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ENGENDERING ARCHAEOLOGY

Women and Prehistory

Edited by

Joan M. Gero and Margaret W. Conkey

4

Households with Faces: the Challenge of Gender in Prehistoric Architectural Remains

Ruth E. Tringham

Prologue: A Story

I begin this paper with a story. As with all good stories, I have created, exaggerated and stylized reality to improve the text.

I was taken kicking and screaming to the conference "Women and Production in Prehistory" in the marshes of South Carolina, convinced that gender differences were not visible in the archaeological record, least of all in the architectural remains of deep prehistory, with which I was most concerned. I was moreover at a loss as to how I should rework my current research strategy to face the question of what women were doing in the houses that I was so busy studying.

I had an "Aha" experience.

I presented a summarized version of my paper about investigating household organization and architectural remains in prehistoric southeast Europe to the 20 or so participants round the table. They listened politely – the archaeologists did – and worried about the validity of the data I presented for my conclusions.

And then someone said, "Yes, but how do you envisage these households?" It was Henrietta Moore, I think.

"You mean how do I imagine their composition?" thinking: Oh, heavens, she wants me to imagine their kinship structure, but I am interested in what households *did* not what they comprised . . .

Henrietta said "No, how do you envisage them going about their daily actions?"

You can imagine, I felt quite defensive. "Archaeologists don't do that. We don't go around envisaging people leading cows to pasture and gossiping around the household chores."

"Yes, but what *if* you were allowed to do it; just relax; no one will tell. Now, just tell us how you see them. What do they look like?"

"Well," I said, "there's a house, and cows, and pigs, and garbage . . ."

"Yes, but the people, tell us about the people."

"Well . . .," I said. And then I realized what I saw. "I see," I said, ". . . a lot of faceless blobs."

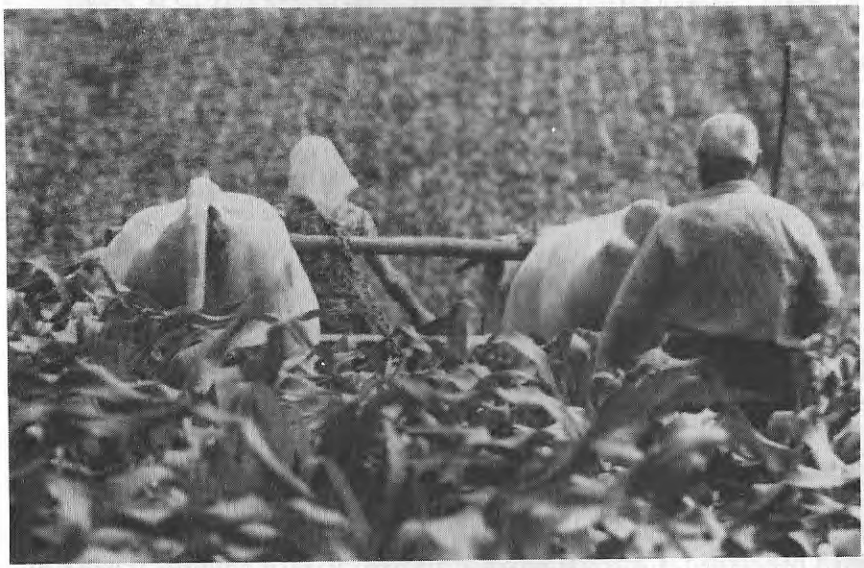

Figure 4.1 Households without faces in the Selevac fields, Yugoslavia. (Photo by Carol Spears)

And then it dawned on me what she wanted me to see. That until, as an archaeologist, you can learn to give your imagined societies faces, you cannot envisage gender. Or, in somebody else's terms (Conkey's?) you cannot engender prehistory.

And until you can engender prehistory, you cannot *think* of your prehistoric constructions as really human entities with a social, political, ideological, and economic life.

Ahaaaa!

Women in European Prehistory

The original plan of this contribution was to consider the question of how gender relations would be reflected in the remains of the built environment on archaeological sites. I had already been struggling for several years with the problem of how household organization would be reflected in the archaeological record, specifically its architectural domain, and thought that the question of gender could be treated as an

Figure 4.2 Households with faces in the Selevac fields, Yugoslavia. (Photo by Carol Spears)

extension, albeit a challenging one, of the "household" question. I realized, as I prepared myself for the original conference, that what I was attempting to do would be classed as a "remedial" feminist archaeology (Harding, 1987; Wylie, this volume). That is, that my theoretical and methodological framework based on the concept of material culture as a *passive* reflection of society's behaviour, would remain unchanged. I was merely going to "add women and stir".

In fact, in my original contribution to the conference papers at "The Wedge" I added a lot of household activities "and stirred", but there were very few women! I was still in my "phase" of embarrassed protest. My wish to retain respectability and credibility as a scientific archaeologist was stronger than my motivation to consider gender relations.

It is not as though gender had never been considererd in the area of my research – prehistoric Europe. There have been archaeologists who have explicitly envisaged women in prehistoric Europe. Two sets of these archaeologists are relevant to the discussions in this paper since they involve the archaeological record of architecture. These sets comprise, on the one hand, archaeologists working within the ideological framework of post-Revolutionary Soviet Union, and, on the other hand, one archaeologist – Marija Gimbutas – with a large non-archaeological following, who focused on the development and untimely destruction of

what she has termed the "Civilization of Old Europe" in prehistoric Southeast Europe.

Both of these groups of archaeologists had strong, if not always explicit, agendas in constructing prehistoric society and focusing on the role of women in prehistory. Both groups envisaged women as an equalizing force in society, who mitigated competition between individuals and the dominance and exploitation of one group or individual by another. Such a society, in both cases, is seen as a universal ideal, which was destroyed by later male-dominated, competitive, production-for-profit motivated societies. The agenda in both groups has been to reconstruct the time-space framework of the prehistoric ideal and its destruction, and to suggest that the fulfillment of human social evolution and a truly human society will only be achieved by a return to this ideal. Both groups used the data on the Neolithic–Copper Age period of Southeast Europe (including the SW USSR) to propose that the early agricultural societies of Europe were organized on a matrilineal, matrilocal egalitarian basis in large households.

Archaeologists of the first group took it as their role to demonstrate such societies as one of the pre-capitalist stages of the unilinear scheme of social evolution suggested by Morgan, Marx, and Engels (e.g. Childe 1958; Engels 1972; Semenov 1980). The excellent excavations in the Soviet Union and elsewhere in East Europe that are characterized by a broad exposure of village house plans and careful attention in recording the spatial distribution of artifacts, furniture, and internal divisions in houses represent their field strategy to gather the primary data for this purpose (Dumitrescu 1965; Kričevski 1940; Passek 1949).

Marija Gimbutas has focused on those parts of the archaeological record which she feels will inform her on the values, the folklore, customs, ceremonies, rituals, and beliefs of the early agriculturalists. Gimbutas has used the prehistoric architectural data to provide evidence of what she sees as the context of the rich ceremonial and ritual life of Old Europe (Gimbutas 1980). But it is through the study in particular of clay anthropomorphic representations (figurines) from archaeological sites within their architectural context that she has shown to her own satisfaction (Gimbutas 1970, 1980, 1982) and to that of a broad public following (Eisler 1987), the contrast between the "peaceful character of most of these art-loving peoples"[1] of "the Civilization of Old Europe" and that of the society which destroyed it: the patriarchal, hierarchical, and war-loving Indo-European Kurgan invaders. "An equalitarian male-female society is demonstrated by the grave equipment in practically all the known cemeteries of Old Europe" writes Gimbutas. "She also notes the presence of numerous indicators that this was a matrilinear society. . . . Moreover, she points out that the archaeological evidence

leaves little doubt that women played key roles in all aspects of Old European life" (Eisler 1987: 14). I shall focus at this point in the paper on one of the major problems inherent in these early attempts to discuss the role of women in prehistory; one, moreover, that has prevented not only these attempts but also the whole topic of gender relations to be taken at all seriously by Establishment (yes, undoubtedly male-dominated) archaeology in Europe.

The Morgan/Engels unilinear scheme of socioeconomic formations has been severely criticized from many different directions (e.g. Harding 1983; Leacock 1972; Thompson 1970; Trigger 1984). At the same time, archaeological research of the Anglo-American academic establishment from the late 1960s to the present, in its attempt to join the ranks of the scientific community at large, demanded rigorous testing and demonstration of hypotheses. In such an intellectual climate, the reconstruction of the role of men and women in economics, ideology, and social and political relations in prehistory was regarded as quite unvalidated and unvalidatable. It is not surprising, therefore, that many of the early Soviet reconstructions of the sexual division of labor amongst the early agriculturalists have been disregarded for their unscientific reasoning and lack of solid supporting archaeological data. At the same time, the work of Gimbutas has been heavily criticized for its inconsistencies and hasty inferences (Hayden 1986).

Explicit statements on the role of men and women in any walk of prehistoric life have virtually ceased to be presented by any archaeologists who wanted to be accepted by the archaeological Establishment of the dominant cultures of the West. Those who have dared to discuss the topic have confined themselves to the direct data on differences of sex, namely burial and other skeletal data (Gibbs 1987; Randsborg 1986; Sørensen 1987; Tilley 1981). Questions on gender in prehistory are not the only ones to have fallen victim to the "scientific method". The demise of social and political questions in general in prehistory in such a climate has been well described in a number of recent works (Hodder 1982; Shanks and Tilley 1987; Wylie, this volume).

Why have archaeologists produced a prehistory of genderless, faceless blobs? This question was originally considered by Conkey and Spector (1984). Now in this volume, the editors and Alison Wylie have suggested that a basic problem is a misunderstanding as to what an engendered prehistory should comprise. One of the beliefs leading to this misunderstanding is that the methodology of an archaeology of gender requires us to be able to identify gender in the archaeological record, that is to be able to attribute certain activities (e.g. ploughing, weaving) or material culture to males or females.

Expanding on this theme, it seems to me that, according to the

methodology of logical positivism, the ability to assign the archaeological record directly or indirectly to behavior is what *enables* hypotheses about human social and economic behavior in the past (as well as about gender relations) to be testable with empirical archaeological data. For this reason it has always seemed an essential step in any engendered or *social* archaeology. Thus, the ability to assign the archaeological record to behavior represents the strength of scientific archaeological enquiry, allowing us to demonstrate "facts" within a framework established by the academic community. Yet it also represents a devastating weakness in that it *inhibits* us from using the riches of modern social theory to construct prehistory: suggesting that certain questions are "untestable" or "marginal".

My personal resolution to this seeming insoluble problem could have come from a number of directions. I could have retained my original research strategy, heavily restricted by the demands of testing empirical hypotheses with the archaeological data, and focused on those sets of data that would give me information on sexual attribution, if not gender. Burials and two- or three-dimensional representations of human figures with clearly diagnostic features of sex could provide such apparently unambiguous sets of data. But I have chosen to work with architecture – a body of data that is not regarded as unambiguous for this purpose. Or, again within my original research strategy, I could have chosen a set of data which was apparently less ambiguous by virtue of being close to or within a period in which historical records were present. Those working in near-historical periods have felt that they could more convincingly attribute architectural units to household units, and even gender, through ethnohistory and close direct historical analogy (Bawden 1982; Ciolek-Torrello 1985, 1989; Donley 1987; Hill 1970; Reid and Whittlesley 1982; Stanish 1989). But I have chosen to work in a period of prehistory – Southeast Europe at 6000–4000 BC – that is separated by many thousand years from any written sources.

My only alternative then, apart from giving up the idea of gender relations, was to change my research strategy, to one which has a different standpoint on ambiguity and the scientific method, one that has been elegantly described by a number of philosophers of science including Alison Wylie (1982, this volume) and Harding (1986, 1987). A requirement was also a recognition that material culture, including architecture, has a richer role to play in archaeology than a passive reflection of human behavior.

Thus, what started out as an unrequited attempt at writing a European prehistory of gender relations on the basis of architectural remains has exploded into a reconceptualization of what writing prehistory is about, including the use of the archaeological record and the engendering of

prehistory. My contribution to this volume will try to indicate some of the directions that this explosion has taken me and, I hope, will take me and others in the future. Many will find most of these directions entirely unacceptable, others will find them disconcerting and unsettling.

I shall focus on the essential linkage between microscale archaeology – the archaeology of the household – and the study of prehistoric gender relations. This will take the form of a mild critique of the recently growing trend towards household archaeology, which is characterized by a lack of explicit consideration of gender relations. I will then discuss the kinds of linkages that can be made, along with the use of the architectural data in these considerations. At this point I shall introduce my field research on prehistoric households in Southeast Europe. The idea here is to avoid presenting this work in a traditional hypothetico-deductive context as a "case-study of a generalized concept/methodology". I shall introduce the field project – the excavation of Opovo, a Late Neolithic village in Yugoslavia – and the archaeological record, that has been created by myself and the team that works with me, within the framework of a dialogue of alternative historical trajectories for the prehistory of Southeast Europe. In each of these trajectories, gender relations are considered, but none can be considered an "engendered prehistory" of Southeast Europe. Thus, the challenge that is taken up in the last section of the paper is to show post-Wedge archaeology in action; to show whether, if at all, it differs from what we have done before.

The Household as Unit of Analysis in Archaeology

What has dominated the interest and energies of archaeologists in the (re)construction of prehistoric life has been what goes on beyond the household: for example, the corporate production of surplus goods, exchange and alliances on a regional and inter-regional scale, the struggle of humans to control the environment, the hierarchies and dominance structures between settlements. This is surprising in view of the pretensions of the discipline to be a social science. In the social sciences in general, the analysis of social change at a microscale of the household or coresidential group or family has long been recognized as an essential scale for the study of the social relations of production, including gender relations, especially in non-capitalist or pre-capitalist social formations (Goody 1958, 1972; Hammel and Laslett 1974; Laslett 1972; Netting, Wilk and Arnould 1984; Yanagisako 1979). It has also more recently been recognized as an essential aspect of understanding the feminist standpoint in the analysis of capitalist social formations (Beechey 1978;

Glazer-Malbin 1976; Hartmann 1981; Hartsock 1983; Rapp 1977; Smith 1978; Yanagisako 1979).

Such a reluctance seems extraordinary also in view of the fact that most archaeological excavation of settlements retrieve data which are most pertinent to the study of households and the products of domestic[2] labor (housework).

It seems to me that this lack of interest in the study of prehistoric households must reflect a willingness to accept the generalized assumptions concerning what goes on in and around the house. This focus on the "other" and the "outside" and a corresponding *lack* of interest in challenging the "givens" of social action at a microscale (along with a devaluation of women and their "labor") is characteristic of social archaeology of both Marxist (Bender 1985; Frankenstein and Rowlands 1978; Gilman 1981) and non-Marxist theoretical standpoints (Champion 1989; Renfrew and Shennan 1982), and is exactly what we can expect according to a "masculinist standpoint" in archaeology (Hartsock 1983).

Nevertheless there has been a growing – as yet still marginal – group of archaeologists who have chosen to carry out what is sometimes called "household archaeology". Since the initial study of households in the late 1970s and early 1980s this scale of analysis has been claimed to offer "a chance to archaeologists to examine social adaptation with direct reference to the empirical details of the archaeological record" (Flannery and Winter 1976); that is, with the methodology of scientific logical positivism, household analysis allows us to "bridge the existing 'mid-level theory gap' in archaeology" (Wilk and Rathje 1982: 617). In other words, the study of the archaeological record at this scale would allow us to attribute it to some sort of homogeneous social behavior.

But there are grave challenges in "household attribution", just as there are with "gender attribution". For every attempt to identify households in prehistory, there is an ethnographic or ethnoarchaeological cautionary tale to warn us of the dangers of linking architectural units with specific social units, such as with the "family" or "household" (Donley 1982; Hayden and Cannon 1982; Kramer 1982a, 1982b; Moore 1986, 1982; Watson 1978, 1979). The identification of units of cooperation is made more challenging archaeologically by the likelihood that cooperative action is being carried out at other levels as well: lineages, or villages (Hayden and Cannon 1982; Wilk and Rathje 1982: 621). Nevertheless, archaeologists have ploughed on in their endeavor. The Mesoamerican archaeologists, who have been particularly active in developing household archaeology, suggest that it is most important to understand what a household *does* rather than what its social form is (who lives there and how they are related) (Ashmore and Wilk 1988: 4–5; Wilk and Netting 1984; Wilk and Rathje 1982). Thus they suggest that we attribute the

archaeological record to units of cooperative production – consumption, generational transmission, coresidence, reproduction – all functional and all without specific active humans – through analysis of architectural remains and associated features and material culture. This path to the attribution of the archaeological record to household organization in prehistory may be one way of dealing with the problem of validating hypotheses about households with scientific rigor. But it nevertheless leaves prehistory hanging in a cloudy nowhere-land of faceless, genderless categories.

The "household scale of analysis" is the vehicle with which we may possibly make the inivisible women of prehistory and their production visible, since at this level – the minimal unit of social reproduction – their presence can be guaranteed. And yet, even in those household studies that have been carried out by archaeologists, gender relations are rarely if at all explicated. My own studies of household organization in southeast Europe have been no less characterized by this phenomenon. I confess that I have been highly inhibited from making such links even as far as "household" – let alone gender relations – by the perceived methodological difficulty of attributing architectural and associated data to household membership and gender. Even those working in historical and near-historical periods have generally balked at the topic of gender relations. There is no doubt, however, that, as with social archaeology at a more regional level, strong *implicit* assumptions about generic gender relations form the foundation to many theoretical formulations in "household archaeology" studies.

It is possible that the identification of units of cooperative production or coresidence will satisfy the ambitions of archaeologists working at this microscale of analysis. There are indications, however, that for the interest in and impact of "household archaeology" to be more sustained, it will need to be incorporated in a more ambitious theoretical program in archaeology (Freidel 1989). Such a theoretical program would challenge the search for universal categories of microscale social units in prehistory, such as the nuclear family, as it has been challenged in anthropology as a whole (Moore 1988; Yanagisako 1979), and would recognize the richness of the variability of the social context of domestic action. Thus, the fact that the definition and nomenclature of microscale social units have received anything but consensus from anthropologists and social theorists, who would recognize the Coresidential Domestic Group, the Household, the Housefull and last but not least the Family, should be treated as a constructive challenge for dialogue rather than as an inhibitor to research (Ashmore and Wilk 1988; Bender 1967; Goody 1972; Hammel and Laslett, 1974; Laslett 1972; Moore 1988; Yanagisako 1979). This is not the place to enter into the dialogue, but obviously an

archaeologist must if he/she is to understand or contribute anything to the discussion of the transformation of the social relations of production and gender relations in prehistory.

In recognizing the rich variability of social arrangements, dominance structures, and tensions produced in the social relations at this "domestic" scale, we see immediately that it is necessary never to forget that action at a microscale is an essential part of the social relations of production at larger scales, such as the village and the region and the known (and unknown) world.

> Despite the great difficulties facing the analysis and interpretation of households, the resolution of really fundamental issues in the study of ancient civilization lies in this empirical arena. Large-scale hierarchical society survives through the constant renegotiation of relationships between those who provide power, the commoners, and those who wield it as elite. The language of such negotiation . . . derives from both sides; and so too do the success and failure of complex societies. (Freidel 1989)

The recognition of this linkage has led researchers of social formations with a Marxist standpoint, but especially those with a *feminist* standpoint, to a very different understanding of the nature and role of labor and social relations at a household level and, most importantly, by definition, the nature of women's labor and the tension of gender relations (Hartmann 1981; Hartsock 1983; Moore 1988; Yanagisako 1979). It seems to me that this is the only theoretical basis that is likely to sustain interest in microscale archaeology, since it at once lifts the household sphere out of its current position of "assumed general knowledge" and as marginal to the "great events" of prehistory and provides a richly based theoretical framework for a consideration/imagination of the role of women's labor.

A second direction that will certainly contribute to the development of microscale studies is in dealing with the archaeological record itself. There is no doubt that the architectural remains of prehistory can be used much more imaginatively (this is *not* the same as speculatively) than the studies most frequently met with, whose primary aim is to reconstruct function and technology of the buildings as reflecting past human behavior. Ethnographic and architectural data show that buildings and their associated material culture act and have acted as both context and media in domestic tensions, gender relations, and dominance structures (Blier 1987; Bourdieu 1973; Donley 1982, 1987; Douglas 1972; Hodder 1986; Moore 1986).

The architectural remains of the archaeological record provide a rich source of information if one can be freed, if only slightly, from the

restrictive effects of the testing requirements of logical positivism and from the need to attribute the record to function, gender, or "domestic unit" before one can think further about the context of gender relations and household tensions. Thus, ethnoarchaeological studies and ethnographic observations of residential architecture may be used for more than cautionary tales. They can be used to help us formulate expectations in terms of variability in architectural remains that relate to and reflect changes in the role, relations and actions of men and women in the household in prehistory. They allow us to formulate empirical hypotheses and/or "read" the archaeological record on architecture and associated debris in a rich variety of ways.

It will be interesting to see if a division of labor develops in archaeology of the kind noted by Gero (this volume) as a result of the potential development in household studies in relation to gender relations and a feminist standpoint. Will we see (can we see already?) that there is a tendency for women professionals to focus on domestic production, households, housework, domestic architecture, microscale analysis with male professionals tending to focus on the "other", the "outside": trade, regional studies, inter-settlement studies, world systems, monumental architecture, elite architecture, surplus production?

Households, Gender and the Archaeological Record of Architecture

If one does not assume households to be faceless units of cooperation, and if one does not assume that housework is a given universal pattern of devalued at-home social action, and if one does not assume that the roles and relations of men and women in domestic space is more or less uniform, and if one does not assume that the built environment looks the same to prehistoric eyes as it does to ours, then where does one start? How far can you allow your imagination take you? Where and how do you draw the parameters of what *can* be said? How do you proceed with your archaeological investigation and construction of an engendered prehistory?

There is no quick recipe. Not even a recipe book!

The investigations of archaeological settlements that I have carried out in Yugoslavia are by no means good models of how to proceed. They reflect my own changing questions, theoretical framework, and epistemological basis of research. I offer them here as the example with which I am most familiar. The research at Opovo–Ugar Bajbuk, in Northeast Yugoslavia, north of the Danube (figure 4.3) was designed specifically to demonstrate the changing role and activities of households in the Late Neolithic/Early Eneolithic of Southeast Europe, by a relatively large

exposure of domestic structures and detailed stratigraphic control (Tringham et al. forthcoming, Tringham, Brukner, and Voytek 1985). In designing the project at Opovo, I assumed that the study of production, and of the distribution and transmission of property and products in its social context through the detailed study of the built environment (i.e. architecture) is a key to understanding the social relations of production and household organization. I am still under that impression and would now extend its importance to the study of gender relations in prehistory.

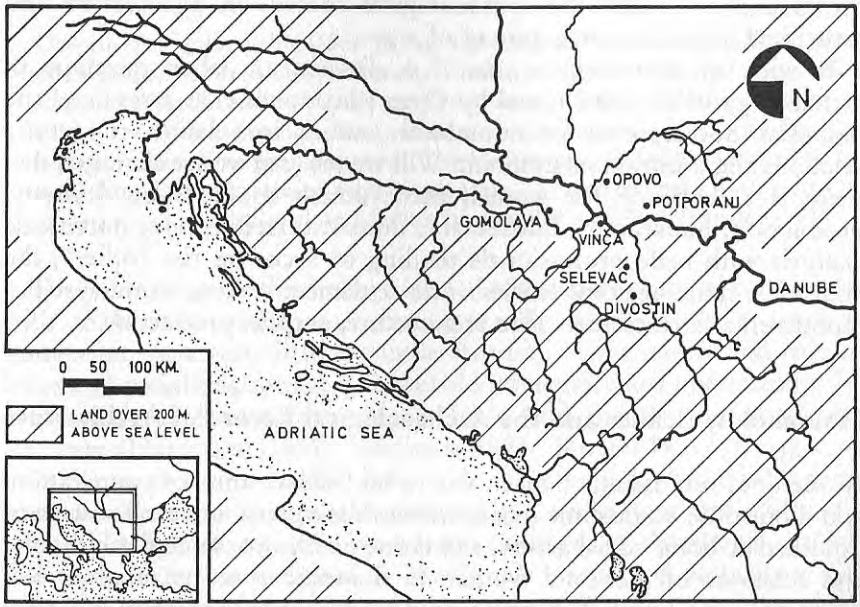

Figure 4.3 Map of Yugoslavia, showing geographical names and archaeological sites mentioned in the text.

At Opovo–Ugar Bajbuk in 1983–9, a clear microstratigraphic sequence of three phases of building activity and house overlapping and replacement has emerged in the 16×20 meter excavated block (figure 4.4). These comprise two building horizons, 1 and 3, with well preserved architectural remains of burned wattle-and-daub buildings, and one building horizon (2) whose architectural remains are less well preserved and/or existed outside the excavated block. The whole settlement demonstrates changes during a roughly 200-year period of the Late Neolithic/Early Eneolithic Vinča culture (ca.4400–4200 BC).

The original idea in 1983 – and it was innovative then for European prehistory – was that the architectural process reflected social processes

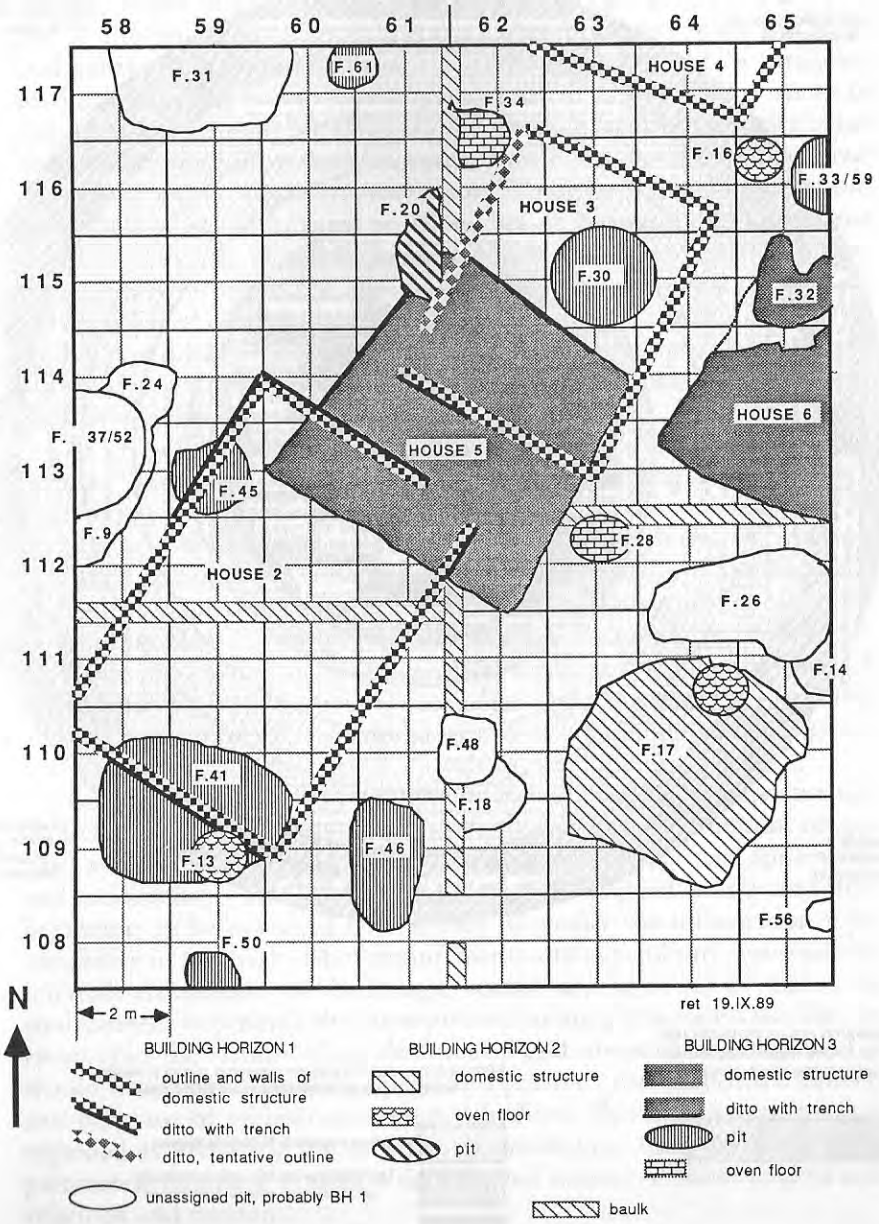

Figure 4.4 The sequence of building horizons at Opovo, showing the pattern of house replacement.

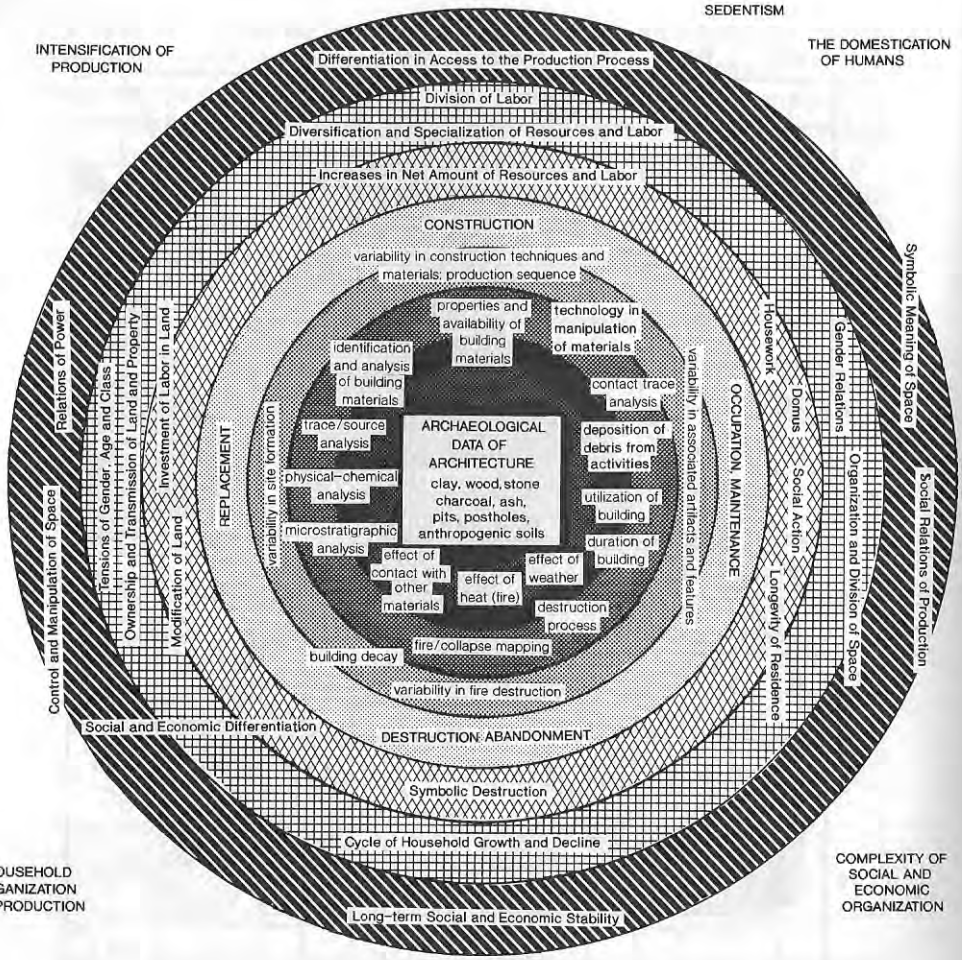

Figure 4.5 Chart for a middle-range research design to study archaeological architecture remains.

at a microscale (family, household, coresidential group) during the use-life of the buildings. This concept contrasted sharply with the more traditional prehistoric architecture studies that have focused on variability of the form and style of buildings as finished artifacts. In a series of complicated charts (figure 4.5) I plotted how these social processes could have been reflected in buildings at different stages of their use-lives (Tringham 1984). Ethnographic and ethnoarchaeological observations were synthesized and experimental studies of various kinds carried out to provide a series of test expectations of how different human actions would be reflected in the archaeological record (Stevanović 1984 and 1985). This whole enterprise may be described as leading towards a model of middle-range research design within the framework of a logical positivist scientific methodology of hypothesis testing (Binford 1981; Raab and Goodyear 1984; Tringham 1978, 1984).

The original aim of the study was to *attribute* certain architectural features and spatial patterns of associated materials, such as the recognition of discrete groups of complete production sequences, to units of economic and social cooperation (i.e. households); this would provide a first step in reconstructing the *transformation* of households at prehistoric Opovo. Thus, I fell smack into that trap of scientific restrictions that I warned against at the beginning of this paper. However, I do not wish, in a fit of self-critique, to denigrate this research design or the wealth of detailed data on construction, duration, utilization, maintenance, abandonment, destruction, and replacement of buildings that was collected as a result of it.

The middle-range research at Opovo has set basic material parameters of architectural variability at the site and provided information on the basis and nature of that variability that go beyond superficial appearance and association. The contrast between the original study and that beginning to be practiced now is that originally the information – on variability in materials, labor requirements and expenditure, external and internal elaboration of buildings, spatial arrangement of the built environment in general, the attempts to prolong a structure's use-life, its reuse, and the nature of its destruction and eventual abandonment – would have been assumed to provide a passive reflection of the actions and behavior of prehistoric households. Now, this same information is regarded as providing for us – the archaeologists – and for them – the prehistoric men and women – the *material context* of those actions and relations and tensions.

The underlying model of the transformation of Neolithic society in Southeast Europe of our original study prompted certain questions about the architectural record of the context of domestic life that went well beyond traditional questions of change in Southeast European

prehistory. Envisaging the prehistoric settlement in terms of *household* organization allowed and encouraged me to think about the implications of the architectural changes on a small scale. It encouraged me to excavate and observe and take into account details of variability that I might otherwise have taken for granted as being the same from one domestic structure to another. It encouraged my efforts to establish contemporaneity or sequence of occupation of various buildings.

From the start, the site of Opovo presented some striking contrasts with what we had come to expect of the Late Neolithic Vinča culture settlements along the Danube river and in the fertile agricultural valleys and hills to its south. It is clear that there are differences in the material remains of the Upper and Lower Building Horizons, but neither horizon reflects the socioeconomic system of the large settlements of the Danube and Morava basin Vinča settlements to the south, nor those of the Tisza culture to the north.

When compared with the domestic structures in other Vinča culture settlements south of the Danube, those excavated at Opovo have many features of construction in common in terms of building materials (plank-post-wattling framework covered by clay daub) and detached quadrangular ground-plan. In the Late Neolithic/Early Eneolithic settlements of Southeast Europe, including Selevac, it had been observed that the anticipated and actual use-life of houses increased. We have interpreted this as provision being made for long-term residence of the "domestic groups" (households) and reflecting the emergence of stable long-lasting households as the main units of cooperative production and distribution (Chapman 1981; Kaiser and Voytek 1983; Sherratt 1982; Tringham 1971, 1990). For example, the addition of rooms onto the ends of dwellings in Neolithic Europe, making square structures rectangular and increasing the length of rectangular structures, has been hypothesized to reflect the growth of a household during its cycle of growth and decline (Coudart 1980; Startin 1978). The predominantly rectangular ground-plan of the Vinča culture houses, therefore, may be thought of as representing the dwellings of long-established households.

We expected Opovo to show a similar development, modified perhaps by the fact that it was not located in prime agricultural soils, but in a predominantly marshland environment with islands of the more difficult to cultivate chernozem soils. At Opovo, we were deliberately looking for information on planned and actual use-life of dwellings by studying the nature of the building materials, technique of construction of buildings and surrounding facilities, such as storage pits, in terms of the relative investment of labour and commitment made by a group in their modification of the landscape, maintenance of dwellings, and the use-life of artifacts associated with the occupation of the house.

Our overall impression of the houses at Opovo, however, is that they were less well prepared and less long-lived than the majority of Vinča culture houses, especially those of the latest building horizon (BH 1). This impression is provided by the lack of clay floor in BH 1, the wooden framework of houses of BH 1 and 3 (even that with two storeys) which comprised a dense network of small vertical poles rather than the thick deep posts noted in the large houses south of the Danube, and the fact that all domestic structures excavated at Opovo at 6–7 meters length are much shorter than the standard Vinča culture dwellings, although they have the same width (Tringham et al. forthcoming). Associated features such as a relative lack of storage facilities and vessels and a high percentage of wild animals among the faunal remains strengthened this impression.

The majority of Vinča culture settlements are characterized by a greater degree of complexity in the use of residential household space than preceding or succeeding settlements in the Danube and Morava basin (Chapman 1981). This complexity is expressed especially in the division of space as separate rooms within the buildings. It is traditionally suggested that separate rooms reflect a more elaborate division of household activities and tasks. Such complexity in the use of space is also thought to be reflected in an increasing number of facilities and activities, such as storage and food preparation and cooking, being moved within the walls of the dwellings. The reconstruction of room function and activities, based on the spatial patterning of the material record of production, consumption, and distribution on and around the house floors (e.g. Ciolek-Torrello 1985; Dodd 1984; Hill 1970; Kramer 1982b; Reid and Whittlesley 1982; Watson 1979), has been an important aspect of traditional architectural studies of Vinča culture settlements (Benac 1973; Stalio 1968) and Neolithic and Eneolithic settlements of Southeast Europe in general (Dumitrescu 1965; Kričevski 1940; Passek 1949).

The houses at Opovo are different from the Vinča pattern of increasing complexity in the use of space on all counts. There is only one example of an excavated dwelling at Opovo with an internal division into a room and with an internal oven. In Building Horizon 1, a low partition (30 cm high) separated a clay-floored area with a bread-oven and food-preparation facilities from the rest of the dwelling in House 2 (figure 4.6). Although not a complete physical separation of these tasks, the partition does reflect nevertheless a subdividing of space. In Building Horizon 3 (the earlier one), no such room subdivision existed on the floor of House 5 or 6, nor was any formal food-producing area identified, for example with an oven or built-in storage pot inside the house. Within House 5, however, there was a vertical subdivision of

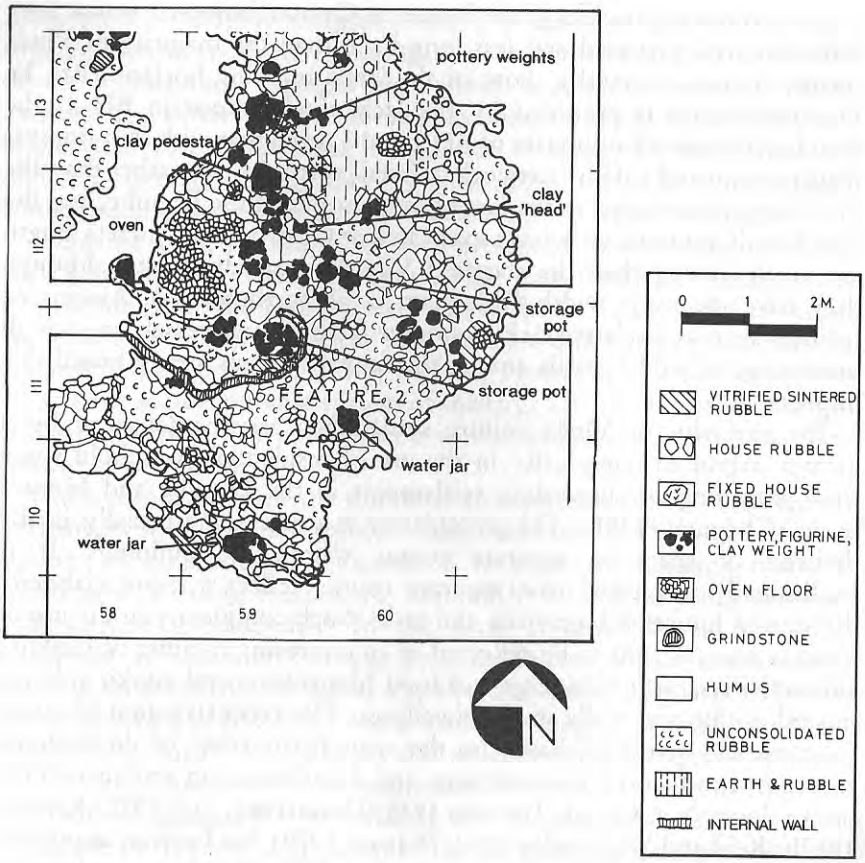

Figure 4.6 Ground-plan of House 2 of Building Horizon 1 at Opovo.

space provided by the construction of a partial second storey or loft (figure 4.7). Upper storeys have traditionally been interpreted as the location of internal food storage, rather than as provision of additional living space for a growing household or as specially segregated space for other purposes. Our conclusions as to the significance of the second storey of House 5 must await the subsequent analysis of associated materials, but I am trying not to assume that the loft was for grain storage. Second storeys have hardly ever been reported from other Late Neolithic settlements of Southeast Europe, but this is probably the effect of focusing excavation of house debris on clearing the floor and its contents, rather than on a detailed recording and excavation of the collapsed superstructural rubble (where the second storey is to be found), as we did in the Opovo project.

Our conclusion in a recent report (within the original theoretical framework of the project) as to the households represented in the excavated houses at Opovo is that they are shorter-lived and/or less well established than those in the large villages such as Selevac, Gomolava,

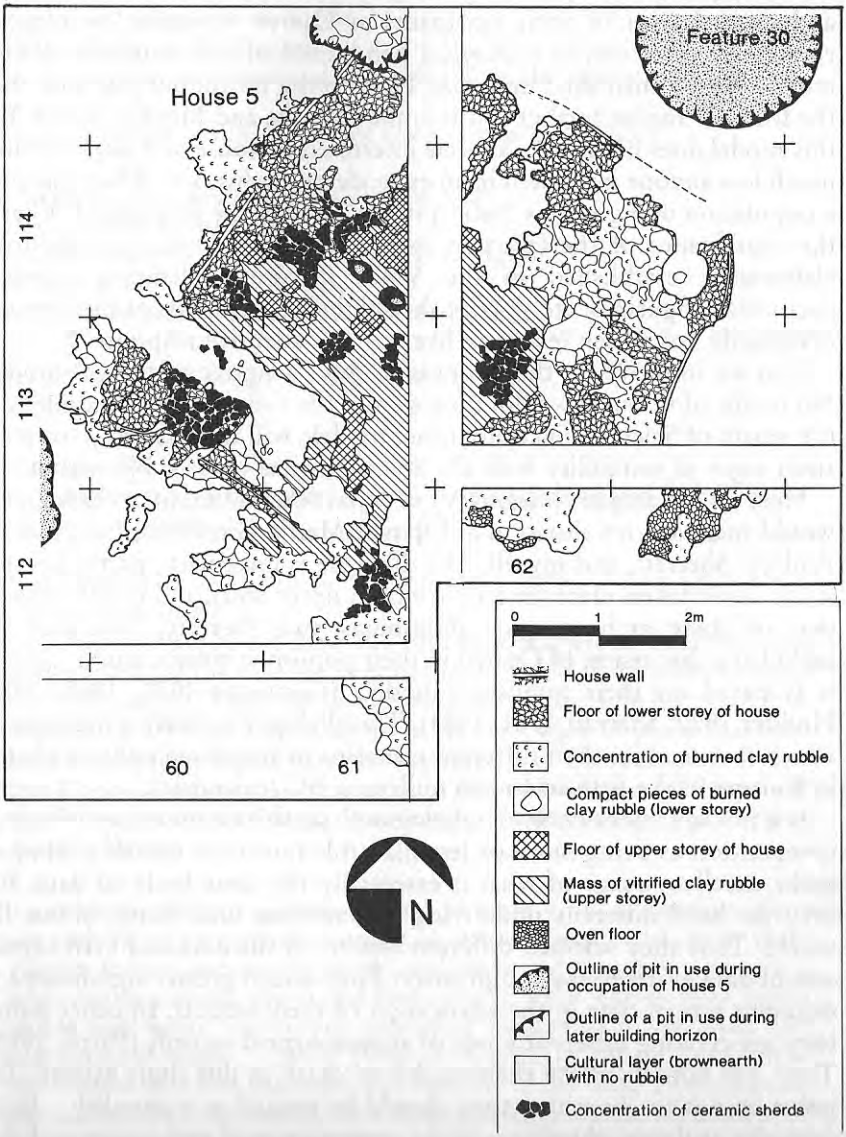

Figure 4.7 Ground-plan of House 5 of Building Horizon 3 at Opovo.

and Vinča in the primary agricultural regions of Southeast Europe at this time, and that their activities and/or size and complexity was very different from the latter (Tringham et al. forthcoming). Moreover, these differences became more marked during the later occupation of the village. Traditionally, archaeologists would have had no problem in explaining these differences by the fact that the "domestic" structures and the behavior of their occupants at Opovo represent an adaptive response to the special ecological conditions of the southwest Banat, more forested than the Hungarian Plain to the north, but marshier than the forested region further south in the Danube and Morava basins. But this model does not satisfy anyone interested in household organization, much less anyone interested in an engendered prehistory. What was such a population doing in this "inhospitable" area in the first place? What is the significance of the fact that they have much of the same cultural elaboration established in the Vinča culture settlements (ceramic decoration, figurines, etc.)? Why should their households or their houses necessarily have been less long-lived as an "adaptive response"?

Can we improve on the richness of this "adaptive response" model? No doubt of it, but if we do, then we run the risk of losing credibility in the world of Science, since any other models will take us away from the strict rigor of testability with the archaeological record. No matter.

Here is a dialogue (imaginary) of what four different archaeologists would make of our situation at Opovo: Marija Gimbutas, Ian Hodder, Andrew Sherratt, and myself. The dialogue is imaginary, partly because it has never taken place (and I am sure it never will), and partly because two of these archaeologists (Gimbutas and Sherratt) have not yet included a discussion of Opovo in their published works. But in general it is based on their published models (Gimbutas 1970, 1980, 1982; Hodder 1990; Sherratt 1981, 1984). This dialogue is really a medium by which to introduce their different scenarios of social and cultural change in Europe in the fifth to fourth millennia BC (calendric).

It is not my intention in this dialogue to set these eminent people up in competition as being more or less plausible (not even myself). They all make excellent usage of what is essentially the same body of data. But they do hold different underlying assumptions and beliefs about the world. Thus they selected different aspects of the data and even certain sets of data as deserving top priority. They attach greater significance to different sets of data in the advocation of their models. In other words they are creating different kinds of archaeological records (Patrik 1985). They will not talk about their models in detail in this short exposé. The point here is to show that they should be treated as a plurality – but a critically analyzed plurality – in the construction of prehistory, and that certain dimensions of the problem have hardly been considered.

A Radio Story

Interviewer: *Here is a picture of the ground of House 2 at Opovo (figure 4.6) What do you think of the "room"?*
AS: A pathetic attempt to recreate the grandeur of the great households in the aggregated villages of the south.
MG: A temple to the Goddess.
IH: A symbolic expression of the richness of the dramatus domesticus, women, domus.
RT: A separation of domestic labor from surplus production.

Interviewer: *Who built the houses of Opovo?*
AS: Men while women were hoeing.
MG: Men under women's (the Goddess's) direction.
IH: Women under men's direction.
RT: The coresidential cooperative productive unit.

Interviewer: *What did the women do at Opovo?*
AS: Hoed.
MG: Everything.
IH: Practiced a secret subversive power.
RT: I don't know; I can't say; I presume they participated in household cooperative action.

Interviewer: *Why do you think Opovo looks so different from other Vinča culture settlements?*
MG: I don't think it does. I think that they have exaggerated the differences. The influence of the Goddess is as strong at Opovo as she is elsewhere in the Vinča culture. These people have been demonstrated to be cooperative and peaceful. There is no evidence of the new wave of patriarchal war-loving Kurgans that destroyed the peacefulness of Old Europe. It is true that Opovo is poorer, and demonstrates fewer of the accomplishments of the civilization of Old Europe. But what else would you expect out there in the marshlands?

IH: I think that Opovo is similar enough to other Vinča culture sites that I can include it in a general treatment of symbolic and social life in those villages. In fact I can use Opovo as a well excavated site typifying a period when the domus was the central focus of life, when women were highly visible and when "individuals submitted themselves to the rules and constraints of larger . . . units" (1990 ch. 4, p. 21) in Europe. Yet it's true that Opovo shows an unusually high visibility of the "wild", the untamed, the uncontrolled, the outside. This could possibly be an early indication of the "collapse of the domus and the outside becoming the new discourse of social life" (1990, ch. 4, p. 28). Statistically, though, it is only one site.

RT: It is a well-known fact that I don't find the model of adaptation to the ecological context of the marshland of SW Banat satisfying. I prefer to interpret the situation at Opovo in the form of two possible scenarios. In one I think that Opovo was a special purpose, short-term, perhaps seasonal, settlement oriented toward a limited range of activities, such as the extraction of certain raw materials, for example antler, or carrying out exchange for minerals from the mountains on the northwestern margin of the marshlands. As an alternative I suggest that Opovo was a more permanent bud-off from one of the larger Danube Vinča culture sites, for example Vinča itself, of a "junior" or "disenfranchised" household(s) into the agriculturally marginal land north of the Danube. In both these alternative models we would expect to see much of the exchange network and symbolic expression and elaboration of the Vinča culture settlements intact at Opovo, as indeed we do. What is changed is that the full complement of production activities (tool production, storage) is not present at Opovo. In my opinion, the architectural evidence and the nature of the tasks selected and resources exploited favors the model of a more seasonal short-term specialized nature for the prehistoric village at Opovo. One of the important sources for enrichening our picture of the village will be the results of the current investigation on the nature of the household organization and the relocation of residence at Opovo in contrast to that of other Vinča culture and Tisza culture sites. We would expect the household to have a different form and activity according to each of these models. But in both cases they would look different from the well-established stable households of the aggregated villages in the Danube and Morava Valleys. According to model of seasonal specialized settlement, only a limited number of people would occupy the settlement, possibly being predominantly one gender, or one age group, so that one would not expect the kind of cooperation in production, distribution, and reproduction as on the larger sites. According to the model of "bud-off" settlement, we would expect to see fully developed stable household organization, with cooperation in production and distribution, but at the beginning of their developmental cycles and with strong ties of alliance with the "homeland" as well as new exchange patterns with other areas. According to the "adaptation response" model – the one I don't like – we would probably expect to see poorly formed household units, probably the loose kinds of cooperation as was described for the Early Neolithic situation in the Danube and Morava Valleys.

AS: What we see at Opovo is the Secondary Products Revolution in action. I think that Opovo is an early example of what became the norm in Southeast Europe in the succeeding Late Copper Age (Eneolithic), that is a small hamlet scattered in an area that should rationally be regarded as "marginal" to the previously occupied easily cultivated soils. Large aggregated villages such as Selevac and Vinča were the more typical form of Late Neolithic/Early Eneolithic settlement. The political community coincided with such villages in which several lineages would have been

aggregated. The role and power of lineages in these villages were subordinated to cross-cutting institutions based on age grades, ritual groupings, etc. Villages such as Selevac and Vinča were "organized on an established territorial basis, with public rituals and symbolic analogies based on female images", and they were "characterized by stable flows of regionally-acquired goods" (Sherratt 1984: 132).

The later Copper Age villages, of which I think Opovo is an early example, by contrast, were dispersed settlements in conditions of unconstrained expansion in which genealogical units (wide-ranging lineages) were the primary units of political and productive and distributive cooperation. The individual head (male) of a lineage provided the focus of allegiances over a wider area and his power/political role is expressed in inter-regional symbols of rank. Along with this system of political control went a change to "a greater emphasis on exotic goods and longer distance trading contacts . . . in which information-carrying items took precedence over more basic commodities. Their territorial basis was less stable, their ethos was a competitive and self-aggrandizing one, with symbolic analogies based on the image of the warrior male" (Sherratt 1984: 132).

The change of settlement pattern represented by sites such as Opovo was ultimately the result of adaptive innovations in technology (plough, milk- and wool-production, wheeled transport) in response to modifications of the resource base caused by such factors as a reduction in soil fertility, growth of population beyond the carrying capacity of the site territory, and deforestation. I hypothesize, that, as part of the social adaptation to these ecological problems, there were marked changes in social and political organization, reflected as "a shift from societies organized primarily on the basis of community . . . to an increased emphasis on the potential of kinship for forming wider networks of alliance" (Sherratt 1984: 132), expressed in the declining importance of settlements and rising prominence of cemeteries (which map social relations symbolically that are no longer visible in residence patterns).

This imaginary dialogue has demonstrated I hope that none of the interviewees is practicing a feminist archaeology. Each, however, has something valuable to offer on the path to creating an engendered prehistory of Southeast Europe.

Marija Gimbutas's study, as mentioned earlier in this paper, is a "remedial feminist study" which has put women into the pot of archaeological interpretation and has stirred vigorously, but her basic epistemological framework and theoretical standpoint remains very much in the center of traditional Establishment archaeology in Europe, although certainly her work does not satisfy the rigors of the New Archaeology program of scientific hypothesis validation. Her ultimate aim is to write prehistory in large (macro) terms. Behavior is normatized on a large-scale in time and space to paint a picture of massive and

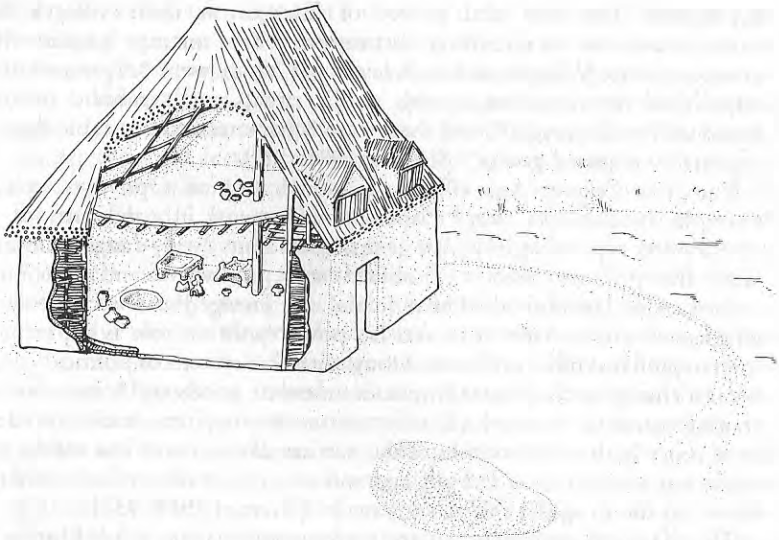

Figure 4.8 Fanciful but faceless reconstruction of House 5 at Opovo.

generalized change, for example from matriarchal to patriarchal, or from equalitarian to hierarchical.

Ian Hodder's is an example of "post-processual archaeology" which turns on its head the traditional "processual" passive reflective role of material culture into that of an "active" medium for and symbolic expression of, for example, the tensions of gender relations in material culture including architecture. This, as mentioned above, is one of the essential steps to viewing and creating the archaeological record from a plurality of standpoints, including that of the women of prehistory, that would characterize an engendered prehistory. Hodder professes to be interested in the social action of individuals within their social and political context. In practice, however, he focuses on the "domus" that has an abstract, emotional, almost psychic quality emphasizing the separation of the "domesti" from the "outside" (Ladurie, 1978). Within this context, the behavior of the actors, their household, their village become normatized as he extrapolates from one site (Opovo) to the whole region.

Andrew Sherratt focuses on the effect of long-term transformations of social and economic relations on regional settlement patterns in prehistory. In keeping with the mainstream of social archaeology, he sees that the most important factors in the transformational process involved demographic growth and increasing control of the material world:

changes in land-use, settlement pattern, subsistence and agricultural and other productive technology. These changes are typically at a macroscale and involve political decisions and power brokerage. It *is* essential to consider this scale of action, but here this is done at the expense of a consideration of variability in the domestic sphere itself. Gender relations are implied throughout his studies, but not specifically addressed. Both Hodder and Sherratt rely heavily on generalizations that correlate material culture with social forms and relations and with social divisions of labor (hoe/women; plough/men).

My own research of the social relations of production in prehistory, architecture, household organization, and domestic production, is the only one of these four that focused on the variability of social action at a microscale, and thus has contributed to this essential prerequisite for an engendered prehistory. In spite of its potential for an engendered prehistory of Southeast Europe, the essential aim of my study at Opovo *until now* has been to attribute aspects in the use-life of a building to a *faceless* unit of cooperative production and distribution and/or coresidence, a unit without gender, a unit without age, a unit without personae (figure 4.8).

The problem has been that "faces" are not demonstrable in the archaeological record, they cannot be "operationalized" archaeologically, they are not testable, and thus they cannot be taken seriously by respectable scientists. It moreover takes a great deal of effort and imaginative power to consider them. But they *are* there (figure 4.9). To "engender" my original topic of the transformation of households in prehistoric Southeast Europe means, in essence, to deal with this problem. It does *not* mean to search for the material correlates of gender roles in the architectural phenomena of deep prehistory as a primary research goal. Nor is it a "remedial" attempt to put women into prehistoric household cooperative action. The aim must be rather to produce a visibility of gender when I visualize such an elemental social unit as the household.

Archaeologists tend to base their visualization of gender relations and the sexual division of labor, either explicitly or enthymematically, on the cross-cultural generalizations about the family or household made by social anthropologists and social historians (Boserup 1970; Goody 1969; Laslett 1972). Such general correlations between social institutions and division of labor and levels of technology as hoe cultivation/women's labor/covert male power or plough cultivation/men's labor/overt male power have been severely criticized as treating the family and household as a social entity whose contextual variability and internal division of activity is not of crucial importance (Yanagisako 1979). These generalizations have also encouraged the highly criticized separation of

the domestic sphere from the extra-domestic or public sphere (Moore 1988: 21–4).

The generalizations about gender relations are somehow more attractive and easier to deal with (can be grasped on a superficial basis more quickly by archaeologists who are always desperate for a quick entrée into social theory, especially those not trained in it from the start of their careers) than those that emphasize variability and the investigation of the social organization at a microscale in a specific historical context (e.g. Moore 1988). Archaeologists need some solid "givens" to hang on to. For prehistorians, at least, the historical context is not given, we have to create it. If household activity and gender relations are also not a given, predictable entity, then where do we start our construction of prehistory? One answer lies with the material culture itself (Hodder 1986).

The solution to "adding faces" to the prehistoric households, however, lies (contrary to the opinion of many of my empiricist friends) *in enriching the archaeologists' models and general knowledge* of gender relations within household and families. The anthropological/ethnographic and historical literature is rich with details of the variability of gender relations at a microscale and the essential relationship of these to

Figure 4.9 The reconstructed House 5, with faces, thanks to Pieter Bruegel.

macroscale contexts. It is rich in examples that illustrate the importance of "domestic" labor in household production and the negotiating and political power that women and families have beyond that of physical reproduction (Moore 1988; Yanagisako 1979). We do not have to "identify" this rich variability in the archaeological record but we must be aware of it. Why simplify prehistory?

To engender prehistory allows and encourages us to go much further in our understanding of architectural variability in terms of the dominance relations and tensions between males and females, between siblings, between neighbors, between age-groups as they move within and between the space that we call the built environment. A significant change in the archaeological pattern of settlement, such as, for example, that of which Opovo might be a part at the end of the Neolithic in Southeast Europe, is the dispersal of settlement on agriculturally marginal lands. This has considerable implications for labour access and resource procurement for a household as a whole, but an interest in gender relations enables us to broaden these implications to include a rapid decrease in the pooling of labor between households and extra-kin support especially for the female members of the households.

Death of the House and the End of the Household Cycle

One way in which the archaeological record can be transformed by the richer set of theoretical formulations that is stimulated by an interest in household organization and gender relations in prehistory is to study the relationship of a household to a locus or loci through time. House replacement, that is the placing of buildings in relation to each other in time as well as in space, has always been an important aspect of archaeological research, usually disguised under the dehumanized title of "chronological sequence of building or occupation horizons". This aspect of the archaeological record, however, takes on new significance when viewed within the context of the rich body of ethnographic observations and historical records of the ownership and inheritance of land. There is much evidence for links between the treatment of the dwelling in the latter days of its use-life and the placing of a new house on the one hand and the ownership and inheritance of property, especially land, as well as gender relations that focus on land on the other hand (Moore 1986: 91–102). The distributive role of the household between generations by inheritance comprises a most important factor in determining the size and nature of a household and its built environment (Goody 1969, 1972; Laslett 1972).

For those who wish to take their prehistory beyond the variability of

the household to the very heart of the transformation of gender relations, one must grasp the richness and complexity of modeling links between domestic organization, forms of inheritance, and the sexual division of labor (Boserup 1970; Goody 1976; Moore 1988: 45). In European prehistory the modeling of changes in inheritance systems has closely followed the generalizations of Goody and Boserup, mentioned above, that are linked to general models of socioeconomic transformation. It has been suggested, for example, that a Neolithic–Early Eneolithic social formation characterized by a hoe cultivation/women's labor/covert male power/control of labor/inheritance through the household correlation was replaced by a Late Eneolithic–Bronze Age social formation that was characterized by a plough cultivation/men's labor/overt male power/control of land/interest in land ownership and inheritance/inheritance through extra-household units correlation (Bradley 1984; Hodder 1984, 1990; Sherratt 1981, 1984). The point is here that these questions of significant social transformation are considered always on a macroscale. If considered at all at a microscale, it has only been by observation of archaeological burials and grave-goods. The question of inheritance has never been linked either conceptually or analytically to changes in the destruction, abandonment, and replacement of houses.

In Southeast Europe, the period of greatest intensification of production and permanence of settlement, as well as the universal practice of burning of houses described below – the Late Neolithic and Early Eneolithic – coincides with the practice of rebuilding a new structure in a completely new location using completely new foundations (figure 4.10) (Chapman 1981; Tringham 1990). At Selevac, for example, such horizontal displacement over a 200–300-year period resulted in occupational materials being spread over a 53-hectare area of the site (Tringham 1990). What is the significance of a change to horizontal displacement of new buildings in a period of supposed decrease in availability of new land as has been hypothesized in the Late Neolithic of Southeast Europe (Chapman 1990; Sherratt 1984)?

A most interesting topic from the point of view of ownership and inheritance of land for the prehistory of Southeast Europe is the variability in the distribution of mound ("tell") settlements and flat settlements (Davidson 1976; Kosse (Krudy) 1968; Rosen 1986; Tringham 1990). What is interesting is that horizontal displacement is not mutually exclusive of the formation of a "tell". The "tell" settlements of Southeast Europe, e.g. Vinča, Gomolava, Karanovo, usually involve a certain amount of horizontal displacement of consecutive buildings, in contrast to those of the Near East, which are characterized by vertical superimposition of buildings (Banning and Byrd 1987). The traditional explanation for the concentration of deposits in mounds or "tells" in

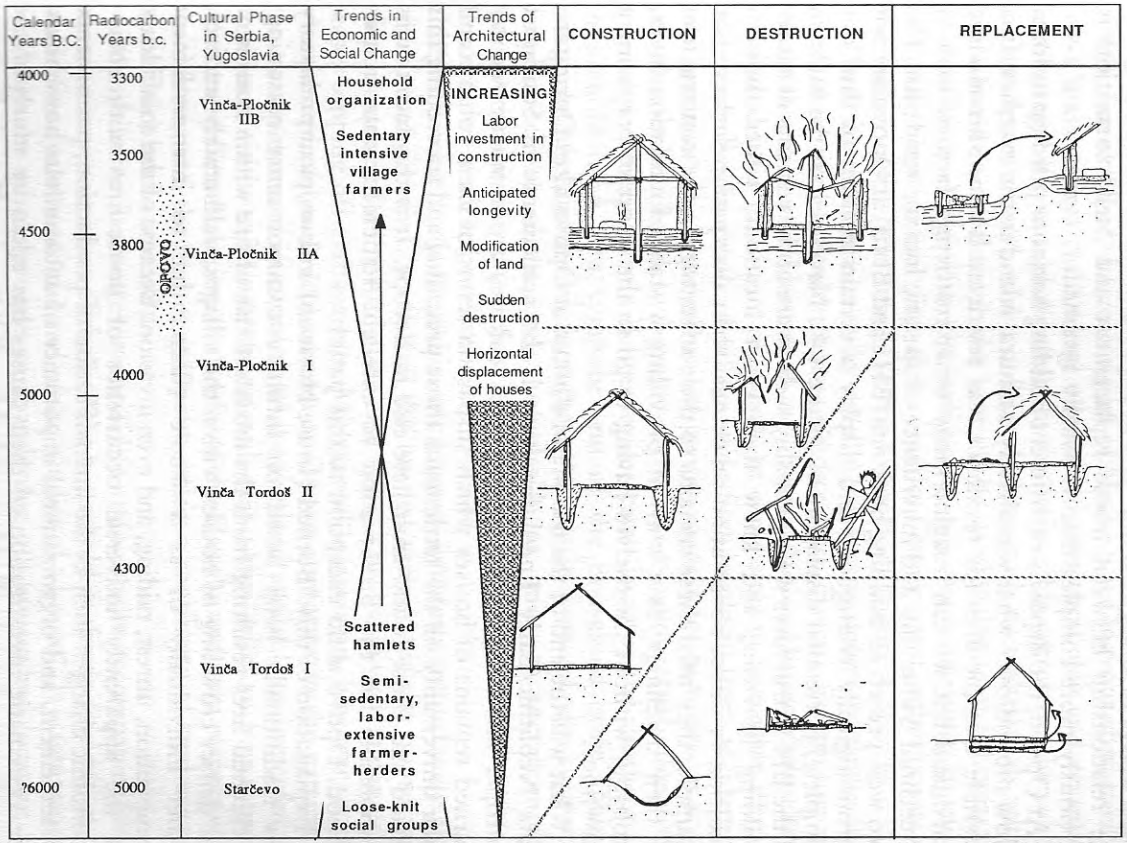

Figure 4.10 Pattern of change in house replacement and socio-economic transformation in Neolithic–Eneolithic Southeast Europe.

Europe is in terms of adaptive response to particular topographic restrictions on settlement, such as river or marshland. But there are certainly many other reasons to restrict residence locus, such as unequal social access to and ownership of land for residence. Thus the nature of the relationship between house replacement and "tell" formation is obviously more complex than is usually assumed.

At Opovo, the deposits of different building horizons piled up to form a low mound over what was already a natural island in the marshes. It is possible that the area for residence was restricted by the surrounding marshland but we can imagine many other restricting factors, such as household ownership and inheritance of specific loci for residence. The two well-preserved building horizons (BH1 and BH 3) at Opovo are not chronologically contiguous and display a certain amount of (but not complete) horizontal displacement (figure 4.4). Nevertheless, the builders of the BH 1 structures were probably well aware of the remains of earlier building horizons, and knew their exact locations, either through tradition or direct observation on or near their occupation surface. For example, they used these remains to their advantage in the construction of their dwellings, as a way of providing a stable foundation layer, probably without themselves having to go to the effort of preparing a clay floor.

What was the end of the use-life of a house at Opovo like? During the Late Neolithic/Early Eneolithic period, the settlements of Southeast Europe, including those of the Vinča culture, are characterized by burned remains of houses which surpass in terms of their volume and their universality on the settlements those preceding and succeeding this period and those found anywhere else in Europe. It is this fact that is responsible for the preservation of excellent settlement data in this period. Opovo is no exception to this pattern.

Explanation of this "Burned House Horizon" in European prehistory has traditionally been based on intuitive common-sense reasoning: accidental fires resulting from the increased use of fire within houses or the denser crowding of houses within the villages; deliberately set fires, either individually or as a whole village, due to inter-settlement competition, unrest, raiding, and even invasion. Because of the coincidence of the apparently universal occurrence of house-fires with socio-economic changes such as the intensification of production, permanence of settlement, and organizational importance of autonomous households, the systematic investigation of their causes has taken up much of the research effort in the architectural sphere at Opovo. Our preliminary results show that there were no houses that did not burn. In addition, the lack of burned materials in the areas between houses indicates that the houses burned in separate fires.

The houses burned at temperatures that in some parts of each house reached over 1000 °C. Such temperatures are regarded as very high for accidental fires of wattle-and-daub houses and indicate that the fires are likely to have been "helped" by deliberate fueling and tending (Kirk 1969). For example, the central part of House 5 at Opovo is characterized by a massive area (ca 3×3 m) of vitrified structural clay and ceramics, indicating a fire of high intensity at this point; this represents the rubble of the upper storey, which collapsed along with its melted pots which were fused to the floor in the heat of the combustion, and fell onto the lower floor along with the collapsing side walls burying, burning, crushing, and preserving the ceramics, stone, and other contents (including textile and string) of the lower floor (figure 4.7).

There are many reasons why dwellings might be destroyed deliberately in separate fires, for example, to eradicate pests, insects, disease. It is also likely that the burned houses of the "Burned House Horizon" should be explained by a multitude of causes. In some sites in Southeast Europe they may be the result of village-wide fires resulting from accident or deliberate arson. Some of us are gently suggesting, however, that the destruction of a house by burning in the Late Neolithic/Early Eneolithic period was frequently a deliberate act carried out at the death of the household head as a symbolic end of the household cycle. This is certainly an "attractive" hypothesis to me in view of the suggested importance of autonomous households as units of socioeconomic cooperation at this time in Southeast Europe. But how would this idea fit into our engendered prehistory of Opovo?

Efforts were made in prehistoric Opovo, evidently immediately after the burning events, to flatten the burned remains of the houses and deposit some of it in pits, in one case filling a probable well, and in other cases topping off garbage pits. My general impression, however, is that the pattern of house replacement at Opovo conforms to the pattern of house replacement in contexts where the land available or preferred for residence seems to be more restricted, that is the "tells" such as those at Vinča and Gomolava, on the banks of the Danube and Sava respectively. In this respect, the houses at Opovo reflect a continuity of land use and (possibly) land ownership.

Many dramatic changes in social life undoubtedly accompanied those terms that we write so blithely: "the household in the decline and at the end of its cycle" and "the end of the use-life of a house". It is tempting to envisage the whole process of abandoning old houses and locating and constructing new ones coldly and objectively. Recent ethnographic and non-Western architectural studies, however, abound with accounts of the significant impact that these processes have on the lives of the men and women involved. A "house" is part of their social lives, with a life-

cycle of its own, even to the extent of being a person (Alexander 1979; Blier 1987: 118).

Fantasy>Fact: How to End the Use-life of a House

She watched the house burn.

He had died. He's strung up in the tree now, safe. Now it's time to kill the house.

Finally after all these years living in these godforsaken marshlands. Stuck in this place, with no one to turn to or to help, except him, or worse, *her*.

It was alright for him, he could escape *that* one by running off to the village. "Time to take the deer timber down there" or "my turn to tell the story to the new ones down there." And off he'd go. Leaving me. And her. And the others, but they don't count.

It's burning nicely now. What a crummy house they built. Nothing but kindling for its bones. Only an outer skin. I'm surprised the loft didn't fall down on our heads, with all his pots and her rugs up there. Well it doesn't take long to collapse and kill it. They wouldn't think much of this house down in the village. You can't kill a house by yourself down there. But then you can't do anything by yourself down there.

Here it goes! Watch the flame! Look at it burn! Yellow, purple, sunlight, moonlight, orange, marshlight. Ugh! What's that smell? Must be her clothes! Or maybe he was hiding something else up there. Ooops, step back a little. My hair's scorching. It makes my eyes water. I'm crying! I haven't done that for a long time.

What a roaring! It sounds like those dreadful other men – the deer – when they come looking for us – the women. Why do they always have to fight? There go his pots. All killed. Finally! They seem to be throbbing in the middle there.

Mustn't let the fire die, or he'll come back. More wood. Pile it up a bit more here. Let in some more air! A house must breathe to die. Push the air into its cavity. That's better. Flaming again! Burn his pots! Kill his stuff! Now I'm in charge. The circle is complete. I can go back to the vilage. Away from the heat, away from the creatures that torture and bite. Back to village noise, complaints, shrieks, laughter, gossip, friends, life.

Afterword

Microscale archaeology of the social relations of production in prehistory – the study of residential architecture and household organization and production – is an essential prerequisite for an engendered prehistory and, I would argue, any kind of social archaeology. When carried out with a feminist standpoint it allows us to engage in the study of a prehistory "with faces", faces behind which lie gender, age, hopes, fears, aspirations, "the whole catastrophe" as Zorba has described to us. Such faces may be less visible to those who prefer to study only large general trends and patterns of adaptive processes, but they are certaintly not irrelevant to the trajectory of human transformation.

NOTES

1 The quotations have been taken from a secondary source, Eisler 1987: 13–14. The book from which they are supposedly quoted is Gimbutas's *The Early Civilization of Europe* (1980), Monograph for Indo-European Studies 131, UCLA. I have not seen this book. It is not listed in Books in Print. Nor is it listed in the long bibliography of Gimbutas's works in her Festschrift: S. Skomal and E. Polomé, eds (1987) *Proto-Indo-European: The Archaeology of a Linguistic Problem*, Institute for the Study of Man. I imagine that Eisler has seen the monograph in manuscript form. In this case, however, I am not sure how reliable are her quotation skills, since she has surely misquoted Gimbutas who, according to Eisler (p. 14), in chapter 2, p. 32–3 of this non-circulating book, describes "the 53-grave cemetary [sic] of Vinča, (in which) hardly any difference in wealth of equipment was discernible between male and female graves . . ." Gimbutas certainly knows that only 11 graves have been found at Vinča that date to the period of her "Old Europe" and that, in the Vinča culture as a whole, no more than 23 graves were ever found together in a cemetery. Perhaps Eisler misread Varna (in northeast Bulgaria) for Vinča. However, this is the only place where Gimbutas sets out her ideas of the nature of Old European society, and for that reason I am using it here.

2 The use of the term "domestic" from this point in the paper will return at frequent intervals. As with many terms that try to express social action at a microscale in more humanistic ways, "domestic" is as fully loaded with meaning for us as are the terms "house", "household", and "family". I have sometimes, however, not been able to avoid the use of the word. In doing so I refer the reader to a discussion of the term "domestic" in Spector's contribution to this volume with which I concur fully. I am also aware of the danger of its usage in leading towards an implicit acceptance of a whole set of dichotomies such as domestic/public, inside/outside. Since it is just these dichotomies which I am trying to avoid in my engendering of the prehistory

of Europe, I would also refer the reader to the excellent discussion of the use of this term in Moore (1988: 21).

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