



The Riddle  
of  
the Bamboo Annals

竹書紀年解謎

David S. Nivison 倪德衛

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In Memory of

Wilbur Knorr

Wilbur Knorr was a Stanford professor of Classics and Philosophy, a scholar devoted to the history of Greek, Arabic and medieval Latin mathematics, whose brilliant career was cut short by early death in 1997. We had long shared an interest in ancient astronomy and chronology. The day before he died, I told him I wished to dedicate to him the book I was then writing. This is that book, already preceded by several monographs, also dedicated to Knorr. In the autumn of 1993, we taught together a “continuing education” course on world archaeoastronomy, which we called “Time and the Stars.” I took responsibility for ancient China and pre-Columbian Mexico, leaving the rest of the world to Wilbur and a third participant, Vladimir S. Tuman. (Tuman was a retired professor of physics; I had retired as a Stanford professor of Chinese and Philosophy in 1988.)

-- DSN, June 2008

# Scholars Recommending This Book

**Ying-shih Yü 余英時**

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美國普林斯頓大學榮休教授

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Director, East Asia Library, Stanford University  
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Equally versed in early classical texts, oracle bone and bronze inscriptions, archaeology as well as astronomy, Professor Nivison has devoted the last 30 years to a reconstruction of Chinese chronology in high antiquity. Taking as his point of departure the authenticity of the “Modern Text” *Bamboo Annals*, an assumption diametrically opposite to the long received view, his approach had not only been revolutionary but also proved to be methodologically more fruitful in riddle-solving. The present volume, written almost wholly from 2000 to 2007, therefore represents what is called “the final conclusions arrived at late in life” (wannian dinglun 晚年定論) in the Chinese scholarly tradition. This book is also a vivid testimonial to its author’s moral and intellectual integrity as a scholar in defense of historical truth. In questioning the validity of the Chinese state-sponsored “Three Dynasties Chronology Project” in a most serious manner, Professor Nivison has exercised the critical power of a historian to its highest possible degree. As a result his courageous fight for the purity of scholarship has not only won the support of many leading specialists of the field but also evoked, through Chinese translation, a widespread sympathetic response from the general reading public in China.

**Ying-Shih Yü**, Professor Emeritus, Princeton University  
美國普林斯頓大學榮休教授 余英時

倪德衛先生為美國著名漢學家，研究中國上古史有年，著作等身，尤其是對於《竹書紀年》及夏商周年代學的研究，在學術界自成一派，具有很大影響。我非常敬佩倪德衛先生年逾八十仍筆耕不輟，並多有創見，也非常盼望早日見到倪德衛先生的新作出版。

《竹書紀年》為先秦重要史籍，圍繞該書，前人作了大量的研究，它不僅關乎先秦史及先秦年代學，同時它也是中國古代文獻學尤其是辨偽學的重要研究對象。近年來，夏商周年代學已然成為全世界漢學界關注的一個焦點，其中許多問題都涉及到了《竹書紀年》。相信隨著倪德衛先生新著的出版，一定會給學術界帶來新的話題，同時也會使先秦史及先秦年代學的研究進一步深入。特此推薦。

**Chen Li**, Deputy Director, National Library of the PRC  
中華人民共和國國家圖書館副館長 **陳力**

Since 1979, a full thirty years and more, Professor David S. Nivison has been dissecting the “Modern Text” of the *Bamboo Annals* (*Jinben Zhushu Jinian*). He has applied a broad battery of analytical techniques to the data in the text, and he has been relentless in his efforts to correlate the dates in the *Bamboo Annals* with materials from historical, inscriptional, and astronomical sources. Nivison’s decades-long dedication to this task has resulted in a succession of publications that have been carefully scrutinized by scholars around the world. Their criticisms have resulted in increasing refinements by Nivison. The entire process of discovery, discussion, and elaboration is now assembled in *The Riddle of the Bamboo Annals*, which constitutes a major contribution to the dating of events before 841 BC.

**Victor H. Mair**, Professor of Chinese Language and Literature,  
Department of East Asian Languages and Civilizations,  
University of Pennsylvania



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# Preface

0. There is one thing above all else that anyone beginning to read this book must recognize: The reader is going to be required to consider fairly and with an open mind argument after argument that a very widely held belief among Chinese scholars is a complete mistake. That belief is that the so-called “Modern Text” *Bamboo Annals* (*Jinben Zhushu jinian* 今本竹書紀年) is a fake, or at best a crude attempt to reconstruct the authentic original text of the ancient chronicle. This brief (2 *juan*) chronicle begins with Huang Di (implied date probably 2402 BCE) and continues to the end of the fourth century BCE. When finished it was buried (in a tomb or cache of treasures in what is now Henan), and was discovered around 280 CE. Its subsequent bibliographical record peters out with the Yuan Dynasty. There was a flurry of printings of it in late Ming. And high Qing scholars noticed that quotations from it in early encyclopedias and commentaries often differed from the received printed text. This is especially true for the last fourth of it, which is, I admit, a mess. This part will not occupy my attention. Part Three of my book will put before you my recovery of what I claim is the actual bamboo strip arrangement of (probably) the first five-sevenths of the original. Part One reprints two monographs of mine that use the *Bamboo Annals* to reconstruct the entire chronology of Ancient China down to the end of Western Zhou. (Attachments are new.) The object of my attention in Part Two is the work of the “Three Dynasties Chronology Project” (Xia-Shang-Zhou Duandai Gongcheng) in the PRC, active from 1996 through 2000 when it published its initial “brief report.”

Professor Edward L. Shaughnessy opens his celebrated article “On the Authenticity of the *Bamboo Annals*” (*HJAS* 46, 1986) with a discussion of method, 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. 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.

0.1 Now consider again Shaughnessy’s last sentence: “But it is never acceptable methodology to prove one unknown with another unknown.” History is an inductive enterprise. We do not have two bins, for the “known” and the “unknown.” What we have are propositions with different degrees of probability. So let us translate: “It is never acceptable methodology to use a proposition with very low probability to show that another proposition with very low probability really ought to be seen as having very high probability.” Or biconditionally (for that is where Shaughnessy sees “circularity” in my work), it is never allowable to put together a bunch of very low probability propositions in such a way as to try to show that they all actually have very high probability.

At this point a flood of counter-examples ought to stream into one's mind. Here is a thought experiment from one of my articles: There has been a bank robbery. The robbers have escaped in a get-away car. There are three witnesses to the escape. All three are known to the police as inveterate liars. But the police think that there could have been no collusion among them or with the robbers. They are questioned separately, and all three give the same license plate number for the car. Finish this story yourself.

Let's do it with some numbers. You have a die-like object with ten faces instead of six, the faces having the numbers 0 through 9. Testing your suspicion that the die is not "fair," you throw it and it turns up 3: one chance in ten. You throw again and get 1: again one chance in ten, but taken together, for 31, one chance in ten times ten; and so on. As you keep on, the chance that you will, just by accident, continue throw by throw to get the next number of the decimal extension of the ratio of the diameter of a perfect circle to its circumference becomes progressively infinitely small. Just for this reason, if you keep on and do actually continue to get the decimal extension of this ratio, the chance that all this has been just by accident becomes itself infinitely small. So some organizing "story" (sc. historical narrative) is ordering the "evidence": instead of having an *a priori* probability of 0.1, each throw had a probability of 1.0.

0.2 A good live example is always better than a mere thought experiment; so let me develop a historical argument that plays a large role in this book. We all know how debatable the Xia Dynasty is. Its reality is taken for granted usually in China, but is usually dismissed by Westerners. In the *Bamboo Annals*, also usually doubted (in both China and the West), there are seventeen reigns, precisely dated. Professor David W. Pankenier is an expert seen by some of us as having a hyper-active imagination that often misfires. In *Early China* 9-10 (1983-85) he sees an obscure account in *Mozi* 19 as referring to a celestial event marking the passing of heaven's approval to Yu of Xia, who in the *Annals* is received by Shun in Shun's 14<sup>th</sup> year counting from Yao's abdication in his favor; and on this occasion Yu is given a "dark baton" which we can interpret as conveying authority. This in turn we should correlate with Shun's conveying authority to Yu in Shun's *de jure* 14<sup>th</sup> year (2029 in the *Annals*), which is treated in the *Annals* as the *de facto* beginning of Xia. The "dark baton" is the imagined shape of a spectacular conjunction in February, 1953 BCE. So that, we are invited to think, is when Xia began.

This is obviously *prima facie* highly doubtable, but I found it tempting, noting that the interval 2029 to 1953 is 76 years, one *bu*,<sup>1</sup> probably not an accident. Accidentally I learned in December 1988 that Kevin

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<sup>1</sup> The ancient *zhang-bu* intercalation cycle assumes 365.25 days per solar year. The winter solstice coincides approximately with the first day of a lunar month every 19 years, therefore 19 years is taken as a basic unit of time, called a *zhang*, of 6939.75 days, rounded to 6940. In this period there are approximately 235 lunar months, =  $(12 \times 19) + 7$ , therefore there must be seven intercalary months in one *zhang*. 4 *zhang* = 1 *bu*, =  $(4 \times 6940)$  minus 1, = 27759 days. 27759 divided by 60 (the *gan* cycle of 60 days) is 462 with a remainder of 39.  $39 \times 20 = 780$ , =  $60 \times 13$ . Therefore the *gan* for the first day of a *bu* is unique in a period of  $20 \times 4 \times 19$  years, = 1520 years, called one *ji*. So counting backward or forward 1520 years (= 555,180 days) from a given *gan* date will theoretically give the same *gan* in the 60-day cycle; and counting back 5 *bu* will require counting back 15 days in the cycle of 60. Thus, knowing that *yihai* (12) was the first day of the (*jian yin*) 7<sup>th</sup> lunar month of 453 (the date of the victory of Zhao, Han and Wei over Zhi Bo), the compilers of the *Bamboo Annals* concluded that in 2353 (Huang Di 50), = 100 *zhang* (=  $20 + 5$  *bu*) before 453, the *gan* for the first of the (*jian yin*) 7<sup>th</sup> lunar month must be *yihai* (12, =  $60 + 12 = 72$ ) minus 15 = *gengshen* (57) – as in strip \*002 (pp. 126-127) below. Actually a calculation in the system is one day short for about every three centuries backward or forward. (A skeptic who thinks the *Jinben Annals* a Ming Dynasty creation must account for the foregoing, and in doing so – in order to get Huang Di 50 = 2353 -- must know of a datum not in the *Jinben* but found only in Luo Bi's *Lu shi*, saying that there was a seven-year period of mourning after Huang Di's death. The skeptic must suppose his Ming inventor knew about this and deliberately left it out of his creation, in order to fool gullible people like me.) On the intercalation cycle, see Sivin 1969 p. 14.

The *zhang-bu* rule for intercalation is explicit in Liu Xin's San Tong Li system. See Christopher Cullen, "The Birthday of the Old Man of Jiang County and Other Puzzles: Work in Progress on Liu Xin's Canon of the Ages," *Asia Major* (third series) 14.2 (2001), p. 33 and note 10. (I am indebted to Howard L. Goodman of Asia Major for calling my attention to this material.) Cullen cites work by Yabuuchi Kiyoshi expressing a common view that systematic placing of an intra-year intercalary month did not begin until the 6th century BCE. I would argue that in much earlier times the division of the tropical year into equal 24ths had been devised, and its existence indicates already a focus on the problem of how to determine when an intra-year intercalation was due: that is precisely what it was for. (There could be non-astronomical

Pang (a consultant at the Jet Propulsion Laboratory in Pasadena) had been using his computer to ferret out ancient dates, and had put his finger on the solar eclipse of October 16, 1876 BCE, as the eclipse of Zhong Kang 5<sup>th</sup> year 9<sup>th</sup> month in the *Annals*. One would need to consider also the story of an eclipse in the *Zuo zhuan*, mentioning another eclipse in “the books of Xia,” possibly this one, when the sun had been in Fang (on one much debated interpretation of the *Zuo* text). Pang’s eclipse was visible no nearer than Mongolia, and was ring-form, unspectacular and of low magnitude, not even considered as a possibility by Zhang Peiyu (*Early China* 15, 1990). To all this I added tentatively theories of my own that almost no one accepts: that reign lengths in the *Annals* were usually accurate; and that reigns of record usually began with the year after completion of mourning for the king’s predecessor, a practice that I guessed was reflected in the Xia chronicle by gaps of one, more often two, or at times three or four years between reigns; theoretically they should all be two years.

Every item in this list was doubtable, and the cumulative doubtability of all of them just happening to be right was consequently enormous. But when I dared to write down “1953” followed by *Annals* reign lengths with two-year gaps, and calculated the exact date of the first day of the Xia-calendar 9<sup>th</sup> month of the 5<sup>th</sup> year of Zhong Kang, I got October 16, 1876. And the sun was in fact in lunar lodge Fang on this day. I concluded that the possibility of this result being accidental was so small I must accept it as correct. And this meant that every item, in the initially highly doubtable list, was also correct.

0.3 There were two flies in this ointment. The *Annals* account of the eclipse gave the day as *gengxu* (47), but it was really *bingchen* (53), and the year given, equivalent to 1948, was only 72 years off, not 76 years. I had to “explain away” these discordant data. I found I could do so by assuming that the author of this sentence was indeed using the *zhang-bu* intercalation cycle, as I had suspected, but was using it together with contemporary observations (late 400’s) to get a date (counting back 20 *bu*) when he thought the sun would be in Fang.\*

So everything fits, and the fit is the proof. If you agree, I urge you to keep on reading my book. I have just demonstrated the kind of reasoning I use in it all the time.

I owe various acknowledgements. To CSLI Stanford, for permitting my use of my article appearing here as Chapter Two. To David N. Keightley, for having pushed me into the Fufeng bronzes (and for informal instruction and encouragement for almost a decade before that, enabling me to read them). To Edward L. Shaughnessy and David W. Pankenier, for discoveries that have made this book possible. To Zhang Peiyu, Xu Fengxian, and Chang Yuzhi, for patiently reading and criticizing some of the monographs presenting the basics of my thinking. To Li Xueqin, for kind words early in my odyssey giving me the courage to go on with it. To all of these Chinese scholars for work which I have used constantly (and probably not enough). To various people unnamed, not necessarily excluding some of the above, for criticism, sometimes not gentle, provoking me into doing more work. To my friend Jiang Zudi of Intel for keeping my computer working and for sharing with me news from China and criticisms of recent archaeology. And especially to my colleague and friend Shao Dongfang, Director of Stanford’s East Asia Library, for provocative scholarship, for translating much of what I have done, and for finding publishers for even more of it. Obviously I alone must answer for the results. Now that I have put it all together, *qing zhizheng*.

Others have been very important in making this book possible. Electronic editors “Peggy” Shih, “Hwanyan” Lu, “Karen” Yen and “David” Lai of Airiti Press Inc have been extraordinarily patient with their trans-Pacific author. And the book itself owes its being indirectly to the Chiang Jing-kuo Foundation, which financed my colleague Shao Dongfang for two years as we began work on our “New Translation and Study” of all texts of the Bamboo Annals, now being considered for publication by an American university

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reasons for not heeding this rule, of course. An administrative decision to change the first month of the year, or mere carelessness, could result in a 13th month “year end” intercalation, or even a 14th month.) I think I have shown that the 24-fold division of the year was a feature of calendar astronomy even before Shang; and in Appendix 4 of this book I identify and analyze late Shang instances of intra-year intercalation that accord with the qi-center rule. (For the early existence of the system of 24 weather periods, see D. Nivison, “The Origin of the Chinese Lunar Lodge System,” in A. Aveni, editor, *World Archaeoastronomy*, Cambridge, 1989.

press. Several years ago I came to see that our joint venture was not the place to publish theories about the Bamboo Annals which were my own, and which were not necessarily shared by Shao; but the Nivison-Shao book was going to have to refer to those theories; so “The Riddle” had to be written and published first.

D.S.N. 7 July, 2008

\* Zhang Peiyu has pointed out to me a difficulty showing that I must assume something more here. I suppose an investigator who believed (1) that the Zhong Kang eclipse was in Fang; and (2) that counting back one whole cycle (20 *bu* = 1 *ji*, i.e., 1520 years) from a date in his own time when the sun was in Fang in Xia month 9 would get him to an ancient date when this would be true. He is trying to stretch back the received chronology so as to get Yao 1 be the numerologically required date 2145 (instead of 2026, my own calculation of the exact date for Yao 1). Toward this objective, he sets 1953 back one *bu* (76 years) to 2029, and sets 1876 (for the eclipse) back one *bu*, getting 1952. This he tests by subtracting one *ji* (1520 years) getting 432, and finds that the sun was not in Fang on the *shuo* of the Xia 9<sup>th</sup> month of that year, or the next. He keeps on trying, and 428 seems to work: the first day of the *zi* month (Xia 11<sup>th</sup> month) of 427 was *jiyou* (46); 59 days earlier, *gengxu* (47), should therefore be the first of the Xia 9<sup>th</sup> month of 428; the sun was in Fang; and  $428 + 1520 = 1948$  (Nivison and Pang, *Early China* 15 (1990) pp. 87-95). But 428, *zi*-calendar, was a 13-month year, and the winter solstice was the first day of 427. Applying intercalation rules, one finds that the identified month (which did in fact begin with *gengxu*) was the *intercalary* (Xia) 9<sup>th</sup> month. This may be irrelevant; but in each of the classical “Six Calendars” based on the *zhang-bu* system this month is taken as beginning with *jiyou* (46), not *gengxu* (47). So I must assume that the investigator is ignoring this (if he knew about it at all), and is simply applying the *zhang-bu* system abstractly.

(I use two ugly but conventional transcriptions. The seventh Western Zhou king I call “Yih Wang,” rather than “Yi Wang,” the ninth king. The ancient state which was the fief of Kangshu I call “Wey.” The later state which was the major fragment of the ancient Jin state, and which gave us the *Bamboo Annals*, I call “Wei.”)



## The Riddle of the *Bamboo Annals*

### Introduction

0.4 This book presents the results of my work, since 1979, on the “*Modern Text*” *Bamboo Annals*, *Jinben Zhushu jinian*. That is far too many words, so from here on I will call it simply “the BA.” The BA is a short book in two *juan* (chapters? – of about 16,000 characters in all), in the form of a chronicle of the early rulers of and in north China, from the supposed first one, Huang Di (the “Yellow Emperor” – supposedly 2402-2303, as I will show) down to 299 BCE, near or at the end of the reign of the second “king” (*wang*) of the state of Wei. The BA has been called “modern” at first disparagingly, to distinguish it from an “ancient text” (*guben*) which appeared to be different, surviving now only in quoted fragments collected from sources such as old encyclopedias and historical commentaries. In other words “modern” meant fake, the firm judgment of the greatest scholars of eighteenth century China.<sup>1</sup> Thirty years ago I would have agreed. Fake it is not; but “modern” it has come to be named.

The original text that became the BA was discovered around 280 CE in a royal tomb or underground royal storehouse in Jixian, in the part of Henan which is north of the Yellow River. (The date given in the *Jin shu* “Annals of the Emperors” is 279.) This tomb or storehouse must have been tunneled into a hillside, because the peasant woodcutter who stumbled on it was able to walk in. He probably found treasures of obvious value, perhaps such as bronze ritual objects, and also convenient bundles of sticks which he was able to use as torches to light his way. Before he had used up too many torches, fortunately his discovery came to the attention of local officials, and then to the imperial court of what we call the Western Jin Dynasty. The bundles of sticks were of course books, written on bundles of carefully prepared strips of bamboo. 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.

This claim is disputed. The judgment of the Qian Long scholars was not absurd. Tang bibliographies record a text in 12, or 13, or 14 *juan*. There is little in Song and Yuan, and nothing after that, until late Ming, when many editions of the two-*juan* text appear. And it is true that there are many quotations or paraphrases of a “Bamboo Annals” or “Annals from Jixian” that differ from the BA, especially from the last third of the second *juan* of it. It seemed that the original was much longer, and had disappeared by Yuan times, and that either someone in the Ming Dynasty had tried to reconstitute a text from quoted fragments and other material, guessing at reign lengths, and the result had gotten accepted as authentic; or else someone less scrupulous had simply put together a deliberate fake, passing it off as the authentic original.

But the difference between 2 and 12 or 13 or 14 could be simply different ways of organizing the same text. And it could well be that two texts were produced by Jin Dynasty court scholars that were almost the same for the most part, but differed considerably otherwise. It is known that there were two teams of Jin scholars working on the discovered original, one including Xun Xu and He Qiao for several years beginning probably in 281, and another including Shu Xi working more carefully on remaining difficulties beginning about 290. It is my opinion that the early bibliographies record the work of the second group, which has not survived except in quoted fragments, and that the first group’s work has survived, and indeed is what accounts for a lot of the quotations, though still a lot of the quotations are from the lost work of the second group. 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.

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<sup>1</sup> One of the strongest condemnations is in the account (*tiyao*) in the published catalog of the Imperial Manuscript Library *Siku quanshu zongmu*. For the concurring opinion of the historical critic Zhang Xuecheng see his “Notes of 1795” (“Yimao Zhaji”), *Zhang shi yishu* (Jiayetang edition), “Waibian” 2, pp. 16b-17a.

## Introduction

Opinion among Chinese scholars in China since the eighteenth century has been divided. By the twentieth century the majority of scholars in China and almost all Western scholars had come to accept the judgment of Wang Guowei (d. 1927). (This is what I myself was taught in graduate school at Harvard.) Wang believed the BA to be an outright forgery. He published a text of it with notes trying to show how every item in it could have been obtained or derived by the forger from previously known material – except, significantly, for lengths of reigns, i.e., for claimed absolute dates. For these he has to suppose that the forger relied heavily on his own imagination. I will be arguing that this is exactly where the forgery hypothesis – or the reconstitution hypothesis, for that matter -- can be shown to be simply impossible.

0.5 The twentieth century has witnessed a wealth of archaeological discovery, tempting many scholars to try to extend back into time our knowledge of Chinese history, especially of datable history. (The earliest generally accepted date is 841 BCE, the first year of the “Gong He” Regency after the flight into exile of the tenth Western Zhou king Li Wang. The *Shiji*, the first general history of China in the early 1<sup>st</sup> century BCE, gives us that date, carefully matched with dates of rulers of contemporary regional states.) Much of the newly discovered material contains dates, but dates that are frustratingly incomplete. The many thousands of oracle bone inscriptions almost always have day dates in the 60-day cycle, sometimes also lunar month dates (in an unknown lunar calendar), very rarely the reign year of the current king (unnamed), and even more rarely referring to an eclipse. Ritual bronze vessels of the Western Zhou often have inscriptions, and over sixty of these have “complete” dates of hotly disputed meaning: typically the year of the king’s reign (almost never naming the king), the lunar month (four possible first months), a technical term probably naming the quarter (much dispute there), and the cycle for the day. With this in hand you use a good lunar calendar (the best is by Zhang Peiyu) and do some educated guessing. I have spent many hours playing this game. One also draws on similar material in ancient literature.

Sooner or later someone was bound to think of bringing the scorned BA into this mix. I was that person, one Sunday night in November 1979, as I was preparing a seminar lecture for the next evening, facing a handful of professors (including David N. Keightley) and graduate students from Berkeley and Stanford. The subject was to be (at Professor Keightley’s request) the recently discovered cache of bronzes from Fufeng, Shaanxi. From recent excavation reports and from published collections of bronze inscriptions I selected some “fully dated” ones that had to be related, four of them describing royal audiences in a building called the “Shilu Palace” and with the same named officer, “Sima Gong,” introducing to the king the guest and maker of the vessel. The year dates were low, so I was probably looking for a short reign, the style of the characters telling me it should be somewhere around 870-850. So why not check the BA? After all, no one was watching me. Guiltily I reached for Legge’s text in his translation of the *Shang shu*. The BA dates Xiao Wang 870-862, Yi Wang 861-854, and Li Wang 853-842.

Within five minutes I realized that I was staring in disbelief at my major work for the rest of my life. The dates in the BA didn’t work, but three of the dates in the inscriptions implied the same *yuan* year, and all were so close to what the *Shiji* “*shijia*” chapters implied that the BA dates though wrong had to be based in some way on the real dates, and I had to find out how and why. 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.

0.6 I had decided that the four “Shilu Palace” inscriptions belonged to the reign of Yi Wang. This created a puzzle, because three of them probably implied 867 as first year, but one implied a date two years later.<sup>2</sup> The same problem confronted me in two inscriptions naming Gong Wang as reigning king: the later one implied a first year two years later than the other. Soon I found more examples of this, in inscriptions with high year numbers requiring assignment to the reign of Xuan Wang, whose first year 827 was not in dispute; yet several from 809 on required 825 as *yuan*. Examples mounted up: The BA gives Wen Wang of Zhou a reign of 52 years; but in the *Shiji* it is 50.

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<sup>2</sup> I am assuming that Yi Wang’s father Yih Wang was still alive during the irregular king Xiao Wang’s first five years and died in 868; and that a Xiao Wang court continued until 864, when Prince Hu (later Li Wang) was born.

The explanation is revealed in the BA itself: the chronicle for the Xia Dynasty begins with Yu, and at his death his hand-picked successor was ignored. “After the three years of mourning the world turned to Qi,” Yu’s son. From then on, after the death of almost every king there is a gap of a few years, most often two, which have to be for completion of mourning. After Xia, mourning for the deceased king did not demand an interregnum but did get reflected in the calendar, the post-mourning “accession” date being observed later in a reign, as Shaughnessy pointed out to me, perhaps after the deceased king’s chief officers had retired, been dismissed, or died, I reasoned.<sup>3</sup>

Shaughnessy alone has accepted this idea. Without exception, as far as I know, scholars in China have rejected it, often emphatically, if they have considered the matter at all.<sup>4</sup> But I have found it to be the primary key explaining how and why the dates in the BA differ from the real ones. For chronicles in the book after the Xia, editing of the original text during Warring States in the fourth century BCE forgot or resisted this principle. First years of reigns got pushed forward or backward as a result. In one case two reigns (Tai Wu and Yong Ji of Shang) actually got reversed. In another case, a whole reign was invented, the reign of the infamous “bad last emperor” Jie (Di Gui) of Xia. There were, of course, other factors involved. I have worked all this out in Chapter Two, which republishes my paper in a conference volume (2000) for a small conference (1998) at Stanford in memory of my late friend Wilbur Knorr. Chapter One is devoted to an ancient vexing problem, the disputed date of the Zhou conquest of Shang. This chapter is the previously unpublished English original of an article I published in China in 1997.

These opening chapters, recovering historical information with the help of the BA, owe much to discoveries in 1982-1984 made by Shaughnessy and Pankenier. In fact the second chapter would have been impossible without them; for I doubt that I would ever have hit on the essential insights myself. In *Early China* 7 (1981-1982, appearing in 1983) -- Pankenier had beaten me into print: I published as quickly as I could (too quickly in fact) in the *Harvard Journal of Asiatic Studies* at the end of 1983 -- Pankenier explained puzzling astronomical data in the Xia Di Gui chronicle (dated 1580, but actually late 1576); and in 1984 (publishing in *EC* 9-10) he identified a spectacular conjunction of the five visible planets in February of 1953 BCE, convincingly linking it to Shun’s transfer of authority to Yu, thus beginning the Xia Dynasty.

Alerted by a phone call in December of 1988, from Ashley Dunn, a science writer for the *Los Angeles Times*, I looked at work on an ancient early Xia solar eclipse by Kevin Pang, an investigator working for the Pasadena Jet Propulsion Laboratory. I thereby discovered another proof of Pankenier’s discovery. (Pang and I published in *EC* 15, in 1990. I had invited Pankenier to be a co-author but he declined.) This soon led me on to an exact chronology of reigns, to the day, for all of Xia and all of Shang. By 1995 (up-dated 1997) I had in hand the draft of a book, now already rendered as a whole obsolete by more discoveries; but two chapters of that are in print (1997 and 1998) in China, one being the Chinese version of Chapter One of the present book, solving (so I claim) the problem of the date of the Zhou conquest. Otherwise the 1995-1997 book draft will probably remain unpublished, but I condensed the essence of it into a long monograph published in Victor Mair’s *Sino-Platonic Papers* in 1999. My friend and collaborator Shao Dongfang, now head of Stanford’s East Asia Library, translated this for me and arranged publication in Taipei in 2002.

0.7 The mourning-completion idea is probably the major stumbling block to acceptance of my work in the PRC. It is probably true that one will accept it if and only if one also accepts the BA as authentic, and the leading mainland Chinese scholars reject both.<sup>5</sup> In 1995 (the year of my book draft!), at the urging of State Councilor Song Jian, the PRC government decided to finance an enormous five-year project to involve two hundred or more scholars and scientists in various fields, to work out scientifically an exact chronology of Chinese history as far back as possible before 841. This “Three Dynasties Project” (Xia-Shang-Zhou Duandai Gongcheng) was to report in 2000. I knew that the Project would simply ignore the BA, and I knew by this time that the BA was essential to working out a chronology (having already done it myself).

<sup>3</sup> 809 was the year following completion of mourning for Gong He, who had been Regent during Li Wang’s exile. (The *Shiji* mixes up Gong He and his probably much younger brother Gong Yu.)

<sup>4</sup> Actually, Wang Guowei refers to the idea, in guessing why his supposed forger handled Xia chronology as he did. Wang treats the theory as so familiar as not to require a reference. (*Jinben Zhushu jinian shuzheng*, at the end of Xia; he might have cited Lei Xueqi, *Yizheng* at Wen Ding 12, who cites *Han shi nei zhuan* and *Zheng Xuan*)

<sup>5</sup> Pankenier is an exception. He rejects the Nivison-Shaughnessy hypothesis but does accept the BA as for the most part authentic, though (he thinks) badly deformed in its present account of conquest era events. Chen Li is another exception, if he still believes the three years mourning is a late invention.