# 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*.

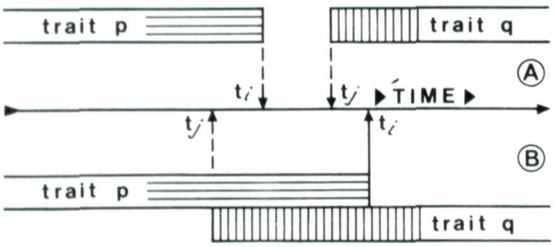



*Fig. 8.* Continuity and discontinuity on a two-trait variable.
A discontinuity: $(t_j\text{-}t_i) \geqslant 0$.
B continuity: $(t_j\text{-}t_i) < 0$
$t_i$: latest appearance of trait p.
$t_j$: earliest appearance of trait q.
$(t_j\text{-}t_i)$: adoptive period.

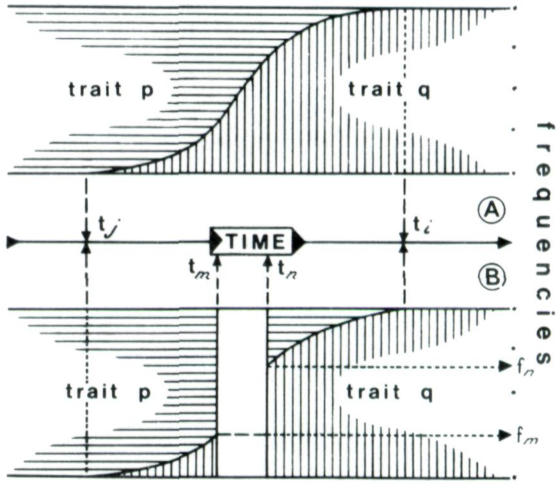



*Fig. 9.* Continuity and discontinuity on a two-trait variable, quantified.
A continuity or: for all t: $\frac{df}{dt} = \varphi_p\,(100\text{-}p)$.
B discontinuity or: for $t_m < t < t_n$: $\frac{df}{dt}$ indeterminate.
$t_j$: latest appearance of trait p.
$t_j$: earliest appearance of trait q.
$(t_j\text{-}t_i)$: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

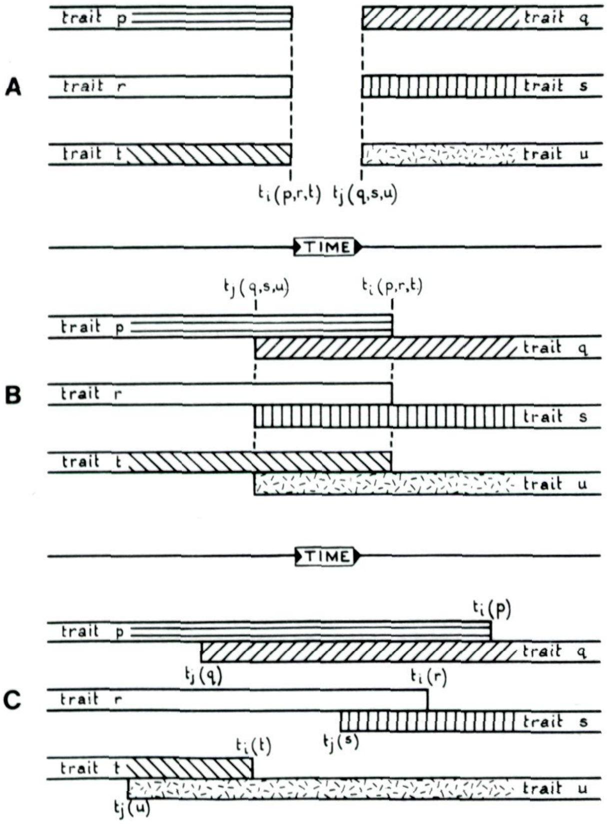



*Fig. 10.* Continuity and discontinuity on three two-trait variables, qualitative representation.

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

$t_i$: 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-

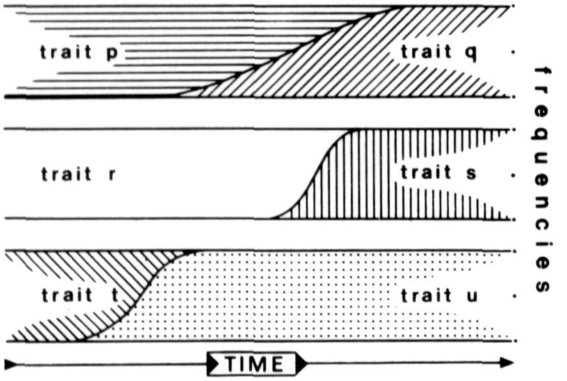



*Fig. 11.* Continuity on three two-trait variables, quantified. ($t_j$-$t_i$), the adoptive periods, differ per variable.
$t_i(p) \neq t_i(r) \neq t_i(t)$;
$t_j(q) \neq t_j(s) \neq t_j(u)$.

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

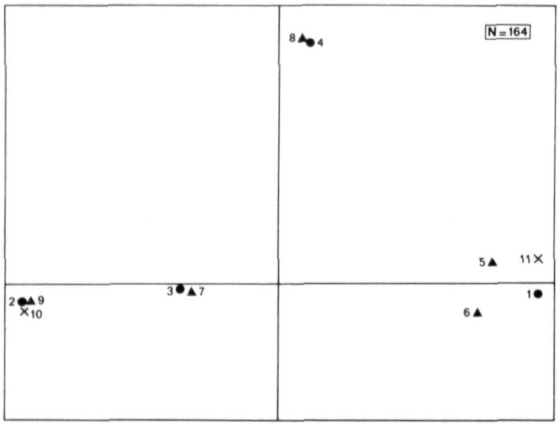



*Fig. 12.* Plot of the "loadings" of 11 traits on the first two (QUARTIMAX rotated) principal components. Horizontally: first principal component; vertically: second principal component.
● TECHNIQUES: single dented spatula; 2: multiple dented; 3: 'goat foot tool', 4: fingertips and nails.
▲ ELEMENTS: 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:

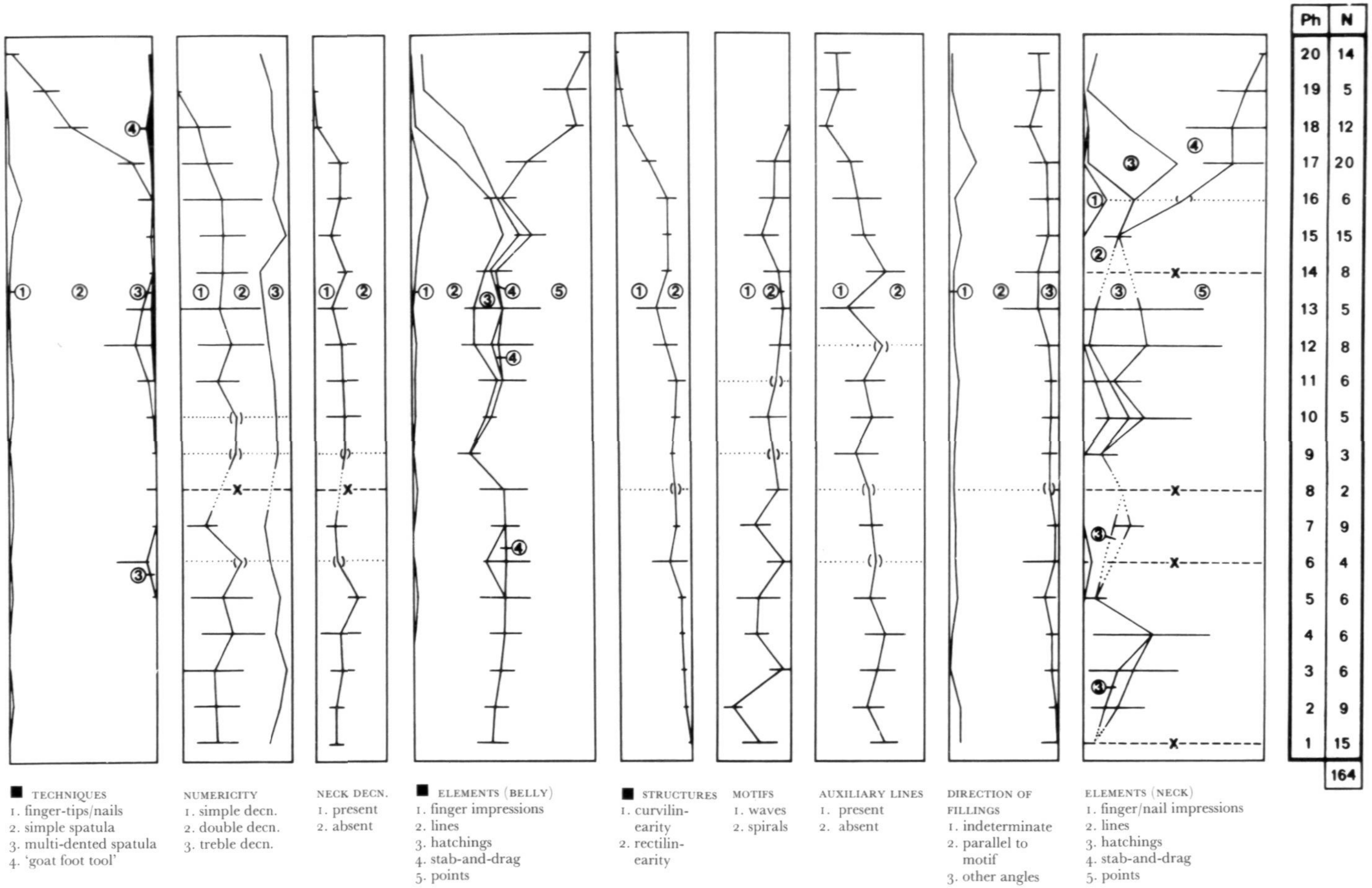


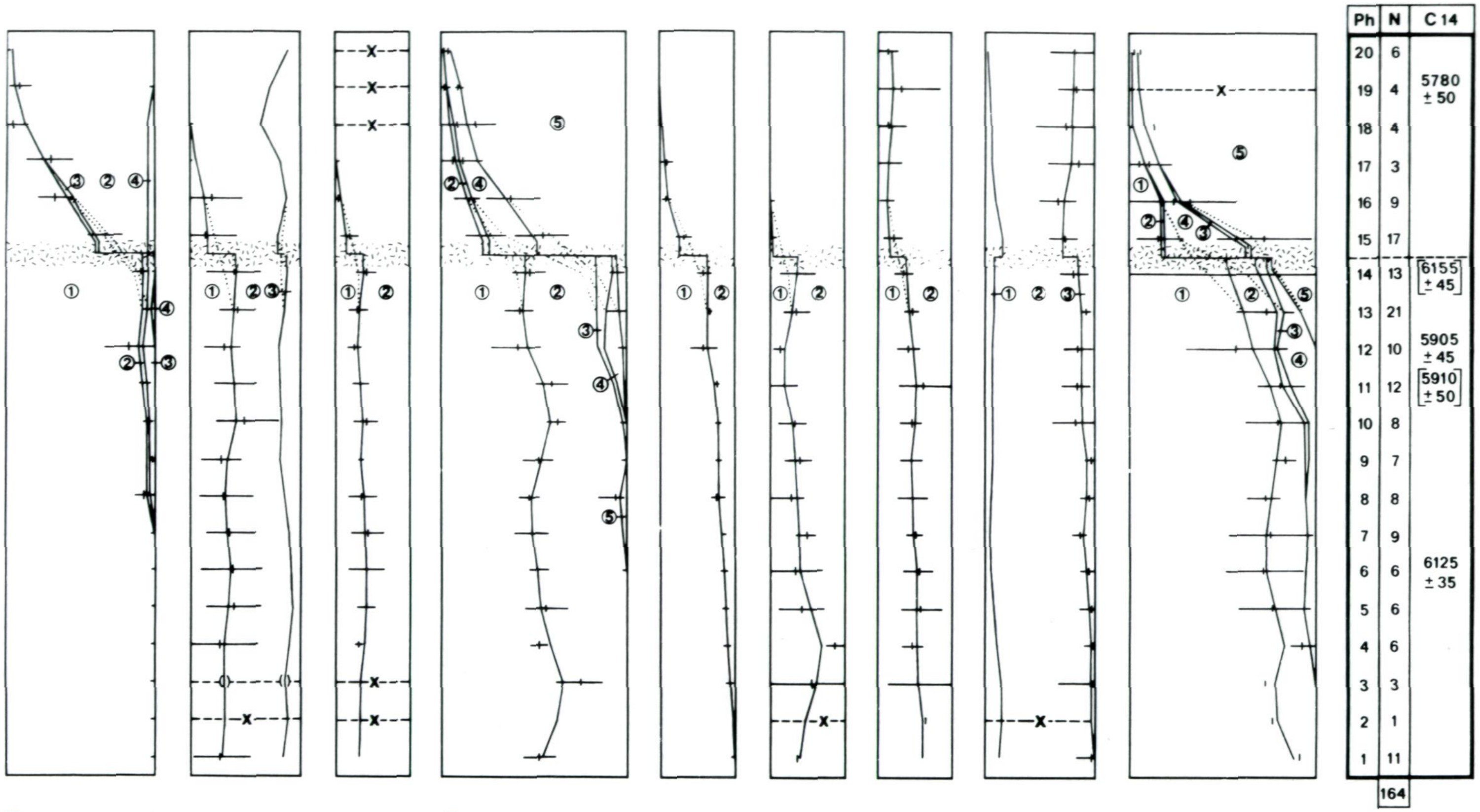

*Fig. 13.* HIENHEIM: Proportions of various attributes per variable of decoration over time. Phases comprise approximately equal numbers of sherds (MODEL I), ordered chronologically by means of a principal components analysis of the variables marked by ■: '1', the oldest phase, '20' the youngest one.

'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.




■ TECHNIQUES
1: simple spatula
2: multi dented spatula
3: finger tips/nails
4: 'goat foot tool'

NUMERICITY
1: simple decoration
2: double decoration
3: treble decoration

NECK DECORN.
1: absent
2: present

■ ELEMENTS (BELLY)
1: points
2: lines
3: finger impressions
4: hatchings
5: stab-and-drag

■ STRUCTURES
1: curvi-linearity
2: recti-linearity

MAIN MOTIFS
1: spirals
2: waves

AUXILIARY LINES
1: present
2: absent

DIRECTION OF FILLINGS
1: indetermi-nate
2: parallel to motif
3: other possibilities

ELEMENTS (NECK)
1: points
2: lines
3: finger impressions
4: hatchings
5: slab-and-drag

*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

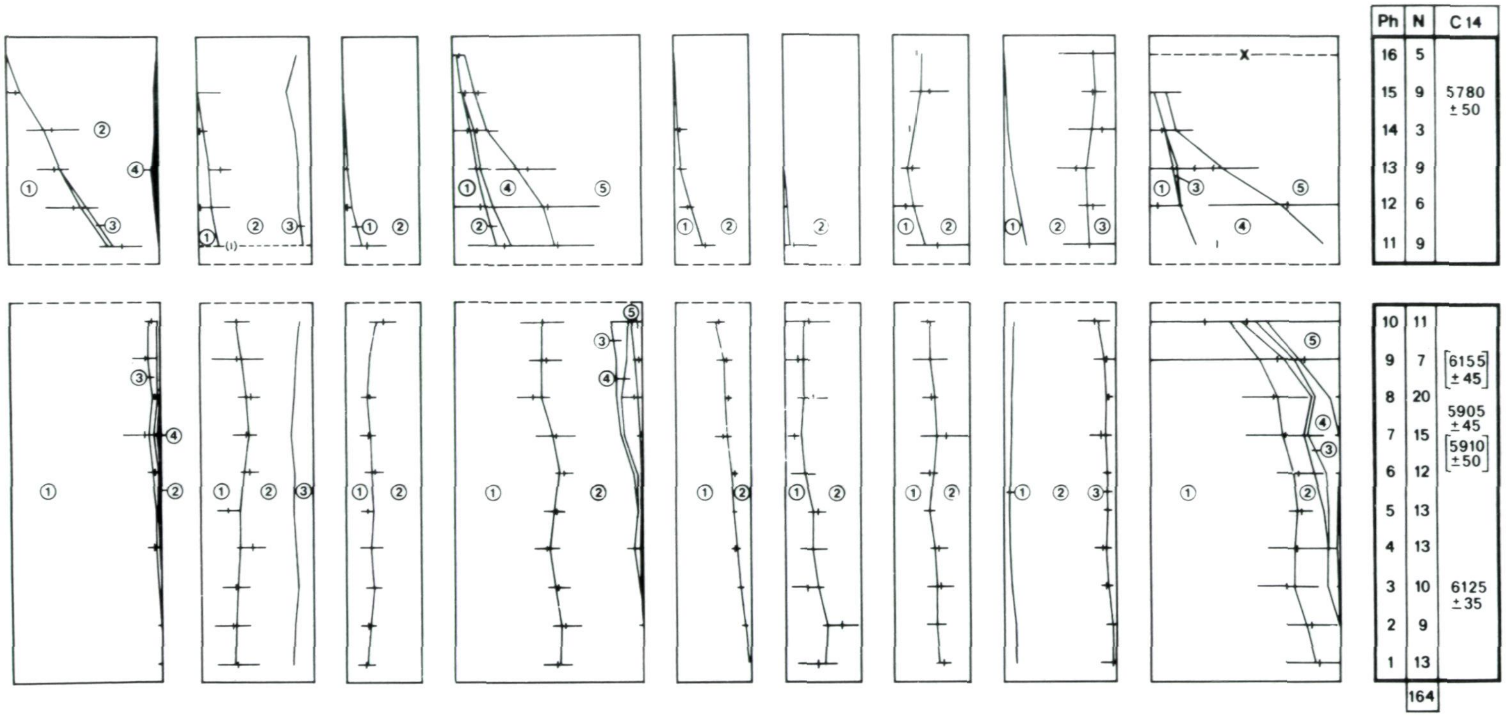



*Fig. 15.* HIENHEIM: counts of traits per variable per phase, when the number of sherds is distributed normally both before and after the presumed discontinuity. Otherwise, similar to presentation of MODEL II.

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

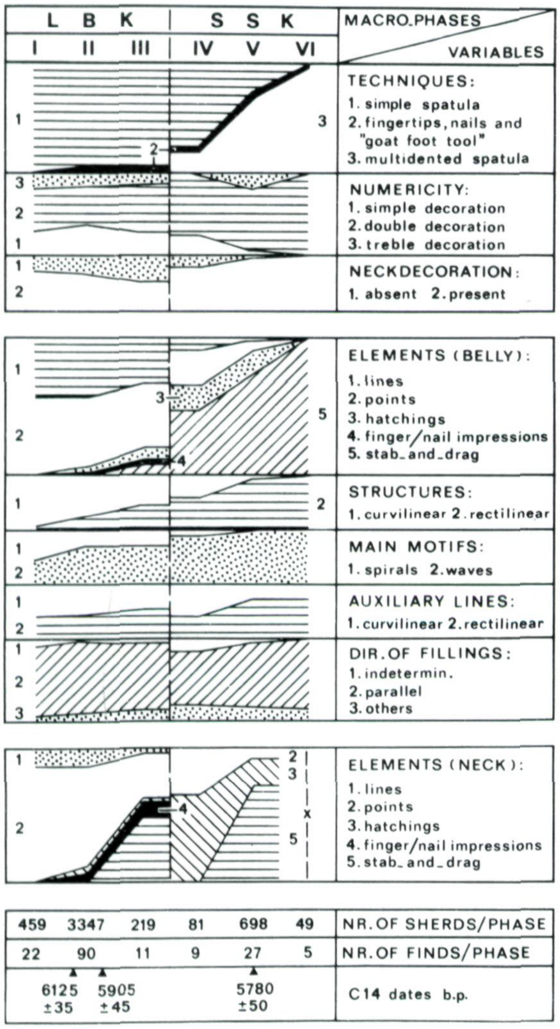



*Fig. 16.* HIENHEIM: summary of MODEL III, condensed to six macro-phases as follows from TABLES 5 & 6 (i.e., according to MODEL III-A).

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

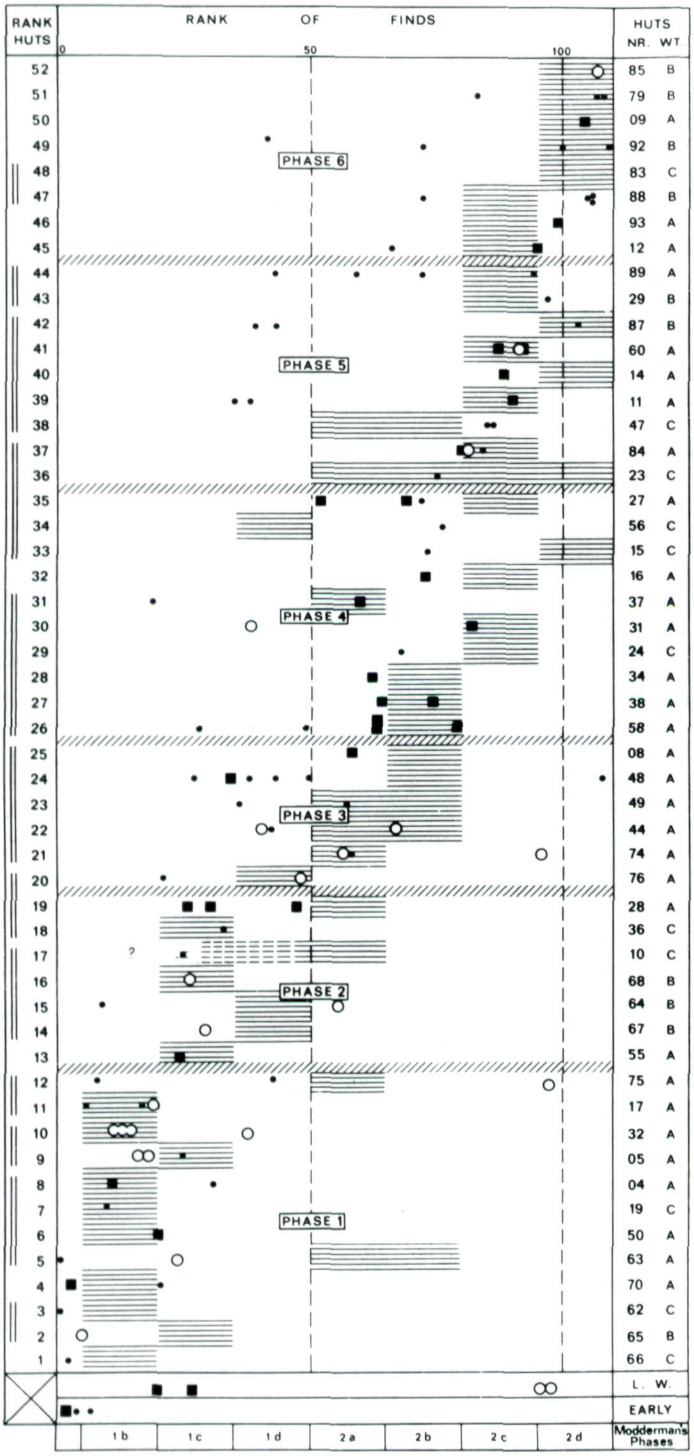



*Fig. 17.* ELSLOO VILLAGE.
A comparison of the pca derived chronologies of huts (vertical scales) and individual finds associated with the huts (top scale) with Modderman's datings.
● C finds, huts with ≤9 sherds.
○ B finds, huts with 10-19 sherds.
■ A finds, huts with ≥20 sherds.
PHASE 1-6: phases suggested 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

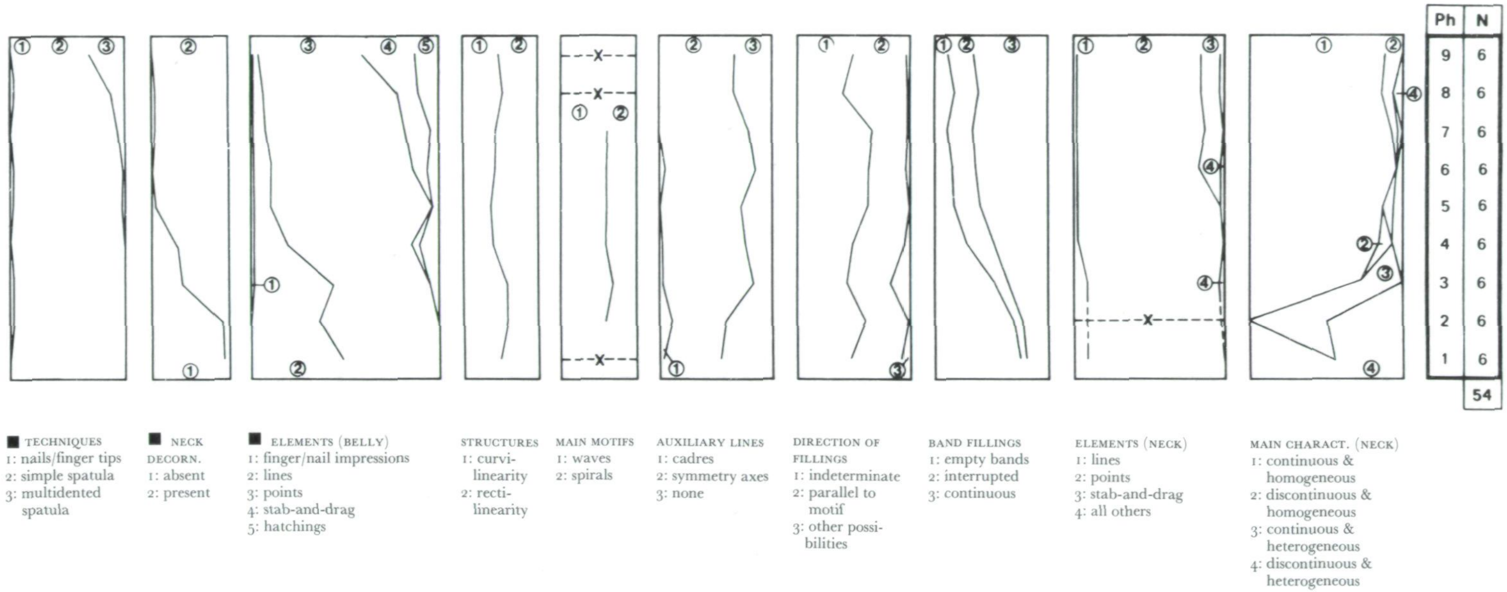
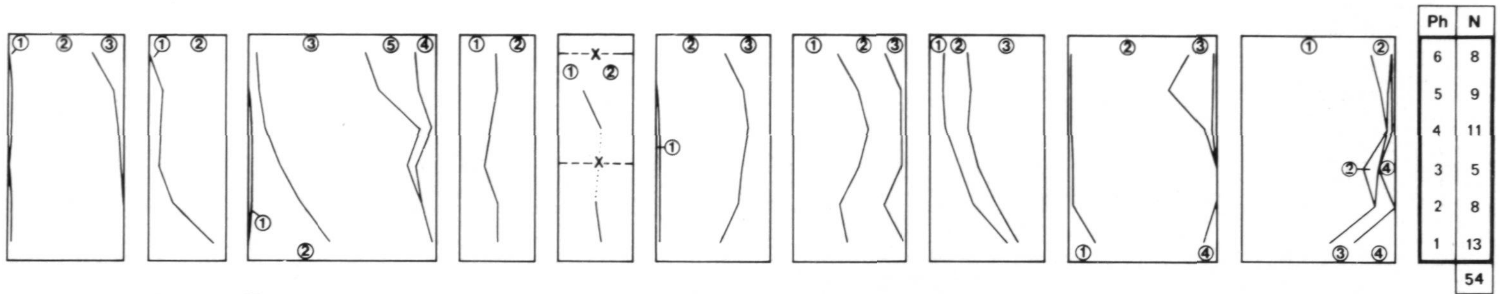



*Fig. 18.* ELSLOO VILLAGE: Proportions of various attributes per variable of decoration over time. Phases comprise equal numbers of houses, ordered chronologically by means of a principal components analysis of the variables marked with ■.

top: youngest phase; bottom: oldest phase.

N: number of huts comprised in phase.

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




■ TECHNIQUES
1: nails/finger tips
2: simple spatula
3: multidented spatula

■ NECK DECORN.
1: absent
2: present

■ ELEMENTS (BELLY)
1: finger/nail impressions
2: lines
3: points
4: hatchings
5: stab-and-drag

STRUCTURES
1: curvi-linearity
2: recti-linearity

MAIN MOTIFS
1: waves
2: spirals

AUXILIARY LINES
1: cadres
2: symmetry axes
3: none

DIRECTION OF FILLINGS
1: indeterminate
2: parallel to motif
3: other possibilities

BAND FILLINGS
1: empty bands
2: interrupted
3: continuous

ELEMENTS (NECK)
1: lines
2: points
3: hatchings
4: stab-and-drag

MAIN CHARACT. (NECK)
1: continuous & homogeneous
2: discontinuous & homogeneous
3: continuous & heterogeneous
4: discontinuous & heterogeneous

*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).

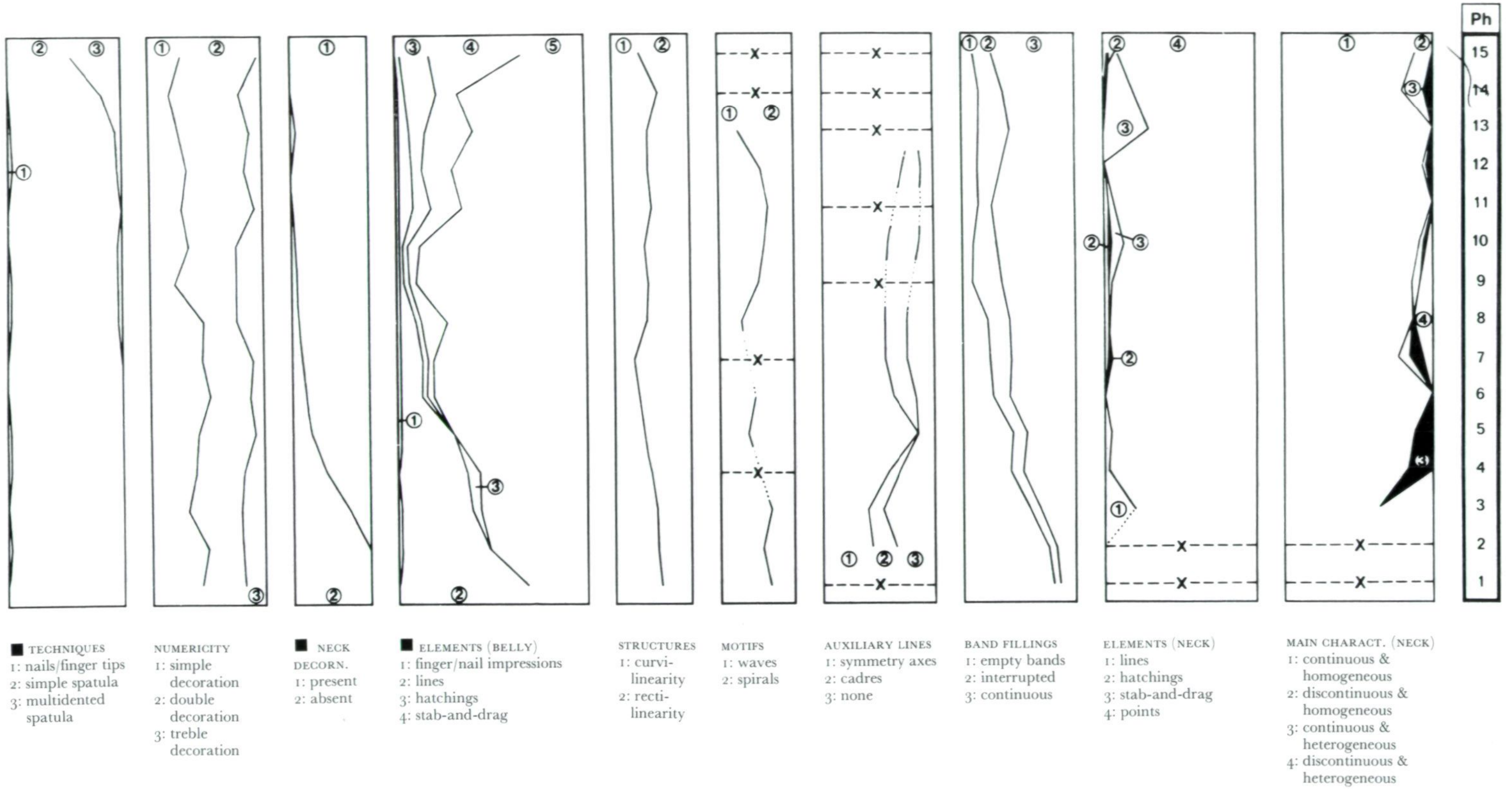



*Fig. 20.* ELSLOO VILLAGE. Proportions of various attributes per variable of pottery decoration over time. Phases comprise equal numbers of finds, arranged chronologically by means of a principal components analysis of the variables marked with ■.

top: youngest phase; bottom: oldest phase.

Data for finds, ten finds to the phase.

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

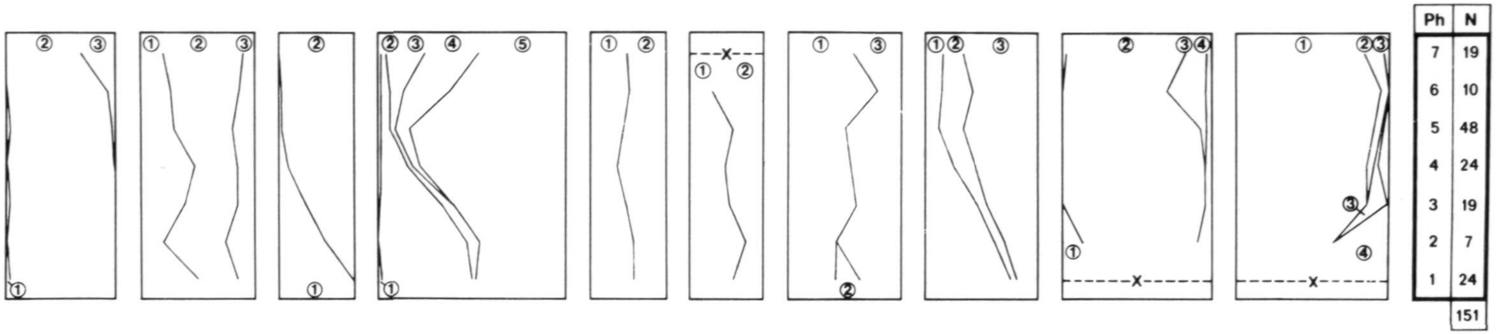



*Fig. 21.* ELSLOO VILLAGE: Proportions of various attributes per variable of pottery decoration over time. Phases derived from a principal components analysis of the variables marked with ■, and according the clustering on the time scale or component.

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:

$$s = ax + by + cz + \ldots.$$

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 re-do 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 I 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):

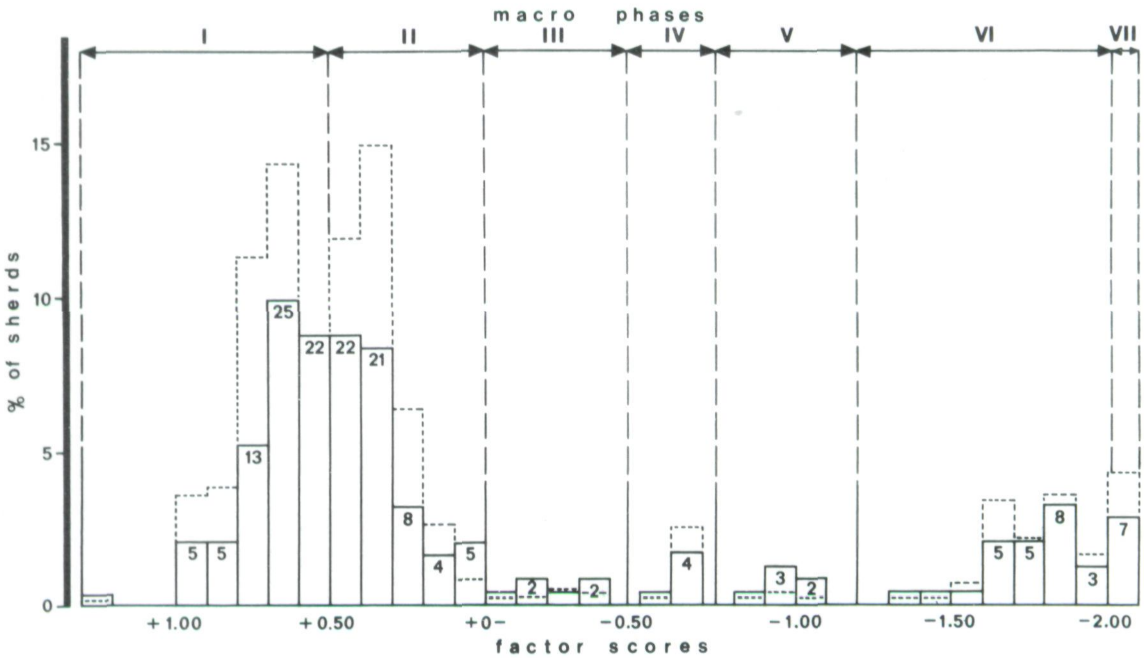


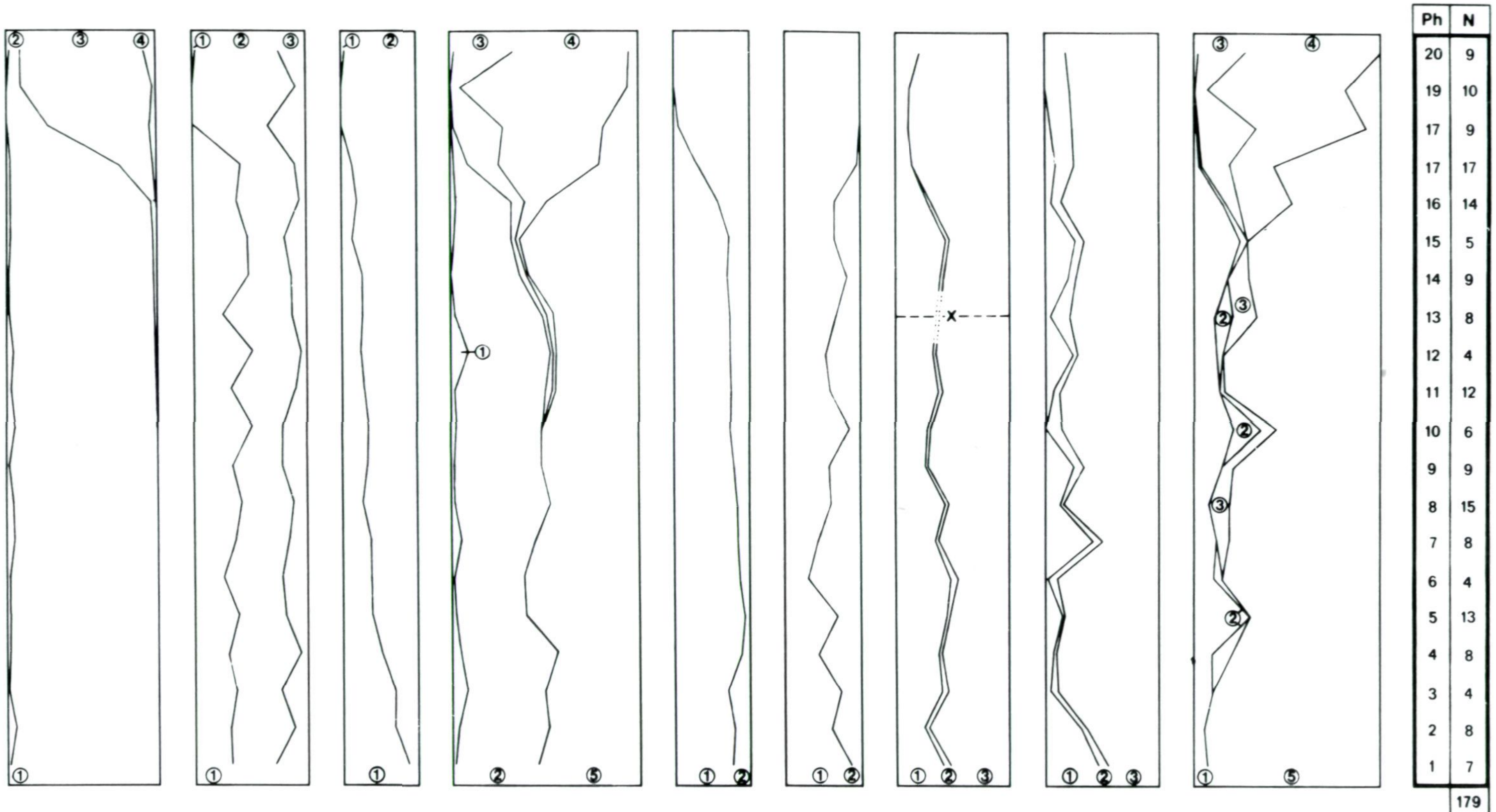

*Fig. 22.* HIENHEIM: distribution of finds and sherds (stippled) along chronological axis in (grouped) factor scores. 'Older' to the left, 'younger' to the right.
No. of finds indicated at the top of the full-drawn bars.
No. of sherds in percentages on scale to the left.
Division into macro-phases indicated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




■ TECHNIQUES
1: nails/finger tips
2: simple spatula
3: multi-dented spatula
4: 'goat foot tool'

NUMERICITY
1: simple decoration
2: double decoration
3: treble decoration

■ NECK DECORN.
1: absent
2: present

■ COMPONENTS (BELLY)
1: finger/nail impressions
2: lines
3: hatchings
4: stab-and-drag points
5: points.

■ STRUCTURES
1: curvi-linearity
2: recti-linearity

MOTIFS
1: waves
2: spirals

AUXILIARY LINES
1: symmetry axes
2: cadres
3: none

BAND FILLINGS
1: empty bands
2: interrupted
3: continuous

ELEMENTS (NECK)
1: lines
2: finger/nail impressions
3: hatchings
4: stab-and-drag
5: points

*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).

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

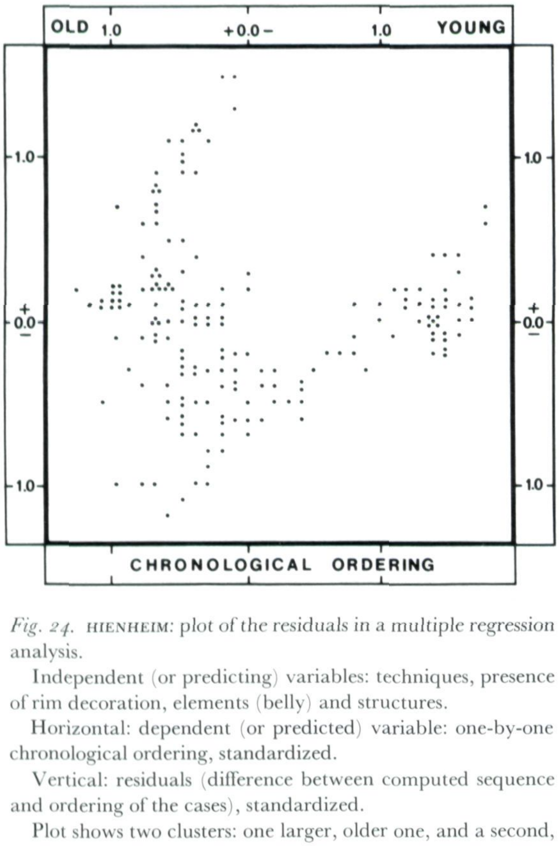



*Fig. 24.* HIENHEIM: plot of the residuals in a multiple regression analysis.

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.

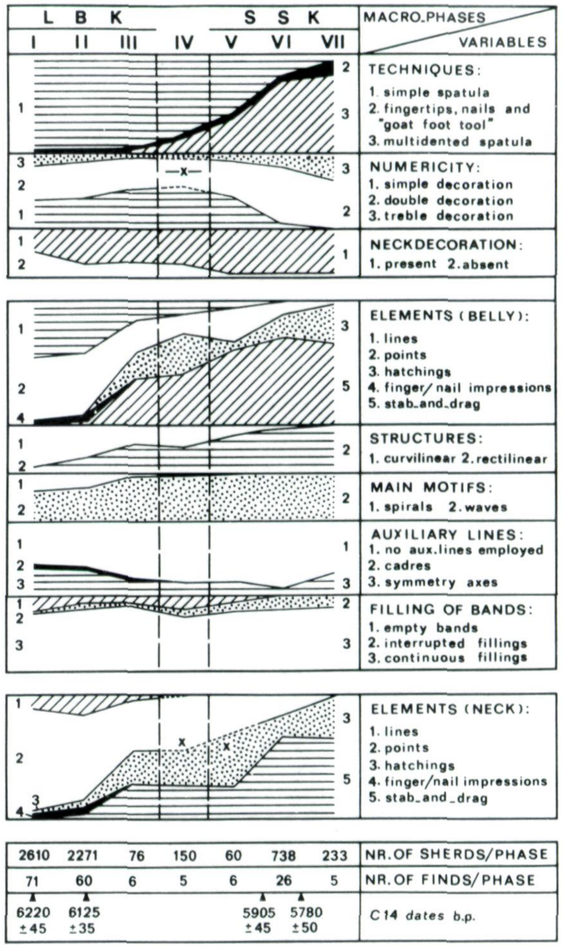



*Fig. 25.* HIENHEIM: proportions of attributes of ceramic decoration per MACRO-PHASE, as 'defined' by discontinuities in the factor scores of the finds. Therefore, the discontinuity derived in the text of Ch. III has not been emphasized; it coincides with the dotted line separating off PHASES IV/III; 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.