

## Historiography and Chronology

An extended commentary on Stephen E. Nash, editor. 2000. *It's About Time: A History of Archaeological Dating in North America*, University of Utah Press, Salt Lake City.

by

David L. Browman  
Washington University - Saint Louis

The *It's About Time* volume, which focuses upon the issues of historiography and chronology in North American archaeology, is the publication of the papers from a 1997 Society for American Archaeology symposium. The papers are for the most part on the development of various dating techniques (such as archaeomagnetism, dendrochronology, radiocarbon, or thermoluminescence) from an archaeological perspective, but there is one extremely thought-provoking critique by a social philosopher on how archaeologists “do” the history of archaeology, that for many readers may be the single most important paper in the volume.

### *The History of Dating Methods Papers:*

Part I of the book is composed of the Introduction (Ch. 1. The surprisingly deficient history of archaeochronology) by Stephen Nash and Jeffrey Dean. They take as their theme Fay-Cooper Cole's 1934 statement that “Chronology is the soul of archaeology”. Prior to 1914, they argue, Americanist archaeology focused on artifact classification, description, and typology. The major birth of Americanist interest in chronology thus developed in large part during the decade following 1914, with the chronological works, and calls to study chronology, by individuals such as Roland Dixon, Alfred Kidder, Alfred Kroeber, Berthold Laufer, Nels Nelson, Leslie Spier, and Clark Wissler. With respect to the current status of chronological issues, however, Nash and Dean argue that the disdain that many cultural-processualists and post-processualists exhibit for spatio-temporal relationships, have led “archaeologists in the last decade of the twentieth century [to] have less of an appreciation for the intricacies and nuances of chronology development” than did their cultural-historical predecessors.

Following this introductory chapter, the papers have been organized into three sections: Part II: Layers, Styles, and Rings — early approaches to archaeological dating (with 3 presentations), Part III: Radiation, magnetism, water, and light — later approaches (with 4 papers), and Part IV: Historical Records and narrative (with 2 contributions).

In the first of the three chapters in Part II on early dating methods (Ch. 2: Stratigraphy and archaeological dating), Julie Stein focuses primarily on “striking differences in the use of stratigraphy” (p. 18) by Americanist archaeologists who study people living in the late Pleistocene/early Holocene periods as contrasted to those who study the complex organization of people living in much more recent periods. She argues that the “shared beginnings aligned Paleolithic archaeology [and also Paleoindian -dlb] with geology and paleontology, and that there remains to this day a close association between archaeologists studying Paleolithic time periods and their geoscientific colleagues”, who “pay more attention to separating units based on physical descriptions of layers than those units based on animal remains, artifacts, and time.” (p. 21).

In contrast, Stein argues, Americanist archaeologists investigating complex societies followed a pathway different than the archaeologists who investigated Paleoindians because these researchers of more recent periods “could not use the methods of the archaeologists working on Paleoindians, who had extinct fauna and thick strata that could be correlated across large distances.” Rather, Stein argues, this second group of scholars focused particularly on the types of pottery found in each site, and that these archaeologists utilized culturally-manufactured objects to delimit the differences in artificially defined units in otherwise rather geologically homogeneous deposits. In essence, Stein argues, the difference between the two groups boils down to the use of ‘cultural’ fossils vs. ‘geological’ fossils. Thus she points out that the Harris matrices are nothing more than an efficient means to improve discovery of superposition of cultural fossils in geologically homogeneous strata, in spite of the fact that Harris so strongly denounces any attempt to borrow terminology from geostratigraphic studies (p. 33). The new focus on geoarchaeology, which considers site formation and context rather than merely superposition, “propels North American archaeologists closer to geoscientific stratigraphy and more elaborate concepts of stratigraphic dating.” (p. 39).

In the second of the three chapters on early dating methods (Ch. 3: the foundations, practice, and limitations of ceramic dating in the American Southwest), Eric Blinman argues that “nearly all developments in ceramic dating” have been applied in Southwestern archaeology (p. 41), so that the Southwest is an appropriate venue to use as the proxy to illustrate this topic. He focuses first on the use and definition of spatial variation in the ceramic collection analyses of Alfred Kidder, Alfred Kroeber, Nels Nelson, and Leslie Spier, to develop the first stage of ceramic chronology in the region. He explains the use of ‘sherd boards’ to define regional variations and types as a refinement of the initial seriation techniques.

For Blinman, the second major advance in ceramic dating was the ‘quantitative revolution’, which he ascribes to the 1945 publication of the Rainbow Bridge-Monument Valley Expedition report, which highlighted George Brainerd’s explicitly quantitative approach to ceramic classification. Thus other quantitative methods (such as mean ceramic dating, etc.) are merely extensions of this approach, he argues.

In the last portion of his chapter, Blinman discusses various problems, such as the considerations of sample size, mixing, and particularly ‘validation’ of inferred sequences. Blinman notes that in many of the modern statistical models, the assumption of randomness is not met, and that these quantification techniques are at risk of being misapplied to ceramic data. He also notes that limitations of using samples from a non-intense occupation (an occupation of a few years or less), which he believes are unlikely to leave a “residue of pottery that is representative of the ideal residential discard assemblage.” (p. 55) He concludes that while “under the best of conditions, both culturally (through rapid change) and archaeologically (through robust samples) claims of resolution down to 20-year spans are defensible”, that “under poorer conditions of either cultural context or sampling, claims of less than 200-year spans can be irresponsible.”

In the third of the three chapters on early dating methods (Ch. 4: Seven decades of archaeological tree-ring dating), Stephen Nash extracts part of the argument from his important book on the history of dendrochronology (1999, *Time, Trees, and Prehistory*). In the present chapter, he outlines a period of initial development of the method from 1914 to 1929, a period of blooming importance of dendrochronology from 1930 to 1914, followed by a period of reduced interest in the method following World War II owing to the introduction of radiocarbon age determinations, although he sees a slight resurgence in interest in the method in recent years because of the contributions of dendrochronology to climatological questions.

The first portion of Nash's chapter focuses on some of the players and the developments between 1914 and 1950. He notes that the first result of A. E. Douglass's initial composite chronology in 1929 was the required reduction in the time-scale previously proposed by archaeologists for South-western cultures, and the attendant interpretative changes regarding cultural interactions (p. 26). Nash believes that the importance of the technique was immediately recognized, citing as evidence the fact that "In January of 1930, less than two months after Douglass published his results, 15 students enrolled in his first course on tree-ring analysis at the University of Arizona" (p.64). Three of the students of this first class, Emil W. Haury, John C. McGregor, and W. Sidney Stallings, went on to make major contributions to the development of the method. A second important outcome of Douglass's 1929 report was the identification of a 'great drought' in the late 1200s (p. 80), which has since defined one major component of the arguments for the abandonment of the American Southwest in the 13th century.

The second part of Nash's chapter briefly summarizes some of the results of dendrochronological dating after 1950. Nash includes brief comments on Terah Smiley's and later Bryant Bannister's synthetic dating reports, the use of the work on the Navajo Land Claims and Wetherill Mesa Archaeological projects, and Jeffrey S. Dean's wood use behavior studies.

Part III of the volume is the four chapter section with reviews of radiocarbon dating, archaeomagnetic dating, obsidian hydration dating, and thermoluminescence dating. In the first of these four chapters (Ch. 5, The introduction to radiocarbon dating), R. E. Taylor focuses on two issues relevant to the first years of radiocarbon determinations: the technical development of the method, and the reaction of archaeologists to the initial application of the method.

In terms of the technical development, Taylor indicates that his review in this chapter is a distillation of the important publications by Ernest C. Anderson, James R. Arnold, and Willard F. Libby (as well as later biographers) on their work. Taylor speaks of a 'Chicago Period' of 1945 to 1954, when most of the important early work was done, and points out the critical technical advances which allowed the procedure to be implemented. The publication of the first 'date list' of 148 archaeological and geological samples in 1950, Taylor suggests, was a key issue in the decision to define A.D. 1950 as the 'zero' time from which all radiocarbon determinations are to be computed (Fn. 4, p. 212).

In terms of the initial reaction of archaeologists, Taylor begins with an attempt to understand the oft-reported initial reluctance of archaeologists to get involved with Libby and his proposed new dating method. Taylor, as have several others, suggests that this may be a mis-reporting of the initial reaction, but he adds a new twist, suggesting that the initial request for 1-2 pounds (his emphasis) of charcoal may have been off-putting, as it would have been exceedingly difficult for most archaeologists to provide samples of that size — first because museums would have been reluctant to part with that large of a portion of a known-age museum exhibit specimen for merely method-calibration purposes, and secondly because most field projects would not have recovered samples of unknown age in that size range. As part of his confirmation of this interpretation that archaeologists were in fact very interested in the method from the start, Taylor notes that in February of 1948, one month after Libby's first presentation to archaeologists of the prospective method at the Viking Fund 'banquet' in January 1948, that the Executive Board of the American Anthropological Association appointed a 'Committee on Radioactive Carbon 14' consisting initially of Donald Collier, Frederick Johnson, and Froelich Rainey, and in partnership with the Geological Society of America, added Richard Foster Flint for a new joint-organizational committee in March of 1948. By July of 1949, this joint AAA/GSA committee had named 10 'collaborators' who were to provide specimens for

specific regional sequences and problems, with an additional 4 collaborators added the next year. Thus the 'just-so-story' that has been repeated in some venues about the initial lack of interest on the part of archaeologists does not seem to be borne out by the facts.

Taylor sees three significant changes in archaeology resulting from radiocarbon dating. First he argues that the ability to have chronology serve as an independent variable allowed archaeologists to focus on other than time-space systematics, giving rise to the cultural-processual 'new archaeology'. Second he believes that the use of radiocarbon determinations had led archaeologists to greater statistical sophistication and more quantitative methods. And third he thinks that the need for good context for samples has resulted in vastly improved field recovery methods.

In the second of this four chapter section (Ch. 6, Thirty years of archaeomagnetic dating), Jeffrey L. Eighmy looks first at the history of development of archaeomagnetism in North America, and second on its integration into Americanist archaeology. This chapter and the remaining ones in this section lack the rich historic detail that Taylor provided on the radiocarbon technique, and thus are more 'thumbnail' sketches. In the first section Eighmy sketches out the initial work by geophysicist Robert DuBois between 1963 and 1967, the recruitment of Daniel Wolfman by DuBois in 1967, and the recruitment of Eighmy by Wolfman during the period of 1973 to 1977. Eighmy suggests that because DuBois overstated the accuracy of his results, that because he had an arrogant attitude toward archaeologists, and that because he failed to provide timely turn-around on samples, that the consumers of the method very quickly became disenchanted with the technique, hence providing, Eighmy feels, the real reasons for the slow adaptation of the method by Americanist archaeologists. Thus it was not until the standardization of reporting methods at a 1980 conference, and the first synthesis of American geomagnetic secular variation by Robert Sternberg in 1982, that Eighmy feels that archaeomagnetism in North America finally 'came of age'. Two problems continue to handicap its use, however: the first, the definition of an accurate, objective, technique for archaeomagnetic curve construction, and second the problem of recruitment of new specialists into the field to continue its growth.

In the third of this four chapter section (Ch. 7, Obsidian hydration dating, past and present), Charlotte Beck and George T. Jones give a brief sketch of some of the history of development of obsidian hydration, and some of the problems that have been addressed. They credit Irving Friedman, C. S. Ross, and Robert L. Smith, as being the pioneers in developing the method between 1955 and 1960.

Problems addressed in perfecting the technique have varied roughly by decade. The 1960s were dedicated to the definition of the correct rate equation, bedeviled by the assumption that artifacts at each successive depth had constant thermal and humidity histories — a situation that rarely obtains in archaeological sites. The 1970s were primarily dedicated to the issue of chemical composition, when it was determined that rate of hydration were source dependent, e.g. dependent on the specific chemical composition of each obsidian flow. The 1980s were focused on experiments to work out EHT (effective hydration temperatures) through various induced temperature experiments, and in developing a variety of thermal cells to this end, while in the 1990s, the practitioners spent a good deal of time working on the impact of relative humidity upon hydration dating, and techniques to more accurately and precisely measure the hydration thickness.

Beck and Jones suggest that the current status of obsidian hydration dating "may actually be more problematic than it was in 1960 when the method was first presented, largely because we know so much more about the hydration process" (p. 142) and its attendant problems. However they suggest that the method still has considerable utility as a relative dating technique to evaluate the

contemporaneity of surface artifacts, and to evaluate individual artifacts within an assemblage.

In the last chapter of this section (Ch. 8, Luminescence dating and why it deserves wider application), James K. Feathers provides a rather cursory attempt to explain why thermoluminescence (TL) has not become a popular method in American archaeology. Feathers suggests two reasons, based on Reagan-ish 'supply-side' economics. The 'supply side' is a brief commentary on the start-up of some of the TL labs in the United States and Germany; the 'demand-side' is essentially the basic observation that Americanist archaeologists fail to perceive the usefulness of TL.

Part IV of the volume consists of two papers: the first, a rambling discourse on the interface between historic records and dendrochronological applications, and the second, a rather critical review by an outsider of what that author sees as the cursory and shallow nature, to date, of the history of archaeology as written by archaeologists.

In the first of these two chapters in Part IV (Ch. 9, Dendrochronology and historical records — concordance and conflict in Navajo archaeology), Ronald Towner suggests that dendrochronology can be employed to evaluate historical document authenticity, but that as well, historical documents can be employed as a means to evaluate dendrochronological reliability. Looking briefly at four Navajo oral history testimonies, Towner finds concordance between Navajo oral history and dendrochronology at Black Mesa, but discordance in three other cases: Navajo stores about movement into the Southwest, movement of refugees after the revolt of 1680, and Navajo flight after the drought of 1748.

*The Sociology of Archaeological Knowledge: A Theoretical Paper Evaluating Limitations of the History of Archaeology:*

HOWEVER: the single most important chapter in the entire volume for students of the history of archaeology is Jennifer L. Croissant's Ch. 11, "Narrating archaeology: a historiography and notes toward a sociology of archaeological knowledge." This is a rather difficult chapter to read and follow, as the author, as an admitted 'postprocessualist' (Fn. 9, p. 216), favors the kind of dense rhetoric typical of many of the postprocessualists tomes. She is not kind to archaeologists who have been writing the history of archaeology, but her critique is well worth pondering.

Croissant breaks her argument into four sections and — as the reader will shortly see — loves to pigeon-hole or order her thoughts into such groupings. Her first section is on theories of narratives and the sociology of knowledge. Here she notes that archaeologists, as historians of archaeology, operate with three basic kinds of historical representations or data: 1. annals or catalogs; 2. chronicles or chronologies, and 3. narrative histories or historical narratives (with implied causality). Most archaeologists who write histories of the discipline, she states later in the article, only create writings of category 1 or 2, while believing they are creating works in category 3.

For the third category only, she then defines 4 genres of historical narratives (p. 189): 1. investigative narratives, accounts, or mimetic appearances of process; 2. conventional narratives of events; 3. didactic narratives with moral lessons; and 4. discursive narratives which relate past events to present questions.

Within each of these 4 genres in the third category, Croissant says that 4 themes can be recognized: 1. pragmatic, with a focus on the intents of the actors; 2. conditional, relating events to material

causes; 3 psychological, with a focus on the will of the actors; and 4. ethical, with a focus on moral life as shaped by material, ideal, and practical events. These categories and subcategories establish the terminology employed in the subsequent sections, where Croissant turns to the analysis of histories of archaeology written by archaeologists. She wraps up this first section with the observation that “the history of archaeology as produced by archaeologists is still largely represented by a particularistic, historical mode, rather than a ‘processual’ mode or theory-driven history” (p. 190), a rather damning assessment, she suggests, because most of the archaeologists who are writing these histories consider themselves to be processualists.

In the second section of her four part argument, the meatiest part of her chapter, this categorization continues. Croissant initially defines four categories or genres of ‘archaeological narratives’: 1. micronarratives or research reports; 2. macronarratives of archaeological history and cultural reconstruction; 3. histories of archaeology by archaeologists; and 4. histories of archaeology by others (p. 187), but almost immediately reduces these four to but three genres: 1,2, and [3 & 4], and notes that the bulk of her paper then will focus upon [3 & 4]. Thus after a detailed review of her conception of the basis for constructing the sociology of history, she gets down to what she want to focus upon, the difference between histories of archaeology written by archaeologists and those penned by non-archaeologists.

To evaluate these disciplinary histories, Croissant asks the rhetorical question: “How do they know?” She takes this question to mean: 1. What constitutes historical evidence for archaeologists?, 2. How is historical evidence legitimated?, 3. What frames the cultural coherence of the history of archaeology?, 4. What are the norms of archaeological conduct?, 5. What models do archaeologists employ and test?, and 6. How is knowledge institutionalized? She argues that an examination of archaeology’s view of its own history, when placed in the context of networks and social roles, and of institutional analyses, will lead us to a sociological conception of archaeological knowledge. The number, strength, and density of intellectual lineages and schools in archaeology affect the framing of research questions in the discipline, and hence she believes these ought to be among the primary issues and interests addressed in any history of archaeology.

Turning to specific examples, Croissant looks at the nature of the concept of chronology as evinced by archaeologists. She argues that “for archaeologists, like historians, chronology and causality, the essences of narrative, are the tools for representation where the object of study is time itself.” (p. 193). In reviewing several histories of archaeology written by archaeologists, she notes that a significant issue “is the problem of the individualizing, and often hagiographic, biographic modes. This mode of scholarship emphasizes individual inventors and downplays the role of networks of competitors and correspondents in developing solutions to intellectual questions.” A ‘hagiography’ she defines (Fn. 4, p. 216) as “a biography of someone under consideration for canonization to sainthood” — an evaluation hardly flattering to the historical works by archaeologists, and a critique bringing to mind immediately some of the treatments in recent histories of our discipline, for example, on Lewis Binford, Daniel Brinton, etc.

Croissant identifies two ‘waves’ in the history of archaeology by archaeologists. The first ‘wave’ is characterized by the works of many individuals, including the well-known summaries by Jeremy Sabloff and Gordon Willey, whose major publication she characterizes as essentially a “chronological dictionary” (e.g. no more than the ‘chronicle’ or ‘chronologies’ category of her initial three part break-down of histories of knowledge, listed above, rather than a true ‘narrative’). The second ‘wave’ she sees beginning with Glyn Daniel and Bruce Trigger, whom she considers as producing the first ‘metanarratives’ of the history of archaeology’s emergence, current state, and potential

directions, and which she believes (particularly from her stance as a post-processualist) are capped by the more recent works of Michael Shanks and Christopher Tilley. She criticizes all of the writing by archaeologists on the history of archaeology, however, for only latently implying causality, and for neglecting archaeology's own precepts for the study of its past.

Croissant saves her praise for two 'outsiders' to the history of archaeology, the works by Embree (1992) and Fagette (1996). For example, she says that Fagette's "argument about the influence of the New Deal in the professionalization and intellectual stabilization of archaeology can only have been written by someone who considers himself a historian, not an archaeologist", and further she sees it of particular note that Fagette "discusses the routinization of field methods, the establishment of academic archaeological credentials as necessary for government employment, and the very dense networks of practitioners that emerged in the 1930s as central to the intellectual development of American archaeology", precisely the kinds of themes that she previously identified as critical to address in her "How do they know" questions, which she believes must be addressed in each and every paper on the history of archaeology.

The third section of her argument in this chapter is a brief review of the other papers in this volume with respect to these criteria. In general, she finds that none of the papers measure up to these standards.

The fourth section of her four-part argument is a discussion of the necessary framework for a socio-logically-grounded history of archaeology, and for studies of narrative in archaeology. Croissant tells us that "without more coherent theories of discipline formation, and the theories of the role of instrumentation in inquiry, histories of archaeology are likely to continue to recapitulate the narratives of unilineal evolution." (pp. 202-203). She tells us that the histories of archaeology as produced by archaeologists are generally ideographic with at best only latent analytic potential, while so far, only the narratives produced by non-archaeologists such as Embree and Fagette are moving to an overtly analytic or nomothetic genre. (p. 203). She tells us that as writers of histories of archaeology, that archaeologists need to grow beyond hagiography and also to grow beyond 'Whiggish histories' (which she defines, Fn. 4, p. 216, as "histories that assess prior knowledge in relation to and support of the status of contemporary knowledge claims"). Further she tells us that if archaeologists want to evolve into true historians of our discipline, that we need to begin writing "social and contextualist histories" (p. 203). She suggests (p. 204) that the professionalization of archaeology resulted in its monopoly by insiders, and suggests that such trajectories often result not only in institutionalization, but in disciplinary stagnation. In general Croissant's critique of the way archaeologists do the history of archaeology provides a lot of ideas for those of us she terms 'insiders' to ponder, as well as providing a number of fresh ways for us to consider addressing issues when writing future papers.

Nash's volume has an extensive bibliography (62 pp.) and an adequate index. Some of the authors, such as Taylor and Croissant, also provide extremely useful and informative footnotes to illuminate important arguments. For students of the history of the development of the various archaeological dating techniques covered in this book, the volume will be a useful addition to one's library. But I think all of us involved in the historiography of archaeology might benefit from Croissant's chapter. While I do not agree with all of her points, I think a thorough understanding of the argument she advances will help us all clarify the perspective of our writings on the history of archaeology.

## References

*Metaarchaeology: Reflections by Archaeologists and Philosophers*

1992 edited by Lester Embree, Kluwer Academic Publishers, Dordrecht.

Paul Fagette

1996 *Digging for Dollars: American Archaeology and the New Deal*, University of New Mexico Press, Albuquerque:

Stephen E. Nash

1999 *Time, Trees, and Prehistory: Archaeological Tree-Ring Dating and the Development of North American Archaeology, 1914-1950*, University of Utah Press, Salt Lake City.

### III. Bibliographic/Archival Material Relating to the History of Archaeology

#### A. Works by Subscribers

Nash, Stephen E.

2001 "A Gentleman Scholar [Paul Sidney Martin]," *Archaeology*, May/June, pp. 60-63.

Noble, William C.

2000 "J. Norman Emerson: Contributions to Canadian Archaeology," in *Bringing Back the Past: Historical Perspectives in Canadian Archaeology*, edited by Pamela Jane Smith and Donald Mitchell, Mercury Series, Archaeological Survey of Canada, Paper 158, Canadian Museum of Civilization, Hull.

Richling, Barnett

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Trigger, Bruce G.

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#### B. Doctoral Dissertations/Masters Theses

None to report.