three time spans in the “Jinben,”* i.e., Tai Kang’s proper 4 years, plus the 2-year gap after Tai Kang, and the 4-year gap before Tai Kang, totaling 10, all of this added to the 2-year increment, more than the “Jinben,” that Yixing gives the Xia.

This argument has been complex, and a table (Table 2) will be useful. Dates in square brackets for the *Li yi* are implied by the “Jinben” but are unexpressed in the *Li yi*. Gaps designated “interregnum” for the “Jinben” are implied in the text by death years and *sui* names. Nivison’s hypothesis is that they were originally all 2 years, for completion if mourning begun in the death year.

I see no way to account for the 12 years Yixing gives Tai Kang except by telling the story I have just told, and that explanation has Yixing checking the “Jinben” at *every* step in his reasoning. Yixing could simply have assumed the 3 years mourning after Yu; but the 8 years of formal reign for Yu seems to be unique to the “Jinben”; the *Shiji* gives Yu 10 years. As for Qi, there is a bewildering variety of reign lengths accorded him, but only the *Lu shi* 路史 gives Qi 16 years (according to Wang Guowei 王國維); a Song Dynasty text, it cannot explain the 16 years assumed in the *Li yi*. As far as I can see, only the “Jinben” or an equivalent text, with its explicit dates using *sui* names, could have given him this information.

	“Jinben”			Yixing		
Yu 1	1989	(Xia 1 of 431 through 1559)	+180+2	2171	(Xia 1 of 431 + 1 through 1740, 431 + 2 through 1739)	
Yu 8, dies mourning 1	1982		+180+2	[2164]		
mourning 2	1981			[2163]		
Qi 1	1979			[2161]		
Qi 16, dies	1978		+180+2	[2160]		
interregnum 1	1963		+180+2	[2145]		
interregnum 2	1962	#1	+180+2	#1 2144	Tai Kang 1	
Tai Kang 1	1959					
Tai Kang 4, dies interregnum 1	1958					
interregnum 2	1955					
Zhong Kang 1	1954					
Zhong Kang 5	1953	#10	+180	#12 2133	Tai Kang "end"	
	1952		+180	2132	Zhong Kang 1	
	1948	eclipse (390 through 1559)	+180	2128	eclipse (390 through 1739)	
				1740	last year of Xia	
Defeat of Jie	1559		+180	1739	Defeat of Jie (1 of 629 through 1111)	
Tang 1 (1 of 496 + 12 through 1051)	1558		+180	1738	Tang 1 (1 of 628 through 1111)	
Zhou "shi fa"	1051		+60	1111	Zhou victory	
Zhou victory	1050		+60	1110	Zhou "ge-ming"	

Tab. 2: Why Yixing Gives Tai Kang 12 Years

6 Yu Guang’s use of the “Jinben”

Now, what about Yu Guang? He was a middle 6th century calendar astronomer (fl. 544; see J. Needham 1959, p. 286), said to have been a *taishiling* 太史令 under the emperor Wu Di of the Liang Dynasty, and the first to have applied technical methods to the Zhong Kang eclipse problem (see Chen Zungui p. 852). Furthermore he was a prominent scientist in his time (having his own “calendar,” bearing his name; see *Tang shu* 27 A, “Li Zhi,” p. 14b). He was criticized by

Yixing for saying that the Zhong Kang eclipse was in Zhong Kang’s first year and not his fifth year. Zhang Peiyu takes this as showing that Yu Guang too did not have the “Jinben,” which would have told him that it was in the 5th year. But it seems to me that it shows just the opposite. Why would Yu Guang have been interested in the matter? As a scientist, obviously because he had tried to calculate the date and found that it did not work; and to know what date he must test, he would need the “Jinben.”

I am assuming that Yu Guang, like Yixing, knew how to determine accurately the *shuo*-days of months in the far past. Asking himself, what was the *shuo* of the Xia 9th month in the year *guisi* (=1948), he would discover that it was not *gengxu* (47) but *jiayin* (51). So the date must be wrong. How should he correct it? He then turned to the “Yin Zheng” in the *Shang shu*, and noticed that one could easily read it as saying that the eclipse and punitive expedition occurred in the first year of Zhong Kang. What did Yu Guang do next? I see two possibilities.

1. He calculates, and finds that this is almost right, and would be exactly right if he makes two reasonable historical assumptions: (1) The Xia court calendar keepers had been neglecting intercalation, so that to right the calendar there would have to be three in the next five years instead of two. And (2), also in the past few years too many long months had been assumed, creating the possibility that at some time the new moon would be visible on the last day of a month; to correct this, they would have to make long and short months alternate, strictly, for several years. Yu Guang then posits intercalations in years 1, 3, and 5; and he assumes that the intercalation in year 1 was after the 9th month, but in year 5 it was before the 9th month. (In year 3 it does not matter where it was made.) The result might look like this:

Year 1 months	1	2	3	4	5	6	7	8	9	10	i10	11	12
Year 1 <i>shuo ganzhi</i>									47	17	46	16	45
Year 2 months	1	2	3	4	5	6	7	8	9	1	1	12	
Year 2 <i>shuo ganzhi</i>	15	44	14	43	13	42	12	41	11	40	10	39	
Year 3 months	1	2	3	4	5	6	7	8	9	10	11	12	13
Year 3 <i>shuo ganzhi</i>	09	38	08	37	07	36	06	35	05	34	04	33	03
Year 4 months	1	2	3	4	5	6	7	8	9	10	11	12	
Year 4 <i>shuo ganzhi</i>	31	01	30	60	29	59	28	58	27	57	26	56	
Year 5 months	1	2	3	4	5	6	7	8	i8	9	10	11	12

Year 1 months	1	2	3	4	5	6	7	8	9	10	i10	11	12
Year 5 <i>shuo ganzhi</i>	25	55	24	54	23	53	22	52	21	51			

Tab. 3: A Possible Argument for Dating the Eclipse to Zhong Kang I

Yixing would have been justified in objecting. There is no possibility of an eclipse on *gengxu* if one is supposing that the *shuo*-day of the Xia 9th month of 1952 counts as *gengxu* only because the calendar had been mismanaged. The foregoing analysis thus has Yu Guang making an almost inconceivable mistake. But it does show that the date Zhong Kang 1 is so close to giving him a solution that he would have tried to find a better account, justifying Zhong Kang 1 (=1952) as the date of the eclipse.

- I am inclined, therefore, to think that his solution may have been as follows: This eclipse is discussed in the *Zuo zhuan*, Zhao Gong 17, 6th month, in an argument concerning an eclipse said to be of that date, that quotes the account in the “Books of Xia” of what we take to be the eclipse of Zhong Kang. This account has seemed to many interpreters, including the *Zuo* commentator Du Yu in the Jin Dynasty, and also Zhang Peiyu today (see *Early China* 15, p. 139), to put this Xia eclipse in the 6th (Zhou) month, hence in the 4th Xia month. This forces strange interpretations of the word “fang” 房: if the eclipse was in the 4th Xia month, the sun could not have been in Fang, next to Antares and where the sun would be in autumn. (I have already given my own refutation of this “Xia 4th month” interpretation. See also *Early China* 15, pp. 164–6.) Nonetheless, this reading of the *Zuo zhuan* text must of course have been known to Yu Guang from Du Yu’s commentary. Perhaps significantly, Yixing deals critically with the dispute about “fang” immediately before saying that Yu Guang was wrong to put the eclipse in the 1st year; and this suggests that the two problems are connected. Trying out the common interpretation of the *Zuo* text, Yu would have found that it worked, for Zhong Kang 1. The date is 22 May 1952 BC, a *gengxu* day, *shuo* of the 4th Xia month (first month of summer), and also the date of a solar eclipse, although the eclipse was seen only in the southern hemisphere, a fact that may have been beyond Yu Guang’s reach.

In any case, Yu Guang was a calendar astronomer who was saying something that interested Yixing in his discourse on calendar science, and so we cannot support that Yu Guang was making no more than a philological argument. To be making a *calendar* argument, he has to have been using data that he could have

gotten only from the *Zhushu jinian*, and most conveniently from a text that used *sui* names as does the “Jinben,” and that like the “Jinben” recorded the eclipse in Zhong Kang 5, so that the year given for the eclipse was plainly the equivalent of 1948 BC. I.e., it must have been a text like the one we have now, that did not insert 60-year cycles as did Yixing (and Liu Xin). This analysis puts a “Jinben”-type text back in late Six Dynasties. Another item of information, in a recent article by Chen Li 陳力 of Sichuan University, shows the existence of such a text even earlier. Chen cites the *Zhen gao* 真誥 by Tao Hongjing 陶弘景, also Liang Dynasty, which (*juan* 33) says that according to the “chronicle from the Ji tomb” 汲冢紀年 the first year of Yao was 2643 years before the year *jiwei* (16) of Qi, i.e., before 499. This gives the year as -2144 BC, which is the date of Yao 1 in the “Jinben” (Chen 1997 p. 80).

As I have noted, the supposed 496 + 12 length for Shang was known before Liu Xin, in Western Han. This does not mean that the *Zhushu jinian* survived above ground after all, even in Han. But some of its chronology did, and there are other indications of this. There is, for example, the curious fact that if you take the *Shiji*'s date for the death of Bo Qin, 999, and add to it the reign length accorded Bo Qin by Liu Xin, i.e., 46 years, you get the date of the first year of the Zhou Hong regency in the “Jinben.” And there is also the intriguing fact that if you take the “Jinben” summary's year of the Zhou conquest, and of Wu Wang as king, 1051, and subtract 500 years, you get the birth date of Confucius that was recognized in Western Han; and if you subtract another 500 years, you get the date of the imperially sponsored Shiqu Conference on the Confucian Classics.

7 Other problems

There are other things in Zhang's article that I would not have put there. On p. 90, near the top, he thinks that the “Guben” gives Xia 471 years, and the “Jinben” gives Xia had 471 years. One will count 431 years if one starts with the year when Yu formally became king, there said to be 1989, after completing mourning for Shun. The intended first year is the supposed real one, 2029 (Shun 14), when Shun transferred power to Yu. So 2029–1990 is the 40 years Zhang thought was missing, and 2029–1559 is the 471 years in the “Jinben” end-of-Xia summary.

Also on p. 90, Zhang expresses puzzlement about the apparent difference between the “Jinben” and what he takes to be the “Guben,” on the length of Western Zhou. The “Jinben,” in the summary at the end of the chronicle for the last king, You Wang, counts from 1062 (Di Xin 41), said in the chronicle to be the year of Wen Wang's death (hence Wu Wang's becoming ruler). The conquest year is given as the year 1051, elsewhere described as the year the Zhou attack

was launched; 24 years later (1027) the mandate-certifying cauldrons were formally placed in Luoyang; and 257 years later (771) the dynasty ends. But quotations, e.g., by Pei Yin 裴駰 in his *Shiji jijie*, say that from Wu Wang to You Wang was 257 years, a statement that has generated the widely held belief that 1027 must be the year of the conquest. “Why?”, Zhang asks. His answer: The “Jinben” chronology was cobbled together from the *Li yi*, which has a quite different chronology; one may attempt twisted explanations if it, but to no avail. Wang Guowei was right.

I deal with this problem at the end of Appendix 4 of my monograph “The Key to the Chronology of the Three Dynasties: the “Modern Text” *Bamboo annals*” (*Sino-Platonic Papers* 93, 1999). Pei Yin reveals, in his commentary at the end of the account of Wei king Xiang 襄 in the “Wei Shijia” 魏世家 that he actually never saw the *Zhushu jinian*, for he has to quote the Jin court restorer He Qiao 和喬 on the scope of the text. So when he “quotes” the *Zhushu jinian* as saying “From Wu Wang to You Wang was 257 years,” he has to be *interpreting* a truncated quotation by someone else. One can see what this quotation must have looked like. Omitting the phrases containing *sui* names (which would not have been in the fully restored text that has been lost), the text must have been this:

When Wu Wang destroyed Yin, in 24 years the cauldrons were deposited in the city of Luo. To You Wang was 257 years.

武王滅殷，二十四年，定鼎洛邑，至幽王，二百五十七年。

In the original, year totals follow, that make clear that the count “257” is from the depositing of the cauldrons. But without these totals the text is ambiguous, and could be taken as saying that from Wu Wang to You Wang was 257 years; and so Pei Yin took it. We should all stop worrying about the matter.

Most of the rest of Zhang’s article he takes up with examples of a “Jinben” date that he shows to be wrong, inviting us to take it from granted that the only possible explanation is that the “Jinben” is a post-Tang fake. But anyone working at all carefully with the “Jinben” *Zhushu jinian* knows that these dates are wrong. I am afraid that I have failed to make clear my idea that the text could have gone through *systematic* alterations in ancient times, that shifted the year dates forward or back, sometimes only one year, sometimes more than a hundred. This is what I have been trying to figure out for the last twenty years. Of course, anyone who doesn’t see this is bound to find everything I do to be inexplicably wrong. I am afraid that this was Zhang Peiyu’s reaction to the Nivison-Pang article that he was invited to criticize, in *Early China* in 1990.

My work needs informed criticism. Zhang is an extraordinarily learned scholar, in areas that I have had to try explore as an amateur. I am sure that I have much to learn from him. I would be deeply grateful for his comments on what I have actually been trying to do. What I have tried to do here is to show that there is a reasonable analysis of the “Jinben” *Zhushu jinian* and Yixing’s *Da Yan Li yi* that shows Yixing using the “Jinben” data as he worked out his *Li yi*. What seems to me to be especially convincing is the problem of explaining why Yixing gave 12 years to Tai Kang of Xia. And just as convincing, Chen Li’s discovery of a Six Dynasties confirmation of the “Jinben”’s date for the first year of Yao, showing that the “Jinben” existed not just before Yixing but even before Tang.

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A Note on Eclipses

Zhang Peiyu has expressed doubt that the eclipse identified in the Nivison-Pang article could have been the famous Xia eclipse, because it was only “around magnitude .4, and would not have induced any great panic.” (*Early China* 15, p. 145.) Not only was it ring-form and relatively inconspicuous, but our research showed it to have been visible far enough north of the Xia capital so that it could have been known to the Xia recorders only from a report. (We know that this was done; there are eclipses in the oracle inscriptions that are said to have been reported.) We had assumed that the account of panic was historical exaggeration; and we believe that it is a mistake to suppose that an eclipse is likely to be found in the historical record only if it is a very impressive one. (That is a way of thinking being read back into ancient minds from our own culture.) Eclipses were important not as “news,” but because it was feared that they might be ominous. It follows that unusual phenomena, eclipses and other things, would be temporarily recorded quite indiscriminately, and would often stay in the record only if some misfortune did follow not long afterward. This would explain the “double dawn” at the beginning of Yih Wang’s reign in 899. It was an eclipse far to the east, not visible in the Zhou capital, probably only seen as a very slight darkening of the sky (in west “Zheng,” east of Zong Zhou) after the very first faint light of dawn. The explanation of the record is that Yih Wang’s reign turned out badly. He was not a competent king, and was probably before he died pushed aside by his uncle Pifang 辟方, who made himself king as Xiao Wang. In Zhong Kang’s case, perhaps the misfortune that caused the eclipse to be remembered was the gradual usurpation of Han Zhuo 寒浞, who murdered the next king Xiang. (The 40-year interregnum in the “Jinben” after that is, I

think, an early invention—perhaps around 400 BC, early enough to have gotten into the *Zuo zhuan*; and the *sui*-names sprinkled into the account must be the guess-work of the restorers, or of later persons.

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- (References to the *Han shu* are to the (Taipei) Yiwen Yinshuguan photographic reprint of Wang Xianqian 王先謙, *Han shu bu zhu* 漢書補註, Guangxu 26 (1990). Likewise references to the (*Xin*) *Tang shu*; the Yiwen reprint is a photographic reprint of the Qian Long Wuyingdian edition.)

15 The 1046 Hypothesis

I am here responding to three articles by David W. Pankenier, that were published in 1992, sharply critical of me and vigorously defending his hypothesis that the date of the defeat of King Di Xin of Shang by King Wu Wang of Zhou was 1046 BC. One will rightly ask, why now, eleven years later. There are two reasons. The first is personal: When I read Pankenier's articles, I saw at once that their importance called for a response, and I wrote one immediately. But having finished it I saw that my work was angry and therefore unusable, so I buried it, resolving to try again much later. I think that I have now succeeded in offering an analysis in a strictly scientific spirit.

The second reason is that Pankenier's hypothesis has now been endorsed by the five-year "Xia-Shang-Zhou Chronology Project" in the PRC in its published report of 2000, and this conclusion is expected and intended to be seen as the centerpiece of that report. It thus gains a prominence that makes an evaluation an urgent matter of current importance. The Project report does not tell us that the hypothesis is Pankenier's, though everyone working in this corner of scholarship knows that it is his; and I think that this is outrageous, even though I shall be arguing that the hypothesis is wrong. It is true, however, that the argument that the Report uses to reach its conclusion is different from Pankenier's. I will try to make all of this clear.

(A third reason for revisiting Pankenier's work is that he has much more recently published Chinese translations (by Xu Fengxian) of the three articles on which I focus attention, in a collection of eleven papers: *Zhongguo Shanggu Shishi Jiemi* 中国上古史实揭秘, Shanghai: Shanghai Guji Chubanshe, 2008. When I refer to specific points in Pankenier's arguments, I will try to discover whether he still holds the view expressed. DSN, 2010)

I have offered Professor Pankenier an opportunity to reply to my criticisms, but he has declined. I think that I must present my case herewith anyway; but one must not forget that the case is a case against Pankenier's arguments as of 1992. He still adheres to his conclusion, but he may by now have corrected some of the mistakes that I will point out. The worst of them, however, are errors that he would not be able to correct without abandoning his conclusion. (DSN, 16 Nov. 2000)

January 2000; revised November 2000 and January 2003; revisited, August–September 2010

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Professor David W. Pankenier (hereafter DWP) has since 1981 been defending his hypothesis that Wu Wang, Lord of the West in Zhou, defeated the armies of the king of Shang early in the year 1046 BC, thereby establishing what we call the Western Zhou Dynasty. For nearly a decade I argued for the date 1045, with diminishing conviction. I published my case in 1983, but discovered an error almost at once, giving me an argument pointing to the date 1040. I published an admission of my mistake immediately.¹ By 1990 I was no longer in doubt that 1040 must be right. In what follows I explain why I reject DWP's theory, concluding with a very brief argument for 1040. I will be looking with special care at three articles by DWP (with occasional references to others):

"The *Bamboo Annals* Revisited: Problems of Method in Using the Chronicle as a Source for the Chronology of Early Zhou," Part 1, in *Bulletin of the School of Oriental and African Studies* (BSOAS) lv.2 (1992) pp. 272–297

"The *Bamboo Annals* Revisited: Problems of Method in Using the Chronicle as a Source for the Chronology of Early Zhou," Part 2: "The Congruent Mandate Chronology in *Yi Zhou Shu*," in *BSOAS* lv.2 (1992) pp. 498–510

"Reflections of the Lunar Aspect on Western Chou Chronology," in *T'oung Pao* (TP) lxxviii (1992), pp. 33–75

1 Points of partial agreement between DWP and myself:

The *Jinben Zhushu jinian*, "Modern Text" *Bamboo Annals*, in two *juan*, is to a large extent authentically a copy of the chronicle discovered in a royal tomb or cache around 280 CE, and then restored by court scholars, who sorted out some disordered bamboo slips and transcribed the text into current script. (Scraps of another text, called the "ancient text," *guben*, exist, thought by most scholars to be the original, now lost. Most scholars, apparently including those writing the Project Report, take the "modern text" to be a late fake and make no use of it. I think that the "modern text" is a copy of the restorers' work before it was finished, and that their completed work, the "ancient text," is now lost except for quoted fragments. I am not sure what DWP thinks about this.)

The record of a conjunction of five planets in the "modern text" *Bamboo Annals*² dated 1071, said to be in Fang (i.e., in Jupiter station 10, Da Huo), is a

1 D.S. Nivison, "The Dates of Western Chou," *Harvard Journal of Asiatic Studies* (HJAS) 43.2 (1983), pp. 481–580; D. S. Nivison, "1040 as the Date of the Chou Conquest," *Early China* (EC) 8 (1982–83, published 1984), pp. 76–78.

2 All references to the *Bamboo Annals* herein are to the so-called "modern text."

rewriting of an earlier and correct text that dated the conjunction 1059, locating it in Chun Shou (Jupiter Station 6). (But DWP thinks the rewriting was done by the restorers in the late 3rd century CE, altering an original text that had the conquest in 1046; I think it was done by chronologists in the court of King Xiang of Wei at the end of the 4th century BC, altering a text that had the conquest in 1045 (1045 being an alteration of an original 1040); I argue that no text of the *Annals* ever had the conquest in 1046.)

The next year 1058 was a year when Jupiter was in Chun Huo, and was the year when Wen Wang claimed the Mandate of Heaven (*Yi Zhou shu*, “Wen Chuan,” not referring to the *Bamboo Annals* or the conjunction). The Project Report draft of April 2000 does not recognize a 9th year death date for Wen Wang, but follows the *Shiji* and the *Shang shu da zhuan* in making Wen Wang die in the “7th year” of his Mandate, using the lunar eclipse of 1065 to deduce 1050 as the death date, and using the *Shiji* to date the conquest in the 11th year of the Mandate, i.e., 1046 (the date I reject; I explain “11th year” as an error in the *Shiji*, below). The Report as published in October 2000 deletes this argument, and DWP would not accept it. But we do agree that Wen Wang died in the 9th Mandate year, i.e., in 1050 (in 1062 in the *Bamboo Annals*). (“Wen Chuan” and *Bamboo Annals*) (I will explain the “7th year”–“9th year” puzzle below.)

The *Yi Zhou shu*, “Xiao Kai,” date and description of a lunar eclipse in the first (Xia) month of [Wen’s] 35th year is accurate; the eclipse occurred on 13 March 1065. (The April draft of the Project Report also uses this evidence, and so agrees that Wen Wang died in 1050 BC. The published Report relies on C14 data to restrict conquest date possibilities to 1050–1020, and then uses text evidence—especially the *Guoyu*, which I explain as error below—to pick the exact date.)

Therefore Wen Wang had a calendar beginning in 1099, and his death was in year 50 in this calendar, i.e., in 1050 BC. The Project Report agrees. But DWP, and also the Report, take this to be the only calendar for Wen Wang. I argue that Wen’s succession year was 1101, and that his tenure was 52 years (as in the *Annals*), the first two being for completion of mourning.

This is the end of the points of (partial) agreement directly related to DWP’s 1046 hypothesis. It would be only fair to add, however, that DWP has two other surprising results that I think are right. These are his date for the beginning of Xia, 1953 BC; and his date for the beginning of Shang, 1554 BC. These dates are confirmable (though not deducible) by using astronomical evidence, but they cannot be obtained without using data from the *Bamboo Annals*; therefore the Project Report does not have them, and has other (earlier) dates (estimates only) for the beginnings of Xia and Shang.

2 The *Guoyu* astrological account

DWP has regarded as authentic the astrological account for the beginning of Wu Wang's victorious campaign against Shang, given in *Guoyu* "Zhou Yu" 3.7, putting Jupiter in Chun Huo at the time, as maintained also by Liu Xin in the Han Dynasty (who also accepted the *Guoyu* account). The Xia-Shang-Zhou Report also takes the *Guoyu* text as primary evidence. Like Pankenier (and me) the Report concludes that Wen Wang died in 1050, and argues (like early Pankenier but unlike me) that the *Guoyu* text preserves an authentic record, and requires that the conquest was in the next Chun Huo year, which was 1046.

(The Report adds another piece of claimed evidence, namely the *Li gui* inscription. It begins, "Wu Wang's attack on Shang was in the morning of day *jiazi*. *Sui ding ke wen su you Shang*" This problematic part of the text is taken to mean "Jupiter being in the right place (*ding* = *dang*), [I] could announce that we would quickly defeat Shang" The theory is that the maker of the vessel was an astrologer, being rewarded for his work.³ The claim is made that the late scholar Yu Xingwu supported this interpretation of "*sui ding*.")

Refutation: DWP at first defended the authenticity of the whole of the *Guoyu* account, and still did so, in his second *BSOAS* article (1992).⁴ Then (in his 1992 *TP* article, pp. 67–68) he decided that dates of events in Wu Wang's campaign had to be read in the Xia calendar (i.e., that began the year with the pre-spring-equinox month), putting the beginning of the campaign in late winter. But the relevant *Guoyu* text says this, of the day when Wu Wang marched forth:

- (1) Jupiter was in Quail Fire (Chun Huo) (about 87 through 131 degrees ca. 1050)
- (2) The moon was in the Sky Quadriga (Tian Si, i.e., Fang) (about 196 through 202 degrees ca. 1050)
- (3) The sun was in the Ford at Split Wood (Xi Mu) (about 223 through 254 degrees ca. 1050)⁵

3 This theory is that of Li Xueqin, argued in his book *Xia Shang Zhou niandai zhaji* (Liaoning University Press, 1999) 204–205.

4 See his second *BSOAS* article, p. 503, note 70, where DWP defends the text as accurately describing the celestial situation at the outset of a campaign beginning in the autumn of 1047. He spends 20 lines of fine print chiding me for arguing that the text was an early Han invention (as I did, in earlier publications). Shortly before he published, I presented a conference paper arguing that it is a calculation done in the early 5th century BC, a view I still hold.

5 The Ford at Split Wood is defined in the *Erya* as "the Ji-Dou region," and I follow this definition. Traditionally the Split Wood Jupiter space was Wei and Ji, i.e., about 214 through 232 ca. 1050.

I omit my guesses as to the meanings of more lines. Applied to late 1047, the above data require that the next lunar month was the month beginning 30 Nov, sun at 240 degrees. Therefore lines (2) and (3) locate the sun at least a month before the winter solstice (at 270 degrees); and it also places the moon in relation to the sun so as to indicate that the time of the beginning of the campaign was about four days before the end of the month, as is indicated by lunar phase dates in the “Wu Cheng” account if interpreted in Wang Guowei’s “4-quarters” system, which DWP rejects (see below). DWP’s analysis of 1046 (*TP* p. 70) takes the calendar to be the Xia annuary with *yin* month as first month, and requires him to assume that the campaign began 9 days before the end of the month *after* the solstice month. (The Xia-Shang-Zhou Report does not make this mistake.)

Therefore DWP now (as of 1992 anyway) in effect rejects almost all of the *Guoyu* account, retaining only the location of Jupiter in Chun Huo. He has no explanation why it is right to accept the one detail if he rejects the rest. He cannot claim Liu Xin as authority, because Liu himself was relying on the *Guoyu* account, and accepted all of it, simply distributing the phenomena over many days instead of holding them to the first day of the campaign.⁶ Thus DWP is in reality relying only on his intuition that since in later times Chun Huo was associated with Zhou, it is *prima facie* reasonable to suppose that Wu Wang would have picked the next Chun Huo year (next after the Mandate year) to attempt his conquest.

Also, there is another account of Jupiter’s position at the time, in Yang Liang’s commentary (Tang Dynasty) to the “Ru Xiao” chapter of *Xunzi*, quoting a 4th century BC philosopher named Shizi, saying that Jupiter was in “the north.” The probable meaning of this is that Jupiter was in a station that would be represented on the “north” side of an astrologer’s chart, i.e., in Xuan Xiao or the station before or after. DWP gives no adequate reason for rejecting this account.⁷ If the *Shiji* “Zhou benji” dates for events in the conquest campaign are

⁶ I suppose it is conceivable that this is now DWP’s view. It is the view taken by Li Xueqin (1999, pp. 211–212), and so probably also by the Project; Li sees the phenomena as spread over the campaign from its inception to the crossing of the Yellow River on the 26th day (but why stop there?). This makes no sense: the moon moves through all spaces of the zodiac in 27 1/3 days, so saying that it was “in Tian Si” says almost nothing, if any day in the campaign can be meant.

⁷ To his credit, Pankenier does discuss the problem in his dissertation (Stanford, 1983) “Early Chinese Astronomy and Cosmology: the “Mandate of Heaven” as Epiphany,” pp. 241–244. He does not, as far as I know, discuss it in later publications, nor have I found discussion of the problem by anyone else who argues for the *Guoyu* “Chun Huo” text as the key to the date of the

used, and are interpreted not as Sima Qian did but correctly, so that the campaign begins near the end of one year and concludes early in the next; and are read as in the Xia calendar (but without commitment to the year numbers “11” and “12”); and if the (Zhou) year is taken to be the year corresponding to 1040, then at the beginning of the campaign Jupiter was in the middle of Xuan Xiao, i.e., dead-center “north.”

This account should be accepted and the *Guoyu* account rejected, if there are other reasons for choosing 1040, and if a reasonable explanation for the *Guoyu* account as fiction can be found. The case for 1040 will be presented below. The probable explanation for the *Guoyu* account is this: Around 500 to 450 BC, an investigator who was using the newly discovered *zhang-bu* system for identifying *ganzhi* for days in past time, and who believed (still) that Jupiter’s period was exactly 12 years, applied these assumptions to the received data for the Zhou victory campaign.⁸ I.e., he knew the victory was in 1040, and he had the dates of campaign events consistent with those now found in the *Shiji*, in particular, dating the victory on day *jiazi* near the end of the 2nd month (Xia calendar, i.e., the spring equinox month). The *zhang-bu* system has a built-in

conquest. The omission is fatal: If there are two contradictory pieces of evidence, one favoring one’s hypothesis and one opposing it, to accept the favorable evidence merely because it is favorable is to argue in a circle. One must first give an *independent* reason for rejecting the evidence that would show one’s hypothesis to be wrong. To his credit, Pankenier sees the problem and does his best. The *Xunzi* text says that Wu Wang “faced east to meet Tai Sui,” the imaginary counter-revolving “Jupiter” that must be in the astrological “east” (chronograms *yin*, *mao*, *chen*) when Jupiter itself is in the “north” (*chou*, *zi*, *hai*). (For this analysis, think of the twelve *zhi* (as chronograms) written around a circle in clockwise order, which is the direction of movement of counter-Jupiter.) There is an equivalent text in *Huainanzi*, with a comment by Gao You (Eastern Han), that Tai Sui was in *yin*, i.e. Jupiter was in *chou*, corresponding to Jupiter station 12, Xing Ji. But Gao You must simply be applying Liu Xin’s algorithm for locating Jupiter, which puts Jupiter in Xing Ji in 1070 BC, the Yin Li date for the conquest. At most this shows that *Shizi* and the Yin Li chronology (whenever it was invented) are by Han calculation very loosely consistent. It does not show that *Shizi*’s (middle Zhan Guo) statement is false or necessarily derivative.

8 I have met with the objection that the Chinese did not discover the 76-year *zhang-bu* intercalation cycle as early as 500–450 BC. One must distinguish between the system of date calculation and its use in assigning exact dates in Shang-Zhou chronology (which gives it the name “Yin Li”). The date of origination of the system can be ascertained by using it to calculate absolute dates for first days of first years of 19-year *zhang* over several centuries, and then taking the midpoint of the time span when the system gives correct dates. The time turns out to be roughly about 470–450 BC. The concept was perhaps used for several decades, and then adjusted to give the system we have. The first use of the system in a “Yin Li” Shang-Zhou historical reconstruction is likely to have been a century or more later.

error giving a *ganzhi* one day early for about 304 years back (half of 304 years back for the first day-error if counting back from the approximate year of adoption of the system, then one more day early for each added 304 years back). This would tell him that *jiazi* must be the first day of the 3rd month, which he would know to be wrong. He would then conclude that the intended calendar must be the “Zhou” calendar taking the solstice month as month 1; i.e., he must move all dates back 30 + 29 days. This would put *jiazi* at the end of month 2, and would have the campaign starting in late autumn of 1041. He then counted forward by 12’s from 1041 to his own time, and found that this gave him a recent year when Jupiter had been in Chun Huo. So he concluded that this must have been true when Wu Wang’s campaign started.

(Note: this explanation serves also to explain the dates in the “Wu cheng,” probably written at the same time or after this: its solution to the hypothetical investigator’s problem is to keep the absolute date for the beginning of the campaign unchanged, but *rename* it “1st month,” as in the Zhou calendar, while leaving “2nd month” *named* as it was, but taking it as month 2 in the Zhou calendar rather than in the Xia calendar, thus moving it back two months.)

(As for the *Li gui*, Yu Xingwu actually argued that the words “*sui ding*” mean “in the annual divination rite” (*ding* = *zhen*).⁹ I will show that the victory was on the first day of Qing Ming, the major ancestor festival of the year. The meaning therefore is “In the [great] divination rite of the year [addressed to the royal ancestors], we were able to announce [to them] that we had quickly defeated the Shang.” The words serve as an “event date” for the inscription, but conceivably also Li as astrologer-diviner is being rewarded for performing his office on the great occasion. The formal purpose of the “divination” would have been to determine whether proffered offerings were acceptable: “We now having defeated the Shang, we offer you” The real purpose, of course, would have been to announce the victory to the ancestors. Yu Xingwu did not know the date was Qing Ming Day, so he imagined that the divination was long before the battle: “In the annual divination, [we learned that] we could announce [to all] that we would quickly defeat the Shang.” It was Li Xueqin who noticed that other information in the inscription makes it likely that Li was a court diviner or astrologer.)

9 Yu Xingwu, “*Li gui mingwen kaoshi*,” *Wenwu* 1977.8.

3 The “Aborted Campaign”

DWP like Liu Xin holds that the victory was in the next Chun Huo year following the Mandate year. This year (unknown to Liu) was 1046. As primary confirmation of his date 1046, DWP points to accounts of a preliminary campaign as far as the Yellow River two years before the conquest year. The *Shiji* says that 800 allied lords and their forces assembled without prior arrangement and urged Wu to continue and attack, but Wu refused, saying “you do not understand the Mandate of Heaven.” Wu’s real meaning, says DWP, was that Jupiter had been approaching Chun Huo, but then stopped, and retrograded, to the surprise and consternation of Wu and everyone else. This retrograde motion of the planet was actually happening in late 1048. (DWP even argues that the allies assembled because they noticed that Jupiter was moving toward Chun Huo; hence no prior understanding was needed.)¹⁰ So DWP always describes this supposed preliminary assembling of forces as an “aborted campaign.”

Refutation: The *Shiji* and other early accounts describe the preliminary expedition as a “*guan bing*” campaign, i.e., Wu’s motive was never to continue with an attack, but was merely to “observe the troops,” to see whether the allies were reliable. DWP’s argument would be less implausible if he had said this, and had represented Wu’s appeal to “the Mandate of Heaven” (= retrograding of Jupiter) as merely Wu’s excuse for not going on. Still, such an account would not really make sense. If the Chinese of the time considered Jupiter’s position and movements as being as important as this, they would be quite familiar with Jupiter’s period (short term), and with retrograde motion, because it happens every year, at just the time when Jupiter is in opposition, and most prominent in the night sky. The allies would not have assembled by watching Jupiter (if we can somehow imagine this happening), because they would know in advance that Jupiter was going to retrograde before reaching Chun Huo.

Furthermore, the story of this preliminary campaign is probably merely a myth. There were several different datings in old secondary sources for conquest era events. One, as in the *Annals*, gave Wen 9 years of “Mandate,” followed by Wu Wang who conquered in his own year 12. Another, in the *Shiji* “Shijia” chapters, put the victory in Wu’s 11th year (for reasons I will explain below). Another, as in the *Shiji* “Zhou benji,” apparently put the victory in the 11th year of a continuing calendar beginning with Wen’s receiving the Mandate, and dating Wen’s death to his 7th Mandate year. (This is the chronology accepted by the Xia-Shang-Zhou Project draft Report.) Yet another, assumed by Liu

¹⁰ D. W. Pankenier, “The Cosmo-Political Background of Heaven’s Mandate,” *Early China (EC)* 20 (1995), pp. 130–131, note 11.

Xin (and followed by DWP), used sources that showed that Wen died in his 9th Mandate year, and therefore put the victory in the 13th Mandate year. Both of these continuing-calendar “11th year” and “13th year” dates assumed that Wu’s victory was in his own 4th year. The cause of the confusion was that there were two calendar counts, from 1058, when Wen claimed the Mandate; and from 1056, when he promulgated a royal calendar (to be explained below).

If the “12th year” date for the conquest (itself an error) is counted from 1056, it puts the conquest in Wu’s 5th year, = 1045. If one counted in the calendar that dated Wen’s death to year 9 (i.e., counting from 1058, in effect), it would seem that the victorious campaign if in year 12 was in Wu’s 3rd year. Wu’s 3rd year would be year 10 in the calendar that had Wen dying in year 7. This would have produced the appearance of two campaign dates two years apart, 10 and 12. It would not be conceivable that the first could have been a failure, so (I suggest) it was conceived as an expedition to observe the turn-out of forces (in year 10, as in the *Annals*, which for other reasons, to be explained, took the 12th-year calendar as Wu’s, beginning only after Wen died). Sima Qian misread dates in his sources (to be explained below), taking the final and real campaign to be in year 11 rather than in year 12. This made the preliminary expedition fall in year 9, i.e., in Wu’s 2nd year, and produced the myth (reflected in the first “*lie zhuan*,” for Bo Yi and Shu Qi) that Wu had started military actions before completing mourning for his father. Finally Liu Xin, seeing Sima Qian’s 9th year and 11th year dates. and knowing that Wen had died in “the 9th year,” concluded that the *Shiji* was wrong by two years, and that the victorious campaign was in year 13, a conclusion confirmed for him by his noticing that this would have been a repeat Chun Huo year, according with the *Guoyu* (if one overlooked the position of the sun, or if one supposed a calendar beginning with the first month of winter, or if one spread the celestial events over the whole campaign). DWP has accepted Liu’s picture, improving on it by adding his hypothesis that the supposed first “campaign” was “aborted,” because everyone was scared off by Jupiter’s retrograde motion.

That the supposed first expedition is a myth is evident from close analysis of texts. What we find is elements of the account of the real campaign refitted into the supposed preliminary movement two years earlier. There are two “Tai Shi” addresses by Wu to the troops, one in each campaign; and the *Shiji* (“Qi Shijia”) is forced to refer to one as “this Tai Shi” to distinguish the one from the other. The *Shijing* line (end of the “Da Ming” ode) “*hui zhao qing ming*” (referring to the Zhou victory over Shang), actually meaning “This occurred in the morning, Qing Ming day” (see below), gets reinterpreted as “The morning of the encounter was clear and bright.” Then a legend is invented to explain: the army

had had to struggle through rain and mud for several days before, but the weather cleared at the last moment (presumably showing Heaven's favor). This story gets better: Wu Wang had given his word that his army would arrive at Muye on *jiazi* day for battle, and for this reason resisted pleas from his lieutenants that he yield to the wretched weather and stop to rest the troops; he must arrive on time. Hence the question “*hui zhao qing ming; he jian wu qi?*” “the morning of the encounter was clear and bright; but how did we manage to get there on time?” This is then transferred to the supposed earlier rendezvous, producing the line in the “Tian Wen” in the *Chu ci*:—but here “*hui zhao qing ming*” makes no sense as “the morning of the encounter was clear and bright,” so the characters “*qing ming*” are realized as “*qing meng*,” and then “*zheng meng*”: “in the morning of our assembly we [asked to make a treaty, competed in making a treaty, =] eagerly pledged allegiance; but how did we get there when we were expected?” And finally we have the legend of the *guan bing* rendezvous in the *Shiji*: Marvel of marvels, 800 lords and their forces converge on Mengjin “*wu qi*,” without prior arrangement.

We see this shifting even in the descriptions of the troop movements: Liu Xin, as quoted in *Han shu* 21B, apparently quoting an earlier source, says “*Shi chu fa yi Yin shiyi yue wuzi*,” “The army first set out in the Yin 11th month, day *wuzi* (25).” This, of course, in the actual conquest campaign; Liu (quoting the lost “Wu Cheng” chapter of the *Shang shu*) has Wu starting out five days after his army, on day *guisi* (30). Farther on Liu says “*Bingwu huan shi*” “On day *bingwu* (43) [Wu Wang] rejoined his army.” But the *Annals* for Di Xin 51, i.e., Wu 10, says (speaking of the preliminary expedition) “*Dong shiyi yue wuzi Zhou shi du Mengjin er huan*,” “Winter, 11th month, day *wuzi* (25), the Zhou army crossed at Mengjin and then returned.” The word *huan* has shifted its meaning. We should expect somewhere to find a statement that in the supposed preliminary campaign Wu Wang *withdrew* his army on day *bingwu*. The expected statement is found in Jiang Sheng's reconstruction of the original “Tai Shi” chapter of the *Shang shu* (Legge, *Shoo King*, p. 298): “*Wei bingwu wang huan shi*,” “On day *bingwu* the king withdrew his army.”

The expedition two years earlier is cloned off the final one. Its mythic character is underscored by marvels: a red bird descending on the king's lodge; a white fish jumping into the king's boat.

4 The supposed 4-year Back-shift

DWP sees more confirmation, in what he perceives as a uniform 4-year back-shift of dates in the *Annals*. If the Conquest really was in Mandate 13, actually a Chun Huo year, 1046, and in the present text of the *Annals* is in 1050, by impli-

cation (for the *Annals*) a year when Jupiter was in Chun Huo (station 7, because the conjunction, dated 1071, is put in Fang, in Da Huo, station 10), we see that the date has been set back 4 years. Similarly, the appointment of Tang-shu Yu as first lord of Jin, said in the *Guoyu* to have been in a Da Huo year, is put in 1035, whereas the nearest Da Huo year in fact was 1031; so again there was a 4-year shift back. If the length of Shang was 496 years, as the *Annals* and apocryphal texts say, then the beginning of Shang must have been 496 years before the Mandate year 1058, i.e., in 1554; but the *Annals* dates it 1558, 4 years back; and the *Annals* date for the *cuo xing*, “movement in succession” of the planets is 1580, whereas the actual sidereal events were in late 1576. So for some reason a rewriting of the chronology had had the effect of moving all dates back four years. The date of the conquest is just one instance of this.

Refutation: There is (I think) a connection between the Da Huo dating of Tang-shu’s appointment and the Da Huo locating of the conjunction. (The appointment of Tang-shu in a Da Huo year is probably true; in any case Da Huo was thought in Warring States to be important for Jin and Wei; and this could well explain why a late 4th century BC revision of the *Annals* would put the conjunction in Fang.) But there are detectable changes in the chronology between the beginning of Shang and the beginning of Zhou that are not 4-year back-shifts. Late pre-conquest Zhou dates are shifted back 12 years; Di Xin’s first year, and some other late Shang dates, are shifted back 16 years. (DWP attempts to explain these shifts, but I find the explanations incoherent; for Di Xin, he apparently counts the 4-year shift twice.) There is now a developing consensus that Wu Ding’s succession year was 1250 (the Project Report, DWP and I agree); but in the *Annals* it is 24 years earlier. And in all received chronologies, including the *Annals*, in the fifth generation of Shang kings Yong Ji precedes Tai Wu, but oracle inscriptions have shown that in fact Tai Wu came first (and his supposed 75-year reign is obviously inflated). As for the 496 years from the beginning of Shang to the beginning of Zhou, the matter cannot be as simple as DWP wants, because the *Annals* counts 496 from 1558 to 1062, but Wu Wang’s succession year in the *Annals* was 1061 (1049 in fact). This indicates that the 4-year shift 1554 to 1558 was for some other reason, which forced picking 1062 as the supposed *de jure* first year of Zhou just 496 years later (and also forced seeing 1051 rather than 1050 as in some sense the conquest year, as stated in the end-of-Zhou summary in the *Annals*).

DWP’s assumption that there was a uniform 4-year back-shift in dates after the conquest date works no better. The *Annals* makes 962 (just 100 years after 1062) the first year of Mu Wang’s reign, stating that “from Wu Wang to Mu Wang [Zhou] had ruled for 100 years”; and DWP infers that the actual date for Mu

Wang's first year must have been 958. But 958 does not work as Mu 1, for dating bronze inscriptions that belong in Mu Wang's reign, whether one uses Wang Guowei's system (rejected by DWP) or Li Changhao's system (which both the Project Report and DWP endorse). DWP recognizes 26 years for Kang Wang's reign (I give him 2 + 26); and Kang Wang's first year for DWP is 1003, four years later than the *Annals*' 1007. This works for interpreting the date of the Xiao Yu *ding* (25th year, with a *jiwang* date), i.e., 979 BC. But the "Bi Ming" chapter of the *Shang shu* as quoted in *Han shu* 21B has as a date in Kang's reign, "12th year, 6th month, gengwu (07), *fei*" (*fei* being the day of first visibility of the new moon). The 12th year from 1003 would be 992, but the date has to be 994. Cheng Wang's reign causes even more trouble. In the *Annals* it is 1044–1008, 37 years, being 7 years for the Zhou Gong regency and 30 years thereafter for Cheng Wang's personal rule. DWP following Saussure recognizes 1036 as the last regency year (allowed by dates in the "Shao gao"; better 1031, I think, used by Zhou Wenkang and He Youqi in China). The 4-year shift requires DWP to count the 37 years from 1040; and he wants 1045, for him Mandate 14, to be Wu Wang's death date; so he is forced to say that 1044–43 were mourning years,¹¹ and 1042–41 were minority years, making the regency (for DWP 1042–36) continue inexplicably five years into the years of Cheng's majority, which become all of 37 years (1040–1004) rather than 30. After Mu Wang, there is more trouble: There is a near consensus now that Yih Wang's first year was marked by a dawn solar eclipse in 899; but the *Annals*' first year is 895, four year *late* rather than early.

One is driven to the conclusion that there was no uniform 4-year back-shift. Even if one were to judge that the independently provable 4-year back-shifts have a single explanation, one would have to add that the shift cannot be simply assumed to apply to intervening dates, such as the date of the conquest. So the 4-year back-shift hypothesis does nothing to support DWP's conquest date 1046.

5 The "Feng Bao"

DWP sees another confirmation that the conquest was accomplished in year 13, 1046, in *Yi Zhou shu* 21, "Feng Bao." This chapter recounts an assembly in the Zhou capital of lords from the "nine regions" on day gengzi (37), first (*shuo*) of an unidentified month of "year 23," with Zhou Gong advising the king, who thus

¹¹ It is interesting that here DWP seems to be in effect accepting my 2-*yuan* hypothesis. See the second BSOAS article, p. 510 note 93. (His appeal to the *Annals*' date Cheng 10 for Tang-shu Yu's appointment, really in 1031, as confirming his date for Cheng 1 is a circular argument.)

must be Wu Wang. DWP reads this as describing a situation just after the conquest, and determines that the date, though not naming the month, must be 26 April 1046, a *gengzi* day which did begin a lunar month. He is probably right about the date. But “23rd year” he sees as a garble for “13th year.”¹²

DWP goes further, and argues that the month is the 4th month, Xia calendar, and therefore is the “4th month” in the “Wu Cheng” account of conquest year events, when Wu Wang celebrated victory rites and sacrifices in his capital beginning on day *gengxu* (47), “6 days (inclusive) after *jipangshengpo*,” therefore probably 9 days inclusive after *zaishengpo*, which one might suppose to be *fei*, day of the first appearance of the new moon. Tables indicate that the month was the fifth counting from the solstice month; but DWP suggests that the solstice for the year, on *guimao* (40), in Tung Tso-pin’s tables the 2nd of the month (in Zhang Peiyu’s tables the 1st) could have been incorrectly identified a few days early, the solstice being very difficult to identify exactly by crude observation only.¹³

Refutation: It doesn’t seem to worry DWP that his whole argument depends on a mere “could have been” premise. It should worry him. Analysis of the system of lunar lodges shows that both forms of this system and forms of the system of dividing the solar year into 24 approximately equal solar weather periods existed long before the beginning of Zhou. To make such a system work, there has to be a way to identify a day that will by convention be recognized as the winter solstice day, because that day is the first day of a weather period, and is the first “*zhong-qi*” (*qi*-center) day, to which the whole annual system is keyed. There had to be a simple rule for identifying the winter solstice, that could be used merely to check the continuing accuracy of the ongoing calendar from year to year. The matter simply could not be left to the uncertain process of guessing that today will turn out to have been the shortest day of the year. So we have to try to figure out what that way was. My own hypothesis, which I have found works in problematic cases, is that the Chinese did what I would do myself: They knew from long observation that the year was 365 days, with occasional need for correction by adding an extra day. There were four mid-season-markers, the shortest and longest days and the times when day and night are

¹² There is another possibility, as I have suggested; see my “The Key to the Chronology of the Three Dynasties: The “Modern Text” *Bamboo Annals*,” *Sino-Platonic Papers* (SPP) 93 (January 1999) p. 19, 7.6.3, and p. 30: it is likely that there was a second Di Xin (Shang) calendar beginning in 1068; 1046 would be the 23rd year.

¹³ References to tables by Tung, Zhang: Tung Tso-pin, *Zhongguo nianli zongpu* (*Chronological Tables of Chinese History*), Hong Kong University Press, 1960; Zhang Peiyu, *Zhongguo Xian-Qin shi li biao*, Jinan: Qi Lu Shu She, 1987.

equal. The latter are relatively easy to determine by watching the change in the position of the sun at sunrise. (They too are first days of weather periods, and *zhong-qi* days.) So one divides the year by four, to the nearest whole number, identifies the autumn equinox, and counts off 91 days. In the 11th century BC this would mark a day to be called winter solstice day, to which the system could be pegged, that would be two days *late*. Not early, but late. The actual interval from equinox to solstice was 89 days. (One of the ancient “Six Calendars,” the Huang Di Li, probably in use 305 BC and later, seems to have assumed a winter solstice 91 days after the autumn equinox. See Appendix.)

So I think it is impossible that in the year approximately corresponding to 1046 BC the pre-winter-solstice month was by mistake taken to be the solstice month. If I am right in this, DWP loses more than his suggestion that the “Feng Bao” event was on the first day of the 4th month, i.e., the month when the “Wu Cheng” says Wu Wang celebrated his victory sacrifices in his capital. (This is unlikely for another reason: *Yi Zhou shu* 37, “Shi Fu,” says of these 4th-month events that on *gengxu* (47) Wu Wang “arrived at dawn,” still in his chariot, to begin the sacrifices; apparently he had just gotten back from the east ten days *later* than *gengzi* (37).) If we have to call the month the 3rd Xia-calendar month, then the preceding month, which has to be the 2nd month, ran from *xinwei* (08) through *jihai* (36), and did not contain the victory day *jiazi* (01) at all. DWP’s entire argument for 1046 turns out to depend on his “could have been” premise.

Furthermore, the event described in the “Feng Bao,” and Zhou Gong’s advice to the king, seem to me clearly to require the interpretation that the time is several years *before* the conquest, when the visiting lords are still subject to Shang. DWP translates (*BSOAS* article Part 1, pp. 278–9, note 14): “... The Lords of the Nine Regions all came to Zhou. The King was at Feng. In the morning twilight, the King stood in the Lesser Hall. The King announced to Dan, Duke of Zhou, “*Wuhu!* The various lords have all come to felicitate us. [They have] suffered bitterly in service to Shang. How shall I preserve and keep [their loyalty]? How shall I employ them and send them off.”” “The Lords of the Nine Regions” could just as well be “lords from the Nine Regions.” “[They have] suffered” could better be “They are suffering.” There is no justification for the insertion “[their loyalty],” implying that they are already Zhou subjects. “How shall I employ them and send them off?” with the same implication, renders “*he yong xing,*” which must mean “How should I deport myself?” with the opposite implication—to judge from the Duke’s reply, in which he first gives a propagandistic catalog of the Shang king’s vicious behavior, and then offers counsel on how Wu Wang should behave as he greets the visiting lords, with ritual propriety, humbly, with mild manner and dignity. (The last time Hu Houxuan visited

Stanford, I showed him the text, and asked for his opinion, not telling him why I wanted it. He immediately said that the account had to be pre-conquest, and that “lords from the nine regions” just means lords from everywhere, not implying Zhou control of all China.)

6 Was the *Bamboo Annals* Rewritten in the Jin Dynasty?

DWP’s principal argument to insure his “13th year, i.e., 1046” theory is his argument that the *Bamboo Annals* was rewritten after the recovery of the text in the 3rd century CE, and that before the rewriting it explicitly dated the conquest to the 13th year, of the Mandate calendar. The argument can be treated as having several parts. First is the claim that a rewriting that did all of this occurred after the recovery of the text in the Jin Dynasty, and not before its burial in Wei in the early 3rd century BC. On this, DWP makes no attempt to refute the possibility of a pre-burial rewriting. His argument is that the evolution of Han Dynasty “five phases” cosmology led to the association of the Zhou Dynasty with Da Huo, and that this must explain the incorrect locating of the conjunction of 1059 BC in Fang (in Da Huo).

Refutation: DWP’s knowledge of this aspect of Han intellectual history is impressive and his account of it is very interesting. But there is at least one decisive objection. An essential part of the reworking of the text is the transposition of a slip’s worth of text from the Cheng Wang chronicle to the end of the Wu Wang chronicle, discovered by Shaughnessy. Shaughnessy, like DWP, thinks this was done in the process of reconstructing the text in the Jin Dynasty. But this is impossible: The slip text recounts events that are mentioned in the *Zuo zhuan* and assigned there to Wu Wang’s reign; i.e., the *Zuo zhuan* reflects chronological beliefs that were caused, apparently, by the moving of the slip.

It is true that a revision locating the conjunction in Fang, i.e. in Jupiter station 10, Da Huo, rather than in Chun Shou, station 6, entailed the other major features of the “modern text” for the conquest era that we must now judge to be false. Calling 1059, actually Di Xin 28 (from 1086), a station 10 year rather than a station 6 year meant that four years earlier (1063) was the station 6 year, presumably Di Xin 28, and would thus force a re-dating of Di Xin’s first year back four (to 1090). It would also imply that 1050 was a Chun Huo (station 7) year, and therefore the year of the conquest, if one believed, following the *Guoyu*, that the conquest was in a Chun Huo year. But Wen Wang had nine years after the conjunction; so if the conjunction were left in 1059, Wen died in the year of the conquest, which was impossible. The solution would be to assume that the conjunction was one full 12-year Jupiter cycle earlier, in 1071, with Wen dying in 1062, and Wu conquering in his own year 12. This would also cause the re-

dating of pre-conquest Zhou events back 12, as in the present text; and a further re-dating of Di Xin 1 back 12, for a total of 16 (to 1102), as in the present text. It seems, then, that the relocating of the conjunction in Fang generated (or at least endorsed) the idea that the conquest was in Wu's 12th year, a view that Han historians had abandoned, though it is found in the (pre-Han) *Lü shi Chunqiu*.¹⁴

I have just described someone's thinking. Does it matter whether this was thought out in Warring States Wei, or in post-Han Jin? It does, because this rewriting has to have been done by someone who believed correctly or incorrectly that the conquest was in a Chun Huo year. It is quite possible for someone in the Wei court to have believed this, rewriting accordingly, even though the text he was reworking didn't say it. But it is very unlikely that a person in the post-Han Jin court would have done this unless he both believed it and found his text saying it, because a text recovered after being buried almost six centuries would carry a strong presumption of veracity for a matter as basic as this. But if the recovered text did both put the conjunction in 1059 (correctly located) and also put the conquest in a Chun Huo year, it would have been saying exactly what DWP thinks it said: The conquest was in the 13th year of the Mandate. And in this case, "13th year" wasn't due to a series of Han historiographical errors, but had a pre-imperial authentication.

(There is another argument for my claim that the revision or revisions of the text that produced the chronology as we have it was done in Warring States, most of it ca. 300 BC. This matter is too important to omit this argument, but it is long, and I give it in the appendix.)

7 More Claimed Evidence for "13th Year"

DWP offers several pieces of evidence for his 13th year hypothesis. One can be mentioned and dismissed. He points out that the *Shang shu* chapter "Tai Shi," pretending to record Wu Wang's speech to the troops before his victory, has the explicit date "13th year."¹⁵ DWP here lets his enthusiasm smother his caution, for he must know that the "Tai Shi" as we have it is a Han composition, and we can therefore assume that its author got "13th year" from Liu Xin.

DWP makes a more serious claim in pointing to the "Shi Fu" chapter of *Yi Zhou shu*. He had before him the translation and analysis by Shaughnessy,¹⁶ and

¹⁴ *Lü shi chunqiu* 14, 3rd section, "Shou Shi."

¹⁵ *TP* article, p. 73. DWP further confuses the "Preface" to the "Tai Shi" with the opening sentence of the text; it is the latter that has "13th year"; the "Preface" has "11th year."

¹⁶ Edward L. Shaughnessy, "'New' Evidence on the Zhou Conquest," *EC* 6 (1980–81), pp. 57–79.

Shaughnessy believes that the “Shi Fu” really is the supposedly lost “Wu cheng.” It cannot be that. It is easy to see that it is not an integral text, but is a composition of seven fragments, probably from different sources. Events are mostly dated to the day, apparently ranging from month 1 to 4, and the person putting the text together obviously believed that all of the pieces narrated events (not in strict order) from the beginning of the conquest campaign through the 4th month celebrations in the Zhou capital. The third and sixth sections are the important ones for someone arguing as DWP does. The sixth section begins “At this time, in the 4th month ... day *gengxu* (47) ...,” and thereafter through day *yimao* (52) are accounts of bloody sacrifice after sacrifice, of both captives and animals. The supreme deity is given the Zhou name “Tian,” “Heaven”; the defeated power is “Shang.” In the third section we have days *xinhai* (48) through *yimao* (52), no month named, and a completely different kind of account, no butchery, formal symbolic offering rites with music, the deity diplomatically given the Shang name “Shang-di,” and Shang referred to as “Yin.” In the sixth section “100 evil ministers” of the slain Shang king are “done away with (*fei*)” as the first of a list of human sacrifices on day *gengxu* (47). In the third section, “100 nobles of the Yin king” are “presented” (which need not mean sacrificed; in both cases “100” is probably just a round number, not necessarily the same) on day *guichou* (50); and on day *xinhai* (48) Wu formally presents the captured cauldrons of Shang in the temple. Also in the third section, on day *renzi* (49) he “confirmed the rulers of the states,” i.e., accepted former vassals of Shang as his own vassals.

These are the details that interest DWP, because the *Annals* specifically has these presentations and re-confirmations in year 13. The obvious inference, for him, is that here in the *Annals* is a residue of the original unrevised text, which he claims had the conquest too, and all these subsequent doings, in year 13.

Refutation: I would argue that the differences between the third and sixth sections of the “Shi Fu” show that the former is not an account of events in the fourth month of the conquest year at all. Only a month and a half can have elapsed between the Muye victory and the 4th month sacrifices. This would not be time enough to get the massive Shang cauldrons back across a now swiftly flowing Yellow River to the Zhou capital, nor time enough to organize an assembly of Shang subject lords in Zhou (for peripheral fighting was still going on). So that third section must be describing events in the next year, when the

warfare had ceased, and the primary Zhou task would be to present a diplomatic peaceful posture.¹⁷

Further, the opening short section of the “Shi Fu” is a single sentence: “Fourth month, day *yiwei* (32). Wu Wang consummated his rule over the Four Regions [of the world], extending it throughout the countries that Yin had commanded.” It is intended as a summary of the result of all of the subsequent action. (Compare, for example, the account of the seventh year of Zhou Gong’s regency in the *Annals*: it begins, “Zhou Gong returned the government to the king.” But this is the final result of the events of the whole year.) And as Shaughnessy points out (in his note 2) the 4th month began with day *yiwei* (32), in his (and at that time my) analysis of the year, taken to be 1045. This remains true for an analysis of the conquest year if it is taken to be 1040, and obviously is strong evidence that the conquest year must be either 1045 or 1040. DWP, not paying attention to this, argues that “*yiwei*” is a garble for “*yimao* (52),” and that the line has been displaced from the end of the third section. He cannot allow it to stand where it is and remain “*yiwei*,” because he has to argue (analyzing the “Feng Bao”) that the first day of this 4th month is *gengzi* (37) (so that the 4th month couldn’t contain *yiwei* at all). Thus DWP has to posit a deformation in a text that strongly supports his opponents’ theories, in order to make it at least not inconsistent with his own theory, with no justification.

8 The “Da Kuang” Argument

One more DWP “proof” that the conquest year in the original *Annals* was “year 13”: *Yi Zhou shu* 38 “Da Kuang,” begins as follows, as translated by Shaughnessy: “It was the thirteenth year. The king was at Guan. Guan shu himself became the Yin overseer. All of the archer-lords of the eastern domain received rewards from the king”¹⁸ DWP follows this interpretation. But the *Annals* explicitly has the appointment of Zhou overseers of Yin in year 12. Therefore (claims DWP) we here have decisive evidence that the present text is a falsification, and has introduced a “12th year” date for the victory and the appointments, which originally were in the “13th year.”

¹⁷ In 1981–83 Shaughnessy and I both argued that the 3rd section events occurred in a temporary Zhou court in Shang soon after the victory. DWP is correct in refuting this interpretation as misinterpretation of the text. I have given it up for another reason: I now accept the relative spacing of events as given in the *Shiji*, “Zhou Benji,” and this does not allow enough time for an earlier set of rites, between victory in Shang and rites back in Zhou.

¹⁸ E. L. Shaughnessy, “On the Authenticity of the Bamboo Annals,” *Harvard Journal of Asiatic Studies* (HJAS) 46 (1986), p. 159.

Refutation: I do not argue that the present text “12th year” for the conquest is correct, but I do argue that it was the date given in the text as discovered. Shaughnessy’s translation of the opening of the “Da Kuang” is incorrect: it does not say that Guan-shu was appointed overseer in year 13. Why, one must ask, the words “himself became” (Shaughnessy’s rendering of “*zi zuo*”)? Were we supposed to be surprised that someone else didn’t get this appointment as “Yin overseer”? Actually there were at least two overseers appointed, not just one, and one would expect all to have been appointed at once. Guan-shu was present because the reception was in Guan; and “*zi zuo*” does not mean “himself became.” The meaning is “in person functioned as,” i.e., in the ceremony. This is a common meaning of the word “*zuo*” even today (e.g., “*zuo guan*,” “be an official”), and examples are easily found in old Chinese.

The occasion, in brief, was ceremonial, and the personal participation of Guan-shu is mentioned so as to emphasize its formal importance. The actual implication is that Guan-shu was appointed much earlier. If this were a description of his appointment, the words would have been “*ming zuo*,” “was commanded to function as.” (For an example, see the *Hai ding*: “*Ming ru zuo ...*,” “I command you to perform the duties of ...,” i.e., “I appoint you to the office of ...”)¹⁹

9 DWP’s Defensive Arguments

The foregoing (2 through 8) were DWP’s positive arguments for his thesis (all of them invalid). He also devotes much space to what is essentially defensive argument, against two fundamental objections. One of these is that lunar aspect terms in day dates in accounts of the conquest campaign and 4th month are not satisfied by the year 1046. The other is my theory that normally in Zhou there was a two-year delay in the issuing of the official calendar for a new king (the calendar that gives the reign length recognized in the *Annals*), to accommodate completion of mourning, in effect giving a king two “first (*yuan*) years” on which inscription dates could be counted.²⁰ The interpretation of lunar aspect terms and the “two *yuan*” hypothesis are interrelated parts of my argument. First, the latter problem:

(a) The “two *yuan*” theory:

¹⁹ Quoted in Shirakawa Shizuka, *Kimbun tsushaku* 16.84 p. 128.

²⁰ For argument and evidence, see D. S. Nivison, “The Dates of Western Chou,” *HJAS* 43 (1983) pp. 524–531, and E. L. Shaughnessy, *Sources of Western Zhou History* (Berkeley: University of California Press, 1991) pp. 148–155.

This, I have argued, is the explanation of the “7th year” vs. “9th year” confusion about the death of Wen Wang; and the theory further indicates a reconstruction of Western Zhou reign dates that allows the dating of bronze inscriptions.²¹ This chronology includes the date 956 as the correct first year of Mu Wang; and so if the statement in the *Annals* that 100 years passed from Wu Wang to Mu Wang is taken to imply a first year date 100 years before Mu 1, that date must be 1056 as the first year of a calendar. This would explain and confirm statements in two sources, the *Shiji* and the *Shang shu da zhuan*, that Wen Wang died in his 7th year as *de jure* king of China. This DWP sees as incompatible with his “13th year” theory. I also find my two *yuan* hypothesis enables me to use Wang Guowei’s “4-quarters” analysis of lunar phase terms to date inscriptions that belong in the same reign, that otherwise would not be thus datable. To defend 1046, DWP needs to defeat Wang Guowei’s system, and so for this reason too he also must try to defeat my two-*yuan* hypothesis.

I find in DWP no criticism of the two-*yuan* theory strictly on the merits. He always represents it as self-serving, adopted simply in order to let me date inscriptions when I want them, so as to validate my own favored chronology of reigns. One would never learn from his discussion that there is strong evidence for the theory that has nothing to do with interpreting bronze inscription dates. This is in spite of the fact that in *EC* 15, in 1990, responding to his criticisms, I made a major point of objecting that his criticisms had avoided the matter altogether, even though linking the posited Zhou 2-*yuan* institution with the *Annals*’ interregnums between Xia reigns had been the lynch-pin of my argument.²² In the 1992 articles, the matter of the interregnums in the Xia chronicle goes untouched. So also Sima Qian’s cross-checking of the first year of Xuan Wang against the calendars of regional lords, getting the accepted date 827, except in one case, the cross-check against the reigns of the lords of Chen, which implies 825, evidence I had offered in publication in 1983.²³ DWP’s first year for Kang Wang is 1003, gotten from his 4-year back-shift hypothesis, and the date does admit the Xiao Yu *ding* inscription’s date. But the date in the “Bi Ming,” as quot-

²¹ My basic argument (together with Shaughnessy) is that in the *Annals* chronology Mu Wang’s reign has been increased by 16 years, his *yuan* moved back 6 by dropping mourning-completion periods for three preceding kings, and his death moved down 10 by dropping periods for the five following kings who succeeded their own fathers; and that the post-Mu chronology was then further altered (in ways that Shaughnessy and I do not quite agree on).

²² “Response: David S. Nivison,” *EC* 15 (1990) pp. 167–168.

²³ “The Dates of Western Chou,” *HJAS* 43 (1983), p. 527.

ed by Liu Xin, requires Kang Wang's first year to be 1005, a point I had also made in 1983.²⁴ DWP's eyes are closed.

Worse, he says (second 1992 *BSOAS* article, p. 509) that his analysis shows that Wen Wang's reign was just 50 years, 1099–1050, and he repeats the claim on p. 130 of his article in *EC* 20.²⁵ Nowhere has he even tried to show this. The closest he comes to the point is on p. 505 of the *BSOAS* article, doubting an "original note" (which he misdescribes as "Shen Yue's original comment") identifying "Wen Ding 12" as "the first year of Zhou Wen Wang." ("Original" notes and Shen Yue notes are clearly distinguished in the *Annals*.) The note has no basis, he suggests, because the *Annals* text for the year contains only a phoenix augury, which (DWP claims) "was no doubt moved to the present location during reconstruction of the *Bamboo Annals*," belonging instead with the Zhou-heralding conjunction. But in that same place DWP explicitly (perhaps accidentally) refers to the year as the year of Wen's "accession." Further, the death date given in the *Annals* for Wen's father in the preceding year goes unchallenged, and simple subtraction of Wen's own death date (a central figure in DWP's argument) tells us that Wen's tenure is represented in the *Annals* as being 52 years, not 50. The fact that one can demonstrate both a 52-year reign 1101–1050 for Wen, and also a 50-year reign 1099–1050 for him, is one of my strongest pieces of evidence. (There is more evidence for the 52-year figure in my monograph in *SPP* 93 (1999) drawn from *Lü shi chungqiu*; see p. 4 note 3 in that monograph.) In 1981-2 DWP himself had given me one of my most interesting bits of evidence for the two-year mourning interval, a quote from *Gongyang zhuan* about Eastern Zhou kings postponing claim to full kingship until after completing mourning.²⁶ In 1992, it seems, he has quite forgotten the matter.

As an application of the "two *yuan*" theory, it was and is my hypothesis that there were two first years for Wen after the conjunction of 1059, a Mandate first year (1058) and two years later a royal calendar first year (1056).²⁷ I find some-

²⁴ *Ibid.* p. 526.

²⁵ *EC* 20, p. 130, note 10.

²⁶ Re-quoted in my *HJAS* 43 article (1983) p. 529, where I acknowledge DWP's help. I do not find any effect of this on royal calendars of record in Eastern Zhou.

²⁷ I give the reasons in *HJAS* 43, p. 530–531. Especially pertinent is the account in *Shiji* "Lu Shijia" (which I quote, p. 531): When Bo Qin (Zhou Gong's son) was first enfeoffed in Lu, "it was three years before he reported to Zhou Gong. Zhou Gong asked, 'Why did you take so long?' Bo Qin replied, 'When one changes (the people's) customs and alters their rituals, it is only after a three years' mourning period that they can put aside (the old forms). That is why I was late.'" I.e., as Confucius later insisted, a man must not change from his dead father's ways during

thing very strange in DWP's rejection of this hypothesis. The issue is whether the "9th year" date and the "7th year" date for Wen Wang's death are both valid, as I think, or the former is valid (certainly true) but the latter an error, as DWP believes. The most interesting discussion is in note 50, p. 499 of the second *BSOAS* article. There DWP asserts incorrectly that the 7th year date for Wen's death derives from the *Shiji*, "Zhou benji," only. He adds that it is due there to an error in "the historian's own presentation of the relative chronology and not from a variant 'royal calendar,'" because Sima Qian dates the Zhou campaign against the Quan Yi (or Kun Yi) 2 years after Wen's receipt of the Mandate, and also six years before Wen's death, whereas the *Shang shu da zhuan* dates it to the 4th year of the Mandate, which would be six years before his death if he died in the 9th year. "It appears, therefore, that Sima Qian was basically correct about the approximate timing of king Wen's death relative to the Quan Yi campaign, but not about the timing of that campaign relative to the conquest." (DWP does not notice that in the *Annals* there are two Quan Yi episodes, in Mandate years 2 and 4 in effect, a detail that probably has something to do with this confusion.) The argument, if one can understand it at all, is circular: to accept it we have to assume with DWP that there was just one "first year" date; but that is the point at issue.

More curious: the reference given for the detail that the *Shang shu da zhuan* dates the Quan Yi campaign to year 4 is to "table 4" in part 1 of the *BSOAS* set. One finds this on p. 294, and as promised it shows the Quan Yi campaign in Mandate 4, citing *Shang shu da zhuan*. But it also dates Wen's death to Mandate 9, as expected, and there the citations are *Yi Zhou shu* and *Han shu*, fair enough; *but also Shang shu da zhuan*. DWP does not tell us that actually the *Shang shu da zhuan*, in the same short list of events, dates Wen's death to year 7, and *not* to year 9. I suppose he was thinking that as he sees it the *Shang shu da zhuan*'s putting the Quan Yi campaign in year 4 is consistent with the 9th year death date not the 7th year, so for him in effect it supports the 9th year date. But in the way he presents the matter he suppresses a crucial detail that completely destroys his argument. If "death in year 7" is in *both* the *Shiji* and the *Shang shu da zhuan*, then it is not due to a mistake by Sima Qian. On the contrary, since the two texts are otherwise not the same, they must have been using different sources, both of which *agreed* on "7th year." The date "7th year" therefore has to be reckoned with, and cannot be brushed aside. This matter is not trivial. The identification of 1056 as Zhou year 1 is basic to my reconstruction of later chro-

mourning (*Lun yu* 1.11, 4.20); this would include using a new ruler's calendar. So the calendar must wait until all mourning obligations among the people had expired.

nology in fundamental ways in which it clashes with DWP's. And it is central to the argument that Shaughnessy and I have urged against him, that some of the dates for the conquest found in ancient texts, notably the *Shiji* "Zhou benji," are based on 1056 as *yuan* date, and not on 1058. DWP does not hesitate to call this claim an "argument from expediency" (p. 499 top).

DWP is wrong in claiming that the *Yi Zhou shu* dates are all consistent one with another (p. 498; I assume that he would limit this claim to the "core" chapters). I have shown that #38 "Da Kuang" dated "13th year" presupposes (incorrectly) 12th year, as conquest date (1045, counting from 1056), not 13th year (1046, counting from 1058); and this must also be true of #39 "Wen Zheng." We learn nothing from them as to which calendar this is, Wu Wang's or a continuing one. #23, #25, #29, #45 and #48 are consistent (and correct): #23 "Xiao Kai" implies 1099 as first year of a Wen Wang calendar, but in no way blocks the possibility that this is not Wen's succession year. #25 "Wen Chuan" assumes 1058 as Mandate first year and indicates 1050 as the year of Wen's death. I will show that #29 "Bao Dian" and #45 "Wu Jing" together require 1049 as succession year for Wu Wang in Zhou, and 1040 as conquest year, thus being inconsistent with "Da Kuang" and "Wen Zheng." It is possible that #37 "Shi Fu" as originally written presupposed either 1045 or 1040 as conquest date. But the day-dates in Zhu Youceng's text assume either Liu Xin's (incorrect) calendar for 1122 as conquest year, or the Yin Li's (also incorrect) date 1070 (as DWP himself argues); so the "Shi Fu" is not consistent with the others. As for #48 "Zuo Luo," I see it as implying that Wu Wang died in the third year counting from the conquest (agreeing with #29), because #38 and #39, though having an incorrect date, do imply that Wu Wang was still engaged in the east in the year after the conquest. The "Bao Dian" and "Wu Jing," although they are related in that the latter mentions the former, seem to have been written at different times perhaps centuries apart. I would not quite call the book a grab bag, but these details point to composition of the so-called "core" chapters by at least four persons, contrary to Shaughnessy's opinion in *Early Chinese Texts*.²⁸ Nothing in *Yi Zhou shu* supports DWP's conquest date 1046.

(b) Lunar phase dates:

DWP's efforts to prove Wang Guowei (and me) wrong occupy much of his article in *T'oung Pao*. To this end, he quotes several pages from the work of a Chinese historian of astronomy, Li Changhao, published in 1981, arguing for a "2-

²⁸ Michael Loewe, editor, *Early Chinese Texts*, Berkeley: University of California Press, 1993, p. 230.

halves” analysis of the lunar terms. (The Project Report agrees; see section 3.5.1 of the Draft Report, or pp. 35–36 of the published Report.) I.e., DWP holds, with Li Changhao, that the terms *jishengpo* and *jisipo*, which for Wang refer to the second and fourth quarters, can be used in dates in the first and second halves of the lunar month. This argument by DWP is question-begging, because Li did not know about my two-*yuan* theory and so could not have weighed it in his reasoning. Therefore one ought to be impressed with DWP’s use of Li *against me* only if one is already convinced that my theory does not merit examination. This is true whatever Li may think now; if he now were to dismiss my theory, we need to be told why, and then we have a new argument, not the old one.

DWP also presses against me the criticism of Asahara Tatsuro, who objects that in adopting my two-*yuan* theory, together with my allowing for the possibility of intercalation, and for the possibility of annual calendars starting in different months (solstice, post-solstice, pre-solstice), etc., I give myself too much latitude in interpreting lunar phase dates for dating inscriptions. This objection I find confusing. A “hypothesis” so framed that in principle it could never be falsified is not an empirical hypothesis at all; likewise a “hypothesis” that could never be satisfied. But short of these extremes, the only question to ask is whether one’s hypothesis is true. Would Asahara (and DWP) be better pleased by a hypothesis so strict that one could *almost* never satisfy it?

Perhaps as extra insurance, DWP makes other points against objections based on lunar phase dates. (1) He argues against my claim that the conquest date problem and the whole problem of Western Zhou chronology involving bronze inscriptions and lunar dates are interrelated in such a way that there must be a comprehensive solution for both at once. (2) He argues (vigorously) that I engage in circular reasoning. I will consider these objections after dealing with two more: (3) He tries to show that struggling with bronze inscription dates is hopeless (with a series of examples selected for that purpose); this, presumably, excuses him from testing Li Changhao’s interpretations in inscriptions that would cause him trouble (such as the *Shanfu Shan ding*). (4) He is convinced, and tries to convince us, that lunar phenomena are too variable to support a precise system of dating, never reflecting that the application of lunar terms might be rule-governed, not depending on constant observation. Consistently with this, he accepts without question the common mistake that intra-year intercalation (which has to apply a simple rule involving weather periods and *qi*-centers) was unknown before Eastern Zhou, and more than once shows that he doesn’t understand what a *qi*-center is (though perhaps he has learned this by

now).²⁹ The whole interrelated set of calendar concepts was rule-constituted; e.g., a *shuo*, being invisible, had to be identified by a counting rule; among the ancient “Six Calendars,” the Huang Di Li apparently followed the rule of identifying the winter solstice by counting a fourth of the solar year from the autumn equinox. All of this is foreign to DWP’s thinking, so naturally he cannot imagine that a lunar phase system too could be rule-governed and not dependent on vagaries of observation.

An example from his long discussion of “Inscriptional Evidence” (*TP* article pp. 49–58) may be interesting. He quotes the *Jing gui*: “In the sixth month, the King was at P’ang Ching, on day *ting-mao* (4) the King ordered Ching ... coming to the eighth month, *ch’u chi*, on day *keng-yin* (27)...” DWP notes the surface meaning: two dates two months apart yet separated by 83 days. “This would seem to indicate that one month had to have been intercalated between the two named, thus suggesting the unprecedented occurrence of intra-year intercalation during the Western Chou period.” (Does “unprecedented” mean that there had never been a case of intra-year intercalation before this? Or that no scholar had ever dared to suggest it?) To drive home the point that the problem is hopeless, he then cites another vessel in the *Jing* group, the *Xiao-chen Jing yi*, “which has a “thirteenth month” date—conclusive evidence of inter-year intercalation that confirms what most scholars believe to have been the practice....”

I was once roundly scolded in public by Nathan Sivin for daring to suggest that intra-year intercalation might have been a very old practice, done in Western Zhou or even in Shang. I make no secret of my preferring to do my own thinking rather than to reverence “authorities” in scholarship. It seems obvious to me that the system of the ideal division of the solar year into twelfths, each divided in two by a *qi*-center, is and always has been an ideal model against which the actual succession of lunar months is measured, to see when there is need for an extra lunar month, a need that can occur at any time of year; and that when you find evidence of one part of this system, you can infer that all of it was present. And I have shown that the system of dividing the year equally in this way existed already at the beginning of Shang.³⁰ Further, I have shown that this system was used, for intra-year intercalation, at least as early as late

²⁹ See DWP’s *TP* article, pp. 43 (note 18), 71; he thinks a *qi*-center is the middle of a *lunar* month. A *qi*-center is the middle of a *solar* month, i.e., a pair of weather periods (*qijie*) constituting a twelfth of the solar year, the year being divided so that the (approximate) solstices and equinoxes count as four of the twelve *qi*-centers. A lunar month is intercalary just in case it does *not* contain a *qi*-center.

³⁰ D. S. Nivison, “The Origin of the Chinese Lunar Lodge System,” in A. F. Aveni, editor, *World Archaeoastronomy*, Cambridge: Cambridge University Press, 1989, pp. 203–218.

Shang.³¹ The occasional practice of adding a 13th month at the end of the year proves nothing to the contrary. If an intercalation is missed, one doesn't arbitrarily insert an extra one in the middle of a year; one adds it at the end. So also, if for some administrative reason one wants to shift the first month forward, e.g., making the year start with the *yin* month rather than with the *zi* month; in that case one would add a 13th and a 14th month at the *end* of the year. What could be more obvious?

In the case of the two Jing vessels that DWP adduces to show that working with inscription dates is hopeless, they simply show an instance of intra-year intercalation in Mu Wang's reign (for they are obviously Mu Wang era vessels and inscriptions). The date of the first one has to be 941 BC. I leave the proof as an exercise.

Meanwhile, here is some work for DWP: On p. 53 of his *TP* article he quotes Li Changhao discussing the *Qiu Wei gui* and other vessels in the Qiu Wei group. He and a number of other scholars would not put the latter in Gong Wang's reign, in spite of the fact that one of them names Gong Wang. The only conceivable alternative is to put the first in Gong's reign and the others in Yih Wang's reign, for the *gui* is obviously Mu style, and could not be later than Gong. But there is no conceivable justification, for anyone taking the *Bamboo Annals* at all seriously, for supposing that Gong Wang had a reign long enough for that: the *Qiu Wei gui*'s date is 27th year, 3rd month, *jishengpo*, day *wuxu* (35). DWP (note 40) says that I am "virtually alone" in assigning it to Mu Wang. Really? Shaughnessy (*Sources*, p. 110) takes it as a Mu Wang standard; and the excavation report has no doubt that it is Mu Wang.³² Li Xueqin puts it later, but Li does not give the *Annals* a moment's thought, and dates the *Qiu Wei gui* later only because he accepts the traditional 55 years for Mu Wang, and sees (correctly) that on that assumption it would be too far removed from the other Wei vessels.³³ DWP does look at the *Annals*, and he does not accept 55 years as Mu's reign; I suspect he may give Mu a reign even shorter than my assignment (2 + 37 years). So he has no excuse to refuse even to test its date in a Mu calendar. He would find that his *yuan* (958) for Mu doesn't work, either with Wang Guowei's system of phase dates or with Li Changhao's. Here is reason enough for DWP—whatever others may think—to reject Li's system. One must count the cost of one's assumptions, all of them, all the time. If one does, one is in this case pushed inexorably toward my 2-*yuan* hypothesis—which becomes the only way to get the

31 *SPP* 93, pp. 27–29.

32 See Shirakawa Shizuka, *Kimbun Tsushaku*, 49 ho 11 p. 275.

33 Li Xueqin, *Xin chu qingtongqi yanjiu* (Beijing: Wenwu chubanshe, 1990), p. 92.

other Qiu Wei vessels and the *Que Cao ding* (which also mentions Gong Wang) into the Gong Wang reign.

DWP's first thrust (1) takes the form of pointing out that Chou Fa-kao and I had agreed on the same conquest date—1045—while differing almost completely on dates of Western Zhou reigns and dates of inscriptions. He concludes that our own performance shows that the conquest date problem is an independent one and can be solved independently. There is something to be said for this. (In a moment I am going to solve the problem independently.) But one's solution is going to remain doubtful if there remains a suspicion that it is inconsistent with the rest of the historical story. DWP himself in effect admits this, in his evident concern lest the "4-quarters" analysis of lunar phase dates, applied to the instance of those dates in accounts of the conquest, turns out to be right. DWP's objection also overlooks the fact—which he and I now agree on—that Chou and I didn't have the right conquest date anyway: we hadn't solved the problem.

The issue of circularity (2) is more interesting. I think that DWP doesn't grasp the difference between a circular argument and an appeal, in argument, to coherence. (Shaughnessy commits the same error.)³⁴ A circular argument is one that appears to have force only because you have in effect assumed its conclusion in advance. (I have pointed to some of DWP's, and could point out more.) But a perfectly good non-circular argument can have the form of showing that several antecedently somewhat doubtful premises fit together in a convincingly coherent way. My argument for (a) the 4-quarters interpretation of phase terms, (b) the 2-*yuan* hypothesis, and (c) the date 956 for Mu 1 (and related dates) is like this. Such an argument becomes rightly more convincing if there is independent evidence for parts of it. (E.g., Wen Wang's dates, as evidence for the 2-*yuan* theory.) Or if it allows an inference that is independently verified. (Implies 1005 as Kang Wang's succession year, borne out by the date in the "Bi Ming.") To a person who doesn't grasp the "coherence" concept to begin with, and who consistently ignores or suppresses the independent evidence, such an argument is bound to appear circular—"an autonomous feedback loop"—bringing out DWP's undeniable gifts as a prose stylist.

10 The Date of the Conquest

With more justification, DWP insists that the accounts of conquest year events using lunar terms, such as that in the "Wu Cheng," are sufficiently doubtful (and multiple) that the date of the conquest must be established by other

³⁴ E. L. Shaughnessy, "On the Authenticity of the Bamboo Annals," *HJAS* 46 (1986), p. 150.

means, before we can decide whether (e.g.) the “Wu Cheng” account is authentic.

This is a reasonable request, to which I now attend. I will show (1) that the account in the *Shiji* “Zhou benji” using Xia-calendar dates (without lunar terms) is right, except that Sima Qian was mistaken in reading them all in the “11th” year; (2) that the actual years, if one were to assume a continuing calendar, must be the 16th–17th; (3) that the victory day was 18 April 1040; and (4) that this solution makes superior historical sense. Then I will see what must be said about lunar phase dates.

Sima Qian says that the Zhou army crossed the Yellow River on day *wuwu* (55) of the 12th month of the 11th year. His mistake was a subtle one, standard in Han: he understood “12th month” in its Xia-calendar sense, as the *name* of the post-solstice month; we might translate it “Duodecember.” That’s right; but then, since he thought that the post-solstice month was the 2nd month in the Zhou calendar, he reasoned that the date, “Duodecember,”—which he didn’t invent: he was following some source—was the 2nd month of *Zhou year 11*. “11th year” is wrong anyway; it results from assuming the continuing calendar from 1056, and taking Zhou Gong’s regency as the 7 years preceding Cheng Wang’s post-mourning 30-year reign 1035–1006, instead of the first 7 of Cheng’s 2+30 years 1037–36, 1035–06. But we must forgive Sima Qian that, because that error was probably introduced in the late 5th century BC, and has been accepted ever since.

The more confusing error he made was to suppose that an old date reading “12th month 11th year” just *meant* “2nd month 11th year” if one is talking Zhou rather than Xia. That is, he took the Xia names of the months just as *names*, so that he could stop thinking of “11th year” as “*Xia* 11th year,” as long as his source did not explicitly say that the next date he gives, “[Xia] 2nd month,” was in the 12th year. The actual meaning was “Xia 11th year 11th–12th months, Xia 12th year 1st–2nd months” = “Zhou 12th year 1st–2nd–3rd–4th months.” The Xia-Shang-Zhou Project Report makes the same mistake, for it gets its date 1046 by counting 11 from Wen Wang’s Mandate year 1 of 7, after proving (correctly) that Wen Wang died in 1050. And DWP, by following Liu Xin in correcting “11th year” to “13th year,” also indirectly makes the same mistake. He differs from the Project Report only by rejecting “7th year” for Wen’s death, and “11th year” for the conquest, as errors by Sima Qian, who, he thinks, should have been counting off 13, not 11, from Wen’s Mandate year 1 of 9, rather than 1 of 7. DWP does this because he knows the date of the Mandate-conferring conjunction, from his study of the *Bamboo Annals* in our Stanford seminar of 1980–81. The Project Report doesn’t recognize

the value of the *Annals*, so it blindly follows Sima Qian. Both the Project Report and DWP are wrong, ultimately for the same reason.

Since the Yellow River crossing was on *wuwu* (55) of (for Sima Qian's source) the Zhou 2nd month of 1045, the campaign has to have started in the preceding month, the Zhou 1st month, with the victory on *jiazi* of the Zhou 4th month. *Jiazi* in the Zhou 4th month of 1045 was almost at the end of the month. This remains true also for 1040. The distribution of day *ganzhi* in months differs sharply from one year to the next but differs little in dates five years apart. (This is why it took me seven years to make sure that the conquest date was 1040 rather than 1045, and why Shaughnessy is still not convinced.)

Why 1040? The date in *Yi Zhou shu* “Bao Dian” is “the *king's* 3rd year 2nd month day *bingchen* (53), first of the month (*shuo*).”³⁵ The only year in Wu Wang's tenure—if we look at the decade and a half after his succession in 1049—that has a *bingchen* day as *shuo*-day of the 2nd month is 1038. This is confirmed by the “Wu Jing” chapter, dated “12th year,” recording Wu Wang's dream foretelling his imminent death, following which Wu ordered that the “Bao Dian” text be sent to his heir Prince Song (to become Cheng Wang). Wu's 12th year counting from his succession year was 1038. If 1038 was both the 12th year of Wu as Lord of the West and 3rd year of Wu as *king*, then 1040 was the *king's* 1st year, the year of the conquest.

(If Wen Wang died in the 7th year, and Wu died in his own 12th year, this being his 3rd year as conqueror, then the conquest if dated in a continuing royal calendar would be in the “17th year,” seemingly anomalous if he died in the “12th year.” When Zhou Gong's regency was re-dated 5 years early, 7 years preceding Cheng Wang's 30 instead of the first 7 of Cheng's 2+30, the conquest date in the continuing calendar became year 12. But year 12 had been the year of his death. So (if one is not keeping track of different calendars) year 17, which had been the year of the conquest, must really be the year of his death—as it is, in the *Bamboo Annals*. This was accomplished by moving that bamboo slip that Shaughnessy discovered: Wu had died two years after the conquest, therefore in year 14 if the conquest was in year 12; adding the slip added three years to 14 making it 17.)

We next have to find out what day was *jiazi* of the Zhou 4th month of 1040. The date is 18 April. In Zhang Peiyu this is the last day of the month. In Tung Tso-pin it is the next-to-last day of that month. Zhang is astronomically correct,

³⁵ *Xin Tang shu* 27A quotes this date, with “1st year” instead of “3rd year” (*yuan*, “1st,” and *san*, “3rd,” are possible garbles either for the other.) There is no year in the possible range of dates, that could be a “1st year,” having a second month beginning with day *bingchen*.

but Tung is probably right, for he allows for regular alternation of long and short months. Either way, we have the day. For confirmation, see my argument in *SPP* 93 p. 8, 4.3.2. *Shijing* Ode #236 “Da Ming” celebrates the glories of the Zhou ancestors, and ends with the victory over Shang. The final line is “*si fa da shang; hui zhao qing ming.*” I translate this “Then he attacked Great Shang; this was in the morning, Qing Ming Day.” If we fix the winter solstice for the year by counting 91 days starting with the autumn equinox, and call the day Dong Zhi, then the first day of Qing Ming in 1040 BC is 18 April.³⁶

For the lunar phase problem, see my table for the first six months of the conquest year on p. 49 in *SPP* 93, applying the Wang Guowei 4-quarters system. The fit is perfect. Thus Wang Guowei’s 4-quarters analysis of phase terms is validated. As a corollary so also is my 2-*yuan* hypothesis: it leads to the deduction of *yuan* dates for Zhou kings that are right if and only if the 4-quarters system of phase dates is right. Try it out on the Shanfu Shan *ding*, 37th year, thus necessarily in Xuan Wang’s reign, but dated in 789, not 791. (Li Changhao would not allow 789, and his system doesn’t work for 791.)³⁷

This solution to the conquest year sequence makes historical sense lacking in our earlier attempts, back in 1979–1982. All of us had accepted the “Wu Cheng” sequence, that puts the victory only about a month after Wu Wang started his march, and only six days after he got his armies across the Yellow River. The concept was that Wu made a lightning strike, knocking out the main Shang force perhaps before it could be fully assembled. (Was the last six days possible? We fantasized: Perhaps we could persuade the PRC government to let us back-pack from Mengjin to Muye, to see how long it would take. We were younger then.) Now, however, it seems that Wu made a “ford-head” across the River quickly, and then *delayed* over two months, only about 150 miles from the Shang capital. Why?

It’s the wrong question. If victory day was both (1) *jiazi*, and (2) Qing Ming Day (the major annual event in the ancestor cult), it must have been selected

36 For other proofs of 1040 as conquest date, see Ni Dewei (D. S. Nivison), “Wu Wang ke Shang zhi riqi,” in Beijing Shifan Daxue, Guoxue Yanjiusuo, compilers, *Wu Wang ke Shang zhi nian yanjiu*, Beijing: Beijing Shifan Daxue Chubanshe (1997) pp. 513–532.

37 Li Xueqin (“*Shanfu Shan ding* nian shi queding,” *Wenwu* 1999.6 pp. 54–56 argues this cauldron must be in the last year of Li Wang (taken as the same as the first year of Gong He), validating the reign length implied in the *Shiji*, “Zhou Benji.” (which is inconsistent with the “Wei Shijia” and also the “Qi Shijia”). One cannot consider this possible if one holds the view (shared by me, Shaughnessy and I think also DWP) that hypothetical dates for Western Zhou reigns must be reconciled with *Annals* dates by finding some reasonable explanation of how the *Annals* dates could have been derived from the real ones.

long in advance, perhaps years. So why did Wu start out so *soon*? And what could have been the strategy of waiting? He started out when he did because he had to secure the crossing, one of the most dangerous parts of the plan, during maximum low water, in late winter (actually 11 February; East Asia has monsoon seasonal rainfall). The wait could be useful: giving allies time to gather, and tempting the Shang to draw forces out of the east, where its own control probably was doubtful. That part of the strategy failed: Shang probably had an army in the east under Lu Fu, heir-king-designate Wu Geng, who was not captured at Muye (or the “Shi Fu” would have said so, and he almost certainly would have perished as a sacrifice). Consequently the Muye victory was not decisive. Historians are pleased to suppose that Wu magnanimously accepted Lu Fu as nominal Shang king to continue the Shang ancestral sacrifices. I don’t believe this for a minute. Wu made a deal with Lu Fu, because he had to. The *Annals* describes it, mysteriously: “*fen tian zhi ming*,” Wu Wang “divided Heaven’s brightness (*ming*)” with him. Euphemistic language, if not bowdlerized. Wu Wang must have *divided Heaven’s Mandate* (*ming*) with Lu Fu, i.e., each would recognize the other as Son of Heaven. It’s no wonder there was consternation in Zhou when Wu suddenly died two years later.

After this unfortunately very negative appraisal of DWP’s work on the *Bamboo Annals* and the date of the conquest, I want to close by pointing to other work by him that is excellent. His “The Metempsychosis in the Moon” (*Bulletin of the Museum of Far Eastern Antiquities* (BMFEA) 58 (1986) pp. 149–159) is a skilful and fascinating study of a strange problem in philological and intellectual history, the reversal of meaning, over several centuries, of the word “*po*,” from the lighted to the unlit part of the lunar disk (marred only by DWP’s weird desire to persuade the reader that the Chinese could not even have conceived of a lunar quarter until after they came to realize that the moon shines by reflected sunlight). His short article in *Early China* 9–10 (1983–85), “*Mozi* and the Dates of Xia, Shang, and Zhou: A Research Note” (pp. 175–181), brief though it is, is one of the most important pieces of work in the history of modern scholarship; and if DWP had never done anything else, that article alone would guarantee him a respected place in that history. In it he caps (and confirms) his earlier achievement of dating the beginning of Shang (date 1554, which is 496 years before the Zhou Mandate year 1058), by actually dating the beginning of Xia (date 1953, coincident with the five-planet conjunction in Ying Shi, and the 14th year of

Shun) with a tight astronomical and textual argument.³⁸ He has, understandably, hoped for a perfect score for the Three Dynasties. He doesn't have it. But two out of three isn't bad.

38 Pankenier's dates and arguments are ignored by the Three Dynasties Project. In both cases, Pankenier is skillfully combining astronomical analysis with analysis of the "modern text" *Bamboo Annals*.


16 Huang Di to Zhi Bo: A Problem in Historical Epistemology

Abstract: I was invited to a conference in Beijing scheduled for October 2003, on “Xia-Shang-Zhou chronology.” (Background: a PRC-sponsored five-year Project to ascertain Three Dynasties dates had been about to publish its “final” results, but in August 2002 the Project’s “Committee of Experts” unexpectedly objected, and decided to invite open criticism through an international conference.) The SARS epidemic has caused an indefinite postponement. I am now making available on request the paper that I had already prepared.

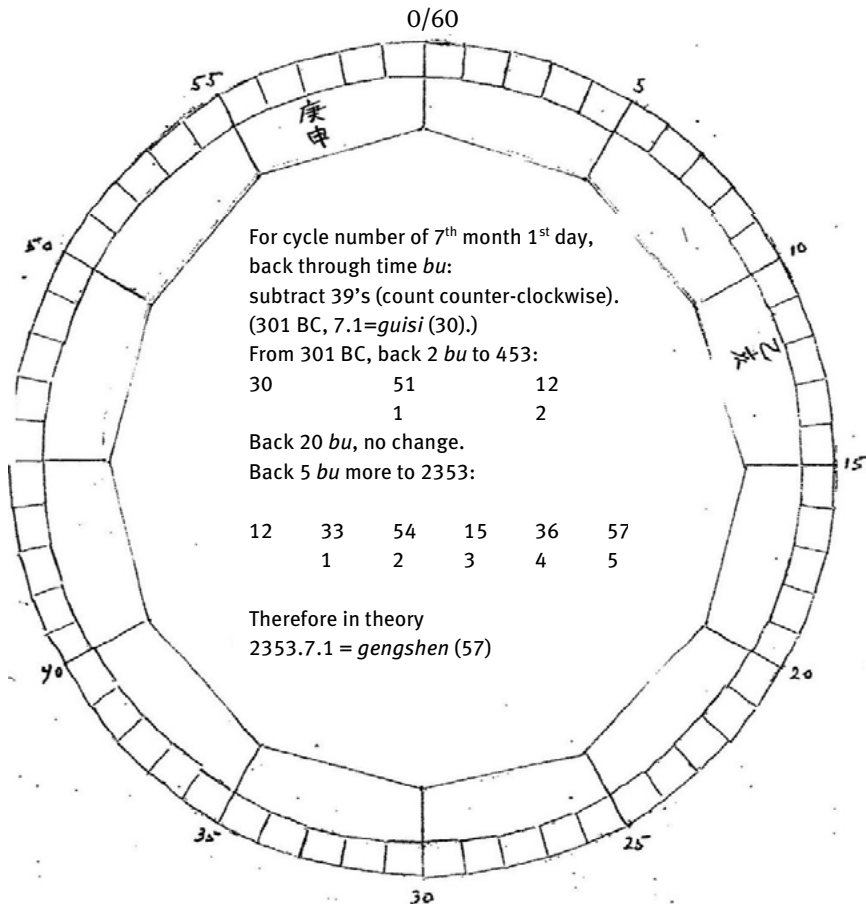
At our WBAOS (Western Branch of the American Oriental Society) meeting, I want to amplify one key argument in the paper. The *Bamboo Annals* was a creation of the ancient state of Wei around 300 BC. Combining information from both the disputed “Modern” Text and supposedly authentic “Ancient” Text, I deduce 2402 BC as the intended first year of Huang Di. In his 50th year we read “7th month day *gengshen* (57), phoenixes came, and the emperor sacrificed by the Luo River.” The year has to be 2353, which is 100 *zhang* (1900 years) before 453, the date of the victory over Zhi Bo that created Wei. It is impossible that this is a record, and unlikely that it is an “off the wall” invention. If it was calculated to celebrate Wei’s beginnings, the intended date is probably the first of the month.

By using the ancient *zhang* system I have discovered what I think must be the calculation, which I will explain carefully. It seems to me that this establishes the authenticity of the disputed “Modern” Text beyond reasonable doubt. I often use arguments of this kind. I have found that some scholars find these arguments immediately compelling, but others are quite indifferent to them, or even seem baffled that I should offer them as arguments at all. I want to engage the audience in this problem, and discover if I can why there such different reactions.

Paper presented at the annual meeting of the Western Branch of the American Oriental Society, October 10–12, 2003

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“Huang Di 50, 7th month [first day], *gengshen* (57)”
 Why is it *gengshen*?



This diagram is merely for the reader’s convenience. Is my whole argument a good one? Most historians seem to pay no attention to me when I use arguments like this; or else they scoff. (For example, consider the Nivison-Pang article in *Early China* 15 (1990).) Should they pay attention? Or should I just stop it? Why? If I am right, one must look at ancient Chinese history anew. The three years’ mourning is prehistoric. Jie is imaginary, Yao is real, Shen and Yi Yin were villains, Wu Wang was not a conqueror. It matters.

Current Text *Bamboo Annals*, Huang Di 50: “Autumn, 7th month, day *gengshen* (57): Phoenixes came; The Emperor sacrificed at the Luo River...” Why *gengshen*???

Current text <i>Bamboo Annals</i>	Other sources (*Ancient Text <i>Bamboo Annals</i>)
2145 Yao 1 of 100	2145 (Tao Hongjing, <i>Zhen gao</i>)
(2154) Zhi 1 of 9	1 of 9 (<i>Di wang shiji / Taiping yulan</i>)
(2217) Di Ku 1 of 63	1 of 63 (Luo Bi, <i>Lu shi</i>)
(2295) Zhuan Xu 1 Of 78	1 of 78 (<i>Di wang shiji / Taiping yulan</i>)
Huang Di 1 of 100	*(2302) Zuo Che 1 of 7 (<i>Lu shi</i>)
(2353) Huang Di 50	(2402) (<i>Di wang shiji / Taiping yulan</i>)
minus 1900 (=100 <i>zhang</i> , = 20+5 <i>bu</i>)	"50 th year," etc. (<i>Song shu</i> "Fu Rui Zhi")
=453	*453 Zhao, Han and Wei destroy Zhi Bo
	(<i>Shiji suoyin</i>)

The *Zhang-Bu* System:

1 year = 365 $\frac{1}{4}$ days

1 *zhang* = 19 years, = 6939 $\frac{3}{4}$ days

1 *bu* = 4 *zhang* = 76 years, = 27759 days, = 462 cycles of 60 days, r.39

20 *bu* = 1 *ji* = 1520 years = 9253 cycles, no remainder

5 *bu* = 380 years, = 2313 cycles, r. 15

Solution to the *gengshen* problem: Assume that the *zhang-bu* system was being used, that the day of the month intended is most probably the first day, and that the *Bamboo Annals* text was being finalized around 300 BC.

2 *bu* after 453 BC = 301: The *Bamboo Annals*' last date is 299

301, 1st of 7th month = *guisi* (30)

453.7.1 = 30 minus (2 × 39) = 12, = *yihai*

1 *ji* (20 *bu*) earlier than 453, 7.1 = *yihai* (12)

5 *bu* still farther back = 2353; so 2353.7.1 = 12 minus 15, = *gengshen* (57). Q.E.D.

Significance:

- (1) The deduction above must be Warring States reasoning. Moreover, of the two texts, only the Ancient Text has the Zuo Che interval and the 453 event, which a forger would not have omitted. So the Current Text is not a forgery. Furthermore, the dates in it after 2145 BC have to be dates that make 2145 be Yao 1.
- (2) Thus, the chronology in the Current Text *Bamboo Annals* is probably the earliest chronology available. Its dates before 841 are mostly wrong; but any theory of chronology claimed to be correct must explain these dates. If it can not, that theory must be admitted to be very doubtful.
- (3) The PRC's Xia-Shang-Zhou Project has ignored the Current Text *Bamboo Annals* altogether. The Project's chronology therefore is far too doubtful to justify the claims being made for it.

One can see from the *Bamboo Annals* that the three years mourning institution was prehistoric. *Annals* evidence shows its requirements on kings to have been most severe in the Xia, least severe in Western Zhou, with no signs of impact after that—as implied in DSN’s publications on the *Bamboo Annals* (below). DSN presented a full argument in his H.G. Creel Lecture at the University of Chicago, 12 April 2002 (not yet published).

For the arguments that Jie of Xia was an early Warring States invention required by chronological puzzling, that Yao probably actually lived and reigned (2026–1969, i.e., 58 years), and that Shun and Yi Yin were not moral exemplars but scoundrels, see DSN’s monograph “The Key to the Chronology of the Three dynasties: The “Modern Text” *Bamboo Annals*,” in *Sino-Platonic Papers* 93 (1999). (A revised version of this is available in a Chinese translation by Shao Dongfang: “Sandai Niandaixue zhi Guanjian: “Jinben” Zhushu Jinian” 三代年代學之關鍵: “今本”竹書紀年 in *Jingxue Yanjiu Luncong* 10 (Taipei, 2002), pp.223–309.)

As for the hypothesis that Wu Wang of Zhou did not actually conquer the Shang: DSN’s argument (not yet published in full) is that Wu Wang’s victory at Muye was not decisive. There probably was a powerful Shang army still in the field farther east, under the command of Lu Fu, the son of Di Xin (Zhou Xin, = Shou). Lu Fu had probably been named heir-king by Di Xin, perhaps in 1068 (probably *hou yuan* year for Di Xin, beginning with a *geng* day), at that time being given the royal name “Wu Geng,” by which he was known posthumously. (See DSN’s articles above.) The collage of early narratives about the Zhou victory at Muye and post-victory celebrations in Zongzhou, provided in the “Shi Fu” chapter (#37) of *Yi Zhou shu*, does not mention Lu Fu. If he had been at the scene at the time, he would have been an important part of the story, and almost certainly would also have been killed in the fight or sacrificed afterward.

The *Bamboo Annals* says that Wu Wang “raised” him [to the position of ruler] after the “capture” of Di Xin, and received history has it that this was a magnanimous act by Wu Wang to allow continuation of the Shang ancestral sacrifices. This may indeed have been what Wu Wang told his people back in Zhou. But between the *Annals*’ record of the demise of Di Xin and the statement about the “raising” of Lu Fu, we read this:

Sui fen Tian zhi ming 遂分天之明

Almost all commentators and translators have understood “*ming*” 明 as “*ming ming*” 明命, “glorious Mandate” of Heaven (*Tian*)—supposing that “*ming*” 命 “Mandate” has accidentally dropped out; or perhaps have understood “*Tian ming*” 天明 (literally “Heaven’s intelligence”) as in effect synonymous with 天命. To avoid having the *Annals* say something unthinkable, some propose that “*fen*” 分

should be read as “*ban*” 頒 “proclaim” (or is a slip for “*shou*” 受 “receive,” suggested by the account in the *Shiji*). E. Biot, however (*Journal Asiatique*, May 1842 p. 381) embraces the unthinkable and takes the line as saying “Then he (Wu Wang) *divided* (and shared: 分 “*partagea*”) Heaven’s Mandate,” i.e., he found himself forced to make a deal with Lu Fu, recognizing him as co-ruler with himself. Thus, each in effect was to have the status later termed *tianzi*, “Son of Heaven.” DSN cautiously agrees with Biot. On the other hand (*contra* DSN), both Wang Guowei and the quotation in Li Daoyuan’s *Shui jing zhu* seem to assume a sentence break after “*fen Tian zhi ming*.” The matter needs more discussion.

Outline of Nivison’s paper for Beijing conference originally scheduled Oct. 2003:

“Zai Tan Jinben *Zhushu Jinian* yu Sandai Niandaixue”

First:

It is a mistake to “standardize” a chronology (or any set of statements of fact). At most, it is reasonable to publish a set of dates describing it as what most scholars accept, if there actually is a consensus. It is especially dangerous for an institution or government enjoying or needing public trust to try to promote a “standard” chronology. If advances in scholarship show that what was published as “standard” is wrong, many people will have been misled, and the institution’s reputation will have been needlessly damaged.

In particular, the PRC government risks misleading the public and damaging its own prestige, if at the present time it endorses a chronology of the Three Dynasties. At the very least, a consensus must first be reached as to the authenticity of the “Modern Text” *Bamboo Annals*, and as to the usability of this text for ascertaining correct dates. (The Xia-Shang-Zhou Project has ignored this text.)

Second:

This risk is not trivial, because (1) there is a reasonable probability that all of the dates in the “Modern Text” are the dates in the original, i.e., they are authentic; and (2) there is also a reasonable probability that if they are authentic, they can be used to infer correct dates that are very different from or other than dates published by the PRC’s Xia-Shang-Zhou Project.

As for (1), consider the date in the *Annals* of rites on day *gengshen* (57), 7th month, year 50, of Huang Di. A date as precise and as far back as this must be a calculation (i.e., neither a record nor an arbitrary invention). I work out the calculation, following *Annals* usage indicating the date should be the first day of the month:

- a) The “Modern Text” and “Ancient Text” together date Huang Di’s rites on *gengshen* (57) to 2353 BC.
- b) 2353 is 100 *zhang* (1900 years) before 453, when the “Three Jin” defeated Zhi Bo, gaining independence.
- c) In the *zhang-bu* system, to get the cycle-day 20 *zhang* earlier than a given date, one counts back 15 days.
- d) The 1st of the Xia 7th month of 453 was *yihai* (12); so the 1st of the 7th month of 2353 must be *gengshen*.

My precise calculation is almost certainly correct, and if correct it almost certainly shows that all of the “Modern Text” dates are authentic.

As for (2), as an example I use data in the “Modern Text” to get a complete chronology (to the exact day) for Xia reigns, with four astronomical confirmations (conjunctions, an eclipse, and first days of lunar months):

- a) Pankenier has shown that the conjunction of 1953 dates Shun’s transfer of power to Yu in Shun 14.
- b) The *Annals* says that on the 1st of the 9th month of the 5th year of Zhong Kang the sun was eclipsed.
- c) The *Zuo zhuan* puts the eclipse in Fang, and implies it was not in Xia but near enough to be reported.
- d) Using *Annals* reign lengths and making interregnums (for mourning) 2 years, the date is 16 Oct 1876.
- e) On that date there was a solar eclipse, north of Xia but reportable. The sun was in Fang at the time.
- f) The same calculation shows that the first day of the reign of the 14th king Kong Jia was 17 Feb 1577.
- g) This day was a *jiazi* day (1st in the 60-day cycle), explaining why the king was called “Kong Jia.”
- h) Pankenier has shown that the last year of Xia was 496 years before the conjunction of 1059, i.e., 1555.
- i) The same count (*Annals* reign lengths, 2-year gaps) gives 1555 as the last year of Fa, next-to-last king.
- j) So Di Gui (= “Jie”) must be an invention. There is abundant evidence for this judgment.

The Project merely offers approximate dates for the beginning and end of Xia, without dates of reigns.

I then show how two implications from (2) can be applied to *Annals* data for Shang and Western Zhou, to recover probably correct dates, very different from the Project's dates: (a) While the *Annals* doesn't indicate gaps between reigns in Shang and Western Zhou, gaps can be assumed, and must have been long enough for completion of mourning. (b) In Shang, first days of reigns determined *gan* names of kings.

Conclusion: Even my critics must see that fixing reign dates for the Three Dynasties is at best too risky at present. We should instead be trying to agree on how to evaluate and use the "Modern Text" *Bamboo Annals*.

17 Was Warring States China Ahead of Greece in Science?

In at least one respect, yes. When the leading states were declaring themselves kingdoms, the philosopher Mengzi (“Mencius”) once said, “Heaven may be high, and the stars in their seasons far off, but if you just study their regularities (*gu* 故 “causes”), you can sit at your desk and still determine the dates of the solstices a thousand years earlier or later.” (天之高也，星辰之遠也，苟求其故，千歲之日至可坐而致也 (4B26)). At this time people marveled at the possibilities of precision in the study of astronomy and the calendar, and Mengzi shared this attitude. He was talking, here, about the 19-year intercalation cycle, familiar enough so that it was echoed even in popular stories—the butcher in *Zhuangzi* who doesn’t need to sharpen his knife for nineteen years; the Lord of Qin in *Mozi* who is granted nineteen more years of life because of his good government.

A century earlier than Mengzi, when the last disciples of Confucius were passing away, this knowledge was perhaps still new, and could sometimes be used in ways we would have to call unscientific. Even so, instances show what technical knowledge was available. I want to examine one such instance which has amazed me. The instance involves some historical calculations done between 432 and 428 BC, concerning two astronomical events in the remote past *fifteen centuries* earlier, in 1953 BC and in 1876 BC. What really amazes me is that the calculator reveals to my analysis that *he had accurate records of these events*. I owe my own knowledge of the first event to Prof. David W. Pankenier (“*Mozi* and the Dates of Xia, Shang and Zhou: A Research Note,” *Early China* 9–10 (1983–85), pp. 175–183) and of the second to Kevin D. Pang (Nivison, D., and Kevin Pang, “Astronomical Evidence for the *Bamboo Annals*’ Chronicle of Early Xia,” *Early China* 15 (1990), pp. 87–95).

It is not known whether the intercalation cycle was introduced into China from the West, or was constructed independently by the Chinese. The cycle attempts to solve a problem confronting any civilization based on agriculture and using a lunar calendar: such a calendar must be kept aligned with the solar seasons for planting and harvesting, by adding a lunar month as needed, either at the end of the year or somewhere in the middle. The Chinese divided the solar year into 24 solar seasons of 15 or 16 days, and used a systematic comparison

9 October and 10 December 2011, and 20 March 2012

with the lunar calendar to indicate when an intercalation was needed: this would be when a lunar month of 29 or 30 days fitted inside a solar interval of two solar seasons without containing a “*qi*-center,” the winter solstice day being the first of twelve *qi*-centers at equal intervals of 30 or 31 days. Over time, this would reveal a cycle of 19 solar years which must contain 235 lunar months, seven of them being intercalations distributed through each nineteen year period.

The Chinese must have been doing this at least as early as the beginning of the Shang Dynasty (1554 BC), when there is evidence that a system of 24 solar seasons was already being used (Nivison, “The Origin of the Chinese Lunar Lodge System,” in A. F. Aveni (ed.), *World Archaeoastronomy*, Cambridge University Press (1989), pp. 203–218). I have found an example of intra-year intercalation on an “oracle bone” which appears to be from 1188 BC, containing two intercalary sixth months (*The Riddle of the Bamboo Annals*, Taipei: Airiti Press 2009, pp. 247–48)—one of them required by the *qi*-center rule, and the next making up for a missed intercalation several years earlier, and making the theoretical summer solstice coincide exactly with the actual solstice.

A 19-year cycle, called a *zhang*, began with a year having the winter solstice at the beginning of the first day of a lunar month. It was assumed that the solar year was 365.25 days. 19 years would be 6939.75 days, rounded to 6940 days. It could be deduced (or after two or three generations of sufficiently careful record-keeping, it would be discovered) that one day must be deleted after four *zhang* making 27759 days, called a *bu*. 27759 divided by 60 leaves a remainder of 39; so 20 *bu*, called a *ji*, were needed to get a number of days (555,180) evenly divisible by 60. Thus a complete cycle was 1520 years, at which point the alignment of day numbers in the cycle of 60 with days of each lunar month in order was expected to be repeated exactly. But 365.25 days per year is not quite correct; back 1520 years, a retrodicted 60-day cycle day number would be four or five days early. So whenever they tried to use the *ji* cycle, the Chinese were misled. But in calculations not exceeding three centuries, their 60-day cycle of *gan-zhi* names for days gave them a great advantage. (It was in use already in the Xia Dynasty.¹)

The Chinese had all of this apparatus in hand by 432 BC, as I will try to show. Meanwhile in Greece there was an almanac-maker named Meton of Athens, who became famed as an astronomer. He announced in 432 BC when ob-

¹ Working down though Xia with my results for early Xia, I discovered that the first day of the reign of the 14th king Kong Jia was *jiazi*, (01) in the sixty-day cycle. All the kings of Shang had such names. This enabled me to work out the chronology of Shang.

serving the summer solstice as the starting date of his almanac, that he would be using a 19-year cycle of 235 lunar months, totaling (to the nearest whole number) 6940 days. Thus he revealed that he knew about the 19-year cycle, but apparently no more. It was said that he got this information from a “metic” (foreign resident in Athens, perhaps from Babylon) named Phaeinos, which was not a Greek name. (Here I use Bowen, Alan C., and Bernard L. Goldstein (in *A Scientific Humanist: Studies in Memory of Abraham Sachs*, E Leichty, M. deJ. Ellis and P. Gerardi eds., Philadelphia, 1988, pp, 39–81, “Meton of Athens and Astronomy in the Late Fifth Century BC.” p. 80) Babylon probably had the 19-year cycle by 490. About a century after Meton there was another Greek calendar scientist in the circle of Aristotle, named Callippus, who deduced that a 4x19 year sequence at 6940 days required subtracting one day, thus giving his name to the “Callippic Cycle,” which the Chinese had been calling a “*bu*” (Bowen and Goldstein p. 51).

On the Chinese side, I cannot name names. But in 1984 Pankenier showed me his paper (to be in *Early China* as above) in which he had demonstrated that a reference in *Mozi* 19 to an astronomical event in lunar lodge Ying Shi must refer to a tight conjunction of the planets in February of 1953 BC. This, he argued, must have occurred when Shun in his 14th year, according to the *Bamboo Annals*, had transferred authority to Yu of Xia, beginning the Xia Dynasty. I knew that the *Annals*' date for the *de facto* beginning of Xia, when Shun transferred authority to Yu, was 2029 BC, and I noticed that 2029 was one *bu* before 1953. So I was almost persuaded: It appeared that a received chronology of Xia had been altered by moving dates back one *bu*.

Confirmation came four years later, when I got a phone call from Ashley Dunn, science writer for the *Los Angeles Times*. Dunn wanted an evaluation (for an article he was writing) of a paper by Kevin Pang (who had been working with UCLA professor Zhou Hongxiang), on a solar eclipse recorded in the *Bamboo Annals* for the *shuo* (sun-moon conjunction) of the 9th month of the 5th year of the fourth Xia king Zhong Kang. In the *Bamboo Annals* system the year corresponded to 1948 BC, and the day was identified as *gengxu* (47 in the cycle of 60). Pang's work identified the Xia eclipse with a solar eclipse he had found dated 16 October 1876 BC. (But the cycle for the day was *bingchen* (53), not *gengxu*.) I at once set 1953 in place of 2029, assumed *Annals* reign lengths, and posited two-year gaps between reigns of Xia kings (instead of the irregular ones at some times found in the *Annals*) for completions of mourning. (I had discovered that in Western Zhou, normally a king's first two years, for completion of mourning, were not counted in the official length of his reign.) This gave me Pang's eclipse date. We published in *Early China* 15 in 1990 (as above).

Next, I did some Collingwood-style rethinking: The calculator I was tracking wanted his dates to be earlier, because he was trying to justify pushing the date Yao 1 back to the numerologically pregnant date 2145. (2145 would be 1000 years before 1145, when the 27th Shang king Wu Yi first recognized Dan Fu as lord of Zhou; 2145 was apparently taken as a *ji* first year in the Lu calendar, one *ji* later being the *bu*-first year 625: see Zhang Peiyu, *Zhongguo Xian Qin shilibiao* p. 252, column for the Lu calendar.) His excuse for moving the date of Xia's first year back would be the possibility that his records had misidentified the *bu*. Trying that out had automatically moved the eclipse date back one *bu* from 1876, to 1952. Why, then, did the *Annals* have the date 1948 BC?

There was a check the calculator could make: The *Zuo zhuan* for Zhao Gong 17.2 has a paragraph discussing eclipses, quoting “the [*Shang*] *shu* for Xia,” as describing an eclipse “between the equinox and the solstice,” when the sun was in lunar lodge Fang.

This must have been Pang's eclipse: on his date the sun was at 188 degrees, in the middle of Fang (which was 187–191 degrees in 1876, if α Sco. was the boundary between Fang and Xin; see Nivison 1989 p. 214). This chapter of the *Shang shu* (“Yin Zheng,” “The Punitive Expedition of Yin”) is spurious, but it contains the *Zuo zhuan* text (*chen bu ji yu Fang* 辰不集于房)², which must therefore be quoting from the authentic original. The essential information is “9th month” and “Fang.” (The trouble, calling for “punitive” action, was probably that the eclipse was only partial in the Xia capital, and the border lord who should have reported it failed to do so. For the actual path of totality see Nivison and Pang 1990.)

So with his eclipse now in 1952, the calculator dropped down one 1520-year *ji* to his own times, getting 432, and checked that year to see if the sun was in Fang on the first day of the (Xia) 9th month. It wasn't. So he tried the next year, and so on, ultimately getting a positive result in 428, finding also that in that year and on that day the cycle date was day *gengxu* (47). Therefore he moved back one *ji* to 1948, and made the cooked *Annals* record say that the day was *gengxu*. (For him, applying the cycle was not following a “law” but was simply following a heuristic strategy.)

² The meaning is disputed. I understand *chen* here to be the area of the sky where all stars are hidden from view by the glare of the sun. To say that the *chen* is “unsettled” (*bu ji*) is therefore to say that a celestial phenomenon, i.e., a solar eclipse blotting out the sun, has reversed this effect, so that the background stars can be seen. There are no other instances of this way of describing an eclipse. So one must decide whether this text is invented or is very old.

Am I mind-reading back 2500 years? The false data 1948 and day *gengxu* have to be explained. How else could they be explained? (To make the date be 1948, the calculator had to increase the total of gap years between reigns before Zhong Kang by four. This he did as follows: having increased the mourning completions for Shun and Yu from two to three years each, he kept his new absolute date for the beginning of Xia unaltered, by reducing the gap after the second king Qi from two years to zero. He now made the gap after Qi be four years instead of zero—as it is, in the present *Bamboo Annals*.)

When was this dating of early Xia worked out? The calculations that produced the year 1948 and day *gengxu* for the Zhong Kang solar eclipse are based on data for the years 432–428. How did the person who did the calculating obtain the data? There are only two possibilities: Either he had access to a table or rule, which could have allowed him to do his work long after the time, even centuries later; or he did his own observing, starting with year 432 and finishing in year 428. The day he picked for the *shuo* of the 9th month is *gengxu* (47), which according to Zhang Peiyu's *Xian Qin shilibiao* (p. 180) would be accurate when I calculate the intercalary 9th month in the Xia calendar for 428; but all of the classic Six Calendars give *jiyou* (46) instead. This is what one would expect, if the Six Calendars were worked out three centuries or more later: they use the *zhang-bu* system, which can give a date one day early when applied to a problem three centuries earlier. Therefore the calculator did his own observing in years 432–428. This is far from being conclusive, because the variation between Six Calendars day dates and true dates is not regular.

But Zhang's *Shilibiao* can be used to give a confirmation of this inference. If the calculator had had a table of solar positions, or had used a set of rules (like Mengzi at his desk) letting him deduce them for earlier times, he would have seen that in year 433—only one year before his target year instead of four years later—the sun was in Fang on the *shuo* of the Xia 9th month. The system for determining intercalations which I described shows me that in 433 there should be an intercalary 5th month in the Xia calendar, making Zhang's 12th month (counting all lunar months from the winter solstice month) be the Xia 9th month (counting all non-intercalary lunar months from the pre-spring-equinox month). Its first day (Zhang, *Shilibiao* p. 90) was *wuyin* (15), 22 October. If the calculator had known this, he would have used “*wuyin*” rather than “*gengxu*” in the *Annals*, and would have made the year be 1953 (not 1948), by cutting the mourning interval after the third king Tai Kang from two years to one year, leaving the interval after Qi at zero years.

Data given in the Nivison-Pang article in *EC* 15 (p. 92) can be extended to show that in 433, 1st of Xia 9th month, the sun must have been at 204 degrees,

which was the first point of Fang in the late –5th century. (Stahlman, W. D., and O. Gingerich, *Solar and Planetary Longitudes for years –2500 to+2000 by 10-Day Intervals*, Madison: The University of Wisconsin Press (1963) gives 205 degrees as the sun’s longitude on this date, clearly in Fang.). The calculator did not know this, and was therefore doing his own observing, and began his observing in 432. His objective was to reconcile Xia and Shang dates with 2145 as the first year of Yao, the key date in a pre-*Bamboo Annals* chronology probably worked out in Lu. The late –5th century was probably when this work was done.

Meton may or may not have been a better scientist, but he knew less; and he did not begin to have the historical and astronomical records that were, it seems, still available in China, at exactly the same time Meton was working. Meton did not have the 76-year cycle of –4th-century Callippus (Bowen and Goldstein pp. 51–52), confirmed as much more accurate by –2nd-century Hipparchus (O. Neugebauer, *A History of Ancient Mathematical Astronomy*, Part Two, p. 624). Chinese experts contemporary with –5th-century Meton were apparently already using the whole set of cycles, *zhang* (19 years), *bu* (4 *zhang*), and *ji* (20 *bu*), trying to apply them to accurate dates fifteen centuries earlier.

This is important, because the person or persons who reveal that they had such accurate records reveal this through my analysis of the *Bamboo Annals*. And the *Bamboo Annals* is a book which probably most of the readers of this page think is a very late forgery or reconstructed text, perhaps as late as the Ming Dynasty. On the contrary, the *Bamboo Annals* text does indeed contain a great deal of chronological invention, but this creative work was not work done after the chronicle discovered in the Jin Dynasty was lost. It is the work of clever people working in early and middle Warring States, in the 5th and 4th centuries BC, at a time when accurate records of the remote past still existed. It is therefore reasonable to hope that we can discover what they were trying to do, and how they did it. If we can do this, perhaps we can recover, even now, those accurate records which their mischief has concealed for twenty-four centuries. This is exactly what I have just done, for part of the records of Early Xia. I think I can do the same for the rest of Xia, for all of Shang, and for Western Zhou.

(For other instances of the use of the *zhang-bu-ji* intercalation cycle in the *Bamboo Annals*, see D. Nivison, “Epilogue to *The Riddle of the Bamboo Annals*” (in *Journal of Chinese Studies*, published by the Institute of Chinese Studies, The Chinese University of Hong Kong), Number 53, July 2011, pp. 7–8, and pp. 17–18 with note 28.)

18 Notes on Royal Ontario Museum, *White* Collection, #1908

I am using an illustration on p. 246 of Chang Yuzhi 常玉芝, *Shang dai zhouji zhidu* 商代周祭制度 (1987):

……其征孟方；夷今……受又，不佯戎。亡……[王]占曰：吉。在十月，王九[祀]。
……We will attack and correct the Yu Fang; let it be now on this [.... day that we march, with no mishap. The ancestors in their sacred altars will] give us aid; we will without doubt be victorious. There are no [misfortunes indicated in the divinations.” The king] reading the cracks says “good fortune!” This is the tenth month, in the king’s ninth [year].

One can fill out the text from *Xiaotun Jiabian* 2416 (Figure 1; Editor’s note: see essay, “A New Study of *Xiaotun Yinxu Wenzhi Jiabian* 2416.”). The words *zheng Yufang* 征孟方 and *zai shi yue* 在十月 identify this fragment as a short text of *Jiabian* 2416; but the longer text lacks the year: *Wang jiu* [si] 王九[祀], “the king’s 9th [year].

In my book *The Riddle of the Bamboo Annals* (Taipei: Airiti, 2009) I used this information as follows (hereafter dates are BC):

- (a) First (*Riddle* p. 232), I dated the set of 70 or more texts for the campaign against the Yi Fang in Di Xin 10–11, accepting the date 1077–76, so that Di Xin 1 = 1086. This required using “23rd year” in *Yi Zhou shu* 21 “Feng Bao”: it describes a gathering of pro-Zhou lords datable to 1046, implying a second Di Xin calendar beginning in 1068 (*Riddle* p. 239), which accounts for otherwise un-datable late Shang inscriptions (*Riddle* pp. 241–244). (Those inscriptions, *jiagu* and bronze, had misled the PRC “Three Dynasties Project” scholars; *Riddle* pp. 251–252.)
- (b) Second, I determined the ritual calendar dates for those years, first getting absolute dates for each rite in months 9 through 12 of 1077. There was an intercalary 9th month immediately after the autumn equinox day 2 October *dingyou* (34), confirming the year 1077 and the date of a *zai* offering to Shang Jia on *jiawu* (31) 29 September (*Riddle* pp. 230–232.)
- (c) Next, I extended the ritual calendar back into the reigns of Di Yi and Wenwu Ding, correcting or confirming guesswork by using more inscriptions. The general principle was that since the ritual calendar year was slightly shorter than the Julian calendar year, I must assume that the farther back I extended the matching, the later would be the starting day of the ritual calendar. (See table, *Riddle* pp. 239–240.)

February 7, 2012; Date of visit to Royal Ontario Museum, Toronto (at AAS meeting) probably March 16, 2012

- (d) I could assume that in general the ritual calendar would alternate between 36 and 37 *xun* in length. But I found that I must assume a resetting of the first month of the civil year from the *hai* month (in early Di Xin) to the *chou* month (in 1077–76); and that for about ten years in late Di Yi and early Di Xin the ritual year had been held at 36 *xun* only.
- (e) I also found—using the set of inscriptions *Ying* #2503 (Li, Qi and Allan)—that the Di Yi reign began in 1105 and was 19 years long, not 9 as in the *Bamboo Annals*. The Li-Qi-Allan set is explicitly “8th year,” and it must have an intercalary 3rd month, if 1077 had an intercalary 9th month, and if the year (as I concluded from the implied starting day of the cycle) was 1098 (see *Riddle* pp. 236–237, and the table for seven intercalations every nineteen years, p. 246).
- (f) I assume that the change in the length of the Di Yi reign had been made in order to avoid a 10-year overlap of the Wu Yi and Wenwu Ding reigns. “9th year?” in *White* #1908 cannot be a Di Xin or Di Yi date because the beginning day of the ritual cycle implied in *Jiabian* (“coinciding with *yi*-day for Da Yi”) is too late. So, Wu Yi must have declared a calendar for his heir Wenwu Ding, starting in a year –1118, which happened to be just ten years before his own death in 1109. If so, the date of *White* #1908 is 26 October 1110 (*Riddle* p. 239).

Confirmation: An entry in the *Bamboo Annals* for Di Yi 3 originally read Di Yi 13, for 1093 (*Riddle* pp. 170–171, 185). It dates an earthquake in Zhou which can be dated to 1093 by analyzing a story in *Lü shi chungiu*; and the story uses the calendar of Zhou Wen Wang (1101/1099–1050), not the Shang calendar (*Riddle* p. 55; Editor’s note: see essay #7: “A Tell-tale Mistake in the *Lü shi Chungiu*: The Earthquake Supposedly in the Eighth Year of Wen Wang of Zhou”). Furthermore, Di Yi 3 also by error records a royal order to “Nanzhong,” shown by bronze inscriptions (*Piqi gui* and *Guoji Zibo pan*) to belong in Zhou Xuan Wang 13; I owe this information to E. L. Shaughnessy and Ma Chengyuan (*Riddle* p. 185). This error could only have been made when “[Di Yi] year 3” was still “[Di Yi] year 13.”

Note: I am aware that the reasoning in these notes is open to challenge, and to invite challenge is why I am distributing these notes. More detail (and more opportunities for error) will be found on pp. 236–240 of *The Riddle of the Bamboo Annals*.

(a)



(b)



(c)



(d)

[Preface:] Day dingmao (4), the king divining by cracks

[Charge:] [1] "Since we have now many times received favorable responses in divining by scapulas and by stalks, I will accompany my many governors and many lords to correct Tan, lord of the Yu Fang. [2] It will be on the day of the public yi ceremony that we start our march; there will be no mishap. [3] From the greater and lesser ancestral spirits in their altar stands I will receive aid in this undertaking; we will surely be victorious. [4] It can be announced to this great city Shang that there is no ancestor-spirit-caused misfortune indicated on the scapulas."

[Prognostication:] The King read the track and said, "Vast good fortune!"

[Postface:] This is in the 10th month, coincident with the yi sacrifice to royal ancestor Da Ding.

- (a) Jia bian 2416
- (b) text in Shang script
- (c) text in modern script
- (d) translation, DSN 1987

19 90th Birthday Address

18 Jan. 2013

Opening: Thanks for coming.

You all know I am pleased you're here, so enough of that. I am going to take advantage of you and try to say something more, at least as interesting.

1. I am 90, with a lot to remember. Stanford for me began in 1948, when I left Harvard and took a job here too quickly, to teach three courses in Chinese, which I was still learning. I was decently prepared only in historical writings. My closest friends were Arthur and Mary Wright in Chinese history. By 1955, I found myself enjoying a 3-year “lectureship” in Philosophy, free to do with it whatever I wished—like, trying to find out what philosophy is. I had taught myself enough logic to persuade Pat Suppes to let me teach the beginning course; and of course there was Chinese philosophy. But I wasn't satisfied with that: I wanted to know what my colleagues were doing, and why—especially Donald Davidson. This led me into ethics and action theory, and the boundary between philosophy of science and philosophy of history, which at that time meant the explanation problem.
 - 1.1. Like other grad students I read Hempel's article on “The Function of General Laws in History”¹—having already been charmed by R. G. Collingwood and his idea of “rethinking,”² as what a good historian has to be doing. (I had been led to read him by Arthur Wright, probably.) So I sensed that something was wrong in Hempel's reduction of explaining something to deducing an account of it from a causal law of nature, compelling though this seemed. At about this time (it must have been 1961–62) two grad students, Sam Gorovitz and Fred Newman,³ came to

1 Carl G. Hempel, in *The Journal of Philosophy* 39, 1942.

2 Robin G. Collingwood, *The Idea of History* (posthumous, assembled and edited by T. M. Knox, Oxford, 1946); especially Part V, Epilegomena.

3 Gorovitz (b. 1938 in Boston, MIT B.S. 1960) and Newman (b. 1935, Bronx, Army in Korea, graduated at CCNY before going to Stanford) both wrote dissertations on explanation theory, working with me and Davidson (who moved to Princeton, leaving the signing to me). The two had very different backgrounds and went on to have utterly different careers, each colorful in its way. Gorovitz has had a very successful academic career, with teaching and administrative positions at Wayne State, Case Western Reserve, the University of Maryland, and (since 1986) the University of Syracuse. He quickly established a reputation in medical ethics, has written or edited eight books, authored almost 120 articles and much more, sought after as a visiting

my office. (I always left the door invitingly open.) The students' "Hume Society" wanted me to present a paper. I knew very well that I was a fraud in philosophy; this would be my first "going public" as a philosopher, and most of the students knew more than I did. (I was primarily occupied in a different world, my book-to-be on Zhang Xuecheng, Stanford, 1966.) But I didn't see how to refuse, and gave them a hesitant "yes"—whereupon Sam turned to Fred and said "Let's leave now!"—before he changes his mind.

- 1.2. I met the challenge by turning Hempel on his head. Very simply, what a historian (or anybody) normally does if he wants to explain a happening is not to use a law of nature from which an account of it can be deduced. What one does is in to reason backwards, and deduce the explanation from the effect. Billy has the measles. He must have been playing with some child who had the measles—maybe Sally, whom we now know had the measles. After all, that's the only way you can get the measles. My talk was thin, but it worked. For some weeks, students and some faculty were talking about "Nivison's backward model" for explanation.
- 1.3. At first this seems to be looking for a necessary condition rather than for a sufficient condition; but that's not right: you could take Hempel's trivial example and twist it my way: Why did this pan of water freeze when I left it in the garden? The temperature last night must have fallen be-

scholar. I assume he is still thriving. Newman returned to New York City, stayed there and died July 3, 2011. One *New York Times* obituary begins: "Fred Newman's influential role in New York life and politics defied easy description. He founded a Marxist-Leninist party, fostered a sexually charged brand of psychotherapy, wrote controversial plays about race and managed the presidential campaign of Lenora Fulani, who was both the first woman and the first black candidate to get on the ballot in all 50 states. He helped the Rev. Al Sharpton get on his feet as a public figure and gave Michael R. Bloomberg the support of his Independence Party in three mayoral elections, arguably providing Mr. Bloomberg's margin of victory in 2001 and 2009. Mr. Newman, who died at 76 in his Manhattan home on July 3, eschewed conventionality. He insisted, for instance, that there was nothing wrong with psychotherapists having sex with patients. He created an empire of nonprofit and for-profit enterprises, including arts groups and a public relations firm. He wrote books on psychology and philosophy as well as plays. One play, about the 1991 riots between blacks and Jews in the Crown Heights section of Brooklyn, was condemned as anti-Semitic by the Anti-Defamation League. His greatest impact came through mobilizing his followers, sometimes called "Newmanites," to build alliances with third parties, including that of the Texas independent H. Ross Perot. "If it weren't for the Independence Party, Mike Bloomberg might not have become mayor," said Douglas Muzzio, a professor of public affairs at Baruch College." Google keeps no secrets.

low freezing. What's explanatory is the story, into which you fit what is to be explained. Normally that story is going to be an account of a causal sequence, which is what we call a narrative. This is as true of my striking this match, as it is of the assassination which touched off World War I. (So there is a unity of the sciences: It was this that Hempel and people like him were really worried about. But the model for explanation is not physics, as Hempel thought; the model is history. An explanation is a narrative, which is a description, never completable, of a causal sequence. This means that Hume was wrong in his account of causality, holding that our apparent knowledge of the connections between causes and their effects is really just habitual expectation, because when you look for the connection you can't see anything there.⁴

- 1.4. Nor is Collingwood right in thinking that the essential distinguishing feature of a historical explanation is the "rethinking" the historian has to do to grasp what historical agents are doing. ("All history is the history of thought.") Contra Collingwood, it makes as much sense to speak of the history of the solar system as the history of the Soviet Union. That means I can't follow Collingwood in seeing in "rethinking" a special way of reasoning that makes a historian's work unique, though I agree that this is what he often has to do. (Mencius, I think, was on to something when he said that when he tired of talking to friends (he didn't suffer fools gladly) he would look back into the past and "make friends" with people in history.⁵)
- 1.5. Here I seemed (but not for long) to find myself agreeing with Theodore Abel, a sociologist who did outstanding work in the 30's and 40's, and who wrote a short article which I assigned to my classes, "The Operation Called *Verstehen*"⁶ (well known: you can get the whole thing from Google; my copy in Feigl and Brodbeck is thickly marked up in penciled comments). Abel is, I think, really talking about what Collingwood called "rethinking," only keeping his distance by naming it in German. His homely example: from my window—upstairs, apparently—I notice my neighbor at his desk get up, dress himself warmly, go down to his yard, chop some wood from his woodpile, carry it back and make a fire

⁴ There isn't, because anything you might point to Hume would probably include as part of the cause or of the effect. Has Hume boxed himself into saying that the (true) *cause* of our (false) idea of *cause* is our repeated experience of conjunctions?

⁵ Mencius (*Mengzi*) 5B8.

⁶ *American Journal of Sociology* 54, 1948.

in his fireplace. I also notice that my outside thermometer registers a temperature below freezing. I reason via *verstehen* that he is feeling the cold, and is taking obvious steps to make himself more comfortable. But I don't really know this. For all I *know*, he could be motivated by a subconscious grudge against his landlord, pressing him for more rent, and hopes that he will "accidentally" burn the house down. The two "explanations" are alike in being not part of what I know. The method of *verstehen* is merely heuristic, Abel says, with luck leading me to a hunch that I can test scientifically by statistical analysis. But *verstehen* itself, he insists, gives no new knowledge. How I would apply such analysis is left as an exercise. A deeper difficulty is that Abel seems insensitive to the possibility of grading candidate explanations and picking out the best one. After the work of Gil Harman (begun as another mere 8-page article in 1965⁷ but kicking up a storm) maybe we are more aware of this than Abel.

1.5.1. Another Abel example: for a specified community, there is a 0.93 correlation between area-wide crop failures and dips in the annual marriage rate. One sees why: John was thinking of proposing to Mary, but looks at his barren fields and sees it would be prudent to wait before taking on a new serious responsibility. But this is, after all, *verstehen* again, so I don't really know. Nothing seems to satisfy Abel, if even a case with statistics doesn't. But reflect: there is nothing sacred about 0.93. There may well be hundreds of purely accidental correlations with this ratio that have nothing to do with each other. It is our ability to tell a story, even if just made up, a narrative of a causal process, that makes this datum look right; and the story looks like the best possible explanation.

2. I am going now to describe a different example of explanation. It is an explanation that I myself worked out, to explain something puzzling. And it seems to me not to be just the best explanation but the only explanation possible. Further, in the process of constructing it I learned something that astonishes me, and that you may well urge me not to believe.

2.1. More than 30 years ago I accidentally discovered that an ancient chronicle always dismissed as a late forgery was not a forgery. It was found in north China around 280 CE, in a royal tomb or storehouse probably dug

7 Harman, Gilbert (1965). "The Inference to the Best Explanation," *The Philosophical Review* 74:1, 88–95.

into the side of a hill, so the objects found were dry, and well-preserved. The book was written on strips of bamboo, and so is called *The Bamboo Annals*. The last date in it can be interpreted with certainty as 299 BC. Dates of reigns in it have been systematically distorted, down through Li Wang, the 10th Western Zhou king, d. 828 BC (BC omitted hereafter). I came to believe it not a fake, when I found I could use it to interpret dates in Western Chou bronze inscriptions, if I assumed what I thought was probably the cause of the distortions. The earliest date is probably 2402. It begins with myth, for several centuries (how many can be debated). Then come the “Three Dynasties,” Xia, beginning 2029 or (corrected) 1953; Shang, 1558 or (corrected) 1554; and Western Zhou, 1050 or (corrected) 1040, ending 771. I have been trying to deduce the correct dates of reigns of kings in those dynasties, using references to astronomical events and other evidence.

- 2.2. The most important internal evidence has been the effect of the (prehistoric, I think) institution of 25 or 27 months of obligatory mourning for a king’s father. In Western Zhou bronze inscriptions this had the effect of requiring that near the end of a reign (exactly when, and why, we don’t know), dates of events—and dates in bronze inscriptions—had to be counted from the year after the king had completed mourning; so usually the king’s reign-of-record lacked his initial two years.⁸ This is partly obscured by over-writing in Western Zhou, but it becomes quite clear for Shang, determining the reign-of-record for 20 of the 30 kings. In Xia, apparently the rule was stricter, and prevented a king from being king at all for the first two years after his father’s death. The “no-king” intervals are explicit in the *Annals*, usually two (or three) years.
- 2.3. The Xia Dynasty (its existence doubted by most Western scholars, but not by Chinese) is dated as beginning in 2029, but D. W. Pankenier (Lehigh University) has argued (correctly, I think) that it began in 1953, coincident with a tight conjunction of the five visible planets in February of that year⁹ (perhaps leading to a *coup d’ état*). Pankenier’s reasons for connecting the conjunction with the beginning of Xia were too

⁸ This is the “Nivison-Shaughnessy 2-yuan hypothesis,” rejected in PR-China and attacked in a question—begging article in the latest issue of *Early China* (33–34). I suppose I must reply to the article.

⁹ Pankenier, D. W., “*Mozi* and the Dates of Xia, Shang and Zhou: A Research Note.” *Early China* 9–10 (1983–85), pp. 175–183. (A Chinese translation of Pankenier’s article is included in Shao and Nivison, *Jinben Zhushu jinian lunji*, pp. 297–303: “*Mozi* yu Xia Shang Zhou di Nian-dai—*Zhushu jinian* Yanjiu Zhaji.”)

speculative for most people: the *Annals* has Yu, the first Xia king, being given a *gui* as a symbol of authority, and in the *Mozi* Pankenier found an apparent (to him) reference to an astronomical event marking the beginning of Xia. He guessed that the event was a conjunction, and found one where he thought it was supposed to be, and found it was *gui*-shaped.

- 2.4. I took Pankenier's idea seriously, because I noticed that 2029 was just 76 years earlier than 1953. One *bu* of 76 years was the basic unit of time in the ancient Chinese intercalation cycle of 1520 years (20 *bu*, a *bu* being 4 *zhang* of 19 years, each beginning with the winter solstice day and containing 235 lunar months, 7 being intercalary). Further, work on the *Annals* in Warring States had pushed dates back, to get the first year of Yao back to 2145. This was 1000 years before 1145. I had found 1145 to be the first year of Wu Yi, the 27th Shang king, who had granted court status to Dan Fu of Zhou in that year. 2145, furthermore, was the first year of a 20-*bu* intercalation cycle in the ancient calendar of Lu, the native state of Confucius. So I reasoned that someone had hopefully adopted the theory that the record of 1953 as the beginning of Xia was wrong, and was one *bu* too late. (He was trying to get Yao's first year back to 2145.)
- 2.5. Another astronomical event in the *Annals* is a solar eclipse on the first day of the 9th month of the 5th year of the 4th Xia king Zhong Kang. Lots of work had been done trying to identify this eclipse, without success. One day in December of 1988 (CE) I got a phone call from a science writer for the *Los Angeles Times*. He was working up an article on work being done by UCLA professor Zhou Hongxiang and people working with him, including one Kevin Pang, who had published an article claiming this eclipse occurred in the morning of 16 October 1876. So I checked: Assuming 1953 for the conjunction and first year of Xia, together with reign-lengths as given in the *Annals* and two-year gaps between reigns, I got Pang's eclipse date, exactly. Pang and I published in the next issue of *Early China*.¹⁰

¹⁰ Nivison, D. S., and Kevin. D. Pang, "Astronomical Evidence for the *Bamboo Annals*' Chronicle of Early Xia." *Early China* 15 (1990), pp. 87–95. Forum: "Response," pp. 151–172. (The article combines work by Pankenier, Pang and Nivison to establish an exact chronology of Xia from 1953 to 1876 BC. I invited Pankenier to join us. He declined, perhaps because doing so would

3. There was one more problem: If 1953 had been moved back one *bu* to 2029, then the eclipse date 1876 must have been moved back one *bu* to 1952. But that is not where the eclipse is in the *Annals*: It is in 1948, four years later; and the preceding inter-reign gaps have been altered just enough to make 1948 the date. Why? Further, the day in the 60-day cycle system is given as *gengxu* (47), and that is not right, for either 1952 or 1948 or 1876; so where did that come from?
- 3.1. I did some thinking—“rethinking” the thoughts of the person(s) responsible, who I could neither name, place nor date. By this time I could guess they were using the intercalation cycle, and were working in Warring States (ca. 479–221), maybe in Lu. The concept of the cycle was that at the end of one you started over, every calendar detail repeated as before; but this was new in China, so they might be guessing, to some extent. They would think they knew one important thing about this eclipse: an eclipse is described in the *Zuo zhuan* (citing the “*Xia Shu*”) as occurring “between the equinox and the solstice” and with the sun located in Fang in the lunar zodiac, possible for the 9th month. (But no day-date in the 60-day cycle is given in the *Zuo zhuan*’s account of what the *Xia Shu* said, and probably the invention of the day-cycle was later than that eclipse.) So to work the problem out, they moved down 20 *bu* (one *ji* of 1520 years) from 1952 to 432—which is when they must have been working—and looked, calculating the sun’s position from their observations on the first of the 9th month. Was the sun in Fang? No. They checked the next year 431, and the next, 430, and the next, 429: still no. But for the next year 428, the sun was in Fang on the first of the 9th month, and the day was a *gengxu* day.¹¹ So they moved back 1520 years to 1948, and made the day be *gengxu*.
- 3.2. That’s my explanation, and I think it is the only explanation possible. Did I learn anything new by working it out, or was I (as Abel seems to think I would have to admit) only moving around knowledge I already had? I learned quite a lot.
- (1) First, I learned when the people who did this were doing their mischief: between 432 and 428.
- (2) Second, I confirmed that the intercalation cycle was known in China by this time—the whole of it. The Greeks had part of it: In 431, an

have committed him to accepting the Nivison–Shaughnessy 2-*yuan* hypothesis, which is incompatible with his date (1046 BC) for the Zhou conquest.)

11 Zhang Peiyu, *Zhongguo xian-Qin shilibiao*, p. 91.

Athenian named Meton published an almanac based on the 19-year cycle.¹² The 76-year cycle came later, with the work of Callippus in the circle of Aristotle. So the Chinese didn't get it from the Greeks, though perhaps they did from Babylon, which had the 19-year cycle by 490 BC.

- (3) Third, I had found another demonstration of what could be called the *chabuduo* ("close enough") syndrome in Chinese thinking (ridiculed by the modern writer Lu Xun in his novelette "Chabuduo Xiansheng zhuan"), here infecting even ancient incipient scientific thinking. Testing the eclipse date 1876, after it had been moved back one *bu* to 1952 (to see if 1952 was really the right date), was done by moving down one *ji* to 432, and exploring the next four years 9th month by 9th month to 428. The cycle formula—not a "law of nature" anyway—gave them no license to do this when working on an eclipse a whole cycle earlier, but doing it was for them, it seems, "close enough."¹³
- (4) I also can conclude that the 60-day cycle probably began later than 1952 or 1876. If it had been in existence at the time of the eclipse, a record would probably have used it. It is likely that the major reason for the investigation I have described was to discover the missing day-cycle date. (I continue to think that the 60-day cycle probably began with the succession day of Shang Jia Wei in 1718, as *jiazi* (01) day, changed retroactively to *jiaxu* (11) by Kong Jia in 1577, and that Kong Jia jumped his own succession day from *jiayin* (51) to *jiazi*

12 Here I use Bowen, Alan C., and Bernard L. Goldstein (in *A Scientific Humanist: Studies in Memory of Abraham Sachs*, E Leichty, M. deJ. Ellis and P. Gerardi eds., Philadelphia, 1988, pp. 39–81, "Meton of Athens and Astronomy in the Late Fifth Century BC."

13 Two other examples of this way of thinking: (1) As I show in my article "The origins of the Chinese Lunar Lodge System" (A. Aveni, ed., *World Archaeoastronomy*, Cambridge University Press, 1989) the Chinese had enough accumulated data by Warring States times to have recognized the precession of the equinoxes. Instead, they didn't get beyond thinking that after a few centuries the calendar and zodiac spaces would have to be adjusted a bit. (2) As I will show in my Chinese revision of my book *The Riddle of the Bamboo Annals* (Chapter Six), Liu Xin's *chaochen* ("jumping a space") ratio 144/145 for Jupiter is probably based on a mistake made by astronomers in 315 due to propaganda in the *Annals*; but if they knew this they continued to honor the traditional ratio 12/12 (12 years/12 spaces, in Jupiter's apparent movement around the zodiac), simply adjusting their application of it. (The actual ratio is about 84/85.)

- (01), thus causing “ten suns rising at once” *shi ri bing chu* 十日并出.¹⁴)
- (5) But look at the records the Chinese must have had! My explanation seems to require me to assume that around 432–428 they knew exact dates back to the 20th century BC.¹⁵ The earliest writing in China that we know of is in shell and bone inscriptions, beginning about 1230, more than seven centuries after Xia began in 1953.
- (6) So perhaps one must take seriously the statement in an early philosophical layer of the text of the *Book of Changes*, the *Yijing*: “In highest antiquity government was carried on by knotted strings; in later times wise men changed this to the use of written characters.”¹⁶

14 This would be a political joke (an old one: 1577 BC): The word *ri* 日 means either “sun” or “day”; D. S. Nivison, *The Riddle of the Bamboo Annals* (Taipei: Airiti, 2009), p. 91; also p. 139 strip 102b.

15 Perhaps not quite: It could be that moving Xia as a whole back one *bu* was done earlier, and the 432 test was only for the date 1952. (Sensing that 1952 needed checking would suggest suspicion that 1952 was not the original date.) But, more likely, the original eclipse record lacked a *ganzhi* day date, and the persons revising the Xia chronicle were trying to find it by a calculation using the intercalation cycle.

16 *Zhou yi*, end of “Xici” *xia* II: 上古結繩而治。後世聖人易之以書契。Any inventory of philosophical reflections on issues in this essay must include the famous words ascribed to the Confucian moral philosopher Mengzi (Mencius, ca. 390–305 BC), undated: “Heaven may be high, and the stars in their seasons far off, but if you just study their regularities in detail (*gu* 故 “causes”), you can sit at your desk and still determine the dates of the solstices a thousand years earlier or later.” 天之高也，星辰之遠也，苟求其故，千歲之日至可坐而致也 (4B26). He had in mind the concept of a 19-year *zhang* as beginning with a year when the winter solstice occurs in the first day of the first lunar month. He and others were unaware that the system is inaccurate when projected forward or backward more than three centuries. Also, there were different ways of determining what day counted as winter solstice day.

20 Two *yuan* and Four quarters

1. I am looking at an article in *Early China* 33–34 (2010–2011, actually 2012), pp. 171–198. The article is an expansion, translated by Prof. D. W. Pankenier (Lehigh University), of an article in *The Chinese Journal of the History of Science and Technology*, 30 (2009), pp. 89–101, by Xu Fengxian, Institute for the History of Natural Sciences, Chinese Academy of Sciences, in Beijing.
 - 1.1. The focus of Xu’s article is the interpretation of four technical terms found in dates in Western Zhou bronze texts (and in some old literary texts—which she does not use—notably the *Shang shu* and the *Yi Zhou shu*): these are *chuji*, *jishengba*, *jiwang*, and *jisiba*—understood by most scholars (including me) following Wang Guowei (d. 1927), as naming approximately the four phases of the moon. Wang presents his view in a short article, “*Shengba Siba Kao*,” published in 1917.
 - 1.2. A radically different interpretation was adopted by the PRC Three Dynasties Project (*Xia Shang Zhou Duandai Gongcheng*, hereafter “Project”), 1996–2000, charged with the task of determining exact dating of reigns prior to 841 BC, the first year of the Gong He Regency during the exile (841–828) of the 10th Western Zhou king Li Wang. The Project took *chuji* not as a phase term, but as referring to the first ten days of the lunar month, and *jishengba* as referring to the 14 days from *fei* (first sighting of the new moon, the 2nd or 3rd of the month) to the full moon (followed by *jiwang*, then *jisiba*).
 - 1.3. Xu’s theory is more radical still: for her, *jishengba* refers to any day in the first half of the month, short of *jiwang*, and the similarly structured term *jisiba* refers to any day in the second half of the month. *Chuji* is optional for any of the first ten days, and *jiwang* is optional for a few days after the full moon. So *chuji* and *jishengba* overlap, and *jiwang* and *jisiba* overlap.
 - 1.4. Xu’s method is to analyze only bronze texts containing the terms, and to count days between them to find constraints on their meaning, disregarding everything else, whether theoretical, chronological, semantic, historical or philological. Her goal, it turns out, is to show that any Wang Guowei-type theory is “without foundation”: she holds it fails no-

Paper presented at the First Annual Society for the Study of Early China Conference, San Diego, CA, March 21, 2013

tably for *jishengba*, which must begin well before the second quarter. So she guesses that *jishengba* and the similarly structured *jisiba* are not names of *quarters* but name the waxing and waning *halves* of a lunation.

2. She mentions a theory advanced by E. L. Shaughnessy and me, that two different *yuan* days are used in dates: the king's succession year, and what we call the accession year, which is the first year after the king completes mourning for his father. Our theory is that the accession *yuan* is used only late in a reign (my own view being that it becomes mandatory after the death of the father-king's last chief minister). A king's death is therefore recorded in his accession calendar. Thus a king's reign-of-record omits mourning years at the beginning of his reign.
 - 2.1. I have shown that this applies also to Xia and Shang. Hence the importance of this dispute: Chronological study of the Three Dynasties, I maintain, cannot even begin without taking the 2-*yuan* principle into account. I demonstrate this in Attachments I, II, and III.
 - 2.2. Having mentioned this theory (without describing it), she thereafter ignores it, for, after all, it is only a theory. In doing this she begs the question, making her entire argument worthless, because our theory was worked out (by me) so as to show what had to be assumed—and proved—if one is to make a Wang Guowei-type system apply to certain inscriptions late in a reign (Kang, Mu, Gong, 1 each; Yih, 3; Yi, 6; Xuan, 8.) I found that always the difficulty vanished, if I simply assumed that the year-date in the problem inscription was counted from a *yuan* two years later than the king's succession date. By refusing to even look at this theory, Xu is in effect insisting on a one-*yuan*-only theory, which guarantees that she will find Wang Guowei wrong.
3. But had she analyzed one of her selected inscriptions correctly, she would have proved Wang right. The inscription is a Jin (not Zhou) inscription on sixteen bells. The text recounts a six-month campaign to the east and south, in which Su, lord Xian Hou of Jin, supports the king with Jin troops. It begins in Zhou year 33, on *jishengba* day *wuwu* (55) in Jin month 1 of 794 (Zhou month 10 of 795), and ends with the king giving rewards in Cheng Zhou (the Zhou eastern capital) on *chujì* day *wuyin* (15) in Jin month 6 (Zhou month 3).
 - 3.1. The *bianzhong* (bell-set) inscription contains all four terms. But there is a frustrating error: In month 2, on day *guìmao* (40) of *jiwang*, the king enters Cheng Zhou. Then on day *renyin* (39) of *jisiba*, he rejoins the ar-

my to march east. These two day-dates are where the text moves from bell 1 to bell 2. Since 40 follows 39, it appear that the carver (the text is carved, not cast) slipped and put *guimao* on the first bell, then was forced to continue the error or discard the first (and biggest) bell; so he just let the dates be switched and hoped for the best.

- 3.2. Xu Fengxian notes that this has been suggested by others—Ma Chengyuan, Ed Shaughnessy and me—but she disregards us. Since *guimao* (40) and *renyin* (39), in that order, cannot be right, she assumes that at least one is wrong. Then she forgets about “at least,” and tries to prove that it is *jiwang*, *guimao* that is wrong (leaving *jisiba*, *renyin* right: for her *jisiba* is any day in the 2nd half of the month). She does this by showing, with tables, that if *jiwang* is limited to six days—a rule she has “adopted for working purposes”—then month 6 cannot contain day *wuyin* (15). Not noticing the circularity, she asks what day *guimao* is a garble for, concluding that it must be either *xinmao* (28) or *guisi* (30). (The PRC Project apparently uses the same specious reasoning, and chooses *xinmao*, as fitting its date 845; the correct date is 795–794.)
- 3.3. All of the historical and technical detail you have been hearing has been supplied by me: Xu on principle brackets it out, relying on many pages of tables, exhausting all possible date combinations—but without proper interpreting of the tables, this method will not reveal that the day dates have been switched. We need another method, one recognizing that all possible evidence must be accounted for. I give it to you in Attachment IV. You will see there that if the king’s reward-giving in Cheng Zhou at the campaign’s end is (appropriately) on the *first* of the (Jin) sixth month, then in the (Jin) second month the last day of *jiwang* and the first day of *jisiba* are *renyin* (39) and *guimao* (40), the 24th and 25th of the month, in the right order. Therefore there is no overlapping, and Wang Guowei is correct in holding that the phase terms *are* phase terms, *jisiba* being the name of the last *phase* of the moon, and not the name of the second *half* of the month. Further, this precisely agrees with my published analysis of the phase terms in the account of the Zhou campaign against Shang (Attachment V).
4. The Project had to do what Xu doesn’t. The Project had to assign real dates to dated inscriptions that belong in late Xuan Wang, but were un-datable there once the Project had rejected Wang (and our 2-yuan theory). So these were all dumped into a supposed pre-exile 37-year reign of Li Wang. But there was no such reign (Attachment VI). The Project has stubbornly steered

itself into a trap. If Xu had tried to apply her conclusion, that's where she would be. But Wang Guowei's four quarters theory is right, making her one-*yuan*-only theory wrong, so our 2-*yuan* theory wins, and we all escape the 37-year trap.

That's it.

Attachment I: Three Dynasties Chronology, using the 2-*yuan* theory

1. Xia: Pankenier's conjunction of February 1953 marks the beginning of Xia. Pang's date 16 Oct 1876 is correct for the Zhong Kang eclipse of the *shuo* of the 9th month of the 5th year of the 4th Xia king Zhong Kang. 1555 (496 years before the conjunction of 1059, confirmed by Pankenier) was the last year of Xia. There are gaps between Xia reigns in the *Bamboo Annals* (BA), seven being 2 years. These were for completion of mourning, so all gaps should be 2 years (as implied by the 2-*yuan* theory). These assumptions (taking BA reign lengths for Xia as correct) are confirmed by deducing 1876 from 1953 (as in *Riddle* p. 45).
 - 1.1. But the BA date for the beginning of Xia is 2029. This is 76 years before 1953, one *bu* in the 20 *bu* intercalation cycle of 1520 years; so the 1876 eclipse ought to be re-dated back one *bu* to 1952. Instead, it is at 1948, four years later. Why? The person who did this had the information (from the *Xia shu*, quoted in *Zuo zhuan* Zhao 17) that the sun was in Fang. But he did not have the *ganzhi* for the day. (This shows that the 60-day cycle probably did not yet exist in 1876.) So he went down one 1520-year complete *ji*-cycle from 1952 to 432, hunting for a date when the sun was in Fang on the *shuo* of the 9th month. This was not true in 432, or 431, or 430, or 429, but was true in 428, and the *ganzhi* was *gengxu* (47). Back one *ji* to 1948, he dated the eclipse accordingly.
 - 1.2. If we carry this analysis to the end of Xia, we find no room for the supposed last king Jie (Di Gui): 1555 was the last year of Fa, next-to-last king; so Jie is a chronological invention. We also get the date 17 Feb 1577 as the first day of the reign of the 14th king Kong Jia. This day was a *jiazi* day, by present calculation, accounting for the *gan* in the name "Kong Jia." The first day of Shang Jia Wei according to the BA ought to have been *jiazi*, if this was when the 60-day cycle began. (See Key pp. 13–14.) But Shang Jia Wei's first day was *jiayu* (11) by present calculation. If it should have been *jiazi*, then Kong Jia's first day should have been *jiayin* (51). Perhaps Kong Jia advanced the cycle ten days, so that he could be inaugurated on *jiazi*. This could have led to a popular political joke: "ten suns rose at once" 十日並出 (as in *Riddle* strip 102), entering the record taken literally, as a baleful omen. (Shang ancestor Shang Jia Wei's first day was 18 January 1718.)

2. Shang: I apply the “first day *gan*” and “2-*yuan*” ideas to all 30 Shang kings in the *BA* (*Riddle* pp. 42, 49). For 20 of the 30 the *BA* has reigns-of-record lacking initial mourning years (more often three years, in Shang). The results are consistent with the *BA* account of Yi Yin as a scoundrel who almost made himself king, Wai Bing and Zhong Ren being his puppets. When chronologists ceased to recognize initial mourning years, the 5th generation kings Tai Wu and Yong Ji were reversed, and Tai Wu was stretched from 60 years to 75 (as in all chronologies). (ZPY is usable from Tai Wu on.)
 - 2.1. There were two brothers in generation 3 through generation 9, probably to have a main line king succeeded by a brother, while the heir did the heavy duties of mourning. But in generation 10 there were four brother kings, the second, Pan Geng, trying to usurp the succession, and the fourth, Xiao Yi, succeeding in passing on the throne to his own son Wu Ding, in 1250, beginning with a *ding* day, for 3+59 years. Wu Ding’s death year 1189 is confirmed in several ways. (See especially *Riddle* pp. 247–248.)
 - 2.2. His successor Zu Geng’s first day was also a *ding* day. The same *gan* must not be used by two kings in a row, so the post-mourning year 1185 was used, first day *jisi*, taken by Wu Ding’s heir Zu Ji, who received cult under Kang Ding as “junior king (*xiao wang*) Father Ji,” ignored by history. First Zu Geng (using next day *gengwu*), then his younger brother Zu Jia (successfully) sought to usurp the succession. Zu Jia claimed Zu Geng’s 11 years in addition to his own 22 years, and gets 33 years in secondary sources. Becoming king, he at once appointed his son Prince Xian “[junior] king,” and when the latter died in four years (nominally becoming “king” Feng Xin or Lin Xin), the appointment went to Prince Xiao, who succeeded as Kang Ding in 1155.
 - 2.3. Later kings continued the practice of before death giving royal status to their heirs: Wu Yi (1145/43–1109), Wenwu Ding (1118–1106), Di Yi (1105–?), Zhou Xin (1086–41), becoming Di Xin in 1068 (with a calendar for his heir Lu Fu, to be Wu Geng).
3. Zhou: Lord of the West Chang died as Zhou Wen Wang in 1050. (*Yi Zhou shu* 23 “Xiao Kai” gives his 35th year as the date of the lunar eclipse of March 1065; he reigned 2 + 50 years.) For political reasons the *BA* makes 1050 the date of the Zhou conquest of Shang, and pushes the conjunction of 1059, together with all pre-conquest Zhou dates, back one 12-year Jupiter cycle; but the Tsinghua bamboo texts say that the conquest of Li, shortly before the conquest of Shang, was in Wu Wang’s 8th year, which must be 1042. *Yi*

Zhou shu 29 “Bao Dian” and 45 “Wu Jing” together imply that Wu Wang became king of China (i.e., conquered Shang) in 1040, dying suddenly in 1038.

- 3.1. The next three Zhou kings were Cheng Wang (1037/35–1006), Kang Wang (1005/03–978) and Zhao Wang (977/75–957). When these three, and five of the last seven Zhou kings, lose their mourning years, the first year of Mu Wang (2+37 years, 956/54–918) becomes 962 in the *BA*, and his last year becomes 908, giving him the 55-year reign in all chronologies (which know nothing of the 2-*yuán* theory). The two kings not in this count are Xiao Wang (872–868), who did not succeed his father, and You Wang (781–771), destroyed without leaving a reign-of-record. (Xiao Wang did not withdraw until Li Wang’s birth in 864; in the *BA* this pushes earlier dates back 4 years, thus reducing Gong Wang, 917/15–900, from 2+16 years to 12 years.)
- 3.2. Xuan Wang’s reign-of-record must be 825–782, but his succession year was 827. 827 is restored in the *BA*, perhaps by the Jin Dynasty editors, following the *Shiji*. But this correction does not reach back to Li Wang (2 + 28 years, 857/55–828, in exile 841–828). Thus the *BA* has 853 (825 + 28) as his first year. Born in 864, he had a royal calendar beginning in 844, revealed by the 3rd-year *Shi Dui gui* (842, 2nd month). The *Shi Li gui* (11th year) reveals that he was still a minor in 847. The “37 years” said to be his pre-exile reign was actually his life-span. (For the explanation of the “37 years” being mistaken as his pre-exile reign, see Attachment VI.)

(1.1 above has the surprising implication that chronologists in China ca. 432–429 BC had access to accurately dated records back to the 20th century BC—seven centuries before the earliest known writing. Perhaps, then, the *Yijing* is right in saying that before writing was invented people used knotted cords (Yi, “Xi Ci” B, 2).)

Key

D. S. Nivison, “The Key to the Chronology of the Three Dynasties: the “Modern Text” *Bamboo Annals*”; in *Sino-Platonic Papers* 93 (January 1999), pp. i–iv, 1–68.

Riddle: Nivison, *The Riddle of the Bamboo Annals*. Taipei: Airiti, Inc., 2009; with index, 293 pp.
 ZPY: Zhang Peiyu, *Zhongguo xian-Qin shilibiao*, Jinan: Qi Lu Shushe, 1987. (Tables of first days of lunar months, starting with 1500 BC.)

Attachment II: Shang kings: dates of first days of first months determine *gan* names

Gan = last digit of cycle number and first syllable of cycle day-name:

$$1 = jia, 2 = yi, 3 = bing, 4 = ding, 5 = wu, \\ 6 = ji, 7 = geng, 8 = xin, 9 = ren, 10 = gui (= jia).$$

The *gan* day date is supposed to be the first day, *shuo*, of the king's first lunar month of his first year, or of his first post-mourning year. A *shuo* (conjunction of sun and moon) is a sidereal event; so these data are astronomical evidence for the 2-*yuan* theory.

An asterisk * indicates an adjustment of one day from Zhang Peiyu (ZPY), *Zhongguo xian Qin shilibiao* (usually when needed to make long and short lunar months alternate).

A king must not use the *gan* of his predecessor; if the *gan* would be the same, he must use his post-mourning *gan*. And *gui* (the *gan* of Tang's father Shi gui) is taboo, defaulting to *jia*.

The double asterisk at #19 calls attention to Pan Geng, who (I conclude) attempted to usurp the succession, claiming Yang Jia's post-mourning four years as the first four years of his own calendar. These years are part of the 28 years given him in all chronologies.

After #24 Zu Jia, the day of appointment as heir is usually what determines the *gan*.

Note that *Bamboo Annals* reign lengths (for 20 out of 30 kings) are normally reigns-of-record, not including mourning years (unlike the *Wenxian tongkao*). This is more evidence for the 2-*yuan* theory.

King	<i>Annals</i> dates	Nivison dates	first days	<i>gan</i> zhi	Generation
	(lengths)	(lengths)	(cycle #)		
2.Wai Bing	1546 (2)	1541–40 (2)	1/11(33)	<i>bingshen</i>	2(?)
3.Zhong Ren	1544 (4)	1542 (3+4) /1539	1/22(39)	<i>renyin</i>	2(?)
4.Tai Jia	1540(12)	1542(3+12) /1539	1/18(51)	<i>jiayin</i>	3

King	Annals dates	Nivison dates	first days	ganzhi	Generation
5. Wo Ding	1528(19)	1527(3+19) /1524	2/4(11) 2/1(24)	<i>jiayu</i> <i>dinghai</i>	3
6. Xiao Geng	1509 (5)	1505 (3+5) /1502	1/3(34) 12/31'03(47)	<i>dingyou</i> <i>gengxu</i>	4
7. Xiao Jia	1504(17)	1497(3+17) /1494	2/3(47) 1/31(60)	<i>gengxu*</i> <i>guihai=jiazi</i>	4
8. Tai Wu	1475(75)	1477(3+60) /1474	1/23(21) 12/22'75(05)	<i>jiashen wu- chen*</i>	5
9. Yong Ji	1487(12)	1414 (2+12) /1412	12/18'15(16)	<i>jimao</i>	5
10. Zhong Ding	1400 (9)	1400 (3+9) /1397	1/11(54)	<i>dingsi*</i>	6
11. Wai Ren	1391(10)	1388 (1+10) /1387	1/28(14) 12/19'88(39)	<i>dingchou ren- yin</i>	6
12. Hedan Jia	1381 (9)	1377 (3+9) /1374	12/29'78(41)	<i>jiachen</i>	7
13. Zu Yi	1372(19)	1365 (2+19) /1363	1/16(02)	<i>yichou</i>	7
14. Zu Xin	1353(14)	1344 (3+14) /1341	1/21(58)	<i>xinyou*</i>	8
15. Kai Jia	1339 (5)	1327 (3+5) /1324	1/15(21)	<i>jiashen</i>	8
16. Zu Ding	1334 (9)	1319 (3+9) /1316	1/16(04)	<i>dingmao</i>	9
17. Nan Geng	1325 (6)	1307 (3+6) /1304	1/3(54) 1/30(37)	<i>dingsi</i> <i>gengzi</i>	9
18. Yang Jia	1319 (4)	1298 (2+4) /1296	1/23(01)	<i>jiazi*</i>	10
19. Pan Geng	1315(28)	1292 (24)**	1/17(27)	<i>gengyin</i>	10
20. Xiao Xin	1287 (3)	1268 (2+3) /1266	1/22(38)	<i>xinchou</i>	10
21. Xiao Yi	1284(10)	1263 (3+10) /1260	1/26(08) 1/24(22)	<i>xinwei</i> <i>yiyou</i>	10
22. Wu Ding	1274(59)	1250 (3+59) /1247	1/4(54)	<i>dingsi</i>	11
23. Zu Ji(heir)	1215(11)	1188 (3+8) /1185	1/8(24) 12/6'86(06)	<i>dinghai</i> <i>jisi</i>	12
Zu Geng		/1185	12/7'86(07)	<i>gengwu</i>	
24. Zu Jia	1204(33)	1177 (2+20) /1175	1/7(20)	<i>guiwei=jiazi</i>	12

King	<i>Annals</i> dates	Nivison dates	first days	<i>gan zhi</i>	Generation
25. Feng Xin	1171 (4)	1175 (4)	1/14(38)	<i>xinchou</i>	13
26. Kang Ding	1167 (8)	1171 ((16+)) 1155/53 (2+8)	1/29(14)	<i>dingchou*</i>	13
27. Wu Yi	1159(35)	1155 ((10+)) 1145/43(2+35)	1/3(12)	<i>yihai</i>	14
28. Wenwu Ding	1124(13)	1145 ((27+)) 1118– 1109, 1108–1106(10+3)	1/13(14)	<i>dingchou</i>	15
29. Di Yi	1111 (9)	1105 (19)	1/21(52)	<i>yimao</i>	16?
30. Di Xin	1102(52)	1106(?)((20+)) 1086–1069 1068–1041 (18+28)	1/2(28)	<i>xinmao</i>	17?

The first king, Tang or “Tai Yi,” declared himself king in 1575, the year after the *cuo xing* celestial display of late 1576 (the planets’ heliacal risings occurring in quick succession). 1572 (allowing three years for mourning-completion in the populace) could have given his *gan*: the post-solstice lunar month began 22 Jan, *yichou*. (In 1542 Yi Yin offered a sacrifice to Tang on an *yichou* day.)

“Di Yi” may simply be a title claimed by Wenwu Ding in 1105 (some later inscriptions mention sacrifices to “Wenwu Di Yi”). Wenwu Ding’s accession year was 1106, perhaps giving Di Xin his *gan*. Wu Yi gave Wenwu Ding a calendar of his own in 1118. Similarly, I assume that “Di Yi” gave his heir (“Zhou Xin”) a calendar in 1086, dying sometime later. A new calendar, I find, began in 1068, first day *gengxu*, and perhaps that year marked both the effective king’s assumption of the title “Di Xin,” and the naming of the heir, Prince Lu Fu, as expectant king “Wu Geng.” The interval 1105–1068 is 37 years, the reign-length assigned to Di Yi in other chronologies.

Events in reigns 2-3-4 appear to be as follows: The 2nd-generation heir Tai Ding died before founder Tang. Tang’s chief minister Yi Yin was trying to make himself king. Tang died early in 1542. His grandson Tai Jia succeeded in that year, 1542-1-0 being mourning years. In 1542 (beginning with a *ren* day) Yi Yin named Zhong Ren acting king while Tai Jia did mourning. Then in 1541 Yi Yin exiled Tai Jia and replaced him by Wai Bing for two years as chief mourner. In 1539–36, four years, Zhong Ren was nominal king, but in 1536, Tai Jia’s seventh year *de jure*, Tai Jia escaped from confinement and killed Yi Yin, taking 1539 as his own *gan* year.

Attachment III: Late Xuan Wang *mingwen* analyzed show that the single *yuan* theory is impossible if these inscriptions are in Xuan Wang's reign

The bronzes in question are (in Nivison, *Riddle*) #53 *Ci ding* through #62 *Qiu ding* II. These two considered alone would allow Xu Fengxian's two halves system. I omit #56 *Yi gui* and #57 *Ge You Cong ding*, because there is a possibility of doubt about how to read the texts' dates. And the latter, as well as #58 *Jin Hou Su bian-zhong*, are provincial inscriptions whose format would not have been controlled by the Zhou court, and #58 at least requires a count from the succession *yuan*. (In #58, "33rd year" applies only to the end of year 33; all of the main action is in ZPY year 34.) I assume a *yin* month calendar in the count from 825 for all except the last two, which require a *chou* month calendar. (I use A, B, C, and D for the four phases, assuming, as does Xu, that A and B must be in the first half of the month and C and D in the second half.)

#53 *Ci ding*, 17/12 B(52): 827 count = 811. 1st of 12th month = (20), lacks (52). 1st of next month = (50), (52) = 3rd. 825 count = 809. 1st of 12th month = (39), (52) = 14th, ok for B. (The 14th is 11 days later than the 3rd.)

#54, *Bo Ju Sheng hu*, 26/10 A(16): 827 count = 802. 1st of 10th month = (59), (16) = 18th, *wrong half* of month for A. 825 count = 800. 12th month 1st = (16), = 1st, ok as A (12 or 13 days later than the 18th)

#55 *Huan pan*, 28/5 C(27): 827 count = 800, 1st of 5th month = (49), lacks (27). 1st of next month = (19), (27) = 9th, *wrong half* of month for C. 825 count = 798, 1st of 7th month = (07), (27) = 21st, ok as C (12 days later than the 9th).

#59 *Bo Kui Fu xu*, 33/8 D(28): 827 count = 795. 1st of 8th month = (49), lacks (28). 1st of next month = (19), (28) = 10th, *wrong half* of month for D. 825 count = 793. 1st of 10th month = (07), (28) = 22nd; D should be 24th or 25th. The 1st of the (ZPY) 10th month probably ought to be (06) (syzygy at 00:21), making the date the 23rd. The *yuexiang* is carelessly "jisi." (The 22nd is 12 days later than the 10th.)

#60 *Shan Fu Shan ding*, 37/1 A(47): 827 count = 791. 1st of 1st month = (30), (47) = 18th, *wrong half* of month for A. 825 count = 789. 1st of 3rd month = (47), ok as A (the 1st is 12 or 13 days later than the 18th).

#61 *Qiu ding* I, 42/5 B(52): 827 count = 786. 1st of 5th month = (58), lacks (52). 1st of next month = (28), (52) = 25th, *wrong half* of month for B. 825 count = 784. 1st of 6th month = (46), (52) = 7th, after long month ok for B. (The 7th is 11 or 12 days later than the 25th.)

#62 *Qiu ding* II, 43/6 B(24): 827 count = 785. 1st of 6th month = (52), lacks (24). 1st of next month = (21); (24) = 4th. 825 count = 783. 1st of 7th month = (10), (24) = 15th, ok for B. (The 15th is 11 days later than the 4th.)

The first and last, #53 and #62, are like the *Que Cao ding* II; a count from the succession *yuan* makes *jishengba* reach back at least to the 3rd of the month; so whatever is said about one of them should be said about each of the three. This is going to tie the three to the five, #54 through #61, because #61 and #62 must be treated alike.

In every one of the five in between, the succession count gives a day date in the *wrong half* of the month, and not just in a wrong quarter. Xu could have handled this situation only by redefining the phase terms making each begin 12 days earlier, thus becoming 12 days longer, each of them overlapping with the preceding two, and spanning more than half a month in total time—a mathematical nightmare, of no conceivable practical use. And the trouble is the same in each case: the unacceptable date is earlier than the acceptable date by exactly the cumulative epact (relative to an ideal 12×30 days) for two years. This ought to be accepted as proof that the 2-*yuan* theory is correct, and the single-*yuan* theory—with its rejection of Wang Guowei—is wrong. The 2-halves theory is the result of accidentally looking at evidence such as the 15th-year *Que Cao ding* first, and doing no more thinking. Anyone looking first at these late Xuan bronze texts would never have thought of 2-halves.

Attachment IV: Dates in the *Jin Hou Su bianzhong*: Zhou 33rd year (795) 10th (*hai*) month = Jin calendar 1st month of 34th year (794)

Day	01	02	03	04	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	
Month																															
1																															
2																															
3																															
4																															
5																															
6																															

After a long month, *jishengba* (after *chuji*) begins on the 7th; similarly after a short month, *jisiba* (after *jiwang*) begins on the 25th. But the *bianzhong* text has “*jiwang, guimao* (40)” followed by “*Jisiba, renyin* (39),” which is impossible. The simplest explanation of the two errors is that the carver (the text was carved) was confused about the *ganzhi* sequence when he moved from bell one to bell two, as argued by Ma Chengyuan. Starting the first date wrong, he was stuck with it.

Why doesn't Xu Fengxian accept this? She notices that Ma, Shaughnessy and I have argued for this solution (p. 182 and note 16), but only after she has decided (p. 181) that the problem is to discover which of the two dates is wrong, assuming just one is. She decides to start with “*jiwang, guimao*,” because “the meaning of *jiwang* is clear.” But it is not, for her: she has decided to adopt “as a working definition” (p. 173, 189) the view that *jiwang* extends to the 21st day (safe: nobody could think it extends that far); after that she treats this “21st day” limit as a fact.

On p. 183 Tables 6 and 7, “in order to better display the trend” (p. 182 top) she extends the depicted range of *jiwang* to the 23rd (Table 6) and 24th (Table 7) but nonetheless holds to the 21st as the allowed limit; and on that basis she finds (p. 182 middle) that there is no day *wuyin* (15) in the 6th month, and therefore of the two problematic *jiwang-jisiba* days it is the former which is erroneous, hence (we should see) the other is ok. It is in the bottom paragraph of p. 182 that she says “others” have suggested switching *guimao* and *renyin* to *renyin* and *guimao* (with footnote 16). But we never hear more of this. For the remainder of the discussion of the bells, she always assumes that “*jisiba renyin*” is correct, and something else must be found for *guimao* after *jiwang*. She considers *xinmao* (the Project's choice) and *guisi*. (But these would be in the wrong half of the month. To this Xu would have to reply that the *bianzhong* belongs in the pre-exile Li Wang reign—revealing that she is not following her own rule, of being blind to actual or claimed dates.)

Why? The second paragraph on p. 185 begins strangely, “If one assumes that ‘2nd month, *jisiba, renyin* (39)’ is not wrong, then 2nd month *jiwang, guimao* (40)’ is wrong,” i.e., “if not not A then not B.” This is logically equivalent to “not A or not B”; and in most natural languages such an “or” statement is ambiguous, easily used to mean “either not A or not B but not both.” This seems to be the way Xu is thinking—conveniently, because if she admits that the *jiwang* day referred to immediately precedes the *jisiba* day referred to, she admits that Wang Guowei is right: the four phases are distinct, with no overlapping, and *jishengba* and *jisiba* are the second and fourth *phases*, not the first and second *halves*; and this implies that the 2-*yuan* theory is correct, because I thought out the 2-*yuan* theory by asking what I had to assume to save Wang Guowei's 4 phases theory. (I quickly found more evidence.)

Working out the absolute date of the *bianzhong* removes room for reasonable doubt about the month and day table above. The phase terms match year 34 (794 BC) as a *hai* year. But other Zhou bronzes at this time call for a *yin* calendar. So the month numbers have to be Jin numbers, although the beginning year number is the Zhou 33rd year. The first date is in the Zhou 10th month of Zhou year 33, which is the Jin 1st month of the next year.

Turning to ZPY for 794, we find *wuyin* first-of-month dates for months 3 and 5, but 5 is called month 6 in the inscription, showing that Jin was using a *hai* calendar. If the *chuji wuyin* 6th month date is the first of the month, as above, this requires *renyin* and *guimao* in month 2 to be the 24th and 25th, which is just where the *jiwang-jisiba* division should be, as shown in Attachment V. In the *bianzhong*, it is between bell 1 and bell 2.

Therefore the correct correction is as Ma Chengyuan argued, simply to put the *ganzhi* in proper order.

Attachment V: The Zhou conquest campaign, showing lunar phases; “Xia” calendar months 11 through 4, 1041 BC 23 Dec through 1040 BC 17 June

	11	12	1	2	3	4
1	(05) 23 Dec	(34) 21 Jan	(04) 20 Feb	(33) 21 Mar	(03) 20 Apr	(32) 19 May
2	(06) 朏	(35)	(05) 朏	(34)	(04) 朏	(33)
3	(07)	(36) 朏	(06)	(35) 朏	(05)	(34) 朏
4	(08)	(37)	(07)	(36)	(06)	(35)
5	(09)	(38)	(08)	(37)	(07)	(36)
6	(10) 在生霸	(39)	(09)在生霸	(38)	(08) 在生霸	(37)
7	(11) 既生霸	(40) 在生霸	(10)既生霸	(39) 在生霸	(09) 既生霸	(38) 在生霸
8	(12)	(41) 既生霸	(11)	(40) 既生霸	(10)	(39) 既生霸
9	(13) 旁生霸	(42)	(12)旁生霸	(41)	(11) 旁生霸	(40)
10	(14) 冬至	(43) 旁生霸	(13)	(42) 旁生霸	(12)	(41) 旁生霸
11	(15)	(44) 既旁生霸	(14)	(43) 既旁生霸	(13)	(42) 既 1 旁生霸
12	(16)	(45)	(15)	(44)	(14)	(43) 2
13	(17)	(46)	(16)	(45) 春分	(15)	(44) 3
14	(18)	(47)	(17)	(46)	(16)	(45) 4
15	(19)	(48)	(18)	(47)	(17)	(46) 5
16	(20) 既望	(49)	(19) 既望	(48)	(18) 既望	(47) 祭 6 祀
17	(21)	(50) 既望	1 (20)	(49) 既望	(19)	(48) 既望
18	(22)	(51)	2 (21)	(50)	(20)	(49)
19	(23)	(52)	3 (22)	(51)	(21)	(50)
20	(24)	(53)	4 (23)	(52)	(22)	(51)
21	(25)	(54)	5 (24)	(53)	(23)	(52)
22	(26)	(55) 渡河	6 (25)	(54)	(24)	(53)
23	(27)	(56)	7 (26)	(55)	(25)	(54)
24	(28) 既死霸	(57) 既望	8 (27)既死霸	(56)	(26) 既死霸	(55)

	11	12	1	2	3	4
25	(29) 旁死霸	(58) 既死霸	(28) 旁死霸	(57) 既死霸	1 (27) 旁死霸	(56) 既死霸
26	(30) 始征	(59)	(29)	(58)	2 (28)	(57)
27	(31)	(60) 立春	(30)	(59)	3 (29)	(58)
28	(32)	(01)	(31)	(60)	4 (30)	(59)
29	(33)	(02)	(32)	(01) 牧野	5 (31) 立夏	(60)
30		(03)		(02)		(01)

Qi-center days are underlined, and named as *qi*-center days, unless an event is named: e.g., Muye 牧野 is Qing Ming 清明 Day, identified as the day of the battle in the last line of the “Da Ming” ode (#236) in the *Shijing*: 肆伐大商，會朝清明. (Traditional commentary, and also Karlgren, has “clear and bright” for 清明—a reasonable guess, if one does not know the date.) Winter Solstice Day (Dong Zhi 冬至, the first *qi*-center day) is two days later than the actual winter solstice day, because the Chinese of the time were not aware that the interval from autumn equinox to winter solstice was 89 days rather than 91 days. All of the remaining *qi*-center days are here dated accordingly, including Qing Ming Day.

I have changed Zhang Peiyu’s first days, for month 1 from (03) to (04), and for month 3 from (02) to (03), to avoid having two short months in sequence.

Prof. Li Xueqin neatly disposes of the question addressed here, in his article “Lunar Phase Terms in the *Shang shu* and the *Yi Zhou shu*” (*Shang shu yu Yi Zhou shu de yuexiang*), *Xia Shang Zhou niandaixue zhaji* pp. 125–133 (January 1998). His clearest evidence is the text beginning the “Shao gao” (*Shang shu* 32). Actions over a sequence of days are dated as follows: 2nd month, *jiwang* (the 16th, after a long month), +5=*yiwei* (32); 3rd month, *bingwu* (43), *fei* (the 3rd, after a short month), +2=*wushen* (45), +2=*gengxu* (47), digging begins on the foundations of Luo. The text beginning the “Kang gao” (*Shang shu* 29) identifies the day as *zaishengba*, which must be the day before *jishengba*. Therefore *jishengba* after a short month is (or begins on) the 8th. The year must be 1031, the last year of the Regency. Zhang’s *Shilibiao* p. 41 has the 2nd month (after a long month) 1st day=*yihai* (12), and 3rd month (after a short month) beginning *jiachen* (41).

If these texts are valid, they refute Xu; but they are not bronze texts, so for her the 4-quarters theory is still “without foundation” (p. 198). For 1040, I use *Han shu* 21B, Liu Xin quoting “Wu Cheng”; and *Yi Zhou shu* “Shi Fu” with *yiwei* (32) as 1st of month 4, and *bingwu* (43) as *pangshengba* in “month 1” (= month 2).

Attachment VI: The Reign of Li Wang According to the *Shiji*

A: Evidence for a reign before exile of 37 years, as in the Xia-Shang-Zhou Project's chronology:

“Zhou Benji”: 夷王崩, 子厲王胡立, 厲王即位三十年, 好利...諸侯不朝。三十四年, 王益嚴, 於是國莫敢出言。三年乃相與畔, 襲厲王; 厲王出奔於虢。King Yi died, and his son Hu, King Li, became king. [When] King Li had ruled for 30 years he was fond of profit.... The regional lords did not come to court. In the 34th year the king became more severe, Thereafter no one in the country dared to speak against him. After three years, they rose together and attacked King Li. King Li fled to Zhi.

34+3=37; but Sima Qian (or his father) must be misunderstanding his source, which probably meant by the words “厲王即位三十年” that Li Wang was king for 30 years—including his exile, and also his initial mourning two years. Actually, Li Wang's life-span was probably 37 years. An original note in the *Bamboo Annals* says he was born in 864. Furthermore, the *Shiji* text continues thus: 厲王太子靜匿召公之家; 國人聞之, 乃圍之。召公 ... 乃以其子代王太子; 太子竟得脫, “King Li's eldest son Jing was hidden in the household of Duke Shao; the people heard about this and surrounded the place.... Duke Shao ... then substituted his own son for the crown prince, who ultimately escaped.” This must put a pre-exile “37 years” in doubt. As Lei Xueqi notices, for the rabble to be so easily deceived into accepting Shao Gong's son for Li Wang's eldest son Prince Jing, both must have been swaddled infants, hardly likely if Li Wang had already been ruling for 37 years.”

B: The evidence for a pre-exile reign of not more than 18 years (actually 16):

“Qi Shijia”: 哀公時, 紀侯譖之周; 周烹哀公, 而立其弟靜; 是為胡公。胡公徙都蒲姑, 而當周夷王之時。哀公之同母少弟山 ... 殺胡公而自立; 是為獻公.... 九年, 獻公卒, 子武公壽立。武公九年, 周厲王出奔。 In the time of Duke Ai, Ji Hou made charges against him to Zhou, and [the] Zhou [king] had him boiled in a cauldron, installing his younger brother Jing; he was Duke Hu. Duke Hu moved his capital to Pugu, and was a contemporary of King Yi of Zhou. Duke Ai's younger brother Shan ... killed Duke Hu and made himself duke, as Duke Xian.... In his 9th year Duke Xian died, and his son Shou succeeded him as Duke Wu. In Duke Wu's 9th year King Li of Zhou fled.

Li Wang fled into exile in Zhi in 842, =Wu Gong 9, so Wu Gong 1=850, and Xian Gong 1 (of 9) =859. Therefore Hu Gong was killed in 860, probably still in Yi Wang's reign (as indicated above). The *Bamboo Annals* dates Ai Gong's execution to Yi Wang 3, which would be (in my reconstruction) 865. (Perhaps the

unfortunate Ai Gong was accused of supporting the party of the usurping Xiao Wang, who probably did not withdraw—or get eliminated—until 864, when Yi Wang finally had an heir. I assume that there were two kings at once, 867–864. My analysis of the *Bamboo Annals* (“modern text”) gives me 2+8 years, 867/865–858 as Yi Wang’s reign, and 2+28 years, 857/855–828, as Li Wang’s (*de jure*) reign.)

“Wei Shijia”: 頃侯厚賂周夷王，夷王命衛為侯。頃侯立十二年卒，子釐侯立。釐侯十三年，周厲王出奔於斃。Qing Hou gave rich gifts to King Yi of Zhou, who raised the Wey lord’s rank to *hou*. When Qing Hou had reigned 12 years he died, being succeeded by his son Xi Hou. In Xi Hou’s 13th year King Li of Zhou fled to Zhi.

Xi Hou 13=842; so Xi Hou 1=854, and Qing Hou 12=855. Thus Qing Hou 1=866. If Qing Hou’s status was raised to “*hou*” by Yi Wang, then Yi Wang was king at least as late as 866.

If one accepts evidence A, one must “explain away” evidence B, and vice versa. The likely explanation eliminating A is obvious: some historian misread his source, and so was led to suppose that “37 years” was Li Wang’s pre-exile reign length rather than his life-span. The original source could have said “Li Wang reigned for 30 years (857–828).... In his 14th year (844)¹ [the *yuan* was changed to ‘1st year’, and] he became more severe. In the 3rd year (842), ...” (etc.). Bronzes #35, #38 and #41 in *Riddle* (by Shi Li and Shi Dui) show that Li Wang succeeded in 857 (at 8 *sui*), still had a regent (probably Gong He) in 847, and had a royal calendar beginning 844 (when he was 21 *sui*). Thus 842 was his 3rd year as king holding full power, which he now used without restraint. “Explaining away” B would not be easy; for the apparently independent narratives in the two “*Shijia*” chapters support each other.

1 Assuming 迄 instead of 三 before original 十四年, and after it an original 改元稱一年 deleted or forgotten.

21 The “31 Years” Problem

I was a member of the faculty of Stanford University during most of the years 1948–1988, teaching Chinese and Philosophy, and doing research in philosophy and in Chinese history. I am now 91. In 1979, I was directing a small seminar on Western Zhou bronze inscriptions. This led to my discovering that the text of an ancient chronicle, long dismissed by everyone as a fake, is actually authentic. The book, unnamed as discovered, has been given the descriptive name *Zhushu jinian* 竹書紀年 (“annals written on bamboo,” or “Bamboo Annals”; hereafter *BA*).

This chronicle was discovered ca. 280 CE (Western Jin Dynasty) by a peasant perhaps foraging for firewood. The site was a royal tomb or storeroom apparently tunneled into a dry hillside, filled with books and other treasures, of a king of the ancient state of Wei 魏, who died in 299 BC or soon after that. The books were bundled bamboo strips threaded together. These were quickly brought to the Jin capital, where some of the books, including the chronicle, were transcribed into currently used script by court scholars. The project was interrupted (by politics, and the death of one of the scholars), but was reopened around 290 CE by another group.

The work of the second group survives only in scattered quoted fragments, and for many centuries it was believed that this must have been the fate also of the first group’s work. But printed texts of the *Bamboo Annals* began to appear in the 16th century (late Ming Dynasty). It soon caught the interest of scholars, and a long reprint with commentary was published in the middle 18th century. But editors of the mammoth “imperial Manuscript Library” *Siku Quanshu* in the late 18th century examined it carefully and concluded that it was a fake. Most prominent scholars agreed. The book continued to have a few defenders; but the matter was settled to general satisfaction in 1917, when Wang Guowei published a collection of the quoted fragments, calling it the “*Guben*” (ancient text), and also a new edition of the Ming text. This he called the “*Jinben*” (“Modern Text”). In this, Wang gave a possible source for every sentence.

But, of course, if it were not a fake, it might sometimes actually be the source of the “sources”; and one crucial class of material Wang could not explain was the dates in the book. It pretends to cover Chinese history from the late 2400’s (my date is 2402 BC) to 299 BC. The earliest date in any history generally agreed

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to be true is 841 BC, identified in the *Shiji* as the first year of the 14-year regency of Gong He during the exile of the 10th Western Zhou king Li Wang.

So the authenticity question is not trivial. After convincing myself that the book is authentic, I have used much of my research time during the past thirty-five years proving my case, and also deciphering what the book gives me. I can prove that most of the chronology in it before 841 is wrong; but I think that I have found ways to deduce the correct dates from the dates the “*Jinben*” gives me. Meanwhile, the Chinese government (PRC) had financed a massive “Three Dynasties Project” (1996–2000) aiming to do what I had been doing. This Project has ignored the *Bamboo Annals*. The Project’s published results are almost completely wrong, and I have become probably one of its most prominent international critics. This led to a book I published in 2009. I am now working on a revision and Chinese translation of that book. All of this is timely: Qinghua University in Beijing has recently acquired the original bamboo text of a similar chronicle. Articles are beginning to be written about it. Its authenticity is beyond question. Its chronological scope is much less than that of the “*Jinben*” *Zhushu jinian*; but points of agreement between the two texts appear to be showing that the authenticity of the “*Jinben*” *Zhushu jinian* too should now be beyond question.

These are the basic dates in what follows (*Annals* dates on left, correct on right):

Yao	2145	2026
Shun	2042	1969
Yu and Xia	2029	1953*
Shang	1558	1554*
Zhou	1050	1040

The Project ventured back to 1250, with a few guesses before that. (*D. W. Pankenier’s dates)

I shall here try an experiment. People are having trouble understanding my methods, and my application of them to explain how actual dates got changed into the dates in the *BA*. So instead of merely filling up another book with this (which I am doing anyway), I am going to try presenting the essence of the argument in a few pages, so that it can be seen all at once. (I will omit my recovered strip text.) At the end, I will try to show why all this matters, by presenting an example of what you can do with a reliable chronology, and then probing the philosophical-epistemological interest of what I have been doing.

On page 180 of my book *The Riddle of the Bamboo Annals* (Taipei: Airiti Press, 2009), I pose a problem which I do not quite solve:

2.4.3 The conjunction marking the beginning of Xia (as discovered by Pankenier) occurred in February of 1953, which was the 14th year of Shun, according to Pankenier, when Shun transferred *de facto* authority to Yu. The extension of chronology backward (to get Yao 1 to be 2145) involved moving this date back one *bu* of 76 years, to 2029, as in the *Jinben*.¹ Pankenier noticed that the date of the conjunction must be Shun 14, because he noticed an echo of the political event of transfer of authority to Yu in the chronicle for Yao: in 2060 = Yao 86, which was the 14th year after Yao abdicated and Shun assumed *de facto* power. At that point Yu is given audience and the use of the Dark Scepter (*Xuan Gui*) symbolizing authority (emblem in the sky, Pankenier argues, by the configuration of the conjunction of the five visible planets). This was exactly 31 years before the presumably actual event of 2029, re-dated from 1953. The 31 years (the length of the inserted reign of Di Gui) begs for an explanation. The question is open, but I will hazard one, hoping that someone else can do better.

I then did hazard one, but I think I can do a bit better now. The explanation solves the last problem in my chronology and ties it all together. I will review the whole argument, and then fit the explanation in place. In doing this, I will be omitting much of the details supporting the argument.

First: mourning for a deceased person of importance—one’s father, or one’s king—was a prehistoric institution, and lasted (25 or 27 months) less than three years, but was normally long enough so that it could not be completed in two years. It was an obligatory period of inactivity for a new king. In the *BA* (I start with Yao, where *ganzhi* names of years begin to be inserted by the Jin editors) at first the record is explicit: there were three calendar years of mourning after Yao, Shun and Yu (altered, I would argue, from an original two calendar years of mourning-completion).

For the rest of the Xia Dynasty mourning is not mentioned; but the use of *sui*-names (*ganzhi* for years) inserted by the Jin Dynasty editors imply gaps between reigns, most often two years, which I assume are for mourning. (One gap is a fictional 40 years, and the last reign is a fictional 31 years, inserted to push the beginning date back: see below.) We can therefore make a distinction between a king’s succession year, following the death of the preceding king, and his accession year, following mourning.

¹ I insert an explanation: Chronologists of the so-called “Warring States” era (sometimes dated 479–221 BC) often used the ancient intercalation cycle: assuming 365.25 days per year, there must be 7 intercalary lunar months in 19 years = 1 *zhang*, 4 *zhang* = 76 years = 1 *bu*, 20 *bu* = 1520 years = 1 *ji*. The first day in a *zhang* is winter solstice day. Dates of solstices and equinoxes, and *ganzhi* dates for days of months, are (it was supposed, incorrectly) repeated from *ji* to *ji*. This is essentially like the system developed by Meton of late 5th-century Athens and refined a century later by Callippus in the circle of Aristotle, echoing work in Babylon. It is not known whether the Chinese system is independent.

In the Shang and Western Zhou parts of the *BA* chronicle there are no gaps between reigns. But mourning continues to shape chronology, because the gaps were present though not recorded, and during the 300’s BC they came to be forgotten or ignored: Normally a king’s year of death in the chronicle is counted from his accession date. The result for both Shang and Western Zhou is that the reigns of the fifth generation kings—Tai Wu in Shang, Mu Wang in Zhou—are stretched. (After Western Zhou, reign counts are always from the succession year, but the king continued to wait until after completion of mourning before formally calling himself “*wang*” (king).) This basic structure of received chronology is further distorted in ways that have to be discovered.

These further distortions are caused by numerology and astrology, motivated by politics, and by *zhengtong* (correct succession) theory. The part of the *Bamboo Annals* into which the Jin editors have inserted *sui*-names for exact dates begins with the reign of Yao; and it is in Yao 75 that Yu, who was to become the first ruler of Xia, first appears. Therefore the Jin scholar-general Du Yu, who examined the text (or most of it) shortly after its discovery ca. 280 CE and reports that it began with Xia, probably means that it began with Yao. The text Du Yu saw must have lacked the first strips, covering Huang Di, Zhuan Xu, and Di Ku. Why this is so is not known; but Pei Yin (*Shiji jijie*) quotes Xun Xu and He Qiao, who worked on the text soon after Du Yu saw it, as saying that it began with Huang Di, as it does now.

A date in the Huang Di part is linked by intercalation cycle arithmetic to 453, the date of the battle (the defeat of Zhi Bo by Zhao, Han and Wei) that made Wei an independent state; so we know it is authentic.² The discovered *BA* text, finalized ca. 300 BC, was based on an earlier one done ca. 400. The later one was Wei propaganda; it is likely that the pre-Yao parts are Wei creations. There are, however, obvious Wei modifications in later parts of the text (as shown below).

The earlier work was done in Lu, and promoted the prestige of Zhou. This required making 2145 the first year of Yao: 2145 was 1000 years before 1145, the first year of the 27th Shang king Wu Yi, and the year when he gave court status to Dan Fu, lord of Zhou. The Zhou founding ancestor Hou Ji had been (it was

² A ritual and supernatural event is found in the chronicle at Huang Di 50, with a long subtext marking it as important. The date in both text and subtext is 7th month (I assume the Xia-*zheng* first day), *gengshen* (57). A fragment of the *Zhushu jinian* from the *Lu shi* says there were seven years of mourning after Huang Di’s death. This implies that the 50th year was 2353. The date is 100 *zhang* (1900 years) before 453. The first of the (Xia-*zheng*) 7th month of 453 was day *yihai* (12). The *zhang-bu* intercalation cycle requires that in calculating back 100 *zhang* (= 25 *bu*) from a given day, one moves the *gan* for the day back 15. In the 60-day cycle, (12) minus 15 is (57). (I assume that Huang Di is mythical.)

claimed) Minister of Agriculture for Yao. The Lu text also must have made 1045, a century after 1145, the date of the Zhou conquest. This gave great prominence to Zhou Gong Dan, the ancestor of the dukes of Lu, by making his seven-year regency a separate regime in history, preceding the accession reign of Cheng Wang, instead of being merely coincident with the first seven years of Cheng Wang’s succession calendar. (2145 was also a *bu* first year in the Lu Li intercalation calendar.³)

But Yao’s actual first year was not 2145; it was 2026, by my calculation. History had to be improved. This was done by extending Yao’s reign from 58 years (when his son Zhu was exiled and Yao himself was retired by Shun) to 100 years. Also, the first year of Xia was moved back one *bu* of 76 years from the conjunction date 1953 to 2029. (The one more year needed came from extending the mourning completion for Yao from two years to three.) Treating Shun 14 as the first year of Xia implied that the long reign of Yu must be first a period of *de facto* power, Shun 14 through Shun 50, plus mourning for Shun, followed by a short 8-year *de jure* reign. (Shun may not have reigned an even 50 years; if he did not, one must assume a correspondingly longer *de jure* reign for Yu.)

Xia became 76 years longer only by “borrowing” time. The debt was cut to 72 years by the handling of the solar eclipse of Zhong Kang 5. Its actual date—1876, 16 October (*shuo* of month 9), as discovered by K. Pang—moved back one *bu* had become 1952. A reference in the *Zuo zhuan* required that the eclipse be when the sun was in Fang, in the lunar zodiac.⁴ I guessed this was tested by checking the date 432, one 1520-year *ji* (20 *bu*) cycle later. (Correlations of *ganzhi* with month dates, and dates of solstices, were supposed to be invariant from *ji* to *ji*.) That did not work, nor did later years until 428, which had a Xia *zheng* 9th month beginning October 28, day *gengxu* (47).⁵ So the eclipse was re-dated (428 plus 1520) to 1948,

3 See Zhang Peiyu, *Zhongguo xian-Qin shilibiao* p. 252; find *wuzi* (25), 625 at left, under Lu Li, and count back 1520 years (= 1 *ji*).

4 *Zuo zhuan*, Zhao 17.2

5 See Zhang, *Shilibiao* p. 91. In the year 428, there must be an intercalary 8th month, so that Zhang’s 12th month is the Xia-*zheng* 9th month. For this calculation I use the system which I employ successfully on late Shang material in my book *The Riddle of the Bamboo Annals*, Appendix 4, Supplement 2. I assume that a lunar month lacking a *qi*-center is intercalary. (A *qi*-center is the winter-solstice day and every other first day of a weather-period thereafter.) I count from the summer solstice, using lengths of the 24 weather periods as given in *Huainanzi*, “Tian Wen,” 12th paragraph. (See *Riddle* section 1.4.11; it follows that the official winter solstice day was two days late.) Every one of the ancient “Six Calendars” gives *jiyou* (46) rather than *gengxu* (47) for this date in 428; see Zhang p. 180. This implies that the “Six Calendars” were much later, and that the persons producing the Xia chronicle were using more nearly contemporary data on the year 428; the real date for the first day of its 9th month was *gengxu*.

9th month, *gengxu*. The four years (1952 to 1948) had to be supplied by increasing the preceding inter-reign gap total by 4. Next, the 72-year debt was much reduced by major historical fiction: the two-year interregnum after the fifth king Xiang was made a 40-year story of a war-lord named Han Zhuo. This used up 38 years leaving a debt of 34. This 34 got increased to 35, when Xia was reshaped to make the first and second 8-reign periods each 200 years. The next year is 1589, the first year of the invented 17th king Di Gui (Jie), whose 31-year reign cuts the debt to four years. Then comes 1558, the *BA* date for the beginning of Shang. This is four years early: Pankenier has shown that Shang began in 1554, which was 496 years before the Zhou Mandate year 1058, the year after the Zhou-heralding conjunction of 1059.

Let us reconstruct Xia’s real history rather than its *BA* history. I assume reign lengths as in the *BA*, but gaps between reigns as always 2 years, for completion of mourning (no gap after 11th king Bu Jiang, who retired). Xia’s beginning I take to be February 1953, Pankenier’s date for the Xia conjunction. This implies Pang’s date 16 October 1876 for the Zhong Kang eclipse. So my reconstruction of *BA* history is being confirmed. Also strongly confirmed is my assumption that the persons working out the *BA* chronology were using the intercalation cycle, and were applying it to actual dates, 1953 and 1876; for this is the only way to explain the errors 1948 and *gengxu*. *Therefore these persons had in front of them an accurate chronology back at least to 1953*, which they were systematically twisting out of shape for political reasons.⁶

Continuing with the real history of Xia, using the same assumptions—reign lengths as given, gaps after a reigning king’s death always two years—I get 17 February 1577 BC (JD 114 5471) as the first day of the reign of the 14th king Kong Jia. This day was a *jiazi* (01) day.⁷ One must therefore try assuming that *gan*-names of kings were determined by the first days of their reigns. There is at least

⁶ This is perhaps the most important argument in my study of the *Zhushu jinian*. It proves, I think, that an accurate chronology of events from the 20th century BC or earlier existed and was used by the persons who produced the Xia part of the *BA*, probably in early to middle Warring States. If this is true, it is a reasonable assumption that later Warring States persons responsible for other parts of the *Zhushu jinian* had the same resources. And if they did, then if one can determine their motives and methods, one can use the dates in the present text to deduce or confirm what the actual dates were, for most of the period covered in my study. This is what I have been trying to do. (We scarcely begin to have this kind of chronological control of the history of the contemporary Near East, though we have more detail for the ancient Near East than the *BA* and other sources provide for ancient China.)

⁷ To obtain the *gan* for a Julian Day number, divide by 60 and subtract 10 from the remainder (or add 50 to the remainder if it is less than 10).

one other example in Xia: The *BA* has 1718 as the succession year of Shang ancestor Shang Jia Wei.⁸ In the Shang calendar the first day would be the first day of the post-solstice month, which was 18 January, JD 109 3941, *jiayu* (11)—a *jia* day. Pankenier’s last year for Xia 1555 turns out to be the last year of Fa, 16th king. So the 17th king Di Gui (Jie), reign 31 years, is fiction. (And so poor king Fa must bear the opprobrium of being the real Di Gui. The first day of his succession year was *guiyou* (10).) The thirty Shang kings all have *gan* names.

Meanwhile Shang history was also being improved. There were four overlapping reigns (frowned on by *zhengtong*-minded chronologists): Zhong Ren, the second of Yi Yin’s puppets during the second king Tai Jia’s imprisonment, whose four years were the first four of Tai Jia’s accession reign; also 19th king Pan Geng, whose claimed first four years were the four of the accession reign of his elder brother Yang Jia. (There were four kings in that generation instead of the standard two, pointing to repeated attempts at fraternal usurpation of the succession.) The 23rd king Zu Geng’s 11 years were claimed by Zu Jia, usurping the succession. And finally the last king Di Xin, killed in 1040, was deemed no longer *de jure* king when Zhou promulgated its royal calendar in 1056, 16 years earlier. The total was 35, in agreement with the Xia remaining year-debt (before inventing Di Gui), moving the first year of Shang back from the correct 1554 to 1589.

Finally, assumed but unrecorded mourning-completions during Shang and Western Zhou disappeared during the 300’s. The 8th Shang king Tai Wu’s first year was set back 1 year from 1474 to 1475, a century after the first year of the founder Tang’s royal calendar. This extended Tai Wu from 60 to 61 years. Four 3-year mourning-completions prior to Tai Wu’s accession disappeared, leaving a gap of 12 years, filled by the 12-year accession reign of Tai Wu’s *successor* Yong Ji (thus reversing their order), and Tai Wu’s credited tenure was extended down through the 2+12 years that had been Yong Ji’s, giving him 75 years. Dropping these four mourning-completions had no further effect.

Neither did mourning-completions after 22nd king Wu Ding. The mourning-completions beginning the reigns of 23rd king Zu Geng and 24th king Zu Jia were included in the 33 years claimed for Zu Jia, and the mourning-completions beginning the reigns of 26th king Kang Ding and 27th king Wu Yi, two years each, were deleted but balanced by giving Zu Jia’s son and first heir 25th king Feng Xin a 4-year reign, although he never reigned but died before his father. After Wu Yi, the problem disappears, because reigning kings made sure their sons succeeded them by appointing those sons “kings” (with calendars) before their own deaths.

⁸ I am assuming that after the chronology as in the present text was worked out, independently known dates were translated into it.

We are left with the problem of mourning-completions beginning the succession reigns of 10th king Zhong Ding through 22nd king Wu Ding. When these were deleted, a 31-year chronological vacuum was created that had to be filled. To understand what had to be done, we must first examine Western Zhou.

The *BA* gives the 5th Zhou king Mu Wang the reign 962–908. He was preceded by three kings whose succession years are between the Zhou conquest and Mu Wang 1: Cheng Wang, Kang Wang and Zhao Wang. Mourning-completions in Western Zhou are always two years; so Mu Wang 1 must be 956 (=962 less 2×3). After Mu Wang there were seven kings, but the 8th king Xiao Wang was the uncle of the 7th king Yih Wang, who was probably still alive at the beginning of Xiao Wang’s reign, so there was no mourning to be completed. And the last king You Wang was killed in the destruction of his capital, so no mourning at the beginning of his reign was reflected in an official record. Therefore Mu Wang’s reign in the *BA* lasts 10 (=2×5) years too long.

Post-Mu Wang chronology is complicated in other ways. The *BA* dates for 6th king Gong Wang ought to be 907–892, 2+16 years minus the “2” and pulled down ten; instead, he gets only 12 years, 907–896. 7th king Yih Wang, 2+25 years, gets the 25 years, but they begin four years earlier than they should. The cause seems to be that Xiao Wang did not withdraw until Yih Wang’s son 9th king Yi Wang had produced a son and heir, after four years of reign; and this overlap was not recognized: Xiao must have 5+4 years, pushing reigns back 4, and cutting Gong Wang to 12. Complicating the picture further, I would expect that 11th king Xuan Wang, dates 827/825–782, would have had years 825–782 in the *BA*, with 10th king Li Wang, dates 857/855–828, getting 28 years (including the Gong He Regency). In the *BA* his reign does begin in 853 as one would expect, but Xuan Wang’s true succession year has been restored, either before the text was buried or by the Jin editors.

It is the deletion of mournings before Mu Wang that determines what is done with the Xia-Shang transition. There were three, 3×2=6, moving Mu Wang 1 back from 956 to 962. But it continued to be remembered that Mu Wang’s reign had begun exactly 100 year after the beginning of Zhou, in some sense. The sense seems to be that Wen Wang had begun a royal calendar (possibly for his heir Wu Wang) in 1056, when he moved to a new capital. But now the beginning of Zhou had to become 1062. How?

There were competing claims to the date of the Zhou conquest. I think I have proved that it was 1040. But the *BA* date 2145 for Yao 1 is probably based on this being 1000 years before 1145, which was Wu Yi 1, the date when Wu Yi recognized

Dan Fu (“Tai Wang,” in Zhou history) as ruler of Zhou. The chronology that developed as a result (by the Lu group of chronologists ca. 400) seems to have dated the Zhou conquest to 1045, just 100 years later.

But the *BA* in its final pre-burial form was the work of chronologists in Da Liang in Wei. They were not interested in Zhou and Lu. They left the Lu features in the text if they had no reason to change them, but they needed to change the conquest date. Their task was to make history justify the claim of Wei to be a kingdom, and this meant supporting the claim of Huicheng to be *wang*. His royal calendar begins in 334, and he had announced this in 335. The Da Liang experts therefore made the appointment of Tangshu Yu to the fief of Tang, beginning the Jin state which became Wei, be in the year 1035, just 700 years earlier. The *Guo Yu* says that when Jin began Jupiter was in Da Huo (station 10 of 12), and also that when Wu Wang set out to conquer Shang Jupiter was in Chun Huo (station 7). This requires that the conquest be in 1050, as it is in the *BA*. But the *BA* also says—truly—that Wen Wang died nine year after the conjunction heralding Zhou, which we know to be the conjunction of May 1059. It was not acceptable for Wen Wang to have died in the year of the conquest; so the conjunction of 1059 was set back one Jupiter cycle to 1071, allowing Wen Wang to die in 1062.

1062, which was 100 years before 962, could now be taken as the beginning of Zhou. It was both the last of Wen Wang’s nine *shou ming* (“receive-Mandate”) years, and the first year of Wu Wang’s exercising power as king.⁹ (Wen Wang died in the 3rd month.) Furthermore, it made 1061 be Wu Wang’s succession year, so that the conquest year became “12th year,” as it had been (in a different sense) for the defenders of the date 1045, who counted 12 from 1056.

But also the *BA* Shang summary (and one of the “apocrypha”) say that Shang lasted 496 years—correct: 1554–1059. If you no longer think of 1058 as the “Mandate” year, nor of 1056 as year 1 of 12, but must instead think of Zhou as beginning in 1062, then the first year of Shang must become 1558.

1558 was just 31 years after 1589. So when the 31 years of mourning-completions for the reigns of Zu Ding through Wu Ding were deleted, all pre-1558 dates moved down 31 years. So what had been 2029, *qua* Shun 14, suddenly became 2029 *qua* Yao 86. (31 years: 14 years for Shun, 3 years before Shun for mourning, plus the difference between 86 and 100 for Yao.¹⁰) *Which was the real Shun 14? I*

⁹ *Yi Zhou shu* 25 “Wen Zhuan” opens with the date “Wen Wang ‘shou ming’ zhi jiu nian” (“the 9th year of Wen Wang’s ‘receiving the Mandate’”).

¹⁰ One must reject the idea that the number “31” was deliberately chosen, other elements of the chronology then being adjusted to fit it. (*Ganzhi* for first days of months are repeated almost exactly at 31-year intervals.) The “Jie” interval must first have been 35 years, reduced to 31 when

know this experience only too well. My right eye has been half blind, and crossed leftward, since birth. Ordinarily my brain simply shuts off awareness of visual input from my right eye. But in the late evening (like right now), when I’m tired, I don’t always get this relief. As a result, I can look at a picture on the wall and see it in two places at once, its real position, sharply, and also its crossed position, dimly, to the left.

I suggest that in Warring States China, people were having this kind of trouble with time. First they would see Yao, Shun and Yu as if 1589 were the first year of Shang. Then they would see a picture letting that first year be 1558. The problem was 2029 as the year when Yu of Xia received divine authority. Where was it? Notice the force of the problem: 1589 as first year of Shang seemed to be proved by two independent calculations, counting back through Shang undoing overlaps, and counting down through Xia, adjusting dates using the intercalation cycle and making 8 reigns + 8 reigns ideally 200 years each. But also 1558 seemed to be proved by two independent calculations, one being pulling dates down 31 by deleting mournings from Zhong Ding through Wu Ding, and the other by counting back 496 years from 1062 instead of 1058. The result was a chronological duck-rabbit dilemma¹¹:

Minus 31		Plus 31
2145 Yao 1	(2114) Yao 1	2145 Yao 1
2073 Yao 73	(2042) Yao abdicates, = Shun 1	2073 Yao abdicates (= Shun 1)
2060 Yao 86	(2029) Yu given <i>gui</i> , = <i>Shun 14</i>	2060 Yu given <i>gui</i> (= <i>Shun 14</i>)
2042 Shun 1	(2011)	2042 Shun 1
2029 Shun 14	(1998)	2029 <i>Shun 14</i> Yu given power
1589 Shang 1	1558 Shang 1	1589–1559 Di Gui (Jie), 31 years
		1558 Shang 1

Zhou mournings were dropped, Mu 1 becoming 962, Zhou 1 becoming 1062, and Shang 1 becoming 1558. The first year of Jie was untouched by this (the *wu xing cuo xing* event was redated from Jie 14 = 1576 to Jie 10 = 1580).

¹¹ For “duck-rabbit” see Ludwig Wittgenstein, *Philosophical Investigations* (translated by G. E. M. Anscombe), New York: Macmillan 1953, p. 194 ff. (I supply this reference to satisfy the reader’s curiosity, making no use of Wittgenstein’s point. He cites Jastrow, *Fact and Fable in Psychology*. Wittgenstein offers a drawing of a head of a duck; if you turn it 90 degrees it becomes the head of a rabbit.)

You will retort, “but 2029 is my kind of date! The Chinese didn’t have that!” No; but they did have a system of absolute dating, and I have shown that they had been using it, on just that date 2029. Their system was the intercalation cycle. They would read an ancient date as just 1520 years later, remembering the date they got by some event or record familiar in their own historical memory. I have shown that they were doing that too, with the date of the Zhong Kang eclipse. (For another example, look at the long subtext after the Yao chronicle: Strip 35 (in my *Riddle*) identifies the year as Yao 70, and strip 40 makes *xinchou* (38) the first of the 2nd month.¹² That was worked out using the Lu Li application of the intercalation cycle, taking 625 as the equivalent of 2145.)

A permanent 31-year pull-down had to be avoided, because it would dislocate the first year of Yao; so a new reign was invented, to fill in the 31-year time-void between 1589 and 1558. Thus was Jie (Di Gui) created. Shun 14 became 2029 again, and Yao 86 became 2060. Shun 1 became Shun 1 in two senses: Shun 1 after the completion of mourning for Yao; but also Shun 1 with Yao’s abdication in Yao 73. The visible detail of the giving of the Dark Scepter to Yu, which had probably belonged originally to 1953, ended adhering to Yao 86.

(Now we see why Yao “abdicates” (Yao 73 = 2073) before his reign ends. How much else in the Yao-Shun myth had its origin in this chronological dilemma? The myth has Yao giving his two daughters to Shun in marriage. In the *BA* this happens in Yao 71 = 2075, and is a mark of Yao’s confidence in Shun’s “virtue.” Similarly the lord of Yu—Shun’s ancestral name—gives his two beautiful daughters to the young Shao Kang as a sign of confidence in him as he struggles against Han Zhuo to restore the Xia Dynasty.)

The changes I analyze did not get made all at once. Nobody held in his mind simultaneously the dates 1045 and 1050 for the Zhou conquest. Probably 1045 was proposed a century before 1050 was proposed. Deletion of mourning-completions is impossible with the date 1045, but is required by the date 1050. And the deletions for Zhou and for Shang did not have to be done at the same time. Extension of dates back for the beginning of Xia and earlier was probably what was done first. Then Di Xin’s last year would be thought of as his last *de jure* year (perhaps 1057), with a *de facto* reign continuing. Only later in Wei would his first year be moved back 16 (for that would destroy 1145 as Wu Yi’s first year, and make Dan Fu’s recognition year doubtful).

The insight gained from the date *jiazi* for Xia 14th king Kong Jia, that *gan* names of kings are determined by the first days of their reigns, must now be usable by finding (by trial and error) a “best explanation” argument, to confirm exact

¹² *Riddle* p. 131.

dates for all of the Shang kings. This took me a long time. I did it in 1990, as follows:

The last character in the king’s name must be the same as the first character in the *gan* for the first day of the reign. There are constraints and options in choosing the date: a king cannot have the *gan* of his predecessor; *gui* (last of 10) is forbidden (it was the *gan* of the first king Tang’s father) and defaults to *jia*; but the choice can be either the succession date or the accession date; and usually the succession year is the predecessor’s year of death (resulting in an apparent 3-year mourning-completion). In two unusual cases (Wai Bing and Wai Ren) uncertainty forced resort to confirmation by divination. (This is indicated by “*wai*” 外, “outside,” = bottom of a turtle shell; in *jiaguwen* it would be “*bu*” 卜, divination crack on a turtle shell, perhaps polyphonic, pronounced “*wai*”.) A very few *BA* dates were impossible, and options among possible explanations created many “down stream” possibilities. I had to find the combination of assumptions that most closely conformed to the entire set of *BA* reign lengths, and that best explained *BA* reigns that were impossible. My result, I think, also turned out to be the best possible explanation of the “31-years” problem.

I learned some important things in the process. For example, Pan Geng (19th king) in generation 10 had only 24 years, because he was aiming at usurping the succession and claimed the 4-year accession reign of his elder brother Yang Jia as part of his own claimed reign (which thus became 28 years).

Pan Geng is betrayed by the fact that there were four kings in his generation, rather than two, as in earlier Shang times. As I see it, the unpleasantness with Yi Yin at the time of the first succession led to having two kings in each generation, in order to prevent a prime minister from usurping power while the heir was mourning. A king, designating his heir, would also select one of his own brothers to serve as interim king, with the heir bearing the major burden of mourning, and becoming king after his uncle’s death. This system failed in generation 10: 11th-generation king Wu Ding was son of the *last* brother in generation 10. Wu Ding tried unsuccessfully to continue the scheme, by using younger sons, since he perhaps lived too long to have brothers to use. His chosen heir Zu Ji¹³ lost out to

13 Oracle inscriptions identify Zu Ji as Wu Ding’s heir. He appears in the brief *Shang shu* chapter “Gao Zong Rong ri,” in which a large bird interrupts a sacrifice which the king is performing, Zu Ji then interpreting the event as a criticism of the king (for trying to make himself chief mourner). But Zu Ji is not recognized there as heir, and most of traditional interpretation since antiquity misinterprets the chapter’s title as “the day of the *Rong* sacrifice by Gao Zong” (= Wu Ding), rather than “the day of the *Rong* sacrifice for Gao Zong,” as oracle idiom requires. The incident (if it actually happened) must have been during the reign of Zu Geng (when the heir Zu Ji as king-to-be (*xiao wang*) would have been chief mourner).

younger brother Zu Jia. After that, father-son succession was very carefully arranged and guaranteed while the father-king was still living.

In this work, I have been exploiting evidence in ways that some people would not approve (failing to notice that they do it themselves): I am always trying to identify the problems faced by the creators of the *BA*, and by the kings whose reigns are the *BA*'s content (or by anyone doing anything), and then I try to re-think the way they solved their problems. This is the way all good history is done. Collingwood was right.¹⁴

*

I have devoted decades to this project. Have I been wasting my time? I think I haven't; but the question is serious, and it allows two different answers. One would be to show what the historian can do only if he has an exact chronology to work with.

In 2001 there was published (U.K.: Curzon; U.S.: Columbia) S. J. Marshall's *The Mandate of Heaven: Hidden History in the I Ching*. (Marshall admits he is not a seasoned sinologist. I have no trouble with that. I could use more seasoning myself.) Marshall's thesis is that Hexagram 55 in the *Yi jing* refers to a solar eclipse. With careful study he concludes that it must be the eclipse of 20 June 1070 BC. I think he is right, in a sense: one can make a case that this eclipse caused parts of the text to be what they are. But I think that from the *Yi jing* alone one can almost never get any information; the book is intentionally so murky that it can be claimed to support almost any result a diviner needs.

Marshall proceeds to claim that this gives him the date of the Zhou conquest of Shang. (He is aware of scholarship holding that the conquest must have been some years after the conjunction of 1059. He makes no attempt to refute this, and sneers at it.)

Marshall is wrong, of course, about this, and misses something more interesting. The fact is that 1070 is a very important date in the events preceding and leading up to the Zhou conquest, and it is likely that an eclipse is involved. The *BA* says that in year 21 of Di Xin of Shang the Zhou court hosted an assembly of regional lords friendly to Zhou. The date must be counted from 1102, the *BA* date for Di Xin 1, giving 1082. This date must then be reduced by 12, because pre-conquest Zhou dates in the *BA* are tied to the *BA* death of Wen Wang, which (as explained above) was moved back one 12-year Jupiter cycle by the chronologists

¹⁴ Collingwood, R. G., *The Idea of History*, Oxford, 1946 (posthumous, edited by T. M. Knox), pp. 282–302, “History as Re-enactment of Past Experience.”

who produced the Wei version of the *BA* ca. 300 BC. Therefore the date of this assembly was 1070. It is probable that the June eclipse, which would be interpreted as predicting the death of a king, was what prompted Wen Wang to host this event.

Di Xin was not a fool, and recognized the threat at once. His response was to stage a royal hunting expedition in the Wei valley the next year. The Wei valley was the Zhou homeland, and a royal hunt was a standard way for a king to demonstrate that he had the power to do anything he wished, anywhere he wished to do it. Di Xin followed this demonstration with a general assembly of lords in his own capital in 1068. This is not in the *BA*. I deduced it by discovering that there was a second Di Xin calendar beginning in 1068. *Yi Zhou shu* 21 “Feng Bao” concerns another pre-conquest assembly of friendly lords in Zhou, closer in time to the conquest. (The tone of anti-Shang ranting is bitter.) The text contains enough information to date the event 1046, and the date given is “23rd year.” Further, tradition—as in *Wenxian tongkao*—has 37 years passing between Di Yi and Di Xin. I had pinned Di Yi to 1105, using inscriptions. It would seem, then, that 1068 was the year when Di Xin promoted himself from *wang* to *di*. The *BA* identifies Lu Fu as Di Xin’s heir, known to history as Wu Geng; so Lu Fu must have been appointed *wang* sometime before Di Xin’s death; and the first day of the year 1068 was *gengxu* (47).

These events required a big celebration, which all regional lords would be required to attend. This was the way in which a “great king” controlled the local rulers to whom he had to entrust local power: if you failed to attend, you identified yourself as a rebel. Wen Wang, titled Xi Bo (“Lord of the West”) had to come, and was promptly arrested. There followed his confinement in the nearby village of Youli for seven years, which from the *BA* can be deduced to be 1068–1062. Apparently Wen Wang had enough support so that Di Xin didn’t dare to kill him; but Wen could hardly forget that his father Ji Li had died in a Shang prison in similar circumstances. It is not surprising, then, that *Yi Zhou shu* 25 “Xiao Kai” has Wen Wang in his 35th year advising his court that it should respect the warning of an unpredicted lunar eclipse—possibly foretelling his own death—and focus attention on the selection of a successor to himself. It is implied that Wen Wang is not in Zhou at the time and must be communicating by letter. The eclipse is datable with certainty to 13 March 1065 BC.

Tradition—I think I can say now at least partly confirmed—has it that it was during his residence in Youli that Wen Wang wrote the part of the *Yi jing* attributed to him—including the text for Hexagram 55, which perhaps was suggested by the eclipse.

So, have I been wasting my time? Knowing more about their past, getting richer and more precise information about it and a deeper understanding of it have been very important to the Chinese. I think this is as it should be, and I hope I have been able to help a little with this. But I want to offer another kind of answer to my question.

An obvious way to start is by looking at what I have just worked out, but now not with the focus on the information gained, but on what I was doing. I put together the eclipse that Marshall put his finger on and dated to 1070, combining this with *BA* entries which I had dated to 1070, 1069 and 1068. I got those dates by inferences from assumptions that seemed to me reasonable and almost necessary. I then used them by *filling in* a historical narrative. The *BA* does not *say* that the royal hunt in the Wei valley in 1069 was intended by the Shang king as a warning to Zhou. And this is only one step in my narrative. The *BA* does not say that Wen Wang was arrested while in the Shang capital; other texts do; but I had to ask why he should have been there, and no text known to me *tells* me that. *Yi Zhou shu* “Xiao Kai” does not *say* that the time was the middle of Wen Wang’s detention in Youli; I deduced that. And that text does not *say* that he was worried about losing his life. I made that up. (I would have been worried too.)

In all of this I notice myself engaging in a complex of filling in data, making deductions from the data, and asking myself why the people doing this and that did those things. This question has the form of asking what I have to assume to make sense of what I read. It seems to me that Collingwood’s “rethinking” is a special case of this procedure; and further, that it amounts to asking what something to be explained implies, that would explain it. This is logically the reverse of trying to find some premises that would imply it, which is often thought to be the proper form of an explanation (in physics, therefore necessarily everywhere). I take what I am doing to be what Charles Sanders Peirce called “abduction,” and to be included in what more recently has been termed “inference to the best explanation.” (An enormous literature has developed on this idea, following Gilbert Harman’s short article a half century ago.) As Peirce warns, abduction is not a form of inference; it is a strategy, and can use formal inferences of various kinds.

I said “included in,” after due reflection. In a famous three-page article in *Analysis*, Gettier had shown that the common definition of knowledge as justified true belief seriously needs to be amended: there can be odd but not uncommon sequences and connections that leave a belief true, and justified, but one wouldn’t say that the person *knows*: What one thought to be the explanation or justification isn’t what happened that actually does justify the belief. Harman applies the idea to a critique of enumerative induction, and argues that such an induction is valid only if it is in effect also an argument to the best explanation. It

requires a sensible effort for me to spring the idea loose from Harman’s context and apply it to history, and especially historical narrative, where any request for enumerative induction would usually be bizarre.¹⁵

Further, imaginary examples usually assume imaginary verifiability. Working with the *Bamboo Annals*, where evidence is thin and usually underdetermines anything one needs to say, I cannot make this pretense. I am typically reconstructing the thinking of persons I can never hope to name or locate or date. That is what I did in working on the deformation of Xia chronology. There are persons who will say to me, “Nivison, you are walking on air: I won’t listen to any of it.” I suggest that any reader who wants to say this read again my proof that chronologists twisting the dating of the Three Dynasties had the use of accurate records for those dynasties, back to the 20th century BC.

I think my conclusion is true. But there is no way to get behind the account I constructed and confront the facts, so as to confirm it; and if someone were to produce an account of a different kind explaining the dates 1948 and *gengxu*, I would have to examine the rival case and give reasons for preferring mine. (One cannot dispose of a counter-argument by sneering at it.) In other words, in this kind of study we must be able to compare explanations, and this means accepting the idea that an explanation does not have to be true to be a possible explanation. A made-up story can be (and often is) told as an explanation, of otherwise puzzling data. The chronicle of Jie is such a story, and there is a great deal of this in what for centuries has gotten accepted as history in China. There is a lot more of it in the *Bamboo Annals* (and in Livy, and in Herodotus). To deal with this problem, we must be able to tell a story about the story, explaining how it could have come into existence, if untrue. If no such story-about-the-story is even imaginable, then the original story is true. If there is no story-about-the-story that is plausible, then the original story is almost certainly true. I did find a story about the Jie story, that is almost unavoidable, and I conclude that the Jie story is false.

On the other hand, I encountered what is offered as information about an eclipse in Zhong Kang’s reign which I *know* is false: no eclipse occurred in 1948, or on *gengxu* day. But then I must be able to explain *that* as false. I find that I can, and I cannot imagine that there could be another explanation at all, let alone a better one. So the explanation I found must be true. But that explanation requires

15 I ought to give notes for all this. Ask Google about “inference to the best explanation.”

me to assume something amazing: The Chinese had been keeping accurate records through dynasty after dynasty, for many centuries before we have any evidence of their using writing.¹⁶

This use of historical imagination cannot be scorned. To scorn it is to throw evidence away. Thinking this out has not been a waste of time, and working on the *Bamboo Annals* has helped me to think it out.

16 Hume would remind me that I may accept such an improbable conclusion only if it would be even more improbable for the argument leading to it to be wrong.

22 The Nivison-Shaughnessy Debate on the Bamboo Annals (*Zhushu jinian*)

1. In 1979, Edward Louis Shaughnessy was a graduate student at Stanford. He had come to Stanford with a dissertation project already under way, on the *Yijing*. He was assigned to me as a person working on ancient texts, even though I had never worked on the *Yijing*. In November of that year our relationship became more meaningful: I was conducting a seminar on Western Zhou ritual bronze inscriptions, and Shaughnessy was participating. One Sunday evening in November, preparing myself for the next evening's meeting, I discovered evidence that an ancient chronicle, the *Jinben Zhushu jinian*, which most scholars believed to be a fake (following the publication of work by Wang Guowei in 1917), was actually authentic. I discovered that the dates of the reigns of Western Zhou kings in the chronicle were systematically skewed in some way, and that the cause apparently was that bronze inscriptions and other materials in a reign could be dated either on the king's succession year, or on a year two years later (the "accession" year), which I assumed to be the year after he had completed mourning for his royal father. (The king's reign-of-record was counted from the later *yuan*, omitting the mourning years.)

2. Shaughnessy has accepted the two-*yuan* theory in dating inscriptions, and improved it by showing that the accession *yuan* began to be used only late in a reign. At first he accepted my view of the *Annals* as well. He from then on spent more than half his time exploring the consequences of this discovery. Five years later he produced a paper proving that a strip of characters near the end of the chronicle for the founding king Wu Wang had been lifted from the middle of the chronicle for the second king Cheng Wang, lengthening Wu Wang's life by three years. The result was his article "On the Authenticity of the *Bamboo Annals*," *Harvard Journal of Asiatic Studies* 46 (1985: he was now on the faculty of the University of Chicago). This he followed with an article in *Early China* 11–12 (1985–87), "The "Current" *Bamboo Annals* and the Date of the Zhou Conquest of Shang." He takes this date to be 1045—following my article in the *Harvard Journal* in 1983, "The Dates of Western Chou"—but just as my article was being published, I had found a mistake in my work, and quickly published a research note in *Early China* in 1984, arguing that the date of the conquest probably was 1040.

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Ed found my argument “unconvincing” (*EC* 11–12 p. 56) and still holds to 1045. In fact, he still defends the *EC* 11–12 article vigorously. In his book *Rewriting Early Chinese Texts* (Albany: SUNY Press, 2006, p. 202), he complains that scholars have paid less attention to his early *EC* article than to his earlier *Harvard Journal* article, though he thinks his second article is more important. I will be arguing that he should be grateful to the community of scholars for this neglect.

3. The foregoing is the background for the following clip from my unpublished autobiography:

In June of 2009, I had published my book *The Riddle of the Bamboo Annals* (Taipei: Airiti; hereafter *Riddle*). I had been delighted to learn, in April or May of 2010, that my friend Prof. Edward Shaughnessy (Chicago) had accepted an invitation to review the book in the *Journal of Chinese Studies* (Chinese University of Hong Kong)—a publication which has world-wide attention. The book had brought together my most important work over thirty years. Shaughnessy and I had agreed on issues that made the two of us leading critics of the work of the five-year “Xia-Shang-Zhou Chronology Project” in PR China; but we had stubborn disagreements on other details, and I had never been able to get him to engage me in argument on them. I had made these issues quite prominent in the book. Now, at last, I thought he would have to face up to them.

Instead, in his long (21 pp.) review, he used half his space to restate and illustrate our criticisms of the Project’s work, and the remainder to dismiss work of mine disagreeing with his as not worth reading. His one attempt at substantive argument was obviously invalid. I refer to his claim that a Zhou Xuan Wang half-strip had been displaced backward 278 years (815 to 1093) into the chronicle for Shang king Di Yi. Actually the part of the Di Yi chronicle that came from the Xuan Wang chronicle must have been a *rewriting* of only part of a half strip (wrong, but not evidence of disorder), at a time when the Di Yi chronicle was still 19 years long rather than 9. The account of the 1093 Zhou earthquake is accurate, and was always in the Di Yi chronicle, never in the Xuan Wang chronicle. Ed’s review appeared January 1. I countered with a reply (30 pp.) in the next issue of the Hong Kong *Journal* published July 1.

The conflict between us is actually quite interesting on a philosophical level. Ed (perhaps without realizing it) has a visceral commitment to a one-problem-at-a-time Baconian historical method, and has no patience with anything else. I am guided by “inference to the best explanation” of total evidence, by Collingwood’s concept of “rethinking,” and Popper’s strategy of discovery by trying to refute far-reaching theories. Ed can’t stand it, and can only see me as “getting ahead of my sources.” (I must not forget that Ed has done much for me:

e.g., making and giving me photocopies of books hard to find.) This took most of my time in the first half of 2011.

4. This is the “Debate” (rather than the relatively trivial dispute about the Zhou conquest date, which is only a moment in that debate). In detail, it starts with Ed’s *Harvard Journal* article. In that article he announces his discovery that a strip of text now in the Wu Wang chronicle from the words “15th year” to the words “17th year” must have come from the Cheng Wang chronicle. In the Wu Wang chronicle the effect is to lengthen Wu Wang’s life by three years, having him die in year 17 rather than in year 14, five years after the conquest of Shang, rather than the two years after the conquest found in other early texts. In the Cheng Wang chronicle there is no subtraction of years: the moved text would exactly fill a gap between year 14 and year 18.

5. Further, the strip at year 15 mentions “an announcement to the city of Mei”; most commentators identify this with the “announcement” recorded in the *Shang shu* chapter “Jiu Gao” warning against drunkenness; and pre-Song commentary dates this chapter to the reign of Cheng Wang. Also, the strip text next has the words “In the winter, the nine cauldrons were moved to Luo”; but apparently Luo did not exist in Wu Wang’s time: the *Shang shu* “Luo Gao” implies that the site was first planned in the last year of Zhou Gong’s regency, hence seven years after Wu Wang’s death; and only then (*Shang shu* “Shao Gao”) did construction begin. So, again, Shaughnessy’s strip seems to belong in the Cheng Wang chronicle. So Shaughnessy concludes that the original text must have been in much disorder at this point. He thinks that his strip was loose, and the rest of the text broken up enough so that the Jin Dynasty scholars charged with the task of restoring it had the choice of inserting it in the Wu Wang chronicle or in the Cheng Wang chronicle, and they chose the wrong place to put it in. The Jin scholars did this, Ed thinks, influenced by the (now mostly lost) contemporary *Di wang shi ji* by Huangfu Mi, a well known third century historian.

6. The trouble with this analysis is a detail Shaughnessy did not notice in his *Harvard* article in 1985, or in its continuation in *Early China* 11–12: In the Cheng Wang chronicle as it is at present, there is a *di* 禘 ritual in the “Zhou Gong *miao*” (temple) for Zhou Gong in year 13, two years before the dates in Ed’s strip. The *di* rite (in effect an apotheosis rite), and his having a *miao*, imply that he is dead. But his death is recorded in year 21, and his burial in year 22. If the text is corrected, either by moving the *di* rite to year 23 or moving the death and burial to years 11 and 12, then the transposed strip text ceases to be in strip position. I

infer from this that the strip text must have been created and moved before the *Zhushu jinian* text was buried. The Jin restorers would not have dared to move Zhou Gong's dates; they would have known that they couldn't get away with that, and if the death-related dates were altered at some earlier time in Warring States, this would require an explanation which I haven't seen.

7. But there is a ready explanation if it was done in the late 300's in Wei. The Wei ruler, to be known as Huicheng Wang after he died, declared himself king in 335, so that his first year as king could be 334, a hundred years after the first year of his grandfather Wei Si (Wei Wen Hou), who had declared himself *hou* in 435 with 434 as *yuan*. The Wei state represented itself as continuing the Jin state after its disintegration in the 400's. So Huicheng Wang apparently directed the experts constructing the *Zhushu jinian* to date the founding grant that created Jin to the year 1035, this being 700 years before his declaration in 335. He was thus committed to other dates: The *Guoyu* ("Jin Yu" 4) says that when Jin began, Jupiter was in Da Huo, Jupiter station 10 of 12. (Actually the nearest Da Huo year was 1031.) The *Guoyu* also ("Zhou Yu" 3.7) says that when Wu Wang attacked Shang Jupiter was in Chun Huo, station 7. This required that the *Zhushu jinian* must show or imply that the conquest was in 1050, as it is in the "modern" *Zhushu jinian*. So, the problem: The actual succession year of Cheng Wang was 1037. The 7-year Zhou Gong Regency could be said (wrongly) to precede Cheng Wang's 30 years (as in the present text); but if Wu Wang lived only two years after the victory over Shang, the conquest would be in 1047, not 1050. Therefore his life had to be extended by three years; so a strip was constructed—Shaughnessy's strip—out of Cheng Wang chronicle material, which would do this, when transposed into the Wu Wang chronicle. This was a Wei project, so it was all done in Wei. The text with created strip still in its Cheng Wang location was needed for display in Wei, and then was moved to its present location, before the burial of the book. The Jin scholars merely copied what they read. What they read was wrong, deliberately wrong, but wrong does not mean disordered. The text they read said exactly what the last editors to touch it before it was buried intended it to say.

8. This destroys Ed's picture of a disordered text that got mended the wrong way. The creation of the strip was done out of Cheng Wang material because that happened to work. It worked (if you ignored the *di* and *miao* problems), but it created another problem, because 1050 was actually the date of the death of Wen Wang, and Wen Wang had to have died well before the conquest. The solution was to move the Zhou-heralding conjunction of planets back one 12-year

Jupiter cycle from 1059 to 1071, and with this all *pre*-conquest Zhou dates were moved back 12 years. This move created (or recreated) a long *pre*-conquest reign for Wu Wang. His succession year had been 1049. It now became 1061, and the conquest year 1050 became his 12th year. Wen Wang's death year became 1062, the 9th and last of his nine "*shou ming*" years after the conjunction (as in *Yi Zhou shu* "Wen zhuan").¹ These 11 *pre*-conquest years for Wu Wang were not empty, because there had been a (real) chronology that had him conquering in 1040. His *pre*-conquest years thus were 1049–1041, which contained *pre*-conquest events for Wu Wang both real and mythical. The mythical ones included a "*guan bing*" campaign two years before the conquest (see *Riddle* pp. 27–29).

9. The real ones included the conquest of Li in Wu Wang 8 (1042, confirmed by the recently discovered *Tsinghua Bamboo Strips*, which thus confirm that 1045 was not the year of the Zhou conquest). This 8th year event became "3rd year," i.e., 1047, in the 1045 chronology (which Ed thinks is real), because 1045 was a move of the conquest date back five from 1040, without changing the date of Wu Wang's succession year 1049. Confirming this, when *pre*-conquest dates were moved back 12, the 1047 event became 1059, as it now is in the "modern text" (Di Xin 44: see ELS in *EC* 11–12, p. 39). All of this gets lumped together by Ed, who in his Harvard article and in his *EC* 11–12 article charges the Jin restorers Xun Xu and associates with having forged it "out of whole cloth"—a charge he repeats in the *EC* article several times: He uses *jiaguwen* and determines correctly that Di Xin 1 was 1086; and he has determined (he thinks) that the conquest was in 1045. Counting, he gets 53 years for the present *Zhushu jinian*'s Di Xin reign (1102–1050) and 42 years (1086–1045) for what his beliefs give him for the true Di Xin, lays one on top of the other, makes a Procrustean cut of the excess eleven years in the "modern text" chronology and throws it away. But as I show here, part of that throw-away was the very real Li campaign.

1 The biography of Shu Xi in the *Jin shu* says that "from Zhou's *shou ming* (receiving the Mandate) to Mu was 100 years." But the present text of the *Annals* at Mu Wang 1 has an in-text note saying that from Wu Wang to Mu Wang there were 100 years of rule. Shaughnessy sees this as requiring that the Jin editors rewrote the text at this point, because the Zhou ruler receiving the Mandate must be Wen Wang. He is right about Wen Wang, but wrong in his judgment. The present text gives us the chronology that results from deletion of mourning-completions, shifting Mu Wang's first year back 6 to 962, and moving the conjunction and other dates back 12 years, putting Wen Wang's death in 1062, the last of his 9 "*shou ming*" years. This made Wu Wang's first year 1061, and from 1061 by inclusive count to 962 was "100 years of rule."

10. Ed was excited by his discovery of the moved strip text and by finding that he had recovered the exact words of a strip (Xun Xu's preface to the edited *Mu Tianzi zhuan* showed him how long it had to be) out of a text in an archaeological discovery made 1700 years ago. Perhaps, he mused, nearly the whole of the "modern text" would turn out to preserve the original "tomb" text accurately. I myself have tried the experiment of assuming that it has in fact done so, trying to apply a rational analysis to the whole of it down to where it is obviously a mess: I stop at 679, which I had reason to suppose was a safe place to stop, and am still optimistic about having succeeded in recovering not only the exact dates but even most of the exact words in this earlier part. (Almost at once after 679 one confronts impossibilities: there is literally nothing, for example, on the prolonged confrontation (with two great battles) between Jin and Chu in the late 600's and early 500's, where the *Zuo zhuan* is rich).

11. Ed, doing a lot of work on modern discoveries of texts of other kinds, has gradually come to think that all old texts recovered out of the ground are alike in being mostly disordered, including the *Annals*, and he treats this as a premise in approaching *Annals* problems. (Perhaps this explains the structure of his 2006 book *Rewriting Early Chinese Texts*: first the "Black Jacket," then the *Annals*.) But this is to ignore the constant aim of the *Annals*, which is to keep dates in consistent order; so there is constant pressure in this direction; failures of consistency have to be explainable, and are. Shaughnessy, however, assumes disorder in the text, and wants to find it, lest he be forced to admit that much of his past work is wrong. He leans heavily on the hope of finding more transposed strips. I have no objection to what he does after 679. But he thinks we should expect similar disorder throughout the text, and sees it in what he thinks are transposed strips in the Wu Wang chronicle and in the Xuan Wang chronicle. I have refuted him in print, but he pays no attention.

12. Ed's other case of disorder caused by transposing is in the texts of Di Yi, 29th Shang king, and Xuan Wang, 11th Zhou king. To give him his due, I too see both texts in need of much editorial attention. The trouble with the Di Yi text begins with the preceding text for the 28th king Wenwu Ding. The *Annals* gives him 13 years; the *Wenxian tongkao* allows him only three years. Why? Di Yi in the *Annals* gets 9 years. But when I work on the *jiagu* inscriptions of the Yi Fang (or "Ren Fang") campaign (as does Shaughnessy, *EC* 11–12 pp. 46–47), they give us 1086 as first year for 30th and last Shang king Di Xin. Using these inscriptions I am able to start assigning absolute dates to more dates in these inscriptions (see *Riddle* pp. 236–240). A ritual cycle averages less than a year (usually 36 or 37

ten-day *xun*). Over time, this means that the first day of the cycle must precess gradually or not, depending on how strictly the 36–37 alternation is followed. Therefore the farther back one searches, the later in the year will be the first day. Combining this approach with other evidence, I worked back into the reign of Di Yi. I found myself looking at a reign beginning not with 1095, which would be nine years, but beginning 1105, therefore 19 years. Apparently a ten-year overlap had been resolved at Di Yi's expense. Working still farther back, I allowed Wenwu Ding only three years, finding no inscriptions for that time, but two years farther got me to 1110, which fitted the ritual cycle information for *Jiabian* 2416 (*HJ* 36511), the longest *jiagu* text known, announcing a campaign against the Yu Fang. The month would be October, appropriate for the beginning of a royal campaign. *Jiabian* 2416 has no year date. But Fragment 1908 in the *White* collection (Royal Ontario Museum, Toronto) is a shorter version of the same text, and it is dated “9th *si*” (*Riddle* p. 239). If this 9th year is 1110, it follows that 27th Shang king Wu Yi (1145/43–1109) gave his heir Wenwu Ding a calendar of his own in 1118, for what turned out to be the last ten years of Wu Yi's reign. Subsequent editors wouldn't allow the overlap, and instead of pushing the Wu Yi reign back ten, they cut the first ten years out of the Di Yi reign, giving them to Wenwu Ding. Thus what had been the 13th year in the Di Yi reign became the 3rd year.

13. This is the 3rd year text (strip 169 top half exactly, *Riddle* pp. 148–149):

3rd year: The king ordered Nan Zhong to oppose the Kun Yi on the west, and to wall off the Northern Region (Shuo Fang). In the 6th month, there was an earthquake in Zhou.

Nan Zhong was widely believed to be a contemporary of Wen Wang (1101/1099–1050, as he could be here), but actually was a general under Xuan Wang (827/25–782). The “3rd year” here has to be 1093. So was there an earthquake in Zhou in the 6th month of 1093? Shaughnessy must hope not. He thinks he can claim another misplaced strip misdating Nan Zhong.

14. Nan Zhong does belong to Xuan Wang. But there was an earthquake in pre-conquest Zhou in 1093: The main source is unusual, because it contains a basic error, which instead of invalidating the source (as Ed will probably tell me) actually makes it completely convincing. I deal with this more fully than I am likely to here in *Riddle* p. 55 and p. 171. The source is a story about Wen Wang in *Lü shi chungiu* 6 “Ji Xia” 4 “Zhi Yue” 2 (Knoblock and Riegel pp. 164–165): “Wen Wang of Zhou had ruled the state for eight years A year later, in the sixth month (*sui liuyue* 歲六月), Wen Wang went to bed sick, and in five days there

was an earthquake.” This translation is my own. I have indicated a break before “A year later, in the sixth month,” which is required by the meaning of the words *sui liuyue*. The contributor of this story omitted something unrelated to his point, and misunderstood the text he was copying, as saying “in the sixth month of the year,” not aware of the (rare) meaning of the word *sui* when used in a date. (I cover this matter in a brief paper included in the present book.) One can see this at once from the way the story ends: “An earthquake occurred when Wen Wang had been ruling for eight years. Forty-three years after the earthquake, when Wen Wang had been ruling for a total of fifty-one years, he died.” Put “nine years” in place of “eight years,” and “fifty-one years” must be changed to “fifty-two years.” Wen Wang in fact ruled Zhou for 52 years.

15. The *Annals* dates for the deaths of Ji Li and Wen Wang are Wen Ding 11 and Di Xin 41, which are 1114 and 1062, correctible (reduced by 12) to 1102 and 1050. The *Yi Zhou shu* “Xiao Kai” lunar eclipse is datable to 1065, and the date is called “35th year”; so Wen Wang’s dates are 1101/1099–1050, i.e., 2 + 50 years. The dates in the *Lü shi chunqiu* story are in his succession calendar. But how did Nan Zhong get mixed up in this? There was a (mistaken) belief that he was an associate of Wen Wang; but there is something else in the *Annals* line for 1093: he is to oppose the Kun-Yi and is to wall off the Northern region. Ode (*Shijing*) 168 celebrates Nan Zhong, and we find the words “*cheng shuo fang*” there, but the enemy is called the Xianyun, the major enemy of the Chinese in the Li Wang and Xuan Wang eras, whereas the major enemy in the Di Yi era was the Kun-Yi.

16. Immediately, this means rewriting, and not the misplacing of a half-strip. Why not both, Ed may reply: first, misplacing the half strip, then enough rewriting to make it fit in its new location. Too much rewriting would be needed. The part of the half strip about the earthquake is true of the date 1093. To ask us to believe that just those words would also be true in the supposed Xuan Wang location of the half strip is asking too much. Further, Ed does not accept the idea that the original Di Yi dates were nineteen years, 1105–1087. He thinks that the supposed Xuan Wang half strip had the date 13th year—next year after the 12th year date of the *Guo Ji Zi Bo pan*, as argued by Ma Chengyuan—and that the character *shi* for ten in the date *shisan nian* was broken off when the half strip was broken loose. But my reconstruction of the Di Yi text has the half strip as the top half, not the bottom half. (Please turn to page 149 in *Riddle* and look at strip 169 top.) Ed will reply, of course, that my reconstruction work is worthless (without saying why). I think it is obvious that Nan Zhong got into the Di Yi chronicle through a rewriting, not through a half strip misplacement, and there-

fore the rewriting must have been done early in Warring States, at a time when Di Yi still had a 19-year reign, and the year 1093 was still “13th year.”

17. Both of Ed’s attempts to find misplaced material in the part of the *Annals* I am defending as well-ordered are failures. Why is he trying to do this? He has brains enough to see what is wrong with his arguments if they were somebody else’s. But they’re his, and so he is misusing his brains trying to defend what is indefensible.

18. One can see how he got into this position. Discovering that his strip must have come from the Cheng Wang chronicle, this meant to him only one thing: an amazing mistake. In the Harvard article, he obviously did not understand what a *di* rite is, and didn’t see that he needed to, and did not dig into it. So he did not notice that Zhou Gong’s dates had been tampered with, and had no reason to notice the problem in his *EC* article written almost at the same time but published a little later. It wasn’t until his 1993 article on the “Shao Gao” problem that he noticed what seems to me now decisive evidence that Zhou Gong died in Cheng Wang year 11 (= 1027). This should have made him reexamine his whole argument, tear it apart and rebuild it.

But the misplaced strip argument had already made him famous. To backtrack at this point would have made him look like another Nivison, who was weathering criticism from everyone (including especially Ed himself) for having changed his mind about the conquest date being 1045, immediately on publishing it in the *Harvard Journal*. I was no help: In the middle of working out a reply to his “Shao Gao” article, I put in at the end of it another theory (that it was the *di* rite date that was wrong), not noticing (and acknowledging) or accepting his date for Zhou Gong’s death, as I should have. Had I praised him for getting the date right and at the same time had urged him to revise his argument to make it consistent with his discoveries, he might have agreed with me (I hadn’t yet published), seeing then that Wei’s Warring States propaganda required putting the conquest in 1050.

19. But he did not see this, and so doesn’t see what is going on in the *Annals* revision of Di Xin chronology. Ed deals with this on pp. 48–50 in his *EC* article and gets into a mess, finding that three key dates in the *Annals* have been “adjusted” forward four years, namely the conjunction, the Mandate, and the death of Wen Wang, because “tradition” selected dates linked with king Wen’s death. But why just these three? Why doesn’t he ask himself why Di Xin’s *yuan* was moved back 16 years? He doesn’t, because to do so would require him to have

seen that the chronology here of the present text was worked out in Wei. Only this could have shown him that 1035 as date of the fief to Tangshu Yu forced moving station Da Huo (Fang) year 1031 back four, moving all Jupiter stage dates in Di Xin's reign back four, and then forced the conquest date to be 1050 as a Chun Huo year, colliding with 1050 as year of Wen's death. Wen Wang's death date, and all related dates—which meant all prior Di Xin dates—thus had to be moved back 12. 12 years preserved the 100 year tradition of the interval from the receipt of the Mandate to the first year of Mu Wang, if at the same time initial mournings were dropped: Mu Wang 1 became 962, and Wen Wang's last *shou ming* year became 1050 + 12. Hence the 16-year move back of Di Xin's *luan* year: $4 + 12 = 16$, from 1086 to 1102. This shows that the “4-year adjustment” applies to all earlier Di Xin dates, not just to Ed's selected three key dates.

20. It also shows that it is not appropriate to say (1) that the Jin restorers mistook the 11-year pre-conquest reign in a Wen-Wu calendar starting in 1056 for an 11-year pre-conquest reign in Wu Wang's calendar; or (2) that they invented “out of whole cloth” an 11-year pre-conquest reign for Wu Wang, revealed by measuring the 53-year reign given to Di Xin in the *Annals* against a supposed 42-year reign counting from 1086. As to (1), the Jin restorers were not the ones who did this; it was done by editors in Warring States Wei, and they did not make a mistake; they were pushed into moving Wen Wang's death back from 1050 to 1062, to avoid having him die in the conquest year, and this made Wu Wang's succession year 1061, so that the conquest year 1050 became Wu Wang's 12th year. As to (2), they probably invented nothing of the content of the 11 years created by the move of Wen Wang's death.

As I argued in *Riddle*, I think there was a theory that the conquest was in 1045. This is suggested by its being 100 years after the court recognition of Dan Fu by Wu Yi in Wu Yi's first year 1145; you would get it by putting the seven-year Regency in place of Cheng Wang's initial two years of mourning; and it is needed to get the Li campaign of Wu Wang 8 (1042: as in the Qinghua strips) back to Wu Wang 3 (1047), so that it could be moved back 12 to 1059 (Di Xin 44), along with other dates of events around the time of Wen Wang's death. But these are possible parts of an explanation of why 1045 came to be thought the date of the conquest, not parts of a proof that it really was the date. Ed has no patience for more discussion of his date 1045 for the conquest. He gets into it on p. 44 only to show that he has proved it in past work, and in this article (and in argument) is interested only in using it as a fact. He can no longer do this. Evidence that the date is impossible is mounting. (Wu Wang 8 for the Li campaign? The victory dated Qing Ming day—per Ode #236—if and only if the year is 1040? Wu Wang's

death in *Yi Zhou shu* dated both to his 12th year and to his 3rd year as king?). If Ed thinks the date 1045 can still be defended, he must defend it.

21. That said, there is more that's wrong in these pages. I will try to bring this out by looking at the chart of ten dates on p. 50, examining what Ed says and saying what I would say.

Actual year	Di Xin year	Event	BA year (Di Xin)	Discrepancy
1059	28	Planetary conjunction	32	4
1058	29	Release of King Wen from Youli	29	0
		Di Xin bestows "mandate" on Wen	33	4
1057	30			
1056	31			
1055	32			
1054	33	Mi surrenders to Zhou*	33	0
1053	34	Zhou defeats Li, Yu and Chong	34	0
1052	35	Transfer of capital to Feng**	35	0
1051	36			
1050	37	Death of King Wen	41	4

* (DSN) The action by Mi which results in its surrender to Zhou is recorded in the BA as in the conjunction year 32, i.e., 1071 = 1059; so the surrender should be in 1058, not 1054.

** (DSN) The date should be 1056, not 1052. See following.

The table above is Shaughnessy's on p. 50 of *EC* 11–12. The one below is mine, trying to replace his with something that makes sense. He does not, here, look at, or even mention, the false *yuan* 1102 for the BA dates and ask, why it is 16 years earlier than the *yuan* 1086 for Di Xin's actual calendar. After all, it's false. So he assumes that the default position must be that the year numbers for events in the two systems must be the same, unless we find that for some reason some few of them get "adjusted" by adding 4, when they appear in the BA column. This seems insane, so I am missing something. What I do he never does: ask myself, what year does the BA year number refer to, and why? So he never sees that it picks out a year 12 years earlier than the true year. If he saw that, he would have to ask, why?—and he would be led to my analysis.

Actual year	Di Xin year number (<i>yuan</i> 1086)	<i>yuan</i> 1086 <i>plus</i> 4, plus 12 = <i>yuan</i> 1102 Event (Bracketed matter expands chronicle into narrative.)	BA year number (Di Xin, <i>yuan</i> 1102)	BA year BC date (less 12 = correct absolute date)
1062	25	Release of King Wen from Youli [not to campaign unless given a <i>ming</i>].*	29	1074 (1062)
1061	26		30	1073 (1061)
1060	27		31	1072 (1060)
1059	28	Planetary conjunction; Mi attacks Ruan; Zhou [defending Shang rule] attacks Mi [without being ordered to]	32	1071 (1059)
1058	29	Mi surrenders to Zhou army; Mi people moved to Cheng. [Shang doesn't protest Zhou action] King Wen then claims [implied] "Mandate" [to act without a <i>ming</i>]; Di Xin grants (retroactive) <i>ming</i> [to counter claim]	33	1070 (1058)
1057	30	Zhou [using claimed authority] defeats Qi (=Li ?)**, Yu and also Chong; [the Shang king doesn't dare to object]	34	1069 (1057)
1056	31	Transfer of Zhou capital to Feng; [new Zhou "shou ming" calendar <i>yuan</i> , calendar years in parentheses]	35 (1)	1068 (1056)
1055	32	Lords assemble in Zhou; [again, Shang doesn't dare to object;] Hao planned; this problem is assigned to prince Fa	36 (2)	1067 (1055)
1054	33		37 (3)	1066 (1054)
1053	34		38 (4)	1065 (1053)
1052	35		39 (5)	1064 (1052)
1051	36		40 (6)	1063 (1051)
1050	37	King Wen dies [3 rd month of year 7, <i>shou ming</i> calendar]***	41 (7)	1062 (1050)

* The *Shiji* accounts of events from Dan Fu to Ji Li to Wen Wang are not dated; we have only the sequence of events; and the record of Wen Wang being given authority to campaign comes after the record of his release and gifts given to him. There is nothing here to require us to assume (with Shaughnessy) that the grant of authority was simultaneous with his being released; and there is no reason to reject the BA's (much more reasonable) dating of the release being four years earlier than the gift of authority.

** Why Qi here but Li at Di Xin 44 (=Wu Wang 8)? There must be some intended difference in reference.

*** This “*shou ming*” is also counted from 1058 ending with year 9, in *Yi Zhou shu* “Wen zhuan.” As I point out (Nivison 1983, pp. 523, 528–531), Wen Wang could not require his subjects to accept a new calendar without allowing them two years to finish any mourning obligations they might have.

The “adjustment” by 4 must apply to *every* year date in the BA column, because it is required by setting the *yuan* at 1102—*caused* to be 1102 because it *contains* the “adjustment” and 12-year set-back. Shaughnessy should have seen this, and should have asked himself why this is so. It is made necessary by Huicheng Wang insisting on dating the original fief of Tang, creating (later) Jin and then Wei, to the year 1035. (1035 was 700 years before 335, his declaration year as *wang*).

This made the year a Da Huo (Fang) year (station 10), instead of a Chun Shou (station 6) year, as is shown by the change in identifying the year of the conjunction of 1059, as in strip 197 where it is identified both ways, ignorantly—a mistake one can imagine in Warring States Wei, but not in post-Han Jin. So station 6 years all became station 10 years, and “station 6” years got pushed back 4 years. This forced moving the *yuan* for Di Xin back 4 years to 1090. Next, the Wei king found he was committed to call the year of Wen Wang’s death, 1050, also the year of the conquest. To avoid this, he had the pre-conquest calendar altered moving the death date back 12 years. (The move back could have been some other number; but only 12 gave a result that could be interpreted as having the first year of Mu Wang just 100 years after what could be called the first year of Zhou). The 12-year move amounts to plugging 12 years into the calendar, so the *yuan* had to be moved back 12 years more, from 1090 to 1102; and the result you get by using it becomes a year you then must correct by subtracting 12.

22. The date Wu Wang 8 for the conquest of Li (1042, counted from Wu Wang’s succession year 1049) proves that the conquest was not in 1045, and is one of many proofs that it was in 1040. The conquest date 1040 requires that Wu Wang actually had 9 (not 11) pre-conquest years, which probably had just the contents of the 11 years in the 1050 chronology that the *Annals* now displays. The 1050 chronology in place now simply has two more blank years, placed somewhere after the record (at Di Xin 44) of the Li campaign. (At present years 45, 46, 47, 49, 50 are blank.)

Shaughnessy’s charge of invention “out of whole cloth” is based on his own mistaken analysis of that chronology. The way he mounts that analysis shows its absurdity: He would have me place his supposed true 1045 chronology’s 42 years on top of the *Annals*’ 53 years in the 1050 chronology, and lop off the 11

years not covered by the 42 years, matching up the two chronologies at their beginnings in 1086 and 1102. Why not instead match them up at their ends, in 1045 and 1050, lopping off the first 11 years of the latter? What one must do is what I have done: look *inside* the 53-year reign given Di Xin and find out exactly why it is so long. You can't do this unless you put the development of the 1050 chronology where it was worked out, in Warring States in the late 300's. And you won't do this unless you notice that Zhou Gong's death dates in the Cheng Wang chronicle have been moved, so as to create the transposed strip Ed discovered.

23. This means that Ed's discovery is strange. We do not have a mistake made in putting back together a text that had gotten torn apart. What we have is a deliberate attempt in Warring States Wei to create a false record, by creating a strip that could be displayed as not belonging where it is—the Cheng Wang chronicle—but belonging in the Wu Wang chronicle, then moving it to that chronicle and displaying it in its new and false location, in order to make history read the way the Wei rulers wanted it to read. The doctored text then got buried among other treasures, and discovered five centuries later, not as a shattered text but most of it as a text in perfect order.

Ed has refused to accept this analysis. Why, he asks, would a Wei king, with the power to order his scribes to produce anything he wanted, go to the trouble to use this kind of trickery? He demands that this question be answered, while we forget about the impossible dates of Zhou Gong's death, that show that this trickery did happen. There were aspects of editing that had to be secret. But the Wei government wanted the results known and accepted, even (and especially) by non-Wei people, and in this case must have prepared the text for display. After all, what was at stake was the acceptance not only within Wei but also abroad, of Wei's claim to historical justification. Similar texts were accessible to persons not involved in their production. The *Chunqiu* of Lu is an example: if this were not true, we probably would not now have this text.

24. I will give another example that may persuade some of my readers. The Middle Han scholar, bibliographer, mathematical astronomer and politician Liu Xin worked out a way of calculating the positions of the planet Jupiter in past time. I have a short paper elsewhere in this book that shows exactly how he did it, but the technical details are not needed here, and were incorrect, but that too is beside my point. Liu did know that the popular belief is false, that belief being that on average Jupiter moved through the zodiac in 12 years, one station a year. Actually Jupiter's apparent motion is faster than that, and covers 7×12 stations

in about 83 years; but Liu didn't know this. His Jupiter was much slower, slow enough so that he could not have checked his result by observation, and must have been using an old record, or following someone who had done this, and had calculated the ratio accordingly.

25. Liu was perhaps using the work of someone who was working at about the time the Wei experts were perfecting the *Bamboo Annals*, in the reign of Xiang Wang of Wei. This person thought he knew that the perfected *Annals* had Tangshu Yu receiving the fief of Tang—the origin of the Jin state—in 1035. This person also knew that the *Guoyu* says that when Jin began Jupiter was in Da Huo, station 10. So he decided to test this by observation. His purpose required him to observe the sky in 315, which was 720 years after 1035. 720 is 12×60 ; so if the popular ratio were true, he ought to be observing Jupiter in Da Huo in 315. But Jupiter was not there: it was five stations farther on. He reasoned: not 720 stations in 720 years, but 720 plus 5 stations in 720 years; so how many years for one extra station? Divide by 5, to reduce “plus 5” to “plus 1”! One discovers that 144 years is the grand period for Jupiter, during which Jupiter “jumps a *chen*” (*chao chen* 超辰), and there have been five such periods since 1035: $720 + 5 = 5(144 + 1)$. So the Jupiter ratio, years to stations, is 144:145; and this is, precisely, Liu Xin's ratio. I am inclined to see this as evidence that work on the *Annals* in the Wei capital Da Liang was being watched, probably everywhere. This was the time, after all, of the Jixia “academy” in Qi, when learning of all kinds was getting interstate attention.

26. In sum: Ed, publishing in 1985, discovered a strip's worth of text in the Wu Wang chronicle, analyzed it and showed that it had been moved from the Cheng Wang chronicle. He saw that this showed that here was at least one strip of text that had survived unchanged for 1700 years. He at once argued that we ought all to consider the possibility that much more, perhaps all, of the *Bamboo Annals* was equally well preserved.

But the story he imagined was that this stretch of text was sufficiently disordered so that the Jin editors had the option of putting the strip back where it had come from, or putting it in the wrong place: the Wu Wang chronicle, making Wu Wang live three years longer. They chose the wrong place, influenced by the 3rd century historian Huangfu Mi.

Ed's story pays no attention to one part of the Cheng Wang context, the impossible dates for the death and subsequent rites for Zhou Gong (which I noticed right off). When I challenged him with this and finally got him to pay attention, his reply was (and is) that the Wei king could order his experts to make

the text say whatever he wanted; so it makes no sense to suppose that he put together the complicated deception I propose. So the king didn't. Not seeing this, Ed says, is the real difference between us.

Very well, but what about the misdating of Zhou Gong's death, burial and *di* rite? Ed's defense of his story leaves that problem untouched. It is not worrying about that problem that reveals the real difference between us. Ed says in effect that you must break up your research into separate manageable parts and solve those parts separately. I say that you must seek a possible solution to all problems and a possible way all can be fitted together. (In doing this you may have to include in your story that some people told some lies, but you must be explicit about it and show that your assumption is plausible.) If you can't even imagine a way of doing this, you are almost certainly wrong, and may be wrong about almost everything. I see this as exactly Ed's situation in his article in *EC* 11–12. To dodge the problem of Zhou Gong's dates was a fatal mistake.

So while his discovery could be taken as evidence for preservation, his story accounting for it made it evidence for disorder. He worked primarily on Western Zhou and later history, but this soon became his view of the whole of the *Annals*: what order there was in it had been edited into it by the Jin scholars who had been assigned to work on it. He was interested in nothing in the *Annals* earlier than very late Shang. Positing disorder is not an explanation. It's an admission that you don't have an explanation.

27. Starting with Ed's strip discovery and my own discovery of reasons to treat the *Annals* as authentic, I chose to see how far I could push the hypothesis of authenticity. Forty graph spaces per strip, blank spaces between years: I found I could handle the Wu Wang and Cheng Wang chronicles, but could go no farther with reconstructing the text, and so I laid this project aside at first.

Turning to chronology, I made use of Pankenier's work (which he had showed me in 1984): a conjunction of planets, dated late February 1953, probably marking the beginning of Xia. I had been working on intercalation and *qi*-centers; so I noticed at once that 1953 was 76 years later than 2029, the *Annals*' date for the beginning of Xia. 76 years = 1 *bu* = 4 *zhang* of 19 years, a *zhang* requiring 7 lunar intercalations. I pushed this idea and found (to my amazement) that it explained how editors had decided that the Zhong Kang eclipse date must be 1948, 9th month, day *gengxu* (47). From there on, one step after another got me (1) the explanation of the *gan* names of kings; (2) the fact that Jie is completely mythical; and (3) the exact dates of all 30 Shang kings. This is the story:

28. In December 1988, a phone call from a science writer at the Los Angeles Times alerted me to amateur work by Kevin D. Pang, using a computer program, who claimed to have found the solar eclipse in the *Annals*, 9th month of the 5th year of the 4th Xia king Zhong Kang: Pang's date was 16 October 1876. I checked it, from Pankenier's date for the conjunction, assuming *Annals* reign lengths and making *Annals* gaps between Xia reigns two years for completion of mourning (which I had discovered worked for Western Zhou). I found that Pang was right. We published in *Early China* in 1990—barely overcoming the determined opposition of Ed Shaughnessy, who had just become editor.

I suspect that Ed's posture in this dispute says a lot about the ongoing debate between us. I was once his teacher, and his need to demonstrate independence of me may be a factor. But looking just at the non-personal, the elements in the Nivison-Pang case that could be regarded as incautious are two: the use of Xia data from any source (most Americans assume Xia is mythical); and the application of the Nivison-Shaughnessy 2-*yuan* thesis to any history earlier than Western Zhou (most scholars probably don't yet accept it at all; and even Ed doesn't accept it for Shang).

29. In the long run I am going to be vindicated on both counts. In the meantime, I grant that controversy has to be respected. Ed does not. This doesn't show in mere email exchange, where he can always say, "Okay, I'm stubborn, but you are too, because you won't agree with me" (thus confusing "equally, stubborn" with "equally stubborn"). It did show, now that he held power. The Nivison-Pang communication was designed as a "research note," a category in *EC* not requiring review. Ed (correctly) saw our communication as implying a fundamental challenge to his own way of thinking, and responded by decreeing that henceforth all "research notes" would be reviewed (thus in effect killing the category), and simultaneously sent our text to two reviewers. He got one "yes" and one "no"—"no" on the ground that the subject was too important for a mere note, with the imprimatur of a review. Ed then assumed decision power and said no. I threatened to appeal to his associate editors. He then sought another review, getting a recommendation that the Nivison-Pang piece be published as a "Forum" target, a solution I welcomed.

This gave me more space, and probably attracted a bigger audience. At least, these were my reasons for accepting the "target" idea. But after a half-year friendly exchange with Shaughnessy this year, I am getting nowhere with him. He still is unconcerned with the moving of Zhou Gong's dates, for which I can see only one explanation: The moving of the dates was intentional. The intention was to create a strip that could be moved into the Wu Wang text giving him

three more years of life. One can imagine the Jin editors wanting this result, but one cannot imagine them daring to move Zhou Gong's dates, or daring to make the text say (as it does now) that there was a *di* rite for Zhou Gong before he died. One can imagine Huicheng Wang of Wei not just wanting but feeling compelled to get this result (of getting the conquest in 1050, requiring Wu Wang to live three more years), and one can also imagine his underlings being willing to do whatever he told them to do, indifferent to the *di* rite difficulty.

Unfortunately, I can also imagine Ed Shaughnessy being indifferent to the *di* rite difficulty. One must, because he was, and still is.

30. In Sum: Ed is still assuming that whatever is wrong with the *BA* is the result of Jin editors' error or worse (inventing "out of whole cloth"), and that if we could just get back to the *guben* "original" (as he thinks), all would be well. He has to hope so; it maintains the possibility for him that the "original" supports him—notably, his date 1045 for the conquest. Consequently, he must make the *jinben* assume a huge burden of error. The date 1045 is incompatible with the deletion of mournings. It assumes 1056 as *yuan* for a Wen-Wu calendar with 1045 as year 12, and as year 1 for a 100-year count to Mu Wang 1 = 956. Deleting mournings raises 956 to 962, forcing year 1 of 100 to become 1062, the last of Wen Wang's nine "*shou ming*" years, and forcing 1050 as conquest year, requiring the moving of Shaughnessy's strip. So even deletion of mournings is part of that burden. 55 years for Mu Wang, and 75 years for Tai Wu, which depend on deletion of mournings, have to be part of that burden too (contra *Shiji*, and contra *Shang shu* "Wu Yi": for they already contain these errors). Everywhere, a mess.

Ed needs to count his costs. And he won't, because the cost of counting costs is to accept the principle that everything that could be relevant must be at least consistently explainable if not actually explained, and he won't do that, nor will he suffer anyone else trying it. Is this why he bridles at my offering him a brief note providing evidence for dating reigns in early Xia? And at my publishing a book daring to work out the changes in the chronology of Xia and Shang? These are things he just knows can't be done. So he asks, "How can Nivison be so wrong?"

Appendix: Notes on Edward L. Shaughnessy (ELS) in *EC* 11–12

Page 33–35: ELS reviews his previous work and emphasizes two points: (1) There was a tradition accepted by most historians, that from the beginning of Zhou to Mu Wang was 100 years; but also (2) that there were two views as to what that beginning was: (a) as in the *jinben Annals*, the beginning was counted from the beginning of Wu Wang’s personal reign; and (b) in what ELS calls the *guben Annals*, the count was from the “receipt of the Mandate” (*shou ming*) by Wen Wang. ELS sees this latter question of great importance for determining the absolute date of the conquest of Shang. ELS also betrays here the common assumption that the *guben* was the correct text, and all errors are confined to the *jinben* and are therefore due to Jin Dynasty editing.

DSN comment: The *guben* is not a text. It is the name that has been given to any *Annals* fragment found quoted in some later historical commentary or encyclopedia, which may or may not differ from the *jinben*. Such a fragment may differ in being the result of more careful work on the discovered text. But whether different or the same, it may be wrong, because the *Annals* went through a long evolution, including deliberate falsification, before the text was buried. Further, the 100 years tradition (1) is not necessarily wrong for being a “tradition”; and as for the two “views,” ELS fails to notice that these two apparently different views may simply be two equally valid descriptions of the same view: 100 years can in Chinese be counted inclusively or exclusively. An inclusive count would start with Wu Wang’s succession year. An exclusive count from Wen Wang’s last year but excluding that year would count exactly the same years. This would be the natural way to count if “*shou ming*” referred to a *period* of years. And this is what happened: Wen Wang’s last nine years (in *Yi Zhou shu*) were his “*shou ming*” years. Not remembering this, ELS has set up his plot so that the tomb text (= *guben* = correct text) could be whatever he wants.

36: ELS, not seeing the possibility I have described, “proves” (first half of p. 36) that the *shou ming* (he thinks *guben*) chronology is correct and the present text account of Wu Wang’s pre-conquest reign is “fabricated out of whole cloth.” He is assuming that *shou ming* must refer to the *first* year of a *shou ming* calendar, with Wen Wang ruling for some time after that, and with Wu Wang ruling pre-conquest necessarily less than the eleven years he gets in the *jinben*. (I agree: 1056–1050, Wen *shou ming*; 1049–1041 Wu pre-conquest; 1040–1038, Wu king. But this isn’t what ELS wants.)

DSN: So far, ELS uses no absolute dates at all, so he cannot begin to ask how the eleven years were arrived at. (The answer: Huicheng Wang of Wei was forced to make the conquest year a Chun Huo year, which had to be 1050, also (impossibly) the death year of Wen Wang; so the conjunction and Zhou dates had to be backed 12 years, putting Wen Wang's death in 1062, which was 100 years before the first year of Mu Wang, 962, after deletion of mournings. ELS is led to his "fabrication" fantasy by refusing to consider even the possibility of chronology doctoring in Wei—which corrupted the tomb text, and shows that the 100 years thesis was an idea taken for granted in Warring States.)

37–38 (and second half of p. 36, Section One): ELS explores three Han sources on conquest chronology: the *Shiji*, Zheng Xuan (from commentaries) on the Yin Li, and Liu Xin's *Santong* 三統 chronology per *Han Shu*, "Lü Li zhi." He finds all three agreeing that conquest events, from Wen Wang's being recognized as having the Mandate, through his death to Wu Wang's victory, are dated in a continuing Wen-Wu calendar. He concludes that we must accept this as fact.

DSN: There was a "continuing calendar" in a "100 years" sense: Mu Wang's first year was 956, and 1056 was year one of 100 before that. 1056 was the year of Zhou moving its capital from Cheng to Feng. I have argued that as did Tang of Shang in 1575, Zhou combined the move of capital with promulgation of a new "Mandate" calendar, 1056–1050. (There was also an informal mandate count from 1058, the year after the conjunction. This is the count that gives the 100 years "*shou ming*" count, after the 12-year backshift of dates made 1062 the last year of Wen Wang's *shou ming* calendar in *Yi Zhou shu* "Wen Zhuan.") The year 1056 became the first year of a continuing calendar only after chronology had moved the conquest from 1040 back 5 to 1045 (by replacing the first two mourning years in Cheng Wang's reign with the 7-year Zhou Gong Regency instead of having the Regency be the first 7 of Cheng Wang's 2 + 30).

The *Shiji*: The "Zhou benji" gives us what looks like a continuing calendar. Wen Wang dies in year 7 (of his 1056 calendar). In year 9 Wu Wang marches east to Mengjin. In winter (months 11 and 12) of year 11 he marches against Shang. In month 2 Zhou wins the battle of Muye, year not stated, but we naturally take it as year 12 (as does ELS). But the "shijia" chapters say that this campaign and victory was in Wu Wang's year 11. Further, the "Lu Shijia" says explicitly that year 9, the Mengjin campaign, occurred not two years after Wen Wang's death but after a long account of Wu Wang's opening years, implying that this was year 9 of Wu Wang's reign, not year 9 in a continuing calendar. Further, it has the Muye battle explicitly in year 11.

First, the explanation of “year 11” and “month 2” sans year number. It is clear from the “shijia” chapters (Lu, Qi) that the scope of “year 11” extends from months 11, 12, ... 2, on. What Han dynasty Chinese are doing is to use a standard way of naming months in order, independently of the identity of the first month in the current calendar (as we do when we speak of “December” = “10th month” even though for us it is the 12th month). The system makes “11th month” the *name* of the winter solstice month, even if (for example in the *Han shu*) the current civil year begins with the pre-solstice month. Applied to dates in earlier history this is anachronistic, but Sima Qian does it anyway. ELS simply stamps his foot at my analysis, calling it “unconvincing,” without argument.

My analysis does not require understanding “12th year” before “2nd month,” and the “shijia” chapters forbid it. But obviously we do need it. The *Shiji* is wrong here, but the belief that the *Shiji* is wrong about—that the conquest was in a 12th year—is also wrong, and that belief must have been earlier than the *Shiji*’s distortion of it. This is interesting, because my way to account for it is to connect it with the placing of ELS’s strip in the Wu Wang chronicle, pushing the conquest back three years, making it coincide with the death year of Wen Wang, which must then be moved back 12 years. ELS can reply that there could well be another way of accounting for the *Shiji*’s “11th year.”

But here is another piece of information which cannot be handled so easily: In the Huang Di chronicle we find the date “50th year, 7th month, day *gengshen* (57),” for important rites and events. The year indicated is 2353, too far back for the date to be a record. Assuming the first of the month is intended, and counting forward 1900 *zhang* gets one to the day *yihai* (12) of the year 453, 7th month 1st day. 453 is a famous date: it dates the battle in which the “Three Jin” states led by Wei defeated and destroyed Zhi Bo, thus gaining their independence. We can speculate as to why this date in the mythical Huang Di reign was invented, but it obviously must have been done by a Wei chronologist. The span of time 2353 to 453 must contain the three years of ELS’ strip. It is therefore impossible not to believe that that strip text was invented in Wei, and—in Wei—transposed into the Wu Wang chronicle.

Next, what ELS does not understand about the Yin Li described by Zheng Xuan: Yin Li time intervals are plucked from different sources. If you (not I) accept 37 years as Li Wang’s pre-exile reign instead of the *BA* 12 years, you are pushing earlier dates back 25 years. The Yin Li has 1579 instead of 1554 for the first year of Shang. Its “29th year of *wuwu bu*” as Mandate year simply substitutes the first year of the current *bu* for the actual first year of Di Xin, 1086. The Yin Li is worthless for chronological work.

So is Liu Xin, but he is interesting (p. 41): He has *Yi Zhou shu* “Wen Zhuan” on his desk, so he knows that Wen Wang was alive in *shou ming* 9, and died then, not (he thinks) in year 7. So he concludes that Sima Qian was simply wrong, by 2 years: year 9, Wen dies; year 11, Mengjin; therefore year 13, conquest. For Liu, this must be right, because he knows that the Mandate year was a Chun Huo year, and (also with *Guoyu* “Zhou Yu” 3.7 on his desk) he knows that the conquest year was also a Chun Huo year, 13 (inclusive) years later. This shows that he was accepting the month-naming convention that in the “Zhou benji” makes year 11 the conquest year.

39: ELS copies the text for the years he claims were invented out of whole cloth, and (p.40) tries to justify the claim by analyzing them. They are years Di Xin 42 = Wu Wang 1 through Di Xin 52—Wu Wang 11, six years and five blanks (45–47, 49–50).

40: (1) ELS points out that gengyin (27) in year 52 is anomalous. (2) Much of the text describes fabulous portents. (3) The 44th year text on the conquest of Li must be the campaign described in *Shang shu* “Xi Bo kan Li,” contradicting the *Shang shu Dazhuan* and the *Shiji*, which assign this campaign to Wen Wang. (4) Dating the Mengjin campaign to year 52 contradicts the *Shiji*, which dates it to year 9, second year after the year of Wen Wang’s death and while Wu Wang was still in mourning, as indicated by the “Bo Yi *liezhuan*.”

DSN:

- (1) “*gengyin*” survives from another editing of the text making this year the beginning of Zhou.
- (2) This objection is irrelevant.
- (3) Di Xin 44 (1059 in the *BA*) here = Wu Wang 3. Wu Wang 1 was actually 1049, so Di Xin 44 is in some way derived from 1047. The Qinghua strips have a text dated Wu Wang 8, and saying that this year (= 1042) is the date of the Li campaign. When the date 1040 for the conquest was moved back 5 to 1045 (by replacing Cheng Wang’s two mourning years with the 7-year Regency), the 1042 event was also moved back 5 to 1047, which is the real Wu Wang 3. Then, when the conquest was reset to 1050 and Wen Wang’s death date with other years had to be move back 12, the Li campaign date became 1059, which is Di Xin 44 (on *yuan* 1102). Therefore this Di Xin 44 event must be identical with the Qinghua Wu Wang 8 event. Either this event is not the “Xi Bo kan Li” event, or assigning the “Xi Bo kan Li” event to Wen Wang is a mistake.
- (4) The Mengjin campaign is a myth. But in any case, the *Shiji* “year 9” for it was not the 2nd year of Wu Wang, because the *Shiji* only seems to be using a Wen-Wu continuous calendar—as the “Lu Shijia” shows. The story of Bo Yi

reproaching Wu Wang for impiety must have been invented to fit the mistaken reading of the “Benji” account. The *Shiji* explicitly makes the Mengjin campaign two years before the conquest campaign. The *BA* does exactly the same, except that there the conquest is dated to year 12, so the Mengjin campaign has to be dated to year 10.

41–42: ELS puts together various data trying to reconstruct the chronology of the immediate pre-conquest years. He concludes that the Jin editors would have found that they couldn’t stuff all of the events into the available time between Wen Wang’s death and the conquest (he says “a two or three year interval”; it should be four years). They therefore invented an 11-year span, taking the conquest date “12th year” to be in a Wu Wang personal regnal calendar rather than in a Wen-Wu continuing calendar, and put into it the events they needed to accommodate—except that he says (pp. 39–40 above) that the events were invented.

DSN: ELS is right that the conquest in the *BA* is dated 12th year (1050) in a Wu Wang calendar that began with a Wu Wang succession year (1061): ELS is avoiding, apparently on principle, using absolute dates in his analysis, here and later, needlessly confusing himself. But he is wrong in blaming the Jin editors. The mischief was all done in late Warring States Wei, by nameless persons following the orders of the first two Wei kings. The result was a collision at 1050, which it became necessary to call the conquest date, though it was already the date of Wen Wang’s death. This forced a revision setting pre-conquest dates back one 12-year Jupiter cycle, so that Wen Wang’s death date became 1062 (100 years before 962, Mu Wang 1 after deletion of 2-year mourning-completions). The supposed Wen-Wu continuing calendar was created out of the 100-year idea, when the true conquest date 1040 was shifted back to 1045, calling it year 12, without altering Wen Wang’s death date. One objective was to avoid having Wu Wang conquer in year 17 (1040, counting from 1056) but die in year 12 (1038, counting from 1049).

Incidentally, ELS is wrong (bottom of p. 42) in saying that Huangfu Mi like the *Jinben* dated the conjunction immediately before the Mandate year. His *Di wang shi ji* explicitly identifies Wen Wang 42 (= 1058) as a Chun Huo year and the year taken by Wen Wang as the first of the Zhou Mandate years (Nivison 1983 p. 522); but another fragment of the book puts the conjunction in a Da Huo (Fang) year. I don’t know how Huangfu Mi resolves this problem.

43–47: Most of this, presenting Pankenier’s work on the conjunction and work by ELS and others on the Ren Fang (Yi Fang) inscriptions, is good. But pp. 44–45 defends ELS’ date 1045 for the conquest. The argument is well handled,

but the date is wrong, and ELS uses his conclusion as fact in later arguments, rendering them invalid. To clarify this, I should here present an argument just like his for the date 1040.

Day	01	02	03	04	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
Month	11	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33							
11	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33		
12	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	01	02	03	
1	04	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31			
2	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	01	02	
3	03	04	05	06	07	08	09	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31		
4	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	01	

D = days in the lunar month. M = lunar months numbered at left in each line, in the *jian yin* system. In the matrix are *ganzhi* numbers for days in each month, assuming the date (from *zi* month) is 1040. I make three minor changes of a first day, to make the recognized first day conform to a long-short alternation of months. Day-numbers in red are *qi*-center days, determined by taking the winter solstice as two days late (14, in month 11), amending Zhang Peiyu who gives me day 12 (*yihai*) instead of day 14: The Chinese weather-period system divided the year into equal 4ths, to the nearest whole number, whereas the astronomical count of days from autumn equinox to winter solstice was 89 days rather than the 91 days of the 24 weather-period system. So defined, the underlined red days are the key dates of the campaign: On *guisi* (30) in month 11, which was the first day of Xiao Han, the campaign begins. On *jiazi* (01) in month 2, which was the first day of Qing Ming, Zhou is victorious at Muye. On *gengxu* (47) in month 4, which was the first day of Xiao Man, and full moon day, Zhou celebrates victory in the Zhou capital.

48, middle paragraph: Trouble begins here. ELS now takes the dates 1086 (Di Xin 1), 1059 (conjunction) and 1045 (conquest) to be certain, and concludes that the tomb text (which he takes as infallible) must have given Di Xin a 42-year reign. For him, this proves that he is right that the Jin scholars forged extra pre-conquest years for Wu Wang, recorded in an extended Di Xin calendar.

DSN's critique: ELS is still unaware that the Cheng Wang chronicle's Zhou Gong dates have been shifted in order to create the strip of text that got moved into the Wu Wang chronicle; so the move was deliberate, was done *in Wei* for a Wei reason, and so was already in the tomb text. It was this move that forced pushing Wen Wang's death date back 12 years from 1050 to 1062, thereby creating the 11-year pre-conquest reign for Wu Wang. The Jin scholars uncritically copied what they read, but otherwise they are blameless. Wei's reason: to clear 1050 as conquest date, required as presumably a Chun Huo year. ELS' reason for resisting: he has an *a priori* conviction that wherever there is trouble in the text, it is due to post-discovery tampering. He refuses to look at the idea that the *Zhushu jinian* text went through a series of changes in Warring States. ELS had discovered his error by 1993, when he published on the "Shao Gao." But by this time he had become famous for his discovery of the transposed strip. To withdraw and rewrite his account of it, even though he could still say it was transposed, would make him look like Nivison, who had withdrawn his conquest date 1045, just as it was being published.

Next comes an astonishing development in ELS' thinking: Turning his attention to the conjunction of 1059, dated "28th year" counting from 1086, he finds that this event is recorded in the *jinben* as in the 32nd year. Why this 4-year

“discrepancy”? The question is insane. But asking it blinds ELS from seeing what is wrong. The trouble, he says, is that once the silly Jin scholars had made the mistake of treating the 12-year Wen-Wu continuing calendar as a 12-year Wu Wang calendar, adding on years 42 through 52 to Di Xin’s reign recording Wu Wang years 1 through 12, they had to treat Wen Wang’s death as in the 41st year rather than in the 37th year: *voilà!* A four-year discrepancy. So they had to go on and apply this 4-year operation to any (but only) events directly related to Wen Wang’s death date—as of course the conjunction was.

What’s wrong with this is ELS’ failure to see what he is looking at as he reviews the last eleven years of the Di Xin chronicle in the *Annals*. He thinks the dates in the *BA* are *tout court* comparable to the dates he computes in a 1086 calendar. Actually, the Di Xin dates are computed on a fictive calendar beginning in 1102. But ELS is eschewing absolute dates and looking only at relative dates. The false *yuan* for Di Xin—1102—does not even appear in ELS’ text until the end, p. 52. He should be asking, what is the difference between the true *yuan* and the false one, and how do we account for it? The difference is 16 years. It gets to be 16 years in two steps: first, there was a 4-year push-back when the year of the fief of Tang was re-dated—in Wei, and therefore in the tomb text—to 1035 from 1031, so that the conjunction year became a Da Huo year rather than a Chun Shou year.

Next (also in Wei), clearing 1050 as conquest date, the date of Wen Wang’s death got pushed back 12 years. The span of four years shows up in the year number of each event computed from *yuan* 1102. The four-year “adjustment” must therefore be made to all events computed from 1086, not just a select few, and since it applies to Jupiter stages—12 years apart—it ordinarily has no visible effect in the text. The remaining 12 years (of the 16) result from moving Wen Wang’s dates, and earlier dates, back 12 years. The date you get by counting off the event date from 1102 is going to be not the absolute date of the event, but that date pushed back 12 years. To return to ELS’ first example, the date of the conjunction of 1059, which is year number 28 on *yuan* 1086, the year number 32 in the *BA* isn’t showing a four year “discrepancy,” it’s the year number for a *different* mathematical *entity*, namely the date 1059 pushed back 12 years to 1071, counted from 1102

ELS has deceived himself by his fetish of avoiding the use of Western dates and restricting his language to Chinese dates. This matter is serious: it causes ELS’ analysis to misdate a string of events by making them four years later than they were. Among these is the event of moving the capital to Feng, which was done in 1056, not in 1052. This year was the first year of the 100-year count to 956 (the first year of Mu Wang); and while I am not sure ELS agrees with that, he

himself has been using 1056 as the first year in Wen Wang’s 7-year quasi “accession” *shou ming* calendar, and the first year of his 12-year Wen-Wu count to 1045 (which I don’t agree with as history in the sense of *res gestae*). Here is ELS’ table on p. 50:

Actual year	Di Xin year	Event	BA year (Di Xin)	Discrepancy
1059	28	Planetary conjunction	32	4
1058	29	Release of King Wen from Youli	29	0
		Di Xin bestows “mandate” on Wen	33	4
1057	30			
1056	31			
1055	32			
1054	33	Mi surrenders to Zhou	33	0
1053	34	Zhou defeats Li, Yu and Chong	34	0
1052	35	Transfer of capital to Feng	35	0
1051	36			
1050	37	Death of King Wen	41	4

Here is my own table, attempting to make sense of what ELS is doing:

Actual year	Di Xin year number (<i>yuan</i> 1086)	<i>yuan</i> 1086 <i>plus 4</i> , plus 12 = <i>yuan</i> 1102 Event (Bracketed matter expands chronicle into narrative.)	BA year number (Di Xin, <i>yuan</i> 1102)	BA year BC date (less 12 = correct absolute date)
1062	25	Release of King Wen from Youli [not to campaign unless given a <i>ming</i>].*	29	1074 (1062)
1061	26		30	1073 (1061)
1060	27		31	1072 (1060)
1059	28	Planetary conjunction; Mi attacks Ruan; Zhou [defending Shang authority] attacks Mi [without being ordered to]	32	1071 (1059)
1058	29	Mi surrenders to Zhou army; Mi people moved to Cheng. [Shang doesn’t protest Zhou action] King Wen then claims [implied]	33	1070 (1058)

Actual year	Di Xin year number (<i>yuan</i> 1086)	<i>yuan</i> 1086 <i>plus 4</i> , plus 12 = <i>yuan</i> 1102 Event (Bracketed matter expands chronicle into narrative.)	BA year number (Di Xin, <i>yuan</i> 1102)	BA year BC date (less 12 = correct absolute date)
		“Mandate” [to act without a <i>ming</i>]; Di Xin grants [retroactive] <i>ming</i> [to counter claim]		
1057	30	Zhou [using claimed authority] defeats Qi (=Li ?)**, Yu and also Chong; [the Shang king doesn’t dare to object]	34	1069 (1057)
1056	31	Transfer of Zhou capital to Feng; [new Zhou “ <i>shou ming</i> ” calendar <i>yuan</i> , calendar years in parentheses]	35 (1)	1068 (1056)
1055	32	Lords assemble in Zhou; [again, Shang doesn’t dare to object;] Hao planned; this problem is assigned to prince Fa	36 (2)	1067 (1055)
1054	33		37 (3)	1066 (1054)
1053	34		38 (4)	1065 (1053)
1052	35		39 (5)	1064 (1052)
1051	36		40 (6)	1063 (1051)
1050	37	Death of King Wen [in 3 rd month of year 7 of new (<i>shou ming</i>) calendar]***	41 (7)	1062 (1050)

* The *Shiji* accounts of events from Dan Fu to Ji Li to Wen Wang are not dated; we have only the sequence of events; and the record of Wen Wang being given authority to campaign comes after the record of his release and gifts given to him. There is nothing here to require us to assume (with Shaughnessy) that the grant of authority was simultaneous with his being released; and there is no reason to reject the BA’s (much more reasonable) dating of the release being four years earlier than the gift of authority.

** Why Qi here but Li at Di Xin 44 (= Wu Wang 8)? There must be some intended difference in reference.

*** This “*shou ming*” is also counted from 1058 ending with year 9, in *Yi Zhou shu* “Wen zhuan.” As I point out (Nivison 1983, pp. 523, 528–531), Wen Wang could not require his subjects to accept a new calendar without allowing them two years to finish any mourning obligations they might have.

51–53, Conclusions: When I first read ELS’ “conclusions,” I didn’t understand it at all. A day’s reflection showed me that actually my trouble was that I couldn’t believe what I was reading. ELS provides a table:

Event	Actual Date	Bamboo Annals Date	Discrepancy Actual Relative
First-year of Di Xin	1086	1102	16 11*
Planetary conjunction	1059	1071	12 (11–4*=) 7*
Zhou conquest	1045*	1050	5*

The utility of this table is damaged by features marked by my added ‘*’ symbol, meaning that these numbers are invalid. (ELS of course does not agree.) So any reasoning he does with them will appear insane; and he does all of his reasoning with them. He is quite explicit: his 11-year “relative discrepancy” is the 11 years he thinks was invented and added to the text. His 4-year “relative discrepancy” is the 4 years he thinks must be added to a few select dates in his table on p. 50. 7* and 5* are obtained by subtraction from flagged numbers. He does not attempt to explain the “Actual Discrepancy” of 16 years between the two *juan*, 1086 and 1102. Had he tried, he would have written a very different article.

53–60, Notes: Note 7. I do not think the “Ancient” *Bamboo Annals* preserves any one textual tradition. It is a collection of notes. And *Yi Zhou shu* 21 “Feng Bao” is not “spurious.” In M. Loewe’s *Early Chinese Texts*, p. 229, ELS has decided that it is acceptable. But the translation is wrong at a vital point (perhaps following Pankenier): what the king says is “*Wuhu!* The many lords have all come to offer me good wishes. They are worn out with service to Shang” – and so on. The situation is pre-conquest, not post-conquest. This is shown by the date “23rd year, first day of the month *gengzi* (37).” This is enough to identify the date as 26 April 1046. The Chinese date is 5th month (*jian zi* calendar). The month is not given, because this is an assembly in Zhou of heads of other states which like Zhou are independent except for service sometimes owed to Shang, and some of these states had calendars beginning the year with various months. ELS is wrong about the calendar: It is Di Xin’s second calendar beginning in 1068 (beginning with a *geng* day, so perhaps created for Di Xin’s heir Wu Geng; for at least a decade the 1086 calendar continued to be used; see *Riddle* p. 240). At the end, ELS must have meant “no earlier” rather than “no later.”

56–57, note 19: ELS accepts critical literature which shows that Qi and Li are names for the same state. Perhaps so. But why does he not think it strange that the two different words are used here? I need an explanation for this. Why doesn’t he? On p. 59 (note 39) he uses Li where the text has Qi.

Note 27: ELS here dismisses my case for the date 1040 for the conquest, unfortunately without giving reasons in enough detail to let me engage with him.

60, note 41: In his text, at p. 52, he writes, “This is not the place to discuss the origin of the five-year discrepancy between the “Current” *Bamboo Annals* date of the Zhou conquest and its actual date of 1045; it suffices for our purposes that it is unrelated to the editorial changes described in this paper.”

DSN: I cannot imagine a statement more completely wrong for him to end his paper with.

To repeat: 1050 was required when the Wei king chose to make 1035 the year of the fief to Tangshu Yu, beginning Jin, 700 years before Huicheng Wang’s decision to declare kingship in 335. 1050 was required, because it was believed that when Jin began Jupiter was in Da Huo (Fang); and it was also believed that when Wu Wang attacked Shang Jupiter was in Chun Huo. We can know that this was a decision made in Wei, because to handle the problem a strip of text had to be created that would extend Wu Wang’s life by three years (years 15, 16, and 17). And to do this it was necessary to move Zhou Gong’s death and burial dates forward ten years from years 11 and 12 to years 21 and 22, disregarding the fact that the Cheng chronicle had Zhou Gong receiving a posthumous *di* rite in his own *miao* in year 13. This change (and the many other changes it required) must have been deliberate, and not accidental; and it could not have been made in Jin. So the Jin scholars simply copied the text as discovered. By the time of his *EC* article in 1993, ELS had discovered his error: Zhou Gong did die in year 11.

But ELS did nothing about it. If he had revised his position then, *or any time since then*, consistency would force him to agree with me now: The changes in chronology that we both agree are wrong were made in Warring States, and therefore do not imply that the discovered text was in disorder, and rewritten by the Jin scholars.

Why does he not do it? Is he unable to think this out?

23 Important Discoveries and Bad Mistakes

Discoveries:

Pre-Xia: 2353 (Huang Di 50)–453 (Defeat of Zhi Bo) = 100 *zhang*; 2145–625 = 1 *ji*.
Xia: 2029–1953 = 1 *bu*; use of intercalation cycle; *gan*-names of kings; Jie (Di Gui) is fiction.
Shang: Yi Yin a villain; fraternal succession; Pan Geng a usurper; reversal of Tai Wu and Yong Ji explained;
Western Zhou: 24 *qijie* oriented to correct Xia Zhi and Qiu Fen but Dong Zhi is 2 days late.
The result: Qing Ming day was conquest day (1040).

Mistakes:

Post-Mu deletions of mournings should include Yi Wang but not You Wang (as I thought at first): dates of last four kings had to be corrected, and some bronze inscriptions redated. *Guoyu* in the *Shiji*: Error in *Guwenzi Yanjiu* article. At first I accepted 1045 as conquest date, then quickly changed, to 1040 eventually; Wen-Wu continuing calendar problem in the *Shiji*.

Discoveries

1. My enabling discovery is in my *HJAS* article in 1983 (that the *Bamboo Annals* (*BA*) had a historical basis): This was seeing that to make a Wang Guowei analysis of lunar phase terms work for Western Zhou *qing tongqi mingwen*, I had to assume that some inscriptions used a second *yuan* counted from completion of mourning. (I could have done nothing with the *BA*, or with recovering Three Dynasties chronology, without that.)

E. L. Shaughnessy accepted this at once, and added the observation that the second *yuan* was used only late in a reign, and normally for all dates late in a reign. (How late was of course a problem.) The king's death was the most important event late in a reign, so this implied that a king's reign-of-record was normally counted from the second *yuan*, omitting initial mourning years.

I then added the suggestion that the shift to use of the post-mourning *yuan* was probably prompted by the death of the preceding king's chief minister. This seems to be true for the Xuan Wang reign. The change occurs in the middle of 809, after the end of mourning for Gong He. (Here I must assume that the *Shiji* and Sima Qian, ignorant of the explanation of the name "Gong He interregnum," mixes up the names of brothers Gong He and Gong Yu.)

It follows that when the unexpressed mourning years are forgotten (or edited out) but the dates of the beginning and end of the dynasty are still known,

the remaining reigns-of-record cluster toward the beginning and end, and a reign in the middle is enlarged. For Shang this reign is the reign of Tai Wu, the main sequence king in the fifth generation, whose reign is stretched from 3 + 60 years to 75 years. The Shang story is complicated: Tai Wu and his successor Yong Ji are actually reversed. (Otherwise, deleting mournings would have broken the 100-year rule for Tai Wu.) For Western Zhou, the stretched reign is that of the fifth king Mu Wang, whose reign is stretched from 2 + 37 years to 55 years. (Xia is unaffected in this way, because mourning intervals become “no king” intervals.)

The post-Mu Wang story is also complicated. At some time Xuan Wang was given his full succession years 827–782; and the immediate post-Mu reigns were overwritten to avoid recognizing overlapping claims of Xiao Wang and Yi Wang to the four years 867–864: These years were given to Yi Wang, and Xiao Wang was given four extra years, pushing dates back through Gong Wang, who loses four years (2 + 16 years reduced to 12 years).

2. The Gong Wang reign (917/915–900) is noteworthy for having two bronze texts both naming Gong Wang as reigning king, one requiring 917 as *yuan*, the other requiring 915. (Or, if one insists on a one-*yuan* only theory rather than the two-*yuan* theory, one is pushed into rejecting Wang Guowei’s lunar quarters interpretation of lunar phase terms—the course chosen by Xu Fengxian, who thinks she has thus shown Wang’s theory to be “utterly without foundation”—a neat if unwitting demonstration of the principle that there is no pure given in experience. Everything from the beginning is theory-laden. (Xu refuses to consider the two-*yuan* theory because it is only a theory. See my review of Xu’s article in this book.)

Also in the Gong Wang reign is what I think is a major discovery: The famous *Mao Gong ding* inscription—the longest known—is a copy, not 19th century ce as Barnard wishes, but a late 9th or early 8th century BC copy, not expected to deceive anyone. This explains the late Western Zhou décor, combined with a text very similar to the *Shi Hong gui*, which must be dated 917. The lost or yet undiscovered original must have celebrated the appointment of Mu Wang general Mao Qian as first minister in Gong Wang 9 (909, actual first day 15 December 910), according to the *BA*. It has long been noticed, with puzzlement, that the text contains no dedication to an ancestor. Examining the text, one finds that the last two columns of characters are short, only twelve instead of fifteen or sixteen *zì*, and the characters are stretched to fill up space. So this is where the dedication must have been, in the original. Confucius will centuries later make the point that you must not sacrifice to a spirit who doesn’t belong to you.

So, if he is to make any religious use of his copy, the copier had to omit the dedication.

There is also the matter of the name of the person receiving the appointment, as he is addressed by the king in the text. The name used is “Fu Yin.” It ought in some way to echo the sense of Mao Qian’s personal name, and it does, for we find the phrases “*de yin*” 德音 “echo of virtue” and “*qian de*” 遷德 “inspired virtue” in the *Shijing*. (Examples: *qian ming de*, Ode 241.2 “inspire bright virtue”; *de yin*, Ode 172.3,4 “reputation for virtue.”)

3. Conquest era events are misdated in the *BA*, and finding out what the correct dates are requires one to show that key dates were two days late (as I will show). I guessed at a reason for this: the Chinese of the time were two days behind in observations that would have corrected for precession (which they had not yet grasped, but close observations would have prompted the necessary corrections). I was wrong, but was making a defensible first approximation. My mistake was apparent to me when I worked out carefully the dates in the set of 70 or more *jiagu* inscriptions in the late Shang Yi Fang campaign. I needed to show that there was an intercalary 9th month at the beginning of the campaign. Showing this required discovering the applicable rule for intra-year intercalation. I guessed (correctly) that the *qi*-center rule used in early Han would turn out to have been in use already in Shang.

The rule: The middle day in a 12-“month” solar calendar counted as a *qi*-center, the first one in the year being the recognized winter solstice day, and the following ones determined as in the set of twenty-four 15- or 16-day solar seasons described in the “Tian Wen” chapter of the *Huainanzi*. The 16-day ones are arranged so that the spring equinox day, the summer solstice day, and the autumn equinox day are all taken to be *qi*-center days, and the other *qi*-center days set accordingly. Any lunar month that *did not* contain a *qi*-center so defined must count as intercalary.

4. What I found when I studied the calendar information in the inscriptions for the campaign against the Yi Fang (a Huai Valley “barbarian” people)—the information is rich enough to pin down the exact year, 1077 BC—is that the suspected intercalary month was immediately preceded by the *qi*-center which was the autumn equinox. And the Chinese calendar was exactly right: on that day the sun was at 180 degrees. This meant that my explanation for the two-day discrepancies could not be right.

A moment of reflection told me that the trouble must lie in the intercalation system itself: The system is built on an approximate division of the solar year

into fourths, to the nearest whole number. It therefore doesn't allow for the fact that the earth moves in its non-circular orbit faster in northern hemisphere winter, when the earth is closer to the sun; the interval from autumn equinox to winter solstice is two days less than a quarter of the solar year, in our part of the 26,000-year precession cycle, and nobody knew this then (nobody: not just the ancient Chinese). Dates I had been checking were in winter and spring. The departure from (Chinese) perfection is absorbed by the time of the summer solstice; the interval from summer solstice to autumn equinox is almost exactly a fourth of the solar year, with the dates for Xia Zhi and Qiu Fen usually correct.

5. This was basic information which led to exciting discoveries in detail. There are more than forty candidate hypotheses for the date of the Zhou conquest of Shang; and it will perhaps take a century for enough scholars to read enough of the available literature for a consensus to be reached. Even if most come to agree with Wang Guowei's analysis of lunar phase terms, and even if most come to agree that the conquest must have been after the conjunction of 1059, two dates, 1045 and 1040, are more or less satisfied by Wang's analysis. I remained in doubt about the date for eight years, from 1983 to 1990. Finally I found evidence that requires the date 1040. Of many proofs, I select two. First, in *Riddle Chapter One*, 1.4.5, "Confirmation of 1040, #5" reads as follows:

Yi Zhou shu 45, "Wu Jing" (Wu Wang warned) begins, "12th year, 4th month. The king reported a dream. On day *bingchen* (53) it was divined...." (We are supposed to understand that this dream was an omen portending the king's imminent death.) "An order then was given for Dan, Duke of Zhou, to appoint the successor, and to give Prince Song the text (of the order), and a copy of the "Bao Dian" (Treasured Document)." When a date is thus incomplete, normally the first of the month is meant, which should be *yimao* (52). *The 12th year (of Wu Wang, counting from his succession in 1049) was 1038, and the 4th month begins with yimao if one supposes that the day counted as winter solstice was two days late.* Shang oracle inscriptions suggest that this was the practice, i.e., the autumn equinox day was determined by observation, and the interval to the winter solstice (89 days) was assumed to be 91 days. The "Bao Dian," which is *Yi Zhou shu* 29, opens with a complete date: "It was the King's 3rd cult-year, 2nd month, day *bingchen* (53), first of the month...." This should be the same year, for if the 2nd month began with *bingchen* (53), the 4th month (30 + 29 days later) should begin with *yimao* (52). The only year that could be *both* "the King's 3rd year" and *also*, in another calendar (counting from the year following his father's death) his 12th year, is 1038, and 1038 only if the Conquest was in 1040. (Emphasis added; the winter solstice was *yiyou* (22), last day of Zhang Peiyu's first month; two days later was *dinghai* (24), second day of Zhang's second month, so we must call it the first month.)

Second, *Riddle Chapter One*, 1.4.11, "Confirmation of 1040, #11" offers the following argument:

One might expect that astrologically weighted days would be preferred for the performance of important ceremonies or the inauguration of great events: the first day of the

year, for major appointments; full moon day, for a holocaust sacrifice; the first day of a month and/or season, for the start of a campaign. Similarly, the first days of the twenty-four weather periods could be expected to be thus favored. If one starts with the true winter solstice day, and counts off the weather periods (*qi jie*) taking that day as the first day of Dong Zhi, the major events of the Conquest campaign fall on such days, if the year was 1045. But if one supposes that the true autumn equinox was taken as Qiu Fen day (making the observed winter solstice day two days late, if one divides the year into equal fourths), then it is 1040 that satisfies this test:

I go on to show that if the same assumption about the Zhou calendar is made, winter solstice two days late, the second *jiazi* day of 1040 is 18 April, Julian Day 134 1671, the first day of the Qing Ming weather period. This is confirmed as Victory Day at Muye, by the last line of *Shijing* Ode #236 “Da Ming” in the *Da Ya*, about Wu Wang, the Zhou king and commander:

肆伐大商，會朝清明

Then he attacked Great Shang; this occurred in the morning, Qing Ming [Day].

No one has ever interpreted the line this way, because you must have both the exact date, and also the knowledge of how the calendar was structured, in order to get it. This argument also gives us the meaning of the opening words of the recently discovered *Li gui* inscription:

武王征商，為甲子朝。歲鼎可聞夙有商

It was when Wu Wang attacked Shang, early in the morning on *jiazi*. In the annual cauldron rite, we were able to report [to the royal ancestors] that we had quickly defeated the Shang.

The day being Qing Ming day, sacred to the ancestors, it was the date of the most important *ding* rite of the year.

What I had learned also enabled me to work out the intercalation schedule for the first twenty years of the reign of Di Xin; and then, with some rule-of-thumb reasoning, to project it back to the time of the Wu Ding—Zu Geng transition, reconfirming Wu Ding’s death date (1189 BC), and analyzing a *jiagu* fragment which turned out to be from the first year of Zu Geng, when he was correcting the calendar disorders of the last years of Wu Ding’s 3 + 59 year reign (when Wu Ding was very old and sick). Zu Geng needed three 6th months to fix the year 1188: (1) the scheduled 6th month; (2) an intercalary 6th month, dictated by the *qi*-center rule; and (3) an extra 6th month needed to update the calendar.

6. If I found intercalation useful in working out chronological problems, it must have been basic in Chinese thinking about chronology, and useful there-

fore to ancient Chinese in creating the problems I wanted to solve. The system: 19 years = 1 *zhang*, containing 7 intercalary months. 4 *zhang* less 1 day = 1 *bu* of 76 years. 20 *bu* = 1 *ji* of 1520 years, completing the cycle. At that point, the correlation of *ganzi* and first days of months was supposed to start over. I applied this to research by Kevin Pang, who had (he hoped) identified the annular solar eclipse of 16 October 1876 as the eclipse of the 9th month of the 5th year of the 4th Xia king Zhong Kang—a challenging problem. Putting 1953 for 2029, and making intervals between reigns all 2 years for mourning-completion, and accepting *BA* reign lengths, I got Pang’s date 1876 for the eclipse. (He and I published at once.)

There is more to that story. The *BA* date of the eclipse converts to 1948, and the day given is 9th month *shuo*, *gengxu* (47). Further, to be consistent with the true date for the beginning of Xia (1 *bu* later than the *BA* date), the invented date for the eclipse ought to be 1952 (1 *bu* earlier than Pang’s date 1876) rather than 1948. There was no eclipse on that day in 1948; so where did the false date come from? I reasoned that the person improving the text must have started with the year 1952. Not having any record of that year, he used a reference to an invented text in the *Zuo zhuan*, to a famous solar eclipse “in the books of Xia between the equinox and the solstice” located in lunar lodge Fang.

Then he dropped down one *ji* (intercalation cycle) of 1520 years to 432, checking to see if on that date the sun was in Fang. It wasn’t, nor in 431, 430, or 429. But in 428, it was. Further, the day, in the 9th month, was *gengxu*. So he went back 1520 years to 1948 and entered the information we find there now. I infer from this that in the late 5th century BC China still had accurate astronomical and political information about events in the 20th century BC—which probably must have been preserved for centuries in records kept in knotted cords.

7. Starting with 1953, accepting *BA* reign lengths for Xia, and positing regular gaps of two years between reigns for mourning, was confirmed by the eclipse. Continuing, I got to 17 February 1577 as the first day of the 14th king Kong Jia—the only Xia king with a *gan* (namely *jia*) in his name. But every Shang king has a name like this. I had the exact date. Could this be the key? Much work by many scholars on Shang kings’ *gan* names had all been guesses; but they didn’t have exact dates, so they could not ask my question. I worked out the *ganzhi* for 17 February 1577: it was *jiazi*. (See paper “Kong Jia of Xia.”) So I applied the idea to all of the Shang kings, and it works:

(See *Riddle* Chapter Two, p. 49, Table V, “Shang Kings: How Dates Determine *gan* Names”)

The *gan* rule must be adapted to other rules governing Shang royal names: *gui* is taboo, and defaults to *jia*; no two successive reigns can have the same *gan*; so when the succession year would dictate a forbidden *gan*, the post-mourning year was used instead.)

I think it is already evident that breaking into a formal system—such as a chronology—must be like breaking into a code. If you are successful, success will show right off. I knew I was on target with the first day of Kong Jia. Pankenier had shown Xia had to stop at 1555. Applying the rules that had gotten me to the first day of Kong Jia, I found that I ran out of years—not approximately, but exactly—with the last year of Fa, the next-to-last king. So I knew that the last king, Di Gui (Jie, infamous as the worst end-of-dynasty king in Chinese history), never existed. This I confirmed with subsequent work, including “The 31 years problem,” contained in this book. This is perhaps my most startling discovery.

8. But there are others. Using the *gan* rule, I could figure out just what went on in the Shang succession after Tang, confirming the *Annals* account and adding details: The *BA* is right: Yi Yin was not a paragon of ministerial virtue; he was a scoundrel, who almost succeeded in stealing the throne (and Mencius’ defense of him is mere philosophical piety). Pan Geng was a usurper, who claimed his father’s post-mourning 4 years as part of his own “28” years. He failed, because the following two younger-brother kings Xiao Xin and Xiao Yi were also looking out for themselves. This means that Wu Ding too was a usurper—perhaps explaining why he is known for fidelity to the mourning rites: this would be a way for him to counteract criticism for illegitimacy. The Shang rule, I think, had been that a dying “main sequence” king was succeeded by a younger brother who held the throne in trust, while the true heir as *xiao wang* was occupied with mourning. This explains Zu Ji, who should have succeeded Zu Geng. (See my study of *Shang shu* “Gao Zong Rong ri” in this book. There is a *jiagu* inscription referring to “*xiao wang* Father Ji, which must be dated to the reign of Kang Ding.) From this time on in Shang, father-son succession replaces fraternal succession, and the king’s *gan* is determined by the year he was appointed *xiao wang*.

9. The great sage kings Yao, Shun and Yu also had feet of clay. Yao did not reign 100 years, of course. His son Zhu was exiled in his 58th year, which was 1969. Yao, old and foggy, was gently retired at the same time, all of this managed by the man who succeeded Yao in 1968, namely Shun, who kept Yao and his son apart. In 9 years (1960) Yao died, and a 2-year calendar break, 1959–58, was observed for completion of mourning for him. 1957 thus counted as Shun 10, and 1953, the year of the tightest planetary conjunction ever witnessed

(about March 1), discovered by Pankenier, counts as Shun 14, the year Shun handed over government functions to Yu, who had already become prominent. I assume that Yu made use of the terror excited by the conjunction to carry out a *coup d'état*.

The stories woven into the accounts of the three are Warring States myth. Perhaps even their names are mythical. But the dates deserve respect. 1953 is certain, and I take it to be Shun 14, leading me to 1968 as Shun 1. If Yao 58 is 1969, Yao 1 is 2026. But in the *BA* Yao 1 is 2145, which is a mere numerological construct. But it is independent evidence: it is 1000 years before 1145, when Shang king Wu Yi granted court status to Dan Fu, lord of Zhou. The difference between 2145 and 2026 is 119 years: 1 year, extending mourning for Yao from 2 to 3 years; plus 42 years, extending his reign from 58 to 100 years; and 76 years = 1 *bu*, moving the first year of Xia from 1953 to 2029. These intervals are unavoidable for the person(s) thinking this out; so the astronomical date 1953 as first year of Xia also supports 2026 as *yuán* for Yao.

Mistakes

10. My worst mistake was at the beginning (*HJAS* 1983). I incorrectly identified the mournings in Western Zhou after Mu Wang. I supposed that only the five kings who immediately succeeded their fathers had mournings which disappeared, and I took those to be kings Gong, Yih, Li, Xuan and You. Actually, You Wang cannot be in that list, because being the last king, killed with the destruction of his regime, he had no reign-of-record counted from a post-mourning *yuán*. And Yi Wang must be in the list, because his father Yih Wang must have lived through the first five years of the reign of the irregular Xiao Wang, at which point Yi Wang claimed the throne and did observe mourning, as bronze inscriptions show. In order to have You Wang begin with mourning years, I assumed that his reign must have begun two years earlier than it did, taking the two years from Xuan Wang. But that won't do: a recently discovered trove of vessels includes a *Qiu ding*, 43rd year 2nd quarter, day *dinghai* (24), and it must be dated 783, on Xuan Wang's post-mourning *yuán* 825.

11. I had compounded this error by dating the *Shi Hong gui* to 783 instead of 917 (both dates fit). Its text is similar to the text of the *Mao Gong ding*, and the *Mao Gong ding* has a distinctive late Western Zhou décor. But an unpublished paper by Shaughnessy on recent discoveries gives us a *Shi You ding*, which will accept only a Gong Wang date; and Shi Hong is probably Shi You's father. Therefore my dating of the *Shi Hong gui* was wrong; it must be dated 917. But what then of

the famous *Mao Gong ding*, which has no expressed date but has a late Western Zhou style? Pushing this question led me to another exciting discovery, details above.

Meanwhile my You Wang error had spawned other errors, correctable but causing tedious work: In my 1983 *HJAS* article I had argued that the *BA*'s date 853 for Li Wang reflected three deleted 2-year mournings, for Li Wang, Xuan Wang, and You Wang. If You Wang must be excluded, it follows that my dates for Li Wang's succession and accession, and for Yi Wang's death, must be moved down two years, throughout my argument: Yi Wang must have died in 858, not 860; and Li Wang's opening years must be 857 and 855, not 859 and 857. Four kings are affected, and dates of bronze vessels must be corrected accordingly.

12. Before I saw that all this was error, I made another embarrassing independent mistake, publishing in the journal *Guwenzi Yanjiu*. With David Pankenier, I had been discussing the astronomy/astrology in *Guoyu* "Zhou Yu" 3.7 describing the heavens at the time of the Zhou conquest. At first I accepted it as historical (as did David, and as does Li Xueqin: it supports their date 1046 for the conquest). Then others (I think Robin Yates was one) persuaded me that it couldn't be historical. So I worked out a "proof" that it was invented in the first century BC and inserted into the *Guoyu*, too late for Sima Qian to have used it in the *Shiji* (as I supposed). David then argued in an article that Qiu Xigui had argued that details in the text pointed to a date in the 5th century BC; and an article by Li Xueqin pointed to a detail in the *Shiji* that must have come from the *Guoyu* text. So I knew my proof was wrong. I then worked out another proof of invention in the 5th century, presenting it at a meeting of the American Oriental Society in Boston in 1992. Perhaps details like this (as well as 1045 as conquest date, which I at first supported) should be described as first approximations rather than as errors. But after a theory you have proposed has been proved wrong—like the date 1045 for the conquest—if you continue to insist on it (as does Ed Shaughnessy), then "error" becomes the right word.

13. I have admitted that 1045 as conquest date is a mistake, and for a while was an error I adhered to myself, actually publishing it in 1983. I withdrew the claim at once, but I was uncertain for six years, and used the date 1045 again in an article in 1989. I ought therefore to explain what led me to this error, and what led me to see it as error.

The case for 1045 is fairly simple: We know that there was an impressive conjunction of planets in 1059. From *Yi Zhou shu* "Xiao Kai" we have an account

of a lunar eclipse in March of 1065, said to be in “year 35,” Wen Wang being the only possibility. The *Shiji* says that he reigned 7 years after being recognized as having the “Mandate” of Heaven, but *Yi Zhou shu* “Wen Zhuan” implies that he died in year 9 of his “*shou ming*” years. “7 years” and “9 years” of *shou ming* are reconciled by an extension of the concept of mourning-completion (as I explain in my 1983 article). The *BA* says Wen Wang reigned 52 years, and died 9 years after the conjunction (conjunction date and death date back-dated 12 years to avoid having him die in the conquest year); but the *Shiji* indicates 50 years; so the “Xiao Kai” “35 years” must be in Wen Wang’s accession calendar, making his death date 1050.

The *Shiji* then describes a preliminary campaign to test his allies, put in the 9th year, with precautions taken to avoid the impression that he is campaigning too soon after his father’s death (Wen Wang’s tablet is placed in a chariot, as if he were in command). (And the idea that Wu Wang is campaigning too soon is immortalized in the “Bo Yi” *liezhuan* chapter. It is also embedded in later moral philosophy; Wang Yangming is forever talking about it, making it the basis of his own situationist ethics.) This produces the idea that the calendar beginning in 1056 continues through Wen Wang’s death and Wu Wang’s succession. The *Shiji* “Zhou Benji” goes on to record details of a final campaign, begun “after 2 years” (*zhu er nian*), with Wu Wang getting his army across the Yellow River “on day *wuwu* (55) in the 12th month of the 11th year.” The account of the victory battle follows “in the 2nd month” (with no year date). This has usually been understood as making the date of the victory the “12th year.” This was my first reading of it (and continues to be Shaughnessy’s). Both of us had taken it as year 12 in a continuing calendar. Hence the date must be 1045.

14. But if you read on into the “shijia” chapters of the *Shiji*, you find that in both the chapter for Qi and the chapter for Lu, the campaign is put in the 11th year of Wu Wang—no hint of a continuing calendar that included Wen Wang. For a while I assumed that the two parts of the *Shiji* were written by different persons. But if you read still more carefully, you find this is not the case: In the “Lu shijia” (*Shiji juan* 33, “shijia” 3) the story starts with Zhou Gong Dan as a young man in the time of Wen Wang, then when Wu Wang became king, Dan, more gifted than the other princes, regularly assisted Wu Wang. “In the 9th year Wu Wang marched east to Mengjin” etc., and in the 11th year there was a second campaign against Shang, all the way to Muye. In other words, the 9th year is the 9th year of Wu Wang’s calendar, and not the second year after the death of Wen Wang in “year 7”; and the victory at Muye, and the whole final campaign, is put in year 11.

The *Shiji* is wrong, but that is what it says in the “Lu shijia,” and that is what it means in the “Zhou Benji.” That is why there is no change of year date for the Muye battle in the “Zhou Benji” account.

But this means that “11th year 12th month” (when the Zhou army had completed its crossing of the Yellow River) must be in the *same year* as a subsequent “2nd month.” How can that be? Simply because Sima Qian is using the familiar “Xia” *jian yin* calendar to *name* months, even though assuming a calendar year governing Zhou actions that begins the year with (probably) the *zi* month. This was not the right way to read dates in old historical texts, but it was common usage in Sima Qian’s own time. We do it ourselves without thinking: “December” *means* 10th month, but is our *name* for our last=12th month.

15. There remains the problem why proper usage for Sima Qian would have had the conquest in year 12. (It was actually in year 10, in Wu Wang’s calendar.) The answer is that editors of the *BA* text in late Warring States Wei were confronted with the political and propagandist need to make 1050 the conquest year, even though they had records telling them (truly) that 1050 was the year of Wen Wang’s death. They “solved” their problem by moving pre-conquest dates back 12, so that Wen Wang’s death date became 1062, and Wu Wang’s succession year became 1061. (Shaughnessy says that the *Lü Shi Chunqiu*’s date “12th year” for “the deed of *jiazi*” is “not unambiguous.” It is absolutely unambiguous. Shaughnessy cannot admit this, because to do so would be the beginning of admitting that his whole analysis of very ancient chronology is wrong.)

16. Was the very idea of a continuing calendar of the last 7 years of Wen Wang through the reign of Wu Wang based on a misreading of the *Shiji* “Zhou benji”? I think the idea is required in any attempt to make the conquest be both 100 years after Dan Fu’s reception by Wu Yi, and in a 12th year, and this points to Warring States thinking. But a very early mis-reader of the *Shiji* was the influential scholar-bibliographer Liu Xin, within a century after Sima Qian. Liu Xin was aware of the “Wen Zhuan” in *Yi Zhou shu* which has Wen Wang in his 9th “*shou ming*” year giving final counsel to Wu Wang, who succeeds to the throne in the following year.

For Liu this was year 10, not year 8. Liu continues “correcting” Sima Qian by adding 2 to each year number, conceived as in a continuing calendar: Year 9, the *guan bing* campaign to Mengjin, becomes year 11, and year 11, the conquest, becomes year 13. This must be right, Liu thinks, because it completes a Jupiter cycle and is therefore a Chun Huo year as the *Guoyu* makes it, like the Mandate year (for us, year 1058). Any modern scholar (like Pankenier) who thinks the

conquest year was year 13, is in effect following Liu's double mistake. And any ancient scholar who does this is in effect following Sima Qian in using *jian yin* names of months as names of months in any calendar.

17. Discovering that 1045 is wrong does not in itself require that 1040 is right. How did I discover that? Once I had convinced myself of Wang Guowei's analysis of lunar phase terms, I was able to deduce that there were only two years at about the right time for the conquest that would satisfy the lunar phase information supplied by Liu Xin in the *Han shu*, given Wang's analysis: 1045 and 1040. (Lunar phase language will usually be satisfied by both of two dates exactly five years apart.) One needs more information that will be valid for one but not the other. Zheng Xuan says that Cheng Wang was born in Wu Wang's succession year (1049).

If that is right, then any information (and there is much) that puts Wu Wang's death in 1038 will imply that Cheng Wang would need a regent for seven years. So, his having had a regent for seven years (as we know he did) implies that his father died in 1038. More beautiful, when I discovered studying the Yi Fang inscriptions both that already in late Shang the Chinese were using a *qi*-center rule for intercalation, and that they had it astronomically correct for the autumn equinox, I saw that this implied making the recognized winter solstice day two days late. On this basis I deduced that the victory at Muye was on Qing Ming day, which was a *jiazi* day if and only if the year was 1040. Then I remembered the last line of the "Da Ming" ode.

Every error you make, when you correct it leads you to a new discovery. When you see this, you hunt for mistakes in your own work, all the time.

Postface 1

Professor Chen Zhi

Editor in Chief, Bulletin of the Jao Tsung-I Academy of Sinology

Dear Professor Chen:

I write to thank you for the gift of two books: The inaugural issue of the *Jao Tsung-I Guoxueyuan yuankan*. (I am already half way through Michael Loewe's fine article on Liu Xin.) And also the book you edited: *Zhongguo shige chuantong ji wenben yanjiu*. Not sure of my health tonight, I am back at my computer at 1:00 AM. More later, when I'm given time.

I thank you for confirming receipt of the pieces of my book that I sent you two days ago. (Not 30: one, on Xu Fengxian's article in *EC*, is in six pieces. That one required careful work.) If I send no more, I think you have a book, after I will add a brief introduction.

I add this now, with the understanding that I may expand it later:

My work on *jiaguwen* and *qingtongqi mingwen* began in 1971, with informal instruction from Prof. David N. Keightley in his office in Berkeley, CA. I had been working on some problems concerning some archaic Chinese *xuzi*, and had gotten to the point where I saw I must move back from early classics into inscriptions. Hearing by accident of Keightley's work, I phoned him from Stanford, and a warm and lasting friendship followed.

In the course of time, we were participating in each other's seminars. One Sunday evening in November of 1979, I was preparing myself for my seminar the next evening. My subject was to be four bronze inscriptions recording royal actions. The introducer was an official named Sima Gong. I half guessed that he might be Gong he, at an earlier stage in his career. This led me to guess that the reign might be Yi Wang or Li Wang. Aware that I might be on dangerous ground, I thought, why not see what I find in the *Bamboo Annals*? I had Legge's text and translation within reach. I knew, of course, that it is supposed to be a fake, but I don't always believe what my teachers tell me.

Within five minutes, I realized I had discovered gold, and that the *Annals* would probably be my major occupation for the rest of my life. This book of papers is a small part of the result: papers that I could not take time yet to deal with. Most of them are on chronological problems, but important problems nonetheless. I address the problem of the exact date of the Zhou conquest of Shang; and I think that some of my readers will accept it. I also think I have the solution to the problem of the *gan* names of all of the Shang kings. And the reader is going to be asked to accept my conclusion that Jie, the worst "bad last ruler" in Chinese history, is not in Chinese history but belongs to the land of myth.

Two months ago, Professor Chen Zhi called on me at my home in Los Altos. He told me of the newly created Jao Tsung-I Academy of Sinology, and wondered if I might have any unpublished work that would fit into its publication plans. This book is my answer. I am very grateful to Prof. Chen; and to Jao Tsung-I, whom I met years ago in a seminar in the office of Prof. Arthur Wright at Yale University.

David S. Nivison, 13 September 2014

Postscript

This e-mail by Professor Nivison was written but never sent. A surge of sorrow filled my heart when I read these words. I first met David Nivison in the summer of 2012, when I paid a visit to my cousin Cindy Chen whose home was near Stanford University. One day, while we were chatting in the living room after a meal, I noticed an aged gentlemen outside of the window walking slowly with his back hunched and his hands holding a wheelchair. One of my family members said that he was an emeritus professor from Stanford University who specialized in Chinese History, and I thought to myself that this was likely David S. Nivison. I went outside and greeted him, and asked whether he was indeed David Nivison. “Yes, I am David Nivison,” he said. We started talking right away, first at my cousin’s house and then at his, which was only some 300 meters away. In the years that followed we exchanged emails from time to time. Whenever I was in North California I would drop by his home in Stanford, and each time I visited I always saw him working in his study: piles of books were stacked next to a desk full of manuscripts and reference materials. On the very same desk there was a computer with a large screen, which was divided into two smaller screens, one for viewing Chinese text, and the other for typing English. During my visit to him in August 2014, one of his legs was put into cast and wrapped in gauze. The Professor told me he just went through an operation but that the cancer had already spread and his days apparently were numbered. He was in a hurry to finish work that was on his mind. As mentioned above, he promised to write a preface to his collected works that the Jao Tsung-I Academy of Sinology had agreed to publish. I said to him, “Please let me know once you finish it.” “In a sense, it’ll never be complete,” he replied wryly. After a pause, he added in an amused tone, “But of course, if I die, it’s complete.” I laughed at his witty reply. There was inevitably a hint of sadness in my laughter but it was evoked from a genuine admiration for this excellent scholar who spent his entire adult life immersed in scholarship. I had no idea at the time that it would be our last meeting together. He passed away two months later.

In due course, the volume has come together and is now ready for publication thanks to the unwavering commitment of Drs. Adam Schwartz (Jao Tsung I Academy of Sinology) and Cheng Yuhei, my other colleagues in the Academy, especially Mses. Lai Wing Mi, Wang Xintong, and Mr. Travis Chan (who has kindly translated this postface into English from its original Chinese), and finally Dr. Nicolas Williams (formerly of the Jao Tsung I Academy of Sinology, and now at Hong Kong University). I also would like to thank Jim Nivison, Professor David Nivison’s son, for his trust and effort to see this valuable publication through. It was Jim who in 2017 sent me David Nivison’s unsent letter. We have decided to

publish the letter in its entirety to conclude the book and as a way to memorialize this zealous and industrious scholar!

Chen Zhi



Fig.1: David Nivison in his home, Aug 2014



Fig. 2: Professors Nivison and Chen Zhi

Postface 2

Adam C. Schwartz
Hong Kong Baptist University

David S. Nivison was Edward Shaughnessy's teacher at Stanford, and Edward Shaughnessy was my teacher at the University of Chicago. Since first hearing about him and reading his scholarship in Shaughnessy's classes on Western Zhou bronze inscriptions, I have always taken "my teacher's teacher" pragmatically to mean that Nivison was my teacher too, although I never had the chance to meet him in person.

As such, I was both honored and felt a duty to edit this assortment of essays when Prof Chen Zhi asked me to do it. Now that it is complete, and rightfully placed as volume 1 of the Library of Sinology from The Jao Tsung-I Academy of Sinology, Hong Kong Baptist University, I am happy that I did it, for I have learned a great deal more about Nivison than I knew previously. What has struck me the most is Nivison's love of numbers, his rigorous methodology and innovative approach, his logic, his garrulousness and attempt at persuasion, and his overall sensitivity in reading primary sources.

The essays have been arranged chronologically, and when applicable a note in the lower margin records the date it was written (/revised) and where and when it was presented. I have left Nivison's handwritten transcriptions of early inscriptions and individual character forms, and have typed out and reformatted handwritten metatext when it occurred in the margins.

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