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The Coming of Age of the History of Archaeology

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Publications and organizational developments relating to the history of archaeology from 1989 until June 1993 are critically examined. Attention is paid to the changing motivations for producing such publications, their shifting intellectual orientation, controversies, especially as they relate to internal vs external approaches and the epistemological status of explanations, problems of verification, and the status of these studies as a subfield within archaeology.

KEY WORDS: archaeology; history of archaeology; history of science; postmodernism.

INTRODUCTION

This paper surveys the study of the history of archaeology since I completed *A History of Archaeological Thought* (Trigger, 1989). My coverage is focused mainly on works that were conceived as contributions to intellectual or scientific history rather than as chronicles of archaeological discoveries for the general public. This period has witnessed a remarkable upsurge of interest in the history of archaeology and, at least in the United States, the beginning of the professional organization of such research. Nor is such interest limited to Europe and North America. Dilip Chakrabarti (1988) has published a history of archaeology in India prior to 1947, Peter Robertshaw (1990) a major collection of papers dealing with the history of African archaeology, and David Horton (1991) a survey of the history of Australian archaeology. Tim Murray is dragooning contributors around the world for two megaprojects: an encyclopedia of the history of archaeology and a biographical encyclopedia of leading archaeologists. At the same time, Paul Bahn is editing a *Cambridge Illustrated History of Archaeology,*

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and the history of archaeology is being accorded its own heading in the New Books section of *Antiquity.* The history of archaeology is also winning unprecedented recognition as a field of research that, rather than being of peripheral interest, is essential for the successful practice of archaeology.

WHY A HISTORY OF ARCHAEOLOGY?

A major question concerns the reasons for this rising interest. It is frequently suggested that it is one more manifestation of a postmodernist emphasis on self-reflection, subjectivity, and relativity. This is seen as negating processual archaeologists' antihistorical tendency and positivism's refusal to accord history a significant role as a basis for scientific analysis. While there is some truth in this argument, it is not the whole story. There has been a scholarly interest in the history of archaeology since Stanley Casson published his *The Discovery of Man* in 1939. In 1950, Glyn Daniel published *A Hundred Years of Archaeology,* Stuart Piggott his biography *William Stukeley,* and T. D. Kendrick his *British Antiquity.* A small number of works dealing with the history of archaeology were published in the 1950s and 1960s, but starting in the 1970s, the rate of publication began to grow at an ever faster rate, until today it is difficult for a scholar to keep up with everything that is being published in this field. Throughout this period practicing archaeologists have continued to record their own activities, recollections of colleagues (often in the form of obituaries), and sentimental or polemical accounts of enterprises of which they have been a part. While such activities constitute a recording of historical data rather than the study of history, they are of great importance for the history of a discipline for which it is estimated that 90% of all its practitioners who ever lived are probably alive today (Christenson, 1989, p. 163).

More ambitious efforts to chronicle the history of archaeology have sought to justify particular approaches and expose the weaknesses of rival ones. Stanley Casson, in addition to establishing that an intellectual history of prehistoric archaeology was possible, provided a belated justification for an already moribund evolutionary approach. Glyn Daniel, while arguing that the "final truth" of any one period breaks down as new evidence accumulates and new explanations are developed, took great pains to demonstrate the superiority of a culture-historical approach over an evolutionary one. In the first edition of *A History of American Archaeology,* Gordon Willey and Jeremy Sabloff (1974) implied that their four successive periods (Speculative, Classificatory-Descriptive, Classificatory-Historical, Explanatory) constituted a logical and largely inevitable development for archaeology. In this way they sought to legitimate processual archaeology historically, while at the

same time criticizing what they regarded as the excesses of the movement. In the third edition of their book (1993), they continue to celebrate the accomplishments of a "mainstream" processual archaeology, which they view as expanding to incorporate a postprocessual concern with mental and ideological as well as with material phenomena. In *A History of Archaeological Thought* (1989), I too praised an apparent synthesis of processual and postprocessual trends but accorded a more transformatory role to some postprocessual developments than Willey and Sabloff do. In addition to its polemical uses, the history of archaeology has long been ascribed didactic value as an efficient means for transmitting the basic concepts of the discipline to graduate students. This suggests that the history of archaeology is of interest not only to postprocessual archaeologists, who are preoccupied with subjectivism, but also to positivists and empiricists.

It was not accidentally, however, that the history of archaeology reached the nadir of its theoretical significance with Michael Schiffer's (1976, p. 193) pronouncement that "graduate courses should cease being histories of thought" and should instead concentrate on communicating the established principles of the discipline and indicating future lines of inquiry. If carefully formulated techniques of analysis and an expanding corpus of data can produce increasingly accurate approximations of the past, the history of archaeology is irrelevant to the discipline's present or future practice. This position seems to have been tacitly shared by most processual archaeologists. Postmodernism, with its emphasis on subjectivism, has reenhanced the value of studying the history of archaeology by encouraging the belief that archaeological practice cannot be explained without taking account of the social context in which it occurs. Both consciously and unconsciously, the questions that archaeologists investigate, the evidence they are predisposed to accept as conclusive, and even what they recognize as evidence are influenced by their intellectual persuasions, class interests, ethnic loyalties, and gender prejudices. Because of this, archaeology does not constitute itself as a discipline independent of the social context in which it is practiced.

These ideas are not new. The classical archaeologist and idealist philosopher, Robin Collingwood, argued in 1939 (p. 132) that "no historical problem should be studied without studying . . . the history of historical thought about it." His ideas in turn influenced Daniel's and Piggott's approach to studying the history of archaeology. They stressed the impact of major Western European intellectual fashions, such as rationalism and romanticism, on the development of archaeology. Similar ideas were later applied by Ole Klindt-Jensen (1975) in *A History of Scandinavian Archaeology*, Ignacio Bernal (1980) in *A History of Mexican Archaeology*, and Karol Sklenář (1983) in *Archaeology in Central Europe*. Bernal's book was strongly influenced by Benjamin Keen's (1971) *The Aztec Image in Western Thought*, a monumental contribution to the study of intellectual history.

The postmodernist agenda laid greater emphasis on specific social and economic interests as major factors influencing archaeological interpretation. While this approach was already manifested in Robert Silverberg's (1968) *Mound Builders of Ancient America,* it was greatly encouraged and disseminated as a result of the first World Archaeological Congress, held in Southampton, England, in 1986 (Ucko, 1987). This meeting encouraged the production of numerous historical studies that seek to reveal the social and economic biases inherent in specific interpretations of archaeological data. While these studies, which are closely aligned with postprocessualism and subjectivism, claim a greater importance for understanding the history of archaeology than a more self-contained positivist view of behavior does, they do not preclude a positivistic approach to the history of archaeology. On the contrary, they may be encouraging by way of a reaction the production of more positivist histories of archaeology.

ORGANIZATIONAL DEVELOPMENT

During the last 5 years the organizational development of the history of archaeology has also taken several major steps forward, at least in the United States. In the 1970s, the promotion of such research was personally encouraged by Glyn Daniel's inviting senior archaeologists to write national histories of archaeology for his "The World of Archaeology" series. In 1978, under the auspices of the International Union of Pre- and Protohistoric Sciences, he and Ole Klindt-Jensen organized an international conference on the history of archaeology at the University of Aarhus in Denmark. The papers presented at this conference were subsequently edited by Glyn Daniel (1981). While these isolated efforts stimulated the study of the history of archaeology in various parts of the world, they did not provide sustained support for such research.

This changed in 1987, at least in the United States, when a Committee on the History of Archaeology was established within the Society for American Archaeology. It was charged with identifying, preserving, and making more accessible documentary materials pertaining to the history of archaeology, as well as with promoting an interest in the history of the discipline (King, 1992). A second development is the biannual *Bulletin of the History of Archaeology,* edited by Douglas Givens, which has been appearing regularly since May 1991. It publishes editorials, short articles, bibliographies, book reviews, and notices of activities and events relevant to the history of archaeology. It also seeks to cover material on a worldwide basis. In a brief period it has established itself as an indispensable research tool for everyone interested in this subject.

Finally, beginning in 1988, a series of annual symposia on the history of archaeology was initiated, which now alternates between the annual meeting of the American Anthropological Association and that of the Society for American Archaeology. Papers from the first two of these symposia have been published in *Rediscovering the Past,* edited by Jonathan Reyman (1992a). The difficulties experienced in finding a publisher, however, suggested that scholarly presses still do not perceive a significant market for volumes of essays about the history of archaeology.

METHODOLOGICAL ISSUES

This flurry of activity stemmed from a 2-day conference on the method and theory of the history of archaeology that was organized by Andrew Christenson at Southern Illinois University in May 1987. The proceedings of this conference, which Christenson (1989a) edited as *Tracing Archaeology's Past,* constitute the first book that examines problems related to studying the history of archaeology. Some of these papers consider the reasons for studying the history of archaeology and the relative merits of internalist and externalist approaches. Others examine the importance of manuscript sources as opposed to published works, oral histories, and old films, speeches, illustrations, artifact collections, and institutional records as relatively untapped sources of information about the history of archaeology. This book was a milestone that both reflected and contributed to a new self-awareness of the history of archaeology. Other examples of a growing concern with methodology are Givens' (1992a) essay on "The Role of Biography in Writing the History of Archaeology" and his (1992b) biography of Alfred Kidder, which is based to a large extent on archival materials and interviews.

While a large number of biographies of archaeologists have been published by professional biographers, historians, and archaeologists in recent years (at least three dealing with Howard Carter, who discovered the tomb of King Tutankhamen in 1922), few show much interest in relating their subjects to their intellectual milieu. Instead they are chronicles of discoveries and titillating accounts of the social (and, in the case of Sir Leonard Woolley, the sexual) life of previous generations. This is also true of the historian J. Thompson's (1992) account of the proto-Egyptologist John Wilkinson and his circle of friends, although Thompson made some effort to set Wilkinson's work into historical context. While it also constitutes a sort of autobiography (a rare genre among American archaeologists), Gordon Willey's (1988) *Portraits in American Archaeology* offers a series of short biographies of deceased archaeologists who influenced his career. This

book is an example of recollection practiced at the highest level and a major contribution to the history of archaeology. Pamela Smith (1993), in her thesis on Grahame Clark, has demonstrated the importance of understanding the early stages of an archaeologist's career, while Mark Bowden (1991) has set a new standard in interrelating the thought and fieldwork of an archaeologist in his biography of Pitt Rivers.

As long as few people studied the history of archaeology, they tended to work in isolation. Today a growing number of researchers are generating healthy debates about both facts and theoretical orientations. The most important of these debates center at least in part on the strengths and weaknesses of internalist and externalist approaches. Internalist approaches concentrate on delineating the changing understanding of a particular problem within archaeology, while externalist approaches focus on the relationship between archaeological understanding and the sociocultural context in which archaeology is practiced (Welter, 1965). One of the best examples of an internalist history produced by an archaeologist is Donald Grayson's (1983) *The Establishment of Human Antiquity,* and one of the best externalist ones is David Meltzer's (1983) "The Antiquity of Man and the Development of American Archaeology." While historians used to believe that only one of these approaches could be applied at a time, it is now generally acknowledged that these approaches are complementary rather than antithetical and that a rounded explanation requires both. Yet the stricter processual archaeologists, who adhere to a positivist epistemology, accord little, if any, importance to an externalist approach. For them the development of archaeology is shaped primarily by the scientific analysis of a growing body of archaeological data. The externalists, in contrast, range from those who see all scientific discourse contaminated by sociocultural preconceptions, but who nevertheless believe that the constraints of data play a major role in shaping the development of archaeology, to the extreme idealists who are prepared to argue that, because no archaeological data exist independently of cultural preconceptions, they exert no restraint whatever on the development of archaeology. In a period preoccupied with discourse, sectarian hackles can be raised by such an apparently simple matter as a reference to "archaeologies" rather than "archaeology" (Kehoe, 1992a, p. 20).

The internalist/externalist dichotomy is also problematical in other ways. As we shall see, there is more than one kind of exteriority: approaches connecting archaeology to trends in intellectual history eliciting very different responses from those that connect it to the economic, social, and political milieu in which archaeology is practiced. There are also, as Thomas Kuhn (1962) recognized, personal and social factors *within* disciplines that may influence their future independently of the social and intellectual milieu in which they are practiced.

Internalist studies continue to be produced, as it is right they should. The most broadly accessible is Michael Coe's (1992) exciting and informative, if partisan, history of the decipherment of the Maya script and its impact on the study of this ancient American civilization. Another internalist study of interest both to specialists and to general readers is the meticulous analysis by Erik Trinkaus and Pat Shipman (1993) of changing images of the nature of Neandertals. In the third edition of their *A History of American Archaeology,* Willey and Sabloff (1993) continue to stress the value of relating changes in archaeological practice to the changing intellectual climate; to theoretical developments in other fields, such as ecology, systems analysis, and art history; to new analytical techniques such as radiocarbon dating and computers; and to changing patterns of funding. But they also continue to regard the most crucial external influence on prehistoric archaeology as being ideas coming from ethnology and social anthropology. P. R. S. Moorey's (1991) *A Century of Biblical Archaeology* provides a welcome intellectual history of the troubled and bitterly divided but, at the same time, often hermetically sealed field of Old Testament archaeology. Ian Jenkin's (1992) *Archaeologists and Aesthetes* documents how the collecting and display policies of the ancient sculpture galleries of the British Museum were influenced between 1800 and 1939 by the gradual abandonment of the belief in fixed artistic values derived from Johann Winckelmann's scheme of the rise and fall of classical art and its replacement by a new, more relativistic approach. Finally, Douglas Charles (1992) has reformulated Stuart Piggott's view [inspired partly by art historians such as Christopher Hussey (1927) and Kenneth Clark (1928)] that the history of European archaeology has been shaped since the eighteenth century by a fluctuating loyalty to rationalist and romantic approaches. Such intellectual history involves an attempt to relate the practice of archaeology to intellectual fashions, which in turn may or may not be directly related to the social milieu in which they flourished.

While all of the above studies consider the impact on archaeology of ideas formulated in related disciplines, some internalist studies have academic relations as their primary focus. Donald McVicker (1992) and Valerie Pinsky (1992) have studied the relationship between prehistoric archaeology and Boasian anthropology. While both see an initial marginalization of archaeology resulting from Boas' disinterest in the discipline, Pinsky views this gap as narrowing as a result of the diversification of Boasian anthropology in the 1930s, while McVicker argues that archaeology remained a "backwater area" until the New Archaeology attempted to overthrow Boasian ethnography's hegemonic control in the 1960s. My personal experience of archaeology in the 1950s suggests that, while Pinsky may be correct in terms of programmatic statements, McVicker more

accurately describes the social reality of archaeology's role within Boasian anthropology. Stephen Dyson (1989) argues that the conservatism of classical archaeology has resulted in large part from the ability of a small number of senior researchers to control a large percentage of the discipline's research funds.

In recent years many papers have been published dealing with the role played by women in the development of American archaeology. Some of these papers were presented at a symposium on "Women in Archaeology" held at the annual meeting of the Society for American Archaeology in 1989 (Reyman, 1992a). Most of them have two goals: to demonstrate that women have played a significant role throughout the history of archaeology and to show that their contributions have been systematically undervalued by a male-dominated profession. Less attention has been paid, in historical studies, to the impact that this has had on the interpretation of archaeological data. The latter issue tends to be addressed in theoretically oriented studies, which at best have minor historical components. Historical studies of gender in archaeology are especially important and sensitive because they address issues that continue to affect the careers of both male and female archaeologists. This creates methodological problems. Reyman (1992b, pp. 72–73) suggests that some younger women are reluctant to address these issues, even from a historical perspective, because they fear retaliation by male colleagues. This invites the counterclaim that archaeologists who seek to use a historical approach to better the position of women in archaeology may be using such innuendos to make the position of female archaeologists seem worse than it actually is. To my knowledge, there have been no conscious efforts to construct a history of archaeology that would refute feminist claims. Apart from considerations of political correctness, it would be factually very difficult to argue such a case. In such circumstances, silence remains the most effective defense of an androcentric archaeology.

The more radical forms of externalism seek to demonstrate how archaeology has been shaped by the ethnic and class interests of those who practice or sponsor it. Thomas Patterson has carried into earlier periods his investigations of the impacts that alliances with the interests of a national and an internationally oriented bourgeoisie have had on the development of American archaeology. At the same time, he has extended Hinsley's (1985) study of the East coast establishment's preference for patronizing classical and biblical, rather than native American, archaeology during the nineteenth century, on the grounds that the former subjects were more morally and aesthetically uplifting than was the study of savagery (Patterson, 1986, 1991). Kehoe (1992b) has argued that, after the 1840s, state support was granted to archaeology in Western Europe and North America in return for its helping to legitimize the evolutionary claims on which modern industrial-capitalist

democracies were being founded. Neil Silberman (1989) has demonstrated how nationalist loyalties in the Balkans and Middle East have, in recent times, determined what questions are and are not investigated archaeologically. He demonstrates, for example, how Western archaeologists have systematically ignored archaeological evidence proving that Western European reorientations of world trade rather than Turkish occupation were responsible for the economic decline of Cyprus and Palestine in recent centuries. Early claims by Robert Silverberg (1968) and Trigger (1980) that White racist perceptions of American Indians shaped the development of American archaeology are now widely accepted.

This sort of approach has achieved new levels of respectability in relation to the history of science as a result of Adrian Desmond's (1982, 1989; Desmond and Moore, 1992) studies of paleontology and biological evolutionism in Victorian England. Yet it is this form of externalism that has received the severest criticism from more conservative archaeologists such as Colin Renfrew (Daniel and Renfrew, 1988, pp. 199–200; Renfrew, cited by Bradley, 1993, p. 81) and Willey and Sabloff (1993, p. 92). Renfrew, in particular, lumps together extreme externalists, such as Shanks and Tilley, who often deny that evidence influences scientific formulations to any degree, and more moderate externalists, who merely claim that social factors influence archaeological interpretation. Yet similar concerns have been expressed by sympathetic individuals such as Reyman (1992c, p. 245). These complaints collectively challenge externalists to clarify on what they base their conclusions.

Any form of historical interpretation, whether internalist or externalist, is, of necessity, an attempt to relive in our own minds what we imagine people did and believed in the past, with every decoding being literally another encoding. The methodology of the historian is, of necessity, an idealist one, regardless of what his or her general epistemological orientation may be. There is also not one history of archaeology, but many, depending in part on the differing assumptions that individual historians bring to their work. The success of historical analysis, from a scientific viewpoint, depends on the thoroughness with which data are mustered and on the historian's ability to contextualize these data. There is no way to prove that the past was precisely as the historian imagines it to have been. By their nature, externalist interpretations require more data and contextualization and hence are more difficult to handle in a convincing manner than are internal ones. There is, however, a large body of evidence that interpretations of archaeological data are influenced by the social milieu in which they are practiced. Hence the greater "objectivity" of internalist interpretations does not rule out the utility or value of externalist ones.

Yet I agree that externalists have an obligation to be more explicit than they generally have been in the past about the relations between social conditions and archaeological practice before they claim, for example, that Victorian archaeology served to legitimize bourgeois democratic governments. To what extent were such processes conscious or unconscious? How is it possible to document unconscious processes? Does archaeological interpretation reflect the personal beliefs of the archaeologist or the dominant ideology of the society in which the archaeologist worked? To what extent can a dominant ideology be questioned, resisted, or ignored? The study of problems relating to the sociology of knowledge has been a prerogative largely of Marxists such as György Lukács, Antonio Gramsci, and their latter-day followers, who are far from being in agreement with one another (Barnes, 1974, 1977).

Empirical evidence indicates that it is necessary to avoid the idea that there is ever a simple and all-embracing relationship between class or ethnicity and the way in which archaeological data are interpreted. In the nineteenth century, many conservative German classicists may have idealized the unique and *sui generis* qualities of ancient Greek civilization, as Martin Bernal (1987, pp. 281–316) has claimed. Yet other patriotic German classical archaeologists of that period interpreted the Mycenaean culture as being of northern derivation, while still other European prehistoric archaeologists, such as Oscar Montelius, derived the most important features of European material culture from the Near East. It would be very difficult to correlate all of the combinations and permutations of these alternative interpretations with specific class or ethnic interests. Likewise, the development of American archaeology during the nineteenth century was powerfully influenced by negative views concerning the American Indians' capacity for progress. Yet not all archaeologists shared the same views. Monogenists, polygenists, and Darwinists offered different explanations for Indian inferiority. White attitudes about Indians tended to be more romantic in New England than they were in the war zone west of the Mississippi. The model of American prehistory constructed by anthropologists at the Smithsonian Institution in the late nineteenth century can be explained only partly by the social attitudes of that period toward native people (Hinsley, 1981).

A wide variety of personal experiences, distinctive personalities, and specific interests may be as important as class differences in creating alternative interpretations of the archaeological record. One of the goals of Jean Paul Sartre's (1971–1972) detailed biography of the French novelist Gustave Flaubert was to demonstrate how individual factors, no less than class position, played a role in shaping an individual's outlook. The intellectual biographies of archaeologists reveal the heterogeneous origins of

the ideas that influenced them and the extraordinary ways in which these ideas were combined and transformed by individual archaeologists, who frequently tend to resemble Levi-Strauss' *bricoleurs* more than they do spokespersons for class positions. This is not to say that significant relations do not exist between class and ethnic interests and the interpretation of archaeological data; it is to acknowledge that these are not the only factors that influence it.

Finally, there is the question of the constraining influence that archaeological finds exert upon archaeological interpretation. What the archaeologist recovers was made by other people and once played a role in a systemic context different from the present. Even though the archaeologist decides what is worth recovering, classifies it, and ascribes meaning to it, the archaeological record constrains the archaeologist's imagination. In 1980, I argued not only that preconceptions about the American Indians had significantly influenced the study and interpretation of the archaeological record, but also that, over time, a growing body of archaeological data necessitated changes in that interpretation and in how Indians were regarded. I maintained that the adjustments were normally the minimum necessary to accommodate new findings and that racial prejudice was not substantially exorcised from the interpretation of archaeological findings before the advent of the New Archaeology in the 1960s.

It is often assumed that the most extravagant examples of politically motivated interpretations of archaeological data, such as those associated with the North American Moundbuilders and Great Zimbabwe, are evident today only because they are far removed from us in time. Alternatively, it is possible that such bizarre interpretations are possible only at an early stage in the study of the archaeological record of a particular region. Gertrude Caton Thompson was able to overturn interpretations of the Zimbabwe stone ruins, which attributed these sites to ancient colonists from the Middle East, by finding medieval glass beads and Chinese porcelain from the Sung dynasty associated with them. Yet her conclusion that the Zimbabwe stone ruins were of comparatively recent origin was accompanied by the observation that these buildings were perhaps not so elaborate as many earlier investigators had claimed (Kuklick, 1991, pp. 152–153). With proper methodology, an archaeologist does not always have to rise above prejudice in order to begin to overcome it.

In a study of changing theories of human evolution, I drew a parallel between scientific theories and natural languages and recalled linguists' claims that any idea can be expressed, albeit with varying degrees of difficulty, in any language. I further suggested that, provided that a diligent search is made for empirical evidence, "valid explanations eventually can be arrived at in spite of the manifold illusions and misconceptions that scientists must share

as members of functioning, and hence myth-ridden, cultures" (Trigger, 1967, p. 176). I stand by this conclusion, although I now believe that this process is even slower and more uncertain than I had once imagined. It is also important to note Gina Barnes' (1990) observations concerning the continuing influence of traditional concepts of time on the conceptualization of chronology in modern Japanese archaeology.

A superficially less controversial study is the impact that totalitarian regimes have exerted on the practice of archaeology. In recent years several papers have investigated the relations between archaeology and the state in Nazi Germany and Franco's Spain. Henry Cleere (1993) has edited a collection of short papers dealing with changes to archaeology in post-Soviet Albania, Bulgaria, Czechoslovakia, East Germany, Hungary, Poland, and the former USSR. Each of these papers provides useful information about the practice of archaeology in these countries prior to the collapse of Soviet power. It also appears that, with the disappearance of Soviet censorship, numerous secrets of Soviet archaeological politics are about to be revealed (Kolpakov and Vishnyatsky, 1990, p. 23).

Among substantive issues that divide historians of archaeology is the question of how much continuity there was between the American archaeology of the 1950s and the New Archaeology of the 1960s. I tend to emphasize continuity (Trigger, 1989, pp. 279–294), while Willey and Sabloff (1993, p. 306) and Pinsky (1992) stress discontinuity. Although these are issues that should be resolvable empirically, even the formulation of precise questions is difficult and leaves room for subjective bias. Alice Kehoe (1991) has identified Daniel Wilson as the real founder of midnineteenth-century evolutionary anthropology. Yet the original 1862 edition of *Prehistoric Man* suggests that George Stocking's (1987, pp. 73, 180) refusal to assign Wilson this role is solidly founded, even if this does not deny interest and importance to Wilson's work.

STATUS OF THE HISTORY OF ARCHAEOLOGY

The history of archaeology continues to be written mainly by archaeologists who are interested, for various reasons, in the history of their own discipline. These are professional archaeologists who also happen to be amateur historians. While theoretical debates in archaeology since the early 1960s have attracted the attention of philosophers of science such as Lester Embree, Merrilee Salmon, Wesley Salmon, Richard Watson, and Alison Wylie (Embree, 1992), only a few professional historians have become interested in archaeology. Most often they study the antiquarian background of archaeology rather than the more recent past, which tends to be of greater

interest to archaeologists. Recent examples include A. B. Ferguson's (1993) study of renaissance scholarship's assault on medieval accounts of British prehistory and S. A. E. Mendyk's (1989) detailed examination of regional studies and antiquarianism in England prior to 1700. These, along with earlier works by Piggott (1950), Hunter (1975), and Kendrick (1950), make English antiquarianism the best understood tradition of premodern archaeology in the world. R. T. Ridley's (1992) account of the classical excavations carried out in Rome during the Napoleonic occupation of that city provides the most detailed description of an archaeological research program prior to the nineteenth century. C. S. Hinsley's (1981, 1985) studies of nineteenth-century American archaeology are invariably excellent, but his interest has so far been focused mainly on anthropology as practiced in the context of major U.S. institutions, and archaeology is considered only insofar as it fits into this context.

For the most part, archaeologists have had to train themselves in historical methods. Most of the early and more general histories of archaeology were based on published sources. More specialized topics have, however, necessitated increasing archival research and the recording and evaluation of informants' recollections. Archaeologists, such as Grayson, Meltzer, and most recently Mark Bowden, in his biography of Pitt Rivers, have demonstrated high levels of proficiency in such research. Yet some other historians of archaeology fail to notice substantial changes between successive editions of the same book or assume that papers were necessarily reproduced in their entirety when they were reprinted in the last century. This casts a pall of amateurism over their work. Another indication of the limited development of the field is a tendency to reinvent concepts. This applies particularly to concepts that relate to the periodization and characterization of trends in archaeology (Chávez, 1992). These are problems encountered by all amateurs who attempt to write history. My first historical study, a hagiography of the early Canadian anthropologist Daniel Wilson that I wrote while a graduate student, now reads like a compendium of elementary historiographical errors, which I only gradually identified and hopefully have overcome (Trigger, 1966). This sort of naivete offers little resistance to natural tendencies for archaeologists to view the history of their discipline from presentistic and provincial viewpoints that the professional historian is trained to avoid.

Like ethnohistory, the history of any scientific discipline requires intimate familiarity with at least two separate fields. Substantive knowledge is needed of the science being investigated, together with knowledge of historical methodology and a detailed understanding of the history of Western thought and culture that has given rise to the science. Only rarely do individual scholars achieve equal proficiency in both fields. This is one

reason why much mutual benefit might be gained from more regular interaction between professional historians who are committed to studying the development of archaeology and archaeologists who are studying the history of their own discipline. While much has been achieved in researching the history of archaeology in recent years, I do not believe that the overall quality of publications relating to the history of archaeology has reached as high a level as that relating to the history of anthropology. That may be in part because fewer archaeologists have had a chance to benefit from working with demanding editors such as the historian of anthropology, George Stocking.

A final question that requires more attention is whether the history of archaeology is, or should be, emerging as a subfield of the discipline. Masters and doctoral dissertations dealing with the history of archaeology are now being written in archaeology and anthropology departments. Yet are archaeologists who have trained themselves to write history fully qualified to teach such techniques to students? Does such research require interdisciplinary training in both history and archaeology departments? Should such students receive a degree in history or anthropology?

Anthropological ethnohistorians, who are most often self-trained in historical methodology, assume that they have the right to train students. They have also frequently argued that historians who have not done ethnographic fieldwork are poorly equipped to study ethnohistory. With historians of archaeology the situation is somewhat different. Can students who have specialized in this subject throughout their graduate careers be regarded as adequately trained practicing archaeologists? And, if they are not so trained, can they legitimately be hired to teach in archaeology programs? This point is particularly important if such students cannot seek employment as professionally trained historians. Their only hope for legitimate employment is if the history of archaeology is recognized as a subfield of archaeology to which full-time teaching and research positions are assigned. Otherwise, it might be better if the history of archaeology continued to be something that archaeologists turned to as they became desk-bound later in their careers. There is strong evidence of a growing appreciation of the importance of studying the history of archaeology, but the terms of its professionalization remain to be established.

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