Commentary

The Late Lower Palaeolithic in Southern Germany. Essay on the Earliest History of Man, 1955 - 2005: A Reflection

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Abstract

The main features of this author's dissertation, completed in 1955, are compared here with the current state of scholarship. A few of the conclusions reached originally—and those following up in 1966—turn out to have remained astonishingly up-to-date. At the same time, it is now possible to provide some supplementary discussion which, though touched upon back then, was not developed in detail.

Bearing the same title as this article (Das Obere Altpaläolithikum in Süddeutschland—Ein Versuch zur ältesten Geschichte des Menschen), the full text of my "Inaugural Dissertation for the Purpose of Achieving the Doctorate" from an "Honorable College of Philosophy at the Eberhard Karl University at Tübingen", was printed by the Hamburg printing and publishing house of Auerdruck—which also produced the newsmagazine *Der Spiegel* (Müller-Beck 1956). Publication was at that time under the name Hansjürgen Müller[1]. The catalogue was published on microfilm. The oral examiners were Professors Dr. G. Riek, Dr. W. Kimmig, and Musicology Professor Dr. W. Gerstenberg, the Dean; the oral examination was held on June 30, 1955. Areas of secondary concentration for the orals were geology and ethnology.

At the graduation party on the evening of the examination in the former Restaurant Spitzberg, of Hohentübingen, Gustav Riek—generally so reticent with respect to more general questions—gave an unusual speech of great emotional depth on the future role of our increasingly world-encompassing field, which he called "Diluvial Prehistory" (Diluviale Urgeschichte). In my passport, the required line for the profession listed the somewhat altered term "Pleistocene Archaeologist." I could not have foreseen that it was this mixture of "oldest archaeology and youngest geology" which attracted attention during all border-crossing checks and, once explained in some detail, was to ease travel considerably, especially behind the Iron Curtain.

Half a century later, this dissertation provides an opportunity for a few comments and reflections intended to gather together some far-flung remarks made over a span of decades. The problems, which remain gratifyingly current, pertain to the transition to *Homo sapiens sapiens* in southern Germany which is still very scantily represented in terms of skeletal remains (these are, however, overly emphasized). The Mount Carmel studies of the late 1930s show this transition to be clearly coeval with the Eurasian Neanderthals. From the start, this topic has engendered ever new speculative theories (e.g., Rust 1942).

The discussions following such speculation always attract widespread interest. But the interpretive possibilities of the much more numerous, and synchronous, stone implements manufactured at the time as immediate primary products of human action continue to receive far less attention. Nevertheless, during the last decade all of the media (including finally, *Der Spiegel*) have at last acknowledged that Neanderthals—in addition to other still neglected synchronous variants of the genus *Homo* in Africa and East Asia—were definitely already very accomplished "other" humans, no matter whether taxonomically classified as *Homo sapiens,* merely as *Homo neanderthalensis*, or even just as *Homo primigenius*. The last is the oldest nomenclature, thus holding the greatest "validity", and may actually be seen as a highly honorific name. In fact, it might even be advisable to revive this old term 150 years after the eponymous remains were discovered. From the standpoint of universal history, this would not harm my tendency to upgrade the Neanderthals as a sub-species of "modern" humans. They would then stand opposite "us"—whatever that may mean (Ickerodt 2005; Porr 2005)—ranking as a species still capable of interbreeding, judging by the indubitable testimony of identical basic cultural behavior based on the stone artefacts.

In terms of universal history, this frequently disproportionate anthropo-Darwinist "human discussion" is of rather secondary significance. After all, the stone implements speak for themselves, and it is of limited interest what their manufacturers actually looked like, unless there is a desire to cling to the outmoded ideas of funda-

1. It was only after marriage to Katharina Beck from Bern that our joint name, Müller-Beck, was officially registered in Hamburg in 1955.

mental qualitative differences. The latter could only be deduced from correlatable archaeological data, never in the past nor in the future from the skull configuration of one individual who might have been responsible for the manufacture of these implements. There is even the unanswered question—as now on Flores or in the case of African *Zinjanthropus*—whether the discovered individuals were really the manufacturers of the archaeologically assessable finds, although that changes neither the objects nor the story they tell. I therefore ask the reader to forgive my continued failure to be greatly interested in the physical appearance of the people who stand for the Late Lower Palaeolithic. Beyond the borders of southern Germany, judging by archaeological classifications, the era's inventories—varying over time and space—surely are accompanied not only by *Homo sapiens neanderthalensis* (or *Homo primigenius*) individuals, but also by representatives of late *Homo erectus* (when Middle Pleistocene Bilzingsleben is included) and, indeed, of early *Homo sapiens sapiens* in the same strata. Nevertheless, no anthropologist has yet been able to present a Neanderthal found in the same horizon with a "Post-Neanderthal". This plainly means that they are definitely populations of different morphology, though culturally connected along a comprehensive but regionally quite variable continuum. More detailed assessments will have to await the presentation of new discoveries (Soressi 2004; Stewart 2005; Straus 2005a, b).

Following the custom in archaeology, and the sole determinant for prehistoric interpretation, any such assessment will depend on the discovery context. Anything else remains speculative, especially when also weighted down by attempts at correlation based on statistically uncritical evaluations and complex laboratory techniques. While such attempts may be "interesting", as is being asserted in the more intellectual press (Lüthi 2006[2]), they can by no means evade archaeological facts. Moreover, in the case of the Late Lower Palaeolithic in southern Germany down to the more recent discoveries at Stuttgart-Cannstatt-Bunker (Wagner 1995; Schatz 2003), i.e., in the full Holstein Interglacial, we actually have a time span of well over 400,000 years—and hence a family tree of more than 40 x 10,000 years with at least 1600 participating generations, provided all partners remained faithful to one another throughout their procreative time and there were no losses due to the woman's death in childbirth. Assuming the analogy of current and historic hunters' populations, there were more probably two or three times as many encounters of couples, thus roughly a magnitude of 3000 to 5000, contacts achievable—subject to the rule established by Luther.

In spite of more recent field research, skeletal remains categorized as Neanderthals have remained a rarity in southern Germany. In addition to a confirmed atavistic femur (Hohlenstein-Stadel in the Lone Valley) and a baby tooth which has now vanished (Klausennische near Essing), both of which were known in 1955, there are: two baby teeth and remains of an evidently buried fetus from the Sesselfels Cave (Rathgeber 2006) and a single tooth from the cave at Hunas (Alt et al. 2006). All of these appear in a stratigraphically indubitable context with stone implements of the Late Lower Palaeolithic (in which I expressly included the emergent "Middle Palaeolithic" in 1955, giving detailed reasons). Such a context has not been established for the disarticulated skeleton imbedded in red chalk found in the Mittlere Klause near Essing, which I recorded. However, it cannot be definitely ruled out without a major series of new datings. More on this later.

The first comment that I would like to make about the dissertation itself regards its title: It retained the then commonly used term "Altpaläolithikum" (Lower Palaeolithic), while also, for chronostratigraphic reasons, specifying a "Late Lower Palaeolithic", although the term "Middle Palaeolithic" in a narrower sense had already been introduced by F. Bordes. Even today, there is no unanimity on the question where such a boundary should be placed. Bordes (1954) assigned it geologically to the lower boundary of what was then defined as the Riss-Würm Interglacial after A. Penck. At least this is now accepted internationally as the lower

2. Among other things, she writes, "However, these insights have only limited value for clarifying genetic identity. For example, the information according to which the (HMB: one) primordial mother came from Syria may be interesting. But about (HMB: only) 400 generations have passed in the last 100,000 years, each experiencing a new mixing of the genetic endowment. But the heredity test can only trace back along two branches (HMB: maternal mitochondrial DNA and paternal Y chromosome); the whole rest of the tree, and thus the genetic contribution of many thousands of ancestors, is disregarded."

boundary of the Upper Pleistocene in biostratigraphic terms—reduced to the Eem Forest Period and the beginning of Oxygen Isotope Stage (OIS) 5. In contrast with Bordes' categorization, arguments of artefact morphology are now frequently used to place it in the upper zones of the Middle Pleistocene. In continental sequencing, this frequently does not permit assignment of find localities to the phases of the Riss or Saale "Ice Age Complexes" and to the preceding "Great Interglacial" (now called Holstein Complex) in a generally reliable, uncomplicated, globally comprehensive, and chronostratigraphically certain manner. Therefore, the term "Oberes Altpaläolithikum—Late Lower Palaeolithic" might still be very viable, being also more open-ended than the denomination "Middle Palaeolithic" with its only vaguely tangible foundation. It was completely clear to me in my research that a dynamically clearer boundary line, such as the subsequent, fully evolved classical Upper Palaeolithic, would have to await better evaluation of its beginnings.

My second direct critical comment regards the expression "of Man" in the subtitle. Regrettably, it represents a linguistic focus upon a too-narrowly-conceived unifying concept not adequately reflected by me at the time; today, we neither would, nor should, so view the genus *Homo* with its initially so clear temporal and regional specifications—even in terms of cultural history. Above all, such a unifying concept prejudices the discussion about the contemporaneity and succession of human types. It was chiefly M. Bloch—whose thoughts were all too soon interrupted by the brutality of the German occupiers and whose reburial in the Pantheon of Paris is probably imminent—who elucidated for all time that we, the palaeohistorians, should always speak of "humans" in their multiplicity. It is only the latter usage that gives cultural space to each individual helping to shape history, and thus his true uniqueness and historic freedom.

Nevertheless, the very brief original preface provides a better introduction to the structure of the dissertation than I had remembered. I was really concerned with all available sources of palaeohistoric significance regarding my topic—in spite of the concomitant firm restriction to the Lower Palaeolithic excavation horizons in the more immediate South German Basin, not including the administrative district of Karlsruhe. This fact also indicates the orientation of archaeological work in those days towards the structures of preservation of antiquities, which controlled access to funding then and is still accountable for it now. Because of the absence of regulations, these various excavated objects had, in the immediate postwar chaos, been at the "private" disposal of the excavators, who had by no means settled their 12-year-long dispute with the unbending life-long proponents of a "Greater Germanic Era"—whose very real, and, until recently well-concealed threat was only exposed by historical research in the last decade. Implicated as well is the fact that the historical preservation agencies in the German states structured the entire organization of prehistory and early history, more rarely protohistory, as a continuation of an old buddy system, since the latter controlled the major part of all available public funding for the profession. In my case, the catalogue could better have been printed in two sections—one in Baden-Württemberg and one in Bavaria—but it also contained—for good reasons that were amply explained—too few illustrations for the expectations of that time.

What had originally triggered my dissertation plan was my intense interest in Oswald Menghin's 1931 "Palaeolithic World History". Before my first year at Heidelberg University in 1950 I was asked by a doctoral candidate at Marburg University, H. Mandera, to excerpt that work in order to aid his preparation for his oral doctoral examination to be conducted by G. von Merhart. The latter regretted throughout his life—as he once told me on a field trip to Veringenstadt—that he was never able to do palaeolithic field research after 1914. Added to that was my frequent contact in Ahrensburg after 1952 with A. Rust, the excavator of Jabrud, during his research in Mauer. Of particular importance, as well, was my inventory, compiled while I was research assistant to H.-G. Bandi in Bern, of the material from the Mount Carmel excavation (cf. Garrod and Bate 1937), which had been given to the Bern Historical Museum by D.E. Garrod (Müller-Beck 1955). This was a work which had to be done in the form of a card catalogue. After some corrections, it was then incorporated into the official inventory ledger of the BHM (Inv. No. BHM

35501-36422) by the person who later became my wife.

Obviously, it then made good sense for me to plan a spatially extensive inventory of material in order to make a scholarly contribution to the earliest prehistory of our own region. Source materials in southern Germany were adequate to the purpose, and G. Riek—ideally combining expertise in geology, palaeontology, and archaeology—was willing to advise me most liberally in my undertaking. It is true enough that he would occasionally exclaim "Müller, you damned lawyer," because of my admittedly rather mathematical eagerness to define things, which seemed to me a matter of urgent necessity. Yet, his critical remarks were most helpful in creating definitions. Moreover, he took it for granted that, in addition to artefacts, I was to include any other available objects and conclusions. The roots of this method of operation lay in my basic studies in Heidelberg from 1950 to 1952, my professors there having been palaeontologist L. Rüger, prehistorian E. Wahle, and cultural sociologist A. Rüstow.

In Tübingen, I was fortunate in my contact with H. Graul, the very active geomorphologist (who was simultaneously a farmer implementing modern techniques). Graul would go about southern Swabia (Oberschwaben) on his little motorbike mapping and interpreting the ice advances and terraces with the utmost care; and he would confront his usually rather small number of students with the newest problems of quaternary classification. In the years following my dissertation, in many a discussion, along with K. Brunnacker, whom I first met while taking inventories of materials in Bavaria, Graul would encourage my wider attempts at stratification.

At the time, I inventoried 33 sites in Baden-Wuerttemberg and 35 in Bavaria. They ranged from single objects to stone inventories exceeding 1000 items. I eliminated six sites in Baden-Wuerttemberg and 16 in Bavaria which turned out to be of uncertain Lower Palaeolithic origin (for consistency, I choose not to use the term "Middle Palaeolithic" in these comments). My own recording of archaeological inventories required two steps. The first was an initial general inspection of all discovery horizons accessible to me, during which a heavy motorcycle was of invaluable assistance. Upon development of the system of categorization—based primarily on our collection in Tübingen with its significant sites—there followed stage two, the inventory of all objects considered in the study, i.e., approximately 9000 (not including the occasional flakes of type 46, subsequently more appropriately characterized as category 46). Only about six months were left to do the latter. The procedure proved to be amazingly efficient, not least of all owing to preceding work published by a variety of authors, and most especially to direct contacts with A. Rust, H.-G. Bandi, G. Riek, and F. Bordes, all of whom were interested in these regions. At the recommendation of F. Zeuner, Bordes made a special trip to Tübingen from Paris in order to explain to me his typological system (Bordes 1953), only outlines of which had been published at that time. As a result of this contact with southern Germany, he included in his Mousterian schema the leaf-shaped points defined by A. Bohmers (1951). What I still needed to do, however, was to determine acceptably certain boundaries for distinctive features, metrically defined whenever possible, between the nominal categories ("types"). This was basically a spherical-geometric system, as it might more graphically be called (Müller-Beck 1983a), and it has been developed in a particularly complex fashion for categorizing the numerous variants of hand axes (by Lopez Junquera 1980 in great detail, for example). My purpose was to ensure an equal approach to all of the collections, including the flakes of 1 cm maximum size (type 46) that eluded further characterization but were nevertheless distinct from fragments (type 45) by the presence of recognizable traces of flaking. These were therefore abstract categories which could often not even be clearly classified on the basis of subsequent explanatory illustrations by F. Bordes (1961) or G. Bosinski (1967).

As a result of this abstraction, selective illustrations of major series seemed to me to make little sense. Therefore—with perhaps an excess of consistency—I only documented each type with one original drawing. One thing was certain, as in all biological categorization, hardly any item among the Lower Palaeolithic artefacts was identical in all regards with any other. This was bound to be the case because of the dependence on random shapes arising from the modification of basic shapes—in contrast to the

Upper Palaeolithic blade industries. This, indeed, is one of the most important principles distinguishing the Lower Palaeolithic over its hundreds of thousands of years from the entire global Upper Palaeolithic, which encompassed a mere "short" forty thousand years.

This resulted in relatively clear categories, such as the important "atypical scrapers" or the frequent "directional flakes," in addition to the much more rarely found genuine "blades". Moreover, the frequency of collected flakes definitely indicated the care taken during each excavation. F. Bordes (1953) included in his system none but all obviously retouched artefacts (i.e., those definitely showing secondary modification). In the rich Murg site (Rogg/Michel brickyard), his system allows only 222 artefacts to be included, omitting the used and unretouched flakes (type 31:156, type 32:289) and fragments (type 44:64) included in my inventory which make up over half of the overall number of items and are of definite importance. The same is true for the 267 flakes (type 46) which clearly reveal the careful fieldwork of all involved, especially Emil and Egon Gersbach as well as G. Kraft, and hence the great value of the site complex.

As the catalogue text reveals, I was in no doubt whatever that I was simply dealing with combinations of morphological-geometrical features—including "scraper edges", "scrapers" or "denticulates" as well as "retouched flakes"; taken as collective terms, they definitely did not rule out possible "cryo-retouching"—then, as even now, not always clearly distinguishable. It seems that my system—more complex, consciously descriptive, and not intended to be functional, in spite of the terminology which was then, and is today, in common use—was too complex for its day and was not widely accepted. An added factor was that it could not be evaluated with edge-punched cards, as I found out when revising the system for publication about 1965. Meanwhile, thanks to the rapid development of information processing, the situation has changed. It is now quite conceivable that my old approach, due to the morphometric differentiation which it provides, may yet prove to be quite fruitful for the current evaluation of the archaic-tradition stone implement inventories just excavated on Cuba in 2006.

For the time being, then, G. Riek has been proven right: Nobody wants to know that much detail—even though there has been growing awareness that, indeed, these are definitions of shape, only partially determined by function, which reveal the scope of their ever changing application when examined in detail. The hafted items found in the Swiss moist-soil Neolithic and the Arctic Palaeo-Eskimo cultures taught me years ago that it would be better to arrange my system into classes according to production steps, as is now increasingly the case. This would also reveal more clearly that, in addition to the three-dimensionally designed "bifaces" (hand axe variants and leaf shapes), the entire "Late Lower Palaeolithic" abounds in geometrically definable, post-adapted "potential" tool edges occurring in variable but carefully selected flaked shapes and diverging in their modification according to the duration and intensity of their use. This becomes particularly evident during analysis of three-dimensionally designed implements, such as the hand axes, as already performed by G. Albrecht (1994) on the Sehremuz inventory. However, such analysis has not attracted much attention so far.

For each major inventory, the catalogue illustrated the distribution of typological frequencies by means of simple block diagrams by percentage ratio, each broken down into three separate illustrations: a) Overall distribution of ratios by type; b) Overall ratio of tools, flakes, and cores; c) Distribution of tool frequency.

Comparison of these major inventories resulted in a type-ratio-based distinction between four morphological groups which, thanks to observations of collections of Vogelherd (BW 27), Weinberg Caves (B 22), and Sirgenstein (BW 33) lent themselves to relative stratigraphic sequencing: I. Late Lower Palaeolithic with flat cores; II. Late Lower Palaeolithic with hand axe scrapers, clearly subdivided into broad-backed hand axe scrapers, and thin-backed hand axe scrapers and hand axes with thin top edges; III. Late Lower Palaeolithic with leaf-shaped points, tentatively subdivided into leaf-shaped points with variable cross-section, leaf-shaped points with only D-shaped cross-section, and large blades; IV. Late Lower Palaeolithic with scrapers and an increasing ratio of blades.

Above all, what thus became clear beyond a

doubt was the tendency for bifacially shaped tools to become thinner, the periodic increase in hand axe scrapers and, along with the disappearance of the late hand axes, the increasing frequency of leaf-shaped points and elongated heavy blades. None of the inventories include cores with oval platforms so typical of the Aurignacian. Cores are limited to those with broad surfaces from which blades were removed and a narrow prepared platform on flat cores.

The only firmly indentified bone implement found was a point in horizon VI of Vogelherd (BW 27), the stone implements of which still belong in a very late Lower Palaeolithic period. A recent direct dating of this point resulted in an uncalibrated ^{14}C age of merely 31,310+240/-230 years (Bolus and Conard 2006). It is therefore more likely to be from the Aurignacian of stratum V in the overburden and had either intruded into horizon VI or had been incorrectly assigned to the stratum during the rapid excavation. Other bone items identified as artefacts are a bone fragment from Sirgenstein (BW 33), definitely retouched along one longitudinal edge, and a chip off a horse's nose that was probably used from stratum VII of Vogelherd (BW 27). It is likely that there were other intentionally modified bone implements or used "auxiliary bone implements" without pronounced preparatory modification, as described by H. Obermaier, which were overlooked. D. and U. Mania (1997) have since shown how great the inventory of such briefly used and often only slightly altered bone implements really is.

For 35 of the sites, the remains of fauna found there had also been classified and published. There was generally little differentiation among them, and it therefore did not permit the definite stratigraphic classification I had hoped for. The represented species ranged from forest elephant (*Elephas antiquus*), deer, and moose to musk oxen and saiga antelope. There were frequent occurrences of mammoth and rhinoceros (Kahlke 1994) including the occasionally confirmed snow hare and ice fox. Reindeer occur in 21 sites, frequently in the company of red deer but occasionally found alone. The reindeer's skin was probably an ideal material even then for winter clothing and tents or as a cover for huts. The red deer is an indicator of more or less extensive forest cover. Relatively large horses are also a frequent occurrence, as is the cave bear, which is found in practically any cave inventory. Cave hyena and Felides are far more rarely encountered.

Beyond a doubt, these discoveries prove that the people who represent the South German Late Lower Palaeolithic hunted a broad spectrum of fauna which varied during the Middle and Upper Pleistocene in the course of climate development. Interglacial fauna can only occasionally be recognized, especially because of the rarity of sedimentation during these phases—almost exclusively travertine; only in horizon IX of Vogelherd (BW 27) was the discovery of a forest elephant's baby molar accompanied by basal cave debris. The vast majority of hunted fauna is part of the "mammoth/woolly rhinoceros complex" (Kahlke 1994), which represents the cooler and cold steppe phases of the Middle and Upper Pleistocene in more northerly Eurasia with its greater sedimentation dynamic. On occasion, elements of more open forest zones increase in frequency, with aurochs, giant stag, and even deer occurring. The steppe zones of grass and herbs are evidenced by horses of the type *Equus germanicus* and, later, by *Equus f. przewalskii* in Haldenstein Cave (BW 30). The saiga antelope, which is another quarry, likewise indicates the advance of dry-cool steppes toward the west. The reindeer occurs with particular frequency and is likely to have been a principal prey species, which proves with great certainty that the representatives of the South German Late Lower Palaeolithic were quite capable of surviving in subarctic and arctic conditions. The association of their artefacts with unquestionable examples of musk oxen (skulls) in the Danube Valley near Regensburg (B 31) indicates this even more clearly. Caves used by cave bears are by no means avoided by humans, while artefacts are less frequently found in caves also used by hyenas.

Meanwhile, reevaluation of fauna found in association with Late Lower Palaeolithic inventories in northern Germany under relatively good taphonomic conditions—including evidence of systematically placed slash marks—has furnished unquestionable proof of definitely planned hunting. At the Salzgitter-Lebenstedt tundra site—in the older Würm/Weichsel—reindeer were the principal prey, as they also were very frequently in southern Germany

(Gaudzinski 1998). In Taubach, a forest site dating from the older Eem or possibly from a preceding warm phase, animals hunted and, most likely, intensively used, were: wood rhinoceros, brown bear, bison/aurochs, stag, and beaver (B. Bratlund 1999).

While plant remains are found far more rarely in the context of stone implements in southern Germany—in only five Late Lower Palaeolithic sites—they do confirm the picture of ecosystems gained from the faunal remains. In Murg (BW 15) wood remains of pine and mountain elm were found—definitely boreal forest elements. In Stuttgart-Untertürkheim/Biedermann (BW 29), the artefacts are in association with warm forest flora. In the Peters Cave (B 16) *Pinus silvestris* and *Taxus baccata* likewise indicate a still relatively warm forest steppe. The same holds true for the northern oak from the Lower Palaeolithic horizon of the Ofnet (B 17), which may well have survived all of the Middle Pleistocene and possibly even the Upper Pleistocene cold phases in the gallery forests along the south German portion of the Danube. This gallery forest is also documented in the vicinity of Weinberg Caves (B 22), thanks to the pollen profile established in the valley. There, in a relatively narrow horizon, the proportion of warmth-loving trees rises to about 5%, revealing a mixed oak forest with oak, elm, alder, and hazel. In the stratum above, the relative proportion of fir increases temporarily, while pine, a boreal element, maintains its high percentage for a relatively long time. It is only in the overlying loesses that the proportions of tree pollen decrease significantly. What I did not realize at the time is the fact that the last full interglacial is not documented there, but merely an optimal wooded phase above the Lower Würm, as we discovered during follow-up investigations there (von Koenigswald et al. 1974; Brande 1975; von Koenigswald and Müller-Beck 1975).

As mentioned above, human skeletal remains are extremely rare in the context of Late Lower Palaeolithic artefacts. Moreover, the femur found in Hohlenstein-Stadel (BW 1) is not particularly characteristic. On the other hand, the skeleton from Mittlere Klause in Essing (B 8), unquestionably that of a *Homo sapiens sapiens* and manipulated with unusual thoroughness in a secondary burial (Gieseler 1953), is of some interest. According to the excavators, it was to be assigned to a sediment horizon in association with artefacts belonging to our morphological group IIb; however, at the time, the latter group was more closely associated with the Solutrean, not least of all because of the very fact that the skeleton was found there. It was expressly ruled out that the skeleton belonged to other strata also present in the cave and containing Upper Palaeolithic items. Meanwhile, a ^{14}C dating of approximately 18,500 BP has been obtained (Terberger and Street 2002), but it should be rechecked by serial re-measuring, as it postulates a time level that has been archaeologically undocumented so far in southern Germany. At the time, I could not definitely rule out the possibility—and cannot do so even today—that typical specimens of *Homo sapiens sapiens* do already occur as early as the later phase of the Late Lower Palaeolithic. What gives me pause to this day, above all, is that this type of secondary burial is not documented anywhere for the Upper Palaeolithic and could easily be assigned to the Lower Palaeolithic. Nevertheless, I had to state then: "In spite of the above arguments, it has to be emphasized once again that it is no longer possible to make a decision regarding the provenance of the skeleton and that our ability to interpret it has been completely eliminated in all respects. We considered ourselves duty-bound to point out the possibility of such an assignment."

In overall stratigraphic terms and in accordance with current Quaternary subdivisions, the described inventories of the Late Lower Palaeolithic in southern Germany extend over an imprecisely assessable portion of the Late Middle Pleistocene and the entire Earlier Upper Pleistocene. It remains uncertain how far back they reach. It is likely that the objects found in Stuttgart-Cannstatt-Bunker, with their location in the lower Holstein Complex, have provided clarification in this regard (Wagner 1995; Schatz 2003). Back then, before the first ^{14}C dating became available for the middle Upper Pleistocene, I did not realize that the gradual temperature increase after the "Lower Würm"—to which the bulk of the finds under consideration belonged, and which I named "Spiezer Fluctuation" in a considerably simplifying manner— actually lasted longer. Shortly afterward, it turned out that this phase, which I then called an Aurignacian Fluctuation in accor-

dance with W. Soergel's model, comprised a complex and repeatedly fluctuating climate phase of over 10,000 years. We have learned since that the end of the South German Late Lower Palaeolithic does extend into this phase, which is now regarded as part of OIS 3.

As mentioned above, the region under study was limited to the South German Basin proper, as a distinct landscape excised from the wider region of southern Germany so designated by Gradmann in 1931. Our terrain is bounded by the Alps, Black Forest, Neckar hill country in the Kraichgau, Odenwald, Spessart, and Rhone; in the north by the Thüringer Wald and Frankenwald up to the Fichtelgebirge; and in the east by the Böhmerwald and Hausruck. Access was possible as climatic conditions permitted and was provided by the Upper Rhine in the southwest, by the Danube near Passau in the southeast, and—probably of lesser relevance for the era under study—the Kraichgau portion of the Neckar River in a northerly direction. The basin is divided into two nearly equal parts by the Schwäbische Alb and the Fränkische Alb. During warm periods, the dense forests would have been rather difficult for gatherers and hunters to traverse. On the other hand, along the larger rivers broad meadows and old water courses were very productive, providing ample opportunity for fishing and for gathering mollusks. By contrast, decrease in mean annual temperatures improved accessibility by thinning the woods, thus widening the available terrain for gathering and hunting as well as the variety of usable biotopes at different altitudes all the way up to the alpine valleys (Andrist et al. 1964). Only in fully developed cold periods were the level plains south of the Alb accessible—earliest and easiest from the southeast—but surely to climatically adapted groups of people only.

Considering the sources available to me at the time, I decided that the most responsible approach was to attempt to assign the inventories to temporally distinct occupations of humans in the South German core basin analogous to the four morphological groups based on the stone implements. I completely avoided definition of any "cultures", as the term itself—proposed again and again then, as well as later on—is in itself problematic and yields little in ecological respects. However, I did not justify this decision in detail. Any archaeologist and prehistorian familiar with the way theory is shaped in our discipline can easily realize today why I recoiled from such definitions. (For example, the term "Kulturkreislehre" [the "Doctrine of Cultural Groups"] coined by the Viennese School still had currency then.) My categorization of "cultural behavior" (an open and viable term used to sum up human activity) in the South German Late Lower Palaeolithic was paired with climatological evidence, which could be evaluated by references to multifaceted sedimentary sequences at the more thoroughly investigated sites. It was often found that the find zone under examination, or even several of them, lay at the base of a subsequent cooling period. This observation can now be readily explained both for loess and for debris and gravel sequences (Welten 1982). Greater quantities of finds are conserved in usually thicker and rather rapidly deposited colluvial wind deposits at the base of colder layers and above accumulation disconformities recognizable as palaeosols or erosive discordances. These greater quantities more frequently include archaeological horizons or their remains, as in Vogelherd (BW 27). The task at hand, was to assign each sequence from these levels of morphological groups and each pertinent assessable climatic period to the most likely South German quaternary classification for the era—a process that can be deduced from my text but which I did not explicitly discuss. I regret my decision in hindsight, especially because it unfortunately is a typical shortcoming of dissertations caused by the fact that the author knows his work in detail but often fails to make the extra effort to facilitate insight into his arguments for readers less familiar with the problems. This becomes an even more critical factor when a reader—as is commonly the case—skips through the text and therefore has a hard time following the arguments.

At the time, my sequences in the Middle Pleistocene were distributed over the phase advancing toward the Maximal Riss and the incipient fluctuation between the Maximal Riss and the Younger Riss (Graul 1952). The latter is now classified as OIS 6, 7, and earlier. In principle, it was then easier to proceed in the Upper Pleistocene, with the Riss-Würm Interglacial at the base, the still poorly understood details of Lower Würm classification (Brunnacker 1954),

and the principal fluctuation (table p. 45[3]) with its as yet unknown duration. These terms—which remain difficult for non-specialists even now—turn up in my text but, because of their merely local use and lack of general acceptance (as, for example, in Büdel 1951) were replaced in the aforementioned table with the more open and more neutral but cumbersome system of F.E. Zeuner (1952). Later, when the first ^{14}C data became available, comparative work beyond the boundaries of southern Germany resulted in a chronostratigraphic classification for the European and American Upper Pleistocene (see Müller-Beck 1966) which closely resembled today's classification.

The first human occupation in the South German region seemed to have occurred near the end of the "penultimate warm summer period", i.e., in the still elusive advance phase of the Riss/Saale Ice Age. Because of its relative proximity to Steinheim (Müller-Beck 1983b), I proposed it was possibly connected with pre-Neanderthals. As this occupation ended before the culmination of the glacial stage, I designed a brief description of landscape development during this protracted climatic deterioration to suggest possible escape routes. Although this model left open an escape route toward the southeast across the Danube, it seemed to allow for withdrawal in a southwesterly direction for a longer period. As a metaphor for my escape model of the roaming bands, I chose the overlapping wave graph used by Ebbe (p. 47).

I assigned the second occupation, a briefer episode, to an interstadial "of higher order" between the Riss Maximum and the Younger Riss—a climatic phase that is even now quite elusive but evidently does exist during the uppermost Middle Pleistocene prior to the Eem, possibly in the site of Neumark-Nord (Mania et al. 1990). It would, therefore, need to be integrated into the top portion of OIS 6 or be defined as OIS 5"f".

At the time, my model (as we would say now) showed the third occupation—including morphological groups IIa and parts of IIb—occurring at the end of the Riss glaciation, at the transition point to the last full warm period, with a first advance across the upper Rhine. In addition to minor inventories, which I wanted to assign hypothetically to "individual families", more complex sites existed which might possibly represent the activities of larger groups. Beyond these rudimentary social concepts, showing activity differences or the existence of primary and secondary camps was out of the question at that time. However, later it was possible to frame a model in the upper Danube (Müller-Beck 1988). At our excavations of the arctic Palaeo-Eskimo site of Umingmak (Müller-Beck 1977), using analogies with the ethnohistoric Neo-Eskimo sites and given the good preservation conditions and complex evaluation of fauna, the classification method proved entirely clear. At best, differences in camp types and activities are revealed by secondary discoveries within larger, well-documented sites.

I reached firmer stratigraphic ground, even for that time, in the study of the beginning of the fourth occupation in the last great warm phase, i.e., the Eem. This was through the means of pollen analysis, though it was poorly assable because of the relatively low density of finds. By comparisons to discoveries in the west and north of southern Germany, palaeoanthropologists concluded that these humans were Neanderthals, albeit with extremely specialized skull configuration and highly characteristic atavistic features. They were considered even more atavistic than earlier humans, which prompted me to make the following comment (p. 50): "This is a view now generally accepted. But it gives one pause regarding the value of anthropological conclusions in view of previously accepted understanding." In my opinion, this fourth occupation resulted in continuous habitation until the transition to the last cold period of the lower Würm whose complex classification had not yet been understood. Above all, there was no concept of the massive cold OIS 4, which, however, did not necessarily result in the complete withdrawal of humans from southern Germany, as its maximum cold phase was probably rather brief.

I then correlated the fifth occupation with the protracted climatic deterioration during the as yet inadequately undifferentiated Würm advances. There were other groups of humans who entered from the east the lowland meadows which were still surrounded by forest. I did not comment on the question of whether initial

3. This page number, and all subsequent ones, refer to my dissertation pagination.

small numbers of these climate-resistant eastern people had reached southern Germany earlier, i.e., during cooler steppe phases. I felt that dramatic clashes with the "native" groups were rather unlikely because of the expanding exploitability of the landscape. It remained unclear to what degree the first groups in the fourth occupation had participated in the rise of these new developments in the east. Moreover, it remained an unanswered question to what degree these occupations, which entailed the younger leaf-shaped point inventories, also involved populations advancing from the south into the regions east of southern Germany. Today, the warmer phases of OIS 4 and OIS 3 could be claimed for this purpose. Overall, the model is therefore considerably more complex and intertwined. Due to the lack of isolation of groups in our region and the resulting contact, the humans must have experienced accelerated development of "modern" features of their skeletal morphology. This intertwining already corresponded to the migration of fauna types adapted to colder and drier exposed steppes, coming from the east as did, for example, the saiga antelope and musk oxen, during colder climate phases. In warmer phases, on the other hand, forest animals advanced, with the forest elephant (*Elephas antiquus*), for example, reaching Warsaw. I attempted to deduce the apparent greater mobility of these steppe hunters, compared to the native forest hunters who had remained in place ever since the fourth occupation, from their long acclimatization to the colder conditions of the northeast. In terms of today's chronostratigraphic insights, the forest hunters had actually persisted through at least 60,000 years (from the Eem to the upper segment of OIS 3), as we suspected at the time. That is twice as long as the 30,000-year span of the Upper Palaeolithic. As a consequence, of course, there was a possibility that these new immigrants from the east who came during the cooler steppe periods, recently postulated by W. Weißmüller (1995) in the Sesselfels Grotto, may have been post-Neanderthals. How early they came, and whether by this time, can only be proven by future discoveries. At any rate, the artefacts themselves permit this interpretation, as well as its ecological contingencies. In no instance do they allow the overly dramatic model so often preferred now, according to which a relatively rapid migration by the new type of humans occurred. The latter also cannot necessarily be seen solely in the context of the earliest Upper Palaeolithic. To my mind, even back in 1955, the Late Lower Palaeolithic in southern Germany—assumed for this reason to have lasted longer—encompassed not only the human type of the pre-Neanderthals next to the Neanderthal, but also a "Post-Neanderthal" coming generally from the east as early as the fifth occupation, with a still poorly understood involvement of southern influences (expressly so stated to avoid "needless misunderstanding").

The terminal phase of the Late Lower Palaeolithic extended into the major fluctuation following the Lower Würm which was indeed significantly underestimated with regard to its length and complexity. While the marine OIS 3 of the Upper Pleistocene is now well understood, it is still difficult to correlate with continent-wide findings. In my opinion, the actual end of the Lower Palaeolithic has remained obscured because of the scarcity of evidence. The breadth of the discontinuity from the well-defined Upper Palaeolithic which followed, therefore has remained uncertain—and remains so to this day.

During the Late Lower Palaeolithic, climatic change in the South German Basin resulted in a variegated, highly diverse, and historically dynamic picture—the latter thanks to the first suggestions of human migration. At the time of my research the mosaic of that era was represented by only a very few discoveries that fitted into the overall technical developments then understood worldwide. The numerous early attempts at mapping (Müller-Beck 1966, 1993) and even more recent drafts by J.K. Kozlowski (2005) cannot really do justice to this dynamism. The distributions shown there indicate only seemingly short-lived stages of development. In reality, they are gigantic summaries of data extending over tens of thousands of years, resulting in dramatic gaps such as those that arise between the maps from the beginning of and the second half of OIS 3 (Kozlowski 2005, fig. 2 and fig. 3). But, moreover, there is an almost complete lack of a critical revision of dates, even though that is now achievable and can be confirmed by at least two approaches. It is overdue as a pan-European project spanning from approximately 420,000 BP—the baseline

of the "Holstein Complex" (OIS 11)—to 10,000 BP—the end of the Upper Pleistocene (OIS 2) (Müller-Beck 2005); and it ought to comprise the entire Late Lower Palaeolithic (including the "Middle Palaeolithic", no matter how its time span is defined) as well as the Upper Palaeolithic. Such a revision is especially important because these highly incomplete mappings ultimately and obviously still determine the discussion about spatial differentiation and the transition from Neanderthals to post-Neanderthals more than do the discovered human remains themselves.

My material inventory suggested initial signs of a rather complex spiritual world—interpretable only with the utmost restraint (p. 57). They would be even more impressive if the secondary burial from the Mittlere Klause could be shown to belong to the terminal phase of the Late Lower Palaeolithic. Additional dating revisions are urgently needed in this regard, in the context of the proven burials in the entire European and Near Eastern region between 80,000 and 20,000 BP. The same is needed for the direct dating of the South German bone tip fragment and for the accompanying faunal remains from Große Grotte (Wagner 1983) which were found in a secure Lower Palaeolithic context. Test samples taken from this context after 30,000 BP would result in significant critical questions regarding the reliability of early AMS radiocarbon dating.

The past 50 years of research have vastly increased the data from throughout central Europe. Nevertheless, many of the questions raised in 1955 still await an answer. The likelihood of the veracity of my hypothetical model has grown. Evidently, this is chiefly due to its attempt to assess the complexity of the actual historic dynamism in the core area of natural and cultural central European migration routes across more than two hundred thousand years. The Danube Corridor is definitely only one of them. This proves to be the case particularly when denser forests give way to open forest steppes, rendering even secondary routes easier to traverse. Movements during the Upper Palaeolithic clearly demonstrate this fact, compared to the later constraints on early Neolithic cultural dynamics with their growing dependence on limiting climatic factors, which are more readily explained by means of ethnohistoric analogies. Still, for our field there remain some particularly difficult questions (Chrisomalis and Trigger 2004), such as the relatively late advance in America of maize growers far into the northern zone of the hunter/gatherer cultures (Wright and Pilon 2004). They survive there in close proximity to the hunter/gatherer cultures in economic respects but under definitely changed climatic and ecological conditions that determine the evolving cultural behavior. □

Translation from German: Ilse Andrews

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