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**ISLAND ARCHAEOLOGY AND THE ORIGINS
OF SEAFARING
IN THE EASTERN MEDITERRANEAN**

*Proceedings of the Wenner Gren Workshop held at Reggio Calabria
on October 19-21, 2012*

In memory of John D. Evans

Eurasian Prehistory Guest Editors:

Albert J. Ammerman and Thomas Davis



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INTRODUCTION

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Abstract

The workshop focused on recent advances in island archaeology and their implications for the origins of seafaring in the Eastern Mediterranean. Both are rapidly developing fields of study today. This was not always the case. For years, island archaeology in this part of the world was preoccupied with the Neolithic period and the colonization of its various islands by first farmers. Now it is time to uncouple the beginnings of voyaging from both of them and to rethink the earliest maritime activity in the region. It is worth noting here that, even as late as the 1990s, conventional wisdom held that hunter-gatherers were reluctant voyagers and that early sites on the islands were hard to find. When I first went out to Cyprus in 2003, there was only one pre-Neolithic site that was known on the island – the much debated rock shelter called Aetokremnos. On Crete, there was still no Mesolithic site in the literature, and in the case of the Aegean Islands, excavations had just started at two pre-Neolithic sites, the Cyclops Cave on Youra and Maroulas on Kythnos. By taking a new approach in the field on Cyprus, ten new early sites, including Aspros and Nissi Beach, soon came to light on the island's coasts. Then, on Crete, the first pre-Neolithic sites were found in 2008. By 2012, archaeologists had discovered one or more sites older than the Neolithic on seven of the Aegean Islands. In short, coastal foragers were going to sea in the time before the Neolithic period. Indeed, a new study of the chipped stone at Aspros indicates that coastal foragers were crossing over to Cyprus as early as the late Palaeolithic (13,000 to 14,000 years ago or the time just before the Younger Dryas). Then voyaging to Cyprus was further animated by the roller-coaster ride of climate change that took place during the time of the Younger Dryas itself (12,800 to 11,600 years ago). The microlithic stone tools found at dive site C in front of Aspros turn out to have close parallels with those recovered from levels at the Öküzini Cave in southern Turkey dating to the Younger Dryas.

Around 10,750 years ago, a new chapter opens in the history of early voyaging to Cyprus. Now the PPNA makes its first appearance on the island, as documented by recent excavations at two settlements in the interior, Klimonas and Asprokremnos. Subsistence is based on hunting (wild boar) in combination with some early plant cultivation (wheat). In effect, one is dealing with “hunter cultivators.” At more or less the same time in the Aegean Islands, there is evidence at Maroulas for the exploitation of pigs (together with a range of marine resources), a number of small circular structures and the working of obsidian from Melos. On the Aegean Islands and the Greek mainland, there is little or no evidence for agro-pastoralism in the time before 9,000 years ago. In contrast, already by 10,300 years ago, the Neolithic way of life is well established on Cyprus, where it takes much the same form as it does at PPNB sites on the mainland. Voyagers are now moving between the mainland and Cyprus on a more regular basis, and they are taking over with them not only obsidian blades but also a wide range of wild and domesticated animals. Of particular interest here are the sheep and goats. In light of their physiology, one can argue that at least some of the crossings are being made within the span of a day or so (otherwise such ruminants would not have survived the trip) – with the implication that boats and sea-going skills have reached a fair level of development by this time. To place what is happening on Cyprus, Crete and the islands of the Aegean in wider context, the workshop also turned to the question of why voyaging in the western part of the Mediterranean appears to have had a slower and later start.

THE WENNER GREN WORKSHOP

The purpose of this chapter is to introduce various aspects of the workshop sponsored by the Wenner Gren Foundation for Anthropological

Research, which was held at Reggio Calabria on 19-21 October 2012. The proceedings published here have the same title as the workshop. The plan is for the first eleven chapters to come out in the present issue of *Eurasian Prehistory*; the

remaining chapters on the Aegean world and the Western Mediterranean will be published in the journal's next issue (for the Table of Contents of the proceedings as a whole, see pages 5–6 in this issue). More will be said about the nature of a Wenner Gren Workshop and the steps leading up to our meeting below. The abstract that appears at the beginning of this chapter is the same as the one that I included in my report to the Wenner Gren Foundation after the workshop. Many of the participants (for their names and affiliations, see pages next issue) had never met one another before because they live and work in different parts of the Mediterranean world. Given the major advances in our field of research over the last ten years, there was much to share and discuss at the meeting. More specifically, there were several questions that we wished to explore in greater depth. For example, how far back in time can we trace the earliest evidence for forager-voyagers in the Eastern Mediterranean? And why does voyaging emerge at a later date in the Western Mediterranean? What forms do the oldest archaeological sites take on the large off shore islands in the Mediterranean Sea? And at what points in time do we begin to see more permanent settlement on islands such as Cyprus, Crete, Malta, Sardinia, Corsica and Majorca? Given the lack of direct evidence for boats older than 8,000 years ago in the current archaeological record, what indirect inferences can we make about the boats that were used in early voyaging between the mainland and the off shore islands? In light of the key role that island archaeology plays in our understanding of how seafaring began in the Mediterranean world, what are the main lessons that we have learned during the last decade? Looking out to the future, what new lines of study – such as the investigation of submerged prehistory – call out for greater attention so that the dual themes that we took up at the meeting will continue to move forward in the years to come?

Each year the Wenner Gren Foundation sponsors and provides funding for a number of workshops. Its grants make it possible for a small group of specialists in a given field of study – commonly one that is developing rapidly or else one that is moving in a new direction – to come together so they can exchange new ideas and take stock of where they stand today. Ideally, those

taking part in a given workshop should come from a range of different countries and backgrounds. In addition, there is the idea that the meeting should be held in a relaxed and informal setting in order to encourage open and wide-ranging discussion. In our case, there was the opportunity to hold the workshop at Altafiumara, an old castle overlooking the Strait of Messina, which has been restored and turned into a resort hotel in recent years. Again, we would like to thank Carlo and Gabriella Montesano for their hospitality and for making the three-day meeting a memorable one. By way of background, it is useful at this point to say a few words about the sequence of events leading up to the decision to organize the workshop on “Island Archaeology and the Origins of Seafaring in the Eastern Mediterranean.” Previously, I had the good fortune to put together a Wenner Gren Workshop on the topic of “The Neolithic Transition in Europe: Looking Back-Looking Forward.” It was held in Venice in October of 1998. Its proceedings then came out several years later in *The Widening Harvest* (Ammerman and Biagi, 2003). In short, I had a good idea of how productive it could be to bring scholars together and convene such a meeting.

In September of 2003, I had the chance to go out to Cyprus and look for the missing pre-Neolithic sites on the island. In retrospect, this was, of course, the right decision to make. At the time, however, it was the view of many archaeologists working there that I was not making a wise move in my careworn and that the chances of finding early site on the island were very slim. In chapter 8, more will be said about why I decided to go out to Cyprus and the new approach that we now took to doing fieldwork there. To make a long story, we soon found a number of pre-Neolithic sites on the island. In turn, this now led to a joint field project that I ran in collaboration with Pavlos Flourentzos, the Director of the Department of Antiquities on Cyprus. By July of 2007, we had discovered more than ten new pre-Neolithic sites on the coastal formations of aeolianite, which occur all around the island; we had started to make trial excavations at two of them, Nissi Beach and Aspros; and we had traced out into the water the Epipalaeolithic site at Aspros (Ammerman *et al.*, 2011; more will be said about the steps in our fieldwork in Ammerman in this issue).

In September of 2007, I gave a paper on my investigation on Cyprus at the conference called “The Global Origins and Development of Seafaring,” which was held at Cambridge University. The proceedings of the meeting were then published in a volume with the same title three years later (Anderson *et al.*, 2010). The conference gave me the chance to meet scholars who were working on early voyaging in other parts of the world. At the same time, it forced me to start thinking in broader terms about what we were finding at Aspros and Nissi Beach. In effect, over a comparatively short span of time, just four years, I had gone from being a complete novice when it comes to the origins of seafaring to a person who now had something new to say about how early voyaging began in the Eastern Mediterranean. Obviously, there was still much to learn and reflect upon. As I began to revise my conference papers and write the chapter called “The First Argonauts: Towards the Study of the Earliest Seafaring in the Mediterranean ” (Ammerman, 2010), it was already clear to me that we as archaeologists were just at the start of a long intellectual Odyssey. And we would have to rethink in fundamental ways our ideas about how voyaging began in the Mediterranean world. To begin with, we would have to uncouple the question of the origins of seafaring from the question of the colonization of the islands in the Mediterranean world, and the same would hold when it comes to the question of the Neolithic transition in Europe (Ammerman, 2010:89). In short, the first forager-voyagers in our part of the world should now be seen as having a greater time depth than its first farmers. To put it in other words, the first attempts at voyaging on a regular basis in the Eastern Mediterranean (and not just rare or accidental crossings of the sea) take us back to a time well before the Neolithic period. This was, of course, heady stuff. What this working hypothesis meant was that I was throwing down a challenge to conventional wisdom on how seafaring began: that is, discourse that had long focused on the Neolithic period and the colonization of the Mediterranean islands at that time. In any event, as the next three years unfolded (2008 through 2010), we continued to do more work on Cyprus. And new results of considerable interest kept coming in each year from Crete and

other island in the Aegean Sea. The literature on island archaeology and the new research on early voyaging could not keep pace with rapid gains in knowledge that were taking place in the field. I began to realize that there was the need to bring together the archaeologists who were doing the current fieldwork and organize another Wenner Gren Workshop. A grant application was put together and submitted to the Foundation in November of 2010. The workshop was given the green light in March of the following year. Now the task was that of finding a good place to hold the meeting. These were, in fact, bad and uncertain economic years in Greece where we first planned to hold the meeting; in the end, the decision was made to meet in Reggio Calabria on 19-12 October of 2012.

To facilitate the organization of the meeting and the dissemination of its results, we developed a web site that includes a wide range of different things: from the abstracts of the papers given at the meeting to information on how to reach the hotel from the local airport and from background material connected with early voyaging (such as “The Obsidian Story in a Nutshell”) to an album of the photographs taken at the meeting. This web site can be found at <http://seafaring.colgate.edu/> At the Workshop itself, the papers themselves were pre-circulated. Instead of reading each paper at length, the speaker was given 15 minutes to summarize the basic argument and then time was given over to discussion and the presentation of the latest supporting evidence (commonly not yet in the literature) for the thesis that the participant was trying to develop. One or two of the sessions turned out to be rather heroic in length. After a long session, one could always look forward to an amicable coffee break or a glass of wine on one of the terraces of the Altafiumara and take in the view of the ships moving through the Strait of Messina. On the last afternoon of the meeting, we left the hotel and made an excursion. The participants climbed aboard a large sailboat, and they had the opportunity to pass through the famous whirlpools that occur on the surface of the Strait of Messina. In *The Odyssey*, Homer describes the challenge that Ulysses and his crew had to face in sailing through the roiling waters between Scylla (still the name of the modern town located just to the north of the Altafiumara

Hotel) and Charybdis on the other side. We have posted video footage of sailing through one of the whirlpools on our web site. At the closing session of the meeting, there was agreement on moving forward and publishing the proceedings of the workshop. It was, of course, important to do this in a timely manner. Toward this end, the editors of this journal, in the days just after the meeting, offered me the possibility to bring out the proceedings rapidly in *Eurasian Prehistory*.

GETTING THE BOAT MOVING

This is the name that I gave to one of the sets of background notes that we handed out to each participant at the start of the meeting. In its longer and original version, this set of notes is still posted on our web site -- as part of the ancient history of the workshop. At the end of this chapter, two of the other sets of notes are included (each now in the form of a separate appendix): (A) the Ötzi Experience and (B) the Forum of the Argument. My aim in this section is not to repeat what I had to say about "Getting the Boat Moving" in the summer of 2012 but to introduce briefly some of the themes and the questions that we took up at the meeting. One of the basic questions to ask is why it took so long for the study of pre-Neolithic voyaging to get moving? Why did fieldwork on this question have such a late start? Here one of the factors to consider is the demanding environmental context in which the prehistoric archaeologist has to work. Indeed, one has the choice between two tough options. One of them involves working at a site in a place on or near the present coastline: that is, where the archaeologist will have to work in the context of a high-energy environment (one driven by a combination of waves, winds and storms). The other one invites the archaeologist to take the plunge and work on the seabed. This constitutes a no less challenging venue where the early site occurs in a submerged position because of the marine transgression that took place at the end of the Pleistocene (Benjamin *et al.*, 2011; see also Bailey in this issue). In short, the archaeologist is dealing with a quandary just in choosing the place to conduct fieldwork. More will be said below and in the chapters of the proceedings about the challenges that we have to

overcome on the environmental side in doing our work in the field. But first we need to step back and consider the bigger picture when it comes to the history of scholarship as it has unfolded over the course of the last forty years.

In the study of human evolution and also in the study of what is now also called "deep history" (Shryock and Smail, 2011), the origins of agriculture in Southwest Asia and the Neolithic transition in Europe represent two topics of major interest. In our enthusiasm to delve into them, there is a tendency to miss the boat. Over the last four decades, major advances have been made in our understanding of how agro-pastoralism began in what formerly was called the Near East and today the Middle East (see Ofer Bar-Yosef in this issue). In the case of the Levant and southeastern Anatolia, early farming was broadly established by around 8,500 cal BC in what is conventionally known as the PPNB period (see Fig. 1 in Davis in this issue, which gives three parallel time lines for each of three neighboring regions as well as background on the chronological terms that are used in this part of the world). At settlements of PPNB age, the subsistence economy is based on the cultivation of domesticated cereals and animal husbandry. As seen at the site of Shillourokambos on Cyprus (Guilaine *et al.*, 2011; see Vigne *et al.* and Briois *et al.* in this issue), the PPNB had already reached the island in the years between 8,400 and 8,300 cal. BC. If we then move back in time some 1,500 years to around 10,000 cal. BC and the period called the PPNA, we find that the first attempts were being made at the cultivation of cereals in the Levant and southeastern Anatolia. In reality, what one is dealing with are the first rather tentative attempts at the domestication of plants and animals, and some of these early "experiments" were less than successful. In the case of Cyprus, we now have at 8,750 cal. BC good evidence at the site of Klimonas for hunter-cultivators with a material culture (architecture and stone tools) in the same tradition as the PPNA on the mainland (Vigne *et al.*, 2011, 2012; Vigne *et al.* and Briois *et al.* in this issue). At Klimonas, there are, in addition, several pieces of worked obsidian from a source on the mainland, which provide a further support for voyaging at the time.

What answer do we come up with if we step back and ask, at the most basic level, the following

question. Which came first agriculture or seafaring? If one had asked this question in 1980, the answer would have been a clear and obvious one. It was agriculture. For the archaeologist, the pathway to early farming – a better term to use than agriculture – was older. The task assigned to early voyaging 30 years ago was a subsequent and secondary one: that is, to move the first farmers out to an unoccupied island such as Cyprus. In other words, those who initially went to sea in the Mediterranean lands were viewed as the delivery boys of the so-called Neolithic Revolution. If one asked the same question in 2000, the answer would still have been much the same. In the literature on island archaeology and early voyaging in the Mediterranean world, the focus had been for years on the Neolithic period and on the colonization of islands large and small by first farmers (e.g., Evans, 1973, 1977; Cherry, 1981, 1990). Even as late as 2000, there was still the belief that foraging and voyaging did not really mix. As mentioned in the abstract, conventional wisdom held that that foragers in the Mediterranean world were reluctant seafarers and that sites dating to the time before the Neolithic period were extremely hard to find on large off short islands such as Cyprus, Crete, Sardinia and Corsica (Cherry, 1990). At that time and for the next decade, the review of the literature by Cherry appeared to be both comprehensive and convincing. In effect, it did not encourage the archaeologist to go out in the field and search for the missing pre-Neolithic sites. Thus, when it comes to early voyaging, there continued to be a preoccupation with the Neolithic period and the colonization of islands. When we ask the same basic question today, the answer is a different one: the first forager-voyagers are older than the first farmers. Indeed, as mentioned in the abstract as well, voyaging to Cyprus can now be traced back to the end of the Upper Palaeolithic. In short, the tables have turned, and this is the story that we wish to tell in the proceedings of the Wenner Gren Workshop.

This new perspective is just beginning to enter the literature (e.g., Broodbank, 2006, 2012; Ammerman, 2010; Knapp, 2010, 2013; Simmons, 2012). The point to underscored here is the recency of the dates of the respective publications. We shall return to the current literature below and

highlight some of its strengths and weaknesses. If we read the pages of *Deep History* (Shryock and Smail, 2011), an ambitious attempt to provide a broad overview and synthesis of what human beings have done over the course of the millennia, what we find is rather surprising. There is no mention of early voyaging. *Deep History* was meant to be a definitive statement. The editors worked in close collaboration with a group of nine leading specialists in putting together what the subtitle of their book calls “the architecture of past and present.” Perhaps the absence of seafaring and early voyaging in the book should come as no real surprise to us. For many years, the conventional wisdom, as indicated above, was that early voyaging was simply one component of the Neolithic transition in Europe. It did not really have a life or history of its own. Today we can look back in retrospect and realize that we are dealing with what amounts to a blind spot in the vision of the remote past. What we find in *Deep History* provides an index of the land-based agenda that has been embedded in the study of prehistory and human evolution for years. As Anderson (2010:1) notes at the beginning of his opening chapter in *The Global Origins and Development of Seafaring*: “There is no global synthesis, epitome or theory of seafaring as a venerable human activity, as exist for foraging, agriculture, settlement patterns, warfare or art”. In the second chapter of the same book, Erlandson (2010:23) draws the following conclusion in his closing remarks: “Evidence for the growing antiquity of coastal adaptations, seafaring and maritime societies is leading to a fundamental reassessment of the role of the sea in human prehistory – including the origins and development of seafaring.”

In many ways, this blind spot is the consequence of the land-based agenda that has dominated the study of prehistoric archaeology since its earliest days. Such a tradition is fully understandable. In fact, it stems from a deeper intellectual tradition that goes back to the time before Archaeology and Anthropology had emerged as academic disciplines. It can be seen, for example, in the following words that Giambattista Vico wrote in the third edition of *Scienze nouva* (the New Science), which appeared in 1744: “This was the order of human institutions: first the forests, after that the huts, then the villages, next the cities,

and finally the academies” (Bergin and Fisch, 1968:78). With the exception of the last step (and even here Vico may be on the right path), Vico has outlined, by using land-based language and metaphors, much the same sequence of human evolution that we still use today. There is apparently no place for seafaring – recent or old – in Vico’s formulation of deep history. Indeed, the whole story of human evolution takes place on land. This chapter is not the place to discuss at greater length the intellectual history of the blind spot that we have identified. The point that I am trying to make here is that it too does have deep time roots. What may be more productive for our present purposes is to give a further example of this blind spot in operation today.

It occurs in the book of considerable interest that Jared Diamond (2012) recently wrote in bringing together his ideas on the human condition and “deep history,” based on his years of experience in the highlands of New Guinea. By the way, I am an admirer of his previous books such as *Guns, Germs, and Steel*, and I have had the good fortune to go out to New Guinea as well. Now Diamond attempts to probe the depths of what he has learned there. In one of the many arresting photographs in his book (Diamond, 2012:plate 32), he shows three men voyaging in a small boat at sea; they are traders carrying goods to exchange with their traditional trade partners. The boat, which measures about 8 m in length and 2 m in width, consists of two dugout canoes with a light wooden super-structure that bridges them and holds them in position. In fact, the boat in the photograph, except for its elaborately decorated prow, looks much like the one that the French team has put forward in its attempt to reconstruct the kind of boat used for making voyages between Cyprus and the mainland in the 9th millennium cal. BC (see Fig. 4 in Vigne *et al.* in this issue). Harding (1967) in *Voyagers of the Vitiaz Strait* gives a fuller account of the exchange systems that are involved on New Guinea. In his ethnographic case study, which was done at a time before the modern world had begun to transform the ways of life on New Guinea), the Siassi make only a few trips as traders each year, and their voyages are comparatively short ones. We shall return to Harding and the Siassi in the last chapter of the proceedings. Beyond the photograph in his

book, however, Diamond (2012) has little to tell us about seafaring or early voyaging in *The World until Yesterday*. Here again we encounter the blind spot in the literature. In the case of Diamond, not picking up on the theme of going to sea is truly a lost opportunity for us all.

As a biogeographer, he potentially has much to say about the contribution of voyaging – either “yesterday” in small boats or else “today” in giant container ships – to the human experience. Among other things, this would include four things: (1) the gain in the means of human mobility beyond what one can achieve on foot, (2) the rise in the diversity of the material culture of a given community (illustrated by the traders in the fascinating photograph), (3) the increasing scope for contact and interaction between human populations that live at some distance from one another, and (4) the key role that voyagers have played in reshuffling the biogeography of plants and animals (see Vigne and see Ammerman in this issue). In thinking about why Diamond chose not to delve into the wider implications of voyaging, one possibility to consider is that the words in the English language that we commonly use in talking about this theme – seafaring and seafarer – are not entirely on the mark. They carry with them connotations of a full-time profession – with long trips and many days spent out at sea and away from home each year. In the context of the small-scale, face-to-face ways of life on New Guinea that Diamond knows so well, such terms have a ring that is rather odd or ill-fitting. They are not really appropriate for societies that are still at a Neolithic stage of cultural development. It is for this reason that we prefer to use terms such as “voyaging” and “voyagers,” as Harding did in his study of the Siassi.

By the first decade of the 21st century, we find that the blind spot is finally beginning to lift. And the interest in finding the missing pre-Neolithic sites in the Eastern Mediterranean and excavating them is taking place on several different fronts: Cyprus, the Aegean Islands and Crete. At the same time, it is possible to recognize a broader change in attitude when it comes to the role of the sea and voyaging in the study of European prehistory. This conceptual shift can be seen in *Europe between the Oceans*, the overview and synthesis written by Barry Cunliffe (2008).

The pathway to his book began with the series of Rhind Lectures on “Peoples between the Oceans” that he gave at Edinburgh in 2002. This is not the time or place to review the many other strands in the story, which have contributed to lifting this traditional blind spot in the literature. At the same time, it is worth commenting at this point that there was a scholar in the previous generation who did have the foresight to recognize the importance of island of archaeology as a field of study. This was John D. Evans at the Institute of Archaeology in London, who worked first on Malta for many years and then excavated the early levels of the mound at Knossos on Crete. Already by the early 1970s, he had put forward his vision of “Islands as Laboratories for the Study of Cultural Process” (Evans, 1973). Today this remains a benchmark paper written at the inception of island archaeology as a field of research. The proceedings of the Wenner Gren Workshop are dedicated to his memory. Three of the participants at the meeting – Andrew Moore, Nikolos Efstratiou and myself – were his students at one time or another. In addition, Małgorzata Kaczanowska and Janusz K. Kozłowski have recently made a new study of the lithic assemblages recovered from the lowest Neolithic levels at Knossos (Kaczanowska and Kozłowski, 2008, 2011). All of us who took part in the meeting owe John D. Evans a debt of gratitude as the father of island archaeology.

The major hurdle in the study of the Mediterranean islands, as mentioned before, was that of finding the sites that date to the time before the Neolithic period. In retrospect, the discovery of the early sites turns out not to be so difficult, if one looks for them in the right ways (Ammerman, 2010). Among other things, this means looking at the right places on the landscape and at the right seasons of the year (when the lithics at the early sites are visible on the land surface). In other words, the search for the early sites has to be a pro-active one. For many years, the assumption was that the early sites would simply come to light during the course of the routine work of the multi-period survey, as it is commonly practiced in the case of Cyprus, Greece, Italy and Spain. My own experience on Cyprus indicates that what was missing all along were not the early sites but the right approach to finding them. If one looks for them on the formations of

aeoliantie on the coast, which the archaeologist had essentially ignored before, they are sitting there on the surface – patiently waiting to be found. In retrospect, the problem of the missing pre-Neolithic sites on Cyprus was due, in many ways, to the very success of the multi-period survey. Embedded in the sampling strategies of archaeological surveys done in the years between 1970 and 2000 was an agricultural agenda, which discouraged the archaeologist from covering the aeolianite (Ammerman *et al.*, 2011:268). In short, those places where old dune sands have become lithified and turned into rock over the course of geological time were considered to be marginal lands. If one wanted to understand the history of settlement patterns in a given area or region from Neolithic times through the medieval period, such “marginal” places were given very low priority. Here we see a variation on the land-based agenda of prehistoric archaeology, which was mentioned before. This time it takes the form of the bias in survey archaeology: namely to sample those parts of the landscape that are well suited for agriculture. And again this agricultural agenda is, in retrospect, fully understandable. However, if one were a forager-voyagers living on the coast of Cyprus, the aeolianite would have been just the right place for a campsite, as we shall see in Ammerman in this issue.

This brings us back to the environmental side of the story. We return to the challenges that one has to face, if one wishes to find and excavate the sites of forager-voyagers. When it comes to the study of the first Argonauts, space is far from a simple and straightforward matter. This becomes clear if we ask the following question. Where should we look on the landscape, if we are interested in learning more about coastal foraging in the time between say 11,000 to 14,000 years ago? Given that sea levels were some 70 to 50 m lower at that time, most of the sites once frequented by coastal foragers are no longer visible on land. In all likelihood, an early site now finds itself in a submerged position on the seabed. In this case, the archaeologist has to take the plunge and do underwater archaeology. This is, of course, no easy task. Indeed, few archaeologists have made the decision to rise to the challenge of working at a submerged site that dates to the time before the Neolithic. On the other hand, what the

archaeologist today has the chance to find on land is just the tip of the iceberg. In short, one may be dealing with a rare or unusual site. To put it another way, we as archaeologists have not chosen the best time to live and work on the sites of forager-voyagers dating to the end of the Pleistocene (see the diagram showing the basic pattern of sea-level change over the last 200,000 years in the preface to *Submerged Prehistory*; Benjamin *et al.*, 2011:xiii). And if along the coast today, the archaeologist does somehow manage to find an early site on land, this context too will present the archaeologist with environmental challenges of its own. In terms of geomorphology and ecology, the coastline is a dynamic and high-energy place. One is dealing with a restless habitat where the sea and the land are in an ongoing dialogue with one another. Anyone who tries to conduct an excavation at an early site such as Aspros on the west coast of Cyprus (Ammerman in this issue) or Ouriakos on east coast of Limnos (Efstatiou in the next issue) will soon learn that the archaeologist has to be ready to fight a battle with the elements. In this light, the archaeologist may be tempted to move to the interior and excavate a much richer and better-preserved early site there. However, in terms of sampling, sites in the interior may not offer a good guide to coastal foraging. Of course, what we need to identify and excavate in the long run are early sites located in three quite different settings: (1) those found in a submerged context today, (2) those on or near the coastline today (that is, at some distance from the shoreline at the end of the Pleistocene) and (3) those located in the interior. All of this, of course, is a tall order.

In this section, I have reviewed the question of why the study of seafaring older than the Neolithic period had such a late start. On one hand, there was a blind spot on the conceptual side. Given the land-based paradigm of prehistoric archaeology for many years, this did not appear to be a productive line of research to pursue. Based on fieldwork carried out in the last decade, we can now move beyond the conventional wisdom that foragers were reluctant voyagers in our part of the world. On the other hand, the contexts in which the early sites are found are less than user-friendly for the archaeologist. In particular, those sites that occur on the seabed today have very low visibility unless we make the effort to get

our feet wet. Thus, in terms of historiography, the study of forager-voyagers began from a position where we as archaeologists were doubly blind. Notwithstanding the limitations on the conceptual side as well as those on the environmental side when it comes to doing fieldwork, we have now managed to get the boat moving.

BRINGING TOGETHER TWO RESEARCH TRADITIONS

The archaeologists who took part in the workshop came from two quite different traditions of doing archaeological research. On one side, there were those with a specialization in Neolithic studies. They had worked for most of their careers on Neolithic sites, and they had done research on such things as Neolithic settlement patterns, the subsistence strategies of early agro-pastoralists and the Neolithic transition. On the other side, there were participants who had worked for most of their lives on Palaeolithic and Mesolithic sites: Geoff Bailey, Ofer Bar-Yosef, Janusz Kozłowski, Curtis Runnels and João Zilhão. In addition, there was Adamantios Sampson who had excavated the Mesolithic sites of the Cave of the Cyclops and Maroulas in Greece and Andrew Moore who had dug the earliest levels at Abu Hureyra in Syria, which date to the Epipalaeolithic period. In an earlier incarnation, I too had been trained in the Palaeolithic tradition; I had once written a doctoral thesis on the Mesolithic period in Italy – before going on to do other things. So there were eight of us at Altafiumara who were fairly well versed in the Palaeolithic tradition. While the two traditions of research share a number of things in common, they have their differences in other respects. More will be said about the differences below. In other words, these two traditions of research are not always on the same page. Indeed, there are even times when tensions arise between them in terms of practice and theory. And as one would expect, each tradition thinks that it knows best. For instance, those on the Neolithic side are often considered by their counterparts on the Palaeolithic side to be novices when it comes to the finer points of the chipped stone assemblages recovered at a Mesolithic or Epipalaeolithic site or the environmental changes that took place at

the end of the Last Glaciation. The Palaeolithic archaeologist is naturally more at home in writing about such matters. On the other hand, those working in the Neolithic tradition have their strong suits as well.

At previous meetings that turned to the themes of island archaeology and the origins of seafaring in the Mediterranean, scholars coming from the Neolithic tradition usually gave most of the papers. Thus, discourse tended to focus on the nature of Neolithic societies and their settlement patterns and, above all, on the colonization of the respective islands by first farmers. If Palaeolithic archaeologists did attend (up until the last ten years, it was understandable, as we have seen in the previous section, why they might take a pass and not attend such a meeting), they were invariably in the minority. At the meeting in Reggio Calabria, what we were striving for was a better balance between the two research traditions. Our intention was to bring together scholars from the two sides and encourage them to engage in dialogue with one another for three days. And on the whole, we managed to achieve what we set out to do. At this point, it is worth adding that there is now a good reason for moving towards greater balance. Since the dates of the earliest sites on islands such as Cyprus and Crete now go back to time before the Neolithic period, those coming from the Neolithic tradition may not have the best tools or the right experience to work at pre-Neolithic site. In moving back in time, there may not be a good fit between the new and now “older” foot and the traditional Neolithic shoe. As we are told in the dialogues of Plato, the person who knows best how to train a horse is obviously the horse trainer. The same would hold in the case of fieldwork at a pre-Neolithic site. In an ideal world, the director of the excavation should be an archaeologist who comes from the Palaeolithic tradition. Of course, things often turn out to be different in the real world. Indeed, in the Mediterranean, it is not uncommon to encounter the case where an archaeologist, who has previously excavated at Neolithic settlements for years, now takes up the task of conducting an excavation at the campsite of coastal foragers. And, in turn, this may lead to an awkward situation. While such an archaeologist is getting up to speed in the first season or two, the results

coming in from the field may be less than smooth in quality. The precedent for such a move goes back to T. W. Jacobsen, a classical archaeologist, who began the excavation at the Franchthi Cave in Greece in 1969. The first year of the excavation was a rather rocky one. However, at the end of the day, it would all turn out to be highly productive (Perlès, 1987). For our present purposes, much the same kind of shift can be seen in the following three cases: (1) when Alan Simmons, who had worked for years at Neolithic mound sites in Jordan, began to excavate at the Epipalaeolithic site of Aetokremnos on Cyprus in 1989, (2) when Nikolas Efstratiou, who had dug at Neolithic sites such as Makri and Knossos in Greece, took up the challenge of making a trial excavation at the Epipalaeolithic campsite of Ouriakos on the island of Lemnos in 2008, and (3) when Thomas Strasser, who had conducted Neolithic studies in Greece for many years, was given the chance to excavate at the Mesolithic site of Damnoni on the south coast of Crete in 2011.

It should come as no surprise then that some archaeologists working in the Palaeolithic tradition have had over the years their doubts about what was being found at pre-Neolithic sites excavated by those who had previously worked in the Neolithic tradition. This is all part of the game when it comes to the dialogue between research traditions. There were, no doubt, some awkward first steps in the field when it comes to the three cases mentioned above. But each of them in the long run, as we have seen in the case of the Franchthi Cave, is making its contribution to the study of coastal foragers. Today there is a name for what happens when a project that involves doing something new in archaeological research manages to get off on the wrong foot at the start. It is called the Ötzi experience – after the popular name given to the famous Ice Man, who unexpectedly came to light at 3,210 m above sea level in the Alps in 1991. Ötzi’s discovery and initial interpretation comprise a quixotic series of wrong moves and misadventures (for more on this saga, see Appendix A; the pictures that go with the text can be found on the web site). Notwithstanding such a poor start, the frozen body of the Ice Man, his clothes, his tools, his diet, his ailments and so forth would then go on to be well studied. We are dealing with one

of the true stories of going from rags to riches in the history of contemporary archaeology. Each year as part of the Venice Study Group of Colgate University, we take our students to see Ötzi at the Bolzano Museum. When we did this in September of 2012 – a month before the workshop – I again went over with the students the sequence of capricious false starts that took place on the first days of the discovery (see Appendix A) and then how archaeologists, by steps of approximation, slowly pieced together a better picture of Ötzi over the course of the next 20 years, including the correct reason for his death (an arrowhead in his shoulder). At one point in our visit to Bolzano, it clicked in my mind that those of us who work on early sites in the Eastern Mediterranean have all had our Ötzi experiences as well (several examples of them are given in Appendix A). In my opening words at the meeting, I retold the story of Ötzi to the participants. What it tells us is that, in an imperfect world, we have to begin somewhere, and the start may be less than pretty. The take-home message is that there is a need both for candor and for generosity in our research on the early forager-voyagers in the Mediterranean world. And we have to take the long view.

At this point, it is worth saying a few words about the differences between the two traditions of research. Only some of the main ones will be outlined here. They fall under four main headings: (1) the approach that is taken to the excavation of a site, (2) the level of interest taken in environmental change and human adaptation to it over the course of prehistory, (3) the depth of one's knowledge and experience when it comes to the study of hunters and gatherers and (4) the emphasis placed on work of high quality when it comes to the analysis of chipped stone assemblages. Most of these differences are fairly obvious, and they make good sense. In short, there is nothing new here. Only a few brief comments will be made on the first three points. The point that deserves further discussion is the fourth one. We shall turn to it at greater length in the next paragraph. To begin with, there are usually differences in size and scale between the excavation of a Neolithic settlement and the dig carried out at a campsite of coastal foragers. The quantities of artifacts recovered by the excavation in the former case are normally much larger

than in the latter case, where the archaeologist commonly takes a more fine-grained and intensive approach to recording the site's stratigraphy and the spatial distribution of artifacts (often plotting the coordinates of each find in three dimensions). Moving on to the second point, archaeologists working in the Palaeolithic tradition usually have spent more time over the years learning about and thinking about changes in climate, vegetation and sea levels at the time of the Last Glaciation than their colleagues on the Neolithic side. Again, this is fully understandable: many aspects of the environment calmed down after the roller coaster ride of climate change associated with the years in and around the Younger Dryas (e.g., Rosen, 2007). In terms of environmental change, the Neolithic is, in effect, a cakewalk in comparison with what came before it. While there are, of course, some notable exceptions on the Neolithic side, those archaeologists who are more attuned to changes in the environment are found on the Palaeolithic side. Turning to the third point, it is natural for the archaeologist who works in the Palaeolithic tradition to look with admiration on those in the past who led a mobile way of life and whose livelihood was based on hunting, gathering and foraging for marine resources. The archaeologist who works in the Neolithic tradition looks back, on one hand, with nostalgia on hunting and gathering and surreptitiously forward to the new way of life that is starting to taking shape on the other hand.

This brings us to the fourth point. In some ways, it may be, for our present purposes, more important than the other three put together. Chipped stone tools represent the main line of material culture that has come down to us when it comes to the study of coastal foragers in the Eastern Mediterranean. In general, it is fair to say that, in most countries in Europe, lithic studies enjoy a higher reputation on the Palaeolithic side than they do on the Neolithic side. The notable exception here would be the case of France where lithic analysis is often done at a high level in both traditions of research. François Briois has carried out high-quality studies of the lithic technologies of the chipped stone assemblages at the PPNA site of Klimonas and the PPNB site of Shillourokambos (see Briois and Guilaine in this issue). On the other hand, lithic studies in countries

such as Greece and Cyprus tend to be less well developed in the case of the Neolithic tradition. In part, this is due to the comparatively small size of these two countries as well as the major emphasis that is placed on the study of the Bronze Age and classical archaeology. There are simply not enough resources to go around and do everything well that needs to be done. At the same time, we need to bear in mind that the blade technologies of the PPNA and PPNA on Cyprus and those also occurring in Greece during the Neolithic period tend to be easier to study than the flaked-based technologies (often with a microlithic component) that one finds at Epipalaeolithic and Mesolithic sites. In short, the level of difficulty in the work of the lithic specialist tends to rise as one moves back in time.

More specifically, some of the differences between the Palaeolithic tradition and the Neolithic tradition, when it comes to the study of chipped stone assemblages, can be illustrated, if we consider two examples. The first one concerns the lithic material recovered from the lowest levels of the excavation at Knossos on Crete. In their re-examination of the material, Kaczanowska and Kozłowski (2008, 2011) were able to make new observations and present a more detailed analysis of the earliest Neolithic chipped stone assemblages at the site than previous scholars working in the Neolithic tradition. For instance, they were able to recognize for the first time connections with the lithics of Mesolithic age found at the site of Maroulas on the island of Kythnos.

The second case involves the lithics recovered from dive site C in front of Aspros on the west coast of Cyprus. In a preliminary study of the lithic material, Carole McCartney identified a total of 38 pieces of worked chipped stone there (Ammerman *et al.*, 2011:266-227). When Kozłowski and Kaczanowska re-examined the same set of material from dive site C in 2012, they now found a total of 60 pieces of chipped stone, including a number of microlithic tools, which had been missed in the study by McCartney (more will be said about the lithics themselves in Ammerman in this issue; the new lithic material will be presented in greater detail in a monograph in preparation that will be published by the Polish Academy of Science). Thus, McCartney working in the Neolithic tradition of lithic

analysis on Cyprus (she had previously studied the chipped stone assemblages at the PPNB sites of Tenta and Mylouthkia) had somehow come up with a completely different picture of the lithics recovered at dive site C than the one obtained by the two scholars from Poland working in the Palaeolithic tradition. In fact, they could now see close parallels with the lithics found in the Epipalaeolithic levels of the excavation at the cave site of Öküzini located not far from the south coast of Turkey (Yalçinkaya *et al.*, 2002). Thus, we had our own Ötzi experience at the site of Aspros. As we shall see in the other chapters in the proceedings, Kozłowski and Kaczanowska have now had the opportunity to study the lithics from six different coastal sites of pre-Neolithic age in the Eastern Mediterranean: the Cave of the Cyclops (Youra), Maroulas (Kythnos), Ouriakos (Lemnos), Ücedutlar (Gallipoli Peninsula), Aspros (Cyprus) and Nissi Beach (Cyprus). It is our good fortune that they have stepped in and done all of this work, using the high standards of the Palaeolithic tradition.

This is a good point to return to the recent literature (Broodbank, 2006, 1012; Knapp, 2010, 2013; Simmons, 2012). To begin with, it is worth noting that all three of the authors come from the Neolithic side (Simmons is currently working at a Neolithic settlement on Cyprus; Knapp has worked mainly at Bronze Age sites on Cyprus; Broodbank's main interest is in the Aegean Islands from the Neolithic onward). Accordingly, what is still missing in the literature is a more balanced perspective on the new pre-Neolithic sites (of the kind that we were trying to achieve at Altoufiumara). Two of the authors, Broodbank and Knapp, have not done fieldwork at any of the pre-Neolithic sites in the Eastern Mediterranean. In short, they lack the firsthand experience that is really called for in order to write well about them. Instead, they have had to piece things together from what they have read in preliminary reports and from their conversations with the archaeologists who have done the fieldwork at the respective sites. In addition, it is fair to say that, in terms of what we know today, all five of the contributions are already out-of-date for one reason or another. This should come as no surprise given the rapid pace of discovery in this new field of research. On the positive side, an active interest in early

voyaging – now going back to the time before the Neolithic – has finally made its appearance on the stage. Each of the five contributions is trying in its own way to get this message out. In short, there is no longer a blind spot in the literature. On the contrary, there is, if anything, the risk of a bandwagon effect: the situation where everyone in the room wants to get in a few words just at the time that something new is taking off.

In terms of the five publications, chapter 3 in *The Archaeology of Cyprus* (Knapp, 2013) is the best of the lot. Knapp's treatment of what is happening on the island in the years between 12,000 and 8,000 cal. BC is comprehensive, well written and developed at the appropriate level of detail. The review article that Knapp published in 2010 was a stepping-stone to the book that he subsequently wrote. The review article is, however, too dense and has its rough edges. The editor of the journal rushed the article into print before Knapp had the chance to make some final revisions. Editors too live in less than perfect world: they have deadlines to meet with their publishers. One limitation of the two publications by Knapp is that he draws heavily on the publications of Carole McCartney. Some caution is called for here. As we recently learned in the case of dive site C at Aspros, there are questions about the reliability of her lithic studies when it comes to assemblages that date to the Epipalaeolithic period.

Broodbank has the high ambition to write not just about one region of the Mediterranean but to take on the whole Middle Sea and produce an all-embracing synthesis that starts in the beginning and runs up to the time of the emergence of the classical world. He has been working on the project for many years, and the book is clearly a major achievement (Broodbank, 2012). On the other hand, in trying to work on such a large canvas, the story of the origins of voyaging tends to get lost in the painting. The seminal review article that Broodbank wrote in 2006 is actually more successful in this respect. One of the challenges and complications that Broodbank and Knapp have to face in writing about the early site of Aetokremnos is the controversy in the literature with regard to layer 4, the lowest stratigraphic unit at the collapsed rock shelter. What is involved here is a long-running debate.

It goes back to the negative reviews of the book by Simmons (1999) that both Binford and Grayson wrote (the references in connection with the debate are not given in this chapter but in Ammerman and in Simmons in this issue; they can also be found in Ammerman and Noller, 2005 and Knapp, 2013). According to the excavator (Simmons, 1999) and he always stuck to his guns in defending his position (see Simmons in this issue), layer 4 is an archaeological deposit where the bones of several hundred pygmy hippos are found in association with a small number of chipped stone tools (a total of 11 in all) as well as a certain number of marine shells, which represent food debris. On the other side, there are a number of scholars, including major figures in the fields of Palaeolithic archaeology (Binford and Mithen) and faunal analysis (Grayson), who view layer 4 as a palaeontological deposit (in particular, because of the lack of cut marks on the hippo bones). For them, one is dealing with a natural bone bed where archaeological material from layer 2 above has become mixed in layer 4 by means of processes of disturbance. In short, there is no consensus when it comes to the interpretation of layer 4. On the other hand, most scholars today are in agreement that layer 2 is an archaeological deposit, and they make selective use of its lithics, C-14 dates, marine shells and bird bones in writing about the site. By way of background, it will be recalled that Aetokremnos was not found by an archaeologist but came to light when an English schoolboy discovered it in 1961. In their explanation of the vast quantity of hippo bones that occur in layer 4, the excavators draw upon the overkill hypothesis put forward to explain the extinction of mammals in North America at the close of the Pleistocene. There is, however, still no consensus with regard to the overkill hypothesis: two leading faunal specialists in North America have even gone so far as to write its requiem (again see the references in Ammerman in this issue). Moreover, this hypothesis has not been used in the interpretation of any other site of Upper Palaeolithic or Epipalaeolithic age in Europe so far. Hence, the controversy is a thorn in the side of anyone trying to write the opening chapter of Cypriot prehistory. To his credit, Broodbank (2012:148-149) seems to enjoy retelling the story of the controversy; it enlivens

and gives a change of pace to his long narrative. Knapp patiently leads the reader through the two sides of the debate and comes to the conclusion that the controversy cannot be resolved at the present time (Knapp, 2013:56).

From a logical or scientific perspective, what we are dealing with is a dilemma. There are two quite different positions or hypotheses: (A) layer 4 is an archaeological deposit and (B) layer 4 is a palaeontological deposit. And only one of them can be correct. According to *The American Heritage Dictionary of the English Language*, a dilemma is defined as the situation that requires a choice between options that are or seem equally unfavorable or mutually exclusive.

In sum, those who are not in a hurry and have the patience of Job can look forward to the day when new evidence will become available and a clear choice can be made between the two horns of the dilemma. In other words, it will all come out in the wash. At that time, either the excavators will raise their arms in triumph and say that they were right all along. Or else those on the other side will raise a toast to the greatest Ötzi experience in the annals of Mediterranean prehistory. And if the latter does turn out to be the case, it will not be such a loss for either side. It will simply remove from the board an implausible start for the island's prehistory (restoring, by the way, the credibility of Cypriot prehistory). Layer 2 at Aetokremnos, which dates to the 11th millennium cal. BC (see Manning in the next issue), will still be left standing on the checkerboard.

Simmons (2012:895-897) recently wrote a short overview called "Mediterranean island voyages." His timing was poor. When the article came out (in mid November of 2012, a month after the workshop), it was already out-of-date. At one point, Simmons makes the claim that his site is firmly established. No mention is made of the controversy that has swirled around the site for years and continues to do so today. Many archaeologists, as we have seen above, view the site of Aetokremnos in a rather different light than Simmons does. Those scholars who know the story and who are charitable in outlook will think that he is showing a lack of candor here. Those who are more critical of his work may even think that he is in a state of denial. In any event, there are still many open and unresolved

questions when it comes to the interpretation of the site, and they came out at the workshop. Most of the participants take the work at Aetokremnos for what it is: a pioneering effort that broke new ground in its day but that has its limitations today. Accordingly, when Simmons gave his presentation at the meeting, they were diplomatic and generous. While they recognized the importance of what was recovered in the case of layer 2 (with the exception of the hippo bones, which may derive from layer 4), they asked a series of probing questions about the site's stratigraphy (in particular, the thin and discontinuous sandy lens called layer 3), the quality of the site's C-14 dates (most of them were run in 1990s and have large measurement errors; the dates on materials other than charcoal are often anomalous), the mixed character of the chipped stone material in layer 2, the shortcoming of not mentioning the Younger Dryas at any point in the book published in 1999 (see Appendix A; the Younger Dryas is held today to be one of the most promising lines of explanation for the extinction of large mammals at the end of the Pleistocene) and so forth. The answers that Simmons gave at the meeting were often less than convincing. In writing his chapter for the proceedings, we had hoped that he would not just dig in his heels, as he had done at the meeting, but engage more openly and positively with the issues that were raised there. This has not happened. He continues to be intransigent when it comes to his views on the hippo hunters and their role in the extinction of the pygmy hippos on the island. It is perhaps worth adding at this point that there is still no other site on Cyprus where hippo bones and early lithics have been found in association with one another. Thomas Davis and I had intended to take an active hand in editing his chapter – with the idea that this would give him the chance to update his thinking and move beyond what he had written in *Faunal extinctions in an island society* (Simmons, 1999). In the end, this was not possible. What is published here is simply the text, as it appears in the manuscript that he sent us; only a few minor changes have been here and there in the style of the writing.

My own views on Aetokremnos have evolved over the years. For this reason, it is useful to add a few words here on where my thinking is now headed. In 2004, I had the chance to visit

the site twice. Working in collaboration with Jay Noller, we made several new observations on the environmental setting of the site and wrote an article the following year, which included a map showing the position of the coastline at respectively 6,000 and 12,000 years ago (Ammerman and Noller, 2005:fig. 1). We tried to be generous in the article and even attempted to move toward a middle ground in the controversy by allowing for the possibility that pygmy hippos in layer 2 may well have been hunted by the foragers who frequented the site at the time (layer 4, in our view, was a natural bone bed). We also suggested that it would be useful to initiate a new cycle of carbon dating based on newer methods that have recently become available. The new dates on hippo bones from layers 2 and 4 might help to resolve the controversy. Subsequently, a French team under the leadership of Jean-Denis Vigne did implement such a dating program. While it worked fairly well in the case of the bones of wild boar (see Vigne *et al.* in this issue), the lack of collagen in the hippo bones meant that good C-14 dates could not be obtained from them. Thus, the absolute chronology of the hippo bones (both those in layers 2 and in layer 4) continues to be an open question (for more on this subject, see Vigne *et al.* in this issue and Manning in the next issue). But more to the point here, the comparative success in coming up with dates for the wild boar (even if they are regarded as approximate dates) and the failure to do so in the case of the hippo bones seems to imply that the latter are older than the former. In addition, when it comes to thinking about the hippos bones at Aetokremnos, we need to add to the mix what we now find in Moore *et al.* and Vigne *et al.* in this issue .

Vigne takes the position that the hippo remains found in layer 4 constitute, in all likelihood, a natural bone bed. This is based on a re-examination of the bones made by palaeontologists working on his team. In their chapter, Moore and Kennett review the current state of research on a major cosmic event and the impact that it may have had on the site of Abu Hureyra. In terms of chronology, the event appears to have taken place at more or less the same time as the start of the Younger Dryas (that is, ca. 12,850 cal. BP, based on the dating methods used in the study of the Greenland ice cores). If such a cosmic impact event did take

place at that time, it may have produced major changes in environmental condition on Cyprus as well – with the implication that it could have kicked started the decline in the pygmy hippo population on the island at that time (note that in Fig. 7 of Manning does a modeling exercise in which he attempts to define boundaries for the start and the end of occupation at Aetokremnos; however, “the boundary start,” as it is called, has low resolution due to the poor quality of most of C-14 dates for the site). Much of this is, of course, still work in progress. However, taken together, the addition of these two new elements to the story (Vigne’s new position on layer 4 and the possibility of the Younger Dryas being kicked started by a cosmic event) leads me to take a more cautious stance on the hippo bones in layer 2 . In Ammerman (this issue), we shall turn to the rock shelter called Agia Napa, a new palaeontological site, which has yielded a rich natural bed of hippo bones but no chipped stone tools. In terms of its size, its elevation and its distance to the sea, Agia Napa and Aetokremnos are quite similar to one another. The suggestion here is that the two rock shelters were the kinds of places where pygmy hippos went to die a natural death. In short, there is a third new element in the story, and it supports position B in the dilemma. Finally, there is a fourth element that we have to take into consideration. Given the “die back” of trees along the coast at the time of the Younger Dryas (Ammerman, 2010:87), one can argue that coastal foragers (those associated with layer 2) exploited the fossil hippo bones in layer 4 as a source of fuel for making their campfires and that, in doing so, they contributed to major disturbances in the bones at the site. In other words, there is good reason to think that Aetokremnos has a disturbed stratigraphy due to the extraction of hippo bones from layer 4 by those who frequented the site. In turn, this raises the possibility that all of the hippo bones recovered in layer 2 may derive from layer 4. Playing the role of the devil’s advocate, this alternative interpretation of what is happening at the site will be sketched in further detail in Ammerman (this issue).

In closing this chapter, it is worth turning briefly to what we need to do next in the study of the two themes that we explored at Altafiumara. What lines of research and what questions warrant

our closer attention? Where should emphasis be placed in this field of investigation over the next ten years? While these are questions that we shall return to in the closing chapter of the proceedings, it would be good, by way of introduction, to say a few words about them at this point so that readers can begin thinking about them as they read the respective chapters assembled here. They will be given briefly under five headings. (1) The first point to consider is a fairly obvious one. There is the need for more excavations to be conducted. Only a small number of sites on the coast dating to the time before the Neolithic period have been excavated so far. And the quality of the excavations – without dwelling on the matter here – needs to improve as well. We need to aim for excavations that are on a par with the best work done in the field of Palaeolithic archaeology today. At the same time, we have to be realistic and recognize, as mentioned before in this chapter, that we are conducting demanding fieldwork in challenging environmental contexts.

(2) The importance of doing lithic studies of high quality cannot be stressed enough. The studies should to be undertaken by those with advanced training in the Mesolithic and Epipalaeolithic periods. In the publication of the lithic material, there is the need for high-quality drawings of the chipped stone tools and cores as well as more figures that one tends to find in previous publications. This has been one of the main bottlenecks in the study of forager-voyagers to date. (3) This heading brings us back to submerged prehistory. This is the message of Geoff Bailey in this issue. We need to get our feet wet. There are two main reasons for doing so. First, if one is really interested in the study of coastal foragers going back to more than 10,000 years ago, then one has to work where the sites are located (when sea level was lower than it is today). Secondly, we need to do this in order to move beyond the land-based bias or agenda, which has long held sway in prehistoric archaeology. In the literature, there are still only two pre-Neolithic sites, in the Eastern Mediterranean where the archaeologist has taken the plunge so far (Ammerman *et al.*, 2011; more on this in Ammerman in this issue).

Under the last two headings, we turn to less obvious topics or themes to put on our wish list. (4) Indeed, the next heading takes us into less

familiar territory quite literally. In shorthand, it is what we may call “living at the edge.” Here I am thinking about the lifeways of coastal voyagers and the remote places that they often reach or pass through as a consequence of their voyages. Their widening mobility entails what I have called “extensification” (Ammerman, 2010:88). There are three key adjectives that we need to keep in mind in writing and thinking about the widening mobility of forager-voyagers. They are respectively ephemeral, expedient and liminal. In short, we have to move away from the language of aggregation that we commonly use in Neolithic studies. Ephemeral is used to describe something that lasts for a short time. This is the right word to use for the campsites that small groups of coastal foragers made on the coastal formations of aeolianite on Cyprus. Voyaging means being on the move, and it calls for a series of places on the coast where the voyager could stop along the way for a short time. And the voyages themselves – much like those of the Siassi traders in New Guinea (Harding, 1967) – may well have lasted in most cases only for a short span of time. In order to make successful voyages in a small boat under the dynamic and uncertain conditions that obtain at sea, the forager-voyager had to learn how to snack expediently on the marine resources that happen to be on hand at any one time. We also see this in the lithic reduction technologies used by forager-voyagers, which can be described as expedient in the sense that, during the course of a voyage, one had to be able to make do with the pebbles and cobbles that were at hand in a given place. The forager-voyager had a style of life where one could not be too picky. At the same time, the spaces that they were voyaging through were often liminal ones. On the coast and on the sea, they often found themselves in places that were betwixt and between. Much like the Argonauts in Greek mythology, what the forager-voyagers had to do from time to time was to live out at the edges of human experience.

(5) The last heading brings us to the need not to lose sight of the social side of the story. It is quite comprehensible that we as archaeologists become fascinated and even pre-occupied with questions that have to do with the nature of the small boats that were used in early voyaging as well as the methods of navigation employed in

the remote past. On the other hand, the focus of discourse on finding or reconstructing “the boat” may distract us from taking an active interest in the social contexts – within a given society as well as relationships with other societies located at some distance – in which boats were made and voyages were undertaken. To begin with, there is a good chance that, in the Eastern Mediterranean, more than one type of boat was in use during the course of any one millennium between 15,000 and 8,000 years ago. And over the course of the millennia, there were, no doubt, changes in boat technology (see Vigne *et al.* in this issue). In other words, just focusing on “the boat” may not get us too far. Here, in trying to lean more toward the social side of things, we might consider taking a page from the book called *Maritime Technology in the Ancient Economy*, which deals with ship-design and navigation in the Roman world. In retrospect, scholars in that field of study now realize that they previously placed too much emphasis on the technological side of things (how boats were designed and so forth) and not enough on the social and institutional contexts in which seafaring was organized in the Roman world (e.g., Scheidel, 2011). This is not the place to make a digression on the history of either Roman or subsequent Venetian maritime activity. In brief, the take-home message here is that the boats themselves represent just part of the story and perhaps not even the most important one. Of course, over the long run, this will call for some heavy lifting on the part of the archaeologist who studies sites of Epipalaeolithic and Mesolithic age. But then again, we are just beginning to open the first chapter in the long history of coastal foraging and the earliest attempts at voyaging on a regular basis in the Mediterranean Sea.

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APPENDIX A. THE ÖTZI EXPERIENCE

This appendix gives the text of the section with the same name to be found on our web site (see the first section above) where the images that go with the text are presented as well. The Ötzi experience is the term that I used in my opening remarks at the Wenner Gren Workshop for the misguided steps and false starts that may go hand-in-hand with an archaeological investigation when it comes to doing something new. It usually happens in the initial or pioneering phase of research at an archaeological site. The aim here is to lower the tone and bring discourse down to earth, as Steven Shapin (2010) has proposed. In short, we need to develop a more realistic sense of what can happen to all of us – yes, even in the case of the Iceman, one of most important and famous discoveries of our time. At the end of the day, as the history of research on Ötzi shows, it will all somehow work out if we are patient, flexible and open-minded.

Ötzi is the name commonly given to the Iceman discovered by two Germans hiking in the Ötztal Alps on September 19, 1991. For just over 5,000 years, his mummified body had been

preserved under ice and snow at an elevation of 3,210 m. There are three major Ötzi experiences that readily come to mind in the case of the Iceman. They illustrate some of the ways in which the archaeologist can get off on the wrong foot as well as how archaeology, in the long run, can eventually get it right. In other words, archaeology works by steps of approximation.

(A) The first attempt to recover his body involved the use of a pneumatic drill (on September 20; it soon ran out of fuel) and then hacking at it with ski poles (by Reinhold Messner, the famous mount climber, and a friend on September 21). Even by September 23 when the body was finally freed from the ice, the archaeology was not pretty. And notwithstanding all of the initial incompetence, the Iceman's body, his clothes and his equipment have all yielded one remarkable discovery after the next. (B) The Iceman was initially dated to the Bronze Age on the basis of the typology of his ax. His body was subsequently carbon-dated to 3,200 cal BC (a much older time than the Bronze Age). And his ax, in turn, was found to be made of copper and not bronze. The implication here is to say farewell to typology. (C) For a number of years, it was unclear how the Iceman died. There was endless speculation on the topic in the literature, which drew upon a wide range of medical studies carried out on his body, including X-rays and body scans. Then on the basis of a new series of X-rays made in 2001, it now came out that he died from an arrowhead shot into his left shoulder. It cut a major artery, and he died within a matter of minutes (how the arrowhead was not observed in the first series of X-rays is another story). Together these three examples constitute a sobering tale for all of us in the field of archaeology to reflect upon.

As mentioned above, I told this story as part of my introduction at the meeting held at Reggio Calabria on October 19-21, 2012. All of us have our own Ötzi experiences. They should not be denied or swept under the carpet. We need to acknowledge them, to laugh about them, to learn from them and then to move on. On Cyprus, there are three Ötzi experiences that I mentioned briefly in my opening remarks. Two of them come from my own projects. A third one comes from the work of Alan Simmons on Aetokremnos. And at the meeting itself, Jean-Denis Vigne came up to

me later and related his own Ötzi experience at Shillourokambos (for a number of years, he had held the view that the small pigs at the site dating to 8,400-8,000 cal. BC were those of a small-sized domesticated animal; only since 2009 had he come to realize that they were instead the bones of a hunted animal – wild boar – with size reduction, which commonly occurs in the case of island fauna).

Only a few words will be said here about each of the three. (1) At Nissi Beach, we initially thought we were dealing with many dark pieces of fire-cracked rock on the site's surface. In January of 2008, we realized that they were pieces of beach rock, and they now provided further evidence for one or more tsunamis throwing material up from the seabed on the site, including chipped stoned tools from submerged early sites (more on this in Ammerman in this issue). (2) At Aetokremnos, Alan Simmons was able to write a whole book, which came out in 1999, concerned with the question of the extinction of pygmy hippos on the island without mentioning the term Younger Dryas even once. By this time, the results of the deep cores on the Greenland ice sheet were well known. Recall that Alley had even published in 2000 a widely read popular book on the subject. And the Younger Dryas is clearly there to be seen in Figure 1 of the chapter by Bar-Yosef, which came out in 2001 (see Fig. 2 in Davis). In short, how was it possible to publish in 1999 a book on the extinction of pygmy hippos at the end of the Pleistocene and miss completely the major and then well-documented evidence for major climate change at that time? Recall, by the way, that the calibrated ages of the eight charcoal dates from stratum 2 at Aetokremnos cluster in the 11th m cal BC (that is, the time of the Younger Dryas; Ammerman 2010:fig. 2).

(3) The final example comes from our work at dive site C in front of Aspros. In the initial analysis of the lithic material by Carole McCartney (in 2007), only 38 pieces of chipped stone were recognized (eight of the pieces were seen to have evidence for intentional retouch, meaning that they could be called stone tools). In the study of the same set of material carried out by Janusz Kozłowski and Małgorzata Kaczanowska in March of 2012, the total now came to 60 pieces of chipped stone – with 17 retouched stone tools

(that is, twice as many as before). Moreover, the assemblage of chipped stone tools at Dive Site C was now seen to have a close parallel with one of the assemblages at the cave site of Okuzini in southern Anatolia, which is both carbon dated and attributed by Palaeolithic specialists to what is called the Dryas III or the Younger Dryas (that is, ca. 10,800-9,600 cal. BC). At the present time, the dive site C assemblage is, in fact, the only early assemblage on Cyprus (that is, one dating to the time before the PPNA), which shows such a close parallel with an assemblage on the mainland. So dive site C, like the arrowhead in the Iceman's shoulder, now tells a completely different story – one of major significance for understanding early voyaging to and from Cyprus.

SHAPIN S. 2010. *Never pure. Historical studies of science as if it was produced by people with bodies, situated in time, space, culture, and society, and struggling for credibility and authority*. John Hopkins University Press, Baltimore.

APPENDIX B. THE FORM OF THE ARGUMENT

The purpose of this appendix, which also appears on the web site (in a slightly different form) is to consider how one goes about making the argument for early seafaring or voyaging on the basis of island archaeology. The task then is to review the logic and the inferences that are drawn when the archaeologist uses what is found on an island in the Mediterranean Sea to make the case for early voyaging. Emphasis will be placed on the large off shore islands where it is easier to show that voyages of some distance were made over the open sea. What is of interest for our present purposes is voyaging as an activity that was done on a repeated or regular basis (that is, where voyages to and from a given offshore island were undertaken with a frequency that falls somewhere in the range between one or two crossing per year and at least one crossing during the span of a human generation and not as an activity that entailed rare events (including the possibility

of accidental crossings). The latter constitute a different matter than the one that we wish to study. In short, we are interested in learning more about how voyaging began as a way of life. Here attention will focus on the form of the argument and not on the details of individual case studies, which are taken up in various chapters of the proceedings.

The basic argument can be summarized in the following terms: (1) the island that one sees on a map today was an island at the time of interest in the remote past, (2) when the voyagers made their first trip to that island, no one was living there, (3) early archaeological remains that are found on the island show that people reached it by voyaging over the sea (note that the first voyagers may have frequented the island only on an occasional or seasonal basis; they need not have occupied, settled or colonized it; indeed, it is reasonable to think that “frequencing” or “visiting” a given island comes before “settling” it), and (4) there is no other way to explain the occurrence of the archaeological material on the island. More will be said below about each of the steps in the argument.

The first step: the inference that the island was, in fact, an island

One starts the argument by making the inference that the island where the archaeologist works was an island at the time of interest in the past. One gives the reasons for why this is so. If the place was still attached to the mainland, in one way or another, then one is in no position to make the case for voyaging. In addition, if the island separated from the mainland fairly recently (for example, a few thousand years before the time of interest), then the distance over the sea to the island would have been, in all likelihood, a comparatively short one. Accordingly, such a case will not support the claim for early voyaging over some distance. More will be said below about the case of short trips that were made in coastal waters.

The key factor at the first step is the bathymetry around an island. This, in combination with the sea-levels curves currently available for the last 18,000 years (see, for example, the one recently published in the preface of *Submerged Prehistory*;

Benjamin *et al.*, 2011:fig. xiii) gives an initial idea of whether the island was an island or not at a given time. For example, if the bathymetric chart shows that the waters all around the island have a depth of 200 m or more, then it will be reasonable to infer that it has been an island for a long time (sea level went down to a low point of ca. 120-130 m below its modern level some 18,000 to 20,000 years ago). Thus, in the case of Cyprus where the waters on all four sides of the island have depths much greater than this (see Vigne in this issue), one can infer with confidence that it has always been an off shore island for the last 20,000 years. While there may be some minor earth movements that have occurred locally, they will probably not affect the big picture. Thus, islands such as Cyprus and Crete are good places to work for the archaeologist who wishes to make the case for early voyaging over some distance on the open sea.

On the other hand, there are islands in the Aegean Sea where it is much more difficult to say when an island actually formed. As we saw at the meeting (see Efstatiou and Özbek and Erdogu in the next issue), this is the case of islands such as Lemnos and Gokceada. Without going into the details here, the challenge is that of working out the interaction between three different factors – bathymetry, trends in sea-level rise and local earth movements – in order to establish the time when the island began to separate from the mainland. Here it is worth commenting that each of the three factors is known today only at a certain level of approximation. In the case of sea-level rise, there are, for instance, different geo-physical models that one can choose between (see various articles by Lambeck, Peltier and Pirazzoli in the literature). As the study of island archaeology in the Mediterranean world keeps moving further back in time over the course of the 21st century, there will be the need to improve and refine what is known about all three of these factors.

The second step: the inference that no one was on the island in the time before the early voyagers first reached it

On the face of things, this is an inference that, for many years, appeared to be a rather simple and

straightforward one. As mentioned before, the conventional wisdom through the 1990s was that the large off shore Mediterranean islands only began to be frequented in the Neolithic period. Hunter-gatherers were taken to be reluctant voyagers. Accordingly, the archaeologist assumed that no one was living on the off shore islands in the time before first farming. Of course, we have now moved beyond this assumption. There is, however, still a tendency for our thinking to remain in the shadow of this old idea. Indeed, once we begin to formulate the matter in terms of hunter and gatherers and coastal foragers, the whole question of early voyaging becomes more complex and demanding. How far back in time on a given island can we really take the first appearance of pre-Neolithic sites? There is no simple answer to this question. It is something that we have to sort out on the basis of fieldwork on the island. In addition, there is an issue of methodology that arises here. One has to be careful and not confuse the lack of current evidence for something on a given island with evidence for its absence there. Due cautious is called for when it comes to making inferences for absence in archaeology. In most case studies concerned with islands in the Mediterranean, the inference that human beings had not made their appearance on a given island before a specific time can only be made as a working hypothesis.

A comment that needs to be added here is that the recovery and documentation of a site on an off shore island dating back to 20,000 years ago does not necessarily mean that there is evidence for the continuous presence of hunter-gatherers on that island for the following 10,000 years. This too remains an open question until more fieldwork is done on the island. At this point, it is also worth noting that on islands such as Cyprus, Corsica and Sardinia, where a fair amount of reconnaissance work has been done by those with Palaeolithic experience over the last decade or two, not much has come to light that dates the time before 14,000 years ago (see Vigne in this issue). On the other hand, when the same archaeologists have conducted fieldwork in various regions on the mainland and also on islands that were once connected with the mainland down to the end of the Pleistocene (such as Lemnos and Gokceada), they have usually been able to recover scatters of

chipped stone dating to the Upper Palaeolithic and the Middle Palaeolithic within a matter of a few weeks. So there does seem to be something behind the shortage of Palaeolithic sites on the large off shore islands.

Finally, an added measure of caution is called for in the case of an island that formed only in more recent times (for instance, 16,000 to 10,000 years ago, as a consequence of sea-level rise). Here the future island has had a long history of being part of the mainland, and at the time of its separation, it may well have witnessed the persistence of hunter-gatherers whose territory for generations had included this place. Thus, an island of this kind should be treated as a special case – one where it may be difficult to make sound inferences at the second step in the argument. In any event, such “young” islands usually do not offer the opportunity to argue for voyaging over any real distance.

The third step: the inferences based on the archaeological remains that are found on the island

At this step, one is dealing with positive evidence and its interpretation. The archaeologist would like the sites and the archaeological materials that occur on a given island to have several positive features. First, the work in the field has led to the recovery of early sites at several different places on and around the island. Second, the excavations that have been conducted at early sites have produced remains that are found *in situ*. Third, there is a series of radiocarbon dates – ideally AMS dates on organic materials from plants or animals with short lives – available for each of the excavated sites. Of course, this is a wish list. Normally, one is working by steps of approximation on all three fronts. A given island at the present time may have only one or two pre-Neolithic sites where excavations have been conducted. Today the archaeologist is making inferences at the start of a long learning curve. It is too soon to regard the inferences that one is making as definitive ones. At the same time, what is coming to light on other islands and the inferences that are being made by the archaeologists who are working there will influence the claims that the archaeologist can make in his or her own case

study. In short, the endeavor that the archaeologist engages in at this step in the argument has to be seen as an open-ended one.

This is a good place to consider two quite different models of early sea going. They may lead to a better understanding of what is involved in making the argument for early voyaging -- again in the sense of voyages on the open sea that cover a certain distance. On one hand, there is the model of the “young” island that had separated recently (in the geological terms) from the mainland. At the time of separation, there would have been a short distance between the mainland and the new island, and it would have been possible to move in coastal waters between the two in a small boat (even in one of rather simple construction). For the hunter-gatherer or the coastal forager who made such a trip, the new island may well have formed part of the territory that one’s family and ancestors had known and exploited for generations (when it was still attached to the mainland). Crossing over to such an island did not constitute going out to a new place. Instead, what it meant was staying in touch with one’s “home” territory. In subsequent generations, as sea level continued to rise, the distance between the island and the mainland would have kept getting larger. And sooner or later, as the distance increased, one would have become more proficient in moving across coastal waters. However, even after many years, such an approach to going to sea need not have involved long-distance voyaging. The island of Lemnos in the northern Aegean would make a good case study here.

On the other hand, there is a quite different model: that of going out to an off shore island, a remote and unknown place, which required moving over a much greater distance. In order to make successful trips between the mainland and the island, it would have made good sense to build a better boat and to have skills and experience in crossing the open sea. In short, this is the model of voyaging, and a good case study would be island of Cyprus. In effect, these two models represent the two ends of a spectrum; one can think of other models of early sea going that may fall at various points between them. In general terms, it is reasonable to think that the first model was the older of the two.

The fourth step: the inference that there is no other way to explain the occurrence of the archaeological material on the island

The task at this step of the argument is to make a concerted effort to think of other ways to explain the archaeological material on the island and to have good reasons for excluding them. In short, one tries on the shoe of the devil's advocate. Here no attempt will be made here to discuss this step in the argument at length. Much will depend on the details of a given case study. To begin with, one has to consider the possibility that the archaeological material derives from a rare event – one that might even involve chance. For example, there is the possible case of accidental rafting that the archaeologist has to consider. In order to exclude it, one will have to focus on those patterns in the archaeological record, which show that early voyaging was intentional in character. In addition, one may have to return to the second step in the argument and make sure that the island really was an island (that is, one has to develop an even stronger geological case for the island not being connected with the mainland at the time of interest).

There is still another complication that comes into play at the fourth step in the argument. Is the archaeologist in a given case study really in a good position to make inferences that are based on all of the main or important elements in the story? Or are some of the key pieces in the puzzle missing? An example may help to explain the nature of the problem. There is, one can argue, a good chance that the archaeologist will make the inference that the time when voyagers first began to arrive on a given island is too recent. Indeed, the date of the time of first arrival may be off by several thousand years. Here the problem stems from the fact that so little is known about the submerged prehistory of the large offshore islands in the Mediterranean Sea. If one goes back to the time of 12,000 years ago, one is dealing with sea levels that were on the order of 60 to 70 m lower and a shoreline that may have been some 1 to 2 km wider in many places than it is today. Now if we are prepared to go back even further in time to say 15,000 years ago, the respective numbers will become even deeper and wider. In other words, there is a large submerged space – one involving just those

places where early coastal foragers probably kept their boats and made their campsites – that has yet to be explored at all so far. In short, a key element is still missing in the field. Thus, much remains to be done. And there may well be surprises in store for us on the large off shore islands in the Mediterranean Sea.