# Akrotiri-Aetokremnos (Cyprus) 20 years later: An assessment of its significance

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# PART ONE

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## PART TWO

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# AKROTIRI-*AETOKREMNOS* (CYPRUS) 20 YEARS LATER: AN ASSESSMENT OF ITS SIGNIFICANCE

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**Abstract**

Over the years, there have been many claims for pre-Neolithic sites on many of the Mediterranean islands. These generally have not been supported by robust data sets. This changed with the interdisciplinary investigation of Akrotiri *Aetokremnos*, a small collapsed rockshelter on the southern coast of Cyprus. The site is well dated to the Late Epipaleolithic (ca. 12,000 cal. BP) by a strong suite of radiocarbon determinations, it has excellent context with well-defined stratigraphy, and it has an artifact assemblage that, at the time of excavation, was unique in Cyprus. All of these criteria are necessary for demonstrating a defensible human presence.

One of the most controversial aspects of *Aetokremnos* was our claim for a human association with the endemic Cypriot pygmy hippopotamus. This had not previously been demonstrated, and considerable debate ensued related to our contention that humans were at least partially implicated in the extinction of these unique animals. In this contribution, the significance of *Aetokremnos* is put into a broader context and some of the issues related to the controversy surrounding the site are addressed. These relate specially to stratigraphy, chronology, artifacts, cutmarks, and taphonomy. I conclude by affirming the integrity of *Aetokremnos* and our interpretation of the site.

**Key words**: Late Epipaleolithic, Cyprus, archaeological controversies

## INTRODUCTION

I first saw Akrotiri *Aetokremnos* in 1986, and started excavations there in 1988. At the time, I had no idea that the site would end up being so controversial, even some 25 years later. In this contribution I provide a summary of the site viewed with the hindsight of over two decades. This is also an opportunity to incorporate more recent studies from *Aetokremnos*. The site is well published (Simmons, 1988, 1991, 1999, 2004), thus I do not need to provide excessive data or go into great detail on specifics. Rather, I want to examine why the site is still so controversial, and address specific criticisms that have been directed to our interpretation of *Aetokremnos*.

### Research background

In the not too distant past, most archaeologists were unconvinced that humans had made their way to many of the Mediterranean islands prior to the Neolithic. A handful of pre-Neolithic claims from several islands were largely unsubstantiated, especially on the more oceanic islands that were never linked to the mainlands (Broodbank, 2006; Cherry, 1990, 1992, 2004; Simmons, 1999:18-21). Even the Neolithic on many islands was relatively late, with Cyprus having the earliest solid evidence in the form of the aceramic Khirokitia Culture, starting about 7,000 cal. B.C. (Knapp, 2010:205-111, 2013:74-158; Knapp *et al.*, 1994; Le Brun *et al.*, 1987; Steel, 2004:45-63). While the earliest Neolithic on any of the islands, this still was relatively late by mainland standards,

although this is no longer the case, with very early Neolithic sites now firmly confirmed on Cyprus (e.g., Simmons, 2007:229-263).

This standard reading of Mediterranean island prehistory was dramatically challenged by Akrotiri *Aetokremnos*, a small collapsed rockshelter located on the southern coast of Cyprus, which dates to ca. 12,000 cal. BP, or Late Epipaleolithic (Figs 1, 2). *Aetokremnos* was, and remains, extremely controversial, not so much because of its chronology, but because our claim of its association of cultural materials with extinct endemic Pleistocene pygmy hippopotami (*Phanourios minutus*) and dwarf elephants. Despite over 30 paleontological sites on the island that contain these fauna (Reese, 1989; Held, 1992), they have never before been associated with humans, in Cyprus or on other Mediterranean islands where they occur. We argued that humans had a role in the extinction of these unique animals, thereby contributing to the controversial global debate on the role of humans in Pleistocene extinctions (cf. Diamond, 1989; Martin and Klein, 1984).

Since the excavations at *Aetokremnos* several additional sites, both in Cyprus and elsewhere, have been presented as pre-Neolithic (Simmons, 2012). Many of these are addressed in this issue. These include claims of extreme antiquity from Crete (in excess of 130,000 years; Strasser *et al.*, 2010) as well as hints of Neanderthal uses of some islands (e.g., Ferentinos *et al.*, 2012). In Cyprus, a few recently investigated sites appear similar to *Aetokremnos*, as addressed by Ammerman in this issue. However, earlier claims remain unsubstantiated (e.g., Stockton, 1968; Vita-Finzi, 1973). This includes perhaps the strongest, made by someone who was qualified to deal with early sites. James Adovasio *et al.* (1975) believed that they had found Middle Paleolithic sites in western Cyprus, but these were based on typological comparisons of a few artifacts, and were not in a defensible context. Thus, at least for the time being, it appears that there were no Neanderthals exploring Cyprus.

## *AETOKREMNOS* IN AN ENVIRONMENTAL CONTEXT

What environmental data do we have for Cyprus at the time that *Aetokremnos* was occupied? Unfortunately, paleoenvironmental data for Cyprus at ca. 12,000 BP are rare and we must rely on substantial proxy data from the mainland. At roughly the time of occupation, the Younger Dryas (ca. 12,700-11,500 cal. BP) was in force, and this cold and dry episode undoubtedly had an effect on human populations throughout the Near East (Rosen, 2007:45, 174). Of direct significance to *Aetokremnos* are two interrelated aspects of the Younger Dryas. The first is that the severe conditions of this event may well have been a stimulus for Late Epipaleolithic peoples on the mainland to initially explore Cyprus. The second is that these adverse conditions likely played a role in the extinction of Cyprus' endemic fauna. Indeed, we have argued that these conditions, coupled with the presence of a new and efficient predator on the island, humans, conspired to eradicate these fauna.

Prevailing paleoenvironmental conditions for *Aetokremnos* have been addressed in Simmons (1999:11-14), and can be briefly summarized here. *Aetokremnos* presently is about 60 meters above the Mediterranean Sea, and a critical issue in assessing its occupation is that of shoreline reconstruction. Fortunately, there is a considerable amount of literature that addresses this for the Mediterranean (van Andel, 1989; Gomez and Pease, 1992; Shackleton *et al.*, 1984). Most researchers believe that the main post-glacial rise in sea-level occurred between ca. 15,000 or 14,000 and 9,000 year ago. But, successfully reconstructing Holocene coastal paleogeography requires an understanding of the relative roles of eustatic and tectonic controls on sea-level (Gomez and Pease, 1992:2). Incorporating such data for much of the Mediterranean, the coastline is assumed to have attained a semblance of its present configuration in the early Holocene (ca. 9,000 BP). For the eastern Mediterranean, however, including Cyprus, it has been more difficult to isolate the effect that localized tectonic activity had on the coastline's configuration during the Holocene (Gomez and Pease, 1992:2).


**Fig. 1.** Map of Cyprus, showing the location of *Aetokremnos* and other possible Late Epipaleolithic sites. "?" indicates no Late Epipaleolithic radiocarbon ages available yet

**Fig. 2**. Photo of the location on *Aetokremnos*

Poole and Robertson (1991) have delimited the coastal paleogeography of Cyprus for ca. 18,000 BP, but this is too early to be of direct significance for the occupation of *Aetokremnos*. Likewise, Gomez and Pease (1992) provide paleogeographic maps for the Cypriot coastline for ca. 9,000 and 5,000 BP, after the site's occupation. According to Gomez and Pease (1992), mean sea-level in the eastern Mediterranean was about 120 m lower at ca. 18,000 BP than its present. By ca. 9,000 BP, mean sea-level had risen to about -35 m, and by 5,000 BP it was within ca. –1 m of its present elevation (Flemming and Webb, 1986). Because of local variations in the relative rates of uplift and submergence (Flemming, 1978), it is difficult to precisely determine the rate of sea-level rise and delimit the exact position of the Cypriot shoreline at any given time in the Holocene. In Flemming and Webb's (1986) analysis of tectonic and eustatic changes for deriving best-fit eustatic curves for the Mediterranean, the sea-level for Cyprus at ca. 5,000 BP was about –6 m. Gifford (1978) had estimated about –8 m for southeast Cyprus, thus these two figures accord relatively well (Gomez and Pease, 1992:2).

Using a variety of data, Gomez and Pease (1992) also constructed paleogeographic maps for the Cypriot coastline. They suggest that there were pronounced differences in the position and configuration of the paleoshoreline for the two periods studied. At 9,000 BP the shoreline along the southern coast of Cyprus is estimated to have been ca. 1.5 to 2.5 km further seaward than it currently is, and by ca. 5,000 BP, the present configuration of the shoreline had been reached (Gomez and Pease, 1992:4). While these time periods skirt the occupation of *Aetokremnos*, Gomez and Pease (1992:4) do address the site specifically, and the bottom line is that they feel that the shoreline is likely to have been some 1.5 km further seaward from its present position during the site's occupation. This is in general concordance with Ammerman and Noller's (2005:538-539) reconstruction, which identifies the sea level at 12,000 BP as ca. 70 m below the present sea level, and is reproduced in Fig. 3. Mandel's (1999) detailed geoarchaeological study of *Aetokremnos* also confirms this, and he also notes that the general topographic setting (i.e., the site's location on a steep cliff) has not substantially changed since its occupation.

Related to sea-levels is the question of whether or not the Akrotiri Peninsula was an island during the time of *Aetokremnos*' occupation. It is likely that at times in the past, at least the southern portion of the peninsula was separated from the mainland. This separation, though, undoubtedly was minimal. If the sea-level already had completed its major rise by the time of *Aetokremnos*' occupation, the possibility of an island may be unlikely. Stanley--Price (1979:8--9), however, believes that the Akrotiri Peninsula may have been "...no more than a shallow gulf between the mainland and the offshore island of Cape Gata" until relatively recently (i.e., the late Roman period). Thus the matter of whether or not the southern edge of the Akrotiri Peninsula was an island when *Aetokremnos* was occupied remains unresolved.

Leaving the issue of sea-levels, it is often surmised that in antiquity the interior of Cyprus was heavily forested (Meiggs, 1982), although actual paleobotanical data are rare. In a general reconstruction of the early Holocene vegetation of Cyprus, it is assumed that the island experienced the same re-forestation generally agreed to have occurred in the less arid zones of the Near East by ca. 10,000 BP. Meikle (1977:4-8) divides the late Holocene vegetation of Cyprus into eight phytogeographic regions, with a primary oak-pine Mediterranean woodland (Zohary, 1973) characterizing most of the island. More specific data are not available for the Akrotiri Peninsula.

## EXCAVATION RESULTS: A SUMMARY

At *Aetokremnos*, we conducted four seasons of highly focused interdisciplinary excavations (Simmons, 1999). These demonstrated that the collapsed rockshelter had, in fact, substantial *in situ* deposits, nearly a meter in depth. Of particular interest were the two levels that contained substantial cultural and faunal materials, Strata 2 and 4. We recovered a considerable and well-preserved faunal assemblage consisted of nearly 300,000 bones, most of which were the remains of pygmy hippos (98.3%). These represented at least 505 individuals. Other fauna included a minimum of 3 dwarf elephants, over 70 birds, primarily great bustards, and over 70,000 shells

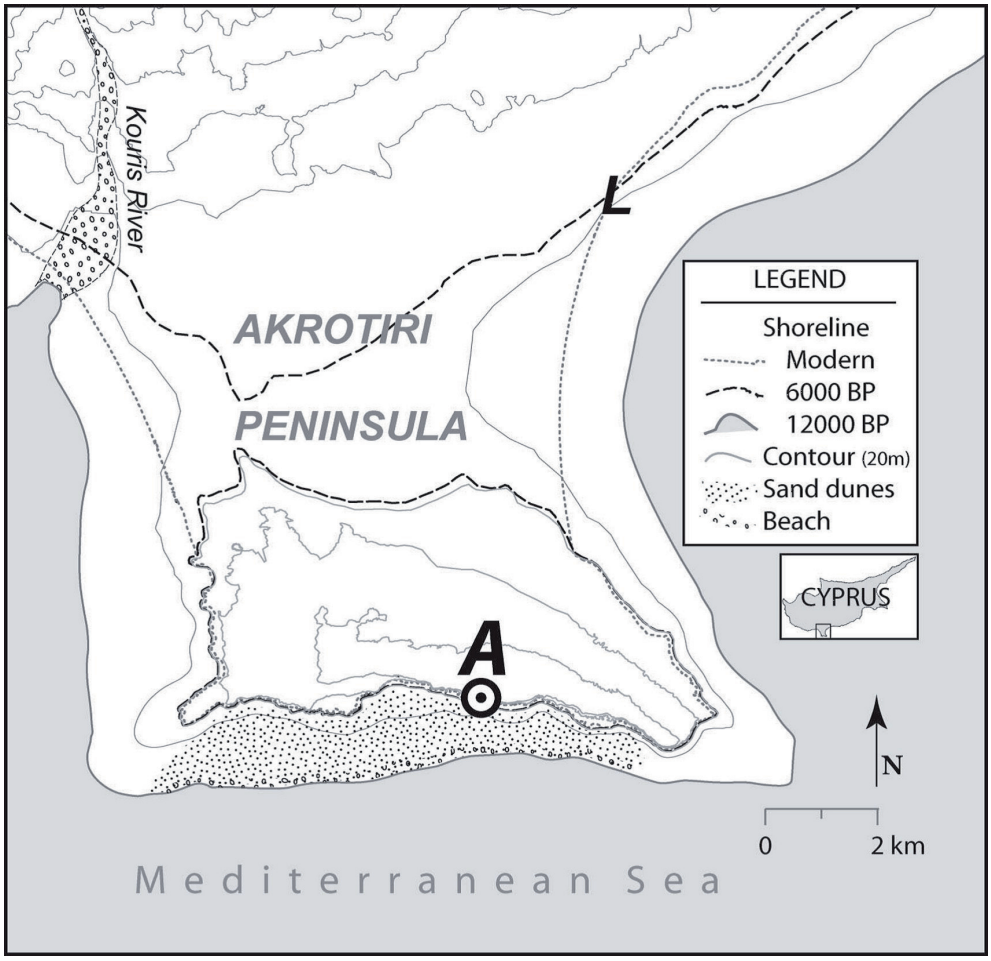


**Fig. 3.** Map of the Akrotiri Peninsula, showing the estimated shoreline the present time, 6,000 BP and 12,000 BP. *Aetokremnos* is located at A and L is the present-day harbor at Limassol. Topographic contour lines are shown at 20 m intervals for the reconstructed12,000 BP landscape without its Holocene deposits and landforms. This figure is from Ammerman and Noller (2005:fig. 1, 539)

representing over 20,000 individuals. In addition, smaller amounts of other fauna were recovered, including, intriguingly 18 pig bones (Vigne *et al.*, 2009). There also was an artifact assemblage exceeding 1,000 pieces of chipped stone. Of importance was that this assemblage did not resemble a typical Cypriot Neolithic assemblage. Rather, it would be perfectly at home in a mainland Late Epipaleolithic context. A few non-chipped stone artifacts also occurred, including shell and picrolite beads. We also documented 11 cultural features, of which 3 are in Stratum 4. Over 30 radiocarbon ages indicated an occupation of the site at around 12,000 calibrated BP (see below).

Table 1 summarizes the distribution of materials by stratum. The majority (61.5%) of the chipped stone occurred in Stratum 2, while the majority of the hippo remains (88.2%) were in Stratum 4. This, of course, is the crux of the controversial aspect of *Aetokremnos*. We argue that Strata 2 and 4 are closely related, both chronologically and culturally, while detractors argue that the bones are not the result of cultural activity, and that their limited presence in Stratum

| Stratum | Hippo[2] | Bird[3] | Shell[4] | Chipped stone |
|---|---|---|---|---|
| Surface | 4,387 (2.0) | 24 (0.1) | 216 (1.0) | 42 (4.1) |
| Mixed[1] | 17,145 (7.9) | 478 (14.9) | 3,870 (18.1) | 181 (17.7) |
| St. 1 | 123 (0.1) | 42 (1.3) | 627 (2.9) | 47 (4.6) |
| St. 2 | 3,966 (1.8) | 2,074 (64.7) | 14,499 (67.8) | 628 (61.5) |
| St. 3 | 503 (0.2) | 40 (1.2) | 104 (0.5) | 3 (>0.1) |
| St. 4 | 192,335 (88.2) | 547 (17.1) | 2,074 (9.7) | 129 (11.8) |

[1] "mixed" is primarily from interface of Strata 2 and 4, where there was no Stratum 3;
[2] Refers to NISP
[3] Refers to NISP
[4] Refers to MNI only

**Table 1.** Stratigraphic association of primary data from *Aetokremnos*. Percents in parentheses

2 (1.8%, but nearly 4,000 bones), as well as the presence of 11.8% of the chipped stone in Stratum 4, is due to stratigraphic disturbance and mixing (see additional discussion below).

In addition to the primary study, we returned to the site in 2009 to complete excavation of the one square meter that remained. We conducted a surface collection and excavated the remaining portion of the site, recovering a very large amount of intact hippo bone, as well as other (mainly bird) bone, shell, a few chipped stone artifacts, and a picrolite pendant blank. The site has now been completely excavated, except for a few remaining pockets in the back of the shelter. The results of this test paralleled the results of the primary excavations.

In 2010, we also conducted a short season at some of the adjacent sand dune sites that had been tested during the major fieldwork period. These sites were believed to be related to the occupation of *Aetokremnos*, but this linkage was made primarily by artifact similarities (Neely and Simmons, 1999). This season re-tested three sites previously examined, to see if erosion and/or deflation have revealed additional artifacts. In addition, we hoped to obtain more datable materials. The results indicated that these small sites likely are related to *Aetokremnos*, based on artifact similarities. They contribute to understanding the settlement system of the earliest Cypriots on the Akrotiri Peninsula. All of these sites have been badly damaged due to natural and cultural forces and the likelihood of their producing much additional information including absolute ages is limited.

## ADDRESSING THE CONTROVERSY

What are some of the enduring and remaining unresolved issues at *Aetokremnos*, and why does it remain so controversial? Even before the publication of the final volume in 1999, there had been some critical articles, and after the publication these continued (e.g., Ammerman and Noller, 2005; Bunimovich and Barkai, 1996; Binford, 2000; Grayson, 2002). We welcomed such discussion in the spirit of healthy archaeological debate. But, given the abundance of radiocarbon determinations and the *in situ* nature of the site, why did it generate so much controversy? The major criticism, of course, is the presence of cultural remains with extinct Pleistocene fauna. What this really boils down to is our claim that Strata 2 and 4 are close in time and both represent cultural activities, while the critics argue to the contrary. That is, while no one now doubts the cultural origin of Stratum 2,

critics contend that Stratum 4 is the result of a natural accumulation of hippo bone, has nothing to do with cultural activity, and by implication is considerably older than Stratum 2. While I fully realize when one makes an assertation that goes against accepted archaeological belief, such as the association of cultural materials with extinct pygmy hippos, it is incumbent on us to prove this. After all, archaeology is by and large a conservative science, and with good reason. However, I also feel that, by using multiple lines of evidence, we have presented an argument that best takes account of all of the data and strongly points to a human and hippo connection at *Aetokremnos*. Certainly disagreement in archaeology is a positive thing, and the fact that after 20 years, this dialogue continues attests to the site's significance. I also would like to note here that the majority of the site's critics, with the exception of Albert Ammerman and Jay Noller, have not actually been to the site and none were at the site when it was under excavation.

In any event, it is not my intention here to discuss each and every criticism of *Aetokremnos*, as this has been done, as summarized in Simmons (1996, 2004) and Simmons and Mandel (2007). It is, however, worthwhile to discuss some of the pertinent issues, as they relate to many topics of current archaeological relevance in terms of how the discipline disseminates and interprets data. Accordingly, there are five specific topics to examine: stratigraphy, chronology, artifacts, cut-marks and taphonomy.

## Stratigraphy

Along with a radiocarbon chronology, stratigraphy is clearly critical in interpreting *Aetokremnos*. We conducted extensive geoarchaeological and stratigraphic investigations at *Aetokremnos* that very clearly addressed site formation processes of the rockshelter, and how its stratigraphy is abundantly clear (Mandel, 1999; Mandel and Simmons, 1997, Simmons and Mandel, 2007:480) (Fig. 4). And yet, many persist in arguing that the site has mixed stratigraphy, which accounts for both the presence of artifacts in Stratum 4 and the presence of hippos in Stratum 2. This is not the case. The stratigraphy is crystal clear, and there is no evidence of mixing. Specifically, Stratum 3, an archaeologically sterile zone, separates Strata 2 and 4[1]. Also, biogenic features indicative of mixing, such as krotovina, do not occur in Strata 2, 3, or 4.

Perhaps one of the most egregious affronts to the site's stratigraphy was presented by Bunimovich and Barkai (1996), who actually re-drew a published stratigraphic section and made it into a pit that did not exist and that cut from Stratum 2 into the Stratum 4 bone bed. This, they argued was evidence of mixing of deposits. While we clearly disputed their re-stratification of the site (Simmons, 1996), I mention this example because it continues to be repeated, most recently in the thorough report on the early Neolithic site of *Shillourokambos*. In a summary discussion, the authors (Guilaine *et al.*, 2011:1220) seem to prefer Bunimovich and Barkai's stratigraphic argument despite its clear inaccuracies. This is a surprising re-writing of the original stratigraphy and simply is not true.

Given the significance of the stratigraphic argument, it is worth addressing the site's stratigraphy here. Stratum 1 is the uppermost stratigraphic unit and includes (1) colluvium that mantles intact and collapsed portions of the rockshelter's roof and (2) colluvium, slopewash, roof fall and attrition sediment above Stratum 2. Stratum 1 does not contain any *in situ* cultural materials and is culturally sterile in terms of any stratigraphic integrity. Hippo remains were found in Stratum 1, but only where bones from Stratum 2 had obviously been displaced upward by the impact of roof fall. Also, the erosion of bone-rich strata within and along the fringes of the collapsed rockshelter has contributed limited faunal remains to Stratum 1. Stratum 1 is of variable thickness, up to 1.2 m in some places, and is subdivided into Strata 1A-E.

Stratum 2 is the uppermost cultural zone and contains the majority of the artifacts, features, and bird remains, although nearly 4,000 hippo bones

[1] This is of considerable importance since in most of the excavated area, Stratum 3 clearly separates Stratum 2 from Stratum 4. I say most, because a few items were in Stratum 3, but these are in minor amounts and their presence can be accounted for by minor excavation admixture. Thus, for all intents and purposes, Stratum 3 is archaeologically sterile.


**Fig. 4**. Photo of the major stratigraphic sequence at *Aetokremnos* (courtesy R. Mandel)

also occur here. It largely consists of attrition and aeolian sediment, though large and small fragments of rock fall are scattered through it. The thickness of Stratum 2 is extremely variable, ranging between about 10 and 50 cm. This unit mantles a culturally sterile zone (Stratum 3) within most of the shelter, but directly overlies Stratum 4 in places at the front of the shelter. The lower boundary of Stratum 2 is usually abrupt. Stratum 2 is subdivided into Stratum 2A and 2B.

Stratum 3 is a sterile zone that separates Stratum 2 from Stratum 2. It consists of loamy sand and sandy loam that represent accumulations of sediment by attrition and aeolian processes. Also, large and small fragments of rock fall are scattered through the fine-grained matrix. This unit is 15–30 cm thick across most of the site, but it is much thinner or absent in some areas, especially near the front and back of the shelter. At a few places, cultural features in Stratum 2 truncate Stratum 3 and intersect Stratum 4.

Stratum 4 consists of an extremely dense accumulation of bones, primarily hippo, resting directly on the shelter's bedrock floor. The bones are in a loose, sandy loam matrix that has been darkened by fine, powdery charcoal. Stratum 4 is 10–50 cm thick and is distributed throughout most of the interior of the collapsed rockshelter. The bulk of the sediment composing this unit accumulated through attrition and aeolian deposition. However, bouldersize and smaller clasts representing roof fall were common. Stratum 4 is subdivided into four sedimentary units: Stratum 4A, 4B, 4A/B, and 4C.

Mandel's (1999) detailed analysis clearly shows the stratigraphic integrity of the site, and demonstrates absolutely no evidence for mixing by humans responsible for Stratum 2 cutting into Stratum 4, which critics contend could have accounted for the association of artifacts with the faunal materials. In addition, there is no geomorphic evidence for a sinkhole, water movement, or other natural displacements that could have resulted in the deposition of the bones. There also is a high amount of phosphorous in both Strata 2 and 4, indicating much organic activity. Finally, the "clean" nature of the shelter's floor, demonstrating no sediment accumulation, suggests that it was a prepared surface.

## Chronology

Along with stratigraphy, a strong radiocarbon chronology is crucial to understanding *Aetokremnos*. This chronology also has been disputed, somewhat surprising given the general lack of numerical ages for most claimed pre-Neolithic sites throughout the Mediterranean islands. We have a total of 36 radiocarbon determinations from *Aetokremnos*, including 5 (Table 2) that have been obtained in recent years, and I stand by our claim that this makes *Aetokremnos* one of the most securely numerically dated sites on any

| Lab Number | Material | Date (uncal. BP) | Date range (cal. B.P), 2 s.d. |
|---|---|---|---|
| AA 79920 | *Sus*, apatite, unburned | 8,588 ± 50 | 9,505-9,598 |
| AA 79921 | *Sus*, apatite, charred | 9,055 ± 52 | 10,194-10,245 |
| AA 79922 | *Sus*, apatite, partly calcined | 9,842 ± 54 | 11,201-11,285 |
| AA 79923 | *Sus*, degraded collagen | 10,045 ± 69 | 11,396-11,764 |
| OxA 15989 | Charcoal | 10,225 ± 50 | 12,055-12,088 |

**Table 2.** Recent additional radiocarbon determinations from *Aetokremnos* (AA dates from Vigne *et al*., 2009)

of the Mediterranean islands. Significantly, these determinations indicate that the time difference between Strata 2 and 4 is so minimal that it cannot be measured in radiocarbon years. While these data have been presented in various ways, we reaffirm our original interpretation of a relatively short occupation of some 300 years centered around 11,775 cal. BP, with a range of 11,652-11,955 ca. BP at one standard deviation, or 11,504-12,096 cal. BP at two standard deviations (Wigand and Simmons, 1999; Simmons, 2004:5). This is in general accord with Manning's (2013:501-503) masterful compilation of all early Cypriot radiocarbon determinations, in which he places *Aetokremnos* within an approximate 12,950-10,950 cal. BP range while also preferring a somewhat longer occupation than we presented.

Of the 36 determinations, 9 are on hippo bone, 4 are on pig bone, 10 are on charcoal, 3 are on sediment, and 10 are on shell. The original determinations and specific associated issues have been thoroughly addressed (Simmons and Wigand 1994; Wigand and Simmons 1999), and it is somewhat surprising that these detailed discussions have apparently not been read in detail by some of the site's critics. A primary source of dispute are the 9 original radiocarbon ages on hippo bone. Clearly, dating bone presents several challenges. Essentially, the critics point to the wide range of bone ages, some of which were quite recent. In our detailed discussion on the site's numerical chronology, we discard the 3 hippo ages determined on exposed surface bone as being contaminated. This still left 6 bone ages (1 of which we also rejected as an outlier; Wigand and Simmons, 1999:204), and these generally were somewhat younger than the charcoal dates. I should note that this is not an uncommon situation with bone ages.

In any event, the critics' argument is that the hippo bone dates are unreliable and that Stratum 4 is considerably older than Stratum 2. To address these charges, with the generous assistance of Sturt Manning, we recently attempted to date additional bone samples (hippo-11, elephant-2, and bird-9) and charcoal (1) using the more precise refinements in the radiocarbon process that are currently available. Unfortunately, the bone samples did not yield any radiocarbon ages, although the charcoal sample (from Stratum 2) was precisely where it should be 12,135 cal. BP (OxA-15989). Other recent samples on pig (*Sus*) yielded 4 ages, two of which were rejected, and with the remaining two exhibiting a range of 11,400-11,700 cal. BP, at two standard deviations (Vigne *et al.*, 2009).

I also should note that this is an on-going process, and that additional bone samples are now being subjected to high precise radiocarbon dating, but we have no results yet. Unless one can show that the hippo bones are considerably older than the cultural materials, which has not been done, there seems little reason to question the synchronicity of both.

Much has been made of the fact that the radiocarbon samples had been processed prior to recent methodological refinements, as summarized by Manning in this issue. This certainly is true, as it was with any site excavated in the 1980s and 1990s. However, it is important to realize that the more precise ages that refinements in AMS dating can provide does not mean that earlier ages are inaccurate. If this were the case, all of the radiocarbon dated sites excavated prior to, say, 2000, would have their chronologies in question.

The bottom line on the bone ages is simple. Discard all of them. By doing so, we still have a strong geochemically defensible chronology for *Aetokremnos*, with 23 ages, which, I think, most would agree is a strong numerical chronology for any site. Seventeen of these ages are from Stratum 2, two are from a mixed Strata 2/4 context, and four are from Stratum 4. When examining these data, it is clear that both Strata 2 and 4 and not separated by much time. This should be the end of the discussion.

## Artifacts

When excavated, the artifacts from *Aetokremnos* were unique to Cyprus, and generally fit within an assemblage that on the mainland could easily be Late Epipaleolithic or Natufian (Simmons, 1999:123-146). Since the excavation, more recent excavations have recovered assemblages similar to those from *Aetokremnos* that may represent other Late Epipaleolithic occupations (Ammerman *et al.*, 2006:11-17; Efstratiou *et al.*, 2012). Initially, some skeptics felt that the chipped

stone from *Aetokremnos* was not cultural, or were perhaps similar to modern "Dhukani" threshing flints. We have demonstrated that this is not the case. It is important to note that until recently, Cyprus has not had a strong history of much attention being paid to chipped stone; fortunately this is no longer the case (e.g., McCartney, 2005 and others). What is clear is that the chipped stone from *Aetokremnos* is unlike any assemblages from the later Cypriot Neolithic. It is largely microlithic and blade/bladelet oriented, and would fit comfortably within any number of Late Epipaleolithic assemblages from the mainland.

There also were a few non-chipped stone artifacts, and recently, Guilaine, Briois, and Vigne (2011:1221) noted that some of these resembled Neolithic artifacts, although they were not disputing the Late Epipaleolithic nature of the overall assemblage. And, indeed, some of the ground items and ornaments, especially the picrolite, would be at home in Neolithic assemblages. But, these are not "type fossil" sorts of artifacts, and can easily occur in both Neolithic and Late Epipaleolithic contexts.

## Cut marks

When Lewis Binford (2000) reviewed our 1999 book it was apparent that he really did not read the entire volume. Rather, he primarily concentrated on the apparent absence of cutmarks on the hippo bones, thereby dismissing the cultural relationship (see Olsen, 1999). Cutmarks, or lack thereof, is an immensely complex topic, and we addressed Olsen's contribution point by point (see footnotes to Olsen, 1999). We reported the data, as they existed. Olsen looked at a large sample, and I am confident of her analysis, if not her conclusions. Suffice it to say that a huge literature exists on cutmarks, and even in clearly cultural faunal assemblages, cutmarks are rare in most cases.

The presence or absence of cutmarks is dependent on several variables, including the type of animal, how it was butchered and processed, and how it was consumed. We argued that an animal with as much fat as a hippo might not necessarily have been cut to the bone in preparation. Furthermore, if the animal was roasted whole, luau style, there might have been no need for cutmarks.

Related to the cutmark issue were criticisms that the chipped stone tools, especially the diagnostic thumbnail scrapers, were too small to have been used to butcher an animal such as a pygmy hippo. This clearly is a spurious argument, as ethnographic analogies have shown that even full sized modern elephants can be processed with nothing more than flakes (Frison, 1979:260, 1989:768-779; Haynes, 1988:185).

## Taphonomy

One of the deficiencies of the 1999 book was the lack of a detailed study on the taphonomy of the faunal remains. There are many reasons for this, but we certainly are aware than a detailed taphonomic study would have contributed to the volume. Indeed, if such a study were available, it would be invaluable to compare *Aetokremnos* to, say, the recently investigated Ayia Napa pygmy hippo assemblage (Theodorou *et al.*, 2004), which contains a large number of animals in a natural context similar to *Aetokremnos* (although details have not been fully published).

What is somewhat curious about this criticism, however, is the often cited reference to Donald Grayson's (2002) review of the book. This is always cited as a negative review in relation to our conclusions. I have always found this somewhat curious, given that Grayson's review was very fair, and that while he expressed some doubt about the cultural association with the hippos, he certainly did not rule it out. Rather, he called for a more detailed taphonomic study. When I asked Grayson about his current thoughts on the assemblage, and the negative tone of his review, he replied as follows:

"Cites me as an opponent? …as you know, that is absolutely not what I meant. I am neither an extreme doubter nor an extreme accepter. I think Akrotiri *Aetokremnos* is an extremely important site in need of a taphonomic assessment, … You've got great dates and a pretty straightforward stratigraphic setting, which has been very well-described….There's no question as to the age of Akrotiri *Aetokremnos*; there's just a question as to the meaning of the pygmy hippos" (Grayson, 2012 personal communication). In relation to this issue, a student is presently doing a Ph.D.

dissertation on the site's pygmy hippo remains, including the taphonomy of that assemblage.

Pat Shipman's (1979) famous taphonomic quote "what are all these bones doing here?" clearly is relevant to *Aetokremnos*. And, while detailed taphonomic analysis is not yet complete, we did address several non-cultural scenarios to account of the dense accumulation (e.g., Simmons, 2004:8). This included the possibility of a sinkhole, an accretional paleontological deposit of animals accidentally falling off the cliffs above the site, or of sick animals going into the shelter to die. All three arguments are unlikely.

There is absolutely no geological evidence of a sinkhole or any geomorphic activity indicating that the bones were washed into the confines of the *Aetokremnos* shelter. For an accretional explanation involving animals falling off the cliffs, the lack of spreading vertically or horizontally on the cliffs argues against this. Furthermore, if they fell over the cliff, how did they get inside of the shelter? And, even if this occurred, why did they only accumulate in the *Aetokremnos* shelter? As to the argument for a natural accumulation by sick animals, again, why would *Aetokremnos* be the only shelter used by ill hippos who had gone somewhere to die? It scertainly is too small to hold over 500 hippos at once, and if it were a natural accumulation through time, why are virtually none of the over 200,000 bones articulated? Also arguing against the old, ill, and infirmed scenario is the fact that 27% of the analyzed hippos are under one year old (Reese and Roler, 1999:156-158). Finally, there are other fauna at the site, including many birds and shell, and these increase over time, suggesting that as the hippos were becoming scarcer, presumably due to overhunting, alternate economic resources were being used.

A final issue that is taphonomic-related is burning. About 29% of the bone at *Aetokremnos* is burned, much of it severely. It is doubtful that this degree and amount of burning could have a non-cultural origin. While some might argue that underlying bone could have been burned by overlying hearths, this is unlikely since even in such a scenario, the bone would not have been severely charred. In an experimental study, Stiner and colleagues point out that "...although bones were buried as deep as 15 cm below the coal bed, only those specimens in the first 5 cm were affected much by heat from the fire. Moreover, these shallowly buried bones were burned only to the point of carbonization..." (Stiner *et al.*, 1995:230). At *Aetokremnos*, Stratum 4 is up to 50 cm. thick, and burned bone occurs throughout the stratum, not only on the top of it. Furthermore, Stratum 2 frequently is separated from Stratum 4 by several cm of Stratum 3, which is not burned. The separation between the bottom of fire hearths in Stratum 2 and bone in Stratum 4 almost always exceeds 15 cm, and yet much of this bone is thoroughly burned. It is therefore exceedingly unlikely that the burning is the result of overlaying hearths. Finally, much of the burned bone is not even located beneath the hearths.

## DISCUSSION

After over two decades, I stand by our original interpretations of *Aetokremnos*, with perhaps some "fine-tuning." I am delighted that *Aetokremnos* continues to generate so much discussion, and welcome constructive criticism. It is healthy to have contrary opinions to one's conclusions; however, they should not be fact free or incorrect citations of the published record. Above, I have cited some examples of how some criticisms have been distorted towards a particular point of view and have ignored published data on the site. I am thus concerned with the lack of attention to detail, attempts to re-write what has been published, and general distortion of data.

And, this is a continuing trend, sometimes perhaps made innocently, as when the usually meticulous Bernard Knapp (2013:59) stated in a masterful summary that "…several archaeologists remain skeptical about the association between humans and the endemic fauna at *Aetokremnos*." He cites an article by Sondaar and van der Geer as evidence of this when, in fact, they strongly supported our conclusions. To wit: "…the taphonomy of Cypriotic Akrotiri is much more easily explained by an overkill of the hippos…the human hunters might well have been responsible for the extinction of the slow moving dwarf hippo" (Sondaar and van der Geer, 2000:71).

In detailed summaries, Knapp (2010:85-94, 2013:52-59) provides considerable discussion on *Aetokremnos*, and he is not alone in noting

that the "perfect storm" of circumstances would certainly implicate humans in having some role in the extinction of Cyprus' endemic Pleistocene fauna. This should come as no surprise, given that we know that in just about every case documented of humans invading unoccupied islands, faunal extinction is a near certainty (Simmons, 2013). This is especially well documented in proto-historic and historic times. Many endemic species, especially "large" ones like the pygmy hippos (or "mini-mega-fauna) were likely naïve, having no natural predators. Humans would have taken advantage of this. Such animals would have been easy to hunt and highly susceptible to the presence of people newly arrived in their island niche. Knapp observes (2010:93) that "Although controversy over both the proximate and ultimate causes of the demise of the Mediterranean's mini-megafauna will continue, the worldwide record of faunal extinctions at least makes it plausible that humans were involved. And in the case of *Aetokremnos*, Cherry (1990:195) maintained that '… man has here been discovered, as it were, holding a smoking gun."

Now, of course, determining what the human role was is not a simple task. As Knapp, Cherry, and others note, in convicting humans of overkill, it is still necessary to consider whether extinction was: 1. the direct result of overkill by humans; 2. the introduction with humans of new species competing with endemic fauna for ecological niches, nourishment, or 3. the indirect consequence of wider modification of the landscape by both climate changes and human (agricultural, pastoral) interference.

I fully agree with this notion. In the case of *Aetokremnos*, we can eliminate the possibility of wide-scale landscape modification by agriculturalists or pastoralists, since the site predates these events. But, hunters and gatherers also affect landscapes. In addition, the recent discussion by Vigne *et al.* (2009) regarding wild pigs possibly being introduced by people slightly earlier than those at *Aetokremnos* lends some credence to the niche competition scenario, although I caution against making too strong a case here based on the presence of 18 pig bones from *Aetokremnos*. In any case, I suspect that in the case of *Aetokremnos*, we are looking at aspects of all three of these cultural scenarios, coupled with the wider climatic consequences of the Younger Dryas.

Following Knapp's (2010:93, 2013:58-59) discussion, animals like pygmy hippos, which are about the size of a large pig, can take a heavy toll on their habitats. They have limited population densities and are highly susceptible to any kind of environmental change, especially in an island context. The colder and dryer weather of the Younger Dryas may have reduced or eliminated some of the dietary sources in the Akrotiri peninsula upon which the hippos depended on. Such developments would have made these pygmy hippos more vulnerable to extinction by the introduction of a new predator: humans. And even without climatic stress, a healthy population of pygmy hippos likely could have been decimated by small groups of efficient hunters.

Thus, I concur with Knapp's (2010:94) statement that "People arrived, and, as the radiocarbon dates indicate, within 1,000 or so years two endemic mini-megafauna became extinct. In other words, even taking into account such factors as climatic and environmental change, the pygmy hippo evidence from elsewhere in Cyprus, or the possibility of competition with introduced species, the people who utilised the site of *Aetokremnos* may very well have played a role in the overkill of the mini-megafauna. The presence of people, some of whom may have been highly specialized hunters, on islands with animals that previously had lacked predators, almost certainly had disastrous results."

## CONCLUSIONS

So, in retrospect, what is the significance of *Aetokremnos*? There is no question that *Aetokremnos* is an extremely important site. It is one of the first of many claimed pre-Neolithic sites on a Mediterranean island, especially an oceanic one, that is supported by empirical evidence and a solid cadre of defensible radiocarbon determinations. There is no doubt of its antiquity. The site remains controversial, however, due to our insistence on the association of cultural materials with extinct Pleistocene fauna. That this controversy has been going on for over 20 years is a testament alone to the site's significance.

Especially in light of the controversy, would I have done anything different in our excavations? With the benefit of hindsight, this is always an interesting question. Certainly analytical techniques have improved over the years that might have assisted interpretation in some ways. But, in terms of the actual archaeology, there is really very little that I would have done differently, given the context of the site and funding[2] considerations. John Cherry, Nicholas Stanley-Price, and others were right many years ago, when they said that to demonstrate an early human presence on the Mediterranean islands (and anywhere, in fact) requires: good numerical ages, context, and artifacts (cf. Cherry, 1992:36). This remains true today, and *Aetokremnos* fulfills all three criteria. This cannot be said for most other pre-Neolithic sites in both Cyprus and other Mediterranean islands. Given the ephemeral nature of most such sites, they will always be difficult to precisely document. Archaeology has, however, made tremendous strides towards unraveling such sites in recent years, as indicated in this issue, and I am confident that with additional refinements, more and more such sites will be better documented. At the same time, however, we cannot uncritically accept sites as pre-Neolithic without empirical justification.

So, what would I have done differently? Specifically, we might have done more point proveniencing of bones and artifacts and attempted to get more funding for a detailed taphonomic analysis of the bones. Publication wise, I have few regrets. One thing that I would have done in the final volume, however, is put *Aetokremnos* into a broader context. Specifically, I would have addressed more fully its context in the Younger Dryas, discussed more thoroughly the site's functional implications, put the site into a broader Mediterranean perspective, examined the sea-faring implications for these pre-Neolithic peoples, addressed in more detail how *Aetokremnos* differs from other, non-cultural pygmy hippo sites, and stressed even more that one site alone, no matter how rich, cannot definitively prove or disprove a human role in the extinction process. These are all points made by others who have invested considerable amounts of time into trying to place *Aetokremnos* within a broader context.

Nonetheless, I stand by our original interpretations of this enigmatic site. Will we ever know for sure if this is true? That is unlikely. Some critics of the site have said that all we have is circumstantial evidence to support our conclusions. Well, yes, that is correct. But, archaeology is circumstantial by its very nature. I have always argued that if *Aetokremnos* were a mainland site and that if the fauna were, say, sheep rather than hippos, no one would question the cultural context. But we dealt with the data that we had. Contemporary archaeology is dependent upon using multiple lines of evidence, and by doing so, the most parsimonious explanation for *Aetokremnos* is a cultural one. By examining multiple lines of evidence from a truly interdisciplinary perspective, we believe that the *Aetokremnos* rockshelter was used for protection and storage, and functioned as a processing site and bone cache; much of the bone may have been used for fuel. We have modeled that a small group of humans could have, within a relatively short period, eradicated remnant hippo populations who may have already been suffering ecological stress due to climatic change.

It is curious that since our excavations, there have been claims made for contemporary or older sites based on far less peer reviewed evidence, and yet the archaeological validity of these seems not be questioned to the degree that *Aetokremnos* was. Perhaps this is simply because the scenario of humans hunting an animal as odd as a pygmy hippo is out of the comfort range of most of our ideas about human predation. Regardless, I would go farther than John Cherry did when he noted that there is a smoking gun to implicate humans and hippos at *Aetokremnos*. Rather, we have not only the smoking gun but also the bullet.

[2] Funding, which of course affects everything on a project, always was a challenge at *Aetokremnos*. Initially, I could not get support since funding agencies were not convinced that *Aetokremnos* was even a site. Ironically, when we demonstrated this, I still had difficulty because some agencies said, essentially, "Well, you've now shown it to be a site so we don't need to fund you." I do not say this to be defensive, but simply to put things in proper context.

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