

ARCHAEOLOGY AS DISCOURSE AN EDITORIAL ESSAY

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The new editor of Ontario Archaeology identifies archaeology as a form of written discourse and outlines the three main functions of the journal. The manner in which the personalities and ideologies of researchers influence the adjudication and legitimation of knowledge is explored. Examples of deficiencies in the use of documentary evidence are cited to illustrate weaknesses in archaeological writing. It is argued that a focus on scholarship is essential if archaeologists are to justify demarcating their discourse from competing constructions of the past and ensure that their labours have a lasting social value. It is concluded that the research community has a collective responsibility to uphold the highest possible standards without limiting the diversity of approaches and ideas.

As a graduate student during the mid-1980s, I narrowly escaped being pigeonholed into one of Kent Flannery's (1982) three caricatures of archaeologists: Old Timer of the Fifties, Born Again Philosopher of the Sixties, or ambitious Child of the Seventies. While exemplary representatives of all three could still be found in the penultimate decade of the twentieth century, it was becoming increasingly difficult to categorise my academic generation, beyond the hackneyed dichotomy between "processualists" and "postprocessualists." To this day, were it not for the trace amounts of glacial till which accumulate under my nails for several weeks per annum, I might not be deemed an archaeologist of any ilk or era. This is not because I have an aversion to the adversities of the field. Although recent seasons have seen me in the rocky and mosquito-infested bush of the Canadian Shield, I continue to agree with Paul Bahn (1989:8) that excavating is the most fun you can have with your pants on. However, I remain unconvinced that archaeology

is fieldwork. I concur with the feelings of Daniel Wilson, a learned forebear who said that "to confine our studies to mere antiquities is like reading by candle-light at noonday" (quoted in Wylie 1985:63). Moreover, I am in accord with those who see archaeology as an intellectual labour (Shanks and Tilley 1988: 186) and who appreciate that theories of knowledge are as much a part of archaeological research as are field survey methods and excavation procedures (Gibbon 1989:6-7). The fact that someone who spends more time with books than with pots has been appointed editor of this journal suggests a growing awareness that there is more to archaeology than soiling the knees in a one-metre square.

Many Ontario archaeologists in my academic generation studied at McGill under Bruce Trigger—a scholar who has not undertaken fieldwork in decades but who has produced a staggering list of internationally acclaimed contributions to archaeology. This should not be surprising since, etymologically, the term "archaeology" refers not to the exhumation and scrutiny of material culture, but to *discoursing* about ancient matters. Consider this simple syllogism: if archaeology is a science (MacNeish 1978; Watson et al. 1984), if science is a branch of literature (Popper 1972:185), and if literature is a body of writing on a subject (English 101), then archaeology is the production of texts. Hence, a journal is not merely the final phase in a linear chain of information processing (Gardin 1980:5-6), but is in a sense the embodiment of a discipline. In short, Ontario *Archaeology* is archaeology in Ontario, as are, of course, Arch Notes, The Annual Archaeological Report, *Kewa*, a number of *Bulletins* and Occasional Publications, as well as other serials, monographs and books.

With this in mind, it becomes reasonable to surmise that, should some calamitous legal, socio-political, or economic transformation

suddenly precipitate a suspension of all fieldwork, archaeology would continue in the province and might even undergo a temporary fluorescence as excavators turn their attention to the neglected task of analyzing and describing extant collections, revisiting long-held axioms, and publishing fresh ideas on old questions. That most of us have been guilty of letting the digging outstrip the writing is evidenced by the oft-expressed lament that vital evidence is cached in a secluded basement or among the folds of a colleague's grey matter, as well as by the plethora of references to "personal communication." Even if a prohibition on fieldwork became permanent, we ran out of sites, and the writing caught up with the excavating, archaeology would not cease. Historians demonstrated long ago that new insights into past cultures are not dependent on new sources of "raw" data.

THE FUNCTIONS OF *ONTARIO* *ARCHAEOLOGY*

This journal has the three main functions shared by all learned volumes: publication, archiving, and legitimation as knowledge (Guedon 1993:4).

To publish is little more than to make publicly known. Since the photocopier, fax, computer disk, and electronic mail have greatly facilitated the informal dissemination of written ideas, a significant number of manuscripts are de facto publications. A good example is the paper on Huron sweat baths originally presented by Allen Tyyska at the 1972 Annual Meeting of the Canadian Archaeological Association and informally circulated thereafter. Tyyska's contribution was "published" so widely that, more than a decade later, other researchers continued to offer summaries and critiques of his principal arguments (e.g., Finlayson 1985:409-410; MacDonald 1988:18-19). If a photocopied typescript can be disseminated and debated in a research community as effectively as an article in a learned journal, then publication cannot be the *raison d'être* of OA.

Although some manuscripts appear to have a lengthy endurance, it is doubtful that Tyyska's paper, which remains in the private

files of individual contemporaries, will be readily accessible to future researchers. For a time, every written contribution rests in short-term memory. However, unless it is a work of Aristotelian or Darwinian influence, it eventually moves into a kind of long-term memory. As Jean-Claude Guedon (1993:4) notes, "the process of constructing long-term memory is of course principally a process of massive collective forgetting, though the process can be reversed and items can be 'unforgotten' by dint of the scholarly work of exhumation of items which have been neglected for decades or centuries."

Exhumation is a procedure especially familiar to archaeologists. All agree that it would have been a great deal easier (albeit not as stimulating) if past peoples had employed shamanic prognostication to anticipate our curiosity, had left maps, and had sent their garbage to a common repository for immediate cataloguing with pictographic Borden designations. If the failure of prehistoric forecasting has led to the inconveniences of fieldwork, graduates of the burgeoning discipline of library sciences have, through ingenious classificatory and indexing systems, made the exhumation of items from our own culture's long-term memory much easier—that is, if the items have been submitted and accepted for archiving in the first place.

Theoretically, a journal need only archive a single copy in a lending institution; the inter-library loan system ensures that it will eventually reach those who have a need to consult it. In practice, libraries are interested in maintaining their own comprehensive holdings, thereby reducing the number of steps required to access information. The archiving function of OA is fulfilled by the more than 100 libraries and other facilities which have institutional membership in the Ontario Archaeological Society. While some scientific journals, particularly those which are despairingly esoteric or outrageously expensive, are obtained and held by very few institutions, a newsletter **like** Arch Notes, which has successfully piggy-backed on the same subscription as its learned parent, may be retrieved in a number of research libraries. Hence, there is no strong link between academic status and accessibility. Moreover, as attested by

Paul Sweetman's (1993:10) complaint that his early archaeological work is now ignored, a wide circulation does not mean that a paper will escape neglect; but it does virtually guarantee that, should the need and curiosity arise, the contribution will be readily forgotten.

The third function of a learned journal, legitimation as knowledge, is the most complex. From the outset it must be recognized that this function is not necessarily linked to archiving. Tyyska's conference paper, by virtue of being widely discussed in the secondary literature, attained a measure of legitimacy that surpassed certain OA volumes which continue to sport mint covers from disuse. Nevertheless, archived materials are much more likely to become part of established "knowledge." Since the library of Alexandria—said to have contained the total aggregate of ancient knowledge—has no modern representative, what constitutes knowledge is now determined by selection. A recent study of literature loss in anthropology has demonstrated that 40 percent of the total published output in the discipline is not held in *any* of the 70 American institutions comprising the Association of Research Libraries (Schwartz 1992:316). By using acquisitions policies to select specific serials and books from the ever-expanding pool of publications, these libraries are indirectly involved in legitimation.

The libraries certainly play a part, but most legitimation occurs either before a contribution goes to press or *after* it has already been archived. To legitimate is to authorize or sanction through authoritative declaration. In academia this usually takes the form of pre-publication peer assessment and/or post-publication reviews, commentaries, and other responses. It is essentially the adjudication of a written work by knowledgeable readers. Although this has an enormous impact on the production of knowledge, and on how the mainstream is distinguished from the fringe, we seldom consider it in any detail; now that an introspective and hermeneutic archaeology is re-examining its own presentation and not only the "facts" about the past, it is time we do. At the risk of sounding moralistic, I will cite specific examples from archaeological discourse in Ontario which I believe illustrate

salient deficiencies in adjudication and, hence, serve as instructive cautionary tales. I restrict my commentary to clues found in the library, although the little I know of the internal politics of Ontario archaeology has persuaded me that much more interesting cases could be plucked from the oral grapevine.

PERSONALITY

Every small community of researchers struggles with the tension between a need for criticism, vital to any healthy science, and a desire to preserve friendly, collegial relations with co-workers. While both may be desirable, one is often sacrificed for the other and occasionally both are lost. Human personality and temperament not only impact on the ability of a research community to adjudicate the writing of an individual, but also affect the ability of an individual to graciously accept the opinions of peers.

Thomas E. Lee had such profound frustrations with the Establishment that he launched and edited his own private organ under the imposing title of *Anthropological Journal of Canada*. This serial not only functioned as an outlet for peripheral writers, such as the hyper-diffusionist George Carter (Williams 1991:277), but also emancipated Lee from the strictures of peer assessment, allowing him to set out on a 20-year crusade to try to salvage his reputation and discredit his mainstream detractors. In one memorable book review, for instance, he proclaimed that, while he had only skimmed through J. V. Wright's (1972) *Ontario Prehistory* ("subscribing as I do to the view that it is not necessary to eat all of an egg to discover that it is rotten"), he felt compelled to suggest that "little credit will accrue to the Queen's Printer, to the National Museum, or to the Government at large in publishing at public expense what I, as a long-experienced archaeologist, regard as unadulterated garbage" (Lee 1973:26).

A stimulus for Lee's cacophonous invective was the apparent refusal of the Establishment to accept a pre-Clovis date for the earliest components of the Sheguiandah site on Manitoulin Island. In the 1950s this extraordinary excavation had received national atten-

tion and the personal support of Lester Pearson, President of the UN General Assembly. All was well until Lee began alluding to artifacts recovered deep beneath glacial till and offering estimates of 30,000 years B.P. It was not long before interest waned, and for the next three decades Sheguiandah was to become one of Ontario's most neglected major sites (Jackson and McKillop 1987:10-14).

Lee's competence as an archaeologist does not seem to have been a major issue. His most comprehensive published report on Sheguiandah, while by no means exhaustive, includes detailed artifact descriptions, stratigraphic charts, profiles, site plans, pollen records, line drawings, photographs, as well as a lengthy discussion of site formation processes (Lee 1957). Apparently, he was no slouch in the field and was not altogether remiss in publishing his findings. Nor did his initial speculations smack of dogma or haughtiness. On the contrary, Lee (1957:117, 123) actively sought and received the advice of numerous scientists in a spirit of interdisciplinary research that remained unsurpassed until W. Roosa's work two decades later (Noble 1982:174).

So what led to the snub of Lee's conclusions on the antiquity of Sheguiandah? In later life, he tended to present himself as an embattled maverick whose brilliant insights on various archaeological matters had been spurned for political reasons by jealous Moguls mired in immutable paradigms (Lee 1980). Perhaps the Clovis hypothesis had become such a paradigm. While Lee may have accepted a realist view of science, the Establishment may have adopted the type of extreme empirical scepticism that demands unequivocal proof. Debates on early man in the New World are not always about data. After all, some mainstream researchers have recently accepted a pre-Clovis migration and colonization of the Americas simply by modifying their philosophy of science (Whitley and Dorn 1993:641-642). On the other hand, Wright (1985:426) recalls that archaeology in the 1950s and 1960s "was not particularly burdened with doctrinaire constraints. Such statements must always be read with some scepticism, but it is important to recognize that Establishment views *did* change with new discoveries. Wright (1972:13), in the same

book blasted by Lee, conceded that humans had likely reached North America at least 30,000 years ago. I doubt that parochial philosophies and academic prejudices, such as those which precluded recognition of Ontario Paleoindians in the 1930s (Jackson et al. 1987:15-17), account for the rejection of the early dates for Sheguiandah.

Lee's unorthodox response to failing his doctoral examination in anthropology at the University of Michigan likely set the stage for his infamy in subsequent years (Lee 1980:28). Early strife with Establishment archaeologist James B. Griffin persisted into the 1980s when Lee invited readers to judge Griffin by looking up his surname in Webster's dictionary (Lee 1980:29). One need only peruse the pages of the *Anthropological Journal of Canada* to recognize that Lee's personality probably played a part in his ostracism. It certainly interfered with his own ability to objectively assess the opinions of archaeologists who disagreed with him.

Despite the personality conflicts, Lee was never entirely ignored and his contemporaries continued to list him among the major contributors to Ontario and Canadian archaeology (Noble 1973:66; Wright 1985:426). In fact, his 1957 publication is on the "suggested reading list" found in the same Establishment book he saw fit to deem rubbish (Wright 1972:115). This raises an interesting question about the process of selective legitimation. At no time was it made absolutely clear why Lee's report on Sheguiandah could serve as evidence for the Paleoindian (Plano) and Archaic periods (Wright 1972:17, 115), but not as evidence for a pre-Clovis occupation. In my opinion, this has served the interests of neither science nor the general public. The illegitimation of a published interpretation by an author whose work has otherwise been legitimated as "suggested reading" cannot be accomplished orally behind the scenes, but only through a corpus of archived peer response.

In the intervening years, Lee's persistence has not been met with a Copernican vindication. Indeed, long after his death, a new generation of scholars has identified Sheguiandah as a Late Paleoindian site (Storck 1984:21), and has continued to relegate Lee's interpretations to a minor footnote in the

prehistory of the Province (Ellis and Deller 1990:37). Now that researchers have revisited the site, substantive archaeological and geological justifications for this are finally beginning to emerge. Nevertheless, Jackson and McKillop (1987:14) remind us that the decades of neglect reflect badly on the scientific objectivity of archaeology in the province.

IDEOLOGY

Personality differences among individuals are exacerbated by ideological rifts along generational lines, between factions of contemporaries, or between professionals and avocationalists. Opinions on what is "important," what is "knowledge," or what is "certainty," significantly affect the legitimation of research, particularly if they become ingrained and exclusive.

Anyone who still insists that Ontario archaeologists lack specific epistemological predilections should read "Gnawing Gently on the Metacarpals" by Nick Adams (1994). In particular, I draw attention to the following passage:

Through archaeology we can gain *certain knowledge*, or a close approximation thereof, of how, when and where people lived in the past, how they got their groceries, the kinds of tools they made and used, how they interacted with their neighbours, and even their physiologies and their pathologies. We stand on a less secure footing once we begin to extrapolate about social organization and structure from the archaeological data. And we are out of our depth in the quaking bog of conjecture once we attempt to describe the realm of symbolism and ideology from the material fragments in our collections [Adams 1994:10; emphasis in original].

If this sounds familiar, it is because Christopher Hawkes (1954:161-162) said essentially the same thing 40 years ago. Hawkes' "climax of degrees of difficulty," or ladder of inference

as it is sometimes known, was partially buried in the 1960s and 1970s when the New Archaeology adopted middle-range theory and the systemic concept of culture, although the funeral was postponed until recent developments in cognitive archaeology (Whitley 1992). It has become increasingly apparent that the methods and quality of inferences relating to prehistoric symbolism and ideation are little different from those relating to lower level subsystems (von Gernet 1992a; 1993a). What is "knowable" is not linked to the relative difficulty of recovering perishables and non-perishables, intangibles and tangibles, or "mind" and material culture. Rather, it is a function of the degree to which a generalization observable in the present may be persuasively related to an unobservable past (von Gernet 1993a:68). Elsewhere, I have offered an example using archaeological material from Ontario (von Gernet and Timmins 1987).

While I personally find Adams' ideas on the nature of archaeological knowledge anachronistic, everyone has the right to chew their own metacarpals as they reinvent the metaphysical wheel. What I am more concerned about, is that these types of ideologies have the potential to interfere with legitimation because they place arbitrary and a priori limitations on what archaeology can accomplish. How, for example, might someone, whose *weltanschauung* is already committed to the unsubstantiated generalization that reconstructions of ideology are inherently more conjectural than reconstructions of social organization, evaluate Fox and Molto's (this volume *infra*) paper on evidence for shamanism at Long Point?

Ontario archaeologists are not immune from letting their ingrained "isms" intrude into their adjudication of scholarly work. An example may be found in a published review of James Pendergast's (1991) monograph on the Massawomeck. Peter Reid judges the work to be "thorough, critical and carefully researched," but laments that it "reflects the particularism which, even at this late date, underlies most of Canadian archaeology." He ventures the opinion that "facts and narratives strung together from facts are not in and of themselves important," but "become important only when related to questions of...culture process"

(Reid 1991:15). This betrays a lingering fidelity to the nomothetic ambitions of what has itself become a well-aged "New" Archeology, whose practitioners were more interested in generalizations about human behaviour than in the particulars of native history and prehistory (Hodder 1985:7; Trigger 1980:671). In my opinion, Pendergast (a retired lieutenant colonel and winner of the 1991 Crabtree Award for avocational archaeology), should *not* be reproached for failing to espouse an epistemology which, while fashionable in American graduate schools in the 1960s and 1970s, is unlikely to have entered conversation in the Officer's Mess. Moreover, the dogmatic assertion that "important" narratives about the past address lofty questions relating to processual matters simultaneously impugns the valuable contributions of our pre-processual forerunners (William J. Wintemberg comes to mind) and our post-processual contemporaries. Such debate has its place in the ponderous discourse known as "archaeological theory" but seems inappropriate in a review of a substantive work that lays no claim to profundity.

SCHOLARSHIP

Avoiding the intrusion of "isms" does not mean that a work published in a learned forum should escape scholarly critique. Indeed, Pendergast's (1991) monograph deserves admonition for reasons unrelated to his world view. Unlike Reid (1991:15), who praises Pendergast's thorough and careful research, Bill Fitzgerald (1992) places considerable emphasis on errors in fact or the uncritical use of documentary sources (a good argument for having several independent reviewers). He concludes that Pendergast's final product "resembles historical fiction more than it does a reasonable interpretation of the scanty historical and non-existent archaeological evidence" (Fitzgerald 1992:129-130). While Fitzgerald's assessment seems excessively uncharitable, it is more pertinent than the one offered by Reid since it is based primarily on an evaluation of erudition, not ideology.

It could be argued that, since archaeologi-

cal discourse is generated and situated in a social and political arena (Shanks and Tilley 1988:186), it is not possible to avoid ideology. This should not mean abandoning the search for a value-free archaeology. As Trigger (1984a:368) has noted, "The findings of archaeology can only have lasting social value if they approximate as closely as possible to an objective understanding of human behaviour." I have been impressed with the ability of researchers to maintain at least some objective distance, even in the politically charged and sensitive disciplines relating to native studies. For example, the suggestion (promulgated by certain natives, non-natives, historians and anthropologists) that Europeans taught Amerindians how to scalp has been effectively repudiated by a detailed study of the historic and prehistoric records (Axtell 1981). A similar analysis has demonstrated that the "fiction" asserting that the Iroquois did not practice cannibalism (Arens 1979:128-129) cannot supplant the historical and archaeological "fact" that they *did* (Abler 1980). Given that those researchers most interested in reversing popular stereotypes about aboriginal peoples have nevertheless risked documenting the types of horrors which might arouse the interest of Amnesty International, I remain hopeful that research, writing, adjudication and legitimation can survive political or ideological pressures.

Scholarship is among the few qualities of discourse that can be appraised, if not with complete objectivity, at least with some semblance of fairness. The evidential support for any archaeological statement is derived from newly excavated artifacts or from the texts of other writers. Competency in scholarship is revealed by the strength of linkages a writer constructs between evidence on the one hand, and interpretations and conclusions on the other. Opinions differ on what level of strength is acceptable, although most agree that, in adjudicating competency, the emphasis should be more on the structure and less on the content of the writing.

The shortcomings Fitzgerald (1992) has identified in Pendergast's (1991) work present a striking paradox: while Ontario archaeologists have continued in the footsteps of some of our indefatigable ancestors, whose meticulous analyses of artifacts were in the

best tradition of empirical observation and classification, they do not always apply their skills to documentary materials. Perhaps this is based on the curious misconception that, unlike in the field of history, the raw materials of archaeology are non-textual, and that writing merely represents the end-product of research. Yet, if this were so, none of our site reports would contain a bibliography, but would consist entirely of original plates, line drawings, statistics, and descriptive narrative. In fact, each of the papers published in the previous five volumes of this journal contain an average of close to 40 references. These sources, which are woven into the authors' arguments in the form of parenthetical citations, serve as evidence in a manner little different from recently unearthed material culture. Scholarship can be assessed with reference to both types of evidence; my preference, here, is to focus on the former.

USE OF DOCUMENTARY MATERIALS

Although uninformed theoretical debates continue to muddy the waters and make consensus elusive, I believe those who spend sufficient time contemplating the philosophical issues eventually come to the realization that archaeological knowledge is primarily dependent on an inferential argument known as analogical reasoning (Wylie 1982; 1985). Some Ontario archaeologists, like Norman Emerson—who employed a psychic truck driver to help reconstruct the lifeways of the inhabitants of the Black Creek Site (Goodman 1977:159; Williams 1991:295-296)—have attempted to skirt such reasoning. However, those scholars not favoured with an aptitude for clairvoyance or unwilling to suspend a scientific scepticism of the paranormal usually end up in the library where their imaginations are inspired by the recorded observations and insights of archaeologists, other anthropologists, as well as ethnologists, missionaries, and explorers. Ironically, evidence is mounting that exposure to narratives is also the primary source for "psychic" archaeology (Feder 1990:163-166), although the narratives are more likely to have been found in popular

book stores. Contrary to prevailing sentiments, prehistoric archaeology, while not entirely dependent on texts, is certainly text-aided; the old notion of a "text-free" mode of reasoning (Hawkes 1954:161) can no longer be sustained.

Given their reliance on documentary records, it stands to reason that archaeologists require at least some competence in exegesis, literary criticism, or other forms of textual analysis. Indeed, some archaeologists list "ethnohistory" among their interests or areas of expertise. Nevertheless, it is an unfortunate circumstance (perhaps attributable to the type of over-specialised training in which archaeology is considered to have affinities with geology but not with history), that a black belt in digging tombs does not prevent reckless floundering in dusty tomes. To stifle the inevitable response that this is innuendo, in accord with the principle of entering critique into the published record, and at the risk of inducing blushes among colleagues, I offer a sample from the writing of Ontario archaeologists.

In an article appearing in an early issue of this journal, an apparent association between pipe effigies and matrilineage totems painted on Huron longhouses (Noble 1969:24) was based on a specific text in Gabriel Sagard's *Le grand voyage*. Apparently, the author was unaware that this was one of the many passages selectively plagiarised by Sagard from Marc Lescarbot's earlier description of the Micmac and New England Algonquians (Sagard 1939:98 cf. Lescarbot 1907-14:3:98-99). In a similar vein, an article in the *Handbook of North American Indians* asserts that Sagard mentioned tobacco cultivation and trade among the Petun (Garrad and Heidenreich 1978:395) when, in fact, the original passage had clearly been plagiarized from Samuel de Champlain's discussion of the Neutral (Biggar 1922-36:3:99 cf. Sagard 1939:158). Fortunately, neither oversight is crucial to an influential conclusion.

More recently, Thor Conway (1983:16-17) not only has Champlain traversing a region the explorer failed to reach until years later, but confuses a seventeenth-century Algonquin sacred site on the lower Ottawa River (Biggar 1922-36:2:301-302) with a site on the French River identified by twentieth-century Nipissing

oral tradition. In this case, the error completely undermines the author's principal argument that a specific Ontario locale has a 370-year record of spiritual associations.

Regarding a work as a "primary" source solely on the grounds that it was written in the seventeenth century is a common mistake that results from a failure to appreciate the complexity of ethnohistorical texts. True, a single researcher may not be able to handle prehistoric artifacts and ethnohistorical materials with the same degree of skill (Bishop 1982:256; Wright 1968:97-98). Yet, documents are an integral part of archaeological research, and one would hope that archaeologists, who already acknowledge the importance of considering site formation processes (Schiffer 1987), make every effort to understand how texts are constructed. There is more to the direct historic approach than citing a "Works Of" or "Journey To."

As tributes pour in for the late Kenneth Kidd, it is worth recalling that praise, even when well deserved, should never bridle a critical assessment of the work of any scholar. While his valuable contributions to the archaeology of Ontario have secured for him a respectable reputation, he *did* commit to print a most embarrassing blunder. Although trained as an historian (Wright 1985:425), Kidd evidently forgot the maxim that "the whole training of the historian is designed to help him dispose of deliberate manufactures" (Elton 1967:97). In a Royal Ontario Museum publication he once quoted an excerpt from what he believed was a seventeenth-century journal written by a Dutch traveller among the Mohawk (Kidd 1954:20). Curious to know why a document germane to Iroquoian studies had remained in obscurity, I located the "journal" and discovered, to my initial disbelief and ensuing vexation, that it was a twentieth-century novel cleverly disguised to appear like an ethnohistorical source (Yager 1953). Such errors have a maddening endurance in the secondary literature. Indeed, this particular mistake was repeated nearly two decades later by an American archaeologist who cited the same passage in a book published by a university press (Rutsch 1973:20).

Unfortunately, autodidact ventures into ethnology convince some archaeologists that they have qualifications in this field as well.

For instance, an article published in the *Canadian Journal of Archaeology* advances the argument that there existed a chiefdom or ranked society in seventeenth-century Ontario. For the most part, the evidence consists of a native oral account, "not made public previously," and allegedly "preserved by a special lineage of females who have continued to memorize and transmit the story over the past 340 years" (Noble 1985:133). Now, here is a document of breathtaking importance; yet, the reference is simply cited as an anonymous "manuscript and notes" in the possession of the author. No efforts are made to explain how a transcript of an oral tradition by an unnamed informant of unspecified ethnicity came into the private possession of an archaeologist; whether the informant is still alive to corroborate the story; what, if any, interview methods were used in compiling the transcription; and why this unparalleled account was not first legitimated by professional ethnologists and archived in an accessible institution. By *any* reasonable standards of scholarly inquiry, the credibility of such an apocryphal source is no different than the twentieth-century novel cited by Kidd. That an elaborate argument based on such spurious supporting documentation was published in a major journal can only be regarded as a grievous deficiency in adjudication. The problem is compounded when the secondary literature conceals the original source by citing the legitimated CIA article. In fact, the latter has already been employed in an international journal to support a rather fanciful argument that the Neutral practiced husbandry of white-tailed deer by penning and tending them inside enclosures (Noble and Crerar 1993:22-23, 35).

THE COMPETITORS

Some will argue that lapses in scholarship should be ignored since (a) they are uncommon, (b) they will soon be forgotten, and (c) efforts to expose them might be viewed as unnecessary pedantry that merely diminishes a colleague's reputation; after all, we all make mistakes (I have discovered enough of them in my own work). To this I respond that

(a) it is the belief in infrequency that leads to a lack of vigilance and concomitant frequency, (b) historians of science ensure that archived statements are unforgotten, and (c) the reputations of individuals are of lesser significance than is the credibility of a discipline as a whole. It must be kept in mind that a published contribution not only reflects the output of an individual author, but is added to the corpus of archived material produced by a collectivity. Archaeology is accountable beyond the pale of its coterie, as both publicly and privately funded research is mediated by a wider social context. There is much more at stake.

Archaeologists once enjoyed a virtual hegemony over North American prehistory and almost exclusive control in the field, the laboratory, and the museum. In recent years other parties have successfully eroded this privilege. For example, not long ago a native organization convinced the Commissioner of the Department of Culture and History in West Virginia that excavation of the Cotiga Mound should not be permitted unless a consulting firm agreed to prohibit menstruating fieldworkers from handling the artifacts (Fogelman 1993:327). Many interventions do not occur in the field, but emerge in various forms of "politically correct" discourse. These range from justifiable revisionism intended to overturn demeaning stereotypes (Doxtator 1992), to simply outrageous drivel catering to specific interest groups.

A decade ago professional anthropologists and historians were warned that their work was in danger of being submerged under a burgeoning popular culture (Trigger 1984b: 19). Ten years later the competition over the right to construct and write the past shows no signs of abating. For the most part, archaeologists respectfully accept limited interventions (Fox 1989), but they have much greater difficulty with wholesale *alternatives*. Some of the competitors are native writers who endorse proprietary history with the racist suggestion that they alone have the right to interpret their past. Others are non-natives who profit from best-sellers outlining "revolutionary" new insights into North American prehistory. Mainstream archaeologists may scoff at these purveyors of alternative literature, but underestimating the competition

invariably leads to trouble, particularly if the competition appropriates your brand name.

In a recent issue of a popular native newspaper it is argued that 12,000 years B.P., the date most widely cited with respect to the earliest evidence for definite human occupation in North America, is "untrue" and "a falsehood popularly taught in all schools." Remarkably, this opinion is not based on the usual oral traditions alluding to a creation *ex nihilo* and occupancy since time immemorial. Instead, it is claimed that archaeologists and anthropologists have found evidence for sites and artifacts dating as early as 70,000, 150,000, 250,000, and even 500,000 years B.P., but that these "truthful studies of North American Indian antiquity, have been hidden for political reasons" (Thohaho'ken 1993:6).

Some might dismiss this as akin to the belief, common in some circles, that the archaeological recovery of Noah's Ark on Mt. Ararat is being thwarted, not by the nonexistence of the vessel beyond the diluvian accounts of the Book of Genesis, but by Turkish machinations and CIA or KGB treachery (Balsiger and Sellier 1976). It is, however, easy to see how such a claim can be seriously entertained and widely accepted. The "archaeologists" who are alleged to have uncovered the hidden truth are not mainstream researchers who have accepted pre-Clovis evidence but, rather, popular pseudoscientists who have advanced entirely different prehistories (Thohaho'ken 1993:6). When Barry Fell's diffusionist work *Bronze Age America* (1982) and Brian Fagan's undergraduate textbook *People of the Earth* (1985) are published and distributed by the same company and archived by the same institutions, we should expect both a confusion over what constitutes "archaeology" and suspicions of politically motivated school curricula.

The archaeological community can only survive in this landscape by defining a niche that is clearly distinct from all others (Gero 1989:103). The contrast must focus on the quality and not the implications of the discourse. It is not enough to merely state, for example, that Barry Fell is racist or denigrates native people by attributing much of their cultural heritage to Old World visitors (Kehoe 1987:19; Trigger 1989:315). In fact, in a recent American Antiquity article, one promi-

nent native scholar, suspicious of the manner in which Fell was dismissed by the Establishment, has implied that he may have been correct after all, adding that, "until we [Indians] are in some way connected with world history as early peoples, perhaps even as refugees from Old World turmoils and persecutions, we will never be accorded full humanity" (Deloria 1992:597). Given these views, the only way to effectively deal with competitors is to identify specific deficiencies in scholarship, as Williams (1991:265-268) has done, and to publish a critique. Recent criticisms (e.g., Michlovic 1990) notwithstanding, such demarcation does not necessarily mean taking the high ground or abandoning an anthropological commitment to cultural relativism. But it does mean that archaeology must have its own house in order.

When the competitors are native peoples, archaeologists often feel sympathy and demarcation efforts become troublesome. I am not convinced that an "integrative approach," merging oral traditions and scientific data (Echo-Hawk 1993), will lead to any resolution. More appropriate, perhaps, is the notion of "sharing the past" (McGhee 1989:17) since this implies that the demarcation of archaeology can survive the acceptance of a pluralistic past. Fortunately, native peoples are not always competitors but are just as likely to be consumers.

THE CONSUMERS: NATIVE PEOPLES

The regrettable scarcity of aboriginal scholars, which continues to make anthropology a predominantly Eurocanadian profession, has not meant that the results of anthropological research are ignored by individuals of native ancestry (Dyck and Waldram 1993:10-11). I am reminded of John Honigmann's Sarcee informant who, prior to an ethnographic interview, "confessed to having borrowed from the Calgary Library many books dealing with the Plains Indians" (Honigmann 1956:36). There is no question that archaeological texts are among the resources being consulted by indigenous peoples (Spurling 1988:74).

Robert McGhee (1989:14) believes "many

Canadian prehistorians would be surprised to learn that their interpretations of the past are considered by many native peoples to have great social and political relevance." Nevertheless, attitudes toward archaeology are not all the same. At one end of the spectrum is the Blackfoot elder who admitted that archaeology had done more for the betterment of native peoples than all of the missionaries and government agents had ever done" (quoted in Fox 1989:31). At the other end are those who believe that archaeology is nothing but an extension of Eurocanadian, colonial control over interpretations of the past, and who view our research results as a denigrating assault on native spirituality. I fear that those who attempt to convert individuals from this persuasion are wasting their time, since it mirrors the classic encounter between science and religion. All that can be done is to point out that this confrontation is not unique to relations between natives and newcomers, and that scientific research and respect for religion are not incompatible (Trigger 1982:6; von Gernet 1994).

Having served on organizing committees for First Nations cultural events, I find that most native reactions to archaeological research follow neither of these two extremes but may be situated somewhere in between. This likely reflects an uneasy compromise between the need to revitalize or maintain pre-contact values and traditions, and a recognition of the power and importance of science and humanism in the twentieth century. When native traditions and archaeological discoveries are perceived to be congruent, the latter are often cited in support of the former. It is as if the stories taught by the elders, while already intrinsically valid representations of the past, are believed to have even greater validity when confirmed by independent evidence.

In a recent polemic, the Wendat scholar Georges Sioui pays homage to "so distinguished a thinker as Bruce G. Trigger," but advocates an "autohistorical approach" written from a native perspective which, *inter alia*, would "help safeguard the right of an Amerindian group to territories denied it by traditional non-Amerindian history" (Sioui 1992:xvii, 82). As expected, this important book is infused with the wisdom and philo-

sophy of a native world view. Ultimately, however, Sioui's autohistory is not much different from the constructs of the Western intellectual tradition. To support his argument that the Wendat of Lorette have traditional rights to Quebec territory he not only recites oral traditions, but also refers to archaeological evidence of a close ethnic relationship between the Huron and the St. Lawrence Iroquoians. His conclusion that, for many of his people, the 1649 diaspora was a return to Quebec, "the capital of their former country" (Sioui 1992:82-89), arises from his unique perspective (some might say political agenda), but archaeological analyses serve as supporting documentation. This suggests that archaeologists would be ill-advised to completely abandon research questions relating to ethnicity. Such questions *are* being asked with or without our involvement, and we should make every effort to either contribute to the answers, or expose the weaknesses inherent in equating material culture with ethnicity (e.g., von Gernet 1993b:77-78).

The use of archaeological discourse in native autohistories has important implications. Imagine how Noble's (1985:134, 141) conclusion that the Neutral Iroquois had "progressed" to a level of organization comprising "a well-defined class hierarchy"—an assertion reminiscent of Lewis Henry Morgan's (1851:54-59) evolutionary construction that placed the Iroquois at the pinnacle of the "hunter state" and on the verge of ascending to civilization—might be interpreted by the Iroquois. Some might dismiss it as an attempt by a white man to demonstrate that certain natives were not much different from Europeans and to challenge the pan-Indian emphasis on a set of shared values common to egalitarian societies. On the other hand, others might proudly cite it as evidence of an autochthonous achievement in the development of their own civilization. Popular acceptance of the latter interpretation is entirely possible despite the fact that Noble's conclusion is, as indicated earlier, based on apocryphal documentation. After all, there are precedents. One need only recall the widespread belief that the League of the Iroquois served as a model for the United States Constitution, a belief ultimately based on, what one authority calls, a "scholarly misap-

prehension" (Tooker 1988:327).

THE CONSUMERS: COURTS

My involvement as an expert witness in the Federal Court of Canada has convinced me of the importance of archaeological, ethnohistorical, and ethnographic evidence in helping to resolve some of the most crucial and outstanding issues with respect to the relationship between natives and newcomers in this country. While there are provisions for the testimony of lay witnesses who are called upon to recite oral traditions, our judicial system places considerable emphasis on expert testimony and supporting materials. This emphasis has been challenged on the grounds that it gives an arbitrary preference to the culturally specific conception of history derived from Western thought and ignores other approaches to understanding the past (Fortune 1993). There is, however, no indication that this preference will change in the near future. Hence, significant financial and human resources are being invested by all sides to ensure that relevant anthropological evidence is considered.

If archaeologists think their contributions are safely tucked away on the F5000 shelf or in some other isle of an academic library (where they will be consulted once every decade until they are scanned onto some zillion-megabyte computer disk and are lost by an indexing glitch), they should think again. Chances are that, irrespective of whether you personally testify, at least one of your archived publications will be scrutinized paragraph by paragraph, sentence by sentence, and word by word, not only by witnesses, but also by plaintiffs, defendants, interveners, lawyers, and judges. The links between your data and conclusions will be deconstructed, reconstructed, challenged and defended—all without your consent, your opinion or, for that matter, even your awareness. It means little whether your contribution is found in a peer-reviewed, scholarly publication such as OA, a newsletter such as Arch Notes, or an unpublished work such as a licence report. Moreover, your reputation as a professional or avocational is of no par-

ticular interest; nor is your personality or bent for particularism, processualism, postprocessualism, or any other "ism." All that matters to the Court, is that your archived conclusions and opinions are supported by the weight of evidence provided. If accepted, the opinions themselves become "evidence," and are cited in the secondary and tertiary literature that constitutes the voluminous testimony submitted at trial. Since many courts require experts not only to furnish bibliographies in support of their testimony, but also copies of their sources, texts not written by the witnesses are reproduced to become part of the official record of the proceedings. It is, in essence, an appropriation of discourse about the past for the purpose of rendering decisions about the future fate of real people living real lives.

Publishing a paper is often accompanied by a mollifying reassurance that the contents are protected by copyright. Adding the title to a curriculum vitae also contributes to a feeling of proprietorship. Ultimately, however, archaeological texts, much like ethnographic studies (Dyck and Waldram 1993:10), can be appropriated as "political facts." While we cannot restrict the construing or misconstruing of our research by others, we do have control over the linkages between the evidence and conclusions we provide. Given that our research has the potential to affect the lives of living contemporaries, it behooves us to exercise that control in a most rigorous manner. An error, like the one made by Conway (*supra*), can become a "fact" significant enough to affect the outcome of a land claim. Even if detected, it has the potential to cast doubt on the veracity of other statements and to raise apprehensions about the integrity of other "expert" evidence tendered.

Brian Spurling (1988:75) has proposed that native communities "must be regarded as clients and, to the extent circumstances warrant, archaeologists their consultants and advocates." This advice fails to address the problematic nature of such relationships (Dyck and Waldram 1993). Well-intentioned advocacy by scholars has led to statements about native peoples which are demonstrably false (Axtell 1981:19-20). The fact of the matter is that native claims and causes are not

always supported by the evidence. I have documented one recent case in which the Head of a Department of Anthropology offered voluminous testimony on the past practices of certain Amerindian groups that turned out to be an elaborate fiction constructed to advance the cause of his clients' litigation (von Gernet 1993c). Landmark judgments in Canadian law have been tainted by the mere suspicion of such bias (Asch 1992:235-236; Fortune 1993:91). Archaeology can best fulfil its obligations to native peoples, not by letting sympathies and middle-class guilt motivate advocacy, but by upholding the standards which have become the hallmark of erudition. Apologists who point out the impossibility of objectivity carry little weight here. What is called for is, as colleagues outside of archaeology are beginning to recognize, a matter less of objectivity than of rigor (Asch 1992: 237).

FOSTERING INTERNAL DIVERSITY

The health of archaeology depends not only on our obligation to the rest of society, but also on diversity within our ranks. While a focus on scholarship sets certain boundary conditions on how discourse is structured, it allows researchers to pursue interests which might otherwise be obstructed by Establishment ideologies. I am convinced that the tremendous creativity we have witnessed in archaeology in other parts of the world can be nurtured here simply by forsaking recon-dite limitations on what is acceptable. Who knows what would have happened if Bill Russell had been encouraged to publish some of his innovative meditations (see Varley 1993).

In soliciting manuscripts for OA, my predecessor announced that "First preference will be given to articles that go beyond description of sites or assemblages to make statements of analytical or theoretical significance" (Reid 1987:4). I have no such preference. The well-worn publications by Wintemberg, which can still be read with profit (Trigger 1978:10), suggest that descriptions serve as fundamental building blocks for culture-chronological

reconstructions, comparative analyses and the development of higher-level inferences. As the Vice-President of the Society for American Archaeology said in the inaugural issue of *American Antiquity*, "We may have no Folsoms, but our little collection, provided it is catalogued, will have some value to the world after we depart. And nobody, not even an archaeologist, can expect to live forever" (Harrington 1935). The Ontario Archaeological Society has had a history of attracting excellent avocational archaeologists who have little interest in formulating theory but who are prepared to furnish meticulous descriptions of important sites or artifacts. Space must be reserved for these, provided they are accompanied by plates, drawings, maps, and accurate references to literature describing similar collections. The information gained by translating basement hoards into archived texts justifies cutting the trees required to disseminate such descriptions.

Sites and artifacts cannot exist in a vacuum, and at least some contributors (not necessarily the individuals providing the descriptions) will feel the need to address methodological, analytical, or theoretical matters of a more general nature. In accord with recent developments, this journal will not embrace a "narrow view of science that would identify being scientific with adopting a particular philosophical view about correct standards of confirmation and explanation" (Salmon 1982:180; emphasis in original). Many years have now passed since the New Archaeology was exposed as an ideology based on a defective model of science (Lamberg-Karlovsky 1989:6), and it seems obvious that our research can flourish with or without it. Fortunately, many Ontario researchers never did quite get the hang of either the **method or processual analysis** and, hence, avoided the seduction of travelling this exclusive road to knowledge. This may foster a much needed creativity and make it somewhat easier to adapt to a post-whatever theoretical terrain. There is no reason why Ontario researchers must be passive consumers rather than active contributors to archaeological theory.

It should by now be clear that I reject the neo-evolutionary dichotomy between science and history. Archaeology is just as "scientific"

when it examines the (pre)histories of particular peoples such as the Algonquin and the Huron, as it is when it employs these (pre)histories to illuminate continuity and change among forager and horticultural societies in general. In my view, there are no analytical, methodological, or theoretical grounds for distinguishing between historical and processual approaches (von Gernet 1993a:77). Nor is this a matter of an "either/or" choice. As Alfred Kroeber (1952:63) once said, history "does not ignore process, but it does refuse to set it as its first objective."

If this journal is to embrace any "ism" under my editorship, it is eclecticism. This does not mean that everything will be acceptable. On the whole, I agree with Michael Graves' (1994:7) concern that "archaeologists identify the intellectual perspective within which research is done and then hold that research to its own standards (and those widely shared in the discipline), especially as regards logical consistency, theoretical coherence, and empirical sufficiency." Moreover, provided an author maintains fidelity to scholarship, it matters little whether his/her insights are derived from a materialist or an idealist philosophy. I will even consider arguments made in the complete or virtual absence of material remains. For example, while no physical vestiges of Paleoindian boats have been recovered, the existence of such watercraft can be demonstrated (Engelbrecht and Seyfert 1994). Similarly, it can also be shown that prehistoric Iroquoians ingested powerful hallucinogens in an ideational setting; while this setting has never been excavated, it can nevertheless be "known" (von Gernet 1992b). In both cases the evidence is not in the ground but in the library, and the arguments are advanced without recourse to conjecture and with the principled application of inferential reasoning. Rigour and creativity need not be mutually exclusive.

CONCLUSIONS

It has been argued that material culture can be "read" much like a text (Hodder 1986; Tilley 1990), and that there is little difference between the interpretation of artifacts and the

interpretation of documentary sources (Young 1988:11). Archaeology not only reads what emerges from the ground, but relies heavily on written materials in the origination and reproduction of its own discourse. In either case, learned journals such as OA serve as long-term vehicles for an interminable cycle of reading and writing.

In his essay on intellectual craftsmanship, C. Wright Mills (1959:218) opined that "to write is to raise a claim for the attention of readers," and "also to claim for oneself at least status enough to be read." A journal offers opportunities to advance such claims by publishing, archiving, and legitimating knowledge. Legitimation, accomplished primarily through adjudication by readers, is affected by individual personalities and ideologies. While these influences may be difficult to avoid, I have cited reasons why an emphasis on scholarship, both by writers and by readers, is imperative for the vitality of archaeology.

There is no question that this focus is, itself, part of an ideology—the Western intellectual tradition that encompasses science, secular humanism, a linear conception of history, and a decided partiality for written over oral narration. However, despite a persuasive post-modernist critique, it remains apparent that this ideology is not just like any other competitor, but is widely recognized as the most influential intellectual force of our time. It is of no trivial interest that the most recent claims made by creationists are based on "scientific" rather than on biblical grounds (Kehoe 1987; Stunkel 1982). It is also significant that many alternative approaches to the past (e.g., Goodman 1977) are published under the rubric "archaeology." Given that "archaeology" is not a registered trademark for a single Establishment and that licensing applies to digging rather than to writing, it is important to clearly demarcate what is offered in this journal from the competitors who peddle their goods with similar pretensions.

Such demarcation should not, of course, be accompanied by the supercilious proclamation or unwritten implication that our archaeology reconstructs the past, but by the modest reminder that our archaeology constructs a past that is as close to an objective understanding as we are capable of achieving. Nor

should demarcation efforts lead to insulation from other fields of study. Cross-fertilization with various sciences and humanities has already served us well and should continue to be a desideratum.

Archaeology may be about matters in the past, but it is mediated by a present social context (Leone 1981). This context is no longer confined to the dinner tables of the Emerson, Kidd, or Pendergast households. Just as society has shaped archaeology, archaeology is highly relevant to society (Trigger 1984a: 357-358). This suggests that archaeologists must recognize and change the moral myopia that confines their sense of responsibility to a single Establishment (Wylie 1992:593). The increasing use of our research by native peoples writing autohistory and by courts (re)writing history means that we are obligated to provide the highest quality research possible with the financial, technological and intellectual resources currently available.

While a writer routinely acknowledges the assistance of others, a curious etiquette often compels him/her to add that the author is solely responsible for the contents, "or words to that effect. This is intended not only to fortify proprietary claims, but to deflect culpability away from peers should anything turn out to be inaccurate or untenable. Quality control in our published, archived, and legitimated discourse is, however, a collective responsibility that requires vigilance by those whose names do not appear on the first page. Deficiencies must be exposed, and efforts to do so not misinterpreted as effrontery.

The current OA will become and remain a cultural artifact long after we are deceased. It is incumbent on all of us to ensure that this artifact reflects our best collective effort. As editor, I pledge to do my part by seeking improvements to the peer review process, introducing democratized decision-making through an editorial board, and preparing a comprehensive guide for standardized manuscript production (this volume *infra*). I also plan to make space available for book reviews, discussions, and critical responses to contributions published in this journal.

Finally, I wish to emphasize that my remarks are not intended to impugn past editorial policies. In fact, I believe a relatively

healthy journal was placed in my lap. No one, however, should be satisfied with the status quo. Jim Wright (1985:431) recalls some advice he once received on an autographed publication: "Dear Wright: Go and do better. Best regards, Diamond Jenness." Passing the torch to a new generation is a tacit admonishment of a similar kind. I look forward to the challenge.

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