Grave Shortcomings: The Evidence for Neandertal Burial [and Comments and Reply]

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Grave Shortcomings

The Evidence for Neandertal Burial[1]

by Robert H. Gargett

Evidence for purposeful disposal of the dead and other inferences of ritual behavior in the Middle Paleolithic are examined geoarchaeologically. Cave geomorphology, sedimentology, and taphonomy form the basis for a reexamination of the Neandertal discoveries most often cited in this connection: La Chapelle-aux-Saints, Le Moustier, La Ferrassie, Teshik-Tash, Regourdou, and Shanidar. Logical incongruencies are identified between the published observations and the conclusion that Neandertals were being buried by their conspecifics.

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The existence of a Middle Paleolithic culture with parallels to that of modern humans is widely accepted. Like *Homo sapiens sapiens*, Neandertals are believed to have buried their dead and performed other rituals suggesting to some that they had a concept of an afterlife or an emotional capacity equal to our own. The paleoanthropological literature contains, besides references to purposeful burial by Neandertals, many inferences of ritual behavior. For example, there are reports of nine burial mounds at La Ferrassie (Peyrony 1934), the burial of a bear at Regourdou (Bonifay 1964), stone "cysts" containing cave bear skulls at Drachenloch (Bächler 1921, cited in Kurtén 1976), and burned and broken bones suggesting cannibalism at Krapina (Gorjanovič-Kramberger 1906, cited in Trinkaus 1985). Usually these anomalous sediments are interpreted as representing some sort of humanlike consciousness or spirituality (see Bergounioux 1958, Vandermeersch 1976). Such inferences have been incorporated into the scholarly and popular view of Neandertal without, it seems, any serious criticism (see, e.g., Bouyssonie 1954, Eddy 1984, Howell 1965, Peyrony 1921, Shackley 1980, Wolpoff 1980, Wymer 1982). Examination of the reports of Neandertal finds suggests, however, that purposeful burial has been inferred in the absence of unequivocal stratigraphic evidence. Consideration of the sedimentological context of the discoveries produces a quite different understanding of the reality of Neandertal mortuary practices.

Given the abundant evidence, both archaeological and ethnographic, of the mortuary practices of anatomically modern *H. sapiens*, the criteria for recognizing such behavior in the archaeological record are usually implicit or ill-defined. It is simply assumed that human remains discovered in an archaeological context were placed there purposely. In the majority of cases the inference is probably well founded. In the Middle Paleolithic, however, one is potentially dealing with the earliest occurrence of purposeful burial, and the criteria for recognizing remains that have been purposely disposed of need to be made explicit. The proportion of fragmentary remains of Neandertals in the fossil record is much higher than that of complete or nearly complete specimens (Mann and Trinkaus 1973). It is clear that not all Neandertal remains underwent similar processes between burial and discovery. This suggests that something special was happening to preserve the relatively few complete specimens in the fossil sample, and that "something" is usually taken to have been purposeful burial (see, e.g., Trinkaus 1985:209). Burial does have the effect of increasing the chances of survival, but it can be demonstrated that natural burial is expectable in certain sedimentary environments. Caves and rockshelters can be places of rapid deposition. Moreover, preservation there is enhanced by other variables: chemical and atmospheric, for example. Interpretation of the archaeological record as a document of Paleolithic behavior has only recently benefitted from advances in taphonomy and geoarchaeology (see, e.g., Binford and Ho 1985, Butzer 1981, Trinkaus 1985). The following review of the archaeological contexts of Neandertal burial will complement other recent

1. A version of this paper was submitted to the Department of Archaeology, Simon Fraser University, as part of the requirement for an Honors B.A. degree. Mark Skinner, Knut Fladmark, Chris Knüsel, Brian Hayden, Randall White, William Farrand, and F. Clark Howell have all graciously commented on earlier draughts and offered their suggestions for its improvement.

and ongoing appraisals of Middle Paleolithic biocultural evolution, all aimed at solving the riddle of the observed cultural disconformity at the Middle/Upper Paleolithic boundary (see White 1982 for a summary of the debate).

The archaeology of Neandertal burial began in France in the early 20th century. At La Chapelle-aux-Saints (in 1908), La Ferrassie (in 1909), and Le Moustier (in 1914), discoveries of Neandertal skeletons, some in anomalous contexts, convinced their excavators that they had been intentionally buried (Bouyssonie, Bouyssonie, and Bardon 1908, 1913; Capitan and Peyrony 1909, 1910, 1911, 1912*a*, *b*, 1921; Peyrony 1930, 1934). Later, at sites such as Tabūn in Israel (Garrod and Bate 1937), Teshik-Tash in the U.S.S.R. (Okladnikov 1949, as reported in Movius 1953), Amud in Israel (Suzuki and Takai 1970), Krapina in Jugoslavia (Gorjanovič-Kramberger 1906, cited in Trinkaus 1985), Drachenloch in Switzerland (Bächler 1921, cited in Kurtén 1976), Regourdou in France (Bonifay 1962, 1964; Bonifay and Vandermeersch 1962), and Shanidar in Iraq (Solecki 1955, 1960, 1961, 1963, 1971; Solecki and Leroi-Gourhan 1961), the body of "evidence" for ritual behavior among Neandertals grew. Wherever complete or nearly complete remains were discovered, they were claimed to have been buried, and inferences of accompanying ritual became more and more common. Archaeologists and human paleontologists continue to accept these findings (see, e.g., Vandermeersch 1965, 1976; Trinkaus 1983, 1985).

Recently, reinterpretations of several important Neandertal sites have resulted in a much more conservative and probably more accurate view of fossil record. For example, Trinkaus (1985) argues that cannibalism was not the cause of the burned and broken bones at Krapina. The burning is arguably incidental; the missing basicrania are a common and predictable taphonomic phenomenon and the splintering of most of the bone probably the result of the rain of limestone rubble from the ceiling of the cave throughout its history (Trinkaus 1985). The evidence for the cult of the cave bear at Drachenloch has been questioned; reports of the excavation disagree on the shape, size, and contents of the alleged "cysts," and an awareness of cave taphonomy has allowed an equally likely natural origin to be posited for the concentration of preserved bear crania—their location in the cave (Kurtén 1976).

Some archaeologists may question the usefulness of taphonomic studies undertaken at a remove in space and time. However, if we are to improve our understanding of the Middle Paleolithic (keeping in mind the rarity of deposits and the conservatism of modern archaeology), earlier findings must be subjected to modern scrutiny. If the evidence is lacking or equivocal, we must assign provisional status to the conclusions of early workers and wait for new discoveries by rigorous methods to improve our understanding. If the spirituality of *H. sapiens neanderthalensis* suffers as a result, it may ultimately simplify rather than complicate our understanding of the Middle/Upper Paleolithic boundary.

After outlining what cave geomorphology, sedimentology, and taphonomy have to contribute to an evaluation of the hypothesis that Neandertals buried their dead, I shall discuss criteria for recognizing purposeful burial in the archaeological record. Then I shall examine the reports of Neandertal remains most often cited as evidence for purposeful burial—La Chapelle-aux-Saints, Le Moustier, La Ferrassie, Teshik-Tash, Regourdou, and Shanidar—in the light of these considerations and draw some general conclusions. Because original field notes were unavailable for this study and only published distillations could be consulted, the conclusions must be considered tentative.

Archaeology in Caves: An Overview

Most discoveries of Middle Paleolithic hominid fossils have been made in caves and rockshelters. Any effort to interpret the context of Neandertal burial must include a consideration of the processes that transform these natural shelters during and after occupation by humans and animals and make them special sedimentary environments for bone preservation. While much remains to be done to improve our understanding of cave sedimentology and taphonomy, our current database is substantial (see, e.g., Colcutt 1979).

The kind of deposit under study is created by the gradual removal, by solution and mechanical processes, of clasts of various sizes from the limestone bedrock in which caves and rockshelters form. The seemingly insignificant solution of carbonates gradually results in complex ramified cave systems. Various erosional and weathering agents create overhanging shelters. Both site types may accumulate deeply stratified natural and archaeological deposits (see, e.g., Jennings 1985, Ford and Cullingford 1976). As natural mechanisms such as running water and wind break down parent material and combine with others to rework the accumulated sediments, the shelter "retreats" upslope, with older sediments adding to the talus outside (fig. 1). A cave evolves in much the same way but is also subject to ongoing karstic processes that may not be seen in rockshelters.

Caves form a unique class of archaeological site. There is greater clastic variation in caves than in any other depositional environment (Jennings 1985:163). Because no two caves are formed in identical parent material, no two are alike (Farrand 1985:23). Thus the interpretation of sediments within each cave is a fresh undertaking, because the rules governing build-up of sediments are peculiar to each.

Frost, earthquake, and other mechanical processes, coupled with chemical weathering, contribute to the gradual accumulation of parent material on the floor as the ceiling and walls break down (Ford 1976:51). The rate at which deposition occurs and, indeed, the character of the deposition itself depend on many factors that vary from cave to cave (Straus 1979:334). Thus, for example, at Shanidar there are many meters of deposit, while at La Chapelle-aux-Saints there is in some places barely a meter.

Friability of the parent material is a major determinant

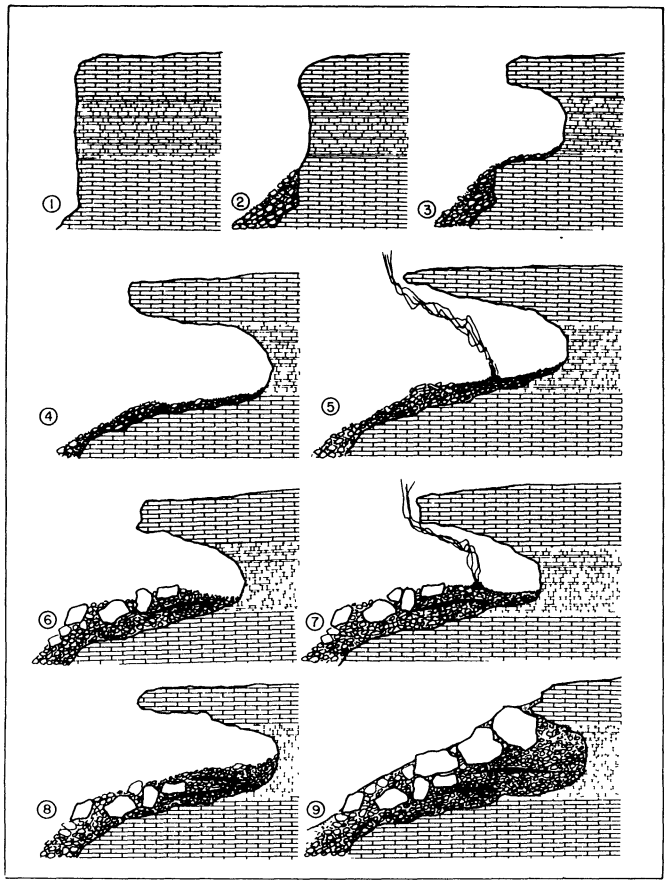

FIG. 1. *Rockshelter evolution (Laville, Rigaud, and Sackett 1980: fig. 3.3, reprinted by permission of the Academic Press).*

of the changes in cave morphology and the composition and build-up of sediments. Differential susceptibility to weathering results in very different sedimentary histories. For example, massive, horizontally bedded limestone will weather differently from thinly bedded, dipping, and fissured material. The latter is much more susceptible to gradual breakdown creating mostly fine or relatively small fragments, such as *éboulis,* while the former will more commonly break down by periodic, possibly catastrophic collapse. The sediments produced include all sizes of particles from dust to car-sized blocks of limestone bedrock. Size, shape, exposure, local topography, and intensity of human or animal occupation also contribute to sedimentological differences between caves (Farrand 1985:23). Deposits that result from such processes are extremely variable within sites as well as between them. Some regularity does exist, however, in deposits that form through gradual breakdown. Distribution of rubble on the floor of a cave is predictable, at least in a broad sense: "Breakdown of roof and wall results in piles of unbedded angular blocks, conical in shape below a roof dome, fan-shaped against walls and extending like rivers into neighboring confining passages. Inside such piles, further fracturing of blocks can be seen to accompany the settling down" (Jennings 1985:164).

Proximity to flowing water may result in periodic fluvial or basin deposition (Farrand 1985:23). Such sediments are easily distinguished by their degree of sorting and the size ranges represented. Aeolian deposition can result in well-sorted fine and medium sand, again easily discerned. Fine sediments such as loess are also easily transported by wind and may enter cave deposits in that way. Vegetable matter such as branches and dead and dying plants or flowers may be incorporated into the deposits by a variety of transport agents (Ford 1976:58). Animals and humans carry sediments into the cave from outside: bone and other food refuse, tools and raw material of stone, and fine sediments that may cling to fur or feet, fuel, and bedding (Butzer 1982:78; Erlandson 1984:788; Wells and Jorgensen 1964:1172). Where "chimneys" are open to the surface, sediments are constantly washed or blown in, and a cone of talus forms inside the cave (Sutcliffe et al. 1976:499). Knowledge of the origin and mode of transport of all sediments, noncultural or cultural, is fundamental to an understanding of the archaeological record in caves and rockshelters.

Flushing out, cementation by calcareous concretion, covering over by travertine, and encrustation due to differences in porosity and the accumulation of seeping water are all expectable in caves. Reactivation of karstic processes after a dormant period during which deposition has occurred can result in gradual subsidence of the deposit, marooning cemented sediments high on the walls or deforming the unconsolidated strata and bringing about further erosional effects (Glover 1979; Straus 1979:333).

Groundwater can color sediments according to the particular broth of chemicals that it carries in solution. Weathering of iron oxides results in the characteristic red or brown "cave earth" often described (Jennings 1985:163). Color can act either as a mask, drawing attention away from real stratigraphic changes, or as a useful indicator of the presence of paleosols, organics, and minerals (Butzer 1971:207), all of which are important to the correct interpretation of occupation and abandonment.

All of the physical erosional mechanisms that act on subaerial sediments are also at work in caves and rockshelters. Wherever slopes exist, gravity produces differential movement of particles downslope according to size and shape with the movement of surface water or water ice in the sediments (Butzer 1982:54). Gullying by surface water or subsidence in sinkholes is also possible (Farrand 1985:25). Trampling by animals or humans, especially when combined with surface water, creates a plastic substrate in which, typically, particles on the surface can be moved downward by as much as 10 cm, obscuring contacts and destroying associations (Butzer 1981:155). Similar effects have been experimentally demonstrated in sandy substrates (Gifford-Gonzalez et al. 1985). Even buried remains may not escape such disturbances (Harrold 1980:196). For example, it is thought that much of the disturbance of the Krapina Neandertal skeletal series was due to the occasional presence in the cave of *Ursus spelaeus,* even though the remains are believed to have been intentionally buried (Trinkaus

1985:209). The complete homogenization of previously stratified deposits is not unheard of (Erlandson 1984, Wood and Johnson 1978). Such features as graves and protective mounds would be difficult to discern after prolonged rodent activity.

Finally, it cannot be assumed that, once humans had left their imprint on a cave, no other animal gained entry or that other animals were not the habitual residents, with humans relying on the shelter only during parts of the year.

Sediments are the prime evidence for use and abandonment of caves by humans and animals and post-depositional reworking by all classes of natural mechanisms (Farrand 1985:22). To interpret archaeological remains correctly, sedimentary structures, color differences, and bedding formation must be examined in detail in situ. Strata are real units, representing the record of deposition. Each has a top, a bottom, and a horizontal extent, and if associations are to be perceived these must be identified (Butzer 1981:155). Without the understanding of the succession of events that comes from knowledge of the stratigraphic sequence, no chronological control is possible (Straus 1979:332–33). To ensure adequate retrieval of information from the site, all of the material, not just individual hominid skeletons or "associated" fauna or artifacts, must be examined and understood. Accurate description of "behaviorally meaningful sediments" (Straus 1979:337) relies on complementary analytical techniques to determine the origin and mode of deposition of the cave sediments and the post-depositional processes affecting them (Colcutt 1979). Determination of whether sediments are naturally or culturally deposited or disturbed depends on knowledge gained from the other sediments in the site (McGuire 1980). Although it is possible that human activity could mimic a natural process, it is useful to ask the question, "How do we tell them apart?" If there is no way of knowing, then it is logical to credit nature first, humans later, or at least to give them equal weight.

Because caves are used as habitations by many kinds of animals, including hominids, they tend to accumulate bone quickly (Farrand 1985:22; Straus 1979:333; Sutcliffe et al. 1976:495). Pack rats, burrowing carnivores, raptorial birds, bats, scavengers, and hunters are all potential agents for deposition of bone in caves (Henschel, Tilson, and von Blottnitz 1979; Klein 1975:286; Sutcliffe 1970:1110; Sutcliffe et al. 1976:495). Both the remains of meals and the remains of deceased occupants are represented. The task of the archaeologist becomes one of separating human from animal vectors and determining likely depositional agents and modes of preservation for the hominid remains. Accumulations due to carnivores and humans can sometimes be distinguished on the basis of skeletal elements represented. The ratio of cranial to post-cranial bone preserved varies with body size of prey in carnivore assemblages (Klein and Cruz-Uribe 1984:82). Fewer carnivore remains occur in hominid occupation strata than in those that cannot be attributed to hominids (Straus 1982). By these criteria and others, it is possible tentatively to identify natural assemblages.

Bone is usually preserved in the special environment of a cave better than in a subaerial locality. Relatively rapid burial by naturally occurring sediments is frequent in caves, and protection from weathering coupled with the (usual) alkalinity of the deposits is ultimately responsible for preservation (Sutcliffe et al. 1976:496). However, preservation can occur differentially even within a cave. The absolute size and relative density of a fragment can affect its preservation. Cancellous bone has a lower chance of survival than dense bone (Trinkaus 1985:208; Sutcliffe 1970:1110). Horns have a high probability of survival (Brain 1980:117). A clast's location in a cave can contribute to its destruction, disturbance, or preservation. Those that find their way, either through conscious or unconscious "housekeeping" or by chance, into protected areas such as rockfall niches or against walls have a higher likelihood of preservation than those subject to trampling (Kurtén 1976:88). In part this may explain why so much of the known Neandertal skeletal sample consists of isolated fragments; they may be remnants of skeletons that were not protected in any way from disturbance by subsequent inhabitants or geological processes.

A subjective assessment of the Lower and Middle Paleolithic hominid fossil record suggests that something different was happening in the later period to result in the several skeletons preserved fairly intact; that "something" is usually considered to have been intentional burial. Whether purposely or naturally buried, however, the chance of disturbance must increase with time. Geological processes such as periodic flushing out, collapse of bedrock, and other disturbance may contribute to the presence of fewer intact remains of the more ancient *H. erectus* in caves from the Lower Paleolithic as well as of the numerous fragmentary remains from the Middle Paleolithic. Some caves are just less likely to have preserved bone, whether buried or not; some would not have accumulated sediments rapidly enough or were too close to communities of scavengers to have allowed preservation of an individual lying exposed on the cave floor. The time of year and the climate may also contribute to preservation by freezing or covering the body with snow or otherwise rendering it unattractive to scavengers. Subsequent animal occupation might be more damaging to surficial or partly buried bones than subsequent human occupation by virtue of different body weight or habits of movement.

If purposeful burial were solely responsible for intact preservation of skeletal remains, we might expect to find complete skeletons in more locations. As it is, complete or nearly complete skeletons are found in numbers greater than one in only a few places: Krapina, La Ferrassie, and Shanidar, for example. It is clear that at Krapina and Shanidar repeated ceiling collapse has sealed the bones, protecting them from further disturbance (save from settling and subsequent shifting of the overlying rubble). The depositional history of La Ferrassie has not yet been elucidated, but it is likely to demonstrate similar peculiarities of rapid deposition. Obviously, something special has to occur for a skeleton to be preserved

intact, perhaps irrespective of the treatment received at death. Other (perhaps random) events may well explain the sample of complete and nearly complete Neandertals.

Archaeological Correlates of Burial

On the basis of their experience at La Chapelle-aux-Saints, Bouyssonie, Bouyssonie, and Bardon (1908) proposed some criteria for the inference of burial: (1) the position of the body, flexed, as if in sleep; (2) a dug grave; (3) protection of the corpse; (4) food or other grave offerings; and (5) magic or ritual manifestations (Shackley 1980:85–86). In a study of mortuary practices in the Middle Paleolithic, Binford's (1968:140–41) criteria were "the presence of an excavated grave and/or an arrangement of the body or body parts which seem to preclude natural agency." Harrold (1980:197) is somewhat more selective: "a case was not counted as a burial without some strong positive indication to that effect, such as strongly-flexed body position, or unequivocal association with a burial trench or grave goods."

Inference of mortuary ritual from archaeological deposits requires that non-human agents be ruled out. The criteria just mentioned are too general for this. The position of the skeleton can tell us nothing by itself. Presumably, if the flexed position mimics sleep, death during sleep can produce a flexed corpse. (Extreme flexion such as that observed in the prehistoric hunter-gatherers of the Northwest Coast is not seen in any of the Neandertal "burials"; their degree of flexion is well within what one would expect to occur naturally in sleeping humans or those who died in other ways, for example, from a fall.) The depression that holds the remains cannot be considered a "dug grave" unless it can be shown to have been created by humans and not by any of a number of natural agencies. Protection of the corpse is a useful criterion as long as it can be shown to have been the result of purposeful behavior. Inferences of offerings require a spatially defined stratum that includes the burial and distinction of the materials from other clasts that may occur in the grave fill by accident. Magic or ritual must, again, be demonstrated by clear association and non–naturally occurring sediments. It is not enough to say that humans *could have* produced a given deposit; it must be shown that nature could not.

A new stratum, created when the grave was dug, is the fundamental criterion for recognizing purposeful interment (Drucker 1972:5; Harris 1979:95). Variable attributes of mortuary ritual, such as presence/absence of offerings, graves, or preparation of the body, can be adduced only when a new stratum has been identified. If the individual was covered by a new stratum excavated from cultural sediments (either in the process of digging a grave or by collecting material to cover a corpse lying on the surface), then care must be taken to separate those objects that were deliberately placed with the deceased from those already part of the deposits. It is obvious that objects lying on or near the underlying stratum will be more likely to have been contemporary with the burial, whereas those that are mixed in with the new stratum will be practically indistinguishable from the rest of the detritus.

Contemporaneous occupation of an area by humans is reason to suspect that they may have created "anomalous" deposits, but it is no reason to assume so. An archaeological inference is "a descriptive statement of high probability," and this "implies that it is necessary to rule out other possibilities before constructing the inference" (Schiffer 1976:12). Most important, an inference, like any other inductive conclusion, depends on current knowledge (Sullivan 1978:188). We are in a position today to combine evidence from many recent actualistic studies with enhanced knowledge of natural sedimentary phenomena to produce other likely explanations for deposition and transformation of important sediments. That different conclusions can result from looking at the same data is an expectable outcome of scientific progress (Kuhn 1970).

The Evidence Reexamined

LA CHAPELLE-AUX-SAINTS

Excavated in 1908, La Chapelle-aux-Saints in the southwest of France yielded the first "evidence" for burial in the Middle Paleolithic: a nearly complete specimen of the then little-known Neandertal (Bouyssonie, Bouyssonie, and Bardon 1908, 1913). The skeleton was discovered in a large, roughly rectangular depression claimed to have been "intentionally excavated" (1908:516). Nothing in the reports, however, unequivocally supports the hypothesis that the burial was purposeful.

The excavators identify six strata (p. 515). Although crude, these descriptions permit reconstruction of the following probable sedimentary history:

The cave appears to have been formed at the contact between strata of different lithological composition. In cross section (fig. 2) it is low, ellipsoid, and flat-bottomed. Thus it is most likely a bedding-plane tunnel created by high-energy underground stream flow (see Warwick 1976:73). Stratum 5, the basal layer, on which the skeleton lay, is variously referred to as "calcareous

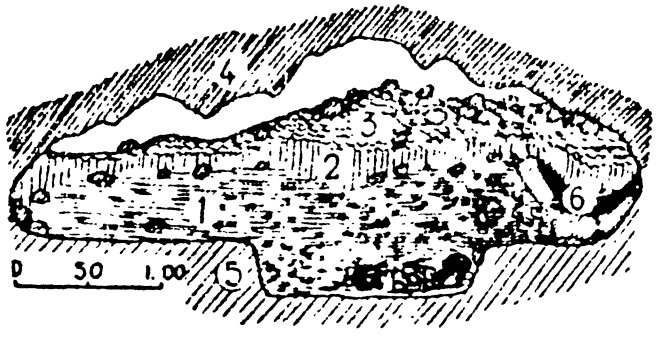

FIG. 2. *La Chapelle-aux-Saints, transverse section (Bouyssonie, Bouyssonie, and Bardon 1980:fig. 3, reprinted by permission of Masson, Paris).*

marl" (Boule 1909:258) and "marly soil" (Bouyssonie, Bouyssonie, and Bardon 1908:516), white and hard to break up. (The term "marl" refers to a calcareous sedimentary rock that alternates with limestone in this area [Boule 1909].)

Once the cave was opened to the air, which must have been shortly after cessation of the vadose condition, archaeological deposits began accumulating. Stratum 1 is described as "yellowish, clayey earth," over 0.5 m deep in places. It has a wide areal distribution inside the modern cave and extends out onto the cave apron beyond the entrance. The description of these sediments closely matches that of the most common class of sediments found in caves, yellow or reddish-yellow "cave earth." This class of sediments can result from a number of processes, any of which may be active at any time: (1) the gradual mechanical breakdown of parent material, which produces clasts grading up from silt size; (2) solution by weakly acid groundwater, which removes carbonates from bedrock and leaves clay-sized particles; (3) trickling in from above of loessic or humic silts through the joints and fissures that characterize karstic limestone; and (4) the introduction of sediments from farther back in the cave. Oxidation of the minerals and input of organics from animal constituents is probably responsible for these sediments' yellow color (Butzer 1971:207; Ford 1976:55).

Stratum 6 is described as "scorched soil," perhaps the result of the presence of a fire. Its position at the contact between Strata 1 and 2 implies that it was one of the last events associated with deposition of archaeological material.

Stratum 4 does not represent a sedimentary unit; it merely identifies certain large clasts derived from the limestone bedrock.

Stratum 2 is clay. It may represent a period of relative inactivity during which the cave received only fine-grained input, either through natural weathering of the bedrock or from fines washed in through the jointed limestone from above. In effect, the cave may have been acting as a "settling tank" for fine sediments (see Ford 1976:55). It may also represent a basin deposit, one that accumulated during a wet period. Some of the sediments may have been introduced from upcave. Sorting out the origins of most of the sediments is no doubt impossible because of the thoroughness with which the excavation was carried out in 1908.

Stratum 3 represents recent breakdown rubble, probably autochthonous and partly composed of weathered particles of the exposed limestone beyond the entrance.

With regard to the "grave" depression, 1.45 m by 1.00 m by 0.30 m, in Stratum 5, Bouyssonie, Bouyssonie, and Bardon (1908, 1913) offer several observations in support of the hypothesis of burial: (1) the position of the skeleton, flexed, "as if asleep, perhaps cold"; (2) the presence of three *or four* (my emphasis) large, flat pieces of bovid long bone above the head, along with a bovid distal metatarsal, two first phalanges, and one second phalanx and, nearby, some reindeer vertebrae, articulated (it is suggested that these were grave offerings); (3) the discovery of two stone tools near the nasal aperture and numerous other flint artifacts throughout the "grave" fill (also seen as offerings); and (4) the occurrence of more bovid remains in a depression in the marl but nearer the mouth of the cave (considered evidence for magical ritual). There is no doubt that the sediments in which the skeleton was found contain numerous stone tools and much bone. Moreover, there is no doubt that the Neandertal remains were found in a roughly rectangular depression in the substrate. However, the hypothesis of intentional burial with associated animal parts and stone tools as grave offerings is not supported by the circumstances of discovery as reported.

To recognize associations, the horizontal and vertical extent of a stratum must be known (Butzer 1981:155). Bouyssonie, Bouyssonie, and Bardon (1908:515) recognize a unit of association that encompasses all of the archaeological sediments at the site, Stratum 1. By definition, every piece of bone and stone at La Chapelle is in association with every other. Because no visible new stratum overlay the skeleton, we will never be sure that any of the items suggested to have been offerings were, in fact, intended as such. It is worth noting that all of the alleged offerings were discovered above the level of the head. If the depression had been dug to receive a burial, we would expect to see at least some of the offerings occurring at or near the contact with Stratum 5. We cannot know for certain the rate of sedimentation at La Chapelle, but it is a safe guess that not all of the archaeological deposits were introduced at once. Thus any inference of grave inclusions becomes questionable. Furthermore, there is no evidence of a contact between the hypothetical fill of the "grave" and the overlying sediments. If there had been such a contact and it had been obscured by trampling and resultant vertical transport of sediments of the kind referred to by Butzer (1982:155), we could not be sure that there had ever been clear-cut associations. In the absence of such data, it cannot be said that this depression was filled as a single event as it would have been if the body had immediately been covered as part of a burial procedure.

As we have seen, caves can be rapid accumulators of sediments of all kinds, and depressions within them are especially likely repositories for sediments. Once a clast enters a depression, there is very little likelihood that it will be dislodged except by a much higher-energy transport agent than the one that deposited it. If the depression in which the "old man" lay had not been immediately filled by his relatives it would nevertheless have been the most likely place for sediments to collect, resulting inevitably in burial and, thus, preservation.

The depression itself could have occurred naturally. It is situated at a point where the slope of Stratum 5 changes from steep to level to steep again (fig. 3). It is here, where energy was lost because of the gradient change in the underground-stream phase, that heavy clasts would have tended to collect until the energy of the stream increased enough to displace them. Turbulence, friction, and increased pressure at the contact between the bedrock and the resting clasts would have

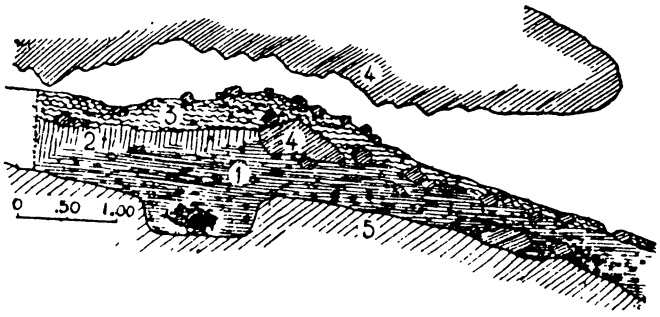

FIG. 3. *La Chapelle-aux-Saints, longitudinal section (Bouyssonie, Bouyssonie, and Bardon 1908: fig. 2, reprinted by permission of Masson, Paris).*

eroded the underlying bedrock, leaving a depression like the "potholes" of surficial bedrock streams (see Warwick 1976:94). Furthermore, any depression in that level stretch of the cave's sloping floor would naturally have accumulated groundwater and promoted solution. The horizontal bottom and nearly vertical sides of the "grave" at La Chapelle parallel descriptions of solution basins (e.g., Sweeting 1973:83). A much fuller understanding of microtopography in caves will be needed before we can say for certain that the "grave" could not in fact be a karst feature.

The small size of the cave is another potential argument against the likelihood that this old Neandertal was buried. It is approximately 2 m wide at the entrance and widens to about 5 m at the point where the skeleton was found. From the base to the ceiling in the center it is roughly 2 m, but at the time of excavation sediments filled it to within 0.3–0.5 m of the ceiling. There is reason to suspect that there never was much headroom in this part of the cave. Since opening, the processes of bedrock breakdown have acted to raise the ceiling and build up the floor, as is evidenced by the large clasts depicted in the profiles. How much of the sediment overlying the skeleton is breakdown and how much allochthonous is now impossible to assess. Some of the sediments in the excavated portion of the cave may have been transported there from farther back, thus increasing the net rate of deposition. The effect of all these processes has been to keep the floor close to the ceiling. Therefore, when the "old man" was deposited, access to his grave might have required a crawl of some meters through an opening that could well have been less than 2 m wide and on the order of 0.5 m high. This sounds more like a den. The presence of so many Mousterian implements is tantalizing evidence that it was occupied over a reasonably long period following the death and (possibly) interment of the individual. The excavators suggest that it was a tomb and that numerous funeral feasts were held there over the years (1908:517–18). Alternatively, we might infer from the position of the corpse, at the contact between the pristine cave substrate and the archaeological deposits, that the "old man" crawled in to sleep or perhaps to escape the cold or predators shortly after this small niche was opened and died there and that the protection afforded by the confined space and (possibly) rapid burial in the cave environment allowed his skeleton to be preserved. We must conclude that the deposition of this Neandertal could have been a natural phenomenon rather than the result of ritual behavior on the part of Late Pleistocene *H. sapiens.*

LE MOUSTIER

The rockshelter at Le Moustier, near Les Eyzies in the Dordogne, was formed in Cretaceous limestone by the action of the Vézère River. The older, upper terrace was the site of excavations that yielded two Neandertal skeletons: a late adolescent discovered by Hauser in 1908 and an infant excavated by Peyrony in 1914. Both were described as burials, but according to Oakley, Campbell, and Molleson (1971:150) Hauser's observations "cannot be relied upon." Therefore only the infant's deposition will be dealt with here.

The record of sedimentation at Le Moustier is one of bedrock breakdown alternating with sterile river gravels and sand (Peyrony 1930). Because the infant was discovered in a pit dug through Strata H and I and sediments of Stratum J are involved, these strata will be described in detail. Stratum K is an Upper Paleolithic deposit, and its presence immediately above the final Mousterian stratum requires that it be considered here as well. From oldest to youngest, the strata are as follows:

Stratum H is 1.2 m deep and includes tools of the Mousterian of Acheulian tradition. It is brown, containing much burned and calcined bone and ash.

Stratum I is fluviatile sand. The artifacts show signs of rolling inconsistent with the indicated mode of deposition. If the particle-size distribution is unimodal, as would be expected in a fluvial environment in which sand is being discharged from the sediment load, then the artifacts must have been deposited at or near this point during the fluvial regime. A stream that had lost the energy to transport sand would long since have lost the ability to transport larger particles of stone, such as artifacts, long distances. Perhaps they were being eroded out of nearby earlier deposits.

Stratum J is brown and includes Mousterian artifacts. The color may be due to the high organic content of occupation sediments or soil development.

Stratum K is yellow and contains artifacts of Chatelperronian and Mousterian industries mixed. The latter are heavily weathered and probably represent a secondary deposition of unknown origin, perhaps the same as those that contributed to Stratum I (Laville, Rigaud, and Sackett 1980).[2]

In 1914, digging, it must be assumed, near the contact

2. Laville and Rigaud (1973) provide an updated description of the deposits at Le Moustier. Their revision of the stratigraphic column includes numerous subdivisions of Peyrony's earlier, coarser schema. For example, Stratum J is defined as being composed of five natural layers differing in the proportion of clay to limestone rubble. Since their study was designed to answer specific questions about palaeoclimate and not to elucidate questions of context, their refinements are of little practical use for present purposes.

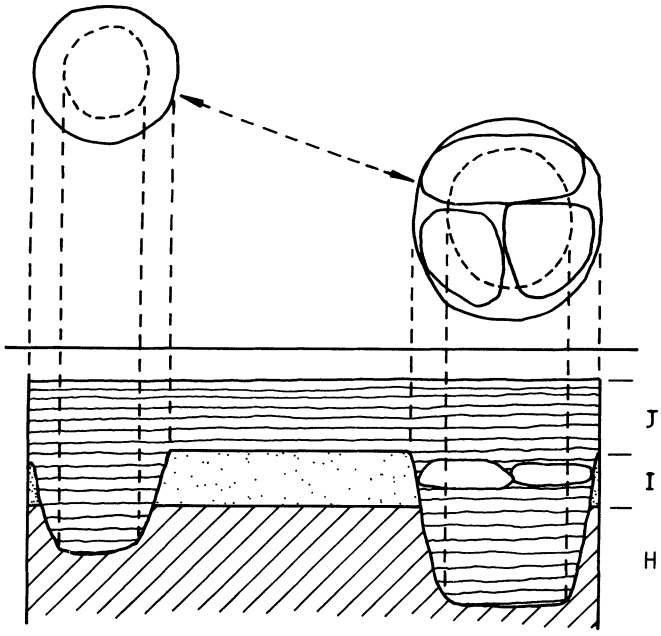

FIG. 4. *Le Moustier, section through infant's "grave" (after Peyrony 1930:fig. 14).* Left, *pit with infant skeleton;* right, *"empty" pit with juxtaposed limestone slabs.*

between Strata I and J, Peyrony encountered two roughly circular features in the sands of Stratum I (fig. 4). They were pits cut through the sand and into Stratum H. The first measured 0.7–0.8 m in diameter at the top and was 0.6 m deep. It was "empty" save for three flattish limestone slabs that formed a "lid" halfway between the top of Stratum H and the original contact between I and J. The other pit measured 0.5 m in superior diameter and about 0.4 m deep and contained some bones of a very young child. The evidence constrains us to accept that this pit was purposely dug, but aspects of it suggest that the situation is not a clear-cut case of Middle Paleolithic burial. Both pits had been filled with sediments of Stratum J (Peyrony 1930:159), and no contact was perceived between the fill of the pit and Stratum J (see fig. 4). While this does not preclude the possibility that there was a contact, it makes it difficult to accept without question the contemporaneity of the creation and the filling of the pit.

Given the data in Peyrony's report, there are three ways in which the features he excavated might have been created:

1. The holes could have been created when the surface was newly laid river sand (Stratum I). If so, the fill should have consisted of a mixture of H and I, because both were removed from their primary context in the creation of the pits. No Stratum J would have existed at the time in this scenario. Peyrony, however, describes the fill as being that of J. Therefore he would have to have mistaken the mixture of H and I for J. Both H and J are described as being brown: in the absence of diagnostic criteria more precise than color, we have to assume that it was possible for Peyrony to have made such a mistake. If, indeed, the pits had been filled with a mixture of H and I, implying that the pit had been dug before J was deposited, we would have to accept that they had been created penecontemporaneously with the Mousterian occupation and that this represented a Neandertal burial. We cannot, however, conclude that the pits were dug before J was deposited, because Peyrony says that they were filled with the sediments of J, and his published profile documents it (see fig. 4). Since the profile only shows the pits being dug through H and I (implying that they had been dug before J was deposited), the only way sediments of J could have become the fill is if the holes had been left open and sediments of J had accumulated over some time. Thus disposal of the infant's bones need not have been purposeful.

2. The holes might have been dug through sufficient quantities of J to have resulted in a mixture of H, I, and J that Peyrony was unable to distinguish from the rest of the sediments in J. The pits must then have been indistinguishable from the surrounding unstratified sediments until the contrasting sands of I were reached (see fig. 4). If this were the case, and we can have no way of knowing what depth of J was dug through, it is conceivable that these pits were created during the Upper Paleolithic occupation represented by Stratum K (J is only 40 cm thick). Even if only equal parts of J and the excavated pit sediments were mixed, the pits could have been dug through at least 20 cm (and quite easily 40 cm) of J. In this case it is possible that the skeleton is not even that of a Neandertal. The proposition that it could be an Upper Paleolithic intrusion might help to explain the presence of the "empty" pit nearby. If it is not a hole dug to house the funeral offerings for the deceased, as Peyrony speculates, it and its neighbor may originally have functioned as storage pits in the Upper Paleolithic. The larger of the two would have been 1.0 m deep and the smaller a minimum of 0.8 m.

3. The pits could have been dug through H and I before J began to accumulate and not filled in at the time. In this scenario, they would have filled with unadulterated sediments of the Neandertal occupation represented by J. This would mean that the remains of the infant had somehow found their way into a pit created by an unknown process for an unknown purpose. The bones are said to have included the "cranium, mandible, and postcranial bone" (Oakley, Campbell, and Molleson 1971:150). Gradual infilling of these pits is suggested by Peyrony's observation that they were filled with sediments of J. To reiterate, it seems to preclude the possibility that a new stratum was created over the deceased, a necessary condition if we are to infer that the remains were disposed of deliberately.

It is impossible to choose among these three scenarios. They either preclude burial in a Mousterian context or are not supported by Peyrony's observations. We have neither the means to test them now nor the data to support one or another. We cannot even go to the fossil record to break the stalemate, since the remains of the infant were lost in World War II (although the difficulty

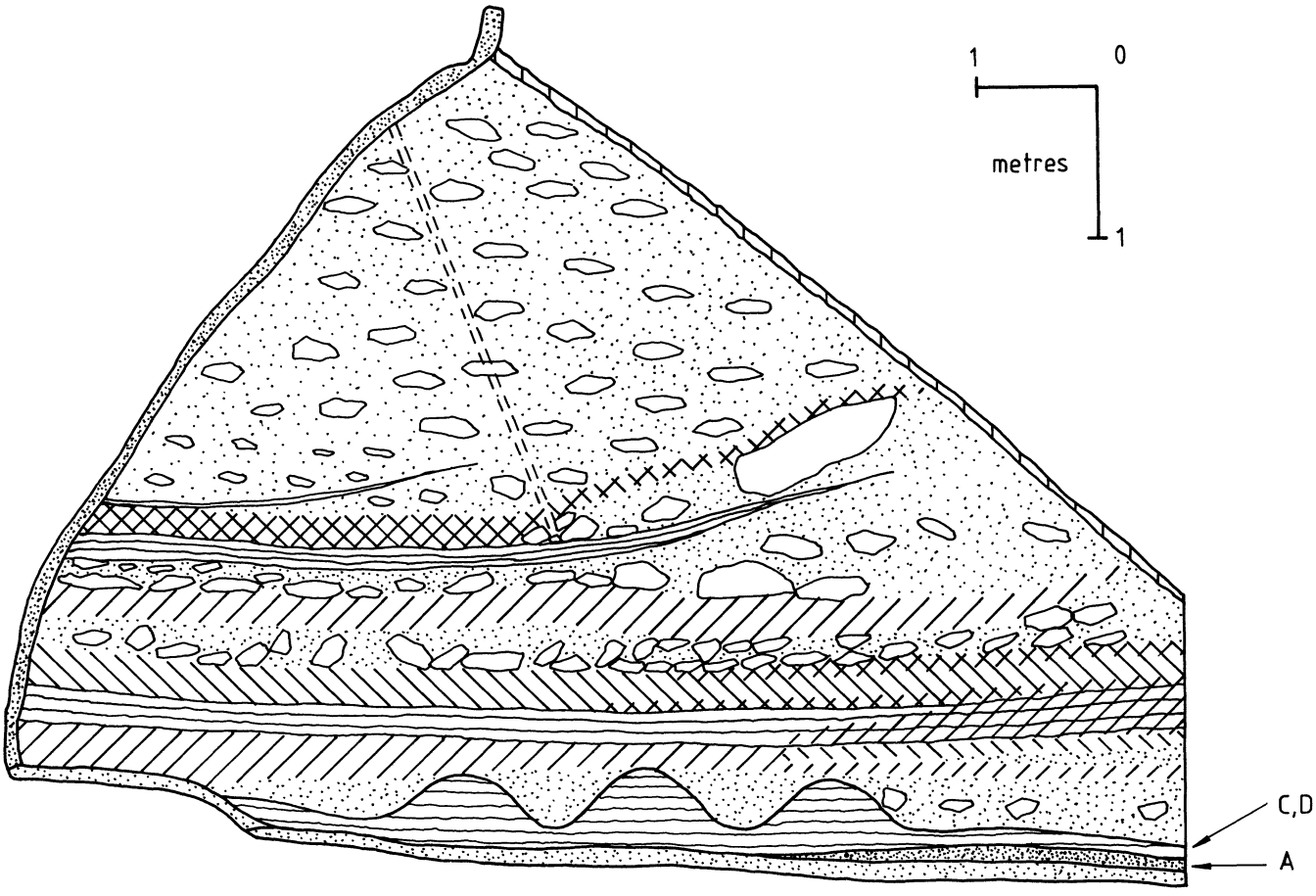

FIG. 5. *La Ferrassie, stratigraphic profile (after Peyrony 1934:fig. 4).*

of ascertaining subspecies from infant remains might well preclude a test even if the bones were available). Because of the ambiguity of the data reported by Peyrony, this Neandertal "burial" is best considered problematic.

LA FERRASSIE

Perhaps the most important Middle Paleolithic "burial" site, La Ferrassie, yielded two nearly complete and five fragmentary Neandertal skeletons between 1909 and 1920. It is a large collapsed rockshelter in the limestone massif near the Dordogne River. Excavated by Capitan and Peyrony and reexcavated in the 1960s and '70s, it has generated much romantic speculation on the nature of Neandertal ritual (see, e.g., Bergounioux 1958, Bouyssonie 1954, Howell 1965, Shackley 1980). A number of sedimentary anomalies occur in the deposits, most of which are not adequately explained by inferred ritual.

Peyrony (1934) describes the relevant sediments (see fig. 5) as follows:

Stratum A is a brick-red stratum lying on the bedrock (or at any rate sterile sediments). It is comprised of a shingle of calcareous elements, sometimes rolled, mixed with the clay that fills the spaces between. The contact with Stratum B shows signs of erosion by a small stream.

Stratum B is a deposit of yellow calcareous sand, appearing to originate from the disaggregation of the wall and of the vault of the rockshelter in between occupations.

Stratum C is the first "Mousterian level," light-brown.

Stratum D is a second level indistinguishable from the former but separated from it by a "pavement" of calcareous blocks. C and D together are about 0.6 m deep near the skeletons of La Ferrassie 1 and 2 but barely 0.2 m deep at the eastern end of the excavation.

Stratum E is a red clay, originating from slopewash, mixed with calcareous elements associated with Mousterian tools and Upper Paleolithic artifacts.

La Ferrassie 1, a nearly complete Neandertal adult, was discovered in 1909 at the contact between B and C. According to the excavators, there was no grave, but the body, with its legs strongly flexed, had been placed near the back wall on sediments left by the previous inhabitants. Three limestone blocks had probably been placed ritually on its head and shoulders and then the body had been covered either with branches or skins or with dirt and occupation debris. (They argue that it could not have

FIG. 6. *La Ferrassie 1 in situ, 1909 (Capitan and Peyrony 1910: pl. 1, reprinted by permission of the Société d'Anthropologie de Paris).*

been left exposed without having been devoured by hyenas.) Capitan and Peyrony (1911:52) reject the idea that this was a natural burial. They find no signs of slumping or collapse that could have accounted for the death of the individual or the relatively intact condition of the remains.

Their photograph (reproduced here as fig. 6) shows the corpse lying on its back, with its head turned full to the left. Its left arm lay along the left side of the body; its lower right arm was flexed and lay at a right angle to the axis of the body. Its left femur was flexed at 135° from the axis and lying to the right of the body. The tibia was flexed to within 45° of the left femur. The right leg is obscured by the left, but it does not appear to have been positioned very differently. The investigators maintain that the corpse was "strongly flexed." I do not concur; the lower limbs are barely flexed at the hip and the knee, and the individual is lying on his back.

The location of the skeleton, on a sloped surface near the back wall of the shelter, may have contributed to its preservation. Whatever caused the change in depositional environment evidenced by the contact between B and C is very likely responsible for the preservation of the corpse lying on B. We simply do not know enough about the sedimentary processes active at the time or about the contemporary ecosystem to assume that natural burial could not have occurred. In the absence of any tangible evidence for purposeful disposal, we have to accept an uncertain status for this case of "ritual burial."

A year later and only 0.5 m away from La Ferrassie 1, Capitan and Peyrony unearthed a second adult Neandertal skeleton in similar circumstances. They do not go into great detail, suggesting that whatever occurred to entomb the first also happened to the second. Peyrony (1921:33) contends that La Ferrassie 2 was placed head to head with La Ferrassie 1 at about the time the latter was interred. Both, he maintains, were watched carefully to protect them from scavengers. Once again, assumptions about what could have happened have no support from stratigraphic or other evidence. Nothing about the disposition of La Ferrassie 2 is suggestive of purposeful burial.

Capitan and Peyrony (1912*b*:439–40) report finding the remains of two immature Neandertals at La Ferrassie. At the base of the Mousterian level (C/D) they identified two pits, each measuring 0.7 m in diameter and 0.3–0.4 m deep, "very precisely dug (half-spherical in form)" into the Acheulian clayey gravel underlying the Mousterian layer and refilled with a mixture of about equal parts of the black earth of the Mousterian layer and of the underlying gravel. The first pit contained the remains of a ten-year-old (La Ferrassie 3), lacking the trunk and the lower limb bones (Oakley, Campbell, and Molleson 1971). The contents of the second, reported as similar, in fact comprised the remains of two individuals, the humerus and femur of a foetus (La Ferrassie 4) and a neonate skeleton (La Ferrassie 4a) (Heim 1968).

These pits were not recognized until the contrasting stratum below was encountered. (It is often difficult to see such features even when excavating with extreme care.) There was also no recognition of a new stratum created when the pits were filled. If, as seems possible from the situation with La Ferrassie 5 (see below), the pits were half-filled with gravel and half with Mousterian sediments, then instead of having been interred the remains were most likely deposited in (possibly) naturally created depressions such as occur elsewhere on the site and then covered first by expectable slumping and infilling with the lower sediments and then gradually by the Mousterian layer that overlies the whole of Stratum B. This may explain why the bones of La Ferrassie 3 were discovered at different depths in the pit (the long bones at the bottom, hand bones somewhat higher, a femur higher still, and near the top a cranial fragment) instead of at the contact with the underlying stratum as one would expect.

These immature remains cannot be assumed to have been burials. Holes in the ground are not necessarily the result of human digging, and any depression is a natural receptacle for sediments.

La Ferrassie 5 and the nine mounds with which it has always been associated have stirred most of the speculation about ritual at the site. The mounds are meter-sized "cones" formed in Stratum C/D (fig. 5), and the Nean-

dertal remains were found under one of them in 1920. At this point C/D is characterized by great irregularity of thickness. The plan (Peyrony 1934:fig. 26) shows the nine mounds in a very regular pattern, and the excavators explain them as part of a funeral rite for the infant whose remains one of them contained (the other eight being "empty").

The "burial" itself was not recognized until the excavators saw the contrast with the lighter-colored sediments of B. This visible portion was flat-bottomed and less than 5 cm deep and 0.3–0.4 m in diameter. Peyrony (1934:32) considered it only the bottom of a pit that extended into the Mousterian layer. The remains had, he thought, been placed in the pit, probably flexed because it was so small, and then three flints had been deposited carefully on top and the dirt piled back into the pit and mounded up over it.

Some of the bones are well preserved, but the ones that "penetrated" the Mousterian layer were completely decomposed (Peyrony 1934:31). Those that were better-preserved were "buried" in the yellow sediments into which the pit had been dug. How the bones of the baby even got into the lower sediments is a mystery, especially if this was the "bottom" of a deeper pit. One would expect the bones to have occurred in Mousterian sediments if they had been purposely buried. If the pit had originally been deeper and had not been filled in when the remains were placed there, it is likely that the sediments of the yellow stratum would have slumped or otherwise gradually covered part of the skeleton and subsequently been covered by the build-up of Mousterian sediments. This can occur in any unconsolidated sediments irrespective of the mode of creation of the pit. However, if it was slumping that partially buried the remains, we would not expect to see the vertical-sided feature that Peyrony shows; the edges would have been much softened by erosion. Thus it is not clear how this feature might have been formed.

It does not seem likely, however, that it was a ritual burial. If it had been purposeful, the "grave" would most likely not have been partly filled with pristine sediments of the lower stratum, then filled with those from above; instead, it would have been filled with a new stratum comprised of a mixture of sediments from both. The situation suggests a slow, natural burial in a (perhaps) naturally formed depression.

There is also no evidence that the mounds were created as new strata to cover a burial. That the eight other mounds were "empty" suggests some explanation other than human agency. Hummocky terrain superficially similar to the mounds at La Ferrassie is reported by Scotter and Zoltai (1982) in the Sunshine area of British Columbia. Mounds like these are known to be a permafrost feature, although the mechanism of their formation is the subject of debate. Sediments in rockshelters often show the effects of freezing (Laville, Rigaud, and Sackett 1980). Finally, Laville and Tuffreau (1984) report a great deal of cryoturbation affecting the sediments of what they term Bed M at La Ferrassie, roughly corresponding to Peyrony's Mousterian levels C and D (see

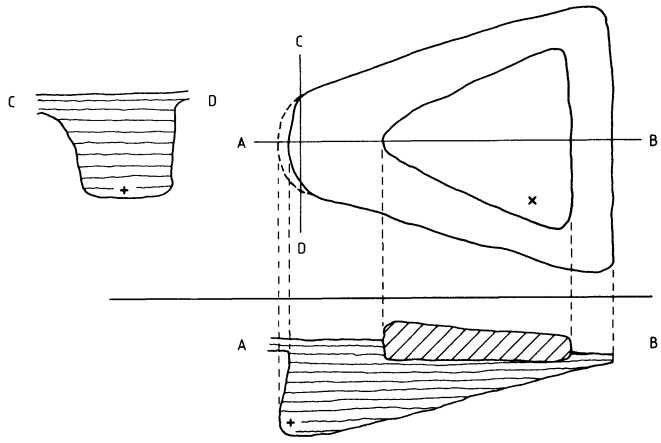

FIG. 7. *La Ferrassie 6, plan of and sections through "grave" (after Peyrony 1934:fig. 34). +, skeleton; ×, cranium;* diagonal shading, *limestone block with cuplike depressions.*

their fig. 9). Previously horizontal strata have been made vertical. It is not difficult to imagine that the "more or less regular" mounds under which La Ferrassie 5 was found were the result of natural deformation of the original sediments.

No good evidence exists for the suggestion that the mounds are what they are said to be—features created in some mysterious mortuary ritual. It would seem as if, like the rest, La Ferrassie 5 is not necessarily a burial.

La Ferrassie 6 was discovered during the 1921 field season. On a surface described as "undulating" and "strongly" sloped (called *le sol*, possibly referring to bedrock or sterile parent material), several irregular depressions filled with sediments of the Mousterian stratum were excavated. One very large pit (fig. 7) contained the partial skeleton of a child of about three years. The major portion of the body was found near the lowest point in the pit, the cranium about 1.25 m away. Above this, in the sediments that filled the pit, was found a limestone block with small cuplike depressions on one surface that the excavators considered an artifact (Peyrony 1934:33–36).

Peyrony speculates on the possibility of ritual involving the severing of the child's head and the removal of the face (p. 35). He admits that the depression was created naturally and that it must have been "chosen" as a "grave," then filled with surrounding sediments. There is, however, no indication that the filling of the pit was a single event; there is no discernible contact between fill and overlying sediments. The "burial" could easily have occurred naturally. A depression on this steep slope would naturally have collected sediments washing downslope. The cranium is very mobile and would probably have been dislocated if the body decomposed in the open, as seems likely given the absence of facial bones. Thus even this last "burial" will have to be viewed as problematic.

TESHIK-TASH

Located in the Jurassic limestone massif of southeastern Uzbekistan, Teshik-Tash is the site not only of an inferred burial but also of an arrangement of animal bones attributed to ritual. Here, in 1938, Okladnikov unearthed the partial skeleton of a 12-year-old Neandertal male (Okladnikov 1949, summarized in Movius 1953). He proposed that the remains had been buried in a shallow grave surrounded by goat horns placed point down in a circle (fig. 8). These inferences are based on the inferred position of the body, the inferred circle of horns, a supposedly non-random occurrence, and the good condition of the bones relative to those elsewhere on the site. As Movius summarizes the finds (pp. 25–28),

> At a depth of 20 cm. below the cranium a small slab of limestone (21 cm. by 18 cm.) was encountered, which seemed to have been inserted in order to support the block of material on which the head had been placed. The femur and fibulae lay roughly parallel to each other and it is possible that they are *in situ*. The cranium, mandible, vertebrae, clavicles, and the ribs, also found more or less together, constitute a second group lying approximately in place and some 50 cm. from the leg bones. On this basis, it is probable that the skeleton originally lay generally parallel to the western wall . . . with its feet toward the entrance. Both its orientation and its stratigraphic position suggest that the body had been intentionally buried in a shallow grave pit excavated in the sterile stratum. . . . This is confirmed by the fact that all the animal bones, other than the goat horns, found in this area had been broken into small fragments, not only by the occupants of the cave, but also by blocks of stones that fell from the vault. But the bones of the skeleton were not broken in this manner, since they were protected by the layer of earth placed over the interment.

Neither of the two profiles published by Okladnikov (Movius 1953: fig. 4) shows the burial, and Movius was unable to correlate either with any of the published plans of the site (p. 20). At one point (p. 25) it is stated the skeleton was found in sterile silty clay; the sediments are described as being "light-gray" and "true water-lain deposits," an assumption presumably based on their being finely "striated" (p. 22 [by which he probably means "laminated"]). They are elsewhere referred to, however, as being "coarsely laminated" and "yellowish" and as containing "limestone fragments the size of a pea and smaller, strongly rounded at the edges" (p. 26). Moreover, a number of larger limestone fragments are apparent in the published photographs and the account of "blocks of stones that fell from the vault."

Although the skull was broken into more than 150 pieces, Okladnikov concluded that the "burial" and the horns had been protected from the processes that broke other bones in the area. He judged the skull to have been "flattened by the weight of the overlying deposits," though how he could distinguish the results of pressure from the overburden from those of direct hits by ceiling spalls is unclear. In any event, horn would be less susceptible to breakage (Brain 1980:117). Near the wall there would have been less scuffling and trampling and more chance for preservation intact (Kurtén 1976:88); this alone could account for the observed "uncharacteristic concentration" of preserved material.

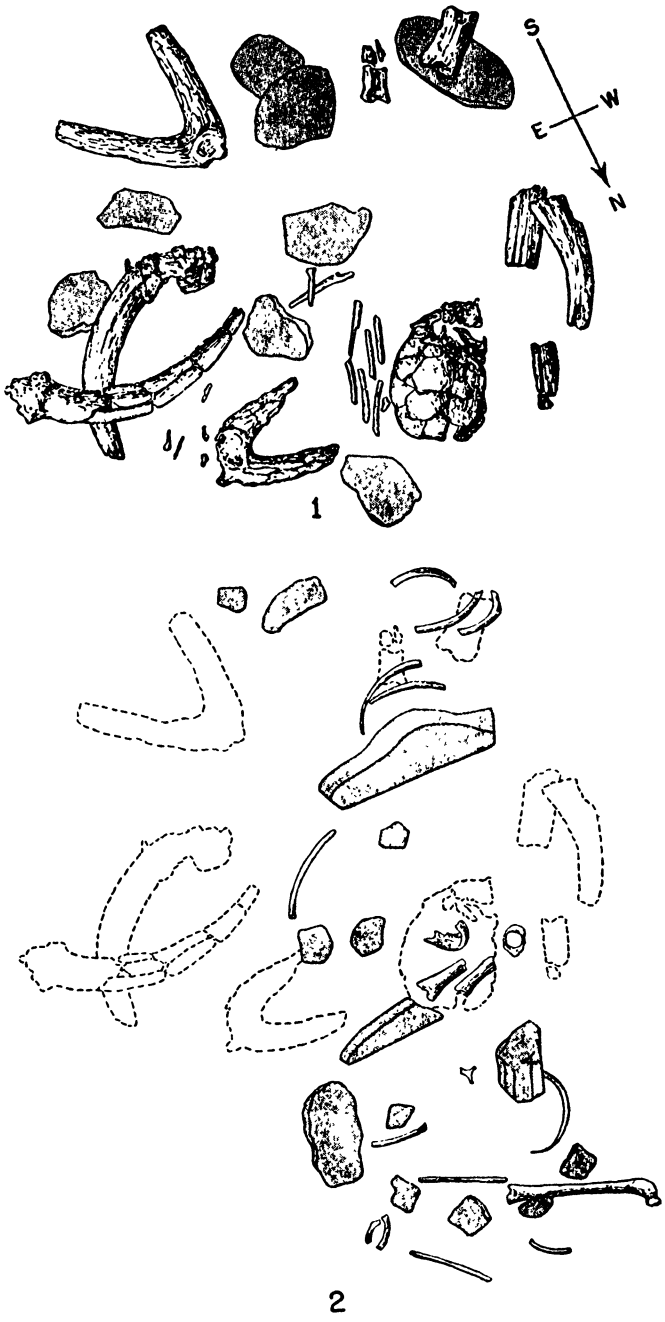

FIG. 8. *Teshik-Tash, plan of "burial" (Movius 1953:fig. 5, reprinted courtesy of Peabody Museum, Harvard University).* 1, *distribution of horns with reference to skull of Neandertal child;* 2, *distribution of post-cranial skeleton after removal of cranium and horns.*

The presence of two halves of a pair of horns, crossed, is advanced as proof that all the goat horns had once been arranged point down in a circle, but if the horns had once encircled a grave, how did any of them come to be horizontal? If they had escaped disturbance by predators as he reports, why were they not still vertically oriented? That they had in fact escaped disturbance seems implausible given the distribution of the Neandertal's ribs at two levels and in two separate horizontal concentrations within the ring of horns. And if any of the horns had been disturbed, the presence of two halves lying horizontal and crossed could not be considered to mean that they had once been placed point down in the dirt. The simplest explanation for the deposition of these skeletal parts, keeping in mind that there is no perceptible grave cut, is carnivore activity. The breakage could have been caused by trampling, ceiling spalls, or pressure.

The bones of the skeleton were not articulated or in anatomical position. An incisor was found 25 cm from the cranium. Yet the investigator notes that the cranium and mandible had no visible gnaw marks on them and suggests that when "some beast of prey . . . exhumed the corpse and devoured the soft parts . . . the skull and jaw were left untouched." It is difficult to see how the cranium could have been undisturbed by the carnivore and still have lost an incisor, much less how the incisor could have been transported to its resting place 25 cm away. The remains must have lain exposed for some time, which would explain the confusion of bone and horn. Gnaw marks on the femur would argue against Okladnikov's view that the leg bones were in their original locations. The parallel disposition of the three long bones could easily have occurred randomly, but even more likely it was a response to the slope. Clasts like these would naturally be oriented non-randomly relative to the trend of the slope (Butzer 1982:102).

That a predator was in evidence is reason enough to suspect that some undetermined, possibly significant portion of the "ritual" assemblage at Teshik-Tash was the result of predator activity. The gnaw marks and the coprolite found lend credence to this hypothesis. The predominance of horns is reminiscent of other carnivore assemblages (e.g., Brain 1980). Indeed, Binford (1981:2, 62, 196) suggests that they may be present in large part because of carnivore activity. Goat remains make up roughly 85% of the faunal assemblage at Teshik-Tash, and, since horn is the most likely skeletal part to survive, the probability of six horns' being preserved in this area of the site by chance is high.

Okladnikov found no evidence for a grave. He suggests that the original position of the skeleton, which he has somehow inferred from the distribution of bone, makes a good case for burial in a shallow pit. (The shallowness is probably deduced from the fact that the cranium lay a mere 25 cm below the contact with the overlying cultural stratum.) Neither the horns nor the sedimentary matrix nor the bones themselves support the inference of purposeful burial. No substantial evidence exists for mortuary ritual at this site.

REGOURDOU

Situated only 500 m from Lascaux in the Dordogne, Regourdou Cave, the upper portion of a complex karst system, has attracted attention as the site both of a Neandertal burial and of ritual involving the brown bear *(Ursus arctos)*. Bonifay (1964) describes a number of piles of rubble and wide-areal erosional events that he terms "tumuli" and "graves" and attributes to the efforts of Neandertals. The deposits include bone of various animals (including a complete bear skeleton) and the remains of a Neandertal adult (minus the cranium and the lower limb bones), all of which are said to have been buried purposely.

Regourdou presents a complex depositional history, beginning, for our purposes, with the opening of a shaft during the early Würm glaciation (Bonifay 1964:58). Wind and water have probably transported much of the sand-size sediment that forms the talus cone visible under the "chimney" (fig. 9). It is safe to say that the overwhelming majority of lithic sediments larger than sand-size are autochthonous: the roof appears to have been collapsing at a relatively rapid rate, judging by the large blocky rubble shown in the illustrations and the vast quantity of smallish (i.e., < 30 cm) rubble that forms the "tumuli" and ultimately filled the cave.

The impression one gets of the cave from the profiles (figs. 9 and 10) and the plan (fig. 11) is that of a roughly rectangular container with an ingress for sediments at the south end through the chimney and an egress into the lower reaches of the cave system to the south-west. From Bonifay's description of the history of the cave complex it appears that the sediments of interest at Regourdou are deposited in the very top of a vault that once formed the ceiling of a lower chamber. In fact, although it is not so described, the basal layer depicted in the profiles is most likely a large cone of roof collapse with its base in the lower cave. Bonifay (1962:58) reports that the original horizontal entrance to the lower part of the cave system was cut off by this pile of rubble, and in figure 10 it is apparent that the basal layer is large breakdown material that forms a steeply sloping pile beneath the vault. This kind of build-up is expectable as the ceiling gradually breaks down to create a vault (see Warwick 1976:fig. 3.16). Thus the chimney that opened at Regourdou during the Middle Paleolithic was the vertical extreme of a vault that had its origin well below the level of interest.

As with most such piles of rubble in caves, as particles broke down inside it and spaces between clasts closed up, the pile settled (see Jennings 1985:164). If this had occurred desultorily, it would probably have led to the opening of exits for sediments to the lower cavern, resulting in erosional events or phases. With water undoubtedly involved to a greater or lesser degree, there is the possibility that periodic flushing out occurred. The trend of the deposits is downward from the chimney to the south-west. An erosional event has removed sediments from Strata IV, V, and VI (see fig. 12) and created the substrate for the bear/Neandertal burial (IVA–D). In

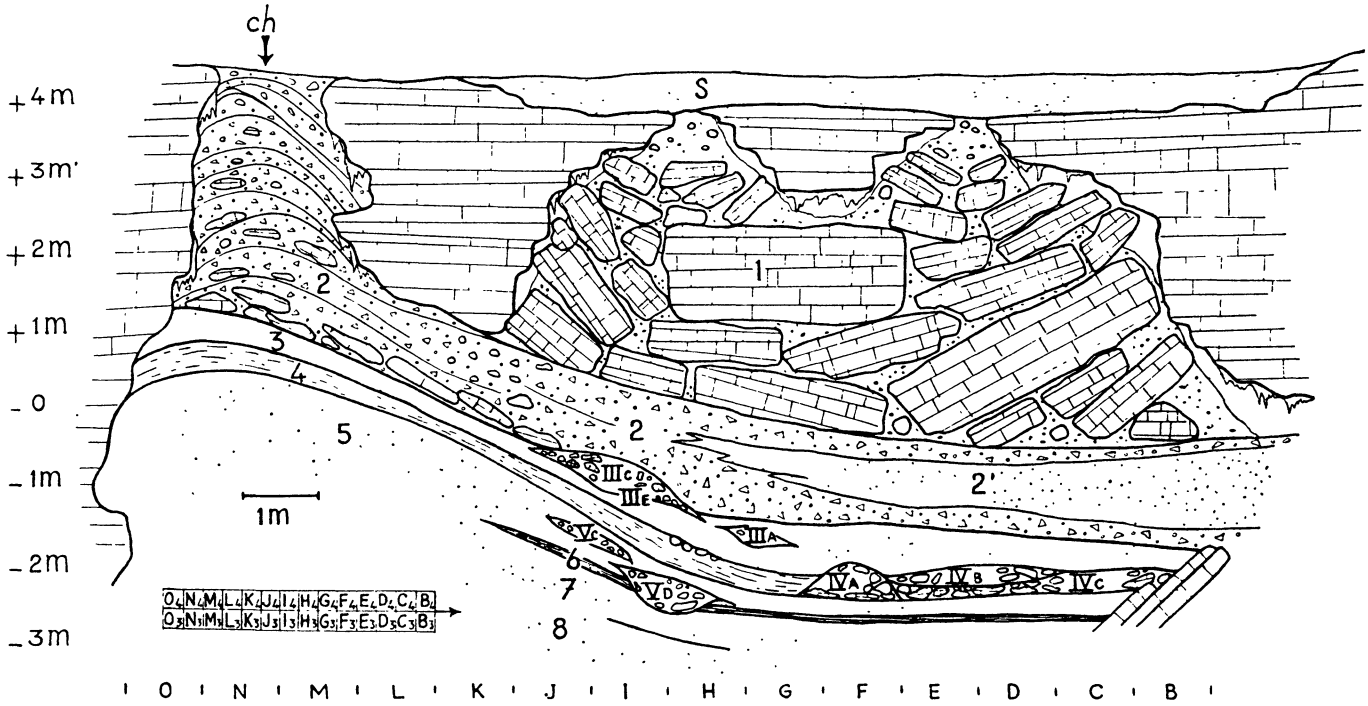

FIG. 9. *Regourdou, longitudinal section (Bonifay 1964:fig. 1, reprinted by permission of Masson, Paris).*

cross section the strata resemble an erosional channel or gulley. The boundaries of this "grave" are the western bedrock wall, the base of the talus cone, the southern bedrock wall, and the sloping east bedrock wall—in short, the entire floor of the cave, an area of approximately 12 m^2 on average 0.5 m deep (i.e., nearly 6 m^3 of sediment).

What evidence do we have that Neandertals dug this depression? The answer is probably to be found in an analysis of the origin and deposition of the sediments that remain. The sediments removed in creating the "grave" might easily have been lost in one pulse of water erosion. Water would always have sought the lowest point, the west wall, and, depending on the location of the opening to the lower chamber, would have exited there. The channel-shaped erosional cut shown in figure 12 may very well be the result of water erosion (although the possibility of wind deflation cannot be ruled out).

The formations that Bonifay sees as "walls," "graves," and "tumuli" are all easily explained by natural phenomena:

Feature VIIA (fig. 10) is a pile of rock, sand, and cinders 2 m in diameter and about 1 m high. In its centre were brown-bear bones, including a cranium, that had been protected by the stones. Parts of beaver, deer, and *Bos* were also found. These are said to have been "offerings." The pile accumulated during the deposition of Stratum VII. Its presence at the base of the talus suggests that at least some of the rubble that contributed to it could have rolled to rest there. Sediments are easily transported downslope by slopewash or solifluction, even when the slope is only 1–2° from the horizontal. Any clasts that fall from the ceiling onto a slope as steep as this talus cone will tend to be "rapidly" transported to its lowest part. Some of the clasts that make up VIIA probably fell from the fissure opening in the ceiling (see Jennings 1985:164 for a description of this process).

Feature VA (fig. 10) is a "grave" about 1 m deep and 1 m in superior diameter, containing a bear skull and other bone and stone, including a "pierced" stone. Possibly created rapidly, it nevertheless filled rather slowly with sediments characteristic of Stratum V. This suggests that it was created before V was laid down; otherwise there should have been a visible contact between the fill and V. One possible explanation for it is an hourglass effect. Fine sediments within larger, blocky rubble would gradually be transported down through the spaces between the larger clasts, perhaps into the lower part of the cave, creating a void into which overlying sediments would subside. Bone and other mobile sediments would naturally collect and be protected in such a depression. Its status as a ritual formation is evidently uncertain.

Feature VB (fig. 10) is a roughly arranged small stone "wall" that also contained the cranium of a brown bear and others, "possibly accidentally introduced." In profile this pile appears no different from any other. There is no need to invoke Neandertal behavior to explain it. Why Bonifay would say that some of the bone in this pile could have been introduced accidentally is unclear.

Feature IVA (figs. 9, 10, 12) is the "tumulus" in which the unarticulated, partial Neandertal remains were found. It rests against the "north wall," which in turn forms the large bear "grave." The remains lay on a bed of flat stone and were covered by others, among which

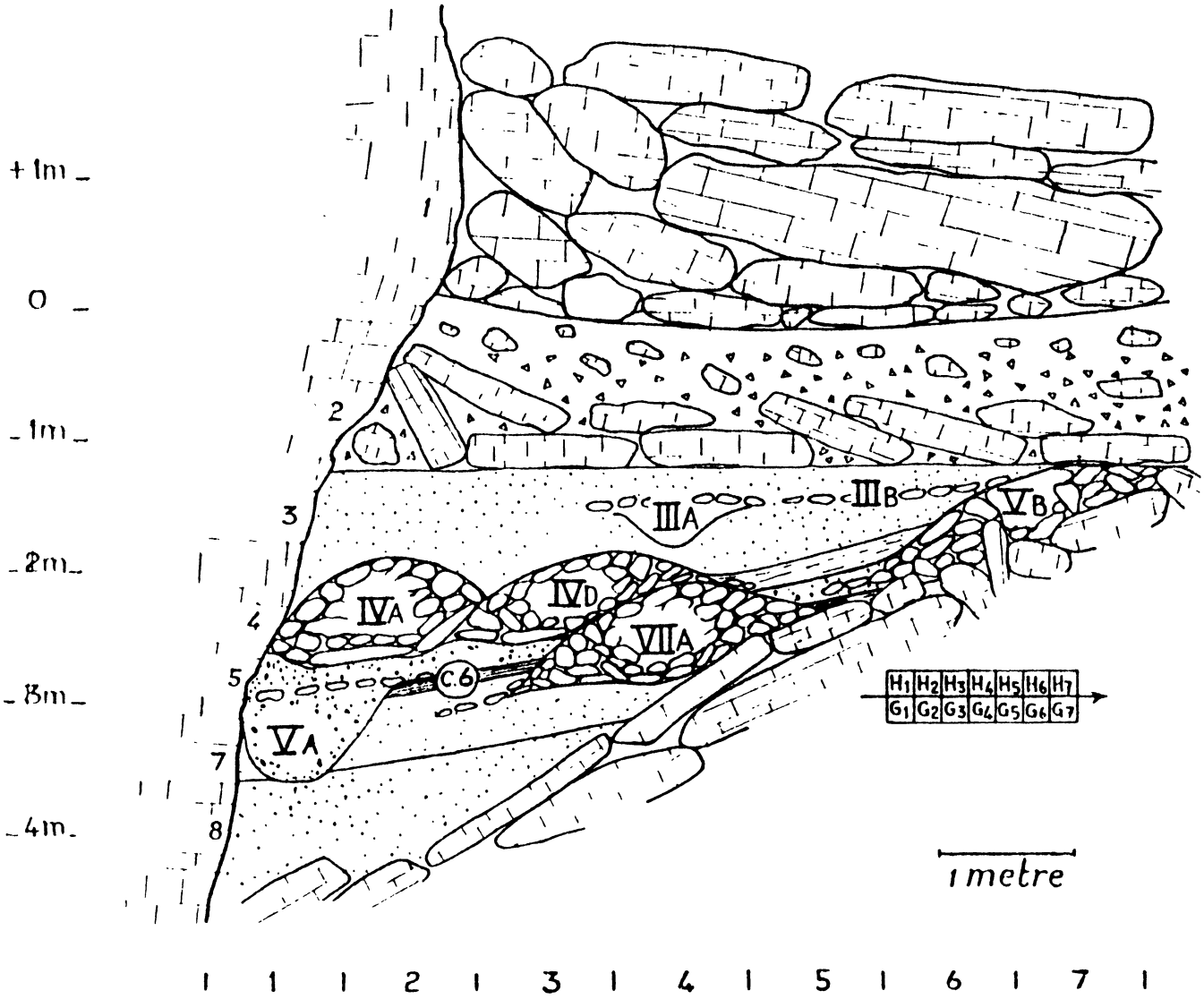

FIG. 10. *Regourdou, transverse section (Bonifay 1964:fig. 2, reprinted by permission of Masson, Paris).*

were "offerings" (cores, flakes, scrapers, a bear humerus) mixed with small rubble, sand, and cinders. Also included were other bones of bear and deer. It is curious that, in figure 12, IVA is shown filling a depression between the "north wall" and the eroded talus alluded to above (i.e., IV–VI). This would indicate that IV had been laid down and eroded before IVA began to accumulate. However, in figures 9 and 10 (sections at right angles to one another), IVA is shown as a typical dome-shaped pile of rubble like VB or VIIA. Also in figure 10, IV is shown as overlying IVA, which would mean that it *post*-dated that feature. There is no doubt that the stones of IVA collected during a period when either Neandertals were using the cave or their detritus was collecting inside as it fell down the shaft. The clasts that form IVA, with all the rest in this chamber, would have been subjected to downslope movement and would have come to rest at the bottom, where the floor levelled out. This process would have been hastened by the action of rapidly running water, which would have created the erosional event and the collection of rocks at the bottom of the slope.

Feature IVB (fig. 13) is a "coffer" about 1 m^2 "carefully constructed" with stones sometimes placed vertically against the wall and a "pavement" of stones at its base. It contained a number of bear bones, some showing cut marks. All parts of the skeleton are represented. These were covered with another "pavement" of stones on which were found three crania: one deer and two bear. There is nothing unusual about vertically oriented stones in this environment. If they happen to fall against something such as a wall or another stone, flat stones can land vertically (Kurtén 1976:88). The space delineated by IVB is in part created by the "north wall," and a space would be expected to appear where it does. Any rubble being transported downslope would come to rest against the "north wall"/IVA complex. The "space" that is called IVB would therefore naturally contain only

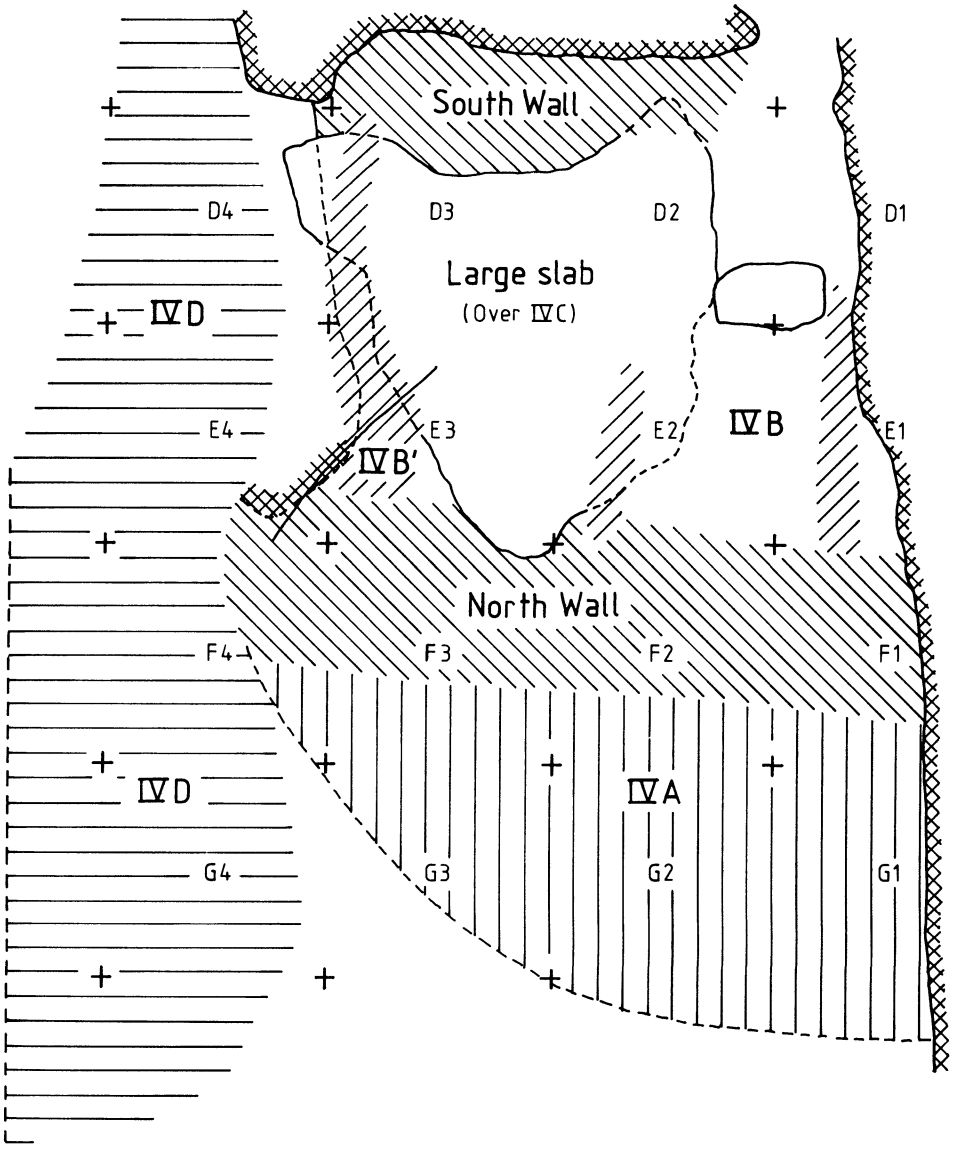

FIG. 11. *Regourdou, plan of excavation (after Bonifay 1962:diagram 22).*

those clasts that fell from the ceiling. We might expect any flattish stones to have come to rest flat inside the "grave," thereby creating the "pavement" that occurs in the middle. With so many parts of bear being discovered throughout the cave, we should begin to think about the possibility that this chamber was home to generations of hibernating bears. It seems likely that the three crania that have been preserved on the top of IVB would have been protected from traffic and subsequent destruction by being against the wall. Nothing about this feature suggests that it could not have occurred naturally.

Feature IVC (figs. 11–13) is the major bear "burial," "arranged" between two large blocks fallen from the vault but "dug out" between them. The "grave" is completed on three sides by "walls" of dry stone—the "north wall," the "south wall," and (presumably) IVB; the fourth is a large flat stone (once again, presumably standing vertically?). The "north wall" is nearly 3 m long and about 0.7 m high. The "grave" itself forms a rectangle 0.6 m by 1.5 m. It contained a complete bear skeleton that according to the excavator had been cut up before disposal; thus the diverse other elements and animal parts must have been introduced later. Also found was a "pierced" stone. All this was covered with a huge flat stone weighing approximately 850 kg. The "north wall," as mentioned above, could easily be accounted for by the fact that it lies directly under the node between the two vaults forming above (see fig. 9). A line of weakness in the ceiling might be expected to produce a linear talus formation. The "north wall," not shown in figure 9, lies along the east-west line in Transect F, according to the plan, and probably grades into IVA in places (if indeed these are different formations), forming a wide accumulation at the base of the main talus. Thus it too is a predictable occurrence in this environment—a combination of ceiling spalls falling in

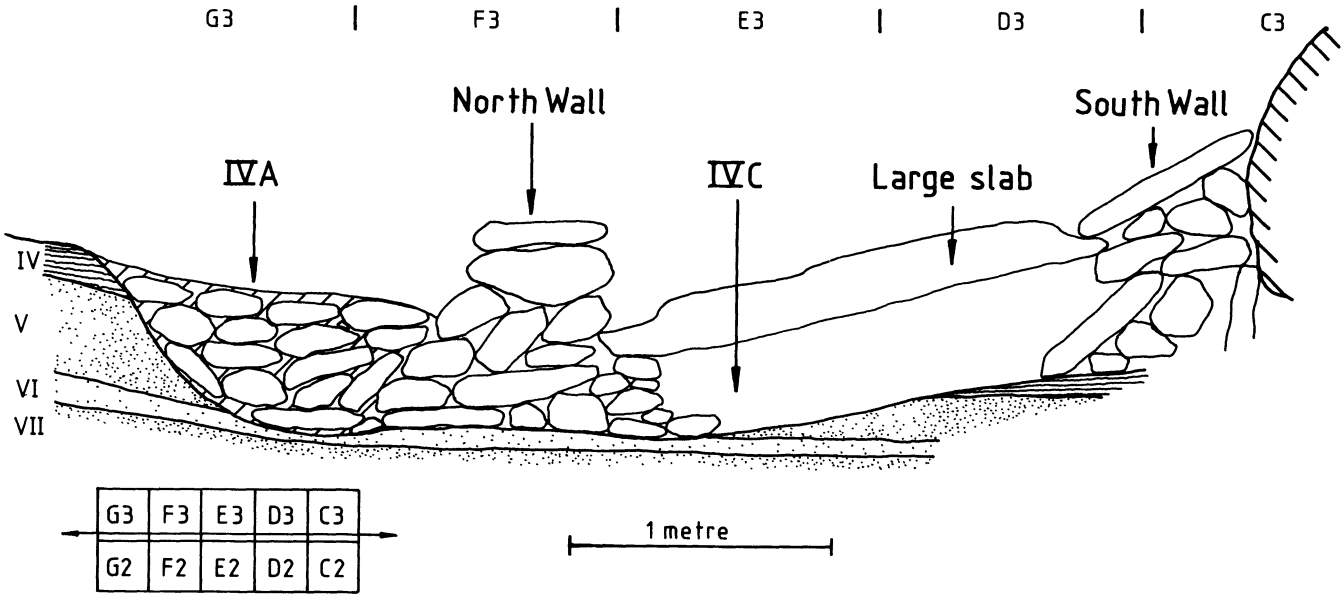

FIG. 12. *Regourdou, section through "burial" along north-south axis (after Bonifay 1962:diagram 23).*

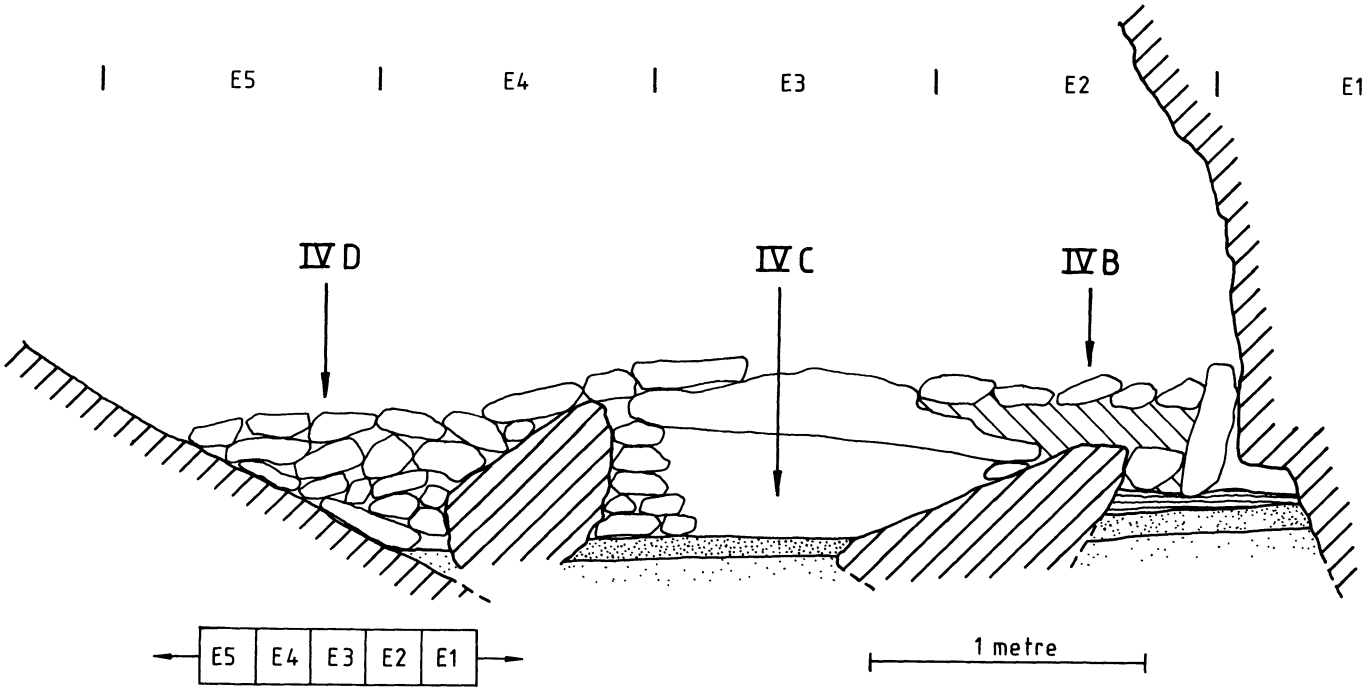

FIG. 13. *Regourdou, section through "burial" along east-west axis (after Bonifay 1962:diagram 23).*

place and clasts of similar origin coming to rest there. The presence of the huge stone slab is not without precedent in this cave (see figs. 9 and 10). That it occurs directly under the vault that is opening up in the southernmost part of the ceiling cannot be a coincidence. It need not have been hefted into place to cover a bear. That the skeletal parts were not in anatomical position suggested to Bonifay that the bear corpse had been cut up before its "entombment." However, if the bear had died on the spot, natural processes could easily account for disarticulation. The space beneath the block would not have filled up immediately with fine sediments (if indeed it ever did; none are shown in figs. 12 and 13). Instead, disturbances produced by, for example, animals or running water could have resulted in the displacement of the bones from their anatomical positions.

Feature IVD (figs. 10, 11, and 13) is a big, elongated pile of stones about 0.5 m deep and more than 4 m long. There is no reason to think that this pile, occurring as it does at the base of what appears to be a bedrock slope, is

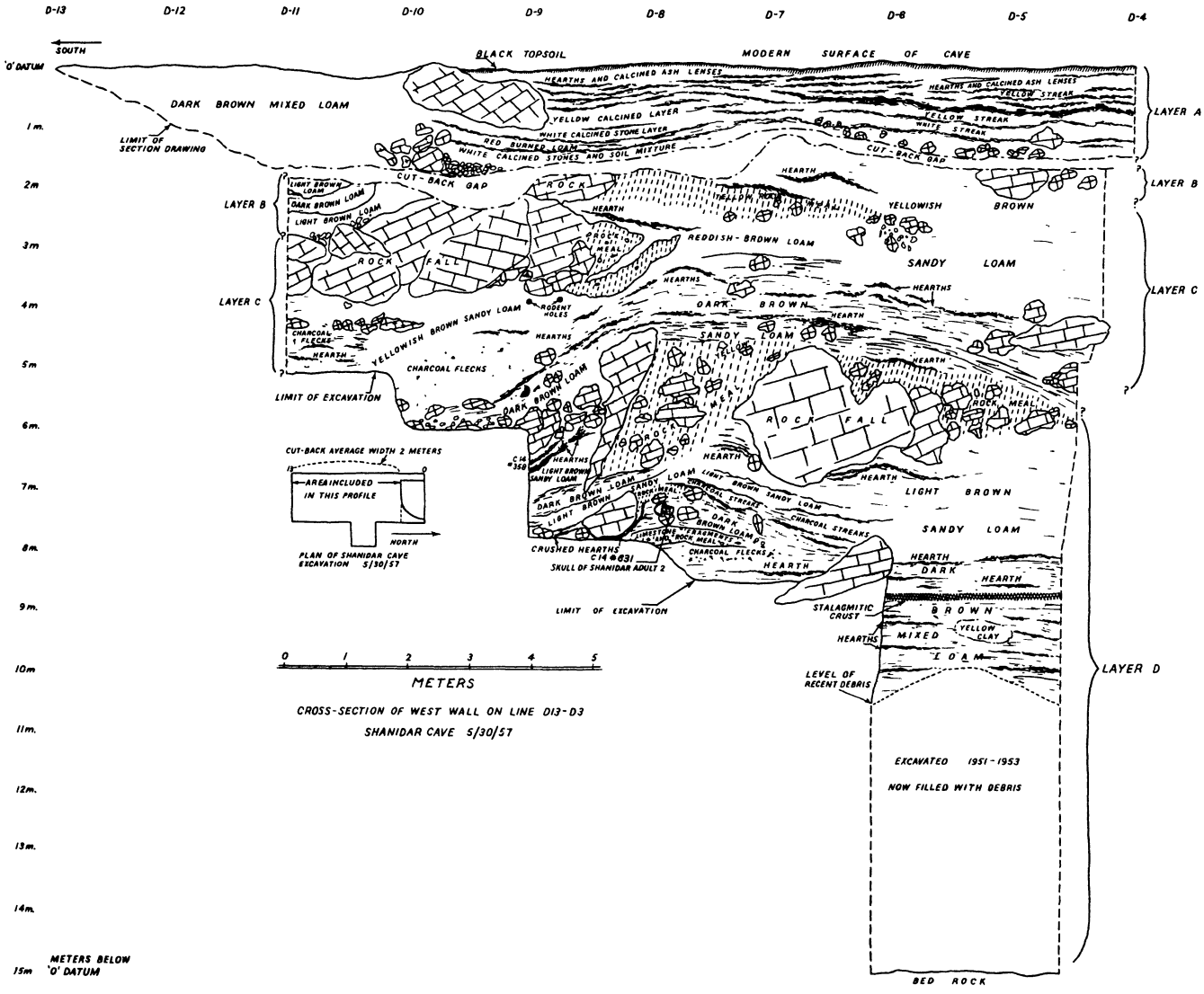

FIG. 14. *Shanidar 2, profile showing location (Solecki 1955 and 1960:fig. 8, reprinted by permission of the Smithsonian Institution Press).*

not another natural phenomenon. Its irregular shape in plan and its size, covering the length of the base of the slope, make natural process an overwhelming likelihood. Overall, the schematic quality of the "walls" in the plan suggests a little artistic license. For example, the line between IVA and the "north wall" is arrow-straight, yet it would be difficult from figure 10 to judge where one ended and the other began. Also, that the "north wall" is missing from the profile in figure 9 is a mystery, since it is shown in plan as butting up against IVA between Transects 3 and 4, the line along which the section of figure 9 is supposed to dissect the cave. All this makes the interpretation of conscious rearrangement of naturally occurring sediments hard to accept.

Feature IIIA (figs. 9 and 10) is a "grave" only 0.5 m in diameter and 0.3 m deep containing rocks and a circular flat stone. The origins of this feature are unclear. Like VA, it is apparently filled with sediments that accumulated over time, not at the time that the hole was eroded. Thus it is probably not a grave.

Feature IIIB (figs. 9 and 10) is a sort of "small stonework" forming a "pavement" covering several square meters. The substrate here is almost level. Downslope movement is very much reduced, and clasts that were falling from the ceiling were likely to remain where they fell.

To sum up, it would seem that the features described at Regourdou can be explained by bedrock breakdown, slope transport, and possibly water erosion. Where slopes existed, linear lags of rubble collected at the bottom; where fissures were opening, small domes of talus were created; elsewhere, large and small rubble occurred that disrupted the normal course of transport and deposition and led to anomalous but explicable morphologies. There is no reason to continue in the belief that any of the deposits at Regourdou are the result of ritual acts.

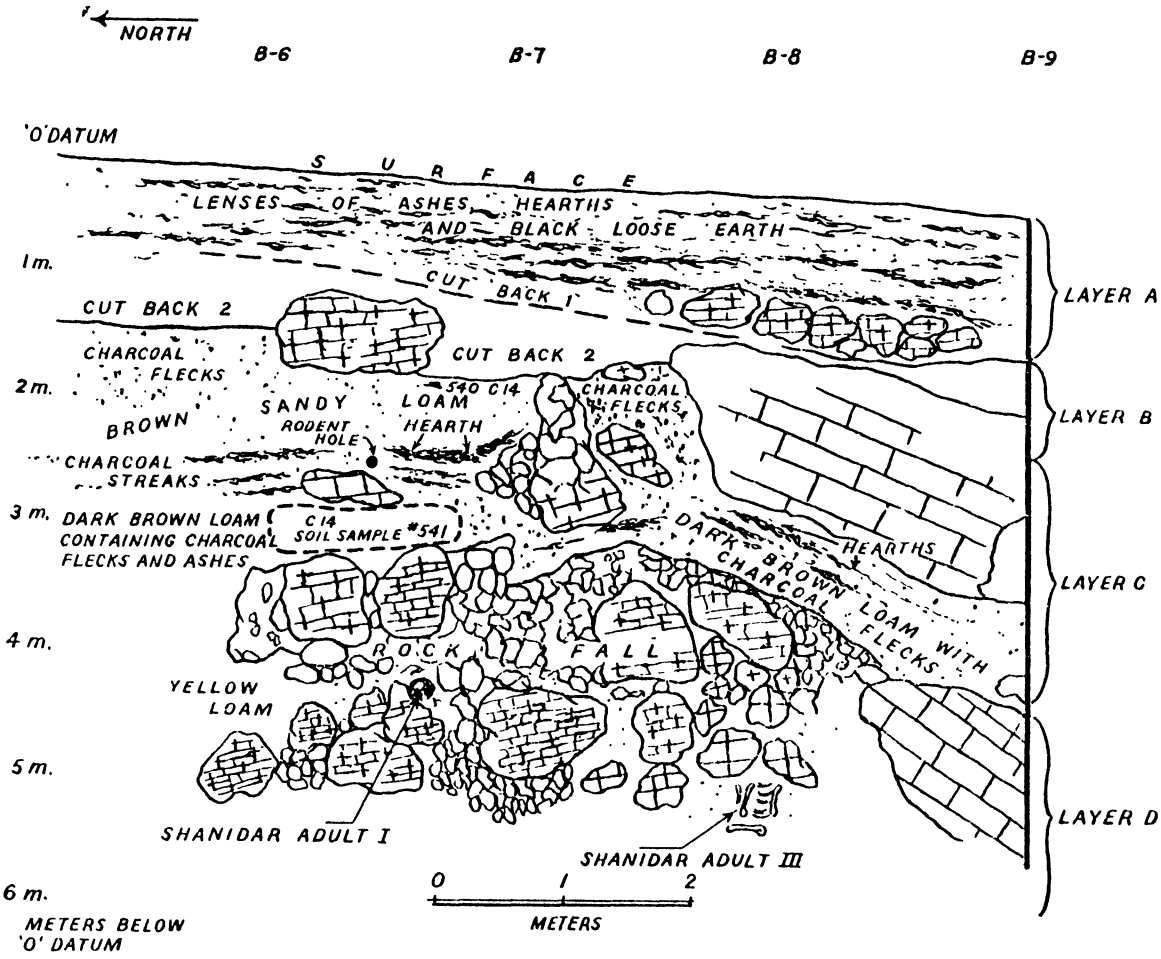

FIG. 15. *Shanidar 1 and 3, profile showing location (Solecki 1955 and 1960:fig. 3, reprinted by permission of the Smithsonian Institution Press).*

SHANIDAR

Shanidar, in Kurdistan, is a solution cave created in gray-brown Middle Cretaceous limestone (Solecki 1955:402). Active tectonism and solution have produced deposits in excess of 14 m deep inside the cave. The likely origin for much of the sedimentation in the cave is the high "crevice" that opens above the excavated portion, an incipient shaft that in all likelihood contributed to the preservation of the abundant Neandertal remains. The fortuitous collection of soil samples in the vicinity of Shanidar 4 and the subsequent discovery of a quantity of flower pollen in those samples led to the conclusion, some seven years after the excavation, that the individual had been buried with flowers. This brought a sharp turnaround in the interpretation of the cause of death and the reason for preservation of Shanidar 4. Originally, Solecki (1963:179) had considered Shanidar 4, 6, 8, and 9[3] to have been killed by rockfalls. Even before he changed his mind about Shanidar 4, however, he had suggested that the survivors of Shanidar 1 had purposely piled rocks on top of the ones that had killed and buried him.

Shanidar 1, 2, 3, 4, 5, 6, and 7 were preserved virtually complete, although reduced to bone meal in places by rockfalls; Shanidar 8 and 9 were incomplete, the latter consisting only of nine vertebrae. Only Shanidar 3 shows any lesion suggesting death prior to burial by rockfall—a partially healed puncture wound on the left ninth rib (Trinkaus 1983:414). Stratigraphic context for the skeletons was, apparently, difficult to discern. Only five so-called cultural layers were identified: A, B1, B2, C, and D. This means that the entire Middle Paleolithic sedimentary history of Shanidar Cave is subsumed under Layer D. Although color and clastic differences are noted on the profiles that show the depositional contexts of Shanidar 1, 2, and 3 (figs. 14 and 15), no meaningful behavioral distinctions are made. The Mousterian sediments at Shanidar appear jumbled and random, as one would expect in a cave that is "rapidly" degrading. There is clearly a great deal of collapsed material surrounding the three individuals illustrated.

3. The new numbering for the Shanidar Neandertals (after Trinkaus 1983) is employed here; Shanidar VII and VIII are referred to as 8 and 9 and the "Shanidar child" as 7.

No clear evidence for purposeful burial exists in the Shanidar deposits. There are no grave pits, no non–naturally occurring protective strata. Solecki infers, however, that the survivors of the deceased individuals were treating them with reverence. Of the rocks overlying Shanidar 1 he reports: "It looked like an unusual pocket of smaller stones among a lot of larger stones—a cluster, superimposed upon a layer" (1960:613). He continues: "I believe that survivors of the rockfall returned after a while and, seeing what had happened, heaped some loose stones, the closest at hand, over the unfortunate's remains" (p. 619). There seems to be no valid claim, other than the belief of the investigator, for ritual disposition of the corpse or of the overlying sediments. The size of roof collapse materials is random: humanly transported stones could not be distinguished from those that naturally fell there.

Shanidar 4 "lay in an occupational deposit of loose brown sandy loam soil, next to a large stone. . . . Stones of small size were found above the skeleton. . . . There were no large stones over it, but the skeleton was evidently crushed: it could have been redeposited in its present find position" (Solecki 1961:695). In the same area, but lower down, the remains of Shanidar 6, 8, and 9 were discovered. Solecki (1971:237) describes the overlying sediments as follows:

> There was a widespread covering of rockfall above Shanidar IV, consisting of about 1 foot 8 inches of broken, fragmented, and jumbled stones and rock meal. There were no large stones immediately above the skeletons. The skeletons of IV and of the associated remains appeared to lie in a niche, bounded on the south and east by large stone blocks. There was no doubt that the rockfall, which was widespread, had sealed in the remains. But it did not contribute to the death of the individuals in the niche, as I had mistakenly published in an earlier report. However, it is still conceivable that the crushed state of the bones could in part have been due to the force of this subsequent rockfall. The average thickness of the fallen zone was about 10 inches. The nature of the soft soil and the position of the stone blocks leads me to believe that a crypt had been scooped out among the rocks, and the individuals had been interred and covered over with earth.

Even though no large rocks occurred above Shanidar 4, a covering of debris 0.5 m deep could easily have killed and interred the individual. Solecki infers that the remains of Shanidar 6, 8, and 9, an infant, were interred at the same time: "Shanidar IV and VI must have obviously been contemporary, since one was on top of the other" (Solecki 1961:696). He reports, however, that they were separated by 19 cm of sediment. In the absence of good stratigraphic evidence it is unlikely that the inference of multiple burial can hold up. In the first place, superposition is usually taken to imply temporal separation, not contemporaneity. Solecki's belief that the niche had been "scooped out" is unsubstantiated. A natural niche such as the one in which Shanidar 4 met his demise is a logical place in which to take shelter. Since contemporaneity cannot be demonstrated, the suggestion that all of these individuals were "interred" at once is specious. Recently developed, refined radiometric techniques could be used to determine, without destroying much bone, if there is any chance that these individuals were contemporary.

But what about the pollen? A preliminary environmental reconstruction was based on 121 pollen grains gleaned from one sample in Layer D: about a third of them were date palm (Solecki and Leroi-Gourhan 1961:734). Whereas the usual pollen counts were quite low relative to the thousands that can be expected from peat bogs or lake bottoms, the tens and hundreds from the vicinity of Shanidar 4 were an unusually high number for the cave. Moreover, the presence of an anther in the sediments suggested that whole flowers were being transported into the cave and deposited in the niche along with the deceased. Because it was already believed that purposeful disposal was a possibility, the discovery of flower pollen convinced the investigators that Neandertals had buried the dead with flowers at Shanidar.

This interpretation ignores the most probable agent for the deposition of the flowers: wind. Shanidar is a large cave. Its mouth is 25.00 m across and 7.92 m high. Inside, it widens to 53.34 m, and the vault in the ceiling is 13.20 m high. There is a good chance that any strong wind blowing at the mouth of the cave would carry tender flowers as well as twigs and branches some distance into the cave. Changing climate, lack of systematic sampling, or the ephemeral character of flowering could all have contributed to the reported absence of similar pollen concentrations elsewhere in the cave. The relative improbability of co-occurrence of the right events (i.e., the right season, aeolian deposition, protection from trampling, and burial by roof-fall ensuring preservation) may also help to explain why there were no similar pollen concentrations in any of the other samples. The rodents that left their fossil burrows everywhere around the skeletal remains are another possible transport agent for the flowers; most rodents build nests of vegetation (Hanney 1975).

Even if arguments for alternative transport agents for the pollen are unacceptable, the question of lack of demonstrable association of the pollen with the burial must be raised. The two samples richest in pollen were "taken from *about the same level* on which the skeleton lay" (Solecki 1971:247, my emphasis). Without having identified a stratum that has distinct horizontal and vertical boundaries, how can association be inferred? That the pollen occurs in proximity to the skeletons is undeniable. That the skeletons of Shanidar 4, 6, 8, and 9 occur in proximity to one another is undeniable. It is not easy, however, to accept Solecki's subjective interpretations of ritual protection and purposeful deposition of the flower pollen. Solecki (1960:606) was unable to locate a burial pit for the "Shanidar child," and acceptable evidence for purposeful burial at Shanidar is similarly elusive.

SUMMARY

At La Chapelle-aux-Saints the inference of burial is based on a depression of unknown origin in the basal sediments. At Le Moustier two pits are evident that may or may not have been created for the purpose of burying a child. At La Ferrassie numerous remains are preserved, all of them in ambiguous contexts. At Teshik-Tash the remains could be a carnivore assemblage rather than a purposeful burial. At Regourdou the single partial Neandertal skeleton and complete though disarticulated brown bear were probably not buried purposely. The evidence from Shanidar has problems of stratigraphy and association and depends on speculation about depositional processes. At the same time, the many Middle Paleolithic localities that have yielded only fragmentary Neandertal fossils bear silent witness to the possibility that something other than purposeful burial has contributed to the preservation of relatively intact remains.

Conclusions

From the above examination of published accounts of the archaeological evidence for burial and ritual in the Middle Paleolithic it is evident that processes other than purposeful human behavior may have produced the deposits in question. In many cases simple and likely explanations have been ignored in favor of complex scenarios invoking enigmatic purposeful behavior. These scenarios often require elaborate arguments from tenuous premises. The relative ease with which they have been accepted and the confidence with which they have been employed in reconstructions of Neandertal lifeways may be the result of a pre-1960s discipline-wide naiveté with regard to the mutually reinforcing processes of geomorphology and taphonomy. Only recently have improvements in method and technique enabled archaeologists to be critical of the kinds of observations reviewed here. Such criticism is not intended to vilify; it is made in the hope that it will facilitate a much-needed reappraisal of Neandertal's cultural capacity. Removing the necessity of accounting for sophisticated spiritual behavior among Neandertals may make it easier in the long run to explain human cultural evolution. If the recent paper by White (1982) is any indication of the lack of agreement on the nature of the differences between Neandertal and anatomically modern *H. sapiens*, then this study should improve the situation. Questions of genetic interchange between the two subspecies may never be answered, but the removal of mortuary ritual from the behavioral repertoire of Neandertal may make the observed disconformity in material culture at the Middle/Upper Paleolithic boundary a little easier to understand.

It cannot be argued that, because there is no physical evidence for burial in the archaeological record of the Middle Paleolithic, Neandertal did not bury the dead. At least until good evidence is recovered or can be gleaned from the as yet unmined field notes of the excavators, however, the working hypothesis should be that Neandertal did not bury the dead or otherwise transform sediments in the course of performing ritual. The onus is now on the fossil record to demonstrate the reality of burial and ritual among Neandertals.

Comments

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Attempting to subject received knowledge that has achieved the status of dogma to the close scrutiny made possible by more recently acquired knowledge is necessary and thoroughly admirable. If reexamination shows that long-accepted "facts" are not based on solid data, the consequent revision is a clear contribution to knowledge. Gargett's conclusions have prompted me to look more closely at these data for La Ferrassie. With respect to at least two of its reported burials, I am forced to conclude that our traditional understanding rests on something quite a bit more solid than what Gargett describes as "a pre-1960s disciplinewide naiveté" and that no considerations raised by him either force or, indeed, authorize us to believe that our predecessors got things all wrong.

Because the first discoveries of Neanderthal skeletal remains at La Ferrassie, in 1909 and 1910, left unresolved the question of whether or not the bones were in intentionally dug graves (see Breuil's 1909 field notes, reproduced in facsimile by Heim [1976:46, fig. 13], and Peyrony's [1948:55] discussion of the pros and cons), the initial discovery in August 1912 of what might be two (or two more) Neanderthal burials was treated by the excavators, Louis Capitan and Denis Peyrony, with appropriate caution and restraint. The excavation of the two pits later shown to contain the remains of three individuals (as summarized by Gargett) was postponed until an ad hoc commission of experts could be convened at the site to witness it. The commission was composed of Pierre Paris, G. A. Blanc, Henri Begouën, Henri Breuil, Jean Bouyssonie, and Hugo Obermaier (Peyrony 1934:30). Modern specialists will recognize that the membership of the commission included several of the most experienced and most competent Palaeolithic archaeologists of the early 20th century. Beginning on August 8, 1912, the commission witnessed, at trench-side at La Ferrassie, the excavation of the two pits in question. When the operation was completed, the members of the commission made their observations and conclusions a matter of formal record by signing a report (*procès-verbal*) drafted by the Abbé Breuil and signed also by the excavators of record, Capitan and Peyrony.

The full text of the report, published later by Peyrony (1939:237–38), is directly relevant to an evaluation of

Gargett's conclusions. The commission understood that the most important general question was whether or not evidence existed of Neanderthal funerary ritual and that the specific question was whether the pits containing the bones were artificially dug graves. Its answers were categorical: "The existence of trenches artificially dug and later filled was shown absolutely" (1939:237, my translation)[1] and "There is, then, in the clearest fashion, the proof of a funerary ritual. That point had been much debated. These excavations establish it in a fashion that allows of no doubt" (1939:238).[2]

What late-20th-century understanding of geomorphology and taphonomy permits or requires us to conclude that the early-20th-century competence of Capitan, Peyrony, Breuil, and others was inadequate? I find nothing of this sort in Gargett's discussion of the 1912 finds at La Ferrassie. Indeed, his criticism seems to rest primarily on a misunderstanding. He states that there was "no recognition of a new stratum created when the pits were filled." He apparently believes that both pits were filled with *two* strata, in superposition—light-colored gravel beneath and a darker, artifact-rich sediment above. He uses this assumed stratigraphic succession within the pits to suggest a scenario whereby human skeletal remains "deposited in (possibly) naturally created depressions" were covered, over an indefinite period of time, by successive episodes of "slumping and infilling" by first the gravels and then the darker Mousterian midden. I believe that this scenario, probable or improbable, is essentially irrelevant because I find no evidence that the pits were filled with the two superposed strata that Gargett attempts to explain. The commission's report (Peyrony 1939:237) states that the pits were "filled with a mixture, in about equal parts, of the black earth of the Mousterian level located above and of the underlying gravel."[3] Context and French grammar indicate clearly that the superposition referred to is the general stratigraphic succession of that part of the site, not some characteristic of the pit-filling (*placé* modifies *foyer*, not *terre*). The use of the term "mixture" (*mélange*) cannot reasonably be understood to describe a case of clear stratigraphic superposition, but it is exactly the term that would be used to describe the back-filled sediment in holes that had been dug through two strata of different colors and compositions. Any possible ambiguity seems to be removed by the penultimate sentence of the report (1939:238): "The discovery of these two small skeletons permitted the demonstration, in an irrefutable manner, of the existence, from the time of the Mousterian epoch, of trenches dug artificially for the purpose of placing bodies in them, which were then covered with the earth coming from the digging."[4] The "new stratum" was explicitly recognized at the time of excavation, and its obvious significance was fully appreciated.

In sum, I find no evidence that the eight scientists who reported Neanderthal burials at La Ferrassie in 1912 were, with respect to the crucial questions at issue, incompetent (by the standards of either their time or ours), naive, or patently in error. A truly conservative view of the fossil record cannot (at least, not yet) accept Gargett's allegation that "there is no physical evidence for burial in the archaeological record of the Middle Paleolithic."

1. "L'existence de fosses artificiellement creusées et remplies ensuite, était d'une évidence absolue."
2. "Il y a donc là, de la façon la plus nette, la preuve d'un rite funéraire. Ce point était encore très discuté. Ces fouilles l'établissent d'une façon qui ne peut laisser aucun doute."
3. "remplies d'un mélange, à peu près par parties égales, de la terre noire du foyer moustérien placé au-dessus et du gravier sous-jacent."
4. "La découverte de ces deux petits squelettes a permis de démontrer, d'une manière irréfutable, dès l'époque moustérienne, l'existence de fosses creusées artificiellement pour y placer les corps, puis recouverts de la terre provenant du creusement."

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Gargett's contribution is another in a series of assaults on the notion of "modern" cultural behavior in the Middle Paleolithic (see also Chase and Dibble 1987). It successfully dismantles the argument for intentional burial of the dead at several well-known Eurasian Neandertal sites and goes on to suggest that the apparent lack of "sophisticated spiritual behavior" on the part of Neandertals allows us better to differentiate Neandertals culturally from modern forms of *Homo sapiens*. While we do not dispute Gargett's reanalysis, we do take issue with his conclusions in respect of the way in which they bear on the sticky problem of biocultural disconformity at the Middle/Upper Paleolithic transition.

It seems to be true that Neandertals did not regularly bury their dead, nor do they demonstrate "modern" symbolic behavior in their adaptations, at least insofar as that can be monitored by select categories of data in the archaeological record (Chase and Dibble 1987). Any analysis of Neandertal symbolic behavior should, however, be juxtaposed with analysis of that of morphologically modern humans. Gargett mistakenly assumes that morphologically modern humans in general had a symbolic component to their behavior and that the lack of evidence for purposeful burial by Neandertals can be used to clarify behavioral distinctions between these hominids. This line of reasoning is unwarranted.

If one compares the behavior of Neandertals en bloc and morphologically modern humans en bloc over the Middle and Upper Paleolithic of Eurasia, there does indeed appear to be a difference between these periods in the incidence of purposeful burial and of graveside ritual and intentional inclusion of grave goods (Harrold 1980; see also Clark and Neeley 1987). However, a coarse-grained comparison like this can be misleading given the unequal lengths of time represented by the two periods (ca. 200,000 years for the Middle Paleolithic, ca. 35,000

years for the Upper), tending to mask variation within periods while deemphasizing similarities between them. In addition, our understanding of the transition itself is limited by differences in paradigmatic biases that scholars from different research traditions bring to bear on the issue (Clark and Lindly 1988, n.d.). We have argued that the evidence points to both biological and cultural continuity across the Middle/Upper Paleolithic boundary and the transition from archaic to modern *H. sapiens* and that these transitions did not coincide in time (see also Wolpoff et al. 1988). Moreover, there is growing support for the idea of no qualitative differences and only clinal changes in adaptation between the Middle Stone Age/Middle Paleolithic and the early Lower Stone Age/Upper Paleolithic (Chase 1986, n.d.; Simek and Price n.d.; Simek and Snyder n.d.; Brooks 1988; Svoboda 1988, n.d.; Geneste 1988; Boeda 1988; Straus n.d.; Clark and Lindly 1988, n.d.). In short, the apparent differences between Neandertal and morphologically modern human burial practices do not stand up to closer inspection.

In response to a related paper on Eurasian Middle Paleolithic symbolism that relied heavily on burial data (Chase and Dibble 1987), we examined evidence for symbolic behavior (including burial of the dead) from Middle Paleolithic and Middle Stone Age sites in southwestern Asia and North and South Africa associated with the remains of what are claimed to be morphologically modern humans (Lindly and Clark n.d.). We found no evidence for symbolic behavior, nor could any differences in archaeological assemblages be detected between sites associated with archaic *H. sapiens* and sites associated with morphologically modern humans. We concluded that neither archaic *H. sapiens* nor Neandertals nor morphologically modern humans demonstrated symbolic behavior prior to the Upper Paleolithic and that evidence for symbolic behavior cannot be correlated with hominid taxa. In other words, that Neandertals apparently did not purposefully bury their dead has nothing whatever to do with the fact that they were Neandertals. Conversely, the systematic occurrence of this kind of behavior in the Upper Paleolithic is not related to the appearance of morphologically modern humans.

A hypothesis of no differences in symbolic behavior between archaic and morphologically modern humans prior to the Upper Paleolithic calls into question conclusions such as Gargett's or at least requires their substantial modification. It also lends support to the position of multiregional-continuity advocates (e.g., Wolpoff, Brace) who argue that archaic *H. sapiens* cannot be relegated to an evolutionary dead end in the biocultural development of modern humans and that there was substantial gene flow across the transition.

In sum, Gargett has presented a convincing reanalysis of the depositional contexts in which many well-known Neandertal "burials" occurred. We do not disagree with his results. However, the argument that his study helps to clarify cultural differences between Neandertals and morphologically modern humans is unfounded. Neither Neandertals nor pre–Upper Paleolithic morphologically modern humans purposefully buried their dead. An understanding of this aspect of the development of symbolic behavior must be sought outside such a dichotomy.

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The problems encountered in the study of Palaeolithic burials are numerous, but the loss of information over the millennia between burial and excavation should be the same for this archaeological structure as for any other. The situation is complicated by the difficulty of learning anything from 19th- and early 20th-century excavations, in which very little information was recorded; human remains were considered important in themselves and the archaeological context hardly noticed. Gargett's aim of questioning the reality of Middle Palaeolithic burials and exploring their context is well founded, though most of what he says about the taphonomy of cave deposits and the dynamics of cave sedimentological mechanisms is already well known. He hopes to show, through a meticulous review of old texts about some Neandertal burials, how questionable these burials are. None of these has really been considered conclusively demonstrated (Quechon 1971), and the latest references for Shanidar are missing (Stewart 1977).

Leroi-Gourhan (1964) pointed out long ago that for each case of possible "burial" it is necessary to take into consideration the whole context. Concluding his review of Middle Palaeolithic burials, he found the evidence inconclusive. He explained, "It appears that two mortuary situations can be identified for Paleanthropians: devoured or buried. The first concerns numerous ill-assorted fragments from which one can establish only that the treatment they underwent was the same as for animal bones devoured by men or beasts. The second implies that the bodies were sheltered, either by natural causes such as rockfall or by burial or covering with heaped-up earth and stones. Since this statistical statement is complemented by few positive facts, and since the Neandertals give us some inkling of non-technical concerns . . . , the scale should positively tip in the direction of true funerary practices, about which nothing precise can yet be said" (p. 59, our translation).

Neandertal remains consist of a few more or less well preserved skeletons and numerous dislocated bones in non-funerary contexts. Strangely, we observe exactly the same situation during the middle Neolithic in northern France and western Germany, where no scholar has called into question the *sapiens sapiens* nature of the populations concerned. Because he often confuses ritual and burial, Gargett does not prove anything about Neandertal behavior.

Instead of this new description of old studies, one would have expected a search for new data and new per-

spectives. For instance, why are late Middle Palaeolithic human remains much more numerous than earlier ones? In reviewing all these reports, why did Gargett not determine the exact or possible position of each skeleton? A serious look at body position can tell us much. The literature provides no unequivocal association with a burial trench or any other structure or with grave goods; only the study of bone articulations, body position, and skeletal taphonomy may help to answer the question of burial, providing information on the length of time the corpse remained exposed. Furthermore, we cannot understand why the new discoveries from the southern Levant (Bar-Yosef et al. 1986) and from France, such as Kebara, Qafzeh, and St.-Césaire, have not been reported and why there is no word about La Quina, Skhūl, Tabūn, or Amud. We are somewhat disappointed by the conclusion, too. "Something special" happened in the Middle Palaeolithic fossil records to preserve some complete specimens. Gargett suggests that this "something special" may not have been purposeful burial, but he fails to tell us what it was.

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We have difficulty finding any scientific merit in this paper. Gargett appears to have no direct familiarity with the sites he reviews, is selective in the cases he covers, lacks historical perspective, and ignores important discoveries that demolish his argument. Recent finds resulting from modern excavation procedures at Saint-Césaire (Lévêque and Vandermeersch 1980) and Kebara (Bar-Yosef et al. 1986) and earlier cases from Roc du Marsal (Bordes and LaFille 1962) and Amud (Suzuki and Takai 1970) indicate the intentional burial of specimens possessing a Neanderthal morphology. Gargett makes no mention of these, concentrating on older finds more vulnerable to criticism by modern standards.

Regardless of his contentions, there is reasonably good evidence for intentional burials in the earlier excavations. The completeness of numerous Neanderthal remains is in itself an indication of intentional burial. In Europe and the Near East prior to the Mousterian there are only isolated human finds, no articulated skeletons. With the Mousterian, reasonably complete skeletons of adults, children, and neonates become relatively common, at least in Western Europe and the Levant. Even without *any* archaeological information, the change in skeletal inventories suggests that human burial began at this time. In addition, we know of no evidence for any carnivore activity associated with the Neanderthal articulated skeletons. If all these skeletons derived from natural burials, it is inconceivable that none would have been disturbed by carnivores. What kind of sedimentological processes in a cave or shelter context (whatever the temperature) could so rapidly have covered a corpse that no carnivore would have noticed it? Despite Gargett's speculations, the number of complete Neanderthal skeletons of all ages makes natural burial in every case incredible.

We disagree with the criteria Gargett proposes for intentional burial. The act need not be complicated by the inclusion of grave goods; disposal of a corpse in a pit (even a natural one) and covering it with dirt or rocks constitutes a mortuary rite. Burial of this kind does not imply complex belief systems but may reflect simple, basic feelings related to fear of the deceased, protection from scavengers, or affection for the person who died.

Gargett's reinterpretation of the sedimentary sequence of the French Mousterian sites is far less probable than the ones offered by the original excavators. The scheme he proposes for La Chapelle-aux-Saints is perhaps the most questionable. Readers should study figure 3 to see if it conforms to his description of the pit's being situated "where the slope of Stratum 5 changes from steep to level to steep again." Also, we know of no example of a naturally produced rectangular, straight-walled, flat-bottomed pit in the middle of a karstic shelter. That such a natural phenomenon would have occurred and a skeleton would have found its way into it is so unlikely as to make it impossible to consider seriously that the pit sunk into the marl was not the result of deliberate human activity. The manner in which the skeleton lay, on its back, one arm folded and legs flexed, is a strong indication of intentional burial. This seems an unlikely position for accidental death and, in any event, is one that is repeated in numerous other interments. Beyond this, the interpretation becomes speculative. We admit that the nonhuman bones, ochre fragments, and stone tools found in the fill around and above the skeleton do not unequivocally represent grave offerings. They may well be part of the surrounding midden that the Neanderthals used to backfill the burial pit. On these points the excavators may have gone too far. In fact, the Bouyssonies and Bardon expressed some hesitation about this (1908:518), though they had none about the evidence for intentional burial (which they stated in italics [p. 516]).

Admittedly, the excavations at La Chapelle-aux-Saints and La Ferrassie lacked the precision of modern ones, but the excavators were not unaware of the controversial nature of the claim that the Neanderthals buried their dead. In fact, there was still at the time some doubt about the association of humans and fossil mammals, and de Mortillet had argued in 1885 that funeral customs did not begin before the Neolithic (Roche 1976:16). Furthermore, Henri-Martin, excavating in the early 1900s at La Quina, denied the existence there of human burials (1936:22). It follows that French prehistorians were cautious in claiming the discovery of a Neanderthal burial. Delporte (1976:11), who reexcavated a portion of La Ferrassie, has commented on the "quality of the work" accomplished by Peyrony, Capitan, and others during this formative period of French archaeology. Moreover, in a review of these old excavations Vandermeersch (1976:

727, our translation)[1] has summarized the evidence for Neanderthal burials as follows: "Despite a certain lack of information—sketchy plans, absence of photographs, etc.—the French Mousterian sites demonstrate beyond a doubt that burial was practiced by the Neandertals." Given the consensus of French prehistorians who have worked at the sites and the other evidence reviewed here, why should anyone take seriously this paper written from the armchair?

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Neanderthal burials have provided a rich source for comments about ancestral behaviour. These have included remarks on the "first flower people" (Solecki 1971), the "missing body" in the Combe Grenal funeral pit (Bordes 1972:134–37), and Neanderthal child care (Rowlett and Schneider 1974). The careful analyses by Binford (1968) and Harrold (1980) showed what could be done if burial was accepted, thus opening up the data to investigation by the methods of mortuary analysis which are commonplace for later prehistory.

I have always been suspicious of these data, since, as Gargett points out, there is often a lack of adequate documentation to support the claims for either graves or the goods apparently associated with them. Stories of reexcavations intended to re-create the moment of discovery for visiting dignitaries (Binford 1968:140) do not inspire confidence. Why the isolated Grotta Guattari cranium should count as a burial (Harrold 1980:199) has always eluded me. In the case of Shanidar, Gargett is probably right that wind accounts for much of the pollen, aided perhaps by the fact that "two of our workmen carried flowers in their sash bands. In the 1956–57 spring season, one of them inserted yellow narcissi into the hollow handles of his pneumatic-tired steel wheelbarrow, which made an incongruous picture" (Solecki 1971:93–94). Turner and Hannon (1988) have pointed out that the analysis of pollens in cave sediments is valueless unless they have been thoroughly assessed for taphonomic factors. Gargett's insistence on a rigorous sedimentological approach is welcome and undoubtedly reveals how shaky are the data on which some cherished reconstructions rest.

I would like to have seen him tackle the Near Eastern sites, where the documentation for the skeletons/burials is generally better. Since he does not deal with these, I think it premature for him to conclude that "there is no physical evidence for burial in the archaeological record of the Middle Palaeolithic." Moreover, the excavated skeleton from St.-Césaire may, when published in detail, shed important light on several of the questions he raises for the Western European material. However, even state-of-the-art field techniques will never put us in the position to evaluate such a meaningless concept as "enigmatic purposeful behaviour," which he currently sees as the unfavourable alternative to "simple and likely explanations" obtained from sediments and the sequence of site formation.

Consequently, while welcoming his critical approach and advocacy for cave sedimentology, I disagree with his choice of Neanderthal "burials" to argue his case. In focusing on some possible archaeological howlers he is in danger of missing the point about the few complete Neanderthals he does discuss. Burial is largely a red herring in these cases and has more to do with our cultural preconceptions of what we should find. Good excavation will never let the body "speak for itself" and so solve these burial issues. Instead, the importance of the Neanderthal "burials" is that we have complete skeletons at all. Irrespective of whether sediments are transformed in the course of ritual and ignoring speculations on the significance of burying the dead, the appearance of complete skeletons in deposits dated after the last interglacial is of considerable interest.

In the first place, the European human fossil record prior to the last interglacial is extremely fragmentary. This goes for both cave and open locations. The most complete skeletons are Ehringsdorf 7 and 8, described by Oakley, Campbell, and Molleson (1971:213) as juvenile and child. Even these, however, are extremely fragmentary when compared with the later Neanderthal "burials," and the rest, such as Swanscombe, Biache St. Vaast, Petralona, Mauer, Steinheim, Bilzingsleben, and Arago, are truly bits and pieces (Gamble 1986:table 5.3).

Secondly, complete skeletons are geographically restricted to local areas in southwestern Europe (Gamble 1986:fig. 3.1). Elsewhere, for example, at Hortus, Krapina, and Gánovce, Neanderthal remains from caves and open sites are highly fragmentary. One possible reason for this is the greater number of carnivores found at these other sites. I have argued that this reflects the contribution of topography and continentality to the selection of different carnivore coping behaviours within Europe (Gamble 1984). Moreover, the levels that contain the Neanderthal fragments here have not only large numbers of carnivore remains but also high species diversity (Gamble 1986:tables 7.3, 7.6–7.8), whereas the opposite holds for the southwestern sample. It is therefore possible that the distribution of complete skeletons is another aspect of these different patterns of carnivore behaviour. The correlation at a regional scale between complete skeletons and few carnivore remains can also be seen in the Near East at the same time and in caves and open sites of the European Upper Palaeolithic (Mussi 1986; Gamble 1986:tables 7.4–7.6).

Understanding why complete skeletons appear during the early last glacial calls for a wider taphonomic study than just the consideration of the sediments. Integrating the lines of evidence suggests that burials/complete

1. "Malgré un certain manque d'information—plans trops schématiques, absence de photographies, etc.—les gisements moustériens français montrent sans conteste que l'inhumation était pratiquée par les Néandertaliens."

skeletons are an aspect of regional signatures in which variation is predicted by environmental selection working on all the agencies which act to transform the archaeological record. If this is the case, then the enigmatic occurrences which require explanation are those which, like the first complete Neanderthal, found in 1856 in a cave in the north-central region, contradict this pattern.

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The archaeological record for the Palaeolithic is so sparse and partial and our knowledge of the processes leading to its formation so rudimentary that it constitutes a sort of Rorschach test. Nowhere is it clearer how far prehistorians impose their preconceptions on the mute, incomplete, poorly understood evidence they interpret. In the past, humanistically oriented scholars have seen evidence of Neandertal spirituality in the articulated skeletal remains found repeatedly in Middle Palaeolithic deposits (but not in those of earlier times). Humanists could think of only human agencies as responsible for the rapid interment which would have preserved corpses from dismemberment. Gargett is right to point out that various natural depositional processes in caves could cover bodies up rapidly. It is also quite clear that in all the principal cases of burials proposed for the Middle Palaeolithic the excavators paid no detailed attention to the process of sedimentation. This should lead Gargett to the conclusion that some doubt must adhere to the claims which have been made for deliberate human burials in the Middle Palaeolithic, but he insists upon going farther. He argues that the absence of probative evidence of Neandertal burials may reflect their *relative* cultural limitations compared to their Upper Palaeolithic successors. I can see no evidence for this further step. Gargett examines no Upper Palaeolithic burials, after all, and it is apparent that the critical procedures used to cast doubt on the evidence from the sites he does examine would sweep away the evidence for burials from virtually all pre-1960 excavations for periods prior to the Neolithic. One can only judge that his conclusions derive, like those of the humanists he criticizes, from his preconceptions. Gargett apparently belongs to the increasingly numerous group of scholars who would seek to explain Middle/Upper Palaeolithic cultural differences in biological terms: "Questions of genetic interchange between [Neandertal and anatomically modern *Homo sapiens*] may never be answered, but the removal of mortuary ritual from the behavioral repertoire of Neandertal may make the observed disconformity in material culture at the Middle/Upper Palaeolithic boundary a little easier to understand." This stance is no more warranted by the evidence than the arguments of humanists for Middle/Upper Palaeolithic continuity. Trigger (1984) and Fowler (1987) have observed how the theory and practice of prehistory are irremediably intertwined with political world views. I would hesitate to characterize the perspective implicit in the biological account of Middle/Upper Palaeolithic culture change, except to say that the humanist account seems preferable.

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My paper on the flowers from Shanidar was published in 1975; I am still waiting for the monograph on the cave of which the palynological work as a whole should be a part.

The 3 samples from the dark-brown loamy soil directly beneath Shanidar IV were unique in the cave in containing numerous (145) clusters of anthers. None of the other 50 samples from occupational deposits from Mousterian to Mesolithic contained any anthers, even though more than 6,000 pollens were identified.

Pollens are transported in two ways: by wind and by animals (mostly insects, sometimes birds). Wind-transported pollens enter caves only if there is a draft. Animal-transported pollens come from brightly colored or perfumed flowers and are carried into caves on animals' fur or feet. Anthers may be found in rockshelter sediments where the plants were near or within the cave entrance, carried there by rodents along with fruits. Gargett imagines the wind's having blown the flowers just into the Neandertal burial soil and having chosen bright-colored flowers belonging to five different genera. It is a pity that he has constructed his argument without considering the dispersion of pollens and without reading my paper on the subject (Leroi-Gourhan 1975) instead of Solecki's.

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Gargett adequately poses the general methodological questions but fails to apply them to the problem of understanding the Middle/Upper Paleolithic boundary. He claims and gallantly exercises the right to criticize finds that have been the basis for hallowed assumptions about the spiritual behavior of Neandertals. He tackles his rereading of the evidence from the perspective of "the mutually reinforcing processes of geomorphology and taphonomy," processes which have not been taken into account because of a "pre-1960s disciplinewide naiveté." Unfortunately, his ability to realize his critical goals is severely limited "because original field notes were unavailable for this study and only published distillations could be consulted." Since the evidence is unclear, his evaluation of it is based on the assumption of an absence of spiritual behavior among Neandertals, a posture which is contradictory to his demand for critical

judgment. It is this assumption that leads him to give greater weight to natural processes in explaining the archaeological record: since "human activity could mimic a natural process . . . , if there is no way of knowing, then it is logical to credit nature first, humans later."

Gargett's partisan stance on Neandertal spirituality is also evident in his failure to evaluate critically the evidence for spiritual behavior among *Homo sapiens sapiens* and in the manner in which he approaches the study of the individual sites. He states that with respect to the Upper Paleolithic "in the majority of cases the inference [of deliberate mortuary interments] is probably well founded" but for the Middle Paleolithic "the criteria for recognizing remains that have been purposely disposed of need to be made explicit." Such differential examination of the archaeological record is methodologically inadequate and unjustifiable given the state of our knowledge concerning the Middle/Upper Paleolithic boundary. There are important gaps in the chronological and physical anthropological documentation between the last Neandertals and the first *H. sapiens sapiens*, just as there are significant links between industries traditionally ascribed to Neandertal and modern men and vice versa (Lévêque and Vandermeersch 1981, Smith 1982). Gargett should at least have compared Neandertal burials with those of the earliest modern humans (middle Aurignacian).

With respect to the individual sites, Gargett gives unequal weight to the stratigraphic interpretations contained in the monographs. He assumes that observations concerning natural agencies are valid but those concerning human agencies are not. Because of this, he simply dismisses the contextualized, firsthand impressions of the archaeological deposits that the excavators were privileged to have. The Bouysonnies, Peyrony, and others perhaps did not make their observations explicit, but it is hard to believe that the contrast between the deposits of the presumed graves and those of the rest of the site did not play an important part in their interpretations. Furthermore, he approaches his critique of the evidence in a decontextualized manner, considering the "ritual" structures individually and their traits in isolation even where, as at La Ferrassie and Regourdou, they occur in groups (nine and six, respectively). "The relative improbability of co-occurrence of the right events" is used to justify the presence of these structures in just some of the areas within the total space in which they might have appeared when this improbability, together with the repetition of such "coincidences" at the same sites, would suggest, rather, a human agency for their occurrence. As Kohl (1985:111) puts it, "maybe there is a perverse God who planted these irrelevant fossils . . . , all of this misinformation, just to dupe the enemy."

These comments do not mean that I fail to recognize positive points in this article. In the first place, such studies should encourage a more careful and objective examination of future physical anthropological finds. In the second place, Gargett makes clear the inconsistencies in earlier publications and the possibility of deriving from them hypotheses other than those originally proposed by the excavators. All the same, only in the Teshik-Tash case does his pleading persuade me that "no substantial evidence exists for mortuary ritual." Even here, however, the use of Movius's (1953) summary instead of Okladnikov's (1949) original publication weakens his position.

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The evidence for Neanderthal burials is not well served by Gargett's review. There are certainly doubts about whether the elaborate burial practices described by various authors did occur. Some of the arguments in this paper, though, are biased toward a legalistic reading of evidence. Old data are debunked for lacking modern method and interpretation, and new data are neglected. I shall cite only two examples:

1. Capitan and Peyrony's excavations at La Ferrassie did not have the advantage of modern sedimentological technique, but the excavators were highly experienced. Some of their interpretations are admittedly hypothetical (Peyrony 1934:36), but these refer mainly to the actions of the Neanderthals rather than to the results of their excavations. The set of coincidences necessary to produce La Ferrassie is too extraordinary to accept as accidental.

2. The Mousterian/Neanderthal burial from the uppermost portion of Unit 12 at Kebara (see Valladas et al. 1987, Arensburg et al. 1985, Bar-Yosef et al. 1986) is not mentioned.

I cannot accept any review of the evidence for Neanderthal burials that neglects these points.

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It is relatively easy to claim, on the basis of excavation reports from early in the 20th century and geologically complicated sites like Regourdou and Shanidar, that there is no evidence for intentional burial among the Neandertals. However, Gargett fails to explain how a number of Neandertal mature and, especially, immature skeletons (e.g., Amud 1, Kebara 1 and 2, Kiik-Koba 1 and 2, La Quina 5, Roc de Marsal 1, Saint-Césaire 1, and Tabūn C1 [Suzuki and Takai 1970, Smith and Arensburg 1977, Arensburg et al. 1985, Bonch-Osmolovskij 1940, Vlček 1973, Martin 1923, Bordes and Lafille 1962, Lévêque and Vandermeersch 1980, McCown and Keith 1939], in addition to La Chapelle-aux-Saints 1, La Ferrassie 1–8, Regourdou 1, and Shanidar 1–7) managed to be preserved in highly accessible Upper Pleistocene rockshelters and caves in near-anatomical position and overall skeletal-part frequencies identical to those of recent cemetery samples (see Trinkaus 1985). These partial skeletons retain many fragile elements largely intact,

despite the ubiquitous presence of carcass-destroying carnivores (especially canids, ursids, hyaenids, and mustelids) and rodents in the vicinities of the sites, the lack of evidence in most cases for sufficiently rapid natural sedimentation rates to shield them from scavengers, and the absence of comparably preserved nonhominid skeletons in similarly accessible Upper Pleistocene locales. The possibly questionable nature of evidence for intentional grave pits in old excavation reports and the complex sedimentology of a few Upper Pleistocene sites is insufficient to refute the skeletal-preservation evidence for Neandertal burial.

This does not mean that all claimed cases of Neandertal burial were in fact intentional or that the imaginative elaborations sometimes accorded them are valid. It does mean that an unusual process, such as intentional burial, is required to explain the nature of preservation of many, if not most, Neandertal partial skeletons. Gargett has simply failed to make a convincing case that all or even most so-called Neandertal burials are the fortuitous results of natural processes.

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Gargett has provided us with a very well founded and sound reinterpretation of the Neanderthal remains that have been claimed to be the oldest examples of *Homo sapiens*'s mortuary practices. Employing the methods of geomorphology, sedimentology, and taphonomy, he arrives at the conclusion that Neanderthals did not bury the dead. Although I entirely support the way in which he has scrutinized the archaeological material from La Chapelle-aux-Saints, Le Moustier, La Ferrassie, Teshik-Tash, Regourdou, and Shanidar and fully agree that these finds do not represent Neanderthal burial, I should like to focus on some theoretical shortcomings of his approach and eventually question his final conclusion.

Ideally, site formation processes and criteria for Neanderthal burial should contribute more or less equally to the reinterpretation of the material in question. Gargett's reinterpretation, however, cannot employ criteria for Neanderthal burial because he does not identify any. What he does is review some criteria for *H. sapiens sapiens* burial, such as position of the body, a dug depression, grave goods, and indicators of magical and ritual activities. He concludes this section with the statement that "a new stratum, created when the grave was dug, is the fundamental criterion for recognizing purposeful interment." None of these criteria, derived from empirical data, archaeological and ethnographic, on prehistoric or contemporary *H. sapiens sapiens*, can be regarded as applying to Neanderthal burials. Reconstruction of the Neanderthal biological and cultural capacity for ritual behavior, including mortuary practices, should precede any interpretation of archaeological material potentially indicating this sort of Neanderthal activity. Such reconstruction, based on theoretical assumptions and data other than those possibly indicating ritual behavior in *H. sapiens sapiens*, would help to establish relevant criteria for Neanderthal burial. I am not aware of any such attempt. It is unjustifiable, then, to draw conclusions for all the Neanderthals that ever lived. Gargett has examined the archaeological evidence for Neanderthal burial from La Chapelle-aux-Saints, Le Moustier, La Ferrassie, Teshik-Tash, Regourdou, and Shanidar, and this allows him to conclude only that Neanderthal did not bury the dead at these six locations. His approach and methodology leave no room for doubt, however, on this slightly though significantly modified conclusion.

My criticism stems neither from any unsubstantiated belief in Neanderthal mortuary practices nor from any desire to defend Neanderthal's "humanness." I strongly believe that if we want to prove that even a rabbit does not bury its dead, we must use fully scientific procedures.

Reply

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My thanks to those who commented. This reply will deal with what I see to be the four issues raised: (1) my failure to include evidence from other putative Neandertal burials, (2) my method, (3) my failure to "explain" what preserved complete and nearly complete Neandertal skeletons, and (4) my conclusions about behavior in the Middle Paleolithic and implications for our understanding of biocultural evolution prior to ca. 35,000 B.P.

The missing critiques. I left out several other Neandertal partial skeletons originally because I thought they added nothing qualitative to the discussion. To answer the criticisms of Farizy and Masset, Frayer and Montet-White, Gamble, Ossa, and Trinkaus, I think it worthwhile to provide pared-down analyses of the remaining Neandertal discoveries.

Amud 1 was discovered at Mount Carmel, in Israel, in the uppermost portion of a poorly stratified unit of limestone breakdown silts and rubble (no pit was described). Burial was simply assumed. Five other individuals at Amud are known only from fragments (Suzuki and Takai 1970:39).

In addition to the child discovered a number of years ago, Kebara Cave (also in Israel) has recently given up an important adult partial skeleton. Smith and Arensburg (1977) call the seven-month-old (Kebara 1) a "deliberate burial" on the basis of the presence of three rocks near 13 teeth, skull fragments, phalanges, and tibia. However, Schick and Stekelis (1977) mention nothing to suggest that its burial was intentional. Kebara 2 (the adult) was found in a "shallow depression" (Arensburg et al. 1985, Bar-Yosef et al. 1986), but, as I have argued, that alone is not good evidence for purposeful burial.

Kiik-Koba 1 and 2 were discovered in depressions said

to have been dug into the bedrock (Bonch-Osmolovskij 1940, cited in Klein 1966:105–6). Given the presence of other, deeper vertical pits known to be solution cavities (see Bonch-Osmolovskij 1940:fig. 11) in proximity to those claimed to be graves, it seems likely that these depressions are natural. Such places are natural sediment traps and likely to experience more rapid deposition than elsewhere. Preservation was undoubtedly enhanced by location in such depressions. Even at that, Kiik-Koba 1 is represented only by the hands and feet, a patella, a tibia, and a fibula.

La Quina 5, according to Martin (1923), was removed from a unit of sandy clay which had been deposited in a low-energy fluvial environment. If so, purposeful burial is more than likely precluded, and the reasons for preservation are clear. The remainder of the La Quina fossil assemblage is less well preserved.

The circumstances surrounding the discovery of Roc de Marsal 1 (Bordes and Lafille 1962) are not extensively reported. No profiles or descriptions of the sediments are presented. The alleged grave was a depression 90 cm long and 70 cm wide which had been "refilled" with breakdown sediments. The report mentions a "strange" arrangement of the bones: femurs bent backwards at 135° relative to the vertebral column, tibia and fibula at right angles, and missing ribs and scapula. The investigators propose that the pressure of overlying sediments on the sloping bottom of the depression may have displaced the bones in this manner. I submit that it is equally likely, given the absence of bones of the thorax, that the bones were the remains of a meal. A hyena mandible was present in the "fill" of the grave, suggesting the possibility that, at least for a time, Roc de Marsal may also have been home to carnivores. Since we do not know the origin of the "depression" and cannot be sure that the remains had been purposely covered, we cannot rule out the possibility that preservation was a natural occurrence. There seems to be no alternative but to add Roc de Marsal 1 to the list of Neandertal "burials" inferred on the basis of insufficient evidence.

There is at present too little available on the specimen from Saint-Césaire to permit assessment of the evidence for purposeful burial. From what is published, however, there seems no reason to suspect that purposeful burial was involved.

The fairly complete Tabūn C1 was found in unstratified cave breakdown sediments approximately 1 m from the wall of the cave and about 1 m below an overhang. Location in the cave probably protected it from disturbance while natural burial occurred. Several other individuals, known only from fragments, were not so lucky (see Garrod and Bate 1937:pls. 37 and 39, facing p. 90).

I hope that this brief résumé of the remainder of the putative Neandertal burials will be sufficient to satisfy the commentators that, even in the case of recent discoveries, evidence for purposeful burial among Neandertals is lacking or insufficient.

My treatment of the evidence. Bricker suggests that I have misread a crucial passage in Capitan and Peyrony's (1912*b*:439–40) report of the discovery of La Ferrassie 3 and 4 (and 4a). My translation is, however, essentially the same as his: "refilled with a mixture of about equal parts of the black earth of the Mousterian layer and of the underlying gravel." I did not infer from this that the "black earth" was placed above the yellow gravel. When I speculated that it might have occurred in this position it was because there is good reason to doubt the observation of a "mixture" of two strata in the fill of the alleged grave. I continue to believe that the excavators did not recognize a new stratum created when the individuals were buried. Logically, a hole dug 30–40 cm into the lower, yellow stratum would not have been filled with a mixture of equal parts of the lower and upper material unless it had been dug through some depth of the overlying sediments. From the description, it appears as if the excavators did not recognize the "grave" until they had dug through the level at which the alleged fill and the overlying sediments would have formed a contact. After the fact, the panel of experts concluded that the fill had been composed of equal parts of the lower and upper sediments—an understandable inference but one lacking a logical premise and clearly based on the belief that the hole was a grave. The presence of limestone breakdown particles (the major constituent of the yellow, underlying sediments) in the anthropogenic, darker sediments of the overlying stratum is to be expected—they form the parent material from the Mousterian levels. While I would agree with Frayer and Montet-White and Bricker that there is no substitute for on-site observation or for the combined experience of a generation, neither necessarily precludes weak inference.

With due respect to Mme. Leroi-Gourhan, I do not see how the criticisms I raised regarding the context of Shanidar 4 or of the plant macrofossils said to have been associated with it are diminished by the color or number of flowers that found their way into the cave. Her 1975 paper provides another intimation that we may be dealing with natural processes. Shanidar is a perfect example of the axiom that each cave represents a unique depositional environment. It is a very large, south-facing cave with a large, triangular entrance. During periods when the floor surface was stable, even areas well inside the cave would have been sunlit for a good part of the day. Protected from extremes of diurnal temperature, the microclimate within the cave could very easily have supported a unique community of plants, perhaps the kind found near Shanidar 4. Leroi-Gourhan (1975:563) refers to "the soil of the grave on which the skeleton lay" as "dark and humic" and not only "richer in pollens" but containing numerous vegetal elements" and "very small pieces of wood." If the sediments on which Shanidar 4 lay were indeed the result of soil development, as their description implies, the presence of plant remains might be expected. There is really no reason, however, to invoke uniformitarian laws of ecology for potential plant growth within the cave; there was insufficient sampling at the same level elsewhere in the cave to warrant the assumption that flowers near the skeleton were a unique occurrence.

With reference to Martínez Navarrete's comments, it

is not my "bias" regarding the humanity of Neandertals that "leads [me] to give greater weight to natural processes in explaining the archaeological record." Natural agency should always be ruled out before invoking human activity as the cause of "peculiar" arrangements of sediments. Brain's (1981) work in caves in South Africa illustrates very well the need for such caution where non-hominid bone collectors are involved. I merely suggest a similar wariness where physical processes may be responsible. Furthermore, I fail to see how a consideration of geomorphology and formation processes in any way constitutes a "decontextualized" appraisal. Before attempting to induce social structures from rock piles at Regourdou or mounds at La Ferrassie, I would want to be certain that they were not natural in origin. It is unwarranted to assume that the nine mounds at La Ferrassie or six of anything at Regourdou were the result of conscious behavior simply because they and Neandertal fossils "coincide."

As for my criteria for burial, I have argued that the disposition of the corpse can tell us nothing by itself. What, I wonder, do we learn from the position of Roc de Marsal, whose legs were bent backward at an unnatural angle? Or La Ferrassie 6, whose head was not found with the rest of the remains? What do the nine vertebrae of Shanidar 9 tell us? Do the crushed and twisted skeletons of the other Shanidar Neandertals tell us of strange ritual? The body position of the La Chapelle-aux-Saints individual is not "a strong indication" that it was buried (Frayer and Montet-White); far from suggesting that its death had been accidental, I stated the obvious corollary to Bouyssonie, Bouyssonie, and Bardon's (1908) statement that its position mimicked sleep—that if a given position mimicked sleep, death during sleep would result in that position.

One example from a recent archaeological situation should once and for all refute the assertion that body position necessarily reflects mortuary preparation. At Utqiagvik, Alaska, the remains of two mummified prehistoric Inuit were unearthed in 1982. They had been entombed when sea ice crushed their house, the weight of the ice and fallen structural material having killed them instantly (Newell 1984). One, at least, had been asleep or resting when the calamity occurred. Both were at least as flexed as any Neandertal skeleton I have seen reports of, and clearly neither had been afforded any mortuary treatment—they had remained frozen under the timbers and sod until they were exhumed six summers ago. Likewise, there is no reason to suggest that the positions of Neandertal skeletons reflect anything other than their positions at death.

I have legitimately questioned the premises and arguments that underlie the present-day "knowledge" that Neandertals buried their dead. In every case sufficient doubt is cast on the investigators' conclusions to warrant serious reconsideration of conventional wisdom. I hope that, lacking repeatable experiments (the essential control in the so-called hard sciences), most archaeologists consider the critical examination of archaeological investigations fundamental to their science rather than, as Frayer and Montet-White suggest, inherently unscientific. The alternative is to accept without question any and all interpretations of the archaeological record.

What is preserving the Neandertals of Europe and Asia? I do not believe that we can assume "the ubiquitous presence of carcass-destroying carnivores" at any of the sites Neandertals occupied. Nor can we assume that the absence of "comparably preserved nonhominid skeletons in similarly accessible Upper Pleistocene locales" (Trinkaus) is indicative of anything that might bear on the question of burial. Neandertals may have been the habitual occupants of caves and rockshelters in which their remains are found. This alone may have precluded the occurrence of non-hominid remains. Furthermore, the behavioral characteristics of the occupant of a cave may inform, in some predictable way, the likelihood of skeletal preservation in their living space. In any event, carnivore disturbance does not rule out preservation. At Teshik-Tash the remains are preserved, although somewhat disarrayed, even though the involvement of a carnivore is almost beyond doubt. At Roc de Marsal, a similar situation may have occurred with less disturbance overall. Simply to assert that the potential for disturbance existed gets us nowhere. It is something that future investigation might elucidate, as Gamble has suggested in his comment and elsewhere (1984).

Gamble and others have noted that the sample of Neandertal fossils from Europe contains more complete specimens than that of earlier archaic forms. Even if we do not postulate increased use of caves during the hegemony of the Neandertals, there are uniformitarian principles of geomorphology that can account for the temporal differential in sample quality prior to the late Middle Paleolithic. As with archaeological sites in general, I would expect that through time the destruction of deposits by various agents would ultimately reduce the number of fossil remnants from the earlier times. Flushing out of cave deposits and settling, for example, are known to occur in later periods and might therefore be expected to have occurred earlier in the Pleistocene. Time and process alone may reduce the probability of finding an equivalent fossil record in the earlier period.

There are few African examples of archaic *H. sapiens* in states of preservation approaching that of "classic" Neandertals. Once again, this may have as much to do with the depositional environment as with behavior, although the partial adolescent *H. erectus* from West Turkana (WT 15000) demonstrates that natural processes other than burial in caves occasionally contributed to the fossil record. In Europe, the discoveries are almost always in caves and rockshelters. The two variables "Neandertal skeletons" and "caves and rockshelters" are clearly not statistically independent when compared with non-European populations of archaic *H. sapiens* fossils. There is clear and (to me) inescapable evidence that it is the depositional environment in caves and rockshelters and not any presumed purposeful behavior that is protecting and preserving European archaic *H. sapiens*, carnivore ecology notwithstanding. Even if the ecological relations of hominids and carnivores pre-

cluded disturbance of hominid remains across the range of archaic *H. sapiens,* those exposed to weathering would surely have been less likely to survive. Better preservation should occur in the special environments of caves and rockshelters.

I have enumerated some of the factors promoting differential preservation of skeletal material in caves and rockshelters. There are no doubt others about which we know nothing. Because each site has a unique depositional environment and history, each discovery of fossil material has to be understood in terms of its setting. The conditions that obtain in each case form a set of circumstances in which there is but one constant—hominid skeletal material was preserved in a state that seems to most of the commentators unlikely. The hypothesis that preservation can be attributed to purposeful burial is only one possibility.

As evidence that well-preserved Neandertal skeletal material *must* have been purposely buried, Trinkaus cites the results of his 1985 work on burial and cannibalism at Krapina. He bases his argument on similar patterns of bone preservation in Native American cemeteries and those found in a small sub-population of the known Neandertal fossil record. First he divides Neandertals into "buried" and "non-buried," on the assumption that fragmentary, isolated remains had to have lain exposed and were thus more susceptible to natural degradation. He places the vast majority of known Neandertal fossils in the "non-buried" category—representing approximately 146 individuals, excluding 43 at Krapina. With the "buried" sample of 16 in hand (about 8% of the total number of known individuals if the remains from Krapina are included, i.e., 189), he demonstrates that the relative proportions of preserved parts in the "buried" sample are virtually identical to those from Native American cemeteries.

To begin with, it is arbitrary to dichotomize the sample on the basis of completeness of specimens. Where do we draw the line? Neandertals are represented by everything from single teeth to nearly complete skeletons. Of course a sample composed of fragments (the "non-buried" assemblage) is not going to correspond to a population drawn from a known cemetery in proportions of parts preserved or presence of associated skeletal parts. As Trinkaus argues, differential preservation and disturbance by all kinds of post-depositional agencies would work to reduce the skeletal remains to isolated fragments. I would expect two buried samples to exhibit the same characteristics regardless of how they came to be buried. Nothing in the argument requires us to accept that the 16 were purposely buried. A hand-picked sample of nearly complete skeletons is naturally going to resemble a cemetery population; I would be surprised if it did not. The point is that the analysis works only if it begins and ends with the assumption that some Neandertals were purposely buried.

If I read Trinkaus correctly, seven of the "buried" Neandertals used in the analysis were recovered from Shanidar Cave. If, as I have argued, the seven were killed and buried by ceiling collapse, the argument that the remaining nine (i.e., about 4.25% of known Neandertals) were purposely buried would be seriously weakened. The similarity of the Shanidar sample to both the Native American population and the other nine Neandertals should render his conclusion suspect. By the same logic, Trinkaus might have concluded that the nine were preserved in the same manner as the Shanidar individuals.

Trinkaus deals with his inability to explain why the Krapina sample should resemble a buried population less than that of Shanidar and the other nine "buried" Neandertals by suggesting that either natural burial or some cultural or natural process intermediate between purposeful burial and recovery could have been responsible for the degree of preservation at Krapina. But this is no solution at all. He is left with but one conclusion—that the individuals at Krapina had been purposely buried, based on the received knowledge that Neandertals buried their dead. The "known" burials that he cites are just those that I have examined here. On the basis of his study of "burial" at Krapina, I am not convinced that purposeful burial explains the really rather small number of better-preserved Neandertals.

I have suggested some natural means by which a number of specimens might have been preserved in a nearly complete state. Burial by catastrophic collapse (as at Shanidar and Krapina or, for that matter, Regourdou [the bear]), location in a protected area of the cave (as at Tabūn, Teshik-Tash, and Shanidar), or coming to rest in natural depressions that act as sediment traps (as at Kiik-Koba, La Chapelle-aux-Saints, and La Ferrassie) are some likely ways in which preservation may have occurred. Of course, it is never possible to rule out burial by Neandertals, since it is illogical to argue from negative evidence. It seems less logical, however, to argue from questionable evidence that they did.

The sticky problem of the Middle/Upper Paleolithic transition. I have to agree with Clark and Lindly and Gilman that workers' expectations weigh heavily on the way they "see" the fossil record. My propensity to view the evolutionary record of this species as having a single origin ultimately requires me, I think, to search for disconformity, both morphological and behavioral.[1] It is the latter that first brought me to the subject of Neandertal burial. I was puzzled by the "fact" that they buried their dead and were believed therefore to have had modern spiritual and emotional capacities even though they apparently had no art (as we would define it), no worked bone (except possibly as an analog of stone), no personal ornamentation, no long-distance exchange (in contrast to mere transport of raw material), and no "real" blades. Clark and Lindly are right to point out that it is dangerous to conceive of anything as complex as the Middle/Upper Paleolithic transition in simplistic terms. But it is clear that their view of a multiregional origin for modern *H. sapiens* requires them to look for continuity in the fossil record. My analysis of Neandertal burial does not necessarily support their hypothesis. I have merely pro-

1. Mine is by no means a minority position (see, for example, Stringer and Andrews 1988).

posed that we can no longer confidently assume that Neandertals possessed a spiritual being or an emotional complex akin to ours or the sense to bury their dead to avoid having them ravaged by carnivores (even if these inferences were possible from "good" archaeological evidence of burial). A satisfying explanation for long stasis in the archaeological record during the time before modern humans appeared in Europe is still elusive. The presence of morphologically modern *H. sapiens* at Qafzeh 92,000 years ago (Valladas et al. 1988) leads to the conclusion that for at least 60,000 years Neandertals and morphologically modern humans coexisted, apparently without intercourse, social or other. Far from providing support for the multiregional-origin hypothesis, I think that the shared behavioral characteristics of the two may only argue for the obvious: that they started out at similar points on a continuum of biocultural evolution. Burial, clearly, is a derived characteristic and one which, on the evidence, is manifested only by Upper Paleolithic, morphologically modern *H. sapiens*.

I have suggested that Neandertals may not have shared their genes with us. We may never know the answer. But if the consensus view of their emotional, spiritual, and intellectual capacity is based on the available evidence for burial, the answer may always elude us while we search for clues to why, in spite of the fact that Neandertals presumably had thoughts like ours, their behavior and that of their contemporaries appears to have stayed the same for so long. This is why, perhaps unwisely, I have said that removing burial from the behavioral repertoire of Neandertals might help us sort out the muddle in the Middle Paleolithic and, ultimately, questions of the transition to "behaviorally" modern humans.

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