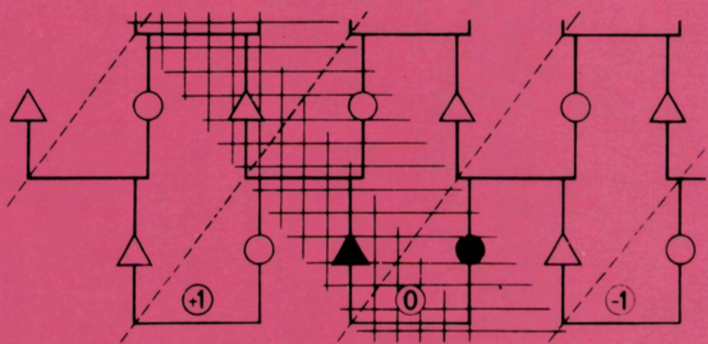
ANALECTA PRAEHISTORICA LEIDENSIA



1979

XII

ERRATA

ON BANDKERAMIK SOCIAL STRUCTURE

P. VAN DE VELDE

ANALECTA PRAEHISTORICA LEIDENSIA XII

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PUBLICATIONS OF THE INSTITUTE OF PREHISTORY
UNIVERSITY OF LEIDEN

P. VAN DE VELDE

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An Analysis of Pot Decoration and Hut Distributions from the Central European Neolithic Communities of Elsloo and Hienheim

1979

LEIDEN UNIVERSITY PRESS

THE HAGUE / BOSTON / LONDON

Distributors:

for the United States and Canada

Kluwer Boston, Inc.
160 Old Derby Street
Hingham, MA 02043
USA

for all other countries

Kluwer Academic Publishers Group
Distribution Center
P.O. Box 322
3300 AH Dordrecht
The Netherlands

ISBN 90 6021 448 X (this volume)
ISBN 90 6021 450 1 (series)

PRINTED IN THE NETHERLANDS

CONTENTS

INTRODUCTION

This study is intended to be a description of the social structure of two Bandkeramik (the first Central European horticulturalists) villages; yet a substantial part of it has to do with their pottery decoration. In fact, it was originally conceived of as a study of Early Neolithic pottery; more specifically, it should have been one of a series of reports on the Hienheim excavations. I was associated with these excavations as a graduate in the 1970 and 1971 seasons; they were conducted by the Department of Prehistory of Leiden University. From the very beginning I have wanted this study to result in more than "mere" pottery description: parallel to my training in prehistory I also studied ethnology. In the end, this latter study has had considerable influence. It was perhaps in my first year that I read Lévi-Strauss' *Tristes Tropiques*, and I was very much impressed by his analysis and interpretation of face paintings and offprints of these produced by the Caduveo of Brazil. Something similar should be possible with pottery decoration, I thought naively. When later, during ethnographical fieldwork in Spain – perhaps one of the heaviest sensitivity trainings imaginable – I read *Structural Anthropology* by the same author, again his analysis caught my fantasy: it should be possible to work out social structure for prehistoric settlements in ways analogous to the Bororó and Winnebago analyses in that book. In the meantime, as a member of the excavating team at Hienheim for a number of months, I became acquinted with the Bandkeramik. Also, by the end of the 'sixties, the influence of Clarke and Binford had become perceptible, Leroi-Gourhan could be read (by some) for a second time, and archaeology was no longer what it had been. That is, when I proposed a thesis on Bandkeramik social structure by means of a study of its pottery decoration, I was not transferred to the nearest asylum but treated instead with the remark: you may prove the impossibility of such an undertaking.

That proof is in the pages which follow, but I do not think the proof is conclusive. As it stands, I have not been able to produce an answer by means of pottery decoration alone – I had to bring in the gravegifts in the cemetery of Elsloo, and data on hut typology and size at the Bandkeramik villages of Elsloo and Hienheim, too. If the book is a bit of a hodgepodge, it is because of these sidesteps.

Yet, there is some structure: the first three chapters are mainly concerned with pottery decoration, the two others with social structure. The book should be seen as a whole, however, no part standing separate from the rest. The pottery part is basic to the social part, and without the latter the detailed analyses of the former must seem pointless.

The first chapter provides a new classification scheme for Bandkeramik pottery decoration: among the classifications in existence, none applied in a satisfactory way to the Hienheim sherds. The second chapter makes a bit of a sidestep: a number of methodical and statistical problems are elaborated on there, and a methodological section has been added. As unrelated as it may appear at first sight, the discussions in this chapter are of considerable importance for the general line of the argument. The third chapter is perhaps the most traditionally archaeological in this study: in an attempt to solve the problem of continuity from Early to Middle Neolithic, the Bandkeramik pottery decoration from Hienheim is presented. The pile of diagrams accompanying that chapter are nothing but the usual sherd corpus appended to such texts, only slightly disguised through a bit of abstraction.

The ethnographic portion of this study has been divided into two parts. The first part, chapter IV, consists of a pilot study of the Linear Bandkeramik cemetery of Elsloo. In order to achieve a more or less rounded picture, *all* kinds of gravegifts have been entered, not just ceramics. Short pieces of ethnological theory are scaled down to alternative and operational models, and the data are matched with these to select the appropriate ones. The result is a set of hypotheses regarding Linear Bandkeramik social structure from a "positional" (or status), a structuralist, and a neo-Marxist point of view. Chapter V, the second part of the ethnography, presents a summary of the existing literature on this topic and abstracts a number of alternative and additional hypotheses from it. Together with the results of chapter IV they are tested against data from the Bandkeramik settlements of Elsloo and Hienheim; the spatial and temporal distributions of ceramic decoration and of hut types and sizes provide the basis. At the end of that chapter the hypotheses and their degree of corroboration have been assembled and presented in listform.

Above, I have named four authors who have been most influential in this study: L.R. Binford, D.L. Clarke, A. Leroi-Gourhan and C. Lévi-Strauss. There are three others who have been as important (and dear) to me: M. Godelier, K.R. Popper, and M.D. Sahlins. It is not customary to enter one's printed counselors among the people acknowledged; I feel, though, that they should be mentioned, if only to let the reader know what to expect.

Leiden/Lingen
3 October 1978 / 24 January 1979

ACKNOWLEDGEMENTS

The present book was precipitated by a study which lasted almost five years. During this time, the Netherlands Organization for the Advancement of Pure Research (Z.W.O.) and the University of Leiden provided financial, institutional and computational support. There is one face in the crowd of administrators I especially want to say thanks to: Mr. O. Brouwer (presently at Groningen State University) who as if by magic succeeded in securing a position for me when I was threatened by a cut-off of salary after my Z.W.O. subsidy had run out.

Intellectual support was provided by many colleagues who went to the trouble of reading and discussing parts of my texts. Among them, there are several who disagree with every single word of this thesis, yet took pains to clarify specific issues. My discussants to whom I owe so much were J.J. Assendorp (now at the Lesser Saxonian Monument Service in Hannover), C.C. Bakels (at the Institute of Prehistory of Leiden University), M. Dohrn-Ihmig (presently at the Landesmuseum in Bonn), A.A. Gerbrands (Institute of Cultural and Social Studies, Leiden University), A. Kuper (at the same institute), L.P. Louwe Kooijmans (of the National Museum of Antiquities at Leiden), J. Lüning (Archaeological Institute, University of Cologne), J.V.S. Megaw (Department of Archaeology, University of Leicester), R.R. Newell (Biological-Archaeological Institute, Groningen State University), J.G. Oosten (Institute of Cultural and Social Studies, Leiden University), P. Stehli (Research Unit for Settlement Archaeology of the Aldenhovener Platte at Weisweiler), M. Tjok Joe (Computer Centre, Leiden University), M. van Bakel (Institute of Cultural and Social Studies, Leiden University), J.F. van Regteren Altena (State Service for Archaeological Investigations in the Netherlands at Amersfoort), W. van Zanten (Institute of Cultural and Social Studies, Leiden University), G.J. Verwers (Institute of Prehistory, Leiden University), A. Voorrips (Institute of Pre- and Protohistory, Municipal University, Amsterdam) and A. Zimmermann (Research Unit for Settlement Archaeology of the Aldenhovener Platte at Weisweiler).

In the preparation of the manuscript I was assisted first and foremost by J.P. Boogerd (Institute of Prehistory, Leiden University) who made the drawings; also, R. Hagesteijn and L. Koolen (Institute of Cultural and Social Studies, Leiden University), W. Meuzelaar and G. Tak (Institute of Prehistory, Leiden University) had hands in it. Then, J. van Iterson-van de Velde and V. Vlek transferred my double-Dutch into readable English.

Besides the support of the scholars directly involved in the preparation of this thesis, F. Tjon Sie Fat (Institute of Cultural and Social Studies, Leiden University) and M.E.Th. de Grooth (Bonnefanten Museum, Maastricht) should be especially mentioned. Without the persistent moral and intellectual support of these people, this thesis could not have been written.

Thank you all.

CHAPTER I

THE CLASSIFICATION OF BANDKERAMIK POTTERY DECORATION

In this chapter a new classification is proposed for Bandkeramik pottery decoration;[1] its novelty lies in its being a closed system in contrast with current, expandable schemes.

1. *Introduction*

The general subject of this thesis is an elucidation of the social structure of the Bandkeramik. Therefore I think a description of the decorated pottery of that culture appropriate: from the Early Neolithic onwards archaeological cultures traditionally equate first and foremost with groups of pottery (Lüning 1972[a],[b]). To be more precise, it has been supposed that if cultures can be distinguished on the basis of their pottery production, then distinctions within a culture should be discernible by means of the variation within that product. Different social groups will have had (and therefore will be traceable by; Schiffer 1976:42) different decorative preferences – no need for assumptions about who *produced* what.

Change occurs in these preferences as a response to gradual shifts in other parts of the cultural system. This change is identical with time – a category to be explained: Godelier (1977 (I): 36); Plog (1973: 21); also cf. C. Lévi-Strauss (in Charbonnier 1969: 38-39). Directly or indirectly, some direction is put this way in the otherwise stochastic variations in the pottery decoration (Binford 1972: 247-248; Clarke 1968: 74-77, 128; König 1969: 20-21). In other words, a very *detailed* analysis of the pottery decoration is necessary.

This is easier said than done: the data to be analysed was excavated in Bavaria and showed its own regional peculiarities for which no classification has been devised to date. Also, a substantial part of it was not of a LBK-*sensu stricto* character, but had more of the characteristics of its successor, the Bavarian Roessen. And although the latter is considered a part of the general Bandkeramik tradition ("Danubian" in Childe's vocabulary), its relations with the former were very poorly understood (cf. Ch. III below). Clarification of these relations (chronological and social) could only result from comparison of the contents of the two cultures.

Research into these relations is conducted within existing theoretical frameworks; concepts like continuity, exchange, etc. will be applied to prehistoric data. The models, or rather elaborations of these concepts are derived from anthropological theory. By means of these models we try to come to grips with the apparent variation. In the models, we attempt to formalize and to structure the relationships between the elements; the meanings originally attached to the relationships and conferred upon the elements are only to be guessed at. A classification is an instrument by which models and data are linked; it renders the data (in this case decoration on ceramics) manageable, amenable to the models; through it, the data can be described in a way relevant to model and research problem alike (also cf. Hill and Evans 1972).

A search through the literature touching on the classification of decorated Bandkeramik pottery (including among others, Butchkow 1935, Buttler and Haberey 1936, Destexhe-Jamotte 1961, Dohrn-Ihmig 1973, Hoffmann 1963, Meier-Arendt 1966, Modderman 1970, Soudský 1956, and Stehli 1973) did not produce any satisfactorily applicable classification (satisfactorily applicable, that is, to my research problem). Deferring a detailed discussion to the third section, it can be stated that the existing classifications were inapplicable because of at least one of the following points: (1) equivocalities in the definition of the classes; (2) openness, or expandability of the classification; and/or (3) regionally, and/or diachronically restricted (again, in relation to *my* research interests. From hindsight, they were not sufficiently detailed either; cf. Ch. IV below).

The apparent conclusion is that a new classification had to be designed which corrected these points. Yet the regional restriction of the other schemes might have been matched by a similar restriction, now to the Bavarian Band-keramik. However, for the very simple reason that the amount of work to be spent on the development of a regional scheme would be only slightly less than that for a pan-classification I put my aim wider. Fortunately, as appeared later on, as some of the models had te be tested and others worked out on data other than that from Hienheim. The data from the Dutch LBK which I used then fitted without difficulty into the classification.

2. *Classification: general requirements*

In the first section I have been using words like "description", "classification", "definition", without defining them. They all relate to one field of ideas which may be indicated by the term "systematics". This latter word is a key term in almost any definition of scientific activity, and implies the ways in which orderly arrangements are made (Dunnell 1971: 25).

It would be superfluous to deal extensively with the interrelations, definitions and backgrounds of these terms, as good textbooks (Spaulding 1974: 513) are available (e.g., Dunnell 1971). However, before criticizing others' classifications and constructing a new one, a short summary of these backgrounds is useful here. Following Dunnell, the most basic distinction to be made is that between "ideational" and "phenomenological". The latter term is intended to cover things or events "out there"; the former relates to things which "have no objective existence, commonly called ideas" (Dunnell 1971:26). Sciences belong to the ideational universe, bur are *about* phenomena. Closely parallel to this dichotomy is that of "definition" *vs.* "description". Definitions are in the ideational realm, and descriptions are about phenomena. As hinted at in the previous section, epistemologically there is more to be said about this dichotomy. Things are slightly more complicated than the simple opposition of ideas and phenomena or of definition and description: definitions pertain to the theoretical *facies*, and descriptions to the phenomenological side of our models. Yet, for a clarification of the process of classification, the dichotomy will do. Sticking to the opposition, things or events are located in time and space; definitions (and science in general) are free of such ties, achronic and achoric. This view of analytical separateness of the two realms implies that they are independent of one another, and though the "ideational" is about the other, only "relevance" or "correspondence" can be established, not "truth". Thus, explanation is derived in the world of ideas (in an explicitly or implicitly logical framework, if scientific explanation is sought), but the facts (rather the description of the facts) will/will not match it. When facts and explanation do not match, and provided the explanation is logically sound, then the explanation as such need not be rejected; it is simply irrelevant (because of wrong premises or conditions), and another explanation has to be designed.

Theory then, belongs to the ideational realm; a theory consists of the set of ideas related to some specified class of phenomena. Archeological theory relates to things which can be excavated: how they were produced, used, discarded, passed into oblivion, were dug out, and studied (Schiffer 1976: 4,5,12). More loftily phrased: theory is about "the classes of phenomena and the principles by means of which these are related" (Dunnell 1971:33).

Insofar as these classes and their relations are of an ideational nature, they pertain to "classification"; their phenomenal (and also their descriptive) pendant is "grouping". Both are procedures to bring order into an unsystematized area, and can be seen as sub-classes of "systematics". From what has been written above, definition has reference to classification; description relates to grouping. Still there are many ways of defining; leaving aside all the more subtle Schoolmen-subdivisions (cf. Bocheński 1954: 90-96), two should be singled out as they have implications for the discussion in the next section: the first type lists all (names of) objects, the second one merely states the necessary and sufficient conditions for class membership (Dunnell 1971: 15-18). To give an example, the first type of definition ("enumerative" or "extensional"), when related to "clan" would consist of a list giving the names of all clans among every society known to possess a clan organization. The second, or "intensional" type of definition of the term "clan" should embody such concepts as exogamy, unilinearity and some others (Lévi-Strauss 1969b: 73). Clearly extensional definitions are easy to conceive; they are difficult to use, however: publication of an ethnography on a hitherto unknown population organized in clan-like groupings would necessitate the amendment of the extensional definition of clan. An intensional definition, though requiring some hard thinking before writing it down, does not run such a risk; an exogamous unilineal group is a clan, where- and whenever it is found.

To return to classification as a procedure to put order into part of the ideational sphere, this is defined by Dunnell (1971: 44) as the creation of units of meaning (i.e., classes) by stipulating the conditions sufficient and necessary to be a member of that class. So, intensional definitions and classifications are at least basically similar. A classificatory scheme (or classification, or typology) is a set of classes which are systematically related one to another through their respective meanings.

As presented here, it should be clear that classification has much to do with theory. In fact, classifications are constructed as an aid in problem solving. The conditions of membership of the various classes are derived from the problem – if only to cut off what is considered irrelevant. Ideally, the classes should together cover the "field of meaning" to which the problem applies (syn.: "universe of discourse"). Also, the relations between the classes are so framed (e.g., what is the most economic subdivision of the classes).
Usually, a field of meaning is called a "dimension" or "variable". A classification may accomodate several such variables, depending upon the problem considered. The classes of a dimension are labelled "attributes"[2] or "traits", and the relative quantities of the traits per variable is called the "mix" (cf. Ch. III). From the problem it should follow which attributes are to be considered relevant (which in turn depends upon the tradition of a discipline); so-called "natural" properties of phenomena have absolutely nothing to do with it (Needham 1975: 365).

So, for instance, if one were interested in the relation between infra- and superstructures of the Inca Empire, one of the relevant dimensions would be that of the titles to the (arable) soil; the attributes of this variable are, then, Sun property, Royal property, and *Ayllu* (community) property; the former two were taken from what had been community property in pre-Inca times (Godelier 1977(I):179), the latter constituted the area to which the *ayllu* titles (i.e., the totality of the local commoner and chiefly lineages) had been left intact (Claessen 1970:171). Obviously, no "natural" property at either the infra- or superstructural level is involved. The classes of a dimension, attributes of a variable, are of a qualitative nature; their definitions state conditions for membership. The members of a class constitute a group, which has a quantitative aspect. In the previous example, if each of the three titles could have been measured as to acreage per community, the relative importance of the superstructural apparatus would be known (Claessen 1970: 171).

From the definition of "class" above, it follows that for the attributes of any dimension each should be unique (or exclusive), and unequivocal (or consistent), and together cover (or exhaust) the entire dimension (Selltiz et al. 1966:392; also Dunnell 1971:52-58, 60). As a classification is part of the ideational realm, it says nothing about the phenomena: some of the classes which are logically possible need not have phenomenal representatives. On the other hand, if the classification is to be adequate, none of the phenomena considered relevant should be put aside as "atypical" or "unclassifiable", as this would imply that data and classification are only partially aligned.

To sum up briefly: a classification has at least one dimension or variable (usually more), every variable has a number of classes or attributes. Furthermore, a classification is an attempt to structure a field of meaning indicated by a theoretical problem (or set of related problems). The definition of an attribute or class consists of the listing of the sufficient and necessary conditions for its membership. Attributes are qualitative (insofar as they are considered ideational); quantities of phenomena may possess them.

Nothing has been said yet about the relations between the dimensions of a classification. The character of these relations differentiates types of classifications (and not of the relations between the classes per se, as Dunnell 1971:65 puts it). These relations are also part and parcel of the theory involved; they are defined in response to the problem under examination. Two possibilities exist: all dimensions are considered of equal importance and independent of one another – the paradigmatic case – or they are considered of hierarchical importance – the taxonomic case – (Dunnell 1971: 68). Of course, these are pure types; in practice every paradigmatic classification has a taxonomic component (there is a statement of what is relevant and what not), and every texonomic classification will show paradigmatic aspects (e.g., in the equivalence of classes per level per branch). I will present examples of this hybrid character in a rapid and partial analysis of two LBK decoration typologies in the next section (pp. 11).

In a paradigmatic classification every attribute (= meaning) is considered only in relation to the alternatives in its dimension or variable; a taxonomic class has meaning only in relation to its super-ordinated dimensions. The paradigmatic type of classification can be likened to a checkerboard approach: two sets of symbols (a to h, 1 to 8) jointly indicate every possible class. In this case, the letters a to h are the attributes (mutually exclusive, together exhaustive) of the dimension or variable "columns"; the numbers 1 to 8 are the attributes of the variable "rows" (Fig. 1a). In the cases of the checkerboard only two variables are needed to describe the entire field. More generally stated, there are no restrictions on the number of dimensions, of course – only the picture becomes more complicated that way.

Much more complicated is the taxonomic classificatory scheme: every attribute presupposes the higher levels, as divisions higher up have produced subsets within any of the lower attributes (Fig. 1b): the attributes m and n may also occur in the classes o and p; they (m, n) are not relevant there, however. In other words: within a taxonomic classification system the statement "o" does not convey any status within it; only when it is added "o", given B, within D, is a non-equivocal assignment made.

In a paradigmatic classification every attribute has its own unambiguous place: column d is the case, or it is not. As in a paradigmatic one, so also in

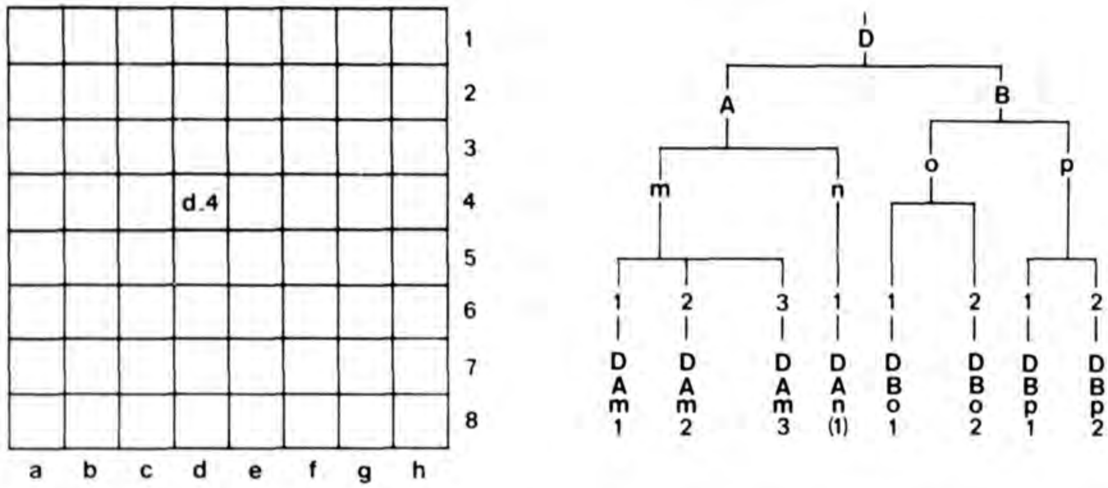


Fig. 1. Two major types of classification: (a) paradigmatic classification: every class requires definition of its position on all demensions (i.e., rows =...; columns =...). (b) taxonomic classification: every class requires definition of its position in its 'branch' (the dichotomy o/p, e.g., being meaningful only in conjunction with the specification: field D, branch B): none of the attributes need be confined to its 'own' branch, although it is relevant only there.

a taxonomic classification: there is no limit to the number of levels; in both cases there will be a statement on the mesh appropriate to the problem.

As an ethnographic example of a taxonomic classificatory system, the traditional territorial division of the province of Kodi on Western Sumba (Indonesia) may be proposed (Van Wouden 1956: 208-219). The province is divided into an Upper (northern) and a Lesser (southern) part. The Lesser part consists of three districts of equal standing with the Upper part as a whole, which in turn is subdivided into two districts. Upper Kodi and the three districts of Lesser Kodi all partake in each other's great ritual tournaments. Upper Kodi's two districts are of unequal status: one has general agricultural rituals to perform, the other horticultural ones. Then too, the agricultural entity is dichotomized, one subdistrict being actually charged with the great ritual tournament, the other with administrative affairs. The horticultural subdistrict of Upper Kodi is not further subdivided (except in villages), although there exists a class of people in the hills who are of lower status but indispensable to them in agricultural fertility matters. These four subdistricts of Upper Kodi can be described as phratries: they are endogamous, ritual units of a number of exogamous patri-clans, each of which is identical with a village. The three districts of the southern half of Kodi are made up of as many sets of phratries (as above), in their turn subdivided into "clan groups" (a local intermediate between clan and phratry). Every clan group consists of a number of exogamous villages or patriclans.

Apparently there is on each level of every branch a different criterion for a further subdivision, which results in a truly hierarchically defined classification.

From the same ethnographic report, an example of a paradigmatic classification can also be drawn (Van Wouden 1956:221-235). The Kodi people employ two dimensions when defining the class of potential brides: patri-affiliation or villages of origin, and matri-affiliation. The patri-dimension has three attributes: non-phratry or outside the district, co-phratry or within the district, and own clan or native village (respectively: not preferred, preferred, and prohibited marriage partner). Matri-affiliation is (nowadays) simply divided into own, and all other matrilineages (respectively: forbidden, required). Intersection of co-(patri)phratry membership and non-matri membership produces the definition of the right partner.

For the construction of a taxonomic classification much must be known about the relationship between the various variables involved; such is not the case for the decoration of Bandkeramik pottery. Taxonomy will therefore be omitted from further consideration: we cannot say whether any dimension of that decoration is more important than any other aspect of it. Thus, due to lack of knowledge a paradigmatic classification has to be constructed. This must be done even though many people think taxonomic structures to be better (Dunnell 1971: 82; perhaps the many cluster analyses published recently may also be taken as a sign of the preference for hierarchical structures. Often, however, the results of an inductive technique are mistaken for a deductive taxonomic classification; cf. blow, p. 7).

Two points need still to be discussed in the context of classification:

1. Off and on, there have been attempts to introduce the idea of "polythetic classes" or ditto "classification" into social science (e.g., Clarke 1968:37-38; Needham 1975). The older Wittgenstein is usually associated with this notion, and then it is called "family resemblances" (Blok 1975: 22-23). It sprang from the realization that some terms like "feudalism" or "peasantry" embody cases which hold *some* characteristics *not* in common. If such a term is defined by (say) five properties, then an object may possess four of them (any selection); in any number of objects, there will be different omissions. For instance:

> the label "prescriptive" does not denote a class of societies to which the law of substitution [i.e. every member of such a grouping is identical to the other] can be applied, for in prescriptive alliance systems (conceived as empirical forms of social life) the categories, marriage rules, groupings, and modes of social action are independent variables (Needham 1975: 361).

Much more extreme versions are possible; cf. Needham (1975:353), or Sokal and Sneath (1963: 14) for identical formulations. Blok and Needham provide different solutions to the problem. The former thinks the best way out is to abstain from "generalization" and "identification" (Blok 1976: 7), and rather to look for a deeper understanding which does justice to both the unique and the general aspects of a case. Needham (1975; he has written more on this subject, cf. Blok 1975: note 86) provides a detailed discussion; his conclusion is very different: perhaps by reference to formal (or logical) principles (such as symmetry, transitivity, and reflexivity) it is possible to achieve results "without reference to any class of entities" (Needham 1975: 365); this leaves the polythetic aspect in a subordinate place (where it belongs; also cf. Chaney 1978).

I think the error made by both is precisely in the discussion of "general terms". They confound the quotidian and the formal theoretical use of words, "emic" and "etic" (cf. Ch.II, section 5). "Classes" and "groups" (to revert to Dunnell's terminology) become telescoped into another. For instance, in the case of "barrow" (a "general" term) one might develop a definition, even a classification, which includes, among other things, absence/presence and position of banks and ditches; absence/presence of a pallisade; spacing of the poles and number of rings in it; horizontal and vertical positions of the interments in it; etc. Then any particular barrow can be considered a variation of the "general" term "barrow" – although it will most certainly not possess all possible characteristics. The main problem in research is to establish the relations between the variants, and for this we need a classification which clearly discriminates the categories.[3]

In other words, their "general" terms are apparently conceived of in an essentialist way (Blok

1975:22) notwithstanding stated purposes (Blok 1975:27) and not as parts of an explanatory framework (or solution) of a problem. For any concrete problem it should be possible to find a terminology to frame the problem, along with proposed and eliminated solutions (cf. Wittgenstein 1922:4.116: "Everything that can be thought at all can be thought clearly").

2. Partially connected with the above are techniques like numerical taxonomy (Sokal and Sneath 1963) or cluster analyses (Doran and Hodson 1975: 158-186). The Sokal and Sneath book has been very influential in archaeology; especially the recommendations by Clarke (1968: 512-567) seem to have stimulated its adoption. Hardly any issue of a journal like *World Archaeology* or *American Antiquity* appears without at least one report on a clustering process. Very loosely phrased, the basic idea of numerical taxonomy is that there exist (objectively) natural taxa, and that these taxa can be recognized by taking into account as many characteristics as possible "without previous arbitrary selection" (Sokal and Sneath 1963:49). Several computational techniques are then developed (and more have been so since) to group or "cluster" the objects studied in a responsible way.

The trouble is, of course, that a procedure normally taking place in the mind of the analyst is now computerized (which is recognized by the authors just quoted). Consequently, the claim of being scientific fades with the apparent inductive nature of the process; but also, and even worse, critique is drowned by the software mystique. Pragmatically, addition of new characteristics (and who would claim to have incorporated all aspects or dimensions?) results in a different structure; addition of new data (even if the first set was truly a random sample, and the second one also) has similar devastating results. Since, however, one of the aims of numerical taxonomy is the reliability (repeatability) of its results (Sokal and Sneath 1963:49), the only conclusion can be that the technique in this way is inappropriate. There can be two ways to use this set of techniques, though: (1) as a *heuristic* device – a computer can handle multidimensional data faster than the human mind i.e., *in the phase between problem perception and solution proposals;* and (2) within an explicitly stated *problem-solving* procedure, with *a priori* established and selected characteristics. See Aldenderfer and Blashfield 1978 and Read and Christenson 1978 for a discussion).

3. *A discussion of existing classifications*

Just as the classificatory scheme to be developed in the next section builds and draws heavily upon existing classifications, and would indeed be impossible without them, so the latter are themselves amended revisions of the earlier ones. In criticizing the more recent schemes, the older ones are automatically covered as well, which allows a considerable shortening of an otherwise tedious discussion. Also, and to the same purpose, I will not discuss all details of every classification but will only single out one or two points of some of them to illustrate the "general requirements" stipulated in the previous section. This necessarily results in a distorted picture of these classifications generally in an adverse way: none of them incorporates all weaknesses. Therefore, *critique below has to be taken to relate only to the specific point discussed.*

The first general requirement (p. 3) is not so much a requirement as it is a differentiation. Enumerative or extensional definitions of classes were opposed to intensional ones. As related to classes, no clean-cut example can be given of such an enumerative definition; the absurdity of listing all pots (or sherds) to indicate some attribute is obvious. On a different level, however, a similar distinction can be made: a dimension, or even a field of discourse may be defined extensionally (by listing the attributes thought to pertain to it) or intensionally (by stating the relevant discriminating conditions). To examplify an enumerative approach, the typology of P. Stehli (1973) for the LBK of the Rhine Westbank may be taken. This typology can possibly be described as being of an extensional kind. At least the sheer number of supposedly independent categories incorporated in the system would suggest this. There are systematics behind it, of course; they remain implicit, however, and the classes are not explicitly and systematically related to one another through their definitions (of which the author is aware: Stehli 1973: 62). It should be possible, in principle, to reconstruct the systems along these lines (cf. Borillo 1970 for an example); then, after such an analysis it might become clear that (not) all classes possible within the research area have been incorporated. Also, some will have been omitted for practical reasons; for instance, when, after consulting the data, only very few examples were found. This may be the case with, e.g., a design like one single row of points over a line in the neck zone, a neck decoration which does not occur in the table (Kuper et al. 1974: 5; Stehli 1973: 69); similarly among the secondary decoration (Stehli 1973:75) an intricate dumbbell motif (Kuper et al. 1975: 213) is absent.

Then, I posed the systematic relation of the classes through their definitions or meanings as a kind of definition of classification itself (p. 3). From the classification by E. Hoffmann (1963: P1.38) of the main motifs of the LBK ware an example may be taken. There, the first two ornaments (volutes) are identical although mirrored, except for an accompanying parallel line on top. What is differentiated this way is *not* the occurrence of the main motif "volutes" (which according to the accompanying scheme is common to all phases) but the occurrence of one type of embellishment which is not that common. In the same way, the fourth ornament in Hoffmann's Table is merely a further wound scroll – which cannot be separated from the first ornament if small sherds are to be classified, and which may be found in a configuration like the main motif no. 1 (e.g., Hoffmann 1963: Pl. 4 no. 13), or with motif no. 3. If the levels (here: main motifs, secondary embellishments, and developments of main motifs – to use the terminology of the section 4 of this chapter) had been kept apart, the systematic variation per level could have been considered, which might have resulted either in a greater chronological precision or in a more economic phase definition (for which the motif by itself is insensitive, apparently), or both. Perhaps, however, the small quantities of finds available in Saxonia (altogether, only 56 decorations on LBK vessels could be reconstructed; Hoffmann 1963:47) are the main cause of the insufficiencies noted here.

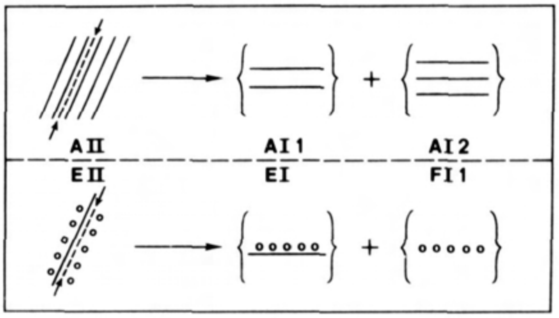


Fig. 2. Breakage and the Modderman (1970: 122) classification: breaking a pot along the dashed lines through the decoration (left) will result in a wrong allocation of the resultant 'elements'.

The most important set of requirements for a "good" classification is the triad of uniqueness, unambiguity and exhaustiveness of the attributes (cf. p. 4), either per class and dimension in a paradigmatic system or per class and branch in a taxonomic system (p. 4). Violation of the requirements of exclusivity and uniqueness has sometimes been explicitly accepted by students of Bandkeramik decoration (Modderman 1970:122). To lessen the effects, transitional classes were introduced (Modderman 1970:123). However, Fig. 2 (not in Modderman 1970, but entirely within the possibilities of the Dutch LBK) gives two examples of this ambiguity: if a pot breaks along a line parallel to (part of) the axis of the motif, a band originally belonging to type A-II would be classified as A-I-1 and/or A-I-2; similarly band E-II as E-I and/or F-I-1. Of course, such dubious sherds will normally be omitted from consideration. Theoretically, though, decoration on an individual sherd classifiable as A-I (or E-I, or F-I) does not rule out the possibility of its having been A-II (or E-II, respectively): it is not different types of decoration but different hazards of breaking that are classified this way. Restricting the argument to Modderman's dimension A (bands filled with parallel lines only), this is unequivocally differentiated into A-III (hatched fillings) and the others (line fillings); also the differentiation A-IV (multi-dented spatula) *vs.* A-I to -III (simple spatula) is a sufficient one. However, A-I and -II (with 2 or 3, respectively 4 or more lines) are practically not exclusive (=distinctive criterion not sufficient). If the band types A-I/III are accorded a chronologically differentiating power (Modderman 1970: 199), it is because they do occur in sufficient quantities to be used in a reliable way. Comparable difficulties will arise with E. Hoffmann's classification of the main motifs of the Stroke Ware ceramic decoration (Hoffmann 1963: Pl. 54).

The ideal of exhaustiveness of a classification (per dimension, that is) is never alluded to in the consulted literature. Yet only in the case of exhaustiveness, have all conceivable variations been considered, i.e., put into an encompassing framework. In my opinion, this omission is mainly due to an (implicit) inductive methodology by means of which the existing classifications have been generated: if data is found at variance with the existing typology, the latter is amended, or rather expanded. Thus, such a classification will accommodate the data for which it was designed, *but nothing more*. Depending upon the qualities of the typologist, and the amount and variability of the excavated pottery, it is more or less valid for a specific region and a specific time span. Therefore, it is impossible to define the various regional or chronological variants of decorated Bandkeramik in a reliable way. What differences exist (and they certainly do) can only be apprehended by comparing the *typologies*, and *not the types*: "types" are defined on the basis of different, and so incomparable, criteria.

Interesting in this context is M. Dohrn-Ihmig's observation, that although the so-called "Cologne type" of Bandkeramik decoration from the Rhineland is very similar to the Late šárka/early Stroke Ware decoration from Bohemia, the former is not called Stroke Ware, because an important parallel phenomenon, the Stroke Ware house type, is not found along the Rhine (Dohrn-Ihmig 1976b). Apparently, the typologies concerned are not geographically neutral.

As will be attempted in the next section, an exhaustive definition of several dimensions of pottery decoration is possible without becoming too unwieldy through size or becoming too trivial through lumping, and still being sufficiently detailed to answer a number of interesting questions. Let it be emphasized that in Bandkeramik typology nobody has ever claimed exhaustiveness or attempted a wider application than a regionally and chronologically limited or a strictly regional one, serving to structure the data from a single area or time span only. And only on the basis of the more restricted classificatory schemes can an attempt be made here towards a wider applicability.

However, from the more limited range of these older typologies a number of implications follow which have never been fully answered:

(A) The widely distributed plastic decoration said to be characteristic for the Belgian (Meuse-Moselle) facies of the LBK (Destexhe-Jamotte 1961: 17, 21) is not incorporated into the Meier-Arendt 1966, and Modderman 1970 typologies. But the applique technique does occur in 'their' areas (Meier-Arendt 1966: 35; Modderman and Waterbolk 1958/9: 175). Also in the typology of Buttler, godparent to many of the typologies considered here, a separate class has been accorded to it ("Type G": Buttler and Haberey 1936:104). Modderman and Waterbolk are obviously right when they state (Modderman and Waterbolk 1958/9:175) that "the plastic element is of a very different kind, as are the criteria basical to our band typology", but perhaps different from what they intended. The alternatives to appliques are moulding and cutting (some more attributes exist on this variable: Shepard 1963: 192-199), which are on a different analytical level than are the band types considered by them (these are only sub-classes of the "incisive" way of surface treatment on vessels). One wonders whether a disentangling of the two dimensions would have resulted in a better understanding of the relative chronological positions of the Dutch and the Belgian LBK (cf. Modderman 1970: 193-194).

(B) Different is the case of the attributes stab-and-drag ("Furchenstich", in German; for a macro-picture of this feature see Modderman 1970 (II): 172B). In southern Germany much is made of the distinction stab-and-drag/hatchings (as fillings of the bands of the main motifs); in fact, the opposition even leads to a differentiation of two stages within the so-called Middle Neolithic Bavarian Roessen, the younger Oberlauterbacher Group and the older Unterislinger Group (Zápotocká 1970: 28-29; cf. Ch.III). Some authors use the *same* term (stab-and-drag) to indicate decorative components produced by what is called here the "goat foot tool" ("Geisfüsslein" or "crowbar") (e.g., Stroh 1938), which in a technical sense is right (cf. next section). Other authors do not distinguish stab-and-drag from goat foot (among others, Goller 1968) whereas in reality the former is what will be called here a component (with alternate attributes like lines, points, hatchings), and the latter a tool type (on a level with round "stick" and pointed spatula). In the northwestern area of the Bandkeramik, stab-and-drag is apparently lumped with the band fillings of the so-called "Plaidt type" (= separate points produced by impression with a multidented spatula; "Kammstich" or "comb teeth"; Dohrn-Ihmig 1976a:99) (cf. Modderman 1970:68, 129; pots decorated by stab-and-drag components from the Elsloo Cemetery – esp. graves 83, 90, 92, 96 – are classed as band type D-III). Again, one wonders whether recognition of the stab-and-drag pointlets as a separate attribute plus the differentiation of the various types of spatula would not have resulted in an earlier settlement of the controversies on the relative chronological positions of the Roessen and Grossgartach cultures (Goller 1968: 256; Mauser-Goller 1969: 45-51; Stehli 1974; Stroh 1938:10,13).

(C) One of the most serious practical limitations is the restriction of the classification to complete (or at least reconstructable) vessels. In that way, the statistical basis of LBK ware studies is drastically reduced. True, this procedure stems from a realization of the ambiguities that arise in the application to sherds of classificatory schemes like those of Buttler, Hoffmann, or Modderman (e.g., Hoffman 1963:47) – yet even a category "incomplete" (Stehli 1973:77) is but a partial remedy. The alternative solution, a re-definition of the categories so as to accomodate broken decoration as well gives much more leeway to statistical analysis than simply discarding potentially useful data. Perhaps the adoption of such a re-defining strategy would have resulted in a shaper or subtler and certainly more reliable chronological framework than that for the Saxonian LBK in Hoffmann (1963: 47, Pl. 38).

A solution to this problem has been attempted by Soudský: apparently his classification allows even small sherds to enter, without artificially garbling the data. Different levels ("abstractions" in his terminology) of decoration are delineated, such as techniques, styles, secondary motifs, etc. Each of these is coded in a separate set of columns, with a blank for those that are not observable (Soudský 1962: 194). However, this method is inflexible as it cannot handle anything besides one sherd at a time, not even a complete pot (cf., however, Soudský 1966). Every single characteristic has its own code number, which fills the column reserved for that level; counts of the characteristics are not entered. They are to be derived from the number of units (= sherds) within a subfile (a find, or a larger entity), which renders it cumbersome, if not impractical, for any other purpose going further than mere description.

A final theoretical point has to do with the type of classification as defined by the interrelations of the dimensions. Usually, neck decoration, motifs of belly decoration, elements or band types, and secondary decoration are considered separately, i.e., in a paradigmatic way (e.g., Hoffmann, Modderman, Soudský, and Stehli classifications). Within every dimension, however, things are different. To illustrate this, again the Modderman scheme is taken. Historically a partial redefinition of a number of categories from the Buttler and Haberey (1936) scheme, it was constructed to mirror the various combinations of lines and points observed in the Northwestern Bandkeramik decoration (Modderman and Waterbolk 1958/9:173). Categories A to E show lines as definitory, and categories B to F, points (Modderman 1970: 122). (Incidentally, the gradual increase in number of points relative to lines from A to F appeared to be a chronological clue) This simple structure of lines and points was complicated, however, through addition of hatchings to the first three categories and incorporation of decoration accomplished by multidented means to some of the dimensions (Modderman 1970: 121). The various attributes are not considered of equal weight, though: multidented spatula differentiates A-IV from A-I and A-II, but not from A-III; in other words, hatched fillings override the single/multidented distinction (cf., e.g., grave nos. 97, 104 from Elsloo). Thus, the A-category of band fillings is differentiated in a taxonomic way: first level, hatchings *vs.* non-hatched (A-III *vs.* (A-I, -II, -III)); second level, multidented *vs.* simple spatula (A-IV *vs.* (A-I, -II)), with in the other branch A-III hatchings not further differentiated; and on the third level, four or more lines *vs.* less than four lines (A-II *vs.* (A-I, -II)), with in the companion branch A-IV, and in the other branch A-III, not further differentiated.

A similar demonstration of the often hidden taxonomic structure of an at-first-sight paradigmatic scheme can be presented by recalling Soudský's coding "instructions" (Soudský 1966:40). On one level he distinguished five main types of decoration: applied; delimited and continuously filled bands; either not fully delimited, or discontinuously filled bands; points on lines, or music notes; and points of a deviating shape. Within each of these types a further partitioning is to be made over two lower levels, for every type and for every level according to different criteria. In this way, a characterization and specification of every type of decoration is achieved, consisting of a combination of three numbers, the respective positions of which indicate the level, and their combination giving the definition. For instance, "470" defines a decoration of points on lines ("4", first level or main distinction), in a regular pattern which in the next section will be dubbed "auxiliary lines" ("7", the second level) a differentiation more or less according to the number of lines and of points involved; and then, unspecified ("0", the third level, not further differentiated) – these three numbers to be entered into specified columns. This example, however, is unreliable, as no full description of Soudský's coding scheme is available to me; therefore, I may have missed some important points. Yet the taxonomic quality is clear from the looks of it – again, an illustration of the hybrid paradigmatic/taxonomic character manifest or latent in many typologies.

After this more-or-less theoretical discussion, the practical problems with the existing typologies can be handled in a few lines. Above (p. 2) I noted equivocality, expandability, and regional and chronological restrictedness as the main obstacles for answering the present research problems. A major technical shortcoming is that they do not fully exploit the data, as can be read from the previous paragraphs. Regarding equivocality, sometimes ambiguous class definitions occur. I demonstrated this with examples drawn from the Modderman (Modderman and Waterbolk 1958/9; Modderman 1970) and the Hoffmann (1963) classificatory schemes: some attributes are more dependent upon pot breaking than upon pot decoration; they are not unique (pp. 8-9).

Also, expandability has been touched upon. The reverse of exhaustiveness – which if implemented allows a judgment of adequacy – one never knows for sure whether all logically and/or practically possible classes have been defined. Such a expandable typology can be thought of as a result of an inductive procedure: starting from the data at hand, generalities within that set are defined as classes. As the data will normally come from a restricted region, such a classification is necessarily applicable to that area only. The example of applique decoration (above, p. 9) served to illustrate this point; it has as its major consequence the incomparability of "types" because of the incompatibility of the typologies. Notwithstanding this, some sort of consensus exists as to what should be considered Bandkeramik: parts of several typologies overlap. This, however, is not by virtue of the classifications, but because the decorative style of the older phases of the LBK are virtually international. When after their expansive era a settling period brought regionalization, the "resultant" typologies became differentiated, too. I suspect that much of the trouble about the relative dates of the various stages in the different regions can be concluded by using a pan-typology instead of by comparing the local classifications. Yet, interregional comparisons have been attempted (some of the better examples are: Buttler 1938, Dohrn-Ihmig 1976b, Meier-Arendt 1966: 61-67). A number of the categories originally defined at the meso-level (the region) are then applied to the macro-level (interregionally). Such arguments usually follow this course: first recording the occurrence of, e.g., the spiral motif, or the multidented spatula in a region, and afterwards a consideration of their geographical range. What is omitted is a consideration of the possible transformations of that motif, or of the alternative tool types and the geographical distribution thereof. A further example is to be found in Zápotocká's Stroke Ware studies. She compares Stroke Ware-like pottery from non-Czech regions in terms of the categories defined for the Bohemian Stroke Ware (Zápotocká 1970: 3,4). Such a regiocentricity obscures the problem of the validity of the regional categories for ceramic decorations elsewhere. For instance, for what reasons are zigzag motifs executed in hatchings (Bavarian Roessen, Hinkelstein) comparable with those executed in points (Stroke Ware *sensu stricto*) *and not with those executed in lines* (LBK general)? For chronological reasons, perhaps, but that is an argument alien to the definition of a classification itself. In other words, Zápotocká's conclusion that Stroke Ware-like ceramic phenomena to the West of Bohemia should be explained as vestiges of a Stroke Ware colonization (Zápotocká 1970: 59-60) is in fact built into her classification already from the beginning.

The same argument holds for attempts at diachronical comparisons. Thus, although cultures later than LBK were certainly present in the Main area (Meier-Arendt 1975) there is no way to grasp the (diachronical?, synchronical?) transition from LBK to Hinkelstein in either his LBK typology for the region (Meier-Arendt 1966) or his Hinkelstein one (Meier-Arendt 1975). As classificatory schemes should be designed to answer specified questions (Dunnell 1971:60) the partial chronological insensitivity of the existing typologies is at odds with the explicitly stated purpose of some of them: Stehli is concerned (Stehli 1973:59) with establishing a chronological sequence for the finds from the Aldenhoven Plateau (roughly between Cologne and Aix-la-Chapelle), but does not consider possible transitions to later cultures; such classes do not exist in his typology (cf., e.g., Stehli 1974). Even less understandable is the shift in focus of Modderman's classification from *elements* (my term) to *bands* (Modderman 1970:121): in an earlier article the chronological significance of the gradual trend in the LBK decoration from lines to points *(elements)* was noted (Modderman and Waterbolk 1958/9: 104). As these two attributes are easily observable, *independent of the fracture patterns of the pots*, and easily counted (thus open to statistical treatment), a potentially powerful chronological indicator has been put aside.

After so much critique – no doubt shared by the originators of the typologies discussed – it remains to be seen if a different typology can be constructed which steers clear of both the Scylla of ambiguity and of the Charibdis of unboundedness at the same time, and which is flexible enough to answer problems as different as intra- and inter-settlement and inter- and intra-phase comparison of social structures.

4. *A paradigmatic classificatory scheme for decorated Bandkeramik pottery*

The primary practical requirement of a classificatory scheme is that it should make possible the description of the variation between the relevant units. In the Bandkeramik case, and with present research interests these units are the contents of the various pits – specifically, the decorated sherds found in the so-called "refuse pits" ("*Abfallgruben*") along the houses, per pit considered a set (cf. Ch. II). A classification of the decorated sherds should make possible a quantification of the differences between the various pits, in order to allow statistical treatment. In that way, inter- and intra-site, intra- and inter-phase comparison can be attempted. However, since nothing is known yet about which characteristics of LBK ceramic decoration are sensible to variability in social structure, and as chronological markers are only partially known, the classification should be as detailed as possible and contain as many dimensions as practicable. In that way, one part of the classification may prove to be of chronological use and another part for the elucidation of kinship, etc.

Perhaps needless to say, I am not pursuing characteristics concerning the pottery itself (manufacture, shape, contents, or even the uses which have been made of it), but about the differential distribution of ceramic phenomena. And only because sherds occur in statistically attractive numbers and because they display considerable variation, it is decorated sherds that are used to gauge differences – just like the other typologists of Bandkeramik ware have done.

The construction of a classification is based on the assumption that the existing literature provides ample material to gain an adequate overview of Bandkeramik designs. The following publications on Bandkeramik pottery (*sensu lato*) were consulted: Butschkow 1935; Buttler and Haberey 1936; Destexhe-Jamotte 1961; Dohrn-Ihmig 1973; Hoffmann 1963; Meier-Arendt 1966 and 1975; Modderman 1970; Quitta 1960; Schietzel 1965; Stehli 1973; Stroh 1938; Zápotocká 1970. I have worked (as of spring, 1978) for three years with this classification on data from Bavaria and Holland. The principles have not been changed at all; only the format has been streamlined in comparison with its first conception. Still, I do not consider the scheme definitive in its present state; further checking, especially on Belgian and Middle Neolithic (Rössen, Grossgartach, perhaps Münchshöfen as well) material should refine its categories.

A definition of the Bandkeramik is difficult to give: for present purposes, reference to the chronologically first cycle of decorated pottery (with no restriction as to fine ware only) in central and western (continental) Europe should suffice (or, roughly, the decorated pottery produced by the "Danubian I" and "Danubian I survivals in the North" of Childe 1957: 105-110, 116-119).

If a definition of what "is" Bandkeramik is difficult, "decoration" is impossible to define; not even Shepard (1963) attempts this. Perhaps it is best considered a primitive term to be used with common sense to indicate embellishment of earthenware (excluding polishing, because of its rare preservation).

Analytically, several independent levels of design, or variables can be distinguished: the tools or TECHNIQUES that have been employed to produce the decoration; the structuring of the zones on a pot or the FORMAT of the decoration; NUMERICITY, or the repeat factor visible on several different dimensions; the AUXILIARY LINES by means of which the format has been meted out and/or subdivided; the MOTIFS filling the zones; the principles or STRUCTURES that govern the execution of the motifs, including their image; characteristics regarding continuity and direction of the FILLINGS of the motifs; the embellishment of ENDS and LIMITS of the motifs; the SECONDARY filling-in of the zones outside the motifs; and finally, the COMPONENTS, with which the decoration has been depicted.

To begin with TECHNIQUES: there are many techniques for decorating pots (for an extensive, if not an exhaustive, listing, cf. Shepard 1963: 192-199); from among these, only three are relevant to Bandkeramik ware: *incising* the surface with knives, spatulas, gouges, and the like; *modelling* the surface with fingertips or nails; and *applique*. Within the category of incisions, a number of subcategories or attributes may be differentiated according to the number (single-, double-, treble-, ... dented) and form (pointed; rounded, and straight-edged) of the tool.

An observation on method: implicit in the discussion of classification in the second section is a distinction between classification as a theoretical construct (discussed there), and the classificatory scheme as the instrument with which to observe the data. The latter, here labelled "codebook", is to be derived from the former, with practical considerations guiding this reduction. In that way, it was from the available literature on Bandkeramik decoration that the number of techniques was reduced to the three above. Going with one eye through Shepard's manual and with the other through the published data, it was possible to drop such categories as gouges, stamping, molding and slipping.[4] Moreover, *after* analysis of a substantial part of the Hienheim data, spatulas with more than two points proved extremely rare; the subcategory "pointed tools" was therefore reduced to two attributes: single dented tool (which includes knives, as they are hardly different in their effect from a well-pointed spatula; cf. Shepard 1963: 200) and multidented spatula. The round-edged "sticks" were defined, or rather differentiated, from the single-dented spatula by the limit of 1 mm. deep grooves, a limit established also by experience with the data. Finally, the square or straight-edged gouges apparently occur in a two-teeth version only, the so-called "goat-foot tool" (cf. p. 10 above). This operationalization of the original classificatory scheme proved quite workable in the subsequent analysis of the ceramic decoration from Elsloo in the Netherlands (where, however, vestiges of the goat foot tool are not found).

The case is different with the category appliques. It does not occur at all in Hienheim (nor in the other Bavarian sherds that I have seen, with the exception of the famous "puppet" on a pot from Gneidingen at the Straubing Museum – also cf. Maier 1965: 23) and only rarely at Elsloo. As appliques are widely applied on Bandkeramik pottery this category has been retained in the codebook. Thus, incisions were first much differentiated (classificatory scheme) and later lumped (codebook); modelling or appliques were never further differentiated. So, the variable TECHNIQUES now consists of the attributes: *simple spatula*, *multidented spatula*, *rounded gouges*, *goat foot tool*, *finger or nail imprints*, and *appliques*.

To facilitate discussion I will now drop all reference to sherds and instead refer to pots: decoration has been executed on complete vessels and not on sherds. However, some of the variables will be observable on pots or large fragments only; other dimensions will still be visible on even small sherds. In order not to forego possible information the latter have to be incorporated as well: the same classificatory scheme should be applicable in both cases. "Unobservable" is *missing data*; it is not "ambiguous".

A major dimension is the FORMAT of the decoration, which consists of two separate variables: subdivision or zoning of the vessel's surface, and the subdivision of each of the zones. Leaving aside the interior of the pot as being only incidentally decorated, the outside can be devided into three zones: rim and neck, belly, and bottom. In the Bandkeramik the bottom is hardly ever decorated, and if so, merely as an extansion of the belly decoration (cf. Fig. 3) — with the exception, of course, of the LBK's mugs (so-called "*steilwandige Becher*") and the Middle Neolithic dishes. Belly and neck design are often different and can be regarded as independent; this may even be accentuated by a line or narrow band separating the two, a kind of shoulder decoration. As this latter never (or hardly ever?) assumes full independence, it is not a separate zone but an attribute of the distinction neck-belly: articulated or not.

Then, the rim may also be decorated, which never has anything to do with the design on the remainder of the pot.

This leaves us with three zones: rim, neck and belly, each of which may be decorated or not. However, if either rim or neck has been decorated, then the belly has been decorated too; conversely, when the latter has been left bare, the former also remained undecorated (almost without exception). Which leaves four attributes to zonation in the codebook: *rim decorated (yes/no)*, and *neck zone decorated (yes/no)*. In the case of presence of either of the two, there will also be decoration in the belly zone.[5]

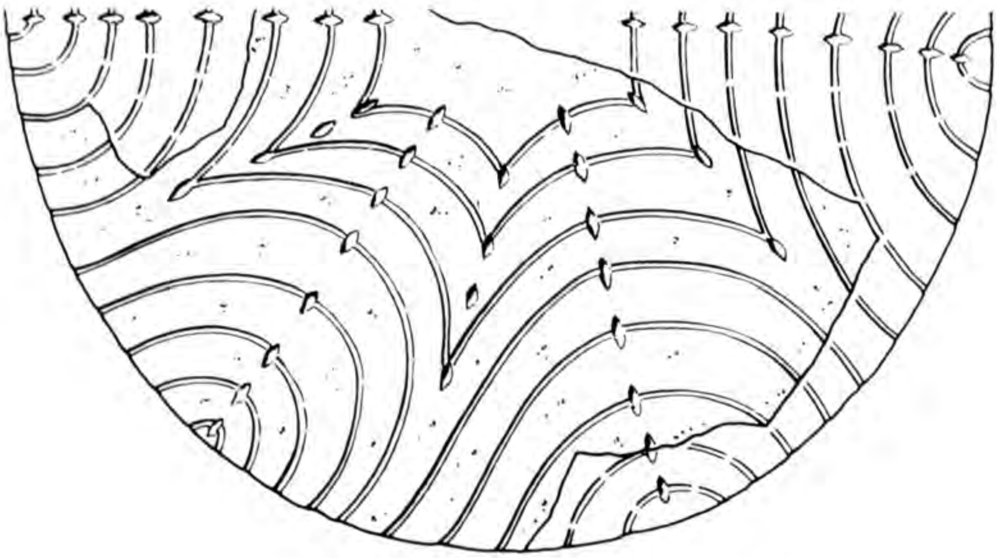


Fig. 3. LBK pot from Hienheim with decorated bottom; the design is a doubling of the motifs in the belly zone.

Within the zones entirely different principles pertain. If the neck zone is decorated, this may be entirely independent of the lower zone, an independency which may even be accentuated (above); sometimes an interruption or accent occurs which is directly related to the decoration below it.

In the belly zone two major dimensions are observable. The first one is the repeat factor (here called "numericity") and indicates the number of motifs occurring around the pot. NUMERICITY should be conceived as an abstract principle: even deviating or irregular patterns result from an orderly concept. So it is sometimes clear that a twelve-fold repetition of the motifs is intended, whereas "lack" of space resulted in only eleven motifs (cf. Fig. 4). In line with the general ideas behind this classification the blueprint number should then be coded (more on numericity below, on p. 20).

The second dimension at work in the belly zone

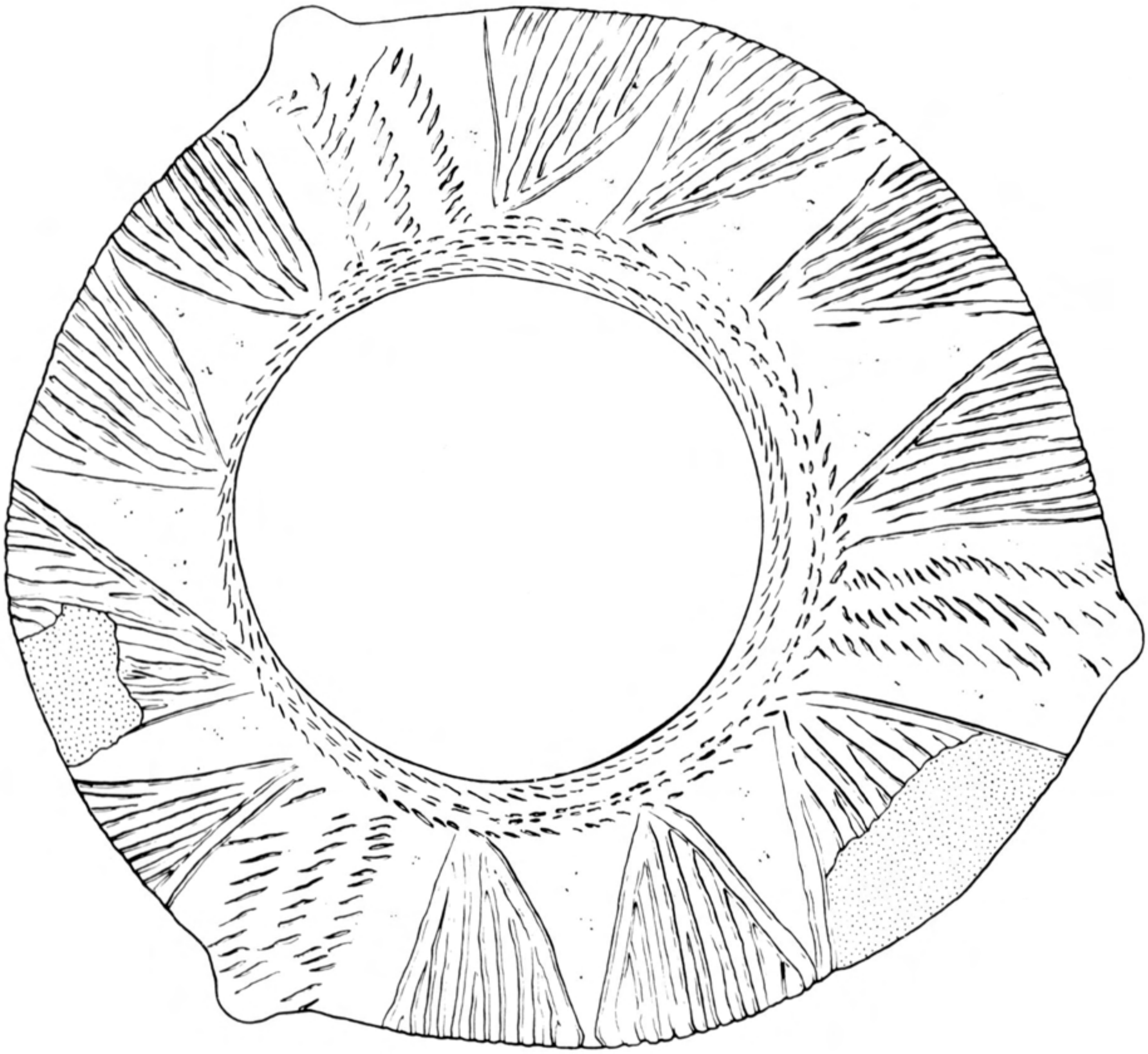


Fig. 4. Four-fold and three-fold execution of format: the auxiliary lines, the circumference (and the neck decoration) are all in three-fold, the rhombs filling the shoulder-belly zone occur in three- and (twice) four-fold. (Vessel seen from top.) Hienheim, find no. 1115.

is that of the AUXILIARY LINES: there may be *none*, but more often than not the subdivision of the zone is indicated by rows of points or lines, forming *symmetry axes* in vertical and/or horizontal directions, and obviously serving to facilitate correct execution of the motifs. The columns of points in combination with an execution of the remainder of the decoration in lines are often erroneously called "music notes" ("Notenköpfe" in German; e.g., Stehli 1973: 83). In reality, symmetry axes are regular in themselves, whereas "music notes" are strung regularly on lines (see below, p. 18). More rarely observed are *cadres* around sets of motifs.

Then, there is the dimension of the MAIN MOTIFS. The not too great variation can be reduced to two main principles: the semi-circle (wave) and the spiral (**Fig. 5**). (That the latter looks like a transformation would involve a different scaling of the two elements, which is not a transformation at all). A motif is here understood to be the repeatedly occurring patern of decoration on a single vessel. The motifs filling the belly zone are developments of basic motifs through symmetric operations and movements as shown in Fig. 5. Together these transformations logically exhaust the possibilities for elaboration of the main motifs; they form a "group" or "pattern" in an algebraic sense (Bell 1966: 112-119; Budden 1972: 504-538; also, see Shepard 1963: 269 and note 3 below). In Bandkeramik practice, however, there are two difficulties with this classification: the glide does not occur, and the wave (in its rectilinear guise) is sometimes reduced to a *single* slant;[6] Zápotocká 1970: Pl. 1, 3, 4; also Fig. 6).

Several structuring variables mediate between the components of the decoration (below) and the motifs. In the belly zone, one of the more important STRUCTURES has but two attributes: *curvilinear* or *rectilinear* execution of the motifs. In Fig. 5 the curved and the straight alternatives have been rendered side by side. Of course, straight lines are impossible on globular surfaces; in accordance with the rest of the argumentation here, the distinction refers to the rolled-off design (or more precisely to a gnomonic projection of the motif).

Another structuring dimension is the IMAGE (for lack of a better term): the image of a motif may have been spared out in a zone filling articulation of the surface, thus creating a negative image of the motif. Or the motifs may be expressed as positive images, setting them off with lining or through filling against an otherwise comparatively empty space. For the "positive" image almost any Bandkeramik decorated pot can serve as an example. For the "negative" one the so-called embroidery-like ("teppich-artige"), working up by means of the goat-foot tool so characteristic of Planig-Friedberg ware will do. With the (perhaps rare?) exception of the Planig-Friedberg ware, no counterparts of these structuring principles of the belly motifs or/of the motifs themselves can be established for the neck decoration.

Also a kind of structuring principle, FILLINGS of the motifs applies equally to belly and to neck decoration: both may have been filled by *continuous* or *interrupted* rows of components or may have been left vacant.[7]

Next to FILLINGS, DIRECTION OF THE FILLINGS is a second variable of the motif execution (and equally of the neck decoration), which refers to the angle of the components relative to the (local) axis of the band: *undetermined*, *parallel*, *oblique*, *perpendicular*, or *crossed*. It will be clear that this variable, combined with that of the components below, is most closely related to the Buttler and Haberey tradition of classification; indeed, it should be possible to work out a calibration of the present classification with, e.g., the Modderman scheme.

Other variables for the description of the execution of motif and neck decoration are LIMITS and ENDS. The latter one is simply a differentiation of the degree of elaboration of the motif ends: *no ends* present through rotation or translation; *not worked up*, which includes the regular association of the ends with symmetry indicators; and *worked up*.

The variable LIMITS should differentiate the various ways in which the motifs (or neck decoration) are outlined, whether *special limits* are drawn or *not*, and if so, whether these limits are elaborated "on line" (*"music notes"*, *"dumb bells"*) or "off line" (with *discontinuous* or with *continuous fringes*). The attributes listed here carry more of an enumerative

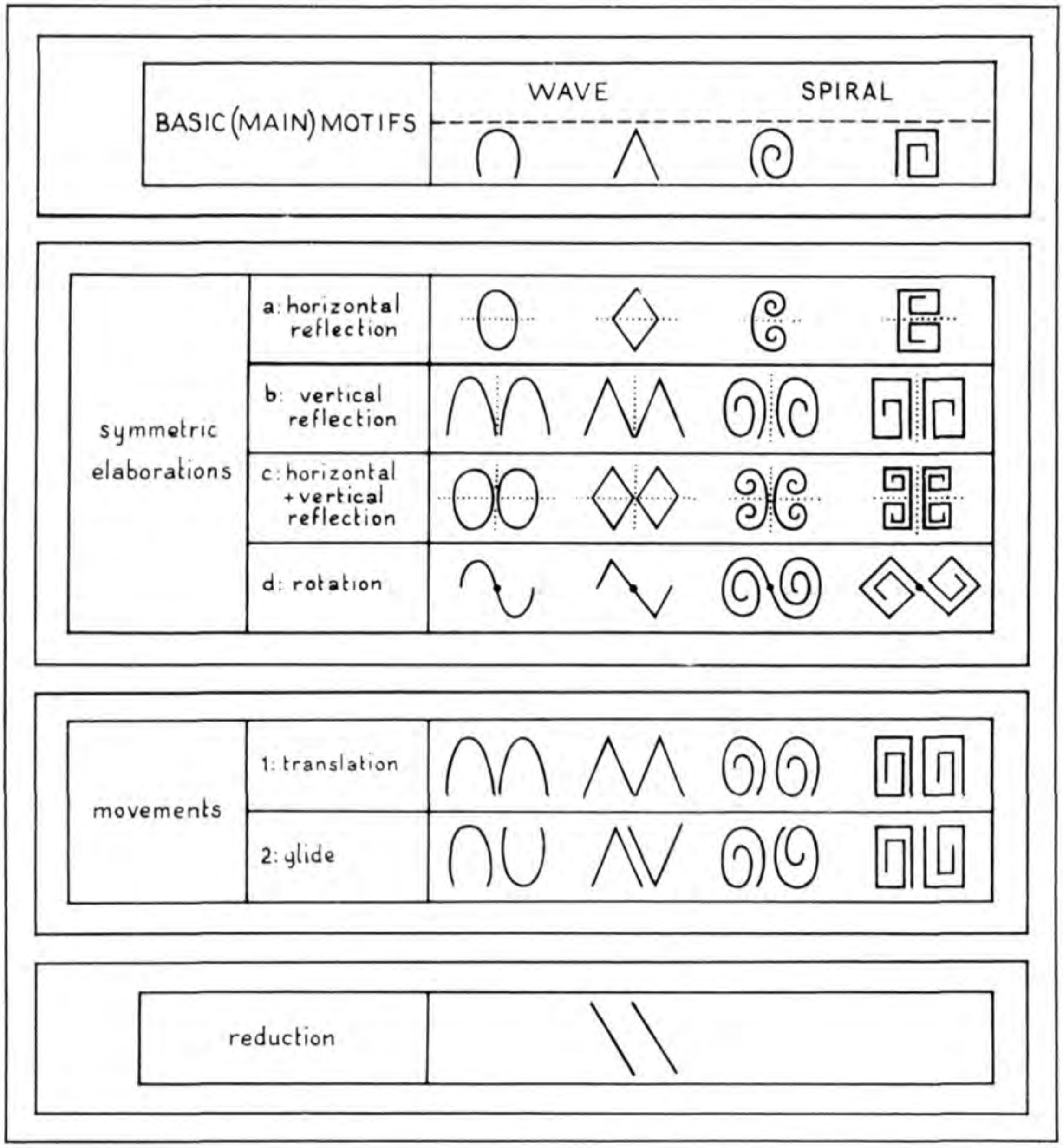


Fig. 5. All logically possible motifs of Bandkeramik pottery decoration, if considered as elaborations of two basic figures, executed in either a rectilinear or a curvilinear fashion. Though possible, 'glides' do not occur; not strictly a member of a transformation group, 'reduction' is sometimes observed.

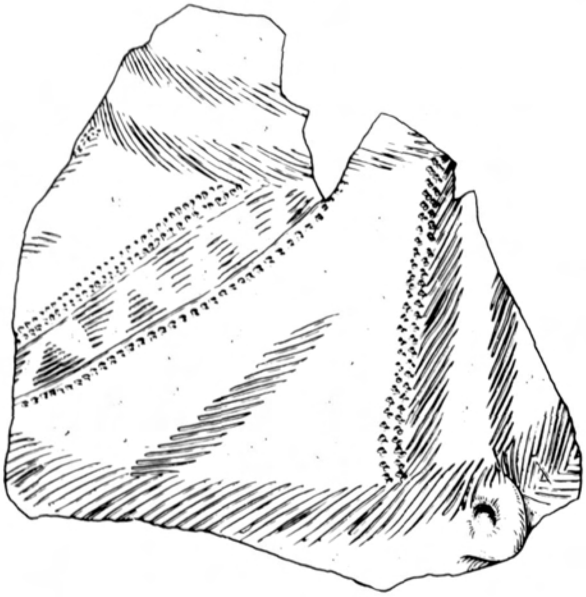


Fig. 6. Large fragment, showing reduction of zigzag motif to simple slants (plus continuous neck decoration) and hatching; the zigzag has been achieved throug smearing. Hienheim, find no. 1115.

than of an intensional definition of this variable, although basically an intensional discrimination is present (no/yes; if so, unelaborated/elaborated; if so, on line/off line). It is the differentiation within the on/off line attributes that is enumerative, and thus liable to amendment.

The field outside the motifs in the belly zone still has to be described. One possibility is implicit in the structuring variable IMAGE: if the motifs are executed in a negative way, then obviously the remainder of the field has been filled. Another filling of the non-motif space is by means of AUXILIARY LINES: cadres or reflection axes may occupy otherwise empty parts of the belly zone. Secondary decoration in a strict sense, however, starts from either of two principles. These are different from the above, which properly belongs to the level of the MOTIFS. The first one is simply (partial) repetition of the motifs in the belly zone – mostly on top, but also to the bottom side (Fig. 3) or even between the motifs (including what are called "Zwickelfiguren" in German). This attribute will be called "*doubling*". The second principle indicates an entirely *independent* working up of the empty field by means of small groups of components, of which numerous different forms exist (cf. Stehli 1973: 75; only the nos. 1-5, 7-10, 21, 26, 33-35, 37 and 43 can be taken independently; 6 and 36 are probably combinations of this type with a symmetry axis; 38-42 and probably 29, too, would be classified as "doublings"; all other configurations shown there are likely to be different variations on the theme "auxiliary lines").

Belonging to both zones is the variable COMPONENTS, defined as the smallest recurrent units of (Bandkeramik pottery) decoration. Although there is certainly an original relation to TECHNIQUES, not all categories of the latter are equally relevant here. For instance, (the number of) appliques is almost always identical on both levels. And although the number of finger/nail imprints is quite different from the employment of this modelling technique, no further differentiation is conceivable. The incising technique has resulted in four different components, however: line drawing (heavily: *lines;* lightly: *hatchings;* the two are almost always distinctive, especially when reference is made to the companion sherds in the same find); the jerking use of spatulas resulted in *stab-and-drag*, and pointed tool pricking produced *pointlets*. It is needless to say that the components can still be observed on even the smallest sherds. As indicated above, both pointed and straight-edged tools can produce stab-and-drag components; the tool-edges may be different, as are the tool marks at first sight, but the components are conceptually identical. In this way the variable COMPONENTS is observable in the following attributes: lines, hatchings, pointlets, stab-and-drag, and *finger impressions* (with the belly and the neck zones considered separately because of their independence).

The dimension of NUMERICITY can be found at several levels: the single, two- or-threefold use (or even more) of the components in the execution of neck decoration, motifs and secondary fillings in the belly zone and/or the auxiliary lines, and the number of motifs in the belly zone; these are all governed by a repeat factor, and not necessarily by the same one. Yet there is some regularity to be observed, since there appear to be two series: 3-6-9 folds, and 2-4-8 folds, with the number one standing alone and not being further developed into a series.[8] In this way developments of the repeat factor 1, 2, or 3 can be found, attributes named *simplex*, *duplex*, and *triplex*.

As I have said before, on any pot several numbers tend to occur together, though generally selected from one single series. Quitta (1960:8) noted the connection between symmetry axes and repeat factor: by means of these auxiliary lines (and/or the ears of the pot), the format of the belly zone is subdivided. Much easier to be observed is the repeat factor in the auxiliary lines themselves: these consist of single, two- or threefold (sometimes more) columns of pointlets, lines, hatchings, etc. Also, the arrangement of the components in a motif and in the neck zone (cf., however, note 8 below) can be a guide. It may appear then that the 2 and 3 series are merged, as on the pot described when FORMAT was discussed (p. 16; Fig. 4). In that case, numericity is determined by a weighting of the various observations against another (in Fig. 4, auxiliary lines show a threefold composition, as does the neck decoration; also, the circumference of the pot has been divided into three fields by ears and auxiliary lines; only the number of motifs is 11 [4+4+3] and should have been 12 [3 × 4], which amounts to attribution of the design to the triplex series).

When *number of sherds* and *identification numbers/find numbers* are added, the classificatory scheme is completed, although no claim to exhaustiveness is made; there will be more dimensions to decoration than listed here. In the next section the codebook will be presented with which to code Bandkeramik decorated sherds.

5. *A codebook for Bandkeramik decorated ware*

Notes

(1) With the exception of the variables COMPONENTS and NUMBER of sherds, all counts are taken from the *number of motifs* in a find. Thereto sherds are to be grouped when apparently belonging to one vessel; then the minimum number of motifs present in such a group is entered. Single, ungroupable sherds represent one motif each (cf. Stheli 1973: 60, 86).

(2) When the count of a trait surpasses 99 – the maximum, as generally only two columns are available – then the total of the VARIABLE is set to one hundred, and the proportions of every *attribute* on that VARIABLE is entered.

(3) For definitions of *attributes* and VARIABLES and a description of the system's structure reference should be made to the previous section.

(4) There are three groups of VARIABLES: a set relating to general characteristics; a set for the belly zone; and a set for the neck decoration. Suffixes: (G), (B), and (N), respectively.

(5) If any attribute cannot be recognized because of the small size of the sherds, no entry is made. "00" is reserved for positively observable absence.

(G): GENERAL CHARACTERISTICS

TECHNIQUES (G) — column numbers

When tools of different types have been used on one vessel, the attribute best characterizing the motif is counted.
Neck decoration is counted as one motif per pot.

01: *single dented spatula, knife*	no. of motifs:	01-02
02: *multidented spatula* (not if 03)	no. of motifs:	03-04
03: *goat-fool tool*	no. of motifs:	05-06
04: *fingers* or *nails*	no. of motifs:	07-08
05: *round stick, deeper than 01 mM.* (not if 03)	no. of motifs:	09-10
06: *applique*	no. of motifs:	11-12

NUMERICITY (G) column numbers

Different repeat factors may occur together on the same pot. For example, the neck zone may show a simplex decoration, with the motifs in the belly zone belonging to the duplex series. These are counted separately, and neck decoration is equal to one motif. Minor deviations from the apparent design are to be ignored.

07: *simplex* design (i.e., one-fold, simple)	no. of motifs:	13-14
08: *duplex* design (2-, 4-, and 8 folds)	no. of motifs:	15-16
09: *triplex* design (3-, 6-, and 9-folds)	no. of motifs:	17-18

ZONING (G)

As the sum total of TECHNIQUES provides the number of motifs in the find, and the number of motifs in the belly zone can be ascertained from STRUCTURES, "absence of rim decoration" (as alternative to attribute 10) is omitted. Shoulder decoration is considered a part of the neck decoration, and the two are merged.

10: *rim decoration present*	no. of rims:	19-20
11: *neck decoration present*	no. of rims:	21-22
12: *neck decoration absent* or ancillary to belly design	no. of rims:	23-24

(B): BELLY ZONE CHARACTERISTICS

STRUCTURES (B)

STRUCTURES refer to the design of the main motifs only.

13: *curvilinearity*	no. of motifs:	25-26
14: *rectilinearity*	no. of motifs:	27-28

IMAGE (B)

As the number of motifs in the belly zone follows from STRUCTURES, "positively executed" as alternative to attribute 15 is omitted.

15: *motifs negatively indicated*	no. of motifs:	29-30

MOTIFS (B)

There are two analytical levels recognized here; the MAIN (or basic) MOTIFS and the MOTIFS which can be derived from them. Examples are to be found in Fig. 5.

MAIN MOTIFS

16: the basic unit is a *wave/zigzag*	no. of motifs:	31-32
17: the basic unit is a *crook/meander*	no. of motifs:	33-34

MOTIFS have been developed through:

18: *horizontal reflection*	no. of motifs:	35-36
19: *vertical reflection*	no. of motifs:	37-38
20: *horizontal plus vertical reflection*	no. of motifs:	39-40
21: *rotation*	no. of motifs:	41-42
22: *reduction*	no. of motifs:	43-44

AUXILIARY LINES (B)

More often than not, the belly zone has been subdivided by means of auxiliary lines to facilitate the rendering of the motifs. These drawing aids sometimes assume the function of secondary motifs; they are readily discriminated, however, as they are more or less independent of the motifs, filling vacant spots, whereas the auxiliary lines either frame the motifs or are laid out across them.

23: *cadres*	no. of motifs:	45-46
24: *symmetry axes*	no. of motifs:	47-48
25: *no auxiliary lines*	no. of motifs:	49-50

cadres

symmetry axes

FILLINGS (B) — column numbers

The variable FILLINGS relates to the interior of the straps of the motifs; the way in which the bands are delimited is coded with the variable LIMITS (B).

The motifs have been executed as:

26: *empty bands*	no. of motifs:	51-52
27: *discontinuously filled bands*	no. of motifs:	53-54
28: *uninterruptedly filled bands*	no. of motifs:	55-56

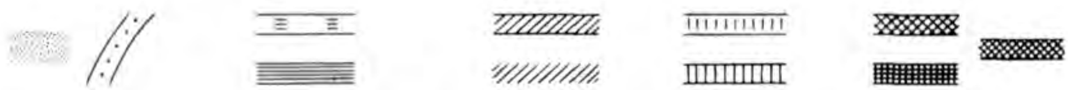
empty — discontinuous — not interrupted

ANGLE (B) OF FILLINGS

In the case of attribute 26, no entries are made here.

The angle of the fillings of the strap, relative to the (local) axis of it, is:

29: *indeterminate* (NOT if the sherd is too small)	no. of motifs:	57-58
30: *parallel*	no. of motifs:	59-60
31: *oblique*	no. of motifs:	61-62
32: *perpendicular*	no. of motifs:	63-64
33: *crossed*	no. of motifs:	65-66



indeterminate — parallel — oblique — perpendicular — crossed

LIMITS (B)

The variable LIMITS specifies the delimination of the motifs (together with ENDS (B)). "Absence" being implicit, as from FILLINGS (B) the number of visible motifs can be derived.

The motifs have been delimited explicitly:

34: *simple delimitation*	no. of motifs:	67-68
35: "*music notes*"	no. of motifs:	69-70
36: "*dumb bells*"	no. of motifs:	71-72

(continued with attribute nr. 40)

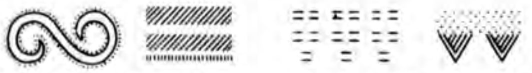
IDENTIFICATION, ETC.

37: *reserve*	73-75
38: *find number*	76-79
39: *card number*: fill out "1"	80

(Continuation of LIMITS (B))

40: *discontinuous fringe*	no. of motifs:	01-02
41: *continuous fringe*	no. of motifs:	03-04

absence (implicit here) — simple delimination — on line, "music notes" — on line, "dumb bells" column numbers



off line, continuous fringe off line, discontinuous fringe

ENDS (B)

The tips or ENDS of the motifs may (not) have been elaborated:

42: *no ends* (translation/rotation resulted in cont. design) 05-06
43: *unelaborated ends* (incl. auxiliary marks) . . . . no. of motifs: 07-08
44: *elaborated ends* (excl. auxiliary marks) . . . . no. of motifs: 09-10

no ends simple ends (symmetry axis) elaborated ends

SECONDARY FILLINGS (B)

SECONDARY FILLINGS occur in the belly zone only; the variable refers to all design elements not incorporated in or appended to the motifs. The attribute *decked out* remains implicit, as it is covered by IMAGE.
"no. of motifs" refers to the primary motifs.
Non-motif space in the belly zone has been:

45: left *blank*, or meted out by aux. lines only . . . . no. of motifs: 11-12
46: filled with *partial doublings* of motifs . . . . no. of motifs: 13-14
47: filled with *independent small markings* . . . . no. of motifs: 15-16

no secondary motifs partially doubled motifs independent secondary motifs

COMPONENTS (B)

COMPONENTS are the smallest recurrent units of decoration. On Bandkeramik pottery *lines* or continuous grooves occur; *points* or independent tool-point impressions; *hatchings* are relatively shallow, closely packed in bundles, and approximately parallel incisions; *stab-and-drag* points originate when the spatula is not lifted clear from the vessels's surface while applying points in a jerking movement, a shallow line with bead-like, deeper points strung on it; *finger* and *nail impressions* are easily recognizable. Implicit is the attribute *appliques*, as this would be almost identical with that attribute of the variable TECHNIQUES.

Lines are counted on the pot's equator.

To reduce the amount of space needed to accomodate the totals, these are converted to 10-logarithms (Appendix to this chapter, p. 217).

The decoration in the belly zone has been executed in:

48: *lines:* 10-log (abs. no. of lines in find) 17-18-19
49: *points:* 10-log (abs. no. of points in find) 20-21-22
50: *hatchings:* 10-log (abs. no. of hatchings in find) 23-24-25
51: *stab-and-drag:* 10-log (abs. no. of points in s + d) 26-27-28
52: *finger/nail impr.:* 10-log (abs. no. of finger/nail impr.) 29-30-31

In the case of non-presence of any of these attributes, enter "999".

(N): NECK ZONE CHARACTERISTICS

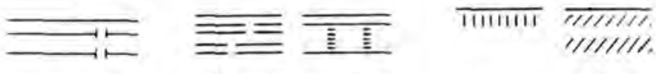
When on rim sherds parts of the belly decoration are visible, these should be entered with the variables above. Neck decoration, as understood here, is independent of the decoration in the belly zone; i.e., neck decoration is not supplementary to the belly design. It includes eventual shoulder decoration (or rather, decoration intermediate between neck and belly zone).

FILLINGS (N) column numbers

The variable NECK FILLINGS relates to the filling up of the neck decoration, not to its delimination (:NECK LIMITS).

The neck decoration has been rendered as:

53: *an empty band*	no. of necks:	32-33
54: *a discontinuously filled band*	no. of necks:	34-35
55: *a continuously filled band*	no. of necks:	36-37



empty band discontinuously filled continuously filled

ANGLE (N) OF FILLINGS

(Cf. illustration of ANGLE (B) OF FILLINGS). The fillings of the neck decoration – only when attributes 54 or 55 pertain; in the case of attribute 53 no entry is made here – may exhibit an angle relative to the rim:

56: *indeterminate*	no. of necks:	38-39
57: *parallel*	no. of necks:	40-41
58: *oblique*	no. of necks:	42-43
59: *perpendicular*	no. of necks:	44-45
60: *crossed*	no. of necks:	46-47

LIMITS

(Cf. illustration of LIMITS (B)). This variable specifies the delimination of the neck decoration. Implicit is the attribute *not delimited* (as from NECK FILLINGS the sum total of decorated necks can be derived).

If the neck decoration has been especially set off, this is done by:

61: *simple delimination* of neck decoration	no. of necks:	48-49
62: "*music notes*"	no. of necks:	50-51
63: "*dumb bells*"	no. of necks:	52-53
64: *discontinuous fringes*	no. of necks:	54-55
65: *continuous fringes*	no. of necks:	56-57

COMPONENTS (N)

Lines are counted per neck, as are the other components. Counts should include rim decoration.

The neck decoration has been constructed by means of:

66: *lines:* 10-log. (abs. no. of lines)	58-59-60
67: *points:* 10-log. (abs. no. of points)	61-62-63
68: *hatchings* 10-log. (abs. no. of hatchings)	64-65-66
69: *stab-and-drag* 10-log. (abs. no. of points in s+d)	67-68-69
70: *finger/nail imp.* 10-log. (abs. no. of finger/nail impr.)	70-71-72

IDENTIFICATION, ETC.

71: *number of sherds* in find	73-74-75
72: *find number*	76-77-78-79
73: *card number:* enter "2"	80

6. *A note on the codebook's efficacy*

It is not too difficult to gauge the efficacy of the codebook presented, efficacy to be understood here as the measure to which the attributes can be made visible in the data. As shown in Table 1 (cols. 1), developed motifs and secondary fillings score worst: only in some 50% of the cases or even less, can their traits be observed – in Hienheim as well as in Elsloo; all the other variables, however, can be scored in at least 80% of the finds.

A more precise estimate of the efficacy of the codebook can be obtained by relating the number of motifs observed to the totality of motifs presented in the material (Table 1, cols. 2). Some variables are apparently difficult to observe; among them, developed motifs are definitely the worst; basic motifs and ends are also quite opaque, whereas the remaining variables are determinable in more than half the number of the motifs. (The figures in the table relate to settlement debris; in the case of other types of data the efficacy would differ, of course; analysis of decorated pots from a graveyard produces complete information.)

Notwithstanding the markedly different qualities of the data at Elsloo and at Hienheim, the close correlation of the evaluations in the table seem to argue that it is the classification's efficacy which is measured, instead of data quality.

There are two standards that are not tested this way: validity and reliability. Validity has to do with the relevance, the appropriateness or the pertinence of the coding scheme to the research questions. There is no sure way to establish relevance; a rough indicator is whether the data can be manipulated so as to conform to the patterns presupposed by the research problem and the degree to which the answers found are acceptable to the audience. In other words: the codebook has a valid application insofar as the conclusions of this study are received approvingly.

Reliability is the degree to which observation is repeatable: are the codings identical when the same data are classified for the second time; do two coders arrive independently at the same "profile"? In a training course on find description (winter 1975/6) a number of students counted and coded a set of finds from Hienheim. Comparison with my own figures indicated virtual identity (there were discrepancies, mainly on the variable NUMERICITY; discussion showed these to be reducible to unfamiliarity).

Again, the close agreement of the proportions in Table 1 for two sites can also be taken as an indicator of the reliability of the codebook's definitions.

Before embarking upon the research problem, a set of miscellaneous points still has to be considered. This will be done in the next chapter.

NOTES

[1] A summary of parts of the present chapter has already appeared in print: Van de Velde (1976). Here, however, much more detail, background, and considerable modifications are offered.

[2] "Attribute" as used here is quite distinct from "attribute" as introduced by Clarke (1968): apart from his intermingling of ideational and phenomenal levels, his attributes are roughly equivalent to "dimensions" as used here. Clarke also recognizes "attribute states" – mostly of a present/absent nature – which correspond to the often dichotomous classes of a dimension. My use of attribute is parallel to that of Dunnell: it applies equally to properties of phenomena and to characteristics of ideas.

[3] From recent anthropological research it has become apparent that such transformations can be very significant, not only on the mythical level (as amply demonstrated in Lévi-Strauss' *Mythologiques*), but also on the level of material culture. For example, Lévi-Strauss' (1975) reports on the variations on the theme of two masks in British Columbia; also, in an unpublished study of South Coast New Guinea shields at the Cultural Anthropological Institute at Leiden (Prof. dr. A.A. Gerbrands, pers. comm.) symmetry operations like those described for the motifs of Bandkeramik decoration (Fig. 5) were observed.

[4] However, microscopic examination of thin sections of sherds from Hienheim has established a slip covering on some of them (Bakels 1978: 98). Yet according to the same author, this technique has not been described for LBK from other sites.

[5] Strictly speaking, this statement is too simple. Whereas a decorated neck zone "presupposes" a decorated belly zone, rim decoration may occur on otherwise undecorated pots. The notches and lumps making up the rim decoration are further left aside, as there is no differentiation possible. Therefore, pots with rim decoration *only* are not considered "decorated" in what follows. For the same reason, if there is decoration elsewhere on the vessel, rim decoration is to be entered only on the level of zonation.

[6] The slant motif might be thought more basic than the bow, a proposed in the text; the latter pattern can be derived from the former through simple vertical reflection. Apart from the absence of a curvilinear transformation, among the seven possible elaborations, the simple glide, rotation, and horizontal-reflection do not occur, whereas a complicated notion like horizontal-reflection-and-glide-combined would have materialized. If the basic motif has been the bow, with the slant a "degeneration", then the glide (and horizontal-reflection-and-glide-combined) do not occur. As this is also the case with other main motif, the crook, for consistency's sake the semi-circle or bow may be assumed basic, instead of the quarter-circle. Thus the Bandkeramik potters used all possible symmetry-operations but realized only one (translation) of the two (also the glide) movements.

[7] In an earlier draft of this classification I assumed two different dimensions relevant to a general description of the fillings of the neck zone and the motifs in the belly area: continuously *vs.* discontinuously filled, and heterogeneous *vs.* homogeneous composition of the band's components (Van de Velde 1976: 112). However, the latter did not relate to the quantitatively important group of non-filled bands, nor did the continuous/discontinuous opposition differentiate the empty bands from the continuously filled ones. Consequently, the redefinition has been worked out as in the text, with much better results.

[8] I first thought 1-3-9 and 2-4-8 (with 6 intermediate) to be the relevant series. However, the 1-3 transition is by no means obvious, and a re-study revealed the ambiguous nature of it: 3 and 6 very often co-occur, but when 1 and 3 are seen together, either the data are insufficient (too small sherds), or a rare exception is found to the general "rule" of sticking to a series (Van de Velde 1976: 112). The combination 1-3 (and also 1-2) was frequently encountered (accounting for the majority of the exceptions) in the middle phases of the LBK, when the first attempts at neck decoration resulted in a single row of points, the belly decoration being developed in a regular way from a two- or threefold root.

CHAPTER II

ON DATA DEFINITION

1. *Introduction*

While in the first chapter I was mainly concerned with theoretical problems of classification, I intend here to discuss the more practical issues of data definition. As the problems involved are quite disparate by nature, this chapter has been subdivided into a number of more or less independent sections.

The first point to be considered is: what to use as the unit of analysis. Thereto a number of assumptions about former states of affairs had to be made. Secondly, when discussing the noise in the data, similar assumptions and statistical notions have to be introduced in an attempt to diminish the influence of this rumble. Thirdly, as most of the computations reported here are based on proportions of the various traits (as are the models to be developed), I shall also include a brief section on the pros and the cons of percentages.

Apart from these base and pragmatic ponderings, in a separate discourse I shall dwell at some length on more lofty, methodological issues. An account of my views shall be rendered there, in order that the practical results of this study may be seen in their proper perspective. Deductive *vs.* inductive *vs.* the "third" way, (again) some very down-to-earth statistical problems, the concept of law in the social sciences, the validity of our propositions in alien affairs (especially in the light of the recent flipping of archaeological mainstream theory), and processual *vs.* structural *vs.* systems approaches – these will be the principal ingredients of the at-times-polemic potpourri concocted in the last section of this chapter.

2. *The find as unit of analysis*

A definition of the unit of analysis should follow from the research problem, with an eye to its eventual observability. As already indicated in the first chapter and the Introduction, the problems to be dealt with concern variations within the Bandkeramik tradition of pottery decoration, at Hienheim, and in a wider context. That tradition is the sum total of all decorated pottery that has ever been present at all the sites discussed; obviously, the quantity recovered is but a minor part of it, a selection of unknown size. To describe the variation, the counts of the traits or attributes of the variables of the classification should serve as a basis for the definition of degrees of "similarity"/"dissimilarity" (Popper 1972: 196; Sokal and Sneath 1963: 50, 128). Generally, because of considerations of representativity, it is most profitable to use units as large as compatible with the research questions (Hays 1973: 422). Differences between two sherds make a less impressive argument than differences between the ceramic contents of phases, to mention but the extremes. Similarly, several variables together allow more precision than examining one frequency distribution of a single attribute over space or time (Sokal and Sneath 1963: 51). Moreover, in large samples the presence or absence of the traits is more easily observed, as the incidence of "missing values" will be *relatively* reduced. This in its turn allows a better description (if wanted) of the polythetic conduct of the individual traits (Blok 1975; Clarke 1968: 37-38; Needham 1975).

If we imagine a scale of the possible sizes of units, then at one end the individual sherd will be found and at the other the ceramic contents of the site. Pots, pits, huts (or "houses" as some would have it) and groups of huts are positioned in between on that scale. "Sherd" and "site" can be discarded right away as being too small and too comprehensive, respectively. "Pots" are very rare in the find material (settlement debris), even if a generous allowance is made for reconstructable vessels: at Hienheim less than fifty pots are reasonably complete when reconstituted. This number is quite uninteresting statistically for almost any conceivable research question.

A possible alternative is presented by the "finds", or rather the finds associated with a find number. The latter is attached to any feature – or, if large, to parts of it – recognized during the excavation. Generally, these are the ancient pits, wall trenches, etc., which show up as dark patches in the light loess, but also, if such be the case, the individual layers of a stratified pit's fillings. At Hienheim, almost 1500, and at Elsloo nearly a thousand of these eminently operational and labeling units are attributable to the Bandkeramik tradition, and these in particular are meant in the discussion below when "finds" are referred to.[1]

From this description, a partial (if not a close) analogy to Montelius' definition of a "closed find" may be sensed by those interested:[2] "a 'close find' is a set of objects which was found under circumstances indicating synchronic deposition" (Montelius 1903: 3). The definition requires some comment in this context; "synchronic", "set", and "circumstances" are the keywords.

Modally, a pit will have been dug into non-virginal soil; waste lying in the vicinity may have come into it (cf. McIntosh 1977 for an ethnoarchaeological description). After digging, it will have been in use as a dumping area for trash until filled up. (It is this rubbish at which our research questions are directed; *pace* McEvedy 1967: 9.) The time required for filling a pit in this way is not set, as far as I know. Yet, *in relation to* the duration of the occupation of the site, this period should be rather small: 10 to 25 years are the usual estimate. If this estimate is close to being true, the contents may be considered synchronous, at least *relative to* the research questions. The things we are asking are structural, i.e., almost by definition slow (Behrens 1973: 180; Bertels 1973: 97).

After filling, the dirt will have settled gradually, causing a secondary depression in the soil. In it, all sorts of things happening in its vicinity will have been caught. One may suspect that at least a part of this secondary filling will be different in nature from the primary contents of the pits. Often very marked differences are to be seen on cross-sections of Bandkeramik pits, with very dark top layers and a rather light (though banded) lower part. In this way, the top layer may be considered a sealing-off of the primary fillings.

Still, this cover is not impermeable: worms, roots, trampling feet and similar agents will diffuse the contents of the layers in all – even vertical – directions, thus contaminating the original depository sequence (cf., e.g., Cahen 1976). If appropriate measures are taken, this "noise" of post-depositional admixtures (and the pre-ones as well) should not pose serious problems, as I shall try to demonstrate in section 3 below.

In the later history of the pits, substantial portions of their tops have been eroded away. A number of them has been subsequently covered up again by coluvial action (Modderman 1976b). Neither erosion (including ploughing) nor covering up would seem to interfere substantially with the distribution of the contents of especially the lower parts of the pits, except in eliminating the top layers (which were secondary anyhow).

To return to the definition of the "closed find", the decorated sherds of a pit may be considered a set insofar as they pertain to the primary fillings. The sherds may be considered "synchronic" inasmuch as it is accepted that their period of accumulation is short in relation to the period of time "covered" by our research questions. And the circumstances of recovery of the sherds should enable us to judge the synchronousness on the basis of the characteristics of the pit fillings. In other words, the recovered contents of the former refuse pits may be called "closed finds", or "finds" (which are to be synonymous for Montelius 1903: 4).

Apart from such finds (to continue the use of this term to indicate the decorated sherds in the fillings of the pits), "huts" were also suggested as possible units of analysis. For the LBK this unit might be of use; the pits belonging to a hut are easily identified as such (e.g., Pavlú 1974), and therefore their contents can be taken together. For younger periods this is not possible, however, as the pits were no longer dug alongside the houses, but farther away; we do not know how to assign to the huts those pits which were dug by their inhabitants. If then, for the LBK the huts, and for the Middle Neolithic the individual finds were entered, the scale of the units would be dissimilar, apparently at variance with the idea "unit of analysis" (Dunnell 1971: 146).

These very considerations also apply to (geographical) "groups of houses" (cf. Ch. V), which were suggested as alternative entities: Middle Neolithic pits do not occur in groups, but are widely dispersed. Perhaps little can be brought forward against the use of *chronological* groupings of pits or huts except that the diachronical ordering is unknown until the individual pits have been analysed.

I therefore prefer to use the "find" as unit of analysis. It is the largest possible grouping of objects at the site "displaying the same degree of inclusiveness" everywhere (Dunnell 1971: 146).

The theories to be tested shortly will concern relations between social units. Presumably, different social groups would show different tastes in many areas of culture, and the more akin such groups are, the more resemblance should be found among their prejudices. Archaeologically speaking, one of the areas most convenient for testing such similarities is that of ceramic decoration; comparatively, a lot of it has been preserved, and it lends itself easily to quantification. A representative sampling of the decorative universe should allow us to specify the variation in it.

It is impossible, unfortunately, to control the sampling process and to take precautions to guarantee randomness. Still, the factors governing deposition and preservation are entirely beyond control and probably independent of the decoration; thus they may be assumed to result in random selection (Hays 1973: 73; I shall come back to this topic in section 5 of this chapter). At the same time, the proportion of the originally available decoration actually in our samples (the sampling level) is also unknown: 2%(?) or 5%(?) Fortunately, however, from a computational position, it is the *absolute size* of the sample which is important and *not the sampling level*, given randomness.

The samples that have come down to us, the "finds", are easily associated with the household groups, as they are adjacent to huts (in the Middle Neolithic: to working areas?), restricted in time and space. It seems best to treat the quantified descriptions of the finds as the closest possible approximation of random samples of the pottery decoration preferred by the nearby household groups.

A more abstract formulation may be as follows: in the first chapter, the concept of "trait" or "attribute" was introduced to indicate any of the various possible states of a variable or dimension of decoration. On the one hand, a trait indicates a class within the entire classificatory system of Bandkeramik decoration; on the other hand, its definition identifies the things to be grouped in that class (Dunnell 1971: 44-45). Thus, the decoration on a sherd can be classified by noting that the variable TECHNIQUES is represented by the attribute *unidented spatula;* that the variable STRUCTURE is represented by the trait *curvilinearity;* and so on. Similarly, the occurrence of the several traits in the group of sherds called "a find" can be counted. As customary, the array of counts is treated as representative of the originally present decoration (at a specific point of time and location). The arrays for a single sherd, or for the set of sherds attributable to a hut, etc., are also samples, of course. Being on different levels, they have different associations though: less or more inclusive; more or less homogeneous in a chronological or a chorological sense.

Summing up, the group of sherds that is called here "a find", yields the data (in the guise of counts) that together constitutes the largest possible sample of the population of decorated earthenware originally associated with a household group.

In this context it seems appropriate to say something about the supposed status of the decorated ware in the entire Bandkeramik ceramic repertoire. Decorated pots constitute only a minority of the totality of earthenware excavated at a settlement site. It is generally thought that the distribution of the decorated pottery in a village is similar to that of the undecorated ware (Butschkow 1935: 10). I do not know of any research concerning this matter; from experience I would say that pits yielding undecorated sherds also produce decorated ones (provided the number of sherds is not too small to introduce sampling biases), and *vice versa*. In line with the discipline's tradition I shall also assume that the distribution of undecorated ware is similar to that of the decorated pottery, and therefore need not be accounted for separately.[3]

3. *Noise*

A perennial problem in any research program is that of "noise", the "E" at the end of our equations. Hardly ever can a statement be found on what an individual researcher has done to remedy it, however (e.g., "noise" is not discussed in Doran and Hudson 1975). Such an omission may suggest either a conscious or an unconscious denial of the problem, leading to the incorporation of all (even suspect) material, or to a casual selection procedure of quite an unsophisticated nature. It will be sufficient to illustrate the latter case by one of the very few explicit statements about noise:

> ... proveniences [i.e., finds] which were mixed were located very erratically [on the chronoligical axis]. Since a mixed provenience has no correct position, these proveniences were eliminated from the scaling ... The ultimate criterion, however, was whether the frequencies of occurrence of the various states [i.e., attributes] showed strong characteristics of more than one segment of the [temporal] sequence (Drennan 1976: 298).

It seems superfluous to criticize such a method; instead I will try to develop an answer, however partial, to the problem.

I start from the notion "closed find", already detailed in the previous section (it should be noted in passing that Drennan's "mixed proveniences" would not qualify as closed finds in a Montelian sense). Then "noise" is everything which interferes with the "closed" quality of the find, admixture generally. For refuse pits acquire sherds (or any other kind of rubbish) through two different processes:

– One process is *non-accidental* and directed, the purposely dumping of waste. Pits used this way will contain relatively large numbers of sherds in their fillings. If all such pits had the same characteristics (circumference, length of use, mean distance to working areas), the resulting frequency distribution of contents per pit should be a normal (Gaussian, bell-shaped) curve. Since it is highly improbable that these parameters would be identical for all refuse pits, the curve will be skewed. These contents, dumped on purpose (sherds, or flint debris, wasted tools, or burnt seeds), are generally speaking the objects of archaeological research; with proper precautions "closed finds" can be made of them.

– The second process of waste accumulation is of an *accidental* nature: sherds (or, again, flint debris, used tools, charred seed) lying around the site sometimes find their way into the pits, stochastically, as it were. This applies both to sherds older and to sherds younger than the contents we are concerned with. Older sherds may have been lying around the site and were casually kicked in by the unconcerned natives while the pit was open. Younger sherds may have accumulated in a similar way in the secondary depression that originated through the settling of the original contents; palaeomoles and -mice as well as -worms will have joined in muddling the neat stratigraphies. This very process is the cause of the noise. In any fair sample of pits, the majority will contain none, or only a few sherds (if subject to this process and not to the first one). The curve of frequency distributions of the numbers of sherds per pit should be Poisson-like (Dixon and Massey 1957: 231; Doran and Hodson 1975: 48; Hays 1973: 202-206).

To check and substantiate the above propositions, a random sample of 92 Bandkeramik finds was drawn in Hienheim. Sampling level .130; 38 of those counted (41.3%) did not contain any decorated sherd; 32 (34.8%) contained only from one to four sherds, and from each of the remaining 22 finds (26.9%) five or more sherds were recovered. In Fig. 7 a more detailed representation of the

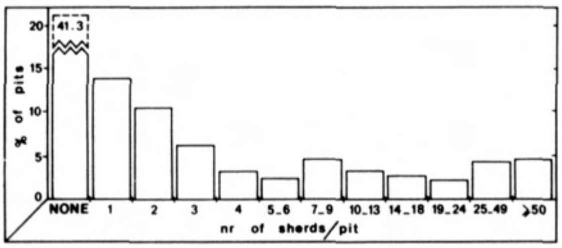


Fig. 7. Frequencies of counts of decorated sherds per pit. Taking the varying width of the classes into account, this distribution is interpretable as two superposited curves summated:

– a Poisson-like distribution with mode at zero, rapidly falling off to the right.

– a normal (Gaussian) curve, skewed, with mode at about eight sherds/pit.

The two curves (thought to be the result of a noise, and a dumping process resp.) meet at between four and five sherds/pit.

Counts are based on:

– a random sample of 92 pits, for the pits containing at most four sherds.

– actual numbers for the excavates part of the site, for the pits with at least five sherds.

relative sizes of the finds is shown, as proportions of the universe sampled. Separated at ca. five sherds per find or pit, in that graph the two distributions that were hypothesized above are to be seen:

1. The Poisson-like distribution with its mode at zero shows up very clearly. Its other characteristics are: average (or: $\lambda = Np$) .685 sherds per find if the pits containing at most four sherds are counted. From a table (e.g., Dixon and Massey 1957, Table A-15) it appears that in 99.9% of the cases in any substantial quantity of finds, at most four sherds will be found, given a true Poisson regime as specified.
2. With its mode at about eight sherds per find, the normal distribution derived above is also visible, though less clear. As the arithmetical mean is at 35.226 sherds per find, the curve is very skewed, as expected.

If this interpretation of the distribution shown in Fig. 7 is accepted, then removal of finds with less than five sherds will take care of the "noise" sherds. It should be noted, however, that the remaining larger finds (26.9% of the total) were also subject to accidental kicking in, so some of them will inevitably contain a few "Poisson" sherds: a find of five sherds is entered into the file, no matter whether it contains three "baddies" and two "goodies"; "goodies", or even "baddies" only. There is no way to reliably solve this, as far as I can see. Truly transitional decoration (either of a progressive or of a regressive appearance) might be thrown out if a sorting by hand were attempted (as Drennan 1976: 298 did). Still, comparing average contents of the retained, larger finds (35.226) to the noise (at a level of 1.97 sherds; empty pits not counted) a rummage of 5.59% would not seem to be very serious.

To conclude: noise will be dealt with in the present analyses by leaving out all pits containing less than five sherds. For the data retained a noise of ca. 5.6% should be reckoned with.

4. *On proportions and measurement scales*

In Chapter I the analytical levels of variables and attributes were introduced. For any variable, at least two traits/attributes are implied, *present* and *absent*, being the minimum. The traits, considered alternate possibilities, are mutually exclusive; together they comprise all possible alternative characteristics; they are exhaustive. In other words, for any find the array of the counts of the several traits of a variable is fully descriptive. To make the arrays comparable it suffices to scale them to unity, i.e., to render the counts of the traits as proportions of the summed number of traits (of that variable) in the find.

By this method, the nature of the data is changed in a number of ways; it is not clear, whether this is (dis)advantageous. In the first place, traits in and by themselves are of a qualitative nature: decoration just exhibits trait *a* or it does not. Since we are observing *finds* (not sherds) it is very well possible to say that in find NNN trait *b* occurs *p* times, that it occurs *q* times as often in find MMM, whereas in find KKK it does not occur at all. Apparently the scale of measurement starts at zero, counts by fixed intervals, and may be handled to establish ratios – in fact, it is a ratio scale.

Now suppose that for a variable A the traits *c* and *d* are the logically possible alternatives. Then one might note that in some find trait *c* occurs *r* times, and trait *d*, *s* times (with either *r* or *s* but not both, possibly equal to zero). Also their relative frequencies may be computed:

freq. $(c) = r / (r + s)$ and
freq. $(d) = s / (r + s)$

always, the sum of the frequencies freq. $(c + d)$ equals 1.0. Per variable, these frequencies will be referred to as the "mix".

The range of possible frequencies of a trait is thus fixed at 0.0 to 1.0. At the same time clear archaeological meanings are implied (e.g., in find SSS the attribute *c* is specifiably more frequent than in find TTT) and statistical exercises are made possible (Hays 1973: 88). For specific statistical purposes some more conditions have to be fulfilled; so, for instance, in contingency table analysis, nonempty cells; in some types of multivariate analysis, linear relations among the variables; etc. A number of the more fundamental requirements will be dealt with in the next section; the more specific ones will be discussed when the occasion arises.

In the second place, the size of the find is eliminated from consideration as each trait total is defined a unity, notwithstanding the sum of its raw numbers. In this way, small and large finds are given equal weights. Yet, "size" has obviously something to tell about the reliability of the counts. The larger a find, the more representative it will be of the state of affairs existing when it was deposited. The comparability achieved through the conversion has not been matched by an increased compatibility of the finds.

In the third place, easier observable attributes are put on a par with those that are more difficult to perceive (again, per definition every variable adds up to unity, regardless of the counts of the constituent traits). For example, virtually every decorated Bandkeramik sherd allows for an unequivocal determination of the variable TECHNIQUES: uni- or multidented spatula, "goat foot tool" etc. are very easily identified. Conversely, some other variables are not so readily observed – among every hundred decorated sherds only some 25 lend themselves to recognition of the spiral or the wavy nature of the motif on them (cf. Table 1). This means that the statement: "in a given find the proportion of the motifs applied with a multidented spatula is .75" carries a different weight from the statement ".75 of the motifs in that very find are waves". On the other hand, the announcement ".60 of the recognizable motifs in a find are spirals" conveys more information than "6 motifs in that find are spirals".

Lastly, all traits are considered of equal importance. This is certainly a difficult point, as it interferes with the diagnostic character of traits which, however sparsely represented, nonetheless may be important markers. For example, a single row of points as neck decoration, execution of the decoration in so-called "music notes", or the use of the "goat foot tool", are all important chronological markers.

Perhaps the importance of these markers has been overstressed in some contexts. A chronological signalling function cannot obscure the fact that even in prehistory predictability, normality and conformism have been the rule and not the exceptions. I do not pretend to give a definitive refutation of the use of "marker traits", although I do not see how to establish *beforehand* which trait is more important than its co-traits. It may be possible, of course, that during an analysis some trait(s) turn up that work well for the research question *then* in hand. So, for instance, at Hienheim the relative proportions of recti- and curvilinearity in a find are a very sensitive chronological marker; in Elsloo the two occur in about a constant ratio throughout the village's inhabitation (cf. Ch. III) Together with the absence of any theory whatsoever about "marker traits", apparently the restricted geographical applicability effectively blocks generalizing attempts. It is possible, however, to leave out unimportant traits when re-doing an analysis: it is as simple as re-punching a data-definition card.

5. *On validity*

When becoming an archaeologist it seems very fashionable these days to discuss extensively the "new" archaeology (e.g., Van der Leeuw 1974; Slofstra 1974). The upshot of such essays is often a fairly pronounced view on a number of *methodological* issues. There are two reasons why I am suspicious of the (scientific) utility of such contributions. In the first place, I lack competence to fully appreciate the significance of the methodological aspect of the discussion and the resulting stances; and in the second place, I also think that some ritualism is involved.

My own lack of competence in methodological matters seems to be shared by a number of other archaeologists. Charges with that purport can be found in the rare statements on the subject by professional methodologists in archaeological journals. According to them, most "new" archaeologists would simply seem to have no overall picture of the field and thus talk only about one disconnected aspect of a wider problem (Morgan 1972; Salmon 1975).

Having been trained as an archaeologist, I cannot value this attack properly.[5] However, I do not think that the conclusion should be that archaeologists would do better to keep clear of methodology; on the contrary. Only we should not try to summarize in a few pages the contents of a discipline that has been evolving since the Iron Age, nor abstract in a dubious way some problems from it and then look for archaeological counterparts. Rather, I think we should work the other way around: select some archaeological problem – insoluble so far – and see whether a relevant formal way out has ever been provided in methodological writings. Methodology is more a descriptive than a normative science; it studies the methods contrived in the "applied" branches of knowledge.

The second point that makes me hesitate to discuss the "new" archaeology is the possibility that ritualism is involved. There is some flavour to it as was to be tasted in Marxist archaeologists' reports in an era now happily past: starting from the general principles of Marxism-Leninism (not to be confused with historical materialism) the presence in a grave of, say, a dagger was accounted for. Also, a similar odour arises from the just as antiquated (but certainly less past) account of a hoard of bronze artifacts somewhere on a river levee as an example of the principle of free enterprise. In the same way sometimes a scent of consecration is to be smelt when Hempel or Popper are invoked in archaeological writing about (what are thought to be) methodological problems.

Be this as it may, the tradition (or, at least the fashion of the 'sixties and the 'seventies) seems to be fairly well established, although nonconformists could be cited as well: Louwe Kooijmans 1974, or Verwers 1972 for example. I shall also take the risk.

However, instead of entering into the methodological debate (and thus falling into the trap outlined above) I would rather summarize part of the archaeologists' discussion very briefly, highlighting its main topics by a small set of examples. And in the remainder of this section I shall then feel free to present one or two points that are not usually considered, though they do have some practical value in my opinion.

What the "new" archaeology is often reported to be about is the opposition inductivists-deductivists (the latter of a hypothetico-so creed or not). The labels refer to proponents of either of two solutions to a central scientific and methodological (and consequently also archaeological) problem: statements generated by research are strictly applicable only to the covered data and not to other relevant material. Implicit or explicit generalization ("inference") from the researched subset to the totality is called "induction". People seeing this as the (only legitimate or only practicable) method of science are called "inductivists". In line with the method's long pedigree, numerous rules have been developed to guarantee reliable results, apparently, however, to no avail (Hempel 1965: 6; Popper 1968: 29).

Deductivist methodology starts from the other end: theories are concoctions based on revelation, hunch, educated guess, analogy, tight reasoning, tradition, induction and what not. From theories, implications ("hypotheses") are derived ("deduced") to be tested in empirical reality. Given a solid, well-constructed framework of hypotheses, conditions, and specifications ("nomological network", De Groot 1961: 84-87), a properly designed experiment will show the viability of the underlying theory. However, pure deductivism leads nowhere, it is observed by its critics. Nothing not already contained in the parent theory can be deduced from it in the hypotheses (Salmon 1976; my reply to this critique would be: then expand the theory).

In the extreme formulations above, both standpoints are as far apart as East and West. Conclusions from inductive reasoning are usually framed in rather general terms, and the very moment somebody turns up with a counter-example the field of applicability will be restricted to exclude the nuisance (Popper 1968: 80, 82). On the other hand, deductivists put out only very preliminarily phrased hypotheses, at best accompanied by a few corroborative tests of their favorites. In practice, both ways suffer from the very small number of theories actually tested in archaeology. The inductivist generalizations remain standing with their (largely) undefined field of applicability, and the deductivist hypotheses with their unspecifiable degree of corroboration (examples of both can be found throughout the present volume). And both result in feelings of uncertainty in the audience (Clarke 1968: 17).

As an illustration the following example is offered. Binford accuses Clarke (both are prominent "new" archaeologists) of rigid inductivism (Binford 1972: 248; also in Clarke 1972a: 114) where Clarke himself accuses Binford of not having systematically defined his categories. According to Clarke, "types" are to be defined by means of taxonomical methods. The definitive or "key" traits of a type should be selected through a hierarchical analysis (practically, cluster analysis) of all traits imposed by hominid action (Clarke 1968: 137) – obviously, an inductive procedure. And definitions arrived at by other means are conducive to conflicting results: "one cannot build sound structures from shoddy components" (Clarke 1968: 188-189). Of course this statement is true, writes Binford. It is, however, also applicable to Clarke's definitions: "[any] classification system depends upon a selection by the investigator of criteria considered "significant" for use in classifying data" (Binford 1972: 248). In other words, "key" traits derive their status from their relevance to a research problem. This is a deductivistic posture, which is characteristic of Binford's scientific behaviour, according to himself (Binford 1972: 90-91).

Indeed, it should be admitted that Binford works with models and sets out to test them; yet he keeps on discussing "verification" (presumably in Hempel's footsteps). Analogously, Clarke was not a rigid inductivist in my opinion, given his explicitly declared intentions in his Beaker studies: "what we have to look for [is] *test* conditions" (Clarke's emphasis; in Lanting and Van der Waals 1976: 481; also cf. Clarke 1964: 181; 1970: 7; 1974). Yet he was not a hard-boiled deductivist, either. In his more theoretical book (which is not free from inductivist – or "empiricist"? – tendencies) he presents a much more flexible approach than straightforward deductivism: models are introduced as predictive frameworks or hypotheses at different levels of abstraction (Clarke 1968: 32). A similar view is stressed in the introduction to his "Models" (Clarke 1972a: 3) and implemented for the Glastonbury simulation (Clarke 1972c) and Bell Beaker exchange networks (Clarke 1974). This evaluation of practical and theoretical stands was also arrived at in Klejn's review article (1977: 23).

From this example it might be understood that practical research apparently suggests modifications of puritan standards; it substantiates the reluctance of methodologists to defend rugged deductivism, as understood by its archaeological proponents (Salmon 1975: 459; Morgan 1974: 133). therefore, I will not further recapitulate the often bitter discussions among the "lesser gods"; the interested and unacquainted reader may turn to summaries such as Binford 1972: 78-104 or Schnapp 1974, both of whom defend deductivist views; to Hawkes' vehement (and *im*proper) 1968 inductivistic counterattack; or to Waterbolk (1974) for a non-reactionary balance that is slightly tipped in favour of inductivism.

The choice in the deductivist-inductivist option (including the weaker derivatives) is a matter of fashion: in due time archaeological procedure will conform to either of the methods and the battles will cease – if they have not ceased already, except for some backland skirmishes – because of lack of combatants (Kuhn 1970: 18-19).

To avoid confusion about my prejudices: I am of a deductivist-modified conviction. A network of laws and/or common sense propositions (the "nomic nexus" of Hempel's 1965: 488), together with specifications of conditions, constitute a *proposed* explanation for some archaeological phenomenon; subsequently, the entire conglomerate will have to be tested (as far as testing goes) against other archaeological data. As theory precedes observation, it is unimportant how the network/model-/situational description-*sensu lato* is arrived at; it is testability that counts. A number of non-negative tests or falsificatory efforts are assumed to corroborate the explanation offered – for the time being, that is – in the vein of Popper (1969: 155).

Pragmatically more important than the above is the hardly ever explicitly faced problem of the prerequisites for using statistical methods. After all, statistical theory is based on mathematical antecedents, so one cannot forego the minimial exigencies to be fulfilled in order to assure a reasonable pertinence of said theory to research practices.

First and foremost among these problems is always that of the randomness of the samples, or their representativeness. This is usually phrased as the requirement that all samples of relevant and possible outcomes are equally likely to occur (Hays 1973: 75). In the present study, this condition is *not* met, as only that part of the site of Hienheim has been excavated which was not too severely affected by pedogenetic processes after the Early Neolithic. This part roughly coincides with the segment of the site assumed to have retained a fairly complete sample of Bandkeramik house constructions, the main research focus (Modderman 1976b). The remainder of the site, the part facing the forest, has been left untouched. This implies that no generalization (in the sense of statistical induction) about the complete settlement is possible for aspects other than (Bandkeramik) house construction. What is possible, however, is to test whether in the dug-up part any propositions are falsifiable, as outlined above. It should be borne in mind that in the forest front of the settlement data may have been completely different, and thus the possible outcome of the tests. Therefore, a statement untenable in the waterfront segment of the site will remain so for the complete settlement, no matter how many data are added; conversely, an as yet non-falsifiable statement stands open to rejection with every possibly conceivable extension of the excavated area to the landward side. Similarly, the graveyard and the settlement site at Elsloo, also to be touched upon in subsequent chapters, have not been excavated to their limits.

Another assumption of many statistical tests is the normal distribution of the data. This is generally interpreted as a normal distribution of the probabilities or frequencies of the intervals between the (to be) observed values or states of the variable considered (Hays 1973: 297-299). Several qualifications pertain:

- Even without full realization of this requirement, the theory of normal distribution leads to reliable results, if sample sizes are at least "moderate" ($n \geqslant 10$ for samples from a normal population; $n \geqslant 30$ from a non-normal population; Hays 1973: 304, 322).
- Again, when the number of samples is fairly large, then the distribution of the sample means is normal, no matter what the distribution is in the population the samples are taken from (Central Limit Theorem; Hays 1973: 317).
- Especially tests regarding sample means are foolproof. For example, Student's t-tests yield reliable results for moderate sample sizes, even under "severely" deviating conditions, as do Chi-square tests of goodness-of-fit. On the other hand, tests of other statistical measures, like the F-tests for the distribution of variance, are better avoided, if normality cannot be assumed (Hays 1973: 410; 736; and 451, respectively).

Finally, some statistical methods (especially among the multivariate variety) assume the observed data to be the result of linear combinations of the effects of conditions and causes (Van de Geer 1967: 105, 117, 168; Nie et al. 1975: 368, 470), clearly a metaphysical problem. Fortunately, this difficulty can be circumnavigated by means of another interpretation of the Central Limit Theorem, which states that with increasing sample size the distribution of linearly combined means approaches normality (Hays 1973: 320).

These collected palliating remarks are to the effect that "juggling the premises" is allowed to a certain extent without seriously affecting the reliability of the results (= repeatability; Selltiz et al.: 1966: 148, 166). Or, "If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are" (Popper 1968: 50).

Another point I would like to bring forward has to do with the concept of "laws". It seems that for the sake of methodology the word had better be avoided – whether laws exist cannot be conclusively established or denied via testable proposals: "We must regard *all laws or theories as hypothetical or conjectural;* that is, as guesses" (to quote Popper again; his emphasis; 1972:9). This does not imply that occasional laws or corroborated hypotheses will be nonsensical – that depends on their contents as the latter determine their possible permutations to fruitful research (Hempel 1965: 343; Popper 1968: 437). Given the considerable number of influences which together result in social behaviour, it may be safely assumed that laws of social or group behaviour will be of a statistical nature (as opposed to universal laws; cf. Hempel 1965: 376-377): they will assert that in x% of the cases y will be observed.

Notwithstanding the complexity of the "causes" of behaviour, even those people who are but vaguely acquainted with the social sciences cannot fail to be impressed by the very predictable conduct of the members of our species, especially when operating in groups (*pace* Van der Leeuw 1976: 74; see, e.g., Harris 1968: 471, but also Hempel 1965: 445, note 28). If such laws are desired to construct an explanatory/predictive framework in accordance with my previous description, and if such statements are not available in archaeological literature, the easiest way to obtain them is to leaf through other social-science texts, especially on the subject of cultural anthropology: archaeology is anthropology is social science (with apologies to David Clarke 1968: 13).

Now to people *not* initiated in ethnological scripture it is probably difficult to locate an explicitly formulated law. The explanations (if any offered) by anthropologists are mostly of a casual nature, which is to be expected of course: rational solutions to understandable problems will only be different because of differing situational constraints (Harris 1974: 12; Lévi-Strauss 1955: 386; also cf. Popper 1966: (II) 264-265). Still, there is more to it than this first approximation. In ethnology, explanation is often tacitly tied to a very general and implicit model, of the type of Kuhn's paradigms (Kuhn 1970: 10). For instance, if the author is a functionalist, he will assume every cultural element to contribute ("function") in one way or another to the continuity of the culture it is found in. Likewise, an evolutionist will seek to separate "survivals" of earlier cultural phases from more recently acquired aspects of the culture under view, or concentrate upon developmental sequences of institutions. And a structuralist will be shuffling x-themes to decipher the underlying message of the cultural aspect X in an area. On the hidden basis of such background constructs – almost attitudes (Kuhn 1970: 47) – more specific models are built, in which conditions of a demographical, a geographical, a historical, etc., nature loom large to explain why a given population acts as it does (a beautiful example is to be found in Onvlee 1949, where on an implicit structuralist paradigm the intricate social implications of an irrigation system are unraveled). It is these latter models which are of a self-evident nature, and of which the "law"-like character is but seldom recognized (Popper 1968: 104-105; 1969: 388; Hempel 1965: 427-428, 360-361).

If the master-models are discussed at all they are often presented as axiomatic in the ambiguous sense of conventions for everyday use and general hypotheses (Popper 1968: 72-75). Common examples are: Sahlins and Service 1960: 5-11; Malinowski 1925: 44; Lévi-Strauss 1967: 21-22; and Harris 1968: 636.

The "law"-like character of parts of the specific explanations in ethnography has been recognized by the much-attacked earlier "new" archaeologists: Binford's smudgepits (Binford 1972: 33, 42-45; originally formulated in 1966), Mrs. Binford's functional variability hypothesis (in Binford and Binford 1968: 49; first public announcement in 1965), Clarke's Beakers (Clarke 1964: 180), the Deh Luran Project (Hole, in Binford and Binford 1968: 252; first reports in 1962), Leroi-Gourhan's cave studies (Leroi-Gourhan 1965: 28, 31, 40), all are more or less stock instances. More could of course be cited, also including Scandinavian, Russian, and Central European publications.

More fundamental than formalistic niceties about the concept of or difficulties with the localization of laws is perhaps the problem of the validity of our propositions about definitely past and distinctly alien culture. Validity, that is not in the sense of formal method, but in the sense of meaning, whether our conclusions bearing on other people's culture would be significant to them.

As I could not find any discussion of the problem in recent archaeological literature, my first thought was that the matter had been settled long ago and was laid to rest in the file of the discipline's basic understandings. From oral discussion with some of my colleagues it appeared that a number of them had not considered the problem at all and even were inclined to question its legitimacy. It was held that a methodically valid ("sound") explanation is valid for *all* concerned, including the object of research, given its human nature. This position is a very fine example of ethnocentric and chronocentric reasoning: projecting one's own experience into other people's on an implicit claim of universality (for a further discussion on this topic, cf. Lévi-Strauss 1973: 385-387). It would be wrong to ascribe this standpoint to the whole Brotherhood of Archaeologists without further sociological and bibliographical research. On the other hand, I think the voicing of this viewpoint is a sufficient reason to make a few remarks on it.

Let me first consider the sometimes cited requirement that in order to be intelligible "sound" explanations are to be non-jargonistic and to be written in the Kings' English (or its equivalents in other languages; Hogarth 1972: 301; Plog 1975: 212; Renfrew 1969: 243). The two are not identical; they are different dialects. If jargon is a specialism's dialect, non-jargon is the remainder of the language, used by people who are not initiates or not (yet) accustomed to it. The King's English has more of a class attribute: it is perfectly recognizable as "alien" for non-members.

Second, much as the use of a dialect has its social determinants, so is scientific explanation understandable for a limited group only. To demonstrate this and to carry the discussion further, I start with Hempel's general description of explanation (1965: 425-427), where he distinguishes "pragmatic" form "objective" explanation. The former type is aimed at a person or group of persons for whom the explanation is intended. As it makes use of the already extant knowledge of its target, it is a relative notion. Objective explanation, however, is absolute. It is couched in objective terms, and gives a methodical account: it is independent of the body of knowledge possessed by the audience. For Hempel, objective explanation is synonymous with scientific explanation. I think this distinction to be much too pronounced; yet it conveys very well the universalistic *claims* (also noted above) of *scientific* explanation, as opposed to the more down-to-earth *pragmatic* kind. Hempel maintains the distinction (and so the claims) even though he observed a few pages earlier that scientific explanation has a pragmatic aspect as well: a *full* explanation of any fact is simply quantitatively (if not qualitatively) unattainable (Hempel 1965: 422, 424). The selection of arguments to be entered into an explanation is conditioned by the current scientific paradigm (Hempel 1965: 428; Kuhn 1970: 19): a number of the relevant arguments will be assumed common knowledge and thus omitted from explicit rendering. From this it follows that any explanation (perhaps the case can be generalized to include all scientific accounts) is intelligible only to a restricted group of people, the specialists of the field, i.e., the archaeological community (for a different line of thought ending in the same conclusion, cf. Popper 1972: 165).

At this juncture I might consider my theorem proven, since by implication the explanations acceptable and valid to the present-day archaeological community are not necessarily so for other groups, whether these are people working in the anthropology department next door or archaeology's long dead and gone research objects. I cannot resist the temptation to examplify the above, however.

As is well known, the paradigmatic shift in archaeology of the late 'sixties involved the substitution of "diffusion" and "invasion" as general explanatory principles by "adaptation to local circumstances" and "continuity"; allochthonous "causes" were replaced by autochthonic systems.[6] There are two particularly clear articles that heralded the change: J.G.D. Clark's famous anti-invasionist article (1966), and W.Y. Adams' second thoughts about Nubian proto-history (1968). At about the same time D.L. Clarke was developing a model of culture in agreement with the new look (Clarke 1968) – yet it was also D.L. Clarke who foundered on the new dogma he had so actively sought to promote. The interpretation of the people behind the pots in his "Beaker Pottery" (Clarke 1970) was based on the antique idea of invasion as explanation (the text had already been formulated in 1964, I understand). In a more recent article on the subject by Lanting and Van der Waals (1974) Clarke's thesis was not even mentioned, notwithstanding its modern method. The only satisfactory explanation I can think of is that Lanting and Van der Waals are working with the newer paradigm of autogenic change, and thus (in spite of their very simple, not to say crude, methods) may rightly forego previous explanations.

If this interpretation of the previous, rather sketchy example is accepted, then it can be seen that what "new" archaeology[7] means to introduce is *neither* a deductive methodology (as Hill 1972: 62 seems to think), *nor* the use of quantitative methods (as noted by Plog 1975: 222), *but* the concept of *culture as a locally adaptive phenomenon* (Binford 1972: 198; original 1964). The methodical and methodological innovations are by-products of the adoption of the new model (Kuhn 1970: 126, 88); as Hogarth (1972: 303) observed, new methods do not make a new archaeology. It is also clear that earlier solutions to the problem of culture change may now be legitimately considered "weak" if not "ad hoc". By no means, however, may dishonesty, or scientific laziness be read into efforts of the invasionists; in their days their explanations were considered sound, adequate, and valid (Snodgrass 1976: 61 gives a similar view from a chronometric point of view).

To rephrase and extend the foregoing discussion, I venture to say that an acceptable archaeological explanation (conceived of as a structured rendering of relevant "facts", "laws" and conditions) is anachronic and anachoric in relation to its object, and only valid in conjunction with the current state of theorizing. This does not amount to saying that such explanations are *therefore* objectively invalid: given sufficiently explicit wording and testing, a theory (explanation of a state of affairs) may stand a shift in paradigm – not all invasions have been exorcized after the 'sixties.

It might be observed, in addition, that the above is partially parallel to the anthropological discussion on "*emic*" *vs.* "*etic*" (Harris 1968: 568-604; Appell 1973). An "etic" or "ethnological" description or explanation conforms to "objective" standards, it is articulated in a "culture-free metalanguage" (Appell 1973: note 10), and thus would belong to Hempel's category of "objective explanation" already alluded to. An "emic" or ethnographical description or explanation of a social phenomenon, on the other hand, employs the conceptual frame of those involved, the native participants. This conforms to Hempel's standards for a "pragmatic explanation". Furthermore, an "emic" account should be worded in such a way that non-participants are able to re-enact the phenomenon in a manner acceptable to its original producers. Schematically, "emic" explanations are of the "if I were a horse . . ." variety, whereas "etic" ones are of the "*equus sp.*" kind.

As a further example, in the context of theorizing on the origins of the state, this institution is considered a means for the perpetuating of differential access by social groups to the factors of production. The principal testability of this proposition makes it of an "etic" nature. Aboriginal informants would voice a legitimation of the differential access in terms of privileges stemming from heredity and sacral backing: an "emic" account employing native categories (Claessen and Skalník 1978). Transferring the ethnographic object to an archaeographical status, it will be obvious that "emic" categories once attributable to it are no longer recoverable (not to speak of testability). And above, I have already done away with the idea of universally valid (i.e., "etic") accounts. The best we may hope to attain, therefore, is a statement of the kind "If they had been archaeologists at x, in the year y . . ."

A final point to be considered briefly in this section is that of structure *vs.* process, which are thought to be incompatible by some (Bertels 1973: 244).

As such, a model or a structure of a reality is different from a model of a process going on in some reality: even their referents (the realities they attempt to mirror) are dissimilar. The idea of "structure" has to do with relations between a number of elements or group of elements, whereas "process" relates to changes in relations. (Besides, change is also supposed to be orderly or structured; for present purposes I shall regard this notion simply as one of a higher level of abstraction, to be dealt with below). As a rough approximation, a model of a structure has some *static* connotations (e.g., Lévi-Strauss 1967: 22, 88), whereas processual models may be thought of as *dynamic* (Binford 1968: 14; also Clarke 1968: 48-52, plus refs.). Given the restricted and heuristic purpose of models, it is hardly surprising that the two cannot be transformed into one another. After all, one does not develop a model of a structure primarily to explain some process, and vice versa.

Yet, on the other hand, if these models are thought of as relating to one and the same system, another reality is introduced. The structure of this reality is in the process of system maintenance, which is a structured consequence of the interrelation of its parts. In this way, a systemic model is an abstraction of a higher order: it contains a static structure or framework of elements and their relations, plus the structured rules of the dynamic articulation of its several elements; *as far as* both are relevant (the essential property of models) for the research question. In other words, Bertels' statement referred to above, must be seen with considerable relativity.

Two rather obvious examples are offered as concluding illustrations:

1. It is possible to describe a demographic system by means of the following equation:

$$\frac{dN}{dt} = \frac{k\,(Nmax - Ni)}{Nmax}.$$

In a simple way growth (a process) is linked to the difference of maximal and actual population size (Nmax and Ni, respectively) (a structure) (Kuenen 1967: 39, 77).

2. Intuitively less abstract is Marx-Engels' general systemic model of history: class struggle is the motor of history (Marx and Engels 1848: 23, 38). Within this statement a structure is hinted at (socially conditioned differential access to the means of production, as implied by a class structure), and a process (the clash of the classes concerning the control of the means of production) – together constituting the system of historical process. Though temporal bounds are implied (history, not all events of the past in general, are covered), geographical bounds of the system remain implicit (as in the previous example).

After these two rather theoretical chapters, we now turn to a consideration of the Bandkeramik phase at Hienheim, for which a number of models on various levels of abstraction have to be constructed.

NOTES

[1] Although subsequent excavation of the feature may reveal the arbitrariness of the subdivision of (the contents of) a pit, for handling, storing, and analytical ease the separate numbers are retained in principle. Needless to say that for many purposes (including some of the problems broached in the present volume), such arbitrary units may be grouped together or used as independent internal checks of homogeneity and outcome.

[2] I am not so much impressed by requirements of "closedness". Uncompromised adherence to this criterion inevitably leads to difficulties, since "noise" in our data is unavoidable. The combination of strict observance of this "closedness" with the rather loose and in my opinion largely implicit standards of

comparison known as "The Method" (i.e., Montelius' comparative method) has little to commend itself. I rather suggest a conscious, controlled lowering of our standards of "closedness", and an upscaling of those of comparison. The downscaling of the standards is discussed in the section 3 of this chapter; the explicitation of comparison has been dealt with in the first chapter.

[3] Actually, I regard this assumption as highly questionable. If, and only if, all pots in the settlement were randomly chosen for every possibly conceivable task in every household, then an even distribution of all kinds of sherds/ceramic waste can be expected. Now imagine somebody fetching water from the Danube or the Meuse in a small bowl or even a dish, across a settlement site infested with pigs, pits, and kids, and ten metres down a steep slope. Surely, even for Bandkeramik people such a proposition is untenable. But at the very moment a special function is regularly (not even exclusively) assigned to one kind of pots the even distribution of the wasted pottery is disturbed.

Some support for this line of reasoning is found in Farruggia et al. 1973: 160-161, where the counts of decorated and undecorated sherds are given *for the largest pits* at the Langweiler 2. The general impression is a fairly constant number of decorated sherds in the pits (*range:* 16 to 533 sherds, *mean* 67 sherds/pit) as compared with rather wildly fluctuating amounts of undecorated sherds (*range:* 68 to 2851;*mean:* 302 sherds/pit). The *ratios* of decorated to undecorated range from .08 to .92, ave. .31, with an overall average (large pits only) of .22. (On pp. 200-202 of the ref.book, a more extensive listing is presented).

[4] Finds with from one to four sherds (inclusive) represent 59.3% of all finds containing any decorated sherd. Retention of them causes the noise level to rise to ca. 4.7% for the entire set and lowers the informative content (as expressed in the number of sherds/pit) to 14.5. Leaving out all finds smaller than five sherds, the lower tail of the normal distribution is cut off as well. But what weight should be given to a mere two or three sherds?

A slight inexactness should be pointed to: the "model" of debris accumulation had to do with pits, whereas the observations (on noise) bear on "finds"; the two are not identical, as described above (p. 27).

[5] The charge that archaeologists do not sufficiently command the field to trespass on methodological precincts may be matched by the claim that methodologists should be very careful not to transcend their own limitations. For example, Salmon's 1976 article, where she discusses archaeological methods as often overlooked, whereas in reality these are part and parcel of any properly conducted excavation. Or a more general view is Popper's on the social sciences *as expounded in his 1957 book*, especially the rather limited set of authors referenced there.

[6] Without further inquiries the cause of this paradigm turnover can merely be guessed at. There are two lines of reasoning:

1. That of Kuhn 1970: 62, where it is claimed that paradigms are rejected because of apparent anomalous outcomes of regular scientific endeavour. The accounts of Neustupný 1974 and Clark 1966 appear to substantiate this theory of endogenic change for the present archaeological case.

2. A more restricted one, bearing on ethnology, is found in Harris 1968: 638 (also: Adams 1977: 272; Klejn 1977: 7). The failure of idealism to eradicate other philosophical systems (probably itself to be understood as a consequence of the finally less uneven balance of power) led to a shift in focus of the general Western "cultural matrix" (Romein 1938: 121) or "episteme" (Foucault 1966: 21). Presumably this exogenic pressure had its consequences in archaeological thought as well.

Note also, that the accounts of the archaeologists referred to, are as subject as any participant's account to the "emic"/"etic" mechanism discussed below.

[7] A full exposé on the "New" archaeology's innovations would include some more levels of analysis. Lacking methodological training, I can but mention what I suppose to be their content; parenthesized are my assumptions on the old way of theorizing on digging:

– *epistemological:* the conviction seems to be common that cultural process is governed by laws which are knowable (Cultures are historically unique and only partially reconstructable).

– *philosophical:* Binford and his disciples profess to be materialists, the English appear to be more of the empiricist faith (With numerous individual exceptions, the general trend seems to have been idealistic, or empiricist).

– *theoretical:* culture is the adaptive system of a population, adaption being the American emphasis, and systemic the British one in theoretical accounts; in practice, both seem to converge (archaeological culture is the sum total of excavatable items; very often referred to the then ecology).

– *methodological:* scientific knowledge is a body of laws/hypotheses tested or to be tested through matching their implications with field data (scientific knowledge is arrived at through generalizing/inferring from field data).

– *descriptive:* descriptions are focused on the relations between items excavated: sites, artifacts, ecology as parts of one system (artifacts are witness to a culture's idiosyncratic selection of and reaction to various influences operating in its historical context).

CHAPTER III

CONTINUITY AND DISCONTINUITY

In this chapter the problem of discontinuity or change in the development of pottery decoration of the Bandkeramik culture at Hienheim is considered.

1. *Introduction*

Of the decorated ware excavated at Hienheim a substantial part has to be assigned to the Early and to the Middle Neolithic, viz. Linear Bandkeramik ("LBK" below) and Bavarian Rössen ("BR"). Present in the same site, but rather different in appearance, they automatically raise the question of a mutual relationship between them.

Regarding this problem, archaeologists working in the general area and period tend to support one of two positions:

1. BR derives from a developed phase of the Stroke Ware Culture ("SBK"), more specifically from that of the Plzen Basin in Bohemia. This view is a result of two observations: similarity of BR in Bavaria and SBK IV in that area, and absence of a transformational phase LBK/SBK in Bavaria (Zápotocká 1970: 29; Mauser-Goller 1969: 43).

2. BR is the local Bavarian transformation of the local Bavarian LBK. This idea is put down as an analogon to similar local developments in South west Germany (LBK-Hinkelstein-Grossgartach) and in Bohemia (LBK-šárka-SBK), and based on the scarcity of both SBK and Grossgartach pottery in Bavaria (Meier-Arendt 1975: 134).

After at least three quarters of a century of intensive and extended research, a definition of the LBK ware seems hardly necessary; if so, reference may be made to Meier-Arendt 1966, or Butschkow 1935, or to any general introduction to European prehistory. Less known is the BR style of decoration, because of its restricted geographical dispersion and because no large-scale excavations have been reported as yet. What is known about it has been compiled recently by Meier-Arendt (1975: 134-135); definitions and excellent illustrations are offered in Zápotocká (1970: 28-29; Pl. 8).

A simple description of the supposed development of BR should be sufficient here; a more elaborated definition can be found below (p. 71-72). In the BR style is it generally thought that three "types" or "phases" can be distinguished, although they are lumped by at least one author (Maier 1964: 32-34). The oldest phase is equated with the *Munzinger type* (Dehn and Sangmeister 1954: 21), contemporaneous with the Bohemian SBK III and IV phases; the second phase is labeled *Unterisling*, and the third one *Oberlauterbach*. The latter occurs probably at the same time as SBK V on the other side of the Bavarian Forest. Opinions about the relations between the types or phases differ. For instance, Meier-Arendt considers the Munzinger type a regular SBK ware, attributable to a half-hearted colonization of Bavaria from Bohemia. Unterisling, on the contrary, he says should be the direct descendant of, and successor to the local late LBK; subsequently, Oberlauterbacher ware was supposedly developed from it (Meier-Arendt 1975: 134).

A quite different view is taken by Zápotocká, who, although she acknowledges the strong SBK

III affinities of the Munzinger type, also notes some differences between the two. This type is thought by her to have been developed in the Plzener Basin, whereas the SBK III belongs to Bohemia proper. According to her, after the introduction of the former into Bavaria (through migratory movements?), the local Middle Neolithic sequence sprang from it. This sequence, customarily called BR, is said to have no direct ties with the South German LBK, which had presumably died out before (Zápotocká 1970: 29).

Although I wish to avoid the more or less implicit sociological and demographical suggestions of the above theories, it is still possible to derive a general proposition about the local evolution of pottery decoration in the Early and Middle Neolithic: either there is an autochthonous, continuous development of LBK pottery decoration to BR, or there is a local (Bavarian) discontinuity between the two.

As no controlled excavations of sites where both LBK and BR occur have been reported yet, the Hienheim material might offer a possibility to decide between the two theories.

2. *Further considerations*

Before attacking the research problem, I first want to clarify and, if possible, to define the concepts of continuity and discontinuity. The deduction of operational hypotheses should then allow a choice between the two options on the basis of the excavated data.

Terms acquire their full meaning only in relation to their opposites (Lévi-Strauss 1962: 31; also cf. Wittgenstein 1922: 5.555) so a description of the field within which both concepts are situated is necessary. Continuity and discontinuity, by some considered the polar ends of a continuum (e.g., Lüning 1975), by others opposites (e.g., Van der Waals 1975), are statements about possible relationships within an area of research – about a gradual or a disrupted development in a stipulated field; they say nothing about states of affairs *outside* that field.

In chapter II the notion of *mix* was developed to refer to the percentages of the various traits of a variable in some find when, for example, at some find, or even at some point of time 30% of the counts for the variable "structure" are curvilinear traits, (and, consequently, the remaining 70% rectilinear) then it will be said that the mix is 30 *vs.* 70.

In the present context the temporal extension of the field of analysis is of consequence, as it is the axis along which continuity and discontinuity are to be assessed (spatial extension may also be considered when dealing with continuity; here, only the chronological aspect is relevant). The simplest field of analysis consists of one single variable (x) with but two traits, p and q. If at some point of time t′ only trait p is found (read: the mix is 100 *vs.* 0),and at another point of time t″ only trait q is observed (the mix is 0 *vs.* 100), then it may be asked whether between t′ and t″ a continuous or a disrupted development has occurred, whether the evolution of the traits p and q of variable (x) has been a gradual replacement or a sudden changeover. In the previous example, curvilinear structures would have been replaced entirely by rectilinear ones, leaving us with the problem of whether this change has been abrupt or gradual.

Referring to Fig. 8 it may be stated that as long as the new trait q (rectilinearity, to remain with the example) is introduced earlier (at t[j]) than the latest occurrence of the old trait p (curvilinearity; at t [i]) there have been mixes in which both traits were jointly present (or, both curvi- and rectilinear structures were to be observed), and therefore the replacement of p by q has not been disjunct – in which case we speak of *continuity*. If, on the other hand, t[j] and t[i] coincide, or if t[j] is later than t[i], then the succession is disjunct, and we speak of *discontinuity*.

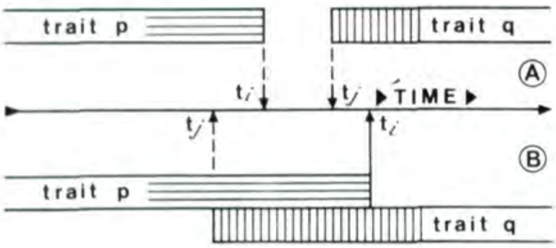


Fig. 8. Continuity and discontinuity on a two-trait variable.
A discontinuity: $(t_j - t_i) \geqslant 0$.
B continuity: $(t_j - t_i) < 0$
t_i: latest appearance of trait p.
t_j: earliest appearance of trait q.
$(t_j - t_i)$: adoptive period.

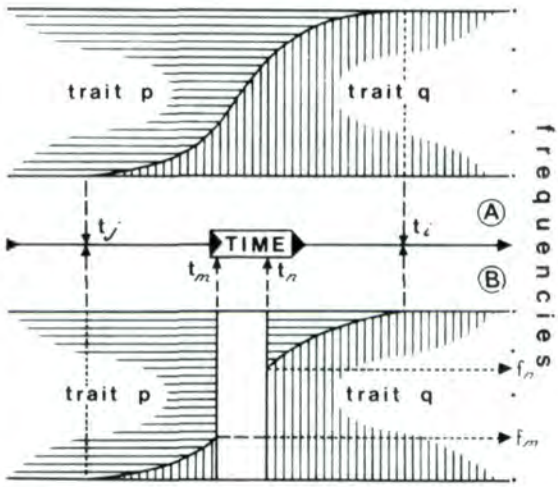


Fig. 9. Continuity and discontinuity on a two-trait variable, quantified.
A continuity or: for all t: $\frac{df}{dt} = \varphi_p\,(100\text{-}p)$.
B discontinuity or: for $t_m < t < t_n$: $\frac{df}{dt}$ indeterminate.
t_j: latest appearance of trait p.
t_j: earliest appearance of trait q.
$(t_j - t_i)$: adoptive period.

The time lapse between the introduction of a new trait and the definite disappearance of its predecessor is called the *adoptive period* of the new trait. Expressed schematically, t[j] – t[i] (the adoptive period) is positive in the case of continuity, and zero or less in the case of discontinuity. Amplifying the continuous case of Fig. 8 to its quantitative form, a frequency distribution over time like the one in Fig. 9 (*i.e.*, an S-shaped curve) will probably describe the succession of the mixes faithfully (Rogers 1962: 109; Kuenen 1967: 53, 61). Such a curve is, of course, a transformation of the familiar double lenticular or "battleship" distributions (e.g., Clarke 1970: 424; and for the theoretical model Clarke 1968: 172).

From this same scheme it can be seen that the concept of continuity, as used here, refers to a situation in which old and new traits coexist; the change in the mix is gradual. Similarly, discontinuity refers to configurations in wich leaps in the mixes are to be observed; in mathematics one would say that the function describing the change in the mix has discontinuities (Fig. 9).

Expanding the field of analysis to incorporate more two-trait variables, the situation becomes as shown in Fig. 10. (The case of two variables yields similar results; with three variables, however, the picture is clearer). Discontinuity remains as above; continuity, on the contrary, shows two distinctive forms:

– *pseudo-continuity* (Fig. 10): all changes occur simultaneously and the length of the adoptive period is equal on all variables ("overlap", in Lüning 1975). One might imagine a general introduction of a new style coupled to a repression of the old one, such as would perhaps follow upon economical or social upheaval. Of course, this is a limiting case of:

– *continuity* proper (Fig. 10): innovations appear and old traits disappear at different points of time, and the lengths of the respective adoptive periods differ also. In this case, a regular development or evolution within the field of analysis seems to have taken place. Introduction of multi-trait variables does not complicate the general picture. Therefore, the following conclusions can be deduced from this model:

If, within a field of analysis, a number of different variables are expressed by different traits at differ-

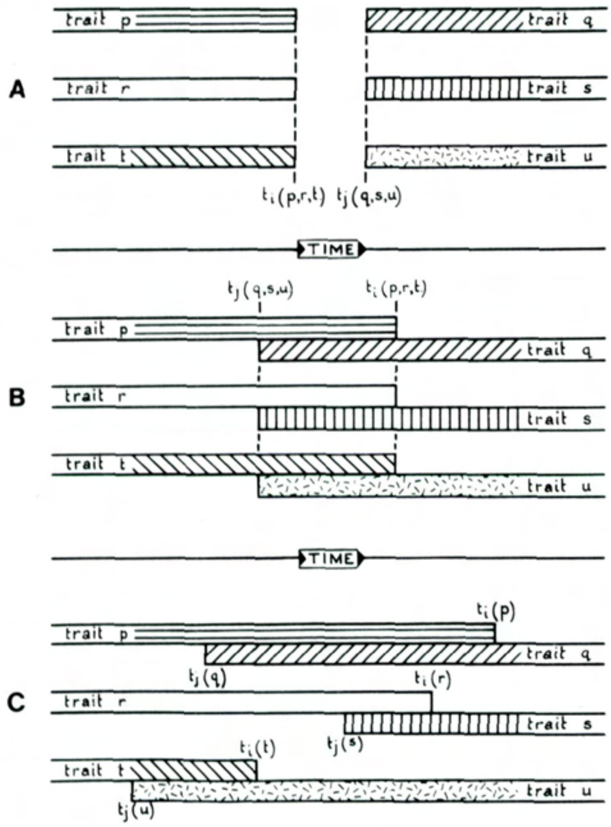


Fig. 10. Continuity and discontinuity on three two-trait variables, qualitative representation.

A discontinuity t_i (p, r, t) $\leq t_j$ (q, s, u)

B pseudo continuity: $\{t_i(p) = t_i(r) = t_i(t)\} > \{t_j(q) = t_j(s) = t_j(u)\}$

C Continuity: $t_i(p) \neq t_i(r) \neq t_i(t)$; $t_j(q) \neq t_j(s) \neq t_j(u)$; $(t_j - t_i) < 0$.

t_i: latest appearance of a trait.

t_j: earliest appearance of a trait.

$(t_j - t_i)$: adoptive period

ent points of time, the intervening change is either

– *continuous*, if the introduction of new and the disappearance of old traits occur at different points of time so that the adoptive periods differ with each variable; or it is:

– *pseudo-continuous*, if the introduction of all new traits occurs at one point of time and the disappearance of all old ones at another point of time so that the lengths of the adoptive periods are equal for all variables; or it is:

– *discontinuous*, if the old traits had disappeared before new ones were introduced to replace them; more generally, if replacement occurred in leaps for a number of variables at a time.

Conceivably, a number of innovations might happen together, even in the case of true continuity. As the variables are assumed independent, this would be a very rare phenomenon; the probability that all subsequent developments would then occur at the same speed is negligible, however.

A comparison of quantities of the adoptive process on the different variables is fairly easy when instead of the verbal notions above, the equation of the logistic curve of Figs. 9 and 11 is introduced: the parameters of that graph are the characteristics of the adoption of the new trait.[1]

It is not too difficult to translate the above model into observations[2] (or "operationalize" the implications). In it, the field of analysis has its empirical referent in the set of closed finds of decorated sherds belonging to the Bandkeramik tradition excavated at Hienheim. Likewise, the characteristics of that decoration are equivalent to the traits of the model. Two or more of the alternative characteristics may be grouped to form a variable, as mentioned above. These variables taken together constitute the field of analysis, which is formally also a classificatory scheme, as already indicated in Ch. I (also cf. Van de Velde 1976); the material expression of this field of analysis is the Bandkeramik tradition of pottery decoration.

Above, the first model of a two-trait variable (Figs. 8 and 9) has already been cited. To resume, at some early point of time the STRUCTURE of the decoration ("variable (x)") was assumed to be entirely *curvilinear* ("trait p", in the model); at a later point, only *rectilinearity* was to be found ("trait q"). It was asked then, what had happened to the variable STRUCTURE in the meantime.

On a more complicated level, the abstract model of continuity and discontinuity can be summarized as follows: if, on sherds excavated at Hienheim, the LBK style is represented by a number of charac-

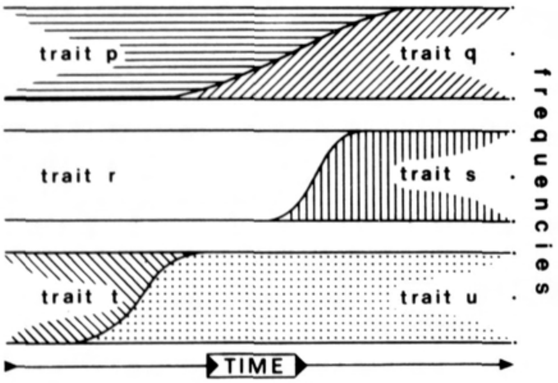


Fig. 11. Continuity on three two-trait variables, quantified. $(t_j\text{-}t_i)$, the adoptive periods, differ per variable.
$t_i(p) \# t_i(r) \# t_i(t)$;
$t_j(q) \# t_j(s) \# t_j(u)$.

teristics, and the BR by other (though comparable) characteristics, the intervening change is attributable to:

– *a continuous development*, if the introduction of new and the disappearance of old traits of pottery decoration all occur at different points of time;

– *a pseudo-continuous development*, if a synchronous appearance of new traits, equal length of adoption periods on all variables, and a simultaneous disappearance of old traits can be detected;

– *a discontinuous evolution*, if the old traits have disappeared before new ones were introduced, or when there were large, simultaneous changes in the counts of the various traits.

Formally speaking, these statements refer to the excavated material only: "time" is but a label to refer to an analytical dimension of the decoration on the sherds, nothing more.

To conclude: the observation of sherds will bear on past habits of pottery decorating only as far as decay between deposition and analysis has been aselective, and as far as the deposited waste is a representative sample of the decorated pottery at the time of deposition – assuming the validity of the model and the reliability of the analysis.

3. *Method*

Before even considering the problem of decorative continuity/discontinuity at the transition from Early to Middle Neolithic at Hienheim, a number of secondary problems must be solved. A very trivial one is that of the apparently different types of pits which the decorated sherds are recovered: there are substantial differences in their positions in relation to the living areas, in their forms, in their numbers in relation to the other immobile objects, and perhaps in their function as well when pits form both periods are compared. The pits have probably been used for different purposes which may have influenced the composition of the waste ultimately filling them (if only the changes reflect an evolving socio-economic structure); however, the effects of this on the present research question are bound to be nihil: I am not asking for the causes of changing habits, but rather how the decoration changed, in a descriptive sense. Related, but in my opinion much more relevant, is the question of whether the quantities of decorated sherds are large enough to allow statistical comparison of both periods. They do: 4029 sherds from 123 Early Neolithic pits, and 828 sherds from 41 BR/Middle Neolithic pits should suffice.

Another secondary problem is the apparent incongruity of discrete data and the continuity of time. In the first place it can be assumed (as customary in archaeological practice) that the contents of closed finds are approximately representative of the population from which they were selected (i.e., the set of mixes current at the time of deposition). Actually, this assumption is a double one: waste and deposition are thought to be representative of the then-current population, and the subsequent decay (including the effects of the research processes) is postulated to have been non-selective. Although the separate or joint effects are untestable, it should be admitted that closed finds are the best attainable approximations of earlier states of affairs, especially when numbers of them are considered together. Therefore, the decoration on the sherds has been analysed and registered by such units of observation, of which more than 300 were entered into the computations.

Secondly, if these pits were dug over a period of 400 years, their use may have been interrupted on a regular basis. Fortunately, the research tradition allows LBK "refuse dumps" to have been in use for quite a long period, at least ten to twenty-five years. If the accumulation of debris in the pits is extended over such a period, then any number between seven and eighteen pits should have been open at any point of time (in the period under discussion, that is), and so the various samples will considerably overlap. If the period of their use has been less than the estimated 400 years (due to either an overestimate or a discontinuity), this overlap is even larger. Yet it should be conceded that it is impossible to rule out discreteness completely.

The last secondary problem to be looked into here is that of the independence of the variables, as required by the model of continuity and discontinuity. When in the first chapter the classificatory scheme was developed, all variables were *defined* independently, each representing a single separate dimension of pottery decoration. This *logical* independence is matched by *empirical* independence of the variables as indicated by the correlation matrix in Table 75. There, it will be observed, some *traits* do correlate highly; however, not a single pair of *variables* shows consistently high coefficients of correlation of their traits. When also the relatively large number of observations from which the coefficients were computed is taken into account, then at least for practical purposes independence of the variables can be assumed.

After settling these points, we can now turn our attention to this chapter's problem: whether or not the Hienheim Bandkeramik tradition of pottery decoration shows a continuous development from the LBK to the BR style. The major difficulty is the arrangement of the finds over time, the essential dimension of the research question. A reliable attachment of the data to this axis is a necessary condition for the applicability of the model developed above (cf. Adams 1977: 274 for some pointed remarks on this topic); indeed, the analysis of the social structure in Chapter V would be impossible without it.

I will consider a number of different solutions to this problem in turn:

– *Stratigraphy*, the oldest method. Although some pits have been dug into others at Hienheim, the rarity of this phenomenon (only one relevant case has been observed) precludes any extended use as required here. Yet, as a control of the final ordering, this instance may prove useful.

– *Direct dating methods*, thermoluminescence and radiocarbon measurements. Again, the rarity of dated pits, in relation to the total body of data (three strongly, and two weakly associated C-14 readings, and only one single TL dating), together with the rather wide confidence estimated (some 50 to 100 years for the C-14 dates, and ca. 600 years for the TL date), render these methods inapplicable here. They too, however, are to be used as a control of the final ordering.

– *Seriation*, or more general, combinative statistics. Because it does not separate chronological from socio-economical factors, seriation has been severly criticized (Mauser-Goller 1969: 20; Lüning 1969: 5) and rejected – rightly, of course. Without such a possibility, the condensation of multidimensional variation into one single dimension seems to be fairly naive (Audouze 1974) as the influence of the various factors is entirely beyond control (cf. Graham et al. 1976 for a rather heuristical solution of this problem). Therefore, the interpretation that the one resulting dimension should be a chronological one is arbitrary.

However, "Kombinations Statistik" (or multivariate analysis, "MVA" below) has been in use as long as axes or pots have been compared, since similarity (almost) always refers to more than one dimension. Unfortunately this has only rarely been realized by archaeologists (until recently), so that the formal tests of similarity and dissimilarity developed for this purpose have largely remained outside "Mainstream Archaeology" (Doran and Hodson 1975: 3).

Several MVA methods have been explicitly designed to abstract meaningful dimensions from the data (for a non-technical survey of a number of relevant MVA methods, with their critiques, see Doran and Hodson 1975; more technical, though still directly bearing on archaeology, is the Hodson, Kendall and Tautu 1971 volume). Statistical methods, whether implicit or explicit, complex or simple, are in and by themselves completely neutral, as long as they are competently applied. Consequently, criticism should not be directed against the method itself, but against the validity of the applications or the reliability of the results; in non-technical terms, against the relevance and the appropriateness in view of both the research problem and the nature of the data. And these problems belong to the pre-punchcard and post-output stages of research. A competent application of a seriating algorithm (even one, yielding stable results; Goldmann 1974; Le Blanc 1975; Wilkinson 1974: 16) should be criticized as being partly invalid, because of the implicit bypassing of ana-chronological dimensions.

The following is intended to facilitate evaluation of validity and reliability of model, methods, and results. The field of analysis within which an answer to the research question of continuity and discontinuity is sought is defined by the variables that are used to classify the data (cf. Ch. I). If the traits entered into the analysis are mutually exclusive, then the model prescribes a behaviour of the mixes as in Fig. 9. If it can be demonstrated that they behave accordingly, the model seems to be valid, at least for its single variable part. Also, the applicability of the computational method used to produce these results seems to be substantiated. The validity of the multivariable model (i.e., as in Fig. 10) cannot be gauged in this way; whatever the results, these may as well reflect a computational (or methodical) artifact as what "really" has been the case. There is no way to decide between the two possibilities on the basis of one single dataset. Therefore, next to those for Hienheim, I will also present the results of a parallel analysis of the decorated pottery from the Bandkeramik settlement of Elsloo (in the southern Netherlands; the data have been published in Modderman 1970). If both outcomes are interpretable by means of the models, chances are reduced to 1:4 that they are bogies – and the probability of the model's validity proportionately enlarged.

An (internal) test of reliability is possible by partitioning the variables into two subsets, performing the analyses on one of the subsets, and then seeing, whether the results make sense for the second subset as well, the so-called split-half method (Selltiz et al. 1966: 174-179). Translated to the present analysis the curves describing the behaviour of the mixes in the second set of variables should be reasonably related to that of Fig. 9 (given such a behaviour of the variables in the first set). Further tests can be found in stratigraphic observations, radiocarbon measurements and TL readings, and in the production of a similar temporal ordering by means of another method of computation.

Turning to the possible methods themselves, we first have to choose between Q- and R-type analysis (not discussed in Doran and Hodson 1975; for an introduction, cf. the references below). In the former, the computational basis is the comparison of *rows* (i.e., pits, in the present context); in the latter, that of *columns* (here, the traits). As the models are about the behaviour of the traits on their variables, rather than the grouping of the cases, an R-procedure would be appropriate for the computation of the matrix of correlations; this is the starting point for many MVA methods (Sokal and Sneath 1963: 207-209). A more practical reason is that data are collected per case and cards are punched per case; machine transposition of the slightly outsized data-matrix (some 30,000 cells) is possible, though a rather costly affair. Finally, the end results of both Q- and R-analyses should be broadly similar anyhow (Clarke 1968: 533).

A second choice to be made is between ordering or sequencing techniques (e.g., seriation, multidimensional scaling, factoring, and principal components analyses) and grouping or clustering ones (e.g., discriminant analysis, cluster analyses). The former group aims at the study of the interrelations of the units of analysis, the latter at the grouping of the units into a limited number of sets. As chronological ordering is necessary to solve the research problem, a sequencing method seems appropriate (Lischka 1975). Multidimensional scaling and principal components analysis should both provide the required ordering (Romney et al. 1972); the latter method is the more convenient one (Hodson 1969a, b; Doran and Hodson 1975: 191), because it is available in SPSS (Nie et al. 1975: 470) and thus easy to implement. (For details, refer to Doran and Hodson 1975: 190-197 (non-technical) or Harman 1967: 136-137; Van de Geer 1967 (technical) with their references).

Regarding the present analysis, a number of details should still be considered. The correlation-matrix which was the starting point of the analysis is presented in Table 75. The R-mode used in the computation of the matrix leads to a sequencing of the finds through a combination of the values observed for the various *traits*. To improve the compatibility of the *variables*, the raw counts of the traits were converted to percentages before the correlations were computed (Doran and Hodson 1975: 194) – in this way the larger finds count as heavily as the smaller ones.

Above (p. 47), it was stated that one of the possible controls on reliability consists in applying the "split half" method. If the sequence produced by the principal components analysis is a reliable one, and if it is based on part of the variables only, the change shown by the other variables should be similar to the model of Fig. 9, not only on the variables used to compute the time scale. Apparently it is irrelevant which of the variables are selected for the computations. Therefore, only those variables were selected that are best related to chronology, and from among these, those that are easy to observe in order to minimize computational noise.

4. *Interpreting and interpretative computing*

In the last section, principal components analysis was selected to sequence the data. The applicability of this method to the present research question apparently hinges upon the possibility of computing and then correctly identifying a principal component ("PC", below) related to chronology from the variation shown by the decorated sherds.

The most subjective part of PCA is the interpretation of the PC's; at the same time it is most crucial, as the validity of the outcome is entirely dependent upon it. Before proceeding to this interpretation I shall first offer some non-technical descriptions of parts of the mathematical model involved, in order to enable evaluation.

There are as many PC's as there are variables, according to the model. Yet, only a few of them are meaningful, so a major step in PCA is fixing the number of PC's with which to proceed. PC's are put out by the computer in descending order of importance, the first one combining as many variables as possible from the entire field of analysis; the second one, from the remainder; and so on. Technically, their importance is expressed as "the amount of variance explained" (with the totality of the variance defined as 100%), and several rules of thumb exist with which to draw the limit between "meaningful" and "noise"; however, no formal criteria exist. Crudest from a mathematical point of view, though intuitively perhaps best defendable is the limit of 5% of the variance. Another possible criterion is based on the relative differences between subsequent PC's, often graphically represented by a curve (Table 76):

where the curve's slope is steepest, the difference is largest (in Table 76 between PC's 1 and 2, 3 and 4, and 7 and 8). Both criteria together suggest (in this case) a cutting off after the third PC, retaining (or "explaining") 40.9% of the variation contained in the correlation matrix from which the PC's are deduced.

Informally, PC's are defined as the best possible linear combinations of a number of variables; indeed, PC's are best visualized as each summing a set of variables. One of the tables put out by the computer gives the correlations of the newly defined PC's with the old observed variables (cf. Table 77). High "loadings" are equivalent to high correlations between them; it is these high loadings that are used to establish the "meaning" of a PC. To give an example: on PC 2 there are three variables that load moderately high (MAIN MOTIFS, and two of the FILLINGS variables) with all other variables showing very low coefficients. Apparently, this PC has something to do with the way in which the motifs on the pots have been executed.

The first PC is of an entirely different nature: there are high, moderate and low correlation coefficients; it is obviously general in character, reflecting some general source of variation. The third PC is like the second one, of the so-called "bipolar" type (Harman 1967: 100).

A final remark about the mathematics involved: it is possible (and routinely done so by standard packages of statistical procedures) to compute the values, or coordinates, of the cases on the PC's, so-called "factor scores". These factor scores are a kind of translation of the old observed values to the new PC's. Their most important property is that cases with high scores on a PC have many of the characteristics compounded by that PC. (For technicalities, the reader is referred to Harman 1967: 153-155; more archaeologically minded are the accounts of Clarke 1968: 563 and of Doran and Hodson 1975: 190-197; less formal, and still more archaeological, is Binford 1972: 271-273).

With this in mind, the identification of a PC having to do with time is fairly easy. Time affects probably all characteristics, so the first PC, with its general nature, is the most likely candidate. In fact, from Table 77 it will be observed that on this PC polar positions are occupied by *uni- vs. multidented spatula*, by *lines* and *points vs. stab-and-drag* COMPONENTS (and, to a lesser extent, by *hatching*), and by *curvi- vs. rectilinearity*. Also, at the same pole *simple spatula*, *lines* and *points*, and *curvilinearity* occur together, and at the opposite pole their alternatives. From what is known about the South German Early and Middle Neolithic pottery decoration, it is evident that this first PC is very much related to the passing of time – Early Neolithic corresponding with negative values, and Middle Neolithic with positive loadings.

PC's being defined mathematically independent of one another, the first conclusion to be drawn is that the traits hardly loading on the first PC (and possibly highly so on other PC's) are apparently chronologically indifferent. A second conclusion is that we need not bother about the other PC's at this moment; they may be related to the social structure.

The next step is to reduce the number of variables in the analysis to allow control of the reliability (cf. pp. 47, 48). If we retain only those variables that show significant loadings on the chronological PC, and if we then select among them those that are best observed, then the result is the following set: TECHNIQUES, COMPONENTS of decoration of belly area, and STRUCTURES, together eleven traits. Repetition of the analysis along the same lines as above (i.e., starting with the correlations of the eleven selected traits) results in a first PC accounting for 54.1% of the summed variation on the eleven traits in the analysis. The loadings are depicted in Fig. 12. The factor score coefficients produced in this way are used to compute the sequence of the various finds on the first PC (i.e., the factor scores), which should be their chronological ordering.[3]

Once this sequence has been obtained, a mere

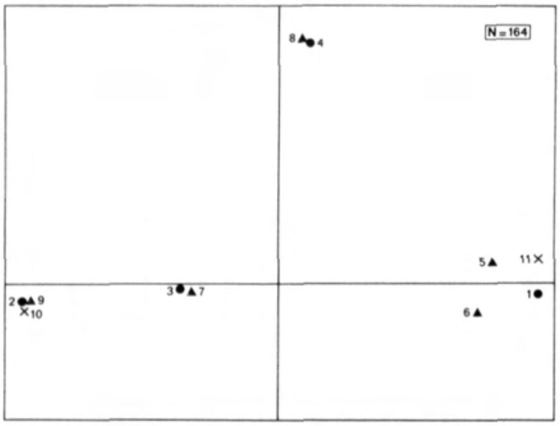


Fig. 12. Plot of the "loadings" of 11 traits on the first two (QUARTIMAX rotated) principal components. Horizontally: first principal component; vertically: second principal component.
● TECHNIQUES: single dented spatula; 2: multiple dented; 3: 'goat foot tool', 4: fingertips and nails.
▲ ELEMENTS: 5: lines; 6: points; 7: hatchings; 8: finger or nail impressions; 9: stab-and-drag.
× STRUCTURES: 10: rectilinearity; 11: curvilinearity.

listing of the proportions of the traits in their mixes should allow the demonstration of continuity or discontinuities in the data along lines of the model in the second section. This cannot be done right away, as two new problems appear: how to distribute the individual finds over the time axis, and how to cope with unsystematic variation.

Discussion of the problem of unsystematic variation will be deferred to the end of this section; distribution of the finds on the time axis, the first problem, arises from the simple fact that like intervals on the PC need not correspond to like chronological intervals. More specifically, differences in factor scores are measures of relative dissimilarity; the grade of this dissimilarity is unspecified, however. Thus it is possible to say that find x is earlier than find y on the basis of their respective scores on PC 1, but not how much earlier: we do not know whether the evolution of (or rather, the quantified change in) the pottery decoration went at a constant rate.

The first PC is conceptually a monotonous transform of (a part of) the chronological continuum.[4] In other words, sequencing of the finds according to their factor scores is indicative of the order in which they were deposited. Except when the factor scores are identical, nothing can be said about the number of pits in use at any single moment, however. This boils down to the problem of finding some more or less likely distribution of the finds on a time axis *that does not violate the ordering indicated by the first principal component.*

Two such possible distributions immediately come to mind: an even one and a normal one. If the chronological axis is arbitrarily cut up into 20 "phases", in the case of an even distribution, 5% of the finds is attributed to every phase. This will be called "Model 1" below.

In the case of the normal distribution ("Model 2" below), the finds are assembled into phases to produce a Gaussian (bell-shaped) frequency curve over time. Note that in either case the original ordering of the finds on PC 1 is not violated.[5] Note also that Model 2 is valid only in the case of continuity of the original depository process, which is conjectural. Model 1, which gives an even spreading out of the data, will be more suitable to discover discontinuities; in between such ruptures Model 2 should perhaps be applied.

We now return to the problems at hand. After spreading the finds over time according to the models, an estimate of the original population (of decorated pottery) is obtained by averaging the counts of the traits per phase. Individual estimates may diverge considerably from the trend, however. A "smoothing" procedure should produce a better approximation of the original state of affairs: jumps in frequencies are thought to be exceptions (Berger 1973: 37). Smoothing should, on the one hand, minimize the influence of unsystematic wandering (i.e., departures from the general trend that are restricted to one single phase). On the other hand, systematic deviation (assumed to be in the same direction for at least two phases) should not be obscured. Weighting the "raw" estimate p(t) with the adjacent ones[6] according to:

$$\text{smoothed estimate } \tilde{p}(t) = (p(t-1) + 2p(t) + p(t+1))/4$$

results in an improved estimate of the original population, the development of which should be checked against the model of continuity.

5. *Presentation of the results*

On the assumption of a constant use-to-waste ratio, the models in the previous section will be reworded to possibly better and certainly less naive approximations of earlier states of affairs. The number of sherds is perhaps a better base to work from than the number of pits, especially with the aspect of distribution over time in mind. Therefore, the analysis has been carried through the following steps:

1. The set of finds containing at least five sherds (164 pits, to a total of 4853 decorated sherds) was arranged on the basis of their scores on PC 1; then followed either step 2a to MODEL I or step 2b to MODEL II.

2a. If a sherd total of 4853 sherds is to be distributed evenly over 20 phases, then each should contain 4853/20 = 242.65 sherds. Now, if it can be stated that closed finds are samples (cf. above, p. 28) there seems to be no reasonable way to split them up without raising hosts of questions; therefore, finds were allocated as entities when the sherds were distributed over the respective phases. As a consequence, for each phase the number of sherds only approaches the required 5%. The resultant more or less evenly spread-out data set will be called MODEL I (cf. Table 2).

2b. If a sherd total of 4853 sherds is to be distributed normally over 20 phases, an estimate of the size of the "tails" of the distribution should be made. Quite arbitrarily, I postulated the extremes to contain together 5% of the totality of the sherds. Then the distribution of the remaining 95% over the 18 phases in-between can be looked up in any table of normal frequency distributions. The conversion of the table's frequencies into class boundaries and the subsequent allocation of the several finds (again, as entities) to the appropriate classes or phases result in a distribution of the finds which approximates a normal one of the sherds: MODEL II (cf. Table 2).

3. From the counts of the traits in the finds in each phase of the MODELS, averages, standard deviations and 90% confidence intervals for the estimates of the means of the original populations of decorative characteristics were computed (De Jonge and Wielenga 1973: 172-173; Moroney 1951: 238-245).

4. Estimates of means and confidence intervals were plotted for both MODELS in Figs. 13 and 14.

5. Finally, smoothed averages were calculated (cf. above) from the estimated means, and the curves of Fig. 14 drawn along these points.

For a discussion of both MODELS and an interpretation of Figs. 13 and 14 I still have to introduce the following notions: when the evolution of the mixes has to be examined, this is done by comparing the positions of adjacent confidence intervals, the horizontal bars in both figures. Now, if the change from some phase to the previous or the succeeding one is so large that both ranges do not overlap at all, I will call such a shift a "large jump". If there is some overlapping (though less than half of either interval) the change will be called "almost a large jump".

Turning to Fig. 13 – that of the evenly distributed data designed for the location of ruptures in the development – if the evolution were discontinuous, the disjunctions should occur simultaneously; i.e., for every variable in the same phase shift. Then leaving aside the ambiguous sections of the graphs (where the number of finds is too small to compute the confidence intervals) the following large jumps are discernible:

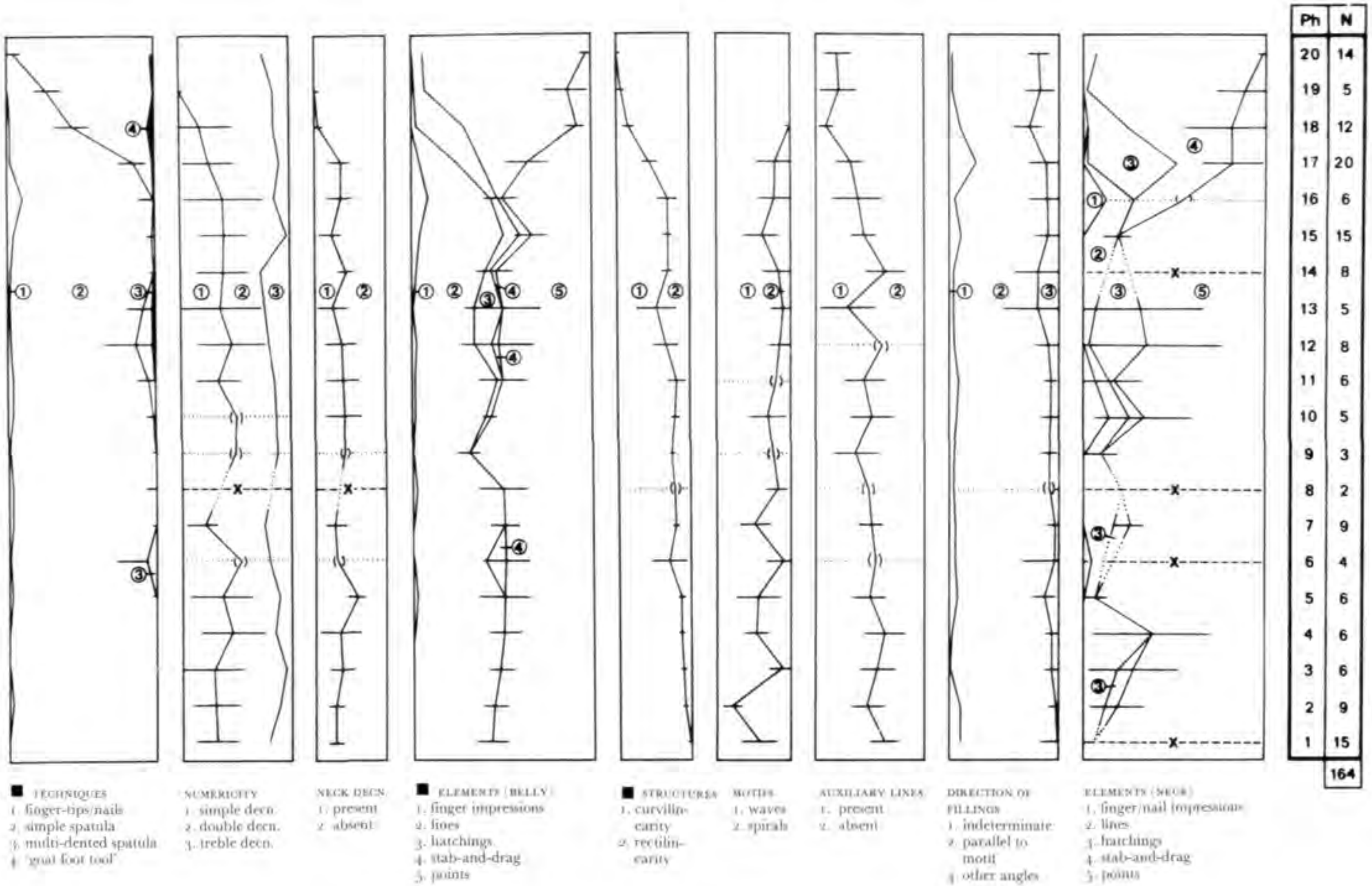


Fig. 13. HIENHEIM: Proportions of various attributes per variable of decoration over time. Phases comprise approximately equal numbers of sherds (MODEL I), ordered chronologically by means of a principal components analysis of the variables marked by ■: '1', the oldest phase, '20' the youngest one.

'N. OF PITS': number of finds in which the sherds were collected.

column width: 100% each.

horizontal bars: 90% confidence intervals for the mean.

–×–: no data; –(.)–: insufficient data.

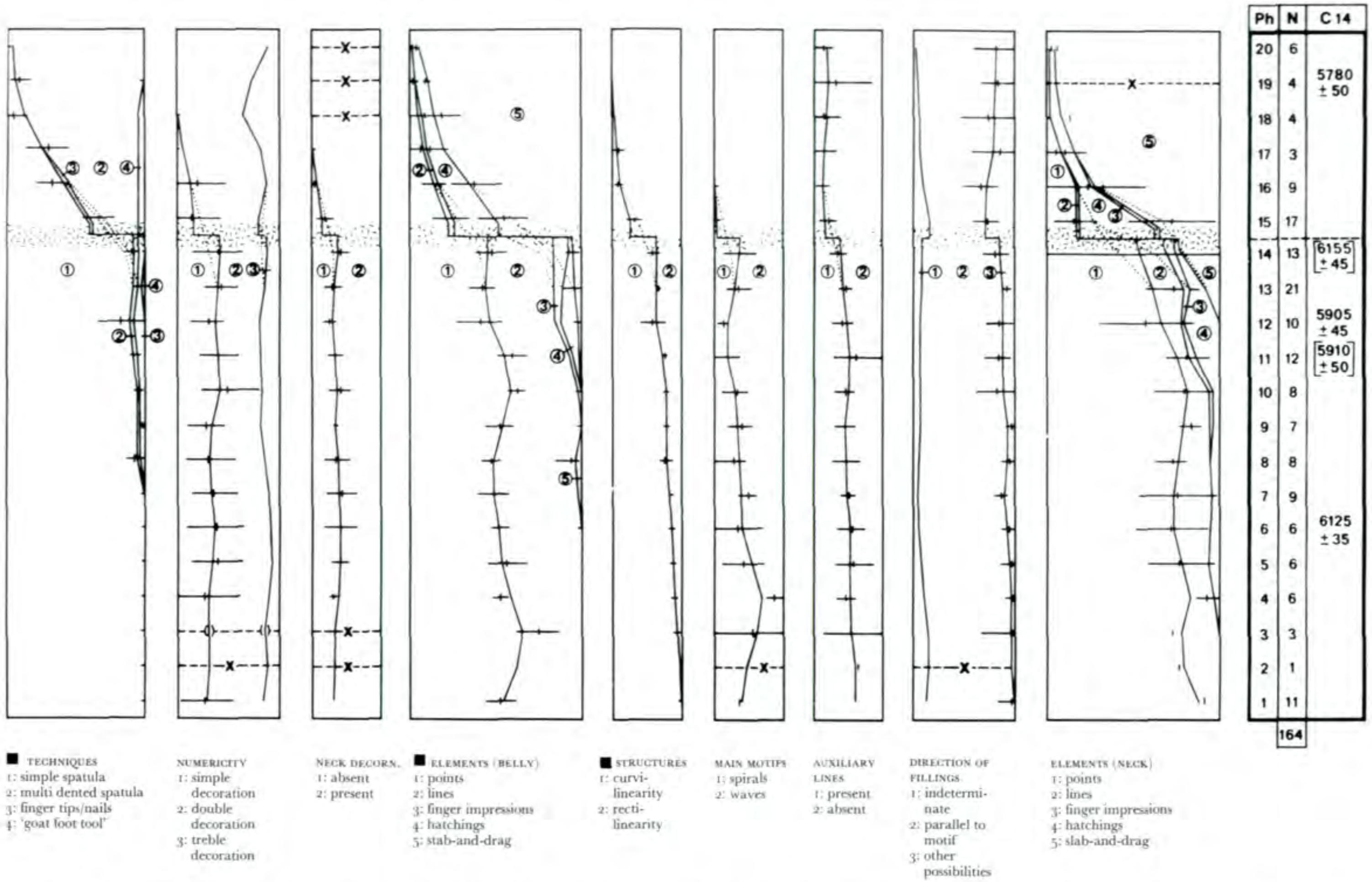


Fig. 14. HIENHEIM: Proportions of atrributes per variable of decoration over time. Phases comprise approximately normally distributed numbers of sherds (MODEL II), ordered chronologically by a principal components analysis of the variables marked by ■ ; '1': the oldest, '20': the youngest phase. Smoothed averages.

PH: Phase number. //////: discontinuity in the development.

N: Number of finds per phase. Column width: 100% each.

C-14: Radiocarbon dates (between parentheses: uncertain association).

Horizontal bars: 90% confidence intervals for the mean.

1. General variables:

TECHNIQUES: between the phases 17-18 and 19-20; almost, 18-19.

NUMERICITY: neither large nor almost large jumps are found.

NECK DECORATION (FORMAT): present between 17-18; in addition, 4-5, 13-14, and 14-15 almost qualify.

2. Variables of the decoration in the belly area:

COMPONENTS: between the phases 8-9, 9-10, 14-15, 15-16, 17-18; almost 16-17, 18-19, 19-20.

STRUCTURES: between 16-17, 17-18 (both very significant because of the narrow confidence intervals).

MAIN MOTIFS: between 1-2, 2-3, 5-6, 6-7; almost, 17-18.

AUXILIARY LINES: present between 17-18; with 12-13, 13-14, 14-15 almost so.

DIRECTION OF FILLINGS: only between 17-18 not too large a jump is found.

3. Variables of rim decoration:

COMPONENTS: uninterpretable because of the large confidence estimates.

When large jumps are noted, two explanations can be invoked:

– in the vicinity of the inflection point of a logistic curve[7] change is faster than anywhere else. This should be considered a regular feature. Therefore, larger confidence intervals can be expected to occur in this vicinity.

– a genuine interruption of the developments at the site, the potters have camped elsewhere for a substantial period. At their return to the old site the change in their repertoire has been large enough to show in the diagrams.

If an interruption would coincide with a period of rapid change (i.e., around the inflection points in our graphs) it is graphically indistinguishable in the case of a *single* variable. When the other variables are also taken into account, however, not only these two cases, but pseudo- and true continuity can be discerned as well (if present). To check for pseudo-continuity, an estimate of the inflection points for the different variables runs:

– for the *general variables*, approx. in the phases 18, 17, 17/18, respectively;

– for those from the *belly area*, approx. in the phases 17/18, 17, (if any:) somewhere in the middle of the scale, 17 (?: perhaps earlier), none, respectively; and in the phases 16/18 for that of the *neck decoration*.

As these points do not coincide, pseudo-continuity may probably be ruled out as far as MODEL I is concerned. The different lengths of the adoptive periods of the traits seem to be further corroborative evidence. To resume, two or more large (or almost large) jumps are found at the interfaces of the phases 16/17 (2 variables), 17/18 (7 variables), 18/19 (2 variables), and 19/20 (2 variables). A number of these coincide with the inflection points of the graphs (such as at the 17-18-19 transitions for TECHNIQUES, or 17-18 for the COMPONENTS of belly decoration). Even when this is taken into account, on both sides of phase 17 there still seems to be something going on: TECHNIQUES, presence of NECK DECORATION (or FORMAT), COMPONENTS (belly), STRUCTURES, MAIN MOTIFS (almost), AUXILIARY LINES, direction of FILLINGS (almost) all show considerable change on either or on both sides of this phase.

With this in mind, we turn to Fig. 14 (MODEL II) and again compare the relative positions of the respective ranges of the confidence estimates (the horizontal bars in Fig. 14).

As a consequence of the altered distribution of the finds over time, several jumps apparent in Fig. 13 have disappeared, some others turned up or received more emphasis. Large or almost large jumps occur at the interfaces of the phases 14-15, 15-16, 17-18 (TECHNIQUES), 14-15 (NUMERICITY), and 14-15 (presence of NECK DECORATION); 3-4, 14-15, 16-17 (COMPONENTS of decoration in belly area), 14-15, 15-16 (STRUCTURES), 4-5, 12-13, 14-15 (MAIN MOTIFS), 14-15 (AUXILIARY LINES), and 6-7, 7-8 (direction of FILLINGS); and 14-15, 15-16

(COMPONENTS of NECK DECORATION).

We next inspect the smoothed graphs (stippled, to indicate their provisional nature) to locate the inflection points (respectively in the phases 15, 15/16, and 15; 15, 14, 6/13, 13/15, and none; and 14/15) and to compare the lengths of the adoptive periods (which are different). It will be apparent that on the one hand only a few large jumps remain when those in the vicinity of the inflection points are substracted, while on the other hand at least eight out of the nine variables here in consideration show substantial differences between phases 14 and 15 – a rather systematic affair. Inflection points are established only *ex post facto;* therefore not too much analytical weight should be given to them. Thus, a discontinuity seems to have been traced here. This 14-15 transition of MODEL II divides the contents of the already suspected phase 17 in MODEL I.

When computing the ultimate, smoothed curves (fully drawn lines in Fig. 14) this disjunction was taken into account: the counts from across the gap were left out in the calculation of the values for phases 14 and 15. A comparison of the final curves with the provisional curves shows that smoothing should be done only *after* interpretation, in order not to obscure potential systematic irregularities.

A listing of the counts that make up the contents of the phases 14 and 15 of MODEL II (Table 3) demonstrates that the discontinuity does not coincide with the interface of the two phases. Rather, the line seems to be located between the finds nos. 0614 and 0823 (ranked 042 and 041, respectively). Following the line of thought which led to the Model 2/MODEL II distribution it seems logical to apply that model to both blocks of data separately (cf. p. 50). After all, in the older half of Fig. 14 virtually no change in the mixes is to be perceived, a rather unlikely state of affairs. So, the data older than the gap (4025 sherds from 123 pits) were redistributed over the time-axis in an approximately normal way (as above), now arbitrarily divided into ten phases. The younger data (828 sherds from 41 pits were given a similar treatment, albeit divided up in six phases. Of course, both distributions respected the original factor score ordering. This doubling of the MODEL II distribution is called MODEL III here; after the calculation of the averages and confidence intervals these were plotted in Fig. 15. (In a general way, this double normal distribution is corroborated by Fig. 24, p. 77, where a simple one-to-one ordering of the data is compared with a linear quantification of change in the data set.) Large jumps do not systematically occur within the blocks so defined, only in between them. And even there, the differences seem to be less than in Fig. 14 at the interface of phases 14 and 15.

6. *Discussion and evaluation*

Generally, models are defined in heuristic terms: if some system X is used to gain insight into another system Y (which is independent of X), the X is said to be a model of Y (Bertels and Nauta 1969: 28). The wording of X will be a set of propositions about elements and relations between them. The elements may be simple data, hypotheses, or laws. A model need not contain laws, however, since the proposed relationships may also be of a self-evident, or of a hypothetical nature. The word "model" in this sense is merely a substitute for "explanation" (Popper 1968: 74-75; also cf. Salmon 1975).

To evaluate such a model, then, is also a heuristic procedure: does the model do what it should do, does it adequately generate and explain a structure in the data, an adequacy in the last instance to be judged by the scientific community (De Groot 1961: 28; Popper 1968: 41-42).

The first model that was introduced should clarify the concepts of continuity and discontinuity and then develop these so that observation would be possible (above section 2). This model rests upon the validity of two propositions: (1) (in accordance with the literature on cultural change:) if one trait

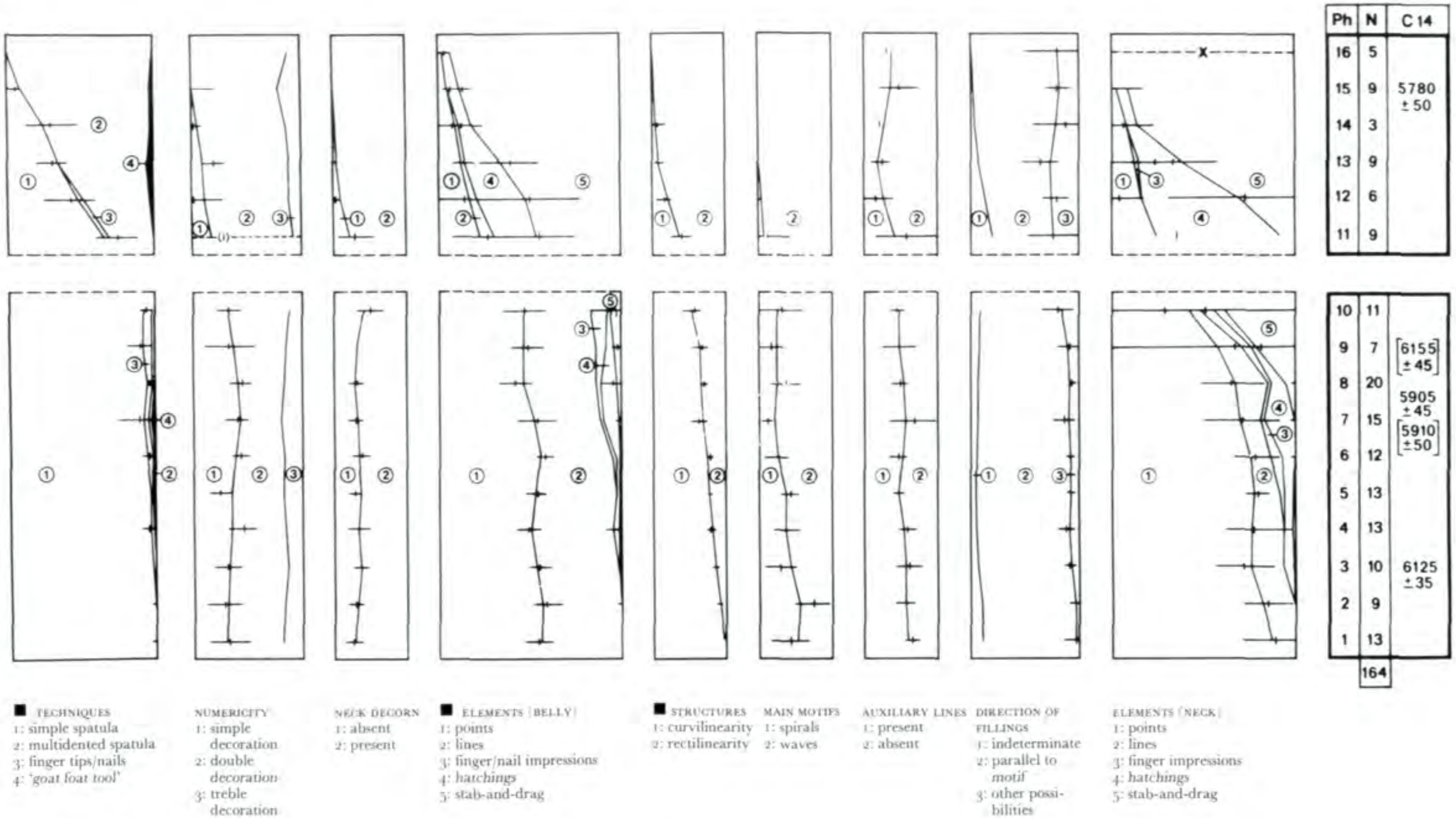


Fig. 15. HIENHEIM: counts of traits per variable per phase, when the number of sherds is distributed normally both before and after the presumed discontinuity. Otherwise, similar to presentation of MODEL II.

drives out another similar one, then a count of the relative frequencies of the two traits over time usually shows a logistic pattern; (2) in the case of more independent variables, the adoption of new traits will start at different points of time and also proceed at unequal speed. As a consequence of the two propositions, a disruption should cause synchronous jumps in the frequency counts of the variables. Formally, the model has generated statements about how to observe continuity and discontinuity, by means of which hypotheses on these subjects can be falsified. Since it has been possible to manipulate the data to conform to the prescribed frequency distributions, and also because an instance of discontinuity could be extracted, the model has at least some heuristic value, if not validity. Its reliability is a matter of further tests, as stated above.

In the fourth section two models for the distribution of the finds over time were proposed. They were slightly amended in the fifth section to sherds counts instead of number of finds. To summarize, MODEL I, while retaining the relative positions of the finds on the time scale, evenly distributes the amount of sherds over this axis. And MODEL II, retaining the relative positions, groups the finds according to a normal frequency curve for the sherd quantities. MODEL III, with its two normal distributions, is merely a logical consequence of the assumptions underlying MODEL II, and does therefore not need to be treated separately here.

The efficacy of MODELS I and II is to be gauged from their respective ability to summarize the data, a measure of which can be found in the respective variances around the means. In Table 4 the averaged standard deviations per phase are presented. Generally, the values for MODEL II are somewhat lower than those for MODEL I; thus, the former seems to be a little more effective (entirely in accordance with Plog 1974: 92). A comparison in terms of the average standard deviations per trait is also slightly in favour of MODEL II: in five out of eight traits this value is less in MODEL II than in MODEL I, and reverse in three traits (Table 5).

Of course this comparison says nothing about the validity of the ordering itself, which should be tested by independent means. Below I will present four such tests on the results obtained for Hienheim; in the next section I will present the outcome of a similar analysis on an entirely independent data set (from Elsloo, in the Netherlands), and finally, I will draw attention to a case study made by R.D. Drennan along roughly parallel lines of thought. The checks on the Hienheim results bear on reliability; the analysis of the Elsloo data should be a check on the method's consistency; and Drennan's case study may perhaps be seen as validating the general idea of my analysis.

1. *Internal evidence:* The behaviour of the mixes as deduced from the sequence computed from the data for three variables (TECHNIQUES, COMPONENTS (belly), STRUCTURES) should make sense on the variables that were left out in the principal components analysis (cf. pp. 47, 48). A glance at Figs. 13 and 14 shows a constellation which is not entirely satisfactory: as a consequence of the discontinuity the postulated logistic curve is masked on the other variables; still, a general trend of change is apparent on them. Nor is the general shape of the curves from the phases 01 to 10 as neat as the model of Fig. 9 prescribes. As a possible explanation of this bears on the entire problem of the evaluation, this will be discussed at the end of this section.

The confidence intervals do not present a very clear picture either; a comparison of the standard deviations computed per phase and averaged per variable (Table 5) shows that the three "guiding" variables have markedly smaller values (and thus are more precise) than the other ones. However, taking the different numbers of observations into account (also Table 5) the scene looks less gloomy: larger variances appear where the number of valid observations is low and where the reliability is wanting (this latter point cannot be quantified, except through the variance – which would obviously introduce a circular reasoning).

2. *Alternative computations: canonical analysis of raw data:* Drs. M. Tjok Joe of the Centraal Reken Instituut of Leyden University was so kind as to check the results of my PCA by means of a canonical analysis of the raw data (i.e., without converting the raw counts to percentages, and working directly with the data, not with a correlation matrix; for details on this method see De Leeuw, n.d.). All finds containing decorated sherds (without measures against noise) were analysed on 43 arbitrarily selected traits. The first non-trivial component resulting from this computation could then be identified as being highly related to the passage of time. A comparison of the relative positions of the various finds on PC 1 and on the first Canonical Component (Table 6) showed a rather strong agreement: a correlation of .70 should be considered "quite good" in this case. Presumably a non-arbitrary selection of traits (to diminish the frequency of missing values) and the imposition of restrictions to size of the finds (to take account of the rumble) would considerably bolster up the correlation of the two sequences. (For a possible explanation of the rather wide scattering in the lower part of the matrix, the reader is again referred to the end of this section).

3. *Non-multivariate checks: direct/absolute dating:* From Hienheim, five radiocarbon dates are available for the Early and Middle Neolithic:
- find nr. 0068: 5910 ± 50 bp (GrN 4830)
- find nr. 0108: 5780 ± 50 bp (GrN 4832)
- find nr. 0414: 6125 ± 35 bp (GrN 5870)
- find nr. 0822: 6155 ± 45 bp (GrN 7156)
- find nr. 1115: 5905 ± 45 bp (GrN 7157)

Among these dates, those for finds nrs. 0068 and 0822 are suspect in one way or another:
- Find nr. 0068 consists of pottery which is truly LBK in appearance; yet its C-14 date is a full century later than the generally accepted end of the range for LBK dates (Neustupný 1968). If only for this reason, the date should be set between parentheses (an analogous example can be found in Milisauskas 1976b:33). Another reason is that the field drawings show slightly layered fillings of the pit. Although the excavator, Prof. dr. P.J.R. Modderman entertains no doubt as to the association of pottery and charcoal (pers. comm.), I am inclined to question it on the grounds presented.
- Find nr. 0822 refers to carbon sampled from a sherdless post hole of a hut, thus dating that structure and its accompanying features. Unfortunately, no pits can be unequivocally associated with it – though pit 0749 might be a candidate. Accompanying a number of overlapping house remains, that pit is one of a complicated set of pits, the relationships between which are but poorly understood. Therefore, the suggested relation is shaky, at best. And the very fact that it would run counter to the results of the principal components analysis (as two of the datings would appear in reversed order) strengthens the doubts about the attribution of this date to find nr. 0749.

With these reservations, the sequence of radiocarbon dates agrees well with the mathematically deduced one (Figs. 14, 15, 16).

Apart from the radiocarbon dates, a number of thermoluminescence readings have been obtained as well. From pit 0414 three thick sherds were measured: 4660, 4390, and 4780, averaging 4610 ± 600 B.C., or 5775 bp in conventional C-14 years (range 5170 to 6295 bp). As this TL date is at variance with the radiocarbon date obtained from the same pit 6125 ± 35 bp; plotted in the Figs. 14, 15, 16), and its extended range allows for several interpretations, no attempt will be made here to reconcile this date with the time scale proposed; an additional reason would be that there is only one single date available, not a series covering several pits and a range of time.

4. *Non-statistical checks – stratigraphy:* As noted before, only one case of stratigraphical superpositions has been noted at Hienheim: find nr. 0548 has been observed to cut into nr. 0555. They are attributed to phases 8 and 9, respectively (MODEL

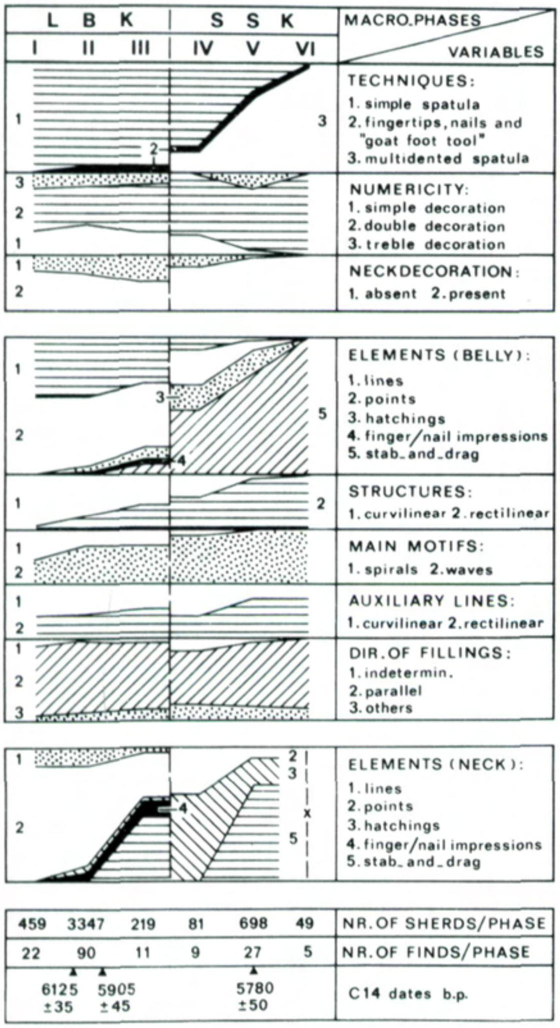


Fig. 16. HIENHEIM: summary of MODEL III, condensed to six macro-phases as follows from TABLES 5 & 6 (i.e., according to MODEL III-A).

II) – in other words, the wrong way round. Simple logic might allow me to evade the problem by stating that the research question concerned continuity and discontinuity only. However, as in some quarters of the discipline stratigraphy is still the only method of relative dating accepted, I feel obliged to face the issue. It has also to do with difficulties encountered in earlier parts of this section.

Generally, a margin of error is to be expected in any determination, *including stratigraphy* (though in the above cases not a shadow of doubt exists as to the accuracy of the observations). The causes of this error are manifold. Most notable among them are noise and sampling errors, defects in the method or the "instrument" of observation plus misreadings and subjectivity (and a total evasion of testability can be invoked by citing nonconformist past behaviour).

To start with the latter point, subjectivity, there seems to be no way to avoid this completely. The explication of all steps involved in an analysis is usually considered a good antidote for the analyst; additionally, it facilitates criticism. It is my objective to conform to this standard.

The source of error commonly labeled "noise" or "rumble" has already been dealt with in Chapter II; I will not recapitulate the arguments here. The next data-dependent error stems from the faulty distribution of the samples; archaeologically speaking, depositional hazards belong to this type: no individual find ("sample") need be fully representative of the original population ("universe") from which it is drawn, as factors other than pure chance may have been involved in the discarding and deposition process. However, a *set* of samples lifted out of the same universe (= a number of finds relevant to one mix) will jointly approximate the original compound. The aggregated change over a number of such sets will constitute a fairly accurate indicator of the original events (Clarke 1968: 163, 170) provided the number of samples is sufficiently large (Hays 1973: 317); – my 164 samples would seem to be well beyond the 100 or 120 which are usually required by rule of thumb. To illustrate this error, 90% intervals of confidence have been calculated and plotted with the averages of the samples per phase in Figs. 13, 14 and 15: in 90% of the cases the "true" (or, original) value of the mix will have been within the computed range. As will be very clear from inspection of the plots, the positioning of any find/sample is subject to a fairly wide margin of error (counting the phases), except in the intervals between phases 12 to 20 (MODEL I) 11 to 18 (MODEL II), or 8 to 10 and 11 to 15 (MODEL III), where change is relatively rapid. Adding more observations is likely to reduce the width of the confidence intervals; also, expansion of the number of variables entered into the analysis (if these variables are as readily observed as those already entered) *should reduce the number of* phases fitting the description of an individual sample (Hays 1973: 317). The simple mistakes of observation when reading the instrument are the counterpart of the noise mentioned above. Mistakes in counting, coding, and punching cannot be evaded; Since I went through data and output *many times in many computational* cycles the magnitude of this error should be relatively small (that is, probably less than 10%). Fortunately, this error is independent of the data, and thus it should show up as a separate principal component; by ignoring all but the first PC (that of time) this noise should have disappeared.

The *second instrumental source of error* is the most serious one, as it is implicit in the models for the distribution of the finds over time. Yet the distributions are a necessary preliminary to calculate and depict the behaviour of the variables over time, as demanded by the model of continuity and discontinuity developed here. Even a superficial glance at Figs. 13 and 14 will suffice to demonstrate the differences in outcome of both MODELS. This same short inspection will also bring forward the fact that the conduct of the variables in the bottom or older half of the diagrams is not as neat as proposed by the model of Fig. 9. While the extent to which this last little model describes reality effectively is open to some doubt (especially in those parts of the curve close to the asymptotes), I believe – without rock-bottom foundation – that this model is the best one in the entire set of models introduced here. Consequently, I also think that there is something wrong with the "earlier" part of MODELS I and II. MODEL III, introduced expressly to remedy this latter point, did not bring any appreciable improvement, as a comparison of Fig. 15 with Figs. 14 and 13 shows.

Turning again to the irregularities noted above, what can be said about them in light of the previous discussion?

– On the subject of internal checks (p. 57) a part of the shape of the curves was found to be *unsatisfactory*. The distributions prescribed by the MODELS I and II have been criticized as being probably not entirely realistic, and shortening of the relevant (earlier) part of the time scale was suggested as a possible remedy. This did not work out as expected, however (Fig. 15); perhaps the scale should be compressed even more, as in Fig. 16.

– Discussing the results of the alternative computations in the context of Table 6, a fairly wide scattering of the elements in the lower or "earlier" parts of the matrix was noted. From this, probably the same cause (partial inadequacy of MODELS I and II) should be supposed in both instances, as it *works out in the rather wide confidence intervals* for the phases 1 to 11.

– Finally, the meager stratigraphic evidence running counter to the time scale should be considered. Referring to Fig. 14, there is a partial overlap of the confidence intervals for the pertinent phases (8 and 9) on the computational variables TECHNIQUES. COMPONENTS (belly), and STRUCTURES. While this is a sufficient explanation (though not necessarily a satisfactory one), a remedy will be found only if more samples can be incorporated in the analysis to narrow the confidence intervals. Such an increase can be obtained by the coding of more data, but also by contraction of the time scale.

A rather off-hand attempt at contraction of the time scale was made starting from the correlations between the various phases of MODEL III; in other words, a Q-type analysis (Clarke 1968: 533; Sokal and Sneath 1963: 207-209). In Table 7 these correlations are presented, both as individual numerical values and as summarized by a contour map. From the latter, three "macro phases" are immediately apparent: a first one of the phases 1 and 2, a second one of the phases 3 through 9, and a third one comprising the phases 12 through 15 (MODEL III). The obvious critique here is that the correlation coefficients in Table 7 reflect nothing but the initial assumptions (i.e., MODEL III), which is true of course. Yet that MODEL may have at least some validity, it was observed above (p. 55) where the MODEL III distribution of the finds was compared with the outcome of a multiple regression analysis (Fig. 24). What I am attempting here is a further condensation of the data *within the framework of MODEL III*, nothing more.

Looking at the upper part of the matrix, a different division can be proposed: instead of the phase groups 1-2 and 3-9, a grouping of the phases 1-6 and 7-9, respectively. Apart form this, the phases 10, 11, and 16 are clearly transitional. Computation of the correlations between the three "macro phases" (as aggregates) and the three transitionals yields Table 8, where the coefficients resulting from both ways of condensation are given. While the correlations reported in this table are all appreciably lower than in Table 7 (thus justifying the condensation in a general way: the macro phases are more independent of one another than are the smaller ones), those above the diagonal are consistently lower than those in the lower part of the table, thereby allowing a preference for the first alternative. In the meantime there seems to be no very good reason to maintain phase I as a separate entity – except that it shows up in the contour map of Table 7. It has been retained for the sake of symmetry, however.

Grouping the finds according to these macro phases produces the trajectories of the mixes shown in Fig. 16, which are more satisfying on the whole than those of Figs. 14, or 15. However, although the stratigraphic contradiction is eliminated this way (the pertinent finds now belong to Phase II), it should be noted that this was achieved only through a considerable loss of discriminatory power.

Thus, a contraction of the time scale is but a partial answer to the difficulties above; the incorporation of more data will surely prove more effective (see the Postscript to this chapter).

7. *Further corroboration*

In a previous section I stated that the models, methods, and techniques introduced here could at best appear plausible when applied to a single data set. After all, however much agreement of results and expectations, the possibility of a computational (or methodical) artifact remains.

Below I will present the outcome of a parallel analysis of a second, different data set, on the assumption that if the analytical procedure is invalid at one stage or another, chances of workable outcomes for two data sets are greatly reduced.

The LBK settlement of Elsloo, in the southeastern part of the Netherlands, has been excavated in the years 1958 to 1966, and has been reported in Modderman 1970. The site is older than that of Hienheim: at Elsloo, the oldest pottery is of the Flomborn (or "international") style (Modderman 1970: 196; Meier-Arendt 1966: 23). Also, the latest (relevant) sherds were deposited before introduction of Hinkelstein (i.e., Middle Neolithic) ware could occur (Modderman 1970: 198), somewhere in the fifth phase of the Main sequence (Meier-Arendt 1966: 45-46; 1975: 142). A consequence of this Early Neolithic date is that most of the houses at Elsloo are accompanied by pits, whereas at Hienheim this is only the case for the older, Early Neolithic part of the occupation. Through their association with a hut, the contents of a number of pits could be lumped to provide better/larger samples in quite a number of instances. In other words, in Hienheim comparability was on the level of finds only (cf. Ch. II), at Elsloo it was also on the level of huts – and though I will present figures for the finds too (Figs. 20 and 21), my argument will be based on the computations made for the houses (Figs. 18 and 19). It should be emphasized that the two data sets are not equivalent, as not every find could be unequivocally assigned to a hut: 53 houses summed 163 finds, but only 151 finds were larger than the noise level.

This "noise" level for the Elsloo sherds could be fixed at two sherds – in Hienheim four; cf. Ch. II, Section 3 – a difference very probably due to the selective process of publication (Modderman 1970: 6; if no more than six sherds pertained to a given hut, they were not published). After coding the decorated ware from the publication, a preliminary PCA of the data indicated that the chronological ordering was to be computed from the variables TECHNIQUES, COMPONENTS of decoration (belly area) and presence of NECK DECORATION (at Hienheim: instead of presence of NECK DECORATION, STRUCTURES; cf. Figs. 15 and 19). In the original PCA, the chronological PC took care of 9.3% of the variance; in the subsequent, special PCA, 47.9%.[8]

With the houses thus chronologically ordered, Fig. 17 presents a comparison of the rankings of individual finds and huts as produced by separate PCA's; also, Modderman's phasing has been rendered. Differences between the three orderings are apparent; however, a substantial overall agreement is very clear. Furthermore, neither of the PCA sequences contradicts any of the stratigraphical observations from the excavation (Modderman 1970: 28-35). By these two parallels (plus the existence of S-distributions of the mixes on other than the computational variables) the PCA technique, in my opinion is validated.

Regarding Fig. 17 a number of comments should be made. They are divided into general and specific remarks.

General (1): The subdivision into phases is derived from the computer output: the factor scores of the huts are not evenly distributed over the chronological axis; rather they show some clusters. From the time span involved (350 to 450 years) a partitioning of the data into smaller sets seems advisable; cutting-off points were "established" between the clusters of factor scores. It should be emphasized that the phases thus produced relate to decorated ceramics only, and also that they do not have any substantive meaning beyond this analysis. Of course, the general agreement of "my" phases with those of Modderman is not purely coincidental; Modderman's phases are also based on pot decoration, yet stratigraphy and hut typology figure too.

General (2): Regarding the actual duration of the phases (be it in years or in generations), nothing can be said. The differences in factor scores depict compounded change in ceramic decoration. As nothing can be said about the rate of change per unit of time, two models were introduced to spread the Hienheim data along the chronological axis (pp. 50, 60-61, also note 5). It will be clear that (non-)application and choice between the models is entirely arbitrary; these will have different consequences for phase length as well.

General (3): Two phase boundaries (between 3 and 4, and 4 and 5) are not very clear-cut: there are no sharp changes in the factor scores at these loci.

General (4): Regarding the ranking of the huts, its reliability is tied to the number of observations (sample size) on which it is based. Especially when the number of sherds is low (less than ten; which is the case for eleven huts), the rank accorded cannot be but indicative; this will hold to a lesser extent for sample sizes of ten to twenty sherds as well (ten huts). (With three variables, in larger samples the number of observations rises to above the conventional rule of thumb size: 31 huts.) Referring to the discussion of confidence intervals above (pp. 50, 59-60), any single observation may fall within a specifiable range, yet through the variation allowed, it may also fit into other, overlapping ranges. Expansion of the number of observations through expansion of the number of variables, or through expansion of the number of units in the sample, results in a narrowing of the confidence limits and

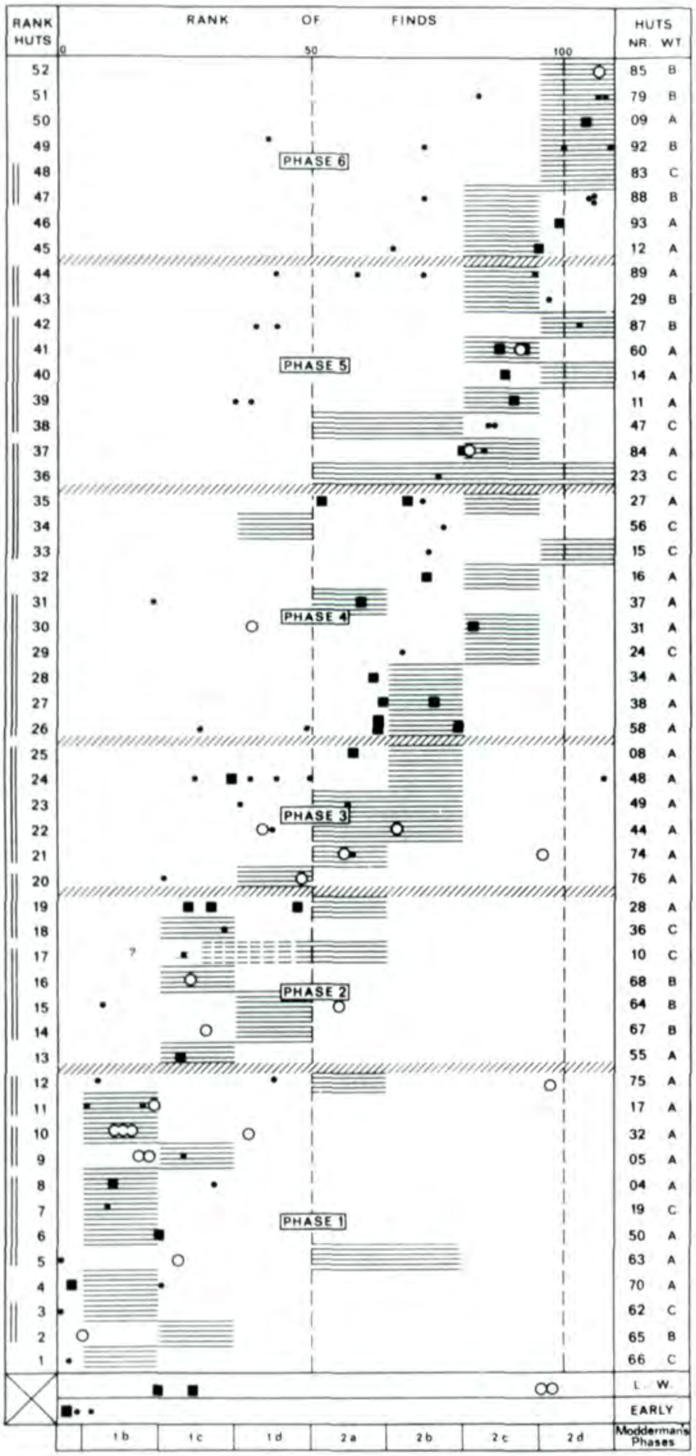


Fig. 17. ELSLOO VILLAGE.

A comparison of the pca derived chronologies of huts (vertical scales) and individual finds associated with the huts (top scale) with Modderman's datings.

● C finds, huts with ≤ 9 sherds.

○ B finds, huts with 10-19 sherds.

■ A finds, huts with $\geqslant 20$ sherds.

PHASE 1-6: phases suggested by clustering of factor scores; old to young.

'RANK HUTS' chronological sequence of huts computed from aggregated finds around them (1-52: old to young). Bars to the left of rank nrs. indicate approximately equal factor scores.

'RANK OF FINDS' chronological sequence of individual finds unequivocally assignable to huts.

'HUTS NR' identification number from Modderman 1970.

'HUTS WT' indicates nr of sherds associated with huts.

'L.W.' finds with Limburg Ware.

'EARLY' finds probably ante-dating construction activities

▤ date of hut according to Modderman 1970 (I):35-42. (from bottom scale).

thus in a securer positioning of the sample. Similar considerations apply to finds.

General (5): Though the figure may be suggestive if not deceptive, there is no direct connection of the finds' rankings with Modderman phases: each of these is entirely independent of one another.

Specific (1): Among the factor scores computed for the huts, similar values indicate chronological nearness of the houses as indicated below.

Ranks	Hut nrs.	Weights
02-03	65, 62	BC
05-08	63, 50, 19, 04	AACA
09-10	05, 32	AA
11-12	17, 75	AA
14-17	67, 64, 68, 10	BBBC
18-19	36, 28	CA
21-25	74, 44, 49, 48, 08	AAAAA
26-31	58, 38, 34, 24, 31, 37	AAACAA
33-37	15, 56, 27, 23, 84	CCACA
38-42	47, 11, 14, 60, 87	CAAAB
43-44	29, 89	BA
47-48	88, 83	BC

(Rank: sequence number of factor score, computed by pca (unrotated) from variables TECHNIQUES, COMPONENTS and NECK DECORATION)
(Hut nrs. acc. to Modderman 1970)
(Weights: in nr. of sherds; A 20 and over; B 10-19; C less than 10)

Specific (2): In Modderman 1970 (1): 35, several finds are discussed which might have been dug before the beginnings of hut construction in the village. For the finds nrs. 214, 323, and 434, rankings were computed (vertical scale to the top) as 004, 002, and 007, respectively; the size of nr. 323 is sufficient (1 pot + 29 sherds) to result in a reliable relative age. Several other finds seem to be very early as well (ranking less than 006):

– unambiguously associated with huts, and appearing in the figure:

(rank 000): finds 238 (3 sherds; Hut 62), 262 (3; H.63)

(rank 002): find 408 (9 sherds; H.09)

(rank 003): find 300 (21 sherds; H.70)

(rank 005): find 303 (15 sherds; H.65)

– not unambiguously associated with one hut only, not in the figure:

(rank 001): find 288 (4 sherds).

Again, only a few finds are large enough to be regarded without serious doubts (nrs. 300, 303, 323; possibly 408 also).

Specific (3): There are four finds in Elsloo containing Limburg Ware (Modderman 1970(I): 141-143; also: Modderman 1974; Gabriel 1976): nrs. 305 (10 sherds; H.74; rank 096), 329 (12; H.75; 098), 356 (23; H.20; 027) and 452 (47; H.50; 020). The Limburg Ware has not been entered along with the LBK ware into the computations of the relative age of the associated huts.

Specific (4): Some minor remarks remain on the positions of finds and huts in Fig. 17:

- Hut 10 (rank 17); find 072 incorporated (Modderman 1970(I): 29; also p. 8)
- Hut 29 (rank 43): finds 234 and 454 are grouped with this hut, although they may belong there only "partially" (ibid, p. 13). This can be given as neither an alternative nor a criterion for dividing the sherds. That is, the dating of this hut is approximate only.
- Hut 48 (rank 23): find nr. 604 is accorded a very high ranking (:108). It derives from a postmold. No reason can be found to exclude this hut's inventory.
- Hut 56 (rank 34): according to Modderman's text, different lines of evidence point to dissimilar datings: absence of a wall-trench yields period I; inner construction, phase IIb; some sherds, phase Id (Modderman 1970(I):18). From its position in Fig. 17, the present author would favour the date indicated by the constructional details.
- Hut 60 (rank 41): according to the description find nr. 434 (rank 007) should be incorporated with it. However, on the plate depicting this find, no attribution is given (as is on other plates for other finds); similarly, from the hut's plan association seems to be less than evident (ibid, p.

19; ibid. Vol II: pl. 51, 27; respectively). In the computation of the hut's rank, find nr. 434 has been left out.

- Hut 62 (rank 03): has been put into Modderman's phase Ib on account of its "very typical" Y-postmold configuration. Modderman only indicates the first Period for the hut's construction (ibid., Vol I: 33, 20; 36, 37).
- Hut 63 (rank 05) is certainly much younger than its ranking indicates. The finds associated with it, though, are older than phase 4, the date suggested by the hut's extraordinary construction (Modderman 1970(I): 20, and (II): Pl. 28). The conclusion seems inevitable: hut 63 is not to be associated with find nos. 262 and 275. Because of these incompatabilities this hut is omitted from further consideration; in Ch. V the date indicated by the hut's construction will be used.
- Hut 64 (rank 15): find nr. 220 is very early (rank 009), which may be due to the small number of sherds (only four). There is no reason, however, to reconsider its association with the hut. Then, Modderman 1970(I): 20 posits this building "early in Period II"; from hindsight, however, a date in Id should seem better (P.J.R. Modderman, pers. comm. 201278). This latter date has been entered accordingly.
- Hut 74 (rank 21) is associated through find no. 305 (ranking 096) with Limburg ware. In the computation of the hut's ranking, the Limburg sherds have not been incorporated (as with finds nr. 329/Hut 75, 452/H. 50).
- Hut 75 (rank 12) has been accorded a relatively early ranking, which is in line with Modderman's observations on the associated pottery. A date for the hut in Id-IIa is narrowed to IIa on account of details of the hut's construction (ibid., p. 22). The high ranking find is nr. 329, which because of its Limburg sherds has scored that high; for the hut's chronological position, the Limburg Ware has been omitted.
- Hut 84 (rank 37) should be younger than hut 83 because of their relative positions. Yet, the associated pottery points to an inversion: H.84 is ceramically older than H.83 (ranks 37 and 48 respectively). Modderman's conclusions are identical (ibid., p. 24).

Specific (5): In the computations of Figs. 18 to 24, huts nr. 26 and 72 have erroneously been entered along with the huts listed in Fig. 17 on ranks 08 and 05, respectively; their small size (eleven and three sherds) will make the effects negligible. Because of this, the numbers of huts per phase in Fig. 19 are not fully identical with those in Fig. 17.

Regarding the model of continuity and discontinuity, the logistic curve hypothesized for the mixes is visible on most variables: TECHNIQUES, NECK DECORATION, COMPONENTS (belly), FILLINGS of bands all show this pattern (Fig. 18; also the other drawings).

The totality of the variables shows a much more diversified picture for Elsloo than for Hienheim: at the latter site almost all visible change is concentrated on the younger end of the scale, whereas at Elsloo change occurs everywhere; the inflection points of the various variables are much more scattered chronologically. And although the curves for the Elsloo ware were not smoothed, they are more regular in appearance than the smoothed ones for Hienheim pottery decoration. For these reasons (regularity and diversity), introduction of confidence estimates is not necessary: if continuity is anywhere archaeologically demonstrable, it is for the decorated pottery from Elsloo, as dissected in Figs. 18 to 21.

Also, the fine interpretability of these graphs is a further corroboration of the usefulness of the continuity/discontinuity model developed in the second section.

Two final notes should be added:

- the drawings for Elsloo houses (Figs. 18, 19), for Elsloo finds (Figs. 20, 21), and for Hienheim (Figs. 13 to 16) are for not-entirely-identical sets of variables. This is due to differences in coding: some

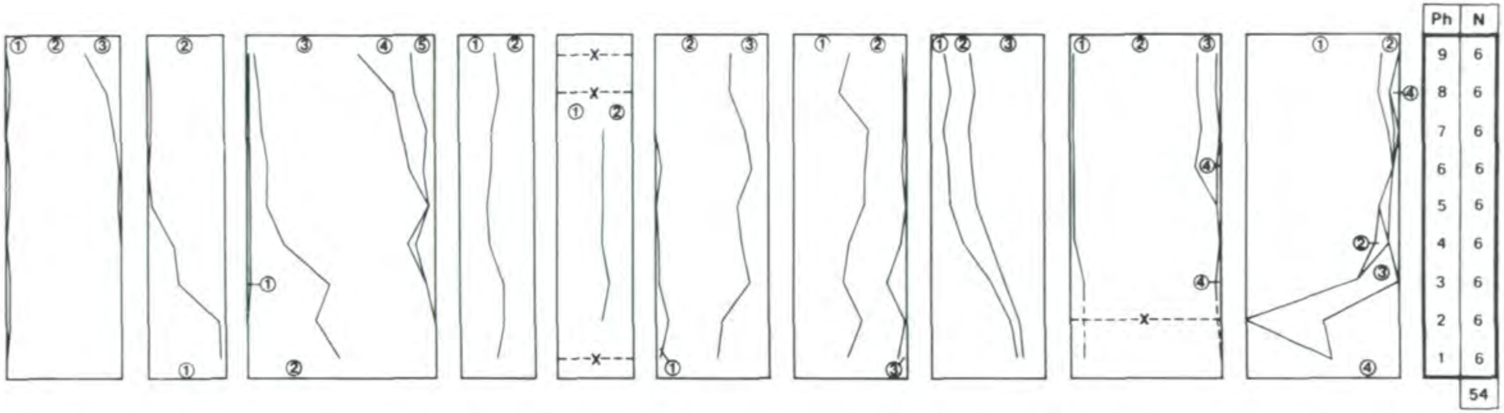


Fig. 18. ELSLOO VILLAGE: Proportions of various attributes per variable of decoration over time. Phases comprise equal numbers of houses, ordered chronologically by means of a principal components analysis of the variables marked with ■.

top: youngest phase; bottom: oldest phase.

N: number of huts comprised in phase.

column width: 100% each (also cf. Fig. 13).

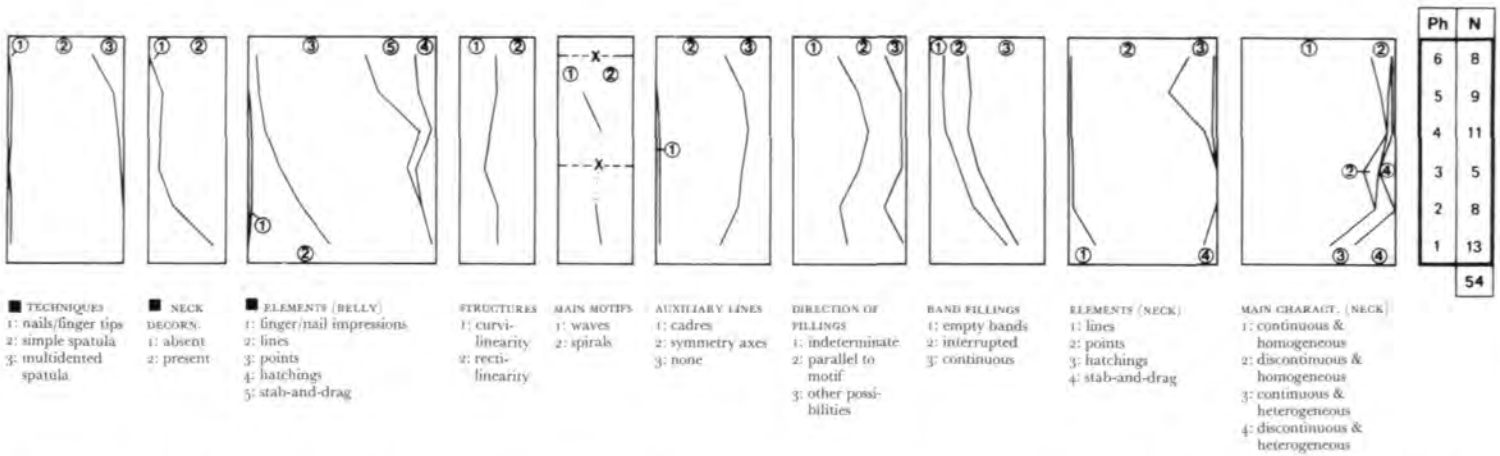


Fig. 19. ELSLOO VILLAGE: Proportions of various attributes per variable of pottery decoration over time. Phases derived from a principal components analysis of the variables marked with ■, and according to clustering on the time scale or component.

top: youngest phase; bottom: oldest phase.

N: number of houses comprised in phase.

column width: 100% each (also cf. fig. 13).

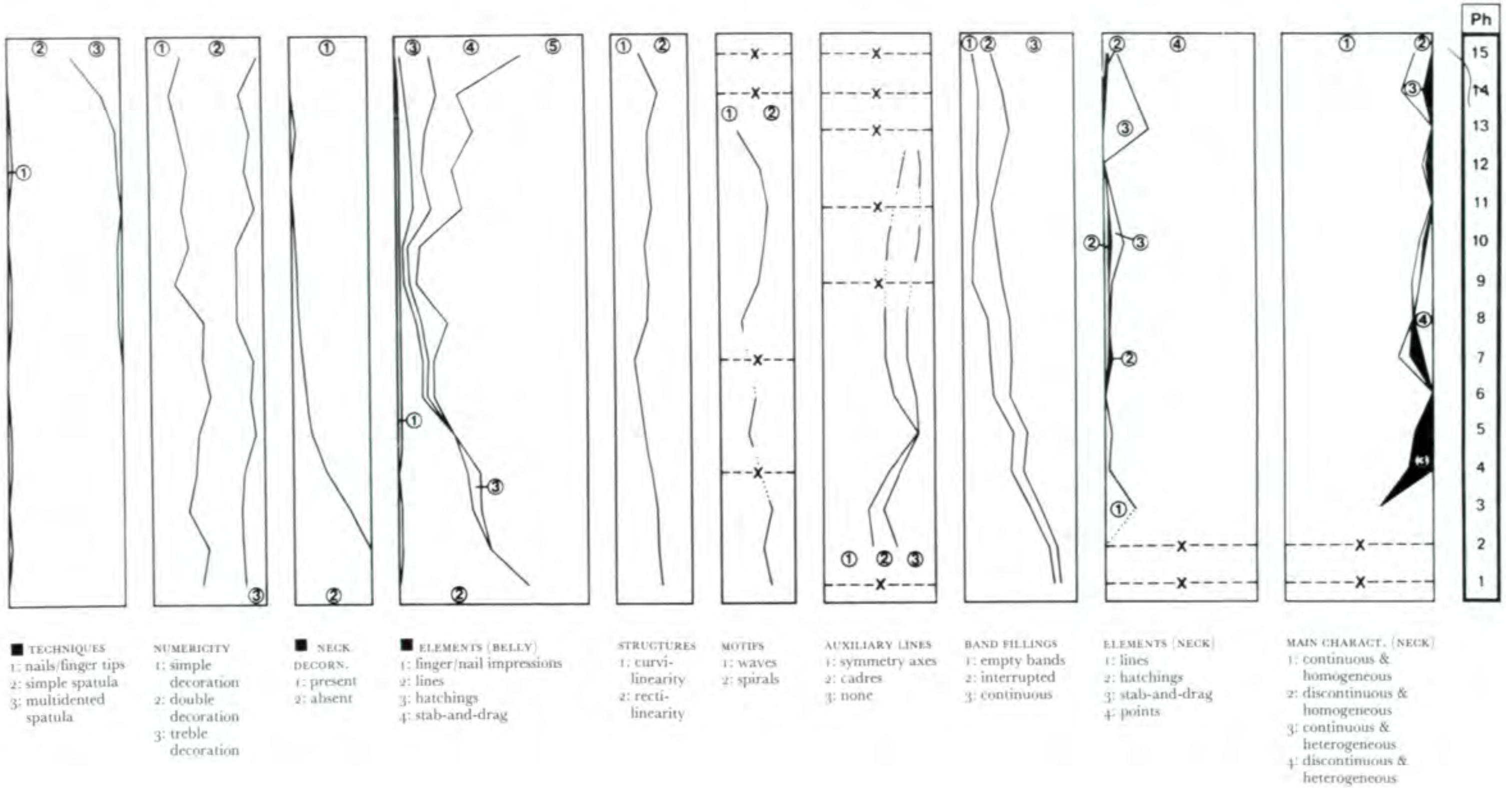


Fig. 20. ELSLOO VILLAGE. Proportions of various attributes per variable of pottery decoration over time. Phases comprise equal numbers of finds, arranged chronologically by means of a principal components analysis of the variables marked with ■.

top: youngest phase; bottom: oldest phase.

Data for finds, ten finds to the phase.

Column width: 100% each. (also cf. fig. 13).

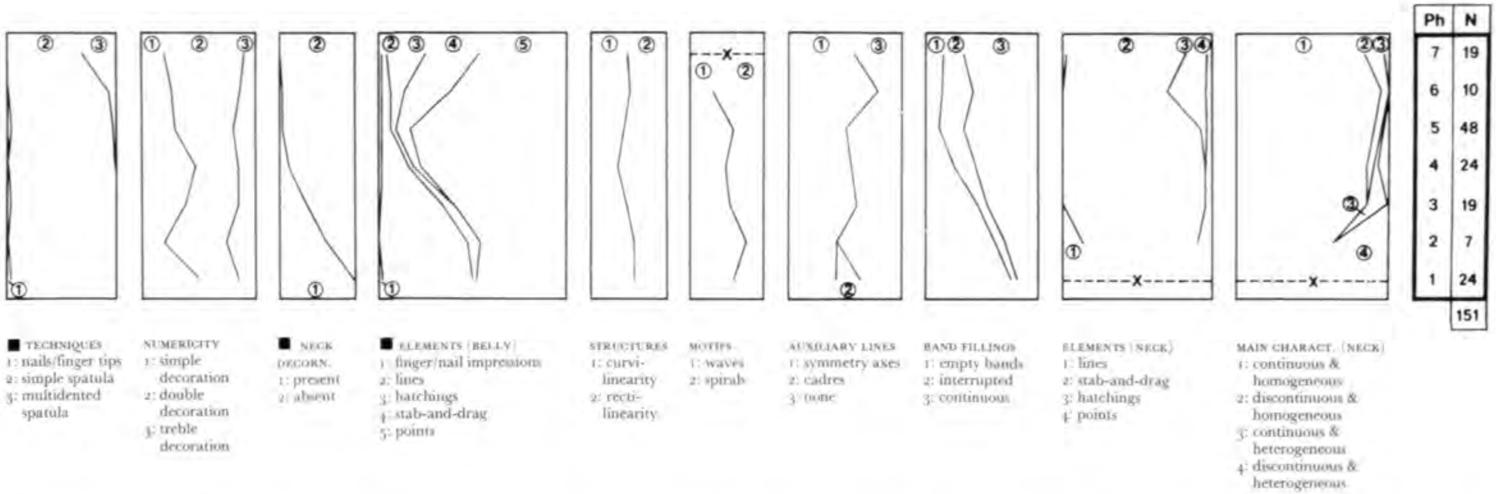


Fig. 21. ELSLOO VILLAGE: Proportions of various attributes per variable of pottery decoration over time. Phases derived from a principal components analysis of the variables marked with ■, and according the clustering on the time scale or component.
top: youngest phase; bottom: oldest phase
No. of pits: number of finds comprised in phase.
column width: 100% each (also cf. fig. 13).

variables have been redefined in the time between the analyses.
– for Elsloo the distributions of the pits/houses over time were not converted to the numbers of sherds. This was the first place because such a procedure involves a substantial investment in time, and in the second place because the final and preferred outcome for Hienheim (MODEL III) is very much parallel to the original distribution of the factor scores. And, as stated before, any (re-)arrangement is arbitrary.

Another corroboration of the general idea underlying the present analysis can be found in a recent paper by Drennan (1976), especially regarding the construction of a chronological series – in his case, for ceramic data from Oaxaca in Mexico. He starts from a Brainerd-Robinson matrix of distances (dissimilarity-coefficients) in a sample of four stratigraphical groups of together 22 finds. The distances are calculated over an unspecified set of traits of decoration and of form. This matrix is then entered into a nonmetric multidimensional scaling program (discussed, among others, by Hodson et al. 1971: 303; and Shepard et al. 1972: 52) to chronologically arrange this basic set; afterwards some 300 finds were added to produce the final series. In the present context, the following points are of special interest:
– Finds as such are found to contain sufficient information for a chronological ordering; co-occurrence of traits on individual sherds is not used as input for the analysis.
– Chronologically insignificant or unreliable variables are omitted after a pilot study.
– As far as stratigraphic controls go, a number of finds is incorrectly placed by the program: noise, small size, and central position on the strongly bent time trajectory are mentioned as possible causes. Noisy finds are dropped (cf., however, Ch. II, Section 3), and small finds *are assigned to sections instead of points on the time axis*.
– Frequency counts are used to monitor ceramic change as an image of the passage of time.

As a critical remark, the disregard of the possibility of discontinuities has to be mentioned, whereas from the description two 'pseudo-continuities' may be inferred: simultaneous change on a number of variables is simply taken to mark the transition between phases (a similar reification of the phase concept as in Lüning 1975: 181).

Apart from this criticism, I consider the parallelism of Drennan's ideas and mine – independently developed – indicative of the validity of the basic principles.

8. *Conclusions*

To shorten the following discussion, I will introduce some symbolic notations:
– "d" will stand for the decorated Early and Middle Neolithic pottery excavated at Hienheim up to and including 1970; this ware is the subject of the present analysis.
– "D" will stand for the decorated Early and Middle Neolithic pottery of the entire modern site of Hienheim, whether excavated or not, yet potentially discoverable; $d \in D$ (or: d is a subset of D).
– "h" will indicate the part of the site that has been excavated until and including 1970, in some unspecifiable way roughly corresponding to d.
– Finally, "H" will represent the entire modern Early and Middle Neolithic site at Hienheim. Again, $h \in H$; h is estimated to be about. 4 H or more; also, h is not a random sample from H, and thus not representative of H. In other words, the probability that any sherd from H is in h is not constant; a smaller percentage of the sherds dumped near the forest front of the settlement are incorporated in h than of those discarded on the river front. D may be thought of as the modern representative of the decorated pottery of the Bandkeramik tradition, and H as the contemporaneous manifestation of the settlement of Old Hienheim.

On the basis of the results established in the fifth section, bearing the qualifications of the sixth section in mind (plus the corroborations in the seventh section), and using the symbols defined above, the following can be said about the research question whether there was a discontinuity or not from LBK to BR:

+1: in d there is a discontinuity. However, since h is not representative of H, nothing can be said of continuity or discontinuity within D. Similarly, as the possible relations between any pair of the terms d, D, h, and H (or between any of the minor terms and the original potters at Old Hienheim) are not known, not even approximately, there is no way to deduce from d's discontinuity a similar discontinuity in H or among the old potters, for that matter. Thus, from the present analysis no "*Siedlungskonstanz*" (continuous occupation at the same site; Berger 1973: 24) is to be concluded.

+2: the research problem has been derived from the general question of continuity of discontinuity in Bavaria from LBK to BR pottery decoration. If the general picture of d, as presented in Figs. 13 and 14, is accepted, then it can be observed that almost all traits that together constitute the style of decoration at the younger end of the scale (i.e., those characteristic of BR pottery) already occur BEFORE the discontinuity spoken of in the last paragraph. This is even clearer from the graphs of Fig. 16, which are in a way condensed transforms of those in Fig. 15. Therefore, no matter whether there is a discontinuity in D, perhaps even among the old potters, a *continuity* in the Bandkeramik tradition of pottery decoration is apparent. So, since the take-off which would later result in the BR style of decoration evidently did occur in a Bavarian LBK milieu, the Zápotocká theory (Zápotocká 1970: 28-29) has been refuted on two important points:

no Bohemian or other allochthonous origins of BR need be assumed;

BR is not a Bavarian variant of the SBK style, but a style of decoration in its own right.

Conversely, her observation that in Bavarian no evidence of the older phases of the SBK can be found (Zápotocká 1970: 13) now falls into place, even gaining perspective from this analysis.

Also, Meier-Arendt's theory (Meier-Arendt 1975: 134-135) of an autochthonous evolution of pottery decoration from LBK to BR appears to be supported ("corroborated") by the present analysis if interpreted as *referring to the region*.

Finally, it would also seem that if there is place for two successive styles within BR (of which I am yet to be persuaded[9]). Unterisling with its *hatched* decoration (Zápotocká 1970: Pl. 8) would precede the Oberlauterbacher style of *stab-and-drag* elements.

+3: in the first part of the sixth section it was stated that the usefulness of a model is a measure of its value. The conclusions above justify my model of continuity and discontinuity as presented in Section 2, p. 42-45, I think. This, then, is an empirical falsification of Van der Waals' statement already alluded to in note 2, that discontinuity can be suggested only: it can be demonstrated, as continuity can be.

Then, I would like to define the BR pottery decoration explicitly. Stroh (1940), who invented the term (and took it to mean the Bavarian facies of the Rössen style) gives only hints as to its meaning; Zápotocká 1970: 29, in attributing SBK principles to the ware, also presents summary descriptions only; Meier-Arendt 1975 seems to be too preoccupied with his analogue models to worry much about definitions (though some indicators as to the appearance of Unterisling are given: Meier-Arendt 1975: 135); Torbrügge and Uenze 1968, Maier 1964, Mauser-Goller 1969 all bypass the issue. This style of pottery decoration is characterized by:

- TECHNIQUES: *multidented spatula*, sometimes in combination with the "goat foot tool".
- NUMERICITY: (absence of *simple* decoration), *double* (and quadruple) and *treble* execution of all motifs, auxiliary lines, etc.

– NECK DECORATION is *present* on every decorated pot; it is generally executed in one single element, and interrupted in a metope-like fashion.
– COMPONENTS (both body and neck): either *stab-and-drag* impressions or *hatching*, which seem to be almost mutually exclusive on individual *pots*,[10] often combined with a fringe of points around the motifs.
– STRUCTURES: *rectilinearly* executed motifs.
– MAIN MOTIFS: derivatives of the *zigzag* (rhombs, zigzags, or simple oblique patterns).
– AUXILIARY LINES: may or may not be present, and if so, disguised as fringes, partitioning lines, etc.

If these traits occur together in a closed find of Middle Neolithic, Bavarian provenience, the find may be named after this style if the listed traits occupy more than (say) 50% of the mixes.

Because the above definition has almost nothing in common with that of Rössen proper, it might be better to follow Meier-Arendt's advice that "the label 'BR' should be rejected as being ambiguous" (Meier-Arendt 1975: 160). In its place "Stab and Hatch Complex" is proposed (in German: "Stich-Strich Komplex" or "SSK", sounding rather different from "Stichband Keramik", "Grossgartach", "Linearband Keramik", or "Münchshöfen", to name but the contiguous styles); the first two words point the two main alternative characteristics of the pottery, and "complex" indicates that it is a variant within the Bandkeramik tradition, and not a separate entity.

Two minor conclusions will end this chapter:
– Neither MODEL II nor MODEL I is entirely adequate to describe d at H. Especially the earlier part of MODEL II should be reconsidered (probably compressed).
– Since h is not representative of H, d will almost certainly not be representative of D. Therefore, expanding the number of units in the analysis might considerably modify Figs. 13 and 14 (see the Postscript to this chapter).

NOTES

[1] As a sideline, the following definitions may be proposed: A *tradition* refers to the set of variables for which (usually within a geographically restricted area) a continuous change over time can be postulated. *Style* will indicate a set of synchronic mixes, a substantial proportion of which show a homogeneous (or single trait) composition. Then the sets that are less extreme in composition could be labeled intermediate.

For Hienheim it can be said that one tradition is object of study, viz., the Bandkeramik tradition of pottery decoration; two styles are to be observed in the data: LBK and BR, definable on the basis of Fig. 15 as the configurations at the bottom, and at the top, respectively.

[2] Two comments: In Lüning 1975 innovation per time phase is stressed, thereby giving the impression that innovations occur in clusters and that evolution is a jumping affair. While this may have been the case a number of times, it should be recognized that regional (or "specific") evolution is usually gradual, the leaps forward being limiting cases only (Berger 1973: 37); or, even worse, more apparent than real through lumping on an ordinal (i.e., discontinuous) time scale, an analytical artifact: "... time is not a series of categories, it is a continuum" (Plog 1974: 44).

In Van der Waals 1975 continuity is considered "demonstrable as an archaeological reality", whereas discontinuity can be suggested only. However, if continuity can be "demonstrated", and if the opposite case cannot be demonstrated but suggested only, then *neither* can be falsified, and the problem of (dis)continuity is transferred to the metaphysical sphere. If, as proposed here, these terms are defined in relation to one another, in a system, then their implications serve to falsify one another in concrete events.

[3] The resulting sequence is given by so-called factor-scores of the individual finds, of the general form of:

$$s = ax + by + cz + \dots$$

in which a, b, c, ... are constants ("factor coefficients") characteristic of the variables used, and x, y, z, ... the counts of the respective variables as observed in the find under consideration. In the case of a missing value for x, y, z, ... the usual procedure is to enter the mean for that variable, thereby introducing a kind of interpretative noise.

[4] This will hold only if the PC has been defined by means of samples truly representative of the original population, and if the evolution of the mixes has been non-regressive. Because of the rather large number of samples, their aggregate will be very close to such a representativity. Conversely, any single sample or find may differ considerably from the "norm" for its time of

deposition, even when it is still within probability bounds; this is most likely to occur when the sample is small.

[5] As discussed here, Models 1 and 2 are reworked in the next section on the basis of numbers of sherds, instead of pits. The implication is of a constant percentage of wasted and deposited sherds *vis-à-vis* the original population of ceramics. Typographically, this change is indicated by MODEL I and MODEL II written large.

The construction of an image of what these models stand for would start with the so-called "Cook method" (Cohen 1975: 472), which could better have been named "Cook's Principle". According to this principle, the relative frequencies of any single attribute or variable of material culture (as excavated, I presume) are directly proportional to the size of the original population of *homo sp*. While I am aware of the shortcomings of this idea (for a summary cf. Cohen 1975) I think that these apply only to too narrow a one-to-one interpretation of this principle in too wide a field. If it is taken to mean "roughly coinciding with" and if its application is restricted to data which from a general evolutional point of view are homogeneous, nothing much can be said against its use.

In this way, the ordering produced by the PCA for the Hienheim data, taken literally, could be "explained" by assuming a massive immigration followed by a rapid exhaustion of the resources, forcing the main body of the population to march on after about 4 "phases", while a small number of tenants is left behind. Model 1 would then stand for the occupancy of the site by a constant number of people instead. And Model 2 might account for two radically different situations:

– Model 2a: When a small group colonizes an area, expands, and gradually exhausts its resources, then the size of the human population responding to the exploitative pattern will follow a normal curve. This is essentially the model used by Plog (1974: 91-92) in a general discussion of change over prehistoric time. Not considered by him, however, is the following model, which is probably equally, if not more, relevant and in any case more general:

– Model 2b: The normally distributed frequency counts of (an attribute of) some tradition T arise when the same human population has produced a tradition S before T and a tradition U after it. Products S and U do not fit the categories used to classify the products of T. Moreover, S or U may be void because a situation similar to Model 2a obtained.

Whatever translation of the above models of material culture into the demographic/social sphere is concocted, the frequency distribution prescribed by the second model will probably be the best, most realistic one (Plog 1974: 92). Also, the "explanation" of Model 2b has two advantages: it is not necessary to assume the validity of Cook's principle, and it seems to tie in neatly with the present state of theorizing about the LBK – whether the LBK was produced by immigrants, or by Mesolithic autochthones (the local Mesolithic has not been defined as yet); and whether after BR the people moved away or started to produce pottery without decoration.

[6] This method of weighting is rather crude. It has the advantage, however, of being easily performed on a primitive desk calculator. Some of the more sophisticated ways of smoothing are merely more complex developments of the same idea (cf. Clark 1975 plus references there).

[7] An inflection point is that point of the graph where the direction of the curvature changes, convex becoming concave (or reverse); in Fig. 9 this point is half-way between t(j) and t(i), where frequency (p) = frequency (q) = 50%. In the case of the logistic curves in the other figures, it is easily found by dividing the overall change in the mix by 2 and then locating the point where half the change has been run through.

[8] Comparable figures for Hienheim are not available because of differences between the final calculations. For Hienheim, the principal components solution was rotated to a "better" description of the data, which renders meaningless the notion "percentage of the variance explained". However, this new chronological axis was confirmed by a multiple regression analysis: R^2 (MODEL III) = .882; R^2 (MODEL IIIa) = .899. In words: 88.2%, resp. 89.9% of the variance of the 11 traits used in the computation of the ordering is explained by the chronological axes of MODELS III and IIIa. For Elsloo, rotation of the PC's did not produce a better interpretable result, on the contrary: trusted markers of early pottery, such as absence of rim decoration and simple spatula, came to oppose one another. The sequence for Elsloo presented in the text is, therefore, the unrotated solution; for this ordering, in a multiple regression analysis R^2 = .952 has been computed.

[9] Hence, possibly, the customary differentiation of the Unterislinger and Oberlauterbacher ware, which would seem real enough on the basis of *surface collected samples*. However, at the Hienheim site both were found in the very same pits. Of course, this does not rule out separate origins – but these now remain to be demonstrated by means of systematic excavations, not with inventories of hazy collections. For a more specific discussion of the distributions of hatched and of stab-and-drag decorated ware at Hienheim, see p. 163.

POSTSCRIPT

Some time after the above had been written, a vast complex of pits was excavated at the site of Hienheim. The pottery that came out of it is comparatively early for this settlement; a C-14 reading from the fillings of pit no. 1397 (one of this complex) gave 6220 ± 45 bp, 65 years older than any of the dates previously obtained. It was decided to incorporate these fresh data (15 pits, 626 sherds) in my analyses. The following text and accompanying graphs are intended to summarize the new results.

Because of the early nature of the data to be added, the variable "presence or absence of neck decoration" (FORMAT), was also entered into the computations of the chronological ordering; together with TECHNIQUES, COMPONENTS (belly), and STRUCTURE summing 13 traits. To make up for the 61 pits (out of 179) of the site without a sufficient number of rim sherds, an allowance had to be made by inserting the average values of the attributes of this variable (the "computational noise" of note 3, Ch. III). The first, or chronological, principal component accounted for 52.3% of the variance of the variables mentioned (at Elsloo, 40.0%). This time, rotation of the factor structure did not produce a better ordering of the finds (as determined from the factor plot, and from a multiple regression analysis) and is therefore not incorporated in the present computations and results. The distribution of the finds on the chronological axis is summarized in Fig. 22. As has been demonstrated in the main text of this chapter, there is an apparent discontinuity: a large, early cluster of finds is separated by a gap from a smaller, younger cluster, with a few finds occurring haphazardly in the gap. Forty-two finds do not belong to the main cluster, a number exactly equal to that of the finds younger than the discontinuity made visible in Figs. 14 to 16.

Given this result I did not think it necessary to redo the entire analysis of the Sections 5 and 6 in Ch. III. Instead, I will briefly note a change in the positions of some finds and say a little on the checks of the principal components solution proposed in Section 6.

The incorporation of the variable "presence of neck decoration" (i.e., FORMAT) into the computations has resulted in an important re-positioning of at least four finds: 1115 and 1116 are now younger than the discontinuity (which seems better, intuitively), with nos. 0364 and 0648 older now (also intuitively more satisfying). Still, the major conclusion of Ch. III (SSK attributes were clearly present *before* the observed discontinuity, and LBK ones *after* it) also holds good for the new ordering (Fig. 23, which presents the MODEL 1 distribution and counts; the graphs have not been smoothed; cf. Fig. 13).

Regarding the checks proposed earlier, the shape of the several curves largely conforms to the presciptions of Figs. 9 and 11, notwithstanding the fluctuations (this is largely the result of their being unsmoothed).

The radiocarbon datings are in complete agreement with the statistically computed ordering, as far as the reliable ones are concerned (cf. discussion on p. 58; the datings have been entered in Fig. 25):

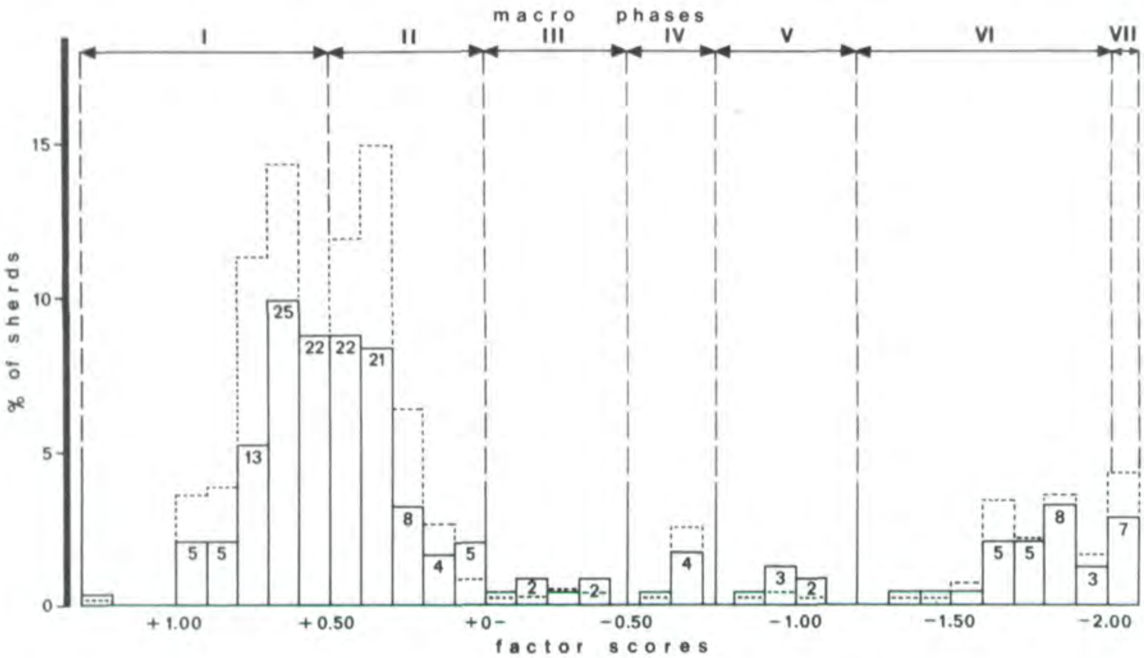


Fig. 22. HIENHEIM: distribution of finds and sherds (stippled) along chronological axis in (grouped) factor scores. 'Older' to the left, 'younger' to the right.
No. of finds indicated at the top of the full-drawn bars.
No. of sherds in percentages on scale to the left.
Division into macro-phases indicated at the top of the figure.

Sequence number	14C date yrs. BP	Find number	Remarks
15	5780 ± 50	0108	
29	5905 ± 45	1115	
69	6000	0620	
78	5910 ± 50	0068	suspect, cf. p. 58
97	6125 ± 35	0414	
82	6155 ± 45	0822	suspect, cf. p. 58
174	6220 ± 45	1397	

On the subject of stratigraphical checks I am able to report agreement now of observed and computed sequence: find no. 0548 has a younger "date" (seq. no. 88) than find no. 0555 (seq. no. 108). The cause of this should perhaps be sought either in the incorporation of the variable FORMAT – resulting in a better instrument – or in the larger number of finds – resulting in a narrowing of the confidence intervals (see discussion on p. 59-60) – or both.

I think that the agreement of the Chapter III analysis with the present one, plus the ironing out of some of the obvious errors of the former here, demonstrate (again) the general validity of the method.

In the sections alluded to, irregularities were observed in the older part of the scale; contraction of that part was proposed as a remedy. It should be (re-)emphasized that the length of the scale or of parts of it is entirely arbitrary: if two finds are found to be very close to one another on the chronological scale, this may legitimately be translated into rankings of, say, 47 and 48. It is quite another thing, however, to make this difference in ranking correspond to, e.g., one millimetre on graph paper

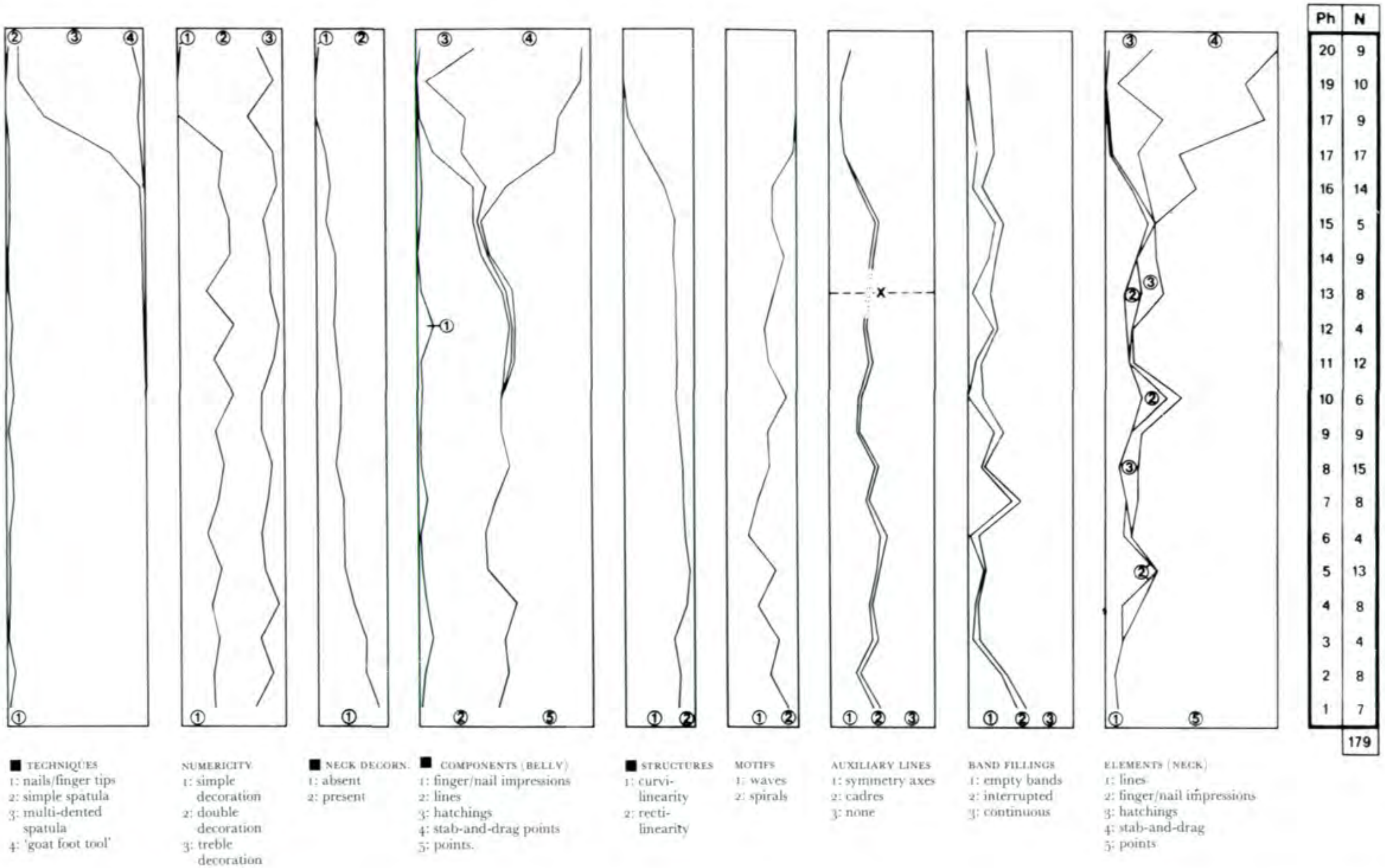


Fig. 23. HIENHEIM: Proportions of various attributes per variable of decoration over time. Phases comprise approximately equal number of sherds (MODEL I), ordered chronologically by means of a principal components analysis of the variables marked with ■: top-youngest phase, bottom-oldest phase (cf. Fig. 13).

N of pits: number of finds in which the sherds were collected.

Column width: 100% each.

– for is this difference equal to the difference between the rankings 42 and 43?

To illustrate this, a multiple regression analysis was run on the ordering as derived from the PCA, with equal differences in ranking given equal meaning. In Fig. 24 the results have been assembled: horizontally the mathematically best approximation of that ordering, and vertically the differences of approximated and input ordering. The distribution of the points (each representing one find) is of course very much similar to that of Fig. 22 – yet here 73.7% of the variance of the four variables has been "explained" (the MODEL III ordering in Ch. III accounted for 88.2% of the variance, and the ordering produced for the Elsloo data 95.2%; these figures are not strictly comparable to those in the Postscript, as the former ones relate to groups of finds/phases and the latter ones to the individual finds and rankings).

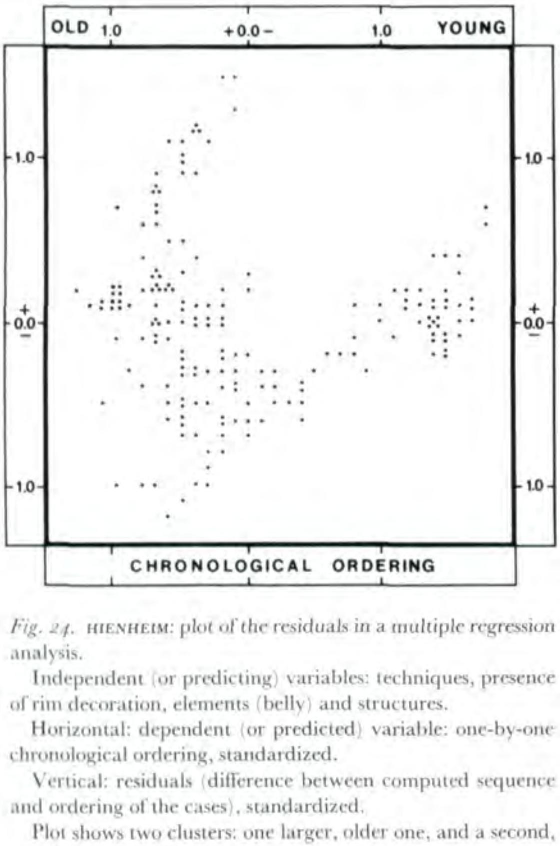


Fig. 24. HIENHEIM: plot of the residuals in a multiple regression analysis.

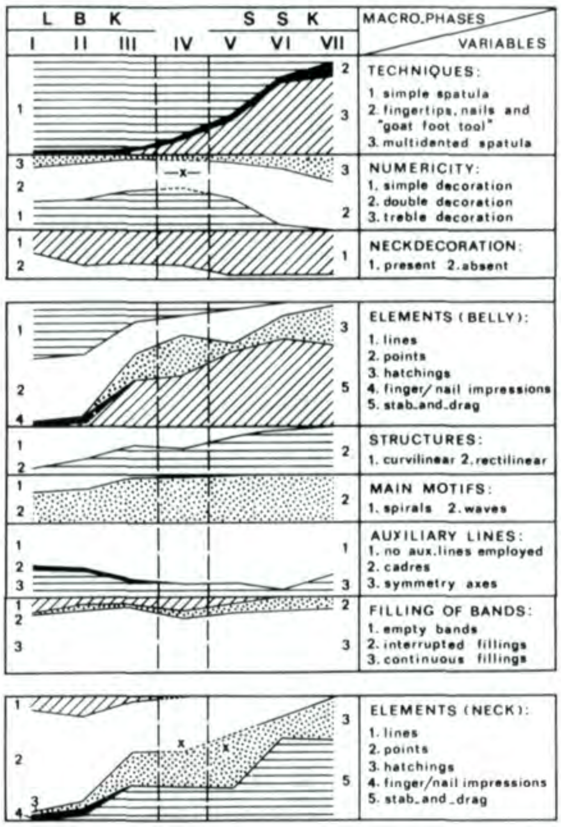
Independent (or predicting) variables: techniques, presence of rim decoration, elements (belly) and structures.

Horizontal: dependent (or predicted) variable: one-by-one chronological ordering, standardized.

Vertical: residuals (difference between computed sequence and ordering of the cases), standardized.

Plot shows two clusters: one larger, older one, and a second, smaller and younger one.

Finally, in Fig. 25 the counts of the several attributes are depicted in a diagram with seven chronological phases. These phases are those sug-



2610	2271	76	150	60	738	233	NR. OF SHERDS/PHASE
71	60	6	5	6	26	5	NR. OF FINDS/PHASE
6220 ±45	6125 ±35				5905 ±45	5780 ±50	C14 dates b.p.

Fig. 25. HIENHEIM: proportions of attributes of ceramic decoration per MACRO-PHASE, as 'defined' by discontinuities in the factor scores of the finds. Therefore, the discontinuity derived in the text of Ch. III has not been emphasized; it coincides with the dotted line separating off PHASES IV/III; PHASE IV is to be regarded as a transition from the LBK to the SSK at Hienheim (cf. Fig. 16). × = insufficient data.

gested by the looks of Figs. 22 and 24: the clusters visible there have been retained here, only split up to make the developments better visible, just as the very thinly spread finds in the gap between "LBK" and "SSK" have been kept thinly spread for the same purpose. After much computational trouble the final distribution in Chapter III of the Hienheim data was much like the one produced initially by the PCA, so it seems pointless to go through that cycle again. In this way the discontinuity in the local development is caught in the transitional phase IV, and an emphasizing of the rupture as in Fig. 16 was therefore not thought necessary.

In Table 78 the chronological ordering as derived in this Postscript is presented.

CHAPTER IV

BANDKERAMIK SOCIAL STRUCTURE: A PILOT STUDY

In the first section of this chapter the fundamental distinction between "participants'" models and "observer's" models is discussed, and the latter are subdivided into "statistical" and "mechanical" models. To arrive at an image of Bandkeramik social life, it is then stated, the path should go from theory through mechanical models to the statistical description. Irrevocably, the participants' views on the subject have been lost. On the assumption that a graveyard presents a clearer record of the social organization behind it than settlement debris does, the four other sections of this chapter are devoted to an analysis of the grave goods in the Elsloo Cemetery. A short general description in the second section precedes three models of LBK social structure: a study of the hierarchy of statuses (Section 3), of the relations between these (Section 4); and Section 5 a more dynamic model is presented, which attempts to transcend the other two in linking sets of relations to sets of social positions. In the next chapter, these models will be tested and amended: the statistical descriptions of Bandkeramik social structure.

1. *Introduction: prescriptions and descriptions*

Reality normally seems to be a complex affair with several different levels: a "pure" level, where laws or rules are universally valid, where "pure" and "impure" are forever separate; and an "impure" or practical level, where even the "pure" Brahmin is necessarily soiled by the impure facts of life (Dumont 1970: 80-87). The first level is perhaps nothing but a construct, "good to think in"; its consistent construction renders it an ideal playground for the intellect. The second level, closer to the facts, is that of the soiled rules: in the Hindu case, the castes are fully dependent one upon the other, if only to neutralize impurity (Dumont 1964: 16) and this side of reality may differ considerably from the ideals of the first level.

The description of social reality (and not just that of traditional Hindu society alone) may be quite different from the organizing principles which govern it, although the latter do have as much reality value as, say, a grammar (Hawkes 1977: 39). The description is called a "statistical model" of the social reality, and the grammatical rules behind it form the "mechanical model" of that world (Lévi-Strauss 1967: 275; Guillaumin 1968).

Together, statistical and mechanical models are called (external) observer's models, because they are not usually recognized, known by the agents (Lévi-Strauss 1967: 273) – although this is not always so: the manipulation with supply and demand effects on prices immediately comes to mind. Several other levels exist too: the members of the group under observation will also have ideas about principles according to which they behave – the "participants' view" (De Josselin de Jong 1956: 149, 157; also cf. De Josselin de Jong 1971). The "observer's"/"participants'" dichotomy is parallel to the "etic"/"emic" distinction discussed in Chapter II.

For instance, during my anthropological fieldwork training in southern Spain (Van de Velde 1971), the peasants told me that they were all equals (if pressed, they would add: "small fry, that is": the participants' view of the Little Tradition in that very hierarchical society. However, some were apparently more equal than their brethren: 78.5% of the holdings was smaller than 10 hectares, whereas 4.2% of the farms were more than 100 hectares in extent; similary, 1% of those who where officially registered as "agriculturally active" never touched a hoe, 74% drew their main income from agriculture, and the remainder had to work additionally in town to be able to subsist: the statistical model. Then they were quite equal since all of them were supportors of one patron or another (and insofar as they had no political influence), but their access to the factors of production was, however, for an important part determined by the centuries-old relations of clientship of their lineages to that of the the local Count (and although the latter had sold his property a few years ago to the peasants, no substantial redistribution had occurred as yet). This is a very sketchy outline of a mechanical model of the hidden constraints that shaped the life of the peasants.

From the above, very loose explication (which I prefer to Lévi-Strauss' deeply obscure definitions by means of the different "scale" of the models relative to the phenomena), it may be taken that in a theoretical consideration mechanical models are more appropriate than statistical ones (cf. Ch. I, Section 2) Especially in a case like the present one, where the outline of a social structure is sought about which practically nothing is known, a mechanical model might provide a structuring of the data that might be hidden in the statistical appearances. Indeed, the very fact that only vague notions exist about Bandkeramik social structure (Ch. 5, Section 2) can be interpreted such that the data – of whatever kind – are ambiguous or confused, that the statistical model is illegible. This opacity may be due either to a truly random distribution of the phenomena resulting from an (original) absence of structuring rules, or it may be rooted in ignorance of what to look for: data do not speak for themselves. *A priori*, the latter possibility should be the case: social life without rules is impossible.

If something like this is the case, the best strategy seems to be to develop a set of alternative hypotheses to be tested against the data (see above, pp. 2-3, 33 and 34). The hypotheses should be derived from theory, and in an intensive study of a limited set of material the relevant part of the theory is selected. Such a "pilot study" is aimed at the construction of the "mechanical model", in the sense of a set of consistent rules or prescriptions from which hypotheses (in this case on social structure) and implications for the main body of the data can be derived (Lévi-Strauss 1967: 281). A testing of these will result not only in a corroboration or falsification of the hypotheses (and thus of the mechanical model), but also in a description of the degree to which reality conformed to the mechanical pattern: the statistical model.

There are two reasons to select a graveyard for a pilot study. A practical one is that the data set is neither too bulky (at least in relation to, say, a village) nor too restricted (as is a hut in comparison). A more theoretical reasons is that in a graveyard social structure has probably been laid to rest more in accordance with the rules of that time. On a settlement site, the mundane affairs of daily life will have compromised the "pure" rules, whereas the graveyard is set apart. Comparative studies (Van Gennep 1977: 146-165; Binford 1972: 208-243) have shown that the world beyond the grave is generally organized like that of the living; specifically the status of the deceased tends to be similar in both worlds. Also, the corpse is furnished with everything necessary to make a safe crossing to the afterworld "as they would a living traveller" (Van Gennep 1977: 154); the funeral ritual is intended to integrate the deceased in his new abode (Van Gennep 1977: 147).

Among the Bandkeramik graveyeards published, that of Elsloo in the southern Netherlands stands out: it is one of the largest LBK cemeteries known (Pavúk 1972b: 92) and the accompanying settlement has been excavated and data on it published (Modderman 1970). Thus if it were possible to produce a mechanical model of the LBK social structure on the basis of the graveyard data, then it could be checked in the village. From this intermediary, the step to Hienheim is much less risky than one which moves directly from the Dutch graveyard to the Bavarian village.

This is not the place for an exhaustive exposé of the various notions that go under the heading "social structure"; several textbooks are available for that purpose (e.g. Harris 1968; Lévi-Strauss 1967, 1969b, 1973; Service 1971)(to name a few protagonists of different schools). Yet a minimum introduction should serve to structure the accounts in this and the next chapter; the notions employed in the analysis of the Elsloo cemetery are further specified at the beginnings of the other sections of the present chapter.

To describe the social aspects of a human body collective, many means have been devised which in due time have become approaches, even paradigms, as understood by Kuhn (especially in their "shared examples" meaning: Kuhn 1970: 187-191), and the analyses from the various schools are hardly comparable, even when relating to the same social entity. Perhaps this joint existence of different meanings of "social structure" in social anthropology has been the major obstacle to archaeological thinking about it.

For want of better terms, two of the major approaches can be indicated concisely as "positional" and "relational". *Both* notions can be referred to Radcliffe-Brown's precise definition: "a structural system is . . . the network of relations connecting the inhabitants *of a region* amongst themselves and with people of ther regions" (Radcliffe-Brown 1952: 193). Indeed, this definition was explicitly designed to unite both viewpoints. By that time, however, the original differences in emphasis had already evolved to marked biases (Harris 1968: 598).

The "positional" concept of social structure (the first approach to be used here) focuses on the various statuses (meaning positions in a social network) that may be discerned in an analysis of the social network of a group; the statuses may be ranked, or rather differentiated according to some ordering principle such as wealth, sex, age, lineage affiliation, etc. Especially in its hierarchic focus, the positional social structure concept easily lends itself to archaeological operationalization. Differences in grave goods are customarily interpreted this way – explicitly or implicitly assuming wealth as ranking principle (e.g., Modderman 1970: 67; Randsborg 1974: 51-52). There is a drawback, however: egalitarian society (Service 1971: 103) – primitive agriculturalists, hunters and gatherers – does not allow marked wealth differences.[1] By their very organization any substantial difference that might arise is immediately appropriated by the entire group, the collective body. This is not to deny the existence of status differences in egalitarian community society; on the contrary. In every society, statuses are at least tied to sex and age. In egalitarian society, however, the social network is mainly based upon these factors (cf. however, Claessen and Kloos 1978: 78-82). A number of archaeologists can be quoted who employ more complex status determinants, while retaining the basic age-plus-sex differentiation (e.g., Hodson 1977: 397, 406; Pavúk 1972b: 97). Yet, except as a preliminary impression, a scanning of the outline of the social structure, the "positional" description of early agricultural communities (and of hunters and gatherers) is hardly illuminating. I will come back to this approach in the third section to present it in a less sketchy manner and to illustrate it in an analysis of the gifts to the deceased at Elsloo.

The "relational" notion of social structure might (at least in principle) provide better insight in the community case: egalitarian society is usually presented as a somewhat unstable amalgam of kin groups. Roughly, differences and relations between the groups are mainly concerned with access to women, i.e., kinship. Consisting of relatively small populations – typically, some 50 to 200 people, 500 at maximum (Birdsell 1973) – demographic fluctuations are clearly visible. Therefore, women are always in demand and in short supply: at the Neolithic level of technological development the only feasible specialization is that of a division of labour along the sex-line (cf. Begler 1978; Leacock 1978; Siskind 1978). In other words, women are likely to be the critical link in the society's system of social, economic, and biological production and reproduction, as opposed to all other factors of production which are virtually inexhaustible (Sahlins 1972: 13, 79). In more complicated societies the other factors of production are in short supply as well, due to, among other things, the larger scale of the procurement systems. There, the relations between the groups are concerned with access to all of these scarce goods, i.e., economics. Parallel to the wielding of economic power in these more advanced social formations stands the manipulation of kinship in primitive economies.

No longer part of familistic structures, archaeologists with their urbanistic background find it very difficult to drop wealth differentials as a classificatory or descriptive principle. And although some might be persuaded of the validity of the previous paragraph, it should be admitted that an archaeological operationalization of, e.g. MBD (read: mother's brother's daughter) as a preferential marriage partner (to call to mind but one of the simpler kinship theory's elementary notions) looks impossible at first sight.

The "positional" and "relational" notions of social structure are not necessarily confined to a within-settlment analysis. The segments (kin groups) of segmental society (hunters and gatherers; primitive agriculturalists) seem to be freely permutable: community society is in fact a society without limits (Service 1975: 65; Fried 1975: 85). If any pressure upon resources makes itself felt, the bonds of alliance with coresidential groups do not interfere with a shift of location. Huts may be set up elsewhere where probably alliance bonds already existed, to alleviate the shortage or to spread the risks (e.g., Bloch 1975b: 215). In more evolved systems, with their economically differentiated and hierarchically ordered groups, settlements may also be ranked according to social distance from the centre of power: modern Hienheim is politically and economically less important than Kelheim, than Regensburg or Landshut, than Munich.

The above distinction between, and almost oppostion of "positional" and "relational" notions of social structure has been described repeatedly. In what is probably the widest read anthropological text in archaeological circles (cf. Van de Velde 1978) Service has very clearly pointed to it (Service 1971: 10-11; an earlier edition was printed in 1962). He uses the labels "*network of statuses*" and "*social structure*", however, to designate the two biases, and offers as a less precise and more general term "*social organization*" to cover both.

Nonetheless, in that way we are left with a terminological difficulty (not faced by Service). The two rather opposed ideas about social organization indicated above are both contained within Radcliffe-Brown's "structural system" concept quoted previously. But as it is fairly close to empirical reality, the latter may be contrasted with "structure" as employed by structuralists, the second approach here. This is a more abstract model of social relations "having nothing to do with empirical reality . . . but built up after reality" (Lévi-Strauss 1967: 271). Excepting Leroi-Gourhan (1965: 74, 88, 105, 111, etc. and 1976; perhaps Laming-Emperaire 1962 must be incorporated as well), who used the contents but not the label, "structure" has not been employed by archaeologists in this meaning, as far as I know (also cf. ethnologist Edmund Leach's sour concluding address to the archaeological symposion on culture change: Leach 1971). Briefly, structuralists hold that relations in various aspects of culture

("Worlds" as Popper 1972: 106 would say) tend to be arranged in analogous ways. From such a structure, alternate possibilities ("transformations") can be derived, which poses the problem: why is the structure implemented the way it is, and why not in an alternative way? (For example, Fig. 5, where the Bandkeramik motif system is shown to be reducible to a limited set of transforms: to all symmetry operations and only one movement. One alternative, the glide, has not been included in their decorative practice. Why this is so should be answered in a study of their symbolic system.) Every aspect of culture will show its own elaborations of that structure or arrangement, and the abstraction of the latter from reality goes beyond appearances.

Therefore, this structuralistic concept of social structure ("social" because it has to do with group behaviour) might be of considerable use for archaeologists, as it surpasses the ethnographic or archaeographic detail which so often effectively hides what has gone on in reality (Rowlands and Gledhill 1977: 146; Van de Velde 1979 reply). Especially analyses of primitive societies may benefit from the theory that has been developed so far by, among others, Lévi-Strauss (specifically 1967, 1969a, 1969b, 1975) and other French structuralists, and by the Leiden School (e.g., De Josselin de Jong 1977b). Yet, the method has a practical drawback for archaeologists: more complex societies, with less homogeneous arrangements (e.g., stratified, or Iron Age, societies, where specializtion of social functions may complicate matters considerably) have been studied less comprehensively, and there is much less "grand" theory available *at present*. Still, parts of the models developed for ranking society should be of use in the (archaeological) study of more advanced communities, as may be expected from the successes of, e.g., Indonesian and Indian ethnology (Van Wouden 1935; De Josselin de Jong 1951; Dumont 1966).

In the fourth section I will present a piece of structural theory, derive some models from it, and subsequently match these with the data from the Elsloo cemetery.

Having described some of the positions in the social structure and having probed into the relations characteristic to it, one may still ask: why these relations, why these positions – which leads to the third approach attempted here: structuralist-Marxist (or neo-Marxist, in other corners). In short, this line of enquiry proceeds from the idea that various distinct types of productive relations are present in almost any "social formation" (very loosely, the set of relations relevant to some problem; in this case, the problem is the social relations within a village). The "productive relations" constitute the social aspect of a way of producing and reproducing; together with the "productive forces" (the way in which work transforms nature) the corresponding productive relations make up a so-called mode of production. It goes without saying that the productive process requires tools, or more general, determinate technological conditions (Godelier 1978: 763). Also, a mode of production brings its own rationalization, mythic embodiment, or "ideology" with it. Production, and most certainly so in community society, is an effect of individual but socially recognized and organized human labour. However, not all social relations are equivalent to relations of production; it is rather the type of social relations (the way in which surplus-labour is appropriated and its effects distributed) which is important (Godelier 1978: 764). Thus, the types of productive relations (and of the forces) distinguish one mode of production from the other, and any individual may simultaneously take part in different modes in his social formation. For example, a peasant with a small holding may be employed in wage labour in a manufacturing firm in town (which may be organized along capitalistic lines), while at the same time running his own holding in a "domestic" way; the productive relations are markedly different in both cases, as are the ways in which the peasant "acts upon nature", the productive forces.

It should be emphasized that "modes of production" are used as theoretical constructs, and mainly for heuristic purposes: what light can they shed on the data at hand? (cf. O'Laughlin 1975: 367-368). Perhaps reanalysis of the present data would merge a number of them. A major drawback of this approach as intended here is that there is no general theory from which to derive all possible modes: every mode of production is historically specific. The main reason for my introducing this concept here lies in the dynamic quality of it: both positional and structural analysis end in a static picture. From the analysis below, along the last path, a rapid change will become apparent in the social formation fossilized in the Elsloo graveyard. In the final, fifth section of this chapter I will attempt to detail the theoretical ideas behind this approach a little more, seeking to avoid exegetics and polemics.

2. *The cemetery at Elsloo: introduction*

Most likely, the Elsloo cemetery belongs to the nearby settlement, 50 metres to the south-southeast of it, which has been excavated and data concerning it has been published simultaneously (Modderman 1970). The graveyard has been excavated only partially: all around is a built-up area; to the northeast the digging of cellars has brought to light "ancient pots", while no settlement stood there. In the other directions, particularly to the west, the limits of the graveyard seem to have been reached by the excavations. Additionally, some cremations may have been ploughed up; interments of burnt bones were always found at a shallow depth (C.C. Bakels, pers. comm.; P.J.R. Modderman, pers. comm.). Perhaps not too many graves have been lost, however: Modderman (1970(I): 205) thinks some twelve graves. If this is true, the picture presented by the excavated graves will not be too much distorted, compared with the original.

The part of the cemetry that has been excavated belongs to the phases 2c and 2d of the Dutch Bandkeramik, i.e., the younger half of the village's inhabitation, perhaps about one hundred years (Modderman 1970(I): 206-207: 75 to 150 years; as I am not concerned with establishing the size of the population, the exact time span is relatively unimportant, the scale, three to five generations, suffices for present purposes). Neither the cemetery nor even a single grave pertaining to the earlier phases of the settlement has been located as yet.

A summary account of Modderman's data runs:

- 113 graves were excavated, 47 of which cremations (Table 9).
- 29 graves did not contain any grave goods any more; 58 graves contained pots or sherds (36 graves held decorated ware); in 33 graves adzes were found, in 13 arrowheads, in 15 flint blades, and in 15 grinding stones; in 42 charcoal (9 graves had only charcoal), and in 26 lumps and/or traces of red ochre were found.
- 59 inhumations (classified as "inhumation" are those graves which did contain vestiges of the corpse, either tooth enamel or a shadow; and those pits which had the same size and shape as these) were aligned to a NW-SE axis, with 4 at right angles to this trend; for 3 graves the orientation could not be ascertained due to the rather round shape of the pit.
- in 22 graves corpse shadows had preserved the posture of the deceased: 13 had their heads to the SE, 9 to the NW; 14 were lying on their left, 2 on their right sides, and 1 probably on his back.

According to Modderman (1970: 66, 67, 68, 71) the Elsloo cemetery is very much like the other known graveyards of the LBK, even in the deviations from the modes. Even so, there are initially three major obstacles on the road towards an anthropological interpretation: chronology (or what was contemporary with what?), and, since the skeletons have completely disappeared, sex and age (as major determinants of status in ranked society; cf. p. 81). To tackle these problems and as a preliminary for the entire analysis, I devised a codebook which took account of all preserved grave gifts in the following way:

1. The pottery was treated according to an abbreviated version of the coding instructions developed in Ch. I, Sections 4 and 5.

2. The other VARIABLES (*attributes*) were:
 UNDECORATED WARE (*pots, sherds*)
 RED OCHRE (*lumps, traces*)
 CHARCOAL (*lumps, traces*)
 ARROWHEADS (flint) (*uni-, bifacially worked*)
 SCRAPERS (flint)
 BLADES (flint) (*blanks, used*)
 ROUGHS (flint) (*nuclei, debris*)
 QUERNS (stone) (*complete, fragments*)
 ADZES (stone) (types I; II; III; and IV to VI)

3. Also, general qualitative characteristics were entered: geographical position, burial rite (cremation, inhumation; with or without remaining grave gifts), orientation and posture.

The codebook is presented in the appendix to this chapter; the attributes appearing in Table 10 are those that occurred in at least five graves and were used in the principal components analysis.

I have dealt at length with the establishment of a chronology in the previous chapter, so there would be little merit in recapitulating that discussion. It should suffice to say that a chronological scale could be computed from the variables TECHNIQUES, COMPONENTS (of belly decoration), and presence–absence of NECK DECORATION or ZONING; (principal component 1 in Table 10; for the chronological ordering, the unrotated solution is preferred; cf. Tables 11 and 12; and Ch. 3, note 8). The uneven spread of the graves on this axis suggests a grouping in five phases. Another three graves, containing type I adzes (but no decorated ceramics) could be

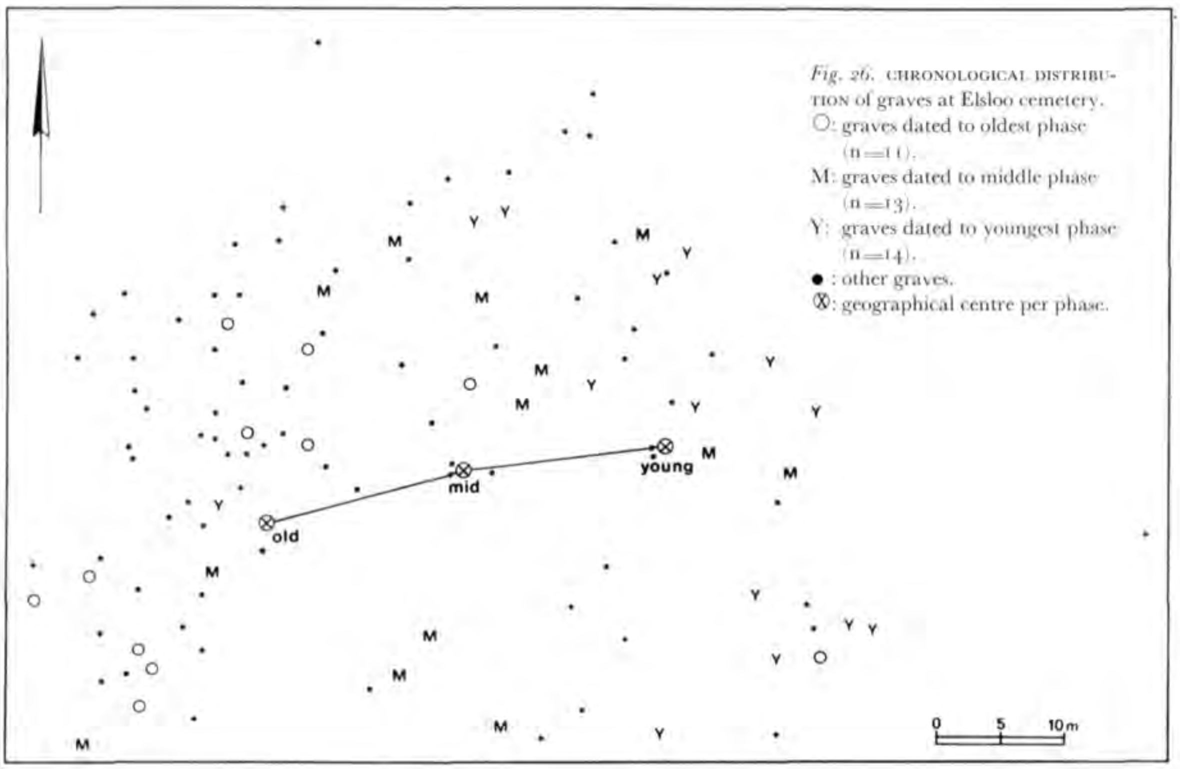


Fig. 26. CHRONOLOGICAL DISTRIBUTION of graves at Elsloo cemetery.
O: graves dated to oldest phase (n=11).
M: graves dated to middle phase (n=13).
Y: graves dated to youngest phase (n=14).
●: other graves.
⊗: geographical centre per phase.

attributed to the fourth or fifth phases: elsewhere in the cemetery, graves with both decorated ware and type I adzes were dated to these youngest two phases (which confirms Modderman 1970: 188); the graves containing these adzes and/or the young pottery were selected by the sixth principal component; also cf. p. 115-116 below.

Because of the small number of graves involved (35 only) I prefer a more condensed division into three phases: Table 13 renders the merging. Then, in Fig. 26 the shifting of the geographical points of gravity of the phases can be made out, which conforms to the general west-east trend noted by the excavator (Modderman 1970: 73). The actual distribution of the graves per phase is quite diffuse, of course: some phase I graves do occur in the northeasterly part of the field, as do phase III graves in the West. [2]

After this clearance of the diachronical problem, we now turn to a consideration of the social structures possibly present in the attributes of the deceased in the Elsloo cemetery, first and foremost among which being sex and age.

3. *The Elsloo graveyard: a positional approach*

In the opening section of this chapter, I indicated that a positional approach to the social structure of an Early Neolithic community is relatively unrewarding, as only few different statuses are likely to have been present here. Also, the status–positional concept is a tricky one (Harris 1968: 394): on the one hand, social structure may be defined as the totality of positions differentiated by those participating in the network, on the other hand as the set of positions horizontally and vertically differentiable in the economic, socio-political and ideational subsystems – definitions of an "emic", and an "etic" type, respectively (cf. Ch. II), or "participants'" and "observers'" as in chapter IV, Section 1. Generally, the two do not coincide; the people may assign the highest status in the nation to the prime minister, whereas in reality the highest status (as defined by access to scarce things; Dahrendorf 1968: 36) is occupied by the president(s) of some transnational corporation(s). The diminutive differentiation of the social structure to be expected at the level of community society allows us to note the theoretical difference and then to neglect if further in practice. We will proceed "as if" the rewards or attributes relate to the same notion of social structure.

Usually, discrimination of social positions is on two dimensions: differentiation and ranking. "Differentiation" refers to the horizontal subdivision of positions of equal rank (Dahrendorf 1968: 19), and "ranking" to a graded and hierarchical scale of levels of like statuses (Sahlins 1958: 240).

"Ranking", when used in an attributive sense, should be differentiated on the one hand from "egalitarian", and on the other from "stratified". The latter term has to do with more or less *closed* groups in relation to one another within a society, having differential access to the factors of production; the former relates to (groups of) *open* positions, to be filled eventually and in principle by every member of the community. Ranking, finally, refers to social systems where no differential access to the factors of production exists, but where some of the statuses are available in limited quantities only – that is, not to everybody (e.g., in the magical or ceremonial sphere, family heads, etc.) (Fried 1967: 33, 109; Cancian 1976).

As aspects of a framework, ranking and differentiation are on the *structural* side; the *functional* aspect can be analysed according to economical, sociopolitical, and ideological criteria (Sahlins 1958: 241; Runciman 1966: 46). Without prejudging the analysis, it is possible to simplify the discussion considerably by noting that economic and social functions are being taken care of by one and the same structure in community society: by the kinship system (Sahlins 1968: 74; Claessen and Kloos 1978: 89, 90; also cf. p. 109 below). Ideology and associated matters like ceremonial functions seem to be more difficult to perceive – although to a large extent these will also be tied down to kinship positions.

Within such a totalizing kinship system the major distinctions are related to age and sex (Sahlins 1958: 239), with sex a differentiating, and age a ranking agent. Functionally, sex roughly equates with division of labour, and age with distribution of products – given a primitive agricultural economy (Sahlins 1968: 75-78; cf. Terray 1975). We should be careful, however, not to take too simple (or Utopian?) a view of kinship society, as some segments of kin system may have been in a more favourite position vis-à-vis the desired things of life than the remainder; it is also conceivable that particular individuals have been successful in manipulating their relations and have turned influence into tangibles. Another possibility is a hereditary higher status for specified members of a minor line in a lineage (e.g., the eldest son born to the eldest . . . born to the lineage founder) going by the name "chief", versus a "big man" (Sahlins 1968: 64, 88-90; Claessen and Kloos 1978: 81-84). As both situations are not isolated cases but reflect systemic properties of different social formations, in any sample a number of cases is likely to be found if the system was of either of these types. It is this additional aspect which is meant by the terminological differentiation of "ranking" from "egalitarian" society; in the latter, statuses are defined by sex and age *only*, and are open to everybody.

Given the initial difficulty that in the Elsloo cemetery no skeletons have been preserved to aid in the determination of sex and age, operationalization of these attributes will be a complicated affair. Therefore, I would rather develop these operationalizations along the way than at this moment.

Since the skeletons have completely vanished, any attempt at determining the sex of the deceased should be based on the grave gifts and not on biometrics. This is no less certain a procedure, however: Shennan (1975: 282) observes that in series where sex is known, in the long run skeletons are by 12% more likely to be wrongly classified as male by these methods. Also, of course, socially recognized sex need not correspondend to biological determinants (e.g., berdache or institutionalized travesty; Häusler 1966: 53-55).

Provided the *sexes* did receive discriminating gifts, it does not seem unreasonable to expect from a principal components analysis (as anounced previously) a *specific* component[3] for each of them, as feminity and masculinity may be expressed by at least partially independent patterns of associations of grave gifts. Regarding these associations, the gifts are at first sight primarily use-valuables; therefore, the more specialization or division of labour to sex, the more independent the patterns will be. In other words, graves not manifestly "male" are not automatically "female" and *vice versa*.

Such a sociological sexing operation may start from the traditional notion that arrowheads are markers of male interments (e.g., Modderman 1970 (I): 67). A cautionary note: even if in all ethnographically known societies hunting is a male activity, there is also the ancient – and unproven – myth of the Amazons; though plausible, the arrowheads-males connection is hypothetical only. And only when on this basis a consistent pattern emerges, may the hypothesis then be considered applicable to the present data set. (For female graves no such label exists.)

Among the entire set of 113 graves, only thirteen contain arrowheads; if some very strange demographic phenomenon is not to be assumed, there should be more male graves, perhaps indicated by other artifacts. Thus, on principal component no. 5, the component correlating best with arrowheads, several other attributes are also loading high: undecorated pots, decorated sherds, blades and type III adzes (cf. Table 10). If it can be assumed because of this association that these are all atributes of masculinity, the twenty graves singled out by this component are labeled "male" interments on this ground. (In this early part of the analysis I take only those graves that score highest, i.e., those marked "A" in Table 11.)

To identify female graves, then, a set of auxiliary hypotheses has to be introduced. Suppose that at least one female activity (because of the supposed *practical* nature of the gifts) has existed which is reflected in the grave goods (like the arrowheads indicating a male activity), then the tool kit for that activity should load high on some principal component (as did arrowheads on the fifth component), and *also be more or less neutral on principal component no. 5* (similar to arrowheads on the other principal components), as do querns, red ochre, and adzes of types I, and IV to VI on the fourth component. This *specific* principal component marks 27 graves – six of which were also indicated by principal component 5 as males. If these six are considered falsifying examples (which they would be only if full-time specialization for either of the sexes were assumed), then there are 35 noncontradictory cases: $20 - 6 = 14$ on component no. 5, plus $27 - 6 = 21$ on the fourth component.

When the remaining principal components have been interpreted as below, the seventh is left; its main contributors are undecorated ware and charcoal. For two reaons I consider this component also an indicator of maleness. First, the pattern of loadings is more or less similar to that of principal component no. 5 (cf. raw counts of earthware, flint tools), while at the same time the more important attributes (in a quantitative sense) of principal component no. 4 are neutral here, too (see red ochre, grinding stones). Second, when in an earlier analysis two types of arrowheads were distinguished, one of them loaded high on the fifth principal component, the other on no. 7, with diffuse (low) correlations on the other principal components; from this I infer that the moderate value for arrowheads on this component is more significant than similar loadings on the other ones. Among the nineteen graves selected by this component, seven cases have high scores on both component no. 5 and no. 7 (confirmatory), four are contradictory (with high scores on the "female" component no. 4 as well as on no. 7), and ten nonfalsifying, in that they were not pinpointed by component no. 4 nor no. 5. A summary of the various distributions of the graves on these components is presented in Table 14.

On the "male" interpretation of principal component no. 7, and a weighting of the individual cases according to their scores on the respective components, a set of 30 male, 17 female, and 1 ambiguous graves can be discerned; the remainder, 65 graves, are indeterminable because of the scantiness or absence of grave goods.

The low number of sexually distinguishable graves is quite unsatisfactory, though. Extension of the above to graves scoring moderately on the relevant components might bring in more of them, but on dubious grounds: irrelevant variables contribute to the scores also, and as yet we do not know how to disentangle these from the relevant ones (relevant being relevant to the sexing operations; cf. note 3). Especially in a case like this, where the analysis is on qualitatively different attributes, it seems advisable to use principal components analysis as a pattern-finding device, but nothing more (in the previous chapter, on chronology, calculations were based upon a small and specified selection of variables of ceramic decoration only; there, the principal components analysis was used as a much more precise instrument). Therefore, the inventories of the graves, the sex of which was indicated by the principal components, were written down, omitting those categories of grave gifts which occur in less than ten graves (traces of red ochre, scrapers, flint debris, adzes of types I and II). This was summed, with results as presented in Table 15.

Two markers for each of the sexes appear: females are apparently labeled by red ochre and querns, and males by adzes of type III and undecorated vessels; the other categories (among which, here, arrowheads are most conspicuous) seem to be more or less evenly distributed among the sexes.

On this argument, two of the graves labeled "male" by the principal components analysis should be grouped with the females; one "female" and one "male" grave also contain one attribute of the opposite sex, along with the two proper sex-specific characteristics. Thus, $n(F) = 17 + 2 = 19$, and $n(M) = 30 - 2 = 28$.

With the two sets of markers I turned again to the listing of the graves' inventories. Some of the graves initially incorporated disappear, and others join the lines: 21 graves remain to either sex. The results are similar to those in Table 15 except that arrowheads are more asymmetrically distributed – though still not very significantly: with Chi-square of 2.00, p is c. .16 (Table 16).

To correct for this curious and unexpected behaviour of the arrowheads, the procedure was amended. (After all, a principal components analysis is but a crude tool.) On the initial assumption that these artifacts are markers of masculinity, the five graves with arrowheads still not entered were grouped with the other "male" interments. As they did not contain any counterevidence of supposedly female gifts, this should pose no problem (except grave no. 087, which was already dubious in the principal components analysis; among its contents an undecorated pot and perhaps one single arrowhead as well might indicate a male interment, while a quern and a lump of red ochre seem to be very clear markers of feminity – cp. the p-values in Table 16). With these extra arrowhead-marked graves grouped with the other males, Table 17 is the result.[4]

Whatever one may think of this procedure, there is a clear dichotomy visible in the Chi-square values, and arrowheads now definitely belong to the marker-categories. Thus:

– *female graves* contain querns and/or red ochre;

– *male graves* contain undecorated vessels and/or adzes of type III and/or arrowheads.

The other categories are not specific to sex, and may be considered "general". In this way, a grave inventory consists of a selection of artifacts from the "general" set of categories, plus one or two specimens drawn from the sex-specific group. Yet there remain some controversial inventories to be noted:

– the ambigious grave no. 087 (which I am inclined to think of as a rich female);

– two female graves containing one arrowhead each (nos. 014 and 106. This latter female has apparently been *killed* by the arrowhead, though, as it was sticking into her skull (see Pl. 160 in Modderman 1970 (II)). As no shadow of the skull of the former woman has survived, no conclusion can be drawn in her case, although the position of the arrowhead is suggestive (here too see Pl. 128 of the same volume);

– and another female grave (no. 105) holding an undecorated pot;

– conversely, two male graves held lumps of red ochre (nos. 001 and 003).

A corroboration (not a proof!) of the above differentiation according to sex of the grave gifts at Elsloo is found in Reinecke's summary of the Lesser Bavarian LBK; in his account of the graveyard at Aiterhofen (Reinecke 1978: 12), where the skeletons have been preserved, and where their sex was biometrically established, he lists the categories of grave goods going with either of them. Male skeletons were accompanied by arrowheads, (unspecified) pots, (unspecified) adzes, and graphite (plus spondylus shell ornaments); female interments held (unspecified) pots, querns, red ochre (plus river shell adornments and bone awls); earthenware and blades were found with both sexes (also cf. Osterhaus 1975).

As a sideline here, in the fifth section of this chapter, on indirect evidence I will push the determination of the sexes to 38 male graves (here, 25 have been established) and 33 female ones (up till now, 21), with a resultant sex-ratio of .46 (as here). Häusler, in his survey of LBK graveyards (Häusler 1966: 46) suggests an overall sex-ratio of ca. .50. At the (younger) LBK cemetery at Nitra, Pavúk found a ratio of .40, which he considered

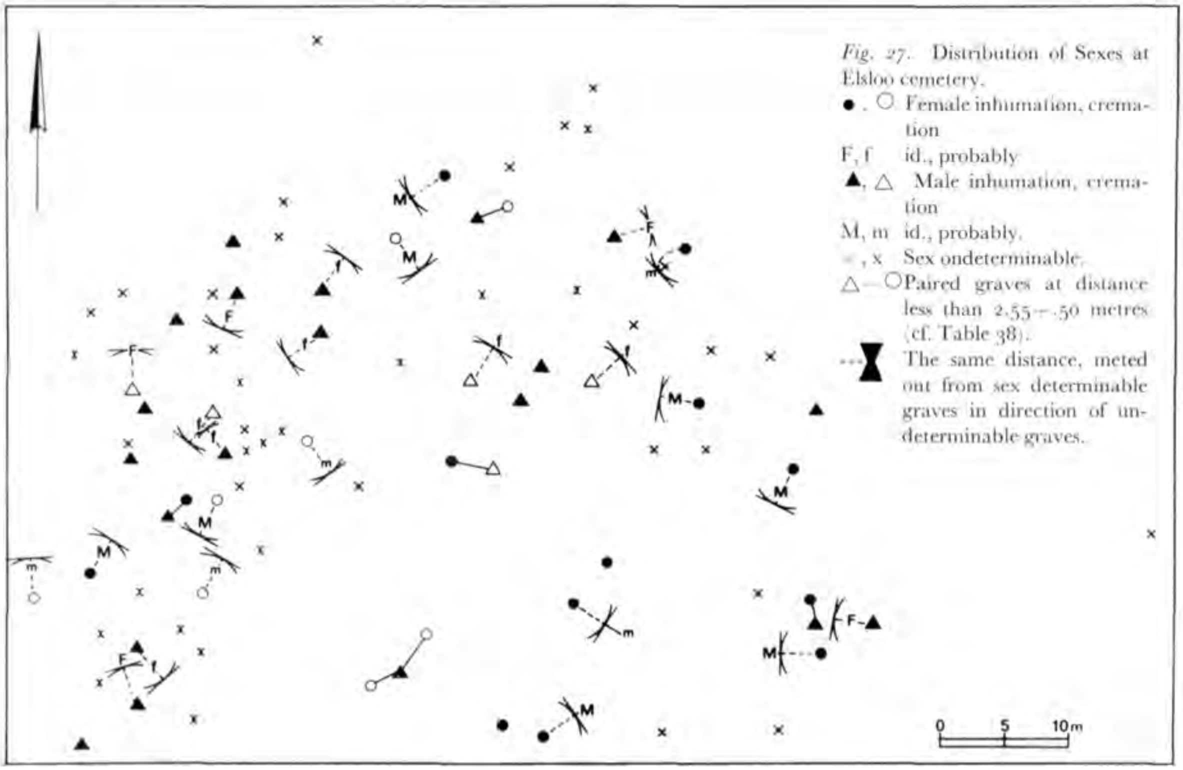


Fig. 27. Distribution of Sexes at Elsloo cemetery.
●, ○ Female inhumation, cremation
F, f id., probably
▲, △ Male inhumation, cremation
M, m id., probably.
×, x Sex ondeterminable.
△—○ Paired graves at distance less than 2.55—.50 metres (cf. Table 38).
---⧗ The same distance, meted out from sex determinable graves in direction of undeterminable graves.

normal in view of other graveyards of the period (Pavúk 1972b: 74-75). In other words, when the grave gifts are not deviant (Modderman 1970 (I): 66-71), the sex-ratio seems to conform at Elsloo.

The sexed graves appear in the plan on Fig. 27; they are listed in Table 18. The indirectly sexed graves, cf. Section 5, this chapter, have been entered separately in plan and table. Below, I will always commence an analysis with the graves for which the sex was determined "directly", i.e., from the grave gifts, and then later bring in the indirectly sexed graves.

Besides sex, *age* is the other likely criterion for a division of labour and status in egalitarian society; again, as the skeletons have disappeared, it is impossible to make direct observations. There are two problems connected with age in an analysis like this. The first one (to be referred to as the "adults-only hypothesis") is concerned with which sections of the community were interred at the graveyard, a qualitative problem. The second one has to do with the grading or ranking of those individuals that were buried in the graveyard.

To begin with the qualitative problem, Binford (1972: 232-234) concluded from a comparative (ethnographic) study that normally people are buried in different places at different stages in their lives; the older they are (i.e., the more important they are in Neolithic, egalitarian contexts), the more effort will be spent on their burial. Thus, people of a similar social age (e.g., all adults) will be buried in the same location.

A comparative analysis of those Bandkeramik graveyards where the skeletons had been preserved and age at death could be established, led Häusler to a similar conclusion: almost without exception adult burials were reported (Häusler 1966: 27-33), which is also in line with Modderman 1970 (I):

205-206. Yet, recent publications suggest otherwise: Osterhaus mentions children's graves at the Aiterhofen and Sengkofen cemeteries (Osterhaus 1975), and Pavúk (1972b: 66) reports 22 (out of 72) children's interments at the Nitra necropolis. Indeed, he even states that Younger LBK graveyards without children's interments are exceptions rather than the rule. And in an analysis of the teeth enamel recovered from the nearby Niedermerz cemetery (Aldenhoven, Kr. Düren, near Aachen, West-Germany) it could be established that children have been buried there, too (M. Dohrn-Ihmig, pers. comm. 170678).

To sum up: theoreticans (Binford, Häusler) postulate a group of like age, and excavators (Dohrn-Ihmig, Osterhaus, Pavúk) report heterogeneous compositions of the populations of the graveyards. If, therefore, it is undecided which age groups can and which age groups cannot be buried in the graveyard, perhaps *within* it different age groups might be present. Thus, when children's graves are there, these should be singled out by a specific principal component, setting off graves with gifts from the "general" category only (as a parallel to the inventories of the children's graves at Nitra: Pavúk 1972b: 74-75). Moreover, as Binford pointed out, one would also expect that the graves specified this way would contain restricted sets of gifts only (again, cf. the Nitra data). Among the components, there is one possible candidate marker (no. 7) of children's graves, as all other components are easier interpretable in other ways. It is specific, and conforms more or less to the expected pattern, given an interpretation of the components nos. 4 and 5 as "female" and "male", respectively: undecorated pots and sherds, arrowheads, and adzes of types I and IV-VI show similar loadings on the sexing components and this one, with only charcoal specific to this component (Table 10). However, it is not the poorer graves that are picked out by this component (Table 19): there is almost no difference in the distribution of gifts in the graves of this component from that of the totality of the cemetery (and so principal component no. 7 remains an auxiliary marker of male graves).

Again, from Pavúk's accounts (1972a: 125; 1972b: 72, 74-75) it can be derived that in Nitra children's graves are doted with "general" categories of grave gifts, rather than with sex-specific ones. When in the Elsloo cemetery *all* graves containing "general" gifts only are called children's graves, then the distribution in Table 20 results (in this table, as in the next, the distribution of the graves without gifts is made to conform to that from Nitra). The table shows some 40% of the graves to be children's at Elsloo (30% at Nitra). If, however, all indirectly sexable graves are entered along with the directly sexable ones, then the distribution would be as in Table 21, approximately: 23% of the graves might then be children's, half of them without gifts.

Neither of the distributions seems improbable, the latter even more or less plausible. Still, this is no argument in favour of or against the adults-only hypothesis; apparently, children's graves are not marked in any specific way by grave gifts.

I am very reluctant to list the graves that might be considered children's, as it is not even sure that this agegroup is to be found in this graveyard. However, if pressed, I would select the following 16 graves: nos. 002, 015, 018*, 028, 053, 061, 069*, 077, 079, 084*, 086*, 091, 095, 101*, 103, 104* (asterisked nos. indicate graves with general gifts). These are the graves at a distance greater than average + one standard deviation from their nearest neighbour and which do not contain sex specific grave gifts.

The above statement should be read that *if* children's graves are present at Elsloo, those indicated stand a chance fairer than average to belong to that group; it should not be interpretated to say that among the other graves no children's will be found.

Another possibility is found in either left/right sidedness, or orientation, which may be specific to age sets. From the literature, this would seem unlikely (Häusler 1964: 57-59; and 1966: 49). The small number of graves in Elsloo where these qualities can be established precludes testing. The cremation/inhumation distinction is also entirely inconclusive.

Finally, I have used elsewhere (Van de Velde 1979) the slightly uneven sex ratio at the Elsloo graveyard as an argument in corroboration of the "adults-only" hypothesis, which seems to be questionable by now: 46 (at best, 65) sexable graves is a basis numerically too weak to sustain it. The most one could say, again, is that there is no contradictory evidence, as follows from the following reformulation:

From sex ratio, period of use of the graveyard, etc., it is possible to give a rough estimate of the population (Also in Modderman 1970 (I): 205-207). Suppose that in a hypothetical starting population of 200 infant children equally distributed over the sexes, half of them die before initiation (i.e., 50 + 50 = 100 not on the adult burial grounds) then the difference between a sex ratio of .50 (hypothetical) and .46 (actual) can be found in the number of adult females dying at or before the birth of their first child; a ratio of .46 stands for 43 women and 50 men. Suppose also that half the number of children of these dying mothers live to be incorporated into the starting population (four infants). Then 43 women should bear 200 − 4 = 196 children; i.e., 4.6 children per woman. This way, an original population of 200 newborn infants results in 50 male plus 43 female graves. Instead, 113 graves have been unearthed, so the population should have been (113/93) × 200 = 243; or 81 people at any time if three generations are assumed (and 61 over four generations).

If, contrary to this "adult-only" picture, Table 20 (or 21) pertains, then the population must have consisted of 67 (or 90) individuals (four generations: 50 and 68, resp.), assuming every female identified in the graveyard to have borne four children, the remainder of which (2.64, and 3.43, resp.) have *not* been recovered.

All these assumptions cannot be too wide off the mark; from the convergence of the outcomes, again no decision can be made between the "adults-only" hypothesis and its alternatives. Moreover, the convergence suggests some reliability of the population estimate. (If the number of graves in the graveyard is set at 125, instead of 113 (cf. Section 2 of this chapter), then three generation estimates of the population would be: 90, 74, and 100 respectively; the corresponding four-generation figures then become 67, 55, and 75 per generation.)

One inescapable conclusion emerges from these games with figures: no matter which of the assumptions is chosen as a starting point, a sizable proportion of the population's children has not been buried at the Elsloo cemetery.

If, then, the non-adult part of the population is not recoverable, it necessarily follows that the older part cannot be made out as a group either: no contrasting attributes are visible. In other words, the undeniable differences in quantities of grave gifts should be explained some other way.

This brings us to the second problem of ranking: the grading of those buried in the cemetery as a correlate of relative age (Binford 1972: 232-234), but now within the group buried at the same place.

Principal component no. 3 (Table 10) seems to set off decorated pots from undecorated sherds and intricately ornamented ware from more simply decorated pottery. Empirically, however, the graves with the "richer" inventories are selected (Table 22). Here the number of categories of grave gifts defines this "wealth", as the analysis was run on a presence/absence basis to avoid mixing up with specialists' graves (cf. p. 95-113).

When position in the local hierarchy (defined as access to scarce things) corresponds in some way with the effort spent at a person's burial, this can be worked out in three ways:

1. Similar to Hodson's reanalysis of the grave gifts at the Hallstatt cemetery: the rarer a thing, the higher its value (Hodson 1977; also cf. Schiffer 1976: 190 for an inspiring discussion). However, there are some difficulties with this idea. In the first place, the regular inflation of status symbols (cf. Schiffer 1976: 190) will block a diachronic perspective of the hierarchy, as things once rare become more frequent with time; worse even, at Elsloo only a small proportion of the graves can be dated reliably. In the second place, and not considered by either Hodson or Schiffer, an equation of rarity and values may be a Western (and capitalistic) notion – at Elsloo, for instance, undecorated pots are scarcer than decorated ones (15 *vs.* 24; in the settlement,

the reverse is the case – so, which alternative should be chosen?), adzes of type I (n = 5) are scarcer, and consequently of higher value than adzes of type III (n = 13) or IV-VI (n = 15), but this would neglect the *chronological* restriction of type I (cf. p. 85-86).

2. Less amenable to an occidental outlook, an attempt at estimating the amount of labour spent to produce the grave gifts can be made. This has the advantage of being an objective standard but also raises problems. For instance, matching value with labour seems to be non-universal (Sahlins 1972: 68, 225, 289), and consequently "scarcity" or any of its analogues can hardly be defined this way – it ultimately leads to a notion of exchange-value (as opposed to use-value, which is more applicable in this kind of society) (for a discussion of these notions cf. Sahlins 1972: 277 [note], 307). Also, an estimate of the production time is likely to be quite arbitrary. Nevertheless, an attempt can be made:

A decorated pot has much more (usually, and perhaps twice as much; e.g., Lauer 1974: 57) work spent on its manufacture than an undecorated vessel, and an arrowhead represents more productive effort than does a blank blade. Granting arbitrary values to the different kinds of artifacts, the following scale was used to evaluate the assemblages per grave:

Ceramics	points	*Flint*	points
undecorated sherd	2	debris	1
decorated sherd	4	blanks	5
undecorated pot	20	used blades	10
decorated pot	40	scrapers	15
		arrowheads	20
Stone			
complete quern	50	*Adzes*	
fragments	5	per piece	50
Charchoal		*Red ochre*	
traces	1	traces	2
lumps	5	lumps	10

The resultant distribution is presented in table 23. As noted already, such an at-first-sight "etic" account, based on a rough estimate of labour incorporated into the grave gifts, may be measuring some kind of exchange-value only, which is less or even entirely inapplicable at this level of socio-political development. Point 3 represents an attempt to circumnavigate these difficulties and to follow the lead indicated by the principal components analysis.

3. The categories of Table 10 were lumped into rather broad "kinds": whether a few undecorated sherds or three well-decorated vessels were in the grave, "ceramics" was scored present; similarly, all flints were grouped; etc. Summing these presences an indicator of the distribution of statuses is found in Table 24; actually, it is the distribution of broad spectrum grave inventories that is made visible this way.

Of course such a grouping is too general to be of much use. The "kinds" introduced do not seem to convey much meaning, although the correlation with labour input apparent from Table 23 is suggestive of at least some ranking dimentions – the one also brought out by the third principal component.

Yet a further development into this direction might bring us to an operationalization of use-value. One should have know the number of activities or uses the grave gifts stand for – probably, the number of activities presented in a grave will correlate with the kind of productive activities the deceased has been involved in during his/her life, thus a correlate of influence.

Mainly drawing on correlation and association, but also on suppositions of the author, the following listing of activities was devised.

Artifacts in grave → (correlate with) *supposed activity*	
pots (decorated and undec.)	*storage*
sherds (decorated and undec.)	*service*
querns, ochre	*adornment*
charcoal	*?*
arrowheads, knives	*hunting and dressing*
adzes types I-III	*male forestry*
adzes types IV-VI	*agriculture*
debris, nuclei	*fire/lighting equipment (?)*

(Of course, "activity" is an entirely unfounded and arbitrary interpretation; however, for the analysis, it is the association *of artifacts that counts.)*

Scoring the several grave inventories according to this list resulted in Fig. 28, Tables 25 and 26; assuming that the above associations have some validity, then the following conclusions can be drawn:

– The two sexes were on equal footing at Elsloo: the distributions in both columns in Table 25 are almost identical. (At Nitra, Pavúk concluded that males were more important than females; Pavúk 1972a: 126; 1972b: 72.)

– The districution of numbers of graves over the numbers of activities is very regular: in Table 25 no clusterings are to be observed at top and bottom with a gap in between. If then, number of activities may be equated with influence, power, and the like, then not a *stratified*, but a *ranked* (or even egalitarian) society is presented by the Elsloo cemetery (as possibly at Nitra also: Pavúk 1972b: 97). When below (pp. 96-98) differential access by groups to goods is observed at Elsloo, it is access to *ceremonial* goods, which indicates a ranked rather than an egalitarian society.

The ranking principle that has been made visible this way should be age, as it applied to (almost) all graves, and as age can be expected to be generally

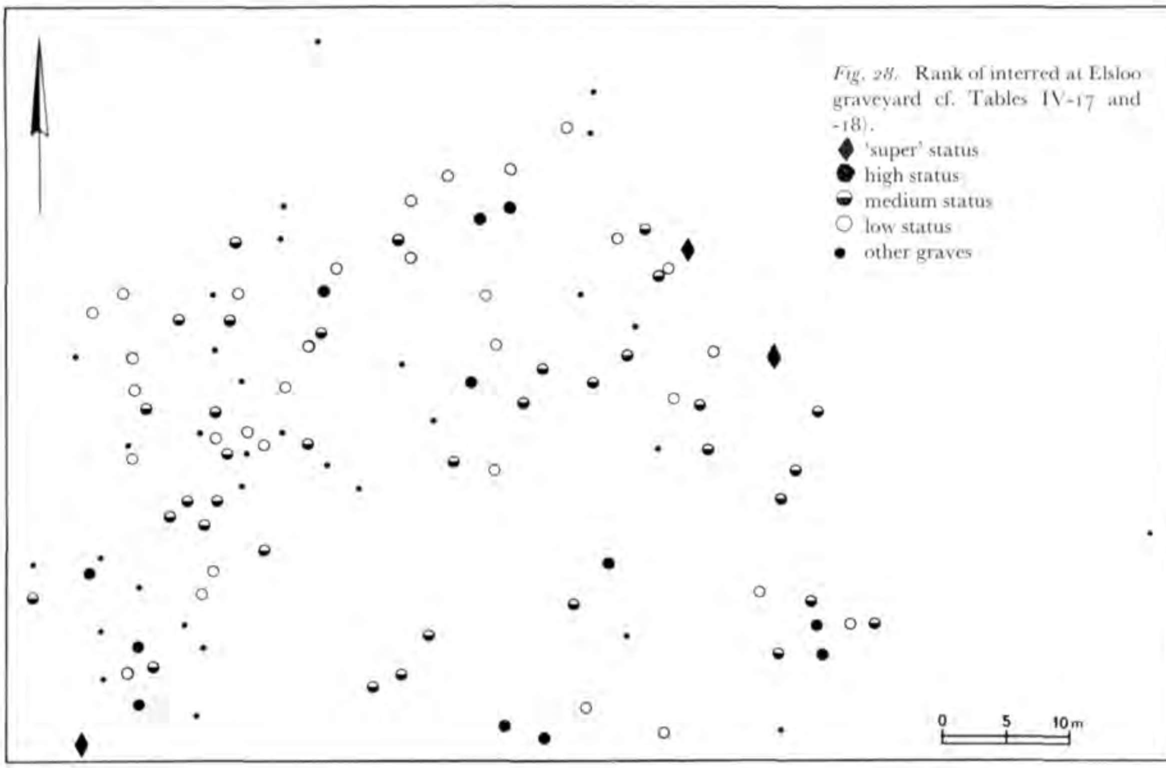


Fig. 28. Rank of interred at Elsloo graveyard cf. Tables IV-17 and -18).

visible. This does not interfere with my previous statement that no conclusive evidence can be forwarded as to what section of the population is buried at Elsloo, and more specifically, that children's graves are not specially marked there. *Within* the group that has been interred, age is a ranking agent, and therefore for the entire social structure probably also.

Apart from ranking by age, two other forms of distinction are visible in the grave gifts. Looking up the estimates of labour involved, a further *qualification of the egalitarianism* is to be made:

– Three graves with six or seven activities represented have more complete inventories than the other ones. In the other graves, most activities are represented by one artifact only (if at all), with usually one activity more complete: here (graves nos. 001, 083 and 087; one male, one female and possibly another female), more activities are completely represented, they have been awarded more than the other people. Specifically, these graves hold what has been interpreted here as hunting equipment (arrowheads *plus* knives) as well as agricultural tools (1-2 adzes), apart from complete marker sets of their sex (i.e., quern *plus* ochre, or an undecorated pot *plus* an arrowhead *plus* a high adze). This same complex was also found in the male grave no. 100 (with five activities represented). Incidentally, this explains the ambiguity of the sex of the deceased in grave no. 087, now determined female.

– Also, two or three specializations seem to have commanded more respect than did regular contribution to survival: the four graves with more extensive hunting equipment (male graves nos. 003, 005, 067, 071) have witnesses of four or five activities and the graves with three or four pots (nos. 005, 096, 112; all male), of three or four activities. Similarly, the adze-rich grave no. 083 (female) netted more than modal, even six activities (but it goes possibly with the first group).

Perhaps the first group would tally with a "big man/woman", or a "chief". That at least one woman occurs among the leading deceased suggests the existance of a *chiefly* lineage at Elsloo, rather than *big-man* type prominent figures: Leacock (1978) failed to locate any big woman in her survey of egalitarian societies, whereas references to females in chiefly (i.e., hereditary) positions abound (also cf. R. Cohen's comment following the Leacock article). Again, ranking rather than egalitarian society is implied.

The second group (hunters and those with the pots) may tell something of the society's preferences, or with a heavier word, its value system.

So we remain with a picture of the social network in which positions were primarily differentiated according to sex and ranked to age, but also, and less pervasive, to specializations and a few chiefly toppers. Together, these form as almost perfect picture of the definition of "ranked society" on p. 86.

One final and perhaps rather marginal observation still has to be made. From Van Gennep's discussion of funerary customs (especially Van Gennep 1977: 152-154), one might expect two different sets of attributes with different functions presented to the deceased: one set indicating his or her position in the social structure and another one consisting of the necessities of travel to the Other Side. Although the grave gifts show differentiation and ranking of those burried, no gifts seem to be *especially* meant as travelling guides at Elsloo. Those artifacts that might be interpreted that way (arrowheads, pots) also seem to function within the social framework. Such a "primacy" of the positional markers is not an analytical artifact: there is not a single category of grave gifts common to even half the number of graves which might therefore be a candidate for such a travelling function. Perhaps the reason must be sought in the LBK's notions about distance, time, and travel, plus in the *general* character of Van Gennep's statements.

After this sketch of the positions in the social structure of the Elsloo people, we now turn to the relations between the people and the groups holding the various positions.

4. *The Elsloo graveyard: a structuralist analysis*

In contrast with the previous section, the present approach is not concerned with counts of the number of grave gifts per grave, but with graves sharing the various categories of gifts. The aim is explicitly collectivistic here: rather than individual positions, relations between groups are our theme, since primitive society seems to revolve around those relations that are commonly indicated by "kinship". And whether these are thought of as a "general institution" (E.E. Evans-Pritchard, quoted in Godelier 1977 (I): 84) and then provide for many functions (and include relations of production; cf. the next section), or as a more restricted phenomenon which mainly provides for a society's biological continuity, there seems to be no doubt about their all-encompassing nature in pre-state communities.

If kin relations are (partially) defined as the social relations between groups, this was since ancient times conceived of as the regulation of recruitment of members to kinship groups; later, kinship groups were considered the elements of an alliance or exchange system (Fox 1967: 23). The first emphasis had to do with consanguinity, with inheritance and succession, the second with affinity, the regulation of exogamy and the definition of incest (cf. Keesing 1965: 345) – in fact, the two are not exclusive, but rather presuppose one another (Fox 1967: 35).

To throw light on the relations, one possible approach is to try to discover the *component groups* in the graveyard: if social life was an affair of groups, these may be mirrored in the graveyard. *Apropos* his own excavations at Nitra, Pavúk has tried to outline groups in a number of Bandkeramik graveyards; among these the Elsloo cemetery (Pavúk 1972b: 87, 92); his five grave groups are outlined on Fig. 29. He maintains a relation of this grouping of the graves with the cyclical return of the population due to their so-called "migrant-farmers" ("Wanderbauern") economy; the groups are chronologically distinct (Pavúk 1972b: 98; also Redlich 1966). In the case of Elsloo, 77 graves cannot be grouped this way (omitting the two outlying graves nos. 061 and 095).[5] Especially this last observation makes me reluctant to use Pavúk's subdivision, and I would rather attempt another approach: if (social) groups are present as groups of graves, these are as likely to be marked by geographical concentration as by the grave gifts in them. In other words, an analysis of the grave gift distributions might be informative. And indeed, not all grave gifts are everywhere. If these differences are sociogenic, then the different groups of deceased apparently had differential access to these goods; not every grave contains a gift out of every category. Still, recognition of a pattern which accounts for all differences is very hard to come by. In the plan of the graveyard some grave groups are visible on the one hand, though these need not conform to Pavúk's formation; on the other hand, a substantial body of graves shows a fuzzy, and almost random distribution, especially in the western, northwestern and northern parts of the graveyard. In a first, trial approach, respecting the rather clear grouping of graves in the southern, southeastern, eastern and central sectors, the remainder was then arbitrarily subdivided as in Fig. 30. This partitition into nine fields was for *a priori* reasons: after a preliminary analysis, regrouping has to be possible – that is, the mesh should be finer than the expected group size. If, then, 111 graves (omitting the two outlying) have been produced by three generations, per generation some 37 interments were made; given the low density of grave gifts, groups smaller than perhaps four graves will hardly be perceptible; thus we arrive at 37 : 4 = 9 "groups". The geographical groupings would then average 10 to 15 graves. With this lattice the distributions of the various categories of grave

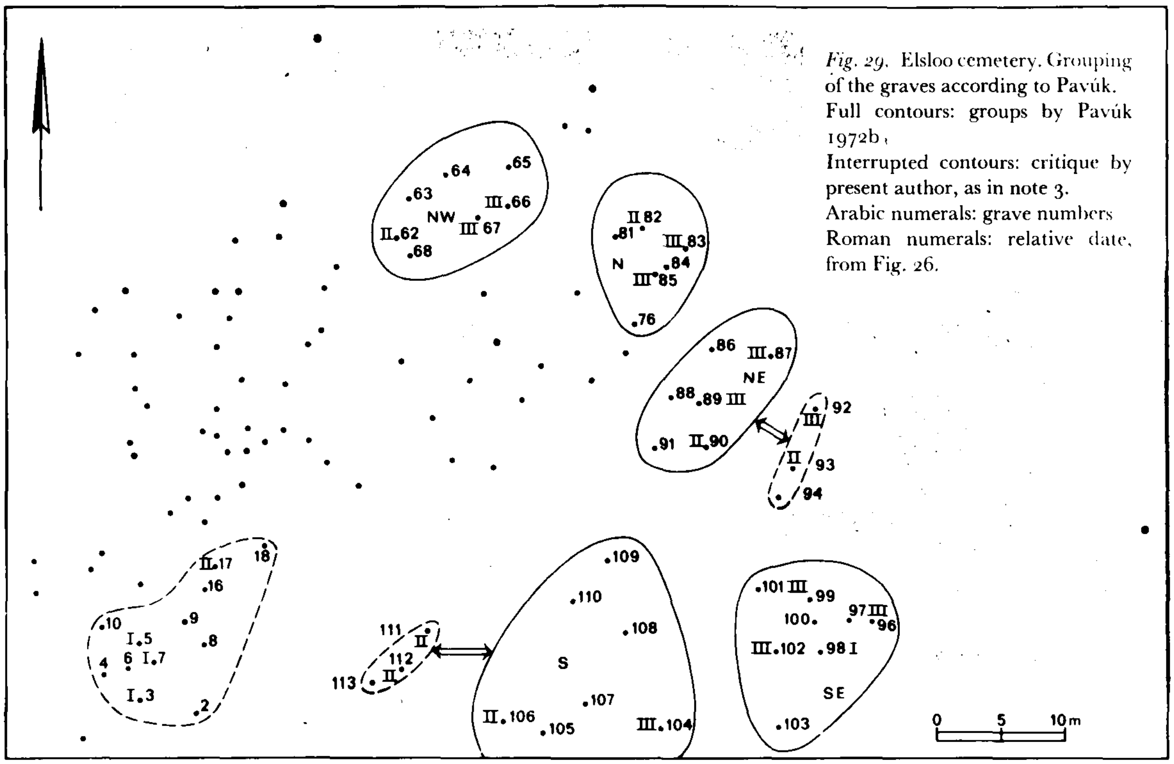


Fig. 29. Elsloo cemetery. Grouping of the graves according to Pavúk.
Full contours: groups by Pavúk 1972b.
Interrupted contours: critique by present author, as in note 3.
Arabic numerals: grave numbers
Roman numerals: relative date, from Fig. 26.

goods were screened. Table 27 renders the number of graves per sector or field. Then, by means of the formula

$$c_i = \frac{n_i}{N} \times k$$

it is possible to calculate an *expected distribution* of graves with grave gifts of category C. This distribution reflects a situation in which c_i = expected number of graves in field i containing category C; n_i = number of graves in field i; N = 111, number of graves in the cemetery. k: observed number of graves with gift C in the cemetery. This calculated distribution reflects a situation in which no differential access to category C exists. A comparison of this hypothetical distribution with the actual one shows where too many, or where too few graves have been donated with this category (cf. Tables 28 for an example and 29 for a summary). From these latter "patterns" it appears that some fields tend to vary jointly, and others independently: an overall structure of four groups emerges.

The picture is very clear for the southwesterly sector, which is always by itself, as against the southern and southeastern fields, which vary together, and western and northwestern fields, also together. Then, it is also clear that the eastern and northeastern sectors go together; the alignment of the central and the northern fields, however, poses problems: in the first place the two always react in opposite ways, with one sometimes conforming to the western neighbourhood and the other to the eastern, and sometimes the other way around. The correlation coefficients (between the fields, summed over the categories; Table 30) are not very impressive, although the northern field shows some affinity for the northeastern and eastern fields. A further investigation dit not produce any reason to

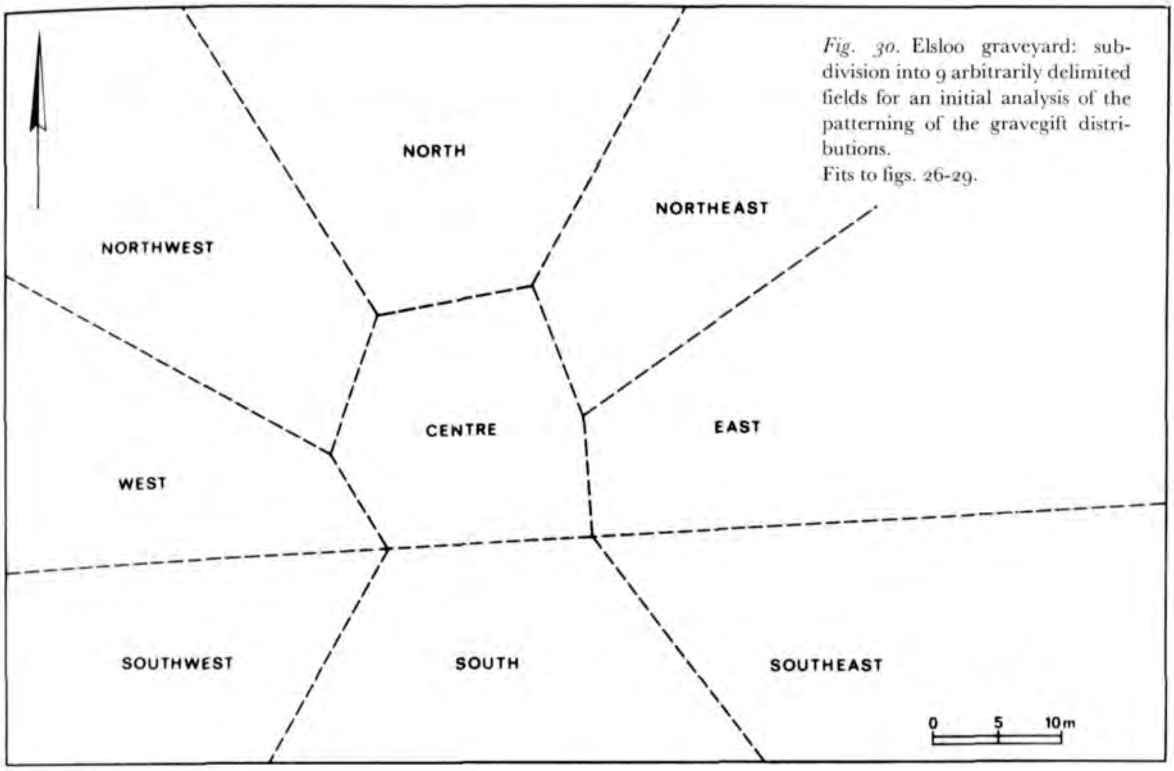


Fig. 30. Elsloo graveyard: subdivision into 9 arbitrarily delimited fields for an initial analysis of the patterning of the gravegift distributions.
Fits to figs. 26-29.

prefer one alignment to the other: on the basis of the grave gifts, the northern field should be grouped with the eastern and northeastern for the red ochre, but with the northwestern and western fields, for undecorated pots and arrowheads. The summed Chi-squares for both divisions are almost the same: 51.2 *vs.* 52.5 (with df. =33). A consideration of the empty zones between the graves was inconclusive (although the limits about to be proposed are slightly less twisted). Only the distribution of the sexes is in favour of a grouping of the centre with the eastern and northeastern, and of the northern field with the western and northwestern (Table 31); however, this argument loses its force when the indirectly sexed graves are added. Finally, correlation coefficients are compounds of multidimensional variation, and they cannot be accorded decisive status.

Yet the size of the resultant northwestern group suggests in *both* solutions a further subdivision, but there is no other argument to follow this through (cf. note 6 below). It is the distribution of the sexes which makes me favour the grouping of central with eastern and northeastern and of northern with western and northwestern.

A closer look at the grave gift distributions suggests some minor adjustments of the borders of the four sectors thus retained: to the sourthwest a part of the western field is added. Fig. 31 shows the geographical redefinition. The zones between the four sectors are then devoid of graves with gifts; and only five graves without gifts cannot be assigned conclusively (Fig. 32). The average width of the no man's land is more than the mean distance between the graves plus two standard deviations (Tables 32 and 34).[6]

A comparison of the distribution of the eleven categories of grave gifts (i.e., those with sufficient

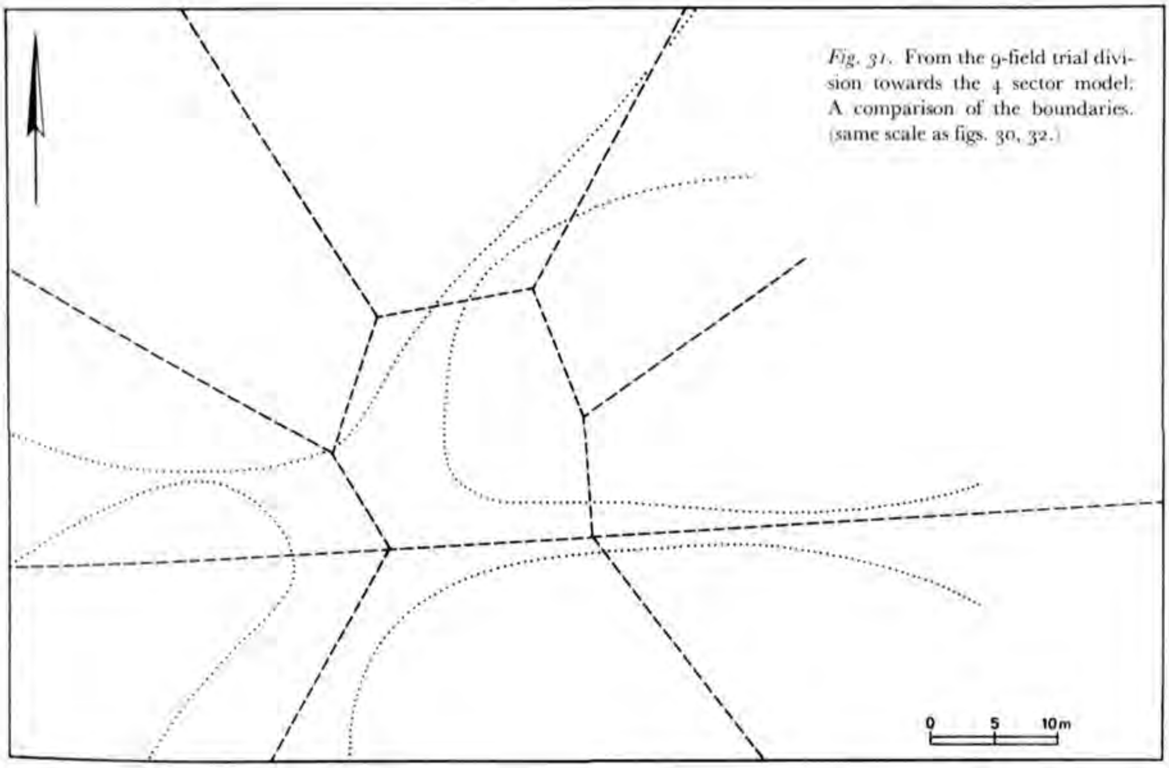


Fig. 31. From the 9-field trial division towards the 4 sector model: A comparison of the boundaries. (same scale as figs. 30, 32.)

representation) gives a summed Chi-square of 88.87 (with df. 11 × (9 − 1) = 88) for the ninefold subdivision – which at best would be interpretable as somewhat skewed – and of 50.42 (with df. 11 × (4 − 1) = 33) for the four-sector model – which is significant at the 1% level (Tables 34 and 35).

Notwithstanding Tables 32 and 33, the limits of the four groups in Fig. 32 should be considered approximate only; similarly, the groups or sectors should be thought of as cores rather than neatly delimited territorial units – after all, a geographical shift has been established in the use of the graveyard with time (Section 2 and Fig. 26). Also, there is not a 1-0 exclusiveness across the zones, but a *falling off* of the frequencies. These restrictions apply specifically to sectors I and II (cf. above), where no very clear-cut separation of the two (except in the *geographical* distribution of the graves) is visible.

When these four sectors are accepted as indicators of a similar number of groups (cf., however, note 2), then the problem of the *relations between these groups* arises. Thinking of these relations in terms of their lasting effects rather than in their personal, volatile, day-to-day manifestations, these group relations are of a *alliance* nature. Alliance, that is, bonds between groups, involves relations of affinity; more general, it involves exchange of all kinds of things, including women (Lévi-Strauss 1969b: 44, 60-61; for a somewhat different interpretation, cf. next section). This exchange is basically reducible to either of the following structures (Lévi-Strauss 1969b: xxiii):

1. complex; i.e., where relations of exchange exist with all other groups.

2. elementary; i.e., where relations of exchange exist with specified groups only.

Among the elementary patterns, a limited set of

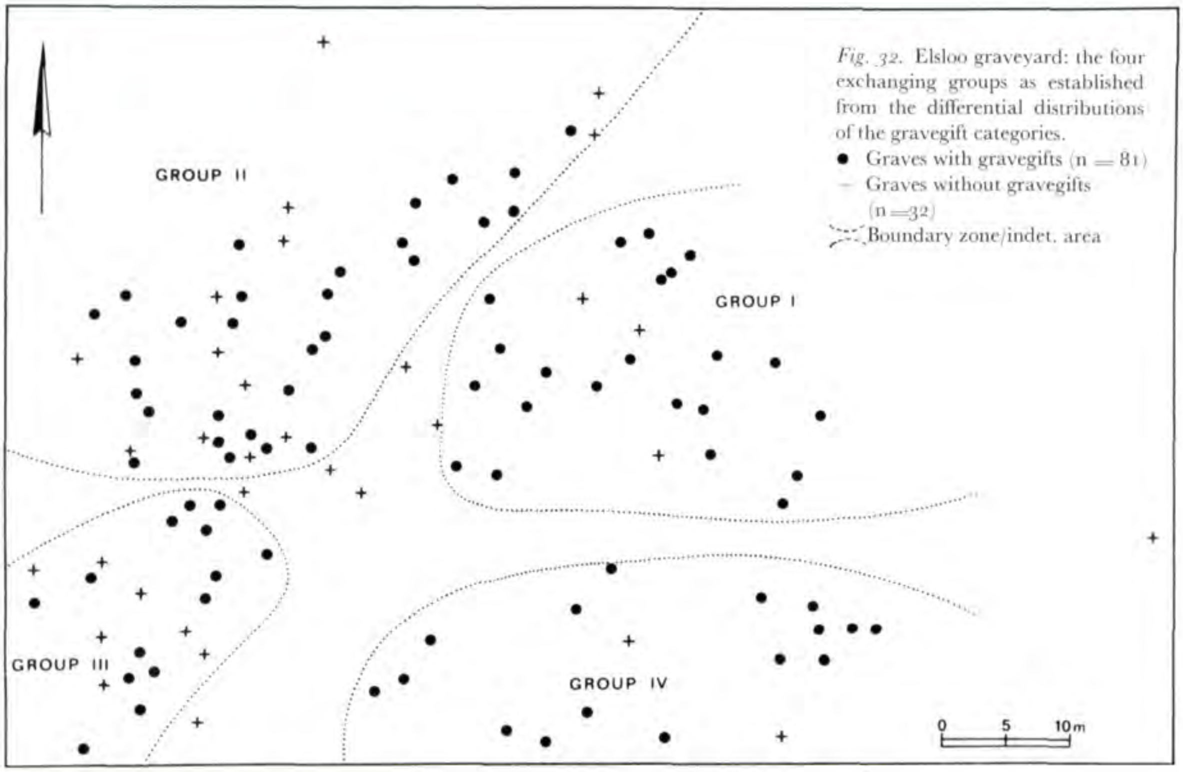


Fig. 32. Elsloo graveyard: the four exchanging groups as established from the differential distributions of the gravegift categories.

alternatives is possible: symmetrical, or restricted exchange (Where relations are between two groups only, i.e., reciprocal; Lévi-Strauss 1969b: 146), and generalized, or asymmetrical exchange (where the relations are unidirectional, or univocal instead of reciprocal, between any number of groups; Lévi-Strauss 1969b: 178-179), with the latter still further differentiated into "open" structures of continuous exhchange (with exchange from group to group) and "closed" structures of discontinuous exchange (dealing with one group in this generation; a return is provided in the next generation; Lévi-Strauss 1969b: 444-445).

To explain these notions I will make use of the model of relations presented in Fig. 33, where, for the sake of clarity (and also because that number of groups has been found) four groups of equal size are practicing exchange. For every category of goods (mostly of a ceremonial nature) one specific group is supposed to be the introducer into the system; for another category, another specific group.

1. In the case of a *complex exchange system* (Fig. 34) group A, the introducer, will have dealings with all other groups involved, and these with each other in turn. Consequently, the category *a* will be diffused evenly to the different groups, with only group A possibly retaining some more in stock.

2. In the case of an *asymmetrical and linear exchange system* (Fig. 35) group A will present its goods *a* to one group only, e.g. to B. Similarly, B will deal with C only, and so on. It is the gist of this system that exchange is clearly directed, univocal; in the example of Fig. 35 the goods *a* flow in a clockwise direction through the network of relations (and, implicitly, other articles flow counterclockwise, the return prestations). Every group will keep a proportion of *a* and present the remainder to its customary partner down the line. The resultant

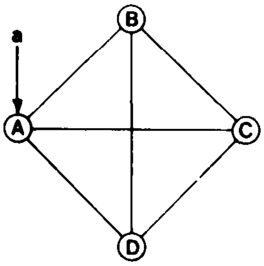


Fig. 33. General model of an exchange system of 4 groups. Group A introduces the category of goods to be exchanged with groups B, C and D, in the network shown. All possible relations have been mapped; in different exchange structures, different relations become articulated (ref. to Figs 34 to 37).

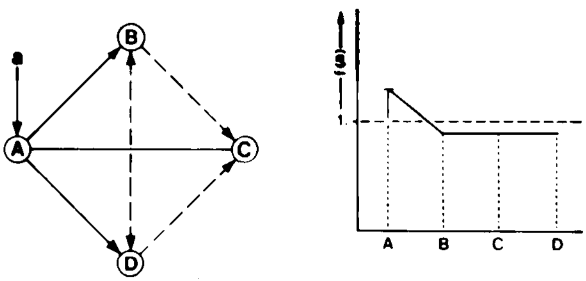


Fig. 34. A model of complex exchange relations in a four-groups system.
Left: direction of exchange + articulated relations.
Right: distribution of *a* over the system after time (horizontal dashed line estimated average); groups have been arranged according to diminishing frequencies.

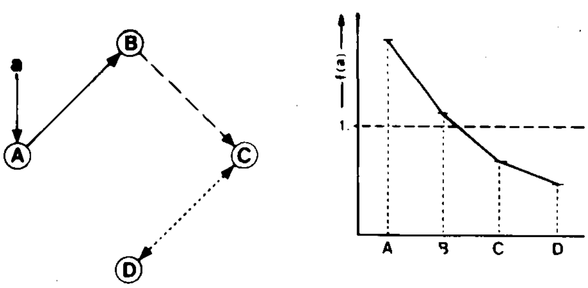


Fig. 35. A model of asymmetrical and linear exchange.
Left: direction of exchange + articulated relationships.
Right: distribution of *a* over the system after time; horizontal dashed line: estimated average frequency; groups arranged according to diminishing frequency of a.

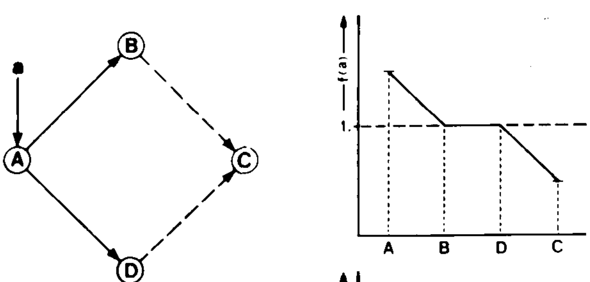


Fig. 36. A model of asymmetrical and alternating exchange.
Left: direction of exchange + articulated relations in contiguous generation.
Right: distribution of *a* over the system after time; dashed line: estimated average frequency: groups arranged in order of diminishing frequency of category a.

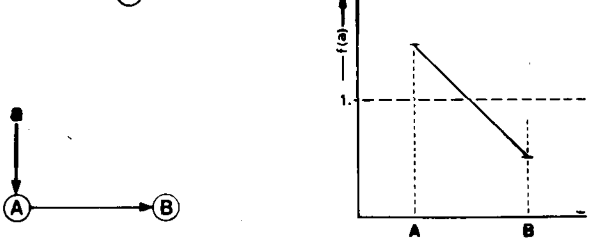


Fig. 37. A model of symmetrical exchange: only two groups take part.
Left: the exchange relation.
Right: distribution of *a* after time; dashed line: averaged frequency.

distribution is one of diminishing quantities of *a* in the direction of the exchange relationships.

3. In the case of an *asymmetrical and alternating* (or discontinuous) *exchange system* (Fig. 36) group A deals alternatively (during a certain time span, mostly per generation) with group B and group D, but not with the fourth group. In such a system, every group will be in similar relationships with two others. The goods *a* will disperse through the system in such a way that in due time groups B and D will possess approximately equal amounts of them, less than group A; the fourth group will possess still less of them.

4. Finally, in the case of *symmetric exchange* the importer group has dealings with one other group only, either directly of indirectly (Fig. 37). It is in the nature of this system that other groups will have to seek access to *a* through extablishing their own import "monopoly", even when living in the same territory as group A.

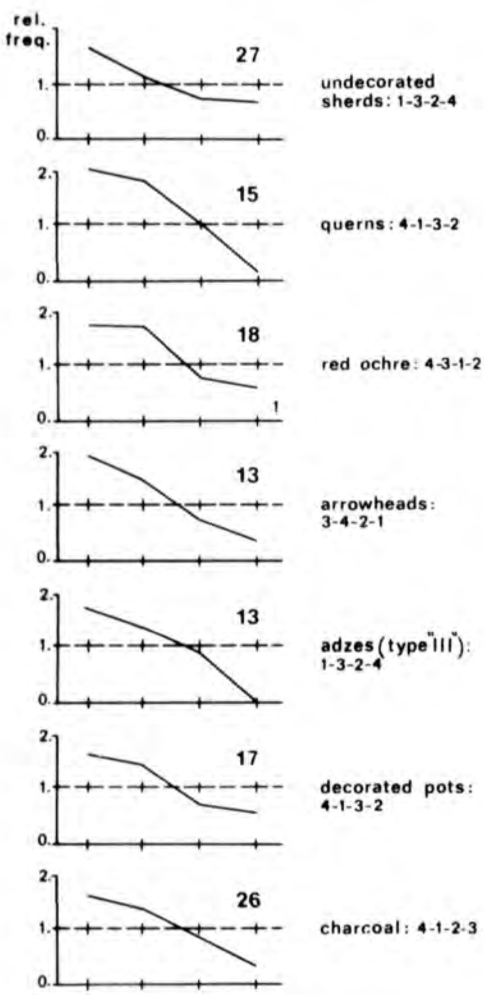
Before quantifying the grave goods in the Elsloo cemetery to match them with the above models, three reservations should be made:

- As long as the groups are of roughly equal size (that is, on the same scale), numerical differences will hardly be consequential upon the functioning of the exchange structure: a group stands in a fixed relationship with another group or not, and the concomitant privileges and duties are fixed by tradition.
- If the exchange systems were closed (i.e., without a "sink" function) the terminal situation would be that all groups possessed equal amounts of every kind of goods. Empirically, closed systems will hardly occur: grave gifts, losses, shoving off into external exchange networks, etc., provide for the vacuum which keeps the goods going down the lines, resulting in distributions resembling those in Figs. 34 to 37 (also cf. the discussion in Salisbury 1956).
- In terms of the model, and related to grave gifts, the meaning attached to the word "exchange" is unimportant: the deceased's group may have accumulated (ceremonial) goods during his/her lifetime along the customary relationships; in fact, they may possess a pool of them. Or, the relations may call at the funeral and register their sympathies by means of surrender of their groups' attributes. In both casse the goods flow through existing networks because of the attendant claims.

The "profiles" drawn in Figs. 34 to 37 are meant to facilitate recognition and interpretation of the distributions perceptible in the Elsloo graveyard. Assuming that the four sectors in Fig. 32 are a tolerable approximation, the different counts of graves per group with a category of goods were corrected for group size and scaled to render them comparable. Rearrangement according to diminishing frequencies resulted in the eleven graphs of Fig. 38.

From the models, it follows that the groups with the greatest amount of a kind of goods are the introducers, no matter which exchange structure prevails. The frequencies down the line may indicate the positions of the different groups in the exchange structure, and the shape of the graph the type of structure. Thus, the seven graphs to the left of Fig. 38 seem to indicate an asymmetrical and linear exchange system; two (top right: undecorated pots and decorated sherds) do not conform to any of the hypothetical patterns. The remaining two (blades, and adzes of types IV-VI) suggest a generalized structure of the relations.

Also, along with the labels, the ordering of the groups has been presented according to diminishing frequencies.[7] Six categories of grave gifts (undecorated sherds; querns; thick and flat adzes; decorated and undecorated pots) show an identical arrangement. When the ordering is permuted so as to have group I in the leading position, the sequence is I →III →II →IV (→I). Charcoal and decorated sherds stipulate a I →II →III →IV network, as may be done by red ochre and blades as well (though here, approximately equal frequencies result in ambiguous arrangements); finally, arrowheads may stand for a I →III → IV →II pattern.



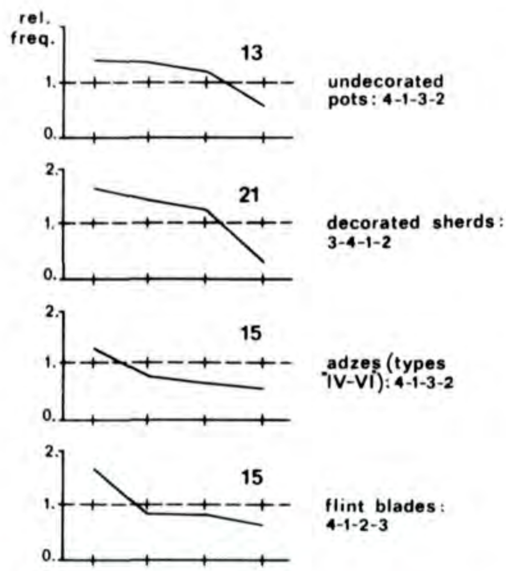


Fig. 38. Elsloo graveyard: frequency counts of graves per grave gift category and per grave group; arranged according to descending proportions, with sequence of groups written to the right. The number of graves containing the category is written to the top right of the diagrams. Frequencies normalized and corrected for group size.

The left-hand series conforms approximately to expected distribution in an asymmetrical linear exchange structure; the right-hand series: top-two unaccountable, bottom-two generalized exchange.

In this way, it is possible to derive from the statistical image of the distributions of grave goods (Fig. 38) the appropriate mechanical model (those rendered in Figs. 34 to 38); in the above case, an asymmetrical and linear structuring of the exchange relations is found. Also, the non-occurrence or few instances of the other "profiles" in the data is an internal check: the possible alternatives are excluded. The directions of the exchange relations are pictured in Fig. 39. The relations imply a moiety structure of groups I and II *vs.* groups III and IV; whether this duality was recognized is difficult to decide – the not completely consistent circuitry (cf. the transition matrices in fig. 40) even seems to suggest otherwise[8] (see below, p. 107).

With all gifts going approximately the same direction, automatically the question arises: what went the other way?

Exchange relations being what they are, bonds of alliance between groups which also incorporate kin relations, the answer is unequivocal: women. In fact, matrilateral cross-cousin preference is synonymous with asymmetrical and linear ex-

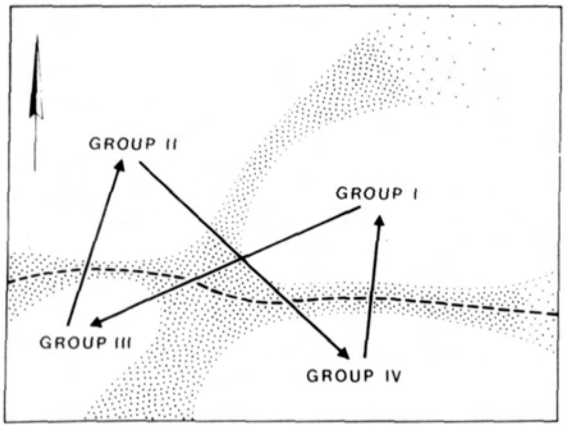


Fig. 39. The most likely exchange relations between the four sectors of the population at Elsloo cemetery.
arrows indicate direction of flow of goods.
dashed line: logically implied moiety division.

→	I	II	III	IV
I			10	
II				10
III		10		
IV	10			

→	I	II	III	IV
I	–	2	7	–
II	3	–	2	4
III	½	5½	–	3½
IV	6	1½	½	–

Fig. 40. Transition matrices of exchange of goods (for transfer of women read from columns to rows) Read: group 1 (1st row) trades to group III (3rd column); etc.

Left matrix: when all exchanges had been conform to the mechanical model I-III-II-IV-I, the matrix would be like this one.

Right matrix: observed pattern in the distribution of grave gifts at the Elsloo cemetery (from fig. 38), 4 ex aequo cases (=undecorated sherds for groups II/IV, red ochre IV/III, undecorated pots IV/I, and blades I/II) have been scored half per possible partner (charcoal omitted).

change (generally called: circulating; De Josselin de Jong 1951: 34-43 gives a lucid summary of the marriage/alliance aspect of exchange relations). Such an exchange system is compatible with uxorilocal and virilocal post-marital residence rules; it is also compatible with patri- and matrilineal descent.

To derive the rules operative at that time, it is necessary to have recourse to theory again. (A preliminary and cautionary note: it is advisable to use pad and pencil to follow the argument through; even athropologists of the Leiden School find it difficult to keep up without such aids.) The composition of the burying group is the result of descent and residence together; as above, I assume the grave gifts to be defined in a similar way. Essentially, only a few combinations of these rules are possible[9]: unilineal systems in which rules of descent and residence coincide ("harmonic regimes") and bilineal systems in which residence and descent are regulated according to different lines ("disharmonic regimes"; Lévi-Strauss 1969b: 215). Basically, if the females are exchanged by the men (Lèvi-Strauss 1967: 45; Leach 1970: 77) then the acquirers of these spouses are likely to be in constant (if only ceremonial) debt to their wives' brothers; yet these men will tend to control (if only ceremonially) their sisters (for a critical assessment, cf. Van Baal 1975: 71). Now, given matrilateral cross-cousin preferences, *unilineal* (or harmonic) *systems* require a very limited set of relations:

– In the *uxorilocal-matrilineal* case (Fig. 41), the females of a residential group are always married to men from the same background: it is always the same descent group which steps in. Also, their brothers go and live with females of the group on the other side, always of the same descent line.

Thus, when a female dies, her residential, natal, and children's group will of course be present, but also her brother, living to the right in the drawing; and when her husband dies, apart from his residential group, his sister's (husband's) group, which is also his own natal group, will be at the grave – the group to the left.

– In the *virilocal-patrilineal* case (Fig. 42) the males of a residential group always acquire their wives from the same residential-cum-descent group (the group to the right in the drawing); similarly, their sisters always go to the same group (the group to the left).

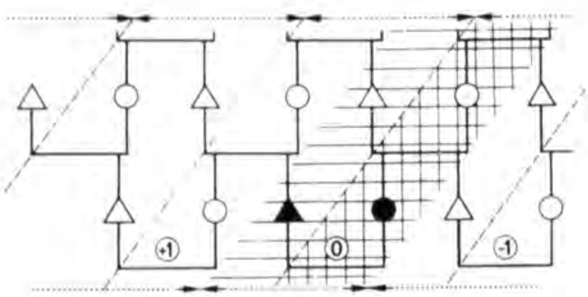


Fig. 41. Cross-cousin marriage and a harmonic combination of descent and residence: the matri-lateral system (i.e. matrilineal descent and uxorilocal residence): women stay put, exchange goods and males move to the right.
△: male ○: female ▲ ● : couple discussed in the text ⊔ : marriage : children

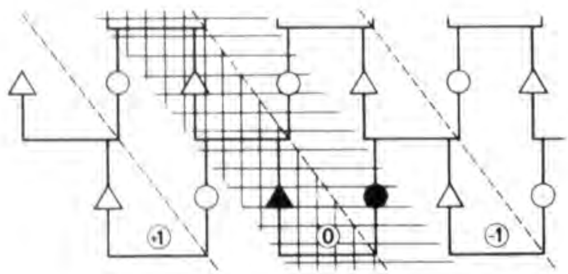


Fig. 42. As Fig. 41. Patrilateral system (i.e. patrilineal descent and virilocal residence): males stay put, females move to the left. Exchange goods to the right.
He who acquires a woman incurs a debt towards her brother.
(+1) (0) (−1) sign of obligations as in Table 34. (signs as in Fig. 41.)

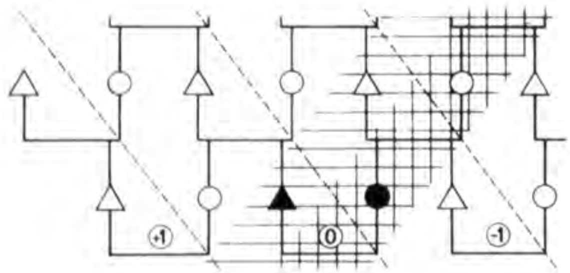


Fig. 43. Cross-cousin marriage and a disharmonic system: uxorilocal and patrilineal descent; women stay put, exchange goods and males move to the right (signs as in Fig. 41).

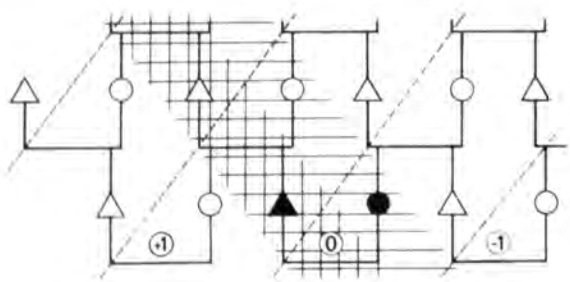


Fig. 44. As Fig. 43, virilocality and matrilinealty: males stay put, females move to the left, and goods in exchange to the right (signs as in Fig 41).

Thus, when a male dies, his natal and children's group will be present, together with representatives of the group where his sister has gone (the group to the left); and besides her children, her brother's and natal group will also be present at the coffin of a female, i.e., the group to the right.

In both cases therefore, when a male has died, representatives from the left-hand group will be at the burial, together with members of his residential group; and where the deceased was a female, the right-hand group will attend the funeral, together with her residential group. Also, all possibly relevant relatives live within the groups mentioned.

Bilineal (disharmonic) *regimes*, in combination with matrilateral cross-cousin preferences, have markedly different consequences, as descent and residence are at right angles to one another:

– In the *uxorilocal and patrilineal* case (Fig. 43) the females of a residential group are always married to men born to the residential group at the right, who are of a different line of descent in every generation. Also, their brothers go to live with the females of the residential group on the other side, who belong to different descent groups from generation to generation.

Thus, when a female dies, her residential, natal, and children's group will of course be present, but also her brother, living to the right in the drawing; her patrilineal relatives live to the right (same generation), in her own group (first degree relatives), and to the left (second degree relatives). And when her husband dies, apart from the residential (his wife's) group, his sister's (husband's) and natal group will be at the grave (the group to the left); his patrilineal relatives live to the left, one (first degree) and two (second degree) residential groups away.

– In the *virilocal and matrilineal* case (Fig. 44) the males of a residential group always acquire their wives from the same residential group (the group to the right in the drawing), who are of a different line of descent in each generation; similarly, their sisters always go to the same residential group (on the other side), where every generation members of a different matriline live.

Thus, when a male dies, his natal and children's group will be present, together with representatives of the group his sister has joined (i.e., the residential group to the left); his matrilineal relatives live to the left (same generation), in his own group (first degree relatives), and to the right (second degree relatives). And when his wife dies, besides her children's and residential group, her brother's and natal group (living to the right) will be present at the coffin; her matrilineal relatives live to the right, one (first ascending generation) and two (second ascending generation) residential groups away.

In both bilineal cases therefore, the resident sex (males in the virilocal, females in the uxorilocal case) will have its most important relations with the adjacent groups (alliances for the males stand to the left, and for females to the right; with their descent line's representatives to both sides), while the relations for the sex that came to live with the residential sex are to one side only, extending some groups away (to the left for males in the patrilineal case, and to the right for females in the matrilineal case). In Table 36 the several obligations have been worked out, with relations to the left marked by + (this being the regular direction from which exchange goods are to be expected) and to the right by – (this being the side to which exchange gifts would normally flow), in the hope that it is possible to trace one of these patterns in the grave gifts.

As a starting point, the importers/monopolists of the grave gifts were derived from Fig. 38: the group or sector showing the highest relative frequency is assumed to be the introducer of a category. Still, there are two difficulties with this approach:

– *ex aequo* rankings of some groups (e.g., groups III and IV for red ochre), from which only an abiguous determination as to originator can be made; and:

– the elusive sector II goods (cf. note 7).

The latter problem can be dealt with by *summing* acorss all sectors which will probably result in a lessening, if not a disappearance of its effects. The former cannot be handled adequately, because of the differences between the minimum and the maximum counts in Table 37. In that table, the burying group is defined as "distance zero"; originating groups in the direction of the exchange chain (i.e., to the right) are posited at a "negative" distance, as the goods are to run against the current; groups on the other side are counted "positively" accordingly. A distance of two groups is ambiguous in its sign in a four group system, obviously.

Two points seem to emerge from the table:

1. From the high incidence of gifts from two groups away, a homogeneous regime (in either of its guises) can be considered unlikely, perhaps even falsified.

2. The female selection is quite clearly more unidirectional than that in the male graves, which fits in with the virilocal-matrilineal model of a disharmonic structure of Table 36.[10] Here also, the number of deviations is an indicator of the discrepancies existing between statistical (Table 37) and mechanical models (Table 36)

The following points also merit attention:

1. The combination of asymmetrical ("generalized") exchange and bilineality ("disharmonic regimes") is categorically negated by Lévi-Strauss (1969b: 233-234). He gives no reasons for that denial, although the examples in Part One of that book are in line with it. Yet apart from different findings in the Leiden-School (e.g., De Josselin de Jong 1977a; Moyer 1976) there also remains Radcliffe-Brown's observation of a marked asymmetrical bias in the workings of the Kariera system (quoted in Lévi-Strauss 1969: 219).

2. Regarding the "invisible" moiety division, several possibilities exist. Although there are four groups in the Elsloo graveyard, this does not imply that there are four matri- and/or four patrilineages:

(a) Virilocal moieties, combined with matrilineal ones (thus the classical "marriage class" system). In this case MBD and FZD ("father's sister's daughter") are identical; at least in a classificatory sense they are the "bilateral cross-cousin". This is at variance with the asymmetrical exchange derived above.

(b) Matrilineal moieties and n patrilineages of a residential nature (in this case, $n = 4$; but any number $n \geqslant 3$ will do). Here, MBD always pertains to the companion moiety, and also to any other lineage than Ego's. The lineage is not specified as in the class system; instead, an alliance between the lineages is necessary to designate the "required" partner, and this would either result in assymetrical exchange when "givers" and "takers of wives" are different groups, or in symmetrical exchange when the two were merged. In Elsloo, the former system seems to have been adopted.

(c) An inverse solution of (b): a patri-duality plus n uxorilocal lineages. However, in the next section, a matri-duality will be made visible; also, a uxorilocal-patrilineal solution seems to be unworkable (cf. note 10).

To sum up:

– Four groups could be derived for the Elsloo cemetery by a consideration of the patterns of relative over- and under-representation of the grave gifts. The groups or sectors are labeled counterclockwise I-II-III-IV, beginning in the northeastern, or upper right-hand corner.

– From the relative frequencies of the various gifts per sector an asymmetrical and linear exchange system could be traced, every group introducing other categories of goods into the network. The exchange relations were unidirectional: I $\rightarrow$ III $\rightarrow$ II $\rightarrow$ IV ($\rightarrow$ I). If this has been the mechanical model, however, statistical aberrations occur: five (out of eleven) categories of goods were exchanged in other, different sequences. And also, for no category of goods could an origin in sector II be derived – perhaps, because of the size of the group which required all its "surplus" to alleviate the demand for women (and of course, not all things exchanged need to have survived 6000 years in the grave). Finally, some categories could not be tied to a single sector due to equal frequencies.

– From the distances of the grave gifts to their supposed originator groups a bilineal definition of the groups was derived: virilocal residence and matrilineal descent seem to constitute the mechanical model of the group relations at Elsloo. Here, too, a number of deviations are to be observed between (mechanical) model and (statistical) practice.
– Although the subdivision into groups is statistically significant, and the other points result from a comparison of the data with a number of alternative theoretical possibilities, the various outcomes are *not* independent of one another. The disharmonic regime hinges upon the correctness of the asymmetrical and linear exchange interpretation, which in its turn depends in part on the sectorial subdivision. It is the coherence of the several results which suggests some reliability, but this should be tested in the nearby village.

5. *The Elsloo graveyard: a neo-Marxist interpretation*

The previous sections were primarily concerned with the form of the social structure; indeed, the structuralist analysis made use of the things exchanged (grave gifts) to elucidate the relations as channels of information between groups. These relations, and the positions united by them, certainly did not exist by themselves, and one may inquire into their functions (Godelier 1977 (I): 115). One possible general answer can be provided in Engels' succinct statement, "the production and reproduction of immediate life" – i.e., the production of subsistence and the reproduction of the species (Engels n.d.: 67), together, the reproduction of man as a social being (Danilova 1971: 278, 312). The reproductive process can be organized in many different ways or modes, characterized by as many different sets of productive relations. "Mode of production" is an abstract concept, not indicating any particular production line or cooperating group, but rather a way in which the relations are organized: wage labour is typical for the capitalist mode of production and occurs in as different economic sectors as farming and heavy industry.

Apart from the relations of production, a mode of production also comprises a technical aspect (the "forces of production", such as labour and means) and the ideological charter. The levels or aspects are quite autonomous vis-à-vis each other, as each level carries its own momentum (Godelier 1978). Yet the levels are also coupled one to another via positive and negative feedback loops (cf. Berger 1976), and it is the development of the factors of production (i.e., the technological and/or organizational sophistication) which sets the limits or constraints within which the mode, the system, can vary. The structures of the levels should be compatible, at least partially; the more discrepancies ("noncorrespondence"), the more tensions ("contradictions"). In that way, development at any one level will result in change at the other levels of the mode of production.

Also, a mode of production is relatively independent or autonomous as a subsystem within the total reproductive system. A number of different but interconnected modes make up a "social formation" (in this case conceived of as the minimal set of relations capable of reproduction of the social life of a village community). Among the modes making up a social formation, one will be dominant; notwithstanding that, the other modes are relatively autonomous in that they consist of their own structured sets of relations. This relative autonomy is, again, subject to change: a mode now dominant may gradually be replaced by another. The mythical or ideological superstructure of the social formation is the locus of integration of the ideological components of the various modes within it; even the various elements may acquire new significance in this integration.

For instance, in Medieval Europe two modes of production existed side by side: a domestic one among the peasantry (comprising the bulk of the population) and a feudal one among the landed gentry (who wielded political power, and thus the feudal one was dominant in that sense; Danilova 1971: 293-294). A slight increase in agricultural efficiency ("development of the productive forces") resulted in a group of jobless people ("free labour") which, when organized in manufacture ("united with capital") gave rise to the capitalist mode of production. Gradually, the latter gained dominance within the western European social formation (Godelier 1977(II): 217).

By now my position should be clear to connoisseurs. I will evade polemics and exegetics, and therefore just list the differences within the various schools of Marx's interpreters. By allowing more than one mode of production within a social formation, the one-formation-one-mode axiom of Marxism-Leninism and of cultural materialism alike, is put aside (Petrova-Averkieva 1977:28; Harris 1968: 240, 258; and Harris 1975). The omission of "dialectics" to describe the relations of opposition or even of identity of the productive forces and the relations of production will alienate orthodox Marixsts (e.g., Wolf 1975). Then a sociological approach to the relations of production, i.e., primarily as *social* relations, is in accordance with statements of, among others, O'Laughlin (1975), but apparently at variance with Meillassoux et al. (Friedman 1974: 446). Regarding the nature of the relations of production in community society, I will not assume that an autonomous and pre-given kin system absorbed the relatively unimportant social aspect of the simple production at that level (as does Danilova 1971) but instead assume that kin relations were that important because they also fulfilled economic functions – in fact, were selected for and determined by these functions from among the possible alternatives (which is in line with, among others, Godelier (1977 (I): 31, 83-84, 89) and Friedman (1974)). That is, the kin relations *are* the social relations of production in community society (Godelier 1978). I do think that continuity of the species is of less immediate concern than the securing and maintenance of the labour force, of which women constitute a critical part in primitive society. Or, the base is determinant "in the last instance", as it is so often phrased.

There is one critique I would like to counter in advance. In Wolf's review of a set of Godelier's articles (Wolf 1975) it is stated that dropping the Hegel-Marx notion of "identity of opposites" (i.e., of productive forces and relations of production) for a "unity of opposites" has quite serious consequences. A dialectical opposition like identity, automatically builds tension ("contradictions") into a mode of production. By foresaking this ontological tension, change should be imported from outside the mode of production, from which necessarily an ecologistic or functionalistic outcome follows (a similar critique is arrived at in H.K. Schneider 1975b). However, conceiving of relations and forces as relatively autonomous structures within a mode of production, developments in either or both are likely to bring about tensions; surely, micro-demographic changes and technological innovations are not confined to ecologistic monopolies. Similarly, change may result from pressure exerted by other modes of production within the same social formation, which can be strictly seen as environment of the mode considered, but certainly not an ecological factor. And if some special formation has a "history" which is "just more of the same" for a considerable period of time (Schneider 1975a), then one cause may be in an absence of technological innovations; other causes may be found in the absence of serious demographic fluctuations (the kin system may be geared to dampening the effects of demographic fluctuation), or in a permanence of ecological equilibria, and perhaps even in an effective blocking from without of any development of the productive forces – all of which may be or have been the case at one time or another.

Description of any historical social formation should then indicate the number and nature of the various modes of production making up its economic base, their corresponding social and ideological structures, the ways in which these modes are interconnected (i.e., their hierarchy). Ideally, the superstructure with its elements from the various modes should also be considered (Godelier 1977 (I): 177-178); it is at this locus that the "etic" infrastructures of the modes of production are integrated and legitimated in an "emic" superstructure. However, given the current state of the art of archaeology the ideological points need not further detain us.

There is no archaeological research along the lines of a neo-Marxist model known to me; perhaps some are to be expected in the near future, as a reading of the Spriggs 1977 volume suggests. Rather favorable examinations by non-Marxist anthropologists of some of the points raised above are to be found in Firth 1972 and Schneider 1975a. Quite a number of anthropological analyses of concrete historical formations have appeared in recent years: Godelier himself on Central Australia, a New Guinea tribe, the Pygmy hunters of the Congo Forest, and the Incas in Godelier 1977, and the several contributions to the Block 1975a volume, are cases in point.

Some consideration should be given as to what to expect, modes of production are not immediately obvious from the data. In a graveyard the manifestation of the productive forces will be little apparent, and (partial) definition of the modes should therefore proceed from the relations of production (that is, appropriative relations) that can be observed. Elucidation of relations, as demonstrated by the previous section, is a matter of tedious analysis, but it can be done. Unfortunately, no listing exists of all models of production known, nor has a formal procedure been developed for the construction of the possibilities.

Scouring the *descriptive* literature (cf. Legros 1977), three (perhaps four) modes of production are reported to exist on the Neolithic level (here in the narrow meaning of simple technology and primitive agriculture), with from case-to-case differring degrees of dominance:

1. The *domestic mode of production sensu stricto* (Sahlins 1972: 77): The household or family as a productive and consumptive unit incorporates both the major divisions of labour (principally according to sex, but also to age) and a way of appropriation (redistribution) of the products of the members per household unit, through which its social and physical continuity is assured. At the same time it also provides for its own biological reproduction through the marriage bond at its core (cf. Rapp 1977: 319). From Sahlins' account, this mode of production seems to be compatible with very divergent levels of development of the forces of production: from the horde of hunters to the community of early modern European peasantry its functioning is reported.

2. The *lineage mode of production* (Sahlins 1961) is generally superimposed upon the previous one. Yet its occurrence is not necessarily dependent on the pre-existence of the domestic mode: the lineage structure may absorb all functions of the latter (Terray 1975: 106-107). Empirically, the lineage way of production seems to be compatible with a limited range of technological achievement only – it is probably restricted to hoe-agriculture and/or slash-and-burn farming. It consists of local kin-segments, each of a number of households arranged in importance according to some ordering principle in the kin/descent sphere. Its separate status derives from the larger organizational cadre (than the domestic mode of production), which is translated in obligations differentiated according to relative place in the hierarchy. Again, the social and biological principles of descent and aging serve to recruit the proper candidates for the positions within the lineage structure. The uneven access to products is maintained through the legitimization provided by the ideological superstructure.

3. A very primitive mode of production, unnamed as yet; it is the organization of the productive process in cooperation, or rather, working side-

by-side, with leadership of a purely instrumental nature based upon individual ability (Legros 1977). The differential possesion of skills will, in conjunction with an egalitarian ideology, secure the persistence of this mode, however transient its articulation may be. This mode is reported to occur at very divergent stages of productive sophistication: Eskimo at one extreme, West African peasants at the other (Nooter 1977: 88; Legros 1977: 30-31). Below, this mode will be referred to as the *loose mode of production*, to emphasize its volatility. In fact, it is the appropriation (apparently without remuneration) of the skills of some individual by the group, which renders this mode distinct.

4. Perhaps a *supralocal mode of production* may have been present in Neolithic social formations as well. It should be co-extensive with the geographically largest kin system. According to Godelier (1975: 9) this productive system is activated in the case of stress: shortage of means (women) or forces (exhaustion of the local milieu) of production will make people turn to other relationships for survival and/or reproduction. Although Godelier is writing about central Australians with a hunting and gathering adaptation and a Mesolithic tool kit, this very mode of spreading the risks seems to be compatible with Neolithic society as well (e.g., Bloch 1975b: 208; also Service 1975: 65). The (analytical) distinctiveness of this mode is in the securing of access to means and factors not ordinarily used by laying kinship claims, however remote. Any instance of these claims is likely to establish a precedent for future reference, which results in a perpetuation of this mode. From this mode the fluidity of "tribal" boundaries (Fried 1975: 14, 67) or of the circumscription of the social formation is immediately apparent.

As said above, no way has as yet been developed of establishing whether all possible modes of production have been listed, neither at the Neolithic nor at any other level. Together, the domestic, lineage, and supralocal modes, however, can be expected to exhaust the possibilities of kin relations. The addition of the loose mode of production is an indicator that other determinants may also play roles; their range is unspecified.

The notion of "modes of production" emphasizes aspects of the attributes of the graveyard other than those indicated in the positional and structural paradigms. Empirically, the *domestic mode of production* will probably be visible in a close and pair-wise positioning of male and female graves as an indicator of the marriage bond, the (re-)productive relation specific to it. Males and females will be accompanied by tool kits that are different to the degree a sexual division of labour was known in the Bandkeramik. In Fig. 27 the differentiation of sexes as derived from this sex-specificity (which was established from a principal components analysis, cf. the third section) is shown, with superimposed upon it the averaged distance from female to male graves (from Table 38). In the plan, and also in Table 46, it can be seen that among 21 female graves 14 have male or indeterminable partners within a distance of 2.55 + .50 metres (mean + one standard deviation), with another four similarly but at a slightly larger distance; the remaining two female interments can be thought of as unaccompanied, because other graves are at a considerably larger distance. Conversely, among 25 male graves, 16 have a partner grave of female or indeterminable sex at less than the distance mentioned, and one at a slightly larger distance; the eight remaining graves seem to be by themselves.[11]

When the paired occurrence of graves of opposite sexes and the partial specialization of the tool kits along sex lines are interpreted as vestiges of a household ("domestic") mode of production, it may be enquired whether a *Lineage mode of production* was also present in the Bandkeramik praxis. One of the attributes of this structure is probably to be found in a grouping of the burials along kin principles. In the previous section the "origin" of the grave goods (introducer groups in the exchange system) was used to derive virilocal residence and matrilineal descent. Thus, the patri-principle manifested itself in the locality of the graves: those sharing this determinant were buried in the same sector of the graveyard. While the matri-system was found to account for the remainder of the variation in the origins of the grave goods the demonstration was quite complex, and we might try to find a more direct sign of this principle. Introducing again the principal components analysis, it can be said *a priori* that two different situations can be thought of: if a dual organization has been the case, this could have found expression in a bipolar component (cf. note 3), as such a dichotomy might (*not necessarily*) have been described by antagonistic symbols. If, on the contrary, more groups have been involved, and/or the associated markers (or group badges) were elements of a symbolic system alien to us (Lévi-Strauss 1955: 136, 258) then that set would appear arbitrary and be visible in any number of specific components (this latter possibility also covers a two-group system, of course). On the idea that a dual organization is a fairly frequent phenomenon in kinship society (Lévi-Strauss 1956; Lévi-Strauss 1969b: 70), the one bipolar principal component (no. 2, Table 10) was first tested for it (later the other components found other explanations unrelated to the present issue). This component opposes only two attributes, curvilinearity and rectilinearity of pottery decoration (the variable STRUCTURE). These traits are very asymmetrically distributed over the sexes (Tables 38, 39); chances are only 1:100 that this asymmetry is due to sampling error because of the relatively small number of the graves (34). Nor is it due to the small number of grave gifts: if marked this way, female graves contain one pot or seven to ten sherds, and male graves up to three vessels of five to forty sherds, with the minimum number of sherds (five) precisely occurring in a doubly marked grave. The asymmetry can be explained by a matrilineal duality: females "defining" this affiliation, males will regularly become associated with the opposite kin grouping (exogamy!) and buried with gifts from both (natal and marital) sides. It is a pity that this duality is only visible in graves containing decorated pottery; the affiliation of the other interments cannot be known. (However, some comfort can be found in the observation that none of the existing classificatory schemes of LBK pottery decoration allows for a discrimination along these lines.) As is to be expected under virilocal residence rules, matrilineal markers are dispersed all over the cemetery, which strengthens the case presented in the previous section.

Besides kin alignment, a hierarchical arrangement of the households or individuals can be taken as a sign of the lineage mode of production: a hierarchy of wealth. "Wealth" at this level of development should be interpreted in a general (qualitative) sense, and not be equated with the possessions of (or being accompanied by) specific tool kits or quantities of gifts (Sahlins 1972: 93) – as, at our technological level, a wealthy family would not buy for its own use a *block* of apartments on, say, the Costa del Sol, but rather one apartment in a number of different pleasant places. In the lineage mode, relations are tied to positions in a kin system and not to differential control of the means of production (i.e., tools, but not women). Moreover, those valuables which existed had use-value at best (Sahlins 1972: 83, 93). Regarding "rank", as this differential is usually called, in ranked society (Fried 1967: 109) wealth is associated with giving away, which sets commitments to the recipients (Sahlins 1972: 205). Such obligations are likely to be cashed at death. Ranking or graded dignity of individual interments has already been derived in the third section and need not to be repeated here; a slightly different distribution can be worked out on the basis of the number of finished objects in the graves (which is essentially, the "labour input" criterium for Table 23 where, however, all excavated objects were included). These interments are grouped in Table 41 and listed in Table 42; this should be compared with

Tables 24, 25 and 26.

Interpretation of these "wealth" differences as vestiges of a lineage mode of production rather than of differences of achievement of the individual household (i.e., within the domestic mode) follows from the character of the latter: nothing more is produced than needed for survival, as any surplus would be siphoned off through sharing mechanisms (Sahlins 1972: 127). The lineage mode provides a counterforce to these egalitarian trends. Its organizational principle of placing the constituent units in hierarchical order of importance is established to assure differential access.

The *loose mode of production* is not necessarily tied to kin principles – manual, corporeal dexterity tends to be distributed in a random way (Dahrendorf 1968: 19). If this mode of production did find expression in the grave gifts, this is likely to be in a specialization of them. As far as specialization is tied to material instruments, a set of similar or related tools will be witness to it; there may have been other specializations, not related to still excavatable items: genealogical specialists, story tellers, dancers, mask makers, tattooing specialists (to mention but a few of the most obvious) will never be traceable at Elsloo. In other words, existence of the loose mode of production can be made probable through the occurrence of specialist tool sets, its extent (i.e., the number of skills exploited this way) will remain unknown.

It should be added that distinction within the lineage mode of production, which is visible in a broad spectrum grave inventory, may crosscut a marking of ability by specialization of gifts. Just as people simultaneously managed their own household in the way of the domestic mode and took part in the lineage mode, so an able hunter may have occupied a prominent position in the lineage structure too. In cases of this kind, several categories of grave gifts will be present, among which one will be quantitatively emphasized.

Table 43 was the result of an attempt to compare the number of items per category to separate skill from dignity. For instance, in the female grave no. 014 a broad spectrum inventory was found: a decorated pot, an arrowhead, an adze (type II), and a grinding stone with attendant lumps of red ochre – no specialization visible here. Likewise, in grave no. 083 (also female) a similarly diversified tool set was found accompanied by three adzes (of types I, II and IV); the status of this woman in the lineage mode accounts for this seemingly specialist set (cf. p. 95).

Among the graves in this table, five are marked by arrowheads or blades (nos. 003, 005, 056, 067, and 071) and two to a lesser extent (nos. 021 and 025), all males. One female grave (no. 106) held a number of pieces of flint debris. Three male graves (nos. 005, 096, 112) held three or four pots and another two (also males) two pots (003, 067). Four graves lying perpendicularly to the regular northwest-southeast orientation are also grouped with the specialists: 014, 023, 041, 058 (one female, 014; the other ones indeterminable). Seven of the specialists occupied high standings in the lineage sphere: grave nos. 003, 005, 014, 056, 067, 071, 106; four were accorded moderate status according to the activities criterion of the third section: 021, 025, 096, and 112; the remainder (023, 041, and 058) a low status. Also cf. Table 50 (N.B.: Those graves thought of as being of a chiefly status – nos. 001, 087, and 100 and possibly 083 also – are omitted from consideration here).

As a sideline, from the low number of (visible) specialist graves – fourteen over a period of a century – an absence of institutionalized craft heredity can be inferred. Such a heredity would run counter to the "definition" of the loose mode of production and perhaps be indicative of another type of specialists' organization.

The fourth *mode of production* occurring in Neolithic societies may be the *supralocal* one. According to descriptions, it is activated in times of stress only: when the supply of women (as an indispensible part of the labour force and as means of biological reproduction of the body collective) runs short, or when the local eco-system does not yield sufficient produce in a broad sense (unbalanced or even insufficient productive level through ecological fluctuation, degradation, political and/or demographic pressure, etc.). *A priori* it can be stated that in the region of Elsloo considerable stress must have been present, at least in the youngest phase of the graveyard's use. With its closure, the LBK occupation of the area came to an end (kind and cause of this stress are still to be unraveled: Bakels 1978: 126-127; Kuper and Lüning 1975: 93). If, then, this stress did indeed result in the activation of a supralocal insurance against risks, this should be visible in foreign-looking grave goods from the "internationalization" (extension) of the relations during the later stages of the use of the graveyard. "Foreign-looking" should be considered in a broad meaning: intra-regional differences were not that marked to be discernible to us as yet (even in this younger period of the LBK). When gifts are recognized as "alien", they will come from farther away. One factor may confuse our sight, though: valuables may have been exchanged in the non-kin sphere, probably based upon the lineage mode of production (Terray 1975: 96, 106). However, parallel to the separation of lineage and loose modes (above, p. 111), a differentiation of lineages and supralocal modes can be established: a prominent (i.e., wealthier) lineage member who bartered foreign things will be accompanied by his conversation pieces among the other, more local artifacts. On the other hand, a genuine allochthone (never likely to have seen been prominent in the local hierarchies) will be marked by at best a few momentos from his/her native villages.

As formulated this way, foreigners' graves should be visible in the principal components analysis, at least when the codebook is sufficiently detailed: different cultural backgrounds should be singled out by as many specific components: one is a foreigner, or one is not. Among the components in Table 10 only no. 6 has not attracted attention as yet. Its specific character, and even more the two variables loading high on it make it a likely candidate for allochthony. Apparently late in relative time (cf. the loadings of single- and multidented spatula in the table), the nine, possibly eleven graves indicated by this component seem to be marked by either stab-and-drag decorated pots and/or by type I adzes. Both characteristics suggest foreign contacts: stab-and-drag are more at home in the southeast than in Limburg (cf. Ch. I, Section 3); and type I adzes have also been reported from non-LBK (Hinkelstein) backgrounds (Modderman 1970 (I): 188).[12] Apparently, some of the pots in the graveyard also allude to that group (cf. Gabriel 1976: 56 and Fig. 4 no. 2, with a number of pots from Elsloo, e.g., graves nos. 047, 056, 067 (no. 2), 071 (no. 8), 073 (no. 2), even 093 (no. 1), 096 (no. 3), 097, 104, 106; all illustrated in Modderman 1970 (II).

Among the eleven graves with foreign flavours (Table 44) there are two interments accompanied by only a few stab-and-drag decorated sherds; their incorporation in the category of allophiles is therefore dubious (graves nos. 074, 101). Of the remainder, one grave is of the dubious female (087) also prominent in the lineage structure, so that the presence of an adze of type I in her grave should perhaps be considered a consequence of her lineage status rather than a foreign pedigree. Then, the female grave 083 held three different types of adzes (I, II, IV) together with a fair sample of the other grave goods, apparently indicating a super-status in the lineage structure; yet as her grave also held a pot with stab-and-drag decoration, the evidence regarding her origins is questionable.

Regarding imported *people* (and if an interpretation as offered here is accepted), no virilocal residence rule is visible in the distribution of allochthony over the sexes (Table 45).

To conclude this section, some points still have to be discussed in connection with the various modes of production:

1. *Domestic mode of production:* when the average distance to the nearest neighbour (as detailed

above, p. 111, Table 38) is accepted as an indicator of closeness of relationship (i.e., of a marriage bond), an attempt can be made to push the interpretation of some of the data further: chronological differences (as far as discernible) do not interfere with an explanation of the pairing of the graves in Table 46 as "caused" by marriage. Then ascribing the appropriate sex to the several previously indeterminable graves, another 13 male and 12 female graves can be added to those listed in Table 18. This is a rather indirect way of sex determination, as it involves more steps than direct conclusion from the grave gifts; therefore, these 25 graves have been kept apart in all analyses, although the evidence was nowhere contradictory.

For instance, the division of labour among the sexes as demonstrated by a differentiation of the associated toolkits of Table 17 (based upon the directly sexed graves) is very much like that in Table 47. And here, even clearer than before, the level of significance is not a matter of arbitrary choice, but follows immediately from the data: p =.05 is right in the wide gap that separates the sex-specific categories of grave gifts from the general ones.

When all these changes are incorporated, the sex ratio becomes 21 + 12 females against 25 × 13 males or .46. In other words: four females to every five males (71 graves counted). This ratio is identical with that established in the third section on the basis of the directly sexed interments.

This pushing of the evidence has hardly any consequences for the distribution of structures of ceramic decoration over the sexes: three indeterminable graves with curvilinear and also three with rectilinear designs can now be grouped with the other female graves with these characteristics; one grave with curvilinear and one grave with both types of decoration can now be labeled "male" (Table 40).

2. *Lineage mode of production:* crossing a dual matrilineal organization with a virilocal system has different consequences with the number of residence groups involved. When there is a duality, then there are four marriage classes, when a four-group system, then eight classes are generated, and so on.[13] Given a matri-duality, addition of a simple patri-duality is sufficient to produce the four groups established in the previous section. Even if the virilocal aspect of the structure is denied, there are two other marker variables to indicate different classes. The curvilinear *vs.* rectilinear design on earthenware has already been mentioned (Table 40); its interpretation as a matrilineal system seems to be almost fool-proof. Then, along with the previous one, there is another conspicuous dualism, which opposes graves with spiral-decorated ware to those with a wavy design on their pots. In this case, however, there is no overlap: graves are either of the crook or of the hook variety (Tables 48, 49); again, both types occur with both sexes, all over the graveyard and in all phases (chronological distribution not shown here).

An interpretation of these phenomena as a four marriage-class system has been rejected on p. 107.[14] The matri-duality observable in the lineage mode, and the asymmetrical exchange relations are at variance with a four-class system.

3. *Loose mode of production:* a glimpse of the Bandkeramik people's appreciative scale can perhaps be wrested from the distribution of the arrowheads in the graveyard. When these tools can be thought of as somehow linked to hunting, then from the standing of the small number of specialists (5 plus 2, to a total of 113 graves) in the lineage system (Table 50; also p. 113) it might be inferred that they made a good thing out of their ability, which was not appropriated by the group without remuneration, as hypothesized above p. 111). Compared with the other specialisms visible, only the specialization associated with pots seems to have met with similar recognition.

4. *Supralocal mode of production:* on p. 114 I stated that allochthones were probably never in a high position in the local hierarchy. Table 51 does not support this statement. However, from note 14 it

can be supposed that many more people in the Elsloo cemetery were not born at Elsloo, but at other neighbouring villages. These people are not recognizable by present methods, and so the statement alluded to cannot be falsified. Another point in this connection is that these foreigners were apparently imported in a relatively late phase of the graveyard's use, that is, insofar as their graves contain decorated pottery. Extending this date to the other allochthones (those marked by adzes of type I only), gives an indirect dating for another three graves (nos. 066, 085; plus the lineage prominent authochtone in grave no. 087), which has been entered on Fig. 26 as well.

5. *Dominance:* no conclusive answer can be given when it is asked which of the four modes of production has been dominant. Some thin lines of evidence can be gathered, however, and their joint direction is suggestive, though hardly surprising. The presence of allogenic artifacts in the graves of lineage prominents, it was argued above (p. 114), might be indicative of use of their position to create an exchange sector of their own, i.e., outside their lineages. In that way, surplus produced in the domestic sphere was siphoned off by the lineage mode. Then, on an *a priori* basis it can be argued that the supralocal mode of production was set up to keep the local social formation running; i.e., both the lineage and the domestic modes.

However, if this indicates a dominant position of the lineage organization, towards the end of the graveyard's use the course of events was possibly reversed. Every structure has an own momentum, and in due time a "parental" structure (like the lineage, in this case) may become dominated by its "offspring" (cf., e.g., Friedman 1975). Thus, in contradiction with the apparent virilocal ideology of the Elsloo natives, at least some four of five males (out of 38) seem to have been imported in a relatively short period – say, the terminal generation.[15] One wonders how many such visitors are hidden among the graves with no gifts to announce their home country. Such an influx may be interpreted as a sign of dominance (if only an initial one) of the lineage structure over the supralocal relations, but also as a sign of the inability of the local social formation to keep the affairs going the way they did before – and this latter interpretation (still a descriptive one, not an explanation!) might provide a beginning towards understanding the process of abandonment of the Dutch Limburg LBK territory.

Let us now turn to the settlements themselves, and see what parts of the various mechanical models can be made visible within a more restricted data set.

NOTES

[1] As a starting point for the discussion and the analysis I assume Early and Middle Neolithic society to have been of a tribal, or rather "community" nature (Fried 1975: 98). Other names for that level of socio-economic integration are:
– "lower and middle barbarism" (opposed to "savagery" on the one hand, and "upper barbarism" and "civilization" on the other; Morgan 1877);
– "ranking society" (as opposed to "egalitarian"; Fried 1975: 98; and to "stratified society"; Fried 1967: 109);
– "segmental society" (as against "hierarchical"; Service 1975: 304).

In short, that part of the evolutionary process which was taking place with kinship and society still largely identical, not transcended by wider political sodalities (Claessen and Kloos 1978: 82-89; Fried 1975: 98; Service 1971: 13-14, 99-100).

[2] Yet, from oral communication, there is disagreement about this interpretation: Prof. Modderman thinks the graveyard was used in two phases, with a relocation in an easterly direction after the first phase. Also, there is an overlap zone extending north-south, located approximately at the division line between the three westerly sectors and the remainder of the field in Fig. 30.

Against this, my interpretation is that a *gradual shift* was made in the use of the graveyard, with the different groups burying their deceased in separate corners. Then, the four group configuration of Fig. 32 and Section 4 should have remained essentially the same throughout the cemetery's use.

In the latter case, the datable graves are distributed as in the following table:

Phase	O	M	Y	
Group I	1	6	4(6)	11(13)
Group II	4	2	1(2)	7(8)
Group III	5	2	1	8
Group IV	1	3	5	9
	11	13	11(14)	35(38)

Items between parentheses indicate graves indirectly dated; cf. text.

which can be condensed to reveal the east-west opposition in the following way:

Phases	O	M	Y	
Westerly groups (II, III)	9	4	2	(15)
Easterly groups (I, IV)	2	9	9	(29)

$\chi^2 = 10.33$ (df = 2) p = .005

which is truly asymmetrical, indeed.

Considering the former case, (i.e., the gradual shift) the new centres of use come closer when (on the basis of the factor score clusterings) the second phase is divided up between the younger and the older, as can be expected, of course. Also, a test for relocation in a two phase division is negative:

Phase	Old	Young
Group I	7	4
Group II	5	2
Group III	6	2
Group IV	3	6
	21	14

This table can be condensed in a similar way as above:

	O	Y	
Westerly groups (II, III)	11	4	15
Easterly groups (I, IV)	10	10	20

$\chi^2 = 1.94$ (df = 1) p = .18

The above might affect Section 4 of the present chapter. However, apart from the tests for asymmetry reported here (based on one-third of the graves), it should also be noted that the low densities of the grave gifts render a rigourous three-fold division unworkable. Therefore, that section has been set up as if all of the graveyard had been in simultaneous use; after all, the notion of social structure employed there is quite insensitive to historical events (cf. Bertels 1973: 259; Sahlins 1976: 41-42).

[3] Principal components are defined as the best possible combinations of a number of variables in a data set. One of the tables produced by the computer gives the correlation of the principal components with the variables (as in Table 10): "loading" is a synonym for "correlating". High loadings are used to establish the "meaning" of a principal component. The loadings on a component will show one of three different patterns: bipolar, general, or specific (or unipolar; Harman 1967: 100). *Bipolar components* have high positive and high negative loading variables, with no moderate correlations; loadings of about .0 to $\pm$.2 are considered neutral and irrelevant (e.g., component no. 2 in Table 10); interpretation will emphasize contrasting attributes in the relevant cases. A *general component* will exhibit the full gamut of loadings (e.g., component no. 1) from very high through moderate and neutral to moderate and low negative; interpretation will be in terms of a general process affecting all cases. Finally, a *specific component* has an unbalanced or one-sided appearance, with one or more variables loading high, but either only negatively or only positively (e.g., components nos. 3 and 4 in Table 10); in its interpretation the possession by a limited number of cases of some attributes will be considered, the other ones falling outside this set as "have nots".

The positions of the cases on the principal components are calculated from their values on the original variables, through multiplying these by the so-called factor-score coefficients and summing the outcomes: all variables in the analysis contribute to these scores.

[4] In Kuper et al. 1977: 407-408 Zimmermann accounts for graves with only two arrowheads in the Niedermerz and Elsloo cemeteries as being accidentally outfitted that way. He proposes a Poisson regime to cover these cases. However, in his table, the expected frequency of graves with one arrowhead (22 out of 115) is almost double the observed rate (13). As obviously a number of graves have been purposively supplied with arrowheads, this frequency distribution is counter to expectation: a number of graves should hold an accidental arrowhead (i.e., more or less in accordance with a Poisson distribution), but some should also hold a purposive arrowhead. In other words, the observed frequency *should* be at least as high (when "some" =0) or higher than (when "some" $\geqslant$ 1) the expected Poisson frequency. With this, I am not denying that there are graves with only an accidental arrowhead (grave no. 106 at Elsloo is a clear case in point), only their incidence should be less than Zimmermann's computation shows.

[5] I am not willing to criticize this grouping in detail, though some remarks should be made. For instance, I fail to see why grave nos. 092-094 are not grouped with the northeastern group, or why graves nos. 111-113 remain apart from the southern group. Moreover, I do not understand why graves 002-010, 016-018 are not called a group. Then, neither chronological homogeneity within nor heterogeneity between the groups seems to be the case. On my relative dating graves are:

In the NW groups 2nd + 3rd phase (3 graves datable)
In the N groups 2nd + 3rd phase (2 graves datable)
In the NE groups 2nd phase (3 graves datable)
In the SE groups 2nd + 3rd phase (2 graves datable)
In the S groups 1st + 3rd phase (4 graves datable)

Even when the wandering cycle is reduced to much less than the customary 25 years, and when the period of use of the graveyard is extended to a possible maximum of 150 years (Modderman 1970 (I): 206-207), even then some difference should be visible between the groups considered as wholes in the rather coarse datings employed here.

[6] In a personal communication, R.R. Newell (150378) stated that my present approach does not account for exceptions, which renders it arbitrary; instead, he proposed an objective solution which he has worked out himself (Newell and Dekin 1977). I think his objections are right with regard to the arbitrary part of the analysis; yet several points can be brought forwards in defense of the present subdivision: (1) any "objective" approach cannot take account of the noise which differentiates statistical from mechanical models; and (2) the coherence and the consistency of the remainder of this section consitute an internal validation (a kind of "construct validation": Selltiz et al. 1966: 160) and there are also independent checks of some parts of it in the next section and in the next chapter. The structure may be arbitrary *a priori*, it is necessary *a posteriori* (Lévi-Strauss 1970: 13). Apart from these two arguments, I also contend that the evidence from Tables 31 and 32 does not refute the proposed patterning.

[7] It will be observed that all groups except group II in their turn assume the leading position in the orderings, thus indicating their "source" or "importer" monopolies. Given the size of group II (double that of the other sectors) this might perhaps be explained by its higher demand for women, which causes all surplus to be collected for the exchange circuit (thus leaving less to put into the graves). In this connection it might be interesting to note that group IV, immediately following II (and thus the appropriate source for the women of group II), labeled twice as many times a source group than the other groups are (group I: two or three times; II: none; III: two or three times = IV: seven or five times). A recalculation of the grave densities with group II scaled down to a size comparable with that of the others (i.e., reducing the number of graves without grave gifts in II) did not substantially change the picture of Fig. 38.

[8] To get an impression of "other" possibilities (meaning to see the extent to which the above interpretation can be thought of as unambiguous or falsifiable) the grouping of the graves was randomized: the find numbers were attributed to one of four groups by means of a random table. Then, for each random group, the graves with a given category of gifts were counted, corrected for groups size, standardized, (etc., as in the text for the factual distribution). The results are presented in the table below.

To facilitate comparison, random group A was put in the leading place and the sequence adjusted correspondingly. This resulted in the following distribution of possibilities (counter-circuits indicated by identical letters to the left). From this distribution, apparently no preferential circuit can be derived (although the first possibility is the mirror of the last, and on this basis could be called the favourite one; but then, no women need move along this circuit). As all possibilities are present, I would not know how to interpret this table. Similarly, a comparison of the frequency profiles with Figs. 34 to 37(b) suggests the following characterizations:

+ complex: 1 category (red ochre)

+ asymmetrical and linear: 5 or 6 categories (undecorated sherds, arrowheads, adzes type III, decorated pots, and possibly querns too)

+ asymmetrical and alternating: perhaps 2 categories (adzes IV-VI, blades)

+ symmetrical: 1 or 2 categories (charcoal, possibly undecorated pots as well).

Random group	A	B	C	D	Total	Sequence of exchange between random groups:
n of graves	24	41	21	27		
Categories						
undecorated sherds	1.6	.6	1.2	.9	27	ACDB
undecorated pots	.4	1.3	.8	1.3	13	DBCA
decorated sherds	.9	1.0	1.8	.4	21	CBAD
decorated pots	1.1.	.8	.6	1.5	17	DABC
arrowheads	.7	.2	2.5	1.3	13	CDAB
blades	.3	1.3	1.1	1.1	15	BDCA
querns	.9	1.8	.4	.3	15	BACD
red ochre	.8	1.5	.6	.7	18	BADC
charcoal	.9	1.1	.8	1.1	27	BDAC
adezes type III	.7	.6	1.7	1.3	13	CDAB
adezes type IV-VI	1.6	.5	1.1	1.1	15	ADCB

Underlined: *ex aequo*

Possible sequences		Observed times
a	ABCD	3
b	ABDC	1
c	ACBD	1
b	ACDB	2
c	ADBC	1
a	ADCB	3

A distribution of possibilities which is more heterogenous than that of Fig. 38.

I conclude therefore, that the interpretation in the main text of the observed grave goods distributions provides a more homogeneous picture than this randomized set. (The idea for this "check" I owe to A. Zimmermann of the SAP.)

[9] For present purposes, bilateral descent and neo-locality are left out of consideration as being more compatible with complex exchange systems; also, Crow-Omaha systems are left aside as these seem to be too complex to be compatible with elementary structures of exchange (Lévi-Strauss 1969b: 465) and require more than four groups, the number found in the Elsloo graveyard (Lévi-Strauss 1969b: xxxviii). Their unimaginable complexity is not a reason for not considering them (Lévi-Strauss 1969b: xxxvi-xxxix).

Then, too, I am aware of Leach's criticism of Lévi-Strauss's theory of harmonic and disharmonic structures (Leach 1970: 81); I think that his critique is ultimately reducible to an epistemological outlook (positivistic and functionalistic) different from that of Lévi-Strauss (rationalistic, structuralist), rather than to falsifying examples.

[10] Ethnographically, the uxorilocal-patrilineal system has not been observed (Fox 1967: 137; Murdock 1949: 212-219).

[11] From the table, male graves among themselves appear to be more than twice the distance M-F apart (5.62 *vs.* 2.55 metres, respectively), as do female graves (5.97 metres). Also, the nearest neighbour coefficients for homogeneous pairs are slightly less than 1.00 (i.e., close to random dispersion), and .36 for heterogenous pairs – which is considerably towards the clustering end of the scale. Compare also the standard deviations: heterogeneous pairs show far less variation than homogeneous ones.

[12] There are two difficulties with this statement, one terminological and one technical. Regarding the first, it should be stressed that *Hinkelstein* background stands for all synchronic cultures to the south and east of the Limburg Plateau, say, in the modern region of the Rhineland; *it certainly and most emphatically does NOT imply that even one single element (event, artifact, person, group) in the Elsloo graveyard should be thought of as from the vicinity of Worms.* That would be glaring nonsense on practical grounds (no Hinkelstein settlement having being published as yet) and on theoretical grounds (with a notion of cultural borders like that expounded by Clarke 1968: 246-254, 365-388 – following Kroeber c.s.; or Fried 1975). Thus, when speaking of such a "background", or of allusions to that group, etc., I want to convey the meaning: "from outside the Dutch Limburg Loess region, and more specifically, from a south/east direction". Then, the technical difficulty with this so-called Hinkelstein background: the chronological status of that group has not been established beyond doubt. Modderman (1970 (I): 198) states that the Dutch LBK disappeared with the beginnings of Hinkelstein. Dohrn-Ihmig (1976b: 115), on the contrary, parallels Hinkelstein I to the Dutch LBK 2c, d; i.e., the period in which the Elsloo graveyard was in use. Gabriel (1976: note 14, p. 56) even assigns early Hinkelstein to LBK 2a. Thus Hinkelstein-like phenomena in the graveyard may be explained in two ways: (1) the supralocal network became active only in the later period of the graveyard's use – as suggested in the text; (2) the supralocal network was active all the time but only became recognizable with cultural change elsewhere (i.e., Rhineland LBK becoming Hinkelstein).

[13] Note that in data of the kind presented here no distinction can be made between a *clan*-like and a *class*-like marriage system (Lévi-Strauss 1969b: 73). Keeping to the virilocal/matrilineal structure, the positive determination in a class system would imply that the men in group X_i had to contract marriage with women from group X_{i+1}. Conversely, in a clan system these men would have to marry women who were not of their matriline and who were not reared in their (patri-)group, a negative specification (Lévi-Strauss 1969b: 269-270, note).

[14] Modderman estimates the population of the village at somewhere between 54 and 160 on the basis of the living space in the huts; or 85 ± 25 from the graveyard (Modderman 1970 (I): 205-207).

Yengoyan (1969: 196-197) provides ideal population estimates for class systems:

simple dual organization: 262 people.
4 section organization: 530 people.
8 section organization: 1070 people.

If his coroborative findings from hunter-gatherer situations in Australia can be transposed to Early Neolithic groups in temperate Europe, then respectively two, four, or eight *villages* would have been involved in regular marital exchange, given full local exogamy.

Of course, this is a *ceteris paribus* reasoning, but at any rate the scale of the various types of social organization is clear. Moreover, the number of settlements in Dutch LBK Limburg occupied when the graveyard at Elsloo was in use, has been about ten to twelve (Bakels 1978: 50), which seems sufficient (but not necessary) to operate these complicated systems.

[15] Four or five males may have been a considerable proportion of the males alive at that time. If the total is put at 38 male graves (i.e., indirectly sexable included), assuming three generations (cf. p. 84), then there were in all probably $38/3 = 13$ males present at closing time.

CHAPTER V

A TALE OF TWO VILLAGES: BANDKERAMIK SOCIAL STRUCTURE

This fifth and final chapter draws together some lines of evidence to test a number of hypotheses on Bandkeramik social structure. The evidence (if one may call it that) is gathered from an analysis of decorated pottery from two LBK villages: the Dutch site of Elsloo, and the Bavarian site of Hienheim. The former has been extensively reported on in Modderman 1970; its graveyard was studied in some detail in the previous chapter; Hienheim, however, has only been partially described – and yet the present volume was originally conceived of as a monograph on the Neolithic pottery from that site.

The hypotheses to be tested against these data derive from two sources:

1. There is a pile of print dealing with the LBK, some of which is also concerned with its social aspect; from it, ideas can be collected which have been or are still to be tested in one form or another.

2. The study of the Elsloo cemetery reported on in Chapter IV has been written especially in view of generating hypotheses on LBK social structure.

From this, the contents of this chapter can be structured in the following way. In a short introduction (first section) I will signal some recent work on non-Bandkeramik, prehistoric, Neolithic social structure, just to provide a background. In the second section I will deal with some other studies explicitly discussing Bandkeramik social structure; I will also briefly summarize the "findings" of the previous chapter – together, these are the hypotheses to be tested. The third section is a very loosely structured one on method: several points are better presented separate from the evidence. They are mainly concerned with preliminary contemplations of the computer output to be inspected in the subsequent sections, especially the weight that should be accorded to the principal components analyses. Also, the subdivision into phases of the settlement data will be briefly treated there. The next two sections contain the confrontation of data and hypotheses; Elsloo and Hienheim are dealt with separately, and in that order. In both sections I will start with a short summary of what has been published on the site; then the statistical analyses will be presented, followed by a reflection on the second section's contents. The sixth and final section comprises a listing of what has been tested and what has not, plus a number of uncleared problems; as such it should serve as a summary of this chapter.

1. *Introduction*

If the previous chapter has implicitly given an impression of being the first one of its kind, this is patently wrong. I would like to correct that picture by the presentation of an array of examples directly concerned with the establishment of prehistoric social structures.[1] I will present a few general studies first, and then some on the more limited field of Neolithic archaeology, other than Bandkeramik. This is not and cannot be a synopsis; it is rather intended to give an impression of the state of the art insofar as it is relevant for the present study – therefore, it will be highly selective. Though most Neolithic studies touch upon both aspects, for ease of presentation they have been grouped under two headings: those dealing with social structure on a regional (intersite) scale, and those concerned with intra-settlement patterns.

Let us first consider the *general*, *theoretical* writings on prehistoric *social structure*. In the previous chapter, Binford's 1972 article on morturay practices was used when analyzing some aspects of the Elsloo graveyard. The article itself is "positionally" biased (in the meaning of that word employed in Chapter IV), although it seeks to test the proposals of the more structurally minded Van Gennep on burial customs (Van Gennep 1977; orig. 1909). Explicitly theoretical, Binford deals with burials as a distinct class of variable phenomena, both within and between societies. His data basis is a sample of ethnographic descriptions of the mortuary practices in forty pre-state societes, ranging in complexity from the band society of the Bushman Kau to the federation of Ashanti; the sample was drawn from the Human Relations Area Files (which are files of standardized ethnographies; cf. Moore 1973; Lebar 1973). The major conclusion is: variability in burial customs in a society reflects the complexity of the status network of that society more than patterns of ideational innovation or communication between societies (Binford 1972: 238). For the present study, the generalization is interesting that social position (being equal to rank, according to Binford), sex, and "subgroup affiliation" (lineage alignment or "differentiation" in the previous chapter) are most commonly expressed in the mortuary practices of primitive agriculturalists. The article is of a general nature; for specific instances, further elaboration is of course necessary. There is only one criticism worth noting: the equation of wealth and rank, it has been stated earlier, is quite ethnocentric; especially in simple societies, there are very few things to be desired apart from influence and esteem. Consequently, these should be the criteria of rank.

Because he is much more structural in his emphasis on social patterns, Clarke (1968: 235-244) should be mentioned. For him, the social pattern behind an archaeological culture consists of a system of interacting subsystems or subcultures. Five broad varieties should register in the archaeological record: ethnic, regional, occupational, social ("the various sections of particular social hierarchies") and sexual subcultures. At this specific locus, Clarke does not consider the possible visibility of either familistic or sodality (Service 1971: 102) subcultures – though at other places Murdock's kinship studies (Murdock 1949) are frequently cited in an approving way (e.g. Clarke 1968: 109); also, general (e.g., p. 105) and specific (e.g., p. 605) references to kinship studies abound. But certainly an archaeologist writing on kinship in the 'sixties would have risked being laughed at by the entire "fraternity" even more than today. For all its dynamic qualities, the nature of the relations ("feedback loops") in this culture model seems to be so self-evident as to need no further discussion; similarly, how to observe them has never been stated in the literature – but then, are all these relations *only* in the mind of the observer?

Two further instances of general theoretical writing on prehistoric social structure are David 1971 (stressing the status network aspect, leaning heavily on Binford 1968), and Williams 1968 (with a structural bias); both are archaeological studies of ethnographic situations within single sites.

If the amount of general discussions on prehistoric social organization is small (and again I emphasize that the studies just mentioned are but a minor and unrepresentative selection), much more is to be found dealing with specific Early Neolithic status networks or social patterns. A subject I will only touch upon when dealing with the LBK is the *intersite aspect of social structure*. True, a supralocal mode of production has been presented (and will turn up again below), but this is not the last word of a systematic pan-site comparative analysis. A fine example of such a study on intersite scale is to be had in the article by Flannery and Coe (1968) on the Mesoamerican Early Neolithic (or "Formative" as it is called there). Based on fieldwork (mainly surveys) in the area, Sahlin's famous 1958 article, and upon ecological premises, two models are developed of Neolithic society in its intersite relations: an egalitarian and segmented model ("ranked" in the terms employed in Ch. IV) is posited against a hierarchical and segmented model ("stratified"), as these two types would be reflected in the locational distribution of villages. In ecologically similar locations, in ecologically zoned areas, different exploitative strategies (generalized *vs.* specialized) would result in clearly different settlement patterns. As a corollary, exchange of products generates an egalitarian (with but ceremonial goods exchanged) or a hierarchical social pattern (with trade of the various products of the different ecozones via a central "market" place). For Service, this is also essentially the motor of development at this stage of cultural evolution (Service 1975; cf., however, Earle 1977). Given the author's aims, the two models do provide insight into the mechanisms behind the different patterns of sites in the two areas. The suggestion of ecological (dis)similarity as a symptom of group hierarchy is of considerable interest for present purposes (though certainly not new; cf. Haggett 1965: Ch. 5). Also, the wording in terms of differentiation of kin sigments in the ranking case, and of hierarchical kinship segments in the stratified case, is in itself a valuable specification of the relational/structural paradigm. One fundamental criticism is that the patrilineal overtones of the models (not detailed above) are superfluous: matriliny can also be made to comply with the stratifying principle. Other roads towards a stratified society are conceivable as well (e.g. Friedman 1975; Lévi-Strauss 1969: 240). Whallon (1968) deals with variability in details of pottery design between (and also within) sites, as a gauge of matrilocal tendencies in an area, changing over time. His ideas are further developed in Engelbrecht (1974).

Ottoway and Strahm (1974), while only obliquely dealing with social organization as a whole in a fairly sophisticated article, look into some Neolithic exchange mechanisms within and between sites, referring to anthropological theories on economics. Philips (1972) also belongs in this context: from the general size of sites she concludes that the Early Neolithic French social formation has been of a pre-tribal character; tribes would have presumably lived in larger units. More "positional" (as defined in Ch. IV) is Hole (1968). Working with ethnographic parallels, he interprets the absence of wealth, village remains and distributions as vestiges of an egalitarian society without redistributive systems, practicing transhumance-pastoralism. Many more studies could be mentioned; intersite analysis seems to be *en vogue* these days. Among the more recent writings, the Grahame Clark Festschrift (Sieveking et al. 1976) and the acts of the Conference on Prehistoric Exchange (Earle and Ericson 1977) should be particularly emphasized. Regarding *intrasite analysis of social organization*, perhaps the best known modern research in Neolithic contexts is that of Longacre (Longacre 1968; and various other publications). Explicitly anchored in the White-Binford (or, materialist and evolutionist) paradigm, his investigations were directed at a clarification of the social pattern of an extinct Pueblo community; this was a preliminary for explaining adaptive changes following environmental deterioration. The latter aspect need not detain us here; rather the way in which the social organization was approached is of use. In view of ethnographical research among present day descendants of the Pueblo people, a matrilineal organization was hypothesized for the target society some 750 to 1000 years back in time. Assuming female potters, matri*locality* (not *matriliny*) should be archaeologically visible in a localized preference for details of ceramic design. Statistical (multiple regression) analysis of earthenware decoration excavated at Broken K Pueblo yielded results interpretable this way, which in its turn would suggest matrilineal descent groups. A very comparable pattern was found among the burials: for every matri-group in the Pueblo a matching corner of the graveyard could be pinpointed. Likewise, on the basis of associated artifacts, modern male religious sodalities could be traced in the prehistoric remains.

Longacre seems to have been heavily criticized for not having considered alternative possibilities (refer to DeGarmo 1977); in my opinion this omission can be better defended (as he was deductively testing) than his hesitating inference of matriliny (p. 100): given a matrilocal permanency over time like that found in the United States' Southwest, matrilineal organization is *inevitably* bound to follow (Murdock 1949: 221-222, 327; abstracted in Clarke 1968: 108; also, Fox 1967: 121). Worse even is Longacre's not having gone into the relations that existed between the matri-groups. For with such a heavy matri-imprint, there should also be a patri-component (De Josselin de Jong 1975: 17; 1977: 93); or, if this aspect were not archaeologically traceable, some other integrating mechanism should have been looked for (such as the religious sodalities actually found by Longacre), counterbalancing the centrifugal forces of the "bag of potatoes" of independent matri-groups.

Within the same research tradition, DeGarmo (1977) tries to correct some of the weaknesses in Longacre's analyses. He starts by noting that in subsequent research two male-associated tool kits were found with mutually exclusive distributions at Broken K, across the boundaries of the matrilocal units. Though DeGarmo does not say so, he seems to think that this evidence runs counter to Longacre's; of course, under a uxorilocal regime males are necessarily dispersed: at marriage a man will move out of his mother's and sister's house. Without realizing it, DeGarmo takes no risks in his analysis of the Site 01 Pueblo. As he employs a wider notion of exchange than one associated only with bargain-hunting and retail trade counters, he is a rare exception (together with Ottaway and Strahm, quoted above; cf. Sahlins 1976: 95) – without reaping full profit from it, however. He uses the notion of reciprocity when he wants to explain productive specialization on the one hand and generalized consumption on the other by the two residential groups at Site 01 (the groups were established from an analysis of architectural details and of the distribution of artifacts). By not considering the "nature" of exchange and reciprocity further progress into Site 01's social organization is blocked: "*I do not know of any archaeological data with which to demonstrate whether kinship relations were present at Site 01, or, if present, what they might have been*" (p. 162). Of course, the apparent reciprocity in fact *implies* an alliance of the groups, and thus kinship (cf. for instance, Dalton 1977). And further investigation into the exchange mechanisms (not just those of the querns and of the arrowheads which were at the root of his system of two exchanging groups) might even have resulted in a definition of the groups' composition.

Another study of social organization with structural tendencies can be found in Renfrew (1975). Primarily based on Sahlins' article on African agriculturalist expansion (Sahlins 1961), Polynesian parallels, and fieldwork on the Orkney Islands off Scotland, a model of segmentary, egalitarian society is developed to account for the wide distribution of megalithic monuments in Europe. Assuming demographic growth, a corollary of neolithization, segmentation would be the mechanism to maintain local group size below limits ("segmentation" meaning splitting and subsequent mutual repulsion of groups). As soon as the Atlantic seaboard hindered further expansion, tensions would rise, causing groups to stress their territorial rights – clearly an instance of what Carneiro earlier had labelled "environmental circumscription" (Carneiro 1970; also 1974). These territorial rights might then be signified by the erection of something conspicuous as a focus of group consciousness: the megalithic monument as a general ceremonial playground. As segmentation is a nearly universal feature of human social organization (Sahlins 1961: 90), Renfrew's model has the advantage of drawing attention to that phenomenon.

However, many megaliths (especially those of the tomb varity) are not conspicuous at all with their cobblestone or sod-hill camouflage, nor are their positions in the landscape very eye-catching. Therefore, Renfrew should have gone one step further: on the very same page Sahlins also mentions "complementary opposition" as a nearly universal structure: related, geneologically equivalent units will tend to stand together in many affairs (cf. e.g., the twin structure at Site 01, and perhaps at Broken K, too). If relations with the afterworld can be considered an important affair, one might expect concerted action (Van Gennep 1977: 148, 165) – which would explain the frequent twin arrangement of megalithic monuments (but these are not my concern). What remains of interest is that from a patterning of comparable monuments conclusions can be drawn regarding relations between groups. By Renfrew these are conceived of as between sites; in the present critique they are tuned down to a more domestic scale.

A third example of a study of prehistoric social organization, one already alluded to, is Randsborg 1974; his subject is not strictly relevant here, as it is directed at chiefdoms in the Early Bronze Age, but his sophisticated approach merits attention. Randsborg works with two possibly heterogeneous, even disjunctive notions of social organization. Within the chiefdoms an attempt is made to define the network of statuses, and between the chiefdoms, the relations are thought of as between groups (they are described in terms of dominance, etc.). As pointed out in the previous chapter, the latter is not necessarily opposed to the status network concept; moreover, Randsborg does not remain content with the strictly positional paradigm, for he also seeks to oppose groups (as sums of like statuses) within the chiefdoms. Specifically, with the background of Service 1971, Randsborg speculates about the geographical distribution of burial mounds as demonstrative of regional patterning, and about the differentials in grave gifts in them as clues to status difference. My first objection has already been voiced (Ch. III, Section 3): when Randsborg assumes weight of (bronze and golden) objects in the graves to be a function of their price (and thus of the material well-being of the deceased), he also takes "wealth" to be the principle according to which the chiefdom's hierarchy was rewarded, quite an ethnocentric view (Dumont 1970: 54-55; 104-105). My second criticism – even more lapidary than the first one – is his not having considered Binfords's 1972 article on mortuary practices from which perhaps other criteria for grouping within the chiefdom could have been derived. However, because his interst was centered on stratification (as seen in the essay's title), Randsborg cannot be reproached for not having gone further into social differentiation. Nevertheless, Randsberg's article presents a refined reworking of the initial positional approach towards a more balanced outcome.

Among the more recent writings on Neolithic social structure as observable in graveyards, the articles by Shennan (1975) and by Tainter (1975) should still be mentioned; both work from positional premises.

With this rough sketch of archaeologists' writings on prehistoric Neolithic social organization in general, the stage is set for a look into what is known about the social formation of the Bandkeramik.

2. *Ideas about Bandkeramik social structure*

In contrast to research traditions such as those for the southwestern United States' Basketmaker-Pueblo cultures (in the context of which DeGarmo and Longacre have been quoted; Plog has also been quoted in Ch. III, p. 57 and especially note 5 to that chapter) or the New York State Iroquois (where Engelbrecht and Whallon have been cited and the writings of many others omitted: e.g., Tuck 1971) the writings on Bandkeramik social structure are more disparate. Perhaps this is due to the continuity of the subject cultures in the United States up to the present day, whereas for the Bandkeramik no such privileged status can be claimed: too many events have accumulated between then and now, and other structures have taken precedence. Be this as it may, writings about this particular social formation can be grouped in three categories. The first one, perhaps traceable to Childe, seems to be mainly concerned with intersite patterns (though the situation within the sites is sometimes also examined); this group consists of people with no direct acqaintance with the Bandkeramik through own excavations, but who use these data as an "illustration" of their models and other theoretical constructs. The second group consists of studies that are Marx-inspired, Marxist in content, dealing with the relations of production; a number of the authors in this group has worked directly with Bandkeramik materials; their orientation is deeply influenced by the historical materialist paradigm. The third category consists of disconnected studies mainly concerned with data description or excavation reports, from which carefully-phrased excursions into theory are made. As I place the present study in the third category, I will discuss it last.

In the first group – here represented by Clark and Piggott (1970); Clarke (1972b) and Sherrat (1972) – Clark and Piggott offer a synthesis of world prehistory, with an emphasis on the social aspect. To that end, for the LBK they accentuate the within-village aspect of social structure. In a general way house types (especially size) are assumed to reflect family types: the larger the houses, the more complicated the families that once inhabited them. Thus, LBK huts have housed "social units comprised within extended families" (1970: 240). It is implicit in their writing that these extended families were on equal footing; it is even more implicit that the settlements were mutually independent. To complete the picture, Clark and Piggott (1970: 237-241) look into the possible yields of the fields and arrive at a kind of rotational agriculture.

In comparison with, e.g., the Longacre/DeGarmo studies, the very marked positional bias of Clark and Piggott becomes clear. If the former brood over matrilocal structures and end up with exchanging groups, the latter authors are mainly concerned with the similar statuses of the residential units. They (Clark and Piggott) even pass by a facile relation between hut size, wealth and authority in favour of size of the inhabitant unit: for them, hut size should be a sign of differentiation rather than of ranking. Having gone into the major dimensions of the positional paradigm (ranking, differentiation) I do not consider it proper to ask for further details: one cannot put everything into a Penguin Book.

Much less specific, but much more complicated and precise is Sherratt's essay (Sherratt 1972). Working from systems theory and ecological models of land use, a dynamic and evolutionary model is developed, parallel to Service 1971 – a model which interrelates economic surplus and degree of social stratification by means of redistributive and centralizing properties of the social formation. Pertinent to the LBK is his hypothesis of an acephalous (non-stratified, or egalitarian) society without much differentiation, integrated very loosely through exchange networks involving reciprocity. From this hypothesis, the conspicuous homogeneity of the material remains of the LBK over vast areas can be explained (Sherratt 1972: 511-512).

It is interesting that Sherratt arrives at a slightly different model of the socially stratifying mechanisms than Flanner and Coe do, as presented above. This may be due to the ethnological bases: the latter authors work from an article by Sahlins (1958), which is mainly concerned with hierarchy;

the former bases himself on Service (1971), a book with a more general intent. In that way, Flannery and Coe postulate ecological zonation and specialized production as conducive to stratification, and Sherratt the (more general) factor of an economic surplus; both articles, however posit the centralizing properties of the redistributive system as the intermediary toward the consitution of a hierarchy with differential access to desirable things. And regarding the mechanism driving towards a hierarchy it can be added here (as it was to the Flannery and Coe article) that factors other than ecological zoning can be causative: asymmetrical exchange may do the trick as well (Friedman 1975; Lévi-Strauss 1969: 240).

Clarke's model of the expansive process of the LBK is as general as Sherratt's (Clarke 1972b, especially pp. 20-24). He assumes shifting cultivation to be a cause of frequent settlement relocation, the effects of which were intensified (in the LBK case) by demographic growth and subsequent fissioning. A random walk process describes the process adequately; the model plus assumptions explain both distribution and expansion to Clarke's satisfaction (a mathematical formulation plus discussion of this random walk model in Ammerman and Cavalli-Sforza 1971). The model also provides for a maximum of contact between communities ("collisions" in terms of the model). Together with the inter-village mobility of families "suggested by the ethno-economic models", a "kinship solution" of "bilocal residence with 50% village endogamy and 50% exogamy" (p. 24) would explain the homogeneity observed in LBK culture.

Parts of Clarke's model are very attractive at first sight; especially the "random walk" part for the formal, descriptive side of the picture is luring – random walk being a kind of threshold condition (any directionality will improve results) which generates an outcome similar to observed reality (Ammerman and Cavalli-Sforza 1971). There are, however, a number of difficulties raised by its less abstract parts. The opening line of shifting cultivation leading to frequent settlement shifts has much of the old "*Wanderbauern*" idea, which is invalid: people simply did *not* often move their residences (for a point-by-point discussion of "nomadic cultivation" (Childe 1957: 105) see Modderman 1970 (I): 208-210, and Modderman 1971; also cf. Berlekamp 1975; Linke 1976:75-76; Soudský 1962: 198. The ethnographic parallel, often alluded to without further reference, is to be found in Buschan 1922 (II): 832). And then also, the LBK case of shifting cultivation (which is a somewhat less drastic form of agriculture than "nomadic agriculture" or "Wanderwirtschaft", as it does not imply a regular shift of residence; on the topic of shifting cultivation cf. Concklin 1961 and 1963) is far from settled: it is not even possible to choose between long- and short-term fallowing systems on the basis of present evidence (Bakels 1978: 146). Intervillage mobility of families can be better worded as "fissioning" (involving segments of lineages, not families). And with that we are on the same tack as Renfrew's mechanism of neolithization, above. Both a bilocal residence pattern and fissioning are compatible with a situation of pressure on local resources, which may or may not have been the case in LBK times; this "pressure" has not been proven or falsified as yet. Finally, the suggested 50/50 endo/exogamy is quite arbitrary as it stands; I prefer Sherratt's formulation in terms of exchange and reciprocity as emphasizing the *continuity* of the intervillage contacts.

The Clarke (1972b) model has been presented here as a stand-in for many other writings (within, and outside the Anglo-Saxon tradition) on the LBK expansion into Central Europe. The mechanism behind it has been discussed by many authors, and three possibilities have turned up: the shifting cultivation/*Wanderbauern* explanation (e.g., Butzer 1972: 574-580; also cf. above); more cautiously, community fissioning has been evoked (e.g., Clarke 1972b); and recently, simple diffusion of the agricultural economy in mesolithic, pre-adapted society (Nandris 1972; and especially Modderman 1976a). Leaving aside the first option as archaic, it is clear that the nature of the problem is generally considered mainly a *social* one (and not primarily demographic, or ideological, or anything comparable). From this, I think it over-hasty to choose at present between the alternative mechanisms: we know next to nothing about the social structures that ran the mesolithic way of life, and we know only a bit of the social formation of the Bandkeramik.

It should still be pointed out why I regard this tradition traceable to Childe. In fact, in Childe's *Dawn of European civilization* (Childe 1957, which is quoted because of its wide diffusion; Childe has written much more on this topic) the various models of the authors discussed can already be found, albeit in fetal form; they differ only in emphasis on the various aspects. Thus both Clark and Piggott's picture of an LBK community as well as Sherratt's are foreshadowed in Childe's statements: "They were democratic and perhaps even communistic: there are no hints of chiefs [or deities, or ancestors] concentrating the communities' wealth" (p. 109). And Clarke's and Nandris' ideas about the expansion of Bandkeramik culture can be parallelled in the alternatives explicitly stated by Childe: nomadic or shifting cultivation (for Childe apparently identical – which they are not) leading to settlement relocation, or secondary neolithization (Childe 1957: 110).

Taken together, the ideas of Clark and Piggott, of Sherratt and of Clarke present a highly general and not too improbable account of LBK social patterns. It does tie in with social anthropology (especially Service 1971 and related work), yet because of its generality, the possibility for testing is quite remote except perhaps for the inter-site relations. In other words, the picture should be completed.

The writings in the second category of research (historical materialist) are much more down to earth, as they probe mainly intra-community patterns. I will briefly consider texts of three authors here: Behrens (1973 and 1975; a regional Neolithic specialist), Klejn (1976; a theorist), and Tabaczynski (1972; also a specialist).

Behrens deals quite extensively with the Early Neolithic social formation in Saxony (Sachsen). He observes that our present knowledge of the productive forces in the Bandkeramik is rather scanty – for instance, nothing sensible can be said about the relative importance of cattle and agriculture, not even on the form of production: swidden or any other type of fallowing (Behrens 1973: 186-187). Therefore, theoretical considerations are brought in to fill this gap in a hypothetical way. The largest cooperative unit of production will have been the population of the village; for smaller undertakings, the household or even the matrimonial couple will have made up the working team. There will have been a minimal division of labour, probably between the sexes only, and more in emphasis than in quality. Specialist production of special purpose pots or tools will have been insufficient to generate a constant surplus for trade (and thus, for centralized redistribution). Similarly, the soil will have been communal property, cattle probably family property, and tools individual property. Archaeological findings indicate settlement autarky, which may be taken as substantiating the above in a general way. The unimposing amount of valuables (amber, obsidian, spondyli) may have been acquired by inter-village barter. In the egalitarian society thus envisaged, social relations will have hinged upon three components interactive with the productive forces: matriliny, male dominance, and ranking. An informal leader of the village, perhaps seconded by a council of the heads of the local clan segments, will have been the general manager of affairs; however, hierarchy will have been at a minimum, as indicated by grave contents. The huts were inhabited by a number of nuclear families, together constituting a matri-sib (matrilineage-segment). Inter-village relations on egalitarian footing will certainly have existed (how else could cultural homogeneity be achieved?), as did partial exogamy ("still not a norm"; Behrens 1973: 213). There is no need to assume any more comprehensive social formation than the village (Behrens 1973: 184-214; Behrens 1975).

Apart from some minor points (e.g., "property" had better be replaced by "rights of use"; inter-village barter will have had ends beyond simple exchange, such as to insure friendly relations or the handing over of wives, etc.; also, I wonder whether village autarky testifies to intra-village egalitarianism) I want to note that Behrens leaves no room for chiefs but rather thinks of the dominance of lineage elders, with an informal leader presumably from among them. But if so, he apparently equates the "matri-sibs" (households) with lineages, and from his wording subordinate status of the domestic mode of production can then be read, if it did exist at all. On the other hand, whatever division of labour existed was between the sexes only, and at various places the matrimonial couple, nuclear family, etc. are mentioned. Therefore I think that a discussion of relative dominance would have cleared the issue.

Very strongly (though implicitly) in line with Behrens is Tabaczynski (1972). He goes a bit further in stressing the egalitarian character of Early Neolithic society. According to him there has been neither a division of labour between the groups (which is in line with Behrens) nor within the groups (as opposed to Behrens' differences in emphasis between the sexes; Tabaczynski 1972: 62).

I would like to register my profound disagreement with the latter statement. Neither on theoretical nor on empirical grounds can it be sustained: theoretically, a cursory reading of even Morgan's and Engels' writings reveals that at the Lower and Middle stages of Barbarism (the Neolithic) the development of the productive forces had gone so far as to result in a division of labour between the sexes and consequently in the formation of the polygonous family as a productive unit. More recently, other authors have substantiated this point (e.g., Sahlins 1968, 1972; Service 1971). Empirically, the objection stands because at every Bandkeramik cemetery a sexually differentiated tool kit is found (cf. the previous chapter plus its references).

Klejn (1976) discusses only the relations between settlements in Neolithic Europe; there was some exchange and some trade, yet these did not affect the economic base (p. 13). Rare basic materials and valuables were occasionally passed down distributive channels in small quantities. Whatever contacts existed (political, military, matrimonial), they were neither intensive nor incessant (p. 14).

I have introduced Klejn as a complementary to Behrens: the latter is less specific about inter-village contacts. It should especially be noted that Klejn differentiates exchange and trade; both are apparently conducted along "distributive channels", which from the sequel can be thought of as of an "alliance" nature (in the meaning attributed to that term in the structuralist paradigm). One would like to know, additionally, who conducted what trade exchange: chiefs (probably not, it can be read into Klejn's account), lineage prominents, or simply anybody old enough to walk and talk.

These Marxist authors treat social relations as an economic epiphenomenon; there is no room for relative autonomy of each of the several levels of the social formation. If this is a matter of taste I have announced my different views in the previous chapter; if it is open to testing, other scholars will have to decide.

The third category of research into the social organization of the LBK is difficult to summarize, as what has been done on the subject is usually hidden in lengthy site reports and the like; Lüning (1976) provides a critical bibliography of the more general literature. The sample below can claim no more representativity than the other two categories, not to mention exhaustiveness. Still, one of the major foci of research interest in the non-pot, non-hut aspect of the Bandkeramik is easily perceived: the size of the population of a settlement. This is clearly linked to probing into the economic base: was it possble for the Bandkeramik people to live off the land they occupied?

This type of investigation generally starts with considerations on the number of people that once inhabited a hut. Careful as always, Modderman starts from a comparison of the different types of huts on LBK sites. From it, the "central" part is postulated to have been the living area – its format is constant from the smallest to the largest hut (the size of the central part may differ, though; Modderman sees this as a function of relative wealth rather than as being connected with the number of inhabitants). This room will have been inhabited by an extended family: at the most three generations, one married couple with their children, plus grandparent(s), say, six to ten people. The village of Elsloo will have counted eleven to seventeen huts at any given time, which will have sheltered 85 ± 25 inhabitants (in the estimate, the graveyard's population has been taken into account; Modderman 1979 (I): 110, 203, 205, 207; also cf. Modderman 1968).

Soudský, too, has been dealing with the number of people per hut and per village. Partially because of different circumstances, partially on different societal models, he arrives at a result which differs fundamentally from that of Modderman. The LBK houses in Bohemia are less differentiated in form than are those in western areas; for instance, the northwestern parts are less often "emphasized" by wall trenches (16% of the huts at Bylany, 48% at Elsloo; Modderman 1970 (I): 116-118); in fact the larger types ("*Grossbauten*") prevail (Soudský and Pavlů 1972: 317). As in western Central Europe, sizes differ (sometimes the central parts are doubled; also, the southeastern parts are larger than in western Europe; Modderman 1970 (I): 116-118) which is interpreted by Soudský as a sign of different household size. The number of couples in a hut is gathered from a comparison with the size of the Middle Neolithic houses at Postoloprty where four hearths were found (LBK huts never show traces of fireplaces); hence, this has been inhabited by four families (the "hearth" interpretation of the Postoloprty data has been contested: Modderman 1973: 132). Simple division of one into the other yields the relation: for every six metres of *hut*-length (*not* central part) one family should be postulated. That is, 200 people (among whom were 40 to 50 adults) were living at the settlement (Soudský 1966a: 75,78; a more moderate account in Soudský and Pavlů 1972; more extensive, Soudský 1969).

An evaluation of both views seems difficult. In my opinion, which is based upon anthropological literature, Soudský assumes a society too egalitarian in outlook when he relates size *only* to number of people; conversely, Modderman's account passes by the established fact of the (statistical) heterogeneity of family types in every society (e.g., Murdock 1949: 27, 28; Shryock and Siegel 1976: 173). Also, the derivation of a relation of length of hut to number of people from one instance seems to be a bit risky, even if the original interpretation is accepted. And with regard to Modderman's estimate of six to ten people to a hut: Naroll estimated from a cross-cultural sample ca. ten square metres of living space per individual (Naroll 1962). Given the range of sizes of the central part (35-112 square metres; Modderman 1970 (I):205) the six to ten people estimate may be a little on the conservative side. Soudský's figures (one family to every five or six metres of hut length) would come down to ca. 30 square metres per family, an area approximately half the Naroll/Modderman estimate. A general conversion formula should be of the form $y = ax + b$, of course; for the majority of societies, Naroll's figure will be a reasonable approximation of the most popular size and form of household groups (cf. Bakels 1978: 143-145 and Phillips 1972: 43-44 for a discussion of these estimates).

Not so much a focus of archaeological research as a basic assumption is the type of family supposed to have lived in the "long houses" of the LBK. Both Modderman and Soudský appear to agree of the idea of an extended family, thought of as being composed of members of three successive generations (technically, a "stem" family; cf. below, Section 4). Recently, Kuper and Lüning (1975) expressed a similar opinion. Only Soudský has specified its matrilocal constitution, for which he alludes to "material proof, like, for instance, the 'familial' tradition of ceramic production" (not further detailed; Soudský 1966: 81).

Several authors have dealt with the social relations within the LBK settlements in one way or another. In a number of reports from the Aldenhovener Platte the coexistence of several huts close together, grouped in small hamlets, is demonstrated. These, and separate, individual farmsteads were strewn along the Merzbach. Moreover, most dwelling places show signs of continuous occupation: when a hut became old and dilapidated, a new one was built in the vicinity. In that way, every cluster of hut remains represents the locus of a lineage (Kuper et al. 1977: 326-328; Kuper and Lüning 1975: 92). In these writings, the Aldenhovener Platte Group is "merely" presenting its data plus initial interpretations; further elaboration and interpretation is delayed until a final synthesis can be made.

The synchronic existence of a number of huts is attested by many authors: Kulczycka-Leciejewiczowa (1970: 46), Modderman (1970 (I): 203), Pavlů (1977: 51), to mention a few of them, all agree on this point. Yet, ideas concerning the mechanism by which the social body's continuity was secured – i.e., the diachronic within settlement relations – differ from one author to another. For Modderman, an abandoned hut was replaced in a nearby place when necessary; accordingly the settlement moved within a very limited area; Kulczycka-Leciejewiczowa, though not giving further details, thinks that filiation of the lineages living in the huts occasioned the setting up of new dwellings, together with the normal rebuilding of shaky houses.[2] Pavlů arrives at still another mechanism. Having outlined some of the possibilities (such as intermittent site occupation, permanent occupation of a village site with rebuilding in adjacent plots; and isolated farmsteads in a limited area with rebuilding as necessity arises), Pavlů cannot find ways to test these alternatives: some sites are densely packed and others are not. There may have thus been different settlement types. To account for the denser sites, he proposes that all huts of a single phase were destroyed together and rebuilt at another place in the same general area, at least once per generation.

While a combination of the Modderman and Kulczycka-Leciejewiczowa standpoints seem probable (if these views are not identical in reality), the Pavlů hypothesis strikes me as bizarre. For even in the capitalistic mode of production capital is not scrapped at that rate – so much less so in any of the precapitalistic modes, I presume (besides, it can be partially tested, as in Sections 4 and 5 below). Again, all the mathematical rigour of the article does not do away with the weak assumption at its root: the cyclical renewals of Bandkeramik villages are postulated as analogous to activities at tell-settlements in southeastern Europe (Pavlů 1976: 51). Even if such purposively destructive activities could be demonstrated for the Balkan Neolithic, still no parallel can be taken for granted without proof of cyclical renewals in LBK villages. If not a reification of the "phase" concept, in my opinion Pavlů attempts to save the settlement relocation part of the old nomadic agriculturalist theory: "micro area" and "period of inhabitation" are but new coins for old values.

Little is written about the relation between the households living together in a settlement. Filiation (see note 2 above) should account for the smaller huts in a village according to Soudský (1962: 198). Relations between the lineages are nowhere dealt with; that is, the relations in the alliance and kin sphere. Above I discussed statements by Modderman and Soudský on the score of a more or less egalitarian society. Milisauskas, basing himself on ethnology, goes furthest in specification and interpretation. Consonant with earlier writings (Milisauskas 1972, 1973) he recently produced a picture of egalitarian communities with big men and men's societies. Yet some households should have been wealthier than others, as more imported materials were found in the vicinity of some huts at Olszanica B1 (Milisauskas 1976b: 41). Big men should have dwelt in the slightly deviant type 1a "longhouses", where the men's clubs also convened: "if the size of a longhouse depended upon the size of the social unit inhabiting it, no structural difference would be expected" (Millisauskas 1976a: 70). By relating status to age, sex and charisma (with no hereditary aspects), a big man comes out as slightly different; his living in the deviantly constructed hut is derived from the higher concentration of rare, imported materials around it, which were involved in the maintenance and expansion of social relations. And the men's societies ("or some such other type of tribal association") meeting there can be inferred from the concentration of (male-associated) adzes around the deviant hut (Millisauskas 1976a: 67-71; 1976b: 35-40).

Less explicitly ethnological, but entirely in line with Milisauskas, is Reinecke's view on Bandkeramik society, based upon the grave gifts excavated in three Lesser Bavarian cemeteries (Reinecke 1978). For him, status was "the expression of the relations of an individual to his family and kin group"; i.e., rank. The community provided an "intact model of society" rather than the divided image of stratification (Reinecke 1978: 14). Pavúk (1972b: 97) arrives at a similar conclusion, also from a graveyard study.

Regarding Milisauskas there is only one thing to criticize in his account: the "big men" part of the story is derived from ethnology (which is acceptable), but not tested (which cannot be done or is very difficult at best on a settlement site; cf. Ch. IV, Section 3). And although the ceremonial meeting place interpretation of the type 1a houses does sound likely, one would like to have it checked against data from more than one such hut. In all fairness, it should be added that Soudský (1966a: 75) and later Soudský and Pavlů (1972: 325) also postulate an assembly hall function for the largest huts. Modderman, however, considers the men's clubhouses interpretation as hinging too much on ethnographic parallels; for him, the identical interior construction/post mold configuration pleads for an explanation similar to that of the other huts (Modderman 1970 (I): 207).

By-passing the "*Wanderbauern*" issue as well as the fissioning aspect, opinions about the inter-village relations do not differ much: the autarky without surplus-production (Reinecke 1978: 14) of a non-centralized political and economic organization (Milisauskas 1976b: 35) provide for an egalitarian footing for the various settlements. Additionally, there was some trade, about the nature of which all authors referred to are quite vague. The things exchanged (rarities like exotic materials) are relevant to a "socio-technic" use (Binford 1972: 24) – say, status symbols – as comparable items were locally available (Milisauskas 1976b: 40).

If the accounts on the village-village contacts are rare and vague, research into the ecological preferences of the Bandkeramik has provided concrete results; e.g., Linke (1976), Sielman (1976) and Bakels (1978). Especially the last-mentioned publication is highly relevant. Bakels analyzes the find material from three Dutch Limburg LBK sites and from Hienheim, as it bears upon the relations of the Bandkeramik to meteorological and pedological factors, to the floral, faunal and demographic components of the environment. From it, a site territory of approximately 200 hectares is derived, probably exploited by one village exclusively; the area beyond ("home range") will have been in joint use by several villages (Bakels 1978: 141). Apart from an almost total village autarky (exceptions: pigments, adzes) perhaps her most important finding is that the size of the site territory is sufficient to carry on with any system between short-term fallowing and swidden cultivation: from 1/3 to 1/10 of its size is enough to feed the population of a Bandkeramik village (Bakels 1978: 146). Milisauskas thinks that even less would suffice: 12.5 to 31 hectares in cultivation (Millisauskas 1976a: 63). But Soudský (1966a: 79) states that at Bylany only 30 hectares were *available* per settlement, so there cannot have been much scope for even a short-term fallowing agricultural technique (however, in a later article this area was more than doubled: both the *a/b* and *d/e* sites, and possibly the *c* settlement as well, are reported to have had 60 to 63 hectares of arable land available; Soudský and Pavlů 1972: 325).

In discussing the views on Bandkeramik social structure, I have restricted my discussion to writings of the last ten years, preferring later to earlier texts. The heterogeneity of the third research category and the coherence of the second made such a limitation feasible. For the first, the Anglo-Saxon category, I had to make an exception: without mentioning Childe's continuous concern for the Central European Neolithic no unity could be suggested for that research. It might be argued that incorporation of Buttler (especially Buttler 1938) and of Sangmeister (1951) would have resulted in a more uniform picture of the third research "tradition", but I do not think so. This is in the first place because Buttler's ethnological concepts were largely based upon the "*Kulturkreis*" paradigm (which has been conclusively proved untenable, and also officially renounced by its latest adherents: Haekel 1956). And secondly my skepticism stands because Buttler's data base has been abandoned: "pit dwellings" are no longer part of the Bandkeramik inventory; Buttler's lasting contribution is in the field of ceramic decoration typology. Then, the influence of Sangmeister's settlement phases as derived from differential house orientation does not go any further than providing one of the pillars of the nomadic-agriculturalist theory, for which I have not the slightest affinity. In fact, the writings of advocates of that theory are mainly interesting because of what they say on other topics.

Thus, the various writings can be summarized in a nutshell as follows:

1. The huts are generally conceived of as being the dwellings of one extended family each (Behrens, Clark and Piggott, Kuper and Lüning, Modderman), the composition of which seems to be open to some debate (Behrens, Modderman, Soudský, Soudský and Pavlů): either a set of related nuclear families (Behrens) or a group of related people from three successive generations (Modderman, Soudský). Whenever the nature of these relations is discussed, some "matri"-adjective turns up (Behrens, Soudský).

2. With the exception of Tabaczynski, the other authors either explicitly agree on a slight division of labour along the sex line (Behrens) or implicitly do so, which is in line with ethnography. When this has led to the constitution of a domestic mode of production, its position relative to the lineage as a unit remains unspecified, though perhaps subordinate (Behrens).

3. On the relations between households no statement has been found; regarding relative status, most authors seem to agree on some form of ranking: there seem to be differences in wealth (Behrens, Milisauskas, Pavúk, Reinecke) although some may deny this (Soudský, and also Clark and Piggott?). The village community was acephalous, i.e., without true chiefs (Sherratt). A council of clan elders may have elected a *primus inter pares* (Behrens); or leadership capacity and ambition may have crystallized in the institution of a big man (Milisauskas). Men's clubs (Milisauskas) are postulated, which should have convened in special clubhouses (Milisauskas, Soudský, Soudský and Pavlů), which are at the same time the huts of the

community's leaders (Milisauskas); a particular (for example ceremonial) function of the special house type is denied by Modderman. The huts stood for periods of between 15 years (Pavlů) and 30 or 40 years (most other authors).

4. As the settlements were economically self-sufficient (Bakels, Behrens, Clark & Piggott, Klejn, Milisauskas, Reinecke, Sherratt) their relations were of a transient nature (Klejn) involving some exchange of rare goods (Behrens, Clarke, Klejn, Sherratt) and women (Behrens, Clarke) or even trade (Milisauskas, Klejn). Also, groups of settlements may have jointly exploited a home range (Bakels).

Since the present study is mainly concerned with the social structure of a mature stage in the development of the Bandkeramik, I have refrained from first detailing and then summarizing the various ideas on the expansion of that culture when it was still young. As the focus of my research is mainly *social* I have also abstained from an extensive exposé on the economic/agricultural basis of Bandkeramik society. Although the development of both the productive forces and the remainder of the social formation are limited by environment and economy, the indeterminateness within these constraints suggests a more direct approach to the social structure (including, even, the relations of production) than by means of the productive forces.

A number of the above hypotheses have been tested, others go uncontested and untested. Several of them find some corroboration in the previous chapter, although I did not mention them there; it seemed better to draw them together, expand the set with the hypotheses from the pilot study and then see what might be tested in the Elsloo and Hienheim data. After all, the graveyard study was explicitly concerned with social structure, whereas that subject was only a sideline in the investigations of the authors discussed. For that reason also, systemization of these latter writings could only be partial.

To summarize the hypotheses following from the study of the Elsloo cemetery:

1. From a "positional" viewpoint, Bandkeramik society knew differentiation according to sex and according to some specialization (most notably: the deceased in the perpendicularly oriented graves). Ranking was found in the Bandkeramik social structure, rather than stratification, with age as a major component. Also, some specialisms were found (those bound to hunting equipment, or to pots), and a chiefly position found expression in high ranking statuses.

2. The structuralist approach resulted in a system of four groups tied to locality. The relations were of an asymmetrical and linear (or circulating) exchange type; marital relations thus must have been predicated upon a preference for the matrilateral cross-cousin (male Ego). Membership in the social groups was bilineally established: apart from virilocal residence, matrilineal descent played a role. The arrangement of the groups must have been a division into moieties on the matrilineal principle, plus in the Elsloo graveyard case there were four virilocal groups or lineages; at other times and/or other places the latter number may have been different (for asymmetrical arrangements, a minimum of three groups are necessary).

3. The neo-Marxist entry was of necessity restricted to the organizational part of production and reproduction: the scant items accompanying the dead, which are also difficult to interpret, do not allow any substantial statement on the factors. A domestic mode of production must have operated on the basis of a division of labour among the sexes, organized in matrimonial couples (presumably in conjunction with offspring); a higher order organization existed in the lineage mode of production, in which individuals had a different status regarding the allocation of their group's surplus production. The lineages were matrilineal. Then, a loose mode of production indicated the existence of non-hereditary specialisms; those that could be made out in the graveyard were in some

way linked to arrows and knives (blades) or to pots. Perhaps the perpendicularly oriented graves should be included as well. Finally, a supralocal mode of production accounts for the import of people and things (presumably also going in the reverse direction), especially towards the end of the Dutch LBK. The lineage mode of production should have been dominant over the other modes. In the end the balance may have shifted, though, in "favour" of a domination by the inter-communal relations – but that is mere speculation.

After a brief consideration in the next section of some minor methodical issues, the fourth and fifth sections will be devoted to testing the above sets of hypotheses.

3. *Some preliminary remarks on method*

Both settlements to be discussed span a considerable time: in Elsloo perhaps some 400 ± 50 years (6350 b.p. to 6000 b.p.: Modderman 1970 (I): 201), and in Hienheim probably even 450 years (6235±45 b.p. to 5780±50 b.p.) to judge from the radiocarbon dates. It would be strange if social structure had remained unchanged during those years; indeed, in the graveyard, an expansion of the kinship system could be made visible (Chapter IV, Section 5). As I do not know of methods capable of handling social dynamics directly, the phenomenon can be approached only in an indirect way. The easiest way to accomplish this is to cut the material into chronological slices in order to attempt a description for such a phase and then to see what changed, and how, in between. Phases have been introduced in Chapter III already; a considerable amount of print was spent in discussing the merits of several ways of partitioning the finds. The rather gloomy conclusion was that after drudgery, toiling and moiling with the various models, the best phasing very much resembled the initial distribution of the data on the unrotated chronology principal component. I will therefore stick to the latter; yet solely to facilitate computation, some of the phases of Chapter III have been lumped together (when there were too few finds to produce a decent analysis) and some others split up (when there were so many finds that an attempt to derive a finer picture made sense) as appears in Table 52. Let it be re-emphasized that *these phases are analytical tools and nothing else:* they have nothing in common with the concept of the same name in writings like Pavlů (1976; cf. previous section). The chronological subdivision here is on the basis of the distribution of the finds over the factor scores (finds being decorated pottery). Factor scores are units expressing amounts of difference; one may imagine equal amounts of change in the data set being mapped onto equal differences in factor scores. (Real) change quantified over (real) time will follow a Gaussian or normal curve *per variable* (as defined in Chapter I; also cf. Fig. 9); the length of the period of change or adoptive period, however, will be *different* per variable (see Fig. 11) and statistical procedures like multiple regression and principal components analysis are not corrected for that phenomenon. Then, the chronological axis is "defined" by a number of variables, each and all liable to run a similar course. The distribution of the changes on the different variables over inferred time will be true to the real distribution when stated as "earlier" or "later than", but not "... years later/earlier". The speed of change per variable being unknown, the quantification and subsequent translation of change into differences in factor scores would inevitably lead to a distorted picture. When from such a distorted picture a partition of the data set into phases is derived, nothing can be said about the length of these "phases"; only their relative chronological positions are to be trusted.

The phases in the Elsloo settlement are not the same as those defined by Modderman: the latter are not based only on decorated ware, but also on details in hut construction and on stratigraphic observations. The former are exclusively ceramic (in Fig. 17 the two arrays are compared). The arbitrariness of phases as meant here will become even clearer when they are further subdivided into occupation or habitation phases.

Having cut up the data into these slices, the variables originally "measuring" time are reincorporated: there may be some social systematics behind the first appearance and subsequent spreading of something innovative or the distribution of the conservatives clinging to old ways. Having subdivided the data into phases, per phase a general principal components analysis was made on all decoration variables represented. In Chapter IV non-ceramic grave gifts were incorporated in the analysis as well; the present chapter deal primarily with ceramic variables. Regarding the principal components analysis, in previous chapters I have already worked with it and explained a number of its aspects; here, I will mention some details not yet discussed.

The number of variables differed from phase to phase: some phases offer more variance to be explained than others. This has its consequences for the number of components; with little variation, only a few factors can be meaningful. The number of factors to work with further was chosen from the three criteria customarily employed: that is, λ should be greater than 1.00; or, at least 5% of the variance accounted for by the smallest component; or, before the relatively largest jump in variance between two adjacent components. $\Lambda = 1.00$ provided the absolute bottom below which no analysis was attempted; otherwise, the middle cut-off value was taken. Thus, in phase 1 at Elsloo the three criteria resulted in an identical number of components to be incorporated (7); whereas in phase 5 of that settlement $\lambda \geq 1.00$ resulted in six components, the 5% of variation criterium in 5, and the largest jump in percentage of variation occurred between the seventh and the eight component; accordingly, six (middle number, and also $\lambda = 1.00$) components were retained.

A technical point should be mentioned: in the present computations the "missing data" declaration was much alleviated in comparison with that in Chapter III in order to lose as little information as possible. This was partially compensated for by retaining the same threshold values for "noise" regarding the numbers of sherds per find as in that chapter: from the Hienheim file, finds with less than five sherds were omitted, and at Elsloo, huts with less than three sherds in certain association were thrown out. Yet the liberalization of the missing data option must have resulted in a slight veiling of the factor patterns, as inevitably more "noise" had gone into the computations.

The components that were retained were then VARIMAX rotated, a rotation maximizing the differences in loadings of the variables (Nie et al. 1975: 485). This means that usually a small number of attributes correlate highly (positively and/or negatively), and the remainder are more or less neutral. Through this rotation the relative weight of the factors is changed; the amount of variance incorporated in an unrotated principal component has no relation at all with that locked up in its rotated counterpart. Also, the original ordering (component no. 1 being the most important, no. 2 next, etc.) is lost: the importance of the rotated factor no. 1 need not be any greater than that of no. 2, in fact it is equally likely to be smaller. What is reained, however, is the independence of the components (the factors are perpendicular to one another). The VARIMAX rotated factor pattern provided the basis for parts of the analyses and interpretations below. Because the number of units per phase was hardly ever very impressive, the importance of any individual factor (or even of the whole output per phase) was assumed to be quite low by itself. Only if a similar factor turned up in an adjacent phase were they considered noteworthy (two factors are considered "similar" when the same variables load high in the same pattern; when a variable is not consistently associated with a factor this will be signalled). This, together with the different numbers of factors per phase resulted in some drop-outs on the one hand, but on the other also in a fairly consistent picture of what happened to the various factors throughout a site's occupation, i.e., a dynamic picture.

There are important differences between the outputs for the Hienheim and Elsloo villages. Most conspicuous is the constancy of the factor pattern emerging from the Elsloo analyses, and the much more fluid, almost elusive Hienheim summaries. To some extent this will relate to "properties" of the sites themselves, but for the largest part differences in the data base will be responsible: for Elsloo, huts were the units fed to the computer; for Hienheim, finds. As "huts" are compounds of "finds", fewer gaps in the data ("missing values") will occur among the former. Descriptions of individual finds incorporate more missing data. That is, much more of the variance in the Hienheim data is spurious. Also, the data set from Hienheim is more heterogeneous than that from Elsloo: the latter is composed of finds relating directly to the dwellings; the former incorporates finds from find complexes and silos as well, and these do not directly relate to habitation units; and moreover, a substantial part is *Middle* Neolithic. At first sight this may appear to be a formidable handicap; in reality things are not that bad. For, as will become clear in the next section, even in Elsloo with its fine factor pattern, relatively little can be done with it, and several factors remain unexplained. Yet the social structure can be brought out without many difficulties. And this holds positively for Hienheim as well. Conversely, only a limited number of aspects of the social structure are represented in the output of the principal components analysis. Not unexpectedly, in order to analyze social structure more data than those on decoration alone should be incorporated – as was empirically demonstrated in Chapter IV.

The present analysis is explanatory in a vague sense only; its main purpose is a *description* in ethnological terms of the ancient social organization. It is "explanatory" insofar as artifact-distributions are related to social patterns. In archaeological practice this is one or two steps removed from excavatable and directly observable things. It is descriptive inasmuch as I am content with a (partially) tested image of the Bandkeramik social structure; no attempt is made to go beyond the social level. Such should be the next step, after further testing, amendment, and elaboration of the statements in the sixth section.

To that purpose, if not for the completion of the testing program, the data base should be expanded to incorporate data on flint artifact and adze distributions, quantitative and qualitative descriptions of huts and other structures (including the geographical dispersion of so-called silos, pit complexes, etc.); but most of all, the data should be *representative* for an entire site, and not just cover part (unspecified) of a settlement only. It is especially this latter aspect which is lacking here: the Elsloo graveyard has been excavated almost completely; the Hienheim settlement for one-third, and Elsloo is known for two-thirds to one-fifth of its territory only, depending upon the period. Therefore, the most could be said concerning the cemetery and the least about the latest phases of Elsloo. When it is added that ecological data should also be introduced into a causal analysis, it is clear that such an attempt is beyond the capacities of one single researcher – I for one am eagerly awaiting the results of the Aldenhovener Platte team, where indeed such an all-encompassing approach is tried.

Finally, I want to return to the distinction between mechanical and statistical models introduced in Ch. IV, Section 1. It was stated there that rules can never be fully observed, no matter whether these are known to the participants (for example, social) or only to the observer (such as statistical norms). And even in a special abode like a graveyard all rules made visible were accompanied by exceptions: besides the normally asymmetric exchange model, two distributions pointed to generalized exchange; besides the normal pairing ("normal", of course, in a statistical sense; cf. Lévi-Strauss

1967: 276) of the sexes in couples as an indicator of the domestic mode of production, graves without partner occurred, too, etc.

When in an analysis of a graveyard in which considerable attention has been paid to the various kinds of goods – from decorated pots to pieces of charcoal – the statistical or empirical distribution differs in a visible way from the distribution expected from the mechanical model, how much more will this be the case in the study of a part comprised only of the settlement debris. That is to say, the observability of the various models may be quite low, and also in an unpredictable way. Thus, chiefs are not visible in the decorated ware of the settlement, and neither is asymmetrical exchange; yet the effects of bilineal group composition are very clearly visible in the distributional patterns of ceramic decoration.

To provide some continuity, the two following sections are set up partially parallel to the previous chapter. In that way, there will be checks on the positional, the structural, and the neo-Marxian hypotheses, in that order; intermittently I shall also dwell upom some of the hypotheses from the second section of this chapter.

4. *The LBK village of Elsloo*

The Linear Band Keramik village of Elsloo was built on the middle terrace of the Maas River overblown by loess in the southern part of Dutch Limburg. The settlement site itself is almost flat; it is bordered to the west, the north and the east by small gullies (which did not carry water in LBK times), and to the south by the high terrace (a more extensive description of the site's surroundings can be found in Bakels 1978: 15-22). The settlement did not completely occupy the available space: in every direction there were at least 150 metres of "open" land between the village and the site's natural boundaries. There will have been little solifluction, because of the flatness of the terrain. Across the western dell another Bandkeramik village was situated, one more of the seven to nine contemporary settlements in the area. The graveyard, discussed in Chapter IV, was lying in the fork of the two valleys (Modderman 1970(II): Pl. 2). The existence of the village has been known since the 1930's, when the modern villages of Elsloo was relocated due to the digging of a shipping canal as a substitute for the Maas River. The site was assigned to become a new housing estate. Until 1950, the site was filed under the name "Koolweg" ("Cabbage Road"), then a first trial pit was dug (Modderman 1950; –1958/9). Large-scale excavations at the site, led by P.J.R. Modderman, were conducted by the Dutch State Service for Archaeological Investigations, R.O.B., in 1958, 1959 and 1963. The graveyard was examined in 1959 and 1966. From the viewpoint of a history of science, these excavations are interesting, as it was here in Elsloo, that for the first time (in 1959) a dragline was introduced to uncover the vast areas necessary for studying house remains, the main purpose of the research. No fine horizontal "stratigraphy" was found at Elsloo, although a partial relocation of the LBK settlement had taken place. The area uncovered in the northwest and west of the site contained vestiges of all periods (LBK 1b to and including 2d), whereas the excavations further to the east revealed traces of the later phases only. The total area of the site is estimated at ten hectares, of which one-third has been dug out. The remains of 95 huts were found; 26 belonged to the Older LBK (i.e., phases 1b-d), and 56 to the Younger period (phases 2a-d); 58 huts could be dated more exactly, but ten huts could not be chronologically filed at all. Extrapolating from the whole site, some 200 or 250 huts must have been built there in the course of time. If each of them was used for twenty-five years, then anywhere between eleven and seventeen of them must have been contemporaneous. In the older LBK the village occupied an estimated area of ca. two to three hectares only (much of which has been uncovered); in the younger LBK a much larger area was inhabited. In the latest phases (i.e., those contemporaneous with the graveyard) the northwestern part of the site was apparently deserted. Consequently, in the earlier phases the number of huts will have tended towards the lower figure, and later the upper figure should have been the closer approximation. In every phase the buildings were more or less evenly distributed over the area. It can also be established that probably only one "*Grossbau*" (hut of type 1a) was standing at any moment. When no more than six sherds could be attributed to a hut, these sherds were not published (Modderman 1970 (I): 1-6, 75-76, 203-207). The data from Elsloo have been published in Modderman (1970).

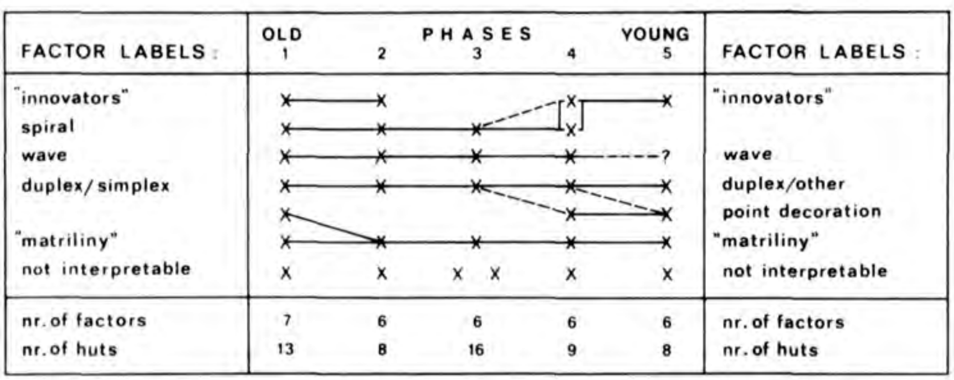
Across the five phases into which the Elsloo material has been divided in the third section, a fairly consistent pattern of constancy and of change among the factors emerges (Fig. 45); this pattern can also be read from Figs. 18-21. To start with the constant factors in the pottery decoration:

– The *wave* and the *spiral:* At the level of the model, the two were considered alternate attributes of the variable MAIN MOTIFS (p. 000). Their distribution in the data set, however, suggests a higher standing than that of *traits:* they behave independently, in every phase a separate component is occupied by each of them (except in the latest phase, where this pattern becomes blurred). Of course, in the cemetry *waves* and *spirals* were never found in the same grave (p. 21).

Associated with the *wave* motif, the following variables occurred: type of FILLINGS (with *uninterrupted* fillings loading similarly, ENDS (with *no ends* regularly accompanying, and AUXILIARY LINES diminishing in importance (with first, *symmetry axes* loading high, gradually becoming neutral). The other attributes of the variables mentioned loaded oppositely.

With the *spiral* motif, the major association is with the *triplex* attribute, sometimes loading slightly higher, sometimes slightly lower than this motif; the other attributes of the variable NUMERICITY constitute a component on their own (see below). None of the other variables is regularly associated with the *spiral*; the stippled line in Fig. 45 drawn from "spirals" to "innovators" stands for a negative association: attributes indicating innovations in the repertory are loading very much opposed to the *spiral*.

– The NUMERICITY factor: As with *wave* and *spiral*, at the level of the model of Chapter I, *simplex*, *duplex*



---- variable hopping over.
? hardly perceptible, not absent.
—— constancy of pattern loadings.

Fig. 45. ELSLOO VILLAGE. Constancy and change in factor patterns.

and *triplex* should be mutually exclusive attributes of the NUMERICITY variable. Regarding *simplex* and *duplex* this can be sustained, but *triplex* seems to be an independent feature: in the first three phases *triplex* is strongly associated with *spiral* motifs, giving rise to a separate factor; in the final two phases *triplex* and *duplex* are loading bipolarly (with also *duplex* and *simplex* loading oppositely on another factor). There is only one attribute consistently related to the *simplex* and *duplex* factor: *uninterrupted* FILLINGS have similar loadings (as was the case with the *wave*).

– The most constant factor in all of the output is a bipolar one, on which *curvilinear* and *rectilinear* STRUCTURES stand very much opposed; from the graveyard, this factor can be called "matriliny": ANGLE OF FILLINGS defines a separate factor in the first phase, but in the other phases this variable always loads moderately on this factor. In some phases still other variables are associated: in phase 1, FILLINGS; in phases 3 and 5 LIMITS; and in phase 4, *simplex* (with *curvilinearity*) and *duplex*. The real matri-markers load very extremely on this factor, the other variables just mentioned only moderately so; their influence will therefore be restricted.

A marked contrast of this factor with those involving the MAIN MOTIFS is apparent. While the latter define a separate factor each (that is, independent of one another), the former is a bipolar one, and its attributes are real *attributes* in that they are in opposition on the same factor.

– Among the non-constant factors, twice during the settlement's occupation factors turned up that have been dubbed "innovators": In the first two phases (which belong to the older Bandkeramik) the huts where neck decoration was applied to the pots were singled out by a specific factor; no other variables seem to be associated. In the latest two phases of the settlement (which fall in the younger period) the *multidented spatula* – a major innovation in the potter's craft towards the end of the Early Neolithic – is singled out by a factor which opposes it to its *single-dented* predecessor. None of the other variables is consistently associated with this factor (although when speaking of two phases only, "consistency" is a somewhat loaded term).

– One non-permanent factor remains to be introduced; in Fig. 45 it has the name "point decoration": In the latest two phases, *point* COMPONENTS of decoration plus the concomitant *indeterminate* ANGLE OF FILLINGS show against all other COMPONENTS (and the other attributes of ANGLE). Once this pattern is known, it can be recognized one phase earlier, when still associated with the *simplex/duplex* factor. In phase 5, the FILLINGS variable also becomes associated, which before had been tied to the *simplex/duplex* factor.

– Finally, in every phase at least one factor has remained uninterpretable – simply because no consistent pattern exists, even no partial concurrence can be found in adjacent phases: Thus, in phase 1, a factor is made of FILLINGS; in phase 2, of the ANGLE OF FILLINGS; in phase 3, of FILLINGS again, and of *elaborated* ENDS; in phase 4 of ANGLE OF FILLINGS, which is as a rule moderately associated with the markers of matriliny; and in phase 5 a factor points to *lines* in the neck decoration.

This completes the generalized description of the decoration on the LBK pottery from Elsloo.

The first analysis attempted in the previous chapter was according to status. There, sex was made visible by specified sets of tools, with decorated ware *not* among them. Any specialization in the use of parts of the site according to sex cannot be determined, therefore, from the data *presently* at hand (cf. e.g., the differential distributions of artifacts at the Olszanica B1 and the Langweiler 9 sites; respectively Milisauskas 1976a: 67-69; and Kuper et al. 1977: 252-254, 259-263).

While the principal differentiating criterion of sex cannot be introduced here, a lot more can be said on ranking. For instance, along the same lines of reasoning as in the graveyard, the considerable differences in the sizes of the huts can be thought of as marking a ranking (or even a stratified) rather than an egalitarian society. To distinguish between these cases several considerations apply:

At a very general level, a hut may have been constructed at a determinate point in the life cycle (e.g., at marriage). In that case, the builders were of approximately equal age, the ranking criterion of egalitarian and ranking society. Then, the differences in type/size must reflect other criteria; i.e., be indicators of ranking or even stratification. Below (pp. 150-151), such a tie will be rejected; the founding date and life cycle are disconnected; from this it necessarily follows that in the aggregate, huts were built at all possible moments in the life cycle of the inhabitants. To separate ranking from stratification we must come down to a more specific level: the different size of the central part of the huts (the living quarters, according to Modderman 1970(I): 110) might be interpreted as reflecting the number of inhabitants, which would be correlated in a general sense to the life cycle:

1. A simple marital couple without, later with, children, polygynous or not, and in the end again by themselves, will count different numbers of people, and accordingly need different areas of floor space at different periods of time. Thus, a conjugal couple will pass through a cycle from two to six or seven (four or five children) and back to two: a fluctuation by a factor of over 2 (on the number of children, cf. Burguière 1974: 80-84; Goubert 1960; Zuidberg 1975: 22, Appendix 3.1; who provide demographic data on both "pre-Malthusian" peasantries and pre-state societies).

2. Yet this variation will be dampened through the effects of agglomeration inherent in the formation of extended households. The extended family will count a couple of grandparents, one or two marital couples plus children, plus some unmarried kin, together some six to twelve people, but this figure will be relatively constant: when the children grow to maturity, the grandparents are on their way to passing out of the picture and all in all the fluctuation may be by at best a factor 2, probably less. (For discussion of the extended family see Wolf 1966; 61-73; also below, pp. 149-150).

When the actual figures are considered (Table 53, which gives the lengths of the central parts) the overall variation is seen to be relatively small: with few exceptions the maximal length is less than double the minimal length. In other words, the table documents the constancy of the household size, a constancy which can be accomplished by extended families alone. Thus, parallel distributions of age/rank (graveyard) and of hut size (village) (Table 54), though suggestive, are spurious.

Still, larger central parts are found in the larger types of houses (Table 55).[3] It is as if larger households with their more extensibe resources (labour power) need more roof to protect their produce from the weather. The important constructional differences between the hut types (or, rather, the additions to the basic central part) seem to point to different functions (Modderman 1970(I): 109-110), and hence for differentiated tasks performed by the members of the households living in these dwellings. Such was precisely the operationalization of "dignity" developed in the graveyard study. When some families needed more space (were larger) they also performed more, and different jobs: the households as units held different positions in a hierarchy. *Part of this ranking must have occurred on criteria other than age.* That is, the Elsloo community will have been ranked. (Lacking data on the distribution of tools, the possibility of specialization cannot be investigated here.)

The argument hinges (among other things) upon the assumption that huts were built in one stroke. From observations during excavations no argument can be formulated against this assumption (Modderman, pers.comm. 200978); also, if it were customary to expand buildings when necessary, a tendency towards type 1b should be observable, which is not the case.

Up till now, I have carefully omitted reference to type 1a huts. Some authors consider these dwellings to indicate more wealthy households, possibly even of chiefs, and others hold to a function such as a men's clubhouse (cf. p. 131); the two views do not exclude one another. Without an exhaustive analysis of tool distributions the point will be impossible to decide. Yet, four weak arguments may be brought forward:

– From a different construction, a different function should be concluded, at least as a working hypothesis. That is, if type 1a huts also had a function as living quarters, then some other function not shared by the other dwellings was fulfilled by them as well.

– Men's clubhouses are fairly common (refer to Milisauskas 1976a for references; other ethnographic examples can be seen in, e.g., Kloos 1971, Rappaport 1968, or Salisbury 1962; a general ethnological discussion on this topic, e.g., in Lévi-Strauss 1969: 118).

– Empirically, at Elsloo, type 1a huts are not selected by the principal components; that is, there is no special type of pot decoration peculiar to them. Also, these huts are not to be distinguished from the other huts on the basis of the number of sherds found near them: the distribution of finds with/without decorated sherds over the huts of type 1a and the other types looks very similar (Table 56). The same is true for their internal construction, which is identical to that of type 1b.

– Chiefly persons were visible in the graveyard because of both a quantative and a qualitative superiority of grave gifts: they were accompanied by more gifts which consisted of more complete tool kits. At least in the grave some differential association with productive means was visible. And it is this latter aspect which set them apart from the ranking order which was seen to operate on the simple principle of quantity. This qualitative difference in the graveyard *may* tally with the qualitative difference of the type 1a hut.

In my opinion, the third point argues for a regular dwelling unit, the first and fourth points for a chiefly status of the household living in it, and the second point is ambiguous: some ethnographical men's houses are lived in by the males of the village, with among them the big man/big men (but in this case, a clear separation of the sexes exists, which in archaeological contexts should be visible in the differential distributions of categories of refuse).

Taken together, a ranked society seems to be attestable from the data for Elsloo; part of the variation cannot be retraced to egalitarian standards. Stratification, however, cannot be substantiated unequivocally; only when pushed into that direction, may some data from both the graveyard and the village) point to incipient stratification.

The other differentiating criterium found operative in the graveyard was that of specialization. Two kinds could be distinguished: arrowheads (etc.), and pottery. The arrowhead-associated specialization cannot be taken into account here; the pot people can only be hesitatingly considered, for there were no special characteristics common to their ware. The status they occupied (2 times moderate, 1 time high) might suggest at first sight an investigation of number of decorated sherds *vs.* hut type (Table 57), when such a correlation of age and type had not been rejected previously (p. 140).

Ever the traditional concern of archaeologists, the chronology of huts (Fig. 17) has been translated into schematized plans of the site, Figs. 46-50. With the idea that huts close together on the chronological component must have stood together, an attempt was made to ascertain which houses were synchronic. Soon it became clear that some groupings of huts showed constancy over time: similar configurations were found to be repetitive. Initially, most of the huts accompanied by decorated ware could be fitted to such a pattern; later the non-ceramic huts were also brought in.

Short-cutting the discussion below, four (or perhaps six) groups of huts appear to be visible. Not all groups in all phases, but four of them have been of longer duration than a single phase – at least in the excavated part of the site (Figs. 46-50, 51). For instance, the *eastern group* may have been in existence for a long time before it moved into the southeastern excavation. However, it has not been possible to assign every single hut to a group: 34

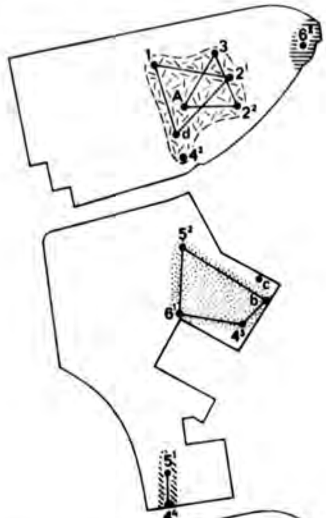


Fig. 46. ELSLOO VILLAGE: Huts in use in the first ceramic phase. Chronological ranking according to factor scores. 1-6, old to young. 'Weight' indicates no. of associated sherds (cf. Fig. 17).

rank	hut	type	weight
1	66	x	C
2^1	65	2	B
2^2	62	1b	C
3	70	x	A
4^1	not entered		(63)
4^2	50	3	A
4^3	19	2	C
4^4	04	x	A
5^1	05	x	A
5^2	32	1b	A
6^1	17	2	A
6^2	75	2	A

huts not datable by decd. pottery

A	61	3	
b	20	2	phase 1 or 2
c	22	2	phase 1 or 2
d	54	3	phase 1 or 2

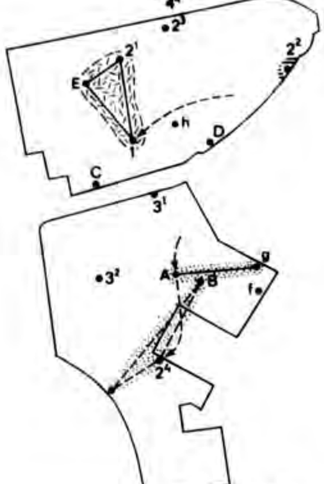


Fig. 47. ELSLOO VILLAGE: Huts in use in second ceramic phase. Chronological ranking according to factor scores 1-3, old to young. 'Weight' indicates no. of associated sherds (cf. Fig. 17).

rank	hut	type	weight
1	55	1a	A
2^1	67	2	B
2^2	64	x	B
2^3	68	1b	B
2^4	10	1b	C
3^1	36	x	C
3^2	28	3	A

huts without decd. pottery:

A	25	1a	
B	26	2	
C	45	x	
D	51	1b	
E	59	2	
f	20	2	phase 1 or 2
g	22	2	phase 1 or 2
h	54	3	phase 1 or 2

Fig. 48. ELSLOO VILLAGE: Huts in use in third ceramic phase. Chronological ranking according to factor scores. 1-5: old to young. 'Weight' indicates no. of associated sherds (cf. Fig. 17).

rank	hut	type	weight
1	76	1b	A
2^1	74	1b	A
2^2	44	x	A
2^3	49	2	A
2^4	48	1b	A
2^5	08	x	A
3^1	58	1b	A
3^2	38	3	A
3^3	34	x	A
3^4	24	x	C
3^5	31	3	A
3^6	37	x	A
4	16	2	A
5^1	15	1b	C
5^2	56	2	C
5^3	47	1a	A

huts without decorated pottery:

A	21	1a	
B	33	x	
c	57	2	either phase 3 or phase 4
d	30	x	phase 3 to 5
e	35	x	phase 3 to 5
f	39	2	phase 3 to 5
g	40	2	phase 3 to 5
h	41	2	phase 3 to 5
i	52	2/1b	phase 3 to 5
j	53	3	phase 3 to 5
k	69	2	phase 3 to 5
l	71	2	phase 3 to 5
o	63	2	(see text, p. 65)

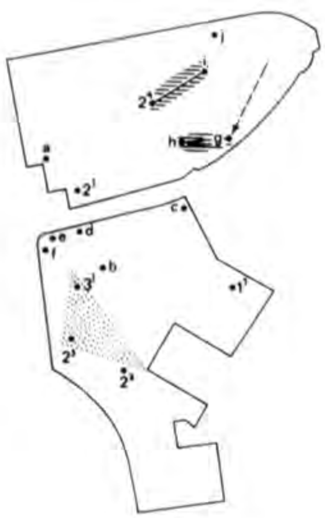
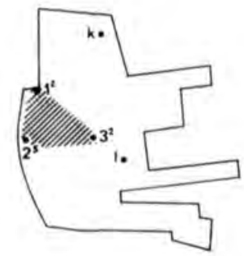


Fig. 49. ELSLOO VILLAGE: Huts in use in fourth ceramic phase. Chronological ranking according to factor scores. 1-3: old to young.
'Weight' indicates no. of associated sherds (cf. Fig. 17).

rank	hut	type	weight
1^1	23	3	C
1^2	84	x	A
2^1	47	x	C
2^2	11	x	A
2^3	14	3	A
2^4	60	2	A
2^5	87	2	B
3^1	29	x	B
3^2	89	1b	A

huts without decorated pottery:

a	57	2	phase 3 or 4
b	30	x	phase 3, 4 or 5
c	35	x	
d	39	2	
e	40	2	
f	41	2	
g	52	2/1b	
h	53	3	
i	69	2	
j	71	2	
k	78	2	phase 4 or 5
l	90	2	



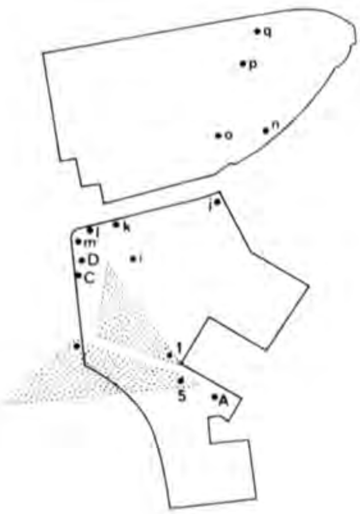
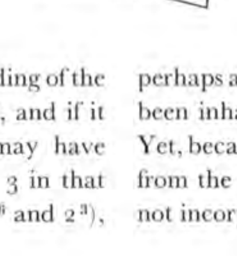


Fig. 50. ELSLOO VILLAGE: Huts in use in fifth ceramin phase. Chronological ranking according to factor scores. 1-7: old to young.
'Weight' indicates no. of associated sherds (cf. Fig. 17).

Rank	Hut	Type	Weight
1	12	2	A
2	93	x	A
3^1	88	1b	B
3^2	83	x	C
4	92	2	B
5	09	2	C
6	79	2	B
7	85	x	B

huts without decorated pottery:

A	07?	x	
B	13	1a	
C	42	x	
D	43	x	
E	91	x	
F	94	1a	
g	78	2	phase 4 or 5
h	90	2	
i	30	x	phase 3, 4, or 5
j	35	x	
k	39	2	
l	40	2	
m	41	2	
n	52	2/1b	
o	53	3	
p	69	2	
q	71	2	



(out of 92) cannot be fitted. Thus, the ending of the *northern group* – if such a thing occurred, and if it happened as outlined in Fig. 48 – may have proceeded beyond the triangle of rank 3 in that figure: huts 35, 37 and 49 (marked e, 3^6 and 2^3), perhaps also 40 and 52 (g, 5^2) may or may not have been inhabited by the descendants of this group. Yet, because of the disappearance of the "*Grossbau*" from the group's ranks these latter five huts were not incorporated into the group's trajectory. Also, huts 57 and 58 (c, 3^1 in Fig. 48) and 40 and 56 (g, 5^2) may possibly belong to a different, unrecognized group. The *northeastern group* has an "ending" similar to that of the northern group (Figs. 48-49). Regarding the *middle group*, in the later part of the third phase it may well be different from that of earlier and later dates; only from its position in the same part of the site has its identification been derived. Hut 16 (phase 3, no. 4) should also be assigned to this middle group, although its whereabouts are abscure.

A *fifth group* may be discernible in the south in phase 1; it may have moved out of observation in the second phase, as all other groups seem to have migrated in counter clockwise trajectories across the site. I am also tempted to propose a *sixth group* in the latest phase of the settlement, for which huts 78 and 79 would stand (g, 6 in Fig. 50).

To arrive at these groups, several considerations applied:

– From Modderman's accounts, a dating, to within a phase in some cases, could be extracted for most of the huts without decorated pottery (those huts that cannot be dated to either the first or the second period of the Dutch LBK have been omitted: N = 10) (Modderman 1970 (I): 6-35).

– Non-ceramic houses were assigned to groups of ceramically accompanied ones on a variety of considerations: apart from phase, stratigraphic observations; constructional details; and distance. Stratigraphic observations: Modderman (1970 (I): 28-35 and (II): Pl. 7-38). Constructional details: P.J.R. Modderman, (pers.comm., 201278). Distance: a choice among alternatives was made on closeness (e.g., hut 59 – phase 2, no. E – has been grouped with huts 55, 67 because it is nearer to hut 55 than to 68; cf. Fig. 47).

– "Synchronousness" and "similar rankings" should not be lightly equated. For example, huts nos. 48 and 49 have been ranked 2^4 and 2^3 in phase 3 (Fig. 48) – that is, *ex aequo* in Chapter III – yet they cannot possibly have stood together (Modderman 1970 (II): Pl. 23). This example is quite clear; similar difficulties may arise, however when dealing with the chronological positions of different hut groups where no stratigraphic controls are available.

When in Fig. 51 synchronism is entered for

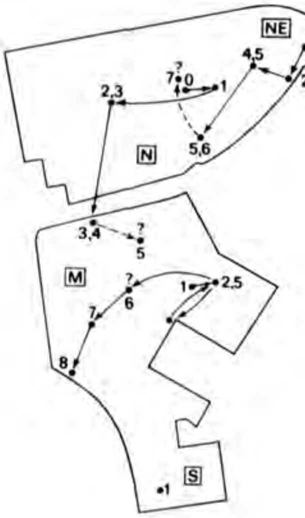


Fig. 51. ELSLOO VILLAGE: Summary of Figs. 46 through 50, showing the successive relocations of each group with time. Numbers indicate synchronic occupations (positions: estimated points of gravity per group). Also cf. Table 15. Example: at point of time 4 (this figure), roughly in the middle of phase 3 (Table 15) the NE, N and M groups existed on the site together; their relative positions are indicated in this figure.

groups, then it is on the assumption that ceramic dates (i.e., rankings of indiviudal huts) provide averages of the period of inhabitation of the huts. A hut ranked t_x will have stood from (t_x − m) to (t_x + n) years B.P. Although m and n are indeterminable by present means in some cases an allowance for this range has been made.

In table 58 the relative chronological positions of the means for every group have been entered. On the bottom scale ten "points" on the chronological scale are indicated at which the synchronisms of Fig. 51 were established, with the allowances mentioned in the previous paragraph.

A number of locational shifts of the groups cannot be dated precisely: the northern group at t_3 and the northeastern group at t_5. As the hut groups will not have moved as groups, but rather hut-by-hut, the lack of precision may be reflecting such shifts.

All in all, 58 huts (20 of which were not accompanied by decorated pottery) plus hut 03 could be assigned to one group or another. 34 huts (13 with and 21 without decorated ware) could not be assigned. They can be derived from Figs. 46-50, and are entered into the right-hand column of Table 62.

What emerges, however, is a fairly constant configuration of the groups. Within these groups the distances between the huts are considerably less than between the groups (Table 59). The northern and eastern groups seem to have been made up of three households each, the middle group of two to four of these units, and the northeastern group of perhaps two or three households (to judge from phases 3 and 4).[4]

Adding the 34 non-assignable dwellings as well (huts nos. 01 to 03 excluded below), on the average one hut should be added to each of the above derived groups. That is, *the excavated part* of the settlement must have comprised six to nine households at the least at any moment. So when Modderman estimates the size of the settlement at two or three hectares in the older period, of which approximately 20,400 square metres have been excavated, and if six to nine huts have stood there simultaneously, then eight to eleven huts should have made up the whole village estimated at $2\frac{1}{2}$ hectares. (With this, the basis of Pavlů's computations is weakened, insofar as it is based upon the Elsloo data: no case can be made for a relatively densely-packed village. Pavlů 1976: 12).

Changing over to the structuralist way of research, we start with the number of groups living in the village of Elsloo. To arrive at that figure, one would have to multiply the number of groups observed by the ratio of estimated over investigated site area, but such a procedure seems a bit risky. However, a minimal number of three groups does not seem too far-fetched: in the first three phases at least three groups are visible, and later at least two (Table 58; Fig. 51). Three is the minimum number of groups with which to operate an asymmetrical exchange system, as hypothesized in the graveyard chapter.

However, this speculation leads to (unsolved) difficulties. If it is to be assumed that throughout the village's existence four groups of three huts each have stood, and if it is also to be assumed that the huts were (re)built ten times, then the total number of huts erected at the site is 120. Modderman's estimate is almost double that number (200 to 250; Modderman 1970(I): 204). Several possible explanations can be thought of: shorter life-span of the huts, more groups on the site in the younger two phases, longer duration of these two phases, etc. For instance, doubling the length of the final two phases (the least unlikely solution) would add another 50 huts. The trouble is, though, that these solutions are merely *ad hoc*, without justification or contradiction in the data.

I do not consider it possible to observe the results of the asymmetrical, lineal exchange system in the village unless all items excavated were brought into play. And even then, only the importing groups might be traceable. This also implies the impossibility of matching (in the *present* study) any or all of

Fig. 52. ELSLOO VILLAGE: Distribution of major VARIABLES of pottery decoration in first ceramic phase (For hut nos., Fig. 46)
□□: MAIN MOTIFS: wave/spiral,
■: present
◤: probably present (via associated attributes)
●: absent
1-9: STRUCTURES: proportion of curvilinearity.
abc: NUMERICITY: a, simplex; b, duplex; c, triplex. CAPITAL LETTER indicates preponderance.
▲: innovators – introducing neck decoration.
⊗: huts with less than 3 decorated sherds.
In parentheses: when less than 10 sherds available.

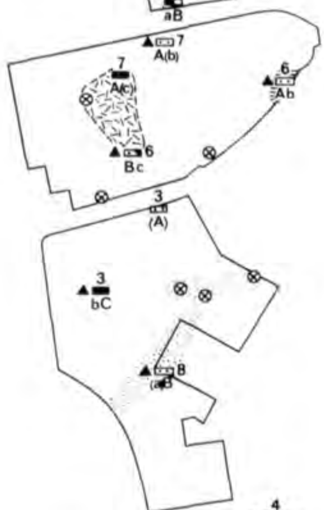


Fig. 53. ELSLOO VILLAGE: Distribution of major variables of pottery decoration in second ceramic phase (for hut nos. Fig. 47). Legend: see Fig. 52.

Fig. 54. ELSLOO VILLAGE: Distribution of major variables of pottery decoration in third ceramic phase (for hut nos., cf. Fig. 48). Legend: see Fig. 52.
▲: innovators, introducing multidented spatula and stab-and-drag decoration

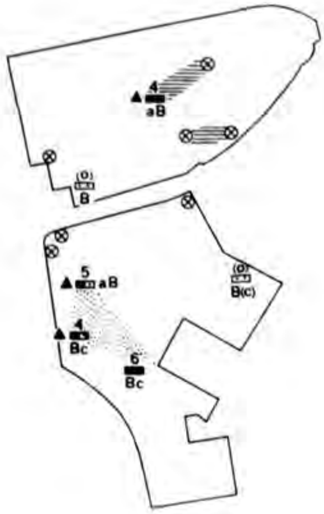
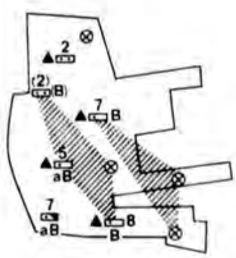


Fig. 55. ELSLOO VILLAGE: Distribution of major variables of pottery decoration in fourth ceramic phase (for hut nos., cf. Fig. 49). Legend: see Fig. 54.

Fig. 56. ELSLOO VILLAGE: Distribution of major variables of pottery decoration in fifth ceramic phase (for hut nos., cf. Fig. 50). Legend: see Fig. 54.



the four graveyard groups to a corresponding group in the village.

While the relations between the groups are difficult to perceive, something more can be said on the topic of group composition – for which unilineal (Behrens, Soudský) and bilineal (graveyard Elsloo) hypotheses have been proposed. When it can be assumed that social groups showed differential preferences for the attributes of ceramic decoration (if structure is anywhere, then it is everywhere) then the geographical distributions of these attributes should provide testing possibilities. To that end, in Figs. 52 to 56 data have been assembled relating to this subject. In fact, these figures are the graphic representation of the outcome of the principal components analysis; the innovator components, matrilineal, main motif, and numericity components are depicted.

A look at the plans reveals that no case can be made for unilineality – the matrilineal mode proposed (the patrilineal mode has not been hypothesized in the literature) would have resulted in a localization of the decorative preferences, so that the various mixes should have been very much one-sided. As the numerical values of the mixes generally vary between .3 and .7, the unilineal case can be ruled out; consequently, a bilineal or disharmonic regime should be adopted.

Perhaps it will be observed that these proportions relate to dichotomous variables. What if the labelling system did not consist of twin symbols but rather of a larger set? Suppose the marker system had four attributes – curvilinear wave, curvilinear spiral, rectilinear wave, and rectilinear spiral – each for one exogamous group. Then the first couple would be using, say, curvilinear wave. Its descendants, in the case of bilinealty, would be associated with curvilinear spiral in the first generation, with rectilinear wave in the next, and its great-grandchildren with rectilinear spiral. Thus, when the mixes are summed, they should exhibit all possible values between .0 and 1., even if the "true" four-group attribute system were reduced through faulty observation to a two attribute one. Uniliny would have resulted, however, in the constant association of the same single attribute with a lineage. Hence, the mixes would be either close to .0 or to 1., with or without reduction, and one can conclude that the precision of the classification is of no consequence in this matter.

Again, when the assumption of group differences in preference for certain ceramic decorations would be invalid, then a more or less even distribution of the attributes is to be expected. This, however, is not the case either: extreme values of the mixes (that is, preponderance of one characteristic to the near exclusion of all other attributes on the same variable) do not occur – except in cases where less than ten sherds are available. In the case that the huts have been lived in for a shorter period, extreme values should be as likely to occur as more moderate figures. Or otherwise, in the case of a longer period of habitation, when summed the mixes should exhibit a normal curve of frequencies – which they do not (p. 151; Table 60). The assumption of group-bound ceramic preferences for ceramic attributes thus underscored leads to the observation that the rules governing residence are not the same as those governing the distribution of these preferences: non-uniliny or rather biliny, would have been the case.[5]

Without addition of further assumptions it is not possible to decide whether this disharmonic regime was of the virilocal-matrilineal or of the uxorilocal-patrilineal variety. But it is possible, for instance, to adduce the ethnographical generalization that the uxorilineal-patrilineal system has only rarely been observed (Murdock 1949: 59); however, as there is no *strict* necessity for non-occurrence this is a weak argument in favour of the first combination. The demonstration of matrilineal succession of preferences for ceramic decoration in the Elsloo graveyard is a much better point to go by. Besides, it leads to the same outcome as the ethnographic argument: a virilocal residence rule plus matrilineal descent. In other words, the hut groups in the Figs. 46 to 50 were inhabited by males who were *de facto* patrilineally related per group – a relation brought out by father-to-son "inheritance" of residence. In more homely phrases: the son succeeded the father on the farmstead. Apart from this succession, women were not only brought in from elsewhere, they were matrilinealy related ("discriminated", or "differentiated" would better describe it), and the web of kinship integrated the residential segments of LBK society. In this way, the seemingly unstructured distribution of all kinds of elements of ceramic decoration over LBK sites finds its necessary explanation: the bilineal social structure prevented localization of any attribute by pumping people in an orderly way out of their birthplaces; the sons became associated with another matriline, the sisters went to live with another group.

I shall now turn to the neo-Marxist frame of reference, where the household was posited as the unit of domestic production and consumption. The conjugal couples in the Elsloo graveyard have already been assembled in extended families (above, p. 140), one associated with each hut. However, "extended families" exist in three types (with intermediates), and these have not been explicitly differentiated and referred to in the Elsloo data. Firstly, there is the opposition of large and small extended families, and secondly, among the small variety, a "stem" and a "joint" type are customarily differentiated (Bohannan 1963: 100-105; Murdock 1949: 32-37, and 1967: 47; Wolf 1966: 61-73). A minimal definition of an extended family includes at least two conjugal groups linked either by descent or as siblings (i.e., polygyny not included here; adapted from Bohannan 1963: 101 and Murdock 1949: 23). Also by definition, a large extended family incorporates at least two conjugal families of siblings or cousins in each of at least two successive generations. When the extended family does not attain such an extent, it is called a "stem family" when there are two related conjugal families from different generations and a "joint family" when there are two related couples from the same generation together with a parent of the siblings. Extended families as residential units are defined by the mode of residence: virilocality results in patrilineal relations between the constituent conjugal couples, and uxorilocality in matrilineal relations (Murdock 1949: 37).

Large extended families consist of at least four conjugal families, which equals eight adult persons, plus supposedly a number of children – all in all, at least ten persons, if not more. A small extended family consists of two couples with a number of children plus perhaps an unmarried relative: some six or seven persons perhaps (children counting for half), which is considerably less. Given the floor area in Bandkeramik huts (between 35 square metres and 112 square metres in Dutch Limburg; Modderman 1970(I): 205; modally some 60-70 square metres) and using the estimate of necessary space as ca 10 square metres per individual (Naroll 1962; see p. 139 above) as a rule of thumb, small extended families appear the more likely hut occupants.

"Stem" and "joint" families can be discerned in the archaeological record from the different patterns of rubbish they may be expected to produce. In a virilocal community like Elsloo a joint family will consist of two brothers plus their wives and offspring; the women will be classificatory (if not actual) sisters, who come from the *same* group. A stem family in similar circumstances will consist of father and son – but they will be married to women from *different* backgrounds (given matrilinear, or double, descent). Thus, decorative preferences on earthenware will be homogeneous in the "joint" case, and heterogenous in the "stem" case.

Table 60 summarizes some data relevant to this point (I have selected the variable STRUCTURES because it is best observable and quantifiable. The other variables in the Figs. 52 to 56 exhibit similar patterns; their poorer distinguishability makes them less suitable to quantification, however). It is apparent that heterogeneity has been the case, and therefore the "stem" family must have been the modal type of household.

An extended family of the "stem" type will have consisted of two couples plus children and unmarried siblings of the younger couple; that is four or five adults and three or four children. This number cannot be directly converted into an estimate of the village's population, however: huts will probably have been built to a size related in some way to the expectable *modal* household size, rather than to actual size. The *average* household size is well below this count – on demographic parallels I would estimate by a factor. .7 (based on demographic statistics for seven nonindustrialized countries, ca. 1960 A.D.; from Table 10-6 in Shryock, Siegel et al. 1976: 172). With this conversion factor added, the population of the Elsloo village must have been in the range of 39 to 69 during the older LBK. As the assumption regarding the relation of hut size to modal household size cannot be substantiated in any way, this population estimate is to be considered a minimum (cf. Modderman's estimate of 54 to 170 people; Modderman 1970(I): 205).

There is another thing that can be derived from Table 60. It has but remote relations with the domestic mode of production, but is, nevertheless, of some archaeological interest: the life-span of the huts. When:

- curvilinearity and rectilinearity are each tied to a matrilineal group (cf. previous chapter);
- the matrigroups were exogamous;
- there were virilocal residence rules;

then, at any *locus* successive generations of women will have used alternately curvilineally and rectilineally decorated pots. That is, summed over space and/or time, the mix will be .5. For individual cases, however, it is the *size* of the sample that is determinant for the mix. If, for instance, two generations are represented in a sample, the mix will be approximately .5 – one woman having left pots marked by one attribute, her daughter-in-law having used the other (because of her origins in a different matri-group). Keeping to this concatenation of alternative preferences per generation and to a sample length of two generations, *it is impossible to frame a sample in which the mix is not .5* (Fig. 57). With a selection of a sample only one generation in length, the mix may attain any value; introduction of a fixed point would result in a two-peaked frequency distribution (one peak for where one attribute prevailed, and another for the alternate attribute).

A sample extending over a different number of generations leads to still another type of distribution. Consider a sample of one and a half generations as an example (Fig. 58). Then, the mix will have a minimum and a maximum limit set by the sample/generation rate:

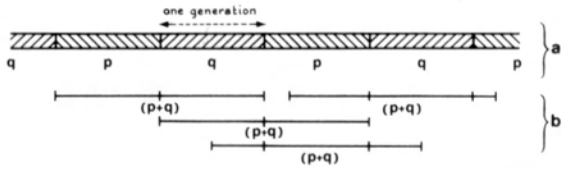


Fig. 57. a: Dichotomous variable, going to its alternate states p or q once per generation
b: Some ways of sampling a two-generation span: sum (p) = sum (q), always, also

$$\text{mix} = \frac{\text{sum (p)}}{\text{sum (p+q)}} = .5$$

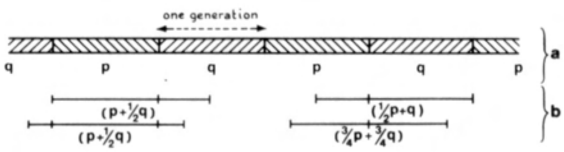


Fig. 58, a: As Fig. 57a.
b: Some ways of sampling a 1½ generation span; sum (p) is always between .5 and 1.0, as sum (q). Therefore:

$$\frac{.5}{1.5} \leq \text{mix} \leq \frac{1}{1.5}$$

$$\text{(the limit value:) } \alpha = \frac{|\,(1\text{-sample length})\,|}{\text{sample length}}$$

the upper limit is $(1-\alpha)$. In this case, $\alpha = .33$, and $(1-\alpha) = .67$. The α-values for the different sample lengths are easily computed; Table 61 provides a summary.

Before evidence and model can be brought together, the types of the distributions still have to be considered. Assuming the starting point of the sample to coincide with the construction of the hut, this point may conceivably be tied to the life cycle (which is a bit difficult with a broken number of generations, but until a few lines below, the Pavlú model of 15-year intervals in construction is still standing). In that case there would be a steep normal curve around one mix-value, plus another one for (1-that value). If, however, huts were built when the old ones were falling down – i.e., a process not linked to the life cycle of individual households but rather to the incidence of dilapidalation – there is no preferred mix-value, and all values within the limits should appear in approximately equal frequencies.

Now turning to Table 60 the α-value at Elsloo is seen to be close to .3, which indicates a sample length of either one and a half or three generations approximately (Table 61). Also, the distribution of the frequencies in Table 60 is less peaked than block-like, which demonstrates an absence of preferred mix-values. Thus the LBK huts at Elsloo have stood for either one and a half or three generations (marriage-death), perhaps some 30 or 40 years, or 60 to 80 years, respectively. Moreover, the building date was not tied to a fixed moment in the individual's life cycle.

A few points should be added. Regarding an eventual tie of the building date to a specific moment in the life cycle, a use of huts over more than one generation makes this hard to imagine, at the very least. Also, extended families of the stem type living in the huts make this an unlikely proposition; to me, it would be very difficult to conceive of a reason why the son would build a hut at a determinate point in his life, when his father had not done so.

There is then no way to choose between the two alternate values for the hut existence except by pointing to the estimated life of the beams. Bakels 1978: 82 presents a table of the life expectancy of native kinds of wood; all are 50 years or less. Elsewhere, on various considerations she estimates the life-span of a hut at ca. 25 years (p. 143). With no extensive repair activities being visible in the excavated huts, it follows that the one and a half generation figure for the life of a LBK hut is to be preferred over the larger number.

Again, this observation does not fit well with Modderman's estimates of 200-250 huts, and 350 to 450 years for the village of Elsloo. With a life-span of 35 years, on the average 15 to 25 huts should have been standing at any moment of the site's occupation. Above (p. 145), four groups of three huts each were "established" – that is, twelve huts, which is clearly less. Again, some "solutions" may be suggested *ad hoc*, such as shortening of the length of a generation, or increasing the number of groups in the latest two phases. For instance, reduction of the length of a generation from ca. 23 to ca. 20 years results in an increase towards 13 to 18 huts at any moment.

It hardly needs to be said that these findings are much at variance with one of the ideas underlying Pavlů's hypothesis of a building cycle of about 15 years: the huts stood considerably longer.

Finally, to conclude this topic, the above has been demonstrated by an analysis of the distribution of the variable STRUCTURES. Other variables might have been used as well, but because they are harder to discern (cf. Ch. I, Section 6), the results would have been less clear-cut. And although I understand neither the transmission mechanism of NUMERICITY nor that of MAIN MOTIFS, extreme mixes do not occur (again, with the exception of small samples); also peaked frequency distributions have not been found.

Returning to the modes of production: just as the huts must have housed the couples visible in the graveyard, so the groups in the cemetery may perhaps be compared to the hut groups – both one step up from the next-to-lowest level of observation. That is, the set of stem families inhabiting the huts belonging to one group (Figs. 46 to 50) should together compose the (local segment of the) lineage; perhaps from this grouping even a lineage mode of production can be derived. Table 62 shows the composition of the groups in terms of hut types per habitation phase. The groups appear to exhibit a regular composition: the better visible and/or the more extensive a group, the clearer a tendency appears for incorporating *one* type 1b hut (sometimes type 1a); Table 63 sums the data on this topic. The other huts in a group are of the types 2 and 3. In the paragraphs on status (p. 141) I have not been able to explain all the variation in hut types by means of simple ranking only. Indeed, it was written there that "the households held different positions in ahierarchy". The repeated reproduction of the same configuration of hut types seems to indicate that these groups were truly groups with their own functions, rather than *only* of an ephemeral nature (which is a function as well). If the two annexes to the central part embodied by types 2 (one annex) and 1b (two annexes) did serve different purposes, then apparently the formation of the groups was geared to incorporate these purposes or to make available the facilities, etc. In other words, there must have been an economic base for these groups.

Given the existence of such groups, an interpretation involving a lineage organization with its attendant status differentials for the component units does not seem unlikely – especially as such an organization was hypothesized from the graveyard findings. Also, we might speculate that the lineage prominents were living in the larger and more extensive huts. Whether the extras indicated by these more complex (and spatious!) dwellings were at their own disposal alone or also came back to the other households of the lineage can not be decided from the data at hand. From the allocation of valuables in the graveyard one would say that to whatever extent control was exerted by the prominents, it did not pass unnoticed. Together, these points make the existence of a lineage mode of production probable.

There are still two other things to be discussed in this connection. In the first place, in the graveyard chapter the matrilineal accent in the lineage organization was much played up. Here, however, the lineage organization is seen to operate on a virilocal basis – that is, with a *de facto* patrilineal principle. The two findings do not contradict one another at all (see also the following paragraph): a bilineal regime was hypothesized from the graveyard analysis, and has been substantiated in the village (p. 148). It is only here that the patrilineal principle appears to be of more consequence than simple virilocal residence: economic and social factors are involved as well.

The second point may have to do with the matrilineal side of the social formation. There are two huts of type 1a that could be dated through the associated pottery (huts 27 and 55); a third one had less than ten sherds (hut 13). All six, however, could each be assigned to a hut group (this is also the case for huts 21, 25 and 94). Type 1a huts may be an alternative to type 1b, as is suggested by Table 62. When a group possesses a type 1a hut, 1b is absent, and (sometimes) vice versa. From this occurrence in different groups, the associated function cannot have been tied to patriliny. If the interpretation of this hut type as a chiefly one is accepted (an interpretation only hesitatingly made on p. 141) the matrilineal principle governing the distribution of chiefly status hypothesized for the graveyard is not contradicted in the settlement. Differently phrased, the patrilines ("minimal segments") inhabiting the type 1b huts also allocated among themselves the chiefly dignities vested in one of the matrilines.

Of course evidence restricted to one case out of six observations is too meagre to be trusted by itself; however, the suggested "explanation" is in line with both ethnology and better substantiated data from the Elsloo graveyard and village alike; and the two observations do not falsify the propositions (though they could conceivably have done so). Where possible, the statements should be tested elsewhere.

Apart from this, and more *ex hypothetico*, there is another line of evidence pointing to the importance of the matrilineal principle in Elsloo. In the graveyard, the male and the female sexes were observed to be of similar status. If the patrilineal principle had been preponderant, then the sexes would have differed in average status, with the females in a lower rank; this not being the case, matriliny must have been accorded considerable weight. However, the functional differentiation of the huts per group and the virilocal definition of the lineage inhabiting the hut groups seem to point to an organization of (part of) the economy along patrilineal lines (considering the level above the individual households) rather than on matrilineal lines. And this argues for a patrilineal mode of production, with matriliny fulfilling other, non-procurement functions (also cf. Lévi-Strauss 1969b: 408).

Turning to the third kin-based mode of production, the supralocal one, some of its effects can be perceived in the excavations. For instance, though it must be stated cautiously, the two groups of innovators (first and second phase, and third to fifth phases, respectively; Figs. 52, 53 and 54 to 56) can be linked to this mode. The caution has two grounds: the hidden assumption of innovations being introduced from elsewhere, which cannot be proven; and the nature of the supralocal contacts: was it people who were imported, or ideas only, or perhaps a mixture of the two? However that may be, the habit of decorating the necks of pots was apparently first adopted in the northern group, when other huts in that group and the other two groups continued in the traditional Flomborn way of leaving the necks bare; it should be admitted, though, that the first neck decoration (associated with hut 50) is quite clumsy in appearance (Modderman 1970(II): Pl. 49). Later, in the second phase, the custom spread to the north-eastern group as well, as it did to an unassignable hut (no. 28), and presumably also to the other, hidden/non-excavated groups. If the idea has come from outside, its occurrence testifies to foreign contacts; if the custom originated at Elsloo, its diffusion to other LBK communities points to similar channels. An analogous case can be made for the introduction of the multidented spatula from the end of the third ceramic phase onwards: in the middle group and in an unassignable hut (no. 58) the implement's traces are found first, in subsequent times diffusing throughout the site. Still another instance is furnished by the presence of point decoration, reminiscent of (but certainly not identical with) the Cologne type of decoration (Dohrn-Ihmig 1976b). It occurs in the southern and eastern groups at the same time (fourth and fifth phases; not rendered in Figs. 55 and 56).

Yet, one does not need a principal components analysis to establish the existence of supralocal contacts: it is sufficient to point to the adzes in graveyard and village; also the sherds (presumably originally in the form of pots) belonging to the enigmatic Limburg group (Modderman 1970(I): 141-143; and, latest, Gabriel 1976) must have come from elsewhere. The huts where this imported ware was found did not distinguish themselves in any appreciable way from the ordinary run (huts nos. 50, 74, 75; respectively Fig. 46, no. 4[2]; 48, no. 2[1]; and 46, no. 6[2]); the three huts are associated with sherds mainly rectilineally decorated with the wave motif.

In one instance it is possible to see different processes at work: in the fourth phase the loadings on the "innovator" principal component of *multidented spatula* and *stab-and-drag components* are very much opposed (but not so in the fifth phase), apparently indicating an initial disjunction of the two attributes. As elsewhere, the one does not presuppose the other – there is no necessary connection between the modernistic traits.

All these signs of foreign contacts do not establish the existence of an autonomous, supralocal mode of production. They demonstrate only that there must have existed a number of inter-community networks, an important part of which will have had the character of an alliance. The distribution of foreign goods (basic materials, valuables, women) will have been the primary goal. But, if the local lineages were segments of a wider system of lineages and clans (that is, every local lineage is a segment, or element in a super-lineage), this transfer was simply another aspect of the lineage organization, in no way distinct from its intra-communal manifestion. However, in the village as in the graveyard there is an appreciable increase in indicators of foreign contacts from the earlier towards the later phases. Whether these contacts did bring about social change cannot be established from the data used here. That is to say, apart from this latter point, no case can be made for or against the supralocal mode of production. The fourth mode of production, the loose one, cannot be demonstrated by means of data on pot decoration only. Even for ceramics, no specialist can be determined. The number of sherds per hut, though showing considerable variation, does not seem to allow such a conclusion (cf. Table 57), nor have kiln remnants been found. A study of the distributions of flint artifacts and debris, of adzes, etc., might very well bring out specialized working areas; it would seem possible, then, to check the inference from the cemetery of a non-hereditary transfer of skills.

In the end we remain with two modes of production fairly well demonstrated and two without conclusive evidence. Consequently, nothing much can be added on the topic of dominance of any of the modes. Both the household as an entity and the lineage as a unit persist throughout the village's existence. The dominance of the lineage over the domestic mode can be inferred from the qualitative differences in hut types per lineage; one would like to have it substantiated from a differential concentration of, for example, foreign goods as well.

On the topic of social change the following remains to be said: the factor indicating matriliny turns up in every phase, and the virilocal arrangements of the huts also persist. It is only the unexplained factors of MAIN MOTIFS and of NUMERICITY that show change: they disappear towards the final phase of the village; a matter of changing tastes only, or of wider implications?

Let us now see what can be said about Bandkeramik social structure at a site that is markedly different: Hienheim.

5. *The BK village of Hienheim*

The Bandkeramik village of Hienheim was built on a left-bank terrace of the Danube, about fifteen metres above the valley floor. From the site there is a magnificent view up (to the south) and down (to the northeast) the river, and of the hills on the right bank. The modern relief does not fundamentally differ from the Neolithic one; from the village a steep slope goes down to the river plain. This slope is the site's natural southeastern border. To the south there is a small dry valley; in earlier times it may have carried water in wet periods; the bottom of this gully is the easiest access road from the site to the river valley. About 650 or 700 metres to the north of this cross valley, a wider gully descends to the Danube. Finally, some distance to the west the land rises to a ridge, a remains of a higher terrace. The settlement site is situated in the southeastern corner of the landscape unit thus circumscribed. The site, locally known as "am Weinberg" ("in the Vineyard") lies on the northern outskirts of the modern village of Hienheim.

Neolithic occupation of the site was discovered in 1955 when H. Neubauer, an engineer of the Danube regularization commission, looked from the hills across the river to the freshly ploughed plot. Reconnaissance revealed numerous Bandkeramik sherds and flint debris lying on the field. The first test pit was dug in 1965 (Modderman 1966); further, large-scale excavations were carried out by a team from Leiden University, led by Prof. dr. P.J.R. Modderman during the summers of 1967, 1968, 1970, 1971, 1973 and 1974. An area of over 1.2 hectares has been investigated. The finds from the excavations up to and including 1970 have been published in Modderman 1977; they are from an area of approximately $^3/_4$ hectare in extent; the present study is mainly concerned with data from the excavations up to 1970.

At the village site, the loess covering the terrace is less flat than at the site of Elsloo: height differences of about two metres occur. In Neolithic times the surface was even more rolling than today. Because of the unevenness of the terrain, erosion has been worse than at Elsloo (Modderman 1966); parts of the Neolithic site have almost disappeared. Many periods from the Middle Paleolithic to the Middle Ages are represented by sometimes considerable amounts of finds from the site; the LBK settlement proper should have covered about 2.2 hectares, and the SSK village (an estimated) 2.7 hectares.

The most densely built part had been investigated by 1970. In all the excavations, 42 huts were uncovered, 25 belonging to the LBK proper, and 17 to the SSK. The dwellings are of different types: in the LBK period: one time type 1b (hut no. 2); twelve times type 2 (nos. 5, 6, 8, 12, 14, 17, 24, 25, 29, 31, 37, 41), and three times type 3 (nos. 1, 35, 43; probably 26, 27 and 51 also); type 2/3, 3 times (nos. 13, 19, 28), three huts from this period are indeterminable (nos. 39, 40, 52). In the SSK period five huts were of type 2 (nos. 3, 4, 15, 22, 42), 10 of type 2/3 (nos. 9, 10, 11, 18, 21, 30, 32, 33, 34, 38) and two of type 3 (nos. 20, 23) (P.J.R. Modderman, pers.comm. 201278). The LBK huts were a bit narrow as LBK huts go: none exceeded six metres in width; the central parts varied between 5 and 19.5 metres in length (at Elsloo, 6 to 16 metres; Modderman 1970(I): 104). There are more things differentiating the Hienheim village from other LBK settlements: there seem to be too few type 1 and type 3 huts; here, contrary to other places, the walls consist sometimes of double rows of posts; the northwestern part has sometimes been subdivided by a transverse wall, etc. Also distinguishing Hienheim from Elsloo is its continuity into the Middle Neolithic (MN, henceforth) (cf. Ch. III).

During the SSK, huts were no longer built with double rows of wall posts, but with single rows instead; the central part of the huts then had a length of 9.3 metres on the average; the huts measured from 5.6 to 7.6 metres in width and between 8.0 to 15.5 in length, with relatively short northwestern sections (1.9 to 2.4 metres; for the LBK huts at this site 1.7 to 11.3 metres). From a research point of view, the SSK huts are considerably more difficult to evaluate than earlier counterparts: in the MN loam/refuse pits were no longer dug parallel to the long walls, but farther afield. To associate such a hut with finds is therefore impossible. The human environment of Neolithic Hienheim must have differed considerably from that of Elsloo, too. The latter village had numerous comparable settlements in its vicinity; Hienheim had no more than three neighbouring villages (if villages they were, and not single field/harvest sheds; cf. Bakels 1978: 141 note) on its loess island within 4 km. Beyond, the woods stretched for 12 km. to the next patch of loess upstream, or 20 km. downstream to the vast, densely occupied Gäuboden (Bakels 1978: 23-26, 52-53; Modderman 1977: 9-11, 121-130).

Across the seven ceramic phases into which the Bandkeramik occupation of Hienheim has been subdivided (Table 53), an unstable and difficult-to-interpret set of factor patterns has been computed (Fig. 59), which is a "verbalization" of Figs. 13-16, and 25:

– "Innovators (1)": At Hienheim, too, innovators have been present, either marked in a positive way, or (artificially) in a negative way (next entry). The introduction and subsequent adoption of the *multidented spatula* (one of the points which already emerged from Ch. III) occurred from the third phase onwards. The low incidence of the new tool in the early phases of its introduction will probably explain its association with *fingertip* decoration, which I consider spurious. In the sixth and seventh phases the preponderance of *rectilinear* STRUCTURES, respectively the infrequent occurrence of *curvilinear* STRUCTURES will have caused the reverse: the (outgoing) *simple spatula* and the (out-going) *curvilinear* STRUCTURES then load high on the same factors. Therefore, it is probably a spurious association again.

– "Innovators (2)": The factor of the "goat foot tool" is indicated. In the two final phases this

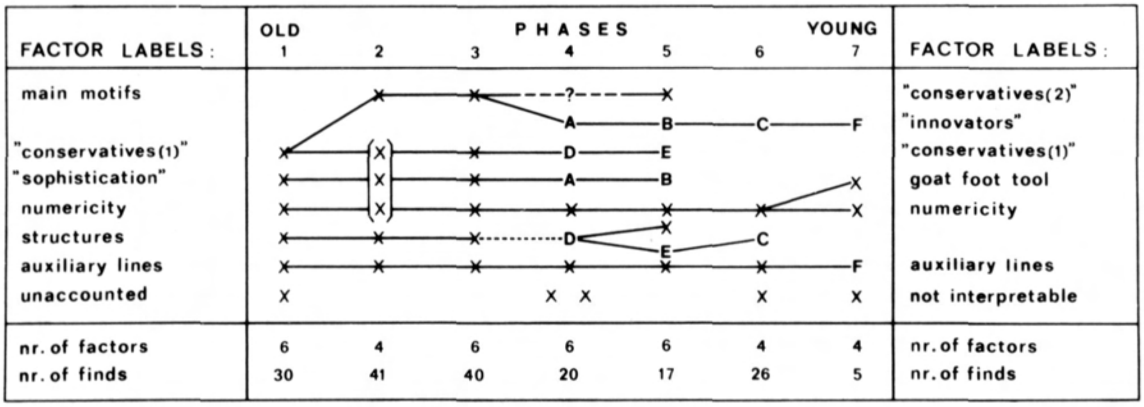


Fig. 59. HIENHEIM: constancy and change in factor patterns.
--?-- connection possible, though not attested.
.... weakly connected.
A to F letters indicate joint occurence on one factor.

instrument is associated with *hatched* COMPONENTS and opposed to *stab-and-drag* and the *multidented spatula* TECHNIQUE. In the sixth phase these loadings still occur on the NUMERICITY factor; more specifically, *duplex* seems associated (but only in that phase).

– "Conservatives (1)": When the LBK occupation of Hienheim began the adoption of (pot)neck decoration was well on its way. Only confusion can be gained by labelling the neck decorators "innovators"; I would rather call the pots decorated in the old way archaic or "conservative". This label, though, is too strong: throughout the Bandkeramik, pots without decoration in the necks keep turning up (cf. e.g., Elsloo). Yet, there is a factor which becomes ever less clearly dominated by this variable, from the first through the fifth phase. As with the previous characteristics, no fixed partner attributes can be found: *simplex* is associated with it in the first phase, and *duplex* in the second; *rectilinearity* in the first, and again in the fourth phase, and so on.

– "Conservatives (2)": As the *wave* MAIN MOTIF in the decoration increases with the transition from LBK to SSK, the pots decorated with *spirals* become an archaic phenomenon. The two main motifs occur in comparable frequencies in the first four phases; it is only afterwards that it makes sense to call one of these conservative. Again, no fixed association with any of the other variables is brought out by the principal components analysis – in most phases where both its attributes are represented, the MAIN MOTIF defines one single factor by itself.

– Structure: As illustrated in Fig. 59 the association pattern of the variable STRUCTURES (at Elsloo, the marker of matrilineal affiliation) is much more complicated than those of the other variables: in the first three phases decoration *without fringes*, and *single-dented spatula* are firmly associated with the *rectilinear* attribute, as are the opposites (simple/multiple spatula spurious?). In the fourth and fifth phases, and weaker so in the sixth, the *multidented spatula* is associated with *rectilinearity* (and simple tool with curvi-). Parallel to these in some phases (1, 3, 4, 5) a factor is found on which *waves* and *non-decorated necks* are associated with *rectilinearity* (as are their opposites: bipolar patterns).

– NUMERICITY is found on an "own" factor in every phase (not separate in phase 2) at Hienheim. In the first three phases, the *duplex* mode is in constant high association with the *point* COMPONENT in the neck decoration and in the later phases with the same COMPONENT in the belly zone. *Triplex* shows a similar conduct of its loadings on this factor as does *discontinuous neck decoration* in the phase 4, 5 and 6. The *simplex* decoration disappears in the sixth and seventh phases.

– "Sophistication" is a factor which is sometimes associated with other factors. It brings out the opposition of decoration applied with *fingertips or nails* against that applied with the *spatula*. In the first phase an independent feature (with no further associations: a specific factor); in the second phase associated with the first conservative ware; in the third phase independent again (specific); in the fourth and fifth phases its association is with the innovative group of *multident-spatula*-decorated pottery; in the final phases this factor is no longer visible. The opposition of the two wares seems a purely analytical one because of this shift of associations; I shall present no further details.

– The factor of the AUXILIARY LINES is always present; its structure is an opposition of *absence* and *presence* of AUXILIARY LINES. Associated with this bipolarity is the occurrence of *fringes* in a reversed way: when fringe, then no auxiliary lines (and vice versa). In the final phases *continuous* and *homogeneous* neck decoration are at variance with the presence of auxiliary lines.

There are some factors left which could not be accounted for in a structured way. They may have to do with some peculiar decoration in a phase; as they are non-repetitive, there is little to be done with them.

A few things should still be said on the above principal components analysis of the Hienheim pottery, in comparison with that of the Elsloo data:

– In contrast to the Elsloo decoration, the MAIN MOTIFS do not load on different factors; at Hienheim their occurrence is therefore not independent of one another.

– Similarly, at Hienheim NUMERICITY is not spread out over two factors, one opposing *simplex* and *duplex*, and the other opposing *triplex* and the other two attributes, but confined to one factor instead.

– *Curvilinearity* and *rectilinearity* (as long as they are present in the data) show a similar behaviour as at Elsloo: they are bipolar attributes, loading on one single factor.

This completes the generalized description of the Bandkeramik pottery from Hienheim.

A recapitulation of the arguments put forward when interpreting the Elsloo village excavations in the previous section would seem redundant. Thus, unless otherwise stated I assume the same chains of reasoning applicable at Hienheim. And this implies, for instance, that I do not think that any sexual division of labour can be made visible on the Bavarian site, as the same type of data is available as for Elsloo: a publication in which hut plans figure predominantly and a punch card corpus of decorated pottery.

On the subject of ranking, however, a few remarks must be made. The pronounced typological differences between the huts at Elsloo do not figure so conspicuously at Hienheim (Table 64) – not at first sight, that is. From the variation in size of the central parts at Hienheim (Table 65) a similar conclusion can be drawn as for the Dutch site: an extended family rather than a nuclear one will have built and inhabited the huts. Although the variation (and the average, but that is not important here) is larger than in the Dutch LBK living quarters, the range of the lengths of the central parts seems to be similar as the two outlying lengths (5, and 19.5 metres) are clearly extremes: the next-following are different by less than a factor 2 (8.5 and 16 metres resp.).

From a simple typological description the uniformity of the dwellings at Hienheim (as compared with Elsloo) is apparent (Table 64). Especially the virtual absence of type 1 huts (cf. note 2) is conspicuous. However, this may well be a typological artifact: the overall lengths of the huts (and disregarding the typological distinctions) are similar (Modderman 1977: 123).

Also, instead of possessing those southeastern sections which define the type 1 huts in Holland, some of the northwestern parts of the huts in Hienheim are further differentiated: six of them are divided by an extra (cross) row of posts (huts nos. 2, 8, 17, 24, 29, 31), a feature not uncommon in the "*Grossbauten*" (types 1) in the Dutch Bandkeramik (Modderman 1977: 126). To complicate matters, three of these huts have even had cross *walls* (rather, trenches; huts nos. 2, 8, 31), for which only with some hesitation two approximate parallels (both at Elsloo) can be found. If the type 1/type 2 differentiation in the northwestern Bandkeramik area may be thought of as parallelled at Hiemheim by the divided/undivided northwestern part of the huts, then the separate room in some of these divided northwestern parts is certainly suggestive of the enclosing wall trench of type 1a elsewhere. Note also, that at Hienheim the only hut belonging to the classical type 1, is also of the analogue type 1.

The argument can even be quantitatively supported (Table 66) – though not proved, of course. From the table, the almost identical proportions of the Hienheim "deviants" and the classical subdivision at other places becomes clear. Also, the associated differentials in lengths of the living quarters is visible, though admittedly less so at Hienheim than at Elsloo. If the argument would prove viable, it would account for the greater homogeneity in terms of the classical typology of the Bavarian LBK architecture, as compared with that of the Westerly areas – given comparable social structures. (To distinguish the classical from the analogue-types, the latter will be typographically marked by a "hat".)

When this proposed typological revision is not accepted, even then the variation in the construction of the northwestern parts of the huts tells of a ranking on other principles than age, as the typological variation at Elsloo did: from constructional differences by way of functional differences towards differentiated tasks for the households.

In parentheses: the Hienheim data alone do not constitute a sufficient reason for reconsidering the classical hut typology. Only when other Central European sites also prove to be consistently anomalous in terms of the quantitative distribution of types[6] *and* when there are grounds for considering the social mechanisms behind hut differentiation similar in all LBK areas, then the typology should be amended (cf. Ch. I for a similar case regarding pot decoration).

Just as in Elsloo, the suggested type î analogues at Hienheim are not selected by principal components analyses of the pottery decoration. Similarly, these parallels do not distinguish themselves in numbers of sherds associated (Tables 67 and 68; the type îa high figure refers to hut no. 31, where the association of pits and hut cannot be established beyond doubt: cf. Figs. 60 to 62 and 64; also, Modderman 1977: 38, 39). Also, the other arguments for and against chiefly figures inhabiting the type îa huts discussed in the Elsloo analysis remain applicable at Hienheim: different construction, different function; and perhaps the qualitative rather than quantitative differentiation of types îa and îb might support the argument (again, not proved).

The aspect of specialization cannot be pursued here, for that would require a distributional analysis of all the kinds of cultural debris. And even without a contrasting attribute a potter's specialization will not be visible: the 25 huts without firmly associated sherds do not prove that the other 12 huts with that attribute have been lived in by potters. And the high status accorded to people buried with more than one pot in the Elsloo cemetery cannot be found reflected in the distribution of sherd quantities as compared to hut types in Hienheim (again, Table 68).

On the topic of chronology the small size of the Hienheim site brings its own difficulties. At Elsloo, 54 huts were accompanied by sufficient numbers of sherds to be dated reliably; at Hienheim, this number is only 12. One of the reasons is that MN huts are no longer surrounded by loam/refuse pits, as were the Early Neolithic dwellings. Another reason is the smaller site/smaller excavation. With ten hectares extent and three hectares excavated, it was possible to ascertain the existence of nearly one hundred huts at Elsloo, and to suppose the existence of 100 or 150 more. At Hienheim there were 2.3 to 2.7 hectares, .7 of them excavated, 37 huts ascertained. Extrapolation from investigated area to all of the site yields the following:

LBK site estimated 2.2 hectares; SSK site estimated 2.7 hectares

LBK huts observed: 19; SSK huts observed: 16

area investigated: .7 hectares (addition of .5 hectares, 5 LBK huts, 1 SSK hut for the excavations after 1970 considerably inflates the estimates resulting from the above data). Averaging the two computations:

LBK huts estimated site total 45-50; id. SSK 40.

If the site was occupied for 450 years (on the basis of the radiocarbon dates; see Ch. III), and if the lifespan of huts derived at Elsloo was comparable (ca. 30-35 years), then some six (LBK; estimated at 300 years) to ten (SSK, estimated at 150 years) huts must have stood at a time in Hienheim. Of these – on the basis of the relative position and the area of the excavations on the site – it may be supposed that at least three to five have been excavated. That is, we should not expect *groups* of huts to be visible as they were in Elsloo. And with only twelve huts datable an unraveling of the site's building history becomes a difficult affair.

As a first approximation in Figs. 60 to 65 the datable pits ("datable" referring to pits with at least five decorated sherds, allowing computation of their relative chronological positions) are rendered in a similar way as for Elsloo in Figs. 46 to 50 (but note the different scales of drawings and phenomena). Those huts that can be associated with pits or dated on non-ceramic grounds have also been indicated (Modderman 1977: 12-45).

A number of comments should precede interpretation:

– Hut 31 is not that firmly associated with finds as it might appear at first sight. With an average duration of the phases of 65 years, its continuous occupation through $2^1/_2$ phases (even if the phases are half the mean length) calls for too much imagination. Moreover, it is not known what happened beyond the limits of the excavation, which are rather close to this hut and the loam pits. From Modderman's description doubts about the association of the pits involved can be read; constructionally the building is "fairly late LBK", which would favour a phase 3, perhaps phase 4 placement (Modderman 1977: 38-39; and pers. comm. 290978). In the fourth phase, however, no loam pits occur in this vicinity; and the almost perfect alignment of *earlier* loam pits to the building would raise at least some questions.

– Hut 17 figures twice on the plans: phases 3 and 4. This paradox is easily explained: in phase 3 the associated loam pits are dated to its later half, and in phase 4 to the very earliest. The structure will have stood, therefore, across the dividing line of phases 3 and 4.

– A similar point can be made regarding huts nos. 1, 2 and 6 (phase 3): hut 6 is dated right at the beginning of that phase (and, consequently, will have been erected in phase 2). Hut 1 is dated exactly at the end of the phase: it will have stood well into phase 4.

– What emerges then, is that at two or three points of time at least three huts have stood together:

(a) late in phase 2, huts nos. 6, 12, and 35.

(b) in phase 3, huts nos. 2, (perhaps) 31, and 24.

(c) late in phase 3, or at the transition from phase 3 to 4, huts nos. 1, 17, and 24.

There is no evidence at all which would indicate synchronic habitation of buildings very close together.

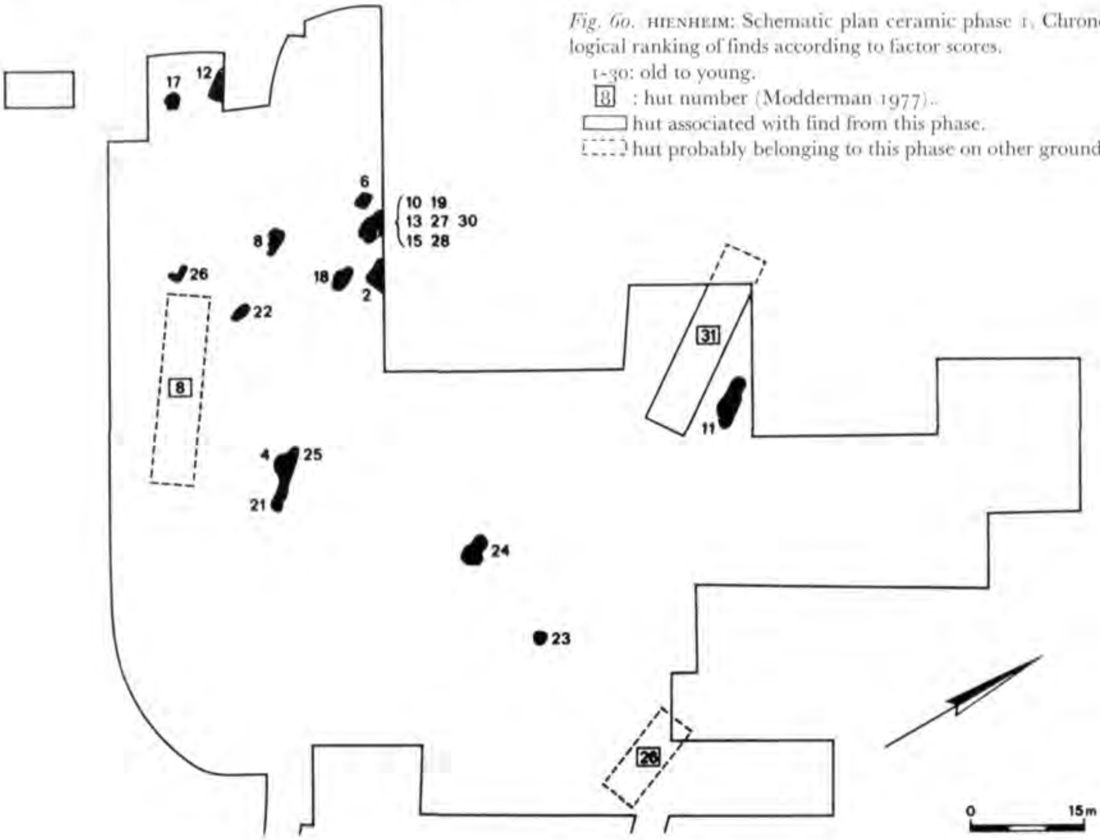


Fig. 60. HIENHEIM: Schematic plan ceramic phase 1. Chronological ranking of finds according to factor scores.
1-30: old to young.
[8] : hut number (Modderman 1977).
▭ hut associated with find from this phase.
⬚ hut probably belonging to this phase on other grounds.

– Taking a diachronical point of view, several hut remains which are geographically close together, seem to have been inhabited consecutively:

(a) huts nos. 6 - 2 - 1 - 5 - 3 - 4 constituting a series from late in phase 2 through to phase 6.

(b) huts nos. 12 - 17 - 14 - 15, a series running from phase 2 to phase 5.

(c) huts nos. 26 - 29 - 24, running from, perhaps, phase 1 into phase 3.

(d) a succession of hut 19 by hut no. 31 is not unlikely.

Combination of the above synchronic and diachronic series with the overall plan of the site (Fig. 66) leads to the suspicion that at least four of five "groups" of remains, or building plots may be visible, with every group being the precipitate of continuous building and habitation activities. The plots have been delimited in Fig. 66;[7] their labels are also indicated there. It is quite satisfying that with the exception of the western group all plots contain approximately equal numbers of house remains: 9-12, which are also almost identically distributed over LBK and SSK periods: southwestern 6/2 (1 not accounted); southeastern 4/5 (2); eastern 6/5 (1); western 2/1 (1); northeastern 6/4 (1). As none of these plots has been investigated completely (i.e., up to the site's margins) perhaps a few more huts must be supposed per series. These plots are analogous to Langweiler 9 sectors (Kuper et al. 1977: 319-328), although the dimensions are different: there, a sector measured 12,400 square metres on the average, at Hienheim 3670 square

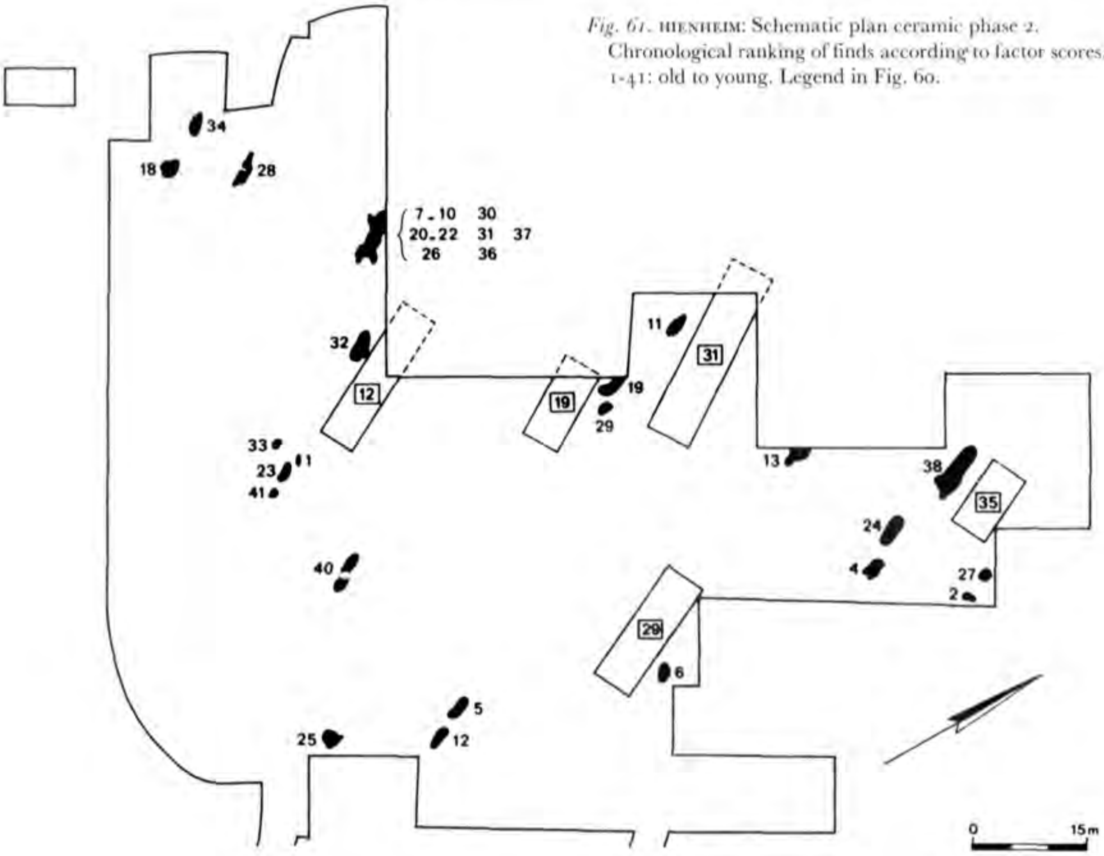


Fig. 61. HIENHEIM: Schematic plan ceramic phase 2. Chronological ranking of finds according to factor scores. 1-41: old to young. Legend in Fig. 60.

metres (6 sectors counted); at LW 9 the distance between the huts was 143 metres, at Hienheim 51 metres (six observations).

From the size of the hut groups at Elsloo (probably three or four huts per group) it follows that no comparable phenomenon can be expected at Hienheim. If anything approaching the lineage group should be sought at Hienheim, it would have to cover the whole site. What is visible in Fig. 66 is the "behaviour" of individual hut builders, who have apparently held to a site and plot demarcation of individual farmsteads for quite a number of generations.

It is even possible to infer the existence of at least one other series of huts to the east of hut no. 34 and to the northeast of no. 28, where there is sufficient space for another plot (now severely eroded); from the occupation of the northwestern plot it almost necessarily follows that in between the latter and the precipice, somebody else's hut has stood. Likewise, the western series should consist of more huts than those recovered.

It will be observed that the above implies that apparently six huts have stood together at Hienheim, which agrees with the LBK estimate but is lower than that for the SSK (p. 159 above). Still, not all plots were built up from the beginning. There is substantial evidence of *use* (digging of pits, wasting of pottery) of the site in the first phase; yet no hut can be unequivocally set to that period. This means that in the first phase a few huts may have stood there, but almost certainly not six of them.

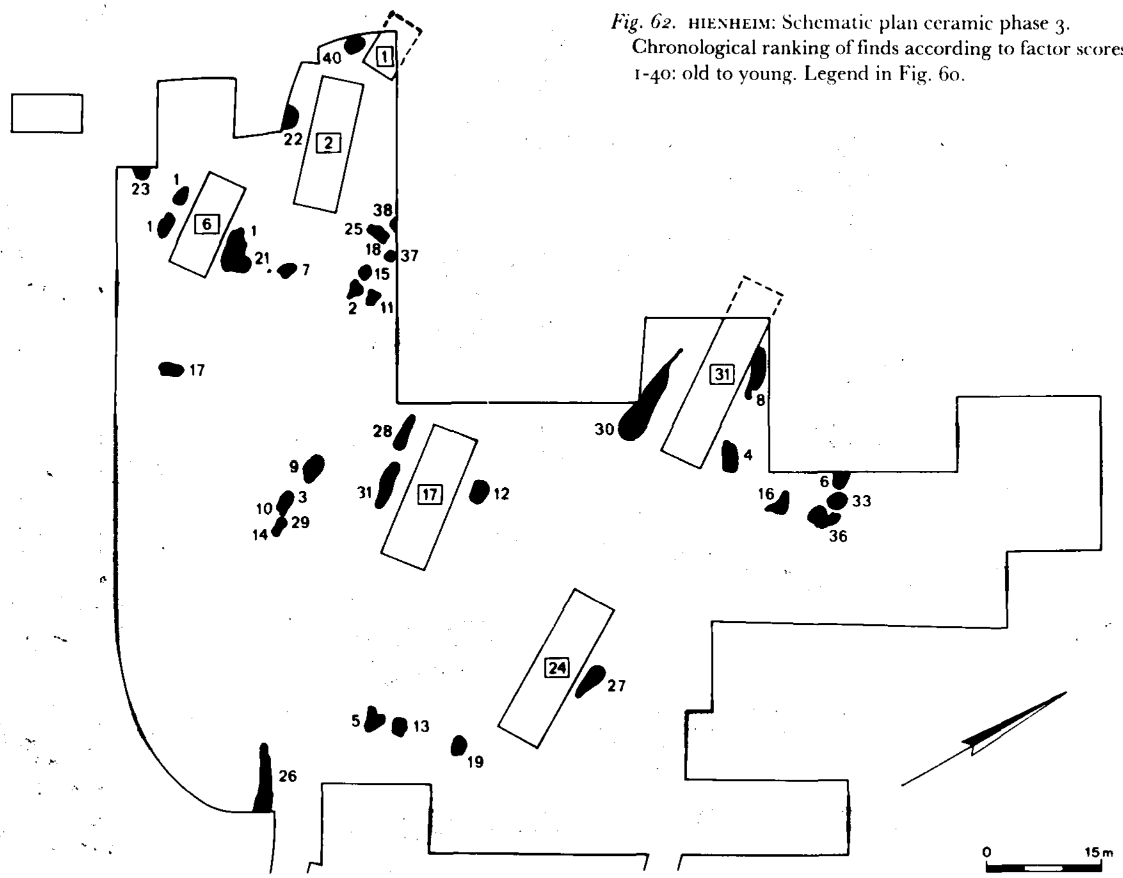


Fig. 62. HIENHEIM: Schematic plan ceramic phase 3. Chronological ranking of finds according to factor scores. 1-40: old to young. Legend in Fig. 60.

Regarding the discrepancy of this six-huts picture and the earlier estimate of ten huts in the SSK, it should be said that the division into plots is very much LBK-centered, although the SSK huts seem to conform neatly to the same pattern. I think this agreement across the Early/Middle Neolithic boundary is a strong argument for continuous occupation of the site (cf. Ch. III). Yet, there should have been more plots and/or huts per plot to account for the difference – the village area was expanded by only 20 percent, whereas the number of huts rose by two-thirds. As the huts are not sharply datable, and also because the excavations at Hienheim have been stopped, these possibilities remain inferential. However, in either case there was a change in the composition of the population by the MN, as more households were set up.

The operation of a ceremonial and connubial exchange system between the households does not seem very likely, if at all possible. The retention of kinship concepts becomes very difficult, as generations never run exactly parallel. For example, normally either the mother's brother's daughter would *not* be of the right age, or she was nonexistent – if she had to be the real mother's real brother's real daughter Kin systems will only operate when the notion of classificatory kin is brought into play to fill up the gaps left by demographical

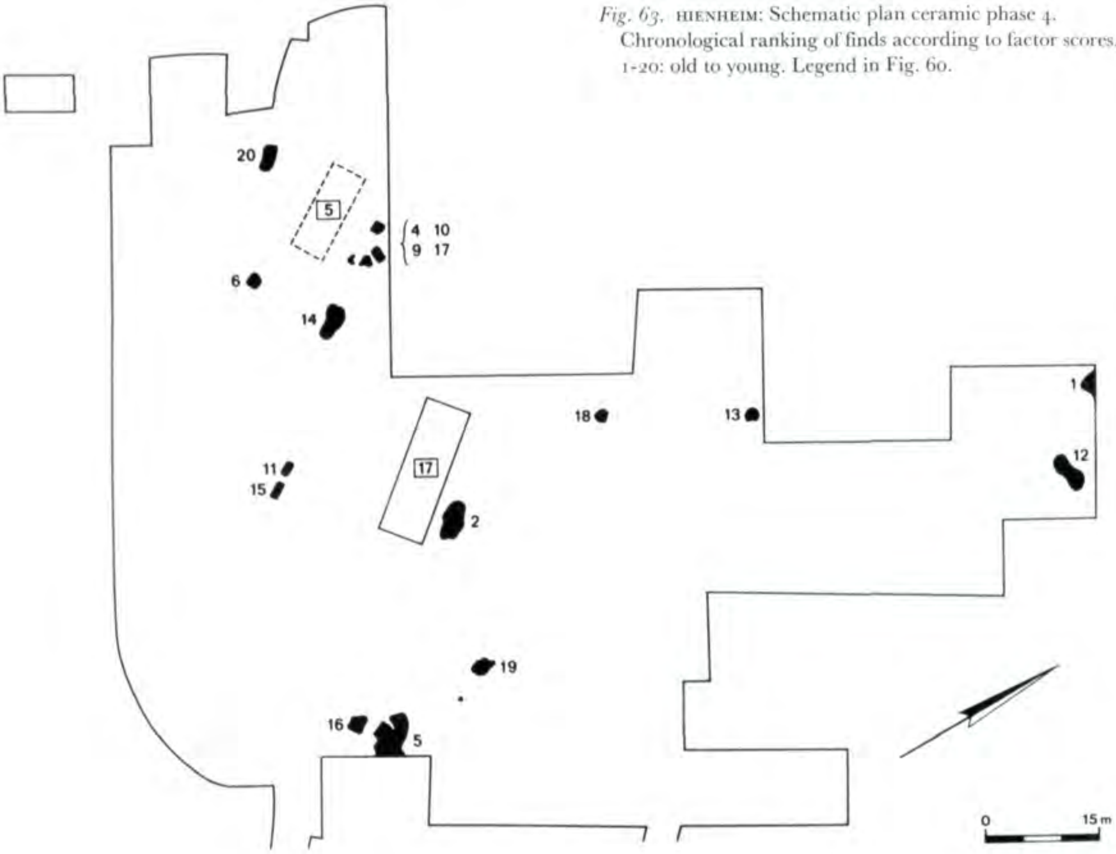


Fig. 63. HIENHEIM: Schematic plan ceramic phase 4. Chronological ranking of finds according to factor scores. 1-20: old to young. Legend in Fig. 60.

fluctuations and thus save the system from these hazards (cf. notes 14, 13 to Chapter IV). We need not even look for such exchanges in a site like this (if other sites in the vicinity were added, this would be different, of course).

Apart from this negative point, the group's composition or recruitment allows more leeway. The bilineal and unilineal options can be approached in a similar way as at Elsloo – that is, through the distributions of ceramic attributes over the site. For Hienheim, however, these have been assembled in a table rather than on plans (Table 69) (in that table, the contents of pit complexes have not been entered; their chronological heterogeneity will mask any pattern ever present in them). The table is, of course, a specification of Fig. 25. From the table, all change appears to have been gradual: old traits just peter out, new ones do not suddenly rise to dominance.

Consider, for example, the introduction of *hatching* and *stab-and-drag* COMPONENTS: qualitatively a peculiar pattern emerges; in the beginning both have a different distribution over the site, to become gradually merged later on (Tables 70 and 71). When the mixes are considered quantitatively, however, they are almost homogeneous from the beginning till the end (Table 69).

More in general this latter table shows an

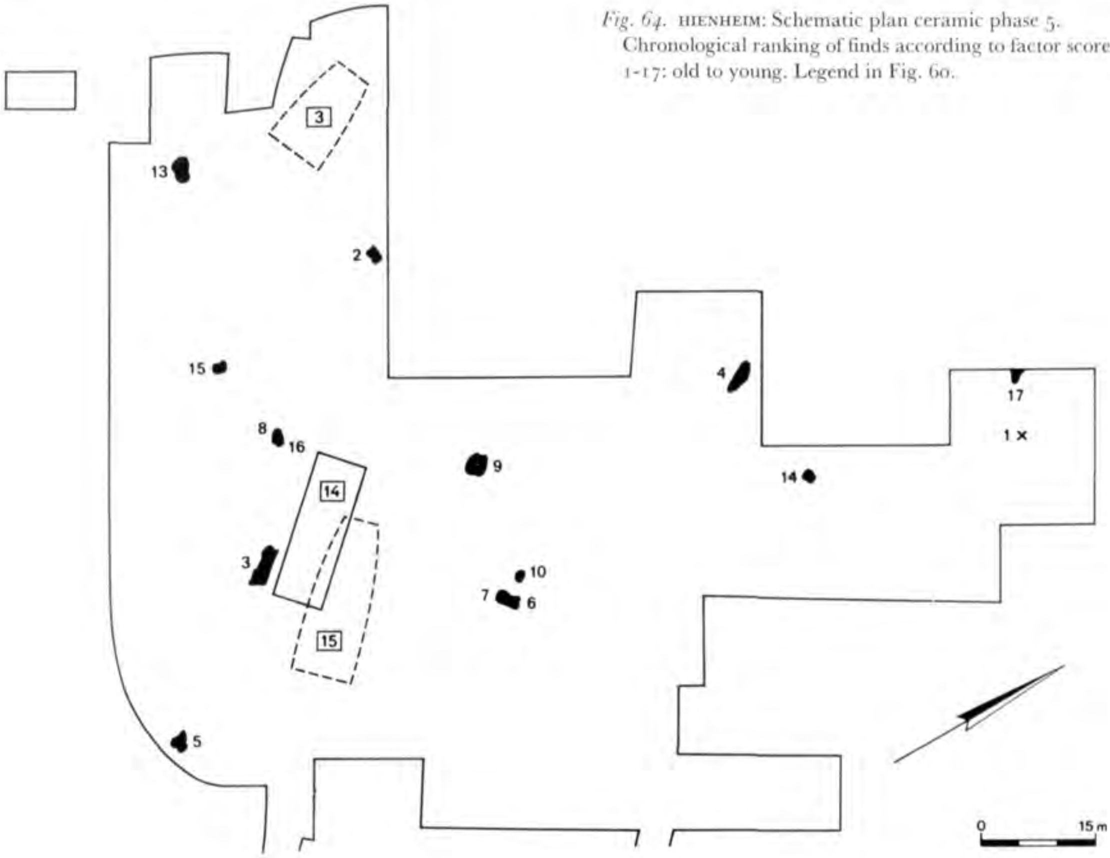


Fig. 64. HIENHEIM: Schematic plan ceramic phase 5. Chronological ranking of finds according to factor scores. 1-17: old to young. Legend in Fig. 60.

important shift. In the phases 1 to 4, spanning the LBK proper, every attribute of every variable seems to be present in every find (with the marked predominance of *curvilinear* STRUCTURES in phases 1 to 3 a major exception). Then, in the transitional fifth phase, an unbalanced distribution becomes apparent for the other mixes, too, which in the sixth phase (the SSK) results in nearly homogeneous mixes on all the variables listed (also cf. Table 72), not even excepting the COMPONENTS. While the distributions in the earlier phases may be explained as evidence for bilineal group recruitment – that is, in line with the findings from the Elsloo village and graveyard – those of the latter phases may stand for a unilineal structure.

A more homogeneous distribution may follow from a number of different types of change. Most drastic would be the breaking up of the exchange chains into small, autonomous cycles of perhaps a symmetrical exchange nature. Chance alone might then well result in homogeneous distributions within these small areas. Across larger regions, however, probability would work in the direction of heterogeneity, especially with a heterogeneous tradition such as the LBK to start from. Such heterogeneity, however, is not the case. Writings on MN pottery from the same general area (Zápotocká 1970), but also from western areas (Meier-Arendt 1975; Stroh 1938) reveal that *no* such diversity is found. Everywhere the *wave* is MAIN MOTIF, and *rectilineally* STRUCTURED decoration.

Less drastic would be a transition from the

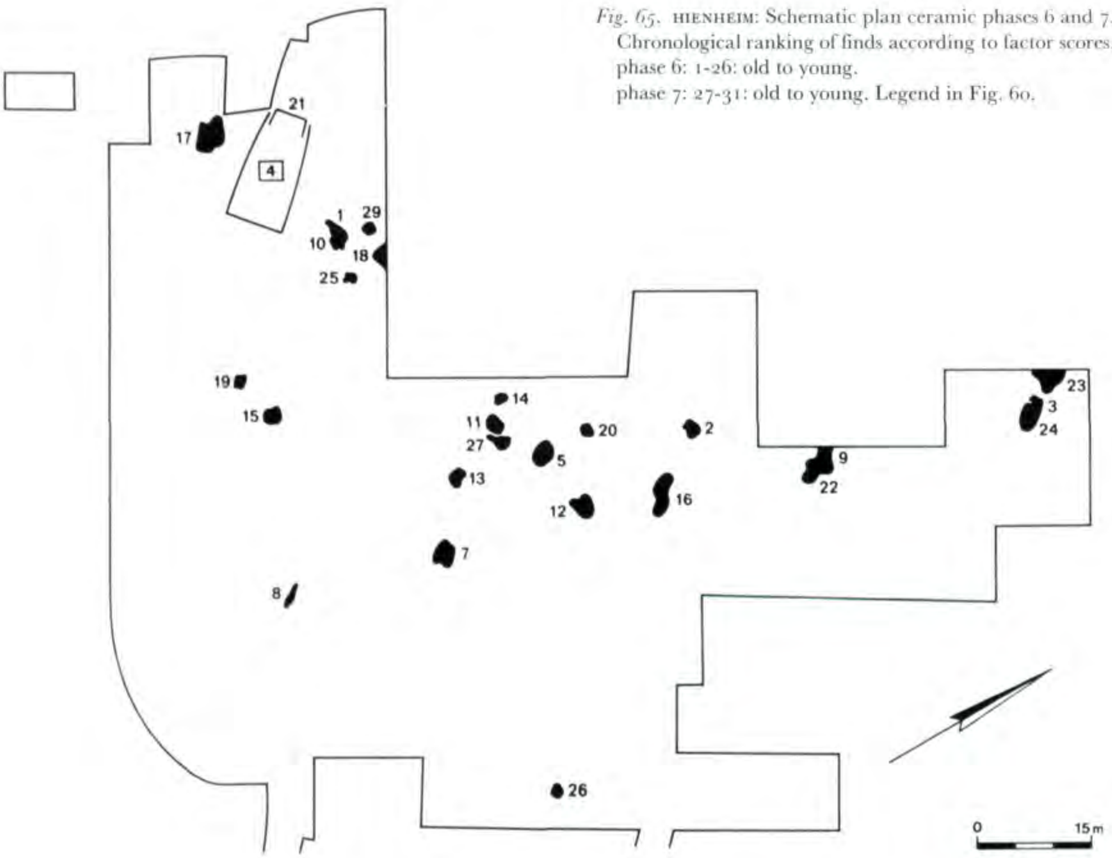


Fig. 65. HIENHEIM: Schematic plan ceramic phases 6 and 7. Chronological ranking of finds according to factor scores. phase 6: 1-26: old to young. phase 7: 27-31: old to young. Legend in Fig. 60.

bilineal group definition towards a unilineal one – less drastic, since this change need not interfere with alliances and exchange relations. Suppression of virilocality when the matrilineal tendencies in the bilineal system became more emphasized would (per locality) result in a very homogeneous pot decoration (e.g., Whallon 1968), for in that case the females stay put, and the males are dispersed over the area. Assuming a tie of decorative preferences to the female part of the community as above, apparently no new variety would be brought in.

The reverse, patri-mode uniliny would arise if matri-descent were dropped in favour of patriliny. Translated to Hienheim, the *de facto* patriliny following from the virilocal residence rule, in the earlier period still crossed, or integrated through a matri-system, became dominant in the later period (for whatever reason favouring the patrilineal option instead of the matrilineal one; for references see Ch. IV). If the marriage preferences, or wider still, the exchange system survived this change – which would be easy enough, because the partner was living in the same locality in the unilineal case as in the bilineal case; only the group names had changed – the females would probably keep bringing in the same preferences, also resulting in a homogeneous outlook of the ceramic decoration. But now, the normal statistical deviations from the stated rules would result in a trickling in of females from other localities as well, presumably bringing other preferences. This latter addition of variety

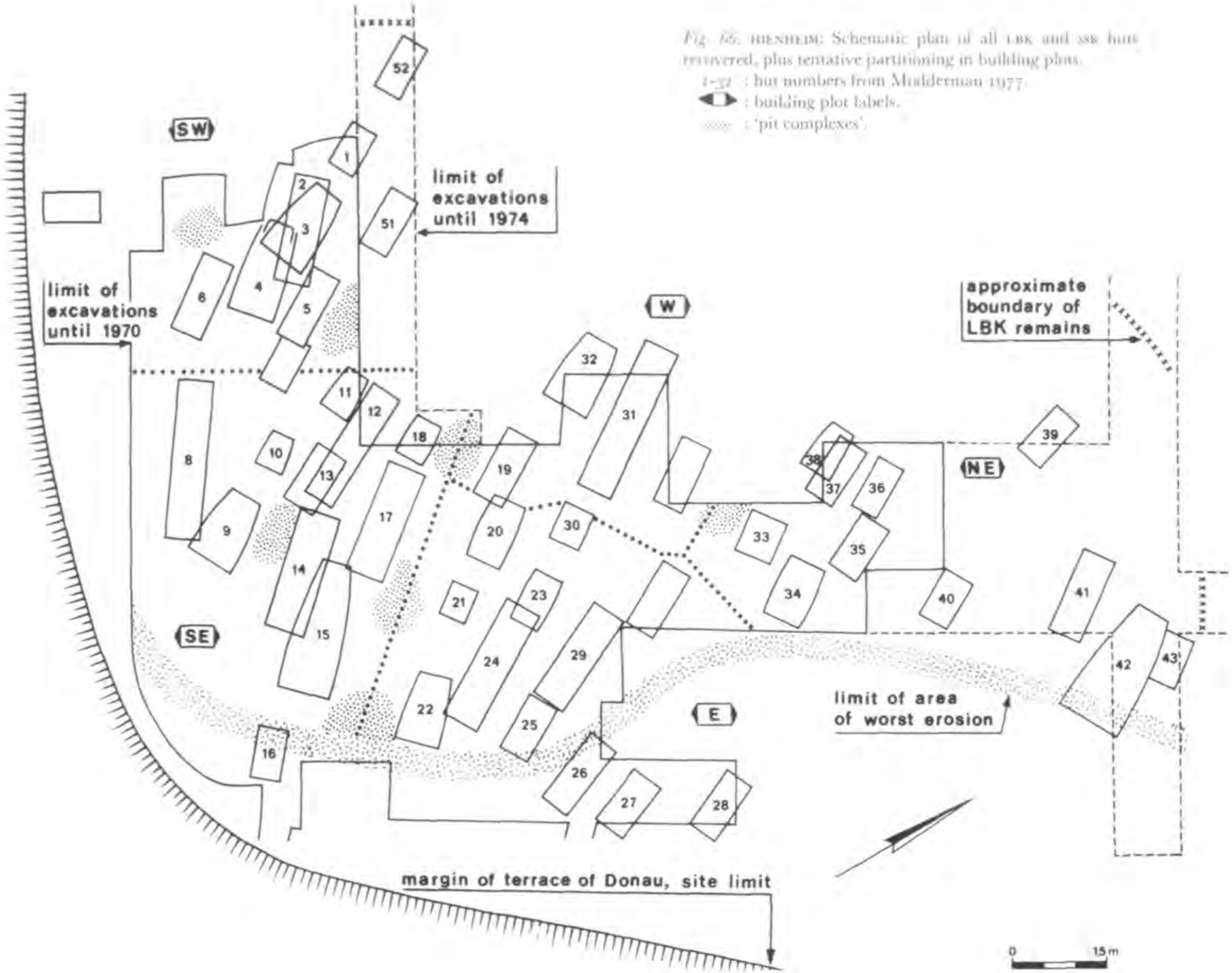


Fig. 66. HIENHEIM: Schematic plan of all LBK and SBK huts recovered, plus tentative partitioning in building plots.
1–52 : hut numbers from Modderman 1977.
: building plot labels.
: 'pit complexes'.

should distinguish the patri- from the matri-mode of uniliny, as in the former the statistical equivocation would only bring in *males* from "non-preferred" localities. From Tables 71 and 72, and especially Table 69 some slight heterogeneity of the mixes is observable, even in the sixth phase (variables TECHNIQUES, STRUCTURES, COMPONENTS; and, perhaps less important, NUMERICITY and AUXILIARY LINES as well). Or, *on the basis of the local data*, virilocal-patrilineal uniliny seems to be fairly well demonstrable. This result hardly comes as a surprise: the transition of patri-descent to matriliny, without intermediary steps, is simply impossible (Murdock 1949: 190).

In this case, however, the general homogeneity of the pot decoration in the MN may be expected to de-emphasize the effects: when there is little variation in the region, there will also be little differentiation visible in the pot decoration at a single site. This, then, underlines the importance of the little variation still visible in the tables just mentioned and strenghtens the patri-unilineal interpretation. From this "hindsight" the biased, one-sided tendencies of the mixes in the LBK period of Hienheim (Table 69) acquire a new meaning. They may testify to a half-hidden unilineal propensity already in that era: more females have been brought in with one set of preferences than with the alternative set. Thus, the exogamy rule which would have resulted in a balance of the two groups (in the aggregate, of course) has not been very strictly adhered to, perhaps an indication of diminished importance accorded to that side of the group definition.

When less value is attributed to the matri-principle in the group composition, this has consequences for the day-to-day affairs as well: females as a class will be accorded less status, and the even footing of the sexes found in the Elsloo graveyard and indirectly attested by the importance of the matrilineal component in the village, will not have occurred similarly in the Bavarian settlement (cf. Pavúk 1972b: 72; in the Nitra LBK necropolis women held less status than men).

In summary, on the basis of the same arguments as given for Elsloo a virilocal-matrilineal system can be found in LBK Hienheim. That we are dealing here with a much smaller social group as well as with smaller units of data than in the Dutch village will be the cause of the sometimes more extreme values of the mixes in individual pits; the one-sided *trend* points to a uni-(patri-)lineal bias already present in this period of the settlement. The probably continuous occupation of the plots on the site indicates a father-to-son inheritance of the farmstead. The women brought in would have come from different social groups. With the onset of the latest LBK phase (no. 4) a shift away from the bilineal pattern towards a unilineal group definition asserts itself; in the sixth ceramic phase of the settlement the matri-duality had probably gone out of use, although the exchange system need not have changed in the meantime.

In the neo-Marxist way of looking at the LBK, at Elsloo the domestic mode of production was found to be organized in stem family households. At Hienheim, the central parts of the huts cover an area of 12.5 metres average length (Table 66) by less than 6 metres width – say, about 70 square metres, a surface area only slightly larger than at Elsloo. The heterogeneous mixes describing the ceramic decoration were used to negate the existence of joint (one-generation, horizontally extended) families in favour of the stem (multiple-generation, or vertically extended) families. Although at Hienheim the mixes for the different variables are partially one-sided they are certainly not homogeneous (Table 69), from which a similar conclusion as that for Elsloo can be drawn: stem families inhabited the huts. Needless to say, for the SSK period with its unassociated and homogeneous pottery decoration, no such conclusion can be drawn here.

Based on comparative figures for the number of inhabitants per hut the Hienheim LBK population must have counted at "any" moment $6 \times 4 = 24$

adults, plus some 25-35 children (cf. p. 149; I assume six huts).

The computation of the life-span of a hut breaks down upon the asymmetry of the mixes at Hienheim (Table 72). For the fourteen pits that can be more or less reasonably associated with a hut, the variable STRUCTURES assumes values centered around .8, and MAIN MOTIFS shows a peak at 1.0. Both distributions are one-peaked, a distribution at variance with the model developed on pp. 150-151. A connection between the building date and the life cycle of the builders should result in a two-peaked distribution, according to that model. Otherwise, if such a correlation were not the case, a block distribution within symmetrical values would be apparent. Finally, construction within a generation cannot account for this distribution either, as the "mirror" values are missing. I will therefore not venture an interpretation of Table 72 on this subject.

At Elsloo, the lineage mode of production could be demonstrated by virtue of a number of these groups: households were found to form compounds which were reproduced over time as such. In Hienheim, only a continuity of the household units can be made plausible (above, the discussion on plots; pp. 159-162). A lineage organization among these households can be suspected only by analogy to the findings in the previous section. Whether or not such an organization existed, status differences between the various households could be demonstrated (p. 158). The various hut types, associated with these status differences are distributed over the "building plots" as indicated by Table 72 (essentially, the huts grouped as "x", indeterminable, belong to the SSK period; judging from the length of the huts, considerable differences have to be assumed for that phase, too). The type î huts are all dated to different points of time (except no. 31, but the difficulties with that building have already been elaborated on p. 159). Their sequence seem to be 8 (phase 1, or 1/2); 29 (phase 2); 2, 24, 17 (respectively early, middle, and late in phase 3). Just as in each of the better observable groups at Elsloo, in Hienheim one household seems to have ranked higher than its contemporaries. From this account, no support for the îa/îb distinction can be found; it cannot be rejected either, as we do not know what is behind the LBK finds from elsewhere on this loess patch. What remains remarkable on this topic is that (as at Elsloo) the two so-called types exclude one another: there was either a type îa hut (2, 8, 31) or a îb one (17, 24, 29).

Regarding the mode of lineage organization, the patrilineal principle must have been at its root. At first, in LBK times, virilocality resulting in patrilineal succession of the households accounts best for the subdivision of the site into "plots"; matriliny will have played its role, too, although from the distribution of the decoration of the pots, it can be derived that this principle was less important. Later in the SSK phases, patriliny/virilocality seems to have been even more dominant, when not to the exclusion of the matri-principle.

The supralocal mode of production can be deduced for Hienheim with rather less hesitation than was the case for Elsloo. The local group must have been too small to be self-sufficient in all respects: with only six households living there endogamy is very unlikely for lack of possible partners; and as soon as "age" is complicated by any restriction following from notions of kinship, the probability of finding an attractive spouse is even more reduced. Considering the changes in hut construction, in format and execution of the pot decoration, in social organization and in tool kit (M.E.Th. de Grooth 1977: 69) – local invention of all these new things can be safely excluded. But, here too, if the village's lineage was merely one branch of a larger unit, then these innovations might have passed down the lineage network straight away instead of with an intermediary network between different lineages or clans, in which case no supralocal mode of production need be assumed.

Finally, the existence of what has been called a loose mode of production could be brought out to a certain degree in the analysis of the Elsloo graveyard. As in the Elsloo village, at Hienheim no traces of specialist potters have been found: there are no kilns. From the table in De Grooth (1977: 70) differential distribution over the site of flint artifacts appears; yet the quantities are too small to infer specialist working areas (M.E.Th. de Grooth, pers. comm. 181278) (the rich inventory of hut 31 indicated in that table should be considered very critically: there are reasons to suspect a non-homogeneous origin; cf. p. 159 above). However, even when specialist working areas could be demonstrated, by themselves they do not make a mode of production, as the productive relations should be known as well. Without a graveyard where these can be made visible/invisible, the inference of a loose mode of production remains speculative.

All in all, the Hienheim data allow us to establish the existence of two modes of production: the domestic one which persisted from the LBK through to the SSK; and another one either of a lineage and/or of a supralocal principle. Some stratification was found among the village's huts, but the smallness of the site indicates an occupation by one lineage only, and so this latter unit is coextensive with the community and will be masked by it. There were interlocal contacts, which in the case of a larger lineage incorporating the local segment may have gone by way of this organization (and then also be part of a lineage mode of production). Or the local community or lineage may have had contacts with other lineages or communities, in which case a different organization existed: the supralocal mode of production. If the differences in hut format and size are indicative of a lineage mode of production, then it also follows that the latter was dominant over the former. It is also clear that during the community's existence, (factors favouring) the patrilineal principle came to dominate (those behind) the matrilineal one which initially had been present as well. Finally, there are no specialist working areas to indicate a loose mode of production; as there is no instrument to probe into the relations either, the existence of a loose mode of production is mere speculation.

6. *Conclusions on Bandkeramik social structure: summary*

Perhaps the major material conslusion of this study is that considerable differences have been found in the social structure from one site to the other – although they are variations to the same theme. And the major formal conclusion can be that although a pilot study may shed light on a host of problems on the one hand, on the other hand such a study cannot be conclusive by itself: there are many differences between the results of Chapter IV (the graveyard pilot study) and those from Section 4 of the present chapter (the study of the settlement next to the graveyard). One hopes, of course, that these differences are real, i.e., they reflect the differences between the "sacred" (the graveyard) and the "profane" (the village); but the possibility cannot be excluded that the differences simply are analytical artifacts.

As differences have been found between the sites of Elsloo and Hienheim, the conclusions drawn from this study cannot be considered definitive nor valid for all Bandkeramik; for the same reason, some statements of other authors are qualified, not falsified – what has been the case in Bavaria may, or may not have been the case in Bohemia.

More specific, regarding the hypotheses derived from the Elsloo graveyard the following can now be said (and I refer to Chapter IV for their contexts):

On the positional paradigm:

A. The social network was *differentiated* according to sex and to some specializations.

Sex: seems plausible from circumstantial evidence.

Some specializations: untested.

B. The social network was *ranked*, rather than stratified (ranking criteria: age, some specialization).

Age: seems plausible from circumstantial evidence.

Some specializations: untested.

Ranked rather than egalitarian or stratified: a ranking social structure has been found at Hienheim and at Elsloo; at Elsloo some evidence may point to a hereditary chieftaincy (perhaps even to incipient stratification).

On the structuralist approach:

A. The social structure consisted of a number of *residential groups*, integrated by way of *asymmetrical and linear exchange* relations; the groups were defined in a *bilineal* or disharmonic way by virilocal residence and matrilineal descent.

Residential groups: these have been found at the Elsloo village; one such a group may or may not have made up the community at Hienheim.

Asymmetrical and linear exchange: untested, though possible on the number of groups living at Elsloo, and not excluded by a different community make-up at Hienheim.

Disharmonic or bilineal group definition: at both sites demonstrated. At Elsloo, (virilocal) residence and (matrilineal) descent rules equally emphasized throughout the village's occupation. At Hienheim a unilineal tendency found already in the LBK period seems to have materialized in a truly unilineal (patri-mode) group definition by the Middle Neolithic SSK.

On the neo-Marxist viewpoint (only the organizational aspect has been handled):

A. A domestic mode of production was built around a *division of labour* according to sex, united in *conjugal pairs* grouped in *households*.

Division of labour: plausible on circumstantial evidence; demonstrated in the graveyard only.

Conjugal pairs: as division of labour.

Households: as separate units these were found to be continuous organizations for the duration of both villages.

B. A lineage mode of production afforded *differential access* to the group's surplus production grounded on *positions in the matrilineage*.

Differential access: not conclusively proven, but possible.

Matrilineal organization, positions therein: at Elsloo, the patrigroups seem to have been as strong as the matri-organization;at Hienheim the matri-principle was only moderately expressed in the LBK, to disappear in the MN. Positions in the lineage organization: the evidence at both sites can be interpreted as pointing to a hereditary status difference between the households making up the group(s).

C. A supralocal mode of production led to the *import* of people and innovative ideas.

Import: well-established, but not specifiable to nature (people, products or ideas); unclear whether within-lineage or between-lineage/ community organizational networks were involved.

D. A loose mode of production applied to *non-hereditary craft specialization.*

Non-hereditary craft specialization: only established in the graveyard.

E. The lineage mode of production *dominated* the other modes.

Dominance of the lineage mode of production: seems fairly well attested at the Elsloo village; at Hienheim from circumstantial evidence only.

Some of the above hypotheses might have been tested in the literature, too; however, as the present research is primarily empirically-directed, that possibility has been left open. From the litarature another set of hypotheses regarding LBK social structure has been derived in the second section of this chapter (to which the reader is referred for context and reference):

A. Huts have been the dwellings of *extended families* of either a *joint* or a *stem* nature.

Extended families: did occupy the huts at both sites.

Joint families: have not been found at Elsloo nor at Hienheim.

Stem families: both at Elsloo and (LBK-) Hienheim (SSK unknown).

B. Extended families were *matrilineally* defined.

Matrilineal definition: at both sites a virilocal definition was found.

C. Huts have stood for *15 to 30/40 years*.

15 to 30/40 years: at Elsloo, one and a half generations (marriage-death).

D. Huts of type 1a have been *men's houses* and/or *dwellings* in which the local chiefly/big man family lived.

Men's houses: for *a priori* reasons, these huts will have had a special function, which could not be specified.

Dwellings of the local headman: seems likely on circumstantial evidence from Elsloo.

E. On *relations between the households* no suggestions have been put forward in the literature.

Kin or economic relations: cf. above at various places.

F. *Status differences* of ranking nature existed *between the households*.

Status differences: could be tied to the lineage organization at Elsloo; that is, a strain towards hereditary hierarchy or stratification.

G. The village community was headed by *a council* of lineage elders, led by a *primus inter pares (elected)*; or by a *big man*.

The council of lineage heads: will have existed, on circumstantial evidence, in the village composed of a number of lineages, such as Elsloo.

A chief-elect: at Elsloo some evidence points to a hereditary chiefly (matri-)line; this is in line with findings from the graveyard.

A big man: cannot be ruled out; but cf. "elected".

H. Settlements were *economically self-sufficient*.

Economic self-sufficiency: as part of the labour force was brought in from elsewhere, the self-sufficiency should be slightly qualified: both at Elsloo and at Hienheim a part of the women came from elsewhere.

I. Relations *between the settlements* consisted of some (quantitatively unimportant) *trade*, *exchange of valuables*.

Between settlements: rather, between lineages (cf. "exchange").

Trade: due to absence of non-pot data, not tested.

Exchange of valuables: as "trade". Women will have been exchanged, as may be derived from the graveyard analysis.

Finally, a few disconnected observations should still be made:

1. Although this study was originally conceived as an analysis of ceramic decoration (of which especially Chapter I is a testimony), no reference at all has been made to earthenware in the present section. General descriptions and conclusions on that topic can be found in Chapter III and in the sections 4 and 5 of the present chapter.

2. From hindsight, the analyses suffer from an important shortcoming: the lack of data on non-ceramic topics. Regarding the huts, this could be improved in a relatively easy way by reference to the relevant publications. For other categories (flint, tools, etc.) this was not possible and has had serious consequences for the description of the social structure at both sites. It seems advisable that in future studies of this kind, from the onset a summary description of the not-directly-involved data should be prepared and entered along with the main body. This applies to every possible principal object of investigation: the other classes of data should not be completely neglected.

3. The markedly different compositions of the communities of Elsloo and Hienheim make comparison difficult. In fact, these differences support Pavlů's (1976) typology of LBK villages: larger and smaller villages and individual farmsteads. The latter have not been discussed here; "smaller villages" might be one-lineage-settlements such as Hienheim; and "larger villages", aggregated lineage communities such as Elsloo. When this typology could be further substantiated, then, e.g., the Langweiler 9 settlement would be better comparable as to type with distant Hienheim than with nearby Elsloo; Langweiler 8 seems to be more like Elsloo; J. Lüning, pers. comm. 151278).

4. Though it is not my specialization, I would yet like to suggest an amendment of the classical typology of LBK architecture (Modderman 1970 (I): 100-120). Such an amendment would drop the threefold division of the house plan in favour of a division based on the complexity of the several parts (cf. p. 158). If this proposition has any grounds, a further differentiation of Middle European huts might be made – which in turn would facilitate site-to-site comparisons of social structure.

NOTES

[1] Several Neolithic ethnographies are available at present. First and foremost, the Sahlins (1968) and (1972) volumes provide general introductions to the subject. Also, Fried (1975) should be mentioned, though a study of a different kind: it is concerned with the delimitation of what is to be considered "tribal", and in this way it touches upon many issues attributed to Neolithic societies. Dalton (1977) deals with the nature of exchange and barter in pre-state societies and should be known to every archaeologist looking beyond one single site. And finally, the volumes in the American Museum Sourcebooks in Anthropology series are readers on almost every conceivable topic in ethnology.

[2] Technically, "filiation" is the recognition of descent through both mother's and father's lines; "complementary filiation" is the recognition of mother-child relationship, or rather, of some rights and duties of the *mother's* lineage *vis-à-vis* a child, in a *patrilineal* society: or of the *father's* lineage in a *matrilineal* society. (Bohannan 1963: 80; Fox 1967: 133.) I think it is Kulczycka-Leciejewiczowa's and also Soudský's intention to indicate simple growth of the number of households pertaining to a lineage.

[3] Type 3 huts consist of an isolated central part only: type 2 huts have an annex with entrenched walls added on the northwestern end of the central parts; type 1b huts can be thought of as type 2 with an extension to the southeastern end of the central part. Type 1a differs from type 1b in one aspect: it has a wall trench all around; all other constructional details are similar. It is remarkable that the northwestern extension is a precondition for the occurrence of the southeastern part. For discussion and illustration of LBK house types see Modderman (1970(I): 100-120), which is mainly concerned with the Dutch LBK. For Hienheim the typology has been expanded and amended: Modderman (1977: 123-129). Also cf. Soudský (1969).

[4] At the Langweiler 9 site, huts were not grouped: four dwellings occupied as many sectors of the area and were apparently rebuilt within their respective sectors. There the average distance between the individual huts was 136 metres and the mean size of the sectors 12,630 square metres (Kuper et al. 1977: 326). At Elsloo, the latter figure must have been smaller, even per group: during the LBK 1 period, two to three hectares were occupied by three or four groups; later (LBK 2), the comparable figures (seven or eight hectares, at least four groups) result in a larger area per group, though still not per individual hut, of course.

[5] Incidentally, from the discussion of the basic assumption on preferences per group, a possible (though unlikely) alternative to group constitution can be ruled out as well: bilaterality (Murdock 1949: 56-58) should distribute attributes according to a normal model also. For in the case of bilaterality, the composition of a household is dictated by exigencies which work counter to possible strains towards lineality. In one marriage, a viri-"lineal" choice will be made, and in the next a uxori-"lineal" one; the structure of bilaterality is non-elementary. Thus, at Elsloo the elementary system has been adopted. Other complex, or non-elementary arrangements (most notably Crow-Omaha types) can be excluded for the same reason.

Finally, the derivation of a bilineal system in the previous chapter was still partially dependent upon the correctness of asymmetrical exchange (of ceremonial gifts). Here, it turns out to be dependent upon the assumption of structured preferences, which seems to be at least as well-founded. Of course, "asymmetry" is but a specification of "structuredness".

[6] For instance, among the 148 huts excavated at Bylany up to 1978, all three types occur in approximately equal proportions: type 1 huts, 14 to 21%; type 2, 5 to 20%, type 3, 14 to 18%; with 67 to 40% indeterminable (P.J.R. Modderman, pers. comm. 201278; cf. Soudský and Pavlů 1972: 317: type 1 huts are most

common in Bylany). At Elsloo type 2 huts preponderate: type 1, 21%; type 2, 30%; type 3, 11%; 38% indeterminable; n = 92 (from Table 64).

[7] There is no definite reason to group hut 8 with the southeastern group rather than with the southwestern group. Yet in the latter case the plots would become narrow strips, and the number of hut remains per plot very unequal (southwestern, 12 huts; southeastern, 8 huts; with the other plots as before). And when the origins of pit complexes should be envisioned as described by Kuper et al. (1977: 74, 331) the complex between hut nos. 9 and 14 (cf. Fig. 66) will not have been a complex (which is a considerable obstacle in the terrain) when hut no. 12 was built; the wide range in time of the finds in this pit complex do not oppose such an interpretation. Perhaps not a good reason, but more in the way of speculation, is a classification based on the size of hut no. 8 being similar to that of no. 12, its alleged southeastern plot successor, but very dissimilar to no. 6, the alternative successor in the southwestern plot.

TABLES

TABLE 1: Efficacy of codebook for the observation of ceramic decoration as measured for the data from Hienheim and Elsloo settlement debris. Counted in noise corrected files (i.e., Hienheim all finds larger than 4 sherds; Elsloo, all finds larger than 2 sherds). Data from Hienheim are younger Early Neolithic + Middle Neolithic; Elsloo, older + younger Early Neolithic.

VARIABLES	HIENHEIM (1)	HIENHEIM (2)	ELSLOO (1)	ELSLOO (2)
GENERAL:				
TECHNIQUES	1.00	1.00	1.00	1.00
NUMERICITY	.90	.34	.91	.43
ZONING	1.00	1.00	.86	1.00
BELLY ZONE:				
STRUCTURES	1.00	1.00	1.00	1.00
IMAGE	not relevant			
BASIC MOTIFS	.80	.28	.54	.20
MOTIFS, DEVELOPED	.38	.06	.10	.04
AUXILIARY LINES	.86	.35	.90	.48
FILLINGS	.92	.89	1.00	.89
ANGLE	.99	.89	.94	.63
LIMITS	.45a	.05a	1.00	1.00
ENDS	.90	.34	.78	.30
SEC. FILLINGS	.54b	.13b	.54b	.15b
COMPONENTS	1.00	n.rel.	1.00	n.rel.
NECK ZONE:				
FILLINGS	1.00	.78	.83	1.00
ANGLE	not entered			
LIMITS	1.00	1.00	.84	.99
COMPONENTS	1.00	n.rel.	.86	n.rel.

Columns 1: proportion of finds in which variable was observed.
Columns 2: proportion of motifs per variable related to the totality of motifs.
a: relates to elaborated limits only.
b: variable with implicit attributes.
n.rel.: not relevant.

	Hienheim	Elsloo
no. of finds	179	151
no. of finds with belly decoration	179	144
no. of finds with decorated necks	93	63
no. of motifs	4646	1984
no. of observable motifs in belly zone	3683	1524
no. of observable neck decorations	733	245
no. of sherds	6138	2345

TABLE 2: Hienheim distributions over time ("PHASE") of finds and sherds, ordered according to the first principal component from a principal components analysis. Respecting this basical sequence, the finds have been regrouped to an even distribution of the sherd quantities ("MODEL I"), a normal distribution of same ("MODEL II"), and twice a normal distribution ("MODEL III").

PHASE	PRINC. COMP. 1				MODEL I				MODEL II				MODEL III				PHASE
	pits		sherds		pits		sherds		pits		sherds		pits		sherds		
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%	
20	6	4	131	3	14	9	223	5	6	4	131	3					20
19	2	1	21	-	5	3	269	6	4	2	36	1					19
18	2	1	15	-	12	7	234	5	4	2	56	1					18
17	3	2	31	1	20	12	247	5	3	2	185	4					17
16	2	1	144	3	6	4	240	5	9	5	214	4	5	3	49	1	16
15	3	2	106	2	15	9	261	5	17	10	272	6	9	5	174	4	15
14	2	1	52	1	8	5	210	4	13	8	304	6	3	2	185	4	14
13	1	1	10	-	5	3	262	5	21	13	391	8	9	5	214	4	13
12	1	1	8	-	8	5	218	4	10	6	414	9	6	4	125	3	12
11	1	1	6	-	6	4	264	5	12	7	425	9	9	5	81	2	11
10	2	1	59	1	5	3	246	5	8	5	527	11	11	7	219	5	10
9	1	1	39	1	3	2	281	6	7	4	319	7	7	4	217	4	9
8	6	4	125	3	2	1	191	4	8	5	356	7	20	12	404	8	8
7	-	-	-	-	9	5	211	4	9	5	367	8	15	9	549	11	7
6	5	3	46	1	4	2	273	6	6	4	245	5	12	7	556	11	6
5	6	4	101	2	6	4	256	5	6	4	208	4	13	8	739	15	5
4	21	13	424	9	6	4	264	5	6	4	160	3	13	8	512	11	4
3	24	15	710	15	6	4	217	4	3	2	60	1	10	6	370	8	3
2	56	34	2446	50	9	5	243	5	1	1	86	2	9	5	240	5	2
1	20	12	379	8	15	9	243	5	11	7	97	2	13	8	219	5	1

The numbers of pits (or, finds) and the numbers of sherds relate to the same finds.
Total number of pits entered: 164; total number of sherds entered: 4853.
Percentages unadjusted.

TABLE 3: Listing of the 20 finds adjacent to the inferred discontinuity, i.e. comprising parts of the phases 14 and 15 of MODEL II. Numbers indicate percentages of trait-occurrence per variable.

TECHNIQUES				COMPONENTS (BODY)					COMPONENTS (RIM)					MAIN MOTIFS		STRUCT-URES		AUX. LINES		RIM DECN.		NO. OF SHERDS	FIND NUMBER	FACTOR SCORE
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22			
66	29	01	04	08	24	21	02	45	01	17	56	.	26	96	04	35	65	76	24	19	81	AVERAGES OF PHASE 15		
26	68	.	05	.	48	52	.	.	.	.	100	.	.	x	x	.	100	100	.	.	100	21	0868	-1.005
25	75	.	.	01	99	.	.	.	x	x	x	x	x	100	.	.	100	.	100	x	x	20	1143	- .917
90	10	.	.	13	06	.	.	82	.	33	67	.	.	x	x	33	67	100	.	.	100	7	0546	- .895
78	22	.	.	22	08	13	.	57	x	x	x	x	x	x	x	40	60	42	58	x	x	9	0697	- .710
100	.	.	.	01	03	10	.	86	x	x	x	x	x	100	.	50	50	100	.	x	x	5	0316	- .706
50	33	.	17	02	52	.	.	47	x	x	x	x	x	x	x	33	67	100	.	x	x	5	0418	- .679
83	08	08	.	09	14	.	.	77	x	x	x	x	x	67	33	80	20	42	58	50	50	10	0364	- .461
100	.	.	.	09	32	07	.	53	.	38	62	.	.	x	x	40	60	100	.	33	67	7	0648	- .379
64	29	.	7	10	21	49	.	20	x	x	x	x	x	x	x	69	31	83	17	x	x	13	0232	- .134
100	.	.	.	18	.	82	.	.	x	x	x	x	x	x	x	25	75	100	.	33	67	5	0823	- .121
																						INFERRED DISCONTINUITY		
86	14	.	.	04	79	.	17	.	x	x	x	x	x	100	.	43	57	34	66	x	x	7	0614	+ .027
92	05	03	.	34	24	.	04	38	08	16	.	.	76	100	.	77	23	47	53	31	69	59	0526	+ .030
																						PHASE BOUNDARY		
88	.	13	.	41	23	08	28	.	x	x	x	x	x	.	100	67	33	69	31	x	x	14	0576	+ .049
100	.	.	.	56	44	.	.	.	x	x	x	x	x	.	100	20	80	76	24	x	x	11	0313	+ .077
83	17	.	.	20	51	.	.	28	.	06	17	.	78	100	.	56	44	54	46	25	75	9	0611	+ .097
100	.	.	.	50	50	.	.	.	x	x	x	x	x	x	x	25	75	50	50	x	x	5	0302	+ .130
100	.	.	.	12	88	.	.	.	x	x	x	x	x	100	.	25	75	100	.	x	x	5	0736	+ .155
100	.	.	.	38	32	.	29	.	x	x	x	x	x	100	.	71	29	40	60	67	33	14	0522	+ .239
100	.	.	.	20	48	06	.	26	x	x	x	x	x	x	x	73	27	51	49	67	33	10	0380	+ .262
89	.	11	.	73	16	.	11	.	x	x	x	x	x	x	x	67	33	84	16	x	x	11	0227	+ .263
92	02	06	.	38	47	01	09	05	09	44	05	09	33	65	35	58	42	36	64	42	58	AVERAGES OF PHASE 14		

'.' equals 'zero'
'x' equals 'missing because of lack of data'
N.B. 'No. of sherds' relates to all wall sherds, excluding rim sherds.

LEGEND

1. simple spatula
2. multidented spatula
3. fingers/nails
4. 'goat foot tool'
5. lines
6. points
7. hatchings
8. finger/nail imp.
9. stab-and-drag
10. lines
11. points
12. hatchings
13. finger/nail imp.
14. stab-and-drag
15. waves
16. spirals
17. curvilinearity
18. rectilinearity
19. absence
20. presence
21. absence
22. presence

TABLE 4: Roots of averaged sums of squares of standard deviations of traits per phase: summed over the 8 traits listed in TABLE 5.

PHASE	MODEL I	MODEL II	MODEL III
20	11.6	7.1	
19	12.7	15.6	
18	22.4	10.7	
17	30.8	7.8	
16	22.2	22.4	1.2
15	21.9	26.8	11.5
14	15.5	27.8	6.7
13	20.4	22.2	22.2
12	38.5	24.7	19.7
11	19.2	28.6	24.8
10	17.4	16.5	28.6
9	10.9	12.1	23.1
8	10.8	18.1	22.8
7	10.9	15.3	30.6
6	22.2	18.4	17.1
5	25.5	12.8	11.0
4	24.2	14.5	19.2
3	17.3	16.3	18.8
2	13.5	.	16.3
1	23.8	20.3	20.3

TABLE 5: As TABLE 4, though summed per trait over phases.

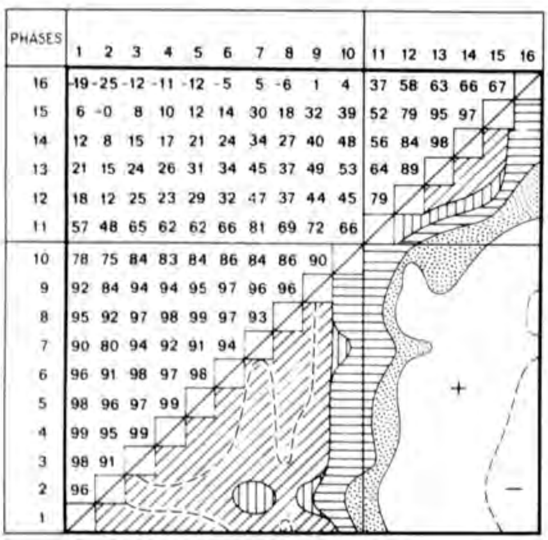
TRAIT	MODEL I	MODEL II	MODEL III	N
single dented spatula	14.3	13.4	12.9	164
points in body decoration	15.7	14.8	16.4	164
stab-and-drag in body decn.	11.4	13.1	15.4	164
points in neck decoration	21.2	21.6	21.9	86
stab-and-drag in neck decn.	17.3	16.6	17.6	86
wave motif	26.9	25.2	27.8	131
curvilineair structure	15.8	13.7	13.2	164
presence of auxiliary lines	25.1	25.3	22.8	138

N: number of valid cases per trait in the complete file of 305 coded finds.

	PHASED SEQUENSE FROM CANONICAL ANALYSIS																				
MODEL I	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	Σ
20																			2	2	4
19																		2	1	2	5
18																	1	3	2	1	7
17															1	1	3				5
16		1														2	1		1		5
15						1						1		1							3
14		1										1			2	1					5
13	1	1												1							3
12				1						1	1										3
11	1		1							1			1		1						5
10	1				2					1											4
9					1					1	1	1		1							5
8						2								1							3
7		1										1	1								3
6	1		1			1					2	1									6
5		1		1				1	1												4
4	1				1		1		1		1		1								6
3				1			1	3	1												6
2			2	1			2		1	1											7
1			1		1	1	1														4
Σ	5	5	5	4	5	5	5	4	4	5	5	5	3	4	4	4	5	5	6	5	93

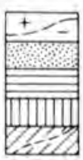
TABLE 6: Fits of sequences of pits distributed according to MODEL I and the first principal component of a principal components analysis (vertical) and from a canonical analysis of raw data (horizontal) resp. based on 11 and on 43 traits (out of 82).

r (hor.; vert.) = .70.



PHASES	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
16	-19	-25	-12	-11	-12	-5	5	-6	1	4	37	58	63	66	67	
15	6	-0	8	10	12	14	30	18	32	39	52	79	95	97		
14	12	8	15	17	21	24	34	27	40	48	56	84	98			
13	21	15	24	26	31	34	45	37	49	53	64	89				
12	18	12	25	23	29	32	47	37	44	45	79					
11	57	48	65	62	62	66	81	69	72	66						
10	78	75	84	83	84	86	84	86	90							
9	92	84	94	94	95	97	96	96								
8	95	92	97	98	99	97	93									
7	90	80	94	92	91	94										
6	96	91	98	97	98											
5	98	96	97	99												
4	99	95	99													
3	98	91														
2	96															
1																

TABLE 7: Between phases correlations for MODEL III, as calculated from 22 traits (7 VARIABLES). Above the diagonal the actual correlation coefficients, below a contour map, shaded as indicated in the bottom.



$r \leq .45$ ('very low')
$.46 \leq r \leq .63$ ('low')
$.64 \leq r \leq .77$ ('moderate')
$.78 \leq r \leq .89$ ('high')
$.90 \leq r$ ('very high')

MACRO PHASE	I	II	III	IV	V	VI
I	+	96	77	53	12	-22
II	96	+	86	69	28	-07
III	84	87	+	66	48	05
IV	62	76	66	+	63	37
V	20	38	48	63	+	66
VI	-12	01	05	37	66	+

TABLE 8: Correlations of macrophases defined from TABLE 7, weighted as to sherd counts.

Above diagonal, MODEL III-A, below, III-B. Definitions as follows:

III-AI : aggregate of phases 1 & 2 of MODEL III.
II : " 3 to 9 " .
III: identical with phase 10 " .
IV : " 11 " .
V : aggregate of phases 12 to 15 " .
VI : identical with phase 16 " .

and

III-BI : aggregate of phases 1 to 6 of MODEL III.
II : " 7 to 9 " .
III-VI: identical with macrophases of III-A.

		INHUM.	CREMN.	
GIFTS	PRESENT	55	29	84
	ABSENT	11	18	29
		66	47	113

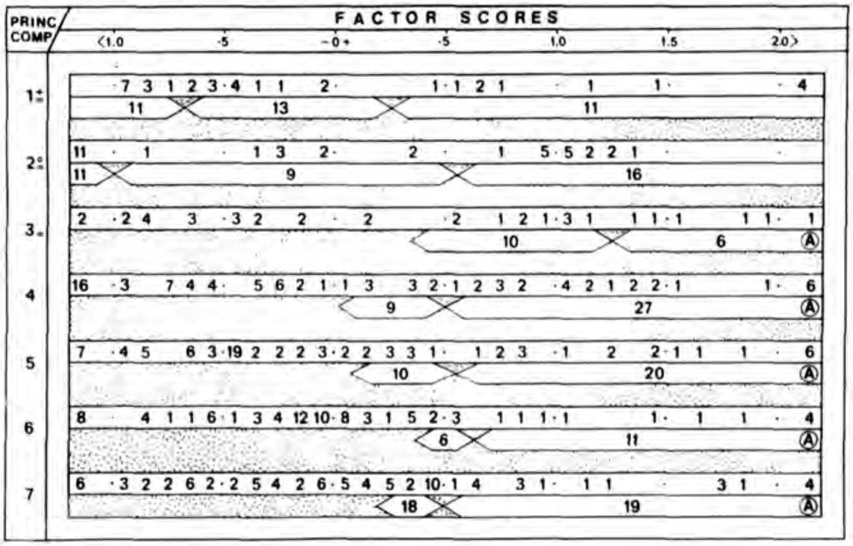
TABLE 9: Grave goods vs. burial rites at Elsloo cemetery.

$\chi^2 = 6.88$ $p = .10$

TABLE 10: Pattern of loadings on the first seven principal components (VARIMAX rotated). Before input, counts were transformed to proportions per variable or category.

PRINCIPAL COMPONENTS			1	2	3	4	5	6	7
RAW COUNTS OF EARTHENWARE		undec. pots	-.18	-.19	-.05	.13	.45	-.03	.42
		undec. sherds	.23	.05	-.64	.40	.07	-.14	-.15
		dec. pots	.13	.11	.62	.14	.06	.14	.07
		dec. sherds	.03	.01	-.20	.10	.66	-.15	.13
DETAILS OF CERAMIC DECORATION	TECHNIQUES	simple spatula	.53	.00	-.03	.09	.03	-.65	-.11
		m.dented spatula	.34	-.02	.25	-.01	-.05	.83	.10
	STRUCTURES	curvilinearity	-.06	-.90	.10	.08	-.02	.06	.07
		rectilinearity	-.01	.94	.08	-.03	.06	-.05	-.04
	FILLINGS OF DECN.	none	.29	.16	-.31	.02	.32	-.24	-.32
		discontinuous	.39	.31	.66	-.07	.25	.33	-.05
		continuous	-.67	.32	-.17	.05	-.28	-.08	.18
	FILLINGS	punctated	-.85	-.20	.13	-.05	.11	-.10	.04
		other types	.68	.37	.22	.06	-.15	.29	-.05
	AUXILIARY LINES	absent	.08	.00	-.06	-.01	-.01	.11	.83
		present	.01	-.02	.69	.28	.07	.10	-.39
	ELEMENTS OF DECORATION	lines	.25	-.10	.78	.26	-.10	-.08	-.06
		points	-.45	.18	.52	-.07	.05	-.13	.20
		stab-and-drag	.06	-.28	.15	.10	.10	.66	-.30
		hatchings	.68	-.15	.30	-.27	-.06	-.01	.30
		red ochre	-.17	-.18	.23	.82	.11	-.01	-.11
		charcoal	-.08	-.06	.11	.12	.25	-.11	.56
FLINT		arrowheads	.00	.03	.29	.28	.51	-.36	.27
	BLADES	blanks	-.08	.08	.12	-.05	.41	.17	.01
		worked	.38	.27	.05	.59	.38	-.15	.17
QUERNS		complete	-.08	-.09	.09	.70	.04	.07	-.05
		fragments	.18	-.05	-.01	.27	-.25	-.34	-.04
ADZES		type I	-.10	.21	-.22	.41	-.02	.46	.35
		type II	.19	.00	.02	.39	.21	-.01	.31
		type III	.04	-.03	.05	-.05	.71	.10	-.02
		types IV-VI	-.04	.03	-.01	.60	-.25	.06	.22

TABLE 11: Distribution of the graves at Elsloo on the first seven principal components; top scales: grouped distributions per .10 intervals of factor scores; bottom scales: grouped distributions for interpretation (cf. text).



= first three components: graves containing decorated ware only.
+ scores computed with relevant variables only, unrotated solution.
o scores computed with relevant variable only.
Ⓐ scores considered most significant (also cf. NOTE 3).

TABLE 12: (VARIMAX) transformation matrix: the cosines between unrotated and rotated principal components.

PRINC. COMP.	1	2	3	4	5	6	7
1	.64	-.14	.58	.11	.09	.47	-.02
2	-.31	-.14	.32	.67	.44	-.24	.29
3	.53	.54	-.47	.34	.22	-.11	.16
4	-.13	.64	.46	.19	-.36	-.19	-.41
5	-.11	.44	.31	-.54	.31	-.00	.56
6	-.22	.12	-.11	.30	-.54	.53	.52
7	-.36	.23	-.16	.04	.49	.63	-.39

PRIMARY PHASES	N. OF GRAVES	FINAL PHASING	N. OF GRAVES
1 (oldest)	11	I	11
2	13	II	13
3	5	III	14
4	2		
5 (youngest)	4		
4/5 (indirect)	3		

TABLE 13: Grouping of the graves at Elsloo into phases.

Primary phases: those suggested by distribution of graves on principal components nos. 1 and 6.

Final phasing: phases as compounded from the original, primary ones; identical with those in fig. 26.

PRINC. COMP.	SEX "MALE"	"FEMALE"	"AMBIGUOUS"
4	-	16	-
5	11	-	-
7	17	-	-
4 + 5	-	-	2
4 + 7	-	-	5
4 + 5 + 7	-	-	4
5 + 7	3	-	-
	21	16	11

TABLE 14: Distribution of graves on the significant parts of the principal components nos. 4, 5 and 7, which are supposedly sex-indicators.

Component no. 4: female.
Component nos. 5, 7: male.

TABLE 15: The distribution of the more frequent categories of grave gifts over the sexes, as determined initially by a principal components analysis.

CATEGORIES OF GRAVEGIFTS	ALLEGED SEX M	F	χ^2	CATEGORY INDICATES)°
undecorated pots	10	1	3.54	male
undecorated sherds	6	6	1.05	general
decorated pots	10	4	.37	general
decorated sherds	8	2	.38	general
red ochre (lumps)	3	11	10.74	female
charcoal (lumps)	12	5	.31	general
arrowheads	9	3	.61	general
blades	9	4	.16	general
querns	2	10	11.56	female
adzes III	12	-	6.70	male
adzes IV - VI	6	7	1.76	general
N. OF GRAVES	30	17		

)° on α = .05.

CATEGORIES OF GRAVE GIFTS	SEX M	SEX F	χ^2	P
undecd. pots	11	1	8.33	.004
undecd. sherds	4	5	.11	.75
decorated pots	4	6	.40	.60
decorated sherds	6	5	.09	.8
red ochre (lumps)	2	15	9.94	.002
charcoal (lumps)	5	8	.69	.45
arrowheads	6	2	2.00	.16
blades	6	6	.00	-
querns	0	13	13.00	<.001
adzes III	13	0	13.00	<.001
adzes IV - VI	3	6	1.00	.33
N. OF GRAVES	21	21		

TABLE 16: The distribution of the more frequent categories of grave gifts over the sexes, as from TABLE 15. It is assumed that female graves are indicated by querns or red ochre, and male graves by undecorated pots and adzes of type III.

CATEGORIES OF GRAVE GIFTS	SEX M	SEX F	χ^2	P
undecd. pots	11	1	6.74	.009
undecd. sherds	5	5	.08	.80
decorated pots	6	6	.09	.78
decorated sherds	7	5	.08	.80
red ochre (lumps)	2	15	12.42	<.001
charcoal (lumps)	6	8	.74	.40
arrowheads	10	1	5.93	.02
blades	7	6	.00	-
querns	-	14	16.67	<.001
adzes III	13	-	10.92	.001
adzes IV - VI	6	6	.09	.78
N. OF GRAVES	25	21		

TABLE 17: The distribution of the more frequent categories of grave gifts over the sexes, as in TABLE 16, and graves with arrowheads added to 'male' (also cf. TABLE 47).

NOTE: the arrowhead in grave no. 106 has been omitted (cf. p. 16).

TABLE 18: A listing of the graves at Elsloo according to sex, as from TABLE 17.

MALE GRAVES: (n = 25)	001, 003, 005, 021, 024, 025, 026, 031, 033, 038, 042, 051, 055, 056, 059, 067, 071, 072, 073, 074, 081, 092, 096, 100, 112.
PROBABLY MALE: (n = 13)	012, 013, 017, 020, 048, 063, 068, 085, 088, 094, 102, 107, 108.
FEMALE GRAVES: (n = 21)	011, 014, 016, 019, 022, 047, 050, 062, 064, 066, 083, 089, 093, 098, 099, 105, 106, 109, 110, 111, 113.
PROBABLY FEMALE: (n = 12)	006, 007, 027, 034, 039, 040, 054, 057, 070, 075, 082, 097.

"probable" refers to indirectly determined sex (cf. section v).
N.B.: grave 087 which is unsexable is grouped with "probably female" in the remainder of the text (also cf. p. w-23).
(grave numbers acc. to Modderman 1970 (I): 45-65).

N. OF KINDS	P.C.7	ALL
5	4	9
4	3	5
3	2	11
2	7	27
1	3	25
	19	77

TABLE 19: Number of kinds of grave gifts (cf. TABLE 24) per grave in the graves selected by principal component no. 7 and in all graves.

$\chi^2 = 5.79$ (p = .22: not significant).

	GRAVE GIFTS WITH	GRAVE GIFTS WITHOUT	
MALES	25	10ˣ	35
FEMALES	21	12ˣ	33
CHILDREN	31⁰	14ˣ	45
	77	36	113

TABLE 20: Hypothetical distribution of graves at Elsloo over adults and children, if graves marked by general categories of gifts only are assumed to be children's.

⁰ all graves with general gifts only.
ˣ estimated partition in analogy to the Nitra-necropolis (Pavúk 1972b: 74-75).

TABLE 21: Hypothetical distribution of graves at Elsloo over adults and children, if graves indirectly sexed are grouped with directly determined ones.

× estimated partition in analogy to the Nitra-necropolis (Pavúk 1972b: 74-75).

	GRAVE GIFTS WITH	GRAVE GIFTS WITHOUT	
MALES	31	10×	41
FEMALES	33	13×	46
CHILDREN	13×	13×	26
	77	36	113

TABLE 22: Distribution of graves scoring significantly on principal component no. 3, probably an indicator of "wealth".

(between parentheses): indirectly sexed graves added.

SCORES ON 3RD COMP.	SEX M	F	x	
VERY HIGH	4	1	1	6
HIGH	1(2)	1(2)	2(-)	4
MODERATE	1	2(3)	3(2)	6
	6(7)	4(6)	6(3)	16

TABLE 23: Distribution of graves ranked according to arbitrarily quantified labour input in grave gifts, and to kinds of grave goods as in TABLE 24 (also cf. TABLE 41).

"POINTS"	"KINDS" 5	4	3	2	1	0	
255-322	1	1	.	.	.	.	2
187-254	2	1	2	.	.	.	5
119-186	1	2	1	2	1	.	7
50-118	.	4	7	10	7	.	28
2-49	.	1	1	8	23	6	39
≤ 1	.	.	.	.	.	32	32
	4	9	11	20	31	38	113

avge. no. of points: 49.75
stand. devn. : 68.09

NO. OF KINDS	SEX M	F	x	
5	.	3(4)	1(.)	4
4	4	5	.	9
3	6	5	.	11
2	9(10)	7(9)	4(1)	20
1	6(12)	1(10)	24(9)	31
0	.(6)	.(1)	38(31)	38
	25(38)	21(34)	67(41)	113

TABLE 24: Frequencies of graves with specified numbers of grouped grave gift categories by sex.

(between parentheses): indirectly sexed graves added.

"kinds" (:grouped grave gift categories): ceramics, flint, querns/whetstones, adzes, red ochre —— entered on an absence-presence basis.

NO. OF ACTIVITIES	SEX M	F	x	
8	.	.	.	.
7	.	.(1)	1(.)	1
6	1	1	.	2
5	2	.	.	2
4	4	6	.	10
3	6	7	.	13
2	7(11)	5(9)	10(2)	22
1	5(10)	2(9)	24(12)	31
0	.(4)	.(1)	32(27)	32
	25(38)	21(34)	67(41)	113

TABLE 25: Ranking of graves from number of activities supposedly represented by grave gifts, by sex.

(between parentheses): indirectly sexed graves added.

TABLE 26: A listing of the graves at Elsloo ranked according to the number of activities represented by the grave gifts (also cf. FIG. 28).

RANKING (N. OF ACT.)	GRAVE NUMBERS)[0]	N
SUPER (6 or 7)	001, 083, 087.	3
HIGH (4, 5)	003, 005, 014, 056, 066, 067, 071, 098, 100, 105, 106, 109.	12
MEDIUM (2, 3)	007, 011, 018, 019, 020, 021, 022, 025, 031, 034, 038, 042, 047, 050, 055, 059, 062, 072, 073, 074, 075, 082, 085, 089, 090, 092, 093, 094, 096, 099, 102, 110, 111, 112, 113.	35
LOW (1)	006, 016, 017, 024, 026, 027, 029, 030, 033, 037, 040, 041, 045, 051, 054, 057, 063, 064, 065, 068, 069, 070, 078, 081, 084, 086, 088, 097, 101, 104, 107.	31
NONE (-)	002, 004, 008, 009, 010, 012, 013, 015, 023, 028, 032, 035, 036, 039, 043, 044, 046, 048, 049, 052, 053, 058, 060, 061, 076, 077, 079, 080, 091, 095, 103, 108.	32

)[0] grave numbers as in Modderman 1970 (I): 45-65.

16	9	9
23	9	9
18	10	8

N = 111

TABLE 27: Number of graves per field of FIG. 30.
(2 outlyer graves omitted)

TABLE 28: The calculation of the pattern of over- and under-representation: example (adzes types IV - VI distrib.).

observed:

2	.	1
2	.	2
1	5	2

–

expected:

2.2	1.2	1.2
3.1	1.2	1.2
2.4	1.4	1.1

=

pattern:

.	-1	.
-1	-1	+1
-1	+4	+1

TABLE 29: Summary of patterns of distribution of grave goods per category; 9-field trial (cf. TABLES 27 and 28 and FIG. 30).

	OBSERVED									PATTERN								
	1	2	3	4	5	6	7	8	9	1	2	3	4	5	6	7	8	9
categories:																		
undecd. pots	2	1	2	3	2	1	1	.	1	.	.	+1	.	+1	.	-1	-1	.
undecd. sherds	6	2	1	4	3	3	1	3	4	+2	.	-1	-2	+1	+1	-3	+1	+2
decorated pots	2	2	2	2	.	4	2	1	2	-1	.	+1	-2	-1	+3	.	.	+1
decorated sherds	6	2	2	1	4	1	1	2	1	+3	.	+1	-3	+2	-1	-2	.	-1
red ochre (lumps)	4	4	1	3	.	2	.	3	1	+1	+2	.	-1	-2	.	-3	+2	.
charcoal (lumps)	1	2	5	4	2	3	4	3	3	-3	.	+3	-2	.	+1	.	+1	+1
arrowheads	4	1	1	2	.	1	2	1	.	+2	.	.	-1	-1	.	.	.	-1
blades	2	3	2	2	1	1	1	2	1	.	+2	+1	-1	.	.	-1	+1	.
querns	2	3	2	1	1	3	.	1	1	.	+2	+1	-2	.	+2	-2	.	.
adzes III	3	.	.	2	3	1	3	.	1	+1	-1	-1	-1	+2	.	+1	-1	.
adzes IV - VI	1	5	2	2	.	2	2	.	1	-1	+4	+1	-1	-1	+1	.	-1	.

field nos.:

7	8	9
4	5	6
1	2	3

↑ N

TABLE 30: Correlation coefficients between trial fields, computed from the patterns of over- and under-representation of grave gift categories, as in TABLE 29.

FIELD NOS.								
1								
-.19	2							
-.64	.10	3						
-1.00	.21	-.34	4					
.32	-.42	-.03	-.28	5				
-.56	.10	-.06	-.20	-.29	6			
-.44	-.15	.13	.27	.00	.13	7		
.02	.08	.05	-.26	-.41	-.02	-.61	8	
-.39	-.22	-.14	-.15	-.03	.59	-.17	.31	9

$\bar{r} = .06$

field nos. :

7	8	9
4	5	6
1	2	3

TABLE 31: Two alternative groupings of the 9 trial fields compared.

	ALL		M(min)		M(max)		F(min)		F(max)	
GROUPING NO.1	44	26	11	7	14	10	4	4	10	7
	23	18	4	3	8	6	5	8	7	9
GROUPING NO.2	41	29	14	4	15	9	2	6	9	8
	23	18	4	3	8	6	5	8	7	9

Top row: N-field with W+NW; C with E+NE.
Bottom row: N-field with NE+E; C with W+NW.
The top-grouping is used in the remainder of the text (cf. FIGS. 31, 32).
(min.): directly sexed graves only.
(max.): indirectly sexed graves added.

TABLE 32: Mean distances to nearest neighbours in metres in the various sectors.

SECTORS	WITH GIFTS ONLY			ALL GRAVES		
	d	sd	n	d	sd	n
I	3.00	1.01	22	3.12	1.01	26
II	2.48	.94	30	2.44	.89	43
III	2.81	1.30	14	2.82	1.17	23
IV	3.16	1.30	16	3.39	1.40	18

d : average distance.
sd: standard deviation.
n : number of graves involved.
(for locations of the sectors see FIG. 32).

TABLE 33: Mean width of zones between sectors in metres.

SECTORS	BETWEEN ZONE LIMITS			ALL GRAVES		
	d	sd	n	d	sd	n
I - II	4.51	2.48	12	7.35	1.86	6
II - III	3.34	2.23	7	5.78	1.97	6
III - IV	10.32	1.83	5	13.80	.30	3
IV - I	4.71	1.85	14	10.38	2.66	5
CENTRE	14.25	.21	2	13.20	3.31	6

between zone limits: as drawn in FIG. 32.
between nearest graves: across boundary zones.
d, sd: as TABLE 32.
n: number of observations.

TABLE 34: Summary of patterns of distributions of grave gift categories; final, 4-sector subdivision (cf. TABLE 31 and FIG. 32).

	OBSERVED				PATTERN			
SECTOR NOS.	I	II	III	IV	I	II	III	IV
categories:								
undecd. pots	4	3	3	3	+1	-2	·	+1
undecd. sherds	10	8	6	3	+4	-2	·	-2
decorated pots	6	5	2	4	+2	-2	-2	+1
decorated sherds	6	3	7	4	+1	-5	+3	+1
red ochre (lumps)	3	4	6	5	-1	-3	+2	+2
charcoal (lumps)	8	9	3	7	+2	-2	-3	+3
arrowheads	1	4	5	2	-2	-1	+2	·
blades	3	5	2	5	·	-1	-1	+2
querns	5	1	3	5	+1	-5	·	+2
adzes III	5	5	3	·	+2	·	·	-2
adzes IV - VI	3	3	2	7	·	-3	-1	+4

$\chi^2 = 50.42$
$p = .01$

SECTOR NOS.				
I	r=.20	II I		
-.08	II	III IV		
-.43	-.32	III		
-.48	-.48	-.15	IV	

TABLE 35: Correlation coefficients between sectors, computed from the patterns of over- and under-representation of grave goods, as in TABLE 34.

TABLE 36: Obligations following from marital alliances at death: the exchange partner groups which had dealings with the group the deceased was living with (assuming matrilateral cross-cousin preferences).(cf. FIGS. 41 to 44).

		FEMALE DIES	MALE DIES
HARMONIC SYSTEMS	uxorilocal + matrilineal	0,-1	0,+1
	virilocal + patrilineal	0,-1	0,+1
DISHARMONIC SYSTEMS	uxorilocal + patrilineal	0,-1(-1,0,+1)	0,+1(0,+1,+2)
	virilocal + matrilineal	0,-1(0,-1,-2)	0,+1(+1,0,-1)

0: from residential groups
-: from groups down the exchange order
+: from groups up the exchange order
(0, -, +): relations of consanguinity and affinity.
(between parentheses): relations of descent.

Example: when a female dies in a matri-unilineal system, the groups with which she has had important relations were: own residential group (0 steps away), and group where her brother has gone (1 step down the exchange circuit).

TABLE 37: Distribution of the provenances of the gifts in the graves at Elsloo, as defined by the 'source' or 'importing' group from FIG. 38.

PROVENANCE OF GIFTS:		±2	-1	0	+1
IN MALE	min.	14(14)	12(13)	18(21)	12(14)
GRAVES	max.	22(22)	17(18)	25(28)	18(20)
IN FEMALE	min.	9(11)	5(11)	17(19)	11(19)
GRAVES	max.	18(20)	13(20)	27(30)	18(28)

Exchange sequence along which distances were maesured: I → II → III → IV (→ I); gifts originating to the left are labeled positively (: regular exchange direction), to the right negatively (counter to regular exchange direction).
(between parentheses): indirectly sexed graves added.
Also cf. TABLE 36.

	d_0	sd	d_e	R)+	n
ALL	2.78	1.12	2.91	.96	111)[0]
M-M	5.62	4.14	6.23	.90	25
F-F	5.97	2.78	6.82	.87	21
F-M	2.55	.50	-	-	10
NON-HOMO-GENEOUS PAIRS:	2.26	.60	6.23	.36	25

TABLE 38: Average distance in metres to nearest neighbours, differentiated to sexes.

d_0: observed average distance to nearest neighbour.
d_e: expected average distance when graves were randomly distributed.
$R = d_0 / d_e$:nearest neighbour coefficient)+
n = number of graves involved.

)[0] two outlying graves omitted.
)+ R-values: R = 0, all points at same place; R = 1.0, random dispersion; R = 2.15, hexagonal pattern.

STRUCTURES OF CERAMIC DECORATION	SEX M	F	x	
RECTILINEAR	4	7(3)	5	16
BOTH	5(1)	-	3	8
CURVILINEAR	2(1)	3(3)	5	10
	11(13)	10(16)	13(5)	34

TABLE 39: Structures of decoration on pots from graves at Elsloo, by sex.

(between parentheses): indirectly sexed graves.
$\chi^2_{M,F}$ = 5.98 (9.36) p = .05(.01).

STRUCTURES	M	F	x	
C or R	6(7)	10(16)	10(3)	26
C and R	5(6)	-	3(2)	8
	11(13)	10(16)	13(5)	34

TABLE 40: Single vs. double marked (by structures of ceramic decoration) graves at Elsloo, by sex.

C: curvilinear decoration;
R: rectilinear decoration.
χ^2 (M,F) = 5.97(9.31) p = .015(.003)
(between parentheses): indirectly sexed graves added.

N. OF FINISHED OBJECTS	SEX M	F	
≧ 5	3	2	5
4	4	3	7
2 or 3	1	6	7
	8	11	19

TABLE 41: Graves with more than modal contents. Differentiated to *number of finished objects* accompanying the deceased, by sex (specialized assemblages omitted).
(Also cf. TABLES 23, 25 and 42).

TABLE 42: A listing of the graves with more than modal contents.

≧ 5 FINISHED OBJECTS:	male graves	001, 003, 100.
	female graves	014, 087.
4 FINISHED OBJECTS:	male graves	005, 021, 056, 071.
	female graves	083, 105, 106.
2 or 3 FINISHED OBJECTS:	male graves	112.
	female graves	062, 066, 089, 098, 109, 111.

VALUE)⁰	SEX M	F	
≧ TREBLE	7	1	8
DOUBLE	5	1	6
	12	2	14

TABLE 43: Specialization of grave gifts, by sex, chiefly figures omitted.

)⁰ value: number of items in specialized category divided by number in other categories.

TABLE 44: A listing of the graves with allochthonous or foreign-looking artefacts.

GRAVE NO.	MARKERS ADZE I	STAB&DRAG	SEX	STATUS)[0]	REMARKS	AUTO-/ALLO-CHTHONE
066 ⸸	+	-	F	4 }	couple	allo
067	-	+	M	4 }		allo
073	+	-	M	3		allo
083 ⸸	+	+	F	6	specialist	dubious
085	+	-	m	2		allo
087 ⸸	+	-	f	7	chiefly	auto
089	-	+	F	2		allo
092	-	+	M	2		allo
096	-	+	M	3		allo
074 ⸸	-	+	M	2 }	2 sherds	dubious
101 ⸸	-	+	x	1 }	each only	dubious

⸸ graves discussed in text.
)[0] status: number of activities represented by gift set (cf. TABLE 26).

ORIGINS	M	F	x	
ALLOCHTHONOUS	4(5)	2	-	6(7)
DUBIOUS	1	1	1	3

TABLE 45: Summary of TABLE 44 with inferred origins of interred, by sex.

TABLE 46: Listing of paired graves for indirect sex-determination. "PAIRED": graves within average F-M distance plus one standard deviation (cf. TABLE 38).

MALE GRAVES	PARTNER GRAVES	SEX	FEMALE GRAVES	PARTNER GRAVES	SEX
003	006	x	011	012	x
005	007	x	014	013	x
021	022	F	016	017	x
026	027	x	019	020	x
033	034	x	022	021	M
038	039	x	047	048	x
042	040	x	050	051	M
051	050	F	062	068	x
055	054	x	064	063?	x
056	057	x	066	067	M
067	066	F	083	085	x
071	070?	x	089	088	x
074	075	x	093	094	x
081	082	x	098	102?	x
096	097	x	099	100	M
100	099	F	105	107?	x
112	113	F	110	108?	x
			111)°	112?	M
			113)°	112	M

)° : duogynous couple?
? : distance slightly larger than criterium.

TABLE 47: The distribution of the more frequent categories of grave gifts over the sexes; as TABLE 17, but with indirectly sexed graves added.

CATEGORIES OF GRAVE GIFTS	SEX M	SEX F	χ^2	p	LABEL
undecd. pots	11	1	7.75	.005	male
undecd. sherds	8	12	1.06	.32	gen.
decorated pots	7	9	.39	.60	gen.
decorated sherds	8	8	.08	.80	gen.
red ochre (lumps)	2	15	10.75	.001	female
charcoal (lumps)	11	9	1.00	.33	gen.
arrowheads	10	1	6.18	.02	male
blades	7	7	.01	.90	gen.
querns	-	14	16.12	<.001	female
adzes III	13	-	12.24	<.001	male
adzes IV - VI	7	6	.03	.85	gen.
no. of graves	38	33			

STRUCTURES	MOTIFS waves	spirals	
CURVILINEAR	3	6	9
CURVI + RECTIL.	6	.	6
RECTILINEAR	10	1	11
	19	7	26

TABLE 48: MAIN MOTIFS vs. STRUCTURES of ceramic decoration, per grave.

MOTIFS	M	F	x	
waves	7(7)	5(8)	7(4)	19
spirals	2(3)	2(3)	3(1)	7
	9(10)	7(11)	10(5)	26

TABLE 49: Motifs of ceramic decoration by sex.

(between parentheses): indirectly sexed graves added.

STATUS)°	SPECIALIZATION ARROWH.	ADZES	POTTERY	*GRAVES	N
SUPER	(2)	(3)	.	.	(3)
HIGH	5(1)	(1)	3	1	7(1)
MEDIUM	2	.	2	.	4
LOW	.	.	.	1	1
NONE	.	.	.	2	2

TABLE 50: Specialists' positions in lineage mode of production.

(between parentheses): chiefly figures.
)° from TABLE 26.
*GRAVES: graves oriented perpendicular to general NW-SE direction.
N: no. of graves involved.

STATUS)°	ALLOCHTHONES	ALL
super	-	3
high	1	12
medium	4	35
low	1	31
none	-	32

TABLE 51: Positions of alleged allochthones in lineage mode of production (see also TABLE 44).

)° from TABLE 26.

<table>
<tr><th colspan="3">ELSLOO</th><th colspan="3">HIENHEIM</th></tr>
<tr><th>Ch.3</th><th>Ch.5</th><th>n</th><th>Ch.3</th><th>Ch.5</th><th>n</th></tr>
<tr><td rowspan="2">6</td><td rowspan="2">5</td><td rowspan="2">8</td><td>7</td><td>7</td><td>5</td></tr>
<tr><td>6</td><td>6</td><td>26</td></tr>
<tr><td>5</td><td>4</td><td>9</td><td>5</td><td rowspan="3">5</td><td rowspan="3">17</td></tr>
<tr><td>4</td><td rowspan="2">3</td><td rowspan="2">16</td><td>4</td></tr>
<tr><td>3</td><td>3</td></tr>
<tr><td>2</td><td>2</td><td>7</td><td rowspan="2">2</td><td>4</td><td>20</td></tr>
<tr><td rowspan="3">1</td><td rowspan="3">1</td><td rowspan="3">12</td><td>3</td><td>40</td></tr>
<tr><td rowspan="2">1</td><td>2</td><td>41</td></tr>
<tr><td>1</td><td>30</td></tr>
<tr><td colspan="2">n of huts</td><td>52</td><td colspan="2">n of finds</td><td>179</td></tr>
</table>

TABLE 52: The phases of chapter 3 compared with those employed in chapter 5, plus the numbers of huts per phase (Elsloo) and the numbers of finds per phase (Hienheim).

vertical axis: chronological sequence approximately proportional to factor scores.

LENGTH, M.	LBK 1	LBK 2
< 7	2	-
7 - 9	11	11
9 - 11	15	22
11 - 13	6	9
13 - 15	2	6
> 15	5	3
n	41	51
avge. length	10.4	10.7

TABLE 53: Length of central parts of huts in metres, all Dutch LBK sites (after Modderman 1970(I): 104).

HUTS	STATUS
type 1a: 7	super : 3
type 1b: 14	high : 12
type 2 : 32	moderate: 35
type 3 : 10	low : 31
	none : 32

TABLE 54: A comparison of the numbers of huts per type at Elsloo during LBK 2, with nos. of graves per status level in the graveyard.

(no. of huts: non-datable and non-classifiable huts added by redistribution according to marginals).

TYPES	LBK 1	LBK 2
1a	13.7 (6)	15.0 (6)
1b	12.1 (12)	11.7 (9)
2 & 3	8.8 (23)	9.8 (36)

TABLE 55: Average lengths of central parts of huts in metres, per type and period, Dutch LBK.

() number of observations.
(from Modderman 1970 (I): 104).

SHERDS	TYPE 1a	ALL OTHER TYPES
PRESENT	3	34
ABSENT	3	17

TABLE 56: A comparison of frequencies of association with decorated sherds; for huts of type 1a, and all other types. Data from Elsloo.

HUTS		N OF SHERDS	
type	n	avge	range
1a	3	38	10-55
1b	11	35.2	6-181
2	16	26.1	4-92
3	7	31.0	3-50

TABLE 57: Numbers of huts per type vs. number of sherds; Elsloo.

TABLE 58: Elsloo village. Micro-phases or grouped use as indicated by chronological ordering of huts with decorated ware. See also FIGS. 46-50 and 51.

	CERAMIC PHASES									
	t_0	t_1	t_2	t_3	t_4	t_5	t_6	t_7	t_8	t_9
NE		x	x	x	x		x	?		
N	x x		x		x	x ?				
M		xx	x	x		x ?		x	x	
S		x								
E								x	x	x

TABLE 59: Average distances in metres between (the centres of the) huts per group and per phase, and between the centres of the various groups. Distances were calculated from schematic plans in FIGS. 46-51, and relate to huts with decorated sherds only.

GROUPS / PHASES	WITHIN DISTANCES NE	N	M	S	E	BETWEEN DISTANCES
1	-	34(6)	38(12)	20(2)	-	140(12)
2	-	32(3)	41 (5)	-	-	118 (9)
3	36(3)	27(6)	19 (3)	-	-	90 (9)
4	21(4)	-	37 (3)	-	34 (3)	115 (4)
5	-	-	50 (3)	-	42(15)	-

( ): number of observations.

TABLE 60: Frequencies of mixes for the variable STRUCTURES at Elsloo Village.

(1)	(2)	(3)
.0	.	3
.1	.	.
.2	2	4
.3	4	5
.4	11	12
.5	9	10
.6	7	8
.7	6	8
.8	2	3
.9	.	.
1.0	.	1

Col. (1): mix = (no. of curvilinear motifs)/(all motifs).
" (2): no. of huts with more than 10 decorated sherds
" (3): no. of huts with decorated sherds.

TABLE 61: The effects of sample length (top row) on the mix of a dichotomous variable: maximal and minimal proportions of any of the two attributes. (also cf. FIGS. 57 & 58).

SAMPLE LENGTH IN GENERATIONS		≦1	1	$1\frac{1}{4}$	$1\frac{1}{2}$	$1\frac{3}{4}$	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4	5	10
LIMIT VAL-	α	x	0	.2	.33	.43	.5	.6	.67	.7	.75	.8	.9
UES OF MIX	1-α	x	1	.8	.67	.57	.5	.4	.33	.3	.25	.2	.1

'x : indeterminable'.

TABLE 62: Elsloo Village. Hut types as distributed over group and occupation phases.

	GROUPS				
PHASES	NE	N	M	S	E
1		1b.2.x 2.3.x (3?)	2.2 1b.2	x.x	
2		1a.2.x 1b.2.x	1b.3		
3	1b 1b	1b.2.x.x 1b.2.3.x.x	3.x 1a.2	x	
4	2		2.2	1b.3.x	x 1b.2
5				2 2	1b.2.x 2.x.x

NO. OF HUTS OBSERVABLE/GROUP:	≥3	2	1
No. of groups with type 1	10	4	1
No. of groups without type 1	3	2	2

TABLE 63: The distribution of type 1a & 1b huts over the observed groups at Elsloo Village (cf. TABLE 62).

HUT TYPES	ELSLOO	HIENHEIM
1a	6	-
1b	13	1
2	28	15
3	10	5
x	35	16
	92	37

TABLE 64: A comparison of the numbers of huts per type at Elsloo and at Hienheim.

(from Modderman 1977: 124).

LENGTH (M)	DUTCH LBK	HIENHEIM
< 7	2	1
7 - 9	22	2
9 - 11	37	3
11 - 13	15	3
13 - 15	8	3
> 15	8	4
n. of huts	92	16
avge.length	10.6	12.4

TABLE 65: The lengths of the central parts of the huts; all Dutch LBK sites vs. Hienheim)°

Dutch LBK, from TABLE 53.
)° Hienheim, estimated from plans in Modderman 1977.

TABLE 66: A comparison of the sizes and numbers of huts per type at Elsloo and at Hienheim.

HUT TYPES	ELSLOO LBK 2			HIENHEIM		
	(1)	(2)	(3)	(1)	(2)	(3)
1a	7	15.0	6	3	14.8	3
1b	14	11.7	9	3	12.8	3
2	32	9.8	36	10	11.7	10
3	10			5		

(for Elsloo: conventional typology; for Hienheim: see text p.).
Col. (1): n. of huts of indicated type.
" (2): mean length in metres of central part.
" (3): n. of huts from which col.(2) has been calculated.

SHERDS	TYPE 1â	ALL OTHER TYPES
PRESENT	2	8
ABSENT	1	10

TABLE 67: Frequencies of association of hut type 1â with at least 5 decorated sherds, and of all other hut types.

HUTS		SHERDS	
type	n	avge.	range
1â	2	188.5	19-358
1b̂	3	48.7	5-82
2̂	3	48.0	25-79
3	2	111.5	72-151

TABLE 68: Hienheim. No. of huts per type vs. no. of sherds. (cf. TABLE 57).

TABLE 69: Hienheim. The mixes of the more important variables of pottery decoration per find (pit complexes omitted).

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
I- 2	528	1.0	.7	..C	.6	1.0			
- 8	728	.3	.9	Ab.	.2	.7			
-11	413	.6	.8	abC	.2	.7			
-12	666	-	1.	-	.0	-			
-17	522	1.	.7	abC	.4	.7			
-18	529	.8	.7	aBc	.4	.7			
-22	550	.3	.9	aBc	.4	.5			
-23	27	.3	.8	Ab.	.6	.6			
-24	275	1.	1.	-	.2	-			
-26	557	.0	1.	aBc	.2	.3			
II- 2	242	-	1.	.B.	.7	-			
4	195	-	1.	A..	.7	-			
5	438	-	1.	-	.8	-			
6	8	1.	1.	AB.	.5	-			
11	401	.5	.9	aBc	.3	.4			
12	319	.0	.9	.Bc	.9	.3			
13	182	.2	.9	Abc	.5	.3			
18	543	.8	.9	aB.	.3	.4			
19	381	.0	.9	aB.	.3	.4			
24	196	.1	.9	Abc	.4	.3			
27	243	-	.8	.B.	.3	-			
28	817	-	.8	-	.7	-			
29	305	.0	1.	aB	.4	.2			
32	507	-	.9	Ab.	1.	.2			
34	542	.4	.8	A..	.4	-			
38	183	.6	.8	ABc	.6	.3			
40	333	1.	.8	.BC	.0	-			

(contd.)

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
III- 1	555	.3	.8	ABc	.1	.3	-		
- 4	359	.2	.7	A..	.1	.4	-		
- 7	729	-	.6	Abc	.3	.5	-		
- 8	414	.1	.9	aBc	.2	.2	-		
-17	549	-	.7	-	.1	-	-		
-19	322	1.	1.	Abc	.2	.0	-		
-21	548	.2	.8	.B.	.3	1.	-		
-22	749	1.	.6	A.C	.4	.3	-		
-23	703	1.	.6	.B.	.7	-	-		
-26	721	.4	.8	aBC	.3	.2	+		
-27	68	1.	.7	Ab.	.3	.3	.		
-28	292	1.	.6	aBc	.6	.3	.		
-30	307	.7	.8	aBc	.2	.2	+		
-31	324	1.	.8	.Bc	.2	.2	.		
-40	620	.8	.8	aBc	.5	.4	.		
IV- 1	431	1.	.6	a.C	.7	-	.		-
- 2	166	1.	.6	Ab.	.2	.3	.		-
- 6	743	-	.5	A..	.4	-	.		-
-12	365	.5	.6	Abc	.1	.2	-		-/1
-13	370	1.	.5	.B.	.2	-	.		1/.
-14	526	1.	.7	abC	.3	.3	+		./4
-18	302	-	.2	.B.	.3	-	.		-
-19	313	.0	.2	A..	.2	-	.		-
-20	736	1.	.2	A..	.0	-	.		-
V- 1	364	.7	.8	ABC	.5	.5	+	.	./8
- 3	714	1.	.8	AB.	.2	-	.	.	7/.
- 4	360	-	.6	.B.	.1	.0	.	.	4/.
- 5	648	-	.4	A..	.0	.3	.	.	1/5
-13	546	-	.3	.B.	.0	.0	+	.	./8
-15	551	1.	.2	.BC	.0	-	+	.	2/7
-17	418	-	.3	.B.	.0	-	+	+	./5
VI- 2	398	1.	.3	aB.	.2	.0	+	.	1/9
3	421	1.	.1	aBc	.0	.1	+	+	1/6
5	174	1.	.1	.Bc	.4	.0	+	.	1/7
8	771	1.	.0	.Bc	.0	-	+	.	1/9
12	145	1.	.1	.B.	.2	.0	+	.	1/9
16	10	1.	.0	.Bc	.1	.0	+	.	1/8

(contd.)

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(VI)-17	540	1.	.0	.B.	.2	-	+	.	./10
-20	302	1.	.0	.B.	.0	-	+	.	1/9
-21	781	1.	.0	-	.0	-	+	.	1/9
-23	422	1.	.0	aBc	.1	.0	+	+	3/5
-24	362	-	.0	.B.	.0	-	+	.	10/.
-26	108	1.	.0	.Bc	.3	.0	+	.	1/9

Col. (1): rank nos.; refer to FIGS. 60 to 65.
" (2): find nos.
" (3): mix (MAIN MOTIFS); no. of waves proportional to all MAIN MOTIFS in find.
" (4): mix (STRUCTURES); no. of curvilinear motifs proportional to all STRUCTURES.
" (5): mix (NUMERICITY); a=simplex, b=duplex, c=triplex; CAPITAL letter indicates preponderance in find.
" (6): mix (AUX. LINES); no. of motifs with AUX. LINES proportional to all motifs.
" (7): mix (FORMAT); no. of pots without neck decoration proportional to minimal number of pots in find.
" (8): multidented spatula: +, presence in find.
" (9): goat foot tool: +, presence in find.
" (10): proportions of hatches/stab-and-drag COMPONENTS in find.

-: indicates missing data; . or .0, absence.

PHASES	BUILDING PLOTS					N.OF PITS	
	NW	W	SW	SE	E	o	x
4	o	x	.	o	.	2	1
5	o	x	o	ox	.	5	4
6	ox	xo	ox	ox	ox	11	11

TABLE 70: Hienheim: the adoption of hatching and stab-and-drag over space and time.

0: stab-and-drag.
x: hatchings.

PHASES	OCCURRENCE	
	PURE	MIXED
4	3	.
5	5	2
6	2	10

TABLE 71: Hienheim: initial exclusivity of hatching and stab-and-drag COMPONENTS becoming inclusive occurrence per pit.

MIXES / PHASES	MAIN MOTIFS <	.5	>	STRUCTURES <	.5	>
1	4	.	5	.	.	10
2	5	1	4	.	.	17
3	5	.	8	.	.	15
4	1	1	5	3	2	4
5	.	.	3	4	.	3
6	.	.	11	12	.	.

TABLE 72: Hienheim. Summed and grouped distributions of mixes on two variables over time.

main motifs: waves to all MOTIFS.
structures : curvilinear to all STRUCTURES.

MIXES	STRUCT.	M. MOTIFS
.9-1.	4	7
.7-.8	7	2
.4-.6	2	1
.2-.3	-	1
.0-.1	1	2
	14	13

TABLE 73: Hienheim. Summed distributions of the mixes of two variables of ceramic decoration as occurring in hut associated pits.

HUT TYPES	BUILDING PLOTS NW	W	SW	SE	E	
1â	.	1	1	1	.	3
$1\hat{b}$	.	.	.	1	2	3
$\hat{2}$	1	.	4	3	2	10
3	1	.	1	1	2	5
x	4	3	.	5	4	16
	6	4	6	11	10	37

TABLE 74: Hienheim. Hut types vs. building plots.

TABLE 75: Hienheim - coefficients of correlation between major dimensions of ceramic decoration.

single dented spatula																	
multiple dented spatula	-99																
"goat foot tool"	-44	36															
finger/nail	05	-18	-07														
simplex	29	-29	-16	09													
duplex	03	-03	08	-11	-54												
triplex	-06	04	06	12	-26	-46											
presence of neck decn.	-54	54	34	-08	21	05	01										
absence of neck decn.	54	-54	-34	08	-21	-05	-01	-100									
lines	73	-73	-38	12	25	05	-06	-48	48								
points	62	-62	-26	05	20	02	09	-43	43		29						
hatchings	-38	36	42	-09	-06	00	-06	33	-33		-42	-39					
stab-and-drag points	-78	80	26	15	-28	-04	-01	47	-47		-70	-74	09				
finger/nail imp.	09	-16	-08	56	03	-02	04	-12	12		12	-01	-09	-17			
curvilinearity	85	-86	-42	21	21	05	01	-51	51		74	60	-43	-76	15		
rectilin.	-85	86	42	-21	-21	-05	-01	51	-51		-74	-60	43	76	-15	-100	
	-38	39	20	-17	-11	-20	18	21	-21		-40	-27	21	39	-09	-48	48
	38	-39	-20	17	11	20	-18	-21	21		40	27	-21	-39	09	48	-48
	36	-35	-22	03	18	-04	-00	-35	35		27	49	-26	-42	05	44	-44
	-36	35	22	-03	-18	04	00	35	-35		-27	-49	26	42	-05	-44	44
	03	06	22	02	-13	21	-01	12	-12		-12	-01	05	05	08	00	-00
	33	-30	-38	01	13	-15	01	-30	30		42	16	-44	-17	-03	40	-40
	-54	51	37	04	-21	-03	12	31	-31		-42	-42	59	30	01	-47	47
	40	-40	-25	01	28	-09	-08	-02	02		43	35	-21	-41	02	43	-43
	74	-74	-50	16	26	03	-09	-59	59		68	70	-44	-69	16	78	-78
	-30	28	42	-18	-12	24	-12	25	-25		-34	-30	68	09	-14	-39	39
	-80	81	38	-14	-37	-07	18	48	-48		-68	-69	11	85	-13	-75	75
	07	-10	-05	37	17	-14	-00	-08	08		07	10	-08	-10	23	08	-08
	TECHNIQUES (G)				NUMERICITY (G)			ZONING (G)			COMPONENTS (B)					STRUCTURES	

(contd.)

TABLE 75 (cont.)

												wave
-100												spiral
-15	15											aux. lines
15	-15	-100										no aux. lines
-05	05	-18	18									indeterminate
-18	18	35	-35	-53								parallel
23	-23	-31	31	-07	-59							others
-40	40	34	-34	-04	28	-27						lines
-27	27	35	-35	-10	41	-51		02				points
21	-21	-21	21	31	-49	33		-23	-46			hatchings
39	-39	-39	39	-07	-23	44		-34	-75	-04		stab-and-drag points
-09	09	08	-08	-06	07	-10		-02	-06	-10	-09	finger/nail impressions
MOTIFS (B)		AUX.LIN. (B)		DIRECTION FILLINGS				COMPONENTS (N)				

n = 179 finds (general variables); n = 164 (belly variables); n = 89 (neck variables).

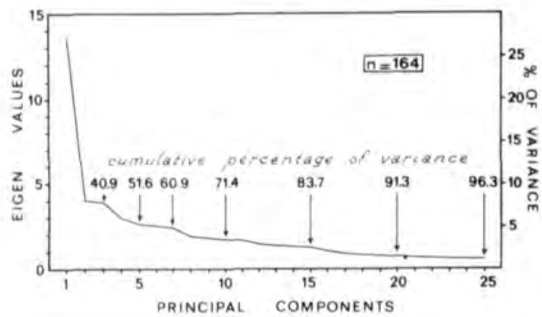


TABLE 76: Hienheim. Eigen-values in principal components analysis of ceramic decoration, analyzed on 52 attributes or 17 variables.

TABLE 77: Hienheim. Matrix of factor loadings (unrotated) for major dimensions of ceramic decoration.

	PC 1	PC 2	PC 3
single-dented spatula	-.896	.023	.099
multi-dented spatula	.896	-.036	-.141
"goat foot tool"	.516	.091	.285
fingers/nails	-.196	-.016	.043
simplex	-.328	.352	-.097
duplex	-.041	-.441	.391
triplex	.069	.089	-.122
presence of neck decn.	.673	-.151	.045
absence of neck decn.	-.673	.151	-.045
lines	-.796	.040	-.010
points	-.720	-.099	.056
hatchings	.487	.118	.488
stab-and-drag points	.820	-.019	-.274
finger/nail impressions	-.180	.058	.034
curvilinearity	-.912	-.024	-.016
rectilinearity	.912	.024	.016
wave	.527	.367	-.126
spiral	-.528	-.367	.126
auxiliary lines	-.504	.089	-.287
no auxiliary lines	.504	-.088	.287
indeterminate	.094	-.118	.458
parallel	-.483	.130	-.566
others	.602	.031	.262
lines	-.398	.028	-.053
points	-.850	-.156	-.072
hatchings	.396	.173	.702
stab-and-drag points	.831	-.009	-.365
finger/nail impressions	-.108	.170	.024

no. of finds: 164.
no. of attributes: 52
or 17 variables

TABLE 78: Hienheim. Size and chronology of find numbers.

(1) find no.
(2) no. of sherds (belly only)
(3) rank phase
(4) rank, within phase
(5) rank, cumulative
(6) remarks

(4) / (5): X possibly unreliable as find belongs to pit complex.

(1)	(2)	(3)	(4)	(5)	(6)
0008	5	2	6	36	
0010	119	6	16	164	0010+0011+0228
0011	-	-	-	-	in 0010
0027	53	1	23	23	
0068	82	3	27	98	C-14: 5910±50
0108	16	6	24	172	C-14: 5780±50
0145	58	6	12	160	
0158	-	-	-	-	in 0275
0162	-	-	-	-	in 0263
0166	28	4	2	113	
0174	40	6	5	153	
0181	44	6	22	170	
0182	36	2	13	43	
0183	151	2	38	68	
0195	7	2	4	34	
0196	24	2	24	54	
0200	36	3	16	87	
0218	-	-	-	-	in 0232
0220	-	-	-	-	in 0245
0221	-	-	-	-	in 0267
0222	7	5	14	145	
0226	-	-	-	-	in 0265
0227A	11	6	9	157	
0227B	11	3	6	77	
0228	-	-	-	-	in 0010
0232	13	5	10	141	0218+0232
0242	5	2	2	32	
0243	9	2	27	57	
0245	26	6	7	155	0220+0245
0247	-	-	-	-	in 0275
0251	-	-	-	-	in 0275
0262	-	-	-	-	in 0275
0263	108	5	9	140	0162+0263+0234
0264	8	6	13	161	
0265	20	3	23	104	0226+0265
0266	-	-	-	-	in 0267
0267	53	3	36	107	0221+0266+0267
0275	18	1	24	24	0158+0247+0251 +0262+0275
0292	16	3	28	99	
0295	16	6	11	159	0295+0296+ +0297+0298
0296	-	-	-	-	in 0295
0297	-	-	-	-	in 0295
0298	-	-	-	-	in 0295
0299	27	6	14	162	
0302A	6	6	20	168	
0302B	5	4	18	129	
0305	15	2	29	59	
0307	97	3	30	101	
0313	11	4	19	130	
0316	5	5	7	138	
0319	78	2	12	42	
0322	11	3	19	90	
0324	31	3	31	102	
0325	182	3	9	80	
0333	5	2	40	70	
0342	5	3	35	106	
0343	21	3	13	84	
0344	40	3	5	76	
0351	10	3	32	103	
0359	30	3	4	75	
0360	22	5	4	135	
0362	8	6	24	172	
0364	10	5	1	132	

(contd.)

(1)	(2)	(3)	(4)	(5)	(6)
0365	50	4	12	123	
0370	15	4	13	124	
0380	10	3	12	83	
0381	12	2	19	49	
0386	-	-	-	-	in 0396
0387	-	-	-	-	in 0396
0391	-	-	-	-	in 0396
0396	82	6	27	175	0386+0387+0391 +0396+0397
0397	-	-	-	-	in 0396
0398	6	6	2	150	
0401	92	2	11	41	
0413	80	1	11	11	
0414	37	3	8	79	C-14: 6125±35
0418	5	5	17	148	
0421	39	6	3	151	
0422	33	6	23	171	
0431	6	4	1	112	
0438	6	2	5	35	
0446	9	5	6	137	
0476	8	2	25	55	
0477	14	4	16	127	
0489	115	4	5	116	
0504	17	1	6 X	6 X	
0506A	6	6	18 X	166 X	
0506B	29	1	10 X	10 X	
0507	9	2	32	62	
0522	14	1	17	17	
0526	59	4	14	125	
0528	25	1	2	2	
0529	82	1	18	18	
0530A	7	6	25 X	173 X	
0530B	83	4	9 X	120 X	
0531	5	2	36 X	66 X	
0532	10	6	1 X	149 X	
0533	8	6	10 X	158 X	
0534	6	3	2 X	73 X	
0540	5	6	17	165	
0542	5	2	34	64	
0543	40	2	18	48	
0545	-	-	-	-	in 0555
0546	7	5	13	144	
0547	-	-	-	-	in 0555
0548	30	3	21	92	
0549	14	3	17	88	
0550	56	1	22	22	
0551	6	5	15	146	
0555	27	3	1	72	0545+0547+0555
0557	5	1	26	26	
0576	14	3	25 X	96 X	
0586	36	3	18 X	89 X	
0587	15	2	26 X	56 X	
0592	9	3	38 X	109 X	
0593A	6	6	29 X	177 X	
0593B	44	2	31 X	61 X	
0594	18	3	37 X	108 X	
0595	13	2	10 X	40 X	
0596	17	4	4 X	115 X	
0597	27	1	13 X	13 X	
0598	22	2	21 X	51 X	
0599	33	1	15 X	15 X	
0600	15	3	15 X	86 X	
0601	7	2	8 X	38 X	
0602	14	2	30 X	60 X	
0603	31	2	22 X	52 X	
0606	8	2	17 X	47 X	
0608	63	1	19 X	19 X	
0609	7	1	27 X	27 X	
0610	18	1	28 X	28 X	
0611	9	5	2 X	133 X	
0613	10	2	7 X	37 X	
0614	7	4	17 X	128 X	
0615	9	4	10 X	121 X	
0616	7	1	30 X	30 X	
0617	53	2	9 X	39 X	
0618	17	3	11 X	82 X	
0619	15	2	20 X	50 X	

(contd.)

(1)	(2)	(3)	(4)	(5)	(6)
0620	72	3	40	111	C-14: 6000
0648	7	5	5	136	
0666	5	1	12	12	
0697	9	5	8 X	139 X	
0698	6	2	23 X	53 X	
0701	74	1	21 X	21 X	
0703	15	3	23	94	
0714	5	5	3	134	
0720	6	3	14 X	85 X	
0721	125	3	26	97	
0728	30	1	8	8	
0729	10	3	7	78	
0736	5	4	20	131	
0743	5	4	6	117	
0749	19	3	22	93	C-14: 6155±45
0758	53	3	29 X	100 X	
0759	13	4	15 X	126 X	
0760	10	2	41 X	71 X	
0761	9	1	25 X	25 X	
0762	32	1	4 X	4 X	
0763	15	3	10 X	81 X	
0764	17	2	1 X	31 X	
0765	24	4	11 X	122 X	
0767	15	2	33 X	63 X	
0771	9	6	8	156	
0781	5	6	21	169	
0794	6	5	16 X	147 X	
0797	5	6	15 X	163 X	
0807	8	6	19 X	167 X	
0817	13	2	28	58	
0821	36	3	3 X	74 X	
0823	5	5	11 X	142 X	
0866	6	3	34	105	
0868	21	6	6	154	0868+0870+ +0874+0875
0870	-	-	-	-	in 0868
0874	-	-	-	-	in 0868
0875	-	-	-	-	in 0868
0876	-	-	-	-	in 1000

(1)	(2)	(3)	(4)	(5)	(6)
0880	-	-	-	-	in 1000
0881	-	-	-	-	in 1000
0887	-	-	-	-	in 1000
0888	-	-	-	-	in 1000
0889	-	-	-	-	in 1000
0895	-	-	-	-	in 1000
0911	40	4	3	114	
0919	155	2	14	44	0919+0921+ +0952+0964
0921	-	-	-	-	in 0919
0937	-	-	-	-	in 1000
0943	-	-	-	-	in 1000
0947	-	-	-	-	in 1000
0952	-	-	-	-	in 0919
0969	-	-	-	-	in 0919
0976	-	-	-	-	in 1000
1000	74	6	28	176	0876+0880+0881+ +0887+0888+0889 +0895+0937+0943 +0947+0976/1011
1011	-	-	-	-	in 1000
1044	77	4	8	119	1044+1045+1046
1045	-	-	-	-	in 1044
1046	-	-	-	-	in 1044
1082	-	-	-	-	in 1101
1083	-	-	-	-	in 1101
1085	-	-	-	-	in 1101
1089	109	1	16	16	
1099	-	-	-	-	in 1101
1101	99	1	20	20	1082+1083+1085+ +1099+1101+1134
1115	76	6	4	152	C-14: 5905±45
1116	13	6	30	178	
1134	-	-	-	-	in 1101
1140	27	3	20	91	
1143A	20	5	12	143	
1143B	86	1	29	29	
1152	-	-	-	-	in 1188
1187	-	-	-	-	in 1188
1188	25	6	31	179	1152+1187+1188

(contd.)

(1)	(2)	(3)	(4)	(5)	(6)
1387	91	2	17 X	47 X	
1392	24	3	39 X	110 X	
1393	5	2	35 X	65 X	
1395	14	2	16 X	46 X	
1396	78	3	24 X	95 X	
1397	83	1	5 X	5 X	C-14: 6220±45
1398	16	1	14 X	14 X	
1399	9	1	1 X	1 X	
1400	26	1	7 X	7 X	
1401	33	1	3 X	3 X	
1402	28	1	9	9	
1403	23	2	39	69	
1404	5	2	3	33	
1405	9	2	15	45	
1407	-	-	-	-	in 1420
1420	11	4	7 X	118 X	1407+1420

conversion of absolute numbers to 10-logarithms.

A logarithm consists of two numbers: the first one (here to be entered in the first column) describing the general magnitutde, and the other (here to be entered in the 2nd and 3rd columns) the precision. Thus:

counts between	1 and	9 (incl.)	enter	0	in the first column.
"	10	99	"	1	"
"	100	999	"	2	"
"	1000	9999	"	3	"
et cetera.					

Similarly, the precision is indicated by:

counts of 1,	10,	100,	1000	enter	00	in 2nd and 3rd column.
	12,	120,	1200		08	"
	14,	140,	1400		15	"
	16,	160,	1600		20	"
	18,	180,	1800		26	"
2,	20,	200,	2000		30	"
	23,	230,	2300		36	"
	27,	270,	2700		43	"
3,	30,	300,	3000		48	"
	35,	350,	3500		54	"
4,	40,	400,	4000		60	"
5,	50,	500,	5000		70	"
6,	60,	600,	6000		78	"
7,	70,	700,	7000		84	"
8,	80,	800,	8000		90	"
9	90,	900,	9000		95	"
10	100,	1000,	10000		00	"

If the counts fall in between table values, the number to be entered can be found through interpolation.

CODEBOOK FOR THE ELSLOO CEMETERY

GROUP	VARIABLE	attrib. no.	REMARKS AND DESCRIPTION	col.nos.
general	grave no.	01	grave number (ref.Modderman 1970(1))	01-02-03
	position	02	S/N, from plan	04-05-06
		03	W/E, from plan	07-08-09
	type of intermittent	04	empty grave-like pit: 0 only gifts in gravepit: 1 corporal remains, no gifts: 2 corp. remains with gifts: 3 cremation, no gifts: 4 cremation with gifts: 5 calcined bone, no gifts: 6 calcined bone, with gifts: 7	10
	orientation	05	azimuth of head: S=00;SW=01;W=02; NW=03;N=04;NE=05;E=06;SE=07. When no remains are visible for axis of pit: add 50. inapplicable, indeterminable = 99	11-12
	posture of the corpse	06	on the back: 0; left: 1; right: 2; inapplicable: 9; unknown: blank	13
ceramics general	nature	07	undecorated pots: number	14
		08	undecorated sherds: number	15
		09	decorated pots: number	16
		10	decorated sherds: number	17-18
decor. ware)°	techniques	11	unidented spatula, no. of motifs	19
		12	multidented spatula, "	20
	components (belly)	13	10-log (lines); if absent: 99	21-22
		14	10-log (points) "	23-24
		15	10-log (stab-and-drag) "	25-26
		16	10-log (hatchings) "	27-28
	components (neck)	17	10-log (lines) "	29-30
		18	10-log (points) "	31-32

C O D E B O O K   F O R   T H E   E L S L O O   C E M E T E R Y

		19	10-log (stab-and-drag); if absent: 99	33-34
		20	10-log (hatchings) "	35-36
	structures	21	curvilinearity, no. of motifs in belly zone	37
		22	rectilinearity "	38
	motifs	23	wave motif "	39
		24	spiral motif "	40
	fillings	25	empty bands "	41
		26	discontinuously filled bands "	42
		27	continuously filled bands "	43
	direction of fill.	28	indeterminate, no. of motifs in belly zone	44
		29	parallel to axis "	45
		30	oblique to axis "	46
		31	perpendicular "	47
	delim. of motif bands	32	no limits to bands "	48
		33	simple limits "	49
		34	fringes "	50
	secondary motifs	35	"Zwickelfiguren" "	51
		36	other types (not if attr. 40-42), no. of motifs	52
	motif ends	37	none (rotated, translated motifs)	53
		38	unelaborated, no. of motifs in belly zone	54
		39	elaborated "	55
	auxiliary lines	40	cadres "	56
		41	symmetry axes "	57
		42	no aux. lines "	58
	numericity	43	simplex decoration "	59
		44	duplex "	60
		45	triplex "	61
		46	quadruplex "	62
red ochre	hematite	47	lumps, number	63
		48	traces, number	64
	bog-ore	49	traces, number	65

C O D E B O O K   F O R   T H E   E L S L O O   C E M E T E R Y

flint	arrow	50	retouch on one side only, no. of points	66
	heads	51	retouch on both sides ,,	67
	scrapers	52	all types, number	68
	blades	53	blanks, number	69
		54	utilized blades, number	70
	other	55	nuclei	71
		56	debris	72
querns	querns	57	querns and quern fragments,no. of pieces	73
		58	whet stone and fragments ,,	74
		59	lumps of abrasive stone, unworked ,,	75
adzes	adzes	60	adzes type I, no. of pieces in grave	76
		61	adzes type II, ,,	77
		62	adzes type III, ,,	78
		63	adzes type, IV, V, VI ,,	79
charcoal	charcoal	64	charcoal not found: 0 present close to interment: 1 present in gravepit fillings: 2	80

N.B. (1): When a 'GROUP' does not occur in a grave, do leave blanks.
N.B. (2): What is described as occurring in the "grave pit filling" should not be entered.
)° refer to Chapter 1.

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A; see also B: at entry 'B' a related subject is found.

def = definition
N = note
T = table