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Berichte und Kommentare

Palaeolithic Love Goddesses of Feminism

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Introduction

The following comments are prompted by a recent polemical paper dealing with the perceived female figurines of certain regions of Eurasia, which are attributed to the Upper Palaeolithic and which are colloquially known as "Venuses." Dobres' (1992) feminist-inspired reanalysis of this much-discussed selection of anthropomorphous finds is to be welcomed, if only to counter the considerable amount of scholarly nonsense written about them – often by authors who had never seen the objects, or only a small number of them. Unfortunately, Dobres has herself not seen the specimens she discusses, so her "reanalysis" is necessarily based on the biased descriptions of others, tempered by her own, stated bias in evaluating their opinions. This does not bode well for the validity of her findings, which are also marred by a surprising number of errors of fact concerning quantitative aspects and details of find sites. She does not seem to be aware of many aspects of the general subject, her grasp of relevant geography is appalling, and her own ideological direction, despite her very valid criticism of that of others, is itself not above reproach.

It seems almost unnecessary to invalidate many of the statements she opposes. Surely no archaeologist in the world would condone the comment she cites by Eaton (1978: 117): "Whether the females were raped, stolen, or killed, they appear to represent brave acts among males of a group and would qualify them as trophies."

We note that this caricature of bravery is from a paper entitled "The evolution of trophy hunting" and appeared in a journal called *Carnivore*. If Dobres' point is that such views are current among the uninformed public then one must agree, but at the same time remind archaeologists that it is they who are responsible for such a state: archaeology is not as well packaged for public consumption as it ought to be. Large sections of the public also believe in Santa Claus, or that a prehistoric vessel landed on the summit of Mt. Ararat; scholars do not attack such beliefs, so why single out Eaton's puerile fantasies? There is any number of large circulation tabloids that publish much worse archaeological flights of fancy than his, which archaeologists ignore consistently.

The fantasies of such authors as Collins and Onians may be a more serious matter, but also one which prompts the question: if these and other authors are seen as the biased writers they may well be, why does Dobres still rely on the descriptive part of their work, while ignoring the work of other, considerably more rigorous writers on the subject? Of course it is highly imaginative to assume, as Collins and Onians do, that the images were carved and fondled by men. Yet it is just as subjective to describe these fantasies, as Dobres does, as a "prehistoric Barbie dolls hypothesis," because of the claim that the figures "were produced by men for the sake of male pleasure." I am not an expert on Barbie dolls, but I do not believe that they are being produced for the sake of male pleasure.

It is not my intention to nitpick my way through Dobres' paper, or to brag with my own firsthand knowledge of the figurines or their find sites as she has seen neither. I merely wish to inform the reader, and encourage more rigorous approaches to the general subject of palaeoart studies. Nevertheless, a few corrections are inevitable, because without them it would not be possible to present some of the subsequent arguments. In her introduction, Dobres mentions that figurines were recovered from caves and rockshelters of several countries, and it is important to point out that those she lists from Austria, (former) Czechoslovakia, and Poland are from open loess terraces, i.e., not from sheltered locations. Several of the sites she lists are thousands of kilometres from the geographic regions she assigns them to.

Formal and Chronological Definition of the Sample

To analyze means to examine minutely and ascertain the constituent elements of the object in question. This is what one would be entitled to expect of a critical reanalysis. To be meaningful, an analysis can be of a particular object, or it can be of an object or a group of objects that are considered to represent a defined class of objects. In the latter case it is essential that they are indeed representative, which is partly a statistical question, partly one of formal attributes. But how does one analyze objects that have been arbitrarily defined, badly described, and even more badly interpreted? What is the basis of Dobres' analysis, other than a randomly defined segment of the "archaeological record" (whatever that may be, for I do concede that I do not know what that phrase means) and the claimed homogeneity or sameness of the finds in question. Surely before we "analyze" any such group of objects we need to ask: what evidence do we have that they ought to be considered together, or that certain other objects need to be excluded from such an exercise?

In her "reanalysis," Dobres implicitly assumes that there exists a class of objects she defines as Upper Palaeolithic female figurines ("Venuses," which she accepts is a misnomer, but still uses in her title, and without quotation marks). She considers *some* of those objects traditionally included in this class, and she examines them on the basis of limited descriptions, interpretations, and illustrations by others.

That Dobres does not consider the question of whether this class of objects is archaeological fact does not necessarily render her findings invalid. She uses as her data base essentially the same material certain others have used before her, and she would defend the procedure on the basis that it would be pointless to reassess what she perceives to be misinterpretations by basing her "analysis" on a different, expanded or contracted "data base." I have no quarrel with that, provided that her only aim is refutation; in other words, I accept that she can (and should) challenge those she challenges and refute their model, as long as she does not claim that her own "interpretation" will be valid. To advance such a superior model would involve a vastly greater effort, and a logical and epistemic framework that I will not attempt to describe here, except in the most general terms.

The question of what is and what is not included in her list of ca. 125 specimens is puzzling. There are many glaring absences and inconsistencies, and there are specimens that are almost devoid of an indication that they represent human figures, let alone females. Dobres includes finds such as Willendorf II, Avdeevo No. 4, and other items that are barely acceptable as depicting humans, but she leaves out many such finds that are at least as distinctly anthropomorphous (e.g., the two carved Avdeevo mammoth phalanges, the Maininskaya clay figurine). Thus there is no consistency in her formal demarcation, the sample is chosen quite haphazardly.

The same applies to her chronological demarcation. Although she places the "Venus" tradition from approximately 29,000 to 23,000 years BP, she includes numerous specimens from much younger as well as older deposits, such as the Mal'ta figurines (Boriskovski 1984: 358), those from Avdeevo (radiocarbon dates range from 22,700 to 12,000 BP), or the Hohlenstein / Stadel carving. Indeed, if she reviewed this aspect rigorously she would discover that less than 35 % of her exhibits can be safely attributed to the time she stipulates. The remainder is essentially undated, or dated to a different time. Very few of her exhibits from western Europe have a credible stratigraphic context, and their assignment to the Gravettian period is often subjective and uncertain. Dobres, having conducted no primary research of the subject, would assume for example that the Willendorf I figurine is safely dated, because this is the impression she would gain from the cursory reports of others, and from the continuing myths spawned by them. In fact the object might be from any of the nine strata reported from the site (consider the controversies concerning its finding and recording, the correction in J. Szombathy's excavation log, and the much later disputes among the three codirectors of the excavation [Bayer 1927]; also that some authors place it in the Aurignacian). Willendorf II is securely provenanced (Eppel 1950), but what bearing should that have on the totally different No. I figurine? The type of gymnastics in logic involved are the basis of much of the "knowledge" we possess about this entire corpus. The situation is considerably better in Russia, admittedly, but there the formal-stylistic consistencies are also much greater, and this is also where Dobres' lacunae of knowledge are greatest.

In short, Dobres' sample has no chronological basis, it is not limited to female depictions, and its formal selection is largely random, but statistically irrelevant: the sample is neither complete, nor representative, nor is it even clear what it is intended to be representative of, except an a priori assumption that there were some uses of female imagery. At this stage it is already clear that she has not in any sense succeeded in meeting the first part of her own criteria, relative control of data in time and space. Her specimens may have been taken from most phases of the Upper Palaeolithic. How well does she control for the spatial dimension?

Spatial and Iconographic Definition of the Sample

Her "phenomenon" has "an extremely broad geographic distribution," she admits (Dobres 1992: 245), noting that it occurs from the Pyrenees to Siberia. Geographic distribution even of a "real" phenomenon (rather than an imaginary one resulting from archaeological, i.e., subjective, taxonomies) is only meaningful if taphonomically tempered. Such a distribution indicates not the extent of a phenomenon, but the extent of the area in which the phenomenon's surviving attributes have been able to survive *and have been reported.* If archaeology is to discontinue its time-honoured practice of creating myths about the past it will have to come to grips with its taphonomic illiteracy (Bednarik 1992*a*, 1994*a*, 1995*a*, 1995*b*). In the present case it means that we need to ask: what characteristics of the phenomenon and its environmental preservation conditions are likely to have contributed to its survival, and how would they have distorted the quantifiable variables of the remnant sample? To tackle this question we need to know much more about the subject than we are likely to learn from the accounts of androcentric European writers whose grasp of archaeological complexity is probably more inadequate than Dobres herself realizes.

Among Dobres' 125 exhibits are only eleven specimens of haematite, fired clay, and steatite (the Buret' specimen she lists as serpentine is in my view pale-green steatite, which is chemically and petrographically similar to serpentine). They are the only ones that could be expected to survive under most preservation conditions. The vast majority of the finds she considers to represent a class warranting collective analysis is of organically derived, mineralized calcareous materials dominated by carbonates (Bednarik 1992*b*), many consist almost entirely of carbonates. They are of limestone (which chalk is also, and which consists chiefly of calcite), ivory (of dentine, other calcium minerals, and cartilage), bone (survives chiefly as calcium phosphates and carbonates), and antler (keratin and carbonates). All of them have been excavated either in loess deposits, the high-pH chemical regime of which is dominated by carbonates (calcite and dolomite, see Bednarik 1994*b*), or in the high-pH sediments of limestone caves. Thus the figurines have been found in the soil conditions most conducive to their survival, and none have

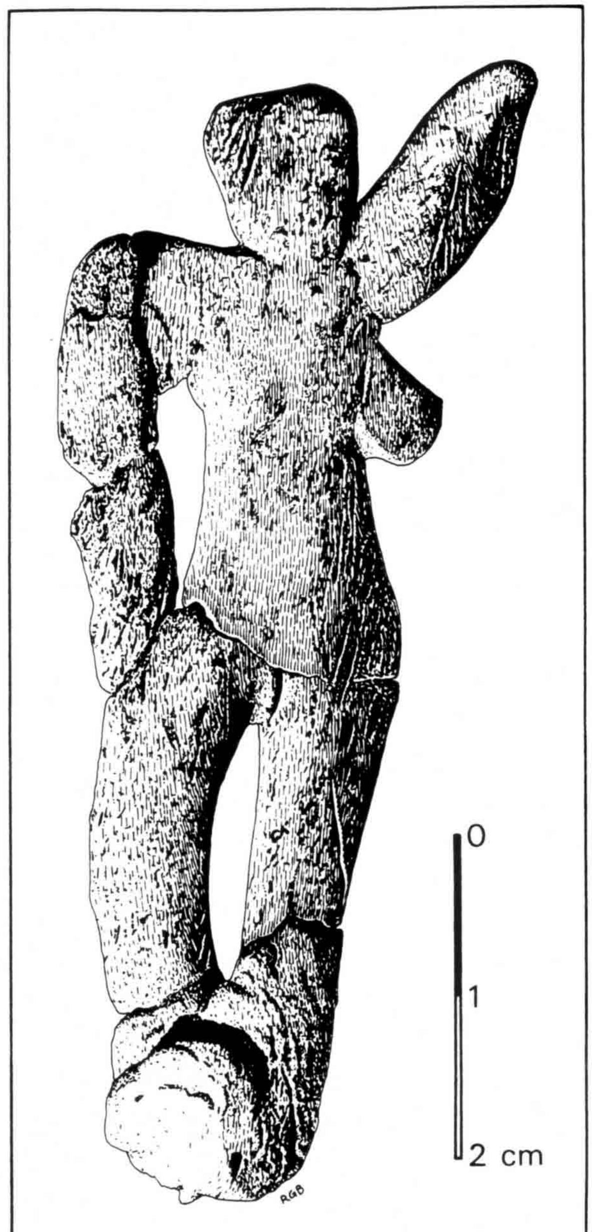

Fig. 1: Small serpentine figurine, Galgenberg near Krems, Lower Austria, 72 mm tall. The Aurignacoid occupation layer it comes from yielded six radiocarbon dates, the one nearest to the sculpture is 31,790 ± 280 years BP (Bednarik 1989).

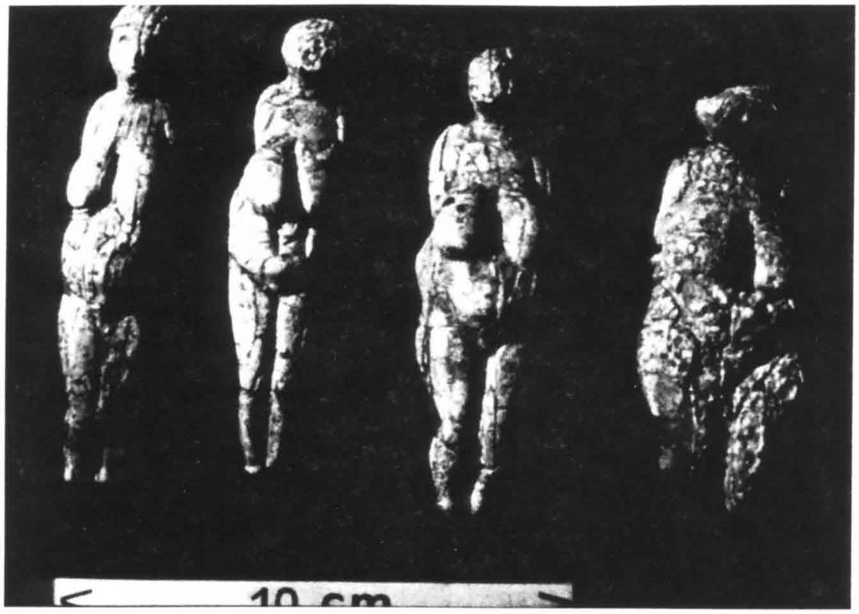

Fig. 2: Four ivory figurines from Avdeevo, Rogozna River, near Kursk, Russia. Middle to late Upper Palaeolithic, mammoth ivory.

been found in soils of detrimental characteristics. To then say that the geographical distribution of this *surviving* sample is equivalent to the figurines' original extent is to imply that they were only deposited in sediments of favourable conditions, i.e., intentionally, which is entirely illogical. This is the type of absurd logic *most* archaeological interpretations have been based on! Clearly, the taphonomically sound deduction (and this is only a significantly simplified version, I emphasize!) is that the figurines occurred also outside the regions to which they are now restricted, but did not survive there. Others also made did not survive at all (e.g., those possibly made of wood). On this basis it is unacceptable to claim that we know the former extent of the "tradition," a tradition which may in any case be the result of circular argument of archaeologists (Bednarik 1990).

Dobres includes some but not all known Palaeolithic anthropomorphous figures from five regions (in France, Italy, central Europe, Russia, and central Siberia), but none from outside these areas (Bednarik 1994*c*). She does not explain why she omits many of the figurines from the five regions she considers, nor why she includes some of the older or younger figures, but not others. She has clearly not controlled her sample in either time or space. For instance, she lists the Hohlenstein / Stadel figurine, which may well be female now that it has been reassembled (Schmid 1989), but which is a therianthrope and thus differs substantially from her other 124 items. But she omits the Galgenberg serpentine figurine (Bednarik 1989), which is typically female and very well executed (Fig. 1), and of the same age as the Hohlenstein figure, around 32,000 years. Several of her site assemblages are incomplete. For instance, she lists only four ivory figures from Avdeevo, and her Table 2 suggests that these are Avdeevo Nos. 1–4. I have examined, described, and photographed a substantially larger assemblage from Avdeevo: seven fairly complete and clearly female figures (she includes none of those in my Fig. 2), four anthropomorphs lacking any indication of sex, and seven incomplete female figures. What is the value of considering only 22 % of a site's known and relevant assemblage in what is purported to be an analysis? The Kostenki I sample is almost certainly also biased, although the vagueness of Dobres' tables does not permit assessment. But she does list six female figures, whereas I have studied ten clearly so identifiable figures, which are either complete, or at least of a complete female torso (Bednarik 1990, Fig. 1).

But these shortcomings are only attributable to inadequate familiarity with the material. More serious are Dobres' endeavours to do precisely what she most criticizes in others: "seeking the female form" in Palaeolithic anthropomorphs. For instance, she lists "ca. 29" figurines from Mal'ta (such quantitative approximation should be a warning signal), and defines "ca. 18" as females. I have examined 26 anthropomorphs from that site, most of which lack breasts (Fig. 3). Only one, Mal'ta No. 5, bears a suggested vulvar cleft. Some figurines, particularly Mal'ta No. 4, could

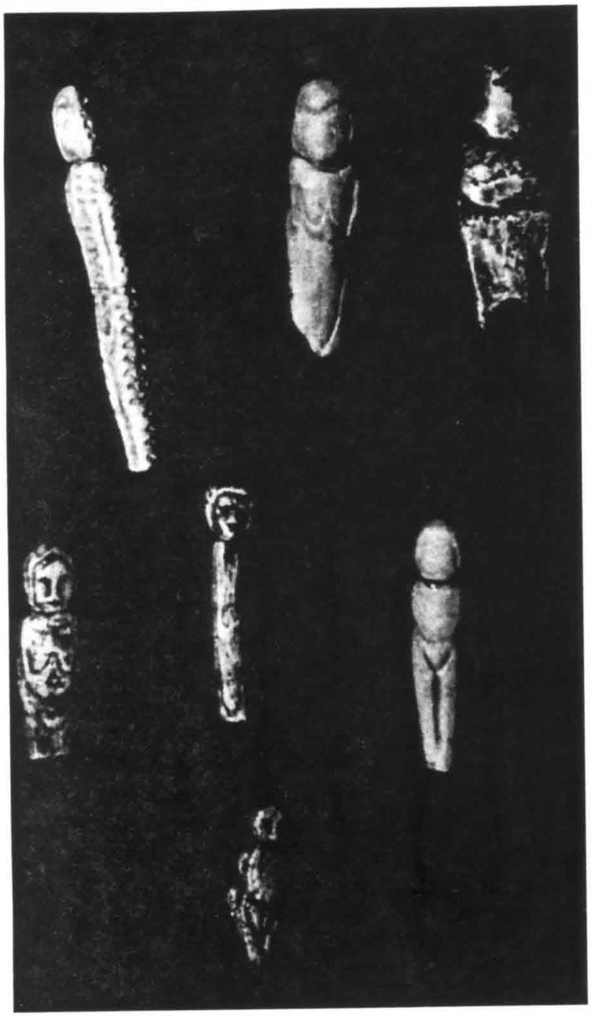

Fig. 3: A few of the anthropomorphous ivory figurines from Mal'ta, Belaya River, north of Irkutsk, Siberia, of a late Upper Palaeolithic culture.

just as easily be phallic objects. Depending on one's iconographic rigour, between 8 and 14 of the Mal'ta and Buret' figurines can be assigned a female gender, and even then it must be cautioned that this would be on the basis of assessing the iconicity of an alien artistic system, which is an unscientific procedure and should never be presented as anything but a working hypothesis.

The same applies to Dobres' overall finding, that 47 % of her sample is of female depictions, which, ironically, is offered as an argument that the incidence of female depiction in this corpus has been over-emphasized. From my examination (which I would not call an analysis) of the limited material she considers I would say that even less than that percentage can be defined as female depictions. The "reliable" minimum percentages of the main sites (Kostenki I, Mal'ta, Brassempouy, Dolní Vestonice) range from 13 %-40 %. This *opinion* (for that is what it is, and it should not be cited as a scientific finding) provides even better support for her feminist-inspired ideas, and yet it is not feminist inspired. Perhaps it does provide some indication that "reanalyses" of data should not be politically inspired, but inspired by a desire for accuracy and scientific reliability.

Discussion

This is not to say that Dobres is entirely wrong. She mentions several valid points: the objectification of the female body by men in pre-History has not been demonstrated, and the stylistic homogeneity of these figurines, as purported by Gamble and even Delporte, is almost certainly invalid. Her illustration of how objects are iconographically identified when we do not even know which way is up certainly does hit the mark, as does her observation that the Brassempouy head provides no discernible sexual characteristics (if, indeed, it is authentic; Bahn 1993). However, there are also many weaknesses of argument in her paper. For instance, her notion that the time and energy invested in an artefact as the primary basis of assigning value is a capitalist principle suggests that she lacks the knowledge of social contexts in hunter / forager / fisher societies necessary to make such deductions. It comes across as a rather over-economical political slogan. Traditional societies, such as that of the Australian Aborigines, place great value on the efforts of artisans (consider burial posts in Arnhem Land, which involve many days of work under the actively critical supervision of elders from neighbouring clans), while the capitalist "time is money" attitude is much more likely to lead to economizing in the production of cultural commodities.

Dobres' central argument is that the "meaning" of the "Venus" figurines cannot be located by "fixating on their depictions of female biology, even if female biology is of central import in defining Woman in the present." She suggests that a materialist orientation, reconceptualized by feminist doctrine, can help us in interpreting the figurines. Her desire to replace androcentric, heterocentric, Eurocentric mythology is most laudable, it certainly has my support. But her attempt to replace it with another mythology, predicated on gynocentric, feminist-inspired interpretation of selective or distorted data extracted entirely from that Eurocentric mythology is not likely to result in heuristic improvements. Others have already examined Palaeolithic female imagery by more objective criteria (e.g., Rosenfeld 1977; Bahn 1986; Bednarik 1989, 1990; Russell 1990), and by ignoring such attempts completely, Dobres does not further the feminist cause. Instead she cites a massive number of feminist publications, all of which are totally unrelated to the subject in question. It is not the role of science to promote the cause of any political movement, but it is part of science's responsibility to guard against the biases such movements inevitably involve. While it is absolutely essential to expose any politically inspired interpretation of the past, it is equally essential to reject the *politically inspired* reorientation Dobres advocates. A scientific reanalysis of the question of female depiction in the Palaeolithic is desirable, and it can be achieved by proceeding along the lines I have suggested in the past. To begin with we need to recognize that this material has never before been analyzed, in a scientific sense, hence it cannot be "reanalyzed." A valid attempt at analysis would have to commence by asking: how do we define the sample without anticipating what we are going to find? The obvious answer is that such a definition can never be entirely free of theory (Bednarik 1990). Hence it is essential to guard against such biases to our best ability, rather than start from a political premiss. After all, even if we were prepared to use as our primary data base all anthropomorphous depictions of the Upper Palaeolithic we would face significant qualifications:

1. The Upper Palaeolithic is not a historical period, it is an archaeological pigeonhole and as such facilitates a biased conceptualization of the past. For instance, it prompts us to place two objects into the same pigeonhole even if they are 6000 km and 20,000 years apart, while placing two similar objects from the same site into different pigeonholes if they are just a few millennia apart, one being outside the Palaeolithic. Female figurines, after all, occur in many pre-Historic contexts, not just in the Upper Palaeolithic, or the Gravettian.

2. We can only consider those specimens that have met certain preconditions: they must have survived, they must have been found, they must have been reported, they must have been recognized as belonging to our chronological pigeonhole. Not only does this limit the sample most severely, it introduces many systematic biases, such as those of primary taphonomy (Bednarik 1994*a*) and metamorphology (Bednarik 1995*a*, 1995*b*).

3. In order to be certain that we are only considering items depicting humans, our iconographic identifications must be correct. If it is our aim to consider only female figures, the same applies, but how would we proceed here if not by the subjective identification of specific parts of female physique? This introduces subjectivity into sample definition.

4. If it is our purpose to consider the female depictions of just one period, we need to know the ages of *all* Palaeolithic female images – which renders our project practically impossible; we have already noted the difficulties of deciding what is and what is not a female image. Similarly, we need to define the geographical area or areas we wish to consider. At this stage, an attempt to isolate and analyze the "Gravettian" "female" figurines is clearly doomed, not only because of the limitations just listed, but because the preconditions of taphonomic logic seem impossible to meet.

5. We have not yet considered whether we will include only full sculptures, relief images (such as those of Laussel, which Dobres does include), or also engraved or painted figures. If we were to say, for instance, that there are no engraved Gravettian females, we would first need to know the ages of all engraved female motifs of the Palaeolithic.

Clearly, at this stage it has become apparent that the task of objectively determining that there is a group of female figurines which belong to a single art tradition and which are so similar that a specific cultural role may be postulated has become entirely futile, on the basis of currently available and secure data. And yet, demonstrating that there is a homogeneous corpus would not help us at all in establishing its meaning and function, it would merely be a precondition to such an attempt.

Summary

None of what I have said is intended to refute that there were Palaeolithic traditions of female imagery. But the sample usually cited in this context is most unlikely to represent a homogeneous tradition. Some figures from different artistic as well as technological traditions have been lumped together, for no obvious reasons except the biases of the commentators, as Dobres correctly notes. The figurines are of greatly differing formal attributes, antiquities, cultural affiliations, geographic proveniences, and materials, and well over half of them provide no indication at all that they depict females. Moreover, there are many more figures and carvings which could have equally well been included in the sample, as they do meet the same broad criteria. To see how incongruous this sample is, and what far more plausible interpretations of it are possible, one might consider the Siberian and Russian samples in isolation. What do the two have in common, other than being apparent depictions of humans? Nothing at all! Most of the Mal'ta and Buret' figurines are surprisingly homogeneous in form, slim and without clear sexual characteristics. Most are small and were probably worn suspended on a string, with the head pointing down. In what way can they be compared to the largest two Russian samples (Kostenki I and Avdeevo), of distinctly female figures, which include components of a homogeneity that almost suggests some sort of serial production (M. D. Gvozdover, pers. comm.)? To lump these two corpora together seems entirely pointless – especially as they are of a large range of ages.

The actual meaning or purpose of this material could not be established even by a vastly more sophisticated "analysis" than Dobres'. To illustrate the point, let us imagine that intergalactic archaeologists were investigating our own culture. Having found some strangely similar, but also dissimilar horse figurines, some being of ivory, some of plastic, they would wonder why there are black duplicates of the white plastic ones, but none of the ivory ones. They would note the different sizes and styles, the wide distribution, the apparent sexual dimorphism indicated by the black and white figures. They would certainly explain them in terms of their perception of the world (as religious or ceremonial artefacts, if their thought patterns were as primitive as ours). How are they to know that they are looking at a few isolated knights from different chess sets? I am not suggesting that the Palaeolithic figurines were parts of games, I am merely illustrating how futile it is to speculate about their cultural context, social role, or ritual significance. At the taphonomic level, there are numerous explanations possible which would be just as likely valid as the one preferred by Eurocentric, archaeocentric researchers. For instance, if there were cultural conventions which demanded that male figurines are to be produced from wood or some other perishable material, it would be utterly pointless to speculate about the meaning of the surviving female figurines, and all the learned arguments we have about them would be ludicrous. We have so many documented examples of the futility of specialist interpretations in rock art studies (Macintosh 1977), and they should show us how worthless the many interpretation attempts of Palaeolithic art are likely to be. Instead of accepting this lesson from rock art research, archaeological commentators continue in the same old Eurocentric vein, including Dobres' androcentrically based feminist "reanalysis." It may tell us about the impotence of this revisionist brand of feminism, but it tells us nothing about the cultural traditions of the past. Naive confirmationist models are only upheld because they are not refutable, having been postulated in nonrefutable terms. It is high time they were all banished from scientific palaeoart studies, and replaced with epistemologically sound propositions and practices. These are realistically possible, they have been proposed, but they attract little interest from archaeologists, while the results of Van Däniken-style explanations continue to steal the lime light in this discipline: they are what the public likes, we are given to understand! The public, I would argue, is more intelligent than archaeologists give it credit for, and it might just survive the revelation that a great deal of what it has been told about the human past (including our "love goddesses" of the Stone Age) is of no more value than the tales of snake oil salespersons. And that applies equally to the androcentric, heterocentric, and gynocentric versions.

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