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LA GESTUELLE DU MEMBRE SUPÉRIEUR DANS LES FIGURATIONS FÉMININES SCULPTÉES PALÉOLITHIQUES

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FURTHER COMMENT

More to Palaeolithic females than meets the eye
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Duhard's paper presents a comparison of a number of carved anthropomorphs which have two things in common: they have been claimed to be of Upper Palaeolithic age (it should be noted that most of those from western and central Europe lack a secure stratigraphic context), and they have been claimed to be female. While both assumptions are probably correct in most cases, there is still room for scepticism. Most particularly, in subjecting such a diverse class of artefacts to statistical treatment (cf. similar attempts, such as Rice 1981) we are postulating that they belong to a single tradition. I would like to examine the validity of that assumption.

The female sculptures of the Upper Palaeolithic number around 140 or so, but they span a period of up to 22 000 years during which they were produced by people of diverse material cultures, from the Atlantic to Lake Baïkal. Collective statistical treatment to determine their purpose or meaning might be useful if these sculptures could be shown to share further characteristics, but such common features seem to be profoundly lacking: many of the figurines appear to be pregnant, but the majority are probably not; some are certainly obese, but most are not; a few are steatopygous, but not the remainder; large breasts may appear to be the hall mark of many, but they are not on the majority – in fact a substantial proportion lack breasts altogether; very few have the vulva indicated, and many of these statuettes provide no clear proof at all of their sex. We have even one case, the Hohlenstein-Stadel therianthrope (*RAR* 7[1], back cover), in which an ivory sculpture had been described for 50 years as a male, by dozens of authors, while the result of a recent remodelling attempt suggests that this '*Sinnbild für Kraft und Aggression*' (Hahn 1986) is in fact a female. (Which is not at all embarrassing for those of us who have been careful enough in the past to refer to the specimen without attributing any sex to it.) This claim confirms an all too familiar pattern: most interpretative knowledge claims in archaeology are unreliable, and our own discipline (Bednarik 1990a, b; Odak 1991) faces the mammoth task of distilling from the 'accepted fiction' of archaeologists (Bahn 1990: 75) those claims that stand up to scientific scrutiny.

Many of the Upper Palaeolithic female figurines are depicted naked or almost naked, but there are also enough that have been suggested to be fully clothed. Quite a number of the Russian examples are thought to have been smashed intentionally (I confirm that I have observed impact fractures on both limestone and ivory specimens) but this practice has been restricted to one particular type (which *could* infer a specific cultural function). The figurines are most frequently of mammoth ivory, but several other materials are also represented. Many are of a typical style with splayed lower legs, others possess a pedestal or plinth. Then again, many are either pointed on the lower end or have a peg-like extension there, prompting the suggestion that they were perhaps planted in the floor of dwellings – which suits those yearning for a 'mother goddess'. But it is even clearer that many others served as pendants and are therefore rather small. Indeed, in size alone these 'Venuses' are of considerable diversity, ranging from the massive limestone specimen from Kostenki I, which must have weighed several kilograms when it was complete (Fig. 1–f), to the minute Buret' steatite specimen, No. 5, of a few grams (Fig. 2–c). Even the apparently most common characteristic of these female anthropomorphs, that of standing erect, is not universal, there are at least 6 specimens of very different poses – and all perforated ones were suspended head down.

Duhard has added one more to the long list of inconsistencies among the female figurines: in a few specimens the hands rest on the abdomen, in a few others he feels they direct attention to either the breasts or the abdominal region, and in a greater number the hands seem to be clasped immediately under the breasts (although Duhard does not treat these as a separate group). Nevertheless, in the majority of the figures the arms are either lacking altogether, or are not associated with the lower torso.

The two common characteristics these figurines do possess are certainly not adequate to treat them as representatives of a single tradition, which becomes even more apparent when we observe the distinct clustering of attributes that is evident in some of the geographical regions. Among the 14 Russian specimens Duhard considers, he finds that 6 possess arm postures which may be intended to draw attention to the abdomen. I believe that there is a more distinctive convention of hand positioning evident among the Russian figures. In the most common style at Kostenki I and Avdeevo, which occurs in only 1 of Duhard's Russian figures (Kostenki No. 1), the arms are tucked under the breasts, and the hands appear immediately below them, in a vertical position. The lower arms are thus presumably concealed by the breasts, which results in a quite unnatural, pendant attitude of the hands. The fingers are sometimes faintly sketched in low relief and wrist bracelets are in several instances clearly shown. The bracelet on the largest of the Kostenki limestone statues (none of these are considered by Duhard; see Fig 1) repeats the pattern on one of the two incomplete Avdeevo torsos (which has not been published, not even in Russian) and resembles the bracelets on both wrists of the Willendorf I

Figure 1. One female figurine (b) with lower legs separated, a broken off similar lower leg (c), and four female torsos. Note detailed belt and bracelet on (f). All specimens of limestone, except (a) which is of ivory. Kostenki, U.S.S.R. Photograph by permission of Leningrad Institute of Archaeology.

figurine from Austria (also of limestone, but with hands above breasts). I have observed this distinctive hands-under-breasts position on 10 Russian figurines. On others, the arms seem to disappear under the breasts without reappearing below (e.g. Gagarino No. 3), or the hands are clasped immediately below the breasts. Indeed, the only Russian figurines whose hands actually rest on the lower abdomen seem to be Kostenki No. 4 and Avdeevo No. 2. I consider that details such as belts, girdles, and possible cicatrices or other decorative elements may have been of more importance to the artists or their clients, than the symbolism of hand positioning.

Of the 7 complete figurines I have examined from Avdeevo, Duhard lists only 2, and he considers none of the incomplete specimens even though some of them can provide the information required for his comparison. Almost none of the many figures he omits from his study provides any support for his premises. Since one of the unpublished specimens from Avdeevo challenges almost every one of the interpretation attempts of the Palaeolithic female figurines, it would be judicious to refrain from generating further hypotheses about them for the time being, and to take more note of the ideas of leading Russian scholars such as M. Gvozdover and N. Praslov.

The implausibility of a universal purpose or meaning becomes even more conspicuous when we examine the 31 anthropomorphous iconic sculptures from Siberia, which are very distinctive and in my view share no common artistic tradition with most of Duhards's other specimens (Bednarik 1990c). Not a single one among them offers any indication of abdominal enlargement, their sex is far from clearly evident on many specimens, a vulvar cleft is suggested only on Mal'ta No. 5, breasts are often lacking or only vaguely indicated (conversely, even a male statuette from Brno has small breasts!). At least 42% of these figures show facial details, in which they differ significantly from the western figurines, and they are of considerably smaller sizes. They were certainly used for purposes quite different from most of those of Russia, central Europe or south-western Europe. Many of them probably served as pendants. No less than 8 of those from Mal'ta actually bear perforations on one end: No's 2, 3, 5, 8, 10, 13, 16 and 23. In other specimens a string may have been attached by different means. Why were all perforated specimens intended to hang with the head downwards? (Has the suggestion that the upside-down position of all perforated female figurines is related to function ever been debated in the English or French literature?)

Far more important than comparing the Mal'ta figures with those from other regions and periods (which can superimpose only the interpreter's bias on the evidence, or present it in a subjective framework) is it to see them within their technological, cultural and artistic context, which after all provides a reasonably factual rather than an imagined context. Of the entire Mal'ta corpus of portable art, 76.6% is perforated. This includes the 13 'flying bird' pendants (No's 1-9, 11, 13, 16 and 17); the full-sculpture 'birds', the plaque bearing the mammoth engraving (Bednarik 1990d) and several other items, all of which are perforated at one end, as well as another 126, centrally perforated items, ranging from the large rectangular ivory plaque to the discs and beads. Additionally, several of the non-perforated objects were clearly intended for attachment to a string, and on others the perforated part may be missing. One of the 5 Buret' anthropomorphs is also perforated between the legs, and the solitary 'flying bird' and a disc from that site are perforated, as are the engraved pendant from Oshurkovo and 41 pieces from Afontova. I have microscopically examined many of the specimens from Mal'ta (and many other sites), and

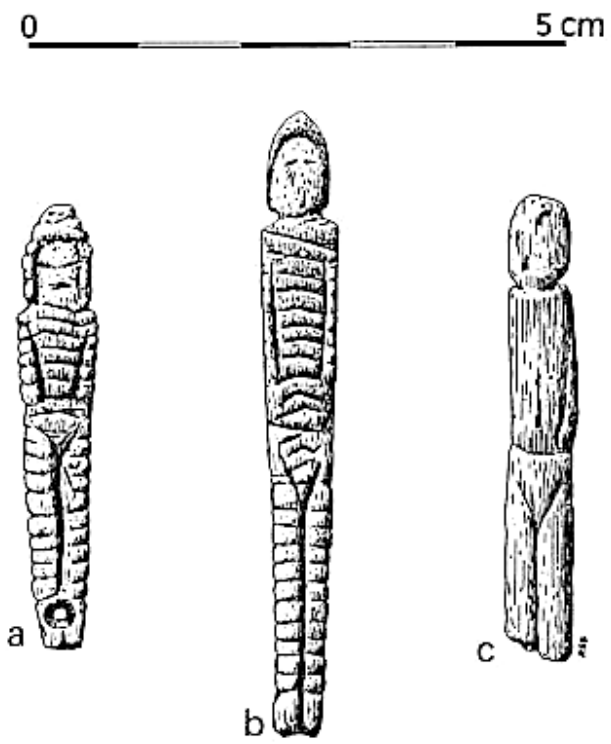

Figure 2. Small Palaeolithic figurines from Siberia: Mal'ta No. 13 (a); Mal'ta No. 27 (b); Buret' No. 5 (c, actual length = 43.3 mm).

detected evidence confirming that these were suspended from a string. The side of the perforations which is furthest from the sculpture's centre of gravity often shows distinctive wear polish, although it is usually not deep. For instance, I have tried to estimate how long the long-necked bird pendant No. 15 (the legs of which form an eyelet that is not apparent in the published illustrations I am aware of) may have been used, speculating that it might have been worn continuously for 6–12 months; in the absence of comparative data or replicative experiments this is of course most tentative, but it does establish clearly the form in which this and other objects were used.

Most portable Siberian art objects of the Palaeolithic (probably over 80% of them) were in some way suspended from strings, and this mode of use is reflected in many of the anthropomorphous figurines. This contrasts sharply with the female figurines of Europe, which provide almost no evidence of having been used as pendants (Sireuil being the exception) while offering direct and indirect evidence to the contrary: some are plainly too large, others were fashioned from fragile and soft Tertiary limestone which is not suitable, and alternative modes of use have been either postulated or demonstrated.

Turning to the question of arm depiction, I find that 11 of the 31 Siberian anthropomorphs (excluding here Krasnyi Yar) lack arms entirely, while on 6 of them they are depicted more or less vertically; on 6 the hands appear to be clasped immediately below the breasts; and on 8 the hands seem to be resting on the abdomen. Collectively one could describe these poses as fairly naturalistic, and before we see any special significance in the hand postures of the last group, we would do well to remember that none of the Siberian figures appears to be of a pregnant woman. Yet it is obvious from Duhard's Tables 1 and 2 that his hypothesis concerning the significance of arm attitudes derives its support largely from the Siberian sample: without it the frequency of such attitudes would be so low that no significance could be attached to it. This only reinforces the idea that this regionally (and probably chronologically) homogeneous assemblage is not culturally connected to any of the other groups within this corpus.

In my view Duhard has synthesised two ideas without realising that the two conventions they seek to define occur largely in two different parts of the material: there is almost no overlap between the apparent depiction of pregnancy and depiction of hands in the abdominal region. It is therefore not appropriate to relate the interpretation of either group to the other.

My impressions of the entire assemblage are probably no less subjective than those of previous writers. My conclusions are rather different from Duhard's. If some special significance had been attributed to arm positioning, surely there would not be such a large number of figures that lack arms. Moreover, when arms *are* depicted they are often vaguely defined or stunted, and their position is ambiguous: they are tucked away under the breasts, they 'fade out' towards the distal end, and I see them as having 'less to do with stylistic conventions and more with technological aspects or conventions of production' (Bednarik 1989: 121–2). The free sculpting of limbs presents significant technological challenges and renders the figures far more fragile, and I see this as the main reason for the manner in which limbs, especially arms, are rendered. The form of ivory statuettes is often determined by the shape of the raw material (the mammoth tusk), and that of limestone figures by the stone's frailty. This is confirmed by the 6 bas-relief figures from Laussel and La Magdelaine, and many engravings in which the depiction of the arms has been freed from technological restrictions; as well as by the Hohlenstein-Stadel and Galgenberg figurines which belong to a technologically more competent but also much earlier art tradition (Bednarik 1989). With the exception of Gagarino No. 2, in which just the distal ends of the arms are sculpted free, all post-30 000 BP Palaeolithic anthropomorphous sculptures in the world lack free arms; and with the exception of the Laugerie-Basse specimen with its fully separated legs, and the several figurines in which only the lower part of the legs is separated (most of which are from a single site), the lower limbs are modelled close together. That the limbs of bas-relief, engraved or painted human figures of the same period are shown extended in a variety of postures can mean either that they are not culturally related to the sculptures, or that the differences are attributable to material, technique and/or purpose. On the other hand, the dated pre-30 000 BP anthropomorphous figures (other than proto-sculpture) have fully separate arms and legs. Finally, if arms are to be depicted close to the body, the range of anatomically possible postures is quite limited and, with the exception of the 'hands clasped on the back' alternative, they are found among the figurines. Again it would appear to be unnecessary to attribute any meaning to such arm positions.

The attempts to interpret this art body (e.g. Darasse 1956; Feustel 1971; Gamble 1982; Graziosi 1956; von Koenigswald 1964; Menghin 1931; Schelsky 1964; Tokarev 1961) usually treat it as representing a single tradition, yet none of them provides an interpretation that is applicable to all of the figures. Does this not suggest that a more sophisticated process of differentiating between the contributing art traditions is called for? And if the purpose of the

exercise is to compare all supposed Palaeolithic female figurines, why stop at Irkutsk, why not include the Pleistocene engraved pebbles from the cave of Kamikuroiwa, Japan (Aikens and Higuchi 1982)? They present more female characteristics than, say, Mal'ta No's 3, 11, 13 or 27, or Buret' No. 5 (Mal'ta No. 27 was omitted by Duhard, although it is almost identical to two of the Siberian figures he lists, see Fig. 2). Strictly speaking, even the Belan valley 'mother goddess' (Misra 1977: 49; Sankalia 1978: 8; Sharma 1975) of India could have been included here, it is, after all, of the Upper Palaeolithic (but cf. Bednarik 1990c regarding its interpretation).

A representative sample should also include the dozens of Magdalenian sculptures from Gönnersdorf, Ölknitz, Petersfels, Mauern, Nebra, Trasimeno, Wandersleben and Pekarna (Bosinski 1979; Rosenfeld 1977; Toepfer 1965; Zotz 1955), the stylistically similar figurine from Krasnyi Yar, Siberia (Abramova 1962: Pl. 56), the several specimens from Mezin (Abramova 1962: Pls 31–33), and earlier figures such as Willendorf II (Felgenhauer 1959). One might even include all the many female figures in the engravings from Gönnersdorf (Bosinski 1970), Grotte de Lalinde, Abri de Fontales and Gare de Couze (Bordes, Fitte and Laurent 1963), Les Combarelles (Leroi-Gourhan 1965), Limeuil (Graziosi 1956) and Hohlenstein-Nördlingen (Narr 1965). The sample could in fact be extended in several directions (e.g. geographically, to other techniques, chronologically, to ambiguous specimens, to incomplete specimens etc.), and it is obvious that in all cases the demarcation between those specimens we would admit and those we would reject will always be artificial, arbitrary and entirely subjective. At which point do we accept a figure as depicting a female, or, for that matter, a human being? (Upon inspecting the 'sorcerer' of Trois Frères with its prominent penis, an American feminist archaeologist recently proposed that it might be a female disguised as a male! Pers. comm. P. G. Bahn.) How are we to treat the presumed hermaphrodites, of which there are several in Palaeolithic art? At what point do we separate portable from non-portable art? Should we impose geographical restrictions for a statistical analysis such as Duhard's, and if so, on the basis of what criteria? The arbitrary separation of three-dimensional specimens from graphic art would present another opportunity for subjectivity, but here Duhard has already admitted bas-relief specimens. At what point do we reject figures as being too incomplete to be statistically relevant (bearing always in mind that it is the 'objective scientist', and not the artist, who decides what is and what is not diagnostic!) and, perhaps most importantly, at what point do we reject figures as being 'too stylised' to permit what is, after all, quite intentionally an iconically based decision? (If the 'most abstracted' specimens embodied the most diagnostic characteristics, which seems likely, the arms would have no significance at all!) These questions demonstrate to us in no uncertain terms that all these arbitrary decisions which shape the final sample are governed by our value judgments, bias, knowledge, ethnicity, social prejudices etc., and cannot be related to the intrinsic qualities of the so-called data.

To make matters worse, the paradoxical idea of 'objective data' is a self-contradiction: there can be no such thing as objective data in a humanly perceived world. We tend to gloss over this significant human shortcoming in scientific discourse – for obvious reasons. The treatment of the 'Venus figurines' is a good example with which to illustrate my model of the human reality 'created' through the establishment of what I call 'crucial common denominators of phenomenon categories' (Bednarik 1990e, in press). One may object that lofty and esoteric philosophical ideals cannot be readily translated into the harsh realities of research and hypothesis-building demands. But can we afford to ignore valid epistemic concepts when they appear to be incompatible with the pragmatic dynamics of a discipline, can we pretend that they are not relevant? Let us take a closer look at our Palaeolithic ladies.

There is a vast body of anthropomorphous figures in prehistoric art (for most contemporary humans, an anthropomorphous figure is one that resembles a human being iconographically, but some of these figures may actually depict seals, lizards or clouds, or they may represent a deity, the name of a Gravettian dentist or personified Spring). These figures occur as paintings, petroglyphs, bas-reliefs and sculptures. We separate them from all other objects on the basis of what we believe is our intuition, our ability to discern iconicity, but what in reality is our culturally determined ability to recognise the visual clues provided by the artists of an alien culture. (What could possibly be 'scientific' in such a procedure?) Next we isolate those specimens that occur within a given geographical region, from which we select the ones we believe to be of Upper Palaeolithic age. At this stage of our relentless abstracting, the 'objective data' have been so much manipulated that their scientific utility and integrity have been seriously compromised. What objective criteria could there be for postulating that the stylistic or cultural affinity of a 30 000-year old figure from France to one of 12 000 years from Siberia should be closer than that of the latter to a Neolithic figure from the same region? While in the first case the two specimens will be retained in the sample even though they are 18 000 years and 6000 km apart, in the second case one will be eliminated even though it is from the same location and only 6000 years younger! (Similar female figurines occur after all in many post-Palaeolithic cultures.)

Much more manipulation of the sample (subjecting it to 'crucial common denominators' under the guise of 'science') is to occur before it is ready for statistical assessment. Next, we deduct all the paintings and petroglyphs from it, because we have decided that sculpted art is more likely to be 'purpose specific', particularly as we perceive certain differences between the art forms: for example in the way the arms are rendered. This is a crucial error because the differences we perceive could well be attributable to some factor other than meaning or purpose, e.g. the arm position may be a function of technological restraints rather than cultural determinants. So to check whether the arms of female figurines were *intended* to be in certain attitudes we should really consult those figures in which technological restraints differ, i.e. the paintings and petroglyphs we have dismissed as irrelevant.

One look at the Magdalenian female figures is sufficient to see that heads, arms and lower legs are conspicuously absent, while aspects of the torso, such as its profile, buttocks and breasts, are emphasised. But it would be equally wrong to jump to the conclusion that the artists or consumers of this art were preoccupied with these parts, or that these communicated some simplistic 'meaning'. We may speculate that marks on an Aboriginal painting represent cicatrices (Huchet 1990), but even this provides no access to the actual meaning which may be emblemic, for instance (Bednarik 1990f), depicting some attribute like rank. In abstracting an image, a certain minimum amount of visual information must be retained, but the level of minimum visual information required to understand a mark can be assumed to be signifi-

cantly lower for a participant in a culture, than for an 'objective' researcher from an alien culture. It would be unrealistic to hope that participants in prehistoric cultures *always* included far more visual information in their arts than *they* required to perceive meaning – sufficient even for *us* to gain access to cultural content. In practical terms this means that we cannot reliably attribute individual figures to specific artistic conventions, or *scientifically* detect an artistic convention in a sample of prehistoric art.

In determining our final sample we submit the remaining corpus of art to still more selective procedures, most of which are again unrelated to the meaning of the art: we exclude those specimens we have not seen, or heard or read about; we ignore the vast numbers that still remain in the ground, and the even greater numbers that were no doubt manufactured from perishable materials, as well as those that were destroyed intentionally. But worst of all, we exclude those specimens on which we cannot discern the very features that will in the final analysis 'reveal themselves' to us as being diagnostic.

Surely the results of a statistical analysis cannot be accepted if they were reached by excluding that part of the data that lacks the characteristics we subsequently describe as being statistically significant.

Despite disagreeing with Duhard on certain matters I find his innovative approaches – in this and other papers – always fascinating, and I value his assiduous work. One aspect of his publications which I admire particularly is that he usually presents his own recordings, drawn from life. One of the persistent problems in publications on Palaeolithic art is the frequent 'recycling' of earlier illustrations, so that in many cases we still have only the one published version. Duhard's drawings often add a 'new dimension' to the objects he writes about and this is in itself of great value – particularly as so many early recordings are believed to be unreliable (Bahn and Vertut 1988: 43–4; Clottes 1986).

Some of the standards I canvass in the present Comment may seem severe and impracticable, and I should emphasise that Duhard's hypothesis is clearly better argued and more authoritative than most others we have seen on these figurines, some of which are about as relevant as the findings of archaeologists from outer space that the black and white figures of an incomplete chess set represent a sexual dichotomy, or that its rooks, bishops and knights represent religious concepts. I have merely tried to illustrate how severely hypotheses – even apparently plausible hypotheses – can be tested. I would arrive at quite different conclusions than Duhard, even by considering just the 75 figures he lists. I would find that the Siberian specimens should not be lumped together with those from Europe, and that among his Russian figures only 2 can be interpreted as possessing what he describes as a '*gestuelle abdominale*'. The status of the Grimaldi figurine and the Laussel 'playing card figure' is so controversial that I would not even take up space here to review it. Thus the number of European figurines that could be construed to possess some form of patterning in the attitudes of their arms is not adequate to demonstrate the existence of such a convention. What Duhard's paper does reinforce, however, is that this much-discussed corpus of art may reflect four, and very possibly more, artistic and cultural traditions. My own examination of the evidence suggests that the objects were made for different purposes and that their 'meaning' is not accessible to simple statistical, or other empirical, analysis. Sophisticated multivariate analysis would probably confirm my views.

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Editor