

REVIEW ARTICLES

Of Riddles and Recoveries: The *Bamboo Annals*, Ancient Chronology, and the Work of David Nivison

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The Riddle of the Bamboo Annals 竹書紀年解謎. By David S. Nivison. Taipei: Airiti Press, 2009. Pp. 293. \$75 cloth, \$37 paper.

David Nivison's *The Riddle of the Bamboo Annals* would be very difficult for anyone to review. It includes some sixteen different essays written over the course of more than ten years (1997–2008), but these in turn reflect research conducted over a thirty-year span (beginning in 1979). Nivison divides these entries into three parts: “Recovery of Information from the *Bamboo Annals*,” “The Challenge of the Three Dynasties Project (1996–2000),” and “Recovery of the Strip Text of the *Bamboo Annals*.” In fact, the material revolves around three distinct topics: Nivison's well-known thesis that kings in ancient China regularly observed a three-year mourning period before formally taking the title of “king,” and that this had important implications for their regnal calendars; his spirited criticism of the “Xia-Shang-Zhou Chronology Project” 夏商周斷代工程, which was very much in the news during and immediately after its five-year duration: 1996–2000; and his reconstruction of much of the text of the *Bamboo Annals*, the annalistic history of China from the time of Huang Di 黃帝 through 299 B.C. (the twentieth year of King Xiang'ai of Wei 魏襄哀王) that was written on bamboo strips and discovered in a tomb in what is now Jixian 汲縣, Henan, in A.D. 279. All three of these topics are predicated on one basic premise: that the *Bamboo Annals* as we have had it since the late Ming dynasty is a faithful copy in almost all of its particulars (at least until the appointment of Duke Wu of Jin 晉武公 in 679 B.C.) of the text that was put into the ground in 299 B.C. or shortly thereafter. At one point (p. 79, n. 12), Nivison comments favourably on the practice of Li Xueqin 李學勤, the great contemporary scholar of all

aspects of early Chinese cultural history, of publishing (and re-publishing) everything that he has written on any given topic, whether subsequent research has caused him to revise it or not, as a way of showing the reader the evolution of his thoughts. To his credit, Nivison provides the reader with more help than does Li, pointing out in notes and appendices where his ideas have changed over the years. Nevertheless, the method results in a repetitious and occasionally contradictory presentation. I suspect that only the most patient reader will be able to work through not only to Nivison's final conclusions, but also to the rationale behind them.

This book is probably even more difficult for me to review than for others—not because I have not been a patient reader, but because I am too thoroughly implicated in every aspect of it. I was present at the seminar at Stanford University in November 1979, when Nivison presented his first ideas about the *Bamboo Annals*, fully-dated bronze inscriptions, and Western Zhou chronology; I was one of two students the next year in another seminar that resulted in three major publications that in many ways constitute the foundation of this book (Nivison's own "The Dates of Western Chou," *Harvard Journal of Asiatic Studies* 43, no. 2 [December 1983], pp. 481–580; David Pankenier's "Astronomical Dates in Shang and Western Zhou," *Early China* 7 [1981–82], pp. 2–37; and my own "'New' Evidence on the Zhou Conquest," *Early China* 6 [1980–81], pp. 57–79); I was editor of an *Early China* forum discussion that featured an article co-authored by Nivison ("Astronomical Evidence for the *Bamboo Annals*' Chronicle of Early Xia," *Early China* 15 [1990], pp. 87–95 and 96–172); I myself co-authored an article with him ("The Jin Hou Su Bells Inscription and Its Implications for the Chronology of Early China," *Early China* 25 [2000], pp. 29–48); I invited Nivison to give the Creel Lecture that constitutes Chapter Five of Part Two of the present book ("The Three Years Mourning Institution and the Chronology of Ancient China"); and my own work on related questions is mentioned by Nivison at least thirty-five times in this book (according to the index in the book), sometimes agreeing with him (as in my acceptance of his suggestion that Western Zhou kings regularly employed two separate regnal calendars, which throughout the book he generously terms the "Nivison-Shaughnessy 2-Yuan Hypothesis"), but often disagreeing. If it is not already clear, full disclosure requires me to state that I was and am David Nivison's student. I have learned enough about Chinese culture, both from him and elsewhere, to know that I am obliged always to treat my teacher with very great respect and deference. On the other hand, I also know David well enough to know that he would not regard a rapidly honorific review as respectful. He has stated himself that he "needs" "critical scrutiny" (p. 171, n. 1), and I propose herewith to give such scrutiny to at least the three main topics of his work (I will leave unmentioned other topics, especially those having to do with the Xia portion of the *Bamboo Annals* and/or astronomy, two topics about which I readily admit my own lack of expertise). It goes without saying that my own views are highly biased, the result of almost as long a period of research as Nivison's own. If it weren't for my great respect for David Nivison's work throughout a long scholarly career (by the time this review is published, David will be approaching ninety,

an age beyond even the traditional Confucian characterizations of longevity), I would not have agreed to write this review.

The Nivison-Shaughnessy 2-*Yuan* Hypothesis

Let me begin by accepting the honour that Nivison has accorded me in including my name in one of his central theses—that the traditional Chinese three-year mourning institution had very early roots and had important ramifications for the chronology of ancient China through the end of the Western Zhou period. In my own work, I have always referred to this thesis as the “Nivison Double-*Yuan* Thesis,” and there can be no doubt as to who first developed it. Nivison recounts at the beginning of the book how one Sunday evening in November 1979, he “realized that I was staring in disbelief at my major work for the rest of my life” (p. 8). What he was staring at was evidence from the just discovered Wei 衛-family cache of inscribed bronze vessels that four fully-dated Western Zhou bronze inscriptions corresponded well with the chronology of the reign of King Yi of Zhou 周夷王 preserved in the *Bamboo Annals*, but that three of these required a calendar based on one first-year of reign (867 B.C.), while the fourth required a regnal calendar beginning two years later. He soon found other evidence of this apparent anomaly—in bronze inscriptions from the reigns of Kings Gong 恭王 and Xuan 宣王, in literary records regarding the reigns of Kings Wen 文王 and Cheng 成王, and from a combination of these two sources for the reign of King Kang 康王; i.e., for six of the twelve reigns of the Western Zhou period. Nivison presented this discovery at scholarly conferences in 1980 and 1981 (I was his interpreter at the 1981 conference, the Fourth Meeting of the Chinese Paleography Association, in Taiyuan 太原, Shanxi 山西), and formally published it in his 1983 article “The Dates of Western Chou.” Over the years, I have been probably the only person persuaded by this idea, and have incorporated it into my own reconstruction of Western Zhou chronology (most fully presented in my book *Sources of Western Zhou History: Inscribed Bronze Vessels* [Berkeley and Los Angeles: University of California Press, 1991], pp. 148–55 and 242–45) and have argued for it in a series of Chinese-language articles.¹ My one contribution to the hypothesis is the observation that the two regnal calendars of these various kings were never in use simultaneously. Rather, there

¹ Xia Hanyi 夏含夷, “*Ci ding mingwen yu Xi Zhou wanqi niandai kao*” 此鼎銘文與西周晚期年代考, *Dalu zazhi* 大陸雜誌 80, no. 6 (June 1990), pp. 16–24 (reprinted in *Xi Zhou zhu wang niandai yanjiu* 西周諸王年代研究, ed. Zhu Fenghan 朱鳳瀚 and Zhang Rongming 張榮明 [Guiyang 貴陽: Guizhou renmin chubanshe 貴州人民出版社, 1998], pp. 248–57); “Shangbo xin huo *Da Zhu Zhui ding dui Xi Zhou duandai yanjiu de yiyi*” 上博新獲大祝追鼎對西周斷代研究的意義, *Wenwu* 文物 2003, no. 5, pp. 53–55; “*Sishier nian Sishisan nian liangjian Wu Lai ding de niandai*” 四十二年、四十三年兩件吳迷鼎的年代, *Zhongguo lishi wenwu* 中國歷史文物 2003, no. 5, pp. 49–52.

seems to have been in effect first a “succession calendar” based on the first year that a king actually “took position” (*ji wei* 即位). This calendar remained in effect throughout the first part of the king’s reign, but was replaced at a certain point in the reign (apparently a different point—at least in terms of the year count—in different reigns) by a regnal calendar beginning two years later, perhaps when the king had formally “stood” (*li* 立) as “king.” Thereafter, this “accession calendar” remained in effect until the king’s own demise. Nivison suggests that it was probably the death of the father’s chief minister or ministers that triggered this change of calendar (p. 212). I do not find this suggestion particularly convincing, but neither do I have a better one. Indeed, I don’t have any explanation for what might have precipitated such a change in calendar. And yet, the evidence is overwhelming in reign after reign of the Western Zhou that such a calendrical change did take place, and any explanation of Western Zhou chronology will have to account for it, even if the rationale behind it remains mysterious.

If this hypothesis, regardless of the name or names attached to it, could be regarded as crazy—or worse, simply disregarded—for more than two decades after its first presentation, it became much more difficult to do so in January 2003, with the discovery of another cache of inscribed bronze vessels: those of the Shan 單 family, discovered in Meixian 眉縣, Shaanxi 陝西.² This cache included bronze *ding* 鼎-caldrons with two lengthy and fully-dated inscriptions certainly dated to the forty-second and forty-third years of the reign of King Xuan, a reign theretofore almost universally believed to have begun in 827 B.C. and ended in 782 B.C. No matter how they might be construed, the dates in these two inscriptions do not fit the calendars of 786 and 785 B.C., which should be the forty-second and forty-third years of such a regnal calendar. Instead, they fit perfectly with a calendar two years later, just as the “Nivison-Shaughnessy 2-Yuan Hypothesis” had predicted.

Perhaps even more important, one of the officials mentioned in the forty-second year inscription, Shi Huo 史馮, is also mentioned in another fully-dated inscription that has long been known: the *Yuan pan* 袁盤, dated to the twenty-eighth year of an unspecified reign. Most evidence suggests that this vessel should also date to the reign of King Xuan, but the full date in the inscription is also incompatible with the traditionally accepted dates for his reign. Prior to the discovery of the Shan-family bronzes at Meixian, this calendrical incompatibility caused most scholars to seek another reign into which to place this vessel, as did the Xia-Shang-Zhou Chronology Project, assigning it to the reign of King Xuan’s father, King Li 厲王, even though it did not really fit the traditional dates of

² For the initial report of this discovery, see Shaanxi sheng Kaogu yanjiusuo, Baoji shi Kaogu gongzuodui, Meixian Wenhua guan Yangjia cun lianhe kaogudui 陝西省考古研究所、寶雞市考古工作隊、眉縣文化館楊家村聯合考古隊, “Shaanxi Meixian Yangjia cun Xi Zhou qingtongqi jiaocang fajue jianbao” 陝西眉縣楊家村西周青銅器窖藏發掘簡報, *Wenwu* 文物 2003, no. 6, pp. 4–42.

that reign either.³ Now with the *Sishier nian Yu Qiu ding* 四十二年虞逖鼎 showing that Shi Huo was active at court in the forty-second year of King Xuan's reign (784 B.C.), there is absolutely no doubt that the *Yuan pan* must also date to the reign of King Xuan—only not to the “twenty-eighth year” of his “succession calendar” (800 B.C.), but rather to the twenty-eighth year of his “accession calendar” (798 B.C.; which its full-date notation fits perfectly), just as both David Nivison and I have long argued.⁴

The reign of King Xuan is particularly important for testing the validity of this 2-*Yuan* hypothesis, both because it is a reign the dates of which have long been accepted as fixed, but also because there is a wealth of fully-dated bronze inscriptions that surely date to the reign. As the following listing of these bronze inscriptions shows, through the year 810 B.C., every date corresponds to a “succession calendar” beginning in 827 B.C., as the traditional chronology of this reign would require. However, after 810 B.C., every other bronze fits only the “accession calendar” that begins in 825 B.C.

Fully-Dated King Xuan Period Bronzes

Vessel Name	Date Notation*	1st Yr. of reign	Year	Day of month
頌鼎	3/5/D/11	827	825	27
兮甲盤	5/3/D/27	827	823	24
虢季子白盤	12/1/A/24	827	816	-1
克鐘	16/9/A/27	827	812	1
吳虎鼎	18/13/B/23	827	810	10
趯鼎	19/4/C/28	827	809	18
此鼎	17/12/B/52	825	809	14
番菊生壺	26/10/A/16	825	800	-1
袁盤	28/5/C/27	825	798	20
斚攸從鼎	31/3/A/29	825	795	9
大祝追鼎	32/8/A/18	825	794	6
伯窺父盥	33/8/D/28	825	793	22
善父山鼎	37/1/A/47	825	789	-1
四十二年虞逖鼎	42/5/B/52	825	784	7
四十三年虞逖鼎	43/6/B/24	825	783	15

* I give the date notations here in summary fashion: year/month/lunar phase (A: *chuji* 初吉; B: *jishengpo* 既生霸; C: *jiwang* 既望; D: *jisipo* 既死霸)/*gan zhi* 干支 (sequence in cycle of 60). Thus, for the *Song ding* 頌鼎, “3/5/D/11” stands for *wei san nian wu yue jisipo jiaxu* 隹三年五月既死霸甲戌.

³ Xia-Shang-Zhou duandai gongcheng zhuanjiazuzhuan 夏商周斷代工程專家組, *Xia-Shang-Zhou duandai gongcheng: 1996–2000 nian jieduan chengguo baogao (jianben)* 夏商周斷代工程：1996–2000年階段成果報告（簡本）（Beijing: Shijie tushu chubanshe 世界圖書出版公司, 2000), p. 33.

⁴ Nivison, *The Riddle of the Bamboo Annals*, p. 221; Shaughnessy, *Sources of Western Zhou History*, p. 285 (the vessel is called *Huan pan* in both of these sources).

Li Xueqin, the director of the Xia-Shang-Zhou Chronology Project, once publicly stated that the Project could disregard the problems raised by these King Xuan-period bronzes, because the Project's mandate was to reconstruct the chronology of ancient China prior to 842 B.C., the earliest commonly accepted date.⁵ I would suggest that any reconstruction that disregards this problem has no hope of resolving any other problems in ancient China's chronology, as David Nivison has so perceptively demonstrated.

Unfortunately, in the case of the book presently under review, Nivison has taken a good idea and, in my opinion, tried to make it bear far more weight than it can. He believes that the origin of this calendrical practice extends back a thousand years before the Western Zhou to the very beginning of the Xia dynasty. Of course, there is no contemporaneous written evidence of this dynasty, and so the only evidence that Nivison has to support this assertion is the much maligned *Bamboo Annals*. Given the evidence that he produces to show that portions of the *Bamboo Annals* are historically accurate, this might be persuasive—if the evidence were in fact in the *Bamboo Annals*. Only some of the reigns mention an interregnum at the beginning of the reign, sometimes of one, sometimes of two, and sometimes of three years—as Nivison notes, “about a third of them 2 years” (p. 41). From this, he asserts that “it is reasonable to suppose that all of them ought to be just two years.” Why is this “reasonable”? Indeed, it seems more reasonable to me that an earlier irregular practice might have become regularized, but only over time. In any event, the evidence, such as it is (the text of the *Bamboo Annals* as we have it), does not, it seems to me, support Nivison's hypothesis. Nivison argues that there is other evidence to support it—that these regular two-year interregnums are required by the chronology that he has reconstructed for the dynasty. One might find this reasoning circular, even if it did not require Nivison to argue that the final king of the Xia, the infamous Di Gui 帝癸 or Jie 桀, “is a fiction. There was no such king.” The *Bamboo Annals*, again as we have it, includes annals for thirty-one years of this king, but Nivison states that his reconstruction of the text shows these to be a later insertion into the original text (apparently inserted at the court of King Xiang'ai of Wei about 300 B.C.).

Nivison also provides a complete chronology of the Shang dynasty, together with a multi-variable explanation that raises even more questions. The first of his premises regarding these kings is that the names by which they are known to history, ending in one of the ten *tiangan* 天干 (heavenly stems), was determined by the *tiangan* of the first day of the year that they succeeded to power; unless that day was the same as the name of their deceased father, in which case they would use the *tiangan* of the first day of the year that they acceded to power two years later; or unless the day was a *gui* 癸 day, in which case they would take the name of the next day, *jia* 甲; or unless he was one of the last

⁵ According to Professor Li Ling 李零, who was in the audience, Professor Li Xueqin made this remark at a public lecture at Peking University in March 2003, just after returning from viewing the Shan-family bronzes.

eight kings of the dynasty, in which case Nivison argues that reigns overlapped, and the name of the first king was determined “immediately” (p. 43). These temple names of the Shang kings have been much discussed ever since Kwang-chih Chang 張光直 pointed out that their regular alternation between *jia* and *yi* 乙 days, on the one hand, and *ding* 丁 days on the other hand, could not readily have been the result of some sort of random or natural selection process, such as the king’s day of birth or day of death,⁶ but nowhere does Nivison mention this scholarship. Neither does he mention the well-known evidence that the Shang determined the temple name by divination after a person’s death.⁷ Instead, he argues that “everything fits, and the fit is the proof” (p. 5). One not inclined to expect regularity of history—or of historical sources—might be tempted to regard all of this as circular: everything fits, because the rules of the system have been created such that everything must fit, unless something doesn’t fit, in which case the rules can be adjusted. Nivison notes in his Preface that this is “the kind of reasoning” he uses “all the time.” He urges the reader, if she agrees that “the fit is the proof,” “to keep on reading my book” (p. 5). Fortunately, even for those of us who disagree that the fit is the proof, there is still enough in the book for us to keep on reading.

The Xia-Shang-Zhou Chronology Project

The second of Nivison’s three major sections is entitled “The Challenge of the Three Dynasties Project (1996–2000).” Nivison begins the section by recounting how in 1994, Song Jian 宋健, a physicist and state councillor for science, found himself embarrassed by the antiquity and specificity of chronologies he saw visiting museums in the Middle East, and upon returning to China proposed a multi-disciplinary project to produce a similar chronology for ancient China. The resulting “Xia-Shang-Zhou Chronology Project” (hereafter simply Chronology Project), which engaged some two hundred scholars for the next five years, has often been called China’s greatest state-sponsored project in the humanities since the *Siku quanshu* 四庫全書 project of the 1770s. As a state-sponsored project, it was certain to be the target of criticism, both at home and abroad. Nivison, who was perhaps more invested in reconstructing the chronology of the Xia, Shang and Zhou dynasties than anyone else, certainly in the Western world, soon became a prominent and vocal critic of the Chronology Project. He begins the section with an Introduction intended to provide context for the debate and a report on a roundtable discussion held at the annual meeting of the Association of Asian Studies in March 1998. He then includes

⁶ Kwang-chih Chang (Zhang Guangzhi) 張光直, “Shang wang miaohao xinkao” 商王廟號新考, *Zhongyang yanjiuyuan Minzuxue yanjiusuo jikan* 中央研究院民族學研究所集刊 15 (1963), pp. 65–95.

⁷ Li Xueqin 李學勤, “Ping Chen Mengjia Yinxu buci zongshu” 評陳夢家殷虛卜辭綜述, *Kaogu xuebao* 考古學報 1957, no. 3, pp. 119–29.

four papers of his own: “Criticism of the Sandai Project ‘Brief Report’” (from 2000), “The Three Dynasties Chronology Project: Two Approaches to Dating” and “The Three Years Mourning Institution and the Chronology of Ancient China” (both from 2002), and “Zai Tan Jinben *Zhushu jinian* yu Sandai Niandaixue” (from 2003, despite the title, this paper is actually in English). He appends to the first of these papers the following note: “The passionate tone of this statement is, I suppose, itself a part of history. I think it accurately reflects many persons’ attitudes eight years ago. I hope it is no longer appropriate, but that remains to be seen.”

The passion that attended the production and reception of the *Brief Report* of the Chronology Project in 2000 has in fact abated. Internet searches for the Chronology Project reveal little mention of it at all for the last several years,⁸ in stark contrast to the constant publicity it received from its inception through about 2003. 2003 did, in fact, mark the turning point in the fortunes of the project. As Nivison describes (p. 61), an international conference was announced for October of that year, and many of the scholars most involved in studies of early Chinese chronology, both in and out of China, received invitations to it (Nivison’s “Zai Tan Jinben *Zhushu jinian* yu Sandai Niandaixue” was intended to be his contribution to this conference). Subsequently, we received word that the conference had to be postponed because of the outbreak of the SARS epidemic in China in the spring of that year. The conference was never re-scheduled. I suspect that the real reason that the conference was never re-scheduled is because of the discovery at the beginning of that year of the Shan-family bronzes in Meixian, Shaanxi. As described above, these bronzes, and especially the full-date notations of the *Sishier nian* and *Sishisan nian Yu Qiu ding* inscriptions, left no doubt that the Chronology Project’s dates for the reigns of Kings Li and Xuan were fundamentally flawed. Chronologies are inherently interrelated; the dates for one reign almost necessarily entail the dates for another, and so on. If even the most securely dated reign of the Western Zhou—that of King Xuan—is wrong, what hope could there be for the rest of the chronology?

As it turns out, the hopelessness of the rest of the chronology has been revealed with each passing year. The most recent publication has been of the *Yao Gong gui* 覲公簋, a vessel the shape and décor of which closely resemble the *Kang Hou gui* 康侯簋, dated by all to the very opening years of the Western Zhou.⁹ The inscription of the *Yao Gong gui* mentions the appointment of Tang Bo 易白 (i.e., 唐伯) to be “lord at Jin” (*hou yu Jin* 侯

⁸ An unfortunate exception to this neglect is Chen Junjian 陳君儉, “Xia-Shang-Zhou duandai gongcheng shi nian ji” 夏商周斷代工程十年祭, posted at Peking University’s *Boya luntan* 博雅論壇 8 March 2010 (accessed 9 September 2010). The stridency of the author’s criticism is surpassed only by his ignorance of the basic methodologies involved in chronology studies.

⁹ Zhu Fenghan 朱鳳瀚, “*Yao Gong gui* yu Tang Bo hou yu Jin” 覲公簋與唐伯侯于晉, *Kaogu* 考古 2007, no. 3, pp. 64–69.

于晉). As Zhu Fenghan 朱鳳瀚 notes in his article presenting this vessel and its inscription, this surely refers to the appointment of Bo Xie 伯燮, who historical sources say moved his state to Jin 晉. Bo Xie was the son of Tang Shu Yu 唐叔虞, who was in turn the younger brother of King Cheng and the first lord of Tang 唐. Crucially, the inscription includes a year date: the king's twenty-eighth year (*wei wang nian you ba si* 佳王廿又八祀). As Zhu Fenghan also notes, though with some equivocation due to the obvious implications of this conclusion, this just as surely refers to the reign of King Cheng.¹⁰ This undercuts the Chronology Project's chronology for this important early Western Zhou reign. Even though traditional chronologies were almost all in agreement in assigning thirty-seven years to this reign, the Project produced an unprecedented figure for it: twenty-two years. We now see that there is good reason for the lack of any precedent for such a length of reign: it is simply wrong. As the *Yao Gong gui* shows, the reign of King Cheng lasted at least twenty-eight years. This is evidence that simply can't be dismissed or explained away as *chabuduo* 差不多.

Unlike the case of King Cheng, for which the Chronology Project invented a length of reign with no support anywhere in the traditional historical record—even though the earliest historian of China's ancient chronology, Liu Xin 劉歆 (46 B.C.–A.D. 23), stated there was explicit evidence for that reign and the following reign of King Kang, but not for any of the subsequent reigns.¹¹ On the other hand, for the reign of King Mu 穆王, three reigns later, the Project insisted on the traditional length of fifty-five years, despite some evidence in the bronze inscriptional record that should have showed this length to be open to doubt. In 1975, the same year as the discovery of the Wei-family bronzes that stimulated David Nivison's studies of Western Zhou chronology, another cache produced four different fully-dated inscribed vessels made for one Qiu Wei 裘衛. Three of these are dated to the third, fifth and ninth years of a reign that one of them, the *Wu si Qiu Wei ding* 五祀裘衛鼎, indicates was that of King Gong, the sixth king of the dynasty and the son of King Mu. The fourth of the Qiu Wei bronzes, stylistically earlier than the other three, bears a twenty-seventh year date notation that virtually all scholars understand to refer to King Mu's reign. What this means, is that if King Mu's reign did in fact last fifty-five years, then Qiu Wei commissioned one bronze vessel for himself in one year, and then waited for more than thirty years before commissioning three more in relatively rapid succession. Of course, this is not impossible by any means. However, a more plausible scenario might be that the four bronzes should be closer in time to each other, entailing a shortening of King Mu's extremely lengthy reign.

¹⁰ Ibid., p. 69.

¹¹ *Han shu* 漢書 (Beijing: Zhonghua shuju 中華書局, 1962), *juan* 卷 21B (“Lü li zhi” 律曆志), p. 1017.

Other evidence of this sort came to light in the course of the Chronology Project. In 1997, the Chronology Project published under its auspices the report of a newly discovered bronze vessel: the *Hu gui gai* 虎簋蓋, commissioned by one Hu 虎.¹² It too bears a full date, to a “thirtieth year” that the initial report on the bronze and all subsequent studies of reliably interpret as referring to the reign of King Mu.¹³ The importance of this vessel is magnified because another fully-dated vessel, the *Shi Hu gui* 師虎簋, commissioned by the same Hu 虎 as the *Hu gui gai* has long been known, and has been dated by most scholars (including the Chronology Project, David Nivison, and myself) to the first year of the reign of King Yih 懿王, the son of King Gong (and thus the grandson of King Mu). In other words, Hu’s life at court began no later than the thirtieth year of King Mu’s reign and continued until at least the first year of King Yih’s reign, two reigns later. Since it is known that the intervening reign of King Gong lasted at least fifteen years (as shown by the *Shiwu nian Jue Cao ding* 十五年趙曹鼎, firmly dated to that reign), if King Mu’s reign lasted fifty-five years, then Hu’s career at court had to have lasted at least forty years (24 years of King Mu’s reign + 15 years of King Gong’s reign + 1 year of King Yih’s reign). This too is not impossible, but especially in antiquity—with relatively shorter life expectancies—it is rather implausible.

Three years after the discovery of the Shan-family bronzes had called into doubt the Chronology Project’s conclusions, yet another King Mu-period bronze would surface to show decisively that King Mu’s reign could not have lasted fifty-five years. In 2006, the National Museum of China reported its purchase of a *Lu gui* 親簋,¹⁴ with an inscription dated to the twenty-fourth year of a reign that is certainly that of King Mu and commemorating the promotion of Lu to be the Chief Supervisor of the Horse (*zhong si ma* 冢嗣馬), the supreme commander of the Zhou armies. Other contemporary bronzes show that this Lu 親 is none other than Sima Jingbo Lu 嗣馬井白親, the most commonly appearing figure in all mid-Western Zhou bronze inscriptions, appearing in inscriptions from the reign of King Mu, from throughout the reign of King Gong, and also in the *Shi Hu gui* inscription from the first year of the reign of King Yi. If King Mu’s reign lasted

¹² Wang Hanzhang 王翰章, Chen Lianghe 陳良和, and Li Baolin 李保林, “*Hu gui gai ming jian-shi*” 虎簋蓋銘簡釋, *Kaogu yu wenwu* 考古與文物 1997, no. 3, pp. 78–80.

¹³ *Ibid.*; see also “*Hu gui gai ming zuotan jiyao*” 虎簋蓋銘座談紀要, *Kaogu yu wenwu* 1997, no. 3, pp. 81–83.

¹⁴ The vessel was first reported in issue no. 3 of 2006 of the museum’s journal *Zhongguo lishi wenwu* 中國歷史文物, with the following articles: Wang Guanying 王冠英, “*Lu gui kaoshi*” 親簋考釋, pp. 4–6; Li Xueqin 李學勤, “*Lun Lu gui de niandai*” 論親簋的年代, pp. 7–8; Xia Hanyi 夏含夷, “*Cong Lu gui kan Zhou Mu Wang zai wei nianshu ji niandai wenti*” 從親簋看周穆王在位年數及年代問題, pp. 9–10; Zhang Yongshan 張永山, “*Lu gui zuoqizhe de niandai*” 親簋作器者的年代, pp. 11–13.

fifty-five years, then Lu would have had to be active at court at least forty-eight years even after his promotion to Chief Supervisor of the Horse, a position that presumably would not have been awarded to a young man. This is simply too implausible to entertain seriously, especially when considered in conjunction with the similar evidence regarding the careers of Qiu Wei and Hu. Instead, it seems clear that King Mu's reign was considerably shorter, probably thirty-nine years, as David Nivison (and I) have long held.

This is not the place to determine whether King Cheng's reign lasted twenty-two years or thirty-seven years, or King Mu's reign lasted fifty-five years or thirty-nine years. It's not that it doesn't matter. It does, and I would hope that there is still time to get at least the chronology of the Western Zhou right. For the purposes of this review, however, perhaps the most important point to take from this concerns methodology. David Nivison makes a very telling statement in the contribution he had hoped to make to the aborted 2003 conference: "To ignore conflicting material simply because you prefer material that supports your conclusion is to argue in a circle" (p. 104). The authors of the Chronology Project's *Brief Report* have been guilty of this at almost every step of their work. Sometimes, as in the case of King Mu's reign, they have accepted what passes for traditional evidence; sometimes, as in the case of King Li's reign, they have accepted traditional evidence, but with an important new interpretation; and sometimes, as in the case of King Cheng's reign, they have just made up dates to "fit" their system. There has been no attempt to explain why they have chosen one approach instead of another, why they have accepted one set of data over another.

For a time, they might have been excused because we expected the full scholarly documentation to be provided in a promised "Full Report" (*Fanben* 繁本). Now, ten years on, it seems as though that "Full Report" will never be forthcoming. It may well be that the directorship of the Chronology Project just wishes the results could quietly slip into oblivion, as has been the fate of chronologies of ancient China proposed by scores of individual scholars over the course of the last generation or so. But the Chronology Project is different from all of those individual attempts. Nivison pointed this out in September 2000, after first seeing a pre-publication version of the *Brief Report*: "Anyone familiar with arguments in this area in recent decades knows that controversy is going to continue. The Project has been brought into existence by the government of the PRC. If the evident intention goes forward, Project-endorsed dates are going to be seen as the dates pronounced to be correct by the PRC. Inevitably this will be the attitude of the PRC itself. What happens, then, when and if the consensus of world scholarship comes to be that one or many of these officially endorsed dates are wrong? It is difficult enough for an individual scholar to admit a mistake, and many never do, no matter how obvious the error. For the government of China to be in this position, on a matter that is obviously one involving much national prestige, would poison scholarship for generations. It can be predicted that government sponsors would insistently defend Project 'results,' and leading PRC scholars in the Project would feel under extreme pressure to do the same, no matter what" (p. 72). The results of the Chronology Project were announced with great fanfare

just two months after Nivison made this prediction, and in the ten years since they have had an influence far beyond those any individual scholar could expect—just as he predicted. The dates of the Chronology Project, especially those of the Western Zhou kings, have been published in the new edition of the *Ci hai* 辭海, they have been incorporated into history books for public schools, and they are posted in museum displays throughout China (and even engraved in stone outside the new Centennial Altar Museum [Shiji tan bowuguan 世紀壇博物館] of Beijing). And, as Nivison also pointed out, they contain dates that are wrong; indeed, I would go so far as to say that for the Western Zhou portion, almost all of the dates are wrong. I do think that Nivison was wrong in predicting that this “would poison scholarship for generations.” I detect no such poison. But it is fairly clear that even though most of the senior leadership of the Chronology Project recognize that the results are seriously flawed, no one is in any position to set matters right. It might be claimed that the project is now defunct and there is simply no one to speak for it. However, if Li Xueqin continues to enjoy the title of “Director” of the Project, then perhaps it should be his responsibility to admit the flaws in the chronology, and invite individual scholars to try their hands in the matter once again.

Recovery of the Strip Text of the *Bamboo Annals*

The third section of Nivison’s book, “Recovery of the Strip Text of the *Bamboo Annals*,” is in some respects derivative from the concerns with chronology animating the first two sections, but he presents it here as if it has priority over them; for him, “the recovery of the strip text” is the proof that “everything fits.” In a note added toward the end of his editorial work on this book (30 July 2007), he admits to a motivation pushing him to handle the text in a certain way: “I feared that a Warring States rewriting of the Cheng chronicle moving the death and burial [of Zhou Gong 周公] from years 11–12 to years 21–22 would have torn the text up so much that my use of the *Annals* to reconstruct Zhou and pre-Zhou dates would have been impossible or at least very doubtful; and I was already quite sure of this reconstruction” (p. 121). I understand the fear; it is the sureness I find disconcerting.

Nivison provides a reconstruction of 303 strips of text from Huang Di (whose name occupies a single strip) through the twenty-seventh year of Jin Hou Min 晉侯緡, the year in which the Zhou king appointed Duke Wu 武公 of Quwo 曲沃 to be “lord” (*hou* 侯) of Jin. This includes both the text of the *Bamboo Annals* itself, as found in the Ming dynasty Fan Qin 范欽 (1506–1585) edition, and also other material that is included in that edition but differentiated from the main text by a different typeface (but of the same size as the main text), with the exception of a handful of notes explicitly marked as written by “Yue” 約 (i.e., Shen Yue 沈約 [441–513]). This second sort of text, apparently commentarial in nature, is in turn of two sorts. First are long narrative passages of a more or less legendary nature. Strips 072–073, following the annal for the eighth year of Yu 禹, represent one such passage. It reads, in Nivison’s translation:

On a tour of inspection to the south, while crossing the Yangzi, in mid-stream there were two yellow dragons that held up the boat on their backs. The boatmen were all terrified. Yu laughed and said: "I have received my appointment from Heaven, and I bend my strength to care for humanity. One lives and dies according to destiny. Why should I worry about dragons?" The dragons at this went away, dragging their tails behind them. Yu reigned for 45 years. Yu presented [his chief minister] Yi to Heaven [for approval]. Seven years later Yu died. When the three-years mourning was completed, the empire gave its allegiance to [Yu's son] Qi. (p. 134)

The second sort of commentary consists of explanatory material, especially the dynasty summations (such as strip 121: "From Yu to Jie there were seventeen reigns. Including when there was a king and when there was not, the total time was 471 years"). In the Fan Qin edition, there are also "small-character double-column notes" (小字雙行夾注) inserted directly into the main text; for the most part, Nivison does not include these, presumably regarding them as insertions by the Jin editors. Nivison separates the main text from the commentarial material by writing the main text in 40-character strips (or, occasionally, 20-character half-strips), and the commentarial material in 34-character strips (or, occasionally, 17-character half-strips), in all cases the strips including *exactly* these numbers of characters (allowing for one-character spaces between years of the main text). To make these complete strips count out exactly, he takes certain liberties with the commentarial material. For instance, what he presents as strips 3–14 is actually written in the Fan Qin edition as three separate notes, the first coming after the name of Huang Di, the second after the main text entry for his twentieth year, and the third after his fiftieth year, though, as Nivison notes, in the "Fu rui zhi" 符瑞志 chapter of the *Song shu* 宋書, this same material is presented as one continuous passage. Within this material, he repeats the name Xianyuan 軒轅, saying "the repetition seems to be needed for sense, and it gives me my word-count" (p. 167). However, he deletes the last seven characters of this extended passage, *jin Han men gu kou shi ye* 今寒門谷口是也 (this is the mouth of the present Hanmen Valley), as "obviously a Jin [i.e., third century A.D.] note," as they obviously are. There is another note attached after the entry for Huang Di's death in his hundredth year:

帝王之崩皆曰陟書稱新陟王謂新崩也帝以土德王應地裂而崩葬群臣有左徹者感思帝德取衣冠几杖而廟饗之諸侯

The deaths of emperors and kings are all called "ascensions." The book refers to newly ascended kings as "newly deceased." The (Yellow) Emperor used the virtue of earth to be king, (and so) responded to the rending of the ground by dying. *When he was buried, one of his ministers named Zuo Che, was moved and thought*

*of the emperor's virtue, and took his cloak, cap, table and staff and in [Huang Di's] temple offered them to the many lords.*¹⁵

Nivison deletes the first twenty six characters of this note, again “as obviously a Jin insertion.” He inserts the word *ji* 既 (after) before *zang* 葬 (to bury). In the Fan Qin edition, this commentary is followed by the following small-character double-column note: 大夫歲時朝焉 “*in each season . . . the grand officers came to court there.*” Nivison includes this in his text.

On the other hand, similar commentarial material attached in the Fan Qin edition after the eighth year of King Cheng of Zhou and concerning in part the appearance in the Luo 洛 River of a turtle with writing on its back is treated differently. First, the entire passage is relocated until after King Cheng's fourteenth year, which is necessary to have the strips count out as Nivison wishes. Within this extended passage is the following sentence: 自周公訖于秦漢盛衰之符 (From Zhou Gong until the Qin and Han, symbols of rise and fall). With its mention of the “Qin and Han (dynasties),” this too would seem to be “obviously a Jin note.” Nevertheless, Nivison includes it, changing the words 訖于秦漢 to 以訖于今 (down to the present). He does this, as he explains at considerable length (an explanation that I cannot possibly replicate here) because he needs this passage to count out the way he has it in order to justify his chronological reconstruction.

These are not insignificant technical details. In a system where “the fit is the proof,” is the need to add characters here, drop characters there, and change other characters elsewhere, not proof that there is something seriously wrong with this approach?

Yet, these technical details are by no means the most serious problem with Nivison's approach. His entire scenario for how this text came to take the shape it has strikes me as entirely implausible. At one point (p. 36), he proposes that there were originally two annals joined together to form the *Bamboo Annals* we have today: an earlier chronicle from Huang Di down to 785 B.C., the first year of Shangshu 殤叔 of Jin, and a “Jin-Wei 晉魏” annals that continued from then until the time of King Xiang'ai of Wei. At other places (e.g., pp. 182, 192), he suggests that the original text, presumably—but never, as far as I can tell, made explicit—only of the earlier of these two annals, was an annals compiled in the state of Lu 魯. This text, or a copy of it, somehow made its way to the Wei 魏 capital of Daliang 大梁. Nivison explains:

I think we can assume that the Wei specialists began with a copy of the Lu book, and made changes in it for their own purposes. One would expect them to change only what interested them, leaving virtually all of the Lu-Zhou oriented material as

¹⁵ *Zhushu jinian* 竹書紀年 (*Sibu congkan* 四部叢刊 ed.), *juan* 1, p. 3a. The italicized portion of the translation reflects Nivison's own translation.

it was, if only to avoid the tedious labor of preparing new strips, as far as possible.
(p. 182)

I do not doubt that there were any number of annals kept in the various states of ancient China and that they enjoyed a certain degree of circulation, or that copyists “made changes in [them] for their own purposes.” But everything that we have learned about Warring States scribal practices over the last twenty-five years suggests that the labour of preparing new strips was by no means tedious; it was routine. The manipulations Nivison describes at pp. 189–92 to account for a misplaced strip that was not really a misplaced strip remind one of the days of pre-computer typists replacing one whited-out word or phrase with another of an equal number of letters to avoid having to re-type an entire page; just as no self-respecting secretary would have done this, so too does it seem foreign to what we know of Warring States scribes.

It is not just the copying practices of Warring States China, about which, after all, we still know entirely too little, with which I find Nivison’s account of how the *Bamboo Annals* may have come into existence to be wrong. We now know at least a little more about how unearthed manuscripts are edited, and the *Bamboo Annals* was, after all, an unearthed manuscript. At the beginning of his book (p. 7), Nivison says of the texts in the tomb at Jixian:

These books had been lying hidden safely underground for almost six centuries. One of them, still relatively intact, was the book that became, after non-significant editing, what I am now calling the BA.

Two paragraphs later, he adds:

I also think that anyone looking at the BA would have to agree that even at best we must assume that the last part of the discovered original was in very bad shape when it reached the Jin court, the bamboo strips at that point unbound and scattered, many damaged, and some lost entirely. I will be dealing with this last part almost not at all.

I certainly agree “that the last part of the discovered original was in very bad shape when it reached the Jin court.” Given this, why then should we suppose that the other parts of the same manuscript were preserved in almost pristine condition, such that they could be reconstructed—then or now—“word after word,” as Nivison claims to “know exactly” (p. 11)? When twenty-five years ago, I proposed that one passage of forty characters currently found in the annals of King Wu of Zhou 周武王 should originally have belonged to the annals of King Cheng, its current placement being the result of the editors at the Western Jin court misplacing a single bamboo strip, it was based on the premise that those editors were trying to make the best sense they could out of a confused bundle

of manuscripts. I would suggest that all we have learned since then about the editing of unearthed bamboo-strip manuscripts supports this view of what may have happened.

Nivison spends a great deal of time in this book discussing this particular passage, proposing ultimately that while the forty-character passage itself was moved from the annals of King Cheng to those of King Wu, this did not entail the physical moving of a strip. Instead, according to Nivison, both the Lu and Wei annalists determined for chronological reasons that King Wu's reign needed to last three years longer than they had evidence for. Nivison would have us believe that rather than just adding three such years to King Wu's reign, the Wei annalists went to great trouble to change a couple of strips so as to move three years worth of annals from King Cheng's reign to his.

Passions evidently run high regarding this particular strip, such that I cannot envision a response that would satisfy him. Let us examine a different passage, also from the apparently pristine early portion of the text. For the third year of Shang king Di Yi 帝乙, the penultimate king of the Shang dynasty, we read (together with Nivison's translation):

三年王命南仲西拒昆夷城朔方夏六月周地震。

3rd year: The king ordered Nan Zhong to oppose the Kunyi on the west, and to wall off the North (Shuofang). In the 6th month there was an earthquake in Zhou.

Nivison allows that I have “demonstrated convincingly that [these words, or at least those referring to Nan Zhong] refer to an event that happened in Xuan Wang 13” (i.e., the thirteen year of King Xuan of Zhou), and adds “he [i.e., Shaughnessy] thinks that this indicates that the Jin restorers have misplaced another strip (or half-strip). My reconstruction will not allow this, so I must find a better explanation.” His explanation is as follows (p. 171):

I have argued (Nivison 1999 and 2002, Appendix 2) that Di Yi's actual first year was 1105 (beginning with an *yi* day), for a reign of 19 years, not 9 years as in the BA. And I have shown (*ibid* 2.3 note 3) that the earthquake in Zhou recorded next actually occurred in 1093, which must be Di Yi 13. Therefore at a time, necessarily before the text was buried, when Di Yi still was accorded 19 years, both of these events, the first by error and the second correctly, were entered under year Di Yi 13. At this point, Wen Ding's reign was made to be only three years (as also in the *Wenxian tongkao*). Later, the Warring States chroniclers learned that Wen Ding had 13 years. Not knowing that the first ten were Wu Yi's last ten years (because as I claim in Nivison 1999 and 2002 Appendix 2 Wu Yi gave his heir his own calendar in 1118), the Warring States experts resolved the problem by cutting out the first ten years of Di Yi, so that “12th year” became “2nd year”, and “13th year” became “3rd year” (and the year of his death became “9th year” instead of “19th year”). (Some explanation would be needed even if Shaughnessy were right

about a strip being misplaced; one would still have to account for an event dated “13th year” getting recorded under “3rd year.” But once one has the explanation, one no longer needs to assume a strip was misplaced.)¹⁶

I think I understand Nivison’s manipulations of chronology here, but I don’t see how it accounts for the passage quoted above. Instead, I have suggested that the Jin editors of the unearthed manuscript identified the “Nan Zhong” 南仲 of this passage with the Nan Zhong of the *Shi jing* 詩經 poem “Chu che” 出車 (Mao 168):

王命南仲，往城于方。

The king did command to Nan Zhong, To go and make a wall at Fang.

They would have known that the Mao Preface to the *Shi jing* explicitly dates this poem to the time of King Wen of Zhou. Since the reign of King Wen overlapped, at least to some extent, with the reign of Shang king Di Yi, it would have been a simple deduction for them to place this record in Di Yi’s annals. However, as Nivison admits, Western Zhou bronze inscriptions leave little doubt that Nan Zhong lived during the time of King Xuan of Zhou, some three hundred years later, and that the Mao Preface—which ascribed

¹⁶ Nivison adds the following note to the end of this paragraph:

Central to my argument here is my claim that Di Yi’s reign was not 9 years, as in the BA, but was 19 years. I have argued this in detail in Nivison 1999 and 2002, section 7 and Appendix 2. But those monographs have not attracted critical scrutiny, which I need. I will therefore outline the argument herewith, in the hope of provoking attention to what is, after all, a daring claim. The claim is that Wu Yi’s 35 years can be got by subtracting 16 years from the BA dates 1159–1125, getting 1143–1109 (then prefixing two years, 1145–1144, for mourning completion)—no surprise there. But I claim that Wenwu Ding’s 13 years actually begin in 1118 rather than the expected 1108, and end in 1106 rather than in 1096. If analysis shows this to be true, one must assume that Wu Yi not only identified his heir in 1145 (first day of the post-solstice month being *dingchou*) but made doubly sure of the succession by giving Wenwu Ding his own calendar in 1118. If this is right, it explains why the *Wenxian tongkao* and other secondary sources give “Wen Ding” (or “Tai Ding”) only three years. This would require that the Di Yi reign begin in 1105, *zi* month first day *yiyou*, next month first day probably *yimao*. To go on, one must reconstruct the ritual calendar. The inscription launching the 1077 Yi Fang campaign in year 10 implies that the cycle began on 9 September, a *jiaxu* (11) day. But two other inscriptions dated year 3 (hence 1084) imply that the cycle began on 16 October, again a *jiaxu* day. So the cycle has been held at 36 *xun*, causing the first day to move back through the calendar more than five days a year. Taking another jump, way back, we find two inscriptions dated years 7 and 8, implying cycle first days *jiawu* (31). The 8th year inscription is part of a set of seven 10-day routine apotropaic inscriptions, making it datable to 1098, with
(Continued on next page)

many *Shi jing* poems to the time of King Wen—was simply wrong. It seems to me that if the *Bamboo Annals* has any value as a source for the history of ancient China, it would have to have placed this entry in the reign of King Xuan. If it were wrong about this, how could we assume that it would be right about anything else?

I don't think we need to dismiss the *Bamboo Annals* quite so completely. As I have demonstrated in a still unpublished paper (now entitled "Si lun *Zhushu jinian cuojian zhengju*" 四論《竹書紀年》錯簡證據; Nivison refers to an earlier draft of this paper, dated 17 December 2003, as "*Zhushu jinian cuojian san zheng*" 《竹書紀年》錯簡三證), the annals of King Xuan contain entries for each of the first nine years, but then until year 15 there is only a single entry for year 12, even though bronze inscriptions show that important military events took place in other of these intervening years. I have argued that these years ought not to have been blank in the original annals placed in the tomb, and that this gap doubtless reflects the situation Nivison described for "the last part of the discovered original": the bamboo strips were "unbound and scattered, many damaged, and some lost entirely." If there were indeed such damage at this point in the manuscript, perhaps strips bearing annals for years 10–11 and 13–14 were damaged or lost entirely. Moreover, when we examine the record for the twelfth year, we find that it consists of twenty characters:

十二年魯武公薨齊人弑其君厲公無忌立公子赤

Twelfth year: Duke Wu of Lu died. The men of Qi assassinated their lord Duke Li Wuji and established the ducal son Chi.

(Note 16—*Continued*)

sacrifice information showing the cycle in the year corresponding to 1098 to have begun on 24 November 1099. So the cycle had been alternating with 36 *xun* and 37 *xun*, keeping the first day moving back an average of only one day in four years. Even at this slow pace, the first day eventually migrated back out of winter into autumn, and from there on the cycle was held at 36 *xun*. This much information gives 1105 as first year—of 19—for Di Yi. One might assume 36–37 *xun* alternation prior to 1098–99, back to 1110; in the Shang sacrificial year corresponding to 1110 the cycle first day would be *jiawu* (31), 27 November 1111. (Probably it was 60 days later; see Appendix 4 sections 6 through 8.) 1110 has to be the year of the longest oracle inscription known, *Jia Bian* 2416, 10th month, but with no year date. The occasion was the launching of the campaign against the Yu Fang. (Routine sacrifice inscriptions in the following spring require a cycle beginning on day *jiachen* (41).) By luck there is a fragment in the White collection in Toronto, with a shorter version of the same text, and it bears the date "9th sacrificial year" (Chang Yuzhi p. 246). So year one was 1118.

As Nivison himself admits, this is a "daring claim," built up upon numerous other daring claims. If any one of these claims happens to be wrong, the entire argument would come crashing down.

Readers familiar with either Nivison's or my own work on the *Bamboo Annals* will note that this does not represent what Xun Xu 荀勗 (d. A.D. 289), the head of the initial editorial team, described of the strips: that they were "all bamboo strips bound with undyed silk; based on Xu's previous determinations of ancient lengths, their strips were two feet four inches long, written with ink, forty characters per strip" (皆竹簡素絲編，以臣勗前所考定古尺度，其簡長二尺四寸，以墨書，一簡四十字).¹⁷ However, it is a simple matter to see that it might reflect half of a strip. We know that strips of the length described by Xun Xu (two feet four inches of the old measure; i.e., approximately 55 cm) would have been bound with three binding straps, one each near top and bottom and one near the middle. What is more, we know from hundreds of examples of recently discovered bamboo strips, that the strips often broke at the points at which these binding straps passed, because the strips had been notched there to secure the straps from slipping up and down. Perhaps the twenty characters of this orphan record reflect the text on one half of a broken bamboo strip. What is more, when we examine more closely the annals of Shang king Di Yi, we find that other than a first-year record that "the king assumed position and dwelled at Yin" (元年庚寅王即位居殷), and a ninth-year record that he "died" (九年陟), it contains only the misplaced third-year record:

三年王命南仲西拒昆夷城朔方夏六月周地震

This record contains nineteen characters. What I have argued in the paper cited by Nivison is that this record may also represent the text on one half of a strip. If I am right, as Nivison says that I have "demonstrated convincingly," that this record actually belongs to the thirteenth year of King Xuan, perhaps it is the bottom half of the broken strip the top half of which contained the twenty characters of the twelfth-year record. And, perhaps further, the character *shi* 十 (ten) at the head of this record that would make it "thirteenth (*shisan* 十三) year" was written at the point where the strip was broken, and was thereby lost. If this was the case, the editors faced with a half strip bearing a "third year" record, and the annals of Xuan Wang already having a third-year record, would naturally have had to seek elsewhere for an appropriate place to put it. And, if further, the annals of Shang king Di Yi were effectively empty, as they are in the received text of the *Bamboo Annals* (perhaps because the strips of the tomb-text at that point were "lost entirely"), then it would have been a natural decision for them to place this record there, especially with the authority of the Mao Preface to the *Shi jing* identifying Nan Zhong as a figure of that time period. All of this is, of course, hypothetical, and I have been careful to phrase it with the requisite "ifs" and "perhapses." Nevertheless, I would suggest, based on the experience of the way unearthed bamboo strips have come out of the ground and come to be edited over the last generation, that it is far more likely than the complicated compositional process Nivison describes.

¹⁷ This description is found in Xun Xu's Preface to the *Mu tianzi zhuan* 穆天子傳.

This is why I think the *Bamboo Annals* as we have it is a very important resource for reconstructing early Chinese history. If we could be sure that Nan Zhong attacked the Kunyi 昆夷 and walled Shuofang 朔方 in the thirteenth year of King Xuan's reign, it would be important information for understanding both late Western Zhou history and the date of several poems of the *Shi jing*. Whether this is such a case or not, I am sure that time and again the *Bamboo Annals* supplies this kind of information. But I do not think that we have the text as it went into the ground—or even anything close to it. True, I think the text on certain individual strips has been preserved with amazing fidelity, and it may also be that some more extended passages have been preserved almost as well. But my own experience with trying to reconstruct the tomb text suggests to me that it is hopeless. Too many of the strips were “unbound and scattered,” too many “damaged,” and too many “lost entirely” for us to be sure that we have an integral text. Moreover, time and again, I can detect the editorial decisions made by Xun Xu and his team of editors at the Jin court. They were honest, hard-working scholars doing the best they could. But they were faced with an unprecedented discovery of texts that had been buried for almost six hundred years and which were written in an archaic script that must have been very difficult for even the most erudite scholars to read. Indeed, we know that shortly after Xun Xu's team finished its work, other scholars at court were thoroughly dissatisfied with it, and produced a new version. I am not at all convinced that this new version would be substantially better, even if it were still extant, which it is not. I am afraid we just have to assume that the complete tomb-text of the *Bamboo Annals* is lost forever. I hasten to add, however, that this is not to say that the text is completely lost. I am certain that the *Bamboo Annals*, and especially the “Current” *Bamboo Annals* preserves a great deal of the original tomb text.

Conclusions

David Nivison has made truly important contributions to all three of the major topics into which he divides *The Riddle of the Bamboo Annals*. Beginning with his discovery concerning the Wei-family bronzes in 1979, he brought studies of Western Zhou chronology to an unprecedented level of sophistication. Fifteen years later, the Chronology Project in China boasted of bringing together two hundred experts from such varied disciplines as archaeology, astronomy, history, and paleography to study this topic, but Nivison's first published article on this question, “The Dates of Western Chou,” was already an exemplary multi-disciplinary study, drawing on virtually all of these fields. As for the second topic of the present book, his criticisms of the Chronology Project itself, his earliest criticisms may have offended some scholars in China, but time has proven them to have been accurate. When he predicted that its *Brief Report* would be “torn to pieces,” the only thing wrong with the prediction—other than being somewhat intemperate—is that the *Report* hasn't needed to be torn to pieces; it has unravelled of itself. Finally, how can one underestimate Nivison's contribution to his third topic: the resuscitation, if not the recovery, of the *Bamboo Annals*, and especially the authenticity of the text of it that has

been current since the late Ming dynasty. If this text, or one like it, were to be unearthed today, it would be hailed as the greatest discovery of Chinese archaeology. Yet, for two centuries scholars disregarded the text that was open to plain view. Some still do, but the work of Nivison has caused even them to reconsider.

Despite all of these contributions, I have been quite critical—one might even say harshly critical—of *The Riddle of the Bamboo Annals* throughout this review. How is it that Nivison has been able to do so much, and yet still be so wrong? I think the answer is simply that he has tried to do too much. He has managed to convince himself that the text he has reconstructed for us is perfect (recall the words of his Introduction: “Now I know exactly what the first five-sevenths of the original looked like, 303 strips, word after word” [p. 11]). I myself have worked with the text of the *Bamboo Annals* almost as much as has Nivison, but I am as unsure of what the text looked like as he is sure. On the other hand, I am sure, or at least pretty sure, of some things, things that I think are pretty important. For instance, I don’t know whether most of the commentarial material that Nivison includes as an integral part of the text was found in the tomb or not, and if it was, whether or not it was written together with the annals (in whatever format); the narrative portions may have been, but I’m quite sure that the explanatory commentary was added by the Western Jin editors. I don’t know how much of the manuscript was damaged or lost, but I’m quite sure that at least some of it—including some of the five-sevenths that Nivison reconstructs—must have been. Finally, I’m also quite sure that the Western Jin editors made at least some mistakes in their editing; some of these mistakes were errors of omission, but others were errors of commission—designed to make the unearthed manuscript conform to their own understanding of early Chinese history. Given my own various uncertainties about the *Bamboo Annals* and how it has come to us, I simply don’t believe that any reconstruction of any more than relatively brief, discrete passages of the text is possible, and I certainly don’t believe that Nivison’s reconstruction is tenable.

And since, as he explains in his Preface, the complete chronology that he has constructed back to the time of Yao 堯 and Shun 舜, and even Huang Di, is part of a complete system based on his reconstruction of the *Bamboo Annals*, so too can I not accept this chronology as tenable. I do think we will someday have an accurate chronology of the Western Zhou period—indeed, we are close to having it now, thanks in large part to Nivison’s work. But for the Western Zhou we have, in addition to the *Bamboo Annals*, a relatively rich empirical basis for such a chronology, including both various kinds of received texts and especially an ever growing corpus of contemporary bronze inscriptions. For the Shang and earlier periods, we have a couple of ambiguous astronomical records and the *Bamboo Annals*. Maybe some new sort of source for these periods will be found—this is the great wonder of the field of early China studies, the possibility, and even expectation, that new sources will be forthcoming. But we need to be patient, and not to get ahead of our sources.

In *The Riddle of the Bamboo Annals*, David Nivison has not only gotten ahead of his sources; to a great extent, he has become the source. His chronology and his recovery of

the text of the *Bamboo Annals* are works of rare genius and not inconsiderable beauty; as he says, “everything fits.” Nevertheless, I am not persuaded that “the fit is the proof.” Nivison has had to work too hard and do too much. I suspect that he has done more than to “recover” the *Bamboo Annals*. He has rewritten and improved the *Bamboo Annals*, but in the process has moved yet another step farther from the original bamboo annals.