Amanda M. Evans · Joseph C. Flatman
Nicholas C. Flemming *Editors*

Prehistoric Archaeology on the Continental Shelf

A Global Review

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To our families, especially to our children
Colin Keith, Zoe Flatman, Kirsten Flemming
and Peter Flemming

Preface

Seabed prehistoric archaeology has arrived during the last decade at what economists like to call 'escape velocity'. Archaeological sites ranging from 5,000 years old to around 1 million years old have been found offshore, mapped and sometimes excavated off all major continents, in both hemispheres, from the shore to depths of over 100 m, and from almost the pole to the equator. Research groups that have durability and funding are becoming established in many countries. The new data are being absorbed and interpreted.

Good ideas, good inventions, and new frontiers of research have a way of being discovered or invented many times before they are finally proven to work or to be intellectually useful. From flying machines to steam engines, from diving gear and safety razors even to the alteration of species through time, the story has been the same. Flood myths such as Deukalion, Noah and Gilgamesh go back thousands of years in written form, and probably 10,000 or more to their oral beginnings. Submerged cities in the Mediterranean were well known to the ancient geographers and historians, sometimes correctly and sometimes with embroidered details. Successive glaciations in the European Alps were deciphered during the mid-nineteenth century, and immediately led to the calculation that the ice volumes on the continents would lead to a global sea level drop of the order of 100 m.

By the early twentieth century, palaeontologists and archaeologists had noted shoreline caves in Algeria and southern France containing bones of extinct megafauna that could only have walked there when the sea level was much lower. Fossil bones, terrestrial peat, and occasional flint tools were trawled up by fishermen, and correctly explained as originating when the continental shelf was occupied by human ancestors. All finds occurred by chance, and there seemed no way of making research on the seabed proactive. The available technology was seriously inadequate. During the twentieth century, steady enhancement of acoustic survey of the seabed through single-beam echo sounding, side-scan sonar, and then multibeam swath bathymetry, resulted in a much fuller understanding of drowned river valleys, periglacial phenomena such as moraines and ice tunnels, fossil coral terraces, and many other terrestrial or fossil coastal features remaining intact on the continental shelf. After 1945, the exploitation of offshore hydrocarbons and dredging for aggregates and navigational channels produced still more data. Divers, both commercial and amateur, reported complex geomorphological features on the seabed, submerged caves that could only be Pleistocene low sea level shorelines, and sometimes found prehistoric remains in sedimentary areas. I started research for my PhD in 1960 when side-scan sonar was a new tool, and just before oil and gas were discovered in the North Sea. Anything seemed possible. However, I also knew that my plans to study submerged Pleistocene caves and tectonically submerged classical ports in the Mediterranean were based on more than a century of previous scholarship. My hero was A. C. Blanc whose work on the west coast of Italy in the 1930s and 1940s showed how it might be plausible to go beneath the surface of the sea and search for prehistoric remains as a deliberate plan with a chance of success. Since then, a host of discoveries by many researchers in the southern Baltic, off the coast of Israel, in the North Sea, off both the Atlantic and Pacific coasts of the Americas have shown how far-sighted Blanc's ideas were.

This book is not an exhaustive global catalogue, which would have to contain references to many thousands of known seabed prehistoric sites. Rather, it is a highly selective set of sites, projects, surveys, and excavations from a wide variety of oceanographic conditions, climates and prehistoric cultures. The cumulative significance of this amalgam of sites is synthesised at the end of the book in the concluding chapter by Geoff Bailey. There are still huge uncertainties about the early migrations of hominins and anatomically modern humans which will only be resolved when we have a much larger data set to study from the sea floor. Equally, the role of the continental shelf as a refugium on the periphery of glaciated areas is still not understood, nor is the effect of the accessibility generally of the continental shelf and its resources during glacial maxima.

This book originated at the Sixth World Archaeology Conference (WAC 6) held in Dublin in June 2008. There was a session on seabed prehistoric research organised by Amanda Evans and Joe Flatman, and Amanda took the initiative to plan a published volume based on the papers in that session. Less than a month later, in July 2008, the Third International Conference on Underwater Archaeology (IKUWA 3) was held in London, with Joe Flatman chairing that conference's organising committee. At IKUWA 3, I organised a session on prehistory, co-chaired by Dimitris Sakellariou. Again, there was discussion of publication, and Amanda and Joe invited me to co-edit the proposed book with them. Inevitably, we found that some speakers were not ready to write fully argued texts, and the ones that were provided resulted in an unbalanced global selection, so we invited further contributors to make a more representative picture of the situation.

I thank the authors and my fellow editors who did much more work than I did, and I hope that my long experience in this field provided some guidance and help when most needed. The subject is entering a new era when new sites will be discovered in critical areas such as the Sunda-Sahul shelf and Beringia, and when the more fully explored sectors of the shelf will provide so many sites with a rich variety of dates, modern interpretation of cultures, demographics, change through time, and social structure will be possible.

Governments are beginning to plan systematic topographic and bedform mapping of their continental shelves at high resolution with multibeam survey for commercial, military and management purposes. This will have the fringe benefit of providing the maps needed to reveal drowned terrestrial landscapes where they are not cloaked in a thick over-burden of marine sediments. Other sonar techniques can then provide maps through the sediments, while Remote Operated Vehicles and Autonomous Underwater Vehicles are opening up new possibilities for systematic photography and optical surveying of large areas. Ultimately, the great majority of prehistoric sites can only be examined in sufficient detail and excavated by divers, with the progress in diving systems, and training the archaeologists to dive, as an essential step. I hope that this book enthuses some of the younger generations to join this exciting research.

Nicholas C. Flemming

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- First and foremost, Teresa Krauss, Hana Nagdimov and Morgan Ryan at Springer have all been tremendously helpful and endlessly patient as we brought this work together;
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Joe Flatman: I must acknowledge here the three people who have been crucial to my involvement in this book. First and foremost, I'd like to acknowledge my fellow editor and author Amanda Evans, who initially got me involved in organising a session on this topic at WAC6 back in 2008, and who since that time has been an extraordinarily patient co-editor, undertaking by far and away the lion's share of the workload. This book is above all the result of her vision and drive, and I thank her for allowing me to be involved in this process. Secondly, I'd like to acknowledge my other co-editor Nic Flemming, who has been a kind and generous contributor, guide and mentor to Amanda and myself in the production of this book. Third and finally, I acknowledge my wife Jennifer Young, who has been a tower of strength over the many years that I have been working with Amanda and Nic on this book, endlessly putting up with my discussion of its contents and forgiving, on occasion, my consumption of possibly one too many glasses of gin to wind down at the end of the day.

Nic Flemming: It has been a privilege to work on this book with Amanda Evans and Joe Flatman. I thank them both. One of my roles was finding authors in those parts of the world where prehistoric research on the continental shelf is a very new subject, and I thank those contributors who agreed to take part in a publication which must have seemed remote to their interests and immediate concerns. Additionally, given my work in this field over many decades, my editorial comments tended to pick up inconsistencies or gaps which might have been missed if the subject had been treated as only "new". I thank the authors and my co-editors for responding positively to such criticisms which may at times have seemed pernickety. My wife, Professor Jay Kleinberg is a historian, and I am grateful to her for the opportunity to learn about the analytic processes of the social sciences, and for her tolerance of my working hours, and my habit of turning holidays into research projects.

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Chapter 1
Prehistoric Archaeology on the Continental Shelf: The State of the Science in 2013

Joseph C. Flatman and Amanda M. Evans

Introduction

Prehistoric Archaeology on the Continental Shelf provides a review of data from submerged continental shelves around the world. In 14 chapters, data on sites, landscapes, analytical methodologies, and management tools from across the globe are discussed and debated. This is a snapshot of a scientific community in the throes of a dramatic phase of ongoing development. The data and analyses outlined in this book contribute to, influence, and, in many cases, drive the analytical agenda of prehistoric archaeology, underwater and terrestrial; the tools and techniques deployed are handled confidently; and the management of such sites is sophisticated and collaborative. Within this, however, it must be recognized that we still have a long way to go and a lot more to achieve; despite the heroic efforts of individuals and teams at work around the world over the past decades, seabed prehistoric research is still an evolving discipline, where, in particular, we have to find more sites. There are significant gaps in space and time where we have no data at all for thousands of years and millions of square kilometers, and we cannot do fully modern integrative and interpretive archaeology without more data and sites. In particular, there is a scalar mismatch between acoustics and signatures of prehistoric sites—that is, of identifying, from a distance, materials like worked lithics, fragments of bone or wood, charcoal, and arranged stones. Much research is at present being devoted to solving that problem. So far, visual inspection by divers or close-up remote sensing (ROV-based photography, etc.) are the only ways to detect lithics unless they have already been found by chance—as is so often still the case—be this the consequence of deliberate survey or industrial happenstance. Large-scale survey and analysis can

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show all sorts of probabilities, but few can afford to search hundreds or thousands of square kilometers visually. Just as still so often occurs on land, predictive modeling enhances probabilities, but not enough to give a reasonable chance of a survey finding lithics except in exceptional circumstances. In the marine zone even more than on land, we are still often in a rather humbling situation of constant iteration between chance finds, modeling, exploitation of known sites, interpolation, and guessing and hoping. Technology helps, but only so far, and technology improves all the time.

Being unafraid to recognize and admit to such methodological issues, and to dedicatedly search for advances on the current situation as the contributors to this book consistently do, is part of the present-day confidence in approach to this subject demonstrated by its practitioners. Such confidence is also the reason for the specific title of this book: It is about prehistoric archaeology that just happens to come from submerged environments on the continental shelf. In the past, such work labored under the niche title “submerged prehistoric archaeology,” reflecting a lack of engagement with mainstream prehistoric archaeology. But this book’s chapters demonstrate a community that has outgrown that niche to play the right and full place in global-level discussions of the prehistoric archaeology of the human race that the data from such contexts provide—including the unambiguous discussion of the pros and cons of the methodologies and approaches deployed. Prehistoric archaeology on the continental shelf is in the process of rewriting our understanding of key aspects of prehistoric civilization, from our earliest origins and first journeys, to our later exploitation, impact upon and exploration of the globe. The really exciting fact is that this data are merely the tip of the iceberg: as several chapters in this book indicate, the best is yet to come. In many parts of the world, the continental shelf represents an under-explored landscape that was available for exploitation throughout prehistory, but whose stories are missing from the archaeological record. Future discoveries and analyses of prehistoric archaeology from submerged contexts on the continental shelf look set to be genuinely earth-shattering, for example, new evidence, of the earliest arrival of humans in Australia, or of the extent of human activity in Beringia. Technology is also changing relatively faster offshore than on land (for example, the development of data storage in terabytes really changes the way one gathers data, the resolution that is usable, and removes the need for sampling data and plotting them as subsampled grids). Thus, in the twenty-first century the cutting edge of prehistoric archaeological research lies in submerged contexts, and that simply is not up for debate.

Prehistory on the Continental Shelf

Archaeologists have recognized the potential of continental shelves to contribute to our knowledge of the human past for over 50 years. Specifically, data from submerged sites contribute to both site-specific and landscape-level narratives, meaning that these analyses contribute to local, regional, and global-level debates.

Archaeologists study past human behavior, and build patterns by scaling-up data observed at the microscale, or site, to larger trends observed across regional, cultural, or temporal scales. An archaeological site is defined differently depending on the purpose, but generally is defined as a spatially delimited accumulation of cultural material that has sufficient quantity and quality to allow inferences to be made about behavior occurring at that location (after Butzer 1982, p. 259). Sites are critical to reconstructing past human behavior, but nonsites or data occurrences may still provide information needed to inform patterns of available resources (Butzer 1982, p. 260). As a science, archaeology is restricted to the data that have been found, but if archaeologists are ignoring entire landscapes it is undoubted that our current knowledge of prehistoric populations is flawed. This is a critical point to consider, since models are inherently biased by the information and variables used in their construction, and more importantly by the information that is omitted from the model.

Methodologies for the Continental Shelf

The methodology used in investigating sites on submerged portions of the continental shelf is intrinsically tied to technology and the specific environment under investigation. In some parts of the world, survey methodologies have been established for a long time—for example, in Denmark on the many submerged prehistoric sites analyzed there for many years (see Fischer 1995, 1997) or Italy on submerged cave-habitats, in particular (see Bard et al. 2002; Dutton et al. 2009)—but in all regions the methodology for investigating areas on the continental shelf has room for ongoing refinement. Like any aspect of archaeology, there is general agreement in some areas on the "baseline" analytical and methodological frameworks; such frameworks allow for more nuanced investigations that are not restricted to general "landscape survey" and which can consequently undertake higher-level analyses. Advances in methodology also encourage developments in technology. For example, advancements in mapping accuracy offshore (such as the change from Loran coordinates to DGPS or RTK positioning), allow for more precise control of context. Remote sensing data systems and the postprocessing capabilities for interpretation have increased exponentially, and will continue to evolve, and as noted above, such technologies are advancing relatively faster at sea than on land at the present time. These technological changes, however, complement a basic methodology used in many continental shelf contexts: it is not surprising—and nothing to be ashamed of—that a dredger, a bottom trawl net or a diver are more likely to find a flint tool, a bone, or charcoal deposit than a remote-sensing survey. For example, the Chilean site reported by Carabias et al. (this volume) was found by chance while undertaking a commercial contract survey on a jetty. Around the world, this is not an exception at sea any more than it is still on land, and is simply part and parcel of the complexities of how sites are found and how fieldwork is undertaken and paid for. It must be stated here, and repeated often, that no one methodology will work in all environments or for all types of sites. As the chapters of this book demonstrate, there is both the room and the need for an array of methodological approaches from the "low-tech" to the "high-tech, " from the site specific to the landscape oriented.

As an example, one early attempt to establish a methodology for investigating prehistoric sites on the continental shelf focused explicitly on the northwestern Gulf of Mexico (CEI 1977; see also Pearson et al. this volume). The recommendations produced by this study stated that any investigation of prehistoric resources on the continental shelf take a three-step approach beginning with remote sensing of the area through either small-scale bathymetry or subbottom profiling to resolve the upper 9 m (30 feet) of sediment coupled with acquisition of a grab or drag sample of seafloor sediments (CEI 1977, p. 341). If a probable site was indicated by the data acquired in step 1, then subsequent data should be collected, either in the form of side-scan-sonar imagery of the area, bottom cores, and or additional grab or drag samples (CEI 1977, p. 341). The final step, if warranted, was recommended as underwater photography or videography, box core sampling, and/or diver investigation (CEI 1977, p. 341). The majority of the recommendations, such as bathymetric survey or diver photography, assumed that the feature of interest was exposed at the seafloor, which is not always the case. The basic investigative methodology developed in 1977 for the northwestern Gulf of Mexico assumed the use of a predictive model that correlated identifiable landforms with archaeological sites as observed in contemporary terrestrial settings. The cultural groups included within this specific geographical and chronological landscape were highly mobile hunter-gatherers with scant material culture (Aten 1983; Neuman 1984; Ricklis 2004). The predictive model included geological reconstruction and landscape change modeling, but recognized that a paucity of artifacts would likely exist at submerged sites associated with this specific region. Cultural signatures of human occupation were, therefore, identified that went beyond artifacts, such as potsherds and lithics, to include signatures more likely to be recovered in core samples, such as shells, faunal fragments, black earth, burned rock, charcoal, and pollen (CEI 1977, p. 172). Subsequent studies have been conducted worldwide that add to the theory and methodology of investigating submerged prehistoric sites. The basic methodology outlined by the 1977 study has benefitted from improvements in the technology, but is specifically intended to identify landscape features as opposed to sites, and assumes that large-scale survey will be conducted. This is appropriate for an area undergoing large-scale development by oil and gas industry, but is not appropriate for all environments, or for investigating other scenarios, such as chance finds.

In 1981, in recognition of advances in paleocoastline reconstruction, archaeologists, anthropologists, geologists, and oceanographers were invited to participate in a symposium addressing Quaternary coastlines and prehistoric archaeology; the resulting papers were published in one of the first edited volumes on the subject (Masters and Flemming 1983). The participants in this symposium noted that, at that time, the majority of prehistoric artifacts from the continental shelf were the result of chance finds by recreational SCUBA divers and fishermen, or activities related to offshore construction (Masters and Flemming 1983, p. 611). Intentional site discovery, they maintained, depended on both physical preservation of the site and ease of detection (Masters and Flemming 1983, p. 622). The participants presented diverse case studies ranging in location from Siberia to Australia, but concluded that a standard framework could be universally applied to site prediction and detection. At minimum, local geomorphology has to be modeled to identify areas of probable feature preservation, recognizable features (such as shell middens) must exist, and basic requirements such as access to fresh water, protection from environmental exposure, and/or availability of food must have existed within the area of interest (Masters and Flemming 1983, p. 623). Recommendations for survey and identification of prehistoric features were similar to those outlined for the Gulf of Mexico: chiefly, bathymetric or subseafloor survey conducted at tight intervals (no greater than 150 m). The authors stressed, however, that this type of survey cannot prove without doubt the existence of prehistoric sites, it can only identify the most probable areas in which sites could be preserved (Masters and Flemming 1983, p. 624). Again, the methodology outlined in the 1983 volume assumes that an investigation of the continental shelf is being driven by survey, and is not immediately applicable to site investigation due to the discovery of chance finds. For example, the Cinmar site off of the US Atlantic coast was discovered by the chance find of a commercial dredging operation (see Stanford et al., this volume)

Methodologies applied to the continental shelf are not restricted to large-scale survey: indeed, if anything the reverse is true, since much of the key work around Europe in particular over the last few decades has been site specific, often the result of chance discoveries of sites. Benjamin (2010) discusses a range of different such projects and gives a noteworthy evaluation of the evolution of attempts to create standard methodology; the SPLASHCOS European Commission COST program (Cooperation in Science and Technology) research network that ran between 2009 and 2013 (http://www.splashcos.org/) includes other such examples. To cite a rather different example, however, Gagliano et al. (1982) published the results of a study that analyzed terrestrial analogues for potential offshore deposits. The results, developed under contract for the United States' National Park Service, analyzed core samples from verified terrestrial prehistoric sites along the Gulf of Mexico coast. Lab analyses of sediment core data indicated that the following variables were credible indicators of modified environment: grain size, pollen content, geochemical composition, point-counts, foraminifera species identification, and radiocarbon dating of appropriate samples (Gagliano et al. 1982). Recognizing that site identification could not be dependent upon the presence of man-made artifacts, the terrestrial corollaries were developed so that landforms could be tested for indicators of prehistoric archaeological site occurrence without the presence of obvious anthropogenic artifacts such as projectile points (Gagliano et al. 1982, p. 115). Numerous studies have been conducted around the world that have employed variations of the continental-shelf methodologies outlined above (e.g., Pearson et al. 1986; Johnson and Stright 1992; Browne 1994; Faught and Donoghue 1997; Momber 2000; Dix et al. 2004; Gaffney and Kenneth 2007; Benjamin et al. 2011).

Some research projects have avoided the complications of working in submerged environments by using evidence from terrestrial contexts to address changes in human subsistence and coastal settlement patterns instigated by changing climate conditions (e.g., Bailey and Parkington 1988). Although the technologies and environments are different, there are some similarities across many of the chapters that follow, representing locations ranging from Beringia to Argentina. For example, we now know that anthropogenic sites with artifacts can survive stratigraphically in context through several glacial cycles and several marine transgressions and regressions, something that was unthinkable less than 30 years ago. The Fermanville site (again found by chance) shows that a deep Paleolithic site can preserve stratigraphy even though exposed to tidal currents on the seabed and several interstadial sea-level changes (Scuvée and Verague 1988).

The techniques outlined above do not represent a universal methodology to all continental shelf sites, but are well established and constitute different tools and options that the research planner can draw upon in order to obtain data. Critical to this volume is an acceptance that good data are good data, irrespective of where they come from. Good data are defined here as trustworthy data, data underlain by solid, reliable, and repeatable methodological tools and techniques. This is the type of data, and type of approach, now consistently being achieved by those working in submerged contexts. The confidence in the approaches deployed means that the archaeologists involved spend more time asking questions of that data and formulating new hypotheses, and less time worrying about how to collect that data and their potential (un)reliability.

Global Significance of Continental Shelf Prehistory

Beyond discussion of the reliability and significance of the data being recovered lies the reality of the untapped potential of prehistoric sites located on the continental shelf, which is huge in terms of the extent of the potential search area, likelihood of any discoveries being significant either because of their location of detailed content, and possibility of discovery due to the level of industrial activity currently being undertaken or planned on the continental shelves alongside the sophistication of the tools and techniques used to survey these areas. Put more simply:

$$W(\text{area}) + X(\text{potential}) + Y(\text{likelihood}) = Z(\text{significance})$$

Studies conducted in an area where there is a strong understanding of the physical environment (W), combined with a predictive model that identifies the landscape or physical features of archaeological interest (X), and that are conducted in an area with a high rate of preservation potential (Y) are likely to yield results of local, regional, and probably global significance (Z). A good starting point for these analyses is the map first produced by Geoff Bailey for Nic Flemming's (2004) *Submerged Prehistoric Archaeology of the North Sea.* As Flatman (2012) outlines, the untapped potential of the continental shelf of SE Asia is but one example of the conjunction outlined above. Bailey's 2004 map also highlights other locations with high potential for finds, the ultimate theme of this book—the continental shelves of

South and Central America, Africa, the Arabian Peninsula, and the Indian Subcontinent. These are areas with unbridled archaeological potential where discoveries are likely to rewrite our understanding of global prehistory, and crucially, they are all areas undergoing active exploration, primarily for industrial objectives, in ever greater detail (see also Bailey 2011). This exploration may not always be beneficial in terms of the survival of prehistoric remains (see Bicket et al. and Faught, this volume), but it is assuredly beneficial in the identification of such remains.

Continental shelf prehistory has the potential to contribute to fundamental questions in archaeology. For example, one of the most prevalent hypotheses, and for a time the only accepted theory, for the peopling of the New World argued that the first Americans walked across the Beringia land bridge during the last glacial maximum, and populated the New World at approximately 11,000 years BP (Bonnichsen and Lepper 2005; Meltzer 2009, p. 3). Consensus could not be reached in explaining how those early inhabitants spread from what is now mainland Alaska throughout the remainder of the western hemisphere (e.g., Wendorf 1966; Fladmark 1979; Dixon 1999). Further complicating the question of modern human's first arrival in the New World were the increasing numbers of archaeological sites that predated 11,000 BP. Early archaeological sites (older than 11,500 BP) were once considered to be anomalous. Absolute dates, stratigraphy, and site integrity were, and continue to be closely scrutinized. In the case of Monte Verde, Chile, one of the first sites to return anomalously early dates, the occupation dates were highly disputed, and subjected to intensive scrutiny by a multidisciplinary panel of over 40 specialists in 1997 (Bonnichsen 2005, p. 15). The findings of the panel, which included several staunch critics of the site, validated some of the dates for Monte Verde and were cited as evidence that the hypothesis of the Bering land bridge as the first and only migration route was inaccurate (Bonnichsen and Lepper 2005, p. 15). Archaeological sites such as the Meadowcroft rock-shelter (Pennsylvania, USA), Monte Verde (Chile), the Debra L. Friedkin site (Texas, USA), and the Channel Islands of California (USA) have produced absolute dates that indicate the presence of modern humans much earlier than 11,000 years BP (Bonnichsen 2005; Goebel et al. 2008; Erlandson et al. 2011; Waters et al. 2011). Evidence from these and other recently published sites continues to push back the date range for possible occupation of the western hemisphere before 12,000 years BP.

Future Directions, Opportunities and Challenges

The levels of collaboration and cooperation currently witnessed between the marine archaeological and industrial communities in many locations around the world are unprecedented, and would have been unimaginable even a decade ago. While such collaboration is by no means universal—one need only think of the lack of archaeological involvement in current continental shelf exploration and exploitation along the coast of Africa—there is in general a good precedent for both continued and expanding relationships in this regard. As outlined in Flatman and Doeser (2010) (see also Flemming 2011), there is a simple reason for this: mutual benefit. Successful marine-zone prehistoric heritage projects always involve some or all of the following characteristics, characteristics that are not always shared by ostensibly similar terrestrial projects:

- *Business facing:* Such projects are strategic, timely, and well managed, responding to currently pressing needs to identify, and help mitigate, shared risks. Many marine-zone heritage projects use the same data sets for archaeological site identification as are used in assessing the presence of shallow seafloor hazards, thereby making the archaeological assessment a cost-effective component of the overall project.
- *Proactive:* Such projects are good at showing immediate functionality and use to all partners, such as modeling the locations of sites or seabed and water column dynamics around particular locations. The efficiency of stakeholder partnership projects is often instrumental to this functionality and cost-effectiveness, such as through the use of legacy data or industry platforms, and frequently involves industry provision of in-kind support via the loan of equipment.
- *Communicative:* Such projects see effective local-level, long-term communication and collaboration between individual industry employees, researchers, and curators.
- *Partnership based:* Many projects are partnerships from the outset, with all partners being included in project development and design, data sharing and collection, and/or data processing.
- *Media friendly:* Such projects undertake outreach, including significant public outreach and media potential for all partners through internal industry media and conferences, and the provision of accessible, user-friendly resources.
- *Mutually beneficial:* Such projects assist industry and the planning sector in the acquisition of new data sets (allowing for better preplanning and risk avoidance); provide historic environment professionals with new investment (supporting management-based research into the historic environment as well as the development of analytical techniques); and provide all sectors with collaborative data acquisition, analysis, and management, together with the additional public relations benefit through media-friendly enterprises, data sharing, and sponsorship.
- *Cross-disciplinary:* Such projects have had at their heart cross-management of projects by both natural and historic environment professionals, intermeshing cultural and natural environment research specialisms and data.

The discussion of cultural resource management (CRM) archaeology and the wider management regimes of prehistoric archaeology from submerged contexts raises three additional points of discussion. The first of these points is with regard to the long-term durability of the marine CRM sector. This sector of the CRM community is currently one of the only parts of the wider CRM community that is currently booming in the midst of the sustained economic depression in place globally since 2007. The extent of industrial activities in the inshore and increasingly offshore zones around the world, stretched across the continental shelves, is staggering. Traditional industries and related infrastructures such as oil and gas exploration and recovery, marine mineral extraction, fishing (including increasingly fish farming), port and harbor development, pipe and cable laying are increasingly being joined by new industries such as wind and wave "renewable" energy development. All of these industries are forecast to grow at an exponential rate over the coming decades, both in traditional areas and also increasingly in new areas of discovery, such as South East Asia, Africa, and South America. But alongside this growth is an increasingly recognized—although not formally analyzed—lack of appropriately trained or experienced archaeologists within marine CRM firms. Anecdotal evidence, such as that discussed at the 2014 Society for Historical Archaeology (SHA) conference forum on capacity building in submerged precontact archaeology, demonstrates a sustained skills gap, with more jobs available than appropriately skilled people to fill them, the inverse of the normal hiring situation within the CRM community. In particular, there is a lack of practical survey data collection and analysis skills among potential new employees. Put simply, postgraduate university programs in archaeology must meet university curriculum standards that do not allow for practical sea time for students. Many students graduate from programs without the ability to run marine surveys and, more importantly, interpret the raw data that such surveys collect. This is a systemic problem, one that is increasingly recognized by the same academic institutions.

A different regulatory issue stems from the management of human remains from prehistoric submerged contexts. So far, such discoveries have been relatively few in number and crucially, have been made in areas with limited or no Indigenous communities involvement in the management of prehistoric sites above or below water. But given the range and intensity of industrial activity discussed in the following chapters, the likelihood is that significant future discoveries of human remains will be made in areas with Indigenous communities who are not afraid to exercise their existing legal rights to the control of ancestral landscapes and material culture. The legal battle over "Kennewick Man" in Washington State (USA) illustrates the potential for ancient remains from submerged contexts, and the complexity of determining legal "ownership" or cultural affiliation (see http://www.nps.gov/archeology/kennewick/). To date, no known legal cases have explicitly addressed archaeological human remains from continental shelf environments. However, in the USA, legal challenges to the proposed Cape Wind offshore wind turbine development illustrate the potential for conflict between indigenous rights and development (Evans et al. 2009). If the types of resource-conflict scenarios outlined in Flatman (2012) become a reality in the resource-hungry mid-twenty-first century, then such claims to legal control and/or ownership of submerged prehistoric sites may become serious issues in their own right, a crucial part of the "politics of the past" debate that has been being played out on land for generations.

A third regulatory issue then concerns the combined protection and crucially public recognition of the significance of prehistoric sites in such environments. At present, such sites are " protected" (when this occurs at all) through different forms of domestic environmental regulation, primarily marine planning regulations in force in many nations' territorial waters, as for example enforced by the Marine Management Organization (MMO) in the territorial waters of the UK, or the Bureau of Ocean Energy Management (BOEM) in the territorial waters of the USA. While this is no different from countless thousands of comparable prehistoric sites on land similarly managed through similar regulatory frameworks, the "higher level" specifically heritage-related regulatory systems that exist and that are used to protect, acknowledge, and celebrate such sites on land and in the intertidal zone are currently absent in relation to such prehistoric continental shelf sites in the marine zone. For example, in the UK, the 1979 Ancient Monuments and Archaeological Areas Act that formally "Schedules" archaeological sites of the highest national importance could be used to protect such sites underwater, as the Act does for many thousands of prehistoric sites of equivalent significance on land (although the terms of the Act restricts it, both on land and underwater, to sites with identifiable structures, a provision that can limit its protection of prehistoric artefact sites of the type in question here). The Act contains provisions for the protection of marine sites; it is purely a matter of the right sites being nominated for such protection, either as a result of one-off recognition on the basis of significance or threat, or, more usefully, as a consequence of sustained, strategic programs of survey and exploration of the type described elsewhere in this book, and already underway in some locations, for example under the auspices of the National Heritage Protection Plan (NHPP) in England, where an ongoing strategic program of work (with its origins in the Aggregates Levy Sustainability Fund that ran between 2002 and 2011) is currently in the process of identifying and proposing submerged prehistoric sites on the English continental shelf for such statutory designation (NHPP Measure 3A1, *Unknown Marine Assets and Landscapes,* see http://www.english-heritage.org.uk/professional/protection/national-heritage-protection-plan/plan/activities/3a1). The success of books such as Gaffney, Fitch and Smith's (2009) *Europe's Lost World: the Rediscovery of Doggerland* (and related TV shows about such sites), demonstrates that there is a public appreciation of an appetite for such prehistoric archaeology; one next step is thus its more formal recognition in the regulatory system, alongside other such nationally—indeed, internationally—important sites. Advances in international regulatory and celebratory systems might also have a role here in due course, for example, thorough the network of World Heritage Sites, potentially under the auspices of the 2001 UNESCO Convention on the Protection of the Underwater Cultural Heritage.

Epilogue

It is crucial that the challenges outlined here are not seen as insurmountable for exploration of the world's continental shelves. Offshore development presents opportunities for investigation and research, but requires that archaeologists undertake training appropriate to investigating formerly exposed landscapes that are now submerged on the continental shelf. As demonstrated by the chapters that follow, as well as elsewhere (see for example Fischer et al. 2011), methodological elements already exist that negate the question of whether continental shelf site investigation is even feasible. There is time and room enough for multiple approaches to prehistoric archaeology of continental shelves; what is required now is that more archaeologists engage in this type of research, refining and improving the methodology, thereby expanding the archaeological record. Only in this way will archaeologists uncover data specific to prehistoric coastal zones, which can in turn lead to new insights about past human migrations, exploration, and adaptations, and ultimately to our understanding of human prehistory.

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Chapter 16
New Developments in Submerged Prehistoric Archaeology: An Overview

Geoffrey N. Bailey

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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