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Method and Theory in Shipwreck Archaeology[1]

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SHIPWRECKS AS ARCHAEOLOGICAL PHENOMENA

Human beings have been working underwater at least since the third millennium B.C. when the Mesopotamian culture-hero, Gilgamesh, used the *petra*-diver's technique (attach one or two large rocks to one's ankles and jump into deep water, then release the rocks to return to the surface) to obtain the plant of immortality from the sea bottom (Speiser 1955: 108). However, it was not until A.D. 1828 that the first diving dress was invented, to be used shortly thereafter in salvaging portions of a well-known and important historical wreck, the *Royal George*, during the 1830s (McKee 1968: Chapter 1). Sponge divers using this same type of dress recovered classical Greek sculpture and other material from ancient Roman wrecks during the early part of this century (McKee 1968; Throckmorton 1969; Bass 1980). The post–World War II invention of a very different form of diving apparatus

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(SCUBA) placed the undersea world within reach of anyone who could afford the moderate expense of air tanks, fins, mask, and wet suit, with predictable impact on the submerged archaeological record in the hospitable Mediterranean sea:

The forest of pines that borders the coast in the harbor provided shelter for the multicolored tents of a vast international camping ground, from which hordes of pink, chubby little divers from Germany, Belgium, and Switzerland set out in feverish pursuit of nautical souvenirs. They were especially fond of archaeological items, and the wrecks in the vicinity were the victims of regularly scheduled clandestine explorations.

Every day toward evening we would see them coming in little groups, swimming along behind the inflatable mattresses that carried their diving gear. They would land on the Ile d'Or and sit all in a row by the edge of the water, waiting patiently for the *Espadon* to leave so that they could exploit the site for their own purposes (Dumas 1972: 72–73).

The Titan wreck was the last surviving known ancient wreck in reasonably shallow water in the south of France. In the short space of ten years the others had been destroyed without a trace, except for a few amphoras and other objects which found their way to the Musée Borely and were duly studied and published by Benoit. There was, for example, the Dramont wreck, a Roman ship of the first century B.C. Found as a heap of amphoras 21 meters long by 8 wide, impacted in a protective covering of seagrowth, it was dynamited by skin divers in 1957. . . . A whole chapter in the history of navigation was blown to rubble by some mindless diver, perhaps hunting nonexistent gold, destroying not from malice but stupidity, like a bored child spilling the sugar on a rainy afternoon. . . .

The glory of the world must indeed pass away, but it seems wrong to speed its passage with dynamite and sledgehammers (Throckmorton 1969: 187–89).

It was not until the late 1960s that underwater archaeology in the Mediterranean emerged as a scientific and scholarly pursuit. Shipwrecks ceased being simply targets for casual looting by holidaying scuba divers and became archaeological phenomena, precious documents of the human past. The research project that introduced control and discipline to shipwreck archaeology in the Mediterranean is that of George Bass at Cape Gelidonya, Turkey (Bass 1966, 1967, 1975). As recently as 1960, few of the recording techniques now used routinely by hundreds of underwater archaeologists all over the world had even been devised. As indicated by Throckmorton (above), the situation in the Mediterranean area was particularly acute, and here Bass had to start at the very beginning to develop appropriate techniques:

The wreck was only ninety feet deep, which meant that each diver could work for forty minutes during the first dive and thirty during the second, with only five minutes of decompression for each dive, provided the dives were six hours apart. Bass drove the professional divers mad. A professional diver learns very early that time on the bottom equals money, and accordingly works with an eerie speed and efficiency. Bass often spent his entire dive contemplating a bit of rotten wood, deciding what to do with it, while the experienced divers wondered how long it would be before he lost his mind completely (Throckmorton 1969: 196).

Far from causing him to lose his mind, Bass's close study of the poorly preserved hull fragments and other remains on the sea bottom at Cape Gelidonya enabled him to recover an amazing amount of information about the anonymous craft whose voyage suddenly ended there some 3,000 years ago. The resulting archaeological report (Bass 1967) is a model of interdisciplinary scholarship and historiography, not just because Bass is a good scholar, but because he approached the shipwreck as a competent and thoroughly professional archaeologist would approach any archaeological site. In other words, he was the first person to demonstrate that Mediterranean shipwrecks—far from being helter-skelter heaps of debris—are orderly universes of archaeological data. He and his staff also developed, adopted, and refined a great many underwater recording techniques that have since become standard (and have themselves been modified and refined). In short, Bass's work revolutionized shipwreck archaeology in the Mediterranean, and, by removing it from the realm of sport diving and relic collecting, reclaimed it for mainstream classical archaeology.

Underwater archaeology in Europe and the New World, although for the most part independent of work in the Mediterranean, has also by now attained a high level of competence and precision (McKee 1968; Muckelroy 1978; Lenihan this volume; Murphy this volume). McKee characterizes British shipwreck archaeology as having had a strong historical emphasis from the beginning:

the British approach was strictly scientific and firmly historical, from the first; an attitude which has been maintained. . . . In brief, the attraction, the importance, the value of the Spithead wrecks was, that they were known, named ships, identified with important historical personages and events, which they brought closer and made more personal (McKee 1968: 75).

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In the New World, however, underwater archaeologists still face enormous difficulties because of sport divers and treasure hunters.

I am sick to death of skin divers. . . . Personally I am tired of clearing sites of sunken vessels, and spending energy and money only to have a bunch of stupid people go under water by day and by night destroying simply for the fun of it.

I know that the problem Mr. Inverarity outlined exists. The wreck of the "New Hampshire" outside of Boston was torn to pieces by skin divers who removed brass nails made by Paul Revere and sold them for fifty cents and a dollar apiece. Skin divers have also ripped to pieces in Narrangansett Bay the first diesel-powered submarine (Inverarity 1963, with comment by J. Jenks).

The great intrinsic worth of many shipwrecks in the Atlantic off Florida, in the Gulf of Mexico, and in the Caribbean has resulted in the flourishing of vicious, multimillion-dollar salvaging schemes that ensure the unrecorded destruction of all known historical wrecks in these waters (Cockrell 1980; Lenihan this volume; Murphy this volume). It is extremely unfortunate that a recent article in *Science* (Wade 1981) condones the principle of treasure hunting among Florida's wrecks. The bewildering complexity of state, federal, and international legislation permitting legalized and unmonitored site destruction in Floridian waters is a sufficient nightmare for professional archaeologists and historians without the added burden of support for the looters from the most important scientific periodical in the U.S.

The unprecedented and unmitigated attack on the archaeological and historical record by profit seekers with no scholarly training is justified by themselves and their supporters on the grounds that the shipwrecks are disorderly scatters of detritus offering no archaeological information, that detailed contemporary plans of them exist so nothing could be learned from them anyway, and that they are prohibitively expensive for noncommercial enterprises to reach and investigate. These allegations are untrue, and are rightfully categorically denied by professional underwater archaeologists (Lenihan 1974a; Bass 1980, 1981; Cockrell 1980; and Lenihan and Murphy in this volume). To date, however, the profit incentive has triumphed over the scholarly one whenever the commercial salvors have been brought to court by the archaeologists. This ugly situation confirms—at least for this part of the New World—McKee's judgment that "the pattern in the Americas

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was, from first almost to last, that of treasure-hunting" (McKee 1968: 75).

Nevertheless, throughout much of the rest of the U.S., as in the Old World, the 1960s–70s phase of rapid development in methods and techniques has now given way to self-conscious and more abstract concern about the identity and goals of professional underwater archaeology. This concern is expressed by the British underwater archaeologist Keith Muckelroy (1978) and in a variety of New World literature (De Borhegyi 1963; Lenihan ed. 1974; Marx 1978; Shiner 1978; Cockrell, Lenihan, and Murphy this volume) and has stimulated the present seminar.

SHIPWRECKS AS HISTORICAL PHENOMENA AND SHIPWRECKS AS ANTHROPOLOGICAL PHENOMENA

It seems clear that shipwreck archaeology is at a critical and exciting juncture. The great logistical difficulties have been mastered to such an extent that underwater archaeologists can and must discuss, debate, and apply alternative research designs and alternative methods of approach to shipwrecks and shipwreck data.

As in any other variant of archaeology, there are two possible emphases with respect to the scope of research designs employed in shipwreck archaeology: a particular wreck may be perceived as a unique phenomenon and of interest primarily for its intrinsic characteristics, including the information it may offer about more general issues, or a general problem may require seeking out one or more particular wrecks in specific places dating from specific periods. Here the concern is primarily with the information the wreck provides about the general problem.

At present, Bass's Cape Gelidonya report as well as a variety of other accounts (Marsden 1967; McKee 1973; Wheeler et al. 1975; Arnold and Weddle 1978; Fenwick 1978) variously represent the first (idiographic) emphasis. These accounts range from rather minutely particularistic in scope (Marsden 1967; Fenwick 1978) to broader historiography (Bass 1967; Arnold and Weddle 1978), but all are characterized by a primary emphasis on the particular wreck or set of wrecks rather than on a general question or problem to which the wrecks were thought to be relevant.

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There seems to be no good published example of the second (no-mothetic) emphasis. This is doubtless because the field is so young, and because in the early stages of its development so many of the wrecks were found by accident. For example, the several millennia of the Bronze Age in the eastern Mediterranean, a time range of great intrinsic and extrinsic interest for many people, are represented by only one well-excavated and well-published shipwreck (Bass 1967), found accidentally by Turkish sponge divers. But even on the basis of the small body of information now available, a generalizing or nomothetic emphasis could be profitably applied to shipwreck archaeology of the eastern Mediterranean Bronze Age. For instance, one of the most significant implications of Bass's findings from the viewpoint of an ancient historian is that Western Asiatics (Phoenicians or Syrians or Canaanites, as they are variously called) were acting as sea-going mid-dlemen and traders much earlier (by the latter second millennium B.C.) and apparently much more intensively than had previously been suspected. Prior to the Gelidonya work, scholars had inferred from available documents that the Mycenaeans monopolized maritime trade at this period in the Bronze Age. The respective roles of Syrian and Mycenaean merchants are thus brought into question by the Gelidonya research. This matter, together with the details of just how the trade was carried out—insofar as the Cape Gelidonya ship represents it—are, of course, fascinating to ancient historians. But these details also bear much of interest to anthropological generalists concerned with long-distance trade, with technological innovation and diffusion (the Gelidonya ship carried a considerable quantity of copper ingots and of scrap bronze with the equipment for working the metal on demand), or with the expression of ethnicity in material goods (the ingot shapes and the signs on them, morphological details of some of the bronzes, the pottery, the weights, the seals). Given the Gelidonya case, an anthropological generalist investigating technological innovation and diffusion in nonindustrialized societies, for example, might devise a research design that required detailed knowledge of the cargoes of several eastern Mediterranean ships, especially information on metal and metallurgical techniques, pre- and post-dating the Gelidonya wreck. This information could be compared with archaeological and historical evidence from terrestrial sites in the same geographic area, with ship-wreck and terrestrial data from, say, the Far East, where there was an

equally early development of bronze and of maritime trade, and with data from some parts of Africa, where there was an emphasis on metal and metallurgy but no maritime distribution of the objects or the techniques. Practically speaking, it would take a great deal of time and money to locate relevant shipwrecks and excavate them, but it could be done. At least for the Mediterranean, there is sufficient knowledge of the routes these ships followed and sufficient technical expertise with search equipment to enable accurate predictions as to locations of wrecks, and then to find them on the sea bottom. But here our anthropological generalist squarely confronts the perennial archaeological dilemma: is the excavator justified in extracting only the information required for the specific research design being implemented? In this case, that could mean documenting and recovering the ships' cargoes in enormous detail but neglecting entirely or giving short shrift to details of hull construction, or of organization and materials in the noncargo part of the vessel. It might even mean ignoring the pottery and other artifact categories not directly relevant to dating the ship or to the transport and processing of the metal cargo.

I hope and believe that professional archaeologists would say the hypothetical Principal Investigator of the project just outlined would not be justified in completely ignoring all data categories other than those of primary concern, but I also believe there to be a very wide and essentially nongeneralizable spectrum of possible compromises for such an investigator. We return to this crucial matter of the confrontation between theory and practicality in field archaeology later in the discussion.

Another possible reason for lack of attention to the second emphasis listed above is the credibility problem still facing nautical archaeologists. A great many terrestrial archaeologists and other scholars do not regard underwater archaeology as a legitimate part of the profession because they fail to realize that excavation and recording at underwater sites has attained a completely professional level. Hence, outside the field of underwater archaeology itself, there are relatively few scholars who know or care about the potential information shipwrecks contain for a wide variety of topics ranging well beyond the particulars of a specific ship. One possible example was just outlined, and other papers in this volume address other such topics. An additional example is the comparative study of transoceanic colonializing processes (Spain in

the New World and the Netherlands in the East Indies, for instance). The nature and quantities of goods carried are not always accurately portrayed in ships' manifests (Schmidt and Mrozowsky, this volume), and excavation of actual cargoes in wrecked ships furnishes vital detail supplementing and complementing that in historical archives. A final example is the study of the world system in ancient times. In the absence of historical documentation, shipwrecks are probably the best way to do this, and they are an invaluable supplement even in the presence of fairly detailed records.

The great potential shipwreck data have to alter our understandings of global communication networks and of resulting broad developmental trends in technology is clearly demonstrated by wrecks recently found on the Korean and Chinese coasts (Keith 1980). The Chinese ship (Sung Dynasty) found in the harbor of Ch'uanchou is built in a manner very similar to that of European carracks, yet it is 100 years older than any of the previously known European carracks that have long been credited with enabling the age of global expansion by the aggressive European traders and sailors who manned them. The ship that went down near Sinan on the west coast of Korea about A.D. 1310 is closely similar in construction to the Ch'uanchou ship. These finds cast considerable doubt on what was thought to be a well-established generalization about the evolution of ship technology. The older ideas are summarized in Muckelroy's book (1978: 136):

As in so many other areas of activity, the craftsmen of China developed their own designs and techniques, owing little if anything to outside influences and having surprisingly little impact on ideas in neighbouring lands.

Clearly, these earlier understandings about the isolation of the Chinese shipbuilding industry from world watercraft technology need to be reevaluated. Such reevaluation would have important implications for our knowledge of global communication dynamics: where the networks ran at particular times, how they functioned, and especially what the mutual effects of contact were on the societies in question.

As indicated by its title, a great deal of discussion during the seminar at Santa Fe was devoted to the question of whether or not shipwreck archaeology should be (or could be) brought into the anthropological fold to some significant degree, and what would be entailed should that occur. Opinions by seminar participants were, of course, varied,

but the crux of the debate seemed to revolve about two points, each of which follows from our mutual understanding of anthropology as a broadly generalizing and cross-culturally comparative social science. Hence, to commit shipwreck archaeology to anthropology is to commit it to a basically nomothetic, or at least cross-culturally generalizing, approach. One node of debate, then, is the issue discussed in the immediately preceding pages (shipwrecks as providing data of primarily intrinsic interest vis-à-vis shipwrecks as providing data of interest primarily for their relevance to general problems). This issue is a variant of the "culture history vs. culture process" discussion in New World archaeology during the late 1960s and the 1970s (Flannery 1967; Watson 1973a), which is in its turn a variant of the perpetual tension between idiographic (particularist) and nomothetic (generalist) approaches in any field.

The logical response to the debate between generalists and particularists is always the same: both emphases are essential and both are present in everyone's work, although individual scholars usually stress one more heavily than the other.

Within the context of shipwreck archaeology one must conclude, now that a wealth of detailed and varied data are demonstrably retrievable at will, that those data can be used for any purpose whatever on the nomothetic-idiographic spectrum. And, as always, although the overall emphasis will differ depending upon the training, experience, and aptitude of the investigator, both perspectives are logistically essential, and, in fact, inseparable in any piece of research.

But in shipwreck archaeology as elsewhere, logic and practicality do not always go hand in hand. Shipwreck archaeology is an acute example of that perpetual archaeological dilemma already briefly illustrated. One's research must be problem oriented, but it must also be thoroughly justifiable as regards comprehensive data recovery, because whatever portion of a site is excavated is destroyed forever.

Logically speaking, as just noted, the empirical data from shipwrecks can be recorded and recovered for any purpose or set of purposes from the highly idiographic to the highly nomothetic. Practically speaking, the crunch comes because in archaeological excavation of any kind—where, by definition, that part of the site being investigated is irrevocably destroyed—some of these purposes are mutually exclusive of others. Probably the most extreme example of deliberate, single-minded destruction in the realm of shipwreck archaeology is commercial trea-

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sure hunting off the coast of Florida and in adjacent southern waters. Here the shipwrecks are being exploited (methodically destroyed by a variety of technologically sophisticated means) for a single purpose (monetary profit for a few individuals) that excludes any other form of data yield potentially present in the debris. At the nondestructive extreme is detailed video surveillance of an entire wreck (analogous to intensive surface mapping and tabulation without collection or excavation on a terrestrial site), as is being done in and on some of the historical wrecks in the Great Lakes.

Again practically speaking, the most desirable procedures to be followed depend a great deal on the nature of the wreck in question and on the context of the proposed work. If a small and/or unique wreck is threatened with destruction, then total excavation of all remains, as was done at Cape Gelidonya, is probably the best solution. But if the wreck is very large and/or very complex, then the excavation must be done in accord with some appropriate sampling design.

Or, once again practically speaking, if the question of how to attack a shipwreck arises in the context of management, as is the case for the hundreds of wrecks in the U.S. for which the National Park Service is responsible (Lenihan 1974b), then answers are constrained in yet other ways.

The individual charged with responsibility for these wrecks and for monitoring all research done on them must be informed of proposed work in sufficient detail to enable evaluation of the potential yield in historical, anthropological, sociological, or other information, and the manager must also be able to assess the impact of the proposed work on the total data corpus. The manager must then weigh the probable returns against the probable amount of attrition or destruction to that corpus. Reciprocally, proposers of research on such managed cultural resources must provide explicit research designs including clearly laid out work plans and honest appraisals of expected returns in knowledge gained.

But, to return to the central issue, other things being equal, what is the most fruitful and productive possible way to do research on shipwrecks? In my opinion the answer must be in an anthropological (meaning a broadly generalizing and cross-culturally comparative) framework within which the highest standards of fieldwork and scholarship are applied.

But there are many built-in complications even to this idealized

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solution. For example, the nature of the particular data as they are recovered day to day will necessitate continual minor—and sometimes major—modification of the original, generalist research design. Another perennial and much more basic problem is that there are very few individuals who are able to function equally effectively as generalists and as particularists; it is nearly always the case that two or more scholars must cooperate, with all the attendant potential for conflict inherent in such collaborative arrangements.

The second node of debate is that of an anthropological approach as meaning not just a generalizing approach, but also a cross-cultural one. Although cross-cultural reasoning is second nature to anthropologists, who are accustomed to range freely through space and time, it may seem very dangerous to scholars whose training has not accustomed them to juxtaposing traits and trait complexes, or various portions of cultural systems, from widely separated chronological and geographical proveniences. Furthermore, such scholars may simply not be interested in, for instance, the insights attained by students of hierarchical, class-based social organization who compare officers' and crews' quarters on World War II Japanese warships at the bottom of Truk lagoon with those on American vessels sunk in Chesapeake Bay during the war of 1812 and those on fighting ships of the Spanish Armada.

ARCHAEOLOGY AS ANTHROPOLOGY, SHIPWRECK ARCHAEOLOGY AS ANTHROPOLOGY, AND ARCHAEOLOGY AS ARCHAEOLOGY

Historically speaking, in North America at least, archaeology has been anthropology for nearly a hundred years. There were good reasons for this, and nearly all of us presently practicing prehistoric archaeology in the Americas (as well as many who work in prehistoric periods elsewhere in the world) were trained as anthropologists first, archaeologists second. However, that situation is changing. In fact, the nature of American anthropology—if such a unitary field can even be defined now—has changed sufficiently that explicit secessionist rhetoric is appearing. Archaeologically oriented secessionists include Butzer (1975, 1980), Dunnell (1980), Gumerman and Phillips (1978), Meltzer (1979), and Wiseman (1980). Even ardently anthropological archaeologists

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have realized they can no longer rely on ethnological or social anthropological colleagues to obtain the information from living societies that is crucial to comprehending extinct ones; hence the burgeoning of ethnoarchaeology. Indeed, it has lately become apparent to all anthropologists how inadequate and shaky much of the older ethnographic literature is because of powerful but unacknowledged colonialist, racist, and sexist biases. For example, Berndt (1981) methodically demonstrates how virtually all information about Australian Aboriginal women in the standard anthropological literature is doubly biased because they were automatically assumed to be inferior on two counts: first because they were Aborigines, and second because they were women. Influential writers like Malinowski (1913, *The Family Among the Australian Aborigines*), Roheim (1933, "Women and Their Life in Central Australia," *Journal of the Royal Anthropological Institute*, vol. 63), and Maddock (1972, *The Australian Aborigines: A Portrait of Their Society*) presented erroneous (sometimes wildly so) conclusions about women in every facet of Aboriginal life.

Another relevant issue is that, in these times of general economic retrenchment, many archaeologists have realized they are severely disadvantaged politically by unquestioning acceptance of subdisciplinary status to sociocultural anthropology, a status all too often interpreted as meaning second-class citizenship within a university departmental community. A strong argument can be made for the proposition that the future of archaeology—politically speaking at any rate—is not within anthropology, but in and of itself as an independently functioning, strongly interdisciplinary pursuit.

But is the "archaeology as archaeology" route the best solution to the difficulties indicated above? As just noted, it certainly offers many practical and political advantages. If all archaeologists could agree on an explicit, basic theoretical orientation (for instance, that the most productive use of the archaeological record is for advancing our scientific understanding of humankind, or, alternatively, for advancing our humanistic understanding of humankind), then archaeology could flourish independently as a special discipline with uniquely varied and demanding requirements for laboratories, equipment, and fieldwork. However, for the reasons noted in the previous section, it is quite unlikely (and perhaps not entirely to be desired) that such consensus on theoretical orientation will be achieved in any meaningful way. It is still essential for archaeologists who identify themselves primarily

with a broadly generalizing, cross-cultural approach to the archaeo-
logical record to retain strong intellectual ties to anthropology, in spite
of the practical difficulties that may sometimes arise from such asso-
ciation. In any case, what structures our inquiry is not the layout of
contemporary academic boundaries, but rather the problems defined
by archaeologists operating as generalizing social scientists.

Yet minutely particularistic data are not to be scorned by archaeo-
logical generalists. On the contrary, such data are essential not just to
particularists but also to generalists in a variety of contexts beginning
with chronology and including evidence for trade or other contact.
The "direct historical approach" and other narrowly analogical and
particularistic techniques (for comprehending nautical technology, for
instance) are as necessary to shipwreck archaeology as they are to
terrestrial archaeology. It must simply be realized that these are not
ends in themselves. Rather, the very limited corpus of shipwreck sites
can be most productively investigated when carefully designed and
executed particularistic studies are employed in the pursuit of broadly
conceived questions about processes generalizable to many human
societies, regardless of their placement in time and space.

SUMMARY AND CONCLUSIONS

Since the end of World War II, shipwreck archaeology in the Med-
iterranean and the New World has developed from looting and un-
recorded destruction of sites to archaeological respectability, except in
Florida where it has gone from illegal or semilegitimate looting on an
individual scale to legalized looting by multimillion dollar corpora-
tions—a sad exception to the preceding generalization. In Britain and
in northern Europe, shipwreck archaeology apparently started and
continues within the bounds of professional history and archaeology.

With technical and professional competence, uncertainty has come
to many underwater archaeologists about the most effective use of the
archaeological record contained in shipwrecks. On an abstract logical
plane, the most crucial issue here, as with any archaeological site, is
one of relevance. Were all shipwrecks safe from threat by looters, or
from other direct and indirect impacts resulting from modern use of
the seas, then there would be no justification for excavation of any of
them except where they alone could furnish information relevant to
solution of some important problem, be it primarily idiographic or

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primarily nomothetic. But this is not the case; many shipwrecks are menaced in many different ways, and hence the lines of debate and discussion are drawn, as was demonstrated in the seminar itself and in several of the papers in this volume.

Given that shipwrecks will be excavated, I would still advocate as little excavation but as much detailed documentation as possible to answer well-thought-out and well-justified questions of demonstrably general intellectual significance. Or, to rephrase using appropriate jargon, logically the ideal approach to get the most from a finite and rapidly dwindling data corpus of shipwrecks is to use nomothetically conceived research designs that guide but are modifiable by idiographically conceived implementation of those designs. Thus I stress the nomothetic approach, but I also stress the fact that that approach is worthless in the absence of careful scholarship.

A final sobering and complicating factor is that, at the present moment in the real world of shipwreck archaeology, the only thoroughly and comprehensively published work is idiographic; the generalists have yet to prove themselves by designing projects, carrying them out, and publishing them in detail so the results can be evaluated and used by interested experts and scholars of all kinds. Therefore, the discussion upon which the seminar focused—intensely interesting though it is to anyone concerned with archaeological theory—is academic, and must remain so until the generalists provide such results.