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## THE DEVELOPMENT OF CONTACT PERIOD ARCHAEOLOGY IN SOUTHERN NEW ENGLAND AND LONG ISLAND: FROM "GEE WHIZ!" TO "SO WHAT?"

Bert Salwen

*It seems that contemporary archaeologists of the Contact Period continue a long tradition of uncritical acceptance of the written word as "God's truth," to be tested against or to inform the incomplete and necessarily biased archaeological record. When documentary history is available, have archaeologists really progressed so little—from excited discoveries of the antiquarians ("gee whiz") to mere confirmation of written accounts ("so what")? No. This paper argues that archaeologists, working as anthropologists and in conjunction with historians, have been producing new, more critical social analyses of the 17th-century culture contact situation in New England.*

*Les archéologues d'aujourd'hui qui étudient la période de contact semblent continuer une longue tradition qui consiste à accepter aveuglément le mot écrit comme "vérité révélée" servant à contrôler et à informer le dossier archéologique incomplet et forcément biaisé. Lorsqu'il existe des données historiques, les archéologues ont-ils vraiment si peu progressé, allant des découvertes enthousiastes des "antiquaires" à la simple confirmation biaisée du compte rendu écrit? Non. L'auteur soutient que les archéologues oeuvrant comme des anthropologues et en liaison avec les historiens, réalisent des analyses sociales nouvelles et plus critiques de la situation de la culture du XVIIe siècle quant à la période de contact.*

### The Antiquarians

As with so many other aspects of North America's historical past, interest in the material evidence for initial contacts between Native Americans and Europeans was first demonstrated by local "antiquarians," many of whom readily accepted the literal reality of the most romantic documentary accounts and folk traditions and eagerly sought to validate them through the discovery of objects and places associated with this exciting "known" history. As Gradie and Poirier have noted in relation to the colonial period, "the result has been research that, rather than explicating the past . . . tended to confirm its myths" (1984: 115).

In southern New England, as elsewhere, these efforts in illustrative historical archaeology were often directed toward demonstrating that early European visitors, rather than the indigenous populations, were responsible for one or another achievement of the Native American societies. These ethnocentric analyses began early, and they have continued to shape the popular image of both the early contact situation and the role of archaeology in its explication (see, for example, Kra 1981). Hence, they must be at

least briefly considered in any discussion of the development of this field of research.

While most of the standard candidates for the role of first "civilized" discoverer—including the Phoenicians, St. Brendan and his Irish monks, Thorfinn and his Norsemen, and Prince Madoc and his Welsh Indians—have their supporters in southern New England, the Vikings have probably received the most concentrated attention. The search for the actors and setting of this conjectural drama of first contact has involved a fair amount of archaeological research and attendant controversy, including Godfrey's efforts to demonstrate the colonial age of the "Viking Tower" in Newport, Rhode Island (1951a, 1951b) and a "dig" by members of the Massachusetts Archaeological Society that explored a purported Viking landing site at Follins Pond, Cape Cod (Smith 1953). It is instructive to note that neither of these efforts convinced the champions of early Viking-Indian contacts (see Pohl 1950, 1953, 1960), nor do they seem to have had any marked effect on the public's perception of these exotic local landmarks.

Another purported early contact was believed to involve the Wampanoag Indians of

southeastern Massachusetts: the avocational research was conducted by a Brown University professor. In the year 1501, according to fragmentary written records, Gaspar Cortereal, a Portuguese explorer, failed to return to Europe after a voyage that may have taken him to the New England coast. A year later, his brother Miguel also disappeared while searching for Gaspar (Morison 1971: 244–247). Here, with attempts to identify the actual site of Miguel Cortereal's New England landfall, history ends and conjecture begins.

On the shore of the Taunton River, a tributary of Narragansett Bay, on Assonet Neck, Town of Berkley, Massachusetts, lies the famous Dighton Rock, a boulder covered with Indian petroglyphs, carved initials and a large collection of undecipherable scratches. For over 200 years antiquarians have theorized about the possible authors of the "inscriptions." In the early 20th century, Edmund B. Delabarre, Professor of Psychology at Brown University, working with a set of side-lighted flash photos designed to show up the shallow surface depressions, came up with a new reading. He saw the following inscription on the face of the rock: "1511. MIGVEL CORTEREAL V DEI HIC DUX IND" together with a roughly triangular design which resembles the royal emblem of Portugal. Delabarre suggested that the V is a contraction for "voluntate" and the IND for "Indorum." The whole then reads: "1511. MIGVEL CORTEREAL, of Portugal. By the will of God, leader of the natives of India in this place."

Delabarre believed that in 1502 Miguel Cortereal came as far as Narragansett Bay in search of his lost brother. After a struggle with the Indians, in which their sachem was killed, Cortereal made himself "dux" of the Wampanoag, taking a native assistant to help him rule. He was still at Assonet Neck in 1511 when the inscription was carved, but must have been gone by 1524 when Verrazano visited the same area (Delabarre 1936: 102–103; see also Delabarre 1932).

In itself, this exercise in conjectural contact history seems harmless, if not particularly convincing. Delabarre, however, goes on to draw some less innocent conclusions: "The Wampanoags were actually, in part, a white people," the result of race mixture with members of Cortereal's crew. He cites as proof Verrazano's account, which says that "some

inclined more to whiteness" (1936: 113). Furthermore, according to Delabarre, the Wampanoag were "a superior race, a fact which might well be accounted for by early white influence and admixture of white blood" (1936: 103). Racist statements like these clearly demand refutation by modern scholars.

One more example may be useful before going on to examine more recent developments. In Charlestown, on the south shore of Rhode Island, lies the site of "Fort Ninigret," generally believed to have been the location of a 17th-century Niantic Indian village (Salwen and Mayer 1978). The rectangular "fort," with three corner bastions, is clearly delineated by a high earth and rock embankment surmounted by an iron fence. The prevailing local tradition maintains that the fort was built by Dutch traders and occupied by chief Ninigret and his people only after the Dutch abandoned it late in the first half of the 17th century.

In 1931, William B. Goodwin, a Hartford antiquarian and collector, dug at the site. His brief description makes it difficult to understand precisely what he did, but he found many European objects and very few Indian ones, and decided that the Dutch had, indeed, constructed the fort (Goodwin 1932). The local consensus was apparently rooted in the idea that Indians would not have been capable of building such a substantial structure. This idea is clearly expressed in the WPA guide to Rhode Island (Federal Writer's Project 1937: 343).

The fort was supposed for many years to have been the stronghold of the Niantic Indians, but it is now generally conceded that it was built by the early Dutch traders and used as a trading post. Bastions and other evidences of military engineering skill found in the fort, whose original outlines are now preserved by an iron fence, seem to support this theory.

Goodwin also noted that some of the visible embankment may have been a relatively recent feature. He assumed that this "restoration" had been accomplished by the State Commission which marked the site in 1881 (Goodwin 1932: 5).

In 1976 and 1977, a New York University field class conducted excavations at the Fort Ninigret site. The NYU work demonstrated that the entire embankment was a post-17th-

century construction, created with fill brought from elsewhere—at least some of it from an unknown prehistoric archaeological site! Beneath this fill was found evidence of a stockade ditch and associated Native American artifactual materials. The State Commission had evidently “confirmed the myths” with many cartloads of earth and stone.

### Early Anthropological Efforts

The few anthropological archaeologists who dealt with Contact Period sites during the early part of the 20th century do not appear to have shared the overt ethnocentric biases discussed above, but archaeology had its own version of what Jennings has called the myth of the “widowed land.” As seen by Jennings, the descendants of the Euroamerican invaders of North America sought to “smother retroactive moral scruples” by invoking a “basic conquest myth [which] postulates that America was a virgin land, or wilderness, inhabited by nonpeople called savages” (Jennings 1975: 15). The archaeological myth, which was almost universally accepted until the demonstration of the contemporaneity of human tools and extinct Pleistocene fauna in the 1930s, held that Native Americans had entered North America from the west only shortly before the Europeans arrived from the east. Presumably, this could be taken to mean that the Indians had no special claim to the continent.

In the Northeast, so distant from Bering Strait, this orientation resulted in a particularly short prehistoric time span: For example, DeForest, the 19th-century Connecticut historian frequently cited by archaeologists, suggested that the Mohegan-Pequot had only recently migrated to their eastern Connecticut location (DeForest 1851: 59–60).

The Pequots and Mohegans were, apparently, of the same race with the Mohicans, Mohegans, or Mohicanders, who lived on the banks of the Hudson. At no very ancient date, and, perhaps, not long before 1600, it is supposed that they resided among their relations. . . . Migrating towards the east, they perhaps moved along the southern border of Massachusetts until they crossed the Connecticut River, when

they changed their course to the southward, and descended upon the seashore.

Alanson Skinner, working with Staten Island materials in the early 20th century, was also attracted to explanations that invoked recent migrations. He saw “no reason why we should not accept as genuine the famous ‘Walum Olum’” of the Lenape (Delaware) Indians (1909: 29). His interpretation of the migration legend brought this group to the east bank of the Delaware River in the 16th century, barely ahead of Henry Hudson, and made it possible for him to assume that “with few exceptions, the objects found on Staten Island belong to one culture and that the historical data enables [sic] us to identify the known inhabitants as a part of the Lenape...” (Skinner 1909: 38). He attempted a partial reconstruction of Lenape culture, using contemporary Dutch and English documents in conjunction with the local archaeological collections. Unfortunately, Skinner’s model obviated the need for stratigraphic controls and thus resulted in the creation of a composite culture with a tool kit in which fluted spearpoints peacefully coexisted with the whole range of Archaic, Transitional, and Woodland artifacts (Skinner 1909: Plate VII).

Beginning about 1950, with acceptance of the reality of the Paleoindian period, the development of radiocarbon dating, and the consequent realization that long sequences of prehistoric occupations were waiting to be explored, New England’s anthropological archaeologists, like their colleagues in other parts of the United States, tended to avoid Contact Period sites, preferring to devote primary attention to earlier, “unacculturated,” manifestations of Native American life and to long sequences of prehistoric culture change.

When considered at all, the documented sites of the Contact Period were viewed as doorways into the more distant past, to be entered through application of the “direct historical approach” (Steward 1942). The method, which has also been called “upstreaming,” uses documentary sources to identify the locations of Contact Period sites of identifiable Native American groups, and then uses the Native artifactual materials from these sites to establish the material “signatures” of the societies. The type-site assemblages can then, hopefully, be used to

identify still earlier sites, or components within sites, occupied by more remote ancestors of the historic Indians.

The direct historical method had been very successfully applied by William Duncan Strong (1935) to the study of sites in the Great Plains, and Ralph Solecki, Strong's student, applied it at Fort Corchaug on eastern Long Island (Solecki 1950). Except for Foster Saville's poorly reported work at Pantigo, the late 17th- and early 18th-century Montauk cemetery on Long Island's south fork (Saville 1920), Solecki's project was the first extensive Contact Period excavation in the Long Island Sound area. Fort Corchaug proved to be a palisaded strong point and yielded large quantities of Native-made materials, European trade goods, and food remains. Solecki noted that there was evidence of "processes of acculturation," but did not choose to discuss these. He was more interested in culture-historical relationships, reporting that the Native American artifacts at Corchaug were most similar to those of the Mohegan-Pequot of Connecticut. His closing comment was optimistic: "Armed with the knowledge of the known historic, we are able to delve into the unknown prehistoric step by step" (1950: 35).

In the early 1960s, Professor Irving Rouse suggested to me that it might be possible to work at Fort Shantok, the 17th-century Mohegan village in New London County, Connecticut. To a student of both Strong and Solecki, the opportunity seemed particularly attractive and it was enthusiastically accepted. The work was designed to produce a doctoral dissertation that would explore the prehistoric origins of the Mohegan-Pequot, testing DeForest's migration theory through the faithful application of the "direct historical approach." When field work began, it was assumed that the Fort Shantok site was a stratified occupation area, where the successively deeper components would lead the researcher back into the prehistoric past.

As it turned out, Fort Shantok was not a stratified site. It was extremely rich in materials relating to the palisaded Mohegan village that occupied the area from the early 17th to the mid-18th century (Salwen 1966), but yielded virtually nothing from earlier periods. It was quite evident that the site would not provide the data required for the proposed dissertation on Connecticut prehistory, though

it did serve as the setting for short summer field class excavation programs for a number of years. Only after some five years of subsequent research, devoted increasingly to the study of anthropological approaches to Native American acculturation, did it become apparent to us that the archaeological record at Fort Shantok could provide useful information about the ways in which the Mohegan Indians handled the problems and possibilities created by the arrival of European traders and settlers.

### **Cultural Process and Cultural Change**

This new orientation toward Native American culture change provided the framework for an NYU doctoral dissertation by Lorraine Williams in which analysis of the archaeological materials from Fort Shantok and Fort Corchaug was augmented by thorough study of the pertinent primary contemporary documents (Williams 1972). This was a major step, but, in retrospect, I realize that we were not yet fully convinced that archaeological data could contribute independently to an understanding of the historical past.

Williams stated in her conclusion that "neither [ethnohistory nor archaeology] can be considered supplementary to the other. They must rather be considered equally vital and complementary sources of information" (1972: 236), but, in practice, the dissertation depended upon the documents in reconstructing the basic patterns of Indian-European relations during the 17th century and then examined the archaeological record in search of a satisfactory "fit" with this ethnohistorically reconstructed "reality." One must conclude that the author—and her dissertation advisor—believed that historical archaeology, while it might fill an occasional gap in the written record, was essentially corroborative and illustrative.

As first conceived, the Williams dissertation was to be an "acculturation" study in the classic American cultural anthropological mode, and its theoretical framework came directly from the Linton/Spicer model of "directed/non-directed" culture change (Spicer 1961). Little attention was paid to pertinent modern historical research, as the dissertation's bibliography clearly demonstrates. This theoretical

orientation seemed quite appropriate to the research goals. We archaeologists were interested in the *Indians*, and in the ways in which they responded to changes in their social environment. Because we were not really very interested in the Europeans, we were quite comfortable within a framework that came from a long anthropological tradition of research among relatively simple non-literate societies.

Shortly before the thesis defense, Williams was urged by one of her committee members to look at her data in a more "processual" way. The attempt resulted in a new chapter which applied a systems theory approach to Mohegan and Corchaug culture change. This considered the linkages among subsistence activities, wampum production, trade, and land sales, and used the shifting proportions of artifacts and food remains recovered from the site to test the predicted directions of change. Even in this context, however, there was no attempt to examine the associated changes in the colonial society. Gradie and Poirier have, quite correctly, criticized this tendency, noting that "Indians and Europeans coexist in an academic apartheid in which it is permissible to compare the inhabitants of Fort Shantok with those of Fort Corchaug but not with the residents of Norwich or New London" (1984: 122).

Two other studies of Contact Period archaeology in southern New England/Long Island region led to doctoral dissertations in the 1970s. Peter Thomas' study of Indian-European cultural interaction in the middle Connecticut River valley combined extensive documentary research with archaeological data obtained through excavation at the Fort Hill Squakheag site in Hinsdale, New Hampshire (Thomas 1979). Lynn Ceci studied documentary records and reexamined archaeological collections to develop a model for settlement system change in response to European contact in coastal New York (Ceci 1977). Broadly characterized, the Williams, Thomas, and Ceci dissertations would seem to fall within the same general archaeological tradition. All of them devote major attention to changing Native American settlement/subsistence systems, and all recognize the key role of Indian-European trade as the major mechanism for mediating contacts: the fur trade

in the inland Connecticut valley case, and wampum production and exchange in the two coastal cases. I would agree with Gradie and Poirier (1984: 122) that Thomas has been most successful in dealing with the reciprocal nature of the interactions between Indians and Europeans. I like to believe that this is, at least in part, due to the fact that the Thomas dissertation is the most recent of the three, and that it reflects the growing sophistication of the scholars working in this area!

In the 1960s, while we were working at Fort Shantok, William Simmons was excavating a Narragansett Indian cemetery at West Ferry, on Conanicut Island in Narragansett Bay, Rhode Island (Simmons 1967, 1970). His analysis of the graves and their contents differs in emphasis from the studies discussed above and has had a lasting influence on Rhode Island studies during the 1980s. Simmons devotes a good deal of attention to Narragansett belief system and religious practices. While this orientation must be attributed, in part, to the particular archaeological context, it also reflects his own special anthropological interests.

It appears that Simmons' work at West Ferry influenced the recent reexamination of the collection from the Burr's Hill Wampanoag cemetery, in Warren, Rhode Island (Gibson 1980), and the ongoing studies of RI-1000, the Narragansett cemetery in North Kingston, Rhode Island (Robinson and Gustafson 1982; Robinson, Kelley, and Rubertone 1985; Turnbaugh 1984). In the case of RI-1000, the researchers have promised to "analyze the material assemblage at several different scales, from that of the individual to that of the world system," in order to facilitate better understanding of the elements attributable to intra-cultural and inter-cultural differences (Robinson, Kelley, and Rubertone 1985: 126).

## Multidisciplinary Analyses of Contact

During the 1970s and 1980s, we have often sought a balanced application of modern social and structural historical research and historiography, coupled with contemporary anthropological theory, to analyses of historical archaeological data from contact contexts. For example, at Fort Ninigret, on the

south coast of Rhode Island, our documentary research had indicated that this was the location of a Niantic Indian occupation area that was most certainly visited by Dutch traders in the early 17th century. We were eager to work at this site because it offered the opportunity to expand our research on Native American culture change by studying another Contact Period site, contemporary with Fort Shantok but occupied by a different sociopolitical group. Excavation and analysis has confirmed both the temporal placement of the site and the presence of Dutch objects (Salwen and Mayer 1978). However, the research has also demonstrated that Fort Ninagret was not a permanently occupied village, like Fort Shantok. Instead, it appears to have functioned more like Fort Corchaug, as a sporadically occupied place of refuge, or as a seasonally utilized special purpose activity area.

In the decade between the Fort Shantok and Fort Ninigret projects, we have come to appreciate some of the ways in which modern historical research can enrich our anthropologically oriented framework. With Fort Ninigret, in contrast to the approach to Fort Shantok, we are determined to give equal consideration to both Europeans and Native Americans, examining their interactions from the points of view of both societies. Susan Mayer Reeves, whose doctoral dissertation will draw upon the Fort Ninigret data, has been encouraged to take formal training in history, and has incorporated the work of modern historians into her research proposal.

It is hardly necessary to point out that it is much easier today than it was only 20 years ago to find historians with serious interest in the histories and cultures of the Native Americans of the Northeast. One need only compare the work of Leach (1958) and Vaughan (1965) with the more recent studies of Jennings (1975), Axtell (1981), Salisbury (1982), and Cronon (1983).

### **Transition into the Mainstream**

This survey of some of the major activities and accomplishments of Contact Period archaeology in southern New England and Long Island has demonstrated that we are safely out of the "Gee Whiz!" or myth-confirmation stage

of development. Since 1950, when Solecki published the first adequate archaeological report on a Contact Period site, our work has both broadened and deepened. Major studies are now available on at least five different southern New England Native American social units (Corchaug, Mohegan, Narragansett, Wampanoag, and Squakheag). We have a synthetic study of Long Island and many shorter reports about specific places and topics. Other work is in progress.

Our research orientation has matured: we have moved from primary concentration on descriptive culture-history, through an "acculturation" approach that tended to treat culture change as an imposed one-way process, to our present concern with viewing the contact situation from the perspectives of all participants. We have also begun to outgrow the relatively narrow technology-subsistence-settlement models that we inherited from our "prehistoric past," and have started to examine social structure, ideology, and values, and to think about analysis at "several scales, from that of the individual to that of the world system" (Robinson, Kelley, and Rubertone 1985: 126).

In the course of this development, we have increasingly recognized the advantages to be gained from interaction with scholars in other pertinent disciplines, historians in particular. These colleagues approach the historical periods and situations with which we both deal from a somewhat different perspective, informed by long experience with the written record of the Western European societies which produced one set of actors in the contact drama. We are learning a lot from them. In addition, social and cultural historians have formulated questions which our particular data and approach may help to answer. The relationship should be mutually beneficial.

But despite these accomplishments, I'm not so sure that we are out of the "So What?" stage in the development of our field of research. Have we really gained any new insights or have we merely enriched our understanding of the social processes that characterize the Contact Period? This uncertainty, of course, has to do with questions regarding the role of archaeological data in situations where written records are available. I suspect that there have been and may still be strong, possibly unrecognized, tendencies in Contact

Period archaeology to relegate studies of the physical evidence of the past—its “material culture”—to the corroborative and illustrative function I referred to above in connection with my discussion of our work at Fort Shantok.

Often, written records are used to formulate models of sociocultural stability or change and the analyzed archaeological data are used to correct, clarify, and/or enhance the documentary history. Both the material remains and written records are not treated as artifacts or products of culturally patterned behavior. Rather, the material and locational remains are viewed as woefully incomplete but less biased than the written documents.

For example, when Froelich Rainey prepared his “Compilation of Historical Data Contributing to the Ethnography of Connecticut and Southern New England Indians,” he did so because he was convinced that this information “was necessary in making archaeological remains intelligible” (Rainey 1936: 3). More recently, Peter Thomas, when exploring Indian-European cultural interaction in the middle Connecticut River valley, felt it necessary to develop his model of change from historical sources, and to then partially evaluate it against the archaeological record. He believed that “a synthesis of the available archaeological literature would . . . contribute little to the problem” (Thomas 1985: 131). Similarly, McBride and Bellantoni (1982) start with an “historically derived” model of settlement systems in the lower Connecticut River valley and “test” it against locational data for Late Woodland and Contact Period sites.

These approaches, not very different from what Williams did with the Fort Shantok material in 1972, seem to be widely and uncritically accepted and practiced today. It is certainly legitimate for some purposes, but I’m not sure that this sequence of research activities utilizes the full potential of the material record. My experience in historical archaeology has convinced me that the material evidence of past human behavior provides insights that are different from, but in no way inferior to those obtainable from the documentary record. If this is so, the pattern and content of the archaeological record should be useful in formulating research questions—not only in testing them. The ideal relationship between the documents and the

archaeologically revealed patterns would appear to be a feedback loop, in which either information source could generate questions which would send us back to the other in search of new insights.

Archaeologists’ current infatuation with the written record at this particular stage in the development of Contact Period archaeology may be due, on the one hand, to its apparent richness as a result of a growing interest in Native American populations by contemporary social and cultural historians. On the other, the relative paucity of the archaeological data base may have contributed to our tendency to raise legitimate questions about the validity of the archaeological record, while accepting the contents of the written documents with very little hesitation, even when they may be “the writings of people with interests to serve” (Jennings 1975: 19). But, as noted above, this archaeological base is expanding rapidly, and its usefulness should grow more quickly than its size. At the same time, we must develop better ways in which to manipulate archaeological data in the solution of serious and important anthropological and historical questions. This is our special field of expertise, and the place where we can make our most important contributions.

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