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MARIJA GIMBUTAS IN THE CLASSROOM, FIELD AND OFFICE: A SHORT PERSONAL REMINISCENCE

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INTRODUCTION

I first met Marija Gimbutas in 1967 when I travelled crosstown to UCLA to inquire about doing a PhD there under her supervision. In 1967 I joined the interdepartmental program in Indo-European Studies with a specialisation in European Archaeology. Marija was on sabbatical during my first year at UCLA (so my first instructor in European archaeology was her replacement, Lili Kaelas) but I caught up with her again when I served on her excavation at Obre in Bosnia in the summer of 1968. In the following academic year I attended two of her classes (the European Neolithic and Bronze Age) after which I had a three year sabbatical (US Army) before returning in 1972 to audit her seminar in Neolithic religion. I also recall a graduate seminar under the aegis of the newly founded Institute of Archaeology where I submitted an essay on the History of the Indo-European Problem which Marija forwarded (unknown to me at the time) to the newly founded *Journal of Indo-European Studies* (Mallory 1973). I went on to serve on her excavation at Akhilleion in Greece in 1973. I completed my PhD with Marija in 1975 by which time I was fairly integrated into her world as she had not only been my PhD supervisor but also my landlady: I and my roommate lived in a small bungalow on her property in Topanga Canyon, paying rent partly in cash and partly in labour. After receiving my PhD I spent one year replacing Marija at UCLA while she was on sabbatical. My last 'official' activity with Marija was joining her joint projects with Santo Tiné in southern Italy where I directed the excavations at Lagnano Da Piede, although by the time the excavations had entered their second year I was – and have been ever since – at Queen's University Belfast. I maintained some correspondence with Marija after that and we met at various conferences such as at Forli, Italy. I visited Marija in 1988 at her house in Topanga when I was on vacation in California with my (then very young) family and the last time we met in person was at an Indo-European conference in Dublin in 1989.

Any attempt to provide an account of someone's activities that began well over a half-century ago, primarily on the basis of a failing memory, requires an introductory disclaimer. Almost everything, other than my account of what Marija Gimbutas taught in 1968–1969, is based on fading recollections. Nevertheless, I will attempt to treat three areas of Marija's life that may be of interest to readers: her teaching, and, very briefly, her excavations and her approach to conducting research. It should be emphasized that I can only write about my own personal experiences that occurred at a specific time in Marija's career (c 1968–1975) and that others may well paint a very different picture that would be entirely valid for their own frame of reference.

TEACHING

The source of the following discussion of Marija's teaching are two notebooks (MGNeo 1968; MGBron 1969) which have miraculously survived in my possession and cover her courses on the European Neolithic and Bronze Ages which she taught in the

1968–1969 academic year (the Neolithic class was certainly taught at this time, the Bronze Age with very high probability as my notebooks were clearly purchased at the same time). These courses were designed to provide students with a very sound, albeit traditional survey of the archaeological cultures and major sites of each period. The Neolithic course was assessed on the basis of an exam and a substantial seminar paper (my essay on the Butmir culture ran to a very painful 59 pages). There may have also been a slide test – Marija certainly prepared us for one – but I cannot recall for certain.

The structure of her lectures sometimes began with any material that might provide a chronological framework, e.g. Jessen's pollen zones from the Late Palaeolithic to the end of the Neolithic, Becker's chronological framework for the Neolithic of Northern Europe, the succession of Neolithic cultures found at Arene Candide, the cultural sequence of Linearbandkeramik, TRB, or Cucuteni-Tripolye, etc. In short, her approach very much resembled that of V. Gordon Childe. This was followed by a description of the major cultures or archaeological sites, followed by a presentation of slides at the end of the lecture. Since detailed notes seemed to be required, I note that her first slide show comprised 47 slides that included material ranging from Çatalhöyük to the Tisza culture. Reading my notes in hindsight there are several things that might be of interest.

First, there is her general detailed presentation of cultural history. She would normally begin with a brief overview of a culture and its location followed by a summary of its architectural remains, an account of its economy, and a description of its ceramics – their appearance through time and possible influences, figurines, mortuary evidence and physical type. On occasion where there was some controversy, e.g. the origins on the TRB culture and its cultural relationships, the various schools of thought were summarized. Also, Marija routinely provided the names of the excavators associated with various sites.

To take a familiar culture as an example, I quote below my notes for the part of her lecture covering the Starčevo culture:

Starčevo

- *known from over 100 sites*
- *spread all over Yugoslavia except for Adriatic, reached SE Hungary to the Körös river,*
- *Karanovo in Bulgaria is related but is a separate variant*
- *Starčevo went up to the border of Soviet Moldavia, surrounding the Carpathians*
- *there is no difference between late Proto-Sesklo and Starčevo*
- *Starčevo had same cereals and domesticated animals (wheat, barley, spelt, vetch)*
    - *lived on river banks and lake edges*
    - *fished more than Proto-Sesklo people*
    - *many fish bones*
    - *had hornless sheep – raised long before woolly sheep*
    - *large species of cattle sim[ular] to Aegean were next in importance*
    - *dogs were kept*
    - *asinus hydruntinus – wild donkey – now extinct – climate was warmer*
- *houses were similar to those in Thessaly but some differences*
    - *subterranean huts*
    - *trapezoidal houses Ex, Gladnica*
    - *at Röszke – house model, quadrangular, gabled roof, animal head*
- *physical aspects of Starčevo people*
    - *not enough graves or skeletons, ca 10-15 skeletons*
    - *pop. of Balkans 6-5th millennium were already mixed (a mélange)*
        - *Old European Upper Palaeolithic type*
        - *Eastern component, brachycranial medium stature*

- *– Nemeskeri: they came from Western Anatolia*

– *chronology*
- *– starts in later part of Proto-Sesklo, continues through Pre-Sesklo and Sesklo*
- *– Vinča – 9m*
- *– 2 floor levels of Starčevo culture at c. 9.30–7.80 m*

– *Gornja Tuzla*
- *– base is Starčevo overlain by Vinča*

– *Gladnice near Pristina in S. Yugoslavia*
- *– Starčevo overlain by Vinča*

– *Porodin*
- *– Vinča is a continuation of Starčevo – many elements were continued*
- *– new elements from Macedonia, Thessaly, and Bulgaria*

– *Obre*
- *– Starčevo culture followed by three more house floors*
- *– It is the westernmost site*
- *– then influences from the west, Naretva – changed the culture to Kakanj*

– *Kakanj site (1954) – late Starčevo site*

– *in Rumania it became Petresti (5th– 4th millennium)*

– *Starčevo starts later 6th and ends ca 4300 BC.*

One can gain a flavour of the level of detail we were exposed to by perusing Marija's description of the ceramics recovered from the Danilo site at Smilčić by Šime Batović:

Vessels: semi globular, conical, or biconical with flat or hollowed bases, poorly fired, large temper, monochrome with brown or orange slip, impressions before firing – indented or unindented shells, randomly impressed, bands of zig-zags, rocker stamped, vertical lines of decoration, stabbing with bone or wooden instrument, finger prints are rare (contrasts with other sites)

Now just a few observations about such a presentation. First, at the same time as we (myself and the other archaeology graduate students in the Indo-European Studies program) were learning the cultural history of Neolithic Europe in a very traditional way we were also taking courses in the Anthropology Department from Jim Hill and Jim Sackett, who introduced us to the 'New Archaeology' (when it really was 'new') or Processual Archaeology as it is frequently called. At that time at UCLA one could even take courses (if you wanted to sit on the floor in an over-flowing classroom) from Lewis Binford. Obviously, one might imagine that our brains were on the point of exploding from cognitive dissonance as we spent half our time with Marija learning detailed cultural histories while the Processual hemisphere of our brains was screaming that all of Marija's lectures were horribly 'normative'. But actually having two very different approaches was a blessing and I have always been thankful that I did have such training as it has permitted me to pick up site reports or articles on just about anywhere in Neolithic Europe and feel that I have had enough background to understand the cultural and temporal context of the paper. Being exposed to two very different routes to the past kept things challenging and a number of Marija's PhD students who came slightly after my time easily combined the two approaches by employing social statistics to test a range of hypotheses suggested by Marija's interpretations of the archaeological record. This is especially apparent in the PhD theses of Steven O'Brien (1979) *The Mortuary Practices of the Late Neolithic Peoples of Central Europe*, and Susan Skomal (1983) *Wealth Distribution as a Measure of Prehistoric Change*. Both of these theses relied heavily on examining the extensive amount of data that Marija had exposed them to with analytical procedures (e.g. chi-square tests, gini coefficients, etc) that they had learned in the Anthropology Department.

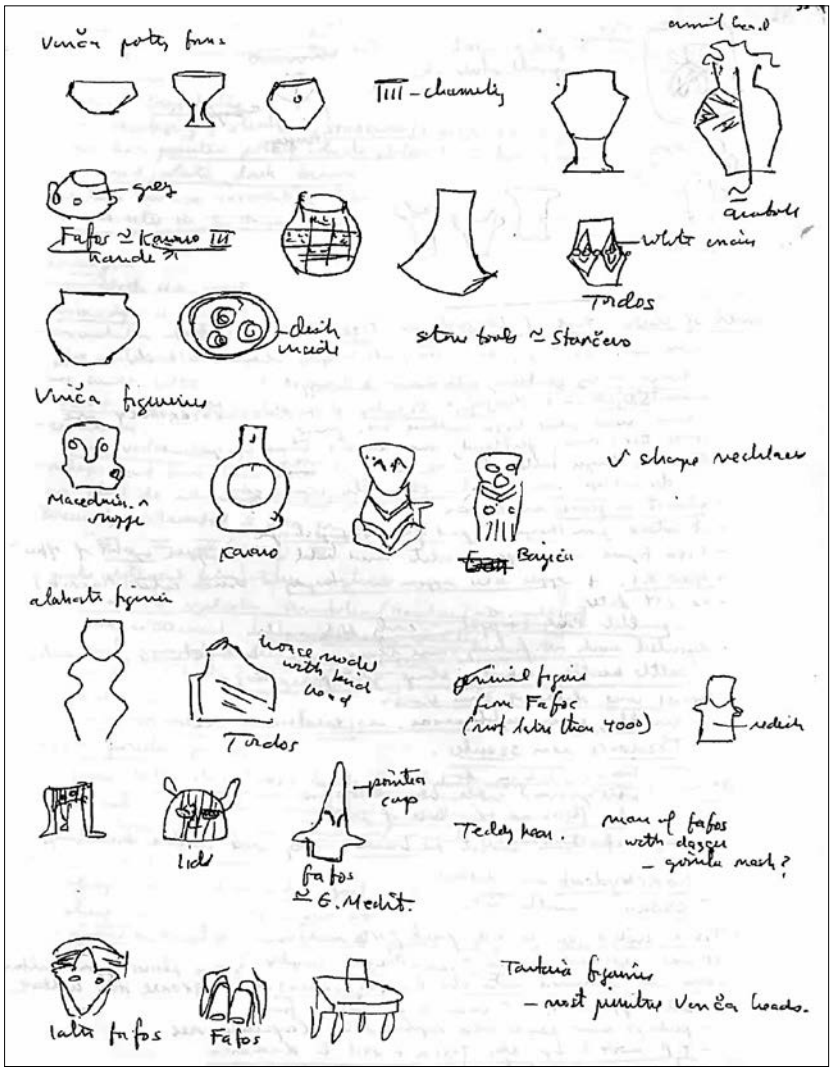

Fig. 1. Sketches of a slide lecture on the Neolithic by Marija Gimbutas in 1968. *Drawing by James Patrick Mallory.*
1 pav. Eskizai pagal M. Gimbutienės paskaitoje apie neolitą rodytas skaidres. 1968 m. *James Patrick Mallory pieš.*

from Sventoji [I am afraid that this is the only Baltic site I find mentioned in Marija's lectures.]) or ornaments, and 4% were of graves.

Marija's course on Bronze Age Europe began with an introduction to metallurgy, arsenical bronzes, sources of tin, and the earliest appearance of metallurgy in Europe. Her lectures concentrated on the late Copper Age cultures of central Europe (Tiszapolgar, Bodrogkeresztur, Baden, Vučedol) and Italy (Rinaldone, Remedello), the Corded Ware, Beaker, and Globular Amphora horizons, and then the classical Unetiče-Tumulus-Urnfield sequence as well as the full series of Aegean cultures (Early Bronze Age to Late Helladic IIIc). She reprised and extended some of her earlier lectures on the Pontic-Caspian region as well and then covered the Jamnaja-Katakombnaja and Srubnaja cultures. Expressed in terms of illustrations, the nature of the Bronze Age material culture shifted the focal balance of illustrations (N= 152) to 59% pottery, 22% tools, 11% ornaments, and then trace percentages (3% or less) of figurines, house plans, wheeled vehicles, and graves.

As for the visual presentation of sites and cultures, the slides were overwhelmingly focused on ceramics and figurines as the iconic markers of the various cultures or sites. Of the roughly 330 objects crudely sketched in my Neolithic notebook (Fig. 1), I note that 55% were of pots, 29% were of figurines, and only 7% were of architectural features (among which were also included clay house models), 5% were of tools (e.g., a Bandkeramik shoe-last celt, a wooden tool

Although Marija provided a very traditional approach to cultural history, she was very well informed about radiocarbon dating. Early in her lectures she provided more than two pages of notes on radiocarbon dating, including the need for calibration,

which was first suggested by Hans Seuss in 1965 and 1967. This also guided her approach to excavations where she prided herself on obtaining as many radiocarbon samples as she could although I doubt that this impressed that great skeptic of radiocarbon dating, Vladimir Milojčić, when he visited Marija's excavation in Greece. In my Neolithic lecture notes she cited approximately 60 radiocarbon dates. Marija very much embraced radiocarbon dating and its calibration which rendered the European Neolithic even earlier than traditionally imagined. I recall that when I first encountered Colin Renfrew's *Before Civilization: Radiocarbon Revolution and Prehistoric Europe* which was first published in 1973, the initial thought that went through my mind was that I had already been exposed to these ideas – the antiquity and independence of the European Neolithic – by Marija years earlier. This was at least one area where these two scholars agreed.

A glance at my notes also reveals another aspect of Marija's approach. You should note that very often what were held as distinct 'national' or regional cultures such as Proto-Sesklo (Greece) and Starčevo (Yugoslavia) are often equated and there is no doubt that in her approach to the myriad of European cultures of all periods, Marija was a 'lumper' rather than a 'splitter'. She was always far more inclined to view what she regarded as the similarities between cultures rather than emphasize the differences between them. Nowhere is this seen more starkly than in her adoption of the label 'Kurgan culture' for a broad range of both geographically and temporally distinct archaeological entities.

Now reviewing my notes, I will try to comment on a few of the major themes of Marija's later work and how they were treated in her lectures is 1968–1969.

Old Europe. I find no explicit mention of the concept of 'Old Europe', i.e. a representation of Neolithic Europe as an independent cultural zone associated with peaceful matrifocal societies. On the other hand, Marija did emphasize the separateness of the Neolithic societies of the Aegean and Balkans from those of Southwest Asia. Following her introduction of the importance of radiocarbon dating, I find in my notes the explicit statement: 'The Balkans and Aegean was a separate cultural center' (MGNeo 1968, 11) and with reference to Anatolian connections with Early Neolithic Greece, she taught: 'evidence for Anatolian origin: ear studs of stone, belt hooks (fish hooks) are common to Catal Höyük (6000 BC), may only indicate some trade. These are the only evidence for connections' (MGNeo 1968, 11). She did summarize Weinberg's evaluation of Proto-Sesklo links with Anatolia as well as eastern influences on the formation of the Sesklo culture but she also emphasized that 'they only reached the east coast of Greece and not north into the Balkans' (MGNeo 1968, 12). And when addressing the origin of Vinča and related cultures of the Balkans she contrasted the 'traditional' viewpoint of Milutin Garašanin that these cultures were stimulated by a migration from Anatolia with her own that argued 'change was not very rapid, much local development – Vinča […] Adriatic area – small imports were developed into great changes' (MGNeo 1968, 28). And in noting the theory that the spread of black burnished ware in the Late Neolithic of the Balkans has been linked to east Mediterranean origins, Marija argued 'but there are no middle man sites. It is only an assumption. It could have been discovered locally in Europe. Some is found in Anatolia (Catal Höyük, Can Hasan) but they differ' (MGNeo 1968, 32). And as for the Danubian region, Marija believed that the spread of the Neolithic there was due to 'slow conversion rather than migration'. So we can see that in 1968 she was moving toward the idea of a relatively autonomous Aegean-Balkan Neolithic but she had not yet developed the full concept of an 'Old European Civilization' which she proposed in 1971 (Gimbutas 1973, 2).

Goddesses. In her 1968–1969 classes, Marija included many figurines in the class illustrations

but, according to both my (less than trustworthy) memory and my notebooks, she had not yet devised her system of classifying them into an 'Old European' pantheon. To be sure she did note things such as a 'bird beak' on a Starčevo figurine or a bird-shaped lamp from Vinča (MGNeo 1968, 6), but there was no specific mention of a 'Bird Goddess' or any of the other reconstructed deities familiar to readers of her later works. I only encountered these when I returned to UCLA in 1972 when I audited her class on Neolithic religion. Here she lectured to a large class sitting around a rectangle of co-joined tables and instead of slides she periodically passed around photographs of various figurines (that were to be published in her *Gods and Goddesses* book) to illustrate her concepts of the various 'Old European' deities. It was a very different archaeological world than the one I had left and I still recall exchanging glances with (I think) Raymond Sidrys as we passed between ourselves the photo of what Marija has just described as a 'crawling uterus' and felt that we had accidentally blundered into the women's shower room. Another aspect of Marija's research into Neolithic religion was that many of the stylized heads, especially those with sharply pointed chins and large expressive eyes, were interpreted as masks. It was my impression that she had come to this conclusion while on sabbatical in 1968 and she certainly shared this explanation with us when we were excavating at Obre and there are frequent references to her mask interpretation throughout my notebook.

Kurgans and Indo-Europeans. Finally, Marija's treatment of IE origins had already been published and developed; in fact, she dated her kurgan theory from her publication in 1952 which she marks as the beginning of the theory (Gimbutas 1952) although she first employed the term 'Kurgan culture' in 1956. From the notebook it is clear that she was already arguing for 'Kurgan' expansions from *c* 3500 BC onwards which were framed in her model of three distinct chronological periods (Kurgan I-III) that was translated into a series of waves (Fig. 2) pushing further west along the Danube, models that were formally published later in detail (1977; 1979). She also integrated into her model the collapse of Neolithic cultures in southeast Europe (MGNeo 1968, 60) although this was not yet framed in its more familiar matrifocal Old Europe collapsing at the hands of patriarchal, warlike Indo-Europeans.

Regarding her treatment of earlier versions of the Kurgan model, one generally looks in vain in her publications for mention of earlier examples of the Steppe Homeland theory before her own initial treatment in 1952. But in her class lecture she did very briefly mention the earlier history of homeland solutions which is perhaps worth quoting in full (as I scribbled it down; MGBron 1969, 19–20):

– *archaeological research on homeland*

mid-19th Otto Schrader (1883) – Neolithic proto-Indo-European

Gustav Kossinna (1902) – homeland in Germany, TRB was IE > Glob[ular] amph[ora], Corded Ware.

Sulimirski (1933) Corded Ware = IE, nomadic culture from the steppes, appeared in Europe ca. 2500 BC (has reversed himself recently).

Ernst Wahle [presumably 1954 – JPM]– *cultural complex is not as important as the way of life*

*Bosch-Gimpera (*1955) [reference unknown to me; Bosch-Gimpera's monograph on Indo-European origins appeared in 1960]– *all Mesolithic cultures are indigenous = IE mother culture = Mesolithic (He ignored Mesolithic differences.)*

On the one hand, this is one if not the only place I have found references to earlier versions of the Steppe Model by other authors. On the other hand, other than Sulimirsky (1933) who did indeed shift his position, there is no mention of the monographs of Childe 1926 (and his later article in 1936), Peake and Fleure (1928), or especially Georges Poisson

(1934) who all articulated with various degrees of certainty a model that involved a steppe origin for the Indo-Europeans and their expansion that resulted in the creation of Marija's 'hybrid Kurgan cultures' forming the different branches of the Indo-European language family.

Her discussions of the formation of Kurgan hybrids (e.g. Baden, Vučedol, Globular Amphora, etc) are well enough known in her published works. It is interesting to compare, however, her lectures regarding the origins of the Bell Beaker culture with her previous treatment of it in her mammoth *Bronze Age Cultures in Central and Eastern Europe* (1965). Here, the Bell Beaker culture has a traditional West Atlantic origin and was 'brought by groups of mobile folk who, before the end of the third millennium or around 2000 BC, reached central Europe as far as Hungary and southern Poland' (Gimbutas 1965, 32, see also 250). I also recall that in the UCLA library's copy of Hugh Hencken's *Indo-European Languages and Archaeology* (1955), where he had attempted to associate the spread of Indo-Europeans with the Beakers, some presumably enterprising student had pencilled into the margins something to the effect that 'Gimbutas says no'. In any event in my 1969 notebook Marija had clearly changed her views and dismissed the Atlantic origin of the Beakers as the traditional theory, and now concluded that 'Vučedol –Zok – Makö are [the] only real candidates for Bell Beaker origins, hence Kurgan people' and 'Bell Beaker folk belong to [the] large Kurgan family', ideas that would later appear in her works (Gimbutas 1970) and that, at least for

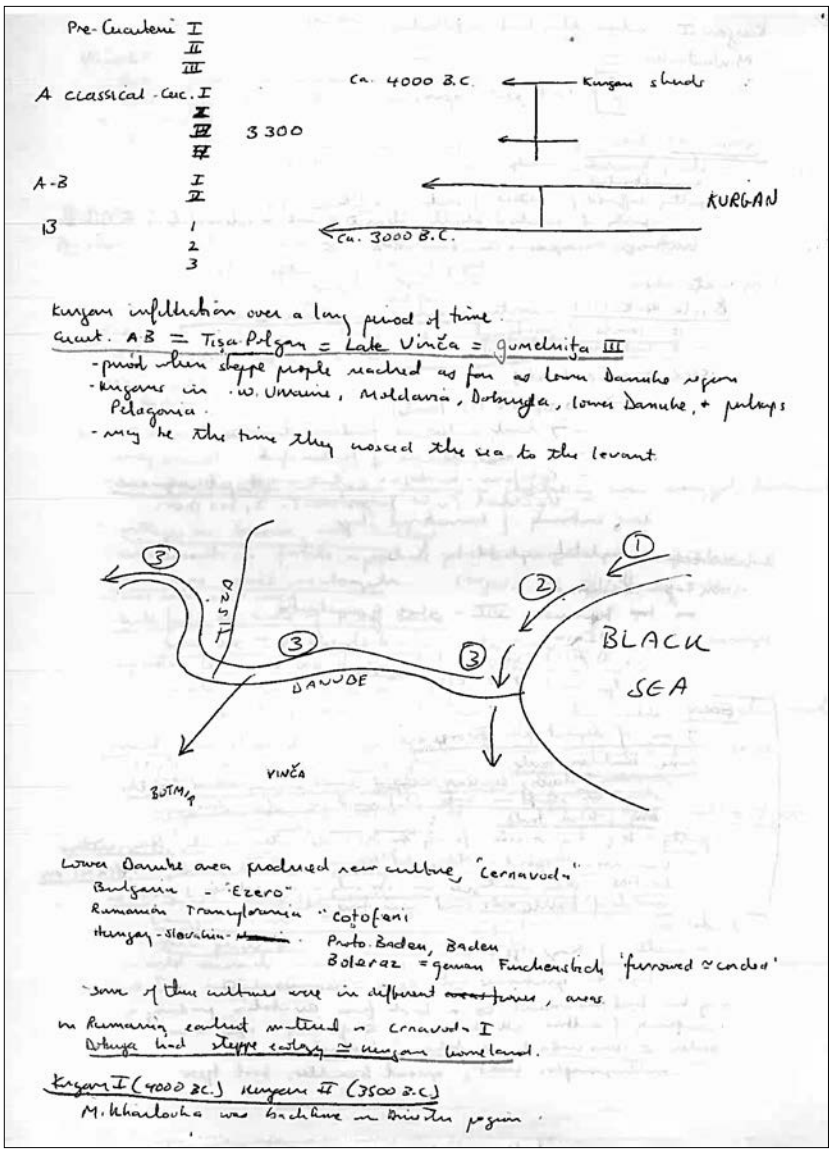

Fig. 2. Notes from Marija Gimbutas' class on the Bronze Age in 1969 with a sketch chart of the chronological phases of Cucuteni-Tripolye and her map of the three waves of steppe invasions. *Drawing by James Patrick Mallory.*
2 pav. Užrašai iš Marijos Gimbutienės paskaitos apie bronzos amžių su chronologinės Kukutenio-Tripolės kultūros fazės brėžiniais ir žemėlapiu, kuriame pažymėtos trys stepių invazijos bangos. 1969 m. *James Patrick Mallory pieš.*

the northern tier of Beakers, have been spectacularly supported by recent aDNA evidence that has routinely found evidence for the genetic signature of steppe populations in the aDNA of the Beaker burials north of Iberia (Olalde *et al*. 2018).

EXCAVATIONS

I served on three excavations on which Marija was the principal investigator (usually jointly with a local archaeologist with Marija having raised the necessary financing for the excavation). On all three excavations Marija appointed or assembled the American archaeological teams (largely from her classes) but left the responsibility for fieldwork with her field director. The three excavations – Obre, Akhilleion, and southern Italy – were each very different experiences.

The excavations at Obre consisted of a partnership with local Yugoslav archaeologists under the direction of Alojz Benac of Sarajevo. Two sites were excavated: Obre I, a site of the Kakanj variant of the Starčevo culture, and Obre II, a Butmir culture site. Obre II was the larger operation (928 m^2) and was divided into two strategies: 1) very large sondages excavated by local village labour under the direction of Yugoslav archaeologists to uncover broad architectural remains and 2) a much smaller sondage dug by an American-British (several students of Prof Colin Renfrew from the University of Sheffield) team under the direction of Eugene Sterud which would focus on a more detailed collection of material and recording, including flotation which at the time was only then being introduced as a recovery technique.

An extremely unusual aspect of the excavation is that the Yugoslav sondages were each overseen by senior archaeologists drawn from all over the former Yugoslavia – I recall Šime Batović, Đuro Basler, Bogdan Bruckner, Tatjana Bregant – and even from abroad, Jiri Pavuk and Viera Němejcová-Pavúková. Moreover, Marija organized this group into giving a weekly seminar at her 'University of Obre' to all of the students working on the site with Marija translating when the lecturer did not employ English. And she was sufficiently networked to ensure that the animal bones on the site were examined over a period of a week or so by Sandor Bökönyi who visited the site.

The writing up of the American part of the excavation followed a common pattern where the field director, Eugene Sterud, wrote up most of the report (the research was part of his PhD) while Marija confined herself to such topics as the introduction, the chronological evidence including the radiocarbon dating, and the place of the site in the region's cultural history (Gimbutas 1974).

In 1973 Marija collaborated with Dimitris Theokharis in excavating the Neolithic site of Akhilleion in Thessaly, a site where a preliminary test pit had reportedly uncovered evidence of a level of the Aceramic Neolithic. Her initial field director was Gary Stickel from UCLA but also on site was Shan Winn, one of her former PhD students, who would eventually write up the excavation report with Daniel Shimabuku (Gimbutas, Winn, and Shimabuku 1989). Much like at Obre, Marija left the field direction to her team, periodically visiting the site to keep abreast of things or to examine the context of the finds, especially figurines. As she had become increasingly interested in Neolithic religion and the site was yielding a large quantity of figurines, Marija spent much of her time recording the figurines in the field laboratory (her catalogue comprises 199 figurines). As with the Obre report, Marija contributed the introduction, the chapter on the chronology, the chapters on figurines and small finds, and the conclusions.

Finally, in 1977 Marija collaborated with Santo Tine of the University of Genoa on excavations in Apulia in southern Italy. Here there were two sites: Grotta Scaloria, a cave site with evidence of a Neolithic cult which was again directed by her

former student Shan Winn (Elster *et al*. 2016), and Lagnano Da Piede, a large multi-ditched settlement site, excavated by myself (Mallory 1991). Because Marija's interest was so firmly in the Neolithic cave site, there is unfortunately absolutely nothing I can recall of her presence at the second site.

RESEARCH

I can say very little about Marija's approach to research, at least in detail. Much of what I saw was fairly routine for a scholar of international status who had at least some access to support, either paid or voluntary. Marija normally worked with an assistant who might also serve as her illustrator, preparing the many drawings or making the maps for her various works. These might also assemble research material for her, fetching books from the UCLA library or making copies of them, and in her later works on goddesses (after my time), provide editorial assistance (i.e. Pat McDonnell and, later, Joanne Marler). But like many of us, when a project was in full swing the floor of her study would be strewn with stacks of offprints, books, and illustrations waiting to be processed or assembled.

Regarding research she did make several comments that may be of interest. One concerned the wide range of sources that she employed and the impression that some of her admirers had of her spending endless time pouring through hundreds of books and articles in a wide variety of languages. She laughed at this and admitted that she always approached an article by first reading the conclusions to see whether it was worth reading any more of it. This was the most efficient way for her to do research.

Second, she revealed to me her motive for writing her largest book, *The Bronze Age Cultures of Central and Eastern Europe* (Gimbutas 1965). This was not the product of a scholar who had spent decades mastering the subject and who finally decided to bequeath her knowledge to the general public. It was rather quite the reverse: Marija had felt that she had not known enough about the Bronze Age and believed that the best way (and probably best motivation) for learning anything was to write a book about it. For her, the writing of the book was not the disgorging of what you already knew but a challenge to learn (and write) about what you previously had not known.

Third, Marija once recommended to me that I watch Jacob Bronowski's television series, *The Ascent of Man* (1973). What she particularly emphasized was her approval that Bronowski's arguments were often generated by his intuition (so she said, at least, I really don't know) and clearly had no problems advancing her ideas and interpretations on the basis of her intuition rather than more formal appeals to logic or evidence. In reviewing Marija's work (as cited in Wikipedia), the (now late) Bernard Wailes described Marija as 'immensely knowledgeable but not very good in critical analysis. ... She amasses all the data and then leaps from it to conclusions without any intervening argument.' Assuming that both my old friends (Bernard died in 2012) have gone to the same place, I suspect that if they encountered one another Marija would have laughed and replied: 'Why Bernard, that's called intuition!'.

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