Chapter 16
New Developments in Submerged Prehistoric Archaeology: An Overview

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It is now 30 years since Patricia Masters and Nicholas Flemming (1983) published *Quaternary Coastlines and Marine Archaeology*: *Towards the Prehistory of Land Bridges and Continental Shelves*, the outcome of a workshop held at the Scripps Institute of Oceanography at La Jolla, California, in 1981. In retrospect, this stands out as a landmark meeting, which first identified the continental shelf as a coherent and worldwide field of study in its own right, the need for systematic research drawing in specialists from multiple disciplines in marine science and archaeology, and some of the challenges as well as the opportunities of such investigations. As a participant in that meeting, I remember well the stimulation of communication across unfamiliar disciplinary boundaries, the potential for new research collaborations, the sense of enthusiasm at the prospect of new frontiers of knowledge to be breached, and the optimism about the prospects for purposeful new investigations and new discoveries.

In the decades since then, it is fair to say that progress has been slow and, at best, intermittent, confronted by a persistent scepticism, at least within the discipline of prehistoric archaeology, as to whether underwater investigations are either feasible or worthwhile. During the 1980s and the 1990s, the most visible work occurred in relatively isolated circumstances, most notably in Denmark with its seemingly unusual conditions of preservation in the calm and shallow waters of the western Baltic (Andersen 1985; Fischer 1995a), and off the Carmel coast of Israel where a group of Neolithic remains includes the unusual Pre-Pottery Neolithic site of Atlit Yam with its evidence of mixed maritime and farming activity (Galili et al. 1993). Both projects were heirs to regional traditions of underwater research already well represented at La Jolla (Larsson 1983; Raban 1983). However, these results could easily be dismissed as exceptions that contributed little new, beyond unusually good preservation of organic materials, to a wider knowledge of the prehistoric periods in question. Indeed, one of the criticisms of underwater research that persists to the present day is that much work represents the development of new techniques and

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the industrious accumulation of new data with relatively little attention to how this might bring new light to bear on the big questions of prehistory (Anderson 2012).

Much of the recent interest in and new research on submerged prehistory has been focused on Europe (see in particular, Benjamin et al. 2011; Bailey et al. 2012). If there is one clear message that emerges from the chapters in this volume, it is that submerged landscapes and archaeological traces of their inhabitants are now being retrieved and systematically examined across the world in all the major continents and in deeper as well as shallower water, and that there is serious and ever-widening engagement with the intellectual and logistical challenges of underwater research. In reflecting on the current state of play as represented in these chapters, I briefly consider three issues: the tortuous pathway towards the acceptance of new ideas and the factors that have variously impeded or stimulated the growth of new knowledge; the research questions that are now coming more clearly into focus and the directions they suggest for future development; and the challenge of developing purposeful strategies of exploration for the discovery of new archaeological material.

An Emergent Discipline

It is characteristic of a pioneer phase in the development of a new field of knowledge that relevant data are initially acquired haphazardly or by chance, and may languish long neglected in unpublished archives, obscure reports, or museum basements until a change in the intellectual climate gives them retrospective significance. That is certainly the case with the submerged archaeology of prehistoric periods, and one of the interesting revelations from many of the chapters in this volume is the number of scattered underwater finds and pioneer investigations that were carried out in the earlier decades of the twentieth century and even into the 1980s and beyond, but with results that were either not published at the time, or disseminated only in unpublished reports or local journals. Examples are the discovery of underwater stone artefacts in Japan in the early decades of the twentieth century, and the 40-year-long tradition of excavating submerged Jomon lake sites using coffer dams (Hayashida et al., Chap. 15); Dixon's 1976 geophysical survey in central Beringia, which must rank as one of the earliest reported examples of purposeful underwater survey using predictive models of archaeological site location (Dixon and Monteleone, Chap. 6); the discovery of submerged archaeological sites in the Gulf of Mexico in the late 1970s using sediment coring and data from oil and gas exploration to predict submerged land forms and site locations (Pearson et al., Chap. 4); the chance recovery in 1970 of the Cinmar leaf-shaped biface and mastodon tusk on the outer continental shelf offshore of Chesapeake Bay, and their display in a local museum for 30 years before their wider significance was appreciated (Stanford et al., Chap. 5); and the early discoveries of submerged and waterlogged materials in Australia, and more recent work there demonstrating the survival after inundation of archaeological material on lake and river banks (Nutley, Chap. 14). All these examples gain significance in the light of more recent developments in the discipline but were scarcely known about or reported to the wider scientific and academic community at the time.

Undoubtedly two persistent impediments to progress have been the widespread belief that nothing worthwhile is likely to have survived the destruction and disturbance of inundation, and the assumed technical difficulties and high ratio of cost to reward involved in underwater research. This volume provides abundant examples to refute both beliefs. It is clear that archaeological material—and the bones of terrestrial fauna—can be preserved and recovered under a great variety of underwater conditions—on high energy coastlines exposed to the open sea (Bayón and Politis, Chap. 7; Carabias et al., Chap. 8; Bicket et al., Chap. 12; Werz et al., Chap. 13) as well as low energy ones (Jöns and Harff, Chap. 10; Hayashida et al., Chap. 15), and in deeper water (Stanford et al., Chap. 5; Dixon and Monteleone, Chap. 6) as well as in shallow conditions. The case of the Argentinian intertidal site of La Olla is instructive (Bayón and Politis, Chap. 7), demonstrating that a long and straight sandy beach facing the open ocean and exposed to large waves and storms can nevertheless preserve material with stratigraphic integrity and good organic preservation.

Shell mounds, that ubiquitous indicator of coastal economies, are a much sought after indicator on submerged palaeoshorelines, not least because of the likelihood that they may register a distinctive geophysical signature in acoustic surveys (Faught, Chap. 3). They occur worldwide in their hundreds of thousands on mid-Holocene shorelines associated with modern sea level, so much so that many archaeologists have seen them as indicators of postglacial intensification and population growth. That interpretation is suspect, given the close association of the earliest shell mounds with the establishment of modern sea level, and just one discovery on a submerged shoreline of significantly earlier date would change thinking on this topic. However, such finds have proved elusive. Nutley (Chap. 14) doubts the ability of unconsolidated shell-mound deposits to survive inundation, given the evidence of site destruction by storm damage on the modern Australian coastline. We have faced similar difficulties in identifying submerged shell mounds in our work in the Red Sea despite the existence of thousands of extensive mid-Holocene mounds on the modern shorelines of the Farasan Islands (Bailey 2011; Bailey et al. 2013). Here, in addition to possible wave dispersal and destruction of shell material, we also have to factor in the dynamic nature of the coastline. Extensive, shallow intertidal bays capable of generating large quantities of molluscs are, in this region, unstable and short-lived phenomena. A further complication is that when sea levels are changing rapidly, even with a continuously available supply of abundant molluscs, shorelines may not remain in the same place long enough for shell consumption to generate archaeologically visible accumulations of shells before people are forced to move on, a point also made by Fischer (1995b, 382).

In contrast, Faught (Chap. 3) provides an actual example of a submerged shell mound off the Florida coastline. Here, survival appears to be due both to consolidation of the shell deposit by vegetation growing on the pre-inundation mound surface and also to the accumulation of protective sediments around the deposit as sea level rose. Several authors draw attention to other types of archaeological materials that have survived submergence, or are likely to do so and to be easily detectable—

stone fish traps and fish weirs, rock outcrops, stone structures, semi-subterranean pit depressions or circular features, rock shelters, rock art, and timber work associated with boats are variously mentioned by Faught (Chap. 3), Dixon and Monteleone (Chap. 6), Momber (Chap. 11), and Nutley (Chap. 14). In addition, Werz et al. (Chap. 13) make the interesting point that inundated land surfaces with shallow gradients and lack of sediment cover, typically to be found in deeper water and further offshore on the South African shelf, may be better places to look for early Stone Age artefacts, given that surface finds are abundant and important indicators of early human settlement on the present-day dry land.

The lesson of these examples is that it is not possible to generalize on a large scale about the sorts of coastlines that will be conducive to archaeological preservation or destruction. Local conditions are the key factor; and site survival and visibility will depend on a complex matrix of interacting variables, which include the balance between sediment accumulation and erosion during and after inundation, the ecological conditions for human activity in the near-shore region, the quantity, durability and visibility of the types of materials left as by-products of past human activity, and the discard behaviour of the people in question. If this sounds like a complex research problem, exactly the same is true of archaeological sites on land, and both domains are still at an early stage in developing understandings about 'landscape taphonomy'—the interaction of human behaviour, archaeological visibility and preservation, landscape evolution, land use, and land degradation—as a research field in its own right.

The cost of underwater work remains a major inhibition for many archaeologists, but several chapters demonstrate what can be achieved with relatively inexpensive methods of shallow-water diving and remote sensing (Faught, Chap. 3; Carabias et al., Chap. 8; Momber, Chap. 11). In deeper water, cooperation with industrial companies working on the seabed has undoubtedly helped to open up new opportunities and new discoveries, reinforced by the extension of national legal obligations to manage the underwater cultural heritage, and international treaties such as the UNESCO 2001 Convention on the Protection of the Underwater Cultural Heritage. The North Sea has been especially well served by these developments (Bicket et al., Chap. 12). But even here, differences of approach between different national jurisdictions can impede integration and understanding (Salter et al., Chap. 9), and in the USA, Faught (Chap. 3) notes that only three out of twenty-two coastal states require evaluations of submerged prehistoric material in advance of industrial work on the seabed.

Research Questions

I am often confronted with the view that the large sums of money required for underwater prehistoric research could be better devoted to archaeological investigation on land. This is a fallacious argument as well as a dangerous one, and in any case one that is increasingly irrelevant—fallacious because underwater archaeology is not necessarily more expensive than work on land; dangerous because it assumes without further demonstration the relative value of different research activities and opens the door to the argument that terrestrial archaeology in its turn should be deprived of funds to the benefit of more valuable research in, say, renewable energy or nanotechnology; irrelevant because some archaeologists are now, in any case, securing large-scale funding for research-driven investigations. Examples of the latter are the National Science Foundation (NSF) Gateway to the Americas project (Dixon and Monteleone, Chap. 6), the German Research Foundation (DFG) SINCOS project (Jöns and Harff, Chap. 10), and the European Research Council (ERC) DISPERSE project in the Red Sea (Bailey et al. 2012). Increasingly, funding bodies are attracted to the support of large-scale collaborative projects involving cooperation across national as well as disciplinary boundaries, and underwater research creates and demands exactly those sorts of collaborations, often with the added bonus of producing new knowledge of wider social and economic relevance, for example in understanding the social impact of sea-level change, or the improved management of the underwater cultural heritage. New opportunities of this sort are now being opened up by international research networks such as the European COST-funded SPLASHCOS project (Bailey et al. 2012; Jöns and Harff, Chap. 10).

If the research problem is worth investigating, it should be worth funding, and it is up to those who wish to work under water to make the case for support. Ship time, of course, is very expensive (unless supplied free of charge through collaboration with industrial operators—see Bailey et al. 2007), but increasingly necessary as one moves into deeper water and outer areas of the continental shelf. The key, then, to the funding of research-driven underwater investigations must be the articulation of research questions that are of central importance to a wider understanding of prehistory—and that cannot be answered in any other way.

One such problem is the dispersal of human populations out of Africa during the Pleistocene, the earliest colonization of new continents, and the early Holocene expansion into the newly deglaciated regions of the northern hemisphere. Most of this process of population expansion was taking place when sea levels were lower than present, and cannot be understood without investigation of now-submerged coastal regions. This has long been on the research agenda in North America (Stanford et al., Chap. 5; Dixon and Monteleone; Chap. 6). Regardless of whether one thinks the earliest colonists were big-game hunters or seafarers and fishers—and the likelihood is that they were adept in both the terrestrial and the marine domain—it is clear that coastal regions on both the Atlantic and Pacific coasts must have played a key role. One hint of how this may play out is provided by Stanford et al. (Chap. 5) in their discussion of the Cinmar finds. These provide unequivocal evidence for the early use of the submerged landscape 100 km offshore of Chesapeake Bay on the Eastern seaboard. If the dates are confirmed—and the arguments in favour of associating the laurel-leaf spear point with the radiocarbon-dated mammoth tusk are persuasive—they extend human presence in the Americas by nearly 10,000 years beyond the current earliest widely accepted date of entry, a dramatic result with serious implications for current debates about the timing and mode of entry of the earliest colonists.

Similar arguments and investigations are under way into the role of the submerged landscape in early population movements from Africa across the southern end of the Red Sea into Arabia and the India Subcontinent (Bailey et al. 2007; Lambeck et al. 2011). In Australia, perhaps because human colonization necessarily involved sea crossings and presumed exploitation of marine resources even at lowered sea level, reconstruction of submerged landscapes has been seen as less critical to understanding the process of dispersal. But, as Nutley (Chap. 14) observes, the earliest sites that acted as points of departure in Southeast Asia, and the earliest landfalls in New Guinea and Australia, must now be under water, and investigation of the submerged landscape, which is extensive in this region, is critical to understanding the ecological and social dynamics that propelled human expansion out of Southeast Asia.

Another problem that is coming more sharply into focus is the social and demographic impact of sea-level change (Lacroix et al., Chap. 2; Jöns and Harff, Chap. 10; Momber, Chap. 11). The idea of flood events as triggers of demographic change has been much popularized by Ryan and Pitman's work in the Black Sea, linking the sudden inundation of coastal terrain with agricultural dispersal (Ryan et al. 1997; see also Turney and Brown 2007). These ideas are controversial because the different marine geoscientists who have worked in the region do not agree on the pattern of sea-level change (Lericolais et al. 2009; Yanko-Hombach 2011); because there has been little exploration of the submerged landscape and no hard evidence for or against pre-inundation farming settlement in low-lying coastal regions, and because agricultural dispersal was likely the outcome of a complex interweaving of ecological, environmental, climatic, and social variables that cannot be pinned down to a single 'prime mover'. At any rate, the Black Sea controversy highlights the need for improved data on sea-level change and on the changing environmental potential and human use of the now submerged landscape, and the need for detailed investigations that integrate sustained and critically evaluated environmental, geophysical and archaeological research. Jöns and Harff (Chap. 10) describe just such a project for the Wismar Bay region of the western Baltic with the discovery of some 20 underwater archaeological sites and the refinement of a sea-level curve that can be projected into the future. This example shows the enormous advances that can be achieved by integrating a multi-disciplinary team and persistent effort over a period of years.

The reality is that sea-level change has been a continuous and world-wide accompaniment to human existence throughout the past 2 million years, and that flood events of greater or lesser severity have occurred repeatedly at many different times and places across the world. Lacroix et al. (Chap. 2) describe a good example from Atlantic Canada 3400 years ago that is still incorporated in the social memory of the present-day indigenous community, and Momber (Chap. 11) considers some of the ways in which progressive and episodic flooding of the North Sea resulted in long-term changes in regional archaeological records. Moreover, it is not only sea-level rise that poses questions about the human implications, but also sea-level lowering, which would have exposed new ecological challenges as well as extensive fresh territory for colonization, in some cases as extensive as the new territory exposed by glacial retreat at the beginning of the Postglacial Period.

Another theme of perennial interest that has seen a recent resurgence is the linkage of Holocene sea-level rise and stabilization to a complex of social and economic changes including intensified use of marine resources, sedentary settlements, increased social complexity, monumental architecture, and the development of early agriculture. The most recent and comprehensive elaboration of this theory (Day et al. 2012) suffers from the difficulties of its many predecessors in discounting or ignoring the contradictory evidence that may exist on the seabed from earlier periods of lowered sea level. Since the archaeological evidence of the social and economic changes in question must occur ex hypothesi in coastal regions, it follows that any similar examples that existed before the stabilization of modern sea level must, by definition, now be submerged and currently unknown, because systematic underwater exploration designed to find the relevant evidence has scarcely begun. The Holocene examples thus gain an exaggerated significance that may be largely illusory. Day et al. reinforce their argument by dismissing the productivity of submerged coastlines on the basis of generalizations about bathymetry and sea-level curves that are oversimplified to the point of caricature. As with everything else that we are learning about submerged prehistory, variability in local conditions and rates of change in the physical character and ecological potential of submerged coastal regions is likely to defy any attempt at simple generalization.

Similar criticisms apply to the belief that the increased representation of marine resources in archaeological sites of Last Interglacial age, notably in Africa, signifies an intensification associated with the appearance of 'modern humans' (e.g. Walter et al. 2000), when the evidence probably indicates no more than the increased archaeological visibility of coastal and marine activities during a period of high sea level; or the belief that the submerged coastline around the rim of the Indian Ocean is so uniformly productive that it must hide the missing evidence that is needed to support the hypothesis of a rapid coastal dispersal of modern humans from South Africa to India 60,000 years ago (Mellars et al. 2013). Until investigations of the type described in this volume are more widely applied, the role of the continental shelf will continue to be discounted or exaggerated according to the particular theoretical preconceptions of the authors in question.

Exploration Strategies

Integrated research that combines critical assessment of archaeological and geoscientific data from the continental shelf is difficult, but the potential rewards are considerable, not only in challenging existing archaeological orthodoxy and creating new knowledge about the deep history of coastal, maritime, and seafaring activity, but in refining the understanding of past sea-level change. New problems will place new demands on methods of exploration, and that challenge should not be minimized. The first step in many cases, and one that can be achieved with a high probability of success, is the reconstruction of the physical features and environmental characteristics of the submerged landscape. Even without the discovery of archaeological material, that first step can provide a new perspective on the interpretation of the existing archaeology on land, as demonstrated by Lacroix et al. (Chap. 2) and Werz et al. (Chap. 13). It also provides an essential baseline for locating earlier archaeological material under water.

When it comes to the location of archaeological finds, the risk of failure is higher. Many of the most impressive archaeological sites were initially found by chance, but future work must develop purposeful and successful strategies of site identification. There are, however, many hopeful signs. The use and adaptation of Anders Fischer's site-fishing model to predict the location of submerged sites in European settings is well known (Fischer 1997; Benjamin 2010). Equally impressive in its success is the work reported in this volume that has been going on for some time in North America. Development of predictive models based on known archaeological sites on land, reconstruction of submerged land forms using a combination of diver inspection, video, and acoustic survey, and taking account of preservation issues, and testing and retrieval of archaeological remains using coring, grab sampling, or excavation, are common ingredients of an evolving research strategy on both sides of the Atlantic (Faught, Chap. 3; Pearson et al., Chap. 4; Dixon and Monteleone, Chap. 6; Jöns and Harff, Chap. 10; Momber, Chap. 11). Similar thinking is informing the research design of underwater exploration in Africa and Australia (Werz et al., Chap. 13; Nutley, Chap. 14).

One of the most impressive examples of site discovery is the work of Daryl Fedje and associates off the coast of British Columbia, reported here by Dixon and Monteleone (Chap. 6), involving bathymetric survey of land forms, lakes and stream channels, identification of a likely site location at a depth of over 50 m, application of a bucket grab, and the retrieval of a stone artefact and some wood. Further work on this site should certainly prove of great interest but appears to be stalled for the moment for lack of funds. Dixon and Monteleone, on the basis of their experience of running transects that combine a remotely operated vehicle with side-scan sonar, go so far as to assert that site survey under water may actually be easier than on land in their region. Whether that optimism can be justified elsewhere remains to be seen, but as more work is carried out and more discoveries are made, so the momentum for new research will grow.

Conclusion

The discipline of continental shelf archaeology, or submerged prehistoric archaeology, is still very young, and the logistic and financial hurdles to be overcome remain formidable. Progress over the past 30 years has been slow, but there has been a marked acceleration of interest and work in the past decade, and the range of research now being carried out suggests that the discipline has reached a critical mass that should provide the momentum for future work. As the results of ongoing work become more widely disseminated, so the research problems capable of being illuminated by underwater research will become refined and expanded, and the justification for funding easier to make, creating a virtuous circle of interaction between new field investigations and new ideas. It is not too much to suggest that we are entering a new phase of development, with a panorama of new research opportunities opening up that will transform our understanding in the coming decades.

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