

Statement by
Seminar Participants
*on the Present Looting of
Shipwrecks in Florida and Texas*

Shipwrecks and wreck sites in Florida and the Gulf Coast are currently being looted on an unprecedented scale by professional treasure hunters and amateur divers. These wrecks, many of them dating back to the sixteenth and seventeenth centuries, are an irreplaceable resource for archaeology and anthropology. They offer unique opportunities for studying human behavior in relation to a wide range of maritime-related problems. Looting by treasure hunters and amateur divers destroys these sites. Recent court decisions in Florida and Texas can be expected to produce even more looting in the near future.

We want the anthropological profession to be aware of this, and we ask our colleagues to take every opportunity to oppose this activity. Our position is that the same scientific, legal, and ethical standards that apply to archaeology on land should also apply to archaeology under water. Archaeology for gain, by selling gold and other materials taken from wrecks for personal or corporate profit, is not acceptable. Nor is any indirect involvement by archaeologists in activities that foster a market in such antiquities. We urge that our colleagues refrain from working or consulting for treasure hunters and avoid trafficking in gold or other loot taken from wrecks. Professional archaeologists will need to consider carefully any action they may take that could

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support treasure hunters, and they should consider the implications of anything they might do that affects these wrecks and the materials taken from them.

The position taken here is entirely consistent with that of the Society for American Archaeology ("Four Statements for Archaeology," *American Antiquity* 1961 (27): 137–38) and with the views expressed recently by Karen D. Vitelli ("The ABCs of the Antiquities Market," *Early Man*, Spring 1982, pp. 29–32).

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Looking Below the Surface:

Shipwreck Archaeology as Anthropology

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Consider the predicament of an underwater archaeologist about to confront a series of wrecks in a setting like Truk Lagoon or the Great Lakes, where the materials consist of ships wrecked fairly recently and on a massive scale. The accepted image of shipwreck archaeology is based on work done on wrecks of great classical or historical antiquity and finite or limited scale—for example, amphora wrecks of the Mediterranean or Spanish wrecks from the Armada of 1588. Descending into Truk Lagoon or Lake Superior, our hypothetical underwater archaeologist soon perceives that the inherited wisdom of wreck archaeology may not help him as much as he would like. For one thing, the sizes and numbers of ships involved preclude any realistic attempt at total excavation. And their relative recency, in some cases as little as thirty-five or forty years, confounds the conventional view of archaeology as dealing only with the ancient past. What is our hypothetical underwater archaeologist doing here anyway?

In fact, as the readers of this book probably know already, there is nothing hypothetical about this situation. Underwater archaeologists, especially those working for various public agencies such as the National Park Service, are often called upon to deal with these kinds of shipwrecks, along with those of a more conventional nature. As this

kind of work proceeds, the question of an appropriate scholarly rationale inevitably arises. Most underwater archaeologists agree that mere relic collecting will not suffice, even though this is the aspect of their work that is likely to have the most appeal to the lay public (along with the adventuring and treasure-hunting aspects that often accompany relic collecting). The dangers of what Ivor Noël-Hume (1969: 10–11) aptly refers to as the “shrine complex” are well known to most archaeologists. That is, an undue emphasis upon the excavation and restoration of relics associated with national heroes and historically famous accomplishments can lead to the neglect of less spectacular, but possibly more informative, sites and materials.

Historical particularism—and I do not use this term here in any negative sense—justifies much of the current work in shipwreck archaeology, since shipwrecks are undeniably part of the total body of material studied by historians and historical archaeologists. Again, most professional archaeologists and enlightened amateurs agree that historical sources should be mastered and used in a detailed and competent way whenever they are available. Noël-Hume’s negative views notwithstanding (1969: 12–13), most of the anthropologically trained archaeologists I know who have become involved in historic archaeology, including the study of shipwrecks, acknowledge the need to know the documented history of the materials they study, including mastery of relevant languages and detailed knowledge of the technologies and historic sources they use. The shipwreck archaeologist, like any historical archaeologist, must first examine the documentary information that is available. This historical expertise can be delegated, to a degree, since no one scholar can reasonably be expected to master all the various special skills needed for competent archaeological research. Indeed, this kind of division of labor has long been a standard practice of both anthropological and historical archaeologists. In other words, historical skills having to do with such things as documentation of pottery, metalworking, and other industries that produce archaeological remains are essentially similar to specialized expertise in such fields as faunal analysis, lithics, palynology, and other fields that have supported archaeology for a long time. The argument I am offering here, by way of an overview of this School of American Research seminar volume, is that we will achieve more by viewing archaeology as a unified approach to the study of human behavior than by arguing over alleged differences between “historical” and “anthropological”

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archaeology. Shipwrecks are part of the legitimate domain of archaeology and can produce results that are as significant for our ability to explain variability in human behavior as any other kind of archaeology, whether it deals with stone tools in a European Paleolithic rockshelter or ceramics contained in a sixteenth-century Spanish shipwreck.

If one provisionally accepts this argument, then the next question is: What is it that unifies the field of archaeology in spite of differences in the materials being studied and the skills required to study them? My suggestion is that the unity of archaeology rests not with the materials being studied or the particular methods that are applied but in the reasoning used to draw conclusions about behavior from the physical remains of past human activities. Discussions and debates that occurred during this seminar and the papers presented by the seminar participants offer ideas and evidence that provide us with a preliminary indication of how the study of shipwrecks can inform and enlarge our general view of man's relationship to his maritime environment, especially with respect to voyaging and matters of commerce, warfare, and other relevant factors.

In planning this seminar, variety was sought and achieved. Participants included both "land" and underwater archaeologists. Historical, classical, and anthropological traditions in archaeology were all represented, as were more specialized approaches like ethnoarchaeology, experimental archaeology, and public archaeology. So, in searching for unifying principles, one should not assume beforehand either that the views of all of the participants in this seminar were identical or that the ideas and conclusions arrived at represent a complete or final statement about the essential nature of shipwreck archaeology. Indeed, the liveliness of the debate at times indicated that differences between land and underwater archaeologists extend considerably beyond the fact that one species wears cowboy boots while the other wears flippers. In this summary chapter I shall try to indicate what I think are the approaches and reasoning that link shipwreck archaeology to the larger domains of social history and science. But I shall also attempt to do justice to the differences that exist under the intellectual umbrella of archaeology, leaving it up to the individual reader to decide how much variety his or her particular brand of archaeology can accommodate.

On a more personal note, let me also explain why this book is dedicated to the memory of Keith Muckelroy. I first met Keith in Cambridge in 1977 while he was seeing his book, *Maritime Archae-*

ology, through the editing process for publication. We had several opportunities to discuss his work and what he saw as the future for shipwreck and underwater archaeology, and I was much impressed by his enthusiasm, his considerable knowledge of the field, and his intellect. So it was only natural, when the idea of this seminar was first suggested to me by Douglas Schwartz, Dan Lenihan, and Larry Murphy, that Keith's name appear at the head of the list of possible participants. In due course, I invited Keith to participate, and he accepted the invitation. It was a profound shock to learn that Keith drowned in a diving accident in Loch Tay on September 8, 1980. This feeling of loss was shared by all of the other seminar participants, and it is therefore our wish to mark his memory by dedicating this volume to him.

In attempting an overview of the seminar, I shall first examine the goals and reasoning that seem to be emerging in shipwreck studies and then look at the implications these goals and arguments have for the ways in which shipwreck archaeology can, or perhaps should, be done.

SHIPWRECKS AS "DEEP STRUCTURES"

This pun is not intended to suggest that French Structuralism or any other particular school of anthropological thought should be brought to bear on shipwreck archaeology. But it does imply that generalizations about various ways the human species has adapted to the conditions of voyaging and its use of the maritime habitat may be possible on the basis of evidence provided by shipwrecks. Anthropology has been a useful source for generalizations about human behavior, but, as Lenihan cautioned during the discussion of his paper, there is nothing inevitable about the relationship between shipwreck archaeology, or any other kind of archaeology, and anthropology. This relationship, which has seen its greatest development among American-trained scholars, is not a necessary one, and its value must be demonstrated by those who advocate it. It is with this point in mind that we must view the questions raised by Bass as to whether or not an anthropological approach really adds anything to our understanding of human behavior connected with shipwrecks. In other words, the burden is on anthropologically oriented archaeologists of all kinds, including shipwreck specialists, to demonstrate the explanatory value of their ap-

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proach. Also, there is what Watson referred to in discussions as the “present inchoate nature of anthropology,” by which she means that anthropology today is too varied to offer a consistent basis for attempting scientific generalizations about human behavior. Thus, she cautions, archaeologists should be careful about jumping onto some sort of anthropological bandwagon when the anthropologists themselves are uncertain about what kind of social science, if any, they are involved with.

With these cautionary points in mind we can begin to consider what kinds of theoretical “glue” might lead shipwreck archaeology to adhere to the rest of archaeology and provide a basis for larger generalizations about human behavior. All of archaeology is based upon the study of various material residues of human activities. The links between these residues and the behavior that produced them are at the center of all archaeological reasoning. Most flaws in archaeological reasoning turn out to be failures of one sort or another to account for all of the links involved in this relationship in any given case. For example, it was common for archaeologists studying Paleolithic and Paleo-Indian sites to identify concentrations of flaked stone artifacts and debris in habitation sites as stone-chipping workshops—in other words, to equate the material by-products of a particular activity with the physical locus of that activity. Increased awareness by archaeologists of the complexities of behavior relating to how different materials come to rest in a given physical context in actual habitation sites has led to a reexamination of that assumption. Indeed, archaeological associations of all kinds are now being reexamined in this manner, with respect to both natural and cultural factors that affect their occurrence, and the results of this kind of examination are proving beneficial to archaeological interpretation.

Wrecks in general, and shipwrecks in particular, are subject to this same kind of examination. Recent work in shipwreck archaeology reveals systematic and skillful efforts to control for a wide array of natural variables such as currents, sea bottom conditions, salinity, and other factors in explaining the particular characteristics of different kinds of wrecks (Muckelroy 1978: 157–213). But comparable controls in explaining how behavioral variables operate to produce different kinds of physical associations are still uncertain and untried. Ambiguities abound in this domain, and this seminar, above all, has ad-

ressed the issue of identifying consistent and reliable relationships between particular kinds of human behavior and certain shipwreck remains.

DIG WE MUST—MUST WE DIG?

If one reads George Bass's (1975) autobiographical account of the development of shipwreck archaeology in America or other, similar books that describe the beginnings of shipwreck studies by English and European scholars (Throckmorton 1969), it becomes clear that this field is now in a period of transition. The pioneers have established momentum for shipwreck archaeology by gaining popular support on the one hand, mainly through books and films, and scholarly results on the other hand in the form of reports on wrecks like the Bronze Age ship at Cape Gelidonya, Turkey (Bass 1961; Bass and Throckmorton 1961, 1967) and systematic publication of research results in special journals like the *International Journal of Nautical Archaeology and Underwater Exploration*. Despite early resistance (Bass 1975: 127–30), classical archaeologists and historians have come increasingly to appreciate shipwreck studies as a valid scholarly approach. The stigma of underwater archaeology as somehow little more than sport diving or treasure hunting has been effectively dispelled.

There have also been dramatic advances in the technology of underwater archaeology, from the invention of scuba apparatus during the 1940s in France by Jacques-Yves Cousteau and Emile Gagnan to recent developments such as side-scanning sonar and minisubmarines. Although many problems remain, it seems fair to say that technological advances and increasing ingenuity in their application mean that the controls available to shipwreck and underwater archaeologists are at least as good as those used by excavators on land.

For land archaeologists in every part of the world there has or will come a time when even greater scholarly and intellectual gains can be made through careful analysis than simply through continued excavation. During the pioneer phase of archaeology in any region, the well-known Consolidated Edison Company's motto "Dig We Must" applies, since the first priority is always to establish secure chronologies and time-space relationships for material assemblages derived from controlled stratigraphic archaeology. But as these assemblages begin

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to repeat themselves within a given area, and as the number of excavated collections and site reports grows, this motto is increasingly replaced by the question, Must We Dig?

This is not a call to archaeologists to drop tools and cease excavating. Digging and its underwater counterpart will always continue to play a key role in mature regional archaeology. Rather, it is a call for more selective digging, and now is the time for shipwreck archaeologists to consider the principles that will guide the process of selection. In looking back over the discussions during this seminar, one can identify several of these guiding principles.

Survey Instead of Search

During an orientation session intended to acquaint the land archaeologists with some of the problems and approaches of underwater archaeology, Larry Murphy emphasized the importance of surveying rather than searching for wrecks. This idea was later reinforced in his paper by this discussion of wrecks found in the vicinity of Isle Royale in Lake Superior. In both instances, Murphy advocated a regional approach to the study of wrecks, emphasizing the need for systematic methods of sampling. He approached this issue from the viewpoint of a public archaeologist whose primary concern is to identify zones for the protection of different sorts of cultural resources. Later discussions, however, emphasized the wider implications of this argument. Perhaps the easiest way to summarize these discussions would be for me to suggest that there may be at least two kinds of survey strategies involved in studying shipwrecks. First, there are *surveys of elimination*, by which I mean surveys that systematically eliminate ambiguities about what wreck materials may or may not be present within a given area. In surveys of elimination, the aim is to cover an area completely by means of a reliable sampling technique in order to determine unambiguously both the presence and absence of wreck materials within the area. And, second, there are *surveys by design*, in which the particular wreck materials being surveyed are selected on the basis of a hypothesis. Those wreck materials are, in fact, whatever is required to test the particular hypothesis. So, for example, when Murphy offered the “one more voyage” hypothesis in his paper, he also specified what materials an underwater archaeologist would need to look at during a survey in

an area like the Great Lakes in order to test this hypothesis effectively.

In this case, the argument is that iron or steel ships have longer use-lives in the Great Lakes, which are freshwater bodies, than in their saltwater counterparts because there is less damage to hulls from corrosion. Or, in freshwater contexts, hulls last longer than machinery such as engines and steering equipment. Many shipowners today tend to extend the life of their ships beyond safe limits, as is amply demonstrated by recent hazards posed by aging supertankers (Mostert 1974) and the widespread use of flags of convenience by owners to avoid regulations that might bring about their ships' retirement. Murphy's hypothesis is that shipowners may always have been tempted to use their vessels beyond their normal, safe use-lives. To test such an idea, one would have to examine the wrecks in the Great Lakes to see if it was failure of the hulls or failure of the machinery that was the primary factor leading to the loss of these ships. By asking this kind of question, Murphy is presenting a hypothesis that is capable of being disproved. Certainly, an underwater survey in the Great Lakes that consistently determines hull failure as the primary cause of these losses could effectively disprove this hypothesis, especially if compared with wrecks in saltwater. In this case, the research design demands a test of this kind, and the survey would, of necessity, focus on those elements of ship construction (i.e., hulls, engines, steering machinery, etc.) that provide this test.

Ideally, both strategies are combined in an effective survey to ensure complete regional coverage as well as attention to relevant details of ship construction, marine architecture, or whatever the hypothesis demands. Anything less than a complete survey, in both senses proposed here, will leave important questions unanswered. For example, during a search an archaeologist may find several exciting wrecks without knowing for sure whether other wrecks of equal or greater importance exist in the same area. Any decision to allocate funds and resources for excavation of these wrecks will need to take this kind of question into consideration. Of course, as in land archaeology, certain regions have been so little explored that searches are still the only feasible first step in opening up the area to archaeology. But as work proceeds further, as it did in places like the American Southwest and Great Basin (Thomas and Bettinger 1976), so, too, grows the importance of systematic regional surveys based upon hypotheses that can be tested.

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It is in these “mature” areas of archaeological research, as opposed to the “pioneer” areas, that shipwreck studies, like land archaeology, need to place greater emphasis on surveys rather than searches.

Partial Instead of Total Excavation

Total excavation of wrecks would be ideal if one were planning to attempt detailed reconstructions of ancient ships and trying to understand such essentials as their cargo capacity and overall dimensions. Yet all kinds of practical limitations impose themselves in the real world of underwater wrecks. Incomplete preservation, damage from looting or teredo worms, and other factors constantly impinge upon the totality of underwater archaeology, just as they do on land. So, understandably, shipwreck specialists can increasingly be heard to argue for more effective use of limited remains:

Nearly complete old and ancient hulls do not solve all the mysteries. They only provide us with enough intelligence to notice new ones. . . . There are . . . discoveries to be made on many poorly preserved hulls, if only we take the trouble to carefully scrutinize them (Steffy 1978: 53).

To the limiting factors mentioned above, we can also add problems of funding and support for excavation that are well known to all archaeologists. In short, there is no perfect world in which total excavation can consistently take place, and we must, as Steffy (1978) exhorts, try to obtain “maximum results from minimum remains.”

But practical problems are not the only ones that may limit the totality of shipwreck excavation. Even if one had unlimited funds and time for research, it would not always be a good idea to attempt total excavation. The kind of careful scrutiny Steffy is advocating can be applied to any wreck, including one in a good state of preservation, if that scrutiny is directed toward solving a problem or testing a hypothesis. Not only is it impossible to attempt total excavation of extremely large wrecks like Great Lakes ore carriers or Japanese transports from World War II, but it may even be undesirable. As Bass pointed out during discussions, the new willingness by archaeologists to examine everything from early, prehistoric wrecks to recent historical or even contemporary wrecks means that we do not have to depend upon well-known historic wrecks in places like the Mediterranean, the Car-

ibbean, or the British Isles, even though we tended to limit our studies to these in the past. These are all fair game for archaeology. This is also what Sonny Cockrell referred to as the “unexamined assumption” of shipwreck archaeology, namely the traditional emphasis on wrecks from historically prominent places like the Mediterranean. Instead of limiting our research to such obvious cases, Cockrell argued, we can now turn our attention to everything from canoes to modern ships. But our success in dealing with this widened range of shipwreck evidence will ultimately depend more upon the questions we ask about them than upon the degree to which we excavate them.

Taking up the theme of sampling along with questions about totality of excavation, Mark Leone raised the possibility, both in his paper and in discussion, of regional approaches to shipwreck archaeology. He disagreed with the argument by Basch (1972: 50–52) that shipwreck archaeology is weakened by its inability to identify with certainty the port of origin of a ship from its remains, or where it was originally built. While many aspects of ships and life aboard ship are undoubtedly independent of particular regions, Leone emphasized that regional studies can be extremely informative about the variability of maritime adaptations in relation to different local or regional conditions. For example, look at the specialized steamboats designed for use on the Tombigbee River and other parts of the “western rivers” system of Alabama and Mississippi. Three of the seminar papers deal explicitly with the explanatory possibilities of using a regional approach and comparing the results—Leone’s study of Chesapeake Bay, Schmidt and Mrozowski’s on Narragansett Bay, and Murphy’s references to Great Lakes archaeology. Each region created special conditions that affected maritime behavior and the wrecks that resulted in different ways. But, as Leone noted, regional approaches to shipwrecks will be effective only if archaeologists are willing to be selective in their excavation and survey practices, by asking important questions and seeking evidence relative to those questions. Thus, when Schmidt and Mrozowski ask general questions about the nature of contraband behavior as inferred from archaeological materials, the Narragansett Bay area takes on special importance for the anthropologist. Similarly, Leone’s ideas about capitalistic political and economic relations can be effectively applied to shipwrecks in the vicinity of Chesapeake Bay. Leone’s assertion that anthropologically useful questions about vari-

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ability in human behavior in relation to shipwrecks and remains connected with seafaring can be approached effectively on a regional basis is strongly supported by these papers.

Explicit Instead of Implicit Research Planning

Possibly one of the most jargon-ridden and misunderstood aspects of contemporary archaeology is the idea of research design. In some quarters this phrase has become almost a catechism, ritually chanted by grant applicants and review committees alike. Like all such formulae, the concept of research design needs to be examined from time to time, especially when it may be applied to a domain of archaeology where it is not usually discussed. Nothing could be more fatuous than declarations by land archaeologists to the effect that shipwreck and underwater specialists do not understand or employ research designs, where the implication is that shipwreck archaeology is a sort of underwater Easter egg hunt. With respect to this particular notion, perhaps, the old stereotypes of the shipwreck archaeologist as a sport diver or treasure hunter linger on most noticeably.

Suffice it to say that shipwreck archaeologists today do ask questions and design their archaeological research to answer those questions. These questions range from matters of nautical detail, such as the differences between “shell first” as opposed to “frame first” types of hull construction (Muckelroy 1978: 59–69), to larger questions involving explanations of major historical developments, like the early influence of Middle Eastern bronze working on the technological and artistic traditions of Mycenaean Greece (Bass 1975: 58–59). These are different sorts of questions from those ordinarily asked by anthropologically oriented archaeologists, in that they emphasize particular details of nautical history and particular historical traditions and issues, but they are no less valid. Land archaeologists, too, must deal effectively with such particularistic questions in their research, whether or not they attempt to extend their findings to encompass larger generalizations about variability in human behavior.

Perhaps it is in the realm of explicitness that shipwreck and land archaeologists today differ most. Gary Stickel’s paper develops this idea most fully, although it is discussed in other papers as well. The question here is whether certain large, perforated stones lying in the waters

offshore from Redondo Beach, California, are a by-product of Asian seafaring or can be accounted for by some other, alternative hypothesis. This paper is not so much about the artifacts themselves as it is about the kind of research design needed to resolve problems of this sort. What might at first seem to be theoretical “overkill” with respect to the formal steps involved in such a project can be rationalized as a way of making explicit steps that have generally been assumed or glossed over in underwater archaeology. Opinions may differ about the utility of such an exercise, since the paper is not about shipwrecks per se, and because some archaeologists may regard these steps in archaeological reasoning as obvious and unnecessary. Yet, in defense of this approach, it should be noted that remains of this nature, whether underwater or on land, lend themselves to science-fictional treatment, in the manner of Mu, Atlantis, and “chariots of the gods,” unless they are dealt with in an unusually rigorous and convincing way by archaeologists. The case of the Bimini pavement (Valentine 1976)—rows of rectangular stone blocks under water in the Bahamas—might well profit from a similarly rigorous archaeological treatment in order to dispel any ambiguities that might linger concerning these remains.

Cheryl Claassen also addresses the problem of explicitness in research design in her paper on experimental approaches in the field of nautical archaeology. She distinguishes between the goals of replication and the controlled handling of variables in experimental archaeology related to ships and shipwrecks, and she identifies the latter as being more consistent with both the procedures and goals of experimental science. When she states that “there is no problem in archaeology that cannot be better understood through the use of a well-designed and well-executed experiment,” she is emphasizing the idea that the effective control and manipulation of variables with the aim of testing alternative hypotheses is the essence of good research design.

A look at the seminar papers reveals many areas where controlled experiments of the kind Claassen is advocating could be usefully applied to problems in shipwreck archaeology. The “cannonball controversy” and Wignall’s (1973) claim that the brittleness of sixteenth-century Spanish shot accounts for the relative ineffectiveness of Spanish gunnery against English ships during the Armada battles of 1588 is a case in point. What is needed now is a controlled test of both quenched and unquenched cast iron to determine the differential properties and behavior of these metals in response to various compressive forces and

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shock. Wignall's hypothesis is fine as far as it goes, but as Claassen pointed out during discussions, it fairly cries out for further by-product testing of the materials involved in order to resolve the question. Further debate of this issue in the literature by historians is unlikely to settle the matter until such test results are available. Then, too, there is the problem of shipwrecks in the Great Lakes, where experiments comparing the relative fatigue rates of metals due to corrosion vs. mechanical stress could be usefully applied to test Murphy's hypothesis. Indeed, the literature on seafaring and nautical history abounds with such possibilities for experimental applications. Take, for example, Drake's raids on Spanish shipping in the vicinity of Sagres in 1587. Historians like Mattingly (1959: 121) have argued that, while the raids' military achievements were not great, they did have a profound effect upon the Armada of 1588 by reducing the Spaniards' ready supply of seasoned barrel staves. Although it may have seemed trivial in 1587, one of the Armada's most acute problems later on concerned spoilage of water and provisions in casks made of unseasoned wood. Controlled experiments to compare spoilage of water and various kinds of food in barrels of seasoned and unseasoned wood could not only test this assertion but also provide information on relative rates of spoilage under these different conditions.

Ethnoarchaeology, too, calls for explicit research design, and there are many opportunities for ethnoarchaeological applications in maritime archaeology. Seminar discussions about the use of ethnoarchaeology in shipwreck studies hinged upon the relevance of uniformitarianist assumptions in connecting events of the past with the present. Ethnoarchaeology goes considerably beyond what Muckelroy (1978: 234) refers to as parallelisms in maritime behavior and technology observed in contemporary ethnology and surviving cultural traditions as applied to the past. He correctly noted that "consideration of other ethnological material can serve a very useful purpose in freeing the researcher from the restricted concepts of his own technical tradition" (Hasslöf, O., 1963, quoted by Muckelroy 1978: 234–35). He further points out that ethnographic studies can provide a greater range of alternative possibilities for explaining particular details of ship or boat handling and construction than might be possible without such studies. While true, such a limited view of ethnoarchaeology can present difficulties, too, because as Leone and Gould both pointed out during the discussions, such ethnographic parallels or analogues can

be self-limiting in that they omit alternative possibilities that may have no existing or known ethnographic counterpart. This problem is well known to ethnoarchaeologists who work on land (Freeman 1968; Gould 1978: 29–36), and it can be expected to apply to maritime archaeology as well, especially in non-Western contexts. For example, what ethnographic analogues are there today for the kind of voyaging out of sight of land that we know, on archaeological grounds, occurred across Wallace’s Line from Southeast Asia to Australia–New Guinea at least 35,000 years ago (Mulvaney 1975: 130)? As the scope of shipwreck archaeology expands beyond the domain of European traditions of boatbuilding and seamanship, so, too, increase the dangers of applying ethnographic and historic parallels directly to our explanations of past human maritime behavior.

Leone and Gould both asked: What can shipwreck archaeologists do to discover general principles that hold true for both past and present-day human nautical behavior? Such an indirect approach offers a way of escaping the limitations imposed by self-limiting ethnographic parallels, provided, of course, that one has first established a reliable uniformitarian basis for such principles and can also specify the test implications of those general principles in relation to wreck remains. Gould’s paper is, in fact, a trial effort to do just that, by specifying the archaeological “signatures” that characterize the behavior of combatants who are adapting to the stresses of extreme defensive isolation during war. This, too, was the intent of Murphy’s “one more voyage” hypothesis, which may or may not emerge as a general principle of commercial seafaring, depending upon its testing in the course of further archaeological research. Whether or not such general principles of human behavior hold true for all times and places is less important than the way these principles can direct our efforts at archaeological research in specific directions that extend beyond any ethnocentric or otherwise self-limiting explanations.

In short, it is important to be explicit about our use of ethnographic and historical observations when attempting to explain past maritime behavior. A further case was discussed in the seminar which reveals how true this is. In his analysis of the metal items recovered from the Cape Gelidonya wreck of 1200 B.C., Bass noted several observations of modern and traditional metalworking technology that furnished important clues to his explanation of the particular characteristics of these items and their further implications for understanding the historic

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relations between Mycenaean Greece and other parts of the classical world. This was especially true of the “ox-hide” ingots of copper, so called because of their four-legged shape and one rough or “hairy” side. In a published account of this analysis, Bass (1975: 51) pointed out that it was widely believed that such ingots were cast intentionally to look like dried ox skins, perhaps in order to equal the price of a cow before the invention and use of coinage. Bass’s observations of contemporary smelting technology in a Philadelphia foundry led him to support an alternative explanation for the particular characteristics of these ingots:

Five minutes at the Kramer Ingot Company in Philadelphia showed me that, surprisingly, none of the scholars who had written about the ingots had ever watched molten metal being poured. The “hairy” side was the naturally bubbly upper surface of the copper cooling in the open air, not a lower surface formed in a purposely uneven mold; modern foundries take care to prevent this rough surface by floating powdered charcoal on the liquid metal. Since the random weights of our ingots varying between thirty-five and fifty-seven pounds were based on no standard at all—another surprise—it was impossible that they had served as currency. There now seemed no reason to believe that the ingots were anything other than simple slabs of copper, their “legs” serving as handles for ease of portage, to be melted down and mixed with tin to form bronze (Bass 1975: 51).

This is a convincing argument, even without the further evidence Bass found for residues of tin among the ingots on the sea bottom and the sixteen Egyptian tomb paintings and reliefs he also found showing such “ox-hide”-shaped ingots being carried by men in exactly the manner proposed above.

The questions were, Why is this explanation convincing? What is it that holds true for both the modern and ancient behavior being described? In this case, the bridging element in the argument has to do with the uniformitarian relationship between the metal and the forces applied to it. Copper always reacts in the same way when subjected to heat in the manner described above. Because that is true, we can regard Bass’s technological hypothesis rather than the alternative economic hypothesis of these ingots as standardized currency as the most parsimonious explanation for the observed evidence that characterizes these archaeological items. Moreover, certain test implications were borne out in the research, namely in evidence from Egyptian tomb paintings and in the relationship between copper ingots

and the wider context of metalworking and trade in the ancient Mediterranean. Uniformity of weights for these ingots would go far toward disproving Bass's technological hypothesis, but, so far, such uniformity has not been shown.

So what we have here is an example of scientific reasoning that can serve as a model for explaining past human behavior using contemporary observations as evidence. As in the case of experimental archaeology, ethnoarchaeology explicitly establishes bridging arguments connecting past and present human behavior based upon acceptable uniformities in the relationships between human behavior and the material by-products of that behavior. Upon hearing his evidence subjected to this kind of analysis during the seminar, George Bass several times uttered the comment: "But we [meaning shipwreck archaeologists in the historic/Classical tradition] have been doing this kind of thing all along!" He meant, of course, that all of the various steps in the archaeological reasoning outlined above were implicit in their research, at least in this case of the so-called ox-hide ingots. In considering this issue, some of the seminar participants were anxious to make these steps explicit in order to make it easier for other scholars to explain shipwreck materials effectively, especially in relation to experimental and ethnographic observations.

Most members of the seminar felt that there was value in being explicit about how one applies the rules of science to explanations of past human behavior, especially when training newcomers to the field or in ensuring that even experienced researchers "touch all the bases" when designing their research. For example, we were never told why Bass went to the Kramer Ingot Company in the first place. Was this by accident or by design? Considering the decisive impact this visit had on his explanation of the characteristics of this particular kind of shipwreck material, it would be reassuring to know that he intended to do this all along as part of his efforts to test the alternative hypotheses bearing on this question. If it was accidental, then the archaeological profession is lucky he was alert to the possibilities presented by these observations. But while serendipity undoubtedly does occur in the process of scientific discovery and should not be underestimated, science in general cannot trust to luck alone. An organized and theoretically self-conscious approach to this question of the "ox-hide" copper ingots from the Cape Gelidonya wreck, with an explicit statement of the questions to be asked, the alternative hypotheses, and the test

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implications of these hypotheses (and, in this case, also specifying the bridging arguments that allow one to link present-day observations with behavior in the past) would offer greater assurance of convincing results than reliance on intuition and serendipity. Both Lenihan and Watson argue in their papers that wreck archaeology has reached a stage in its historical development where explicit research designs are needed, not so much to replace the insights of established shipwreck scholars, but to provide a basis for both maintaining and improving upon the standards set during the course of this earlier work.

OLD GOLD AND NEW CONCERNS

The United States may well be the only country left in the world where individuals and private companies can legally go out and loot shipwrecks. So no discussion of new directions in shipwreck archaeology is complete without some consideration of this problem. Sonny Cockrell's paper represents an effort to classify various kinds of shipwrecks in relation to what they might offer in the way of anthropologically useful ideas and information, and it provided the basis for our understanding of the magnitude of what might be lost if shipwrecks are destroyed by looters. During our discussions, Cockrell made it clear that this is not simply an academic concern, since large-scale efforts are in progress, mainly along the Florida and Texas coasts, by private companies like Treasure Salvors, to remove gold and other valuables from Spanish wrecks (see also Wade 1981). Adding to this discussion, Barbara Purdy emphasized the peculiar role that avarice connected with gold has played in leading to this large-scale destruction of historic wrecks in Florida. Cockrell's paper, together with his discussions and Purdy's, revealed the extent of destruction of wrecks and wreck sites from looting activities and the damaging implications these activities have for the future of shipwreck archaeology. Bass, Lenihan, and Murphy strongly echoed this view and offered examples from their own experiences.

For the land archaeologists in this seminar this was a new concern, though not entirely an unfamiliar one. Archaeology in general has been and continues to be plagued by looting of all kinds, which persists in the face of legal constraints as long as there are markets for such goods. However, the land archaeologists learned several surprising things

from their underwater counterparts during this seminar about the nature and extent of wreck looting.

Underwater looting is unusually destructive, since it involves the use of techniques that irrevocably and totally obliterate site associations and fragile materials. Boat-mounted “blasters” (powerful jets of water reated by funneling a boat’s propellor wash through a cylindrical cowl-ing, euphemistically called a “mailbox” in recent treasure hunters’ promotional literature [Lyon 1981: 5, 16; 1982: 235]), heavy-duty airlifts, and even explosives are sometimes used. Even when less destructive methods are applied, the results in no way resemble the sort of standards set by the archaeological profession.

Underwater looting may be legal. At least, it will be if the present trend set by the U.S. Federal Appeals Court in Miami, which recently found in favor of Treasure Salvors and against the state of Florida in a major case involving two important Spanish wrecks, continues (Wade 1981).

There is a spurious romance about treasure hunting that garners far more public support than this activity is entitled to. Some of the worst offenders have been recently portrayed in a heroic fashion by the media, without the opportunity for “equal time” by professional archaeologists and shipwreck historians.

The result of all these factors, in addition to the usual ones associated with pot hunting and other forms of looting in land archaeology, has been the rapid destruction of an irreplaceable part of our historic and cultural heritage as well as an important source of anthropological knowledge in general. In comparison with other countries, where both law and public sentiment support preservation and scientifically controlled research on wrecks, the United States appears curiously “underdeveloped” and backward in its treatment of this important cultural resource. Considering the urgency of this matter, the participants in this seminar unanimously agreed to prepare a statement (see p. xiii), which was then sent to archaeological and scientific journals for further dissemination. This statement should be read particularly with reference to the recent legal case over the wrecks of the *Atocha* and *Santa Margarita* between Treasure Salvors and the state of Florida. It would certainly help if we could enlist the general support of the archaeological profession to discourage the looting of wrecks and any involvement or aid by archaeologists in this activity.

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THINK OR SWIM? NEW DIRECTIONS IN SHIPWRECK ARCHAEOLOGY

While important differences remain, both among the seminar participants and in the field at large, at least some of the important trends in shipwrecks as anthropological phenomena became clear during the discussions and can be posited as prime considerations for further work in this field. These include:

1. An expansion of the domain of shipwreck archaeology to include wrecks of all kinds—ancient and modern, prehistoric and historic, and non-Western as well as European-derived. Although impressive results have been achieved on historic shipwrecks of European origin, especially in the Mediterranean, the Caribbean, and the waters of northern Europe, there is no necessary reason why shipwreck archaeologists should limit their scope to major historic events within these much-studied areas.

2. Greater explicitness in planning and carrying out research on wrecks. This should not be viewed as a criticism of earlier work, but simply reflects an increased need for self-conscious rationalization of each step in a program of research. For some this may appear pedantic. But experience in land archaeology has shown that there are important benefits to be gained from such explicitness in research design. It is easier to train students and newcomers to the field, and they are trained better. It can eliminate or reduce wasted effort such as the excessive multiplication of descriptive site reports or the preoccupation with material objects for their own sake. And, if used effectively, research designs can lead to our asking more interesting questions about wrecks on the one hand and to achieving more convincing results on the other hand.

3. An interest in applying approaches that emphasize systematic sampling and survey methods, selective or problem-oriented excavation, and experimental and ethnoarchaeological approaches. None of these approaches is new to shipwreck studies, but there are many new ideas entering the field about how to make these approaches more effective.

4. Urgent concern for the conservation and selective study of shipwreck remains as a resource capable of providing unique information and ideas about human behavior.

5. A new willingness to posit generalizations about past and present-day human behavior based upon shipwreck remains. For some of us, shipwreck archaeology is viewed as a part of social science. What makes it a science is not the use of scientific techniques and apparatus, but an organized process of reasoning based on the application of certain rules of science, such as the testing of alternative hypotheses, the principle of parsimony, the need for repeatability of results, and the ability to extend the results from a particular case to the realm of general propositions about the nature of variability in the behavior of the human species in a convincing manner.

When V. Gordon Childe attempted to generalize about the course of human history through his concept of the Neolithic Revolution, he changed the direction of archaeology in a way that affected nearly all of the research done since. One does not have to accept his theories about human behavior today in order to appreciate the influence of his scholarship on land archaeology. Seafaring, with its historic effects upon commerce, technology, war, and other key aspects of human behavior, is no less important a topic for potential generalization by archaeologists than agriculture. This seminar demonstrated that the possibility for such generalization exists in the field of shipwreck archaeology. It would be unfair to paraphrase the old Willey and Phillips dictum by claiming that "Shipwreck archaeology is anthropology or it is all wet!" because this is not really true. Classical and historical archaeologists have demonstrated the scholarly importance of their approach to this kind of material, and no doubt their efforts will continue. But there clearly is an anthropological dimension to shipwrecks and wreck sites, and the time has come for anthropologically oriented archaeologists to recognize and explore it in an intelligent and convincing manner.