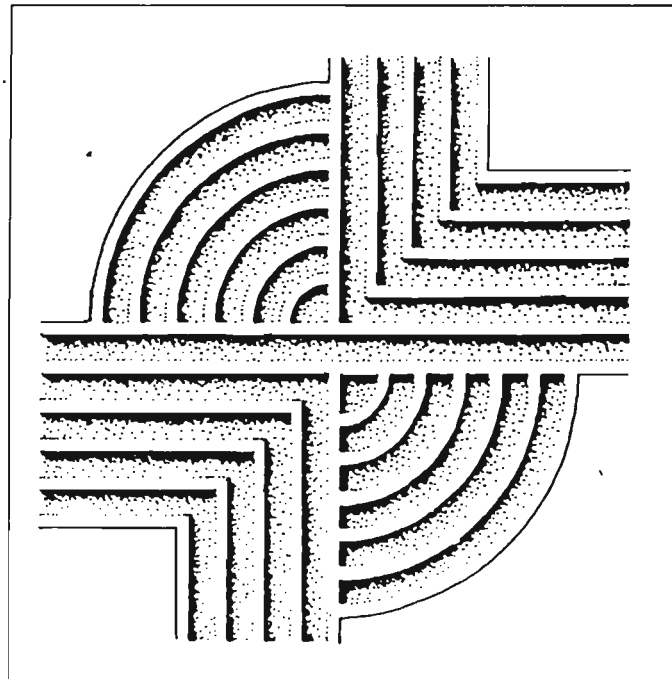


IS THERE A FUTURE FOR SHELL MIDDEN RESEARCH?



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This paper represents remarks made at the S.C. Shell Midden Workshop on October 22, 1993.

Although a previous obligation prevents me from attending the meeting, I am taking the liberty of offering a few comments from afar. Prepared ahead of time, my observations may be less cogent today than they seemed as I wrote them. I am also departing from the agenda, as I understand it, by discussing some philosophical issues rather than recounting previous investigations. Since all of our work is published and readily available, it seems more important to consider broad issues and principles rather than concentrating on potsherds and animal bones. Natalie Adams, however, will offer some observations on a recent testing project conducted to explore many of the issues raised in my discussions (Trinkley and Adams 1993). Hopefully, we will cover both the philosophical and practical sides of what we feel are the most significant issues worthy of attention during this meeting.

Before going on, let me also explain how Chicora Foundation intends to participate. We believe it is important to avoid polemics, but to offer candid observations and assessments. In other words, we will be honest, without (I hope) being abrasive. It seems that archaeologists are their own worst enemy. Too often we believe that it is not enough to succeed -- our colleagues must also fail. Too often we are very quick to use the work of colleagues as a foil against which we can make ourselves appear brilliant by comparison. Too often we fail to provide publicly available commentary on sites, since that would allow our colleagues to form judgements which might be as valid as our own. So, we are hoping that this conference will open a new decade of cooperation and group initiatives. As will become clear, we believe that if we are to change the face of shell midden archaeology such cooperation is essential.

It also seems appropriate to explain what we have seen the purpose of this meeting to be. Or more correctly, what we understand the purpose to be. We have been told that the SHPO is not convinced that shell midden research is productive. We have been told that eligibility determinations for shell middens won't continue to be accepted unless there is evidence of new and innovative research. But, I suppose these concerns have already been presented.

For what it is worth, I agree with my colleagues who have expressed concern over the quality of "shell midden" research. I agree that we (meaning both the discipline and the public) are not getting all that we could get from these sites. But I suspect that it may be at this point that I and some of my colleagues part company.

As I understand the argument, or at least that portion which has been conveyed to me, there are those who believe that this failure to obtain new information means that the sites themselves are unable to provide more information and, consequently, deserve

less archaeological attention. I believe that it represents a leap of faith, or a lapse of deductive reasoning, to believe that it is the fault of the shell middens that we are not getting all of the data we might. To paraphrase Shakespeare, the problem lies in ourselves, not in the sites.

If we wish to obtain more information from Middle and Late Woodland shell middens (it is important to mention that I have chosen to confine my comments to Middle and Late Woodland middens) there is no shortage of questions which they may address. I am sure that many of the possible questions have already been discussed over the past few days, but I will add a few:

- The ceramics themselves can be examined for information on kin based groups using cordage analysis at an intrasite level, comparing materials between a variety of discrete midden piles. Similar analysis can also be accomplished using chemical analyses of the paste, perhaps concentrating on a small array of trace elements.
- Chemical analyses of the pottery may provide clues to the clay sources, which in turn may provide information regarding seasonal (or other) rounds. These analyses may also be able, once there is a sufficient data base, to project the limits of different groups.
- Both chemical analyses and cordage studies may be useful to refine typological issues, especially when conducted in addition to more traditional paste studies. For example, this battery of analytic approaches may be able to refine our understanding of the array of clay and grog tempered Wilmington, Hanover, and St. Catherines pottery. Perhaps there is good reason to review the Mattassee Lake report (Anderson et al. 1979) and adopt a type-variety system.
- Even using different analytic approaches, such as the concept of estimated vessel equivalence, may provide a better understanding of inter and intrasite ceramic diversity. Likewise, making complete cordage analysis a standard feature of all studies would assist in allowing others to adopt a colleagues work to new and different theoretical approaches.
- Radiocarbon dating, based on relatively large charcoal samples, could be used to date a variety of discrete shell middens within one site, with 10 to 20 dates refining our understanding of site function. It might be possible to identify sufficient charcoal samples from distinct levels within the midden to allow for beginning and ending dates for individual middens (accepting one or two sigma deviations), providing even closer temporal control. Further, each charcoal date could be compared to a shell date from the same midden in an effort to develop better alternatives when there is

insufficient charcoal for a reliable date.

- Pollen analysis at individual middens could explore the nature of site vegetation, testing for evidence of site disturbance, second growth or weedy species. This information might better help us understand how, and how intensively, the sites were used. Such studies could be combined with more traditional ethnobotanical research to identify wood species for cross-checking.
- Incorporation of additional shellfish studies may be able to further refine our understanding of seasonal use, especially when several seasonal indicators are used as cross-checks from discrete midden areas. It may also be useful to examine middens on a shellfish assemblage basis in an effort to reconstruct specific ecotonal use areas.

Just as it is possible to point out some potential research questions, it is also possible to outline successful research techniques. For example:

- Expanding recovery techniques to incorporate 1/8-inch and even 1/16-inch screening allows better dietary reconstructions by providing more complete, and representative, recovery of floral and faunal materials. For example, we have found that at one tested site we were unable to document the presence of fish bones unless 1/8-inch mesh was used. Failure to use this level of recovery would have resulted in a flawed interpretation of site dietetics. We have also found from investigations at several Middle Woodland middens that this level of recovery can be achieved only through water screening.
- Close interval auger testing is a fast, reliable approach to better understanding intrasite variability and patterning. We emphasize close interval, meaning minimally 20 feet, since larger intervals completely fail to identify discrete midden areas. While 20 foot intervals provide basic information, we have good reason to believe that 10-foot intervals will provide data essential to understanding site organization. Auger testing is selected over shovel testing since it provides a consistent sampling approach unaffected by tree roots, shell density, and enthusiasm. This consistency is essential for the creation of meaningful density mapping.
- Site contour mapping at a 0.2 foot interval, combined with close interval auger testing, provides a comprehensive view of shell midden dispersal and allows the most accurate site patterning studies. Intervals even of 0.5 foot will often fail to provide the degree of precision essential for this type of study.

- The samples that are traditionally used at shell midden sites are totally inappropriate for the level of precision necessary to address comprehensive research questions or safe guard the public's right to know their heritage is being adequately protected. While this has gradually become apparent to us, it is perhaps best demonstrated quantifiably by Dennis O'Neil's recent article in *American Antiquity* (O'Neil 1993). Many of the research questions we have outlined, and likely an equal proportion of those questions offered by our colleagues, require large sample sizes to be effectively examined. We need to think at the level of minimally 20 to 30% sampling, not at the level of 0.5% sampling.

- We also believe that the only way to address these significant research topics is through intensive hand excavation. While data recovery programs which rely on large scale stripping as the primary recovery approach might, in some circumstances, allow features to be rapidly uncovered, such programs also destroy the context of these features, eliminates the small quantity of artifacts typically found at Middle Woodland shell middens, reduces the opportunity to explore intra-site research questions, eliminates the opportunity to explore extensive radiocarbon dating, and seriously constrains the research options at any given site.

I could go on, but I believe the point is made -- Middle Woodland shell middens have the potential to offer us much information. But we will need to expand the questions being asked and, perhaps most importantly, expand the ways in which we ask these questions. In other words, we can't continue with business as usual and expect to get exciting new information. Put another way, you get what you pay for. And all of the research strategies and research questions I have outlined cost money.

This brings us to a central theme of my observations, which is that we must change the way we do archaeology. As long as shell midden archaeology is low-bid compliance archaeology we are doing a disservice to both the public and the discipline. We are doing the public a disservice since we are failing to adequately protect and preserve their heritage. We are doing a disservice to the discipline of archaeology since we are failing to explore the limits of our field, we are ignoring the anthropological roots of archaeology, and we are retreating into a self-serving shell of mediocrity.

If we want research then we must be willing to stand up and demand that archaeology be funded at a level to allow research.

There must be funding sufficient to allow large segments of the site to be explored.

There must be funding sufficient to allow interdisciplinary research with chemists, soil scientists, geologists, and others.

There must be funding sufficient to allow comparison with other sites.

There must be funding sufficient to allow adequate publication and dissemination of reports.

There must be funding to explore sites outside the realm of normal compliance, to ensure that we are seeing the total range of cultural behavior.

There must be funding to explore many sites of the same type, not just a small handful.

Failing this level of adequate funding the site should not be touched -- by either archaeologist or developer. From our perspective, this is not a debatable point. We are dealing with a non-renewable resource which is quickly being depleted. To do otherwise is unthinkable.

Before going on, it is important to back track briefly and comment on the need for quantity, as well as quality. In spite of our experience with "hard" science, archaeologists often have difficulty speaking up for research. After looking at a small handful of sites of a particular type we are satisfied to go on to something different -- feeling that we have exhausted research, proven our point, or that no one is likely to continue funding further research. This is very troubling for a variety of reasons.

First, there is still much more that we don't know than there is that we do know. This means that we are nowhere near redundancy at any site type in South Carolina. Second, since we can never prove scientific theories, even those concerning cultural behavior, but can only disprove them, continued research is essential for the growth of the discipline. The establisher of a new cultural theorem must be "lucky" every time it is tested, while the researcher seeking to debunk the new theorem must be lucky only once. Third, much research is at the level of only offering "signposts" for those that follow. This is essential and represents the gradual growth of knowledge. Halting research because we have looked at three or thirty such sites also halts our learning process. As long as valid research questions can be posed, or as long as previous research can be refined or further tested, then research at the site should be funded. Finally, while some may see this as nothing more than supporting archaeological employment, I suggest that as long as our state and its heritage are being developed for profit, some of that profit can be used to promote a better understanding of what we are losing.

The next question is how do we break the cycle of low bid archaeology? I would like to think that one outcome of a meeting such as this might be an agreement to expand the level of research, accepting that the minimal level of research at a Middle Woodland shell midden might be \$90,000 and 4 weeks, rather than \$20,000 and 2 weeks, but I know that such approaches don't work. Even OPEC, with basic cultural, ethnic, and

philosophical principals, can't agree on basic pricing. I doubt that archaeologists would be likely to agree on basic research goals. Consequently, it may be obvious that I am calling on the State Historic Preservation Office to break the cycle of low-bid archaeology, recalling my earlier warning that research costs and that you can't get something for nothing.

This will not be easy, since it must be done in such a way as to:

1. ensure a level playing field, providing no undue advantages to any organization, group, or individual;
2. allow maximum freedom to explore approaches, questions, and technology, while still ensuring that basic goals are met;
3. ensure comparability and availability of research;
4. emphasize research goals and ensure adequate funding levels; and
5. provide adequate incentives to encourage research.

This last issue is worthy of special, albeit brief attention. As we all know, research is best undertaken, and results are most likely to be achieved, in an open framework of discussion, debate, and interaction -- both in-house and among the profession. This raises a number of issues. For example, in the reality of both for-profit firms and even non-profit foundations, time is money. We are different from, for lack of a better term, hard-money tax-supported organizations where individuals are paid to sit around and "think."

How can the organizations which conduct the vast majority of research on shell middens be assisted and encouraged to engage in brain storming?

When many of the research issues outlined will require weeks to months of investigation, how can clients be convinced to wait for reports to be completed before they can begin the development process, especially since time is also money for them?

Who will pay for the extensive amount of comparative research required to synthesize and extrapolate?

Since so much of the research we have outlined involves special expertise, how can we encourage inter-disciplinary research efforts? In other disciplines, just as in archaeology, research interest hinges on funding. This is especially true for rather esoteric research on trace chemical studies of clay where there is relatively little available research. If we wish to make advances we will need to fund much base-line research, which translates into money for graduate students, as well as the use of

specialized equipment. How will this be funded?

In summary, I realize that I have raised many more questions than I have answered, but it seems essential that we explore the issues. We are faced with complex problems and they cannot be solved by simple solutions, or edicts. Too often we (meaning Americans) lack patience and rush into complex problems expecting simple solutions to be sufficient. Then we are stunned when the problem goes from bad to worse. Perhaps Albert Einstein said it best:

The significant problems we face cannot be solved at the same level of thinking we were at when we created them.

Consequently, I offer a suggestion. Rather than using this workshop as an immediate spring board for mandated solutions, why not try a different approach. Why not ask that all of those conducting shell midden research (including the State Historic Preservation Office) to take five steps:

1st, to strive to open communication and cooperation by putting aside differences. This goal would be achieved by providing colleagues with complimentary copies of all shell midden research -- management summaries and reports.

2nd, to incorporate new research into existing projects on a voluntary basis and to incorporate new research into new projects based on a formulae developed by the SHPO, in consultation with the professionals conducting research. This goal would be achieved by the SHPO actively supporting research endeavors through project review and the publication of an open letter to developers outlining that the research needs of the state will cost more money -- period.

3rd, to widen the scope of shell midden research to include the development of special expertise and conduct feasibility studies. This goal would be achieved by the State Historic Preservation Office releasing additional state grant funds ear marked for interdisciplinary shell midden research, available at a 75-25% match, and allowing maximum flexibility of match sources.

4th, to ensure that shell midden research is made available to the public, helping to ensure that the public's heritage is advertised. This goal would be achieved by the State Historic Preservation Office offering incentives to developers for support of public education programs, making educational programs involved with shell midden research a priority for FY1995 survey and planning funding, and exploring alternative sources of collaborative efforts.

5th, to re-open discussions a year from now for the specific purpose of evaluating the progress made.

Such a program would allow maximum flexibility of in-house research, even in the compliance frame work; allow for new and innovative research ideas to be explored outside of the compliance frame work; provide the public with access to their heritage; ensure that the successes and failures were made widely available to other researchers; and provide for an opportunity to determine whether the program was working.

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