Wahle, E. 1941. *Zur ethnischen Deutung frühgeschichtlicher Kulturprovinzen. Grenzen der frühgeschichtlichen Erkenntnis I.* Sitzungsberichte der Heidelberger Akademie der Wissenschaften, Philologisch–historische Klasse. Jahrgang 1940–1, 2nd edn. Heidelberg: Winter.

Wahle, E. 1950–1951. Geschichte der prähistorischen Forschung. *Anthropos* **45**, 497–538; **46**, 49–112.

Wenskus, R. 1961. *Stammesbildung und Verfassung. Das Werden der frühmittelalterlichen gentes.* Cologne and Graz: Böhlau.

Wenskus, R. 1979. Randbemerkungen zum Verhältnis von Historie und Archäologie, insbesondere mittelalterlicher Geschichte und Mittelalterarchäologie. In *Geschichtswissenschaft und Archäologie*, H. Jankuhn & R. Wenskus (eds). Forschungen und Vorträge 22, 637–57. Sigmaringen: Thorbecke.

Werner, J. 1950. Zur Entstehung der Reihengräberzivilisation. Ein Beitrag zur Methode der frühgeschichtlichen Archäologie. *Archaeologica Geographica* **1**(2), 23–32.

Wernhardt, K. 1979. 'Ethnosnotiz' – Bemerkungen zu einem Zentralbegriff der anthropologischen Disziplinen. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **109**, 173–9.

Winkler, E.-M. 1983. Volk, Kultur, Ethnos, Population, Typus. Zur Methodik der 'ethnischen Deutung'. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **113**, 5–14.

Wissler, C. 1917. *The American Indian. An introduction to the anthropology of the New World.* New York.

Wissler, C. 1923. *Man and culture.* New York.

Wolfram, S. 1986. *Zur Theoriediskussion in der prähistorischen Archäologie Großbritanniens. Ein forschungsgeschichtlicher überblick über die Jahre 1968–1982.* BAR International Series 306. Oxford: BAR.

ARCHAEOLOGICAL APPROACHES TO CULTURAL IDENTITY

Edited by Stephen Shennan

Department of Archaeology, University of Southampton

1989

1 Ethnic concepts in German prehistory: a case study on the relationship between cultural identity and archaeological objectivity

ULRICH VEIT

(translated by Stephen Shennan)

It is almost fashionable to be derogatory about Kossinna's theories, but his methods were perhaps not as bad as the way he himself misused them.

(McWhite 1956, p. 7)

Historians of the development of anthropology seem to be united in the belief that the past decade has seen a major change in the whole scene. Just as there has been a trend in society as a whole towards a new traditionalism, so the science of man has largely said goodbye to modernism, and consequently to a belief in the progress of civilization and to the idea of a single world society (Friedman 1988). Thus, it is not surprising that in the field of prehistoric archaeology too the self-styled guardians of modernism are on the retreat.

In the 1960s the liberation of archaeology from the fetters of culture history was proclaimed under the banner of a 'New Archaeology'. Today we can perceive an opposite trend. History has a future once more. It is no longer the pursuit of cultural universals that is at stake. It is the variety and specificity of cultural developments on which people's efforts are focused. In the context of systems theoretical approaches the term 'culture' was at times reduced to the level of an extrasomatic means of adaptation to the natural environment. However, now there is a renewed interest in 'cultures' in the plural and not merely 'culture' in general. One result of this development is that 'cultural identity' is increasingly becoming a key term for the self-definition of a 'post-processual' archaeology. Questions about problems such as ethnicity and multiculturalism, which people long thought they could avoid by regarding them as unimportant, or even

unscientific (for example, Hagen 1980, p. 8) are again open to archaeological debate.

In the light of these developments, it seems to me to be important to remember that there were areas of prehistoric research where problems of ethnicity and multiculturalism had always remained of interest. However, these lay outside the approaches mentioned above which have determined the direction of theoretical discussion in the subject since the 1960s. This is also true of prehistoric research in the German-speaking countries. Here the topic of cultures and ethnicity has traditionally had considerable significance, although specific academic and historical circumstances to which we will return below have meant that in recent decades this line of enquiry has not always been looked at with as much open-mindedness and soberness as one would wish.

Apart from language difficulties, historical circumstances may also be one of the reasons why, outside Germany, approaches to the question of 'ethnic interpretations' of archaeological data have not taken the corresponding German tradition into account. Remarkably enough, even in a recent work by McGuire (1982) on 'The study of ethnicity in historical archaeology' one finds not a single reference to the old Central European research tradition concerning the 'ethnic interpretation' of archaeological data. On the other hand, German research since World War II has largely ignored such discussions going on outside Germany, and even more so outside Europe, although occasional forays over the language barrier convincingly demonstrate how profitable an argument with the other side can be (McWhite 1956, Cullberg 1977, Trigger 1978, 1984, Narr 1981, 1985). What follows below cannot in itself provide such an argument, but it does aim to make a contribution, mainly from the perspective of the history of research, as an attempt to provide admittedly subjective glimpses into the German tradition of the ethnic interpretation of archaeological finds. Thus, the following presentation is intended more as a discussion of some general theoretical and methodological problems in prehistoric archaeology (on the question of different technical terms in the German- and English-speaking archaeological traditions; see Narr 1966) than as a treatment of the problem of the *Germani*, which is, in fact, inseparable from the question of ethnic interpretation. However, this is not an area in which the author can claim the necessary factual knowledge and technical competence (for recent summaries of this topic, see Hachmann 1975, Mildenberger 1986, cf. Martens, Ch. 2, this volume).

The case of Kossinna and its consequences

As far as German research is concerned, the argument about the problem of 'the ethnic interpretation of archaeological culture areas' remains inseparably linked with the name of Gustaf Kossinna. Although competing with other schools of archaeological thought, such as the 'Marburg school', the

fate of his teachings exemplifies the rise and fall of German archaeology in the first half of the 20th century. Indeed, in many respects, even today he casts a shadow over the subject, a phenomenon which Smolla (1979–1980, 1984–1985, 1986) recently characterized by the convenient term, 'the Kossinna syndrome'. Therefore, it is necessary to start by at least spotlighting the causes and symptoms of this syndrome, which can serve us as a case study of the relationship between archaeological objectivity and cultural identity.

First it must be made clear that Gustaf Kossinna (b. 1858), who was originally trained in Germanic philology and entered prehistory via his antiquarian study of the *Germani* (see Stampfuß 1935, Schwerin von Krosigk 1982, Smolla 1978–1980, 1984–1985, 1986), was by no means the first person who attempted to ascribe archaeological finds to specific peoples (on the question of his predecessors see, for example, Wahle 1941, Eggers 1959, Meinander 1981, Hachmann 1987). Certainly today it is his name which is associated with this idea; this is because he, like no other person, brought the question of ethnic interpretation to the centre of prehistoric thought. In doing this he made a lasting contribution to the establishment of prehistory as an academic discipline. Undoubtedly, the rising tide of nationalism at the beginning of this century was remarkably convenient for him in this respect. Indeed, one could go so far as to say that it was this which made possible the rise in the status of prehistory to that of an independent academic subject (Smolla 1979–1980).

Kossinna first stepped on to the archaeological stage with a paper on 'The prehistoric distribution of the Germani in Germany', presented at a meeting of the Anthropological Society in Kassel in 1895 (Kossinna 1896). In this paper he had already sketched out the principles of his so-called 'settlement archaeological method'. He continued to develop these in the following decades, and tried to apply them on a large scale to European prehistory. An extended presentation of his methodological basis combined with a polemical settling of accounts with his academic opponents appeared in 1911 under the title *The origin of the Germani. On the settlement archaeological method* (Kossinna 1911a). It was in this that he made his famous statement 'Sharply defined archaeological culture areas correspond unquestionably with the areas of particular peoples or tribes' (*ibid.*, p. 3). Fifteen years later, after Kossinna had in the meantime succeeded in obtaining a professorship in German archaeology at the University of Berlin, a revised version of his volume on methods of 1911 appeared, under the title *Origin and distribution of the Germani in the prehistoric and early historic periods* (Kossinna 1926). Between these two dates lay a period of extremely intensive work as an author, as a university lecturer and as an organizer. The last of these applies especially to his presidency of the newly founded German Society for Prehistory, and his editorship of the journal *Mannus* and of the monograph series of the same name associated with it.

As well as academic publications in the strict sense, Kossinna produced a whole series of publications intended to influence a wider non-academic audience. The title of his popular book, *German prehistory, a pre-eminently*

national discipline (Kossinna 1914) gives an adequate impression of the nationalistic, indeed racist, attitude which was inseparably associated with Kossinna's work. In his concept of an Aryan, Nordic ideal race, superior to all other peoples – his *Germani*, or their supposedly even more upright predecessors the Indo-Europeans – he saw the key to an unwritten history, as it lay hidden in his prehistoric find groups. According to him, in ever-repeated advances towards the south these *Germani* gave the decisive push to the course of history (Schwerin von Krosigk 1982, p. 71) – a slim, tall, light-complexioned, blonde race, calm and firm in character, constantly striving, intellectually brilliant, with an almost ideal attitude towards the world and life in general.

In the light of these ideas, it comes as no surprise that Kossinna finally attempted to derive political demands from the results of his ethnohistoric research. Apart from his explicit war propaganda during World War I, these included as a political footnote his flawed attempt to influence the political decisions made at Versailles. His demands were laid down in his book *The German Ostmark, a homeland of the Germani* (Eggers 1959, pp. 231ff.)

Kossinna died in 1931, and did not live to experience the upsurge of his subject, and especially of his theories, which followed the seizure of power by the Nazis. However, it goes without saying that, had he still been alive, he would have hailed it with considerable satisfaction, even if the new propagandists did not do adequate justice to his work. Posthumously, Kossinna became, albeit less on the basis of his academic achievements and more because of his 'political influence', the conceptual father and the leading figure of a National Socialist popular (*völkischen*) prehistory (Stampfuß 1935). After the Nazi seizure of power its representatives occupied the key positions in the discipline, once the academic world of Germany had been brought into line according to the ideological prescriptions of the 'Rosenberg office' (Bolmus 1970, Kater 1974). Most members of the discipline, however, especially in those circles which had no direct connection with Kossinna and his school, behaved more discreetly and waited to see what would happen. More direct opposition to the ideological takeover of the discipline, which was connected with a vulgarization of the subject, is certainly not to be detected. The courageous methodological criticisms concerning the 'Kossinna method', raised by Wahle (1941), will be discussed below.

If the rise of Kossinna with the National Socialist takeover was logical, then his fall after 1945 was equally inevitable. Apart from a few of his pupils (Wahle 1950–1951, Jahn 1952), hardly any of those still using his methodological principles were prepared to take his side. The name Kossinna became a non-word. Enormous quantities of paper were printed with explanations that were supposed to demonstrate that the working methods of their respective authors had nothing to do with the Kossinna method, now fallen into disrepute.

However, inasmuch as people from now on anathematized Kossinna's work, and thus did not subject it to a proper critique, they were committing

the same mistake as in 1933, albeit with the opposite premises. With the verbal damnation of Kossinna's method and his convenient branding as the only guilty party – a view which was also widely taken up outside Germany (for example, Clark 1957, Renfrew 1976, p. 38) – the reasons for the ideological misuse of his ideas, which were, after all, based on the nature of archaeological knowledge, remained largely unexplained.

On the other hand, most scholars continued to work with Kossinna's principles, and not just in Germany (cf. Martens, Ch. 2, this volume). Probably the best-known pupil of Kossinna was no less than Childe, who had introduced Kossinna's principles into Great Britain during the 1920s, but stripped of their ideological baggage (Childe 1927, 1929). In view of the political developments in Germany after 1933, this connection tended to be forgotten (Childe 1933). It was Childe himself who, late in life, pointed it out again (Childe 1958, Trigger 1980, McNairn 1980; also, in greater detail, Veit 1984).

'Settlement archaeology'. Kossinna's method and contemporary criticisms

It is now necessary to ask ourselves what there is really of consequence in the 'Kossinna method', much maligned but actually frequently used by its critics (in addition, see in detail Wahle 1941, Eggers 1959, Hachmann 1970, Klejn 1974a). The core of Kossinna's methodological principles is summed up in his well-known axiom of 1911. In its expanded 1926 version this states: 'Clearly defined, sharply distinctive, bounded archaeological provinces correspond unquestionably to the territories of particular peoples and tribes' (Kossinna 1926, p. 21). This guiding principle is linked with the retrospective method, which involves using the (ethnic) conditions of the present (or the historically documented past) to infer the situation in prehistory. The two together make up the so-called 'settlement archaeological method'. Working backwards from early historical times, Kossinna tried to throw light on the development of peoples in prehistory by tracing continuities within particular settlement areas. The basis for this was provided by the 'typological method', which he had taken over from Montelius. Typology enabled him to establish time horizons for the chronological ordering of the material remains of the past (although for Kossinna the principle of the closed find, which had been so important for Montelius, as well as the stratigraphic principle, were both less important than typology; Schwerin von Krosigk 1982, p. 35). Once these chronological horizons had been defined, Kossinna's next step was to make use of the cartographic method in order to distinguish those specific spatial units – find areas or culture provinces – which were supposed to be characterized by the greatest possible homogeneity of material, but most of all by being sharply bounded from neighbouring culture provinces. Kossinna's interpretation of these units had two aspects, which it is important to differentiate:

(a) on the one hand, they were regarded as an expression of ethnic groups, or peoples; and
(b) on the other hand, they were equated with the peoples or tribes first documented historically in a given area.

It is obvious that the hypothetical character of such identifications of peoples increases as one goes further back in time. Kossinna tried to come to terms with this problem by means of an idea influenced by evolutionary principles and deriving from linguistic concepts. It was the notion of apparently less-complex 'primeval cultures' or 'primeval peoples', which supposedly enabled him to 'reconstruct' the former relationships between 'peoples' over a timespan stretching as far back as the Mesolithic. In reality he simply deluded himself about the limited possibility of archaeological knowledge arising from the fragmentary nature of the sources.

Even during his lifetime Kossinna did not lack critics, a fact due at least in part to the provocative and polemical style of his work. Kossinna's argument with Carl Schuchardt is well known and still has an almost legendary ring to it. Josef Kostrzewski was one of Kossinna's own pupils who turned against him. However, this did not represent a refutation of his method, but only of his results with regard to the ethnic identity of the archaeological groups on what is now Polish soil (Eggers 1959, p. 236, also Martens, Ch. 2, this volume).

Two factors in particular rendered difficult, if not impossible, a proper critique of Kossinna's methodological approaches, thus preventing a sober reassessment of the problem of ethnic interpretation.

> The first was his inadmissible equation of people and race (probably a secondary accretion to his method) and the way this notion slipped into an ideology of the Germanic master race due to the nationalistic euphoria of the time.
> Secondly, it must be held against Kossinna that, despite his verbal rigour, he quite frequently did not stick to his own methodological principles, with regard both to the definition of sharply defined culture provinces and to the evidence for continuity from purely prehistoric cultures to cultures in early historical times. Given the state of research at the time, he could probably not do this without renouncing altogether the reconstruction of the supposed prehistoric peoples. Thus, it is typical of much of the lively debate about the 'Kossinna method' that, under the guise of examining the method, it mainly criticized its applications.

Luckily, the extreme racist component of Kossinna's ideas was not taken over by many of his pupils. Thus, even at an early date Blume, Jahn and others (Blume 1912, Hahne 1922, Jahn 1941, 1952) concerned themselves with a further development of the factual and theoretical basis of Kossinna's settlement archaeology, while largely excluding racial aspects. For example, Blume speaks more appropriately of the 'ethnographic method' and Jahn

later of the 'ethnohistorical method', even if the idea of the 'people' or 'folk' as a unit that is objectively present and above all historically significant remains untouched. This is also true of Menghin (1931, 1936, 1952). He was the main representative of the prehistoric branch of the Kulturkreislehre, who made Kossinna's principles his own and tried to apply them universally. However, in Menghin's work we begin to see a replacement of the term 'people' by the more neutral and supposedly less dubious term 'culture', a development which continued after World War II. However, Kossinna's followers continued to ignore the more fundamental doubts that were being expressed with regard to the equation between peoples and cultures, and more generally about the true culture-historical character of the discipline.

Several scholars come to mind in this connection, including Hoernes (1905, p. 238) and Sprockhoff (1930, p. vii), but the most important is K. H. Jacob-Friesen. Already in Kossinna's lifetime he published a wide-ranging theoretical volume entitled Basic questions of prehistoric research (K. H. Jacob-Friesen 1928), with the more explicit subtitle: A critique of the current state of research on races, peoples and cultures in prehistory. Here he concluded (p. 144):

> Today it is still extraordinarily difficult to identify the areas of cultures with the areas of peoples when we know little more than the names of those peoples from historical sources. To make this kind of equation in periods millennia earlier than the first historical mention of those peoples is a claim which can only be rejected.

Jacob-Friesen was, of course, influenced by the ethnological 'Kulturkreis' school, as represented by Leo Frobenius; accordingly, he saw the practical task of prehistory first of all as 'defining as many individual distribution areas of given forms as possible, gathering these together into Kulturkreise and establishing their chronological succession' (K. H. Jacob-Friesen 1928, p. 145).

It was during World War II, in 1941, against the background of the dominance of Nazi prehistory, that Wahle published a small book that became famous, entitled On the ethnic interpretation of early historical culture provinces. It was Wahle who rekindled the debate about the Kossinna method, which under the surface had never quite died out. Because of the particular circumstances of the time, the immediate published reaction was confined to a rejection of the critique of Kossinna by his pupil Jahn (1941, at greater length 1952). On the basis of various examples from the early historical period, Wahle presented cases of ethnic boundaries which did not find an expression in clearly defined cultural provinces. However, with this he basically criticized only the reversal of Kossinna's principle (Narr 1985, pp. 58f.). Recognizing the legitimacy of the ethnic interpretation at least of early historical culture provinces, he used this as a basis for criticizing the schematic treatment of prehistory apparent in Kossinna's method. In opposition to this, he demanded that 'rational–ahistorical' thought in prehistory should be overcome. By this he meant especially the typological–

evolutionary approach. This, Wahle held, should be superseded in future by the investigation of the driving forces operating in history, above all in the history of 'prehistoric peoples'. It is apparent from the idealistic concepts with which Wahle approached his subject – for example, the concept of 'vital power' (*Lebenskraft*) – that his demands are difficult to realize in practice. However, even his more concrete hypotheses about the possible factors which could have produced particular distributions of finds often remain very vague. Apart from his demand for a greater emphasis on the question of the association of various find groups with one another (Wahle 1941, p. 133), these did not lead to any new perspectives. His ethnic reconstructions depend largely on written sources, and therefore they are not transferable to periods where such evidence is lacking. Indeed, on the basis of *a priori* assumptions about the nature of sociopolitical evolution, he tried to cast doubt on the existence of peoples or other units for the earlier periods of prehistory, and in certain areas called them into question altogether (*ibid.*, p. 116), a procedure which is just as problematical as Kossinna's completely unhistorical concept of 'peoples'.

'Archaeological culture' and ethnicity since 1945: some examples

Wahle's (1941) book largely determined the direction of discussion in the discipline after 1945. Two lines of enquiry were pursued. In the field of early history, on the basis of an increasingly refined source-critical approach, an increasingly good grip was obtained on both the possibility and the necessity of ethnic interpretation. However, the possibility of doing this on the basis of archaeological sources alone was completely rejected, as being beyond the range of possibility of archaeological knowledge (Kirchner 1950, Werner 1950, Jankuhn 1952, Eggers 1959, von Uslar 1955, 1961, 1965, Kilian 1960; most recently Daim 1982). However, this last principle immediately came into conflict with current research practice, in which – hidden behind the supposedly more neutral term 'archaeological culture' – the old ethnic concepts continued to survive. Although many scholars gave up speaking of Indo-Europeans or even Germani, the notion that 'peoples' must be hiding behind the various archaeological groupings remained something taken for granted, albeit not made explicit (Lüning 1972, Bergmann 1972, 1973–1974, 1974, Angeli 1976). The 'archaeological culture' became – as for Childe (1929) – a quasi-ideology-free substitute for the term 'ethnic unit'. By means of this safeguard the problem of ethnic interpretation was removed from explicit discussion. Incidentally, this observation is equally true of prehistoric research in the eastern part of Germany, newly oriented as it was to the principles of historical materialism. Here we are faced with a vehement polemic directed against both Kossinna and his followers, and against more-recent 'bourgeois' approaches apparently thought to be outdated. Nevertheless, the term 'socio-economic areas' meant essentially the same as

the term 'archaeological culture' (Behrens 1984, p. 57; cf., for example, Otto 1953, Hermann 1965, 1977). Lack of space here precludes an extensive treatment of materialist approaches (cf. also Russian and Polish authors: Klejn 1974a, 1974b, 1981, Hensel 1977).

As far as ethnic interpretation in the field of early historical cultures is concerned, a strict division between the various arguments of the individual disciplines involved became the standard demand. With the methodological maxim 'march separately, strike together' – a rather disturbing slogan in the light of recent history (see Wenskus 1979, p. 637) – people thought they could avoid the mistakes of that past. The disciplines involved – history, linguistics and prehistory, i.e. archaeology – should first of all evaluate their respective sources, and only at the final stage should their various results be brought together. A classic example of this approach is provided in a book written jointly by Hachmann, Kossack, and Kuhn. This impressive study by two archaeologists and one philologist appeared in 1962 under the title, *Peoples between Germani and Celts. Written sources, archaeological finds and information from names, for the history of North-West Germany around the time of Christ.* Similarly, Eggers' *Introduction to prehistory* (1959), a trail-blazing textbook in post-World War II Germany, recommends a three-stage dialectical sequence in archaeological source criticism: 'archaeological thesis, literary antithesis, historical synthesis'.

Although at first sight the application of these principles seems advisable, it soon became apparent that they did not do justice to the actual procedures current in the discipline. According to the historian Wenskus, they are both unsubtle and impracticable. Instead he argues for a broad interdisciplinary approach, in a similar fashion to Klejn (1974b, 1981). Wenskus' book on the formation and composition of the gentes of the early middle ages, published in 1961, remains today the starting point for all attempts at an interdisciplinary approach to the problem of ethnic interpretation in the early historical period, and these have been attempted from various sides (for example, Hachmann 1970, 1975, Capelle 1971, F. Fischer 1972; summaries in Daim 1982, Mildenberger 1986).

Here, however, I wish to restrict my reflections to the following question: how significant a rôle have ethnic concepts played since 1945 in that larger segment of the discipline for which no historical data are available as a corrective? The significance which the term 'archaeological culture' came to have in this connection has already been pointed out above (cf. recently Hachmann 1987). It increasingly became an ideologically untainted and therefore useful synonym for the term 'ethnic unit'. Unfortunately, in doing this it encouraged a tendency to obscure the real problem, a point of criticism already made by Hodson (1980) of the English-speaking archaeological scene. Of course, it is important to note here that in this connection the phrase 'ethnic interpretation' no longer referred to the equation of specific groups of finds with historically attested ethnic units, but solely to the interpretation of certain find groups as the expression of once-existing ethnic groups whose names have not come down to us.

Apart from Bronze Age work (for example, Bergmann 1970), a look at research on the Central European Neolithic is worthwhile in this regard. This is a topic which provides the practical prerequisites for a discussion of this type of question, because of the considerable expansion of the available data base which has taken place over the years. In this respect U. Fischer's (1956) book, *The graves of the Stone Age in the Saale area*, was of outstanding importance for German research in the post-World War II period, from both a practical and a theoretical point of view. Starting from the rich corpus of finds of Neolithic burials in the central German area, the question was raised by the author of whether the grave finds would confirm or even refine the existing ordering of 'cultural groups', predominantly based on an appraisal of the pottery. From this investigation it emerged clearly that, on the whole, the pottery groups correspond to the groups based on burial ritual. From this U. Fischer (1956, p. 256) concluded, contrary to his original hope, that

> the burial rite cannot be used to bridge cultural differences diachronically. Our problem field thus appears to have gained a truly historical dimension. The quest for historical continuity lies beyond the range of our sources. The changes in grave and burial form appear completely embedded in the change of cultural forms, in that 'historische Tiefendimension' which must remain inaccessible to a purely ethnographic treatment.

Thus, within this limited area, Fischer provided the empirical evidence for the 'recurrent assemblage of types' (here pottery and burial types) which Childe (1956) had demanded as the characteristic feature of an 'archaeological culture'. He rightly saw the problem area as raised to an 'historical dimension'. This means nothing more nor less than that he saw appearing behind the groupings of finds historically influential units, 'peoples', or if one wishes, more non-committally, 'ethnic groups'. On the other hand, in establishing not only the spatial but also the chronological discontinuity of his material, it was made clear by Fischer that a pursuit of such groups across various periods is impossible, at least on the basis of the material currently available.

It is unfortunate that subsequently people have not always kept to the standards set by Fischer for the analysis of archaeological cultures. Such cultures have been postulated even without the possibility of being able to demonstrate a combination of various cultural elements, functionally independent of one another. This kind of approach has been rightly criticized in recent years. In the field of Neolithic research, fundamental observations go to the credit of Lüning (1972, 1979; cf. Mandera 1965). Taking as his starting point the results of recent empirical investigations which point to a marked degree of continuity within the Neolithic cultural development of western Central Europe, Lüning (1979, p. 101) demanded a rethinking of the term 'culture' in its old ethnically influenced sense:

Under the influence of an ethnically based cultural theory, earlier research worked in the main from the assumption that the neolithic pottery groups represented 'cultures' in an inclusive and organic sense. This produced a 'block-like' cultural model which regarded the pottery as an exact expression of sociocultural, ethnic and economic entities, and in effect as a passive reflection of them. Their duration and their spatial distribution could thus be regarded as a substitute for a 'culture' in the wider sense. This approach made ethno-historic and politico-historic interpretations a great deal easier.

Contrary to this approach, Lüning argued, it is necessary to free the term 'archaeological culture' from its ethnic and other implications and to restrict it to its chronological dimension. In the context of this new 'paradigm' for the early and late Neolithic periods in Central Europe, influenced by the concept of an extensive continuity, 'archaeological cultures' should be regarded solely 'as components of a chronological–terminological system' which should not be overburdened with too much weight of meaning (Lüning 1972, p. 169):

> This term culture is thus suitable for quite specific tasks, and only for these. In the context of the present state of knowledge it gives us full information about the chronological position of the material, but implies little about its spatial position and almost nothing about either the combined treatment of these two, possible functional connections between individual cultural elements or areas, or about the relationship to political, social, religious, military, economic and other categories of neolithic people. It is important to be clear about this if one wants to look at the neolithic from other than a chronological point of view and in doing this mistakenly makes use of the cultures as apparently given entities.

What, then, is the value of such a concept of culture with its narrowly bounded explanatory potential? Here I cannot go into all of the practical problems connected with the Lüning paradigm. Nevertheless, it should be pointed out that the assumption of a strong degree of continuity may be supported by the material only under certain conditions. With his concept of a prevailing continuity – debatable because it may only be maintained on the assumption that apparent breaks in the development are to be interpreted exclusively as gaps in the finds record – Lüning explicitly contradicts the results of Fischer and others. It is worth calling to mind that they had to conclude – in some cases contrary to their initial assumptions – that discontinuity rather than continuity characterized the archaeological material. When Lüning starts from the assumption of a general continuity in Neolithic development, in contrast with earlier research, then he is not simply basing his argument on empirical observations, but primarily replacing one set of premises with another. In fact, the implicit postulate of

continuity makes one wonder whether his culture concept is unsuitable for detecting continuity or discontinuity not only in the spatial dimension, but also in the chronological dimension. At the more practical level this seems to be borne out by Lüning's habit of frequently using the terms 'chronological succession' and 'genetic succession' interchangeably. In this regard Sangmeister (1973, p. 387, more generally 1967) has pointed out that a chronological succession, 'B comes after A', is not necessarily the same as a genetic derivation, 'B developed from A'. In the last analysis a chronological *a priori* of this kind is as problematical as a comparable chorological or geographical *a priori*, such as the culture-area concept of American archaeology (Wissler 1917, 1923).

As far as the theoretical framework of the discipline is concerned, the new empirical observations which Lüning adduces as a basis for his argument ultimately do not necessitate a change of paradigm. The stress laid on unity and continuity in the Central European early and middle Neolithic appears (but only appears) to be an emphasis emerging from certain empirical investigations. Their results could equally well be explained exclusively in terms of the 'culture-historical' paradigm, and not by recourse to a concept which excludes the existence of such entities at the outset. The overhasty equation made between archaeological cultures and quasi-ethnic entities was less the result of incorrect methods, and much more the consequence of two shortcomings: a lack of methodological purity and an overestimation of the possibilities of acquiring knowledge in the context of a comparatively poor state of research. The quest for groupings which extend beyond the individual settlement unit remains valid, for the Neolithic as well as for other periods, even if today it can no longer claim its earlier monopoly position, and even if the problems involved may never be completely resolved by archaeological means.

The practical conclusion of this discussion must surely be that we shall continue to use the old culture concept as conceived by Kossinna and developed by others, even though we are conscious that in talking of an 'archaeological culture' we are not necessarily dealing with the material expression of a 'people', but primarily with an archaeological heuristic device. In this way, and contrary to Lüning's demand, the term, as the most important unit of archaeological classification, should finally be freed from its burden of chronostratigraphic implications (Müller-Beck 1977, p. 195). An 'archaeological culture' is thus to be understood as a term for an entity which is spatially and chronologically distinguishable within the general cultural development. The degree of spatial (or chronological) uniformity (Kossinna) or the extent to which a coincidence of individual features can be detected (Childe) – that is, the combination of individual, functionally independent elements (Narr 1981) – must be investigated empirically in each particular case (on methodological questions, see Hodder 1978, Shennan 1978).

Furthermore, depending on the degree of uniformity within the distribution of types and the extent to which individual elements coincide, it must

be permissible to accept as an heuristic principle the ethnic nature of such entities, if other more-simple explanations, such as ecological or economic factors, can be excluded. Since Wahle's (1941) work, early historical archaeology has provided a variety of important methodological assessments (for example, Werner 1950, Kirchner 1950, Jankuhn 1952, Eggers 1959, Hachmann *et al.* 1962, Hachmann 1970). As far as the Neolithic is concerned, such reflections have only begun recently. Here special mention should be made of the work of Narr (1985), dealing with the 'Schönfeld Group' of the central German Neolithic and archaeological groups from the southwestern USA.

However, in all these endeavours it is important to be aware that no more than a rough approximation to former conditions is possible. Thus, if one accepts that the decisive feature of an ethnic unit is the consciousness of individuals in it of belonging to it (Wenskus 1961) – a consciousness of belonging together within a definite group extending beyond the local settlement unit – it is apparent that evidence for this can never be observed by archaeological means alone. Nevertheless, we may take as an heuristic starting point that the greater the differences between such a group and other groups are, the greater the probability is that these will be reflected in the field of material culture. Ethnic groups are not primarily objective organic entities detectable by means of language, material culture or race whose ancestry can be traced back through the ages. They are rather to be considered as structures or entities which can only be experienced subjectively, in the sense of belonging or not belonging to them, and whose individual form and content depend on a variety of cultural, social, religious, economic and other factors (Hodder 1977, 1978, especially pp. 248ff., Wernhardt 1979, Girtler 1982, Geary 1983). In this connection it would be mistaken to regard a mutual cultural distinction between such groups as a measure of their isolation. On the contrary, ethnic boundaries have their justification and gain importance through intensive interethnic contact (Barth 1969). To this extent the supposedly more neutral term 'interaction sphere' (Mischung 1986, also critically Shennan 1978), often valued as a substitute, is at least misleading.

To sum up, the problem of the ethnic interpretation of archaeological data which has been the object of controversial discussion for so long does not differ significantly in nature from the problem of their sociological interpretation. In both cases we are faced with the question of inferring the status of a specific group of individuals when that status is not directly detectable by archaeological means. In the final analysis it is a matter of demonstrating a symbolic connection which is not in itself evident. It is therefore not credible for people to engage in the business of sociological interpretation while damning the enterprise of ethnic interpretation; of course, the reverse also applies.

Cultural identity and archaeological objectivity

The relationship between culture and ethnicity has certainly not been the primary goal of German prehistory in the post-World War II years.

However, as I have tried to show, despite Kossinna and his consequences, the time-honoured discussion of the problem of the interpretation of find groups, types and assemblages in terms of ethnic units has never ceased, even though this discussion has frequently made use of a strangely coded form of expression, due to the special historical circumstances prevailing. Nevertheless, as a result of the events which followed 1933, this paradigm, and with it the discipline as a whole, has lost most of its former influence on historical thinking in Germany. At the beginning of the 20th century prehistory was able to establish itself as a discipline within our universities, above all because it succeeded in securing a strong position in the writing of the history of Germany before it became a nation. Inevitably the vulgarization and misuse of the subject by the Nazis shook this initiative. The attempt to establish prehistory as an historical discipline was almost wrecked, thereby confirming the views of those who had always expressed doubts about whether archaeology could produce real historical knowledge. Prehistoric archaeology became what many earlier had wished to make of it, 'a preeminently antiquarian discipline', to use the phrase with which Torbrügge (1959, p. 4) played on Kossinna's (1914) well-known dictum. This change of direction is expressed in the predominantly pragmatic orientation of the discipline as far as prehistory is concerned, and its strong emphasis on descriptive–classificatory and chronological problems (Narr 1966, Eggert 1978a). This restriction of the discipline to a specific area of method and the refusal to set theoretical goals of any consequence can only be understood as a reaction against the inflated knowledge claims of Nazi studies in prehistory. However, it had the opposite effect to that intended, in at least one respect – it did not lead to the rehabilitation of German prehistoric studies outside Germany.

In the 1920s German prehistory pioneered the development of the discipline as a whole, thanks above all to Kossinna and his school. However, this position, which was lost at the latest by 1933, could not be regained after World War II. In the English-speaking world in particular, ecological, economic and sociological questions came increasingly to the forefront of research, notably thanks to the works of Childe and Clark, and consequently theoretical perspectives broadened. In the Federal Republic of Germany pragmatism prevailed (with a few notable exceptions: for example, Jankuhn 1952, 1977, Narr 1954, some of the contributions to the short-lived journal *Archaeologia Geographica*) and the discipline moved increasingly towards the theoretically irrelevant (Eggert 1978a, Härke 1983). This 'common-sense attitude' is documented not least by the fact that people shunned the influences now coming from abroad and from neighbouring disciplines. Thus, a serious debate with the protagonists of the 'New Archaeology' never took place – apart from a few significant exceptions (Eggert 1978b, Wolfram 1986) – despite the availability of good arguments for a culture-historical approach.

The attitude that prevailed in the post-World War II period is still reflected where theoretical abstinence was abandoned in favour of a new theoretical foundation. Thus, the 'Lüning paradigm' discussed above basically repre-

sents nothing other than a justification of the archaeological practice of the post-World War II period, and therefore of a position rejecting theory as such. This is borne out by the fact that Lüning believes that he can finally reject such concepts as people, ethnos or culture which transcend the practice of prehistorians, and which have been an indispensable part of anthropological thought since the beginning of this century. In his view prehistory should be satisfied with the goal of demonstrating the 'development, correlation and structure of individual cultural phenomena' (Lüning 1972, pp. 169f.). Eggert (1978a, p. 18) instead was certainly right to remark, 'that archaeology, like all the other human sciences, cannot do without a definition of culture which is primarily theoretical, in other words, explanatory and interpretative'. For this reason he urges prehistorians to take as active a part as possible in theoretical discussions within anthropology, a point of view which has long been taken for granted elsewhere. However, here too the problem arises of the nature of ethnic entities or 'cultures' and their possible material expression, a question which has received a great variety of different answers within cultural anthropology over the past 100 years.

In my view it has been one of the shortcomings of post-World War II German prehistory with major implications that, apart from a few exceptions, it has failed to take up anew the extremely fruitful debate between the anthropological and historical traditions already established at the origin of our discipline. It was the assumption of Kossinna and many of his followers that the so-called Aryan–Germanic culture was superior to other cultures and incomparable, that discredited a co-operation with anthropology (the German *Ethnologie*) which could still be successfully practised in the 1920s (Kossinna 1911b, pp. 128f.). However, after 1945 it is a mistaken claim of methodological absolutism which must be adduced as the basis of the supposed uselessness of such a co-operation. In this respect the shambles of the patriotic–nationalist and the sceptical tendencies formed an unholy alliance which led in most parts of prehistoric archaeology to the general abandonment of the anthropological roots of the discipline (an exception is palaeolithic research). As I see it, this is the major reason for the deep-rooted reluctance still prevalent in the German-speaking world to use ethnographic data in the interpretation of prehistoric facts, particularly from the more recent periods. Inasmuch as people restricted themselves to analogies from the present and historical past of Europe in order to interpret archaeological facts, they were bound to restrict the potential of our knowledge in an inadmissible fashion. Following the implicit premise that 'European prehistory can *only* be explained through European history', ethnocentric prejudice partly took the place of empirical comparison. Thus, in a certain fashion Kossinna, who had only recently been publicly banned, re-entered the discipline through the back door (however, in this context it is worth noting that it was a similar belief in the superiority of European culture, albeit based on different motives, which was unconsciously at least partly responsible for Childe's negative attitude to ethnographic parallels). This unconscious ethnocentric fixation continues to hinder in Germany the

breakthrough of ethno-archaeology which has taken place in the English-speaking world in recent decades. Attempts from the ethnological side, especially on the subject of 'ethnicity' (Vossen 1969, Liesegang 1973), were largely ignored. The same is true of similar attempts at an interdisciplinary rapprochement on this subject in the context of a certain renaissance of the 'old anthropology' (Wernhardt 1979, Girtler 1982, Daim 1982, Spindler 1983, Winkler 1983, *Studien zur Ethnogenese* 1985).

Finally, if we look from a more abstract point of view at the relationship of the aspirations of 'ethnic archaeology' and 'ethnoarchaeology', there appear to be certain parallels. An ideological one-sidedness was at the root of both trends: in the one case an excessive nationalism has been noted, in the other case a similarly authoritarian internationalism may be detected. Both were specific products of their time, but hopefully both have now outlived their stage of ideological excess, if the indications are correct. Are they not turning out to be theoretically complementary concepts that may prove to be useful in the long term for the study of archaeological data as the expression of past human thought and action?

However, what does all this signify for the relationship between archaeology and politics? I believe that if we are able to draw one lesson from the German example, it is that archaeology is not an appropriate medium for the contemporary debate and foundation of ethnic or national interests. I emphasize this not least in relation to the variety of archaeologies now being established outside Europe. The current problems arising out of the factor of ethnicity which are increasingly impinging on the general consciousness (for example, Smith 1981) can certainly not be solved by the introduction of archaeological or prehistoric arguments into the debate. The amusing case of Kossinna's unsatisfactory intervention at the Versailles peace conference exemplifies this opinion in an almost surrealistic fashion. A prehistory which makes a claim to be taken seriously as an academic discipline must, like history, defend itself all the more vigorously against every form of takeover by outside interests. This is a demand which Kossinna's contemporaries did not take sufficiently seriously. Like all of the other human sciences, prehistoric archaeology too must insist on a division between archaeological knowledge and the process of life, for the sake of its objectivity and its capacity to make progress. Prehistory does not provide a finished picture of the past which can be applied without further ado to directing social activity.

On the other hand, there can be no prehistoric research outside the interests of society. Archaeological knowledge is not neutral and apolitical by virtue of its very nature (Hodder 1984, 1986, Ucko 1983). However, the acknowledgement that an objective and value-free archaeology is impossible in principle leads directly to the demand for permanent self-reflection within the discipline (Rüsen 1977, from the position of a historian). There will be a great deal of work on this subject in the future – especially in western Germany. This point is equally valid for the discussion of the rôle of archaeology in public education. Here, too, it was the shock of Nazi

prehistory in Germany which allowed statements on the subject from the 1920s to lapse into oblivion (Marienfeld 1979).

The qualifications made above do not mean that we can draw no lessons from prehistory. However, in going ahead with drawing lessons we should always bear in mind that 'History "obtained from archaeology" ', as Smolla (1979–1980, p. 8) put it, 'is exposed to greater dangers, because the facts can have more than one meaning, and are thus more prone to manipulation; moreover, because the "beginnings" and "origins" can so easily be turned into myth'.

Acknowledgements

References

Angeli, W. 1976. Zum Kulturbegriff in der Urgeschichtswissenschaft. In *Festschrift für R. Pittioni. Archaeologia Austriaca*, Beiheft **13**, I, 3–6. Wien: Deuticke.

Barth, F. (ed.) 1969. *Ethnic groups and boundaries. The social organization of culture difference*. Bergen, Oslo: Universitets Forlaget and London: Allen & Unwin.

Behrens, H. 1984. *Die Ur- und Frühgeschichtswissenschaft in der DDR von 1945–1980.* Arbeiten zur Urgeschichte des Menschen. H. Ziegert (ed.). Frankfurt-am-Main: Lang.

Bergmann, J. 1970. *Die ältere Bronzezeit Nordwestdeutschlands. Neue Methoden zur ethnischen und historischen Interpretation urgeschichtlicher Quellen.* Kasseler Beiträge zur Vor- und Frühgeschichte **2**, Teil A/B. Marburg: Elgwert.

Bergmann, J. 1972. Ethnos und Kulturkreis. Zur Methodik der Urgeschichtswissenschaft. *Prähistorische Zeitschrift* **47**, 105–10.

Bergmann, J. 1973–1974. Zum Kulturkreis. Zur Denkweise in der Urgeschichtswissenschaft. *Archäologische Informationen* **2–3**, 189–91.

Bergmann, J. 1974. Zum Begriff des Kulturkreises in der Vorgeschichtswissenschaft. *Prähistorische Zeitschrift* **49**, 129–38.

Blume, E. 1912. *Die germanischen Stämme und die Kulturen zwischen Oder und Passarge zur römischen Kaiserzeit.* Mannus-Bibliothek 8. Würzburg: Kabitzsch.

Bolmus, R. 1970. *Das Amt Rosenberg und seine Gegener. Zum Machtkampf im nationalsozialistischen Herrschaftssystem.* Stuttgart: Deutsche Verlags-Anstalt.

Capelle, T. 1971. *Studien über elbgermanische Gräberfelder in der ausgehenden Latènezeit*

und der älteren römischen Kaiserzeit. Münstersche Beiträge zur Vor- und Frühgeschichte 6. Hildesheim: Lax.

Childe, V. G. 1927. Rezension von: Kossinna 1926. *Man* **27**, 54f.

Childe, V. G. 1929. *The Danube in prehistory.* Oxford: Clarendon Press.

Childe, V. G. 1933. Is prehistory practical? *Antiquity* **7**, 410–18.

Childe, V. G. 1956. *Piecing together the past. The interpretation of archaeological data.* London: Routledge & Kegan Paul.

Childe, V. G. 1958. Retrospect. *Antiquity* **32**, 69–74.

Clark, G. 1957. *Archaeology and society. Reconstructing the prehistoric past,* 3rd edn. London: Methuen. (1st edn 1939; 2nd edn 1947.)

Cullberg, C. 1977. Kossinna in Florida. In *Festschrift zum 50jährigen Bestehen des Vorgeschichtlichen Seminars Marburg.* O.-H. Frey (ed.). Marburger Studien zur Vor- und Frühgeschichte 1. Gladenbach: Kempkes.

Daim, F. 1982. Gedanken zum Ethnosbegriff. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **112**, 58–71.

Eggers, H. J. 1959. *Einführung in die Vorgeschichte* (new edn 1974). Munich: Piper.

Eggert, M. K. H. 1978a. Zum Kulturkonzept in der prähistorischen Archäologie. *Bonner Jahrbücher* **178**, 1–20.

Eggert, M. K. H. 1978b. Prähistorische Archäologie und Ethnologie. Studien zur amerikanischen New Archaeology. *Prähistorische Zeitschrift* **53**, 6–164.

Fischer, F. 1972. Die Kelten bei Herodot. *Madrider Mitteilungen* **13**, 109ff.

Fischer, U. 1956. *Die Gräber der Steinzeit im Saalegebiet. Studien über neolithische und frühbronzezeitliche Grab- und Bestattungsformen in Sachsen-Thüringen.* Berlin: de Gruyter.

Friedman, J. 1988. Culture, identity and world process. In *Domination and resistance,* D. Miller, M. Rowlands & C. Tilley (eds), ch. 1. London: Unwin Hyman.

Geary, P. J. 1983. Ethnic identity as a situational construct in the early middle ages. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **113**, 15–26.

Girtler, R. 1982. 'Ethnos', 'Volk' und soziale Gruppe. Zum Problem eines zentralen Themas in den anthropologischen Wissenschaften. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **112**, 42–57.

Hachmann, R. 1970. *Die Goten und Skandinavien.* Berlin: de Gruyter.

Hachmann, R. 1975. Der Begriff des Germanischen. *Jahrbuch für Internationale Germanistik* **7**, 113–44.

Hachmann, R. (ed.) 1987. *Studien zum Kulturbegriff in der Vor- und Frühgeschichtsforschung.* (Saarbrückener Beiträge zur Altertumskunde, Bd. 48.) Bonn: Habelt.

Hachmann, R., G. Kossack & H. Kuhn 1962. *Völker zwischen Germanen und Kelten. Schriftquellen, Bodenfunde und Namengut zur Geschichte des nördlichen Westdeutschlands um Christi Geburt.* Neumünster: Wachholtz.

Hagen, A. 1980. Trends in Scandinavian archaeology at the transition to the 1980s. *Norwegian Archaeological Review* **13**, 1–8.

Hahne, H. (ed.) 1922. *25 Jahre Siedlungsarchäologie. Arbeiten aus dem Kreise der Berliner Schule.* Mannus-Bibliothek Nr 22. Leipzig: Kabitzsch.

Härke, H. (ed.) 1983. *Archäologie und Kulturgeschichte. Symposium zu Zielvorstellungen der deutschen Archäologie.* Unkel.

Hermann, J. 1965. Archäologische Kulturen und sozialökonomische Gebiete. *Ethnographisch-Archäologische Zeitschrift* **6**, 97–128.

Hermann, J. 1977. Archäologie als Geschichtswissenschaft. In *Archäologie als Geschichtswissenschaft. Studien und Untersuchungen,* J. Hermann (ed.), 9–28. Berlin: Akademie-Verlag.

Hensel, W. 1977. A method of ethnic qualification of archaeological sources. *Archaeologia Polona* **18**, 7–35.

Hodder, I. 1977. The distribution of material culture items in the Baringo district, Western Kenya. *Man (New Series)* **12**, 239–69.

Hodder, I. (ed.) 1978. *The spatial organization of culture.* New Approaches in Archaeology. London: Duckworth.

Hodder, I. 1984. Archaeology in 1984. *Antiquity* **58**, 25–32.

Hodder, I. 1986. Politics and ideology in the World Archaeological Congress 1986. *Archaeological Review for Cambridge* **5**(1), 113–19.

Hodson, F. R. 1980. Cultures as types? Some elements of classification theory. *Bulletin of the Institute of Archaeology, University of London* **17**, 1–10.

Hoernes, M. 1905. Die Hallstattperiode. *Archiv für Anthropologie (Neue Folge)* **3**, 233–81.

Jacob-Friesen, K. H. 1928. *Grundfragen der Urgeschichtsforschung. Stand und Kritik der Forschung über Rassen, Völker und Kulturen in urgeschichtlicher Zeit.* Hannover: Helwing.

Jahn, M. 1941. Die deutsche Vorgeschichtsforschung in einer Sackgasse? *Nachrichtenblatt für deutsche Vorzeit* **17**, 73–82.

Jahn, M. 1952. *Die Abgrenzung von Kulturgruppen und Völkern in der Vorgeschichte.* Berichte über die Verhandlungen der sächsischen Akademie der Wissenschaften zu Leipzig. Philologisch-historische Klasse, Vol. 99, No. 3. Berlin: Akademie Verlag.

Jankuhn, H. 1952. Klima, Besiedlung und Wirtschaft der älteren Eisenzeit im westlichen Ostseebecken. *Archaeological Geographica* **3** (1/3), 23–35.

Jankuhn, H. 1977. *Einführung in die Siedlungsarchäologie.* Berlin and New York: de Gruyter.

Kater, M. H. 1974. *Das 'Ahnenerbe' der SS 1935–1945. Ein Beitrag zur Kulturpolitik des Dritten Reiches.* Stuttgart: Deutsche Verlags-Anstalt.

Kilian, L. 1960. Zum Aussagewert von Fund- und Kulturprovinzen. *Swiatowit* **23**, 41–85.

Kirchner, H. (ed.) 1950. *Ur- und Frühgeschichte als historische Wissenschaft. Festschrift zum 60. Geburtstag von E. Wahle.* Heidelberg: Winter.

Klejn, L. S. 1974a. Kossinna im Abstand von vierzig Jahren. *Jahresschrift für mitteldeutsche Vorgeschichte* **58**, 7–55.

Klejn, L. S. 1974b. Regressive Purifizierung und exemplarische Betrachtung. Polemische Bemerkungen zur Integration der Archäologie mit der schriftlichen Geschichte und Sprachwissenschaft bei der ethnischen Deutung des Fundgutes. *Ethnographisch-Archäologische Zeitschrift* **15**, 223–54.

Klejn, L. S. 1981. Die Ethnogenese als Kulturgeschichte, archäologisch betrachtet, neue Grundlagen. In *Beiträge zur Ur- und Frühgeschichte I. Festschrift für W. Coblenz,* 13–25. Berlin: Akademie Verlag.

Kossinna, G. 1896. Die vorgeschichtliche Ausbreitung der Germanen in Deutschland. *Zeitschrift des Vereins für Volkskunde* **6**, 1–14.

Kossinna, G. 1911a. *Die Herkunft der Germanen. Zur Methode der Siedlungsarchäologie.* Mannus-Bibliothek 6. Würzburg: Kabitzsch.

Kossinna, G. 1911b. Anmerkungen zum heutigen Stand der Vorgeschichtsforschung. *Mannus* **3**, 127–30.

Kossinna, G. 1914. *Die deutsche Vorgeschichte eine hervorragend nationale Wissenschaft,* 2nd edn. Mannus-Bibliothek 9. Würzburg: Kabitzsch.

Kossinna, G. 1926. *Ursprung und Verbreitung der Germanen in vor- und frühgeschichtlicher Zeit.* Mannus-Bibliothek 6. Würzburg: Kabitzsch.

Liesegang, G. 1973. 'Ethnische Gruppen' und rezente keramische Komplexe (oder Formenkreise) im südlichen Mosambik: ein Beitrag zur Frage der 'ethnischen Deutung'. In *Festschrift zum 65. Geburtstag von Helmut Petri*, K. Tauchmann (ed.). Kölner Ethnologische Mitteilungen 5, 289–317. Cologne: Böhlau.

Lüning, J. 1972. Zum Kulturbegriff im Neolithikum. *Prähistorische Zeitschrift* **47**, 145–73.

Lüning, J. 1979. Über den Stand der neolithischen Stilfrage in Südwestdeutschland. *Jahrbuch des Römisch–Germanischen Zentralmuseum Mainz* **26**, 75–113.

McGuire, R. H. 1982. The study of ethnicity in historical archaeology. *Journal of Anthropological Archaeology* **1**, 159–78.

McNairn, B. 1980. *The theory and method of V. G. Childe*. Edinburgh: Edinburgh University Press.

McWhite, E. 1956. On the interpretation of archaeological evidence in historical and sociological terms. *American Anthropologist* **58**, 3–25.

Mandera, H.-E. 1965. Zur Deutung neolithischer Kulturen. Probleme urgeschichtlicher Methodik. *Nassauische Annalen* **76**, 1–14.

Marienfeld, W. 1979. *Ur- und Frühgeschichte im Unterricht. Zugleich ein Beitrag zur Geschichte des Geschichtsunterrichts*. Frankfurt-am-Main: Diesterweg.

Martens, J. 1989. The Vandals: myths and facts about a Germanic tribe of the first half of the first millennium AD. In *Archaeological approaches to cultural identity*, S. J. Shennan (ed.), ch. 2. London: Unwin Hyman.

Meinander, C. F. 1981. The concept of culture in European archaeological literature. In *Towards a history of archaeology. Conference on the history of archaeology in Aarhus 1978*, G. Daniel (ed.), 100–11. London: Thames & Hudson.

Menghin, O. 1931. *Weltgeschichte der Steinzeit*. Wien: Schroll.

Menghin, O. 1936. Grundlinien einer Methodik der urgeschichtlichen Stammeskunde. In *Germanen und Indogermanen. Festschrift für H. Hirt*, 41–67. Heidelberg: Winter.

Menghin, O. 1952. Urgeschichtliche Grundfragen. In *Historia Mundi*, Vol. 1: *Frühe Menschheit*, 229–58. Munich: Lehnen.

Mildenberger, G. 1986. Die Germanen in der archäologischen Forschung nach Kossinna. In *Germanenprobleme in heutiger Sicht*, H. Beck (ed.). Reallexikon der germanischen Altertumskunde, Ergänzungsband 1. Berlin and New York: de Gruyter.

Mischung, R. 1986. Rezension von: Studien zur Ethnogenese. In *Germania* **64**(2), 677–82.

Müller-Beck, H. 1977. Ein Beitrag zur urgeschichtlich-archäologischen Terminologie. *Regio Basiliensis* **18**(I), 187–95.

Narr, K. J. 1954. Formengruppen und Kulturkreise im europäischen Paläolithikum. *Berichte der Römisch–Germanischen Kommission* **34**, (1951–1953), 1–40.

Narr, K. J. 1966. Archäologie und Vorgeschichte. In *Sowjetsystem und demokratische Gesellschaft – eine vergleichende Enzyklopädie*, C. D. Kernig (ed.), Vol. 1, 369–86. Freiburg: Herder. (English edn: Archaeology and prehistory. In *Marxism, communism and Western society. A comparative encyclopedia*, 161–70. New York: Herder & Herder (1972).)

Narr, K. J. 1981. Struktur und Ereignis: einige urgeschichtliche Aspekte. *Grenzfragen* **11**, 35–61.

Narr, K. J. 1985. Kulturelle Vereinheitlichung und sprachliche Zersplitterung: Ein Beispiel aus dem Südwesten der Vereinigten Staaten. In *Studien zur Ethnogenese*, 57–99. Opladen: Westdeutscher Verlag.

Otto, K. H. 1953. Archäologische Kulturen und die Erforschung der konkreten

Geschichte von Stämmen und Völkerschaften. *Ethnographisch–Archäologische Forschungen* **1**, 1–27.

Renfrew, C. 1976. *Before civilisation: the radiocarbon revolution and prehistoric Europe*. Harmondsworth: Penguin.

Rüsen, J. 1977. Historik und Didaktik. Ort und Funktion der Geschichtstheorie im Zusammenhang von Geschichtsforschung und historischer Bildung. In *Geschichtswissenschaft. Didaktik – Forschung – Theorie*, E. Kosthorst (ed.), 48–64. Göttingen: Vandenhoeck & Ruprecht.

Sangmeister, E. 1967. Methoden der Urgeschichtswissenschaft. *Saeculum* **18**, 199–245.

Sangmeister, E. 1973. Zur relativen Chronologie des Neolithikums in Südwestdeutschland und der Schweiz. *Germania* **51**(2), 387–403.

Schwerin von Krosigk, H. 1982. *G. Kossinna. Der Nachlaß – Versuch einer Analyse*. Offa-Ergänzungsreihe Vol. 6. Neumünster: Wachholtz.

Shennan, S. J. 1978. Archaeological cultures: an empirical investigation. In *The spatial organization of culture*, I. Hodder (ed.), 113–39. London: Duckworth.

Smith, A. D. 1981. *The ethnic revival in the modern world*. Cambridge: Cambridge University Press.

Smolla, G. 1979–1980. Das Kossinna-Syndrom. *Fundberichte aus Hessen* **19/20**, 1–9.

Smolla, G. 1984–1985. Gustaf Kossinna nach 50 Jahren. Kein Nachruf. *Acta Praehistorica et Archaeologica* **16/17**, 9–14.

Smolla, G. 1986. Rezension von Schwerin von Krosigk 1982. *Germania* **64**(2), 682–6.

Spindler, P. 1983. Das Ethnos-Problem vom Standpunkt der Humanethologie. *Mitteilungen der Anthropologischen Gesellschaft in Wien* **113**, 1–3.

Sprockhoff, E. 1930. *Zur Handelsgeschichte der germanischen Bronzezeit*. Vorgeschichtliche Forschungen 7. Berlin: de Gruyter.

Stampfuß, R. 1935. *Gustaf Kossinna, ein Leben für die deutsche Vorgeschichte*. Leipzig: Kabitzsch.

Studien zur Ethnogenese 1985. Abhandlungen der Rheinisch–Westfälischen Akademie der Wissenschaften, Vol. 72. Opladen: Westdeutscher Verlag.

Torbrügge, W. 1959. Die Bronzezeit in Bayern. Stand und Forschungen zur relativen Chronologie. *Berichte der Römisch–Germanischen Kommission* **40**, 1–78.

Trigger, B. G. 1978. *Time and traditions. Essays in archaeological interpretation*. Edinburgh: Edinburgh University Press.

Trigger, B. G. 1980. *Gordon Childe. Revolutions in archaeology*. London: Thames & Hudson.

Trigger, B. G. 1984. Alternative archaeologies. Nationalist, colonialist, imperialist. *Man* **19**, 355–70.

Ucko, P. J. 1983. The politics of the indigenous minority. *Journal of Biosocial Science Supplement* **8**, 25–40.

Veit, U. 1984. Gustaf Kossinna und V. G. Childe. Ansätze zu einer theoretischen Grundlegung der Vorgeschichte. *Saeculum* **35**, 326–64.

von Uslar, R. 1955. Über den Nutzen spekulativer Betrachtung vorgeschichtlicher Funde. *Jahrbuch des Römisch-Germanischen Zentralmuseums Mainz* **2**, 1–20.

von Uslar, R. 1961. Germanische Bodenaltertümer um Christi Geburt als Interpretationsbeispiel. *Jahrbuch des Römisch–Germanischen Zentralmuseums Mainz* **8**, 38–65.

von Uslar, R. 1965. Stämme und Fundgruppen. Bemerkungen zu 'Stammesbildung und Verfassung' von R. Wenskus. *Germania* **43**, 138–48.

Vossen, R. 1969. *Archäologische Interpretation und ethnographischer Befund. Eine Analyse anhand rezenter Keramik des westlichen Amazonasbeckens*. Hamburger Reihe zur Kultur und Sprachwissenschaft 1. Munich: Renner.