



BOOK REVIEW

The Archaeological diet research in prehistoric Greece (in Greek)

by Dr. S.M. Valamoti, University Studio Press, Thessaloniki (2009)

(ISBN 978-960-12-1749-9) (p. 220). Price: 38 Euros

Reviewer: Dr. A. Sarpaki

The author is introducing us into the world of archaeobotany, without it being a handbook for the subject (p.23), using the smooth language of a fairy tale, without jargon and yet referring to every stage as if all is done in easy stride, an approach which will prove very appealing, mainly, to the student, the dilettanti but, also to the scholar.

In her introduction (chapter 1) she refers to the outline of the book and raises some very important points which will be used further in her following chapters. Although she values ethnographic work, she spells out some shortcomings, especially as the choice and particular use and processing of plants is considered a cultural choice. She cautions about the presence and/or absence of some plants and how this should be interpreted due to differential preservation connected to methods of processing, storage habits, and collection of samples on excavations as well as problems of taphonomy. The research on "cuisine" is also indicated as needing some particular caution not only in its detection but in interpretation as well. Special "meals" which celebrate aspects of social, economic,

political and metaphysical aspects of social relations, also, need an integrated approach.

Chapter 2 is entitled collection of data and presents sampling and all the methods used for the recovery of archaeobotanical data. It is important that she stressed the need to use different recovery methods with material that comes from different environments, so that waterlogged material can be retrieved as accurately in the few instances when it is needed in Greece. Her "action" pictures are a must for students who, already, believe that archaeology can be conducted from an armchair! Her rapid overview covers down to the work in the laboratory.

The beginning of agriculture in Greece refers to her chapter 3. She begins with a very concise and clear enumeration of the stages from foraging to agriculture which could be very useful to the student and the scholar who is not a specialist. These are difficult subjects and new for mainstream Greek archaeology, such as referring to the problem of a Neolithic stage, not necessarily connected to the domestication of plants, which is, definitely, a new con-

cept in Greek academia. Even, the possibility of the co-existence of societies which depend on domestication and others which depend on foraging (p42), although tackled in international bibliography, had not officially been stated before. She puts forward challenging ideas and questions such as that Mesolithic Franchthi might have been a site which did not choose to adopt domestication, although they might have been aware of it (p.45). Valamoti raises another question which is also important to consider and that is the dichotomy between <Wild or domesticated plants> as it is not the right terminology used for the beginning of the Neolithic. There is a "grey" area where plants are neither wild nor have signs of domestication. She tantalizes with the idea of different types of plants and variety of species in prehistory, but whose spectrum is still needed to be enlarged.

Chapter 4 deals with Prehistoric Bread and, of course, she refers to the varieties of wheat and cereals in general but also, as is natural, discusses and compares findings between the north and south of Greece. Although her main work is on Prehistoric finds, she sometimes chooses to erratically include finds from other periods (cf. Bawman H., 1993, Micha-Lambaki A., 1984), that is Classical and even Byzantine, so one wonders, if she wants to be consistent, why does she leave out all of the scholarship of people discussing cereals and food in the historic periods such as, *inter alia*, Wilkins, J.. 1995, Foxhall & Forbes (1982), Garnsey (1999), Jardé (1925), Jasny (1950), Moritz (1958), and other neighbouring cultures (*inter alia* Ertuž 2004). Students need to know that there are no chronological lines in the study of human culture and that one culture

blends to some degree with the other and about continuity and discontinuity within and between cultures and that 'convenience tags' such as Prehistoric, Protohistoric and Classical, are artificial «artifacts». If the department of archaeology is so strict about evaluating written papers in their chrono-stratigraphic niche, then they have missed the complexity in issues of continuity and discontinuity of cultures.

Other points, rightly, raised are ethnographic parallels, of paramount importance in our study, and the value of experiment in order to understand certain processes and evaluate their by-products. Together with Mike Charles (Valamoti & Charles 2005) they had experimented with feeding *T. monococcum* to sheep/goat in order to detect the breakage pattern and that is very laudable. Yet there is no mention about this kind of work in other cultures (*inter alia* Miller 1984; Charles & Bogaard 2005). This tradition in Greek academia of keeping archaeology segregated from all that is happening in other neighbouring cultures, as in a capsule, does us no good and dries up our field and our capabilities. This openness to the "other" I only, timidly, detected in the photographs of crop processing, whose bibliography, fortunately, was also included. Here, though, I would act like an advocate of the devil and say why not include/ compare photographs of our own ethnographic processing (Photographic Museums) and tools which are hidden in various Ethnographic/Folklore Museums scattered around Greece?

The next chapter is chapter 5 and deals with the pulses and "fava". The reference to the importance of legumes in the Prehistoric meals is made and

discussion on the absence of the chickpea (*Cicer arietinum*) and the horsebean (*Vicia faba*) in the Neolithic is discussed. The absence of these crops leads the way to her reinstatement of her disagreement, rightly so, of the Neolithic "package" as sometimes mentioned for the coming of crops to Greece. In the discussion about the importance of legumes in both the Neolithic and the Bronze Age, she does not take a position, as she mentions that they are absent from the Linear B tablets and the Palace sites. The lack of mention of the legumes in Linear B tablets is explained as "the day-to-day agriculture of the Late Bronze Age was, rather, of no concern to the bureaucracy of the Mycenaean centres" (p.76) and that "they (legumes) were not found in Palatial centres". This is a lack of integration of archaeological discourse and archaeobotanical finds. The argument about the lack of interest in the day-to-day agricultural pursuits is refuted by the content of the archives themselves, which refer to wheat, barley, figs, flour etc. Therefore, the absence of legumes needs to be archaeologically/archaeobotanically explained. The second stated reason, which is the lack of legumes in the 'Palaces', this too, is calling to be clarified. As Valamoti knows, the palatial centres were excavated when the retrieval of bio-archaeological remains was reserved only to what was seen by eye, so our knowledge is fragmented, but, leaving this aside, if she browses through the pages of old excavations such as Knossos, Malia and so forth, mentions of legumes are noted. Valamoti ends the chapter by commenting on legume crop processing and toxicity but, unfortunately, mistakes on p.80 *Lathyrus clymenum*, whose common

name is Spanish vetchling, to *L.ochrus* which is the Cyprus vetch. The beginning of research in common with a student of hers (Moniaki A.) into the changes of pulses whilst soaking and/or boiling is something we do look forward to.

The sixth chapter is devoted to olive oil and it begins by initiating us into all the oil producing plants of Prehistory and we cannot but marvel at this variety. On p. 85 she disputes the findings of olive pollen (Bottema & Sarpaki 2003) dated to c.6000 BP. on the basis that the olive pollen travels far, so she presumes –I suppose-contamination could have come from the Near East and yet the high number of olive pollen argues against long travel. In her discussion of the olive absence in the Neolithic of Greece, she ignores altogether the chemical analysis (Tzedakis & Martlew 1999) done on 2 bowls from Gerani (M.N. date) (p.82, fig.45 & 46) which had included olives/olive oil and confirming the pollen date. On p.86 the interpretation of the fragments found at Chamalevri, Tzambakas Building (MMIA-B) as being the by-products of olive extraction are being disputed and it is suggested that they might have been remnants of coprolites. Of course, one can hypothesize as much as our imagination can permit but we need to be constrained by archaeological contexts, as after all we are trying to answer archaeological questions. How could one assume that people or animals (pigs are referred on p. 88) were defecating on the floors of a Middle Minoan building (inside) which had no sign of being a stable? To be fair I need to say that this publication was summarily provided but her objections needed to have an alternative phrasing. Experiments

done on olive fragments in order to detect patterns of breakage before or after charring (Adam-Veleni & Mangafa 1996) are mentioned (p.86, 88) whereby the results were conclusive about the *trapetum* and the author believes that either the *trapetum* had not broken the olive stones, or else the olives were waiting to be pressed. One needs to compare also Neef (1990, p. 298) which is referenced in the book where he claims that olive waste, “jift” are olive fragments with “rounded fractures”, just comparable to the Chamalevri material. Her argument is enriched by stating that it is not necessary to break the olive stones in order to extract olive oil and refers to the *trapetum*, and assumingly to the Latin authors. This is an anachronism which should have been avoided, by all means, in a book directed to students. What is the relation of a *trapetum* with Prehistoric olive oil extraction? Experiments with the *trapetum* have shown that it is a myth that olives do not break and it is logical that early extraction methods must have broken the olives in order to extract oil from even the endocarp. Moreover, we only need to look at the possible tools which could have been used in order to see that the method of olive oil extraction had no connection in its methods between the Prehistoric and the Roman periods. We should grant her that there is the need to do some experiments with animals in order to see the alterations of the olive fragments within their gut system, but the particular archaeological contexts do not allude to the existence of coprolites and the breaks are definitely archaeological, in other words have no sign of freshness and, moreover, are worn on the edges.

She uses the difference of quantities of olive stones found in the Prehistoric period (few in general) to the large quantities found in the historic periods as evidence of relative use in the respective periods. What she forgets is that the consumption habits might be responsible for these differences and that olives might have been consumed whole more in later periods when preparation recipes could have been more varied and would have made them more appealing to eat as olives. How can one say something as outrageous as that there is no evidence of the food consumption of olive oil in the Minoan period and referring to Boulotis (Μπουλώτης 1996) and Hamilakis (1996). The sources need to be read more attentively. After all we have not yet deciphered Linear A and even if we had, food preparations might not have been mentioned in bureaucratic texts but just the raw material. The need and use of this raw material is a subject which needs much further research.

She also disputes the use of olive fragments, as fuel (p.89) without any good argument, and refers to the alleged find of wax in some lamps from Crete (p.89). These are, anyway, two different kinds of fuel (see Sarpaki & Bending 2004 where olive fragments and stones were found in a kiln at Mochlos). This use of wax is rather problematic as, anybody who is an archaeologist, knows that wax was a very valuable item in Mycenaean times and would most probably, have been the same in Minoan times. Chemical analysis, as we all know, is not always a panacea and is fraught with dangers of interpretation.

Her conclusions are, rather, written in haste, more intuitive than poised.

More discussion needs to be generated about a great deal of aspects such as agricultural tendencies, personal taste, culinary recipes, technological know-how and changes and even problems of the archaeobotanical visibility of particular crops.

Chapter 7 is devoted to Prehistoric wine. She travels us smoothly into the embrace of wine and wine making, the areas where it is found in its wild state and the sites which produced evidence of the drink. The photo of the grape pips with their skins still visible from the site of Dikili Tash is exceptionally good.

She returns again to the fact that she faces with skepticism any theory based on the premise of "one centre" of diffusion (p.97), rightly so, when there is evidence of a plant's presence in more than one area.

A very important point is raised and that is (p.98) that cultivation and domestication could have happened independently of each other and in areas covering the zone where wild vine exists in nature, and she mentions the north of Greece as possibly being one of the areas of domestication. However, we should see her Table 9 where the finds of *Vitis spp.* are mentioned and note that most (5 out of the 6 sites) of the E. Neolithic & Aceramic sites are located in Middle Greece (Thessaly/Boiotia) and one as far south as Crete. The same occurs in the M. Neolithic sites where (3 out of 6 sites) occur in Mid-to-South Greece, even as far south as Knossos and Franchthi. Therefore, the north-south divide should be accepted with more caution. After all why should it be so important for the grape to have been domesticated in the north (p.100) rather than the middle or

south of Greece. Interpretations should try to be objective devoid of feelings of "localism"!

Her conclusive paragraph (p.100) is trying to "iron" all reservations, wipen up problems, which had been expressed in her chapter on wine and come forward with conclusions. Yet, "smooth" conclusions are not what is needed but more the stressing of major 'lacuna' in research for future directions. Her conclusion that from the early Neolithic, the inhabitants of Greece were using the juice of the vine, culminating in the preparation of wine in the 5th millennium in the North of Greece (p.100), whereas it was extensively used in the Bronze Age, still needs to be proven. When we check Fig.9, p.208, her allusion to *Vitis* pips falling into the category of morphologically domesticated pips (key of Fig.9), we are faced with a qualitative description and cannot be as yet quantitatively proven. What can one say about the Toumba Balomenou (E.N.-M.N.) and the Knossos pips (E.N.II) which fall into the same description and that is morphologically domesticated? It is catch-22!! What is a morphologically domesticated pip versus a cultivated one? As she had decided from her previous arguments, the problem is still not solved. Rightly so she states that archaeobotanical research in the south of Greece is not as widespread as in the north for various reasons, and refers to the myth of Dionysos which places the home of wine to belong to Thrace. The use of myth, although could enclose inklings of truth, when she really leaves out most of plant material and all scholarly work from historical periods, is rather better left untouched. There is really an intertwining of ideas which provides a nebulous

feeling and does not state in detail the problem. In textbooks, such as the present book, where students will be the primary readers, questions need to be stressed clearly and succinctly, and made clear from hypotheses, which could be mistaken for facts, in the hands of a young researcher.

Chapter 8 is concerned with fruits and other crops. This is a straightforward chapter where the fig, the blackberry, the raspberry, strawberries, *cornus mas*, the pear, *Sambucus nigra*, almond, *Quercus*, *Pistacia terebinthus* are mentioned. Very interesting ways of preparation of the wild pear and acorns are discussed. However, the presence of almond in Greece (p.106) is underestimated, even if one compares with the tables (11 & 12). This chapter ends abruptly with the mention of *Pistacia terebinthus* with no discussion about the taphonomical difficulties of finding charred fruit, many of which might have been eaten fresh and never stored for long periods, and although problems of preservation of archaeobotanical remains are referred to in the introduction (p.16) they do need to be discussed in relation to the crops at hand. The absence of some fruit, like the pomegranate and the carob, amongst others, is not discussed. The impossibility of locating cess-pits in the Neolithic periods and most of the Bronze Age would have reduced these finds to relative or total invisibility, unless their preparation in foods might permit a glimpse of them. However, as the author well knows and does not state overtly is the problem of even primary identification of vegetal material. Very often charcoal is stored for decades and never seen nor studied and, often, within this bulk of material, bulbous

plants, fruit and items of "food" could be "hiding", as they are often not recognized by excavators and are waiting to, at least, be identified as a "mass of dough/food residues". As a general routine, archaeobotanists do not always mention these finds in their lists, especially as not all archaeobotanists check the charcoal found 'en masse' on sites, due, mostly, I would like to think, to time constraints.

It is followed by chapter 9 which presents aromatic and pharmaceutical plants.

Her introduction into the uses of aromatic and pharmaceutical plants is presented in an appealing style of Greek, however, she gets trapped into the realm of Linear B with its multitude of possible meanings and interpretations, without dealing in depth with all of the archaeological discourse. Such an example is "KI-TA-NO" where it is decided, light heartedly, without referring to other alternative interpretations that it is *Pistacia* spp. (cf. Sarpaki 2001 for discussion). Her list of plants (Table 9.1) is interesting but her pharmaceutical information is not extracted from indigenous knowledge, as tapped from ethnographic information but is rather of an erudite nature, in other words, what can be read from the international or national literature. On the contrary for us, of utmost importance, is what is common knowledge, handed down from generation to generation and partakes to what we can name the "peoples' " bank of memory and knowledge.

Chapter 10 concentrates on "foods that travel: the traditional tastes of the Neolithic and the food innovations of the Bronze Age". This chapter starts with a cautious introduction about ideas of food as the evidence, she be-

lieves is not present, and, therefore, no hypothesis can be made about pre-Neolithic cooking traditions as well as Early and Middle Neolithic cuisine. The Late Neolithic she believes is better represented and it is believed that there is a tendency towards the consumption of *T.monococcum*, einkorn, for the north of Greece and *T.dicoccum*, emmer, for the south of the country. It is doubtful how she comes to this conclusion as her table 3, where both crops are marked, seem to provide, equally, parallel, evidence for both crops. The same cannot be said about the pulses, which already present some variety: *Lens sp.*, *Lathyrus spp.*, *Pisum sp.*, and *Vicia sp.* She introduces the concept of "traditional" in food, which is interesting, when a food is consumed for some 3000 years. In the Bronze Age (p.121), however, it is obvious from table 3 that *T. spelta* gets introduced, whereas *Panicum sp.* seems to "trickle" in constantly since the whole of the Neolithic but intensifies its presence in the Late Neolithic (Olynthos, Agios Ioannis Loucas, Mandalo). After all, the great divide between the Bronze Age and the Neolithic is totally a figment of our imagination and has no reflection on reality other than making it possible for us to communicate more easily. In some way, she refers to the slow "trickling" in of various plants such as *Lallemantia sp.*, *Vicia faba*, and *Coriandrum sativum* in the 3rd millennium, *T.spelta* at the end of the 3rd millennium, whereas *Papaver somniferum*, *Camelina sativa* and *Panicum sp.* from the 2nd millennium. Her discussions about area of origin of *Lallemantia*, *T.spelta* as well as *Papaver somniferum* are informative but I would have liked to see more primary referencing rather than just the book by Zohary and Hopf (2000).

The mention of the relevance of the movement of other goods together with the arrival of cultivated plants is not new but remains rather interesting. Her integration of physical anthropology (research by S. Triantaphyllou), zooarchaeology (possible arrival of the horse at the site of Archontiko (Valamoti 2008); Kostopoulos 2002), the study of metals is an interesting concept, something which needs to be enriched even more and provides the much awaited archaeological context to the understanding of what can be some of the mechanisms which makes plants move and arrive to other parts of the world, especially as new plants means new technology (sometime), new foodstuffs, new cuisine, new tastes. These are potent with traditionalism and symbolism which cannot be easily altered in sedentary societies. It, also, rather provides a spectrum of the area(s) where circulation exists.

Chapter 11 is on "Prehistoric cooking: from the seeds to the Prehistoric cuisine".

The role of food in defining cultural identity is a subject tackled in the beginning of this discussion and although it is a notion that has existed within some 2 decades in the international bibliography, it has not been discussed to any length in the archaeology published in the Greek language, especially not as a textbook for students of archaeology. This is a very welcome note for people who only read Greek as it opens up young scholars to the real anthropology of food. She also discusses ways of how we do think about reconstructing Prehistoric cuisine and all the shortcomings connected to the subject, constructive ways of approaching the evidence we have and differentiating, but still dar-

ing, to hypothesize. Her discussions about Bronze Age cuisine is an eye-opener to possibilities one can do with the evidence at hand as she refers to many recent studies on the subject by several other scholars, including herself. It is her best chapter!

The last chapter, number 12 is the epilogue and is entitled "Searching for our common food past by studying Prehistoric plants".

In this last chapter, the author deals with her experiences teaching a module on archaeobotany at the University of Thessaloniki and the appeal which the knowledge of Prehistoric food has on the media, the students and, of course, on the wider public at large. This is a laudable experiment as this knowledge comes from the people and an archaeological department needs to bridge the gap between its intellectual pursuits and what it can offer to its community.

The book is a milestone in Greek archaeology as it is the first archaeobotanical book in the Greek language, something which was much awaited. It is a good compilation of the research which revolves around archaeobotanical problems but, to my view, it is, still, centered, too much, on Greece. The *comparanda*, of how one can make things differently, is not present. Having said that though, and criticized as duty called, I still, wholeheartedly, believe that the effort put into its making by my colleague is commendable and it is a great tool which, hopefully, would wet the appetite of students to investigate everyday life and not be indifferent towards the collection of the material which, when read/interpreted, opens up a new and fresh page into daily life.

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Reviewer: Dr. A. Sarpaki

Reply by

Soultana-Maria Valamoti, 2009

I am deeply grateful to my colleague and friend, Dr Anaya Sarpaki, for her thorough, in-depth reading of my book. A pioneer in Greek archaeobotany, as the first native archaeobotanist, she opened the way for all of us who followed. Her comments and criticism are therefore much appreciated. Yet, I would like to express my views on certain points in Dr Sarpaki's review of my book *Η Αρχαιοβοτανική Έρευνα της Διατροφής στην Προϊστορική Ελλάδα* (An Archaeobotanical investigation of Prehistoric Diet in Greece).

The book is about prehistoric diet, therefore my references to foods from later periods (e.g. classical, Byzantine), concern very specific cases of rather exceptional archaeobotanical finds, such as bulgur and trachanas, or fava, foodstuffs we still use today and for which I felt the need to show the considerable depth of time over which these have been consumed in the region. I agree with Dr Sarpaki that my references to periods later than prehistory are not consistent. This is intentional as the aim of the book is to highlight aspects of prehistoric diet, not to form a comprehensive account of plant foods in Greece in both prehistory and antiquity. Bringing all available literature on an-

cient Greek / Roman / Byzantine plant food into a book titled 'An Archaeobotanical Investigation of Prehistoric Diet in Greece' would present the student with a great deal of additional information that could in fact be the subject of another book. My selective inclusion of elements from later periods (rather than a total exclusion) was dictated by my intention to let students realize that aspects of prehistoric food are connected to later periods, and that continuities and discontinuities can be traced through the archaeobotanical record, from prehistoric times to the present. With regard to ethnographic evidence, I chose to be brief and selective, otherwise our students might have had to wait yet another decade before they had in their hands a book on prehistoric plant-based food in Greece.

In my discussion of pulses, Sarpaki misunderstands the point I am making as regards the discrepancy between the archaeobotanical record and textual evidence for Late Bronze Age Greece. In fact this is a point very nicely presented by Paul Halstead, a long time ago, through the integration of archaeobotanical and archival data: pulses are present in archaeobotanical samples but not mentioned in texts. I never say

in the book that the palaces were not interested in the "day to day agriculture" (sic), I say that pulses do not appear to be of archival interest. I am grateful for bringing to my attention my wrong use of the English common name for *Lathyrus clymenum*; fortunately though, throughout the discussion of *L. clymenum* in chapter 5, the Latin and common Greek names are correct.

I have been very reluctant in chapter 6 of the book to attribute a culinary use to olive oil in prehistoric Greece. Dr Sarpaki refers to the identification of olives/olive oil in two bowls from Middle Neolithic Gerani, on the basis of chemical analysis, reported in Tzedakis and Martlew (1999). In this publication the graphs with the chemical compounds detected by analysis, were not provided, and the identification of olive oil in their more recent publication (Tzedakis et al. 2008) has been criticized by specialists in chemical analysis (Roumpou et al. in press). I think it was a wise thing not to refer to these finds, though in a revised version of the book, it would be possible to bring information from chemical analysis into the discussion of oil use in the prehistoric Aegean. Sarpaki finds it outrageous that I say that there is no "evidence for olive oil consumption in the Minoan period", and refers me to Boulotis 1996 and Hamilakis 1996. We probably understand different things by reading the same papers so I quote (Hamilakis 1996, p. 19-20): "... but archival evidence for the use of olive oil as a basic foodstuff is very doubtful or non-existent. In contrast, the archives offer very clear evidence for the use of oil in the perfume and unguent manufacturing industry..." and "Thus olive oil moved towards the palaces as a sort of tribute and within

the palace it was used either for festivals and other ritual activities or for allocations to palace personnel and officials for perfume and unguent manufacture and probably for personal use". Also, (Hamilakis 1996, p.25): "The evidence for the beginning of the systematic exploitation of oil during the second palace period coincides with the evidence for intensification of external contacts from this period onwards, supporting the argument for its use as an elite item, most probably as a base for perfumes and unguents". In 1999, Hamilakis also states that "Despite conventional wisdom, there is almost no evidence which could indicate with certainty olive oil production for culinary use" (Hamilakis 1999, 45-46), quoting the same source I use in the book, Boulotis 1996. Boulotis states on page 24: "The use of oil (olive oil) in diet, something we could consider as likely to have happened on a limited scale, is not clearly testified" (in greek). Being sceptical about the consumption of olive oil as food in Bronze Age Greece may indeed seem outrageous to those who associate modern Mediterranean landscapes and food habits with prehistoric times, yet our task is to base our arguments on the archaeological evidence, not on assumptions and extrapolations of the present to a distant past.

As regards the use of olive fragments as fuel, nowhere in the text on page 89 do I dispute their potential use as fuel. It is the use of olive oil as fuel that I discuss and I quote: "*There are, however, no indications for the use of olive oil as food in Minoan times (Boulotis 1996) and even less so as fuel*". As regards animals defecating on the floors of a Middle Minoan building, I am certainly not assuming that this is the case for the Tzambakas

olive stone fragments, I am merely suggesting that this should be a hypothesis to exclude if we wanted to argue more positively for olive oil extraction in that room. One should not forget that original uses of buildings may alter after their abandonment. To prove my point I would like to refer Dr Sarpaki to the article by G. Stamatakis (2008) in the magazine *Kritiko Panorama*, where, on page 88, two pigs together with their fecal waste are shown penned in an early Christian grave, currently incorporated into a modern living compound including this pigsty, in the village of Agios Thomas in Crete. My reference to the *trapetum* was of course not done to imply that such equipment was available in prehistoric times, rather to show that wooden equipment that does not necessarily crush the olives can be used, a known example of such practice being the *trapetum* of Roman times. For Neolithic times one could envisage a wooden pestle and mortar to start with, a possibility also mentioned by Hamilakis (1996).

In her evaluation of chapter 7 and the discussion of prehistoric wine, I think Sarpaki conveys a message different to what is actually being said. Never do I say that the inhabitants of Greece were using the juice of the grape for making wine from the Early Neolithic. I am rather on the cautious side as regards inferring wine from grape pressings. As regards the Bronze Age, finds of grape pressings are encountered both in the north and the south, indicating juice extraction and possibly wine mak-

ing. The scale of wine use in the Bronze Age is inferred on the basis of large quantities of pottery related to serving and consuming liquids found in Bronze Age contexts. Having said this, I do not mean that people were drinking a glass of wine on a daily basis alongside daily meals. My referring to myth and Dionysus, was rather risky and certainly unnecessary. I could not, however, resist the temptation to relate the archaeobotanical finds from Dikili Tash to one of (not the only one, as Sarpaki's comment implies) the birth places of Dionysus according to myth, i.e. the region known as Thrace to ancient Greeks. The reference to myth was not intended, however, to back up the archaeobotanical data, especially given that in ancient Greece myth was used and modified in order to meet various political ends.

Leaving aside the points discussed above, my colleague's review, the result of a very careful reading of the book, is more than welcome. It has helped me identify points that need improving and further work that needs to be done, and for this I greatly value her thoughtful advice. I would very much have liked to expand various aspects of past plant consumption as food, and to have had a much broader geographical and temporal perspective. But in this archaeobotanical investigation of prehistoric diet in Greece, I tried to combine the available evidence for a specific area, here and now, providing (I hope) incentive for future archaeobotanists to further elaborate on the subject.

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