A Response to Robert Bagley's Review of My Book, 
Monumentality in Early Chinese Art and Architecture (Stanford University Press, 1995)

WU HUNG
UNIVERSITY OF CHICAGO

Robert Bagley's review of Wu Hung's Monumentality (Stanford University Press, 1995) in the Harvard Journal of Asiatic Studies, volume 51, no. 1 (June 1998), pp. 221-56, raised several issues of importance to Chinese art history, archaeology, and Asian studies in general. As it is the long-standing policy of HJAS not to print responses to reviews, the Editorial Board of this journal decided that it would print Wu Hung's response, in order to air important disagreements in our field, and to encourage continued reflection on fundamental issues of the methods and purposes of art history, archaeology, and cultural studies.

Richard M. Barnhart and Marsha Weidner, for the Editorial Board of Archives of Asian Art

Unlike a research essay with independent arguments, a book review may be written to negate someone else's work. Such negation becomes an attack when the reviewer's intense dislike of the work in question overwhelms an objective reading and evaluation. Typical symptoms of such a review include an enraged attitude, a cynical tone, and a willful distortion of the author's approaches and arguments. All of these symptoms are found in Robert Bagley's review of my book, Monumentality in Early Chinese Art and Architecture, published in Harvard Journal of Asiatic Studies 58.1 (June 1998, pp. 221-56). It is regrettable that the field of early Chinese art and archaeology has to endure such antagonism, which the present response would seem to prolong. But this is not a situation in which one should avoid confrontation, partly because Bagley's antagonism is targeted at something much larger than my book. By dividing scholars into cultural "insiders" and "outsiders," by questioning concepts such as "Chinese culture" and "Chinese art" in describing pre-imperial China, and by rejecting historical texts as a useful source for studying early Chinese art and culture, his criticism is aimed at many scholars as well as large scholarly traditions. Before focusing on these more general issues, however, I should first respond to his specific treatment of my book.

1. In many places in the review Bagley misrepresents the book and makes spurious allegations based on misrepresentations. Here are some examples:

(1) Bagley "suppose[s]" that an abstract developmental pattern constitutes my explanation of ancient Chinese art and that I am "lulled . . . into the belief that the unfolding of an abstraction can be a historical cause" (p. 230). To support this observation he writes: "In his [i.e., Wu's] account of art history, patrons play only a shadowy role (as the users of 'monumental' artifacts), while artists and craftsmen play no role at all: the actions and decisions of makers have nothing to do with the changes in material culture he describes" (p. 230). This is far from true. My published work has been consistently challenging interpretations of Chinese art based on universal evolutionary theories.1 In the introduction to Monumentality, I define my approach as a "historical" and "cultural" one, as opposed to a "transhistorical" and "transcultural" one (p. 3). The book's goal is described as:

Rather than attempting to find another universal 'common ground' that accounts for various kinds of monuments in a 'broad theoretical way' a more urgent and plausible goal is to historicize the phenomenon of the monument—to explore indigenous concepts and forms within well-defined cultural and political traditions, to contextualize these concepts and forms, and to observe conflicting notions and manifestations of the monuments in specific situations (p. 3).

Within this general interpretative framework, the developmental logic of early Chinese art—the historical links between large artistic phenomena such as the art of ritual objects during the prehistorical and early historical period and the art of palatial and funerary monuments during the early imperial period—is sought in the intrinsic relationships between specific art and architectural forms and specific cultural, political and religious practices. Bagley may criticize whether this interpretation is successfully realized in the book; but it is irresponsible to ignore the author's intention and to distort it as a search for an abstract evolutionary pattern.

Contrary to Bagley's characterization, the book utilizes newly discovered textual and pictorial evidence to reach a better understanding of ancient artisans and builders (pp. 238-50); the patrons' role is stressed throughout the book. I
link the appearance of conspicuous ritual objects with the social stratification and the emergence of rich, powerful patrons on the prehistoric east coast (p. 26). I explain the changing meaning and function of Western Zhou bronzes through a detailed discussion of sacrificial vessels made by generations of the aristocratic Wei family (pp. 92–99). I attribute the Eastern Zhou penchant for imposing palaces and mausoleums to the political ambition of their patrons (pp. 99–109, 112–14). The entire chapter three, on the Western Han capital, Chang’an, examines the role of royal patrons in constructing this city, and I state the central theme of this chapter explicitly on p. 149: “Chang’an’s 200-year-long construction was continuously undertaken because of the ambition of individual rulers to establish their own monuments with specific political and religious values. These monuments also reflect the rise and fall of different social forces and attest to changing modes of political rhetoric.” Chapter four, on Eastern Han funerary monuments, again investigates the role of patrons; but in this case these were not great kings but family members of the deceased, his former friends and associates, and himself (pp. 192–238).

Having paid such extensive attention to patrons, I cannot comprehend why Bagley claims that “patrons play only a shadowy role” in my account of Chinese art. Since Bagley presents himself as a defender of patronage studies (pp. 26). I explain the changing meaning and function of Western Zhou bronzes through a detailed discussion of sacrificial vessels made by generations of the aristocratic Wei family (pp. 92–99). I attribute the Eastern Zhou penchant for imposing palaces and mausoleums to the political ambition of their patrons (pp. 99–109, 112–14). The entire chapter three, on the Western Han capital, Chang’an, examines the role of royal patrons in constructing this city, and I state the central theme of this chapter explicitly on p. 149: “Chang’an’s 200-year-long construction was continuously undertaken because of the ambition of individual rulers to establish their own monuments with specific political and religious values. These monuments also reflect the rise and fall of different social forces and attest to changing modes of political rhetoric.” Chapter four, on Eastern Han funerary monuments, again investigates the role of patrons; but in this case these were not great kings but family members of the deceased, his former friends and associates, and himself (pp. 192–238).

Having paid such extensive attention to patrons, I cannot comprehend why Bagley claims that “patrons play only a shadowy role” in my account of Chinese art. Since Bagley presents himself as a defender of patronage studies (pp. 26). I explain the changing meaning and function of Western Zhou bronzes through a detailed discussion of sacrificial vessels made by generations of the aristocratic Wei family (pp. 92–99). I attribute the Eastern Zhou penchant for imposing palaces and mausoleums to the political ambition of their patrons (pp. 99–109, 112–14). The entire chapter three, on the Western Han capital, Chang’an, examines the role of royal patrons in constructing this city, and I state the central theme of this chapter explicitly on p. 149: “Chang’an’s 200-year-long construction was continuously undertaken because of the ambition of individual rulers to establish their own monuments with specific political and religious values. These monuments also reflect the rise and fall of different social forces and attest to changing modes of political rhetoric.” Chapter four, on Eastern Han funerary monuments, again investigates the role of patrons; but in this case these were not great kings but family members of the deceased, his former friends and associates, and himself (pp. 192–238).

(2) I discuss the legend of the Nine Tripods in the introduction to illuminate a particular concept of monumentality in ancient China (pp. 4–11). To Bagley this discussion is “an instance of sustained and bewildering self-contradiction” (pp. 252–53). This is his evidence:

Wu seems quite unable to make up his mind whether or not the tripods really existed, and quite unable to face the fact that he cannot have it both ways. If a particular set of Nine Tripods really was cast around 2000 B.C. and passed from king to king for the next 1700 years, it would be a remarkable phenomenon indeed; but evidence for the phenomenon is absent. If the story of the tripods is instead a late Zhou myth, then it constitutes a different kind of evidence, evidence bearing on the period in which it was told (p. 253).

Here Bagley ignores my statement that Wangsun Man’s account of the Nine Tripods in 605 BC reflects a retrospective view, as it attributes elements of bronze art in different periods to a single set of mythical objects (p. 11). Also omitted is my discussion of other versions of the Tripod legend, which reflect a different mentality in the late Eastern Zhou (pp. 8–9). Bagley feels at liberty to brush away a crucial statement I have made to conclude my discussion of the Tripod legend, a statement which contradicts his allegation (about whether I think that the Tripods actually existed) and highlights the significance of the Tripod legend as a historical discourse on ritual art (a significance which is never mentioned in the review):

The story of the Nine Tripods is probably sheer legend; although many ancient writers recorded and discussed the Tripods, no one ever claimed to have seen them and could thus describe them in detail. Nothing seems more unsuitable for an art-historical inquiry than such elusive objects. But to me, their value as historical evidence lies not in their physical form, not even in their existence, but in the myth surrounding them. Instead of informing us what the Nine Tripods were, the ancient authors told us what they were supposed to be. They were supposed to commemorate an important historical event and to symbolize political unity and its public. Concealed in the royal temple, their location defined the center of the capital and the country; the common knowledge of their location made them a focus of social attention. They could change hands, and their possession by different owners, or their “movement” from one place to another, indicated the course of history. They took the form of a cooking utensil but exceeded the utilitarian usage and productive requirements of any ordinary vessel (p. 10).

(3) One of Bagley’s major criticisms of the book concerns my use of the “three ritual books” (“San li”: Zhou li, Yi li, and Li ji) in interpreting the tradition of ritual art. Later in this response I will address this criticism as a methodological issue; here I want to point out that Bagley misrepresents my view of these texts, as he writes: “Wu does not view these texts as the work of fallible authors motivated by the concerns of their own day but rather as trustworthy descriptions of a timeless social order that from the beginning was founded on rites (li) served by ‘ritual paraphernalia’ (liqi)” (p. 232). Nowhere in the book do I view the three ritual books as “trustworthy descriptions of a timeless social order.” Instead, I consider them later compilations whose importance lies in offering some important clues for understanding the tradition of ritual art in ancient China. This idea is stated clearly in the following statement, which Bagley ignores completely:

Compiled at the end of the Bronze Age, these records were written by ritual specialists who attempted to revitalize (by systematization and idealization) earlier ritual procedures and institutions. These are not art-historical or historical studies, and their authors treated bronzes or carved jades as contemporary, functional objects rather than as relics of the remote past. Differing from later medium-oriented scholarship, which often isolates works of art from their contexts, these books always situate manufactured objects in groups and in specific ritual
occasions. In terms of both chronology and ideology, therefore, these records are close to the period during which ancient bronzes and jades were produced and used, and they preserve important clues for a proper understanding of early Chinese art (p. 19).

Bagley comments on chapter two, “From Temple to Tomb”:

If the sole function of bronzes was to serve in ceremonies performed in ancestral temples, why were they ever put into tombs? Wu does not ask this question; indeed he barely mentions that bronzes are found in tombs, for his argument that the locus of ancestor worship shifted from the temple to the tomb in the course of Eastern Zhou requires us to believe that the tomb was not a locus of ancestor worship before then. This is wholly at variance with the evidence of archaeology (p. 240).

Since nowhere in the book do I claim that “the sole function of bronzes was to serve in ceremonies performed in the ancestral temples,” Bagley’s initial question sets up a straw man to ridicule. (How could anyone make such a claim?) Likewise, I never ask readers “to believe that the tomb was not a locus of ancestor worship” before the Eastern Zhou; instead I state clearly: “In fact, even during the Shang and Western Zhou, tomb and temple coexisted as twin centers of ancestor worship, but their function and architectural principle were entirely different” (p. 111; also see p. 114, where I discuss the complementary symbolism and social function of temple and tomb during the late Eastern Zhou). My basic approach is that temple and tomb were complementary centers of ancestral worship in a dynamic relationship. Although this approach underlies the entirety of chapter two, it disappears from Bagley’s review.

Turning to the relationship between the ancestral temple and the ritual bronzes it housed, Bagley summarizes my view as: “Until Eastern Zhou, public display of power was not necessary for this purpose [of maintaining the political order]; bronzes kept secret deep within temples were sufficient to inspire awe, and buildings, no matter what activities went on inside them, were not ‘monumental’” (p. 241). The degree of misrepresentation in this summary can be determined by comparing it with my statement on p. 78: “A dialectic relationship existed between liqi and their architectural context: sacred bronzes gave meaning and authority to a temple, but these bronzes became functional and meaningful only during temple rituals.” Bagley creates an impression that in emphasizing the role of “secret” ritual bronzes I dismiss the importance of architecture. But a central thesis in chapter two is in fact the interdependence of objects and architectural framing. A whole section is devoted to the significance of temple architecture: described as “closed,” “vast,” “hallowed,” “solemn,” and “mysterious” in Zhou hymns, the design of an ancestral temple effectively guided members of a lineage or family back to their Origin through routine ritual performances (pp. 82–88).

Bagley further discredits my description of the layout of the Western Zhou royal temple because this description, in his judgment, “is based chiefly on the late Zhou and Han ritual texts and will be found compelling only by readers prepared to assume that the authors of those texts were well informed about early Zhou” (p. 239). But in fact, the principal texts I use in the section he refers to ("The Temple," pp. 79–88) are Shi jing ("Zhou song"), Shang shu ("Zhao gao" and "Luo gao"), and Yi Zhou shu ("Zuo Luo," "Shi fu," and "Du yi"). Shi jing is cited ten times in this short section; Shang shu and Yi Zhou shu are referred to more than ten times in the notes. Other crucial materials for understanding Zhou temple architecture and temple rituals come from archaeology, including both excavated architectural sites (such as the Fengchu structure and the city of Qufu) and inscribed bronzes (such as He zun and Wei family vessels). I indeed use texts such as Zuo zhuan, Li Shi chunqiu, Shi ji, Zhou li, and Li ji, but these are treated as supporting evidence. For example, when discussing the internal structure of the Zhou royal temple, I first cite a passage from the "Shi fu" chapter in Yi Zhou shu, which records the royal ancestors worshipped in the temple at Feng. The "Zuo Luo" chapter in Yi Zhou shu then reveals some new features in the next royal temple established by King Cheng at Chengzhou. I argue that these features reflected "a new pattern of ancestral worship" and initiated the further expansion of the temple (p. 81). Only at this point do I use other sources, including Zheng Xuan’s commentary on Zhou li, in describing the temple’s later development. It is strange that Bagley would conceal all my uses of early texts, and mentions Zheng Xuan’s commentary as my only “source for the internal arrangement of the Western Zhou ‘dynastic temple’ at Chengzhou” (p. 239, n. 29).

Bagley does not bother to tell readers about the importance of this second-century tomb in Shandong: it is the only known Han-dynasty tomb that contains an inscription describing in detail the tomb’s decorative program. He does not say that the tomb’s decoration actually closely follows the inscription; only the ceiling carvings appear

The decoration of the Cangshan tomb does not quite agree with the program described in the inscription; from this Wu illogically deduced that the inscription must represent the intended design, and that it must therefore have been composed by the builder, since the patron could not know the intended design. It is perhaps more likely that both inscription and decoration were formulas and that neither builder nor patron worried too much about harmonizing them (p. 246, n. 37).

The decoration of the Cangshan tomb does not quite agree with the program described in the inscription; from this Wu illogically deduced that the inscription must represent the intended design, and that it must therefore have been composed by the builder, since the patron could not know the intended design. It is perhaps more likely that both inscription and decoration were formulas and that neither builder nor patron worried too much about harmonizing them (p. 246, n. 37).
as simplified versions of the scenes described in the inscription. He also does not say that the proposed authorship of the inscription is not based solely on this discrepancy, but is also based on an analysis of the language and style of the inscription, which differ markedly from all known Han-dynasty funerary inscriptions composed by the deceased’s family members or former associates. Unless Bagley has reviewed and negated all this research, I cannot see how he can call my interpretation “illogical.” Unless he can demonstrate that other Han inscriptions share the structure and content of the Cangshan text, I cannot see how he can call this text a “formula.”

These are some typical cases in which my evidence and interpretations are seriously misrepresented. Another form of misrepresentation is found in Bagley’s drastic reduction of a complex discussion. Major themes and historical reconstructions in the book are frequently left out; minor arguments and occasional remarks are picked up and strung together to support an allegation. (In Bagley’s own words, “Let me assemble the scattered remarks that seem to describe a ‘developmental logic’ and try to piece together Wu’s view of causation” [p. 228].) This tendency becomes intensified when he begins to criticize the book chapter by chapter after a perfunctory “broad overview.” This part of the review is thirty pages long and repeats certain allegations over and over (pp. 225-54), but it never discusses a chapter as an organized whole. Bagley presents each chapter in such a reductive and fragmentary way that his review gives little sense of what the chapter actually says. For example, one of his major criticisms of chapter one is my use of the three ritual texts; but nowhere does he reflect upon the concept of liqi (ritual paraphernalia) that these texts are used to elucidate (pp. 18–24). Because of the fundamental importance of this concept to my discussion in the following sections, such omission is not just a minor oversight, but debases the whole discussion in the chapter.

In reviewing chapter two, Bagley says nothing about my discussion of the development of Eastern Zhou cities, palaces, and mausoleums, a development which culminated in the founding of the Qin capital at Xianyang and the construction of the First Emperor’s Lishan tomb (pp. 100–110, 112–20). With this substantial omission, he can then call my reconstruction of a large historical shift of religious and administrative centers from temple to tomb and palace fictional, “simplistic formulas,” and “a failure of Wu’s historical imagination” (pp. 241–42). His review of chapter three only criticizes the first section on “Two Views of Chang’an.” The remaining and main part of the chapter, which is 37 pages long and discusses the construction of Chang’an over two centuries, is dealt with in half a sentence: “...Wu’s narrative, filled with tales of Han Wudi and his necromancers, reads like storytelling” (p. 245).

2. Even more serious than these misrepresentations and irresponsible allegations, Bagley reframes my discussion of early Chinese art into an opposition between “cultural insiders” and “other scholars.” The two terms “cultural insiders” and “cultural outsiders” appear repeatedly in the review (pp. 226, 231, 233, 234, 256). These are Bagley’s terms, but he never spells out their meaning. His way of using them, however, makes clear that they refer not only to the ancients but also to contemporary scholars’ ethnic identities, as we read in the concluding sentence of the whole review: “To begin by declaring that ancient China is a phenomenon to be understood only on its own terms, by cultural insiders, is to forfeit all claim to the attention of cultural outsiders” (p. 256). In other places, he calls me one who adopts “the pose of a cultural insider” (p. 226) and compares me with a Western scholar who is “not hyp-notized by notions of Chinese identity” (p. 232, n. 14). When I try to uncover an ancient system of classification buried beneath later systems, he immediately tells readers that these later systems are “usually equated with Western views” (p. 231).

The problem, however, is that Bagley can never specify how I equate these later systems with “Western views” and how I posit myself as a “cultural insider,” because these are artificial accusations without any basis. Let me summarize my discussion on pp. 18–19, which has provided Bagley with most of his evidence for these accusations. I start from two basic ways in which social phenomena are classified: “internal classifications” made by contemporary members of a society and “external classifications” pursued by people outside the society, “either from alien cultures or from later periods” (p. 18). This are basic anthropological concepts, and I cite Robert J. Sharer and Wendy Ashmore’s study to clarify them. I then review a major “external classification” in the study of early Chinese art. Starting from the Song dynasty, antiquarians systematically classified ancient objects into medium-based categories; the emergence of jinshi xue (the study of metal and stone works) is itself the best proof of this development. To demonstrate the continuing influence of this scholarly tradition in the modern period I give three examples: Zhao Ruzhen’s early twentieth-century Guide to Antiques (Guwan zhinan), Stephen Bushell’s 1910 Chinese Art, and Liang Qichao’s 1926 lecture “Archaeology in China.” These three authors came from different backgrounds and their works had divergent focuses, but they all employed the medium-based classification of objects in their accounts of Chinese art. Bushell is the only Western scholar mentioned here, whom I describe as “a learned sinologist who had lived in Beijing for some thirty years” and who “combined the Chinese antiquarian tradition with a Western classification of collectible objects’ popular in his day” (p. 19).

It is clear that this discussion is purely historiographical. The medium-based classification is defined as a broad his-
torical phenomenon and a system of knowledge, and is never identified with a particular group of scholars distinguished by origin, time, or race. As a system of knowledge, this classification "has the obvious advantage of arranging the vast and often chaotic collection of ancient objects into comprehensible units" (p. 19). But I also cite Stephen Owen's reflection on Chinese poetry to indicate the need to think beyond this system of knowledge, as he writes: "But when an art is displaced—by transfer to another civilization, by time's long spans, or by the disruption of its continuity—then we must discover a way to give voice to those very dimensions of the poetic art which are usually left in tacit silence." It is at this point I propose to reread the "three ritual books" written mainly during the Eastern Zhou, which may allow us "to discover the 'internal classification' of early artworks or, more practically, to find ancient classification systems closer to the original classification than our own" (p. 19).

Bagley completely abolishes the historiographical nature of this discussion, and turns it into a scheme of academic politics. He tells readers that "Wu asserts not just that his texts are competent sources for the Neolithic and Bronze Age but also that they give his approach to ancient artifacts special authority, the authority of the cultural insider" (p. 233), and that I dismiss "other scholarship as distorted by the bias of cultural outsider" (p. 234). He says not a single word about my account of historical anti-urbanism, but presents my whole discussion as an attack on "other scholars" who "impose 'external classifications' on the artifacts" (p. 233). While no scholar would argue that to uncover the lost meanings and intentions of ancient art is not a fundamental goal of art-historical research, to Bagley such a goal in my case can only signify "the pose of a cultural insider" who is "hypnotized by notions of Chinese identity." My desire to understand a historical discourse on art is described as a "pretension to originality [that] depends on misrepresenting the state of the field and the views of other scholars" (p. 235).

Bagley announces that "his [i.e., Wu's] case would collapse if he acknowledged the existence of more recent scholarship" (p. 233). But this is his case, not mine. As summarized above, my focus is a reflection on the different methods used to construct the history of ancient Chinese art, not a criticism of contemporary scholars. Unable to list any individual scholar who is mistreated in my book, Bagley cites the publications of Jessica Rawson to exemplify the kind of cross-mediums studies of Chinese art that I have "dismissed" (p. 234). The fact is, however, that not only are Rawson's writings frequently consulted and cited in the book (see notes 119, 123, 149, 150, 168, 181 in chapter one alone), but I have always admired her writings for their impressive scope and depth. There are also other scholars who in recent years have paid increasing attention to the interrelationship between objects made of different materials. But thinking about how many books on early Chinese art published every year still focus on individual mediums (bronze, jade, ceramic, lacquer, and others), and that even archaeological reports from China still classify excavated materials into medium-based categories, the kind of cross-mediums study pioneered by Rawson and other scholars can only be described as a newly emerging trend, not the main stream in early Chinese art history. I thus maintain my general appraisal of the field in the book: "True, there have been efforts to reunite these separated branches [of Chinese art] into a coherent narrative, but until recently the boundaries separating categories were rarely crossed" (p. 18); and I do not think that this is a misrepresentation of the state of the field or of any individual scholar.

What happens here, therefore, is that although Rawson and I both advocate contextual and cross-mediums studies of early Chinese art, we are artificially divided into two opposite camps. The division is drawn between Chinese and Western scholars. Thus, when Bagley goes on to examine my discussion of the Chinese Bronze Age as "an instance in which the state of the field is misrepresented," he begins exactly from this opposition: "Wu attributes to Western scholars (on the authority of a thirty-year-old Chinese book) a sequence of three ages: stone, bronze, and iron" (p. 235). In order to present this Chinese-Western opposition as a feature of my scholarship, he deliberately misidentifies my source as "a thirty-year-old Chinese book," while in fact I cite the original definitions of the three-age sequence by both Lucretius and Christian Thomsen (p. 63, pp. 295–96, n. 184). He also omits my account of the Chinese Communist Party's adaptation of the three-stage sequence and its persecution of scholars who attempted to challenge the universality of this theory (pp. 66–68). Bagley acts as if I have said nothing about this when he writes: "Yet the three-stage sequence as he [i.e., Wu] sums it up, . . . has more to do with the views of Chinese Marxist ideologues than with the thinking of any contemporary Western archaeologist, while his own redefinition of the Bronze Age, adopted from K. C. Chang, agrees precisely with the ideas of Western archaeologists" (p. 216).

Bagley's logic seems to become twisted here: Why is there a problem if my view agrees with those of Western archaeologists? This puzzle persists when he continues: "He [i.e., Wu] quotes Chang to the effect that in China, 'the Bronze Age was not achieved primarily through a revolution in productive technology. If there was a revolution, it was in the realm of social organization.' This idea did not originate with Chang, nor did it originate in studies of China; it has been at the center of Western anthropological thinking for decades" (p. 216). It is difficult to understand Bagley's rage, especially because neither K. C. Chang nor I claim copyright for a general idea; what he
and I try to understand is the historical reality of the Chinese Bronze Age. What makes Bagley uncomfortable seems to be that “the ideas of Western archaeologists” and “Western anthropological thinking” are used to explore the particularity of ancient China. It is alarming to see that even K. C. Chang, who has contributed so much to the study of ancient China in the West, is somehow excluded from “Western archaeologists” and “Western anthropological thinking.” Bagley’s problem is that he must distinguish, either consciously or unconsciously, a scholar of non-Western origin from Western scholarship, even when this scholar is trained in the West and is active mainly in the Western academic world. Only such a mentality can explain his grouping and separation of “cultural insiders” and “cultural outsiders.” In my case, three times in the review he associates me with other scholars and scholarly traditions. Beside K. C. Chang, he notes my connection with scholars in China: “Perhaps familiarity with the rhetoric of his mainland colleagues has lulled Wu into the belief that the unfolding of an abstraction can be a historical cause” (pp. 229–230). And there is of course my debt to traditional Chinese scholarship: “Quoting Zhou li and Li ji to explain Shang bronzes and Neolithic jades is the most tiresomely familiar form of traditional Chinese scholarship; “Quoting Zhou li and Li ji to explain Shang bronzes and Neolithic jades is the least tiresomely familiar form of traditional Chinese scholarship, thoroughly discredited in the age of scientific archaeology. It is presented here as a new and iconoclastic approach . . . ” (p. 235).

3. With this and similar statements, Bagley sets up oppositions not only between “cultural insiders” and “cultural outsiders” but also between “traditional Chinese scholarship” and “scientific [Western] archaeology.” These oppositions constitute the basis for his two major criticisms, concerning (1) the notion of “Chinese culture” and (2) the use of texts in studying this culture. These are large, complex historical and methodological issues; but Bagley does not explore their complexities. Rather, he rushes to paint a caricature of an author obsessed with his Chinese identity, whose every argument shows an “insistence on Chinese uniqueness” (p. 237). By raising a simple rhetorical question—“What makes any of this Chinese?”—Bagley can thus dismiss these arguments (see examples on pp. 236–37). Here Bagley’s distortion of my view is twofold. First, I never make a claim for “Chinese uniqueness” in the examples he provides, whether about the development of ritual art or about the use of advanced technology in making precious objects. Second, I have indeed compared early Chinese art with other art traditions and have identified a specific characteristic of early Chinese art; but Bagley is silent on this point. This comparison, which underlies much of the book’s introduction, is summarized on p. 10.

Thorstein Veblen’s idea, that wasteful spending can enhance social prestige and power, has enabled anthropologists to find an essential feature of monumental architecture: its vast scale requires an extraordinary large amount of human energy. But to the ancient Chinese, who did not pursue colossal buildings until the end of the Three Dynasties, conspicuous ritual objects signified the power to control and “squander” human labor.

This statement consists of two interrelated arguments. On the one hand, as in many other traditions in the ancient world, ritual art in early China enhanced social prestige and power. On the other hand, unlike many other traditions (such as ancient Egypt, Mesopotamia, and Mayan civilization), colossal architectural monuments were not pursued in ancient China until the end of the Bronze Age; the dominant form of ancient Chinese art before this moment was liqi—portable ritual objects of extraordinary social and religious significance. My point here is therefore not to prove the “uniqueness” of Chinese art, but to demonstrate the necessity of approaching this art historically and culturally. Nowhere do I imply that precious ritual objects were only manufactured in ancient China; but I do ask why in ancient China such objects absorbed so much wealth, energy, and creativity.

Bagley equates this approach (which is by nature comparative) with “declaring that ancient China is a phenomenon to be understood only on its own terms” (p. 236). Disregarding my citations of various definitions of the monumental by Aloise Riegl, John Brinckerhoff Jackson, Georges Bataille, and Barbara Rose, and omitting my references to Karl Marx, Michel Foucault, B. Benton, and a number of anthropologists and archaeologists working in non-Chinese fields, Bagley tells readers that “Wu’s categorical rejection of cross-cultural study is not a defensible intellectual position; it is either chauvinism or a cloak for ignorance” (p. 254). Since from the very beginning of the book I situate early Chinese art (and the study of early Chinese art) in a broader context, what Bagley complains about cannot be a general rejection of cross-cultural study, but can only be a rejection of a particular position in crosscultural study that he promotes. This position becomes known from his insistent dismissal of any particularity of ancient Chinese art (“What makes any of this Chinese?”), and is stated plainly by him toward the end of the review: “Human experience is not cordoned off by culture” (p. 254). We have heard similar statements before: “There is nothing surprising in this process [of the evolution of Chinese pictorial art]; Western precedent has prepared us for the classically happy conclusion.”

But Bagley not only asks students of early Chinese art to ignore the specific features of this art, but also demands that they abandon “Chinese culture” (and “Chinese art” as part of this culture) as a historical and analytical concept, at least in studying prehistorical and early historical periods. In advancing this second argument he relies on universal “human experience” as well as regional “cultural diversity”—two seemingly opposite positions but both
allowing him to keep away from "Chinese culture." I should say that I also believe in the importance of studying cultural diversity; and quite agree with Bagley when he writes: "In the last three or four decades archaeology has steadily broken down the idea that there was a monolithic 'Chinese culture' even in historical periods, and regional diversity is now a major theme of both Western and Chinese scholarship on the archaeology of ancient China" (p. 232). But to break down a monolithic Chinese culture is one thing, to abolish the concept of Chinese culture is another thing. Bagley collapses these two different intellectual agendas into a single proposal which, instead of suggesting a more complex approach in studying early Chinese culture, questions the legitimacy of such study itself. Throughout his review, he puts the term Chinese culture in quotes (either as "Chinese" culture or as "Chinese culture"). The reason for doing so is given at the end of the review:

A study of ancient China must acknowledge cultural diversity; indeed in the pre-imperial period it must take diversity as its point of departure. The ongoing construction of "Chineseness" is a phenomenon we cannot come to grips with until we desist from using the word "Chinese" unreflectively (p. 255-56).

The content of this "construction of Chineseness" is not specified, but it cannot just mean my book because it is identified here as an ongoing phenomenon. Based on ideas proclaimed in the review, this "phenomenon" would include all scholarly efforts to study the origin, formation, and early development of Chinese culture during prehistorical and early historical periods, as well as all scholarly practices of affiliating prehistorical and historical regional cultures with Chinese culture based on non-linguistic evidence. According to Bagley, the term "Chinese" should definitely be disassociated from all prehistorical cultures:

We cannot sensibly apply the same label "Chinese" to the people of archaeological cultures as distinct as Liangzhu, Dawenkou, Hongshan, Longshan, Shiliangtou, Majiayao, and Mairidigou, to mention only a few. To equate all these prehistorical cultures with the Chinese culture of some later period (Anyang or Zhou or Han) is to equate them with one another, in flagrant disregard of everything that archaeology has told us about them (p. 232).

This statement is another instance of making an assertion based on a distortion: Who "equates" prehistorical cultures with later Chinese cultures? Does a study of the origin of Chinese civilization automatically "equate" prehistorical cultures with later Chinese cultures? But Bagley is not just talking about prehistorical cultures; in his opinion the bronze cultures in Hunan, Jiangxi, Jiangsu, Anhui, Sichuan, and northern Shanxi during the late Shang should also not be considered part of an early Chinese civilization because of the lack of linguistic evidence: "Though it would be defensible to call the late second-millennium inhabitants of Anyang 'Chinese' on the strength of their language, we do not know which or how many of their neighbors spoke the same language" (p. 236). In fact, in this and other places Bagley takes language to be the only criterion for Chinese culture. Criticizing my inclusion of some East Coastal prehistorical cultures in a study of early Chinese art, he writes:

For the Neolithic, this amounts to assuming that the prehistoric inhabitants of both the lower Yangzi region and the Shandong peninsula were Chinese. But what exactly does this mean? It cannot mean that they spoke Chinese, for we do not know what languages they spoke. If it means only that their material culture has some visible connection with the material culture of the Chinese speakers of historical times, then it does not mean much: a dozen other prehistoric peoples with very little in common have at least equal (and equally small) claims to be Chinese in this sense (pp. 231-32).

It is surprising to hear such words from a writer who in other places in the review insists on the primary significance of archaeology in studying early China. Based on archaeological evidence, scholars in the East and the West have undertaken serious research on the interrelationship between late Neolithic cultures and on their connections with Shang-Zhou cultures. One of their major achievements concerns a large cultural complex that developed mainly along China's east coast during the late Neolithic period. Multiple criteria have been taken into account in defining this cultural complex. For K. C. Chang, these criteria include settlement and human occupation; geographical expansion and interaction; domestication of animals; form and manufacture of pottery vessels; typology and function of tools and weapons; architectural style and construction technique; burial practice and social stratification; jade artifacts and status differentiation; and evidence indicating organized religious practices and specialization in material production. For David Keightley, these criteria include sedentary agriculture; burial custom; mortuary ritual; male dominance; a particular theology of the afterlife; status distinction; sacrifices; architecture; design, technology and function of pottery vessels; jade carving; and "some system of notation, or proto-writing." Keightley concludes:

Strong affiliations, which link the Shang culturally to their East Coast antecedents, confirm the degree to which certain roots of later Chinese culture, to say nothing of social and political organization and technical skills, are to be found in the religion, craft, and mentality of the prehistoric cultures of the East Coast where, as we have seen, the archaeological record indicates a developing emphasis on models, mensuration, control, planning, manipulation, specialization, efficiency, and ritual, all of which served to enhance and concentrate, as they ordered and constrained, the productive and organizational capacities of the society.

A similar approach has determined the scope of my discussion of Neolithic art. Rather than including all
Neolithic cultures found within the territory of present-day China, I focus on a number of east coastal cultures that demonstrate what seem to me the strongest filiations with Chinese art of the early historical period. These filiations are found, among other things, in the typology, material, and craftsmanship of objects, in the function and burial context of objects, and in their surface carvings, including decoration and emblem. This regional focus is therefore based on an awareness of cultural diversity. I have indeed not tried (and am also not qualified) to decide whether the people of these cultures spoke Chinese and could thus (according to Bagley) be called Chinese. But even without linguistic evidence, for anyone to say that all evidence found in material culture “does not mean much” is not only untrue but is intellectually irresponsible.

4. Earlier I pointed out Bagley’s misrepresentations of my use of texts: my qualification of some “late” texts is ignored; my references to earlier texts are concealed; and the “three ritual books” are exaggerated to be the “principal sources” for the book’s first two chapters (p. 232). I have also noted how he links textual evidence with cultural identity: to him, texts give an author “the authority of the cultural insider” (p. 233), and my book is said to follow traditional Chinese scholarship, whose “tiresomely familiar” reliance on texts has been “thoroughly discredited in the age of scientific archaeology” (p. 235). On a methodological level, Bagley conceives texts only in terms of their role as direct historical evidence. According to him, since “we have no reason to suppose that third-century [should be “third-century BCE”—WH] authors knew anything about the prehistoric inhabitants of the lower Yangzi region” (p. 234), the three ritual books can only provide “anachronistic” evidence for any kind of interpretation of prehistorical and early historical art. His conclusion is thus: “All that we will ever know about the Liangzhu culture must come from archaeology” (p. 234).

This claim may be made so emphatically only if one believes that archaeological knowledge is free from disciplinary discourses. But Bagley’s disagreement with other archaeologists makes it clear that his understanding of early China is inseparable from his specific research methods and interpretative agenda. In other words, what we “know” about the Liangzhu, or what we think we know about the Liangzhu, is not just informed by archaeological “finds,” but is also determined by how these finds are discovered, observed, recorded, classified, and explained. All such treatment of archaeological materials, including Bagley’s treatment, is conditioned by the later development of human culture. This basic understanding of archaeological knowledge requires us to reflect upon disciplinary discourses. The idea is that although we cannot escape our own time, we should become conscious of our own premises, concepts, and approaches in studying ancient art, and should also investigate other premises, concepts and approaches, both past and present. While to discover a bygone discourse on art is itself a historical or historiographical practice, the discovered discourse may provide important perspectives for understanding actual works of art, especially when this discourse is closer than ours to the works under study. Here I use the term “discourse” for a body of texts “dealing with a non-literary and apparently non-textual practice.” Griselda Pollock thus considers modern art history a particular historical discourse on art.11 The “three ritual books” interest me precisely because they contain a different historical discourse on art. The initial section of chapter one in my book, “The Concept of ‘Liqi’,” aims to elucidate this discourse. From the second section I shift my focus from texts to artifacts and from discourse to practice, deriving evidence from archaeological excavations to reconstruct the development of liqi.

Bagley has little sympathy with this effort to uncover a historical discourse. In fact he simply refuses to understand this effort. The mention of the ritual texts alone is enough for him to identify my study with a “discredited” scholarly tradition.12 The same refusal also underlies his criticism of chapter three on Chang’an. In this chapter I first discuss two retrospective views of Chang’an as exemplified by the Eastern Han authors Ban Gu and Zhang Heng, and then examine the city’s actual construction as recorded in historical documents and revealed by archaeological excavations. This bipartite structure is based on my belief that any writing on Chang’an must imply a particular point of view and reflect a particular mode of historical imagination; and it is important for a modern historian to be aware of the view he or she adopts. I thus write at the beginning of the chapter:

Like previous reconstruction plans, the present one is not free from cultural and intellectual influences. But I hope that as a latecomer, I will have the privilege of pondering the premises and goals of earlier works, the various factors that have shaped an author’s thinking, and the traditions and conventions that have both encouraged and restrained an attempt to depict the historical change (p. 144).

To Bagley, however, such a historical reflection is entirely unnecessary: “The excursus on Ban Gu and Zhang Heng is irrelevant to the chapter’s description of Han Chang’an, which merely surrounds a series of imperial building projects with a rambling narrative derived mainly from Shi ji and the dynastic histories” (p. 245).13

Restricting the role of texts to direct evidence for historical facts, Bagley makes a complex scholarly practice extremely simple: only contemporary records can be used; and this means all received texts on Shang and early Zhou, not to mention Xia and prehistory, can largely be ruled out.14 Still, this would be a defensible position if Bagley is prepared to use texts after all. A lingering question throughout the review is Bagley’s real view of texts—not
just those “anachronistic” and “unreliable” texts but the
general role of texts in studying early China. Though by
no means specific, an answer to this question is finally
offered at the end of the review:

A study of ancient China should be informed by archaeology: it
should be alive to the fact that there existed a past which Han
writers did not know and could not record. The key to understanding
ancient China is not to be obtained by reinterpreting a few lines
from the Shi jing or the Zuo zhuan; ancient China is too big and too
complex to be unlocked with a skeleton key. The perfectly legitimate
wish to put artifacts in context must not be used as a subterfuge for
forcing archaeology into the mold of traditional historiography. It is
idle to pretend that the mental world of Han writers can accom-
modate the societies and artifacts that have been revealed to us by
prehistorical archaeology (p. 256).

Should we understand this statement as a rejection of all
Chinese texts by “Han writers” in studying early China? Should we
take it as a serious proposal or just another example of caricaturing? Who has said that ancient China
can be understood “by interpreting a few lines from the
Shi jing or the Zuo zhuan”? And who has tried to unlock
ancient China “with a skeleton key”? Such exaggeration
and distortion of different scholarly positions serve to
humiliate and intimidate, not to facilitate a genuine debate
based on reasoning.

5. Knowing Bagley’s self-confessed suspicion toward
“scholars of an anthropological bent” and his consistent
refusal of any “symbolic meaning” in bronze decoration,15
I was not surprised that he would disagree with my inter-
pretation of early Chinese art, which is to a large extent
about the meaning of this art in its broad social and reli-
gious contexts. But I was truly shocked by the degree of
animosity in his review. It shows little sense of respect for
another scholar and his work; instead it is filled with con-
descending judgements and sarcastic remarks: “The con-
nection Wu proposes between costly non-utilitarian
objects and daily utensils and between art and crafts is
telling” (p. 245); the discussion of the Nine Tripods is “an
instance of sustained and bewildering self-contradiction”
(pp. 252–53) and proves that the author “does not know
what he means” (p. 253); the distinction between ritual
objects and daily utensils and between art and crafts is
“absurd” (p. 235). The best words said about the book in
this 35-page-long review concern my account of the
development of Neolithic jades and pottery; an account, in
Bagley’s opinion, which “would have to be judged too
rigid a scheme to be useful, though not wholly wrong”
(p. 228).

Two things distinguish a critical review from an attack:
the first is an objective attitude in representing the author’s
approach; the second is a clear statement of the reviewer’s
own position. Bagley follows neither guideline. In terms of
representing the author’s approach, he is more interested
in finding what is not in the book than what is in it; the
absence of a perspective is taken as evidence for the
author’s deliberate rejection of this perspective. Thus, if the
book is not about regional variation, then the author must be
hostile to a study of cultural diversity. If the book does
not focus on individual tombs, then the author must be
blind to “complex realities” (p. 242). Many allegations in
the review are based on this logic, and are made by isolat-
ing the book from all other writings by the same author,
which deal with precisely these aspects of early Chinese
art.16 It is my belief, however, that a book or article can
and should have a specific goal, and this goal can and
should determine a specific frame of observation and level
of discussion. Depending on the author’s objective, a study
of early Chinese art can be an investigation of an indivi-
dual building or object, can be a comparison between con-
temporary regional cultures and art traditions, and can also
be a broad narrative of a general artistic development. In
my view, all these studies are important for our under-
standing of Chinese art and should not be held to be
mutually exclusive. On a very general level of comparison
an author may even focus on transcultural patterns of
human behavior and experience; but such a focus goes
beyond a study of early Chinese art.

Although Bagley’s review is long and detailed, it does
not contain a clear statement of his own critical position,
especially in terms of methodology. Certain scholarly
practices—archaeology, cross-cultural comparison, and the
study of cultural diversity—are repeatedly evoked as posi-
tive directions of scholarly endeavor; but each practice is
so general and vague that it scarcely indicates any defin-
able direction. At the end of the review he summarizes
these practices into a series of “intellectual responsibilities
of an author writing about the art of ancient China,”
including a “manageable objective,” a focus on cultural
diversity, reliance on archaeology, and an open attitude
towards other traditions (pp. 255–56). But how can one
understand these “responsibilities”? Is there a common
standard for a “manageable objective”? What is the brand
of archaeology Bagley is referring to? Are there various
levels in studying cultural diversity? How to compare
Chinese art with other traditions? One cannot find
answers to these questions either in this conclusion or in
the whole review. Without even a minimal explanation,
these large concepts and strategies constitute an opaque
facade, denying rather than providing an entrance. Indeed,
these “intellectual responsibilities” only gain a certain
measure of substance when they are understood as ges-
tures of negativity. As Bagley states unmistakably, to study
cultural diversity means to get away from “the ongoing
construction of ‘Chineseness’”; to rely on archaeology
means to get away from texts. The question then becomes:
can such negativity guide the development of the field?
Should methodological reflections be turned into academic politics? Shouldn’t different scholarly traditions and their contributions be properly recognized? Shouldn’t we encourage collaboration between scholars of diverse backgrounds and research interests?

Bagley does not conceal his negative answer to these last two questions, because in his view Chinese scholars in general represent a backward, nonscientific tradition that needs to be abandoned. He has expressed this view most clearly in the newly published Cambridge History of Ancient China, in which he concludes his chapter on Shang archaeology quite pointedly with a criticism of what he calls collectively “Chinese archaeologists.” For these scholars, in his view, “the always thorny problem of interpreting mute archaeological evidence has been complicated by national pride, which insists that tradition is reliable and that the task of archaeology is to vindicate it. Searching always for correspondence with the written record, inclined to overlook or explain away evidence that conflicts with it, archaeology sadly misses its chance of giving us an independent view of the second millennium.”

By inventing the fiction of a unified ideological and political body of “Chinese archaeologists,” Bagley posits himself as a representative of a fictional front of “Western archaeologists.” It is commonplace knowledge that multiple approaches and identities exist in today’s archaeology practiced in the West. It is also commonplace knowledge that Chinese historical scholarship and practices have gone through fundamental changes from the beginning of this century, especially after the Cultural Revolution. Traditional historiography lost its grip during the May Fourth Movement; field archaeology was introduced to China in the 1920s and has produced much new evidence for rethinking China’s past; many Chinese students have studied abroad under Western professors, and have in turn trained Western students, some of whom have become established scholars; many collaborative projects between Chinese and foreign scholars have increasingly blurred national boundaries. To ignore the progress and complexity of Chinese historical scholarship in the twentieth century is, to borrow Bagley’s own words, “either chauvinism or a cloak for ignorance.” Ironically, Bagley’s mental framework of confining intellectuals within political or racial boundaries is shared by a conservative faction in Chinese academic circles. But the increasing transcendence of national boundaries in intellectual pursuits is not only an ongoing phenomenon but also represents the future of the field of early China studies.

Notes

1. Such a critique can be found, for example, in The Wu Liang Shrine: The Ideology of Early Chinese Patinated Art (Stanford, 1989), pp. 50–56. My discussion of Shang and Western Zhou bronze art in Monumentality also tries to depart from a strict evolutionary pattern of decorative styles (pp. 48–63). It should be noted that an empirical study of archaeological evidence serves as the basis for my observation that both Neolithic jade art and Shang–Western Zhou bronze art developed from imitating existing forms in fine materials to further elaborating these forms with surface decoration and inscription. Although these two developments show certain parallels, I never attribute them to “the unfolding of an abstraction.” It should also be noted that rather than reflecting the belief in a universal developmental logic, the broad scope of chapter one is partly determined by available research materials. This idea is stated in the introduction: “Chapter I on Li ji tries to reconstruct the history of ritual art based primarily on its intrinsic visual properties. I select the mode of a broad historical narrative partly because of the lack of detailed information about artisans, workshops, and patrons, and about the design and manufacture of specific objects” (p. 15).


3. Scholars generally agree that Zhou li and Yi li were written during the late Eastern Zhou, and Li ji was compiled during the Han. But chapters of Li ji found recently in Eastern Zhou tombs have provided new evidence for an earlier date of at least some part of this text.

4. At least one reviewer has commented on this feature of the book. Charles Lachman writes in The Journal of Asian Studies, vol. 56, no. 1 (February 1997), p. 196: “It is worth noting that Wu Hung . . . generously credits the research of others, often when no citation would be strictly required, and manages to avoid contentiousness even when disagreeing with earlier opinions.”


6. In his article “The Chinese Bronze Age: A Modern Synthesis,” K. C. Chang first summarizes Gordon Childe’s discussion of the Bronze Age, indicating that for Childe, the Bronze Age means much more than just a technological stage. He then asks: “If, as we have noted, the Chinese Bronze Age essentially coincided in its commencement and its termination with a stage of Chinese social and cultural history characterized by other criteria, how can this be explained? What is in the bronzes that seems to make them the symbol of an age? Can our answers to these questions reinforce the conclusions reached by Childe on the basis of Near Eastern and European data or, perhaps, can they add to them some still newer insights?” These questions then lead Chang to examine some essential features of Chinese Bronze Age civilization. In Wen Fong, ed., The Great Bronze Age of China (New York: Metropolitan Museum of Art, 1980), p. 36.

When he took his first post in 1946, many of his colleagues were learned specialists. Though he himself lacked modernist analysis and includes many text-based studies. It should also be noted that Bagley makes another spurious allegation at this point. Commenting on my observation that "in such an analysis the researcher, either consciously or unconsciously, equates the Chinese example with post-Renaissance painting that employs linear perspective as the most powerful means to create pictorial illusions," he writes: "But the one researcher he cites, Alexander Soper, writing fifty years ago, says nothing to justify this criticism." (p. 249). Probably Bagley did not bother to read or re-read Soper's article before making this allegation.


12. Although in this review Bagley takes a strong stand against using Eastern Zhou texts in making arguments about earlier art, he does exactly this when he tries to support his own argument that Shang bronze decoration has no symbolic meaning.

Generation after generation, scholars have racked their brains over one or two cryptic occurrences of the word taotie in Eastern Zhou texts, hoping to find in a few obscure phrases the key to the symbolism of the bronzes. The texts tell us that a marquis was entitled to a certain number of ding and gui vessels, a duke was entitled to a different number, and so on; what the texts don't tell us is that a marquis used this kind of decoration, a duke used that kind. Eastern Zhou bronzes were symbols but there is little reason to believe that their symbolism resided in their decoration. Perhaps the same is true of Shang. The ceremonial presentation of sacrifices may simply have required an art of lavish and awesome display" ("Meaning and Explanation," in The Problem of Meaning in Early Chinese Ritual Bronzes, Colloquies on Art & Archaeology in Asia No. 15, ed. Roderick Whitfield [London: Percival David Foundation of Chinese Art, 1993], p. 45.)

Bagley is not even reluctant to appeal to modern experience as long as it accords to his need: "We seldom feel a pressing need to find meanings in the decoration of our own tradition, the decoration we see in use all around us; few of us would think of asking what the acanthus leaves of a Corinthian capital mean. We should remember that Shang decoration seemed familiar and natural to Shang." Shang Ritual Bronzes in the Arthur M. Sackler Collections (Washington D.C. and Cambridge, Mass.: The Arthur M. Sackler Foundation and the Arthur M. Sackler Museum, Harvard University, 1987), p. 50.

13. As in many other places in the review, here Bagley's allegation is based on distorting my evidence. The fact is that in writing this chapter I tried to utilize all published archaeological data concerning Western Han Chang'an that I could find. It would be more honest for Bagley to acknowledge this effort and to specify what archaeological material is overlooked in my study.

14. It is a common assumption that texts compiled during the Eastern Zhou and Han dynasties sometimes contain "fragments" of earlier beliefs. But to identify such fragments is a delicate task and requires certain research methods. For a discussion related to this problem, see my paper, "Bird Motifs in Eastern Yi Art," Orientations, vol. 16, no. 10 (October 1985), p. 41.

15. To Bagley, "The notion that art devoted to religion must involve a religious symbolism is certainly false." Those who hold this false assumption are "scholars of an anthropological bent," for whom "no account of the bronze decoration can be satisfactory unless it makes the decoration a link between the bronze vessels and the society they served" (Shang Ritual Bronzes in the Arthur M. Sackler Collections, p. 50).


Henry Trubner (1920-1999)

The career of Henry Trubner, who died suddenly in Seattle at age seventy-nine, spanned a time of great change among curators of Asian art in Europe and America. When he took his first post in 1946, many of his colleagues were patrician amateurs; when he retired in 1987, most were learned specialists. Though he himself lacked advanced Asian language skills, he recognized the primacy of empirical research methods in his own special field of Chinese and Japanese ceramics, and he worked closely with skilled technicians. In 1988 the Idemitsu Museum bestowed on him the Koyama Fujio Medal for distinguished contributions to the history of Asian ceramics; he was the first non-Japanese to so honored.

Henry Trubner was born in Munich, Germany, on June 10, 1920, under circumstances that shaped his future career. His grandfather, Wilhelm Trübner, (1851-1917), was a well-known painter in a style derived in part from Gustave Courbet; Wilhelm later became an art teacher and school administrator in Frankfurt and Karlsruhe. Jörg Trübner...