

**"Das sehen wir auch den Rädern ab":  
some thoughts on M. Vosteen's "Unter die Räder gekommen"**

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*"In the Federal Republic, rather than ingenious theories providing a preconceived system to place finds into the contexts of events and interpret them, it was the observed evidence that proved the stronger in archaeological interpretation. Yet there were no criteria which could facilitate any such interpretation, and this dilemma is yet to be resolved."* (KOSSACK 1992, 102)

### Introduction

In its relatively short existence, the monograph-series *"Archäologische Berichte"* has established itself as a vehicle for the consideration of some important topics of discussion in Pre- and Proto-history: ploughmarks, prestige goods, the Neolithic outside Europe. The recently-published volume 7 (1996)<sup>1</sup> is particularly unusual, in that it is devoted to an extended consideration of a single article published some 15 years earlier, in a *Festschrift* - sadly, it was in fact a memorial volume - for the path-breaking English prehistorian, David CLARKE. The article, of which I was the author (SHERRATT 1981), was an attempt to make sense of the long sequence of prehistoric cultures in Europe between the introduction of farming and the emergence of urbanism: a period which radiocarbon dating was demonstrating to have lasted some 5.000 years.<sup>2</sup> Should all such communities be described just as "simple farmers", or were there some fundamental differences between the techniques of the first farmers and those of their successors in the later Neolithic and metal ages? It seemed to me that an important set of innovations could be identified, which were not part of the initial package of farming practices but which made their appearance about halfway through this long prehistoric sequence; and, moreover, that many of them represented introductions from outside Europe - and broadly from the "nuclear" area which had seen the beginnings of farming itself. Just as the initiation of farming constituted a *"Neolithic Revolution"*,<sup>3</sup> so this bundle of innovations could be thought of as a second generation of farming techniques.<sup>4</sup> Because their common characteristic was that

they involved the use of **living** animals (or, indeed, plants<sup>5</sup>), I chose the rather cumbersome label of *"secondary products<sup>6</sup> revolution"* to describe this process. It seemed a useful way of avoiding the uniformitarian assumption that farming was a single way of life that was always and everywhere the same,<sup>7</sup> and at the same time it provided a term to designate a major historical *conjoncture* (*sensu* BRAUDEL), since what I discerned was an impact on Europe from outside.<sup>8</sup>

It will be understood, then, that this conception was painted on a broad canvas. It was intended to apply to the whole of the Old World (though primarily to its western part, since Chinese culture is notable for its minimal reliance on ungulates and their products, and for its late reception of the wheel and the horse), and my account therefore considered Europe simply as one part of a cultural landscape which necessarily included both western Asia (the Near East) and the Pontic and Ural steppes. The title-phrase *"plough and pastoralism"* postulated a symmetrical transformation, on the one hand in the genesis of pastoralism on the steppes, and on the other hand in the transformation of hoe-based horticulture to plough-based agriculture in forested regions.<sup>9</sup> It was an essay in the spirit of Eduard HAHN (reflected in its opening quotation from *"Die Haustiere und ihre Beziehungen zur Wirtschaft des Menschen"*: *"Wenn man Milch trank, und den Ochsen an den Pflug spannte, waren wesentlich alle Bedingungen für unsere asiatisch-europäische Kultur vorhanden"*), and in a genre perhaps more familiar in late-19th century Berlin than in Departments of Archaeology or of Pre- and Proto-history in German-speaking universities more recently.<sup>10</sup> It nevertheless seemed to be an opportunity to draw together a series of discourses which had taken place largely in isolation, in rather separate fields of research - Near Eastern and European archaeology, archaeo-zoology, Indo-European linguistics, comparative ethnography, medical biochemistry - and to explore possible connections between them in the light of the common importance of changes in the usage of



domestic animals. After all, if the phenomena identified by HAHN (and increasingly recognised by prehistoric archaeologists as reflected - however indirectly - in their own material) could be dated, this should have implications for all of these areas of investigation.

An important part of the 1981 article, therefore, was to use the rich accumulation of archaeological observations in Europe to try to date the appearance/arrival of various elements of the ways of life characteristic of historical Europe and thus of much of the modern world.<sup>11</sup> This involved an integration of archaeological phenomena not often discussed together: settlement-patterns, archaeo-zoology, fibre-traces, figurines, ceramic typology.<sup>12</sup> The attempt to integrate these diverse forms of evidence proved well worthwhile: my formulation of the problem has been found useful in characterising the course of economic change in later prehistory, from Spain to Iran;<sup>13</sup> and the idea of a "second stage" in animal husbandry, characterised by an increase in secondary uses and products and the domestication of specialised transport animals, has been adopted by many archaeozoological specialists (eg DAVIS 1987, Chapter 7). Most recently, Norbert BENECKE has made use of the idea, both in his wide-ranging popular account (1994a, 121-161), and in his monographic study of the central European evidence (1994b, 45-92 and especially 93-100: not cited by VOSTEEN). This latter work ranks as the most serious professional assessment of the evidence from central Europe, and the reader is referred to its discussion for an authoritative contemporary view of this material.<sup>14</sup> An evaluation of the European evidence, however, was only one part of my goal, and the article was also successful in initiating discussion of the inter-relationships between the various wider bodies of knowledge. Bill DURHAM, for instance, has recently (1991, 242-252) used it in considering cultural factors in the evolution of lactose tolerance; while the discussion following Colin RENFREW's (1987) brave foray into Indo-European linguistic history (eg *Current Anthropology* 1988, 29/3, 437-468) took extensive account of this post-Neolithic economic transformation as a necessary background to understanding the pattern of linguistic dispersals. Although the relationship between social structure and mode of subsistence is a large problem that deserves further examination,<sup>15</sup> a particularly useful discussion has recently opened up (BOGUCKI 1993; HALSTEAD 1996) over the socio-economics of keeping draught animals, and its significance as a primary reason for the emergence of social inequalities. This comes to the core of the problem of the distinctiveness of west Eurasian societies, as indicated in

HAHN's quotation. It is this linkage between pre-history and cultural ecology that can help to resolve questions traditionally discussed on the basis of artifacts alone. The perennial question of "Bandkeramik ploughs" (eg LÜNING 1980), for instance, can soon be seen as an impossibility, when it is recognised that the limited areas of grazing indicated by settlement-archaeology and environmental reconstruction (eg KALIS 1988; LÜNING & KALIS 1988) would not have permitted the luxury of maintaining specialised draught-animals, and nor would the limited areas of intensive garden-cultivation have required them. This opens the (to me) fascinating prospect of societies in Europe that were profoundly different from those that we think of as characteristically "European" - a reason why archaeology is potentially of major intellectual importance, even though unfortunately that potential is often not achieved.

One conclusion of my 1981 reconsideration of the evidence for new forms of animal utilisation was that this was not just a problem of prehistoric subsistence but a question of culture-history. The relatively sudden appearance in temperate Europe of several innovations within a short space of time implied their introduction from neighbouring regions. The appearance of the traction-complex and woollen textiles at approximately the same time as arsenical alloying of copper and use of the two-piece mould (CHERNYKH 1992) suggested a comparable transfer of technology. This new openness of Europe to Near Eastern influences was in contrast to the long period of relative isolation which followed the introduction of farming,<sup>16</sup> and therefore marked an important change in inter-regional relationships. The later fourth and earlier third millennium BC would thus mark a watershed in later European prehistory.

#### "Economic" or "cultural" change?

While the observational basis of the 1981 article seems to me to have remained relatively unaltered, the **meaning** which I would give to these phenomena has, however, altered fundamentally. In 1980, still under the influence of the ecological enthusiasm of New Archaeology - necessary as that movement was, in reaction to unthinking typology - it was still possible to discuss technology in the simple, "instrumentalist" way that it had been treated by Gordon CHILDE. Thus "the plough" could be seen as a simple solution to the problem of increasing production (under assumed conditions of demographic pressure, cf. BOSERUP 1965), much in the manner in which Socialist Realism might have depicted "The Victory of the



Workers in Preparing the Soil": it allowed the opening up of new territories, the shortening of fallow, higher yields per unit labour (though not per hectare), etc, etc. A much better paradigm for today's thinking would be Stuart PIGGOTT's (1992) *Wagon, Chariot and Carriage: symbol and status in the history of transport*: a treatment of innovations as partly symbolic actions, introduced for human motives, rather than simply as more efficient methods of transport, field-cultivation, or whatever. Even though practical effectiveness was part of their appeal, and such innovations may sooner or later come to be very widely applied (and so bring about fundamental transformations in energy efficiency etc), this is not usually the reason for their **initial** adoption, which is usually of advantage only to a few people. Such an approach seems to make better sense of why evidence for paired-animal draught in the Baden culture, for instance, is so closely connected with equipment for conspicuous consumption (in the form of drinking-sets, for instance in graves 3 and 28 in the cemetery of Alsónémedi: BANNER 1956, Abb. 9 and Taf. xliii, xiv). It is a disappointment that VOSTEEN's perceptive first chapter, which deals with the recent history of archaeological ideologies, does not explicitly make this kind of connection with the implications of Post-processualism for such interpretations. Recent critical discussions have greatly enhanced the conceptual framework within which such apparently "technological" questions may now be set - without, however, throwing them back into a realm of inexplicable "belief", "cult", or arbitrary "fashion".

It is a striking aspect of the first evidence for wheeled vehicles in central Europe, in the form of the Budakalász and Szigetszentmárton models and the two Alsónémedi paired-cattle burials, that they are associated with equipment for drinking: the Alsónémedi graves both contain drinking-sets, and the two waggon-models are themselves cups. Moreover the drinking-vessels, as has long been observed (eg MILOJČIĆ 1949), are specifically of types which take their style and design from forerunners in beaten metal (Bandhenkel, omphalos base, channelling) and so at this time have Anatolian-Aegean connections - connections that reach as far north as Oldendorf, Kr. Lüneburg, where unusual, metal-skeuomorphic forms have been found in a primary context in an MN TRB passage-grave (SPROCKHOFF 1952). In fact, the complementary distribution-pattern in time and space of ploughmarks and representations of wheeled vehicles forms a very specific alignment (both chronologically and geographically) with these other indications of south-eastern influences. This is hardly a surprise, since it was a standard element of the "traditional"

picture of European prehistory put forward by CHILDE or MILOJČIĆ that such features were of south-east European, and ultimately Near Eastern origin - with the sole difference that a calibrated radiocarbon chronology puts all these developments in the later fourth millennium BC. All that I have so far done is to add the plough to the list of foreign novelties. Since this coincides with the major Mesopotamian impact on Anatolia and Iran, marked by the Uruk IV colonies, there is thus a very plausible historical context for such a reconstruction. What is interesting about these fourth-millennium European introductions is the directionality of their spread: these early examples fall along an axis that was to become in the late third and early second millennium the major highway of Early Bronze Age culture-contacts: the first (Danubian) "Amber Route" (cf. HACHMANN 1957; GERLOFF 1993), before its replacement by the more directly N/S Tumulus culture Amber Route (SHERRATT 1994, Figs. 6; 9). Areas to either side of this axial route may have been much slower in the acquisition of such novelties.<sup>17</sup>

This close connection between technological or agrarian change and the main currents of cultural change situates the various innovations summarised as "secondary products usage" not in a story about the gradual improvement of farming practice but in wider patterns of cultural change. The role of the Pit-Grave culture on the Pontic steppes, and the Baden culture in south-central Europe, become important in linking European developments to those in the Near East. The most disappointing aspect of VOSTEEN's treatment, therefore (in common with several English-language commentators: CHAPMAN 1983; RENFREW 1994, 165), is his hostility to what was one of the most distinctive features of my reconstruction, given the milieu within which it was conceived: namely, its willingness to see innovations as being introduced from outside Europe. The ecological emphasis of New Archaeology was usually combined with an emphasis on local processes of change ("cultural autonomy"), and this suspicion of outside influences has to some extent carried over into successor-movements in the English-speaking world, including Post-processualism. This has led to my account sometimes being stigmatised as essentially "diffusionist", and hence failing to locate the innovations within locally antecedent contexts. The underlying assumptions of this persistently autonomist attitude on the part of English-speaking writers are, indeed, precisely the stadial and evolutionary approach with which VOSTEEN introduces his account: but this characterisation applies to my critics, not to me!



This "autonomist" perception of English-speaking prehistorians is, in my opinion, about to change.<sup>18</sup> In terms of more general trends in archaeological interpretation at the present time, it seems a propitious moment to re-assert the importance of inter-regional links, and of the continuing importance of certain nuclear regions in the process of cultural change.<sup>19</sup> Recent accounts of the scale of effects of urbanisation in Mesopotamia (eg MARFOE 1987; KOHL 1987; AL-GAZE 1993), involving the foundation of colonial settlements in neighbouring areas and the transport of precious substances such as lapis lazuli over a distance from Afghanistan to Egypt in the later fourth millennium BC, fit very well with a reconstruction that would bring the traction complex (plough and cart) from Greater Mesopotamia to Temperate Europe within the same half-millennial span. This is not the same kind of reconstruction which once brought north-west European megaliths from Egyptian *mastabas*, along the Mediterranean and up the Atlantic façade: that kind of diffusionism is dead, and was killed off both by radiocarbon and by practical considerations of seafaring; but the penetration of Anatolian influences by the Danube route - precisely as CHILDE envisaged it - seems to me to be an entirely defensible reconstruction of episodes in the culture-history of the fourth, third and second millennia BC - albeit giving rise to original responses, not pale reflections, as CHILDE always insisted (1957). This "world-systems" view of later prehistory (or, more accurately now, *para*-history) seems set to become the new orthodoxy of the early third millennium AD, and is perfectly compatible with a calibrated radiocarbon chronology for European prehistory (SHERRATT 1994). From this perspective, the concept of a separate "*Secondary Products Revolution*" is unnecessary: the phenomena so far described under this rubric are better regarded as spinoff from the processes leading to the Urban Revolution in Mesopotamia, creating a marginal area<sup>20</sup> which was affected by the onset of urbanisation, even if not actively engaging in material exchanges with it. Let us therefore abolish this unnecessary concept: **Secondary Products Revolution abschaffen!**

### Punctuated or continuous change?

Given this willingness to abolish my creation, why should I be annoyed at seeing it described as "*unter die Räder gekommen*"?<sup>21</sup> It is not simply the horri-fying image of my ideas being crushed under the pressure of a Neolithic disc-wheel - the gentler English equivalent phrase would probably be "fallen by the wayside", which I would not mind - but rather more the dismissal of my reconstruction on quite

specious **empirical** grounds, rather than the conscious rejection of an intellectual construct. Anyone who believes that they are working purely by induction from the "facts"<sup>22</sup> is suffering from an illusion, since in the very process of selecting observations they are employing a model, and in coming to an interpretation they are invoking credibility within a certain set of assumptions. It is not possible in this case just to check off atomic "facts", one by one, and then total up the result, because all the indications are indirect ones, and require a degree of judgement in assessing the plausibility of a reconstruction. The process has to be an iterative one, searching for pattern and then re-assessing each other element in the light of its implications, and in the perspective of conclusions drawn from quite different forms of evidence.<sup>23</sup> Even the simplest operation, that of setting up a basic typochronology, requires some source-criticism and pattern assessment, against stated assumptions - for instance that an independent origin is as probable as a dependent one. VOSTEEN's account has the merit of applying a calibrated radiocarbon chronology, but seems at times (eg in ascribing use of the cart to the practice of erecting megaliths: 1996, 80<sup>24</sup>) to prefer autonomist explanations to interactionist ones - an association between a high chronology and a philosophy of independent development that have often gone together, from KOSSINNA to New Archaeology.<sup>25</sup> This observation concerning the history of investigation is not, of course, an argument one way or the other: but it is a useful reminder that these issues relate to an overall pattern of reconstruction, and not just to judgements in individual instances. This overall reconstruction is in many ways more important than the "facts" themselves, which can always be interpreted in more than one way to fit more than one reconstruction. It is thus the structural coherence of the entire edifice that is the most important issue. This book irritates me because it reduces the problem to a series of ticks in boxes.<sup>26</sup> It is not a problem that is capable of solution in that way.

That is not to say that the idea of a Secondary Products Revolution (or Urban Revolution spinoff) is a purely mental construction, with no congruence to the evidence on the ground. Such an idea would be purely metaphysical. But it is characteristic of my argument that such traces as do exist will be rather indirect ones. That is why the idea was a novel perception, rather than an accumulation of individual observations (like the growing refinement of a typology of bronze implements, for instance). The most concrete observations are those which relate most closely to specific artifacts of a particular functional type, and it is with these that the process of "pattern-recognition" should



begin. This procedure is set out at length in the appendix to this paper, where the evidence can be considered in detail. It is clear from this that much of the observational basis of my reconstruction is not in dispute. A principal point in writing this response, therefore, is to clarify how, with such a large area of common empirical agreement, scholars with different backgrounds can come to such different judgements. I see some sort of revolution; for VOSTEEN the process is one of slow and continuous improvement.

The basic issue is thus one of gradualist *versus* "punctuated" change, as the biologists usefully term it. My account stresses the "punctuation"; VOSTEEN's alternative picture is a gradualist one, with various elements appearing at different times since the start of the Neolithic. In part, this is simply two perspectives from different standpoints. From the point of view of world prehistory (the outlook of Eduard HAHN), there is no problem with a "revolution" that is, like the Neolithic Revolution, several millennia long. On a canvas the size of Eurasia and the length of the Holocene, this does not seem unreasonable - though whether the metaphor is an appealing one is a matter of individual taste. What is at issue in this discussion, however, is whether the metaphor usefully describes the individual experience of a constituent region. In the particular historical circumstances of when these various innovations were introduced to Europe, my reconstruction was rather specific: these novelties followed one another quite rapidly, within a few hundred years. It is this aspect, in relation to central Europe, that VOSTEEN chose to investigate. Leaving aside the practice of milking, which I agree (and said in 1983) may be rather different, my reconstruction is one that I would still defend. The difference between my reading of the evidence and VOSTEEN's is attributable to two major differences: one methodological, the other theoretical. Let me discuss them in turn.

### Methodology

In defining the beginning of a type of human activity from archaeological remains, two contrary principles may be invoked. A practice may have begun long before its first archaeological evidence, which may appear in the record for instance only when an object is put into graves (or traces preserved by the erection of grave-mounds!). This is worth bearing in mind, though as recovery of the archaeological record improves there should be a greater chance of finding earlier instances. On the other hand, one or two indications of an apparently early date may be misleading, because they are really quite irrelevant. In many

of VOSTEEN's lists, of which details are given in the appendix to this paper, there are one or two problematic "early" examples, eg "*Joch(?)*", "*Pflug(?)*", which are then included in the time-span, instead of being rejected as dubious outliers from the pattern; and in addition there are irrelevant data (a pit **under** the ploughmarks in question; evidence for castration, not necessarily for traction purposes), ambiguous data (ceramic sieves, so-called "herd pattern for traction") and undiagnostic data (horse-bones on archaeological sites). All these are given equal weight with the mass of more reliable indicators of the practices in question. The results of VOSTEEN's operation are tabulated in the following Table, along with my own successively adjusted estimates.

Earliest.....	Draught	Riding	Milk	Wool
VOSTEEN 1996	5th. mill. (Sarnowo)	4400 BC (Dereivka)	5500 BC (LBK)	6th-3rd mill.
SHERRATT 1981	3500 BC (Baden/ TRB MN)	4400 BC (Dereivka)	?3500 BC (Baden/ TRB MN)	2500 BC (EBA)
SHERRATT 1983	3500 BC (Baden/ TRB MN)	4400 BC (Dereivka)	?4000 BC?	3000 BC (SnK)
SHERRATT 1996	3500 BC (Baden/ TRB MN)	3500 BC (Pit-Graves)	?5th. mill.?	3000 BC (SnK)

**Table** Postulated dates for the introduction to central Europe of the four main secondary products innovations.

For each class of material, the earliest possible indication has been chosen by VOSTEEN, irrespective of its merits. (These are then called "facts"; but they are really judgements.) This decision to suspend critical assessment and to accept even the most ambiguous indications, long before the mass of reliable evidence, is related to the expectation that the practices in question had appeared early, and only later became regularly reflected in the archaeological record; in short, it is a reading that makes sense in the context of a gradualist ("evolutionary" as opposed to "revolutionary") view of change, of the kind which is often associated with an autonomist model of cultural development - namely, that it results from local processes of change rather than because of outside contacts. I do not hold this up as a pathology of VOSTEEN's approach, because in fact all archaeological arguments are of this kind: interpreting particular bodies of evidence in the light of more general reconstructions. But is clear that



VOSTEEN is doing this quite unconsciously, and in the erroneous belief that his procedure is somehow free until the last minute of any wider intellectual context. As Leo KLEJN has pointed out (1993), this illusion is rather widespread in German archaeology.

VOSTEEN has chosen, moreover, to present his work as a methodological paradigm for others to follow (despite a rather large gap between his own theory and practice<sup>27</sup>); and he explicitly contrasts what he characterises as "New Archaeology" with his own intellectual procedures. Convinced by the force of his own rhetoric, he does not hesitate to accuse those who disagree with him of intellectual dishonesty.<sup>28</sup> This he attributes to the erroneous procedure of what he calls New Archaeology, in putting ideas before fact-gathering. Personally, I find no need to inhabit such a schizophrenic world: ideas and observations seem to me to be intimately related, and induction and deduction to be different points in the same cycle of argumentation. The idea that information must be gathered "... mittels einer neutralen, wertfreien Fragestellung" (1996, 113) is unworkable: in this context, *wertfrei* = *wertlos*. What is important is to be able to argue **both** in terms of observations and of "theory" - explicitly argued general considerations - and moreover to do so at the same time.<sup>29</sup> I am delighted that VOSTEEN has chosen to give prominence to the ideas contained in my articles on this subject, since this can now be opened up for further discussion: the sometimes rather patronising tone of his remarks is a small price to pay for the chance to begin a debate on such an important set of issues.

## Theory

The major question raised by this topic is: how large an area is necessary to understand what was going on in European prehistory? A major drawback of the English-speaking schools of Processual (= "New") and Post-processual archaeology has been their reliance on "case-studies", with the assumption that change can be understood within a very local framework, as a result of local causes. One of the strengths of archaeology in the German-speaking world over this period has been its willingness to explore the implications of long-distance contacts between cultures. The full usefulness of such an approach in later prehistory, however, has been blunted by the use of a false chronology: either a pre-radiocarbon chronology, or an uncalibrated radiocarbon chronology. It is a most promising development that English- and German-speaking prehistorians are increasingly using the same language of dendrochronologically calibrated radiocarbon dates.

It is rather ironic, therefore, that just as they have achieved a reliable and generally accepted chronological framework, certain German prehistorians should begin to lose their larger perspective, and turn again to a localised approach in which every innovation is a response to a local problem: "... jede Neuerung [scheint] eine Antwort auf eine bestimmte Situation zu sein" (VOSTEEN 1996, 115). Every development is thus independent of all the others: the domestication of the horse, for instance, "... unabhängig von anderen Entwicklungen als Antwort auf Problematiken bei der Jagd ... erfolgt ist" (VOSTEEN 1996, 106). In this view of the past, everything had been present from the beginning, and slowly unfolded (literally "evolution"): "... es gab im Verlauf des gesamten Neolithikums nach und nach, je nach Bedarf und Region, eine regelmässiger Anwendung von schon teilweise seit dem Mesolithikum bekannten Techniken" (VOSTEEN 1996, 112). Thus the plough is called forth by the need to expand off the loess (VOSTEEN 1996, 110); the cart is created by the need to move large stones to build megaliths (VOSTEEN 1996, 80)! And the driving force behind these processes? - "... die Lösung von Problemen, die sich aus dem Bevölkerungswachstum und der damit in Verbindung stehenden Expansion" (VOSTEEN 1996, 103). VOSTEEN has invented the Anglo-American archaeology of the 1960s - **angelsächsischer als die Angelsachsen!** Along with this KOSSINNA-RENFREW position of high dating, cultural autonomy and local invention, there comes the ritual denunciation of "diffusionistic" reconstructions (1996, 105; 106), and an appeal to "ökologische Gegebenheiten" (1996, 109).<sup>30</sup>

It would be a great pity if sections of German archaeology were to lose their international outlook in favour of an introverted ecologism, for there is a relationship between a restricted *Forschungsbereich* and restricted vision: if observations are "auf Mitteleuropa beschränkt", then explanations will be, too. If you don't look for connections, you won't see them. Part of the originality of my reconstruction was that it tied together patterns of cultural change with patterns of economic change, and saw in both a major episode of contact and transmission in the fourth millennium. Certainly the pattern of **cultural** change is punctuated rather than continuous; and there is no reason to suppose that **economic** change was any smoother and more gradual. In fact the two aspects of prehistoric life turn out to be in reality very much the same thing: woollen clothes and bronze metalwork were both devices for Bronze Age elites to show off with; economic changes reflect new forms of consumption as well as production.<sup>31</sup> Metallurgy and alcohol-production are thus parts of the same process as carts and horses,



ploughs and cheesemaking. It is these ramifications which give me the incentive to continue exploring and renewing the set of ideas with which I began in 1981. As it happens, I have just completed putting together a collection of my published papers from the last two decades, including not only those discussed by VOSTEEN but also ones which take the discussion further and wider in pursuing these complementary aspects (SHERRATT 1997). Although the book contains passages which update the earlier articles by recording new evidence and analyses, its more interesting contribution is perhaps to show how the theoretical nature of the interpretations has changed over the time that has elapsed since their publication, with an increasing appreciation of this inter-connectedness. The sub-title of the book is "*changing perspectives*"; for, like the mill-wheels - die Räder,

*Die gar nicht gerne stille stehen,  
die sich mein Tag nicht müde drehn,*<sup>32</sup>

there are always new angles of vision to explore - a "revolutionary" change indeed!

## APPENDIX

### EVALUATION OF THE EVIDENCE

#### Introduction

This appendix considers the various categories of evidence for secondary products use which I put forward in 1981, and examines VOSTEEN's treatment of them. Although it will be of interest principally for specialists in the period, there are some general points which arise from the practical necessities of decision-making in the face of ambiguous evidence.

After 15 years I can see the deficiencies of my original presentation rather more clearly: so let me dispose now of three elements that I have discussed subsequently and about which I have come to rather different conclusions from the ones I reached in 1981.

- First, the settlement-pattern shift associated with TRB in the North European Plain, and exemplified (for Little Poland) in SHERRATT (1981) Figure 10.18, summarising KRUK: this is the **Early TRB** alteration that is associated with Mesolithic acculturation and the emergence of megalith-building communities beyond the loess - it has nothing to do with my postulated plough horizon at the onset of **Middle TRB** [Nordic terminology] in the mid-fourth millennium. The context of the set of early-fourth millennium developments is considered as part of a more general survey of megalithism in SHERRATT (1990).

- Secondly, the postulated connection between milk-drinking and the proliferation of vessels used for manipulating liquids in the Aegeo-Anatolian EBA and the Baden culture (eg SHERRATT 1981, Figure 10.15): this is much more plausibly related to the spread of an elite consumption of alcoholic drinks (which at this date probably did not include kumish). This has been considered at length in SHERRATT (1987b). The beginning of milk-drinking is the most difficult practice to date (hence my question-mark in 1981, Table 10.1). While it was undoubtedly **not** part of the initial pattern of domestic animal exploitation in the Near East (DAVIS 1993), it is not yet possible to specify confidently its beginning in Europe, with current techniques and osteological samples.<sup>33</sup>

- Thirdly, the dating of the domestication and spread of the horse by reference to the Dereivka stallion and supposed antler bridle-bit cheek-pieces (SHERRATT 1981, 272-273; 1983, 92-93): these have been the subject to a variety of reconsiderations (DIETZ 1992; several papers in HÄNSEL & ZIMMER 1994), and are discussed further below. It now seems likely that horse-domestication is actually a feature of the succeeding Pit-Grave period.

With these exceptions, the evidence adduced by me in 1981 - and now reconsidered by VOSTEEN - seems to me to support my chronological case to a remarkable degree.<sup>34</sup> I shall defend this attitude below, in considering VOSTEEN's comments in relation to the material record.<sup>35</sup>

#### Artifacts

Since the tools relating to the problems considered here were mostly of organic materials, rather than the stone, pottery and metal which are the most resilient archaeological remnants, even their artifactual traces may be rare or indirect. Let us begin with **Wheeled vehicles** (VOSTEEN 1996, Tab. 1, p. 78) where some 26 observations are tabulated. Inspection of the column of absolute dates reveals a certain consistency: only a single date is earlier than 3500 BC. This exception? "*Egolzwil 4, Joch(?) Cortaillod*". Two possibilities: the earliest yoke in Europe; or, not a yoke. Probably, not a yoke.<sup>36</sup> Next phenomenon (Tab. 2, p. 80), **Wheelruts**: one example, "*Mitte 4. Jt.*" Next phenomenon (Tab. 3, p. 81), **Representational evidence of draught animals**: 10 observations, all after 3500 BC.<sup>37</sup> Tables 4 and 5 are archaeo-zoological interpretations, to which I shall return; Tab. 6 lists 4 **Bog-roads** dated to the third millennium; Tab. 7 lists 3 **Double-burials of cattle**, all dated to the second half



of the fourth millennium; Tab. 8 lists three "**Parts of ploughs**", none of which is in fact such an object - the first is a third-millennium "*Pflugfragment (?)*", the second a mid-fourth millennium "*Furchenstock*", the third (while labelled "*Pflug(?)*") is actually an Ertebølle/Ellerbek canoe-paddle of a well-known type (ANDERSEN 1983), unsurprisingly dated 4942-4255. So far, a rather consistent picture - only a stray canoe-paddle before 3500.

### Traces of use

Then we come to Tab. 9, p. 89: **Ploughmarks**, 31 examples. Of these, 30 examples<sup>38</sup> after 3500; a single one, Sarnowo, is listed as "*4459-4343, vor Wiorek-TRB*". Ah, Sarnowo, Włocławek district, Bydgoszcz voivodship: what confusion you have caused! Jan-Albert BAKKER, in 1966, brought back for dating at Groningen a piece of charcoal from a pit underneath a (late fourth-millennium) Kujavian long-mound, which covered both an earlier Neolithic pit (with "TRB A/B" pottery) and also a set of parallel furrows plausibly but not unambiguously interpreted as ploughmarks. The date therefore refers to the underlying pit, which thus offers a *terminus post quem*, just as the barrow-mound itself offers a *terminus ante quem*.<sup>39</sup> This single date, only tenuously associated with the furrows in question, is the sole evidence for pre-3500 ploughing in Europe; yet VOSTEEN joins a long line of uncritical commentators (most recently MIDGLEY 1992, 388-90) who have used it to push back the beginnings of plough cultivation in Europe by up to 1000 years!<sup>40</sup> **Quellenkritik!** Earlier German-speaking archaeologists were rightly suspicious of radiocarbon-dating, because it could apparently produce nonsense; and this is a good example. But there is a coherent explanation: one date out of 70 is "out of line", **because it is a single determination, poorly associated with the phenomenon in question.** It makes no sense to distort European prehistory to accommodate this single aberrant date. It is much more sensible to conclude (as the overwhelming mass of observations indicate<sup>39</sup>) that paired-draught traction, for the plough and for the cart, began in Europe around 3500 BC or very shortly thereafter; and it is interesting to note that this date is closely similar to the best estimate (since there are far fewer radiocarbon dates for this period in Mesopotamia than in Europe) for the time of the Uruk expansion and the foundation of Uruk colonies in the areas immediately surrounding Mesopotamia, stimulating their own growth and outward connections (eg STROMMINGER 1980; ALGAZE 1993).

### The structure of domestic animal populations

Both the mid-fourth millennium dating of the introduction of plough and cart, and the historical reconstruction proposed for it, make perfect sense of all the artefactual evidence. Having begun with this apparently robust set of observations, let us consider now the rather indirect evidence of livestock mortality profiles. Trying to reconstruct the nature of animal management and exploitation from age- and sex-data inferred from osteological measurements is a relatively new methodology: it critically depends on large samples of well excavated animal bones from representative archaeological contexts, subjected to very painstaking analysis. These criteria are fulfilled at no more than a handful of sites.<sup>42</sup> Even when the sex-specific age profiles have been calculated (and there is as yet no standardised method of calculating and displaying such data), the interpretation must make certain assumptions.<sup>43</sup> What we have at the moment, for the period in question, is no more than a handful of rather suggestive statistics. Partly because the preservation in tell-sites is better, and partly because animal domestication has been a primary role of research excavations there, information from the Neolithic Near East is in many ways more reliable (DAVIS 1984; 1993; HESSE 1984 etc). This demonstrates that the initial domestication process was concerned with obtaining milk, not meat; that the latter aspects began to become important in the Chalcolithic period; and therefore that what spread to Europe was the initial, "primitive" mode of exploitation. The growing utilisation of goat's, sheep's or cow's milk and its products is a process which can, of course, take place anywhere these domesticated species are present,<sup>44</sup> and at the moment it is hard to specify where and when it began in Europe. I am prepared to believe that it could have been present by Balkan Late Neolithic and Bandkeramik times, though on a rather small scale.

A more crucial question is traction, pack-use and riding, ie the use of animal **energy**. This is the heart of the problem, since it is central to the character of western Old World agricultural and technological systems (CIPOLLA 1978). Pack-use and riding are relatively easy (though not entirely unproblematic) to date, since they involve the domestication of new, specialised species of animals: equids and camels.<sup>45</sup> (The evidence discussed below converges on a date in the later fourth millennium for this episode.) Effective traction, ie paired draught by yoked bovids (since other species were less useful for traction before the invention of a specialised traction-collar, in the first millennium AD) depended on **the maintenance of specialised traction animals**, ie draught-oxen.<sup>46</sup> This,



rather than the technology of harnessing a pair of animals by a yoke to a draught-pole, is probably the real distinguishing feature of the later fourth millennium BC in Europe; but it may well have been the prestige associated with wheeled vehicles which finally convinced prehistoric European societies of the desirability of such an extravagant use of resources - and so made the plough into a practical option for farming. Both driving and ploughing would initially have been **elite** (or, in still megalithic northern Europe, perhaps better described as **religious**) practices. So also with equids (and in the Near East with camels): these were very expensive animals, kept initially for rather special purposes and owned only by the few. Of course, such a formulation does not exclude the use of meat- or milk-providing animals such as cattle from providing some *ad hoc* functions as traction-providers at an earlier date. Animals, like everyone else in the social group, might be pressed into service from time to time in sharing heavy burdens.<sup>47</sup> This is quite a different matter from keeping specialised traction-animals and beasts of burden. It is when this latter stage has been reached that animal-keeping has its decisive effects on the human economy, and indeed on human society - since not all family groups are wealthy enough to own such animals. Indeed, this inequality in animal ownership is perhaps the most fundamental mechanism of social stratification.

For all of the reasons discussed above, currently available mortality statistics for Neolithic domestic livestock are inevitably somewhat ambiguous. It is nevertheless an emerging pattern in Europe that ovicaprids (the not always osteologically separable sheep and goats<sup>48</sup>) show a more marked contrast between earlier and later patterns, the former meat-oriented and the latter secondary-products-oriented, than do bovids (eg DÖHLE 1994). Moreover the change in ovicaprid mortality-patterns, towards the end of the fourth millennium, often coincides with a morphological shift that seems to represent a new, presumably wool-bearing, breed (BENECKE 1994b, 98-99; MÜLLER 1994, 181): though the effect could also be enhanced by a greater proportion of adult male (ie larger) animals in the population. This would certainly not contradict the only entry in Tab. 18, p. 99, **Wool-use**: the Wiepenkathen dagger, dated to 2400-1950 BC, and also the other evidence which I cited for a third-millennium date (1983, 93).<sup>49</sup> Cattle provide a more complex and continuous pattern through time, perhaps because of the relatively early use of milk, even though the technological traces discussed in the previous section clearly indicate a major change of role in the fourth millennium. The provision of specialised traction animals is, however, a matter of keeping a

few herd-members in relative luxury, rather than doubling the usefulness of most of the population, as with keeping wool-bearing sheep rather than hairy sheep, so the statistical effect may be weaker. (The practice of **Castration** [Tab. 5, p. 84], variously dated by different authors, on different criteria, between the Neolithic, Copper Age and later, is not in itself an indication of secondary products use.) These considerations make it impossible to recognise a simple pattern that can be labelled, as in Tab. 4, p. 83, **Herd-pattern for traction**, since this combines such different criteria which could apply to such different practices as rearing for meat (castration) or rearing for milk (high proportion of adult animals), and the data are taken from sites published in the 60s or 70s on the basis of rather small samples.<sup>50</sup> Of the four pre-3500 BC settlements, only Twann (upper layers) offers the evidence that most animals were older than 3 years, which is judiciously summed up in words (p. 45) as showing "... möglicherweise einen Wechsel in der Rindernutzung hin zu der Nutzung von SP während des Verlaufs der Cortaillod-Kultur an" - not quite the same as "**Herdenbild für Zugkraft**". Bronocice and Zürichsee, on the other hand, both **after** 3500, seem more convincing.

Concerning milk I am less dogmatic. I observe only that Tab. 16, p. 98, **Representational evidence for milk-use**, refers to late-fourth millennium Uruk-IV pictograms, and could be accompanied by a reference to the fascinating work now taking place on this material in *FU Berlin's Seminar für Vorderasiatische Altertumskunde* (NISSEN et al. 1993, esp. Ch. 12), which has a veritable *Abteilung Käseforschung* (ENGLUND 1995, cf. STOL 1993; TEUBER 1995) that can usefully supplement VOSTEEN's use of MÜNSTER's "*Käse selbstgemacht*" (1993).<sup>51</sup> The supposed evidence in Tab. 15, p. 97, **Archaeological data for milk-use** is simply the ceramic sieve, and this entry is somewhat prematurely labelled "*Siebgefäße zur Käseherstellung*", when the only examples to have been examined for organic residues have yielded traces of specifically vegetable oils (ROTTLÄNDER 1995). On Tab. 17, p. 99 (*Archaeological data for the reconstruction of herd-patterns [sheep and goats]*), I merely note in passing that Twann has now become "*Herdenbild Milch/Wolle*", but no more convincingly than it was for "*Zugkraft*".<sup>52</sup> While I would certainly acknowledge some possibility of milking before 3500, it seems perverse to claim the use of wool a millennium earlier than any archaeological evidence for its use, and when plant-fibre textiles are ubiquitously demonstrated from Neolithic sites all over Europe (BARBER 1991; BENDER-JØRGENSEN 1992; WINIGER 1995).<sup>53</sup>



Like the analysis of organic residues, therefore, the main contribution of animal-bone evidence to solving this problem in Europe still lies largely in the future. It is the artifacts themselves which are at the moment the most convincing clues - whether in the realm of transport and field cultivation using animal power, or in the creation of textiles from animal fibres. For the former, there is a cluster of evidence around 3500 BC; for the latter, no evidence before 3000 BC, but growing hints of wool during the third millennium, and complete costumes by the second millennium. This was no instant change (who said it was?); but after four millennia of simple farming in Europe, it was a thoroughgoing transformation in the ways in which domestic animals were used, and the kinds of material culture that could be created from their products.

### Horses

Horses existed in Europe during the Pleistocene, and as small relict populations in several areas during the early Holocene; though they died out in places like the British Isles. As farming groups slowly began to open the central European forest from Neolithic times onwards, the numbers of these wild horses gradually grew. When encountered by human groups, they were hunted. The presence of horse remains on archaeological sites is thus only diagnostic for domestication in places like Ireland, or in the Near East (and even this is not **quite** sure) where they clearly represent an introduced species. (See in general UERPMANN 1990). It follows that inspection of Tab. 13, p. 94, **Archaeological data on horse-use on the basis of faunal remains** (in central Europe), cannot distinguish wild from domesticated animals: and, as expected, the table contains sites from the early Neolithic onwards. The use of particular types of antler cheek-pieces for bridle-bits in the second millennium BC (when the chariot was in use, so they may not have been needed for riding) has encouraged the interpretation of perforated antler-tines of all sorts as possible harness-equipment. While those pieces defined as constituting an "Ostorf type" have some typological and chronological coherence (LICHARDUS 1980 - though they are not necessarily bridle-bits!), examples like those from a Cortaillod context at Seeberg (Abb. 48, p. 56) have no such distinguishing characteristics. Thus Tab. 11, p. 91, **Archaeological data on control of horses** (?), contains no diagnostic information, either. Somewhat more meaningful is Tab. 12, p. 92, **Traces of horse-use in burial practice**, which essentially points to the third millennium BC,<sup>54</sup> but this feature can only be a straw in the wind rather than a definite proof, since there are earlier ritual uses of horse-skulls

unassociated with domestication (eg Mane Lud, Locmariaquer).

That leaves only Tab. 14, p. 96, **Indications of the place of domestication**, which simply notes (with a query) the much-discussed site of Dereivka near Kiev. I originally (1981; 1983) accepted the consensus of archaeo-zoological opinion that this was indeed a hearth of domestication, and I am still of the opinion that horse domestication began somewhere on the Pontic steppes and spread from there eastwards and westwards (into temperate Europe and the central Asian steppes respectively, where there were wild horse populations where secondary domestication might be expected), and southwards (to the Near East, with no wild populations but where introduced horses hybridised with other equids). How long before the later fourth and early third millennia, when domestic horses began to appear in these wider territories, did domestication occur in this region? Dereivka itself dates (on the mean of the available radiocarbon dates, which is reasonable for a short-lived settlement) to around 4000 BC; but were its inhabitants domesticating horses, in an evolutionary scenario of gradual familiarisation leading to domestication: or did the critical stimulus come from outside, as the need for a new mode of transport arose in a situation of culture-contact? Now two new investigations offer conflicting results: LEVINE (1990), on the mortality patterns, infers hunting; ANTHONY and BROWN (1991), on the basis of tooth-wear on a single stallion-skull, claim riding and control by bridle and bit. Like most other interested observers, I await the results of a radiocarbon assay of the skull, and leave a space here [ ] to insert the date. If this date is c. 4000, then horses were apparently ridden in order to hunt other horses. If this date is substantially later, then the skull is intrusive and Eneolithic Dereivka is probably just a hunting site. This is a scientifically soluble problem.

If the first evidence for horse domestication turns out not to belong to the Sredni Stog and contemporary cultures but to the succeeding Pit-Grave complex, then this domestication-episode coincided with the introduction of ox-drawn wheeled vehicles as a result of direct input from the Near East via the Maikop culture of the northern Caucasus, parallel to the impact I reconstruct on central Europe around 3500 BC, and thus fitting well with my historical reconstruction (see above). This would integrate the domestication of the horse much more closely with the Near Eastern focus of secondary products uses, and specifically with the transport needs in the "colony period" of expanding urbanisation in late Uruk. Horses were apparently imported via the upper Euphrates (eg horse-bones at



later fourth millennium sites in the Keban region of eastern Anatolia: BÖKÖNYI 1991),<sup>55</sup> probably in order to hybridise with donkeys. Domestic horses, which on this model would be expected to appear in east-central Europe for the first time with the intrusive Pit-Grave populations there (ECSEDY 1979), could have stimulated attempts to domesticate local populations in central and western Europe. It is the coherence of this description as a historical model, rather than any simple procedure of adding up points in a table, which makes it attractive as an interpretation. Nevertheless it does not violate the "facts" as they now appear.

### Summary

Of the various pieces of evidence cited by VOSTEEN, about 60% were discussed in my own publications; the others have mostly appeared since the original articles. On 27 out of 28 occasions, VOSTEEN comes to the conclusion that "... *der bei SHERRATT angeführte Hinweis ist korrekt*". The new observations are principally further finds of wooden wheels from circum-Alpine lakeside contexts (many usefully catalogued in HÖNEISEN 1989), and faunal reports in recently published excavation-publications (for which BENECKE's [1994b] assessments must be preferred). The more recent pieces of evidence which he cites do not radically alter the "facts" on which I came to my original interpretation, but largely duplicate them. While it is clear from his treatment of faunal reports that VOSTEEN has greater faith in palaeo-economic reconstructions from small samples than I have, this on its own does not explain our disparity in evaluation of the types of evidence which we both accept. It is in the evaluation of one or two cases (which in fact formed part of my original discussions) that the difference in opinion lies; and the difference in evaluation comes principally from our differing expectations. Observations do not speak for themselves, in the absence of theory; and nor is theory a disease to be avoided or put off until the last minute: it is an intellectual coherence which must pervade the whole exercise.

### Notes

1 Markus VOSTEEN (1966) *Unter die Räder gekommen. Untersuchungen zu Sheratts 'Secondary Products Revolution'*. Archäologische Berichte 7. Bonn 1996. HOLOS - Wissenschaftlicher Verlag und Medien.

2 Since most German-speaking prehistorians were not to accept the implications of radiocarbon dating, and more

particularly of its dendro-calibration, for another decade, this was a problem perhaps felt more acutely in the English-speaking world.

3 The term '*Neolithic Revolution*' ("*neolitische Umgestaltung*") was invented by Gordon CHILDE (1936) and the terminology has been widely adopted in both Marxist and non-Marxist prehistoriography (eg SCHLETTE 1971; HOIKA 1993). But what is "revolutionary" in one perspective is "evolutionary" in another: "*Eine plötzliche, durchgreifende 'Neolithic Revolution' hätte dann gar nicht stattgefunden. Eher wäre wie bei der Industriellen Revolution mit einem evolutionären Vorgang zu rechnen*" (HOIKA 1993, 15).

4 Like the Neolithic and the Industrial Revolutions, it was a "revolution" in the perspective of the deep time of prehistory: a punctuation point in the development of humankind, not an **event** (like the French Revolution). As I made rather clear (eg SHERRATT 1981, Table 10.1, p. 271), its elements were NOT introduced to Europe as a single, synchronous horizon.

5 The use of perennial tree-crops in the Mediterranean, from which fruits such as grapes, olives, figs, dates etc are gathered on a continuing basis (as opposed to the yearly cropping of annuals like cereals) is an analogous and indeed precisely contemporary development, which I would now include as an integral part of the phenomenon. Since such plants have a much narrower range of climatic tolerance than many of the animals used for secondary products, they did not spread to temperate Europe at the same time as the innovations in animal husbandry, but gradually extended (along with the donkey) westward along the Mediterranean as part of a process of capital concentration closely connected with trade and urbanisation (cf. SHERRATT & SHERRATT 1991).

6 Secondary products are those which are yielded continuously, and do not require the death of the animal: milk, wool, sometimes blood; and by extension also uses such as traction and transport. The term is easily understood in English, despite the rather common use of "primary" and "secondary" to distinguish many different phenomena: colours, types of school, forms of feather, etc. Anneke CLASON (in GREENFIELD 1988, 589) was making rather heavy weather of this fact in pointing out that ecologists sometimes call autotrophs "primary producers" and heterotrophs (by extension) "secondary producers". (A pupil at a *Gymnasium* could probably also be called a "*secondary product*"!)

7 An impression given, for instance in G. BARKER's (1985) book, '*Prehistoric Farming*'. See my review in *Scottish Archaeological Review* (SHERRATT 1987a).

8 Such a reconstruction was not fashionable at the time (or even now) amongst English-speaking prehistorians, since for good reason there was a strong reaction against the naive use of "diffusion" as an explanation of cultural change (eg RENFREW 1973). Such diffusionist explanations - often involving unexamined assumptions, and especially



dangerous when combined with a pre-radiocarbon chronology - were more common on the Continent. I am still characterised as a "neo-diffusionist" by those who insist on the autonomy of European cultures.

9 I am not sure that VOSTEEN quite appreciates this, for he sometimes gives the impression (eg 1996, 109) that I believe that there were "pastoralists" in central Europe, rather in the old-fashioned way that Corded Ware populations were once seen as Neolithic Scythians. For early Neolithic cultivation systems as essentially "horticultural" in character, see SHERRATT (1980).

10 "*Later [in the 19th century] the theory of evolution, derived from the natural sciences, moved in a socio-historical direction... Prehistoric archaeology began to search for evolutionary processes among those primitive peoples who preceded the highly developed cultures of Antiquity and the Middle Ages. The development of a comparative and general archaeology was on its way. In Germany this process came to a standstill in its early stages, soon after the turn of the century*" (KOSSACK 1992, 104).

11 Not, incidentally, because I believe in any essentialist quality of "European-ness", or even any inherent relationship between possession of these elements and the temporary global hegemony exerted by Europe in the later second millennium AD (but not much longer); Europe shot to prominence after the discovery of the New World because it became central to the pattern of global trading networks - but that is another story.

12 Had the study of organic residues explored - and it has not, even now - more than a fraction of its potential, such evidence would have figured, too: it will undoubtedly be the major growth-area of archaeological science in the next decade, and will help to solve the milk-problem (cf. GRÜSS 1933).

13 eg HARRISON (1985) for Spain, GREENFIELD (1986) for the Balkans, LEVY (1992) for Israel, DAVIS (1984) for Iran, etc.

14 **Milchnutzung:** "... die Milchgewinnung bei Rindern und Kleinwiederkäuern in Mittel- und Südosteuropa [hat sich] in frühen 5. Jt. v. Chr. als eigenständige Nutzungsrichtung durchgesetzt" (1994b, 97); **Wollnutzung:** "Im Gegensatz zu der sich über einen längeren Zeitraum entwickelnden Milchnutzung... scheint die Wollnutzung der Schafe in Mittel- und Südosteuropa... relativ unvermittelt, in einem kurzen Zeitraum im Übergang vom 4. zum 3. Jt. v. Chr., herausgebildet zu haben. Dies spricht eher für eine Übertragung dieses Nutzungszweiges der Schafhaltung als für deren autochthone Entstehung" (1994b, 98-9); **Nutzung von Rindern als Zugtiere:** "Insgesamt lassen die vorstehenden Befunde erkennen, daß die Nutzung von Rindern als zugtiere vor Pflug und Wagen mit der Ausbreitung dieser Geräte nach Mitteleuropa in der zweiten Hälfte des 4. Jt. v. Chr. einsetzte" (BENECKE 1994b, 100).

15 Prehistoric and ethnographic phenomena must be seen as occupying their specific positions in world-time: each must be explained in its own historical context, rather than using ethnography as a complete model for prehistory. The contrast drawn by GOODY (1976) between plough-using societies in Eurasia (with diverging devolution) and non-plough-using societies in Africa (without it) leaves no room for the extinct, mobile, **light**-plough-using societies of later Neolithic Europe such as the users of Corded Ware, who arguably form an intermediate case.

16 Since features such as megaliths, traditionally attributed (eg by CHILDE) to diffusion from the Near East, were being demonstrated by radiocarbon dating to be indigenous European developments (RENFREW 1973).

17 "*Toutes ces civilisations nouvelles [SOM, Horgen, and contemporaries] sont enfin marquées, au plan technique, par ce que A. Sherratt appelle la révolution des produits secondaires et qui est un phénomène de diffusion européenne. La roue, le chariot [d.h. Wagen] l'utilisation de la laine et des laitages, peut-être l'acquisition des premiers araires sont quelques aspects de cette "révolution", terme mal venu lorsque l'on sait ces techniques étaient en gestation depuis longtemps et que leur diffusion a été bien progressive.*" (PÉTREQUIN 1988, 193, my emphasis). The "long gestation" was (with the exception of milking) in the Near East and the nearby steppes, not in temperate Europe; and the slow pace of adoption was perhaps more characteristic of certain rural hinterlands like the circum-Alpine lake districts, than of more axial regions such as the Carpathian Basin and central Germany.

18 In the English-speaking world of archaeology, that is; though conversely, I sense a trend in the German-speaking archaeological world to react against the previously-believed short chronologies by asserting European cultural autonomism once again.

19 In opposition to the "autonomist" tendencies of New Archaeology, but to avoid confusion with naive diffusionism, I use the term "interactionist" to describe my own attitude.

20 If the urban areas are regarded as the **core**, the area tapped for its raw material supplies by colonies as the **periphery**, then Europe in the fourth millennium can be regarded as its **margin**, following the terminology suggested by Jane SCHNEIDER (1977).

21 This metaphoric description of what he wanted to do is elsewhere made more explicit: "*Eine zweifelhafte These zu kritisieren und zu verdammen...*" (1996, 114).

22 "*Um mögliche Fehlerquellen auszuschalten, die sich durch die deduktive Arbeitsweise ergeben können, stand bei der Untersuchung des Verf. die Materialaufnahme mittels der Literaturrecherche am Anfang. Dann erst erfolgte die Interpretation*". Summary in *Dissertationen und Magisterarbeiten, Archäologische Informationen 18/1, 1995, 119.*



"Hierfür wurde versucht, sämtliche publizierten archäologischen Daten zu diesem Komplex zu fassen... Die so gewonnenen Fakten liefern das Gerüst für die Vergleiche mit SHERRATTs Befunden..." (Cover blurb, 1996). It is useful to remember the etymology, Latin *facta*, "things made"! (It is even more important to suspect *data*, "things given", and to ask the questions: Who gave them? Why?).

23 For this reason, I am unmoved by the complaint in VOSTEEN's Footnote 25 ("*Leider vermisst man in SHERRATTs Arbeit eine systematische Gliederung seiner Gedanken. Auch eine Inhaltsangabe fehlt.*"), since this relates to our different approaches: his "atomic" and fact-by-fact; mine an attempt to do "joined up thinking". Personally, I find his own style of presentation unduly splits up the discussion of the same items and hence is very repetitive, thereby inflating the length of what might have been a journal-article to its present monographic proportions.

24 This seems particularly illogical, since "megaliths" are far too heavy to be carried in carts!

25 Indeed, in his combination of a calibrated radiocarbon chronology, naive-positivist methodology (with surrogate data) and autonomist assumptions, VOSTEEN seems to be closer now to the classic (American) form of New Archaeology than I was in 1981!

26 Let me state clearly, however, that it is a worthwhile MA thesis - but now I look forward to a work of doctoral sophistication. It is depressing that the work here discussed can see no higher goal than to provide "*eine phaseologische Gliederung*" for later European prehistory (1996, 115).

27 THEORIE: "*Die dazu notwendige neutrale Fragestellung kommt durch Reflexion oder Intuition beim Umgang mit archäologischen Daten zustande und ist dabei als gliedernder Teil der Materialsammlung zu sehen, da man keine sinnvollen und schlüssigen Aussagen aus undifferenziert angehäuften Datenmengen ziehen kann.*" (VOSTEEN 1996, 113). REALITÄT: "*Um in einem vernünftigen Rahmen zu bleiben, wurde das Arbeitsgebiet auf Mitteleuropa und auf die SHERRATT'schen Beispiele beschränkt.*" (VOSTEEN 1994).

28 Witness the extraordinary outburst on page 108, which contains the following allegations concerning my scholarly probity; "*Es stellt sich heraus, daß SHERRATT zum Teil eher 'lässig' mit dem ihm vorliegenden archäologischen Material und vor allem 'lässig' mit dessen Datierung umgeht. So nennt er eine ganze Reihe von 'neolithischen' Funden, die seine Theorien stützen, ohne deren Datierungen durch Quellenbelege nachvollziehbar zu machen oder gibt er schlichtweg falsche Quellen an.*" This appears to refer (no details are given) to my failure to emphasise the fifth (rather than fourth) millennium dating of Dereivka and Sarvovo. It is a novel experience for a British author to be criticised in German for not calibrating radiocarbon dates; but it has been worth waiting for. The accusation of having provided "misleadingly incorrect sources" is, however, a gra-

tuitous slur - all the more insolent for the accompanying suggestion that the supposed manipulation was unconscious: "*Es soll Herrn SHERRATT hiermit nicht unterstellt werden, daß er absichtlich Datierungen von Funden in seinem Sinne manipuliert hat....*" (1996, 108).

29 The practical implication of this is that prehistoric archaeology should always - as it was in the 19th century - be studied in conjunction with anthropology/ethnology; the study of artifacts on their own, without a systematic theory of the relevant practices, makes no sense.

30 Unfortunately, VOSTEEN doesn't know much ecology, since he thinks (1996, 110) that Alpine pastures are natural. They are, in fact, artificial and transhumance is therefore a late feature, not one that goes back to the beginning of farming.

31 That is why it is sad to see a return to the New Archaeological view that calories always come first: "*Genau wie für den urgeschichtlichen Menschen die Möglichkeiten seiner Subsistenzsicherung sicherlich eine größere Bedeutung hatten als die Form des Gefäßes...*" (1996, 114). Erst kommt das Fressen, dann kommt die Ästhetik?

32 Ebenso wie das Zitat im Titel dieses Aufsatzes aus dem Kunstlied von Franz Schubert "*Die schöne Müllerin*" (Anmerkung der Redaktion).

33 Ceramic sieves could have many other uses, but organic residue-analysis might in future resolve this question, as it has already begun to do with Rössen examples (ROTT-LÄNDER 1995).

34 Some other references could have been added for examples outside Europe: the Near Eastern representational evidence for milking was first discussed in this context by ZEUNER (1963, 219); pictographic evidence for the plough in second-millennium Crete is given by a sign in Minoan hieroglyphic script (EVANS 1909, 190-191) and in Cyprus by the Vounous model (conveniently illustrated in KARAGEORGHIS 1982, Fig. 29).

35 VOSTEEN makes great play with what he believes to be deficient details of sources in my 1981 article. The sources for information listed in VOSTEEN's Footnotes 27-30 are all contained in PIGGOTT (1968), cited once (1981, 264) but not thereafter tediously repeated. I am taken to task in Footnote 31 for what is claimed to be a misleading reference: "*Die Quellenangabe von SHERRATT zur Datierung dieses Fundes [d.h. Züschen], 'UENZE 1958', ist nicht nachvollziehbar, da UENZE (1958, 99 ff.) dort kein Datum angibt*"; but if UENZE had given a date, it would have been the wrong one, since he was not using a radiocarbon-based framework! I was perhaps expecting too much intelligence in a reader to take an expert typological attribution and mentally to give a currently acceptable date to a *Hessische Steinkiste*. The equid tablet which VOSTEEN (Footnote 70) could not find in SCHEIL (1923) was No. 105, illustrated in Plate XVII. Finally, VOSTEEN accuses me in



Footnote 91 of failing to emphasise the differential survival of linen and wool, when I explicitly discussed it at some length (SHERRATT 1983, 93).

36 This is where **expectations** affect judgement. If you think it probable that a specialised instrument for animal traction is likely to have been in use since the beginning of farming, then this enigmatic piece of wood could well be a yoke. If you do not believe this, you will be more inclined to doubt it. But the most prudent attitude, in either case, is to be very sceptical of it.

37 In three cases, which are less precisely dated, the objects are assigned to range of time which begins c. 3600 and extends to c. 3300 or 3000 BC - so the mean date is thus after 3500 BC, with a very small statistical probability of being slightly earlier.

38 One scribal error: Flintbek (vor TRB MN I) "*vor 1. Hälfte 4. Jt.*", recte "*vor 2. Hälfte 4. Jt.*". And note, incidentally, for "SCHABLOW" read **SCHLABOW** throughout (both text and bibliography).

39 VOSTEEN (Footnote 123) correctly points out some now potentially misleading usages in my 1981 article. Since calibration was then a relatively novel practice (and hardly used at all in the continental literature), my phrase "*a radiocarbon date ... in the early fourth millennium*" would have been understood by readers at the time to mean "*an uncalibrated radiocarbon determination*" (1981, 270). I did not explicitly calibrate this particular date, from Sarnowo, in the text (since I argued that it was stratigraphically irrelevant), though I did so for presentation on the accompanying diagram on the same page. VOSTEEN asks for the details of the source used in compiling my (1981) Fig. 10.8: it was DĄBROWSKI (1971, Fig. 4), since although this is a specialist pollen-analytical report, it was at that time the only illustrated account of the sub-barrow surface (cf. GABALÓWNA 1970: the excavator, Lidia GABALÓWNA, sadly died in that year). The source which I cited in the text (BAKKER, VOGEL & WISLANSKI 1969), contained details of the associated finds and a stratigraphic description. The excavations have since been published in somewhat more detail by WIKLAK (1980), though there is no section that includes the pit, the sub-barrow surface with the "ploughmarks", and the mound itself.

40 Not merely is this date accepted for ploughing activity, but "*Für die Ansprache des Stückes aus Rude als Pflugsprache die ungefähr zeitgleiche Datierung der Pflugspuren von Sarnowo*" - it has the miraculous ability to transform canoe-paddles into plough-shares! (cf. Micah 4,3: "*Dann werden sie ihre Schwerter zu Pflugscharen umschmieden*".)

41 There are now more than 100 third- and second-millennium ploughmarks in Denmark alone, along with 12 Middle Neolithic and 10 EN C/Fuchsberg (ie circa 3500 BC) - but none earlier, despite the excavation of earlier mounds (THRANE 1989).

42 It is always possible to simulate an age-profile indicating secondary products use, by bad excavation: collect only the larger and more obvious bones, and the population looks as if it is dominated by larger (ie older) animals!

43 One problem in interpreting age-specific slaughter patterns is that if the presence of calves is necessary for milking (as it is in primitive breeds), then many immature animals would need to be retained; if not, they would have been slaughtered at birth.

44 Unlike wool-production, for which a specific breed is necessary, which archaeo-zoologists believe was introduced from outside European Neolithic sheep populations (BÖKÖNYI 1987; MÜLLER 1994, 181; BENECKE 1994a; 1994b).

45 Recognising their domestication is not in itself unproblematic, of course, though it is perhaps most clearly shown by their introduction to new areas. (This in itself is not always clear, since the early Holocene ranges of these species is not always well known.)

46 Note that the paired cattle from the Alsónémedi graves 3 and 28 are in fact a cow and a calf, and in Budakalász grave 3 are both calves. This was presumably to avoid killing valuable specialised draught animals; and note that "ox-carts" are nowadays not infrequently pulled by cows - but only light (ie not solid-wheeled) carts, on modern roads. To use cows for ploughing is an act of desperation.

47 Sledges and rollers would have facilitated the movement of loads pulled by human muscle-power; sledges might on occasion be pulled by bovids, preferably castrated males; and any animal (though usually a strong male: to use females or juveniles is to risk injury) might have a burden strapped to its back. This would explain the pathologies noted eg by MATEESCU (1975 - which in this case imply the carriage of burdens rather than the pulling of a plough). Such pathologies, however, are precisely indicative of the **inappropriate** uses of animals, which is why specialist draught animals or beasts of burden are necessary for the regular utilisation of these qualities, and why the "secondary products revolution" was necessary in the history of farming!

48 Though only sheep (and, moreover, of certain specific breeds) can produce **wool**, as opposed to the hair that is typical of goats and primitive types of sheep (RYDER 1983). There are places where VOSTEEN seems unclear about this (eg p. 100 : "*Die Herdenbilder der möglichen Wolllieferanten Schaf und Ziege geben nur den Hinweis, daß die Nutzung tierischer Fasern seit dem sechsten Jt. in Mitteleuropa denkbar wäre*"). If VOSTEEN cannot appreciate the difference between wool and goat-hair (or dog-hair, for that matter, since dogs had been domesticated since the Mesolithic), then he should not be discussing textiles, and certainly should never buy a pullover! Feral populations of non-woolbearing (hairy) sheep of Neolithic type are still living in Sardinia: the mouflon.



49 One of the other indicators of changing textile-types is, as I discussed (1981, 283), the appearance of the clothing-pin; and the example noted by VOSTEEN (Note 92) from Blekendorf is a good example of one of the variety of indicators for wool-use which I had in mind in 1983 (p. 93) in pushing the date back to circa 3000 BC - and, as a type with Caucasian links, the specific hammerhead form of the Blekendorf pin is a potential pointer to the arrival of wool-sheep via the north Pontic route, rather than via Anatolia. While the later-fourth millennium pattern of transmission (for paired-draught) is an Anatolian/Danubian one, the third millennium pattern (wool, horse, metallurgical techniques) is a north-Pontic one. WINIGER (1995, 172) remarks: "Wäre aber mit der Schnurkeramischen Kultur... die Wolle als neues Grundmaterial der Weberei eingeführt worden, müßte der in Zürich 'Mozartstraße' spürbar gewordene Rückgang der Leinenverarbeitung nicht mehr erstaunen."

50 Eg Runstedt, Kr. Helmstedt (p. 45): "Allerdings liegen nur sehr wenige Rinderknochen aus dieser Siedlung vor, so daß sich die Frage der Repräsentativität dieser Untersuchung stellt."

51 While VOSTEEN was disappointed (Footnote 88) to find that the vagino-rectal insufflator identified by BANNER was merely a pottery tube, one wonders what he expected! As always, it is essential to search beyond the artifact and *cognoscere causas*.

52 The last entry of the table, "Kein Fundort; Herdenbild Milch/Wolle; 2. Hälfte 6. Jt.; LBK", is just hopelessly unscientific.

53 "Für das europäische Neolithikum kommen wir gesamt-haft zum Schluß, daß das Textilhandwerk vorwiegend auf der Verarbeitung pflanzlicher Fasern (Gräser, Baumbaste, Flachs) beruhte und damit hauptsächlich Bindemittel und Behälter hergestellt wurden, jedoch auch kleinere Trachtbestandteile wie Gürtel, Sandalen oder Kegelmützen" (WINIGER 1995, 143). WINIGER believes that plant fibres, including linen, were hardly used for clothing, and that whole garments of textiles only began with the use of wool. The economic significance of the introduction of woollen clothing must have been as profound as the introduction of bronze itself.

54 It should be noted, however, that the famous grave from Großhöflein in the Burgenland, is not "Schnurkeramik; 1. Hälfte - Mitte 3. Jt.": it was originally thought (PITTIONI 1954, 247) to belong to the Guntramsdorf-Draßburg culture ("Corded Ware" only in the broadest sense, and later third millennium); but in any case it is now thought to be Mad'arovce, and of early second millennium date.

55 I should like to dedicate this paper to the memory of Dr Sándor Bökönyi (1926-1994), for his perceptive studies of early animal husbandry in Europe and elsewhere, and for his contribution to Hungarian archaeology and its international reputation, as well as his personal kindness and friendship.

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