THE ANALYSIS OF QUATERNARY CAVE SEDIMENTS AND ITS BEARING UPON PALAEOLITHIC ARCHAEOLOGY, WITH SPECIAL REFERENCE TO SELECTED SITES FROM WESTERN BRITAIN

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11. WIND ACTION

11.1. General Aeolian Processes

The main sedimentary effects of wind are the erosion, transport and deposition of relatively fine particles (cf. Bagnold 1941; Smalley 1975; Warren 1979). Because air is a comparatively rarified fluid (with a dynamic viscosity one hundredth and a density of a little over one thousandth of the respective values for water), the three sedimentary aspects of air motion are rather well differentiated, especially with respect to particle sizes. Also, pressure gradients are the dominant driving force for wind, with topographic factors usually playing only a localised role and only becoming regionally important in areas of extremely high relief. Thus, a whole landscape, rather than restricted channels, may be affected on a scale limited only by atmospheric circulation patterns.

Wind erosion, just like water erosion, is initiated mainly through shear, that is, the lateral stress generated when a particle obstructs the flow. As surface roughness increases, more turbulence is produced, with higher point velocities, higher point stresses and thus a greater likelihood of particle movement. However, energy is also lost to a rough surface through friction and any elastic absorption, so that the lateral component of velocity steadily decreases with increasing roughness. Even a comparatively sparse vegetation cover may be enough to reduce wind velocity just above the ground to a level at which sufficient shear is no longer generated to move sediment particles (cf. Chepil & Woodruff 1963).

Given a relatively bare ground surface, entrainment of a particle is primarily a function of its grain size. For instance, the smallest sand grains at 63 microns require a wind speed of c.9km/h (measured at c.2m above the ground surface), whilst grains of 1mm require a speed of c.36km/h. Inter-particle attraction is inversely proportional to grain size (for a standard mineral type); at 100 microns, the attraction is approximately equal to the weight of the particle whilst, with electrochemically active clay minerals, it may be several orders of magnitude greater. Furthermore, small grains no longer project sufficiently far into the flow to initiate significant turbulence, so that a fine sediment presents a 'smooth' surface over which even a high wind may pass with little disturbance. Therefore, the wind speed required to entrain particles below about 63 microns increases rapidly with decreasing diameter. The behaviour of particles is not only a function of their size but also of their density; larger shell fragments and organic particles may be entrained when only much smaller quartz grains are susceptible. Shape also affects entrainment, with platey particles presenting less of an obstacle and therefore generating less shear. Water content increases the resistance to entrainment in all sediments but, since water is held at lower tension in coarser deposits, it may thence be removed much more easily by evaporation.

Coarse particles (over c.2mm) leave the ground only in exceptionally high winds or due to momentary high velocities associated with extreme turbulence. In normal winds, such particles tend to roll, slide or 'creep' across the surface, and they are easily trapped by irregularities. Nett movement of coarse particles is therefore very slow and they are not often found very far from their source. Sand grains above c.0.2mm leave the ground but soon fall back once they have escaped (in time or space) from the zone of high velocity (turbulence) which caused their entrainment (<u>infra</u>). Nevertheless, because air has such a low viscosity, grains saltate much further in air than in water and, when they land, they still have considerable momentum. This momentum may be passed on to other grains so that, given a theoretical sand with only one grain size, a slightly lower wind velocity is sufficient to entrain new grains than was necessary for the first grain. Bagnold (1941:32-33) termed the higher velocity (entrainment by direct wind action) the "fluid threshold", and the lower velocity (entrainment by a combination of direct wind action and ballistic bombardment) the "impact threshold". The effect of bombardment is obviously important in connection with the movement of sand. Not only is a lower average velocity sufficient to maintain movement (with particles of 1mm, there is a theoretical reduction of over 8km/h), but the effect also spreads the available driving force more evenly over the surface, reducing interference from slight irregularities or changes in grain size. However, bombardment by sand grains is also extremely important with respect to the mobilisation of smaller particles. Smooth silty or clayey surfaces would often be more or less invulnerable to wind, if it were not for the transfer of energy brought about by impacting sand grains. Fine sediments with rougher surfaces are composed of relatively large aggregates; bombardment by sand grains breaks down the aggregates into more vulnerable sizes.

The entrainment velocity for sand grains coarser than c.80 microns is not greatly in excess of the velocity which would be necessary to maintain them in suspension, especially when the bombardment effect is taken into account. A grain will remain in suspension as long as the turbulent lift in the air flow is greater than the fall velocity (determined by Stoke's Law) for that particle size. At 1mm diameter, a particle will fall at over 20km/h but, at 80 microns, it will fall at only a little over 1km/h. Turbulent lift is related to shear stress, which itself is related to velocity gradient (cf. Bagnold 1941 for the exact mathematical relationships). As a rough approximation, we may say that turbulent lift decreases away from the ground surface. The overall effect of these considerations is that most sand, moved by wind at observed terrestrial velocities, will remain within a metre or two of the ground surface. In contrast, as particles become smaller, their fall speed continues to decrease whilst the entrainment velocity threshold rises. Even when erosion is due to sand bombardment, wind velocities and turbulent lift will often be sufficient to transport finer particles high into the air (cf. plates in Péwé 1951). At altitude, significant turbulence is still constantly present, because shear is generated between air masses. In fact, the finest silts and clays would never fall to the ground if other factors did not come into play.

The nett deposition of sand occurs essentially whenever the wind speed falls below the level necessary to erode that sediment. On a very short time scale, this may be a function of meteorology, that is, the wind speed simply drops. Seen on a longer time scale, most sand is deposited where there is physical interference with the flow pattern near the ground. Thus, sand collects in the lee of obstacles (from a pebble to a hill) or is trapped by a belt of vegetation, a body of water or a steep hill slope. It should be noted that, in reasonably flat and open terrain, sand is most easily deposited on sand, since energy is lost to the shock-absorbant surface, causing a drop in near surface wind speed. On bare rock, very stony ground or any other hard surface, sand grains continue to bounce along with a minimum loss of momentum.

Finer sediment, or 'dust' (the finest sand, silt and clay grades), may be deposited in vegetation traps if the base of the dust cloud remains close to the ground. Decreasing wind velocity is probably sufficient to allow the fine sand and coarser silts to fall out from suspension with no additional catalyst. However, the finest dusts require precipitation through electrochemical effects. When dust comes into contact with more humid air, the particles act as condensation nuclei. Movement of air and particles within a dust cloud may produce static electrical effects that may cause certain minerals to fall out. Air masses containing particles with different electrochemical properties may cause mutual precipitation of dust (cf. Beavers 1957). Dust is generally deposited as a blanket over comparatively large areas. A statement by Warren, in connection with loess (aeolian silts, infra), is difficult to understand: "... loess tends to cloak the windward sides of valleys more than the leeward (Yaalon, 1974) and long loess-free 'shadows' occur to the lee of higher hills." (1979:346). It is not clear what is meant by "windward" and "leeward" sides of valleys, although the second part of the statement suggests that the meanings 'wind-facing' and 'sheltered', respectively, were intended. The article to which Warren refers, which was in fact written by Yaalon and Dan (1974), clearly indicates that loess is often deepest, in parts of Israel, on the sides of hills farthest from the source (not necessarily the lee, with respect to prevailing winds). Pleistocene loess in in Central Europe is indeed sometimes thicker on the sides of hills towards the presumed source but, as in any fossil deposit, the present thickness in hilly terrain is mainly a function of the degree of contemporary and subsequent erosion, not the depth of original deposition. Again, the suggestion by Laville et al. (1980:26) that dust ("loess") is deposited in sheltered sites, such as topographic depressions, is not generally acceptable. The only case in which there is a really convincing relationship between wind and geometry in deposits of fossil aeolian silt is provided by the accretive (stationary) longitudinal dunes of Bulgaria (cf. Rozycki 1968).

11.2. The Environmental Implications of Aeolian Deposits

As has been stated above, wind erosion is best developed in dry areas with little or no vegetation. These conditions are most prevalent today in warm deserts. Smaller scale phenomena also occur in cold, dry regions (cf. bibliography in Washburn 1979:262), but they are almost insignificant when compared with the widespread pleistocene deposits that undoubtedly developed all over the world at times of cooler/colder climates. Recognition of this general environmental link has often resulted in rather facile interpretation of pleistocene deposits, especially those in small scale sites such as caves.

The presence of aeolian sand is primarily a function of a superabundance of the raw material in the source area. All that is necessary is that sand become available faster than it can be stabilised by vegetation or polluted by other sediments. For sand to move in the wind it must be dry, but coarse sand may begin to saltate only minutes after the end of a rainstorm if a moderate wind continues to blow. Climatic factors, such as humidity and temperature, not to mention wind speed and direction, are indeed important, but in an indirect manner in that they affect the availability of sand and vegetation and thus the balance between the two. The only situation in which the mere presence of aeolian sand may be reasonably interpreted as due to at least seasonally dry and, in the Western European context, cold conditions is when it can be demonstrated that the sand was able to move freely across relatively wide areas which would have been vegetated under less severe climatic conditions. Such is the case, for instance, with the coversands of the Low Countries and Eastern England or the dune fields of Poland. To achieve a degree of certainty concerning the prevailing climate, either the source must be recognised or a continuous, or formerly continuous, regional aeolian sand body must be demonstrated. Most of the recognisable aeolian sand bodies in British caves are to be found in coastal sites (cf. section 10.4.), but deposition was by no means restricted to periods when the sea shore was as close as it is today. In general, sand sources may be classified according to two main criteria: (1) the distinction between 'fossil' and constantly renewed sand sources; (2) availability due either to emergence from a body of water or to destruction of a pre-existing vegetation cover. Identification of a source in these terms will go a long way towards recognising the environmental implications.

Aeolian deposits consisting mainly of silt, with some colloids and fine sand, are locally common in modern cold regions. Extensive fossil deposits, known as loess, occur in many areas of the world and, where they are not obviously due to the proximity of warm deserts (as in parts of Western China), they are interpreted as cold climate sediments. Aeolian silts have been widely recognised for nearly a century but, despite or rather because of extensive research, the loess phenomenon remains imperfectly understood. First, there is some difficulty over the definition of loess. The INQUA Loess Commission (Fink 1974) defines loess as material with a clearly dominant size mode in the range 60-20 microns, a lack of stratification, a significant carbonate content and a relatively porous capilliary network. 'Clay-loess' is also recognised, which may have up to 30% of particles less than 2 microns and a lower porosity than loess. These are objective definitions and there is no explicit requirement that the material be aeolian. Indeed, some of the criteria used cannot possibly be the <u>result</u> of deposition by wind and are in fact pedogenic features (<u>infra</u>). However, the Commission goes on to define other loess-like sediments, not by purely objective criteria, but by factors that would indicate that an original loess has been moved, redeposited and/or modified <u>in situ</u>. The overall implication is that, theoretically, all loess and loess-like sediments are derived from originally wind deposited silty material and that, in practice, every attempt should be made to exclude similar sediments that were not originally aeolian. Loess, or 'primary loess' as it is often called, is the purest expression of aeolian origin, although not all of its diagnostic characteristics are a direct result of this origin. This viewpoint probably constitutes the most reasonable set of working definitions, although the present author finds rather restrictive the implication that all originally aeolian loess-like deposits must have passed through a loess stage. The difficulties inherent in the study of loess, which are not merely a matter of semantics, are best illustrated by reconstructing the theoretical 'history' of loessic deposits.

The main source for pleistocene loess is generally accepted to have been the 'rock-flour' produced by glacial grinding, with a secondary but possibly locally important input from cryoclastis (cf. Smalley & Krinsley 1978). Such material becomes available for wind erosion in vast quantities on the sandur plains beyond the glacial margins. The most important factors in this environment are driness, strong anticyclonic and catabatic winds, and shifting and constantly replenished sediments of a wide particle size range including large quantities of silt. If silt is to be eroded, true meteorological driness is required since fine sediments retain moisture for long periods after wetting. A wide range of particle sizes would appear to be important in order to quarantee the maximum bombardment effect. The present author cannot accept the constantly repeated opinion (cf. Embleton & King 1975:182; Washburn 1979:264) that 'cold' loess is better sorted than 'warm' loess because it is water-sorted before it is eroded by the wind. Fluvial sorting in a sandur or braided river environment is active on a tiny horizontal scale compared with the wide areas affected by a major wind erosion event. That fluvial deposits may be well sorted at given points in space is certain, but massive wind erosion can only 'unsort' this material. The sorting of major aeolian silt bodies must be a function of the parameters of transport and deposition, parameters which can safely be assumed to differ in 'warm' and 'cold' environments. For example, the extreme instability of air masses near warm deserts might allow coarser material to remain suspended for longer and to be deposited with the finer material beyond the desert margin. Other factors which are important during erosion are lack of effective vegetation (due to a combination of driness, low temperatures and high sedimentation and erosion rates) and, possibly, destruction of the coherent structure of superficial fine sediments by frost action. Outside the main source areas near the glacial margins, the valleys of major braided rivers, such as the forerunners of the Rhine or the Mississippi, would still have acted as important regional sources, as can be demonstrated from the present distribution of aeolian deposits. Large distributary systems, such as fans or deltas, would also provide much material.

The question now arises as to how much pre-existing aeolian deposits might have acted as new sources. Sand moves in a series of 'jumps'; how much of the observed distribution of loess, in Europe for instance, is due to successive 'jumps' (on a scale of perhaps a hundred kilometres or so) and how much to a simple fallout mechanism operative over the whole continent? Loess is certainly susceptible to wind erosion after human interference with its structure and the vegetation cover. However, it is even more susceptible to mass movement, wash and fluvial erosion if there is insufficient binding vegetation. Given a wetter environment, not only should wind erosion become increasingly reduced because of cohesion, but it should also be pre-empted by water-associated erosion. This brings us to a consideration of the environment(s) under which loess was deposited in the first place.

On theoretical grounds, the question as to whether there was an environmental/climatic control over the deposition of pleistocene aeolian silts can only be answered by a tentative 'yes and no'. On the one hand, given the possibility and even probability of long distance transport, it is clear that silts could have been deposited in areas that were much wetter, warmer and less windy than the distant source. The ground conditions would not directly affect the deposition of silt, except in cases where dust clouds were sufficiently near the ground to be intercepted by vegetation traps, a situation which, in any case, would tend to arise only comparatively close to the source. On the other hand, the distribution, stability and humidity of air masses over a continent are climatic factors, and they must have controlled the movement of suspended sediment. Now, it might appear to be a relatively simple matter to observe continent-wide aeolian sediments and to deduce, by sedimentological and palaeontological methods, the climatic conditions under which they were deposited. Unfortunately, this is not the case because, although the environment at ground level may not have a direct influence on deposition, it most certainly has an influence on the survival of the sediment once deposited. Furthermore, aeolian silts of the type from which pleistocene loess was developed are chemically and physically unstable and are thus highly susceptible to radical modification at any time after their deposition, a proposition which is underlined by the quasi-total absence of pre-pleistocene siltstones of certain, or even probable, aeolian origin.

The environment under which loess (*sensu stricto*) was deposited and developed is quite well understood. First, it is necessary to point out that the mineralogy of loess from all over the world presents certain gross similarities (cf. Doeglas 1952; Swineford & Frye 1955), not withstanding significant local variation due to different sources and mineral sorting by characteristic particle size and density. Loess presents something of a 'continental average mineralogy', subject only to the fact that minerals are present in a comparatively oxidised state and the mechanically weak species are under-represented. This 'average' dust fell on landscapes which were certainly vegetated. The vegetation played no great role in causing deposition but it was most important in stabilising the dust once it had settled, protecting it from wind and water erosion. Furthermore, the vegetation gave structure to the dust, first by inducing a vertically oriented system of pores (root moulds) and, second, by extremely small scale reorganisation of carbonates and clay minerals so as to form 'bridges' between the loosely packed silt grains. These pedogenic processes are referred to as 'loessification' (cf. Kukla 1975); without them, no loess would form and the dust would not survive _in situ_. Although the landscape was vegetated, it was also rather dry and cold. This reduced the probability of water-associated erosion and also provided a severe, and necessary, limitation to both pedogenesis and cryogenic disturbance. The characteristics of loess show that dust input was more or less constant (in geological terms) and that loessification was largely restricted to the first few centimetres below the accreting surface. There was no major mobilisation of colloids or solutes (horizon differentiation) and even some of the clay minerals remained under-saturated in bases despite the abundance of carbonates. Because of the open structure, oxidation could continue, producing increasingly 'redder' colours through time. In wetter, warmer and better vegetated environments, loess (_sensu stricto_) simply could not form, even if the dust input was just as high, because any 'aeolian' structure would be too weak to counteract the modifying forces. Moreover, loess has only survived in large quantities to the present day in areas which have remained relatively dry over their entire history (e.g. continental interior lowlands; cf. Kukla 1975). Not surprisingly, true loess represents a rather small percentage of all the known loess-like sediments. Indeed, more and more careful investigation is proving that superficially 'massive' deposits often show some degree of departure from the theoretical type, a situation which is largely responsible for the current difficulties in nomenclature.

Some of the most characteristic loesses occur in Central Europe, where they appear to have accumulated mostly under grassland (steppe vegetation). However, even these deposits are interspersed, both vertically and horizontally, with finely laminated zones, indicative of deposition in water. The mollusca included in these zones indicate a rather 'marshy' biotope, so that the general environment may be envisaged as dominantly steppe with a patchwork of more tundra-like areas, the latter probably associated with the build-up of ground ice (cf. Ložek 1968; Kukla 1975). Increased mobilisation of carbonates, probably associated with slightly heavier vegetation and seasonal moisture, is evidenced by calcareous nodules and pillars, often referred to as 'loess dolls', as well as rhizoliths (cf. p.529). As long as there is a good supply of dust and there is a marked dry season, loess may even form and survive relatively unmodified in open parkland landscapes, as has occurred in some parts of the U.S.A. (cf. French 1976). Once one moves away from the areas dominated by primary loess, two further groups of loess-like sediments become increasingly common: loess loams and loessic derivatives that have demonstrably been affected by mass transport (wash, fluvial, soliflual, etc.) or by mass mixing (cryoturbation). Loess loams show some of the characteristics of loess, but they are denser and have lower, or even insignificant, carbonate contents; they may also have a higher clay content than local loess. In Central Europe, loess loams often occur on slightly higher and more sloping land than do loesses, a factor which has led most authorities (cf. Kukla 1975) to interpret the loams as pedogenically modified loess. The loams do not now contain mollusca, but some pollen assemblages suggest a slightly more varied vegetation, including some shrubs and bushes (cf. Frenzel 1964).

Britain is on the extreme western edge of the European loessic belt. More or less unmodified loess still appears as very thin deposits in S.E. England (cf. Gruhn et al. 1974). The mollusca are quite similar to other European loess faunas, and indicate a patchwork of dry and marshy areas (cf. Sparks & West 1972). Further west still, loessic material occurs as 'silty drift' or 'coverloam' (cf. Catt 1977). These thin non-calcareous veneers do not even conform to the definition of loess loam, since it can usually be demonstrated that they have been radically affected by both alteration and physical disturbance. Nevertheless, they must have originated as aeolian deposits, since they occur at various altitudes and on different rock types, and the silt mineralogy is usually exotic and quite uniform over wide areas (cf. Perrin et al. 1974). Catt considers that British Late Devensian loessic deposits must once have been very much more extensive. He notes that the silts have been preferentially removed from clayey and sandy substrata and preserved on limestone.

> The only way in which loess [sensu lato] could have been stabilized on limestone surfaces while it was eroded almost completely from other substrata is by secondary carbonate cementation of subsoil horizons in periglacial deposits composed of loess and frost-shattered limestone. Such cementation is most likely to have occurred early in the history of soil development, ... (1977:226-7)

It is these silty drift deposits that occur in some British cave sequences. It is therefore rather important that the environmental implications be identified. Did the silt originate from local sources (Embleton & King 1975; Lill & Smalley 1978) or was it part of a more extensive east-west continuum (Perrin et al. 1974), perhaps with an important input from the North Sea basin (Catt 1977)? Was the silt originally calcareous? How much silt reached these western areas and how much has since been removed? Was a true loess ever formed or was the silt altered and/or reworked as soon as it settled out? If silt and frost-shattered limestone debris are associated, were they broadly syndepositional and, if so, is cryoclastis compatible with a dry environment? To the present author's mind, it would seem sheer folly to suggest that the occurrence of aeolian silt in Devon implies an environment similar to that deduced from Hungarian loess, 1700km further into the continental interior. Indeed, it is difficult to accept that even Kent could once have been subjected to such a continental environment, despite the similarities in sediments and mollusca. It should be noted that, although the local environmental reconstructions derived from the mollusca seem reasonable, none of the molluscan assemblages, in Kent, Hungary, Argentina or anywhere else, can be matched with existing faunas. The associations (which are remarkably constant over wide geographical areas and therefore undoubtedly represent true biocoenoses) are all composed of species that are cold- and drought-tolerant, but which today are found in quite a variety of environments, most species with distinctly different geographical ranges one from another. One wonders how much the similarities in loess mollusca are the result of edaphic factors. Perhaps the Kent loess represents rapid fallout of dust caused by blocking by damp air masses from the west. Sheer speed of sedimentation might possibly have imparted a misleadingly 'severe' character to sediments and fauna alike, when the meteorological conditions were much less continental. The silty drift further to the west might then represent comparatively rare incursions of continental air, together with seasonal cold and dry winds from the north, bringing silts that fell on a landscape that was in no way 'steppic' and which was never blanketed by true loess. This interpretation may turn out to be completely wrong, but the mere fact that there is no compelling reason to reject it outright surely suggests that loess and loessic deposits deserve to be treated on a more regional basis. In this connection, occurrences of loessic sediments in caves may prove very useful since, although they will not have escaped varying degrees of modification, the conditions of preservation will have been very different from those which affected exterior deposits, with the possible result that some original characteristics may be easier to isolate through a comparison of the two types of site in the same region.

The environment, or rather the environmental sequence, deduced from loessic deposits and associated phenomena may be of particular importance in some areas. The sequence from Central Europe (cf. Kukla 1975) probably provides one of the most detailed terrestrial stratigraphic records from anywhere in the world. However, it is in areas outside the main loessic belt of Europe that loess stratigraphy has had most impact upon cave sediment research. For instance, the cave and shelter deposits of the Périgord are placed in a stratigraphic framework with labels such as "Würm" or "Riss", although these units are not based upon the classic Alpine glacial sequence. Instead, each 'glacial' is defined, and subdivided by a set number of 'interstadials', according to the loess sequence recognised by Bordes (1953) in the Paris Basin, despite the fact that Bordes's work has since been superseded (cf. Michel 1976). This thirty-year-old interpretation of loessic deposits is therefore the basis for the definition of the stratigraphic (climatostratigraphic, chronostratigraphic?) pigeon-holes into which the cave deposits, over 400km distant to the south, must now be placed (cf. Laville et al. 1980:27).

11.3. Criteria for the Recognition of Wind Action

Cailleux (1973) has listed eleven main classes of currently observable aeolian phenomena, including erosional and depositional forms as well as modes of transport:

(1) Zones of deflation (blow-outs, pavements, etc.).

(2) Sculpturing of rock outcrops by sand abrasion.

(3) Facetting of stones in the range 20-2000mm (ventifacts).

(4) Frosting of sand grains in the range 0.4-1.5mm (grain surfaces of the 'worn-matt' [Fr. émoussés-mats] and 'spherical-matt' [Fr. ronds-mats] types, as well as less obvious modification observed by SEM techniques).

(5) Transport of stones, in the range 20-60mm, by sliding on frozen surfaces.

(6) Transport of sand and dust.

(7) Sand dunes.

(8) Blankets and drifts of aeolian material.

(9) Blankets, drifts and dunes of niveo-aeolian sand.

(10) Aeolian fill of contraction cracks.

(11) Dust deposits with the grain size distribution of loess.

Only items (5), (9) and (10) are considered to be truly diagnostic of periglacial wind action but, since Cailleux is also considering the warm desert environment, many of the other items may be at least suggestive in the context of the European Quaternary.

Deflation, rock sculpturing and ventifacts are extreme erosional forms associated with areas of sand mobility. To the author's knowledge, the first two forms have not been described from European caves and there are very few examples of ventifacts. As one approaches rock faces, it is reasonable to assume that, with respect to wind action, one would move from a balanced erosional/depositional environment to a zone where deposition was dominant. Nevertheless, limestone is comparatively easily abraded by sand blasting and minor erosional forms may have been overlooked in the past. Miskovsky (1974) indicates that Escalon de Fonton has recovered ventifacts from shelter deposits in the area of Lamanon (Bouches-du-Rhône) but the present author has been unable to find a primary publication. Miskovsky also states that ventifacts are "très beaux et très nombreux [very well developed and numerous]"(ibid., p.39) at the cave of La Salpêtrière (Gard) but, even though the sedimentology of this site is discussed at length later in Miskovsky's text, no further details are given, not even the stratigraphic level(s) from which the ventifacts were recovered (*infra*). This is most disappointing since the distribution of ventifacts at a cave mouth should prove of great interest. The larger examples are the most informative; when they have only one or two facets they may well be *in situ*, whilst multifacetted types must have been moved by an agent other than the wind.

The surface modification of quartz sand grains is a criterion widely used by cave sedimentologists for the recognition of wind action. It is usually supposed that the presence of 'aeolian' grains indicates contemporary 'dry' (and 'cold') conditions in the exterior environment, even when the cave sediment itself is clearly not a structured aeolian deposit and not particularly sandy or silty. The dangers of misinterpretation are obvious. The subject falls into two areas: the recognition of frosting and rounding using low powered optical microscopy (the most common approach), and the recognition of micron-scale features using SEM techniques.

Cailleux (1942), in a remarkable empirical demonstration involving thousands of samples from large areas of Europe, showed how the proportion of pleistocene frosted grains varies directly with increased windiness, continentality and proximity to sources, as deduced from all available evidence (e.g. continent-wide distribution of dunes, loess and other aeolian phenomena). In many further publications, Cailleux has since extended the project to vast areas of North America and Asia. The sheer scope of this demonstration admits of no doubt that mutual abrasion of sand grains may be extreme during wind transport, a proposition which, in any case, is supported by modern observations and experimental replication. However, the question remains as to whether other processes may cause frosting and whether wind transport may occur without frosting. Kuenen and Perdok (1962), in what appears to be an attempt to refute Cailleux's arguments, pointed out that frosting may also be caused by chemical etching and fluvial abrasion/corrosion. This article is very difficult to evaluate, since it comprises extremely convoluted inferential arguments, some rather shaky theoretical considerations and a set of far from 'natural' experiments using reagents such as hydrofluoric acid. Although Cailleux's position is in no way threatened, it nevertheless seems possible that some frosting may be chemically induced, a proposition which may receive some support from allied discussions in the later SEM literature. The present author has not come across any further references to fluvial frosting, although he himself has observed a few large (c.1mm) frosted and spherical grains in rocky and potholed streamways in some active British caves (cf. also p.407). Kuenen and Perdok recognised frosting on natural grains down to c.40 microns and Cailleux (1973) accepts that wind frosting should not be active below c.0.2mm. All three of these authors state that frosting is not common in beach dune sands and Cailleux (1942) suggests that grains must travel at least 1-2km in order to be affected.

Attempts to interpret frosted grains from cave sites do not appear to have provided any unequivocal results. The present author examined ten samples from Sun Hole (Mendip), five from Unit I (apparently cold and damp) and five from Unit II (apparently cold and drier, with a probable derived loessic component; cf. Chapter 22). From each sample, a thousand grains in the range 0.35-0.50mm and a thousand in the range 0.125-0.088mm were examined using a binocular microscope, oblique lighting and magnification up to x62.5. Frosted grains appeared to be well differentiated from other surface types in both size ranges and in all samples; results were therefore expressed as percentages with 95% confidence intervals derived from the binomial distribution. There was no significant difference between nine of the ten samples, there being c.7% coarser frosted grains and c.0.8% finer frosted grains; one sample from Unit I produced c.12% coarser frosted grains and a similar percentage of finer frosted grains to the other samples. There would appear to be no grounds to suggest that these figures have any bearing upon the question of aeolian input. The author has never noticed high proportions of frosted grains from any of his study sites, whether aeolian sediments (sand or silt) were involved or not. Laville (1975:39) concluded that all surface morphologies on quartz grains at c.0.7mm were of little significance in his shelter sediments; he did not record frosted grains from deeper cave sediments. Miskovsky has studied quartz grains at c.0.2mm from most of his cave sites; he unerringly interprets frosted grains as being directly proportional to contemporary wind action. At the site of La Salpêtrière (Gard), for instance, the layers of the phase known as Würm III c2 are said to contain frosted grains:

> Les grains de quartz ronds mats deviennent plus abondants (entre 15 et 20%), mettant ainsi en evidence des actions éoliennes non négligeables. (1974:264)
>
> [Trans.: Spherical-matt quartz grains become more abundant (15-20%), thus demonstrating a not insignificant degree of wind action.]

This percentage range should be compared with a range of 3-7% for the underlying layers of phases Würm II a to III c1, for which no aeolian influence is claimed. The difference appears impressive until it is realised that Miskovsky counts only 50-100 grains (ibid., p.44). A simple calculation shows that the 'increase' in frosted grains in the Würm III c2 is not statistically significant. Indeed, because of the ludicrously small sample size, very few of Miskovsky's quartz grain surface texture counts, from this or any other site, are of any interest. Yet Miskovsky prefers to discuss La Salpêtrière in terms of this non-information rather than telling us about the ventifacts from the site or commenting upon the intriguing (aeolian?) bedding features that can be seen on the section drawings (ibid., fig.161, p.260) supplied by Escalon de Fonton.

The observation of quartz grains using SEM techniques has resulted in the identification of a suite of typically aeolian surface features, not only on sand sized material but also on silt (cf. Krinsley & McCoy 1978). These potentially powerful techniques have not yet been applied to very many cave sequences and, to the present author's knowledge, cave silt has never been examined. Tankard and Schweitzer (1976) were able to recognise some aeolian features (primarily 'upturned plates') on sand grains from clearly aeolian deposits in Die Kelders I (Cape Province, South Africa), despite heavy post-depositional modification. However, Bull (pers.comm. in Stringer 1977) records no aeolian features from the Coarse Grey Sands at Bacon Hole (Gower), although these are almost certainly of immediate aeolian origin (cf. p.1066). No aeolian deposits or grain surface features have been recorded from Pontnewydd Cave (Clwyd; Green et al. 1981; Bull in Green et al., in press), a fact which may be compared with the present author's pilot study of frosting (under low powered optical microscopy) in the same sediments. Sand in the ranges 0.35-0.50mm (>500 grains) and 0.075-0.125mm (>300 grains) was examined and frosting, varying between 0 and 10%, was recognised in both size groups. There was no proportionality, either direct or inverse, between the occurrence of frosting on grains from the two size groups of individual samples. Bull notes that grains from this site show little post-depositional modification and, although the quality of the information derived from the SEM studies does not, unfortunately, permit a precise layer-by-layer reconstruction of depositional environment, Bull records important fluvial modification from many levels, a feature which might possibly be related to optical frosting. It would seem desirable to set up a project whereby individual grains from cave sediments, that have been classed as 'frosted' by optical techniques, could be described in detail using SEM techniques, in order to decide whether or not optical methods are of any value at all in this context. The present author is reluctant to abandon optical techniques out of hand merely because it seems probable that SEM techniques are considerably more accurate, if only for the reasons that the former are so much more accessible and the sample size may be very large (infra) if necessary.

SEM techniques themselves may present some major problems that would call for extreme caution in environmental interpretation. It has already been noted that Tankard and Schweitzer recognised severe, but in this case not totally overwhelming, chemical modification of grains.

> Post-depositional diagenesis of the quartz and shell grain surfaces is common and reflects the highly active chemical environment of the Die Kelders I, sediments (pH range 7.9 to 8.9). (1976:310)

This degree of alkalinity is extremely common in cave sediments and it is well within the range of pH at which silica is mildly soluble (cf. Ollier 1969). One has only to observe most flint artefacts from European caves to realise that appreciable 'patination' often occurs after only a few millennia or even centuries. However, the results published by Texier (1981), from the shelter of Pie-Lombard (Alpes-Maritimes) are rather puzzling in this context. Sand grains from the archaeological layers are said to show a clear derivation from ancient marine sediments, followed by minor fluvial modification, whilst post-depositional modification was not sufficient to obscure the earlier history of grains. SEM techniques were used, but the results are oddly expressed in a terminology similar to that used by Cailleux for standard optical analyses. It is not clear what grains said to be "ronds mats polis [spherical-matt-polished]" (1981:161) might represent. The primary object of Texier's article is not, however, the description of sand grains but, rather, a discussion of the desilicification of mousterian flint artefacts from these layers. Flint and quartz are not physico-chemically identical, but one still wonders how 0.5mm thick skins, showing radical silica solution (demonstrated by SEM and X-ray diffraction studies, which also showed the absence of vulnerable opaline materials), could have formed on the flints, whilst sand grains retained clear pre-depositional features only microns deep. The present author is willing to accept the power of environmental reconstruction using SEM techniques, but only if researchers can convincingly prove that they are never likely to be misled by post-depositional modification of the quartz grains.

Another problem with SEM techniques is the necessarily small sample size. The cost is so high that it is rarely possible to examine more than about 50 grains per sample. Considerable statistical sophistication has been achieved by such researchers as Bull (1978b) in order to reduce the likelihood of error in the recognition of environments of modification. The present author accepts that such statistical methods may well suffice when one is dealing with one environment of modification, or a series of environments through which all, or most, of the grains of a whole sediment body have passed. However, the suggestion by Bull (1981) that 50 grains are still enough even when two separate grain populations are present is not only a misrepresentation of Baker (1976), whom Bull cites as his authority, but is simply not tenable from general theory. Given a multiple source sediment and unicrystalline grains of similar size from each source, the likelihood that a grain from one particular source will be chosen for a sample is governed by the behaviour of the binomial distribution, and by no other statistical relationship. The constraints of sample size therefore mean that, when using SEM techniques, one will not be able to quantify mixed inputs with any accuracy. For instance, if 10% of the grains examined come from a recognisably discrete source, one cannot ignore the fact that there is an associated 95% confidence interval of ± 9.3%. This is of course true for any source in a mixed sediment but, because it seems likely that wind modified grains will have been syndepositionally or subsequently mixed with other sediments in pleistocene cave deposits, it may be our understanding of aeolian processes which will suffer most.

The transport of sands and silts has already been discussed above. The extent to which a sediment could be transported by wind into, or even through, a cave should be largely a function of cave morphology, local topography and the interior meteorological phenomena considered in section 2.2. Humidity conditions, which are seasonally variable in most caves, should constitute a highly efficient controlling factor with respect to aeolian transport. However, even humid air is still capable of carrying a little very fine material, often including organic matter. Such dust is thought to take part in the formation of vermiculations (cf. Bögli 1980), irregular mosaics of dark lines etched into limestone cave walls, initiated by the segregation of mildly corrosive matter by water condensation and developed by positive feedback due to the hydrophilic properties of the dust. Vermiculated surfaces face towards the effective cave wind. At Pontnewydd (Clwyd), such a surface faces into the cave (in Area C; cf. p.1138) and there is today a weak summer outdraught. The vermiculations have probably formed in little over a century, since they uniformly cover rock both above and below the original level of the sediments before the first excavations at this site but they do not continue beneath undisturbed deposits. Vermiculations are quite common but they are usually difficult to date; the author is unaware of any occurrences buried beneath ancient sediments and it seems likely that such superficial forms have a low survival potential. An observational project, combining studies of meteorology and aeolian transport of mineral and organic matter (including pollen) in selected cave sites, would obviously prove of great interest. The movement of small stones over frozen surfaces by wind 'pushing' is, according to Cailleux (1973), a comparatively rare phenomenon, even in the open. The suggestion of such a process would need to be supported by a demonstration that the underlying sediment had indeed been frozen and that no other mechanism is likely to have been responsible, requirements that would be difficult to meet in most cases. Nevertheless, wind 'pushing' presents an interesting alternative to the normal interpretation of weak stone lines in aeolian deposits as solifluction features.

Primary aeolian deposits take a variety of forms, depending upon the particle sizes and the precise depositional mechanisms. Classic dune bedding in sands, with clear cross-laminations, appears to be rather rare in cave sequences, probably because even the largest cave still represents a significant disruptive factor with respect to the organised air flow that produces dunes (but cf. p.1099). Laminar bedding is much more common, sometimes with patches of inverse grading indicating the development of weak and transitory ripple systems (cf. Bacon Hole, p.1083). If large limestone fragments occur in such a deposit, or if really extensive sections are available, it is worthwhile recording the lateral variation in minor unit thickness, as this may show a relationship with air flow patterns given truly undisturbed wind-blown sand. Thickening of beds in the lee of obstacles is much more extreme in aeolian than in fluvial deposits. More massive deposits, which are sometimes truly featureless, may result from the dumping of sand (cf. Bacon Hole, p.1066). This appears to be particularly common in cases where sand enters via holes in the roof or is dropped from an overhang, situations in which the cave itself is not penetrated by the main air flow. Thick deposits may build up through rapid dumping but, given a truly stable cave environment, similar thicknesses may represent longer periods during which sand entered as a gentle 'rain'.

Most regional coversands are interpreted as being niveo-aeolian phenomena. Aeolian sand builds up during the snowless part of the year, sometimes with the formation of a ripple system. Snow then falls, or is drifted, across the sands, sometimes preserving ripple morphology. Finer material (fine sands, silts and colloids) is often included in drifting snow, and stones may slide onto the snowbanks if there is an available source. When the snow melts, wash and minor mass movement modify the deposit. Coversands therefore often show alternation between thicker sands and thinner finer material with occasional stones. Deformation may be apparent, especially if snow layers did not always melt each summer. Such deposits are difficult to recognise in small exposures and wavy bedding, which is characteristic of niveo-aeolian sands, is not absolutely diagnostic (cf. Bacon Hole, p.1084). Near rock exposures, more complex patterns may develop, culminating in *pseudo-grèzes* (p.416). Theoretically, niveo-aeolian sediments should be relatively common in shelters and cave entrances, but the author has never come across the extremely detailed description which would be necessary to prove the point. Although, in Britain, large expanses of coversands are restricted to eastern England, more local sediment bodies also occur in the west (cf. Gilbertson & Hawkins 1978). Aeolian material filling contraction cracks, as 'sand wedges', has already been discussed on p.275.

Primary aeolian silt bodies were introduced in the last section. Whether or not such deposits might form and survive in caves is a moot point. If one accepts the strict definition of loess (supra), including the requirement of a thin vegetation cover, such deposits cannot form far within caves. Without loessification, a primary aeolian silt body might be so modified that its true nature would be difficult to prove. The author has never seen a deposit that is certainly, or even probably, a primary aeolian silt in a cave or shelter in Western Europe, despite the fact that loess-like sediments are quite common. Vértes (1959) concluded that many deposits in the caves of central and eastern Europe were primary 'loess', principally because of the mineralogy and particle size distribution. Furthermore, these deposits corresponded with the climatic phases (deduced, avoiding the most obvious circular arguments, from the cave sequences themselves) in which one would expect loess to be formed. Kukla speaks of "the last occurrences of autochthonous wind-blown loesses at the Magdalenian cave sites (Kukla and Kočí 1972; Valoch 1968)" (1975:122). Kukla and Kočí (1972) merely reference Valoch (1968) who, in turn, discusses only the archaeology, not the loessic deposits. The present author is happy to accept that such an authority on loess as G.J. Kukla is most unlikely to be mistaken in this matter. Caves well within the European loess province simply must have been subject to true aeolian sedimentation on many occasions and some of these sediment bodies may have survived more or less unchanged (e.g. by very gentle secondary cementation, perhaps). Nevertheless, it would be extremely helpful if detailed descriptions of these 'classic' deposits could be made available, together with a discussion of the diagnostic characteristics of primary aeolian silts in caves. In the meantime, we may recall some of those characteristics that have been commonly used to identify primary deposits (loess, *sensu stricto*) in the open.

Loess is 'unstratified', that is, it shows no internal bedding features. It is possible that macroscopically massive deposits may in fact show laminations, due to wash transport, when examined microscopically (cf. p.409). Theoretically, it is possible that some laminations could be due to variations in source (e.g. far travelled silt and local organic-rich dust), although such variation would have to be quite extreme in order to survive restructuring by plant roots. Loess is usually uniformly calcareous, although some more localised sources may be free of carbonates. The structure of loess is characteristic but not always diagnostic at every level of perception. In the field, the most obvious features are vertical (columnar) jointing, generally brittle fracture when dry, and regular, continuous macroporosity. These characteristics are a function of particle size range and root penetration, and are not restricted to primary deposits. For instance, loessic sediments at Tornewton Cave (Devon) show classic loess macrostructure but it may be demonstrated, using several lines of argument, that they are very far from being primary (cf. p.728). The common suggestion that a hand specimen of loess immediately collapses when saturated with water applies only to some types of loess ('clean', coarse silt types); small blocks of finer or less well sorted loess (and clay loess) do not necessarily behave in this way. The high microporosity and metastable structure of loess, features which are indeed diagnostic, may be demonstrated in several ways. Loess has a low unit weight (bulk density), below c.1.55g/cm^3 and sometimes as low as c.1.15g/cm^3; post-depositional settling may raise the unit weight to c.1.70g/cm^3, the normal value for water-laid silts (cf. Fookes & Best 1969). Laboratory oedometer tests may also be used to measure wet compaction as the metastable structure is destroyed (cf. Fookes & Best 1969), a process which is less welcome in nature when the loading is provided by a building on loess foundations (cf. Obruchev 1945). Some of this dilation is of course referable to major root channels (supra), but much of it is due the generally open structure, with silt particles held apart by clay and carbonate bridges, structure that can be seen by using SEM techniques with undisturbed samples. It is worth noting that the material from Tornewton Cave, mentioned above, has a unit weight of c.1.64g/cm^3, despite the fact that there are c.47 macropores (>0.25mm diameter) per cm^2 in the horizontal plane. The presence of extremely small and fragile mollusca, evenly distributed throughout the deposit, is commonly cited as a characteristic of loess; undisturbed owl pellets might serve the same purpose in the cave situation.

When aeolian sediment is present either in a derived (redeposited or highly modified) state, or as a component diluted by syndepositional input due to other processes, the structures which are characteristic of wind deposition will be absent or, at best, difficult to recognise. Textural and compositional parameters must then be used to identify the original aeolian input. The quartz grain surface textures, mentioned above, are often used in the absence of structural data.

One of the most obvious features of sediments that have a high aeolian component is their characteristic particle size distribution. Descriptive statistics, such as those of Folk and

Ward (1957), may be useful as long as the distribution is still close to that of the aeolian component. Tankard and Schweitzer (1976) make proper use of such statistics to identify wind-blown sand at the cave of Die Kelders I (Cape Province, South Africa). The approach necessarily involves the simultaneous examination of all statistics, usually achieved by plotting all possible pairs of statistics (e.g. mean against skewness, etc.) on scatter diagrams or by treating the results mathematically in multi-dimensional space. Statements, commonly found in textbooks, such as 'dune sands are well sorted and slightly positively skewed', should be treated as mere generalities and should never be used for diagnostic purposes. Similarly, undiluted aeolian sand consists almost entirely of a saltation population and will therefore plot approximately as a straight line on a gaussian cumulative frequency curve (cf. p.216). However, this will only be true if there is insignificant variation in source and transport energy. Loessic sediments, and even loess itself, have extremely varied particle size distributions. The requirement (supra) that loess should have a clearly dominant mode in the range 60-20 microns is based upon observation of 'typical' loesses. There is no other textural common denominator between loesses, save that they are generally quite badly sorted (sic), a proposition which cannot often be expressed in terms of graphic statistics since the colloid content is usually high. Simplistic 'environmental' recognition based upon slight differences in values of statistics such as kurtosis is worthless (contra Shackley 1975:144-5).

Mineralogical analysis, especially the study of heavy minerals, is often useful in recognising an aeolian source. Farrand (1975) noted that several layers at the Abri Pataud

(Dordogne) contained a sand component with a similar particle size distribution to that of material from the modern Vézère floodplain below the site. In order to differentiate this sand from other possible sources, a heavy mineral analysis was carried out which confirmed the suggested source. Farrand also presented reasonable geographical arguments as to why the Vézère sand should be mineralogically distinct today, reasons which would also be valid during the Upper Pleistocene. Since there were clearly no fluvial beds in the shelter, Farrand argued that the sand represented an aeolian input which had not been able to form aeolian structures because of simultaneous sedimentation of materials from the rock and slopes around the shelter. This example shows that arguments concerning heavy minerals must usually be highly inferential, a fact which requires that each step in the argument be reasoned with the greatest care. Farrand presents no proof of aeolian input _per se_; he merely suggests that no other process is as likely. If a terrace remnant were to be discovered above and close to the shelter, Farrand's argument would collapse. This careful chain of reasoning is sometimes neglected, especially when the heavy minerals of loessic deposits are under discussion. For instance, Dalrymple (1960) discussed the "brickearths" of south-east England with a degree of detail that has rarely since been rivalled. Heavy mineral analysis showed a suite very similar to the local Eocene (Thanet Sands) and Dalrymple therefore concluded that the aeolian silts were of local origin. However, heavy minerals were identified only from the size range 0.25-0.07mm, that is, only from the sand fraction of these deposits. Most heavy mineral work is done on sand because heavy liquid separation techniques become increasingly inefficient with smaller particles (cf. McGreal 1981), deflocculation, centrifuging, freezing and even multiple runs being necessary for particles below 60-50 microns (cf. Catt & Weir 1976). But the sand fraction of a loessic deposit is likely to be derived from a much more local source than the dominant silt fraction. Thus, when Weir and Catt (1969) studied material from north-east Kent, they found, like Dalrymple, that the mineralogy of the sand fraction was similar to the local substrate but that the minerals of the silt fraction suggested a non-local source. The mineralogy of the 'silty drifts' of Devon shows a similar division into more local sand and mainly exotic silt (Harrod et al. 1973). This is not really surprising when the nature of aeolian transport is considered.

Miskovsky (1974) has suggested that the wind may blow large quantities of plant debris into caves, leading to the formation of black, humic layers during cold, wet periods. Although this seems to be a generally reasonable proposition, the examples cited by Miskovsky (e.g. parts of Layer VI at La Madonna dell'Arma, Liguria) do not show convincing evidence of wind action.

One last type of airbourne material should be mentioned, namely volcanic debris. The cave of Crvena Stijena (Montenegro, Yugoslavia) contains a mid-Würm tuff composed of glassy volcanic ash, probably derived from the Albanian highlands to the east (J. Frechen in Brunnacker 1966). At the Grotte de Verlaine (Province de Luxembourg, Belgium), derived aeolian silts contain important proportions of fresh green hornblende and epidote, as well as a varied suite of accessory minerals. This material is considered to be characteristic of eruptions in the Eifel during the Alleröd, because earlier eruptions are known to have involved the orthopyroxene, enstatite, which is absent from this site

(Delvenne 1956). However, Juvigné (in Dewez et al. 1974) has indeed recognised volcanic material from the early Würm at the cave of Remouchamps (Province de Liège, Belgium). There was no vulcanism in Britain during the Quaternary and no far travelled volcanic dust has yet been identified.

In conclusion, it may be said that the identification of aeolian and derived aeolian deposits is often rather difficult, especially in small exposures such as caves. Arguments should be based upon as many different characteristics as possible. Considerably greater precision may be achieved if cave occurrences can be correlated with regional sediment bodies and a conscious effort should be made to set up such links, even if this requires primary fieldwork outside the cave.

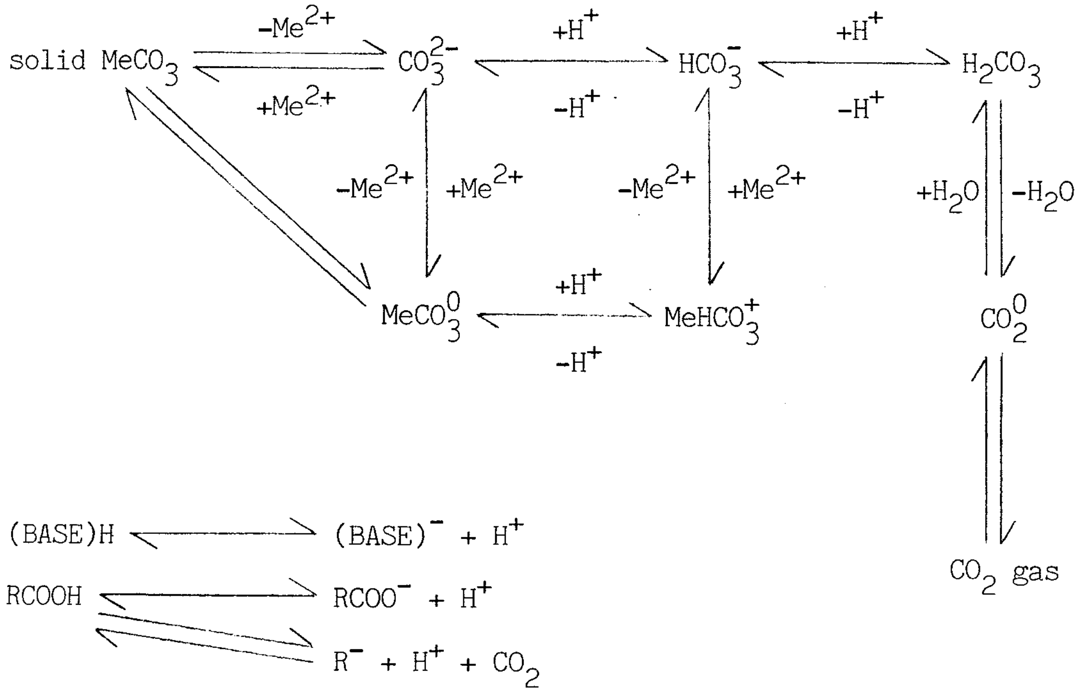
12. ALTERATION AND SOILS

12.1. Processes Involved in Alteration

A material which is 'altered' differs from the presumed 'original' state of that material in that its chemistry, and often its structure, have been completely or partially changed. In the case of relatively simple and pure materials such as limestone, the emphasis shifts to structure, since many chemical processes will then result in displacement of the dominant mineral rather than major chemical changes. The 'original' state is largely a matter of viewpoint: the state at the time when the material became available at the earth's surface; the state at the time when the material became available as a particle or as other types of mobile matter; the state at the time of deposition; the state at the time when the material became sufficiently deeply buried to reduce to a minimum further modification by processes acting specifically from the surface. Definition of the relevant 'original' state reflects the need to recognise a chronology, a sequence of 'events'. However, when faced with the end result of an unknown sequence of 'events', it is often far from easy to demonstrate the order, let alone the time scale. The most important distinction is probably that between alteration that occurred prior to deposition and alteration that occurred during or after deposition. Syndepositional and postdepositional alteration involves not only chemical changes in the constituent particles but also translocation of solutes and fine material and a more or less pronounced restructuring of the sediment body as a whole. Alteration may continue for extremely long periods after the deposition of a sediment, or it may occur or recur long after the sediment has been deeply buried. Again because we are primarily interested in sequences of 'events' and their relationships to surface environments, such alteration, which does not allow sufficient chronological distinction for our purpose, may be qualified as diagenetic (cf. Chapter 16). The concept of 'alteration', as used here, includes those processes often referred to as chemical 'weathering', as well as the complex of physical, chemical and biological processes involved in pedogenesis (cf. section 12.3.). Alteration is by no means always a subtractive phenomenon and it would be impossible to discuss the subject without reference to such processes as chemical precipitation. However, larger scale and relatively pure precipitates (speleothems) are considered separately in the next chapter.

Caves and shelters formed in limestone will naturally receive large quantities of this rock type as a primary component of their sediments. Limestone is dominantly composed of calcium carbonate (usually in the form of calcite), calcium carbonate with magnesium carbonate in solid solution (magnesian calcite), and combined magnesium-calcium carbonate (dolomite). All these minerals are highly soluble and it is the alteration phenomena associated with limestone that are the most obvious in the cave situation. Writing 'Me' for the metallic elements, magnesium and/or calcium, and 'R' for hydrocarbon groups, the following general reactions (not including all possible routes) characterise the alteration of limestone, with 'slow' reactions shown as double arrows and 'fast' reactions as single arrows (adapted from

Picknett et al. 1976):



Starting from the solid carbonate at the top left, solution reactions involve the replacement of the metallic cations by hydrogen, leading to the formation of the 'bicarbonate' anion (HCO_3^-) and carbonic acid (H_2CO_3). All these reactions are reversible; the direction in which a reaction will proceed depends upon the relative concentrations of the species on either side. Solution of the solid will slow as the concentrations of the metallic cations, the ion-pair $MeCO_3^0$ (a common species at higher pH) and the carbonate anion (CO_3^{2-}) increase. The concentrations of the last two species are lowered by combination with hydrogen, thus permitting more solution of the solid carbonate. Solution is therefore hydrogen (pH) dependent. Ionic hydrogen is always present in water so that even alkaline solutions may retain a little aggressiveness as long as they are not saturated with solutes. Hydrogen availability may be raised by two main processes. The first involves the actual addition of hydrogen in the form of an acid ('acidification'); acids may be genarally symbolised as (BASE)H, with the special case of organic acids represented by RCOOH. Note that the decomposition of organic matter also produces CO_2. Solution is accelerated by base exchange, with hydrogen moving from the acid to the species of the carbonate system. The second process is 'carbonation', whereby carbon dioxide dissolves in water and combines to form carbonic acid, which may itself dissociate to provide more available hydrogen. Obviously, no addition of hydrogen is involved and the production of HCO_3^- ions will lead to some interference with the solution of solid carbonates. Carbonation is therefore said to be a weak acid reaction, as opposed to a strong acid reaction involving the addition of significant quantities of hydrogen. Since the availability of hydrogen will be one of the most important factors in limestone solution, it will be necessary to examine the circumstances under which acidification and carbonation may occur.

A very large number of acids are produced in nature, many of which are so complex as to defy analysis. These may be divided into two groups, organic and inorganic. The organic acids are produced by plants, during both growth and decay, and by animals, in excreta and carcasses (cf. Trudgill 1977). Inorganic acids are produced in the atmosphere (infra) and when reactions occur between the atmosphere, water and certain minerals. For example, pyrites (FeS_2), which is a relatively common mineral in British limestones, reacts with water and dissolved oxygen to produce other iron compounds and sulphuric acid (cf. Morehouse 1968). Pyrites can also be either reduced or oxidised by many bacteria to give aggressive products. Acidification produces extreme solution of limestone but, because the formation of acids (except carbonic acid, infra) often involves irreversible reactions and the destruction of finite sources, the process is severely limited in scope. Strong acid reactions are very efficient but they probably play only a secondary role in the general solution of limestone terrain.

The difference between carbonation, with its product carbonic acid, and acidification involving other acids is that the atmosphere is a ubiquitous and almost inexhaustible source of carbon dioxide. The weak acid reaction of carbonation is thus the dominant factor in the general solution of limestone. The amount of carbon dioxide that will dissolve in water is proportional to the amount present in the adjacent atmosphere. The normal atmosphere has only c.60mg of CO_2 per litre of air, but the soil atmosphere commonly has c.200-460mg/litre and the level may even reach c.670mg/litre under certain circumstances (cf. Picknett et al. 1976). The high values in soils are produced by expiration from plant roots, and by decomposition of organic matter due to the activities of micro-organisms and the oxidation of organic acids. The solution of carbon dioxide, the formation of carbonic acid and all the subsequent reactions involving the carbonate species are easily reversible. The heavily carbonated water derived from a soil is able to dissolve appreciable quantities of limestone but this material will only remain in solution as long as any atmosphere with which the water comes into contact has a correspondingly high CO_2 content. Soil water will reach saturation very quickly as it penetrates the underlying limestone and this level of solutes will be more or less maintained in dominantly water-filled microfissures. However, when the water reaches the cave atmosphere, with a CO_2 content of c.70-180mg/litre, a new equilibrium must be set up by degassing of CO_2 from the water, resulting in carbonate precipitation.

The above discussion may be summarised in the following terms. Acidification occurs dominantly in soils, since organic matter is at least a slowly renewable source whilst minerals are not. Carbonation occurs dominantly in soils, since this is where the highest CO_2 partial pressures are generated. Carbonation is the most important solutional process, since CO_2 is the most readily available source of aggressiveness. Carbonates are precipitated in caves by degassing of CO_2, a process which is also responsible for the slightly higher CO_2 content in the cave atmosphere as compared with the normal atmosphere. Significant cave winds will cause even more precipitation, both through replacement of cave air by exterior air (with a lower CO_2 content) and through evaporation. This model implies that limestone alteration will be greatest in soils, that it will become very much weaker in shelter and cave entrance situations, and that the cave interior will be a zone of precipitation, not solution. However, this model fits the majority of observations on real cave sediments only at a very general level, especially in the context of ancient cave sequences in present temperate latitudes. The alteration of limestone clasts may often be extreme deep within caves and some explanation of this phenomenon must be found. Similarly, many shelters and entrances contain zones where the sediments are highly altered and others where the sediments are much fresher, patterns which cannot possibly be explained by differential soil development.

The most important additional factor which must be considered when attempting to explain the alteration of limestone and limestone debris at specific points in space is the nature of the water circulation pattern. Consider a situation in which a uniform, limestone-rich sediment body with high permeability occupies a cave entrance or a shelter. Theoretically, the main control over alteration should be the areal distribution of water input. Allowing that the aggressiveness of the water is the same in all areas, those areas which receive large quantities of water (e.g. below the dripline) will suffer greater alteration than drier areas, irrespective of the precise degree of aggressiveness. Now consider a situation in which water input and aggressiveness are uniform in all areas. One area contains a permeable sediment, as before, but another has an impermeable deposit. A degree of alteration will occur in the permeable area, spread over a significant depth of deposit because of the fast through-flow rate. In the impermeable area, the total aggressiveness will be concentrated in a shallow surface zone, with individual limestone clasts therefore suffering greater 'unit' alteration than those in the permeable area. In a more realistic situation, with variations in both water input and retention, we may expect differences not only in the overall degree of alteration but also in the type of alteration in the different areas. For instance, a wet and permeable area would suffer alteration but would lose most of the products. A dry and permeable area would suffer little alteration but many of the products might be reprecipitated by evaporation. A moist and impermeable area would suffer more alteration but many of the products might still remain. In a permanently damp deposit, alteration might penetrate much more deeply into limestone clasts. All these variations are possible with water and sediments of uniform chemistry. Further variations occur if the solubility of the sediment is not uniform. For instance, in a deposit with a carbonate-rich matrix, much of the aggressiveness will be expended in the alteration of fine carbonate particles whilst, in a deposit with a quartzitic matrix, the aggressiveness will be concentrated on larger limestone clasts. Sediments vary not only horizontally but also vertically, so that, in a stratified deposit, there may be 'stratified' alteration effects. All these factors are, of course, part of a dynamic system with interactions through time between water availability and aggressiveness, sedimentation rate, permeability and sediment chemistry.

In exposed areas of a shelter or cave entrance, incoming water might be expected to have a small degree of aggressiveness. Direct rainfall will contain a little carbonic acid and sparse vegetation would contribute more. However, further into the cave, percolation water will usually be saturated in carbonates. Nevertheless, observation will show that some drips do not fall on zones of carbonate precipitation, as might be expected, but on zones of carbonate solution. This is particularly puzzling if the area in question is a clean limestone surface, with no obvious input of acid-generating substances. At Pontnewydd Cave (Clwyd), many drips falling on recently exposed limestone can be seen to be somehow involved in aggressive reactions, despite the fact that response to exterior rainfall is relatively slow and there are some 25m of limestone above the cave chamber. Similarly, many cave sediments contain clasts showing degrees of alteration, culminating in 'ghosts' and residual 'stains', that must be the result of authigenic processes. The effects of aggressiveness are most easily observed in sediments, containing limestone clasts, that are overlain by stream deposits. It is in the context of cave streams that the subject of penetration and rejuvenation of aggressiveness has been studied, primarily because an understanding of the processes involved is essential to any study of speleogenesis, but also because the chemistry is comparatively simple. The author knows of no studies that have been specifically designed to monitor alteration of cave sediments. Furthermore, most studies of percolation ('drip') water concentrate upon transport and deposition of solutes. Nevertheless, the processes identified as operative in cave streams will function in any cave situation as long as water is present. The rate and significance of given processes will, however, vary radically under different circumstances, especially with respect to localised water and atmosphere types.

A surface stream running over non-limestone terrain will have a relatively low pH, dependent upon such factors as lithology, vegetation, rainfall, soil drainage, etc. (consider, for example, the heath and marsh areas on the Old Red Sandstone on top of Mendip). This aggressive water will go underground as soon as it reaches limestone. The depth into the system to which solution from this source will penetrate will depend upon the initial pH, the discharge rate and the ratio of water volume to surface contact with limestone (conduit morphology, proportion of conduit circumference in contact with air rather than water, permeability and chemistry of bed sediments, etc.). It is to be expected that the potential of strong acid reactions will be exhausted rather quickly, as is suggested by the rapid increase in pH usually observed as a stream penetrates a cave system. Percolation water, which moves much more slowly and has a much better contact with the limestone, should lose its initial strong acids over an even shorter distance. Nevertheless, sections of cave that lie very close to the surface and which have comparatively large connecting fissures might still receive small quantities of strong acids.

A surface stream, especially if it is broad, shallow and clear, will have a CO_2 content approximately in equilibrium with the normal exterior atmosphere. As this water passes underground, it will come into contact with a cave atmosphere that has a higher CO_2 content, as a function of degassing of percolation water. Given a slow moving stream and little air mixing, there might be enough time for a new CO_2 equilibrium to be achieved, with a consequent minor rejuvenation of aggressiveness (cf. Picknett 1977a). Rapid flooding, leaving behind water low in CO_2 in pools, might produce the same result. Similarly, in the rare situations where the percolation response to external rainfall is extremely rapid, some potential for rejuvenation might exist, although it would be more difficult to maintain a cave atmosphere richer in CO_2 under such circumstances.

Rejuvenation of aggressiveness will sometimes occur when warmer water passes into a colder environment (cf. Picknett 1977a). CO_2 is more soluble in water at lower temperatures for the same atmospheric CO_2 partial pressure. This effect should be seasonal (summer) and should reinforce the CO_2 equilibrium effect, noted in the last paragraph, because cave atmospheres are richest in CO_2 during the season of greatest biological activity in exterior soils. The temperature effect should be concentrated in cave streams. It seems unlikely that even rapidly penetrating percolation water would often be significantly warmer than cave air.

The amount and state of decomposition of organic matter carried into a cave by water will obviously have an important effect. Organic matter is only immediately aggressive in the form of organic acids. More complex substances are gradually broken down, by bacteria and direct oxidation, with the liberation of acids and CO_2. There is thus a latent potential for rejuvenation (cf. Bray 1977). Stream water often contains organic matter, but percolation water deep within a system does not usually carry significant quantities of particulate organics. However, small amounts of dissolved organic substances, such as amino acids and carbohydrates, may still be present (cf. Mason-Williams 1967). An additional source of organics is dust in the cave air (cf. vermiculations, p.481).

Bögli (1964) identified a process, which he called Mischungskorrosion, involving the mixing of waters with different solute contents. Originally, the process was seen in terms of different carbonate and CO_2 contents, but Runnels stated:

> The following discussion introduces and offers the broader hypothesis that the mixing of natural waters is, in fact, a general geochemical process, with interesting implications in the study of the origin and alteration of sediments. The hypothesis is based upon the simple experimental fact that the solubility of most minerals is a non-linear function of added dissolved salts. (1969:362)

This proposition is best illustrated first using the classic example suggested by Bögli. The relationship between the CO_2 content of water and the total amount of carbonate that this water can hold (the saturation level) is not linear: the rate of increase in carbonate solubility slows as CO_2 content increases. Thus, if any two saturated solutions are mixed, in any proportions, the resulting mixture will be undersaturated. Larger increases in aggressiveness are produced when there are greater differences between the two component solutions (expressed either in terms of CO_2 or carbonate content, since these are mutually determinate in a saturated solution) and when the mixture is (approximately) one-to-one. Overall increases in aggressiveness may also result from certain mixtures of unsaturated solutions. The great difficulty with this particular example has always been that, although a highly significant increase in aggressiveness seems possible on paper, it is difficult to see how sufficiently disparate solutions might arise in nature, because, in vadose stream flow, all waters in close proximity should be similar due to the requirement of approximate equilibrium with the cave atmosphere. Only under phreatic conditions might this process contribute much to rejuvenation of aggressiveness in large bodies of water. However, other mixing phenomena, which do not require equilibrium with a gas phase, may be much more important. Picknett (1977b; cf. also Picknett et al. 1976) has discussed this subject in some detail. Two main types of process are involved: the 'common ion effect', in which a second substance provides an ion the same as one of those in the carbonate system, and the 'ionic strength effect', in which all ions of the second substance are different from those in the carbonate system. The common ion effect causes a reduction in the amount of each substance that can be dissolved. For instance, the presence of magnesium carbonate reduces the maximum amount of calcium carbonate that can be held in solution, and vice versa. If two saturated waters that have different proportions of calcium and magnesium carbonates are mixed, the resulting solution will be supersaturated in calcium carbonate. However, the ionic strength effect causes an increase in the possible carbonate content of the solution. One of the most impressive examples of this effect in the cave situation is produced by the addition of sodium chloride; sea water and salty air blowing off the sea may almost double calcite solubility, as can be readily observed in most coastal caves. The appreciation of ionic effects is not, in fact, a simple matter of comparing standard chemical formulae. All species in solution must be considered and the resulting ionic activity coefficients must be calculated. Such calculations show, for instance, that, although magnesium carbonate will cause a common ion effect at higher concentrations, at very low concentrations a most significant increase in calcium carbonate solubility may result (Picknett 1977a). Similarly, calcium and magnesium carbonates may reduce each other's maximum concentration, but the joint amount of carbonates that may be dissolved is greater than could be achieved by the same water acting on one of the carbonates alone. Even more complex relationships may arise, such as the increase in calcium carbonate solubility in the presence of both magnesium and sulphate ions, a process which should be of importance in areas such as Derbyshire, where the Magnesian Limestone also contains gypsum.

Finally, calcite solution may be inhibited by the absorption onto crystal faces of metals such as lead, copper or manganese. Picknett (in Picknett et al. 1976) considers that this effect may be important, even at very low concentrations, but Stenner (1977), arguing from observation in G.B. Cave and the influent caves of St. Cuthbert's Stream (all on Mendip), suggests that the nett influence of these metals is small if they are only present in trace amounts (but <u>vide infra</u>).

When one turns to cave sediments themselves, it would seem most important to stress the factor of heterogeneity. It has already been noted that, in the shelter and cave entrance situations, spatial and temporal variation in water availability and initial aggressiveness, sedimentation rate, permeability and sediment chemistry will markedly affect alteration. Deeper into the cave system, the pattern of variation, as well as the significance of the pattern of variation, will change. Overall water availability will probably rise or become more uniform, so that most sediments will be permanently damp. There will still be variation, of course, but gross changes are likely to occur only over relatively large distances. Initial aggressiveness of water will usually be low or non-existent. Sedimentation rates will usually be low, but there will be rare events of very high sediment input. Permeability will usually be relatively low. On a gross scale, therefore, it would appear that there is likely to be less overall variation in the controlling factors deeper within caves than in threshold deposits. However, if certain factors that favour alteration, such as initial aggressiveness, are weaker, other factors may take on greater importance. Water, the 'universal solvent', is more or less constantly available. Generally low sedimentation rates allow concentrated exposure of material to alteration. Low permeability will no longer be particularly important with respect to water retention as such. It is slow water circulation that is the most interesting aspect.

In a situation where initial aggressiveness of water is high, alteration is obviously increased if this water is constantly renewed. However, water reaching cave sediments will often be saturated in carbonates. In this case, any alteration will depend upon rejuvenated aggressiveness, which will require time. Furthermore, given the likelihood that potentially aggressive substances, such as organics and pyrites, will only be present in small quantities, the maximum amount of solution can only be achieved if the potential of these substances is re-used as much as possible. This requires that hydrogen and carbon dioxide be retained within the chemical system for as long as possible, a requirement that has the important corollary that carbonates must also be retained within the system, since they cannot be removed in solution without an equivalent loss of the mobilising agents. All other things being equal, a system which promoted rapid switching between solution and precipitation of carbonates would allow more alteration of structured limestone fragments than would a system which promoted rapid leaching of both carbonates and aggressive substances. When considered on a microscale, individual pore atmospheres, localised water films and extremely variable sediment grain chemistry might allow such switching, as long as the system was not constantly homogenised by rapid water circulation. Naturally, excess CO_2 would eventually diffuse to the sediment surface and some water would leave the system, slowly removing all types of solutes. Such losses might be partially compensated for by the input of percolation water carrying very small amounts of particulate or dissolved organic matter. Similarly, percolation water might seep into the sediments before it was able to achieve equilibrium with the cave atmosphere, thus adding both carbonates and CO_2 to the system. That a significant time is, in fact, required for percolation water to achieve equilibrium is proven by the occurrence (and often the volumetric dominance) of floor speleothems, as opposed to roof speleothems. Another factor which would maintain alteration through time would be the slow destruction of limestone itself, with the liberation of aggressive substances (fossil hydrocarbons, pyrites, etc.). It should be noted that this 'microscale switching' is only a hypothesis, but it would be difficult to explain in any other way the very common occurrence in caves of relatively impermeable sediments that appear to have undergone contemporaneous solution and precipitation of carbonates without the formation of vertical gradients. Conversely, where significant spatial changes in the degree of solution and/or precipitation do occur, we may suspect either a spatial change in the pore water circulation speed or the localisation of a source of aggressiveness (initial aggressiveness in the percolation water itself, a layer of bat guano or plant debris, etc.), or both.

The general interpretation of limestone alteration phenomena by cave sedimentologists is usually couched in climatic terms, with little consideration of the actual chemistry involved.

Vértes (1959) recognised corrosion of limestone primarily as a function of warmer and more humid climate, although he did note some cases of minor alteration in layers thought to date from continental (glacial) periods. He recognised a common association between limestone alteration and organic matter, and he stated that corroded material was sometimes derived from exterior slopes. Vértes also made the following comments:

> Den Erfahrungen zufolge übt die in den Höhlen wirkende Diagenese nur auf die jeweilige oberste Schicht eine Wirkung aus, die tiefer liegenden, bereits eingebetteten Schichten werden weniger berührt. Es is ein wichtige Beobachtung, dass während bei der Löss-Diagenese die interstadialen und interglazialen Böden-Modifikationen des im vorangehenden glazialen Abschnitt abgelagerten Lösses sind, sind die Schichten in Höhlen, die die entsprechende Periode anzeigen, neue Gebilde und stammen nicht aus einer Veränderung früherer Schichten. (1959:16)
>
> [Trans.: In our experience, diagenesis acting within caves influenced only the layer which was currently at the surface, the deeper lying layers that had already been buried being little affected. It is a significant observation that, whilst, during loess diagenesis, interstadial and interglacial soils modify the loess deposited in the preceding glacial periods, the contemporary layers in caves are new formations and do not originate from the alteration of earlier layers.]
>
> Das eingebettete scharfkantige Schuttmaterial einer ozeanischen, einleitenden Phase verändert sich infolge der korrodierenden Faktoren des auf das Glazial folgenden Interstadials oder der heutigen, niederschlagreichen, gemassigten Zeit nicht mehr, das starke Sickern des Niederschlages - selbstverständlich, falls man in Höhlen von aggressivem, ungesättigtem Wasser sprechen kann - mochte die Kanten etwas ablösen, doch ist der frostige Ursprung des Schuttes auch in diesen Fallen mit Sicherheit zu erkennen. (ibid., p.45)
>
> [Trans.: The buried angular scree of an oceanic phase, at the begining [of a glacial], is not further altered as a result of the corrosive factors associated with the interstadial following that glacial or with the present, high rainfall, temperate period. Important infiltration of precipitation [atmospheric] must reduce the angularity somewhat (always supposing, of course, that one can speak of aggressive, unsaturated water in caves), but the cryoclastic origin of the scree may still be recognised with certainty in this case.]

The present author has some sympathy for this point of view, since it is clear that Vértes was trying to discredit the idea that true soils could form inside caves. Nevertheless, although the differences between the climatic sequences of Western Europe and Hungary may have resulted in less alteration in the latter area, it is difficult to believe that Hungarian caves exhibit no subsurface alteration phenomena and no true diagenesis at all.

Schmid writes:

> There are two causes of limestone rubble breaking off from the walls and roof of a cave: one cause is water escaping through cracks, clefts and breakages in the limestone and carrying with it calcium carbonate together with loosened clay and sandy substances, which limestone contains in varying quantities; these are deposited on the floor of the cave. In this this way the stone pieces surrounded by cracks lose their adhesion and fall to the floor. Evidence of corrosion, leaching of superficial alkalis, eroded and rounded edges are the indication of this on stone fragments of all sizes, which are embedded in an earth or travertine matrix. Such weathering is only possible in a temperate or warm climate with considerable precipitation.
> [...]
> [The] atmosphere and plants in the entrance area make an additional attack on the rubble by a soil-forming process. Here also the loosening power of percolating water can be increased by the outer climate, so that more residues are freed from the stone. (1969a:157-8)

Although this statement is not very clear (there would appear to be at least one mistranslation of the German original and 'to loosen' should probably read 'to dissolve'), Schmid seems to allow only pre-depositional and pedogenic alteration.

Miskovsky, in his general discussion, states:

> Or, nous savons que le gaz carbonique est beaucoup plus soluble dans les eaux froides. [...] Tout au long des grands stades froids et humides du Riss et du Würm, les eaux qui imbibaient la totalité du remplissage ont lentement décalcifié les sédiments faisant peu à peu disparaître les blocs tombés de la voûte par l'action du gel.
> [...]
> La dissolution des calcaires, bien que beaucoup moins forte qu'en période froide, se poursuivra aussi en climat chaud. Les eaux provenant des zones où des matières organiques sont en décomposition (sols de forêt, charnier), riches en bactéries, sont très agressives. (1974:35-6)

> [Trans.: Now, we know that carbon dioxide is much more soluble in cold water. [...] Right through the major cold and humid stadials of the Riss and Würm, the water which impregnated the whole of the fill slowly decalcified the sediments, obliterating little by little the blocks which had fallen from the roof due to ice action.
> [...]
> Limestone solution, although much less strong than during cold periods,

will also continue under a warm climate. Waters derived from zones where organic matter is decomposing (forest soils, accumulations of carcasses), which are rich in bacteria, are very aggressive.]

Miskovsky's study sites are in southern France; the suggested general timing of alteration may well be correct, but it is much more likely that simple water availability, rather than temperature effects, is the main control in this relatively arid region. Miskovsky makes no comment upon the possible influence on alteration of the proximity of the sea in his coastal sites. Solution is seen as quite distinct from deposition of carbonates, which Miskovsky interprets as due to a warm climate, that may be humid (bulk cementation and deposition at depth) or dry (surface concretions and encrustations). Presumably, corrosion of limestone clasts and redeposition of carbonates at depth are considered to be contemporary in Miskovsky's cave palaeosols (cf. section 12.3.).

Laville (1975) considers the aggressiveness developed by biological activity to be more important in the alteration of his shelter sediments than that due to increased solubility of CO_2 at lower temperatures. Solution and precipitation of carbonates are seen as essentially contemporaneous, but spatially distinct (cf. section 12.3.), and due to relatively warmer and more humid conditions. Laville also rejects the likelihood of chemical precipitation due to freezing (cf. p.270), apparently merely because sediment concretions are always found within deposits that Laville interprets (usually because of alteration effects) as having been altered under more temperate conditions. The precipitation of carbonate concretions within deposits inside caves is said to imply a break in clastic sedimentation of relatively long duration, although no reason for such a generalisation is given. Finally, the presence of abundant organic matter, either in shelter or cave deposits, is said to cause later preferential precipitation of carbonates, but no examples of such a phenomenon are offered in the body of Laville's text. This is a little surprising since, although associations of organic matter and carbonates are quite common in caves, such associations are not discussed in the cave literature; Laville's comment therefore suggests that he has actually seen such a phenomenon.

The second most common group of substances in cave sediments comprises the silicon minerals. Silicon is dominantly present in sand and silt as the oxide, quartz, although significant quantities of feldspar may also occur. Silicon is also present in all clay minerals and in the majority of heavy minerals. Silicic acid and silica gels may be imported with stream and percolation water. These substances have a very wide range of susceptibility to alteration, so that variations in the 'silica' content of sediments are difficult to interpret. It has long been a practice in the German school of cave sedimentology to quantify the silicon (as an oxide equivalent) in cave deposits; the present author has yet to find a report in which a really significant point is made using such data. Le Tensorer (1973), working at the shelter of Le Martinet (Lot-et-Garonne), has produced more interesting results by varying the extraction techniques so as to compare contents of silicon at two different levels of solubility. Nevertheless, interpretation of the results in terms of coupled eluvial and illuvial phenomena would appear to be dangerous until the source(s) of the silicon can be identified. Even if individual grains, such as the heavy minerals, can be shown to have been altered, there is always the possibility, indeed the probability, that they were imported in this condition. Similarly, characterisation of mineral suites as more or less altered ('weathering stability series', 'Bowen reaction series', etc.) usually only reflects inherited properties. Arguments must then be adduced concerning such matters as whether or not this alteration is likely to have occurred in exterior soils and, if so, whether or not transport into the cave is likely to have been penecontemporaneous with soil formation. There are very few instances in which alteration and neoformation of silicon minerals have been proven as processes of significance in cave deposits. The rare cases in which the question has arisen usually involve the clay minerals.

Laville's (1975) use of the ΔpH method for his shelter sequences automatically implies that he believes clay minerals to be subject to _in situ_ pedogenic alteration in such situations. However, the present author has shown (section 7.2.) that this method is not a reliable indicator of clay mineral type or degree of alteration. Farrand (1975) makes a reasonable case for clay mineral alteration at the Abri Pataud (Dordogne), using X-ray diffraction techniques to monitor lattice changes in the illites and deficiencies of smectites which appear to correspond well with other alteration phenomena, although proof that this is _in situ_ alteration rather than penecontemporaneous derivation from soils is lacking. Miskovsky (1969, 1974) has suggested neoformation of kaolinite from illite as a pedogenic phenomenon _inside_ Mediterranean caves. At Le Lazaret (Alpes-Maritimes), in an area over 10m inside the overhang, deep carbonate concretions contain a preponderance of illite but, where it is not concreted, the otherwise very similar substrate has more colloidal material with some 50% kaolinite. Miskovsky claims that early illuviation of carbonates has protected the enclosed illites. No explanation is given for the apparent inconsistency which arises if one compares this concept of neoformation of kaolinite with the mineralogical data provided by Duplaix (1969), who reports that the heavy mineral suites of the different horizons of this same palaeosol are uniform, including the 'vulnerable' minerals, epidote, amphiboles and pyroxenes, which always total c.20%. Similarly, there is no difference between the heavy minerals caught in concretions and those of the unconcreted substrate. Nevertheless, Miskovsky's observation is puzzling and it may be relevant that kaolinite can theoretically form merely by extreme desiccation of illite, a process which might not affect the other minerals present. It should be stressed, however, that, when dealing with interior cave sediments, most researchers, if they recognise a climatic influence at all in the clay minerals present, still interpret the suites as inherited. This is true, for example, of Frank (1969) at Douglas Cave (New South Wales), Bögli (1980) at the Hölloch Cave (Switzerland) or Jenkins (cf. p.1199) at Pontnewydd Cave (Clwyd).

If it is accepted that silicon minerals are generally stable in the cave environment, it should not be assumed that they are necessarily totally immutable. Where there is water, there is always a potential for alteration. It has already been noted (p.480) that some chemical modification of the surface of quartz grains may occur in caves and that alteration of chalcedonic rocks (including flint) is demonstrably common. The difficulty with the silicon minerals, as compared with the carbonate minerals, lies mainly in the extreme chemical diversity of the former group. The signs of weak alteration phenomena, which may well operate in caves, will be swamped by this diversity unless more detailed analysis, including greater consideration of lateral variation, are carried out than have been so far undertaken. Moreover, a much clearer distinction must be made in future between the processes of silicon liberation, translocation and deposition, each of which may respond to very different conditions. For example , the dissolution of limestone in weak acids often liberates small quantities of silica gel as well as particulate silicon minerals. In the natural chemical environment, this gel must be highly unstable; the factors which will govern whether or not the gel will be crystallised, combined with other substances, translocated or dissolved will not necessarily be those factors which determined the dissolution of the source limestone. An excess of silicon in a given deposit might also be due to fixation of the small amounts of this substance present in all natural waters, rather than to in situ alteration, although reports of well structured authigenic silicon minerals in caves are rare (cf. W.B. White 1976).

This brings us to a discussion of the metallic elements, manganese and iron, substances which in some respects should behave rather like silicon. Both manganese and iron occur in cave sediments, often in relatively important quantities, as components of discrete particles. Much of this material will be allochthonous but some may represent relatively stable minerals, such as hematite, that have been liberated from the surrounding limestone. If silicon is introduced into a sediment in solution or is actually mobilised within the sediment, it is difficult to recognise the fact on site unless quite large quantities are involved. However, even very small quantities of manganese and iron, that have been mobile at some time, are relatively easy to recognise because of the strong colours of the resulting precipitates. The gross distribution and structure of these precipitates can often be examined on site, so that a clearer idea of their origin can be had than is usually possible for macroscopically 'invisible' substances such as silicon precipitates.

Given the slightly alkaline and moderately oxidising conditions within most caves, the forms in which manganese and iron will be present may be predicted. First, either of these elements may be 'locked' within comparatively complex minerals (e.g. the garnet group and various silicates). It has already been suggested that such minerals are more or less stable in caves. In any case, quantitative arguments would show that these minerals could not possibly be the immediate source for the relatively high levels of manganese and iron often observed in cave sediments. Most simpler compounds involving these two minerals will also be stable: MnO_2 (pyrolusite and its polymorphs), the hydrated iron oxides (the crystalline form, goethite, and the amorphous form, limonite) and various crystalline (e.g. the spinel group) or amorphous ('wad', 'bog iron') mixtures of manganese and iron oxides and hydroxides. Zinc, lead or even aluminium may often be present in most dominantly manganese and/or iron compounds. Under these pH-Eh conditions, iron is usually present in a trivalent (ferric) form and manganese in a mixture of trivalent and quadrivalent forms. These simple manganese and iron compounds may often be present as allochthonous particles, but these are also the forms in which authigenic alteration products and precipitates are to be expected. One of the sources for these precipitates must be substances which are unstable in the cave environment. Unstable manganese minerals are likely to be quantitatively unimportant in most situations, although the carbonate, dialogite, may be present in some metamorphosed limestones. Unstable iron minerals are quite common as carbonates, phosphates and, especially, sulphides.

These minerals may be oxidised and hydrated, often with the help of bacteria, eventually giving rise to limonite or goethite. The mustard-coloured 'ochre' found in many caves (e.g. the sites of the Banwell area of Mendip; cf. Barrington & Stanton 1977) has a high limonite content formed by alteration of the pyrite veins in the surrounding limestone. The other common iron oxide, hematite, is also ferric, so that it is relatively stable although particles may show limonitic hydration crusts or inclusions in deposits that have remained damp over long periods. The reverse reaction, with neoformation of hematite, can occur after prolonged desiccation. Apart from major mineral inclusions and veins, manganese and iron are also present in limestone in a finely divided form. Weak organic acids will liberate these metals and it is possible that they may be, or become, associated with the silica gel which is also liberated. At the Abri Pataud (Dordogne), Farrand (1975) has shown how in some layers the contents of clays, iron oxides and easily soluble silica are closely interrelated and inversely proportional to carbonate content, all in a manner that is predictable from the composition of the local limestone. In the case of finely divided material the exact mineralogy is unclear, but limonite is probably present since some iron can often be extracted in very weak hydrochloric acid. It seems highly likely that some of the manganese and iron concentrations found in cave sediments represent residues left behind after solution of limestone. However, in situ alteration of limestone still cannot account for the large quantities of these metals found in some deposits.

Manganese and iron, at various valencies, are often present in ground water. These substances are taken into solution in chemical environments that are radically different from the usual cave environment. Significant mobilisation is normally associated with biological activity in soils, especially wet, acid soils (that are by no means restricted to temperate climates). The metals are often maintained in solution by chellation, the complexing of the metals with an organic ring structure. As water penetrates a cave, the rising pH would certainly cause any free metal ions to oxidise in the insoluble, high valency forms, with resulting precipitation. Complexed metals might travel much further into the system if the organic rings were able to survive. This allochthonous source of dissolved metals is almost certainly responsible for the excess observed in many cave sediments (cf. Stenner 1977).

Further speculation upon one possible route taken by manganese and iron precipitation may help to explain certain observed concentrations. Where limestone solution has been extreme, such as in a scree-rich layer overlain by stream deposits, limestone ghosts are often associated with significant quantities of metal oxides and hydroxides (cf. Pontnewydd Cave, p.1206). These minerals occur as irregular 'haloes' around the ghosts, but they are too rich to have been derived from the limestone clasts themselves. Particularly extreme forms of this phenomenon occur at Joint Mitnor Cave (Devon; cf. p.797). Sutcliffe (1966) described an earthy manganiferous deposit capped by an iron pan; he believed all this material to be water-laid insoluble residue from the limestone. In fact, this material is a huge limestone slab which fell from the roof, due to the incompetence of a hematitic mineral vein (the 'iron pan'), and which shattered into a series of large blocks. The carbonate has been totally replaced by dark brown manganese and iron compounds in such a way that the sharp contours of the blocks have been maintained, the dendritic calcite veins of the original have been preserved as clear red lines, and the stratification of overlying deposits, some of which are finely laminated, has not been disturbed. All limestone clasts in the older deposits at Joint Mitnor have suffered similar replacement, the phenomenon being less complete (more or less sound carbonate cores) as one passes upwards to younger layers. Furthermore, the cave walls themselves have been totally altered in places (especially at the artificial easterly entrance to the cave), so that a vertical band of 'wad', up to 40cm thick, separates the sound limestone from cave deposits showing undisturbed horizontal bedding. The contact between the 'wad' and the limestone is remarkably sharp, although calcite veins can be traced across the junction. Similar phenomena may be seen at Tornewton Cave and the Chudleigh caves (all in Devon), although zones of replacement are only a maximum of a few centimetres thick. Large deposits of 'wad' have often been reported from the caves of the South-West (cf. Edmonds et al. 1975; Barrington & Stanton 1977) but the origin of such deposits has rarely been discussed. It was noted above (p.503) that some metallic ions, including manganese, may be adsorbed onto the surface of carbonates, thus inhibiting solution, albeit not very efficiently. It follows that limestone surfaces should provide preferential zones of manganese fixation, since concentrations of adsorbed ions cannot remain stable for very long and must therefore be converted to insoluble oxides. These oxides form greasy, spongy masses which are highly permeable; carbonate solution and manganese fixation should continue as a coupled system under an ever thickening layer of 'wad', as long as no mechanical process strips the vulnerable product away. Therefore, 'wad' should only survive in significant thicknesses when it is being formed under or within a sediment, or possibly in a still pool. When limestone solution is rapid (e.g. directly below stream deposits), most of the limestone structure should be lost and manganese should be present only as a relatively diffuse 'halo' in the surrounding matrix. When solution is much slower (e.g. in deposits with a more stagnant water content) and when cave water contains significant amounts of metals in solution, true replacement might occur, with crude preservation of limestone structural features and little if any loss of volume. Vein calcite should be particularly prone to discrete replacement, since it usually contains a much higher iron component, in its own right, than the surrounding limestone. Such a replacement process is merely hypothetical, but it would seem difficult to explain occurrences, such as those at Joint Mitnor, in any other way. Since 'wad' usually contains large quantities of iron hydroxides, as well as manganese oxides, iron must also be fixed at carbonate surfaces (cf. Stenner 1977). Certainly, high concentrations of iron cause carbonate deposition; at Pontnewydd Cave (Clwyd), red stalactites form on rusty steel pins, set in the walls, at a rate of 2-4cm (length) per year when, elsewhere in the cave, contemporary speleothem formation is too slow to be easily appreciated in a five year period. Indeed, where iron is the dominant metal in natural solutions, replacement of limestone by purer limonite might be possible. Stanton (1973) has suggested that hollow shells of limonite in the lower deposits at the Westbury-sub-Mendip Fissure may have formed by the imperfect replacement of limestone clasts; the iron might have been derived from glauconite sands (Greensand) in the vicinity. The present author has also observed staining and what is best described as minor 'case hardening' of the limestone bedrock at this site.

Another substance which appears to have an affinity for manganese, and possibly for iron as well, is bone. At Pixie's Hole (Devon; cf. p.835), a sandy stream deposit overlies a thick, composite stalagmitic floor. The base of the sand is indurated with limonite, and the underlying stalagmite has been altered to a soft, chalky carbonate, with small quantities of metal oxides and hydroxides dispersed throughout as tiny flecks. Within the stalagmite are pockets of bear bones, all of which are very hard, brittle and uniformly brown/black. On a fresh break, patches of metallic lustre can be seen. Bones in this condition are quite common in caves, especially if they have been in proximity to a stream. Even in merely damp deposits, dendritic 'blotches' of manganese oxide often occur on bones. At an older level in Pixie's Hole, the rare bones found actually within stream gravels are bright red, suggesting a higher iron content. Either bone apatite actually fixes metals, or bacteria are responsible (cf. Cubbon 1976). In the latter case, it is possible that the bacteria use the organic content of the bone as an additional fuel source to that provided by oxidation of the metals.

The interpretations placed upon concentrations of manganese and iron by cave sedimentologists are extremely varied and often linked, quite rightly, with arguments concerning the geological sources of these metals. For instance, Laville (1975) recognises small nodules and encrusted sand grains which were probably mechanically derived from local cretaceous sands (Sidérolithique). However, Miskovsky places a purely climatic (pedogenic) interpretation on any authigenic precipitates of these metals. It should be remembered that Miskovsky's sites are in Mediterranean France, where movement of manganese and iron in true soils is far from unlikely under present climatic conditions. However, certain occurrences appear questionable as climatically controlled phenomena. At La Baume-Bonne (Basses-Alpes), Miskovsky describes a rissian sequence from an unspecified area within the cave. That this area must once have been well within the true cave environment is shown by the occurrence of thick stalagmitic floors within younger (würmian) deposits. Two layers, L and G, within the rissian sequence are qualified by Miskovsky as "sols à croûte superficielle de fer et de manganèse [soils with superficial iron and manganese crusts]"(1974:151). It is clear that Miskovsky sees these crusts, which may be up to 20cm thick with manganese nodules and goethite to even greater depths, as in situ soils, formed as the climate became warm and arid (ibid., p.41). However, Miskovsky fails to describe a mechanism that would be able to mobilise such stable elements within a cave and, more importantly, he fails to identify a source for all this iron and manganese. The substrate for both these crusts contains abundant limestone clasts, which are relatively sound below crust L but altered (partially decalcified) below crust G. Miskovsky states that the formation of both crusts was followed by a period of stream erosion. The present author can see little difference between the precipitational phenomena described from La Baume-Bonne and those in British caves, such as Pontnewydd; the latter are not due to any direct climatic control and they are most certainly not the result of any sort of soil formation. Miskovsky (1969, 1974) also describes a 'soil' from the Grotte du Lazaret (Alpes-Maritimes), with quantification of the iron and manganese oxides (cf. also section 12.3. below). It will be interesting to use these figures merely to gain an 'order-of-magnitude' estimate of the sort of phenomenon Miskovsky is suggesting. The deposit involved is not less than c.30cm thick. It is said to have formed during only a part of the Riss-Würm Interglacial, let us say a period of c.10ka, which is probably quite generous. The iron and manganese contents are given only for the colloid+silt fraction, which on average accounts for c.40% of the sediments. Iron oxides amount to c.6% of this material, manganese oxides to c.0.22%. These figures are equivalent to a layer of pure iron oxides c.0.72cm thick and a layer of pure manganese c.0.0264cm thick. If the figure for iron oxides is taken to represent *in situ* weathering of an iron bearing substrate, the rate of formation is of roughly the same order of magnitude as that suggested by Goudie (1973) for rates of laterisation in open sites in tropical latitudes. However, at Le Lazaret, the immediate substrate cannot possibly have provided these high levels of oxides since the mineralogy is hardly comparable with that of the igneous rocks from which most tropical laterite is formed. We must therefore assume that the metals were predominantly introduced in solution. Miskovsky reports that the iron oxide measurements represent a mixture of Fe_2O_3 and FeO, and that manganese is expressed as MnO. We may allow concentrations in solution of 0.2ppm for iron and 0.002ppm for manganese, figures which in Britain today would suggest the proximity of mining waste or of a naturally rich metal source (cf. Picknett et al. 1976; Stenner 1977), but which would not seem unreasonable in the Mediterranean context. The observed accumulation of iron would therefore represent a throughflow of water in the range c.2.5-2.8m/y, depending upon the proportions of the two oxides actually present and granted total removal of iron from solution. The figures for manganese would represent a throughflow of c.10.2m/y, again granted total removal. Observations in many caves suggest to the present author that fixation of manganese is more efficient than that of iron, so that the apparent discrepancy between the above figures is not surprising. The calculations presented here would overestimate throughflow in proportion to the original oxide content of the sediment but, since the values chosen for all other parameters are likely to have the reverse effect (high concentrations in solution, total removal from solution, oxides adhering to sand grains not accounted for, etc.), the estimate does not appear unreasonable. Now, Miskovsky would have us believe that this 'soil' formed inside a cave, in an arid climate. Even in a very wet climate, it is difficult to see how mere percolation water could amount to 10m per year - 1m per year would be impressive! However, a stream (in any climate) could easily supply such throughflow in immediately subjacent sediments. Much of the substrate for the 'soil' at Le Lazaret is very well bedded alluvial sediment, containing altered limestone clasts. The material immediately above the 'soil' is also alluvial, but there is no limestone and metallic oxides appear not to be present in sufficient amounts (or in sufficiently obvious segregations) to have prompted quantification.

Many other substances occur in caves, either as minor constituents or as comparatively rare concentrations. An example would be the phosphates and nitrates associated with decaying organic matter. Very little is known about the practical chemistry of such reactions (but cf. Penaud 1978), save that increased acidification will increase alteration of many more common minerals. Concentrations of phosphates are often observed directly under surfaces once occupied by animals or man, but the use of this parameter as a 'prospecting' tool or as an argument in the reconstruction of activity areas would be unwise without a very thorough investigation of the chemistry involved. The present author has never come across a report of such an investigation. General discussion of phosphates (cf. Proudfoot 1976; W.B. White 1976) would suggest that the whole subject is far from straightforward. Schmid (1969a) claims that the mineral, scharizerite, said to be related to the phosphate earths, is specifically associated with the decomposition of animal carcasses and excreta. The present author has been unable to find an exact description of this mineral, beyond the fact that it is not a phosphate but, rather, a nitrogenous hydrocarbon isolated in 1926 from black speckles in phosphate-rich sediments from a single Styrian cave; glossaries do not report verification by modern methods.

12.2. The Recognition of Alteration Involving Carbonates

Since the abundant carbonate material in caves is so easily altered, at least the initial discussion of alteration is usually couched in terms of carbonate solution and precipitation. It will therefore be of interest to examine briefly some of the criteria used. Apart from variation in bulk carbonate content, solution is normally approximated by the recognition of changes in limestone clasts, and precipitation by the recognition of concretions of various morphologies within the sediments.

The most common measure of limestone alteration is probably clast rounding. Most limestones will fracture into relatively angular fragments; subsequent solution will tend to round the angles. Care should be taken to differentiate between rounding and overall sphericity since, in the present author's experience, solution may often result in a lowering of sphericity. This is especially true of limestones with heterogeneous lithology or structure, which may weather to produce a complex pattern of ridges and re-entrant forms. High sphericity usually indicates that mechanical modification has occurred at some stage, unless it can be demonstrated that the limestone tends to fracture into cubes in the first place. Extreme solution may re-instate quite sharp angles as the clast crumbles. Similarly, clasts of massive limestone may have very smooth surfaces but may retain some major sharp angles, often bounded by slightly concave surfaces. Rounding is usually estimated visually, with separation into four or five subjective classes (cf. Brunnacker & Streit 1965; Miskovsky 1974; Laville 1975). The calculation of 'indices' from the basic data is not recommended and minor fluctuations should not be interpreted as significant without corroborating evidence (cf. Chapter 6). The present author doubts that the use of more formal methods (e.g. Wadell's Rounding Index; cf. Collcutt 1975 for an application at Mother Grundy's Parlour, Derbyshire) results in an increase in accuracy commensurate with the enormous cost (time) involved, although comparator charts may be helpful. The possibility that rounding is mechanical rather than chemical should always be borne in mind.

A parameter, often referred to as 'porosity', involves an increase in the water absorption potential of clasts. The usefulness of this measurement is entirely dependent upon the lithology of the limestone. Laville (1975), working with limestones that are intrinsically quite porous, interprets water absorption, measured after total immersion, as proportional to alteration, a suggestion which would appear to be generally reasonable as long as pores have not later been occluded by surface or interior concretions. However, the parameter is only stable if uniform solution throughout the clast has occurred; if solution is greater near any surface, the shape (surface/volume ratio) of clasts must be at least roughly standardised for comparison between samples. Laville does not appear to have taken this precaution. The present author has subjected clasts (5-10cm diameter) of several limestones, mostly calcarenites and bioclastic types, from cretaceous outcrops around Les Eyzies (Dordogne) to very gentle attack by organic acids for up to two years. Microscopic examination of cross-sections shows that solution is far from uniformly distributed throughout the body of the clasts. Observation of natural clasts and rockfaces leads to the same conclusion. However, when a selection of the experimentally altered clasts had their porosity 'corrected' by weights proportional to their surface/volume ratios, porosity was found to be reasonably stable for clasts that had suffered the same alteration regime, granted only that they were of similar lithology. The only limestones which the present author has come across that appear to allow more or less uniform increases in porosity, at least across clasts below c.5cm thick, are certain types of coarsely crystalline ('saccharoidal') dolomitic rocks. Measurements of increased bulk porosity are not very useful for British limestones, since they are mostly either calcite mudstones or massive types, both of which tend to weather, except under extreme circumstances (*infra*), by the formation and exfoliation of crusts and by splitting along structural planes. An attractive alternative to the total immersion technique was suggested by Brunnacker and Streit (1965), who simply let a standard drip fall onto a dry limestone surface and noted the time it took for the water to be absorbed until only a film was left. This method is still subjective, of course, and the measurement based on timing is highly dependent upon pore size. Nevertheless, it is possible to investigate on site the variation of porosity on one surface, or differences between different surfaces, of a single clast. The method also allows very rapid appreciation of spatial variations within sediment bodies, information that is much more important than bulk quantification.

Some authors have calculated the density of limestone clasts which, in theory, should be proportional to the porosity. Miskovsky (1969,1972,1974) provides results of such analyses but he does not describe the exact method used. The present author has tried various methods of measurement, including sealing of clasts in waxes of known density before weighing in air and water, but sufficient accuracy (estimated by reproducibility) could only be achieved to differentiate between already rather obvious degrees of alteration.

Laville (1975) reports an attempt to divide clasts into subjective classes of friability, but he feels that the results are less meaningful than porosity measurements. The mechanical properties of clasts could, in fact, be accurately measured using standard engineering techniques, either in terms of surface hardness ('penetrability') or of competence under stress.

The overall state of corrosion may be estimated subjectively (cf. Brunnacker & Streit 1965; Miskovsky 1974; Laville 1975). This involves the recognition of different alteration states of surfaces, and sometimes clast interiors, usually without explicit definition of just what it is that one is observing.

In general, one could wish for more precise methods of measuring alteration than those listed above. There is a general tendency, which the author finds disturbing, for researchers to trust mechanistic quantification, no matter how obscure or variable the measured parameter may be, rather than their own judgement. Thus, most authors prefer a measurement of 'porosity' to an estimate of the degree of alteration. Clearly, more mechanical and statistical aids could be used, but the present author feels that more accurate quantification is not the only possible approach. It is obvious that different limestones will behave in different ways under the same conditions of alteration. More importantly, however, the same limestone will often behave in different ways under different conditions of alteration. It was stated above that denser British limestones often weather by exfoliation of crusts or by splitting. The former pattern is common in 'average' cave fills, whilst the latter often occurs in matrix-poor deposits. In close proximity to running water, there is often little surface expression of weathering but clasts may become unsound with a very blocky fracture. In very damp deposits, clasts may eventually be reduced to chalky 'ghosts', with more or less pronounced fixation of metallic compounds if there is significant movement of pore water (cf. section 12.1.). In deposits where carbonates are only a minor component and the matrix is permeable, clasts may be highly porous to the extent that the carbonate framework becomes an open tracery ('honeycomb'). In dominantly silty deposits, clasts surfaces are often 'pock-marked'. In acid deposits, calcite veins and fossils (even when not silicified) stand out above the surrounding surfaces. Most British dolomitic limestones tend to weather by granular disintegration, with internal colour banding if the deposit is damp. These are merely a few broad generalisations based upon the author's observations of modern environments, but they suggest that simple quantification of a parameter such as porosity throughout a long cave sequence may not, in fact, reflect only changes in the degree of alteration. Whether or not 'fissured' clasts are due to chemical processes, as has been suggested in a previous chapter (p.301), it is obvious that the 'porosity' of such clasts has nothing in common with the 'porosity' developed by solutional enlargement of existing pores. It would seem desirable for each researcher to build up a study collection of clasts of each different lithology in his area. For a given lithology, clasts should be collected from as many different sedimentary environments (matrix type, drainage, pH, etc.) as possible, with a view to recognising different types of alteration, granted of course that such types do exist. A type would be defined by those qualitative and quantitative measures which were necessary to differentiate between environments. The environments which could be recognised in ancient deposits would probably be rather vague, but at least the eventual measurement of the actual degree of alteration would be based upon those parameters which are most relevant to a particular alteration type.

Finally, it should not be forgotten that limestone clasts occur in the sand grades. Small clasts should be very sensitive to alteration and, as pointed out by Goldberg (1979a), microscopic examination of thin sections should also help to overcome some of the problems inherent in the interpretation of bulk carbonate content, in which clasts and precipitates cannot normally be differentiated.

Carbonates precipitated within the body of cave sediments have received little attention, apart from the general recognition of their presence. For instance, Laville (1975:37-8) recognises only one main type, which he calls 'illuvial concretions'. This type is split into four subdivisions: (1) particles of various material bound by calcite into aggregates; (2) purer calcite crusts around stones or passing through the sediment; (3) tubes of calcite developed around roots; (4) "concrétions bourgeonnantes ['cauliflower-head' concretions]", usually found on the underside of stones in openwork deposits. To the present author's mind, there is no reason why any of these forms should be exclusively illuvial. Having recognised these types, Laville quantifies only two categories, stones (10-100mm) with concretions, and 'loose' concretions (2-5mm); 'root tubes' are noted in general sediment descriptions. Miskovsky (1974) recognises larger and/or composite forms, such as columnar and massive concretions, said to equate with illuvial horizons of palaeosols. He also notes 'bedded crusts' at l'Hortus (Hérault), thought to result from periodic violent storms in a generally arid environment; no explanation is given for this interpretation. Most researchers of the German School see the majority of sediment concretions as due to input of carbonate-rich water rather than to significant illuviation or authigenesis.

The general lack of interest in cave sediment concretions would appear to stem from individual authors' beliefs that all types are due to more or less the same process, and that this process is a primary function of climate (rising humidity and, often, temperature). Phenomena that cannot be explained in this manner, such as massive and relatively homogeneous cementation of 'cold' deposits, can always be referred to the higher potential CO_2 content of cold water (cf. section 12.1.). Such blind determinism is hardly likely to lead to better understanding. The only way to examine assumptions concerning formation, and ultimately climate, is to set up a much more objective and detailed vocabulary for primary descriptive purposes. Researchers concerned with open-air calcrete have developed a vocabulary for many large scale, composite phenomena (cf. Goudie 1973), whilst soil micromorphologists have a terminology for small scale phenomena seen in thin section (cf. Brewer 1964; Goldberg 1979a). Sedimentary geology has a vocabulary for crystalline structures and solutional grain contacts (cf. Pettijohn 1975). It is this branch of geology that has also produced much of the systematic work on fossils and trace fossils, a less objective subject relying upon basic arguments concerning genesis. For instance, Klappa discusses "rhizoliths", the "organosedimentary structures produced by roots"(1980:613). Where Laville recognises only one type, Klappa has five (each with subdivisions): *root moulds*, tubular voids left by decaying roots; *root casts*, carbonate or sediment filled root moulds; *root tubules*, cemented cylinders around root mould or cast; *rhizocretions* (*sensu stricto*), cemented accumulations around mould or cast, with etching and other "pedodiagenetic" modification of the sediment, often associated with root hairs; *root petrifactions*, impregnated roots with organic structures preserved by replacement. These types are described by field observations and by optical and scanning electron microscope observations. Mention is also made of how rhizoliths may play a part in such features as layered or brecciated (*sensu stricto*, i.e. internally fractured) concretions. Dendritic forms, which are probably related to rhizoliths, can also be found as surface concretions on limestone clasts (cf. Farrand 1975). Alternatively, clasts may be quite deeply etched by roots, giving rise to 'hemi-moulds'. Not all tubular and branching concretions are rhizoliths. The present author once spent rather a long time excavating a weakly cemented ants' nest at the cave of Vaufrey (Dordogne) before an area which was still occupied was reached, finally giving the game away. Small burrowing creatures are not likely to be very common in most caves, but even earthworms may sometimes be found in organic-rich sediments within the diffuse light zone. Wherever voids are present, whether their origin is biological (root channels, burrows, etc.) or geological (pores, fissures, gas bubbles, etc.), there is a potential for different forms of concretion. In some cases, it may be possible to differentiate between concretions produced by degassing, evaporation, freezing (cf. p.270), biological precipitation or inorganic catalysts. The source of the carbonates is important, with the possibilities of input by drip or stream water, illuviation and strictly local (authigenic) derivation. Finally, the time of formation, the duration of formation and any distinct stages of formation must be deduced. This will require detailed examination of spatial information, a subject which, sooner or later, will force us to recognise different types of spatio-temporal organisations and to consider whether or not some of these may be usefully defined as 'soils'.

12.3. Alteration Systems and Soils

An alteration system is here defined as a spatial organisation of alteration phenomena in a sediment substrate, that conveys temporal and, eventually, environmental information based upon deduction of the depositional and postdepositional processes involved. Depositional factors must not be underestimated and it is not necessary for depositional and postdepositional phases to be discrete. Temporal properties are most important since, for the concept to be useful, it must be decided when, in a stratigraphic sense, the system formed. A system may itself show an internal sequence of events or phases. As the time interval involved becomes longer and as the processes involved become more and more independent of contemporary surface processes, it will eventually be more useful to speak of diagenetic systems rather than alteration systems. If recurring patterns of alteration can be recognised, the resulting generalised alteration system will become a concept akin to a sedimentary motif or facies (cf. Chapter 3) and the term alteration motif may be applied. As an example of an alteration motif, one might suggest the common occurrence of water-laid sediment stratified above relatively impermeable carbonate-rich sediment, with manganese and iron precipitates and more or less altered carbonate particles in the relatively indurated contact zone (cf. Pontnewydd Cave, p.1206). Many variants may occur, which may be recognised either in individual systems grouped into the motif or as lateral variation common to all or groups of systems within the motif. In the example cited, one might recognise a variant with specifically sandy water-laid sediment, often itself slightly indurated, usually by iron compounds. Another variant might be the composite systems which might be expected to form by concurrent or alternating deposition and alteration. Common lateral variants would be the relatively thin and homogeneous alteration zone beneath the centre of a stream channel, as compared with the thicker and heterogeneous (colour mottles representing segregation of precipitates, with recognisable carbonate ghosts) alteration zone towards channel sides and cave walls. The 'power' (usefulness) of an alteration system or motif is increased if lateral variation, showing logical relationships with the processes thought to be responsible, can be recognised. Note that an alteration system is a lithogenetic concept; the equivalent lithostratigraphic concept is a carefully described lithozone, part of a lithozone or group of lithozones, or possibly a lithohorizon (cf. Chapter 5).

Having recognised phenomena that could be classed as alteration systems, there is a great temptation to refer to at least some of them as 'soils' or 'palaeosols'. The literature on cave and shelter sediments shows a general division into two camps: those authors who recognise soils more or less whenever a clear alteration system occurs in an area assumed to have received at least a little light, and those authors who vehemently deny the possibility of soils in these sites. Unfortunately, few if any of these authors actually define what they mean by a 'soil', regardless of whether they are suggesting or refuting such a phenomenon. The concept of a 'soil' comes with an implicit load of environmental baggage, but this baggage is not the same for different scientific disciplines or even for individual researchers within one discipline. Shackley write:

> Much confusion exists over the interchange of the terms 'soil' and 'sediment' in archaeological contexts. Numerous definitions of a 'soil' exist, varying from the simple ('a medium in which plants grow') to the more complex ('the products of the decomposition of the land surface under the influence of weather and vegetation')(Zeuner 1959) and to the frankly obscure ('the result of the action of pedogenic processes'). American geologists tend to regard the term as referring to all materials produced by weathering _in situ_, regardless of their depth or whether or not they have been penetrated by plants. For the engineer soils and sediments are lumped together as deposits which can be moved by earthmoving machinery without the need for blasting, and for the agriculturist a 'soil' is only the weathered uppermost layers of surface deposits in which plants will grow. For the purpose of this book a 'soil' is taken to mean a deposit which has been weathered and altered _in situ_ to such a point that a vertical section taken through it will show some interior zonation, a division into horizons which are the result of the movement through the profile of certain constituents. (1975:3)

Shackley's statement illustrates the difficulty rather well. The present author would expect each of the groups of specialists cited by Shackley to object violently to her assumptions concerning their own attitudes to soils, and yet they would probably accept her statements about the attitudes of other groups. Furthermore, Shackley's own definition, which might seem straightforward and sufficient, would in fact allow the inclusion of alteration systems which would probably be unacceptable to her, and the exclusion of systems which pedologists, if not Shackley herself, would normally include in the category of soils. For instance, alteration below a stream may often result in the formation of clear horizons and/or vertical trends, and the resulting heightened availability of various minerals may often attract roots (and thus cause the formation of rhizoliths) tens or even hundreds of metres into a cave system. Alternatively, some quite respectable soils show little if any horizon development. Arctic soils, even leaving lithosols and regosols aside, quite often have no mineral horizons and they may also lack significant organic matter (cf. Tedrow 1977; Washburn 1979), although they are by no means featureless. Even rendzinas, so common in British limestone terrain, frequently show only an organic mat, a 'middle' and an altered bedrock interface. Moreover, many pedologists (cf. Brewer 1968) would dispute Shackley's suggestion that horizon differentiation is necessarily a result of significant translocation (eluviation/illuviation).

The definition of the concept of a 'palaeosol' would be relatively easy if a good definition of a soil could be suggested first. A palaeosol is simply a soil which formed under environmental conditions that were significantly different from those now acting in that area. There are several types of palaeosol, but the one which most concerns the cave sedimentologist is the 'buried palaeosol', consisting of an ancient soil that, since its formation, has been covered by a significant depth of sediment. Whatever the exact meaning of the term 'soil' in this context, it is at least limited to the various usages applied in pedology. Thus, palaeosols are recognised on actualistic principles: an alteration system is a palaeosol if it can be argued that it was once a system that would be acceptable to pedologists as a soil. Naturally, we will have to take into account anything that might have happened to the soil since its burial that might cause it to differ from its modern analogues. However, is it possible to invert the proposition? Are all modern systems that are acceptable to pedologists as soils potential palaeosols? The answer to this question is probably 'no', thus casting some doubt upon the original proposition. The difficulty here is that, although pedologists are prepared to classify superficial phenomena in all modern subaerially exposed sediments (and even in some sediments covered by minor water bodies) as soils of one type or another, it would be meaningless (i.e. not useful) to suggest that all sediments are palaeosols that, at some time during their existence, have come within hailing distance of a subaerially exposed surface. The author is not competent to give an accurate and all-inclusive definition of soils and palaeosols, but it would appear that it is important to pursue this concept of 'usefulness'. Why is it useful for a pedologist to call the surface of a modern scree a lithosol, whilst anyone labelling an identical fossil scree in the same way would be accused, at best, of being pedantic? Perhaps the difference lies in the fact that the pedologist sees the modern scree as part of a whole landscape, whilst the fossil scree is usually only scree exposed in the side of a trench. Furthermore, since pedologists see all modern soils as part of a landscape, any reticence about accepting a buried phenomenon as a palaeosol might be due to fears that nothing very useful could be safely deduced about the ancient landscape in that particular case.

Valentine and Dalrymple (1975) approach the problem of identifying palaeosols by detailed description and discussion of buried podzol-like phenomena from two open-air sites, one in Lincolnshire and the other in Norfolk. These authors' general views on palaeosols are expressed in a later publication (Valentine & Dalrymple 1976), and together the two articles present a balanced and highly accessible summary of the problems. First, the need to recognise the possibility of a palaeosol is pointed out. Field observations of such features as colour, structure, concretions, clay skins, decalcified horizons, etc., will provide the initial clues. Horizons, if present, should be recognised, and it should be decided whether the profile seems complete (with the top defined by organic horizons) or truncated. Any special features, such as ice-wedge casts, should be noted. Laboratory work is then needed with quantification of such features as pH, Eh, particle size distribution, organic content, or the content of elements (N, K, P) which are often associated with biological systems. Weathering indices of different minerals may be assessed. Micromorphology is particularly important in order to see how the various 'bulk' properties are related. The object of all this work is to see whether the phenomenon shows a logical progression of properties down the profile, in accordance with what is known about modern pedogenic processes. Similarly, one would wish to identify a logical order of events during the formation of the phenomenon; for instance, carbonates should usually have moved before clays. A stage is reached when one can say that a buried phenomenon looks very like a particular modern soil type. Then one must decide whether or not this similarity might be due to features inherited from the sediment substrate or to features developed after burial. If the phenomenon was not a soil (or at least a biologically active soil), any pre-existing sedimentary fabric should not have been disturbed by roots and soil fauna. If the phenomenon shows horizons that do not correspond with pre-existing sedimentary units (best defined by the particle size distribution of sands and the proportions of the more stable mineral types), it may well have been a soil, although correspondence between horizons and layers does not necessarily disqualify the phenomenon from being a palaeosol. Many soil features, such as the characteristic colouring (thus mineralogy) of gleyed horizons or many types of organic matter, are extremely vulnerable to post-burial changes, so that one would not base one's arguments solely upon the presence or absence of such features. One must be careful to exclude all obviously post-burial additions (such as secondary cutans or concretions) and, in order to assess the likelihood of post-burial derivation, it is a good idea to examine closely the material immediately above the top of the phenomenon thought to be a palaeosol. All this information should be compared with a logical prediction of what a given soil type ought to look like after post-burial changes in a given subsurface environment. Finally, one may check that any fossils that might be present (pollen, phytoliths, mollusca, etc.) would not be out of place in the soil type suggested. For the future, it is possible that infra-red spectra of humic acids (cf. Dormaar 1967) or the analysis of amino acids (cf. Goh 1972) may provide criteria for the recognition of palaeosols and palaeosol types.

There are certain key statements in these articles by Valentine and Dalrymple that appear to converge with the ideas discussed earlier in the present section.

> There are very few features, if any at all, which occur in soils and which cannot be found in sediments .... (1975:552)

> ... buried paleosols must be recognized within the hypothesis of soil formation rather than from the occurrence of individual features. (1976:213)

The onus is placed squarely upon the need for a buried phenomenon to have been demonstrably formed in the same way that a modern soil, or soil type, is formed. Some difficulty remains because of the lack of a precise definition of 'soil', but recognition of palaeosols becomes viable at least within certain limits. Individual features are not diagnostic. A polythetic classification, with a random selection of features from a list of possible soil features, is not diagnostic. It is the logical relationship between those features which are present, coupled with logical explanations for the absence of other features, which are potentially diagnostic. The problem over the definition of 'soil' is largely classificatory, that is, there is overlap with other phenomena. However, many soil types and soil process systems are quite well understood (cf. Fenwick & Knapp 1981) and there is no lack of consensus concerning their significance. A palaeosol will therefore be identified more easily and more plausibly if it seems likely that it is an analogue of a modern soil type which is well understood in terms of pedogenesis, which entails a good range of interrelated features, and which involves as many features as possible that would be stable if they were to be subjected to the post-burial environment thought to have affected the supposed palaeosol. Valentine and Dalrymple go even further:

> ... in order to prove beyond reasonable doubt the existence of a buried paleosol, it is necessary to show that its morphology changes laterally as well as vertically. It should vary logically across a landscape and should form a paleocatena. This is the one ubiquitous soil characteristic that sedimentation and diagenesis will not produce. (1975:589)

Unfortunately, there is some disagreement about the meaning of the terms 'catena' and 'pal(a)eocatena'. Valentine and Dalrymple use the latter to refer to lateral changes brought about by downslope changes in soil water regimes. Other authors introduce considerations of whether or not variations in substrate, aspect or regularity of slope profiles should be allowed to affect the definition of a catena. Nevertheless, the general proposition is clear: modern soils are part of a landscape, so palaeosols are most unequivocally recognised if they too can be demonstrated to be part of an ancient landscape. Such an approach automatically vindicates the usefulness of the recognition of buried phenomena as palaeosols. Catenas do not develop as a response to sedimentation or diagenesis. However, all alteration systems respond to lateral variation in such features as moisture regime (supra), so that logical patterns of a generalised nature should not be mistaken for catenas. For instance, the simple pattern of aerated (oxidised) deposits in a well drained area (e.g. the top of a slope) changing to anaerobic (reduced) deposits in a waterlogged area (e.g. the bottom of a slope), may well be a striking feature of a given (palaeo)catena, but it is by no means restricted to soils. It is the particular combination of logical soil profiles and logical lateral variation in these profiles, as seen in modern soil systems, which is diagnostic.

The relationships between landscapes, surfaces, pedogenesis, sedimentation and erosion must be briefly examined. In the literature related to archaeological sites, palaeosols are often seen as static patterns bounded by an ancient land surface. This is a rather dangerous oversimplification of the facts. Similarly, the presence of a palaeosol is often said to imply, automatically, a break in sedimentation. This is quite simply untrue. A soil develops from a land surface, by pedological definition, but the land surface will very rarely remain significantly unchanged during pedogenesis. Sedimentation may, and often does continue. Recognising the effects of concurrent sedimentation is an integral part of the task of recognising the mechanisms of soil formation (cf. Gerrard 1981). It is true, however, that as sedimentation rates increase, the effects of pedogenesis become more and more diffuse and unorganised, especially with respect to horizon differentiation. Furthermore, soils may develop on substrates that are suffering nett denudation, with a similar degree (but not pattern) of blurring of the profile. A soil is always related to a surface, but this surface is always changing. If the environment changes relatively slowly in such a way as to have a significant effect upon soil formation, new patterns may be superimposed on the old, producing a complex 'palimpsest'. Many modern soils are known to be the end result of a series of pedogenetic phases. The more complex the palimpsest, the more difficult it is to disentangle the various stages. If, however, there is a relatively rapid increase in sedimentation rate, which may or may not be associated with a change in general environment, the development of a particular type of soil may be more or less abruptly arrested. Most known palaeosols are of this sort, simple because they are the easiest to recognise. The top of such a palaeosol is indeed an ancient land surface, but the phenomenon as a whole represents a 'frozen' state of what was a dynamic system, including sedimentation, erosion and pedogenesis. Furthermore, the soil was not necessarily 'frozen' at a particularly characteristic stage of development. This has important implications with respect to the likelihood of the lateral persistence of recognisable palaeosols. Whole landscapes might be 'fossilised' by the onset of a major depositional process such as loess formation.

Incidentally, it is interesting to recall here that some of the properties of loess are pedogenic (cf. p.468). Soil formation always continues but, because the change in sediment input and general environment is so radical, we are justified in recognising the soil buried by the loess as a more or less discrete system, despite the fact that the buried soil may still be physically within reach of the processes of loessification (e.g. the buried soil profile may receive carbonates from the developing loess). Palaeosols, or palaeocatinas, in loess may sometimes be traced over very large distances. If, on the other hand, the deposition of sediments (in terms of either type or rate) is extremely variable laterally, only small portions of ancient landscapes will be preserved in a state that is sufficiently obvious for us to recognise them. Alternatively, palaeosols are perhaps the most important potentially time-transgressive phenomena likely to be encountered in relatively small scale quaternary sites. Such time-transgressiveness may often be expessed in a disjunctive manner. A palaeosol sealed under a deposit such as a rockfall might be traceable beyond the rockfall, but the observed 'frozen' states of the palaeosol in the two areas might be significantly separated in time, that is, two distinct landscape fragments might be present even if the soil profiles were very similar. This could be important in the context of archaeological stratigraphy, which seeks to differentiate very short periods.

We must now try to decide whether or not palaeosols are present in shelters and caves. The obvious way to approach this problem would be to look and see whether or not soils are now present in shelters and caves but, as in most areas of earth science research in caves, the actualistic data base is at present almost totally lacking. Theoretical arguments must therefore be considered. Frankly, the author is instinctively suspicious of the idea of a soil within a cave, a feeling which would probably be shared by the majority of pedologists and cave sedimentologists. However, one wonders how much this attitude is based upon rigid preconceptions concerning the role of vegetation and upon uncertainties over the exact definition of a 'soil'. Rather than dismiss the idea of cave soils out of hand, let us try to pursue the concept of usefulness. What would we do if we followed a remarkably well developed and preserved palaeocatena across an ancient landscape, right up to the mouth of a cave, and then found that the phenomenon continued right into the cave, with no apparent discontinuity, as a logically organised alteration system clearly akin to the exterior palaeocatena? Would it not be perverse to dissociate the cave phenomenon from the open-air phenomenon, merely because of our possibly biased preconceptions about soils? However, if we are going to allow the cave phenomenon as a palaeosol, we will probably generate considerable misunderstanding, so that a special term (a 'speleosol'?) would probably be necessary. There are two main objections to this idea. First, what do we do with a soil-like cave phenomenon which, because of erosion or other factors, cannot be traced to a 'classic' palaeosol in the open? Second, where does it all stop? Must we now recognise soils perhaps thousands of metres underground? The only answer presently available to these objections lies in a decision as to whether or not it would be useful to recognise a soil-related phenomenon, whether or not we could safely draw the same sort of environmental conclusions from this sort of phenomenon as we could from 'classic' palaeosols. For the time being, the author therefore adopts the following attitude. In order that a claim for a cave or shelter palaeosol be taken at all seriously, it must be demonstrated, not that the phenomenon has a random selection of soil-like characteristics, but that it was formed in a soil-like manner, and that there are no obvious signs of the operation of other processes which are more likely to have produced the phenomenon. In order that a claim for a cave or shelter palaeosol be taken as proven, it must be demonstrated that the phenomenon has features which are the logical result of its having been part of a well defined type of landscape. Until we can discover whether or not pedogenic processes do in fact penetrate caves, and how such processes might be modified by the cave environment, it is best to disqualify all isolated cave alteration systems from certain membership of the palaeosol category, no matter how soil-like they may appear. However, some shelter sites, which are shallow enough for the exterior environment not to have been significantly modified, might possibly contain phenomena that are so well developed as to be almost certainly palaeosols, even if no link with an exterior palaeosol or palaeocatena can be demonstrated. Given these requirements, the author cannot suggest any buried phenomenon from any British cave known to him that is even a possible palaeosol. Excavated shelter sequences are rather rare in this country, although there are a few cases in which buried soil-like phenomena occur in slope deposits very near caves but not actually within the shelter of a rock overhang (e.g. in the holocene deposits just outside Three Holes Cave, Devon). On the continent, and especially in France, palaeosols are often recognised in shelters and sometimes up to about 15m inside a cave, measured from the present dripline. Interestingly, the author has never come across a detailed description of such a palaeosol that is totally complete, the organic horizons and nearly always the so-called 'eluvial' horizons supposedly having been removed by subsequent erosion.

> Un stade et un interstade représentent un cycle complet, comportant une phase de sédimentation, une phase d'altération et une phase d'érosion; c'est la raison pour laquelle les sols interstadiaires, au même titre que les sols interglaciaires, sont des sols tronqués dont il ne subsiste fréquemment que les horizons d'accumulation profonds. (Laville 1975:359)
>
> [Trans.: A stadial and an interstadial represent a complete cycle, comprising a phase of sedimentation, a phase of alteration and a phase of erosion. This is the reason why interstadial soils, just like interglacial soils, are truncated soils, frequently with only the deep horizons of accumulation [illuvial horizons] surviving.]

This is certainly a very neat explanation, but the present author would like to see a few convincing exceptions, just to prove the rule, as it were. If this sort of reasoning is indeed correct, it is going to make the recognition of definite palaeosols infinitely more difficult.

The Riss-Würm palaeosol recognised at le Lazaret (de Lumley & Tavoso 1969; Miskovsky 1969, 1974) has already been mentioned several times in this chapter. The phenomenon is certainly described at least up to 15m into the cave beyond the modern dripline; it is not clear how far towards the exterior the phenomenon extends. Five horizons of accumulation are recognised, all 'eluvial' and organic horizons having been everywhere removed by erosion. Thicknesses of the individual horizons are not explicitly reported, but the whole phenomenon appears to occur within c.30-40cm. Percentages of constituents will be given below without reference to the nature of the 100% points, since it is the profile variation that is important to this discussion. Horizon 1 (argillic) is a reddish, compact and plastic clay (colloid content 23.5% and silt content 45.5%). There is a very well developed prismatic macro-structure (c.8cm spacing) and a polyhedral sub-structure (c.2-3cm spacing). There are hematite and goethite films on structural units. Limestone clasts are highly altered. It is suggested that pedogenesis has destroyed the original bedding of the substrate, although rhizoliths or other trace fossils are not mentioned and it is not explained what the original bedding is thought to have been like. Horizon 2 (argillic) is a reddish yellow, sandy and clayey silt (colloid content 25% and silt content 44.5%). There is a weakly developed prismatic macro-structure (c.4cm spacing) and a polyhedral sub-structure (c.1cm spacing). There are hematite and goethite films on structural units. Limestone clasts are highly altered. Traces of laminated bedding are present. Horizon 3 (argillic) is a reddish yellow, sandy and clayey silt (colloid content c.25-14% and silt content c.43-45%). There is a polyhedral structure with a spacing of c.0.5cm or less. Oxide films are rarer. Limestone clasts are still highly altered. Laminated bedding is clear. There are some carbonate concretions, well crystallised near the cave entrance but more diffuse deeper into the cave. There are white 'pseudo-mycelia' (rhizoliths?). Unspecified signs of hydromorphy are reported. Incidentally, Fedoroff (1969) reports a micromorphology lacking any pedogenic features but with a clear survival of fragile sedimentary features in this horizon, the only one analysed in thin section. Fedoroff suggests that the colour and nature of the plasma was probably inherited from reworking of older soils. Horizons 1-3 together are said to show increasing carbonate downwards (H1, 4.5%; H2, 8.5%; H3, 4-5%), increasing silica downwards (H1, 55.1%; H2, 58.0%; H3, 60%), increasing kaolinite downwards (H1, 33%; H2, 45%; H3, 41-50%), and more iron near the top of the sequence (H1, 7.1%; H2, 7.3%; H3, 6.6-6.3%). Desilicification of flint artefacts is noted in the argillic horizons. Horizon 4 (calcic) is composed of well crystallised spheres, cylinders or solid masses of carbonate, showing a tendency to develop at the boundaries between sedimentary units and within spreads of limestone fine gravel. Horizon 5 (calcic) has well crystallised, spherical or tubular (much larger than rhizoliths) concretions scattered throughout the substrate, as well as diffuse concretions around particles. In comparison with the argillic horizons, the calcic Horizons 4-5 have 2-12% carbonates, 50.0-70.1% silica, 36.5-45.0% kaolinite, 6.9-5.2% iron, 20-40% colloids and 25-42% silts. Heavy minerals are roughly similar throughout the profile and there are no alteration gradients. Kaolinisation of illite is suggested, even as low as Horizon 4. Manganese is present in small but very similar quantities at all levels. This whole phenomenon with its five surviving horizons is said to represent the lower part of a quite well developed red lessivé soil. It is overlain by alluvial sediments. No sign of an exterior palaeosol is reported.

Is this alteration system at Le Lazaret a palaeosol? First, it should be noted that the verbal and numerical data do not exactly match and that a pedologist (Fedoroff) has been unable to find significant traces of pedogenesis in a 'horizon' where such traces should be obvious. No gradients have been demonstrated that could not be accounted for by variations in the original components of the complex sequence of sediments. Carbonate concretions occur either at the contact between fine, laminated sediments and coarser, but still well stratified deposits, or in association with spreads of limestone gravel. The structure observed in the 'argillic horizons' could easily have been developed by variations in moisture content. All 'horizon' boundaries seem to be parallel, or even equivalent, to bedding features. Only the 'pseudo-mycelia', if they are in fact rhizoliths, suggest biological activity at some time. Much of this sequence, including the deposits immediately above the alteration system as defined, is composed of water-laid sediment. Finally, absolutely no explanation is given of how such a strong 'soil' might form well inside a cave. The present author sees no reason whatsoever to suspect a palaeosol, nor to infer the sort of interglacial mediterranean climate suggested.

The shelter and shallow cave site of Pech-de-l'Azé II (Dordogne) is said to contain several palaeosols (Laville 1975). The best developed surviving phenomenon would appear to be the Riss II/III interstadial palaeosol. The profile is described as it appears close to the modern dripline, but the phenomenon is said to persist for an unspecified distance (at least 6m) into the cave but to disappear before the limit of excavation at c.12m. Much of the sedimentary substrate also seems to wedge out into the cave. All organic and 'eluvial' horizons have been removed by subsequent erosion. Horizon B2 is a brownish red, sandy and silty clay with common limestone gravel, 35-55cm thick. There is a well developed 'medium' polyhedral structure. There are clay skins on structural elements, becoming less common towards the base of the horizon. During the excavation, 'pseudo-mycelia' were recognised. Limestone clasts are heavily altered and often present as ghosts. There is a distinct undulating lower boundary to the horizon. Horizon B3 is a reddish yellow, clay-sand-silt mix, becoming browner and sandier towards the base; the horizon is 10-15cm thick. At the top, the structure is still polyhedral or sub-angular, but it becomes more crumb-like towards the base. Hydromorphy is indicated by black or rust blotches. There is some altered limestone including ghosts. The lower boundary is diffuse, and may sometimes, but not always, correspond with a major sedimentary boundary. Horizon C is a strong brown, slightly clayey sand, 10-20cm thick. The structure is friable and crumb-like. Hydromorphy is indicated by black or rust blotches. There are very rare altered limestone clasts. The base of the horizon is variously extremely diffuse, marked by a strong induration of the sediment or abrupt and marked by thin black streaks; it is often equivalent to a sedimentary boundary. Below Horizon C, the sediments sometimes contain carbonate concretions or are locally cemented with carbonates. The percentage of non-carbonate colloids, c.25-30% at the top of the extant profile, drops to c.10-12% towards the base. Sand is always the dominant component of the matrix. Carbonates, c.12-23% at the top of the profile, generally decrease downwards, reaching a value of 6-12% in Horizon C; carbonates may rise to a maximum of c.20% in lower layers. ΔpH is relatively high at the top of the profile and decreases slightly, but irregularly, downwards. Colloid content, carbonate content and the development of all profile gradients increase laterally towards the exterior. Translocation of silt as opposed to colloids is said to have occurred deeper into the cave. Because of the presence of both structure and signs of hydromorphy in this phenomenon, the latter is thought to post-date pedogenesis. The substrates for this phenomenon are interpreted as basically autochthonous (Horizons B2 and B3) or as wash deposits (Horizon C and below). The sediments immediately above the palaeosol are screes. The palaeosol is not explicitly classified, but there is a clear implication that it is a lessivé type. The palaeosol outcrops at an erosion face at the entrance to the shelter and cave.

This phenomenon has also been described by Goldberg (1979a), using micromorphological techniques. The sequence is quite complex but certain general results may be mentioned. Horizons B2 and B3 have high plasma contents, typically rich in iron oxides.

There are many ferri-argillans, sometimes rather thick, on bedrock fragments, which are suggestive of a sedimentary rather than an illuvial origin. There are traces of sedimentary clay laminations mid-way down the profile. There is much secondary calcite in voids or as plasma segregations, concretions which clearly post-date the deposition of colloids. Some of these concretions may be rhizoliths. The substrate for all three horizons, as well as the sediment immediately below, include areas of "banded fabric" (cf. p.269 of the present text). The sediments immediately above the palaeosol could not be extensively sampled, but one discontinuous unit contains "clay clumps" (cf. p.349 of the present text). Because Horizons B2 and B3 lack traces of clay illuviation, Goldberg could only suggest that they are in fact the lowermost calcic horizons of a palaeosol. Features recognised in deposits below these horizons would therefore be referable to earlier events. Goldberg was unable to find any illuvial features in any of the palaeosols claimed by Laville from this site.

Is the Riss II/III phenomenon at Pech-de-l'Azé II a palaeosol? There are no convincing gradients. 'Horizons' are at least radically affected by pre-existing bedding. The colloids would appear to be sedimentary rather than illuvial. Again, structure in the highest 'horizon' is not diagnostic of pedogenesis. However, the presence of rhizoliths seems to be at least probable, although it is not demonstrated that this feature actually originated at the suggested point in the site chronostratigraphy. There are no certainly alluvial sediments in the upper part of the phenomenon or immediately above it, although doubts remain over the origin of the "banded fabric" and the minor clay laminations. The present author can see no evidence for a palaeosol, as described by Laville. Nor does it seem acceptable to suggest that a palaeosol was present, but that everything except the lowest calcic horizon has been destroyed by erosion; one might as well say that a palaeosol was present, but that all of it has been removed by erosion. One reasonable interpretation of the facts is that there is a possibility, and only a possibility, of the presence of an extremely weak palaeosol, with no significant horizon differentiation. If such a palaeosol were to be proven, the climatic implications would merely be that it was not too cold or dry for plants to grow. Another reasonable interpretation is that this is not even an alteration system but, rather, a whole series of stratified phenomena, some of which may involve rhizoliths.

Many other claims for palaeosols can be found in the cave literature, but none seem very much more plausible than the two examples given above. However, the reader's attention is drawn to a phenomenon described by Laville (1975, paricularly Plate 101, p.201) from the Abri Caminade (Dordogne). The profile is described near the back wall of the shelter but the nature of the rock overhang is not specified. Laville's nomenclature will not be used, the neutral term 'zone' replacing both sedimentary and pedological features. The sequence, capped by the modern vegetation and organic litter, is as follows. Zone 1 is a brownish yellow, sandy deposit with quite common limestone gravel; it is c.50cm thick. The structure is merely granular, reflecting the substrate. The common modern roots are often sheathed in diffuse carbonate concretions. Limestone clasts are very 'worn'. Zone 2 is a discontinuous and extremely irregular zone of strong carbonate infiltration, developed around a sedimentary boundary. Modern root layering (horizontal development) is apparent in the photograph. Zone 3 is a yellowish brown, sandy deposit with a relatively minor colloid content. There is a weakly developed, 'medium' sub-angular to angular structure. Zone 4 is similar to Zone 3, but has a slightly better developed structure. Zone 5 is again similar, but its structure is crumb-like or granular. The sedimentary unit, upon which Zones 3-5 and part of Zone 2 are developed, is c.150cm thick. Modern roots and extremely clear rhizoliths are common throughout Zones 3-5, with the latter best developed in Zone 4. In this exposure, only Zone 3 has a few altered limestone clasts but, further out from the rockface, clasts survive thoughout Zones 3-5. Laville interprets the substrate of Zone 1 as a colluvial deposit, made up of clasts and limestone residue. The phenomena of Zones 1 and 2 are said to equate with the modern soil. Laville believes that Zones 3-5 represent a truncated palaeosol. Presumably because there is an apparent c.3% rise in colloid content as one proceeds from the top of Zone 3 downwards towards Zone 4, Laville recognises Zone 3 as an eluvial ("A2") horizon, Zone 4 as an immature illuvial ("(B)") horizon, and the whole palaeosol as a "sol brun calcaire [a brown earth on a carbonate substrate]". Dating of the palaeosol would be problematical but, on stratigraphic grounds, it should not be older than the Late Würm. Whether one calls it a 'brown earth' or a 'rendzina', the present author believes that it is more likely that this whole profile represents a soil which has developed, and continues to develop, upon an irregularly aggrading surface. This example was given here because this is precisely the sort of weak pedological phenomenon, drastically interfered with by comparatively rapid and variable sedimentation, that one would expect in a limestone shelter. If we are to stand any chance of recognising real shelter palaeosols, we must ask ourselves what this sort of soil will look like tens of thousands of years from now.

Throughout the present chapter, it has been stressed that alteration, and alteration systems, should not be automatically equated with periods of warmer climate. Certainly, alteration is often more likely to be extreme under less severe climates, but this coarse generalisation, which may have been useful in the early days of cave sedimentology, is now proving to be more of a hindrance than a help. In fact, the study of alteration is probably the most primitive area of cave sediment research at present. The situation will not improve until this fact is generally admitted and a systematic programme of observation of both modern and fossil phenomena is begun, with special attention to moisture regimes, edaphic factors and dynamic relationships.

13. SPELEOTHEMS

The term 'speleothem' has been applied to a wide variety of cave formations since its introduction to the literature (Moore 1952). Theoretically, it may be applied to any crystalline precipitate formed within a cave but, in practice, it is usually restricted to those precipitates which are relatively hard, macroscopically visible in varied but characteristic shapes, and which are assumed to have formed at a major interface between air/water and rock/sediment. Many authors would also include such soft material as moonmilk, although whether fossil occurrences could be unequivocally recognised seems highly doubtful. Speleothems may be composed of any mineral that is available, but the great majority are carbonate precipitates, with calcite as the dominant mineral.

There is a bewilderingly diverse and somewhat 'fluid' vocabulary that has been developed to classify modern speleothems. Classes are usually defined according to shape, location and assumed mode of formation, as well as to mineralogy if this is out of the ordinary. Dripstone includes stalactites (hanging forms) and stalagmites (upward growing forms). Stalactites may be straws (cylindrical forms with a central feeder tube) or more conical. Stalagmites are generally dome shaped but more complex terraced forms may occur. Flowstone includes 'stalagmitic' sheets and floors, and wall formations, often referred to as curtain stalagmite or draperies. Horizontal or downward sloping projections from the cave walls may be shields (growth concentric upon the point of attachment), ledges (growth initiated on some pre-existing 'step' and developed by precipitation on upper and lateral surfaces) or shelves ('hanging' remnants of an old floor, often with younger additions on exposed surfaces). Stalactites may develop from the edges and undersides of any projections, and stalagmites may build up on top of them. Rimstone may occur as dams, ponding back water, or as sheets that extend out over the surface of the water, sometimes in the discontinuous forms known graphically as 'lily pads'. Particularly pure precipitates, often referred to as crystal linings, may form on the bottom of pools. Such linings may also include accumulations of calcite 'rafts', which are tiny flakes formed at the water surface which sink as soon as they grow large enough to break the surface tension. There are various 'erratic' speleothems, such as helictites (markedly curved or even spiral stalactites), that may be due to the effects of foreign substances forcing crystal growth to change direction. In some cases, excentric dripstones can be seen to be the result of cave winds. Various globular or botryoidal forms may result from growth on tiny irregularities that project above the normal surface film of water, or from splash phenomena. Loose, spherical concretions (cave pearls) form around a foreign nucleus and continue to grow regularly because of constant agitation in splash craters in the floor. If large quantities of fine sediment are present in drip water, mud stalagmites may form; these may develop a carbonate root which, when the fragile mud stalagmite is destroyed, is left in the sediment as a down-pointing conical form with a depression in the top, known as a conulite. Tufa, or calc-tufa, is a spongy textured deposit formed by rapid deposition, usually aided by biological activity (often algal), in and around springs. Eucladioliths are tufaceous deposits formed around plants (algae, mosses, etc.) growing on damp limestone surfaces. Two other terms are commonly applied to some speleothems: sinter and travertine. Some authors use the term 'sinter' to refer to irregular tufaceous masses on the floor of caves, produced by a combination of in situ evaporative precipitation and sedimentation of insoluble residues and degraded roof/wall concretions. However, many German authors use the term 'sinter' more or less as a synonym for 'speleothem'. Similarly, 'travertine' and 'speleothem' are often interchangeable, although there is a tendency to use the former more specifically for bulkier, well crystallised phenomena. Finally, a whole class of different styles occurs if minerals other than calcite (e.g. aragonite) are dominant. The literature on speleothem forms is enormous, and constantly growing, since cavers take a not unreasonable delight in describing new discoveries (for an introduction cf. Trombe 1952; W.B. White 1976; Bögli 1980).

In ancient deposits, we may envisage four general categories of speleothem, although it may sometimes be difficult to allocate individual occurrences. The first, and most important, category includes all those forms which grew upon a sediment surface and which are still in growth position. This category provides good stratigraphic information. The second category includes relatively fragile forms (such as small stalactites) which have fallen and been incorporated as clasts in the sediment. It may sometimes be assumed that these disturbed forms fell either during or shortly after the phase of formation, although the dangers inherent in any such assumption are obvious and some corroborative evidence is called for. The third category comprises any loose speleothem fragment (e.g. slabs of stalagmitic floor) found in a deposit that is younger, to an unknown degree, than the speleothem itself. The fourth category includes in situ wall or roof speleothems which, because of a lack of a normal superpositional relationship, can only be classed as older than contiguous sediments. Note that, should a large enough cavity develop within an existing sediment body, various forms of speleothem, including ordinary stalagmites and stalactites, may occur (cf. p.446). The detailed vocabulary developed for modern speleothem forms may be used for ancient occurrences but, in order to avoid misunderstandings, it is probably best to include an objective description, together with an explicit statement concerning any implications thought to be associated with such forms.

In order to make use of the recognition of ancient phenomena, we need to know how carbonate speleothems form. The general principles have already been summarised in Chapter 12: carbonates are precipitated primarily due to CO_2 degassing and evaporation of cave waters. There is, of course, a continuum between alteration and sediment concretions, on the one hand, and speleothems, on the other. The distinction is rather arbitrary but it is very useful in practice. Our first category of ancient speleothems, the in situ floor formations, may result from more or less the same processes as sediment concretions, but they represent a different balance of these processes. First, precipitation must be sufficiently strong (in terms of either speed or persistence) to block off the porosity of the underlying sediment. Once this has happened, a pure speleothem will begin to build up, as long as no aggressiveness develops and there is no clastic sedimentation. In reality, this never occurs; speleothems all contain discontinuities resulting from impurities and re-solution. If this interference increases, the speleothem will gradually lose its characteristic organisation. This requirement of a relatively stable depositional environment is the main reason why coherent speleothems are not always found at levels dating from periods when conditions for formation ought otherwise to have been favourable.

The shape of speleothems is primarily a function of the source of carbonate solutions, a fact which accounts for the utility of classes such as dripstone, flowstone or pool speleothems. At this general level, the different forms are related more to the physical pattern of hydrology, and to cave and sediment morphology, than to chemical controls. For instance, a smoothly arched roof will favour flowstone formation, whilst an irregular roof, with rock pendants and fissures, will favour dripstone. The horizontal distribution of ancient stalagmites and floor forms can be used to gain a rough idea of how the cave roof has changed, or not changed, since their deposition. Given a particular mode of water input, theoretical considerations of the chemistry may be applied to explain some of the details of speleothem morphology. The easiest way to go about this is to consider gross superficial morphology. Franke (1965) points out that the vertical rate of stalagmite growth is mainly a function of the CO_2 disequilibrium between solutions and cave atmosphere. The width of stalagmites, however, is a function of the amount of drip water and the quantity of solutes supplied. This sort of generalisation may be applied to the recognition of gross environmental changes evidenced by ancient speleothems (cf. Franke 1971). More quantitative estimates have enabled Curl (1972, 1973) to suggest more precise conditions for deposition. One of his findings is that there is a minimum possible diameter for both stalagmites and stalactites. Dreybrodt (1980) provides a detailed discussion of calcite precipitation, together with a sophisticated model which appears to be sufficiently accurate to account for many features seen in a control set of real speleothems.

The crystallography and internal structure of speleothems has also received much attention. At a simple level, stalactites can be differentiated from stalagmites, because in longitudinal section the former show growth bands that are roughly parallel to the long axis whilst the latter show bands that are roughly orthogonal to the long axis, although they are curved, concave-down. In a very stable environment with slow growth, crystals will be large; some stalactites may even be monocrystalline and crystal linings of pools are often composed of large scalenohedra. In a fluctuating environment, crystals will be of varying sizes. If very fast deposition occurs, such as that caused by rapid evaporation, speleothems may be cryptocrystalline or tufaceous. Marked growth bands are caused by major fluctuations in the controlling parameters, such as the quality of the water, or the nature and quantity of impurities in either the water or air. Etching of existing calcite may often occur if the cave is flooded by more aggressive water (cf. Bourguignon & Melon 1963). Some banding in modern speleothems has even been recognised as an annual phenomenon (cf. Dreybrodt 1980), although this is a demonstrably rare occurrence. Naturally, the structure of ancient speleothems also reflects any modification due to alteration or diagenesis. Originally banded speleothems, the structure of which is discontinuous in proportion to the amount of impurities, seem to produce coarsely granular textures upon weathering. The better formed crystals of more massive speleothems may still be apparent as bundles of loose 'rods' or 'needles', even when the speleothem itself has lost overall coherence. Care must be taken to differentiate between primary and secondary textures. For instance, the difference between the older 'crystalline' stalagmite and the younder 'granular' stalagmite at Kent's Cavern (Devon) is primarily a function of the strong alteration of the latter (contra Rosenfeld 1964a). Even apparently sound speleothems may have suffered considerable alteration. Basically, crystals can only grow more or less at right angles to the contemporary surface, because of competion from neighbouring crystals; if, in thin section, the speleothem shows significant crystal growth parallel to the original stratification, or an irregular mosaic of crystals, the original structure has been modified. More detailed studies may show such features as crystal overgrowths, solutional vugs and secondary cements.

Impurities in speleothems may prove useful in several contexts. Speleothems often contain microfossils and other small objects, such as pollen (cf. Miskovsky 1974:90; Bastin et al. 1977), impressions of soft parts (e.g. the leaf and caddis fly wing impressions at Elderbush Cave, Derbyshire; Bramwell & Shotton 1982), rodent remains (cf. Bramwell & Shotton 1982) or artefacts (e.g. the mesolithic flints at Le Seuil des Chèvres, Savoie; Chaline 1972). There would seem to be a potential for the fossilisation of creatures which actually live on the surface of speleothems, such as some ostracods. Eucladioliths often contain structured organics; the suspected eucladioliths from Sun Hole (p.919) are presently being examined with a view to identifying the plants responsible for their formation. Inorganic impurities may be particulate (clays and even coarser particles) or may represent various minerals precipitated with the calcite (cf. Gascoyne 1977a). The inclusion of iron bearing substances may allow the study of palaeomagnetism (cf. Latham 1977).

One of the most promising areas of research with respect to climatic information is the study of stable isotopes in speleothems. The basic argument is that, when an element is available as two or more isotopes, fractionation, dependent (among other things) upon temperature, will occur during precipitation; assuming that the isotopic ratios in the solution are known, the ratios in speleothems should provide a 'geothermometer'. The ratio of ^{13}C to ^{12}C has been examined in this respect (cf. Harmon 1971; Hendy 1971), although there seem to be major problems in practice. More plausible results have been achieved with the ratio between ^{18}O and ^{16}O, with oxygen supplied either from the calcite itself (cf. Duplessy et al. 1970) or from fluid inclusions assumed to be contemporary with surrounding calcite (cf. Schwarcz et al. 1976). One of the main problems with all isotope studies is that predictable fractionation is inhibited if any precipitation is occurring due to evaporation, a process which simply 'dumps' all available solutes. Thus, these techniques are only useful in (formerly) deep cave areas that were very humid and stable at the time of speleothem formation. Furthermore, there may be some doubt as to the original isotopic ratios in the solution. Most temporal changes in these ratios will themselves be due to temperature fractionation occurring (sometimes on a planetary level) before the elements reach the cave, so that the cave fractionation will usually be re-inforced. Thus, changes in isotopic ratios in speleothems cannot always be calibrated in absolute terms, and only generally cooling or warming trends can then be recognised.

No great number of oxygen isotope studies have yet been carried out on speleothems, but those which are available show that growth tended to occur during comparatively warm periods. This brings us to a consideration of the environmental and climatic implications of ancient speleothems. It has long been noted that speleothems are often found at stratigraphic levels that are equivalent to at least milder phases. Recently, global climatic curves have been established, using data from deep marine sediments; much faith is placed in these curves and the present author only hopes that the assumptions of continuous sedimentation are indeed justified. If a speleothem can be dated directly, it should be an easy matter to check whether its formation occurred during a milder phase as evidenced in the marine record. Uranium series dates and other methods (cf. Chapter 18) provide such an opportunity and, again, the tendency for there to be a climatic control may be seen (cf. Gascoyne 1977b). There is also a predictable latitudinal effect, so that speleothems dating from the height of the last glacial, for instance, are known from the Bahamas (cf. Spalding & Mathews 1972) but are absent, or at least very rare, at higher latitudes. Higher temperature is not of course the only requirement; in present arid environments, ancient speleothems must indicate more humid conditions (cf. Cooke & Verhagen 1977).

Rather than compare individual speleothem dates with climatic curves, it is possible to examine a number of dates in order to observe whether there are interesting groupings that correspond to the known milder phases. Atkinson et al. (1978) seem to be the first to attempt such an approach, with a sample of 21 dates on British speleothems. Gascoyne (1981) collected "over 80" dates from caves in north-west England; oxygen isotope analysis was also carried out on a few suitable samples. Hennig et al. (1983) have produced the most impressive compendium so far, with 805 dates from all over the world, 664 on speleothems (sensu stricto) and 141 on spring calc-tufas. These authors argue that such a large sample is necessary to overcome problems of apparent, but in fact random, groupings that may occur with smaller samples. They then make a most remarkable statement:

> We did not exclude any of the collected U-series ages, even if they are denoted as being questionable or less reliable, except for the 234U/238U data from speleothems which are regarded as completely unreliable .... (1983:3)

Hennig and his colleagues have merely searched the literature to find as many published dates as possible, to add to those produced during their own research. If a date was originally quoted without an error bracket, they simply invent one. Similarly, if a date is open-ended on the older side, they allocate a finite limit. These authors wish to construct histograms of frequency against membership of 5000 year age classes, but they recognise that they must take some notice of error brackets. Their solution is to weight individual dates with the quoted or invented errors in such a way that the date still appears, and only appears, in the age class of the mean date, but with a lower contribution to 'frequency' in proportion to the width of the error bracket. Naturally, this defeats the object of the concept of 'variance'. The end result of this most questionable procedure is shockingly predictable: at the younger end of the scale, where dates are intrinsically more reliable, there is reasonable differentiation between frequency peaks and troughs whilst, beyond about 120ka, there is nothing but noise. Obviously, definition will automatically be lost as one proceeds further back in time but, given a much more critical sample selection procedure and careful adherence to standard statistical practice, a more trustworthy, and perhaps a more useful, result could be achieved. In addition to the division between speleothems and calc-tufas, Hennig and his colleagues separate their sample into two groups, depending upon whether or not the sites involved lie within the general limits of pleistocene glaciation. Presumably, they expected to see better grouping of dates into 'milder' periods in the generally higher latitude sites, an expectation which was frustrated primarily by their method. In the future, it would be interesting to see whether multivariate techniques, taking into account estimates of such factors as depth into the cave system, thickness of rock overburden, growth rates and local surface conditions, would produce useful results. Uranium series dates are not the only data that may be used in this way. Srdoč et al. (1983) have based similar palaeoclimatic arguments on 146 radiocarbon dates from randomly selected tufas in the Croatian Karst; since these dates cover only the last thirty millennia or so, it is to be hoped that statistical errors will be low enough not to obscure meaningful general trends.

Despite the detailed problems, most authorities are agreed upon the general environmental implications of major speleothems, at least in the British context:

> In general, stalagmite deposits are indicators of warm events in the paleoclimate record. However, present-day growth of speleothems in caves in a glacial environment [specifically, northern Canada] serves as a warning that paleotemperature data may not directly reflect the true surface conditions. (Gascoyne 1977:209)
>
> ... there are good reasons for supposing that speleothem deposition ceased, or was at least drastically reduced, during past periods of periglacial and glaciated conditions. (Atkinson et al. 1978:26)

This proposition is reasonable, not only because of evidence from dating, stable isotopes and included impurities and fossils, but also because of the clear tendency for biological activity in overlying surface soils to be (slightly) greater, and (markedly) less seasonal, under milder conditions. Speleothems are less equivocal than sediment concretions and alteration phenomena because they are stratigraphically distinct and one does not have to worry too much about contributions of calcite from limestone clasts within the depositional system or about continued precipitation long after burial. Nevertheless, it is also clear that the presence of speleothems does not necessarily indicate a major climatic amelioration. It would seem very dangerous to equate stalagmitic floors with interstadials or interglacials on purely a priori grounds, as has been done by Laville (1975) at some Périgord caves such as La Grotte de l'Eglise and La Grotte de Font de Gaume. Even within a major warm phase, speleothem formation is far from uniform. Many British caves have thick stalagmitic floors that date from periods often starting from the very beginning of the Holocene (as roughly estimated from associated microfauna) and continuing until perhaps c.5000 B.P. (as roughly estimated from superimposed archaeological material), although later activity may often be minor. Indeed, there would seem to be a trend for speleothems to occur towards the beginning (and perhaps also the end) of warmer phases. We will learn more about the implications of speleothems by observing the details of individual occurrences than by striving to bolster up a generalisation which has already been proven to be acceptable but limited.

One of the main areas of research which has been somewhat neglected is the study of spatial variation in both ancient and recent speleothems. Four main zones may be defined, which will generally succeed one another as one proceeds deeper into the cave system. The outermost zone will lack major speleothems, because the CO_2 disequilibrium between percolation water and the cave atmosphere will be negligible. The next zone, in which the CO_2 disequilibrium may become significant, will have mainly roof formations to start with, and speleothems of any type will tend to be tufaceous because evaporation and contamination by dust will be important this close to the cave mouth. The third zone will contain better crystallised forms, as interference from evaporation and dustfall wanes. The deepest zone will contain speleothems in isotopic equilibrium. The exact distribution and scale of these zones in particular caves ought to be primarily a function of cave meteorology, although the present author has never seen this carefully demonstrated. Further zonation of speleothems might be possible based upon signs of the activities of various plants and other organisms. Many speleothems show banding which is clearly primary stratification, but how persistent is such stratification? This will depend upon the nature of the source solutions, which will be a function of the route taken by the water and a number of more or less localised environmental parameters. Under what circumstances could microstratigraphic sequences through speleothems in different parts of a cave, or in neighbouring caves, be correlated? Except in the context of stable isotope analysis and, all too rarely, of radiometric dating, speleothems are normally treated by sedimentologists as if they had no time or space dimension, as if they were just useful reference points in vertical sequences, rather than complex sedimentary phenomena of interest in their own right.

14. BIOLOGICAL MATERIAL

14.1. Is Biological Material from Caves Worth Collecting?

That caves contain a variety of biological material is certain. It is the implications of that biological material which are often the subject of contention.

> Cave deposits are often stratigraphically disturbed, and fossils are often difficult to date accurately. In addition the fossils may not be representative of the contemporary fauna, due to the selectivity of predators for different prey, and the occupation of the cave by a few species only, which may be mutually exclusive.
> In addition to the cave dwellers, cave earths may also contain remains of animals which became accidentally trapped, for example, by falling down a hidden fissure. (Birks & Birks 1980:141)

> Most of the vertebrate fossils found in caves result from the operation of a natural trap mechanism, were brought in by various carnivores, or are the remains of carnivores, which used the site as a den, and their prey. In some cases material is washed in by streams from the ground surface or from older cave deposits.
> [...]
> As in caves, the stratigraphy of fissure deposits can be chaotic.
> (Stuart 1982:67-8)

The present author does not dispute the general points made in the above quotations, although the suggestion that cave deposits are 'often disturbed' is something of an exaggeration. However, the author fails to see how this makes cave sites significantly different from many other types of site (such as open-air fluvial sequences), where very similar criticisms may be raised. The unacceptable point is not the criticisms themselves, but the fact that such criticisms are often used to justify an <u>a priori</u> dismissal of cave fossils as worthy of little if any consideration. Such an attitude sets up a conflict which has further unacceptable repercussions. Biological specialists tend to discuss cave sites only in terms of vertebrate fossils, probably because caves contain embarassingly large quantities of bones that cannot be totally ignored. Other types of cave fossils are rarely even mentioned in synthetic English language texts. Those researchers who are actively involved in cave excavations, finding themselves with large quantities of fossil material on their hands, are often rebuffed by a generally unsympathetic community of professional palaeobiologists. The result will automatically be that untrained individuals will try to handle the fossil material themselves. Campbell (1977), an archaeologist, not only identified microfauna and megafauna from a number of his British cave sites but he also dealt with pollen. The present author, during his research at Creswell Crags (Collcutt 1975), was also sufficiently frustrated to try pollen analysis at Key Hole (latterly known as the Arch) and to pay a then inexperienced commercial firm (Archaeozoological Services, based at Edinburgh Zoo) to identify faunal material from Pin Hole. Subsequent basic training in both palynology and mammalian palaeontology taught the author that, although a good general knowledge of palaeobiology facilitates communication and the initial framing of hypotheses, the work of identification of fossils and reconstruction of ecological systems cannot be safely undertaken without a substantial degree of expertise. Eventually, the author also learnt that some highly competent palaeobiologists in this country could, in fact, be interested in caves, as long as it could be demonstrated that they would receive adequate support in matters of context.

> Cave deposits of Devensian age are common in many parts of the British Isles but have seldom been dated with any accuracy. Radiocarbon dates on bone collagen from well-stratified samples are the most promising line

> of approach, but few sites have been carefully excavated and systematically recorded. (Stuart 1982:12)

This is a serious indictment but it must be admitted that it is too near the truth for comfort. Note that the emphasis on the supposed intrinsic inadequacy of caves, expressed by Birks and Birks (supra), has here changed to the more reasonable emphasis upon the inadequacy of cave research to date. It is to be hoped that, within a decade, no palaeobiologist will feel compelled to make even this criticism.

Since Stuart wrote his book on British pleistocene vertebrates, from which the above quotes are taken (Stuart 1982), he has visited several on-going cave excavations and his attitude has changed so markedly that he has actually written an enthusiastic paper on bone caves in the British Isles.

> The main reasons for this relative neglect of cave deposits on this side of the Channel include the difficulties of correlating between cave and open sites, notably the serious problems in the interpretation of pollen from cave deposits, and the poor, often atrocious, standards of excavation and recording of British cave sites which were largely prevalent in the past.
>
> Bone caves are, however, of enormous interest to the student of Pleistocene vertebrates because they commonly yield far richer fossil assemblages than do open sites. In particular, remains of carnivores, normally rare at open sites, tend to be well-represented in caves. Moreover, when assemblages can be correlated, for example by radiocarbon or by distinctive faunal elements such as hippopotamus, much can be learnt from comparing faunas of the same age represented both in cave and open sites, with very different taphonomic histories. The preservation of skeletal material in the deposits of limestone caves is usually excellent because of the calcium carbonate content of the enclosing sediments which protects bones from solution by percolating acid waters.
>
> Of particular interest and importance is the development in recent years of radiometric dating techniques, especially uranium-series dating, applicable at present mainly to cave deposits rather than open sites. These techniques promise to give us at least an outline chronology, at present lacking, for the Middle and Upper Pleistocene beyond the range of radiocarbon dating. A situation might develop whereby it is possible to obtain absolute dates from cave deposits, while most open sites can be dated only indirectly by correlation of faunas.
>
> [...] Bone caves therefore, offer exciting potential for future research, by meticulous excavation and recording of finds together with various methods of radiometric dating and other advanced techniques.
>
> In this review I have included an outline of the stratigraphy and chronology of the British Pleistocene as background, and a short account of the taphonomy of bone caves. [...] Emphasis is given to the best excavated, most informative sites. (1983:9-10)

This forthright statement from a member of the Cambridge School of quaternary studies is extremely welcome. Indeed, the present author feels that this statement will have sufficiently important repercussions to offset what he himself regards as an unfortunate degree of inaccuracy and oversimplification in some subsequent passages of this (1983) review paper (see, for example, p.935 of the present text). It is to be hoped that, in the future, cave specialists will make the greatest effort to provide much needed professional recruits, such as Stuart, with extensive opportunities to gain experience in this field and, also, to apprise cave researchers of the requirements of other specialities.

14.2. The Recovery of Biological Material from Caves

The sedimentologist is responsible for setting up the lithostratigraphic framework in a cave and for analysing samples of the sediments. It has been noted in Chapter 5 how the sedimentologist must respond to the stratigraphic needs of other specialists, including palaeobiologists. Frequently, the sedimentologist will be the first person to be in a position to recognise the presence of biological material, especially of remains that are generally too small to be easily noticed during normal excavation. It is therefore important that the sedimentologist, just like any other sedimentary geologist, should be capable of recognising as wide a range of biological material as possible. There are basically five classes of biological material.

Organic matter (*sensu stricto*) is rapidly destroyed in the presence of oxygen, more or less independently of the pH of pore water. However, if an anaerobic context is also either moderately acid or alkaline, the activities of even anaerobic micro-organisms will be reduced, and the organic matter may survive for very long periods. Many limestones still contain fossil hydrocarbons of the same age as the rock. Cave sediments are generally rather well aerated but some organic matter does persist, especially in finer, impermeable deposits and within carbonate concretions. The bulk of the organic matter in caves will be in a degraded, rather amorphous form, but easily recognisable structures sometimes occur, such as fish scales (cf. Armstrong 1929; Collcutt 1975). Pollen and spores, which are constructed of a remarkably tough but not indestructible polysaccharide, are nearly always present in extremely low concentrations, but more significant quantities may be associated with carbonates, amorphous organics and dense, impermeable strata.

The second class of biological material comprises carbonised vegetable matter. Charcoal is almost chemically inert in most earth surface environments but, because of its high porosity and low strength, it is extremely vulnerable to physical comminution. In caves, especially where pore water has been mobile for long periods, various physical stresses (wetting and drying, minor calcite crystallisation, etc) often leave charcoal in a very fragile state that will not survive 'loose' sampling. It should be noted that, in well aerated deposits, roots may sometimes become carbonised by slow combustion (rapid oxidation) rather than by fire.

The third class of biological material comprises true fossils (biological material replaced by geological precipitates) and trace fossils (the external features of biological material preserved by fine sediments or precipitates). It was noted in Chapter 13 that speleothems may preserve some soft organic structures and, in Chapter 12, that roots may survive as rhizoliths. Another example would be the calcified myriapods from the Grotte d'Isturitz (Basses-Pyrénées; Chaline 1972:250). This class also contains the ancient fossils from the surrounding rocks, such as crinoid ossicles, corals or brachiopods, which may be present as discrete particles, as well as in rock clasts, and which may be calcitic, dolomitised or silicified.

The fourth class of biological material comprises excreta. Bird and bat guano is quite common in modern temperate caves, although not in the huge quantities found in tropical caves. Such thin spreads are unlikely to survive for very long because the material is acid and chemically unstable; ancient occurrences must usually be inferred from chemical data and the more durable remains of the animals thought to have been responsible. The excreta of hyaenas (and perhaps of some other carnivores) have a built-in potential for preservation because of these animals' habit of consuming even the bones of their prey. Although quite fragile, hyaena coprolites are very common in alkaline cave sediments and are important, not only in their own right, but also because they may be quite rich in well preserved pollen (cf. Renault-Miskovsky 1972:95). Coprolite fragments down to sand size may even form a significant proportion of a sediment. Phytoliths, which could be classed either as opaline plant 'excreta' or as 'auto-fossils', should survive at least in less alkaline deposits, although the present author has not come across any reports from caves.

The last grouping of biological material, and by far the bulkiest, comprises phosphate and carbonate hard parts that will be chemically stable in caves as long as they are not subjected to aggressive solutions. Phosphates of the apatite group are less vulnerable to solution than carbonates. These hard parts also contain organic matter, mainly in the form of proteins (e.g. collagen, conchyolin, etc.), which is gradually destroyed under oxidising conditions. Geological precipitates may impregnate or combine with the inorganic components of the hard parts. The bones (and ivory and antler where applicable) of most Classes, indeed of most Orders, of vertebrates may be found in caves, as well as the shells of both terrestrial and aquatic (including marine) mollusca. Examples of more unusual occurrences include fish otoliths (cf. Desse & Granier 1976) and bird eggshells (Armstrong 1929). Marine sediments, whether in situ in a cave or derived, are something of a special case and, apart from mollusca and fish, may contain such diverse materials as echinoids, sponges, foraminifera, ostracods or larger crustaceans (cf. La Grotte du Vallonnet, Alpes-Maritimes; de Lumley-Woodyear 1969), each with a different potential for preservation in different chemical environments.

From the sedimentologist's point of view, it is not important that the exact, or even the approximate, taxonomic status of a fossil be recognised. It is the ability to distinguish biological from geological structures that enables the sedimentologist to be of help to the palaeobiologist, as well as to avoid erroneous geological conclusions. Normally, the sedimentologist will only be interested in recovering organised biological material in the approximate size range 5-0.5mm. No matter how unpromising they may seem, the present author systematically scans this size fraction of all sediment samples under the microscope in order to recover any fossils before any potentially destructive treatments. It should be noted that the sedimentologist's activities should only represent a pilot scheme with respect to fossil material; once the palaeobiologist has been alerted, he will normally set up his own sampling and extraction programme. The recent excavations at Pontnewydd Cave (cf. p.1118) provide an example of the sort of interaction which is helpful. Large mammal remains were already known to exist from earlier excavations and it proved possible to extract further specimens with no great difficulty, even from the common cemented deposits in this cave. This material was passed on to A.P. Currant, at the British Museum (Natural History), for identification and interpretation. The present author undertook to look for smaller remains in his sediment samples. This entailed a special approach for the cemented deposits which would not have been applied if only geological information or only biological information had been required (curing of samples, involving many very gentle oscillating changes in moisture content so as to break down the diffuse cement without damaging the biological material and to leave the remainder as a valid geological sample). Microfauna, consisting mostly of rodent and amphibian remains, and small bone fragments were recovered from a number of strata and a rough idea of the 'yield' of each sediment unit was gained, enabling Currant to design his own sampling plan. At one point the present author was able to extract scales, to which he confidently assigned a biological origin; the fact that he believed these to be fish scales, when actually they were slow-worm scales, is of no consequence. Biological material below c.0.5mm will require even more complex extraction techniques which will have an increasingly important effect upon the nature of the material eventually recovered. The sedimentologist is not usually involved in the extraction of such material, unless his help is specifically requested by a specialist.

One other area in which the sedimentologist, and anyone else involved in excavation and sampling, should be competent is general vertebrate anatomy, so that bones in 'articulated' association can be recognised before the association is destroyed. Anatomical groups are not only useful for taphonomic purposes (infra), but they often make exact taxonomic determination more easy. Two 'articulated' wing bones of a bird, recovered whilst the present author was sampling sediments at Bacon Hole (Gower), provided the most certain identification of Cory's shearwater (Calonectris diomedea) from this site. Furthermore, it is extremely difficult to decide upon the exact anatomical position of some loose bones, such as those of bear paws, a fact which hampers biometrical studies. It is also useful to recognise other biological structures, such as undisturbed owl pellets.

Some biological or biogenic phenomena will be of direct interest to the sedimentologist. Quantification of finely divided or amorphous phosphates and humates may be necessary. Similarly, the presence of any type of biological material may be an integral part of a lithologic description. It may even be left to the sedimentologist to quantify the occurrence of bone or shell debris. Rhizoliths and burrows will only be of specific interest to the palaeobiologist on the rare occasions where there is a chance to identify the organism responsible.

14.3. Taphonomy and Palaeoecology

The sedimentologist makes use of biological data to help explain sedimentary processes. Conversely, the palaeobiologist makes use of geological data to help explain biological assemblages. The two disciplines meet in the area known as taphonomy.

The explicit definition of 'taphonomy' labels it as a palaeontological science (Efremov 1940; cf. also the historical summary in Olson 1980). Taphonomy, as it is practised today, concerns the recognition of the processes which intervene between living organisms or communities of the past and our interpretations of the fossil remains of these organisms. Numerous concepts in sedimentology (cf. Fedele 1976) and archaeology (cf. Chapter 20) converge upon these ideas and the basic tenets of taphonomy are now recognised as generally valid for many sorts of data retrieval concerning ancient structures and events. The vocabulary is still largely couched in biological terms, but it is not difficult to see how it relates to other fields or how sedimentology is an integral part of biological taphonomy. Gilbert and Singer (1982) have adopted the vocabulary, used by 'mainstream' palaeontologists (Clark et al. 1967), to discuss faunal remains in archaeological sites; this vocabulary will serve as a framework for the present discussion.

Biotic ('pertaining to life') factors are those concerned with the initial composition of the life assemblage (biocoenosis). The ecology of biological communities and of individual taxa are important here, as well as the initial physical composition of biological materials (e.g. the 'survivability' of different parts of a bone, the fact that pine produces greater quantities of more robust pollen than poplar, etc.). Thanatic ('pertaining to death') factors are those concerned with the composition of the death assemblage (thanatocoenosis), which may be radically different from the biocoenosis in such respects as the age of individuals or because of the effects of predator pressure. Periodic events, such as droughts, natural fires or rodent population explosions, may cause different thanatic effects. Perthotaxic ('pertaining to arrangements produced by destructive effects') factors are those concerned with decay, dismemberment and dispersal of organisms and parts of organisms. Taphic ('pertaining to burial') factors are those concerned both with the effects of sedimentation and burial, and with *in situ* post-burial changes. This grouping is rather coarse and one could wish for subdivisions, such as 'syntaphic' (depositional factors), 'peritaphic' (factors important during and shortly after burial) and 'apotaphic' (factors important long after burial). Anataxic ('pertaining to new arrangements') factors are those concerned with erosion, mixing and reburial. Again, at least theoretically, one could envisage subdivisions involving a whole second cycle of 'neoperthotaxic' and 'neotaphic' factors. Sullegic ('pertaining to collection') factors are those concerned with sampling and data retrieval. By definition, any sullegic changes in the fossil assemblage represent errors on the part of the researchers.

In the context of biological material in caves, the sedimentologist is concerned mainly with helping to recognise taphic, anataxic and potential sullegic factors, although sedimentary data on localised dispersal systems can also prove useful at the perthotaxic level. There has been a recent boom in interest in bone taphonomy, particularly in archaeological contexts (cf. various papers in Behrensmeyer & Hill 1980; Binford 1981; Gilbert & Singer 1982; papers in Clutton-Brock & Grigson 1983), and there have even been a few texts primarily concerned with caves (cf. Brain 1980, 1981; Klein 1980; Gamble 1983; Payne 1983). In fact, much implicitly taphonomic data has long been coming to light. The work of Brain, for instance, is the result of many years' experience in this field (cf. Brain 1958). It is basically because of such patient work that taphonomy has now achieved such popularity. Nevertheless, most reports are still dominantly concerned with biotic, thanatic and perthotaxic factors, although consideration of at least 'syntaphic' factors is growing. There is therefore considerable scope for the sedimentologist to become involved, especially since the 'higher' levels of taphonomy will be invalidated by underestimation of the 'lower' levels, a weakness which seems to have crept into some recent, archaeologically oriented publications.

Useful taphonomic information can often be remarkably simple to acquire. Sometimes, it becomes apparent through careful comparison of the views of separate specialists, but by far the most productive approach is achieved when specialists are sufficiently confident in each other's abilities that they can ignore inter-disciplinary 'boundaries' and pool their experience to solve specific problems. A few examples of situations, in which the present author was involved, will show how straightforward such an approach may be, given the right frame of mind.

At Bacon Hole (Gower), the Upper Sands comprise quite well sorted, fine to medium quartzitic sands (cf. p.1083). The study of this material provides an example of the identification of various taphic factors. Bone is almost totally absent from these sands and, because of their generally non-carbonate nature, it was not known whether this lack of bone was due to an absence at the depositional stage or to post-burial destruction. However, the deposit does contain some very small (<1mm) discrete HCl-soluble particles, dispersed uniformly throughout the sands. It was clear from observation under a hand lens that these were structured biogenic particles but, because of their size, it was not possible to be more precise. Currant suggested that they might be mollusk shell fragments and the author suggested disaggregated bone as a futher possibility. Differentiation on site using a test for phosphates proved inconclusive, since the whole sequence of deposits gave a strongly positive result. Samples were taken, and Currant and the author independently identified the particles as mollusk shell using microscope techniques. It was therefore clear that significant bone could never have been present, since the more soluble carbonate of the tiny shell fragments would not have survived the alteration which would have been necessary to remove bone. In order to check this conclusion, the author looked for evidence of the sedimentary origin of the shell fragments. Because of particle size distribution and bedding features, the Upper Sands are interpreted as an aeolian deposit. Comparison of the quartzitic sands and the shell fragments, using Stoke's Law with parameters for air, showed that, although the shell fragments were generally larger, they were dynamically compatible with the mineral sands because of their lower density (and also their platy shape). In the coastal context of this site, the presence of shell fragments (and foraminifera; cf. Stringer 1977) would be expected in an aeolian deposit. The shell fragments were clearly deposited with the sands and would have been available from the start as very sensitive 'tracers' for any solutional effects. Solutional effects are indeed obvious in the deposits below the Upper Sands, deposits which contain limestone clasts, shell fragments and bone. The degree of solution in these strata is logically related to the scale of solubility, with small limestone clasts losing their structure first and then small shell fragments; bone is present at all levels, since truly aggressive conditions never developed.

A number of strata at Pontnewydd Cave (Clwyd) have been interpreted by the author as debris flows (cf. p.1181). Currant has noted that the condition of the mammal bones, with fragmentation, abrasion, compaction and even bending, is consistent with this mode of emplacement (cf. Green et al. 1981). Because of various lithologic gradients up through the sequence, the present author has suggested that each new flow reworked and incorporated part of earlier flows. Anataxic factors will therefore be of great importance in these deposits. At one point, the author was engaged in documenting differences between the Lower Breccia bed (LBb) and the Upper Breccia bed (UBb), deposits which are often contiguous and sometimes rather difficult to distinguish on site. In one exposure (Area D), the proportions of phosphates were found to be variable but usually high (up to 5% of the sediment <4mm; colorimetric determination using the molybdenum-blue reaction), with no significant differentiation between LBb and UBb. LBb had approximately six times more bone than UBb in the 4-16mm fraction (microscopic extraction), and over twice as much bone as UBb in the 1.0-0.063mm fraction (differential weight loss between treatments with 8% cold acetic acid and 30% cold hydrochloric acid; there is no shell in these deposits and soluble organic matter is not rich enough to affect the results significantly). These results might have suggested that the bone of UBb was totally derived from LBb. Furthermore, LBb had three times more sediment than UBb in the 1-4mm fraction, but the bone content of this fraction (microscopic extraction) amounted to c.4% in both units. However, the nature of the bone in the 1-4mm fraction showed that UBb most certainly did not derive all its fauna from below. In LBb, under 5% of the material consisted of recognisable microfaunal remains, the rest being fragments of bone from larger animals. The microfaunal remains were mostly tougher parts of the skeleton (e.g. teeth, and bones of the extremities). Both the microfauna and the bone fragments from larger animals were a light buff colour with dark spots. In UBb, over 50% of the material consisted of microfaunal remains, this time including more fragile parts (e.g. complete long bones, bones of the shoulder and hip girdles, and even some cranial fragments). Whilst the bone fragments from larger animals often had a similar appearance to those from LBb, the microfauna in UBb was mostly a relatively uniform dark brown. These results could only be explained by suggesting a new input of at least microfauna to UBb, in addition to the material reworked from LBb. These findings were passed on to Currant, who has confirmed the differences in further samples and who has documented additional criteria so that several 'preservation types', including both megafauna and microfauna, are now recognised (cf. Green & Currant 1982; Currant in Green et al., in press). Recent studies by Currant have demonstrated that, although different 'preservation types' obviously represent separate inputs to this part of the cave, the tendency for younger debris flows to incorporate older material is still clear. This is very well evidenced by more detailed work on the whole stratigraphy. For instance, LBb and UBb are locally separated by a pool deposit, the Silt beds, which itself may be underlain by speleothems. This deposit represents a comparatively gentle depositional system and one would not therefore expect to find the 'spotted' fauna of the LBb as a major derived component within these silts. Currant has indeed noted the scarcity of the 'spotted' fauna, but he also recognises the presence of a discrete 'dark' fauna, identical to that component in UBb. When the UBb debris flow entered the cave, in addition to any fauna that it might originally have been carrying, it incorporated both 'spotted' and 'dark' faunas by cutting into both the pool deposits and LBb (which is stratigraphically demonstrable). The distribution of the faunal remains therefore matches exactly the suggested lithogenesis of the deposits. Although the 'preservation type assemblages' defined by Currant show general ecological integrity and considerable taxonomic differences one from another, Currant is wise enough not to suggest that these assemblages represent true biocoenoses. A step has been taken up the anataxic 'ladder', which, with care, can be used to suggest broad environmental and chronological implications.

Another example from Pontnewydd will illustrate the possibilities for the removal of sullegic effects. It should be noted that Currant took no part in the specific sampling decisions reported below, since he was unable to spend much time at the site during the period involved. The Red Cave Earth bed (RCEb) was defined by the author in the early stages of excavation, and three samples were taken by him. One of these samples was examined for faunal remains but very little material was found, and none that Currant could identify beyond generalities. The author interpreted RCEb as a possible debris flow incorporating typical 'cold' climate entrance facies sediments, a proposition framed as a mere hypothesis needing further testing. During the next season, a much larger sample was taken but, because of organisational pressures, this work was done by a careful yet comparatively inexperienced excavator. RCEb turned out to be only present as a very small remnant against the cave wall and it was totally removed during sampling. After later extraction and identification of the fauna from this large sample, Currant informed the author that there were common remains and that they represented a relatively temperate environment. Although it was by no means impossible that this could have been the result of anataxic factors, or of an incorrect environmental inference from the sediments, both Currant and the author felt that further investigation was necessary, especially in view of the fact that the 'yield' from the sediment sample and from the large sample appeared to be so different. The author therefore extracted fauna from his two remaining samples of what was, by lithostratigraphic definition, RCEb. He also extracted fauna from his samples of the overlying water-laid sediments, the Upper Clay and Sand beds (UCSb), deposits that had not until then been analysed in detail. During earlier observation of deposit geometry, the author had noticed that material from UCSb often penetrated down between the cave walls and older deposits in a thin and diffuse vertical band. A sample of this material, that had previously been taken by the author at a point c.1.5m laterally away from the exposure of RCEb and altitudinally at a lower level than the bulk of RCEb, was also examined for fauna. All these samples of faunal material were sent to Currant for identification but, upon agreement with Currant, without any details of provenance. Currant reported that one set of samples (those from UCSb and from the infiltrated band derived from UCSb) were very rich in microfauna and that this material was identical to that already identified from the large sample of supposed RCEb. The other set of samples (those from the definitely unpolluted RCEb) were very poor in fauna, but each contained one tooth of the lemming *Dicrostonyx*. It was therefore clear that, during bulk sampling of RCEb, massive pollution had occurred because a small proportion of the younger, faunally rich infiltrated material had been inadvertently included. It is not very often that a sullegic effect can be corrected in this way. It must be admitted that the error should never have arisen in the first place. Although the author knew nothing of the faunal content of UCSb at the time, he did know of the downward penetration of material derived from UCSb; he should have warned the director of the possibility of pollution near the walls and should have counselled against an inexperienced excavator being allowed to take the sample and, most certainly, against the total removal of a lithostratigraphic unit. Note that the association which was finally proven between a 'cold' fauna and a 'cold' sediment is interesting but not conclusive, since RCEb is a debris flow and its sparse fauna would certainly have been subject to anataxic factors. Although the sullegic effect was noticed, the destruction of the deposit means that we will never know anything more about its fauna or any of its other properties.

The sedimentologist may sometimes be involved in the study of a number of changes in biological material that occur through time due to what might be called taphic and secular factors, although, hopefully, such changes will not have resulted in modification of the taxonomic composition of assemblages. This topic includes both radiometric dating and 'relative' dating (the decay of organic matter and the input of inorganic components that make up the process of 'bone fossilisation', amino acid racemisation, etc.) of biological material (cf. Chapter 18). Sadly, however, such analyses are usually conducted by specialists who do not feel the need to consult the sedimentologist.

The more 'biological' levels of taphonomy, involving biotic, thanatic and many perthotaxic factors are usually handled by palaeobiologists. Eventual environmental information from this source will be useful in the interpretation of the sediments. Many of the archaeological texts cited earlier in this section involve discussion of the best methods of differentiating anthropogenic bone debris from that produced by other agencies, and of the ways in which man selects particular game from the total available fauna. More general palaeoecological reconstruction in cave sites still tends to be on a rather primitive footing when compared to the complex statistical techniques used in other types of site (cf. Birks & Birks 1980). The best palaeoecological indicators are groups of organisms which are small, which occur in large numbers and which are represented by as many identifiable taxa as possible, each with different ecological requirements. This last point assumes that simple actualistic principles are applicable, an assumption which often seems generally reasonable but which probably requires more detailed consideration. The present environmental range of some species may not always represent their true possibilities, since 'present' is obviously defined as the time at which the census was taken. For instance, the common hampster, Cricetus cricetus, is now (this century) found in the Eurasian steppe, with some populations occurring on slightly lusher grassland and open cultivated land; although this hampster avoids the driest environments, its fossil occurrence is usually interpreted as indicating drier, open habitats. Stuart (1982:78-9) therefore suggests that the ecological preference of this animal has changed radically since the Cromerian, because it is found with a temperate woodland fauna in open-air sites at West Runton (Norfolk). However, until a century ago, this hampster could be found on wet moorland and in dense woodland in the hilly area to the south and east of Liège, Belgium (pers. comm. M. Dewez and J.-M. Cordy). Even if some anomalies can be explained by the patchy nature of our modern data base, there is still no pressing reason why some creatures should not have changed their ecological preferences or tolerances.

The most promising group of organisms in the cave situation is probably the micromammals, which are usually present either because they are themselves habitual trogloxenes (e.g. bats) or because they are the prey species of various trogloxene predators (cf. Mellet 1974; Mayhew 1977). Palaeoecological reconstruction is normally carried out simply by showing the changing proportions of remains indicating major habitats (woodland, grassland, wetland, etc.), as deduced from the ecology of the nearest modern relatives of the fossil taxa. There have also been attempts to apply the known temperature requirements of some taxa to the construction of a "vole thermometer" (cf. Kordos 1977). It would be interesting to investigate whether a transfer function approach, similar to that now used in marine micropalaeontology and in pollen analysis, could be applied to cave micromammal assemblages; such techniques involve the identification of the empirical relationships between ecology and whole assemblages, rather than individual taxa. These multivariate techniques would also be helpful in the investigation of departures from expectations derived from the actualistic data base. Apart from palaeoecological reconstruction, micromammals, and especially the rodents (a comparatively rapidly evolving group), provide the main source for cave biostratigraphy (cf. Chaline 1972; Jánossy 1975; Sutcliffe & Kowalski 1976). Other groups, such as birds and amphibians, may also be used for at least palaeoecological purposes and, when the taphonomic factors seem comparable, they are often grouped with the micromammals as 'microfauna'.

14.4. Cave Palynology

Most palaeobiological work in caves involves faunal remains. However, as has been stated above, cave sediments also contain pollen. The study of quaternary pollen has two intrinsic and very important drawbacks. First, there is little evolutionary or even major phytogeographic change in pollen during the Quaternary. Second, except in the case of some groups (such as the genera Pinus, Quercus or Betula) often observed using SEM techniques, identification cannot normally be taken beyond the generic level or, in some cases, beyond even the family level. On the other hand, pollen is small and may survive in vast numbers in anaerobic deposits. Even allowing for the lack of specific identification, pollen assemblages usually contain a wide variety of recognisable taxa and they are usually very sensitive indicators of environment. Assemblage integrity is therefore of the utmost importance. If pollen from two or more assemblages is somehow mixed, the artificial result will often resemble a natural environment and only the most painstaking work on taxonomic detail or states of preservation would allow an expert to recognise the error. It is for this reason that palynologists in Britain, Holland or Germany work mainly with very fine, preferably organic sediments, situations in which the taphonomic process is best understood. Furthermore, work is concentrated upon good sequences, so that temporal changes can be demonstrated to be logical. For obvious, and by no means totally unjustifiable, reasons, the word 'cave' does not exist in the professional vocabulary of these palynologists.

In contrast, palynologists from southern Europe, and especially from France, often extract and interpret pollen from caves. Indeed, many of them specialise in archaeological cave and shelter sites (cf. Leroi-Gourhan & Renault-Miskovsky 1977). This polarity between 'northern' and 'southern' approaches is particularly confusing for researchers in other disciplines, such as archaeology, who have no way of knowing whether or not they should ignore published pollen results from caves, especially since proponents of each palynological school make absolutely no comment upon the attitude towards cave pollen of the other school. Synthetic works, such as Goudie (1977), West (1977) or Birks and Birks (1980), give admirable accounts of what is significant, but the non-specialist also needs to be told explicitly what is *not* significant, and why. The present author's firm, if non-specialist, opinion is that, in general, the pollen results produced from caves by the 'southern' school are practically worthless. The reason for this is that little more than lip-service is paid to taphonomic factors. In practice, if pollen is present, it will usually be interpreted as if it represented an unbiased sample of the pollen available at the time of deposition of the sediments from which it was extracted, sometimes even if the only 'decipherable' parameter is the arboreal/non-arboreal ratio. The author is not, of course, a palynologist, but certain attitudes held by the leading members of the 'southern' school can be recognised as unacceptable, even by a sedimentologist. A few examples of such attitudes will be discussed here.

The first area of taphonomy of interest here involves perthotaxic factors.

> Les sites que nous avons étudiés, sont nous l'avons vu protégés en grande partie des agents atmosphériques; il ne peut donc pas y avoir en principe à cause des vents, de disproportion importante entre les pollens anémophiles et les autres grains. Par contre, les pollens des plantes basses facilement traversées par les animaux ou foulées au pied et surtout les pollens entomogames seront nombreux; pour les mêmes raisons, les espèces rudérales poussant aux abords immédiats des gisements habités seront bien représentées. (Renault-Miskovsky 1972:70)
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> [Trans.: As has been stated above, the sites which we have studied are for the most part protected from atmospheric agencies. In principle, there cannot therefore be an important disproportion due to wind between the pollen of anemogams and other types. Conversely, the pollen of low-lying plants, which are easily penetrated by animals or trampled under

> foot, and especially the pollen of entomogams, will be common. For the same reasons, ruderal species growing in the immediate vicinity of occupied sites will be well represented.]

Renault-Miskovsky's sites range from shelters to quite deep cave situations. Her discussion suggests that she believes that, apart from major reworking of sediment (anataxic factors), pollen in caves can be totally explained in terms of primary pollen dispersal mechanisms: an anemogam grain will reach the site only if the wind blows it there, an entomogam grain represents the passage of an insect, and so on. Even if this is not so, her statement concerning the role of wind is illogical, whichever way one interprets it. If anemogam grains are introduced by the wind, and cave sites are variously protected from the wind, then there will indeed be an unpredictable fractionation effect. If anemogam grains are finally introduced by other mechanisms (such as slope wash), then the fact that a cave may be more or less protected from the wind is totally irrelevant. The discussion can be widened to include lateral effects.

> Les remplissages archéologiques recueillent des pollens de provenances différentes selon leur position par rapport à l'abri ou à la grotte. Près de l'entrée du gisement, il y a le mélange des pollens tombés naturellement, auxquels s'ajoutent les pollens amenés par l'homme et les animaux. Dans le fond de la grotte, ne sont représentés principalement que les pollens des plantes entourant immédiatement l'habitat, apportés avec la terre sous les pieds humains, et dans les fourrures animales qui sont d'excellents pièges à pollens. L'interprétation d'un diagramme obtenu à partir de l'analyse d'une coupe sous abri est donc plus près de l'interprétation en "tourbière" que de celle effectuée "en grotte". Mais si les pourcentages des espèces sont différents d'un site à l'autre, l'allure générale des courbes reste la même et la conclusion paléoclimatique qui s'en suit doit être sensiblement identique. (Leroi-Gourhan & Renault-Miskovsky 1977:37)

> [Trans.: Archaeological fills collect pollen from different sources according to their position with respect to the shelter or the cave. Near the entrance to the site, there is mixing of pollen which has fallen naturally, to which is added pollen brought in by man and animals. In the depths of the cave, the assemblage represents principally only the pollen of plants growing immediately around the habitation site, brought in with earth adhering to human feet, and within the fur of animals which is an excellent pollen trap. The interpretation of a diagram obtained from the analysis of an exposure in a shelter is therefore nearer the interpretation appropriate to the "peat situation" than that appropriate to the "cave

> situation". But, if the percentages of species [i.e. taxa] differ from one site to another, the general trend of the curves remains the same and the resulting palaeoclimatic conclusions should be to all intents and purposes identical.]

Leaving dominantly inorganic sedimentation aside, Leroi-Gourhan and Renault-Miskovsky are here suggesting that the only significant input to deeper parts of caves is through transport by man and animals. The present author would like to see proof of this statement. When the author was working as an excavator at the site of Vaufrey (Dordogne) in 1969, Mlle Bu-Ti-Maï was engaged in a research project (at Bordeaux University) which involved long term trapping of the modern airborne pollen throughout this cave. The author has not seen the resulting thesis but, from comments by various interested parties at a discussion meeting (Le Tensorer 1975:42-3), it is clear that such pollen was trapped even at the back of the cave, some 26m from the wide entrance. At the same discussion meeting (ibid., p.42), Leroi-Gourhan herself reported that she had been able to trap airborne pollen at a point 200m inside the Grotte des Fées (Yonne). She quickly set the audience's mind at rest by suggesting that man did not normally inhabit draughty caves. This generalisation did not seem to worry anyone at the meeting, and no-one pointed out that the significance of a pollen assemblage from an archaeological layer would usually be identified by comparison with the assemblages from other, non-archaeological layers and that air circulation patterns in caves change through time, due to both climatic and morphological factors. Air movement and animal transport are not the only sources for cave pollen. The present author would be very surprised indeed if the dominant source in many caves were not found to be water. Certainly, any researcher who has been involved in water tracing using exotic spores, or in identification of energy sources for troglobite or troglophile organisms, knows that it is quite difficult to take a water sample, be it from a drip or a stream, that does not contain some pollen. Furthermore, the exact route which the water has taken may be expected to produce rather interesting fractionation, at least by size of pollen grains.

It should be noted that French cave palynologists appear to be aware of the effects of general patterns of sedimentation:

> Les remaniements sédimentaires sont fréquents. Certains occasionnés par les eaux de ruissellement ou par les animaux sont mis en évidence par le fouilleur ou par le sédimentologue. Les aménagements humains (trous de poteaux, applanissements, lits de galets ...) sont visibles à la fouille ou mis en évidence grâce aux dessins et aux plans des niveaux d'habitat. Mais dans tous ces cas, le palynologue observe des décrochements dans les courbes des diagrammes, indices d'un manque de continuité dans la sédimentation.
> Les apports de sédiments exogènes, ..., sont eux aussi mis en évidence par la fouille ou par l'étude sédimentologique et entrainent également des interruptions dans l'évolution des courbes polliniques. (Leroi-Gourhan & Renault-Miskovsky 1977:37)
>
> [Trans.: Reworking of sediments occurs frequently. Certain occurrences, due to wash processes or animals, are demonstrated by the excavator or the sedimentologist. Anthropogenic structures (post-holes, regularisation of living floors, spreads of pebbles, etc.) are visible during excavation or are demonstrated thanks to drawings and plans of habitation levels. But in any case, the palynologist observes dislocations in the curves of pollen diagrams which are indicative of a lack of continuity in sedimentation. Inputs of allochthonous sediments, ..., are also demonstrated during excavation or sedimentological analysis. Such inputs cause interruptions in the evolution of the pollen curves as well.]

If Leroi-Gourhan and Renault-Miskovsky are suggesting that help should be provided by the sedimentologist, the present author could not agree more. However, since much of the present thesis involves criticism of the French School of cave sedimentology, the author places little faith in the reliability of the sedimentary deductions supplied to the palynologists. In any case, a review of the literature will show that, except where it is necessary to explain away a major anomaly, French cave palynologists take no practical notice of sedimentary effects. As for the insistence upon breaks in pollen curves, sceptics, such as the present author, are not particulary interested in these palynologists' ability to spot 'a lack of continuity in sedimentation'. It is their inability to demonstrate that they can consistently spot mixing of ecologically and temporally diverse pollen assemblages that is most worrying. Given mixing, any 'continuity' or 'discontinuity' is but an artefact.

The present discussion could easily be extended to include the radical underestimation of post-burial effects, such as differential preservation and subsurface translocation, of which most palynologists of the 'southern' school would seem to be guilty. The number of cases in which pollen from air-hole scree has been interpreted as representing the contemporary flora is truly alarming. However, it would appear to the present author that the point has already been made.

One more anomaly between the findings of the 'southern' and 'northern' schools of palynology should be noted, since it is not apparent in the literature. The present author knows of several occasions upon which British palynologists have, in fact, tried to extract pollen from ancient clastic cave sediments; the result was nearly always that there was no pollen to be found. Setting aside the suspicion that, in some cases, pollen was indeed recovered but that the palynologist, in all good faith, was unwilling to place what, in his professional opinion, were uninterpretable results in the hands of non-specialists, one still wonders why there appears to be a consistent lack of pollen in British clastic cave sediments, at least according to the professionals (several non-specialists having succeeded in extracting pollen). Palynologists from the 'southern' school nearly always find pollen at most levels in any cave sequence. One answer could be the extraction techniques developed especially for this sort of sediment (cf. Girard 1975, specifically for cave sediments; Juvigné 1975, for coarse sediments in general). The main difference between these special techniques and standard methods is that they allow very much more rapid recovery of pollen. This enables palynologists of the 'southern' school (and unwise non-specialists in this country) to extract a 'significant number' of grains from sediments that have very little pollen, simply by using very large sediment samples which could not in practice be processed by standard methods. Naturally, the larger the sample of coarse sediment, the less likely it is that the included pollen represents a temporally and ecologically discrete assemblage, a fact which is conveniently ignored in the literature. No doubt two hundred pollen grains could be extracted from sediment of the Early Archaean if one tried hard enough.

It would seem to the present author that palynology will be useful in any situation, whether in a cave or in the open, only as long as plausible approximations can be suggested to allow for the effects of every stage in the taphonomic process. From taphonomic theory, it would appear highly unlikely that sequences of clastic sediments in caves could often be safely interpreted in terms of their pollen and spore content. Any attempt at palynology in caves should probably be accompanied by arguments concerning viability which are independent of the ancient pollen itself. Some useful results could nevertheless be routinely achieved in caves, such as the identification of plants responsible for eucladioliths or the reconstruction of the general habitat of hyaenas from analysis of their coprolites, although even these examples should be supported with evidence of significance from modern occurrences.

However, this recognition of the general unsuitability of caves for pollen analysis highlights a very important problem, already touched upon by Stuart (supra, p.567): the chronostratigraphic units of the British Quaternary are defined almost exclusively by pollen zonation. There is a growing feeling amongst some British quaternarists that this chronostratigraphy is too narrowly based and potentially subject to homotaxial error. Gascoyne et al., working at Victoria Cave (North Yorkshire), have recently succeeded in producing uranium series dates on stalagmite which was intimately draped over and around diagnostic elements of what is known as the "hippopotamus fauna". The dates show that the 'hippopotamus fauna chronozone' may be correlated with the marine Oxygen Isotope Substage 5e. These authors' dissatisfaction with the current British chronostratigraphy is clear:

> The association of Ipswichian pollen assemblages with the 'hippopotamus fauna' at several [open-air] sites would suggest that the Ipswichian interglacial sensu stricto must therefore also coincide with substage 5e. However, the paucity of vertebrate fauna at the Ipswichian type locality at Bobbitshole [which, ironically, is not a cave], near Ipswich, Suffolk, makes a direct correlation with the Lower Cave Earth in Victoria Cave impossible. Elsewhere [in open-air sites], there are reported discrepancies between what are apparently Ipswichian pollen assemblages and the associated mammal faunas, suggesting that some of the pollen assemblages assigned to the Ipswichian may actually represent other warmer stages (Sutcliffe & Kowalski 1976:55; Bowen 1978:148). Many of these difficulties arise from the unsuitability of the chosen type locality for this important phase of the Pleistocene. Despite such problems, the term 'Ipswichian' is widely applied to the last period in which the climate of the British Isles was as warm as, or warmer than, at present, though this is not strictly within the original definition of the term. This procedure is necessitated by the absence of any other acceptable name for the same phase. With this qualification, we feel justified in correlating the 'Ipswichian interglacial' of the British terrestrial sequence with substage 5e of the marine isotope record. (1981:654)

This is a clear challenge to palynologists to justify their material, as well as a demonstration that, from the point of view of chronology, cave research can sometimes produce more reliable results than can pollen from open-air sites. The present author is certainly in favour of a wider base for both regional and national chronostratigraphy. However, it is possible that the present situation may develop into a schism, with the setting up of rival systems to the current chronostratigraphy and a general breakdown in communication. One of the ways in which this might be avoided would involve a concerted attempt by palynologists, geochemists, geophysicists, karst geomorphologists and sedimentologists to extract meaningful assemblages of pollen from stalagmite sequences. The taphonomic factors involved should, in theory, be simpler than in clastic cave sediments. Input by animals would be unlikely, input from the air could be monitored by stable isotope and insoluble residue studies, and postdepositional effects would often be minimal but in any case identifiable (even in the case of preferential oxidation). The main problem would lie in proving a predictable link between surface ecosystems and the pollen content of cave percolation water. It is possible that this might prove feasible in specific sites, after careful study of the modern pollen dispersal system and of sub-recent speleothems, with a view to setting up a predictive model of the relationship between pollen content and the dynamics of speleothem formation (e.g. there might be a correlation between physical and chemical pollen fractionation effects occurring in transit fissures and the rate/morphology of stalagmite growth). Such a project would require considerable expenditure of time, effort and money and strict adherence to the highest professional standards but, above all, it would require the constant good will and enthusiasm of all those involved.

15. PROVENANCE AND PALAEOGEOGRAPHY

The cave sedimentologist should be primarily concerned with depositional environments and with environments of postdepositional modification, acting within the cave. However, any sediment is only a selection of what is available in the local landscape, and these source materials will radically affect the nature of the cave sediments constructed from them. From a 'speleocentric' point of view, variations in source materials are often a nuisance which must nevertheless be compensated for in order to reconstruct past conditions within the cave. Throughout the present text, the author has argued that most cave sedimentologists are not 'speleocentric' enough, that they are too ready to make facile interpretations of cave sediments in terms of regional environment, without proper consideration of all the possible factors which intervene, both temporally and spatially, between the sediments of the cave and the landscape as a whole. The introverted, 'speleocentric' approach allows the detailed reconstruction of how the cave sequence was formed. During this work, the sedimentologist will recognise many characteristics which, from his general experience of sediments, he will know to be uninterpretable, at that point in the research, except in the most basic terms (e.g. the variation in mean particle size of stream deposits). Many inputs of allochthonous sediments will be obvious and, again, these will be recognised only in basic terms. For instance, at Pontnewydd Cave (cf. p.1190) there are several units which, demonstrably, were emplaced by various forms of mass movement and fluvial action, but which contain rock types that do not outcrop within, or even particularly near, the drainage basin in which the cave is now situated. Either a radical change in regional topography and drainage is indicated, or a transport mechanism capable of bringing material from distant basins must be suggested. In the case of Pontnewydd, glacial transport seems the most likely explanation for the availability of these exotic rocks within the cave's catchment area. This is as far as 'speleocentric' and general 'common sense' reasoning can go. Naturally, it would be folly to suggest that, with a cave like Pontnewydd, the mere presence of exotic rocks proves the contemporary existence of an ice-sheet within a few kilometres.

Although the author advocates the 'speleocentric' approach, it should not be thought that he believes the conclusions that can be drawn from this type of study to be the ultimate goal of cave sediment research. It would be a sad day indeed if all we could deduce about the Quaternary from cave sediments were information such as the flow speed of a stream, in a given section of cave passage at a given point in time, useful as such information may be to contextual studies. If we wish to learn something more about ancient landscapes, we must reason our way out of the cave, but in a controlled and cautious manner, not via extravagant flights of fancy. Miskovsky (1972,1974) makes much of the shape and condition of limestone clasts at l'Hortus (Hérault), suggesting that these parameters are sometimes linked with such factors as the stability of exterior slope deposits, which in turn should be linked with the presence or absence of binding vegetation. The present author would be more than willing to consider such a possibility, if only Miskovsky would indicate which slopes he has in mind - the cave entrance is half way up a near-vertical cliff.

By far the best way to link cave sediments with exterior sediments should be by continuous excavation. This will often prove topographically impossible but, to the present author's mind, the difficulty is generally overestimated, simply because the attempt has only very rarely been made. Sutcliffe and Zeuner excavated outside Tornewton Cave (Devon) and extended their trench to over 13m from the rockface, connecting in the process with a deposit which they called "Head".

> Unlike the upper strata of the talus, which are laterally restricted to a narrow zone along the foot of the cliff, the [underlying] "Head" extends into the floor of the valley and appears to occupy its entire width, though this has not yet been proved by continuous sectioning. (1962:136)

The present author will present data (p.726) which suggests that this "Head" contains an important loessic component and that the deposit as a whole can be followed, without significant physical gaps, for several kilometres in various directions away from the cave.

When cave deposits are not actually continuous with external sediments, it may nevertheless be obvious that some of the cave strata once formed part of an extensive sediment body, remnants of which are still to be seen in the open. The Patella Beach at Minchin Hole (Gower) is probably part of a once continuous unit that now survives in patches all along the coast of Gower, and perhaps beyond. The problem then becomes one of correlation (cf. Chapter 17).

When there is no obvious continuity, either actual or reconstructed, between cave deposits and exterior sediments, it is still highly probable that there were often sedimentary links between the two domains. All cave sediments contain an allochthonous clastic component, which may be volumetrically minimal (e.g. in many speleothems) or totally dominant (e.g an impinging regional sediment body, either in situ or reworked). Even the so-called 'residual clays' in caves are not completely autochthonous, a fact which can be proven by simple calculations of the insufficient insoluble residue that would result from the formation of the observed cave cavity (cf. Kerekes 1951), let alone by more sophisticated techniques. It is of no great interest, indeed it is not even possible, to identify and source all this allochthonous material. One is interested in answering particular questions, such as 'Where did this unusual material come from and how did it get into the cave sediment?' or 'Did this exterior deposit or rock type, of known lithology and perhaps age, contribute to sediments in a nearby cave and, if so, how?'. As these formats suggest, the subject may be approached from either end of the sedimentary system. Particular allochthonous components of the cave sediments can be isolated and characterised in detail, prior to a search for the source in the surrounding landscape. Alternatively, at least the more obvious potential sources (soils, rock types, unusual sediment bodies, etc.) can be characterised, prior to a search for any input to the cave deposits. In practice, a combination of both approaches will normally prove useful. Linking a sedimentary component with its source (either in general or in terms of an actual outcrop) involves provenance studies, whilst deducing the route and process(es) of transport involves palaeogeographic studies.

> We might ask how and under what conditions sediment originates. For clastic sediments, this is the question of provenance - the climate, relief, and lithology of the source area. The answer to this question is found largely in a study of the composition of gravel clasts and the detrital mineralogy of sandstones, which constitute direct evidence of the kinds of source rocks. But the composition of sediment is not precisely that of the source region, inasmuch as debris from that area has been

> through a geologic "sieve" so that its composition is altered by selective losses and enrichment (a question involving mineral stability by weathering in the source area), by abrasion during transit, and by alteration or solution during diagenesis. The problem then involves mineralogical analysis coupled with a knowledge of mineral stability, both mechanical and chemical.
> A second question we might ask is, where and at what distance did the source area lie, and what is its relation to the configuration and bathymetry of the depositional basin? Or, in short, what was the paleogeography of the epoch during the depostion of a particular sedimentary formation? To answer this question we need to ascertain the paleoslope, the sedimentary strike, the paleocurrent system operative during the depositional process, and the facies distribution. The paleocurrent system, reconstructed from primary directional structures, distribution patterns (dispersal "fans") of debris, and lateral facies variations, including variations in such scalar properties as pebble size and roundness, will assist in solving the paleogeographic riddle. This approach involves measurement and mapping of such commonplace features as cross-bedding, pebble size, and grain fabric. It involves extended field studies. (Pettijohn 1975:483)

This summary is relevant to sedimentary geology in general, but there are certain aspects which are of greater or lesser importance in the context of the Quaternary. Mineralogy (cf. Farrand 1975) and petrology (cf. Bull 1978a) are still of paramount importance. Mineral suites in cave sediments are usually identified at quite a basic level; it would be interesting to see whether more complex classification (e.g. by stoichiometry) could give better resolution in some cases. However, in the frequent cases where the immediate source is itself a sediment and is not likely to have been too distant from the cave, even geologically ephemeral characteristics, such as particle shape and size distribution, may give useful clues, as long as the effects of probable transport, depositional and diagenetic processes are taken into account. In fact, it is a common occurrence that the original 'rock' source will only be of general interest. For instance, the lower deposits of Pixie's Hole (cf. p.844) contain large quantities of flint gravel which can be identified as originating from the Upper Cretaceous. However, the fact that the identical material of the Haldon Gravels (Eocene, with extensive gravelly 'head' dating from the Pleistocene) lies above and only a few kilometres up a valley to the east of the cave is much more important than the identification of the exact area of original flint formation within the Cretaceous Province.

The effects of the geologic "sieve" are of interest in their own right, since they help to fill the 'gap' between the source and the cave, even when it seems likely that deposits representing each stage of 'temporary storage' will not have survived in the open. Thus, at Pontnewydd Cave (cf. Green et al. 1981; Green et al., in press), Bevins has narrowed down the source of the exotic rocks, mentioned above, to either Snowdonia or Cumbria, most probably the former. All geological specialists working at this site are agreed, for various reasons (e.g. deposits containing these rocks are plastered all over the countryside, more or less irrespective of topography), that glacial transport was involved. Embleton and his associates are working upon the geomorphological implications and are searching for undisturbed till bodies. However, Bull, using SEM techniques, has noted that the quartz grains from several of the cave deposits exhibit fluvial surface modification, which probably post-dates any glacial transport and, because some time would be necessary to allow such modification, pre-dates final emplacement in the cave. Because of the presence of younger ice-sheets in this area (after the emplacement of most of the deposits in question), which may have left some older till intact but which would almost certainly have destroyed any restricted fluvial deposits, it is extremely unlikely that this fluvial phase could have been spotted by any other means.

Palaeogeography is often expressed by pre-quaternary geologists in terms of 'average' conditions, evidenced in deposits of enormous extent that were laid down over very considerable lengths of time. Quaternary deposits will often be very much less extensive or continuous and the 'sedimentary basin' concept may be more difficult to apply. However, the Quaternary has the advantage that more precise dating is often available and also that, as one approaches the present, the ancient topography will have been increasingly similar to the modern land surface. Topography is never static, however, even on quite short time scales. Probably during or shortly after the Ipswichian, a stream ran into Pixie's Hole, a cave which is now perched over 15m above the narrow valley floor. In areas where abundant sediment has been periodically available (especially under glacial or proglacial conditions), the effective valley 'floors' may rise and fall in very short periods. Ford (1976) gives a probable example of the effects of glaciation at the Masson Hill Caves (Derbyshire); the caves contain sediments derived from the Millstone Grit, the nearest outcrop of which is several kilometres away at a lower altitude and on the other side of a valley, 250m deep.

If pre-quaternary palaeogeography usually involves basins that are tens or hundreds of kilometres wide, quaternary palaeogeography is best considered as having no set geographic scale. The study of deposit geometry and lateral variation within a few metres of the cave sediments themselves is an exercise in miniature palaeogeography, and areas between fallen roof blocks can often be usefully considered as discrete basins of deposition. A simple example of small scale palaeogeography is provided by Laville (1975) at the shelters of Le Flageolet (Dordogne). The local limestone is divided into four, lithologically distinct sets of strata, clasts of which can be sourced quite easily. Although the present author does not necessarily agree with Laville's suggestions concerning the transport and depositional processes involved in the formation of the shelter sequences, Laville has certainly demonstrated how the sources of clasts (the 'palaeocurrents') varied through time, variation that can often be linked, via a consideration of the relationship between rockface morphology and lithologic facies, with general arguments concerning slope stability. At these sites, routes for the input of slope debris are clear (the shelter deposits are all less than c.40ka old, and mostly between 20-10ka old), and Laville has even described one nearby (undated) exposure of coarse slope mantle which contains clasts of the expected lithology.

There are very few cases in which a cave deposit has been placed within a detailed reconstruction of a wider landscape. Some quite plausible attemps have been made at quantifying exterior slope stability using theoretical arguments (cf. Butzer 1981; also section 3.3. of the present text). Some normally less plausible attempts have been made to reconstruct biological aspects of the landscape, simply by placing faunal and floral taxa, recorded in a cave, at points within the modern topography, according to their most likely ecological preferences. However, no really comprehensive study of a landscape including caves has yet been made in this country, although promising areas, such as the region around Torbryan (Devon), are not difficult to suggest. Indeed, the groundwork has already been laid in Gower (cf. Bowen 1977b). Cave studies seem to be on the point of 'overflowing' into the open in the Near East (cf. Farrand 1979) and in South Africa (cf. Deacon 1979). The present author cannot see why a cave sedimentologist should not come out of his cave, if there is relevant information to be had in the surrounding area. It is to be hoped that he would then meet specialists in open-air sites moving in the other direction.

16. DIAGENESIS

The term 'diagenetic' means literally 'pertaining to features developed during formation'. The 'formation' referred to is that of sedimentary rock. Those processes which affect a sediment whilst it is at or near the earth's surface, and thus subject to comparatively low temperature and pressure, are said to be diagenetic, as opposed to metamorphic processes which usually occur at depth. The concept of diagenesis was first developed by sedimentary geologists in order to work back from observed rocks, with a view to recognising the history of lithification and eventually the nature of the original sediments. Comparatively recently, advances in marine sedimentology, especially with respect to carbonate systems, have allowed diagenetic studies to include detailed observations of functioning sedimentary basins, where input of sediment has remained relatively stable over long periods; by noting the progressive changes in the shortest possible chronostratigraphic intervals, each stage of diagenesis, from the loose sediment to the indurated rock, can be recognised and used as a model for the formation of ancient sedimentary rocks. In pre-quaternary terms, the 'rock' is the end member of the process and, to all intents and purposes, diagenesis may be considered to be complete. In quaternary terms, this is not often the case, the observed sediment usually being in some intermediate and often unstable state, between fresh material and indurated rock. Moreover, in pre-quaternary terms, the nature of the diagenetic effects will provide useful environmental information, concerning such features as the chemistry and depth of the sea or the rate of regional subsidence, since the time resolution required is very coarse. In quaternary terms, diagenetic changes are often too slow to reflect the environmental changes that are of interest.

If, in quaternary studies, the 'rock end member' is not usually achieved, particular care must be taken to define the other end of the system, the earliest diagenetic effects. Theoretically, we might say that any postdepositional effect is diagenetic, but even pre-quaternary geologists do not go this far and processes such as subaerial weathering are excluded. Although the reasons for such exclusions are not usually discussed explicitly, it would appear that the discrete nature of such phenomena is the main cause. A subaerial weathering zone occupies a precise stratigraphic position, which defines both the time during which it was forming and, even more importantly, the time at which it ceased to form. Diagenetic effects show no such simple stratigraphic relationships. It would therefore seem to the present author that the only useful way to define diagenesis, at least in the quaternary context, is by the subjective assessment of the information content of its effects. If a sediment has been modified in such a way that the effects cannot be plausibly related to a meaningful (temporally discrete) surface environment (_sensu lato_), these effects may be said to be diagenetic. Alteration effects in a deposit underlying a stream, and probably caused by that stream, are not most usefully perceived as diagenetic, whilst many suffosion effects can only be classed as diagenetic. Note that stress is laid upon the effects, not upon the processes. The actual processes of diagenesis (_infra_) may be identical to those giving rise to 'non-diagenetic' (stratified) effects.

However, in some cases, it may be useful to ignore temporarily the stratigraphic information provided by specific phenomena and to include them in a general discussion of diagenesis as a complex of all postdepositional processes. In the cave context, the study of diagenesis will be primarily concerned with the 'removal' of postdepositional effects in order to reconstruct the original sediment or, at least, with the recognition that some effects are diagenetic, either in a subtractive or additive manner, and that a sediment, or component of a sediment, as it appears now may no longer be interpreted as wholly representative of the environment in which it was deposited. From this viewpoint, one may consider the degree of bias introduced into a mineral assemblage by postdepositional alteration, whether in a well stratified palaeosol or in a stratigraphically ill defined context, as a function of diagenesis, if the object of the exercise is to reconstruct the mineral assemblage so that it may be sourced more easily.

Diagenesis was defined above, in a general manner, as the set of processes which convert sediment into rock. Strictly speaking, this is merely lithification. Diagenesis is more accurately defined as the set of processes which convert a sediment into the most stable state, given the nature of the surrounding environment. Stalagmite is a rock which is in reasonable equilibrium with its immediate environment but, because the environmental range even within a cave is quite wide, stalagmite can only be considered to be a metastable rock which will be modified as soon as the inevitable environmental change occurs. Diagenesis is thus a tendency towards a dynamic equilibrium between minerals and their current environment. Environment often changes too fast for a true equilibrium to be achieved, even momentarily. Depending upon the nature of the original environment in which a mineral was formed, different mineral groups react more or less quickly to particular shifts in current environment. For instance, within the normal range of earth surface environments, carbonates will usually react very much faster than many common silicon minerals such as quartz. As particular minerals are adapting to a new environment, the sediment as a whole changes, principally by compaction and cementation, in such a way that the 'internal' environment is stabilised. Rather than a collection of individual particles, the material is now reacting more as a unit, which may or may not be stable in the 'external' environment; if it is not, it will continue to be modified, both from its 'surfaces' and from any discontinuities that allow the 'external' environment to penetrate into its mass. As a general analogy, diagenetic systems are quite similar to biological systems in the way they react to the more 'unpleasant' external stimuli.

Diagenesis in a cave is slightly different from that in most other geological situations. First, the cave sediment is surrounded by the more or less rigid mass of the limestone, so that compaction and cementation effects will often be rather more irregular, both in space and time, than in less restricted circumstances. Second, in diagenetic terms, the limestone and its caves represent a different system from the cave sediments themselves. When a part of the cave system is well above the phreatic zone, it will still act as a preferential drainage route. The tendency towards mass lithification of the cave sediments will constantly be disrupted by water which is seeking to pass right through the sediments. There is thus both input and output of solutes and particulate matter, with preferential gains 'upstream' and preferential losses 'downstream', factors which necessitate perpetual readjustment. Dense, lithified cave sediments are indeed known, but most of these are geologically ancient (pre-quaternary) deposits which have achieved approximate equilibrium, either after reincorporation into the deep phreatic zone (regional subsidence), or because they were formed in this zone and remained there long enough to become lithified. Even such dense deposits often show signs of 'reactivation' when they have been brought once again nearer the surface, and new cave passages may sometimes be created within their mass.

The most obvious diagenetic process in cave sediments is carbonate segregation. Under the normal processes of alteration (cf. Chapter 12), small carbonate particles are extremely vulnerable to solution, because of the high surface area to volume ratio. The material is therefore stabilised by 'chemical compaction', involving solution followed by reprecipitation in a much denser form (concretions). The original voids between particles are also 'segregated' into fewer, larger voids. Because this is not a closed system, carbonates may often be translocated before reprecipitation and a varying proportion may be lost from the immediate area by removal in solution. Similarly, carbonates may be introduced from elsewhere, to fill voids of any size, at any stage in the segregation process. However, once segregation is advanced, incoming solutes will tend to be precipitated on better crystal surfaces, thus perpetuating the segregation process. Non-carbonate particulate (and possibly even non-particulate) materials will also tend to be segregated, because any impurities in concretions will render these concretions slightly more susceptible to further attack than purer carbonates. However, this process is not very efficient and good differentiation between zones of non-carbonates and zones of carbonates is rarely achieved at scales of over a few centimetres or even millimetres. Carbonates will be preferentially precipitated at particular points in the deposit. Many concretions form on the undersides of larger clasts, since this is where percolation water collects and super-saturation most often occurs. This is particularly common under limestone clasts which may themselves be contributing carbonates. Away from larger clasts, concretions may form around any convenient nucleus and the matrix may be converted into clusters of roughly spherical concretions, from 'pinhead' to 'grape' size. Larger spheres may even contain groups of small spheres which have coalesced. This spherical form is very characteristic of an environment which is not dominantly precipitational, the sphere being the most stable long term response to fluctuating saturation conditions (aggressiveness); under dominantly precipitational conditions, crystallographic factors play a greater part in overall morphology (cf. Chapter 13), producing drusy linings to voids. Even concretions on the underside of clasts may take on a botryoidal habit. Microscopic examination of most diagenetic carbonates usually shows the effects of a complex history of switching between solution and precipitation (mosaics, replacement features, various generations and textures of cement, corroded calcite crystals floating in younger cement, staining, anastomoses and vugs, tiny zones of stratified non-carbonate material, etc.). Diagenesis tends towards the formation of denser carbonates; where porous carbonates are present in a deposit, either diagenesis is not very advanced or its effects have been temporarily reversed by a relatively massive input of new carbonates or by strong and generalised solution.

The matrix around limestone clasts serves to distribute overburden pressure evenly over most of the surface of the clasts. When this support is removed or redistributed, either by the segregation effects noted above or by processes such as suffosion (cf. section 10.3.), the load becomes increasingly concentrated at the points of contact between clasts. An originally clast-supported deposit is already in a similar situation. As time passes and overburden pressure increases, pressure effects may become apparent. The rising pressure at contact points will cause solution and, with time, may even give rise to microstylolites ('saw-tooth' solutional contacts between clasts). Away from the contacts, the carbonates will be immediately precipitated to give the point-to-point cementation so common in ancient deposits, although the effect is usually macroscopically noticeable because the tiny amounts of carbonate derived from pressure solution act as nuclei for precipitation of ordinary solutes. When a limestone clast or limestone bedrock is in contact with a clast of a less soluble rock type, the other rock may slowly penetrate into the limestone by pressure solution, an effect which is usually only really significant in comparatively ancient and large scale (high overburden) deposits. Rising and unevenly distributed pressure, and increasing fatigue in both clasts and concretions, will eventually cause failure. Microfaults may pass through limestone clasts and concretions, but they are often 'healed' very quickly by renewed carbonate precipitation. Sometimes a whole clast may shatter and fall apart as angular fragments before recementation; the degree of dispersal of the fragments is dependent upon the constraints of the surrounding clasts and voids. Faulting is usually frequent and on a small scale in coarse deposits, so that it may be called 'cataclasis' or 'crush-brecciation', terms which the author prefers to 'collapse, founder or solution breccias' which often imply a large underlying cavity into which originally sound rock collapses. However, larger faults, crossing several particles or even several sedimentary units, may sometimes occur.

The processes of carbonate segregation, removal of solutes and suffosion create larger voids which are filled by collapse of denser material under pressure. Overburden pressure will also cause fine sediments to compact in a more normal way, although the only obvious result may be unusually high bulk density. All these factors will automatically cause subsidence, although the input of new material may considerably retard the process. Older deposits in a sequence, which will usually (but not always) be the deepest deposits, will have suffered most subsidence. This effect is accentuated in the common situation where water leaves the deposits through fissures ('sinks') in the underlying bedrock. As drainage is concentrated towards these exit points, more and more material is actually lost (rather than merely redistributed) by suffosion and solution. If subsidence is regular and gentle, the deposits may become increasingly down-warped in an organised manner. However, even under gentle conditions, some sediments, such as clayey silts, will often fracture. The faults which are most obvious are usually those which are roughly parallel (but usually convergent downwards) to the axis of subsidence, but radial faults and low angle thrust faults are normally also present. If support is removed from below more quickly, the deposits will either collapse more or less chaotically or, if induration and texture are not too variable, the deposits may adjust along one or more major faults. Depending upon factors such as sediment plasticity, a variety of deformation structures may arise during subsidence. The difference between subsidence and collapse of sediments at a free face, such as an erosion bench, is usually quite easy to recognise (but cf. p.1018), at least in larger exposures, because subsidence normally involves the whole deposit along the axis of subsidence and there are very rarely any undisturbed beds towards the bottom of the sequence. Only where subsidence is due to the removal of a particularly vulnerable component (e.g. abundant organic matter, or carbonates in a dominantly quartzitic sequence) from only certain strata may underlying deposits, that did not originally contain the vulnerable component, remain more or less undisturbed. Although subsidence must often be considered to be diagenetic, in the sense of being ambiguous, both in terms of stratigraphy and surface environment, its effects upon the shape of the sedimentary 'basin' at the surface, at given points in time, may be very important. Major stages in down-warping may thus be 'dated' by the formation of depressions at the surface which then become available to collect new sediments.

Many of the diagenetic effects mentioned above are evidenced at the Westbury-sub-Mendip Fissure and these will be discussed in detail in Chapter 22. This site contains sediments that are of considerable antiquity, the youngest probably being older than 300ka, so that diagenesis is well advanced. However, diagenetic effects are not just a function of time and many instances in much younger sites will be noted in Part II of the present text. Whilst diagenesis is mainly obvious in terms of the carbonate system and of subsidence, some of the other alteration phenomena mentioned in Chapter 12 may also be involved, although it must be reiterated that very little direct research has been conducted into general mineral stability in caves. The most common diagenetic features noted in the cave literature are certainly major subsidence structures. At Die Kelders I (Cape Province, South Africa), Tankard and Schweitzer (1976) have shown how considerable down-warping has been caused by removal of "mud-humus" associated with the frequent archaeological layers. At La Caune de l'Arago (Pyrénées-Orientales), de Lumley and his colleagues (cf. de Lumley 1979) have described important down-warping, decalcification, deposition of metallic compounds and modification of phosphates; these phenomena are interpreted in terms of a paragenesis (a sequence of alterational or diagenetic events) for which the proof seems a little tenuous, especially with respect to the suggestion that several temporally discrete phases of deposition, followed by breaks in deposition with modification of the deposits, are represented. At Tabun Cave (Mount Carmel), Jelinek et al. (1973) have described a more convincing sequence of events, involving subsidence (probably related to a drop in the local water table) and dislocation between cemented and non-cemented zones of the same deposits, a feature which had earlier caused misinterpretation of the archaeological sequence. Glover (1979) has shown how, in various Indonesian caves, cemented zones may remain 'perched' on cave walls when the operation of sinks below the deposits causes the bulk of the sediments to warp and subside through time.

One last point must be made here. Diagenesis will usually involve a certain loss of original information. Nevertheless, although diagenesis may have caused quite violent modification and deformation, much useful data can still be extracted given careful analysis. The researcher should not immediately reject a site just because the sequence at first appears to be a hopeless jumble, especially since this would involve ignoring most older deposits. Stratigraphic principles are still valid even with a deposit that has been turned upside-down.

17. CORRELATION

A correlation is a mutual relationship between two or more phenomena. The observation that bed A overlies bed B is not a correlation, since the relationship is here complementary not mutual. However, a correlation does not necessarily imply equality, interchangeability or even similarity between the phenomena in question. It is important to realise that there is no such thing as an 'objective correlation', that phenomena do not have the intrinsic property of 'correlativeness'. A correlation takes its meaning from the subjective choice of the observer concerning the type of relationship that will prove most useful in a given situation. Two phenomena may be correlative for one purpose, and not correlative for another. To state that two phenomena are correlative, without stating in what way they are correlative, is at best equivocal and at worst meaningless.

The term 'correlation' is usually applied in quaternary studies to those high level relationships that concern stratigraphy.

> To correlate, in a stratigraphic sense, is to show correspondence in character and in stratigraphic position. There are different kinds of correlation depending on the feature to be emphasized. Lithologic correlation demonstrates correspondence in lithologic character and lithostratigraphic position; a correlation of two fossil-bearing beds demonstrates correspondence in their fossil content and in their biostratigraphic position; and chronocorrelation demonstrates correspondence in age and in chronostratigraphic position. (Hedberg 1976:14)

This statement is clear but it is also a little cowardly, since it carefully avoids explicit discussion of the different levels of temporal involvement. Each of the three main types of correlation depend upon both character and stratigraphic position. Correlation is therefore strictly equivalent to the decision to place two or more phenomena in the same stratigraphic unit. However, it is not clear how the concept of 'unit rank' should affect correlation. Consider a situation in which two exposures of sediment, A and B, are available; examination of the general scale of the sequence of deposits and of the individual character of sediments A and B suggests that they should both be allotted a low rank ('bed' or 'lithozone'). If it is then decided that A and B are most probably part of a single sediment body (a single 'bed'), there is a clear case for lithologic correlation of an unequivocal type. However, if A and B are similar but are not likely to be parts of a single sediment body, they are not correlative in that sense. Now, these similar but probably not co-extensive sediments are placed in a single, higher ranking unit (e.g. a 'member'). May the two sediments A and B now be considered to be lithologically correlative in the sense that they both belong to the same member, that member occupying the same lithostratigraphic position in the two exposures? In the case where several low ranking units belonging to this member are present in each exposure and, judging from the other units, it appears that A and B occupy very different lithostratigraphic positions inside the member, one would not usually correlate A and B; a more reasonable step would probably be to allow a lithologic correlation of the two exposures at member level (which is equivalent to saying that A and B belong to the same member). However, what should we do with two sediments A and B, which are similar but probably not co-extensive, but which occupy exactly the same lithostratigraphic position in the two exposures? Such a situation will occur commonly in caves, and the two sediments are then best grouped into a 'complex' (cf. p.162).

If this complex contains only sediments A and B, are they now lithologically correlative? Indeed, have we not 'inadvertently' correlated them simply by placing them in the same complex? We may know the overall lithostratigraphic position of these two units, but we have no idea of their relationship one to another.

This sort of discussion could be continued by involving more complicated issues, such as facies changes across boundaries that are oblique to the bedding structures. However, it is already clear that, even when one is honestly trying to restrict the scope of correlation to the lithologic level, and when one has been very careful not to mention the word 'time', it is extremely difficult to keep temporal considerations out of the arguments. Correlation is intimately linked with ideas of contemporaneity and sequence, not because such links are unavoidable but because they are useful. Indeed, one may say that the essence of correlation is to replace the normal observation of stratigraphic relationships, when these relationships are obscured by the absense of spatial continuity in the physical record. This is not to say that all correlation should be immediately reduced to chronocorrelation. Only by explicit definition of the type of correlation proposed at each stage of an argument can errors of interpretation be avoided. If sediments A and B are very similar and occupy the same lithostratigraphic position, they may be lithologically correlated. However, it is positively dangerous to leave the argument there. Consideration must be given to the question of whether or not A and B are parts of the same sediment body, whether this sediment body is time-transgressive or uniformly representative of a discrete 'event' or 'phase', and whether any other temporal information may be deduced. It may well be that such information is not available, in which case it must be explicitly stated that the temporal implications of the lithologic correlation are not clear. Just as much confusion can be generated by a refusal to discuss temporal implications as by the assumption that all correlation is automatically temporal. This is true not only for geological data but also for biological and archaeological information. Moreover, it is also imperative that the temporal implications of so-called 'chronometric' data be discussed in full, since it is often forgotten that no 'dating method' actually measures time (cf. Chapter 18).

Since correlation will be necessary whenever there is a gap in the physical record, it is clear that correlation techniques may be useful, not only to link different sites, but also to link different exposures within a single site. No matter what the scale, gaps may be the result of discontinuous deposition, lack of intervening exposures despite the presence of sediment, or of natural or anthropogenic erosion. Intra-site correlation will often have a strong lithologic component since a significant degree of original continuity in sediment bodies may be expected. Correlation between closely spaced exposures is normally quite easy, but situations arise in which much more effort is required. A good example of a 'problem site' is provided by Pontnewydd Cave (Chapter 24). Here, the deposits have been fragmented by earlier excavations and, in any case, the lithostratigraphy is not particularly clear because of the disruptive mode of input (debris flows) of much of the sediment. It was therefore necessary to define separate sequences of lithozones for each exposure and to correlate these sequences before a composite site lithostratigraphy, including high ranking units, could be established. The precise methods used for correlation will depend upon the exact problems presented at different sites. Lithogenetic arguments will often be important since, when one is trying to prove that two sediment exposures represent the same sediment body, it is necessary to evaluate the maximum degree of lateral variation that is permissible before it becomes unlikely that the sediments do in fact correlate. Much iterative reasoning is required but, given due care, this should not degenerate into circular argument, especially if several independent chains of evidence are examined.

Correlation between sites in a comparatively small region would represent a major advance in cave studies, since it should then be possible to allow for the peculiarities of individual sites and to establish a composite regional stratigraphy. The most concerted attempt at such a correlation is represented by the work of Laville in the Périgord region of south-west France. Moreover, a certain 'evolution' in this correlation scheme is recognisable in various publications between the presentation of Laville's thesis (1973) and the appearance of the latest synthesis (Laville et al. 1980).

As has been pointed out many times in the present text, Laville deduces climatic sequences from his cave and shelter sediments:

> Les valeurs de pourcentages et d'indices qui caractérisent les différents dépôts d'un remplissage ne doivent donc pas être considérées comme des valeurs absolues; elles sont seulement représentatives de l'intensité relative des différents phénomènes climatiques qui sont intervenus dans l'édification de ce même remplissage. (1973:57)
>
> [Trans.: Therefore, the percentage and index values which characterise the different deposits of a fill should not be considered to be absolute values; they are only representative of the relative intensity of the different climatic phenomena which took part in the formation of this same [individual] fill.]

The attribution of a sequence to a particular "glaciation" is based upon general considerations of stratigraphy, archaeology and palaeontology. However, Laville claims that correlation within "glaciations" is achieved by matching of the climatic oscillations deduced from the sediments.

> Le découpage de chaque glaciation en stades et en interstades a porté sur des éléments précis.
>
> C'est ainsi que, à une exception près, nous avons réservé le nom d'"interstade" à des coupures climatiques majeures, au cours desquelles l'élévation de la température et l'augmentation de l'humidité ont été suffisamment marquées pour favoriser un arrêt de sédimentation et entraîner une profonde modification physique et chimique des dépôts; sous abri, les interstades sont marqués par les sols d'altération véritables, caractérisés par la présence d'horizons d'accumulation bien individualisés et le développement d'une structure à l'intérieur du dépôt; sous grotte, les mêmes phénomènes ont souvent abouti à la formation de planchers stalagmitiques.
>
> Un stade et un interstade représentent un cycle complet, comportant une phase de sédimentation, une phase d'altération et une phase d'érosion; c'est la raison pour laquelle les sols interstadiaires, au même titre que les sols interglaciaires, sont des sols tronqués, dont il ne subsiste fréquemment que les horizons d'accumulation profonds.
>
> Dans le domaine étudié, et plus particulièrement sous abri, le propre d'un interglaciaire ou d'un interstade est donc de ne pouvoir être mis en évidence, sauf rares exceptions, par la palynologie; l'oubli de cette notion dans l'étude d'autres dépôts quaternaires est, sans doute, à l'origine d'un certain nombre d'erreurs stratigraphiques.
>
> Nous avons, par contre, appelé indifféremment oscillation ou phase climatique, les périodes de réchauffement ou de refroidissement, de récession et d'augmentation de l'humidité, à l'intérieur des stades glaciaires; chaque oscillation ou phase est elle-même scindée en pulsations ou en sous-phases qui peuvent correspondre à son amorce, à son développement et à son déclin, mais aussi à la succession d'évènements climatiques complexes à l'intérieur de l'oscillation. L'optimum climatique d'une phase de réchauffement peut avoir conduit à un bref arrêt de sédimentation; il s'agit seulement, à notre sens, d'une étape dans l'oscillation en question, si l'altération contemporaine n'a pas été suivie de phénomènes d'érosion.
>
> En matière de terminologie, nous avons, par souci de prudence, délibérément fait abstraction des noms locaux proposés par d'autres chercheurs en France ou à l'étranger.
>
> [...]
>
> En l'absence d'un cadre chronologique "absolu" de référence, nous n'avons pu traduire sous forme de graphique la durée et l'intensité relative des différentes oscillations qui se sont succédées en Périgord au cours de la glaciation rissienne.
>
> Les différentes oscillations sont figurées par des intervalles d'égale épaisseur. A l'intérieur de chacune d'elles, chaque dépôt occupe l'emplacement précis que nous avons cru pouvoir lui assigner, par rapport aux trois termes principaux de l'oscillation à laquelle il appartient et qui sont, son amorce, son maximum (ou son optimum) climatique et son déclin. Il en sera de même pour l'évolution climatique du Würm ancien et du Würm récent.
>
> Nous tenterons néanmoins de tracer la courbe climatique de ces deux dernières périodes, en prenant pour référence, dans le premier cas, l'épaisseur relative des dépôts correspondants, dans le second, une moyenne des datations "absolues" concernant la période correspondante. (1973:628-31)
>
> [Trans.: The division of each glaciation into stadials and interstadials has been based upon precise criteria.

Thus, with one exception, we have reserved the term "interstadial" for major climatic breaks, during which the rise in temperature and the increase in humidity were sufficiently marked to favour a halt in sedimentation and to bring about a profound physical and chemical modification of the deposits. In shelters, interstadials are marked by true alterational soils, characterised by the presence of well individualised horizons of accumulation and the development of a structure within the deposit; in caves, the same phenomena have often culminated in the formation of stalagmitic floors.

A stadial and an interstadial represent a complete cycle, comprising a phase of sedimentation, a phase of alteration and a phase of erosion. This is the reason why interstadial soils, just like interglacial soils, are truncated soils, frequently with only the deep horizons of accumulation surviving.

Within the domain studied, and more particularly in shelters, an intrinsic feature of an interglacial or an interstadial is therefore not to be susceptible to demonstration by palynology, apart from rare exceptions; failure to remember this concept during the study of other quaternary deposits has, no doubt, been the cause of a certain number of stratigraphic errors.

On the other hand, the terms 'climatic oscillation' or 'phase' have been used interchangeably for periods of warming or cooling, of decrease or increase in humidity, within stadials; each oscillation or phase is itself split into pulsations or subphases which may correspond to its beginning, its development and its decline, but also to the succession of complex climatic events within the oscillation. The climatic optimum of a warming phase may have led to a brief halt in sedimentation; by our definition, it is only a stage within the oscillation in question, if the contemporary alteration was not followed by erosion phenomena.

With respect to terminology, for the sake of caution, we have deliberately ignored local names proposed by other researchers in France or abroad.
[...]

In the absence of an "absolute" chronological framework, we have not been able to represent as graphs the duration and relative intensity of the different oscillations which succeeded one another in the Périgord during the Riss glaciation.

The different oscillations are represented by intervals of equal width. Inside each of them, each deposit occupies the precise position which we believe we may assign it with respect to the three principal divisions of the oscillation to which it belongs (i.e. the beginning, the climatic maximum (or optimum), and the decline).

This also holds for the climatic evolution of the Early Würm and the Late Würm.

Nevertheless, we will try to trace the climatic curve of the last two periods by taking as reference points, in the first case, the relative thickness of the corresponding deposits, and in the second, an average of the "absolute" dates concerning the corresponding period.]

En matière de chronologie, nous avons mené nos recherches sans idée préconçue. Nous n'avons eu aucun mérite à retrouver les grandes subdivisions proposées par F. BORDES, refusant, pour le moment, tout essai de corrélation avec d'autres régions ou d'autres pays. (ibid., p.718)

[Trans.: Concerning chronology, we have conducted our research without preconceived ideas. We can take no credit for having rediscovered the major subdivisions proposed by F. BORDES; for the moment, we decline all attempts at correlation with other regions or other countries.]

This is all Laville has to say on the theory of his correlation scheme. Note that he does not even try to separate the concepts of correlation and chronostratigraphy. The approach may be summarised as follows:

(1) Deduction of a climatic sequence for each individual site, in such a way that the intensity of climatic variation, although on no absolute scale, is valid on a relative scale for the whole site.

(2) Recognition of interglacials and interstadials in each site purely on the grounds of strict definitions of the expected sedimentary effects of such climatic phases.

(3) Tagging of individual sequences to the correct glaciation(s) using any available data from all interested disciplines.

(4) Inter-site matching of climatic phases within stadials (methodology not discussed) in order to establish a composite regional chronostratigraphy.

(5) Rough estimation of relative duration of phases, based upon various types of data but including some form of "absolute" dating for the second half of the Würm.

It will be obvious to the reader, from many previous statements in the present text, that the author believes Laville's approach to be fundamentally invalid; if Laville has indeed followed his own 'rules' to the letter, then any proposed correlation will be correct only by sheer luck. The intricacies of climatic deduction will not be discussed again here. Rather, we will examine Laville's thesis and subsequent publications in order to obtain a better idea of how he went about inter-site correlation and how, if at all, his viewpoint has changed over the last few years.

Originally, Laville suggested that climatic variation within a site was on a relative scale that was valid for the whole of that site sequence. This meant that he would have theoretically been able to make such statements as 'Layer 1 is about twice as cold as Layer 20, irrespective of the climate prevailing during the intervening units'. Compare this position with a later statement:

> To the extent that they imply comparison to other periods (e.g.,"colder and drier"), the climatic designations attached to these periods in the figures are made with reference to the phase or subphase that precedes the one in question. (Laville et al. 1980:136).

The figures mentioned illustrate the sequence of climatic phases after correlation of individual site sequences. Why is it not possible to say that 'Phase 1 is about twice as cold as Phase 20' if it was originally possible to say 'Layer 1 is about twice as cold as Layer 20' in each of the sites that have been correlated to define the climatic phases? Why, until after the publication of his thesis (1975), was Laville happy to illustrate changes in temperature and humidity which, although relative, were still claimed as being on a valid ordinal scale for the whole of the Riss and Würm, whilst no claim for such precision is made in later publications (e.g. Laville 1976b, 1979; Laville et al. 1980)? If Laville no longer believes that there is a stable relative scale of climate even within individual sites (a conclusion to which Laville never explicitly admits, but which the present author finds logically inescapable within Laville's own frame of reasoning), what grounds remain for inter-site correlation?

Laville claims in all publications that interglacials and interstadials are recognised by a priori sedimentological characteristics (although it is painfully clear from reading Laville's work that these are in fact totally arbitrary a posteriori definitions). However, it has already been noted in the present text (p.472) that Bordes's 1953 loess sequence in the Paris Basin is in fact the framework used, not any empirical, iterative, or theoretical scheme based upon cave sediments.

> Bordes attributes the Older loesses to the Riss glaciation and the Younger loesses to the succeeding Würm. The Older series indicates that the Riss should be divided into three stadials, identified successively as Riss I, II and III, separated by two interstadials (that is, Riss I/II and Riss II/III). The Younger series in turn subdivides the Würm glaciation into four successive stadials. The sedimentological evidence for the fourth and last of these is considerably less well established than for its predecessors, and it is perhaps more likely to be found represented in certain eolian deposits in the Rhône Valley and the Provence area of southern France than around Paris itself. While the full loess sequence has not been independently established in the Périgord, stratified soil deposits are now being reported there which reveal exactly the same three-part cycle of eolian deposition, soil weathering, and solifluction and which seem to provide a direct analogue to Bordes' loess sequence. In any case, it is this sequence that provides the basis for defining the stadials which in Fig. 2.1. subdivide the Riss and Würm glaciations. (Laville et al. 1980:27)

The figure mentioned is entitled "Principal Geological and Archaeological Subdivisions of Stone Age Périgord [sic]" (ibid., p.19). A chronological scale is offered right back to 200ka, with the comment that dates may be in error by as much as 10%; no mention is made of the source of these dates and, to the present author's knowledge, no such detailed chronological evidence existed in 1980 in any site, or combination of sites, in the whole of France, let alone the Périgord. Claims that sequences of slope deposits in the Périgord prove the validity of the scheme are unsupported, since, unless some very recent information has become available unbeknown to the present author, there are no aeolian silt bodies in the Périgord, merely slope deposits containing derived silt components which may have had an ultimately aeolian source. In any case, the classification of slope deposits in this region, when it has not been based directly upon Laville's work, has itself been modelled, in an a priori manner, on Bordes's scheme. Given this rigid chronostratigraphic framework, the fact that

Laville recognises three stadials in the Riss and four in the Würm might arouse some suspicions concerning his claims that interstadials were objectively recognised. To give Laville his due, he recognised the Würm III/IV interstadial only under some duress (the 'one exception' noted in his general discussion, supra) since the cave phenomena eventually correlated with this episode did not fit his definition. Although never explained, the placing of this interstadial within specific cave sequences could only have been based upon the best available fit between the 'traditional' position of this phase within the regional archaeological stratigraphy and the more altered deposits in the cave sequences that were thought to span this general period. Basically, the interstadial had to be there and so it was found. It is interesting that, shortly after this interstadial, "we begin to navigate in a somewhat misty realm."(Laville et al. 1980:316).

In order to see how Laville's correlation method works, let us take a particular example, albeit one unsympathetically chosen by the present author for its patent weaknesses. The Würm II chronostratigraphy is based principally upon the complex sequences at Combe-Grenal and Pech-de-l'Azé I; both these sequences are capped by truncated 'palaeosols' which Laville would like to correlate with the Würm II/III interstadial, but neither of the palaeosols have significant overlying deposits, so the 'palaeosols' might be much younger. The Würm III chronostratigraphy is based upon a number of sites, including La Ferrassie and Les Jambes. Only at Le Roc-de-Combe is there an eroded deposit of cemented scree, with mousterian artefacts, that might have been modified during the Würm II/III interstadial (but there is no 'interstadial' that fits Laville's definition), before the emplacement of the Würm III (i.e. containing Upper Palaeolithic artefacts) sequence.

At the Lower Shelter of Le Moustier, the Perigordian (earlier Upper Palaeolithic) follows the Mousterian with no sign of an intervening palaeosol; indeed, there are not even any traces of major erosion and the two deposits are lithologically very similar. There is therefore no example of the Würm II/III interstadial phenomena which would fit Laville's strict definition and also demonstrably occupy the right chronostratigraphic position- until we consider L'Abri Caminade, that is. Now, this site contains deposits with mousterian artefacts; these deposits have been modified by a (truncated) 'palaeosol'. Above the 'palaeosol', there was a layer (destroyed before Laville examined the site) that contained two Châtelperron points (i.e. this layer is said to represent the earliest Upper Palaeolithic), and then two more layers (seen by Laville) containing Aurignacian I. As long as it can be proven that the mousterian levels may be referred to the Würm II, the 'truncated palaeosol' will provide a demonstration of the existence of the Würm II/III interstadial (always assuming of course that the artefacts are contemporary with the deposits in which they were found, a complication which Laville never sees fit to consider), because, although Laville has 'no preconceived ideas concerning chronology', an interstadial lying between deposits of the Würm II and the Würm III must be the Würm II/III interstadial - must it not? Speaking of the mousterian layers, M1-3, Laville argues:

> Privés de leur contexte stratigraphique, ces dépôts ne pourraient être datés, a priori, que du Würm ancien sensu lato, les industries du Moustérien typique, de type Quina et de type Ferrassie ayant évoluées, selon F. BORDES, simultanément aux autres groupes moustériens, pendant les deux premiers stades de la glaciation würmienne.
> Un certain nombre d'arguments concourent cependant à dater ces dépôts du Würm II.
> a - Ils sont surmontés par des formations contenant des industries aurignaciennes, dont on sait qu'elles se sont développées au cours du Würm III.
> b - Ils en sont séparés, chronologiquement parlant, par une phase de

pédogénèse, dont les effets ont profondément modifié la texture originelle du sédiment. Le paléosol correspondant peut donc être daté, en toute certitude, d'un interstade majeur de la glaciation würmienne, en l'occurence, de l'interstade Würm II - Wurm III.
c - Les manifestations d'érosion, dont le sommet des dépôts moustériens portent l'empreinte, ont detruit les horizons superficiels du paléosol; elles ne peuvent donc être que postérieures à la phase de pédogénèse et n'ont pu, par conséquent, affecter que des dépôts édifiés avant l'interstade Würm II - Würm III.
Aucun élément ne laissant supposer qu'il y ait eu arrêt de sédimentation pendant tout la durée du Würm II, c'est donc à ce stade que l'ensemble des couches M1 à M3 doit être attribué. (1973:361)

[Trans.: Deprived of their stratigraphic context, these deposits could only be dated, deductively, to the Early Würm, sensu lato [? redundant phrase], industries of Typical, Quina and Ferrassie Mousterian having evolved, according to F. BORDES, simultaneously with the other mousterian groups, during the first two stadials of the Würm glaciation.
However, a certain number of arguments converge to date these deposits to the Würm II.
a - They are overlain by formations containing aurignacian industries, which are known to have developed during the Würm III.
b - They are separated from the overlying deposits, chronologically speaking, by a phase of pedogenesis, the effects of which profoundly modified the original texture of the sediment. The corresponding palaeosol may therefore be dated, in all certainty, to a major interstadial of the Würm glaciation, in this case, the Würm II/III interstadial.
c - The manifestations of erosion, the signs of which are seen at the top of the mousterian deposits, destroyed the upper horizons of the palaeosol; these manifestations of erosion can therefore only postdate the phase of pedogenesis and, consequently, could only have affected deposits formed before the Würm II/III interstadial.
Since there are no factors which might suggest that there was a break in sedimentation at any point during the Würm II, it is therefore to this stadial that all the layers M1 to M3 should be attributed.]

Oof! One could not wish for a finer example of circular argument. Even Laville seems a little embarrassed since he tries to bolster his reasoning by matching the climatic oscillations recognised at l'Abri Caminade with those in other Würm II sites. Thus the three mousterian layers at Caminade represent six discrete climatic phases, evidenced at Combe-Grenal by twenty-seven different layers. The present author wonders whether Laville would be prepared to make a public statement to the effect that he recognised the climatic oscillations at l'Abri Caminade before he became aware of the pressing need to prove that these deposits date from the Würm II. Note that this complex Würm II correlation is maintained (Laville et al. 1980, Fig. 7.11.) even though l'Abri

Caminade has mysteriously changed its name to "La Micoque".

Laville also notes signs of the Würm II/III interstadial at two other sites.

> La couche 6 de la galerie principale de la grotte de Font de Gaume, que nous avons attribuée au remaniement du plancher stalagmitique sous-jacent, est contemporaine de cette phase de ravinement. (1973:668)
>
> [Trans.: Layer 6 of the main gallery of the cave of Font de Gaume, a layer which we have attributed to the reworking of the underlying stalagmitic floor, is contemporary with this phase of channelling [erosion].]

The stalagmite is older than the Würm II/III interstadial and Laville is thus suggesting that an interstadial can be reduced to a mere phase of erosion (cf. the present author's comments on debris flow at this site, p.389).

> Signalons enfin que le remplissage de la grotte-abri de Tourtoirac, actuellement en cours d'étude, constitue la seule séquence où le paléosol de l'interstade Würm II - Würm III subsiste à peu près complet. Il affecte le sommet du remplissage moustérien et est immédiatement surmonté par des dépôts à industries du Paléolithique supérieur. L'accroissement de l'humidité qui, ailleurs, a entraîné le ravinement des dépôts moustériens et celui du paléosol, en fin d'interstade, s'est manifesté ici par une reprise d'écoulement du karst qui prolonge l'abri. L'arrivée d'eau a favorisé le lavage des dépôts sous-jacents au sol d'altération: à ce niveau, le remplissage, composé d'éboulis cryoclastiques volumineux, est lacuneux et privé de sédiment interstitiel; seul un enduit argileux subsiste au pourtour de ces éléments et des silex qui leur sont associés. (1973:668)
>
> [Trans.: Finally, note that the fill of the cave/shelter of Tourtoirac, at present under study, constitutes the only sequence where the Würm II/III interstadial palaeosol survives more or less complete. It affects the top of the mousterian fill and is immediately overlain by deposits with Upper Palaeolithic industries. The rise in humidity, which elsewhere caused the channelling of the mousterian deposts and of the palaeosol at the end of the interstadial, here took the form of renewed flow in the karst which leads off from the shelter. The arrival of water favoured the suffosion of the deposits underlying the alterational soil. At this level, the fill, composed of coarse cryoclastic scree, contains voids and is deprived of interstitial sediment; only a clayey coating survives on the surfaces of these elements and on the flints which are associated with them.]

It would appear that Laville is suggesting that the intact palaeosol (with horizon differentiation?) remained perched above some sort of suffosion, or even elutriation, feature in the underlying deposits. No further details concerning this site are given. In any case, here is a perfect example of Laville's definition of an interstadial. Again with no further details, Tourtoirac is mentioned in this context in Laville (1975) and (1976b). Why is there not a single word about this site in Laville et al. (1980)? Since the difficulties with the Würm II/III interstadial are now, if anything, more apparent, we must assume that Tourtoirac does not in fact provide the information suggested earlier.

We must now consider Laville's concept of phase duration. Little can be said about the earlier periods, although one wonders how the equation between a single scree body at Pech-de-l'Azé II and each and every one of the seven phases of the Riss III stadial can be justified (Laville 1973, foldout, p.643/4). It will be remembered that Laville suggested that phase duration in the Würm III and IV stadials could be approximated using unspecified absolute dates. These dates remain unspecified throughout the 1973 and 1975 texts, since there is no reference to radiocarbon or other results, although footnotes in the latter text suggest that the correlation scheme is under pressure from conflicting 'chronometric' data. By 1980, a larger set of radiocarbon dates has become available. The question now is not so much concerned with phase duration as with whether or not the correlation scheme can be salvaged. During the Würm III and the beginning of the Würm IV, only two sites, the Abri Pataud and Laugerie-Haute, provide series of dates. These sites, which together cover all the fourteen phases of the Würm III and the first three phases of the Würm IV, only overlap in one phase (plus one subphase) and, if one ignores several dates that are "too young" from the top of the Pataud sequence, the radiocarbon chronology appears consistent with the original chronostratigraphic scheme. However, some other radiocarbon dates from Le Facteur and La Ferrassie are dismissed as obviously polluted, since they suggest that the correlation may be in error by as much as c.2,500 years. Moving up into the later part of the Würm IV, difficulties increase (with the increasing number of available radiocarbon dates) and Laville and his colleagues are forced to recognise "core sites" and "problem sites". The former comprise La Madeleine, Gare de Couze (an addition to the 1973 list) and Faurélie II. The sequence from La Madeleine is more or less intact (compared with 1973), although the top layers, which formerly represented three phases, have had to be compressed into a single phase in order to remove magdalenian industries from the Holocene, and an erosion surface has had to be opened up in the middle of a layer in order to incorporate most of the Gare de Couze sequence. The sequence at La Faurélie has been compacted from six to four phases. These three sites "provide a congruent basis for designing a later Group III [Würm IV] scheme, ..."(Laville et al. 1980:317). The problem sites comprise Le Flageolet II and Pont d'Ambon, the latter being another addition since 1973. The top of the Flageolet sequence has had to be moved back four phases, layers which once represented three phases have had to be compressed into one, and the base of the sequence is now placed two phases earlier than originally thought. At Pont d'Ambon a rather erratic set of radiocarbon dates has allowed Laville to recognise a major 'cryoclastic' scree body, dating from the last cold phase of the Würm, despite the fact that Delpech (1979) has identified a forest fauna from this <u>and</u> older layers, which she allocates with certainty to the Holocene. Other sites (Le Cap Blanc, Laugerie-Basse), originally correlated with the later Würm IV scheme, are not discussed at all in Laville et al. (1980), presumably because even the changes with respect to the "core sites" would make correlation even more ludicrous. The final assessment is as follows:

> Here is found the only segment of our scheme in which there appear significant discrepancies, and indeed contradictions, among the different lines of evidence that hitherto [concerning earlier periods in the scheme] we have seen behave in a fashion that is either highly congruent or at least explicable. New information will be required before the issue can be resolved, particularly since it is by no means clear how much the discrepancies are variously to be attributed to problems arising from the quality of the archaeological and natural data themselves, the manner in which they have been analysed and interpreted, and significant errors in the available radiocarbon dates. Again, however, we seem to be dealing with archaeological industries that may not line up in the simple, straightforward temporal succession that traditional systematics attributed to them. (Laville et al. 1980:348)

The present author could not agree more with this statement. It is the rider tagged onto the statement which is laughable:

> In any case, it bears stressing that the difficulties we encounter in this final segment of the Périgord sequence are not of such a nature as to bring into question the results that have been obtained for earlier time periods or to cast doubts upon the basic validity of the assumptions and methodology upon which our approach is based. (ibid.)

Finally, one may comment upon Laville's disinclination to correlate his phases with other stratigraphic schemes in other parts of France and Europe, a position which is restated in most of his publications. Such caution is admirable but why, having decided that long distance correlation is dangerous, did he go ahead and do it just the same: by linking Combe-Grenal with l'Hortus (in de Lumley 1972 : 353); by linking the Dordogne with the Gers, Charente, Basses-Pyrénées, Lot, Lot-et-Garonne, Gironde, Corrèze, and Landes (Laville 1976b); and by linking the Périgord with the North European pollen zonation (Laville 1979)? This last correlation is particularly amusing since the Dryas II cold phase is said to start some 800 years <u>earlier</u> in the Périgord than it does in northern Europe, a miscalculation which did not escape certain criticism at the conference in question.

This long description of Laville's correlation methods is included here for two reasons. First, any archaeologist working with the European Palaeolithic must sooner or later encounter this scheme or others like it. What is of greater importance, however, is the fact that, even if the climatic attribution for each layer in each site is allowed, Laville still ignores the most basic property of the lithostratigraphic record everywhere - its incompleteness. Again and again, Laville describes abrupt boundaries in his sequences and, again and again, he claims there is no evidence for discontinuity. It does not even appear to worry him that he sometimes needs to recognise erosive gaps within previously defined layers in order to make his correlation scheme work, a practice which is not restricted to the 1980 publication but which is already apparent from a comparison of some early site reports and the later, 1973, scheme. All that Laville is prepared to admit is that there is less detail as one goes back in time. The entire period from 200ka to 10ka is said to be represented by sediment, except for interglacial and interstadial phases which, for some perverse reason, must not, by definition, be represented. Laville and his colleagues state:

> For example, assuming our radiocarbon frame is trustworthy in its general outline, the average oscillation that is given phase definition in the Group II scheme [phases 1 to 9 of the Würm III] would have endured a full millennium, and even subphases might individually represent several centuries of time. (1980:349-50)

Laville's phases have somehow taken on the nature of objective time units that exist without reference to sediments. This is even more clearly suggested by the fact that phase 9 of the Würm IV, for instance, is still there, just as cold as it has always been, despite the fact that the sediments now placed in this phase are different and include material that used to be 'temperate'. Laville does not seem to understand that, if he moves a single layer, his scheme, as he has constructed it, automatically founders because the climatic phases must remain rigidly confined within the chronozones of the sediments from which these phases were originally deduced. It is the chronozones which are objective time units, whether or not they are explicit (dated), not the climatic phases.

Inter-site correlation should be attempted only with full recognition of the nature of the problem. What we have in a site sequence is a vertical column of lithostratigraphic units, each of which represents a finite chronozone. Assuming that time-transgressive lithostratigraphic units will not introduce significant difficulties in this context, all we know initially about the chronozones is that they must retain their relative stratigraphic positions and that they do not overlap; we know nothing of their age or duration, nor how much time is unrepresented by sediment. Given the likelihood that some time will be missing at every abrupt boundary, and that we may sometimes miss or underestimate a sedimentary hiatus, we dare not assume that a truly continuous chronostratigraphy can ever be constructed, no matter how many separate sequences we try to correlate. This is why boundary stratotypes (cf. Hedberg 1976) are so important in the Quaternary. It seems to the present author that the best way to go about correlation is, in fact, to use the 'null hypothesis' approach. Stratigraphic correlation, in the widest sense, is all about constructing a composite sequence. What we should be trying to do is to prove that individual lithostratigraphic units and their chronozones, in two sites, are <u>not</u> correlative, that one of the units postdates the other, a procedure which, intuitively, should be easier, and more in keeping with the nature of the record, than attempting to recognise true correlations from the outset. If we succeed in this 'sequential' approach, we have set up a useful chronostratigraphic (but not correlative) link, which is not dependent upon being tied down to an absolute chronological scale. A given unit at site A will be demonstrably older than some units at site B, and demonstrably younger than other units at site B, with a variable number of 'don't-knows' in the middle. The sequential information that will be necessary to make such decisions will be provided by any and all disciplines that can contribute sound arguments concerning relative chronology, arguments that must be valid for a comparison of two specific units. It will not be possible, or necessarily desirable, to use the same class of criteria for all comparisons. When everything has been done to recognise the relative order of units in two sites, a number of units in either site will probably still be unplaced (with respect to a precise position in the composite stratigraphy), in such a way that they might be truly correlative with certain units, parts of units or groups of units in the other site. It is at this point that one must carefully define what one means by 'correlative'. Are a pair of units indicative of a well characterised regional sediment body and may they therefore be considered as 'marker horizons'? Are a pair of units the result of some 'event' which caused a similar, or alternatively a disparate, sedimentary response in the two sites? Do the two units contain identical faunas and, if so, what degree of proximity in time does this imply? Do the two units have similar 'chronometric' dates and how wide is the temporal range in which the 'true' ages of both dated samples and, hopefully, both units most probably fall? If given units still refuse to show either sequential or meaningful correlative relationships with units in another site, we must let them be. There is no point in forcing them into dubious relationships or into pigeon-holes in some rigid stratigraphic scheme based on data totally external to the sites in question. As more sites are compared, some of the formally unplaced units will be brought into more precise relationships and new unplaced units will be included (with a floating status) to await further information in their turn. Mistakes are sure to be made from time to time, but the approach described here is more likely to result in a scheme that will not collapse every time a few changes are needed. It is important to realise that both correlative and sequential relationships must be recognised between pairs of units, each unit having been judged to be free of significant gaps. Patterns of change in either measured or inferred parameters (such as Laville's climatic attributions) that show up in two sequences of strata may well be suggestive (as long as any inferred parameters are themselves valid, unlike Laville's climatic attributions) but they do not constitute specific temporal links between the units in the two sequences. This is particularly true of parameters that oscillate through time as opposed to parameters that follow a more steady unidirectional trend. The recognition of more or less continuous sedimentation, on the one hand, and physical gaps, on the other, is a difficult and rather subjective stage in the process of lithostratigraphic assessment but, without this stage, relatively secure inter-site correlation is impossible.

It is a sad fact that quaternary correlation, as it is usually practised today all over the world, has largely degenerated into a synthetic exercise, often undertaken by individuals who have not even seen some of the sites in question, let alone actually worked in them. Correlation, just like the stratigraphic techniques it replaces, should be based upon fieldwork and tested by fieldwork. If one is forced to attempt correlation when a sequence is not physically available for study, one must take into consideration the fact that the original researchers (whether or not the individual now attempting correlation was one of these researchers) will have collected and published information about that site, and not about the way in which one layer from that site compares specifically with one layer from another site. Such correlation will necessarily be on a less precise level than that which may be achieved by conscious comparison of physical sequences. Why else does every stratigraphic guide ever written stress the importance of the conservation of stratotypes?

The present author is of the opinion that secure correlation may only be achieved by a group of specialists, representing as wide a range of interests as possible, who are not merely writing a synthetic text or the last chapter to an individual site report, but who have consciously undertaken a programme of correlation involving fieldwork. The author has not yet had the opportunity to take part in such an exercise and he is not aware of any published account of such an exercise involving cave sites. It is therefore not possible to cite an example of a secure and detailed correlation scheme. The author considers this last statement to be a fact, not a pretext designed to avoid further discussion or to protect himself from the sort of criticism he has lavished upon Laville. Hopefully, all potential critics will soon get their chance. Nevertheless, in Part II of the present text, an attempt will be made to indicate at least the most probable general relationships between sites in given regions, and to point out some of the factors which seem most promising with respect to eventual correlation.

18. DATING METHODS

A lithostratigraphic or biostratigraphic unit is a physical reality, consisting of a stratum, of a set of strata or of a defined component of a stratum or set of strata. The time during which a lithostratigraphic or biostratigraphic unit formed is its corresponding and unique chronostratigraphic unit, which has no reality if divorced from the defining physical unit (cf. Hedberg 1976). This concept may be generalised to the proposition that every physical phenomenon corresponds to a formative event. This proposition is absolute and is not dependent upon any sort of qualification or quantification of time itself. However, the ultimate aim of chronostratigraphy, and of any other chronological study, is indeed to quantify, as precisely as possible, the duration, antiquity and general temporal interrelationships of the formative events which produced the observed physical phenomena. By far the most reliable techniques for achieving this aim are stratigraphic methods, which produce high quality sequential information. By far the most unreliable techniques are the group of procedures referred to collectively as 'dating methods', although such methods may often be the only way to produce information concerning duration, antiquity and even, in some cases, sequence. Stratigraphic methods rely upon simple applications of actualistic principles, an approach which, on empirical grounds, can be shown to achieve great accuracy. Dating methods rely first upon stratigraphic methods, since the likely temporal significance of any sample to be dated must first be assessed, and second upon a series of more or less plausible assumptions about the nature of long term temporal changes in the physical world. This is why stratigraphic methods were discussed in Chapter 5 of this text, whilst the discussion of dating methods has been left to the present chapter. It is the author's opinion that dating methods, which already have sufficient drawbacks with respect to the necessary method-dependent assumptions, are further weakened, in practice, by the common underestimation of their inescapable dependence upon stratigraphic methods, especially with respect to ambiguity concerning the definition of the exact formative event that has been dated. These problems will be discussed below. Whether or not dating methods are considered to be problematical, it is nevertheless certain that they are crucial, both because the actualistic approach affords a very poor estimate of process rate and also because the physical stratigraphic record is disjointed and seriously incomplete.

The traditional division of dating methods into such categories as 'relative', 'chronometric', 'radiometric' or 'absolute' is not particularly useful and may often be misleading. Dating methods are best divided into two broad classes: (1) those methods which identify the state of a phenomenon as it was fixed at the time of the formative event and (2) those methods which assume an initial state that has changed, according to some temporal trend, since the formative event. These two classes will be referred to as <u>static</u> and <u>dynamic</u> methods, respectively.

The first set of static methods monitors systems that are assumed to undergo irreversible changes through time, but which are composed of elements that do not themselves change once formed. Evolutionary changes in biological taxa are the most commonly used criteria, with sequences of archaeological cultures providing a rather more questionable example. The difficulty here lies in the identification of those parameters which are indeed 'evolutionary'. Such methods may be extended, at least in the biological context, to consideration of the points of appearance and disappearance of particular taxa in regional sequences, which is more a question of biogeography than evolution. Other static methods monitor systems, which are again composed of elements that do not themselves change once formed, but which involve system changes through time that are at least theoretically reversible. The study of biological assemblage changes, on a shorter scale than evolutionary changes, is such a method. Geological methods include palaeomagnetism and the study of oxygen or carbon isotopes. Rhythmic phenomena, assumed (sometimes incorrectly) to represent annual cycles, such as tree-rings or varves, may sometimes give an idea of duration (producing 'floating chronologies', if they do not continue up to the present). It is not the actual state of a system which is used to date material using these methods but, rather, the sequences of states (assumed to be uninterrupted over a significant span of variation) that may be diagnostic of particular periods. In cave research, static dating methods involving biological material are commonly used (cf. Chapter 14), as, unfortunately, are often shaky archaeological 'seriation' methods (cf. Chapter 17). Stable isotope studies (cf. Chapter 13) have not been developed as a dating technique in caves because the material of interest is usually also susceptible to dynamic dating techniques. Palaeomagnetism is a subject which may prove useful (cf. Creer & Kopper 1970) but which, at present, suffers from a lack of detailed intensity, inclination and declination data for most of the Pleistocene. So far only very coarse variations (reversals and major excursions) have been widely recognised, although even these may be informative. For instance, the deposits at La Belle Roche (Belgium) contain an early Middle Pleistocene rather than a late Lower Pleistocene mammal fauna, a proposition which is supported by the fact that associated water-laid sediments show normal polarity (pers.comm. J.-M. Cordy) and are thus less than c.700ka old. Latham and Schwarcz (reported in Cook et al. 1982) also noted normal polarity at Vértesszöllös (Hungary), but they were unable to obtain a reading at Petralona (Greece) because of a strong viscous component of magnetism (due to materials that react to the earth's magnetic field within the normal range of environmental temperatures). In addition to formal static methods, any 'event' that can be recognised with certainty in several sites may be considered as an 'event or marker horizon' (cf. Chapter 17).

It is the dynamic methods which provide the most promising techniques for improved dating of cave and other quaternary sites. Radiocarbon and uranium series dating are similar in their initial assumptions. First, the isotopic composition is considered to be set at the time of formation and, second, the present isotopic composition is considered to be due to subsequent radioactive decay in a closed system. Neither of these assumptions is beyond suspicion; indeed, secular variation in atmospheric radiocarbon is known to occur, although there is little that can be done at present to compensate for this effect in pleistocene materials. Radiocarbon dating is most commonly applied to bone, and if possible to the organic component rather than to the total sample (cf. Burleigh 1972), although shell and speleothems may also be dated but with considerably less reliable results. The use of microsamples of purified substances in conjunction with accelerator mass spectrometry (cf. Stuiver & Kra 1983, section VIII, pp.677-792) will allow much more exact identification of what is actually being dated, as soon as these techniques become generally available. U-series dating is best applied to speleothems (cf. Schwarcz 1980; Hennig et al. 1983). Dates on bone would not seem to be valid, given the present imperfect understanding of the chemistry involved, even though some generally plausible dates have been obtained (e.g. at La Caune de l'Arago, Bischoff & Rosenbauer 1981). Indeed, it would seem to the present author that U-series dating on bone or shell will never be valid, except perhaps in extremely stable cave environments in more arid regions where there may be a better approximation to a closed system.

The second set of dynamic methods assumes an initial 'zeroed' state which is then modified in an open, but hopefully predictable, system. These methods include the quantification of thermoluminescence (cf. Aitken 1974) and electron spin resonance (cf. Ikeya 1978) in crystalline substances. Zeroing may be achieved by heating of an existing substance to relatively high temperatures, by bleaching in sunlight or by formation of a new substance. The techniques are not valid for the dating of material that has changed position relative to the context in a zone about 30cm around the sample (i.e. reworking of the sample itself, or the close approach of an erosion surface) at a time significantly later than the initial zeroing; the techniques do not include intrinsic methods that will always identify such disturbance. Since the effects measured are the result of the input of radiation from the surrounding context, the water content of the context at all times since zeroing is of paramount importance because of the shielding effect of water. Given relatively high porosity in the context (which may have changed due to compaction, infiltration, cementation, etc.), published 'dates' may vary by up to 20%, depending merely upon the 'average' water content chosen for the calculations. Note that the water effect is not included in (is additional to) the blanket 10% error bracket usually published with such dates. Apart from pottery, TL dates have been produced for burnt stone (e.g. l'Abri Suard, Charente, Schvoerer et al. 1977; Pontnewydd Cave, Green et al. 1981) and speleothems (e.g. Pontnewydd Cave, Debenham, Aitken, Walton and Winter in Green et al., in press), and it may prove possible to extend the technique to some other types of sediment (e.g. direct dating of aeolian deposits; cf. Wintle & Huntley 1982). ESR dating has been attempted on both bone and speleothems (e.g. Petralona Cave, Greece, Hennig et al. 1981). There are considerable problems with both TL and ESR dating (especially with the latter, which many authorities consider to be theoretically invalid as a dating technique) and there has been much criticism in the professional literature (cf. Wintle 1978; Wintle & Jacobs 1982).

The last set of dynamic methods assumes an original state which is then modified, not by a generally environment-independent factor such as radioactive decay, but by variable components of the environment itself. Such methods include rates of bone 'fossilisation' (cf. Oakley 1969) and amino acid racemisation (cf. Bada & Helfman 1975). Bone 'fossilisation' is perhaps less popular now than when fewer dating techniques were available, but attempts are still made to solve specific problems (e.g. Pontnewydd Cave, Molleson in Green et al., in press). AAR techniques have been used on bone (e.g. Petralona Cave, reported in Stringer et al. 1979) and shell (e.g. Minchin Hole, Andrews et al. 1979). Most interested parties seem to be well aware of the problems involved in these techniques, although there would not yet appear to be sufficient recognition of the fact that, even with AAR, the effects of interest are by no means irreversible (cf. Hare 1980).

Some of the techniques mentioned above, especially those dynamic methods which should involve closed system radioactive decay, promise to attract the attention of quaternary specialists in general back to caves (cf. p.567). Very few open-air sites have such dating potential. However, in the present author's opinion, this potential is being wasted. The following comments are couched in terms of the dating of speleothems and are based upon observations at the sites in which the author has worked or which he has visited, upon discussions that the author has had with dating specialists, and upon published discussions in the literature. The names of individual sites and researchers are irrelevant, since the problems seem to be more or less universal. Some of the criticisms voiced here may seem unreasonable, since the dating experts are clearly aware of many of these factors and have often discussed them in print. However, the present author is not primarily concerned with what should happen in theory, but with what to his certain knowledge is happening in practice.

The dating of a speleothem suggests an age for the formation of that speleothem and no other information, not even sequential information about objects that lie below, within or above that speleothem. Any sequential information is supplied solely through stratigraphic methods, applied before the sample for dating is removed from its context. Many cases have arisen in which the stratigraphic relationships between a dated speleothem and the surrounding deposits, or some object of particular interest, are unknown, whether or not this has been admitted in the literature. A speleothem has a dual identity, in that it comprises both a morphological expression (a sediment body) as well as a population of individual mineral crystals.

Dating techniques measure the age of the latter not of the former, but it is the age of the morphological expression which is of interest, and any date is usually considered, always erroneously but sometimes significantly so, to refer to that expression. An extreme, but sadly not a theoretical, example of such error would be the dating of a carbonate breccia, involving clasts of 'old' stalagmite set in 'younger' calcite; the clasts might be carefully extracted and dated (perhaps because they are better crystallised than the cement) and the date incorrectly applied to the morphological expression of the breccia bed itself. Such disparate composition is obvious to the naked eye, but similar problems occur at a much smaller scale. Calcitic material (as tiny clasts of speleothems, limestone or vein calcite) may be included that is considerably older than the sediment body, as may be younger crystals which have grown within the sediment body. Younger crystals affect different dating techniques in different ways. Crystals formed from infiltration of new carbonates will 'decrease' both the TL and U-series ages, but neomorphism will affect only the TL age. None of these effects, involving either older or younger calcitic material, appear to be consistently recognisable using the techniques intrinsic to the dating methods. Only microscopic 'stratigraphic' examination of the samples before dating, using detailed petrographic techniques, can identify all such effects. Although such microscopic examination is often advocated in synthetic texts (cf. Schwarcz 1980), it is not used routinely and there are very few published descriptions of even the gross petrographic features of real samples, let alone detailed descriptions accompanied by illustrations of thin sections. Other forms of 'contamination' (e.g. due to isotopic fractionation during re-solution of calcite or incorporation of non-carbonate material containing U-series isotopes) can usually be identified by techniques intrinsic to the dating methods (granted that TL dating is always accompanied by the same checks that are used for U-series dating), and the distribution of radioactive sites may even be examined using fission-track and other methods (cf. Wintle 1978; Walton & Debenham 1980). Oxygen isotope studies may also be used to investigate the integrity of the calcite structure. There is a disquieting tendency for rigorous checking to be applied after the dating, especially in those cases where the 'date' does not agree with the expected age. There is also a tendency for an age to be accepted if similar 'dates' can be achieved by two or more methods, such as U-series, TL and ESR, despite the fact that it is patently obvious that most 'contamination' will affect all such techniques in similar ways. Another worrying practice often occurs with dates from fragmented speleothems included in clastic deposits. The speleothem is dated and then allocated a chronostratigraphic position according to the date. This also produces a *terminus post quem* for the whole clastic deposit. Thus far, this procedure is acceptable. However, such dates have been used to override, in a most peremptory manner, good stratigraphic data from other sources, without any attempt at correlation of speleothem clasts by parameters other than their 'dates' (e.g. microfossil content, heavy and clay mineral content, trace element content, matching of speleothem microstratigraphy, etc.). The most common response to 'problem sites' is not a co-ordinated research project, involving careful re-examination of all assumptions and physical evidence in order to identify the actual (not just the possible) interference; rather, the response is 'saturation dating', using as many different methods as possible (and often different personnel who make no attempt to communicate with each other), an approach which often leaves even the dating specialists in a state of bewilderment, let alone other interested quaternarists. When a series of dates have been determined on a single speleothem, they may be combined to identify the 'true' age. Regression methods to compensate for such factors as thorium contamination may well be valid, but simplistic averaging is not. Consider a theoretical example of two dates from the same speleothem of 290±19ka and 10±1ka. Substituting these contrived figures, situations have arisen where the 'true' age would have been expressed as 150±10ka, a figure which corresponds to no meaningful formative event and which is associated with a spurious indication of its accuracy. The 'ludicrous' theoretical figures used here represent a situation which is only about four times as extreme as the worst real example known to the author. There are only two possible reasons why any two dates (and their error brackets) are not identical: (1) variation in the performance of the equipment, reagents and personnel involved in the dating; (2) the two samples are not identical. Since it is to be hoped that the first factor will not introduce significant uncertainty, the second factor must be held responsible. The average number of peas in a hundred pods is a meaningful statistic. The average of a number of dates on different pieces of speleothem, whether they come from the same level of the same sediment body or not, is not meaningful. It is already sufficiently problematical that even a single date represents a weighted average of the formative events that gave rise to every crystal in a sample. In fact, the term 'sample' is responsible for much misunderstanding, since even the specialists often seem to confuse the target population (the sediment body) with the available population (a diachronous collection of crystals).

The dating of stalagmite (indeed, the dating of most substances) requires considerable expertise in stratigraphy, petrography, geochemistry, geophysics, and statistics. This expertise is required for every single date, not just for the initial formulation of techniques. Even granted that an individual or a whole research team commands such expertise, the present author has never once encountered a situation in which that expertise was actually brought to bear on the problems at hand in a really co-ordinated and logical manner.

Schwarcz lists a series of criteria that should be used to select speleothem samples for U-series dating and to ensure the accuracy of the date. All these criteria are patently valid, although the present author could think of a few more stratigraphic points which could be usefully added. Schwarcz appends the following comment to his list:

> These are only suggested criteria, and should always be taken _cum grano salis_. (1980:13)

If a concerted effort is not soon made to apply, on a routine and rigorous basis, such criteria as those suggested by Schwarcz, it is not the criteria but the 'dates' themselves which will need to be taken _cum grano salis_.

19. SYNTHESIS OF RESULTS

19.1. Multidisciplinary Research

When data on the sediments of a cave have been collected and digested, and when the researcher has put forward his hypotheses, duly supported by references to other relevant sites or technical publications, it might be thought that his work is complete. Study of a cave site is today a multidisciplinary affair. The term 'multidisciplinary' is here defined simply as 'involving specialists from various disciplines'. Thus, we might generalise and say that it might be thought that the work of each and every specialist is complete once he has finished discussing his own speciality.

Sadly, such insular thinking is all too often adopted. This point is best illustrated by a detailed example.

19.2. A Case Study: The Multidisciplinary Study of a Cave Site - L'Hortus (Hérault)

The site of l'Hortus has been chosen as an example of what often happens when a large group of specialists comes together to analyse a single sequence. The publication, edited by de Lumley (1972), from which are taken all the extracts quoted in this section, contains fifty chapters, written by thirty-six different authors.

The cave of l'Hortus is quite an extensive system, formed in Lower Cretaceous limestone, at an altitude of 390m O.D. One entrance to the system, where the majority of the deposits investigated were found, faces almost due south, with extensive vertical cliffs, but no feeder slopes, above it. Sequences of deposits, starting at the end of the Riss and continuing right into the Holocene, have been identified. Most of the artefacts are mousterian, but there are traces of many later cultures, even as late as the Early Christian period.

A full appreciation of this site is beyond the scope of the present work. Let us instead concentrate upon one reasonably representative part of the sequence: Sub-Phase IVB of the Würm II (Layers 21A to 18). These deposits were found c.2m down in a closed transverse rift ("le grand fossé") in the floor at the main entrance to the cave. They were present only in about three square metres, to a thickness of c.0.6m (i.e. c.10% of the whole Würm II sequence), and they have been totally excavated. Several living floors were identified, with Typical Mousterian rich in denticulates. The deposits, and the chronozone based upon them, seem to be rather important as they are said to represent a major turning point in the development of the physical and biological environment:

> La relative abondance de blocs et de cailloutis anguleux de grandes dimensions correspond à un climat très froid; la raréfaction du pourcentage des éléments argileux allochtones indique un assèchement du climat qui a été confirmé par les analyses polliniques: disparition des arbres. (de Lumley, p.67)
>
> [Trans.: The relative abundance of blocks and of angular scree of large calibre corresponds to a very cold climate; the depletion in allochthonous colloidal elements indicates a drying of the climate which has been confirmed by pollen analasis showing the disappearance of trees.]
>
> Pendant cette période, le climat est resté très rigoureux: abondance de blocs et forte proportion de cailloux de grandes dimensions. Il est cependant possible de mettre en évidence des variations dans l'intensité du froid: rigoureux dans la couche 21A, le climat devient encore plus

> froid dans la couche 20 ... et il se radoucit ensuite progressivement en se rapprochant de l'inter-phase IV-V ....
> Les éléments allochtones entraînés par le ruissellement .... Le faible pourcentage de ces éléments dans les niveaux de la sous-phase IVB, témoigne d'un climat aride.
> L'augmentation du pourcentage des polyèdres et des prismes permet de penser que la végétation ne devait plus être suffisante pour stabiliser les éboulis de pente. En effet, les résultats de la palynologie ont révélé à partir de la couche 21A, une disparition des arbres et l'installation d'une steppe à Composées et à Graminées. (Miskovsky, p.140)
>
> [Trans.: During this period, the climate remained very harsh: abundance of blocks and high proportions of large calibre scree. However, it is possible to demonstrate variations in the intensity of the cold: harsh in Layer 21A, the climate becomes even colder in Layer 20 ... and it then becomes progressively milder towards the Inter-Phase IV-V ....
> Allochthonous elements brought in by stream [or wash] action .... The low percentage of these elements in the levels of Sub-Phase IVB bear witness to an arid climate.
> The rise in the percentages of polyhedrals and prisms [shapes of limestone clasts] allows the inference that the vegetation could not have been sufficient to stabilise talus on [exterior] slopes. Indeed, the results of pollen analysis show, starting from Layer 21A, the disappearance of trees and the establishment of steppe with composites and grasses.]

The climatic reconstruction of this period therefore seems quite straightforward and unequivocal. In the initial description of the individual strata, de Lumley sets the tone, and Miskovsky fills out the reconstruction from sedimentological evidence. The pollen results of Renault-Miskovsky represent the only other data brought in at this stage.

> A partir de la couche 21$_A$D nous assistons donc à une disparition quasi totale des principaux arbres; ....
> Le reste du paysage est constitué par une steppe à Graminées et à Composées ....
> [...] Cette steppe coupée de quelques bouquets de Pins, évoque donc l'installation d'un froid vif doublé d'une réelle sécheresse.
> [...]
> C. Mourer-Chauviré détermine des oiseaux de climat froid, réfugiés, surtout dans les arbres montagnards.
> R. Jullien note la présence des Taupes et conclut à un paysage plus steppique que pendant la phase IVA.
> B. Pillard souligne à cet endroit, l'apparition du Cheval, animal de prairie.
> J. Chaline décrit quelques espèces de forêts, de prairies, de garrigues. Au niveau de la couche 21A, le recul du Loir semble coïncider avec la disparition de la forêt.
> J.L. Vernet détermine dans cette sous-phase quelques bois, ...; enfin le développement d'associations plus ou moins xérophiles, semble bien en accord avec la brutale arrivée de la sécheresse soulignée par les autres chercheurs. (Renault-Miskovsky, pp.317-8)

> [Trans.: Starting from Layer 21AD [the lowest pollen sample position in Layer 21A], therefore, we are dealing with the almost absolute disappearance of the main tree taxa; ....
> The remainder of the landscape comprises a steppe with Graminae and Compositae ....
> This steppe, spotted with a few stands of Pine, therefore evokes the establishment of harsh cold reinforced by really dry conditions.
> [...]
> C. Mourer-Chauviré identifies cold climate birds, living in environmental refuges, especially in [amongst] the mountain trees.
> R. Jullien notes the presence of Moles and infers a more steppic landscape than during [Sub-] Phase IVA.
> B. Pillard stresses the appearance at this point of the Horse, a grassland animal.
> J. Chaline describes a few species associated with forest, grassland and garrigues [stoney, dry moorland - a term not usually applied to ecosystems other than those resembling zones within the modern Mediterranean region]. The diminishing importance of the Fat Dormouse, seen in Layer 21A, seems to coincide with the disappearance of the forest.
> J.L. Vernet identifies a few woods [charcoal] in this sub-phase ...; finally, the development of more or less xerophile associations [evidenced by the charcoal] seems to be concordant with the abrupt arrival of dry conditions stressed by the other researchers.]

The pollen evidence appears consistent with the sediments. However, Renault-Miskovsky cites five other authors to support her case. There are signs that the climatic reconstruction might not be quite so straightforward as we might have thought, but the main theme seems to hold up. Let us examine what these other authors actually say.

> Le pourcentage des oiseaux de montagne passe par un maximum tandis que celui des oiseaux 'chauds' continue à augmenter légèrement. (Mourer-Chauviré, p.287)

> [Trans.: The percentage of mountain birds reaches a maximum whilst that of 'warm' birds continues to rise slightly.]

No mention is made of tree-living species or of refuges.

> Dans l'ensemble, les oiseaux des niveaux pléistocènes de l'Hortus indiquent un climat plus froid que le climat actuel, mais toujours ensoleillé et nettement méditerranéen. Les espèces qui correspondent au maximum du froid, ..., communes aussi bien dans les niveaux rissiens que dans les niveaux würmiens, y sont totalement absentes. Un autre caractère très frappant est la sécheresse indiquée par ces oiseaux. Il n'y a aucun oiseau aquatique .... (Mourer-Chauviré, p.287)

> [Trans.: On the whole, the birds of the pleistocene levels at l'Hortus indicate a climate colder than the present day, but still sunny and markedly mediterranean. The species which correspond to the maximum of cold, ..., as common in rissian levels as in würmian levels [in other sites], are here totally absent.
> Another very striking characteristic is the dryness indicated by these

birds. There is not a single aquatic form, ...]

These comments refer to all pleistocene levels with birds, that is, to almost the whole sequence at l'Hortus, not just to Sub-Phase IVB. When Renault-Miskovsky notes the 'abrupt arrival of dry conditions stressed by the other researchers', she neglects to say that the birds have suggested such dry conditions all along. Of course, this would conflict with the palynology which is said to indicate much wetter conditions before Sub-Phase IVB.

Concerning the moles of Layers 21A, 19, 14 and 12:

> On aurait pu être tenté d'attribuer les premières [infra], à cause de leur taille, à la Taupe aveugle, de répartition méridionale, T. caeva Savi, 1822. Nous avons abandonné cette hypothèse après lecture des graphiques d'oscillations climatiques. Cet animal n'aurait probablement pas pu s'accomoder des froids réellement très vifs qui régnaient aux époques où se sont déposés les débris recueillis. (Jullien, pp.243-4)
>
> [Trans.: One might have been tempted to attribute the first occurrences [those from the four layers noted above], owing to their [small] size, to the species T. caeva Savi, 1822 (the blind mole), which has a southerly distribution [with respect to modern France]. We abandoned this hypothesis after seeing the graphs of climatic oscillations. This animal would probably not have been able to adjust to the really harsh cold conditions which prevailed during the periods when the study material was deposited.]

Thus, rather than deducing a steppic environment from the moles, as claimed by Renault-Miskovsky, Jullien actually identifies the species of mole on the basis of climatic information provided by others, notably by Renault-Miskovsky.

Concerning the large mammals:

> La sous-phase IVB amène un assèchement brusque et très important du climat qui modifie la faune en même temps qu'il fait disparaître la couverture forestière remplacée alors par la steppe à composées. Le froid d'abord légèrement atténué, comme à la fin de la sous-phase précédente, devient progressivement plus intense puis, fait place au réchauffement court mais important de l'inter-phase IVB-VA. (Pillard, p.196)
>
> [Trans.: Sub-Phase IVB sees an abrupt and very important drying of the climate which modifies the fauna at the same time as it causes the disappearance of the forest cover, which is then replaced by steppe with composites. The cold, at first slightly attenuated as at the end of the preceding sub-phase, becomes progressively more intense and then gives way to the brief but important warming of Inter-Phase IVB-VA.]

Here, Pillard appears to be presenting faunal evidence which backs up even the minor detail of the pollen and sediment records. Unfortunately, there is no way that such detail can be deduced from the actual megafaunal record. Pillard's climatic inference is based *entirely* upon the ratio between *Cervus* and *Equus* (which, incidentally, is characterised as an "animal d'espace découvert, de paysage steppique [an animal of open spaces, of steppic landscapes]"(Pillard, p.196)). The horse in fact appears in Layer 22D (1 individual), before Sub-Phase IVB. There is one individual of *Cervus* in Layer 21A. There are five individuals of *Equus* from the grouped Layers 21A to 17; Layer 17 is, of course, later than Sub-Phase IVB (cf. de Lumley 1972, Table XL, p.198). There is no megafauna at all from Inter-Phase IVB-VA.

Concerning the rodents:

> La faune des Rongeurs du remplissage de l'Hortus est relativement homogène de la base au sommet. On peut cependant distinguer deux ensembles superposés.
> Le premier ensemble comprend les couches 21A à 18. Il se caractérise par la présence d'une grande abondance d'*Apodemus sylvaticus*, de *Glis glis*, d'*Eliomys quercinus*, du grand campagnol *Microtus* cf. *mediterraneus-dentatus* et l'absence de *Microtus arvalis* et *Microtus nivalis*. Ces rongeurs impliquent l'existence de la forêt (*Apodemus*, *Eliomys*, *Glis*, *Clethrionomys*), de prairies (*Pitymys*) et de la garrigue (*M. mediterraneus-dentatus*).
> On remarquera dans les couches 21 la disparition des Loirs qui pourrait indiquer un recul de la forêt. (Chaline, p.239)
>
> [Trans.: The rodent fauna from l'Hortus is relatively homogeneous from the base to the top [including the holocene fauna]. One can, however, distinguish two groups, one stratified above the other.
> The first group comprises Layers 21A to 18. It is characterised by the presence of a great abundance of *Apodemus sylvaticus* [wood mouse], *Glis glis* [fat dormouse], *Eliomys quercinus* [garden dormouse], and of the large vole *Microtus* cf. *mediterraneus-dentatus*, and by the absence of *Microtus arvalis* [common vole] and *Microtus nivalis* [snow vole]. These rodents imply the existence of forest (*Apodemus*, *Eliomys*, *Glis*, *Clethrionomys* [bank vole]), grassland (*Pitymys* [pine vole]) and garrigue (*M. mediterraneus-dentatus*). Note, in Layers 21, the disappearance of fat dormice which could indicate a retreat of the forest cover.]

Note that, concerning *Glis*, Chaline clearly indicates its absence in Layers 21; it does not occur in Layer 21B, which is earlier than Sub-Phase IVB. *Glis* reappears in Layer 20 and is a reasonably constant element of the microfauna right through the upper layers. Clethrionomys, claimed as part of the Sub-Phase IVB fauna, is absent from the faunal list (de Lumley 1972, Table 1, p.233); this inconsistency is repeated in another publication (Chaline 1972:235-7).

Chaline continues:

> Ces conclusions concordent et complètent celles tirées de l'étude palynologique du remplissage. D'après les enseignements de la palynologie la steppe apparaît dès la couche 21A. Elle commence à se manifester dans la faune de Rongeurs dès la couche 21A, mais ne s'affirme qu'à partir de la couche 17. Ce décalage est normal puisque l'installation du biotope végétal précède nécessairement l'arrivée de ses habitants. La palynologie met en évidence l'existence de la steppe des niveaux 21A à 7. La présence de Rongeurs vivant dans les arbres et les bosquets montre qu'il faut nuancer ce jugement. Si la steppe était dominante dans la région, il est certains que des enclaves forestières devaient persister dans des endroits abrités des environs immédiats de la grotte de l'Hortus. (Chaline, p.239)
>
> [Trans.: These conclusions agree with and complete those drawn from the palynological study of the cave fill. According to the information gained from pollen analysis, steppe first appears in Layer 21A. It [the steppe] begins to show itself in the rodent fauna as early as Layer 21A, but only becomes strongly marked starting from Layer 17. This time-lag is normal since the establishment of a vegetational biotope necessarily precedes the arrival of its inhabitants. The palynology demonstrates the existence of steppe in levels 21A to 7. The presence of tree- or shrub-living rodents shows that one must slightly adapt this judgement. If steppe was dominant in the region, it is certain that forest enclaves must have survived in sheltered spots in the immediate vicinity of the cave of l'Hortus.]

Chaline clearly feels that there is no great clash between the rodents and the palynology. The ecological argument concerning plant biotope and the related fauna is most impressive; it is a pity that Chaline does not comment upon the important warming of Inter-Phase IVB-VA, which immediately precedes Layer 17, and the possible effects upon recolonisation. However, conciliation has been taken too far when the primary data is misrepresented. Of the 19 identified specimens of rodents from Sub-Phase IVB, 16 are from forest biotopes, as defined by Chaline himself. There is no change, either in species or in proportional representation from phases III to IVA to IVB. Why then did Chaline offer no interpretation whatsoever of Phase III (24 identified elements) or of Sub-Phase IVA (36 identified elements)? On what evidence does he claim that a steppic influence begins to show up in the rodent fauna as early, and only as early, as Layer 21A?

Concerning the wood charcoal:

> Nous pourrions donc en déduire un fort développement de la végétation méditerranéenne, favorisée par une saison sèche estivale très marquée et assez longue. [...]
> Si l'on compare nos résultats avec ceux de l'analyse pollinique, il y a, semble-t-il, désaccord complet. [...]
> [...] ... l'installation d'un climat méditerranéen assez aride par rapport aux phases précédentes mais ressemblant sans doute beaucoup au climat méditerranéen actuel, sub-humide au sens d'Emberger. (Vernet, p.337)
>
> [Trans.: We may therefore deduce from this [the charcoal] that there was a major development of mediterranean vegetation, favoured by a strongly marked and quite long summer dry season. [...]
> If one compares our results with those from the pollen analyses, it would appear that there is total disagreement. [...]
> [The data shows] the establishment of a mediterranean climate, quite arid in comparison with the preceding phases, but without doubt closely resembling the modern mediterranean climate, Emberger's sub-humid type.]

Here we have an author who is prepared to speak his mind. Vernet even goes on to re-interpret Renault-Miskovsky's pollen results! This does not seem to displease Renault-Miskovsky since, as we have seen above, she makes no comment about it.

However, Vernet, like most of his colleagues, is not as careful with other people's opinions as he should be.

> L'étude des Insectivores conduit R. Jullien à interpréter le paysage comme étant un peu plus clair qu'en IVA, prairies avec taillis et buissons. (Vernet, p.337)
>
> [Trans.: The study of the insectivores leads R. Jullien to interpret the landscape [of Sub-Phase IVB] as being a little more open than during IVA: grassland with copses and bushes.]

Jullien's comments about the insectivores of Sub-Phase IVB have been given in full above; the order is represented by moles alone in these deposits. Vernet's attribution of this ecological interpretation to Jullien would appear either to be complete invention or perhaps a transposition of Jullien's (p.244) interpretation of the presence of hedgehogs in Layers 12 and 10.

Vernet seems to be a little confused about his own data too.

> Quelle a été alors la végétation au cours de la sous-phase IVB? Elle découle de l'arrivée plus ou moins rapide de la sécheresse au niveau 21B. Le phénomène est très bien marqué, en particulier, par les charbons de bois. (Vernet, p.337)
>
> [Trans.: What then was the vegetation like during Sub-Phase IVB? It is a function of the more or less rapid arrival of dry conditions at level 21B. The phenomenon is very well evidenced, in particular, by the wood charcoal.]

Layer 21B predates Sub-Phase IVB and does not contain any wood charcoal. A printing error, perhaps? Layer 21A does not contain any wood charcoal either. Layer 20:

> Couche 20: L'association Quercus à feuillage caduc et Evonymus semble indiquer, ici aussi, pour des raisons assez semblables à celles indiquées plus haut (couche 23) un climat assez froid. (Vernet, p.336)
>
> [Trans.: The association of deciduous Quercus [oak] and Evonymus [spindle-tree] seems to indicate, here as well, for reasons quite similar to those given above (Layer 23), quite a cold climate.]
>
> Couche 23: La combinaison Chene à feuillage caduc et Pin est, en elle-même, peu significative, surtout en l'absence de données plus précises. Cependant, par rapport aux niveaux postérieurs, dans lesquels sont présents divers taxons méditerranéens, il semble permis de croire à un climat assez frais. Ceci est en accord avec la sédimentologie. (Vernet, p.334)
>
> [Trans.: The combination of a deciduous oak and the pine is, in itself, of little significance, especially in the absence of more precise data. However, in comparison with the subsequent levels in which various mediterranean taxa are present, it would seem permissible to infer quite a cool climate. This is in agreement with the sedimentology.]

There are further wrangles and tortured arguments in other individual contributions to this publication. By the time one reaches p.349, one is certainly ready for a chapter entitled "Evolution du paysage et du climat pendant le Würmien II en Languedoc méditerranéen [Evolution of the landscape and the climate during the Würm II in the mediterranean Languedoc]". Here we may expect a balanced argument and proper representation of each contribution. The chapter was written by H. de Lumley, E. Guerrier (Professor of Architecture; also responsible for the beautiful drawings of the reconstructed landscapes) and Alain Fournier (a laboratory assistant). The other thirty-three authors presumably had no opinions to voice. The section on Sub-Phase IVB is given here in full, together with the original references to individual contributors:

> Au début de la phase IVB (couches 21A à 18), le changement climatique qui s'était amorcé dès la phase IVA devient radical. Le climat est froid et aride, l'action du vent devient violente et la végétation est bouleversée. Les arbres disparaissent peu à peu, seuls subsistent quelques bouquets de pins et taillis buissonnants où se regroupent les boeufs primitifs (cf. fig.8).
> La forêt a cédé la place à une prairie à graminées et composées (Renault-Miskovsky) où les chevaux peuvent calmer leur fougue. La disparition du loir (Chaline) confirme le retrait de la forêt. Sur les pentes ensoleillées du massif de l'Hortus, quelques espèces végétales plus ou moins thermophiles, subsistent dans de véritables refuges écologiques: érable, prunier, chêne vert et chêne blanc, tilleul, phillaire (Vernet). Le buis, la lentisque et le genèvrier se développent au pied de la falaise et sur les corniches bien exposées où nichaient les chocards et s'abritaient les lérots.
> Au bord du Terrieu, le noisetier semble s'accomoder d'un faible couvert végétal et résister à la sécheresse. (de Lumley, Guerrier & Fournier, p.351)
>
> [Trans.: At the beginning of Phase IVB (Layers 21A to 18), the climatic change already begun in Phase IVA becomes extreme. The climate is cold and arid, the winds become violent and the vegetation patterns are upset. Trees disappear little by little; the only survivors are a few stands of pine and brushwood where the primitive cattle gather (cf. fig.8 [artist's reconstruction]).
> The forest has given way to grassland, with grasses and composites (Renault-Miskovsky), where the horses can assuage their fiery spirits. The disappearance of the fat dormouse (Chaline) confirms the retreat of the forest. On the sunny slopes of the Hortus Massif, a few more or less thermophilous plant species survive in veritable ecological refuges: maple, plum tree, holm oak and deciduous oak, lime tree, filaria [mediterranean evergreen bush] (Vernet). The box tree, the lentiscus and the juniper spread along the foot of the cliff and onto well exposed ledges where yellow-beaked chough would nest and the garden dormouse take shelter.
> On the banks of the Terrieu [the local valley stream], the hazel seems to make the best of a light vegetation cover and to withstand the dry conditions.]

This disappointing passage is the sum total of the discussion of Sub-Phase IVB. Beyond those matters which we have already noted, there are further errors of fact in this colourful account. Renault-Miskovsky never refers to "prairie" but always to "steppe" - a very different ecological concept. The lime tree (Tilia) is not reported by Vernet but by Renault-Miskovsky. The juniper (Juniperus) and the lentiscus (Pistacia lentiscus) are not recorded, either by Vernet or Renault-Miskovsky, from Sub-Phase IVB. However, the subsequent 'warmer' phase, Inter-Phase IVB-VA, is said by Renault-Miskovsky to have unspecified representatives of the Cupressaceae (of which juniper is a member) and Pistacia sp. "Prunier" (plum tree) presumably refers to Prunus mahaleb, a form of wild cherry tree, recorded by Vernet.

At the beginning of the present section, Sub-Phase IVB at l'Hortus was selected as an example of the multidisciplinary study of a cave sequence. It is not, however, an example of interdisciplinary research. In his conclusions, de Lumley states:

> La première conclusion est, nous semble-t-il, de souligner la nécessité pour le chercheur des civilisations disparues de travailler en équipe, chaque spécialiste prenant une part active à la synthèse commune. [...] La synthèse est élaborée grâce à la fructueuse confrontation des résultats obtenus dans des disciplines variées. (de Lumley, p.665)
>
> [Trans.: It seems to us that the first thing to stress is the necessity for the researcher into vanished cultures to work within a team, each specialist taking an active part in the synthesis of the grouped information. [...] The synthesis is built up thanks to the fruitful comparison of the results obtained from varied disciplines.]

No quarrel can be had with these statements; it is a great pity that the theory was not put into practice. What we have seen, in fact, are authors warping their data to suit their arguments; authors warping their data to suit other people's, more dominant, arguments; authors warping other people's data to suit their own arguments. It is of no importance here who is right, what the climate was really like during Sub-Phase IVB. What matters is the apparent contempt shown by these researchers towards each other; any claim of team-work is laughable. One good point about this publication lies in the fact that the attentive reader is at least able to recognise this lack of co-operation under the superficial layer of unanimity. The present author has worked in situations where attempts were made to suppress discordant data completely. The sort of attitude illustrated in this section is by far the most common to be found at present in multidisciplinary research in caves.

One last point must be made before leaving l'Hortus.

> La structure ouverte des cailloutis a favorisé l'infiltration vers les niveaux inférieurs de certains objets de petite taille: dents, silex, etc. Certaines de ces pièces appartenant à une mandibule complète ou à un même galet ont pu être replacées dans leur niveau primitif (cf. dents du loup de la couche 13A, infiltrées dans les couches 14 à 16B et dents de la mandibule d'enfant Hortus II de la couche 14 infiltrées dans les couches 15 à 17). D'autres pièces ont été arrachées par le piètinement à un niveau plus ancien. Ainsi, tel fragment d'os découvert dans une couche supérieure peut être raccordé à un os long d'une couche sous-jacente. [...]
> Enfin, l'infiltration des eaux à travers la masse du remplissage, principalement à proximité des parois et dans les niveaux supérieurs, a entraîné les terres organiques, les cendres et même une grande partie des charbons de bois. (de Lumley, p.528)
>
> [Trans.: The open-work structure of the screes has favoured the infiltration, towards lower levels, of certain small objects: teeth, flints, etc. Some of these pieces, belonging to a complete mandible or to a single pebble [nodule], can be replaced at their original level (cf. the wolf teeth of Layer 13A, infiltrated into Layers 14 to 16B [i.e. up to 50cm of vertical displacement] and teeth of the child mandible, Hortus II, of layer 14, infiltrated into Layers 15 to 17 [i.e. up to 50cm of vertical displacement]). Other pieces have been torn up, by trampling, from an older level. Thus, such and such a bone fragment discovered in an upper layer can be refitted to a long bone from an underlying layer. [...]
> Lastly, the infiltration of water through the body of the fill, principally near the rock walls and in the upper levels, has entrained organic earth, ashes and even a large part of the wood charcoal.]

Although specific examples are only given from layers just above those of Sub-Phase IVB, it is patently obvious - indeed, it is strongly implied even by de Lumley - that these observations must apply to the fill as a whole. Note that entrainment of small particles can only result in their displacement within the stratigraphy, not in their removal, since the deposits lie in a totally confined fissure.

> [...] ... sur les très nombreux ossements de lapins que nous avons observés (3000 env.) nous n'avons relevé aucun indice de l'activité humaine; .... (Pillard, p.231)
>
> [Trans.: [...] ... we have identified no trace of human activity on the very numerous bones of rabbit which we have studied (c.3000 [c.250 from Sub-Phase IVB]); ....]

To what extent were all the foregoing arguments over environment and climate based upon unreliable provenance data?

19.3. Professionalism

The author is almost embarassed to have to include this short section, since the following statements appear to him to be such conspicuous truisms that no mention should be necessary. However, truisms are often so conspicuous that people seem to forget them. Unpleasant as such discussions may be (and pompous though anyone raising the subject may sound), the author cannot accept the opinion, which he has heard so often, that they are in themselves 'unprofessional'. That way lies recidivism.

It is sometimes claimed that carrying out one's research in isolation from others working on the same problem guarantees objective results. This, of course, is untrue. All that is guaranteed is a lack of bias introduced by indiscriminate acceptance of the opinions of others, a pitfall which we should be able to avoid without isolating ourselves. The nearest thing to objectivity available to us lies in the concept of primary data, information which relates to physical realities, information that can be collected again and again if a check proves necessary. Isolating oneself, or even just neglecting to take an active part, means that others cannot use one's primary data, usually until close to the publication date. Similarly, one cannot use other people's primary data. This may result in much useless work based upon false premises. Furthermore, many significant problems may not become apparent until practical work has finished. Researchers are often adverse to passing on their primary data too soon, since they feel they would like to digest them first.

This sounds reasonable, but how can such data be truly primary if 'digestion' is required? Passing on data, and indeed working hypotheses, in no way negates one's right to further discussion or one's 'standing' as a specialist. Many of the author's colleagues, highly competent researchers, seem to be suffering from a growing aversion to making simple statements of fact (especially in print) for fear that their data will be misinterpreted. It would appear that we are faced with a choice between overt communication, with the risk that the inexperienced may build patently untenable arguments with our data, or ultimate paranoia.

There are, however, two extremes that must be avoided during synthesis of results. If communication is left too late, massive contradictions will almost certainly arise, resulting in bitter and usually fruitless arguments. On the other hand, continuous synthesis of all data and ideas, as they become available, merely clouds the issue. Given the 'state of the art', it is a foregone conclusion that apparently contradictory information will frequently come to light; only by omission and misrepresentation can everything be made to fit all of the time.

The obvious answer is a sort of iterative mechanism (the 'heuristic approach' of some archaeologists). Although most of the disciplines involved are not truly experimental sciences (i.e. the truth of a statement cannot often be proven by exact replication under rigid experimental control), a good hypothesis should still be framed in such a way that major consequences will follow. This gives the hypothesis predictive power which may be used for at least informal testing with new data. Such an approach, repeated several times before the practical side of a project is shut down or before a major publication, will remove the chance of a damaging brawl or a 'cold war' at the last moment. Furthermore, we might even learn something. Contradictions are not in the least threatening, they are interesting. Not only do they indicate that we are overlooking something, but they usually carry substantial clues as to where the solution might be found.

In the example from l'Hortus, quoted in the last section, there is not the slightest indication of pre-publication co-operation, there is not a single instance of data collection to solve a stated problem, and sometimes opinions seem to be framed as if the authors considered themselves to be in some sort of competition. Far from being testable hypotheses, most of the ideas in the Hortus report are more or less credible conjecture. By ensuring that no-one can actually prove them wrong at this level, these authors have guaranteed that any professional person will find fault at the procedural level. Thus, if we cannot decide who may or may not be right, logically we are left with no alternative but to reject the entire report!

Such problems of procedure as those mentioned here are most obvious in a multidisciplinary report when the various authors disagree. However, the same reasoning applies to the work of an individual, or a group of individuals when they are in agreement - that is, of course, unless it is decided that the general professional community has no right to participate in the discussion.

One final point must be made here. As was said in section 7.1. above, it is the archaeologist's job to make sure that all specialists understand the archaeological problems that arise at a site under study. Because the archaeologist is often the director of an excavation at an 'archaeological' site, he may also have the responsibility of general editor for any publications.

Nevertheless, it is most certainly not the archaeologist's job to re-interpret all the specialist reports in order to gild the archaeological picture, or to simplify the 'scientific' aspect for his archaeological readers. In the author's experience, archaeological readers, as a group, are usually rather better informed than archaeologists, as individuals, often suppose. Misuse of this position as co-ordinator, whatever the motive, has been the cause of the great majority of failures in multidisciplinary cave research in the past.

20. ARCHAEOLOGICAL CONCEPTS

The author considers that the present thesis constitutes an archaeological statement. It is hoped that the reader will have recognised that the discussion so far is relevant, even fundamental, to cave-based archaeology. Nevertheless, there will still be many who would not accept that the subject matter of these pages actually is archaeology, not just a useful but peripheral speciality. On the other hand, there does seem to be a growing awareness amongst archaeologists of the inalienable right of contextual studies to claim the foremost position in all archaeological research. Consider the following statement, drawn from the conclusion of a recent review concerning this subject.

> I almost entitled this paper "archaeology as sedimentology" and it would not have been inappropriate. The first order of business for the archaeologist is to identify the nature of the cultural and noncultural formation processes that created a given deposit or set of deposits. To accomplish this, we may consider artifacts as merely peculiar particles in a sedimentary matrix ... that potentially have been subjected by cultural and natural formation processes to a variety of mechanical and chemical alterations. [...]
> The importance of identifying formation processes before behavioral or environmental inferences are developed cannot be overemphasized. In far too many cases, the evidence used by an archaeologist owes many of its properties, not to the past phenomena of interest, but to various formation processes. [...]
> Superficially, the directions I am advocating seem to take us further away from the behavioral and organizational properties of past societies that are so important to contemporary theorists. That is true, but only in the short run. In the long run, enhanced understanding of formation processes permits inferences about past phenomena that have a logical and scientific basis. When any archaeological inference is put forth, the investigator has inevitably made assumptions, usually tacit ones, about the nature of formation processes. These assumptions frequently assert that formation processes have slight effects or have random effects that cancel out each other. More than a decade of work on formation processes has shown, however, that these and similar assumptions are wrong and dangerous. Inappropriate assumptions must be replaced by thoughtful efforts to

> understand how specific deposits formed. Although much basic research remains to be undertaken, enough information is now at hand to make the rigorous study of formation processes a practical component of all fieldwork and analysis. Until such studies are carried out routinely, archaeologists cannot properly claim any behavioral significance for their inferences. (Schiffer 1983:696-7)

Schiffer is a 'mainstream' archaeologist, with no specifically sedimentological pretensions, and yet the present author, who cheerfully admits to such pretensions, can think of no way whatsoever in which the above statement could be bettered, either in content or emphasis. However, although the present author has a common aim with a growing number of 'mainstream' archaeologists, it would be misleading to suggest that all paths have actually converged, or even that such convergence is desirable.

Schiffer's article, which gives an accurate picture of the better aspects of current, context-oriented archaeological thinking, consists of twenty-three pages of text, followed by over two hundred references. Of these twenty-three pages, ten are taken up with general discussion, whilst thirteen cover specific methods for investigating site formation processes. The methods include the study of the size, specific gravity, shape, orientation and dip, "use-life" parameters, and damage parameters of artefacts, and the nature of material adhering to artefacts. In addition, there are the complex properties of artefacts, such as artefact quantity, vertical distribution, horizontal distribution, artefact diversity, measures of "disorganisation" (fragmentation), artefact reassembly (refitting) and the differential occurrence of parts of artefacts. These methods account for about ten and a half pages out of the thirteen. A little under half a page is given over to consideration of "ecofacts" (noncultural biological remains and effects) and a little over half a page to chemical recognition of archaeological residues within deposits and the general morphology and context of deposits. Finally, a page and a half is allowed for sediments themselves; under ten percent of the references in this article are more or less directly concerned with sediments, only one of these being a (very) general textbook on sedimentology. The sedimentary characteristics singled out for discussion are colour, texture, surface morphology of sediment particles, rhizoliths and compaction. Schiffer concludes this section on sediments as follows:

> Many advances in sedimentology are to be expected in the years ahead, particularly as the traces of various cultural formation processes are sought, perhaps initially in experimental archaeology and ethnoarchaeology. The ubiquitous dirt we labor so hard to remove is itself an artefact that has much information to disclose. (ibid., p.690)

Now, the present author welcomes the sort of approach advocated by Schiffer, and this article is certainly recommended reading. However, there is still too strong a dividing line between artefacts (or archaeology) and sediments (or sedimentology) and, if a demarcation line must exist (which the present author doubts), it is drawn in the wrong place. Archaeologists are notoriously jealous of what they consider to be their own territory. They will often accept advice but, in the old tradition of archaeological eclecticism , they will not readily consider fields of study that they cannot themselves, as individuals, grasp and apply. Schiffer is suggesting that the 'mainstream' archaeologist should interpret parameters such as artefact weight and orientation in terms of complex depositional and postdepositional processes. No doubt many will take his advice, even though their training has never prepared them to undertake such studies. This is akin to the situation which developed some twenty years ago, when archaeologists were told (by archaeologists) that they were obsolete if they did not immediately integrate cybernetics, statistics and computer science into their research. Similarly, experiments in site formation processes are all the rage at the moment but, in the present author's experience, very few 'mainstream' archaeologists have a clear idea of what an 'experiment' involves. This is very plainly seen in two basic tendencies common to most archaeological experiments. First, given a parameter which will obviously affect the outcome, it is assumed that there is no need to control this parameter if there is no obvious way of deducing an accurate value for the parameter in the ancient system which the experiment is designed to model. Second, it is assumed that an experiment should replicate, as closely as possible, the ancient system. In fact, a good experiment should be designed to control, at least approximately, as many parameters as possible that are likely to influence the outcome, so that the specific effects of each parameter can be identified. This often involves deliberately setting up situations that are markedly different from the ancient system. The author has never seen a simple technique, such as a factorial design (cf. Cox 1958), used in an archaeological experiment, although he has indeed been told that such an approach is 'unnecessarily complex' (i.e. the archaeologist cannot easily grasp and apply the approach himself). By keeping the approach 'simple', it is thought that generally valid conclusions can be drawn. This is piffle of the worst ilk! A generalisation is based upon a judgement (and not necessarily a complex mathematical one) that the greatest likely variation in a parameter, or combination of parameters, is not likely to affect the conclusion to a significant degree. One must therefore estimate, even very roughly, the effects of individual parameters and the range of parameter values within which the true values in the ancient system are likely to have fallen. If this can only be done very approximately, so be it. The conclusion will then be very approximate, but one will at least be able to indicate clearly how the results would change, either if more precise estimates could be provided or if the original estimates could be shown to be wrong. Herein lies the importance of exact experimentation even in the absence of much data about the ancient system.

Schiffer notes:

> For example, larger objects are moved upward and displaced laterally [by trampling], whereas in loose substrates smaller ones are pressed downward (Stockton 1973; Wilk and Schiffer 1979). (ibid., p.679)

This is a forceful statement which will no doubt be used as a facile diagnostic tool in other sites. The article by Stockton (1973) involves research in a rockshelter, so that it might appear to some readers to be of even more specific interest here, although the type of site is of course irrelevant in this case. However, Courtin and Villa (1982) have conducted similar experiments outside the cave site of La Baume Fontbrégoua (Var); they conclude that there is no correlation between size and either vertical or horizontal displacement. They are aware of the experiments of Stockton and they suggest, but do not demonstrate, that the inconsistency may be due to such factors as differences in artefacts (Stockton - small glass fragments; Courtin & Villa - flint, bones, marine shells, pottery sherds) or the nature of the trampling (Stockton - intensive 'artificial' trampling; Courtin & Villa - the casual passage of site personnel over the area during normal activities). All these authors are aware of the possible involvement of the sediments but they make no attempt to estimate the effects of sediment parameters, or even to name all the obviously relevant factors. Thus, we are left with the general proposition that trampling displaces artefacts and that certain artefact, sediment and energetics parameters are probably involved, information which was rather obvious without any experiments; we have not been told how individual parameters affect the results and we cannot safely extrapolate these results beyond the exact (and not necessarily explicit) conditions represented by the experiments.

Compare the experiments carried out by Moeyersons (1978) on the behavior of large objects (including quartzitic artefacts) acted upon by postdepositional processes in a specific sediment matrix. The results of these experiments are irrelevant here; what is of importance is the way Moeyersons went about tackling the problem. First, sections of deposits at the (open-air) archaeological site were examined; a study of structure (biogenic in this case) was made, backed up by quantification of particle size, plasticity index and unit dry weight variation through the sequence (generally silty, clayey sands). Four processes, that might have caused displacement of large objects in that context, were suggested: bioturbation, wetting and drying, sediment compaction and gentle sediment creep. It was decided to investigate the effects of the last three factors by laboratory experiment. The sediment was reconstructed in the laboratory with the correct natural properties. Artefacts were described by their length, width, thickness, weight and specific weight; in addition, depending upon the way in which artefacts were made to lie in/on the sediment, they were characterised by a 'weight/vertical projection surface' index which indicated how much force per unit area the artefact's own mass supplied. A range of environmental conditions were then set up (wetting and drying regimes and surface slopes). Further experiments were carried out on artefacts buried in sediment, with overburden (i.e. depth of burial) simulated by artificially applied pressure. In order to investigate more closely the effects of object shape and orientation, the irregularly shaped artefacts were then replaced with regular wooden blocks with different initial orientations in the sediment (e.g. blocks of triangular section with the apex up or down). Whilst the experiments were in progress, the exact position and orientation of objects were monitored by watching fine metal wires attached to the objects. The exact position of the water table was constantly noted. Any structure developed within the sediment itself during an experiment was noted. Because of the careful design of these experiments, the results allowed the effects of the different parameters to be recognised, as well as linear or curvilinear relationships between parameters. Finally, it was noted that, because of the restraints on movement due to rising compaction, the effects investigated would not have produced the observed vertical displacement of artefacts in the ancient context (in this case, conjoinable cores with their elements dispersed over a vertical range of 1m) if it had not been for the fourth process, bioturbation by small organisms (demonstrable from the sediment structure on site), which would have caused repeated dilation of the sediment counteracting the other tendencies towards compaction. Neither Moeyersons nor the present author claims that everything that could possibly have been learnt about this problem is apparent in the experiments. Certain factors were recognised as important and these factors were accurately investigated. This is one of the best examples of an archaeological experiment that the present author has ever encountered - yet it was carried out by a sedimentologist, because he could supply the necessary expertise. On the other hand, the experiment would never have taken place if an archaeologist, D. Cahen, had not framed the problem in the first place by using his special skills to refit artefacts.

Some processes might be considered to be wholly within the province of 'mainstream' archaeologists. Schiffer (1983) cites the examples of the "McKeller Hypothesis" ('smaller artefactual items are more likely to become primary refuse in activity areas') and the "Frison Effect" ('recycled stone artefacts become progressively smaller'). Surely these are valid observations? Certainly, as long as groupings of smaller artefacts are not automatically interpreted as due, or wholly due, to either recycling or to the presence of 'manufacturing areas'. An 'inverse' example of the overenthusiastic application of pattern recognition, involving the 'original knapping scatter hypothesis', was once encountered by the present author. Large artefacts with a horizontal distribution of several metres had been refitted to a single core. This distribution was much too wide to be an undisturbed knapping scatter so that it was immediately decided that some geological process must have been responsible. When the author argued that no such process was evidenced in the deposit in question and, furthermore, that any such process would certainly have shown up had it occurred, it was suggested that he must be wrong. When the author persisted and asked might not the people have kicked or trampled the knapping scatter, or thrown or carried artefacts about, or knapped the different pieces in different places, he was told in no uncertain terms to stick to his own speciality. The author's immediate reaction was to tell the archaeologists to stick to their speciality and not to tell him he had made an error of geological interpretation. And there matters might well have rested had not all concerned realised that this silly 'chauvinism' was preventing a proper investigation of the phenomenon.

It is extremely encouraging that archaeologists are becoming more aware of the complex taphonomic information that is available from artefacts themselves. The present author has often used patterning in artefacts to support sedimentological inferences. The important thing to remember when one is trying to interpret artefacts, sediments or any other entities, is the procedural order that is essential to all such studies. First comes the basic question 'How did this object come to be in the observed position and condition, and in the observed relationship with other objects?'. Then comes the decision as to the range of expertise that will be necessary to answer the question. Last come the conclusions, which will be of archaeological or geological interest, or both. The whole process is invalidated if one starts with the basic proposition that artefacts are the exclusive concern of archaeologists and sediments are the exclusive concern of sedimentologists. It should not be forgotten that some individual archaeologists, who have recognised a major weakness in archaeological theory and practice, have made the decision to go out on a limb to acquire considerable training that will help to strengthen this weakness. Usually, this will mean that these individuals must also make do with only a basic knowledge of many of the more complex areas of 'mainstream' archaeology. It is nevertheless somewhat distressing to find that, when one has made this effort, one is nearly always disowned by the rest of the archaeological community. The more one learns about an allied subject, the less one is allowed to participate directly in archaeological enquiry, a situation which is patently absurd and which defeats the whole object of the special training.

There are three specific areas of archaeological contextual studies which deserve brief discussion here: stratigraphy, structures and excavation.

Archaeological stratigraphy, often called 'archaeostratigraphy' in recent texts, is sometimes considered to be the only sort of stratigraphy that exists in archaeological sites. Indeed, in some cases, the archaeologist will define <u>the</u> stratigraphy and geological and biological experts will be asked to comment upon the contents of the defined units, whether or not these units are suitable for such purposes. The works of E.C. Harris (1975, 1979a, 1979b) are now widely cited as the basic texts on archaeostratigraphy. Harris's opinions are certainly generally acceptable and the formalisation of the approach is welcome, even if most geologists would be surprised to discover that it has taken archaeology well over a century to catch up with the principles clearly set out by Smith, Hutton and Lyell. In fact, Harris's suggestions are not nearly so 'new' as one might believe from reading his work, and wiser archaeologists have long been using the great majority of these ideas. Even the graphic device now widely known as the 'Harris Matrix' has been in use in other fields for decades. However, the fact that archaeology often borrows what is useful from other fields must be seen as a strength, not a weakness, and the fact that Harris's "Laws of Archaeological Stratigraphy" include the "Law of Superposition", the "Law of Original Horizontality" and the "Law of Original Continuity" is acceptable (cf. Chapter 5 of the present text), even if certain statements raise a smile:

> These three laws [those noted above] are of course adapted from geological sources. A fourth axiom, the 'law of stratigraphical succession', is an archaeological invention ....
> [...]
> <u>The Law of Stratigraphical Succession</u>: any given unit of archaeological <u>stratification takes its place in the</u> stratigraphic sequence of a site from its position between the undermost of all units which lie above it and the uppermost of all those units which lie below it and with which it has a physical contact, all other superpositional relationships being regarded as redundant. (1979a:112-3)

However, if one forgets too readily that an item has been borrowed, one may become a little overconfident.

> ... archaeological stratification may exist without artefacts. (ibid., p.112)

This is a little worrying. Note that the <u>International Stratigraphic Guide</u> (Hedberg 1976) clearly states that a lithostratigraphic or biostratigraphic unit cannot be followed in the absence of the defining characteristics. Nevertheless, a few 'sterile' intrazones and interzones are allowed and we must assume that this is what Harris means. However, even this generous reading cannot be maintained when one encounters the following statement:

> In this paper the primary laws of archaeological stratigraphy will be discussed. They may be applied to archaeological stratification without regard to the artefactual content of the given body of strata. (ibid., p.112)

How, then, is one supposed to define an archaeological unit? Harris, of course, merely supplies a very brief and shaky account of what is effectively a mix of lithostratigraphy and chronostratigraphy, subsumed under the title of 'archaeological stratigraphy', providing another example of the less acceptable side of archaeological plagiarism.

A more interesting approach is now being explored by a Working Group of the <u>International Geological Correlation Programme</u>, specifically set up to study archaeostratigraphy, and composed predominantly of active field archaeologists (Gasche & Tunca 1981). The first point made very firmly by this Working Group is that lithostratigraphy, biostratigraphy and chronostratigraphy do not need to be, and should not be, renamed and redefined simply because an archaeological site is involved. A summary of the <u>International Stratigraphic Guide</u>, covering these areas, is included in order to acquaint archaeologists with the approach (and, in most cases, the existence) of this document. Having efficiently stripped archaeostratigraphy of all the disguised accretions which properly belong to other areas of stratigraphy, the Working Group is then able to address the real problem of whether or not specifically archaeological phenomena require a discrete stratigraphic approach and, if so, what should this approach be. To the present author's mind, the very fact that this question has been posed shows that the Working Group is on the right track. Archaeostratigraphy is now redefined as the sum of all stratigraphic investigations involving archaeological sites; it comprises lithostratigraphy, biostratigraphy and chronostratigraphy, each with its proper methodology, and also a new class: ethnostratigraphy.

> Ethnostratigraphy is a stratigraphic classification whose units are characterized by their contents of anthropic origin, i.e. by artefacts*.
> *artefact: all objects, all constructions or all remains of an anthropic origin, which have a known or supposed purpose.
> (Gasche & Tunca 1981, p.39 and glossary)

The present author would have preferred the definition to be widened slightly to include all demonstrably anthropogenic effects, whether they are the result of conscious activity (as implied in the above definition of an 'artefact') or not.

One of the difficulties involved in trying to set up a system which will have the broadest possible validity is that archaeology in general covers such a wide range of phenomena. It is relatively easy to construct a system for sediments, but how does one suggest a scheme that will be equally valid for a tell, consisting of perhaps thousands of dominantly anthropogenic units, and a cave, with perhaps only a single level containing a few artefacts? All the details of how the Working Group tackles this problem will not be discussed here. The reader is encouraged to refer to the initial document (Gasche & Tunca 1981) and to the various notes that have since been circulated; hopefully, all interested parties will join in the continuing debate, as explicitly requested by the Working Group. The preliminary discussion centred upon using the International Stratigraphic Guide as a framework. First, it was suggested that ethnostratigraphic units might be set in a hierarchy, like lithostratigraphic units. The present author does not believe that archaeological phenomena are amenable to such an approach. Then, it was suggested that biostratigraphy might provide a model, with such units as 'total-assemblage-zones', 'partial-assemblage-zones', 'taxon-range-zones', 'concurrent-range--zones', 'acme-zones' and 'interval-zones'. This approach would seem to be much more promising. Such an approach would be useful, even if classification were kept to a very basic level. In the context of presence/absence of artefacts, what is currently implied by the terms 'archaeological layer' and 'archaeological level'? The present author believes that, beyond treating archaeological phenomena as assemblages of objects, it would also be possible to integrate many of the existing concepts of archaeological structures (a pit, a hearth, a post-hole, a knapping scatter) into the ethnostratigraphic system (infra), perhaps under the general heading of 'ethnostructures'. This would allow considerable flexibility, since the structures found in different types of archaeological site could then be defined by those researchers directly involved, a method which, together with the zonal concept, would largely obviate the difficulties imposed by the sheer variety of archaeological phenomena.

It should be noted that the object of ethnostratigraphy is defined by the Working Group as follows:

> The aim of ethnostratigraphic classification is to organize the sequences of strata into units characterized by their artefacts and thus to establish definite correlations between deposits interrupted by stratigraphic ruptures and, especially, by stratigraphic gaps.
> (Gasche & Tunca 1981:41)

By "stratigraphic rupture" is meant a true hiatus, such as that caused by erosion; a "stratigraphic gap" is the effective hiatus caused by intervening unexcavated deposits. Ethnostratigraphy is thus a true stratigraphic subject. The present author would add that the aim is also to construct a system which will allow better communication, both in general and, more specifically, between the various specialists working on a site. Ethnostratigraphy is not meant to absorb all the higher interpretative levels of archaeology; it is a practical, field-oriented subject, that seeks to define entities that are as objective and immediately useful as possible.

A topic which is allied to archaeostratigraphy concerns the stratigraphic integrity of artefact assemblages produced by individual 'occupations'. Much has been written lately upon the recognition of 'living floors', with a particularly heated argument over the relative merits of refitting, on the one hand, and of what is incorrectly called 'stratigraphy' but which is usually only spatial integrity, on the other (cf. especially Bordes 1975, 1980; Cahen 1980 and reply by Bordes). First, it should be pointed out that, contrary to many statements in the literature, there is absolutely no difficulty over the definition of a living floor: a living floor is a surface upon which people are (present tense) living. Similarly, a lake is a basin full of water, not a sequence of well bedded fine sediments in a palaeotopographic depression. One cannot recognise a living floor in ancient deposits because no such entity exists, by definition. What one tries to recognise are the remains left by people over a relatively short period, the likely duration of which must be demonstrated. As the IGCP Working Group (Gasche & Tunca 1981:39) so rightly points out, this is a chronostratigraphic problem. A thin layer of compatible and conjoinable artefacts does not constitute a living floor, it constitutes a thin layer of compatible and conjoinable artefacts. Similarly, it is unacceptable to say that a diffuse band of artefacts does not constitute a living floor, because the question simply does not arise. The present author disagrees with the IGCP Working Group in that a living floor is there defined as "an isochronous surface"(ibid., p.39). If the present author found artefacts lying on a demonstrably isochronous surface within fine, well bedded sediments (a criterion often cited as necessary for the recognition of a 'living floor'), he would immediately suspect that the artefacts represent either a truly momentary anthropogenic process (such as the discarding of a large amount of debris into an otherwise totally unused area) or a natural process (such as wind or water deflation of fine deposits so as to let artefacts down onto a surface). People living on the surface of fine sediment usually create a band of artefactual phenomena that may be tens of centimetres thick, except in the case of compacted and cohesive sediments. The search for the 'one-artefact-thick living floor' is illusory if the only additional criterion is a generally fine sediment. Each individual case must be examined according to the available evidence. The archaeologist starts off with the general proposition that he would like to be able to recognise the shortest possible chronozones and that he would like to be able to relate spatial attributes to behavioral patterns. He, and any other individual whose expertise is required, must then go through the whole taphonomic process, with consideration of geological, biological and anthropic factors, acting at various different times. If he is lucky, he may be able to demonstrate that the artefactual phenomena are likely to have been produced by people living on a surface for a relatively short period of time. If not, he will often be able to give an approximate idea of the degree of resolution which the particular case allows. The present author will suggest (p.853) that, at Pixie's Hole (Devon), some archaeological phenomena indeed represent a portion (probably rather a small one) of the activities of a group of people over a very short period. The best information comes from a badly sorted deposit, containing many large limestone clasts, in which artefacts are found in a band up to 15cm thick! The author's suggestion is not based upon any _a_ _priori_ definition of a 'living floor' as a presently observable phenomenon but, rather, upon the probable implications of all the available contextual evidence. Returning to generalities, a 'living floor' is by no means the finest possible chronostratigraphic division; phenomena such as knapping scatters often represent much more discrete 'events'. All in all, the concept of a 'living floor' may become something of a hindrance, because it suggests that what is essentially a continuum may be divided into separate and stable categories, examples of which are now observable in archaeological sites.

In the above discussion of living floors, the present author adopts a position that is very similar to that of Villa (1982). As a result of her studies of conjoinable stone artefacts, Villa suggests, among other things, that artefacts may move considerable distances through deposits, even crossing lithostratigraphic boundaries in the process. Villa concludes:

> Conjoinable pieces have clearly indicated that considerable vertical movement can occur _in the absence of visible traces of disturbance_. Such displacement - which may be either postdepositional or contemporaneous with the time of burial - alters the original stratigraphic relationships of archaeological items and creates false stratigraphic associations. (1982:287)

Note that Villa is not here referring to mass disturbance of sediments containing artefacts but to displacements of individual artefacts within the sediments. She therefore suggests that the use of increasingly complex "microstratigraphic" techniques may give rise to unwarranted assumptions about temporal associations of artefacts. Much of Villa's article is constructive and thought-provoking, but the present author cannot agree with the above statement as it stands. The problem is not that our 'microstratigraphic' techniques are spuriously detailed in this context, it is that they are not detailed enough. The author suggests the following general proposition: an object cannot move through a sequence of deposits if it is significantly larger than the original voids in those deposits without leaving observable traces of its passage, granted only that no other process has later destroyed those traces. Thus, the word "visible" in the above quotation from Villa's article should not be read as meaning 'seeable' but, rather, as meaning 'seen'. Refitting is not the only technique that can be used to monitor artefact displacement of this sort, although it is extremely useful in homogeneous deposits. At the cave of Lazaret (Alpes-Maritimes; de Lumley 1969), for instance, a 'living floor' is said to comprise artefacts in a band up to 15cm thick. In places, the enclosing sediments include laminated sands and even a thin, continuous clay/silt/sand bed. The present author simply refuses to believe that the movement of artefacts through this sequence could not have been demonstrated beyond any possible doubt, had the excavators wished or known how to observe deformation structures. Furthermore, it would have been a comparatively easy matter to differentiate between trampling and more general 'artefact drift'. A refit over a significant vertical distance often suggests artefact movement, but it does not prove it; a deformation structure does. In those cases where artefacts are smaller than the deposit voids, refitting is useful, but it should not be necessary as a warning against possible artefact movement; such a possibility (probability) is obvious and any excavator who does not take this into account from the outset is incompetent. Even in the case of open screes, careful excavation can differentiate between those artefacts which could not have moved and those which might have done so. If the reader feels that such precise control will be time consuming, the author would agree, but if the reader also feels that this is a good reason not to attempt such control, he is referred to the last sentence of the quotation from Schiffer on p.662 of the present text.

Many different types of archaeological structures have been recognised from cave and shelter sediments (cf. de Lumley & Boone 1976a, 1976b; Leroi-Gourhan 1976a; Schmider 1979). Larger scale constructional features include pavements, hut/tent bases and walls, often composed of clasts of limestone or other rock types that are readily available in the cave habitat. It has already been pointed out that natural processes may quite often (i.e. much more often than ancient man) produce such features (see, for example, p.395). One wonders whether it is altogether coincidental that the foundation wall of the large tent at Le Lazaret (de Lumley 1969) lies directly below the major roof fissure in this cave, a point not apparently considered by the excavators. At Three Holes Cave (Devon), Rosenfeld (1964b) recognised mesolithic walling, with cultural debris banked up against it; although she did not provide a detailed description of this feature, she did note that it included large fragments of an earlier stalagmitic floor, a detail which seems to support her suggestion that this was indeed an artefactual feature. Concerning 'pavements', one wonders why the remarkable observations of Chavaillon-Dutriévoz (1955) in and around the caves of

Arcy-sur-Cure (Yonne) are never, to the present author's knowledge, cited in the recent literature. Chavaillon-Dutriévoz was engaged in a careful analysis of the surface features of limestone clasts and bone fragments. Supposedly 'paved' floors were known from several of these sites; these floors had been defined on grounds of both the above-average size of clasts and of the overall layout of clasts. Chavaillon-Dutriévoz also noted a significant degree of 'lustre' on the tops of the clasts in the 'pavements' which she suggested was due to the coming and going of the cave occupants. Not satisfied with this most reasonable inference, she then went about analysing the effects that the modern excavators were having by merely walking over the surface. In some areas, she found the same 'lustre' and, furthermore, the same 'pavement' effect; larger clasts had been driven firmly into the matrix, whilst smaller clasts had been scuffed to the side, near the cave walls, and all this with absolutely no conscious modification of the deposits by the personnel. In other areas, less plastic matrix and less flattened limestone clasts (features encouraging constant turning of clasts) had inhibited the formation of 'lustred pavements', despite the same amount of traffic. All this information and more was ploughed back into the analysis of the archaeological phenomena, allowing a rough estimate of the relative duration of the different occupations that was independent of the other artefactual evidence, and also allowing the differentiation of a few areas of probably truly deliberate paving. Anyone who believes that the study of site formation processes only started a decade ago should read this extraordinarily 'modern' article, which, incidentally, is by an archaeological sedimentologist. Other structures noted in caves are 'excavated' features, including post-holes, pits and trenches, structures that are generally very badly documented in the literature. Sutcliffe and Zeuner (1962) noted a shallow trench, with the spoil piled up on the side, at Tornewton Cave (Devon), cut apparently to facilitate access to the cave; no further details are given concerning this interesting feature. More complex features, such as burials, hearths or caches usually present fewer problems, although more careful description would be useful. There is still a tendency to allocate more equivocal phenomena, such as a set of articulated bones, simple lenses of charcoal or a group of three or four artefacts in a slight depression, to these emotive categories (cf. the broad concept of a 'hearth' employed by Campbell 1977). Cave art represents another set of 'structures' which need careful analysis and conscious repression of the urge to let the apparent pattern override detailed investigation of the parts. Britain has as infamous a history of misinterpretation of natural phenomena as most European countries, with respect to both parietal art (cf. p.302) and mobiliary art (cf. Jackson 1925).

The whole process of recognising archaeological structures is an extremely delicate operation since, more often than not, one is dealing with naturally available substances and spatially discontinuous information. The time definition required is, in fact, usually far beyond our present capabilities. It is all too easy to let one inference generate another. If one recognises an apparent alignment of stones as the limit of a 'hut' or 'tent', one almost automatically recognises any break in this alignment as an 'entrance', and so on. Many archaeologists just do not seem to be prepared to use Occam's Razor. In the case of the 'tent' from Le Lazaret, noted above, the proof that the stone alignment is anthropogenic is said to be that larger stones do not occur within the area defined by the alignment, whilst much cultural debris does. If we accept this proposition, could not the stones merely represent a clearing operation rather than a tent foundation? The excavators were unable to find any post-holes at this site, so that , in order to support their case, they recognised what, to the present author, appear to be totally arbitrary groupings of stones as 'chocks' for posts. By the time that the whole 'tent' has been completely reconstructed, with long poles and heavy skins, it is obvious that a few stones (most of which are quite small) around the base of near-vertical posts would not have stopped the posts sinking into the fine sediments (thus leaving traces like post-holes) and would not have made the slightest difference to the stability of the structure. So why were these stones placed there? Perhaps they do not indicate posts. And if they do not indicate posts, there is no framework upon which to reconstruct the 'tent'. With site reports like that concerning Le Lazaret, one simply goes round in circles because the observed phenomena are never clearly separated from inference. There are very few published cave or shelter sites where a really good case for interrelated structures and activity areas has been made. Even at Le Flageolet I (Rigaud 1976, 1978), which is probably one of the most convincing examples, useful information on activity areas was recovered not so much because of the techniques and concepts used but, rather, because the topography of the site, with large limestone blocks dividing up the available living space, forced both ancient occupants and modern excavators to think along similar 'compartmentalised' lines.

The will to observe is a very important prerequisite for any good excavator. For instance, ochre is an interesting substance since, if much of it is present, whole 'living floors' may be stained and thus more easily recognised (cf. the site of Spy, Province de Namur; Dewez 1980). But did the occupants of a site stain their living areas for our benefit, indeed, did they do it deliberately at all? Very careful excavation at La Grotte du Coléoptère (Province de Luxembourg; Dewez 1975) showed that, whereas the sediments were uniformly red over quite wide areas, under larger stones that had fallen after the occupation, ochre was present as discrete particles, suggesting that the general reddening is, at this site, largely due to post-occupation alteration of a particular type of archaeological material. As more accurate information becomes available from excavated sites, so the synthetic level of archaeology is stretched beyond the complacency of a mere cataloguing exercise. In the case of ochre, one may cite a paper by Audouin and Plisson (1982), which, apart from giving much useful and critical information about occurrences in many French cave (and open-air) sites, also presents a detailed discussion of the possible uses of this material and the ways in which different activities might be evidenced in ancient sites. Such an article is a powerful tool that immediately enlarges the excavator's knowledge of what to look for in a new site.

The typological approach has long proved useful in the study of 'portable' artefacts, but it can also be applied to structures. Articles by Perlès (1976) and Thiebault (1982) have greatly clarified the concepts behind the recognition of different structures involving fire. Indeed, it was at an earlier seminar on this subject (Leroi-Gourhan 1973) that a most promising approach began to emerge:

> Il a été remarqué, au cours des débats, que l'effort de réflexion se développait dans deux directions apparemment opposées. La première, traditionnelle, consiste à procéder du connu vers l'inconnu:"c'est un foyer domestique parce que ..."; la seconde, à tenir la démarche inverse: "c'est un dépot charbonneux, inséré dans une aire circulaire à haute densité de témoins de fabrication et de consommation, en nappe continue

> aux abords du dépôt, dispersés vers la périphérie, il s'agit (probablement) d'un foyer domestique". (Leroi-Gourhan 1973:1)
>
> [Trans.: During the debates, it was noticed that the thrust of the discussion was developing in two, apparently opposed directions. The first, the traditional approach, involves proceeding from the known towards the unknown: "it is a domestic hearth because ...". The second involves the inverse approach: "it is a charcoal-rich deposit, inserted into a circular area with a high density of remains indicating manufacturing and eating activities, remains that are present as a continuous spread directly around the charcoal-rich deposit but which become more dispersed towards the edge of the area; it is (probably) a domestic hearth".]

This is merely a general statement concerning various degrees of subjectivity and objectivity. However, the group did not stop at a simple plea for more detailed observation; they actually set up a 'vocabulary', a sort of 'attribute list', that can be used on site during excavation of fire-associated phenomena.

> La seconde démarche correspond à celle du vocabulaire "d'attente", c'est-à-dire qu'elle se coule dans le processus de fouille et garde jusqu'à l'imposition du nom de la structure, le champ libre aux autres éventualités. (ibid.)
>
> [Trans.: The second approach [mentioned above] corresponds to that of the "interim" vocabulary, that is, this approach fits into the process of excavation and keeps the field free for other eventualities up until the [final] naming of the structure.]
>
> Parce que les faits observables n'apparaissent dans un ordre fixe ni dans la fouille, ni dans l'esprit du fouilleur, il n'a pas été établi d'ordre préférenciel dans la hiérarchie des traits observés (A à I), sinon celui qu'impose un minimum de dépendance des faits les uns par rapport aux autres. Ce dispositif est à considérer, non comme un tableau à deux dimensions, mais comme une grappe de questions, ou comme les éléments d'un puzzle, dont l'ordre d'ajustement n'est contraignant que par rapport aux points du tableau par lesquels a commencé l'assemblage. Ce n'est, au demeurant, qu'une ébauche à reviser pour chacun dans chaque fouille, une sorte d'échafaudage à ajuster aux formes de chaque recherche. (ibid., p.41)
>
> [Trans.: Because observable facts do not appear in a fixed order, either during excavation or in the mind of the excavator, no preferential order has been established in the hierarchy of attributes observed (A to I), excepting only that which is imposed by a minimum of interdependence between facts. This device should be considered not as a two-dimensional table, but as a bunch of questions, or as the elements of a jig-saw puzzle, the order in which pieces are put into place constituting a constraint only with regard to the points in the table at which the assembly was started. However, this is only an outline to be revised by each researcher for each excavation, a sort of scaffolding to be adjusted to the form of each enquiry.]

The nine main headings (A-I) concern: the nature of the burnt material; homogeneity of the phenomenon; plan geometry; vertical relationship to the relevant 'living floor'; section geometry; the nature of any postdepositional disruption; the nature of specifically constructed attributes; the nature of any covering feature (e.g. stone piles); whether a stationary or mobile phenomenon (e.g. brazier). Each of the nine headings is followed by suggested 'angles of approach' and there is a commentary on the terms used. Thus, the heading concerning possible disruption includes decisions as to whether the feature is or is not disturbed, whether any disturbance is in the nature of dispersal or of bulk evacuation, whether any disturbance is more vertical or more horizontal, whether the cause is 'biological' or mechanical, and whether any 'biological' disturbance is due to human, animal or vegetable agencies.

This example has been discussed in some detail because, rather than being a rigid typology or even a 'key', the approach constitutes more of a 'mental discipline', which reminds the researcher of the sort of information he should be looking for without in any way forcing specific conclusions upon him. Leroi-Gourhan (1976b) has suggested that this approach may be applied to many, if not most, archaeological structures, a suggestion with which the present author totally agrees. Indeed, the author has seen a photocopy of a general outline by Leroi-Gourhan, D. Baffier and F. David, which, unfortunately, he has not yet been able to source (it is probably either in the seminar series of the Collège de France (Paris) or in the Cahiers du Centre de Recherches Préhistoriques (Paris), for 1978 or later, both of which are polycopied documents that are not very accessible). What is so attractive about this approach is that, not only is it so useful and powerful from a purely archaeological perspective, but it will also be eminently clear to researchers from other disciplines, both with respect to what the archaeologist is currently suggesting and to ways in which supporting data can be sought and integrated. For instance, in the case of a disturbed hearth, it is obvious how sedimentological and biological data could be inserted into the framework discussed above. Moreover, it is also clear how such an approach to structures could be linked with the concept of ethnostratigraphy (<u>supra</u>). Leroi-Gourhan (1976b) suggests that archaeological structures are better defined and preserved in open-air sites than in caves and shelters. As a generalisation, the present author would agree, but there is therefore all the more reason to apply a disciplined approach in these less straightforward sites.

Finally, if archaeologists can learn to see excavation as a problem-solving exercise, using any and all data that are available without too much emphasis upon 'who does what', and if so-called 'specialists' can learn to take a more active interest in archaeological problems, without necessarily waiting to be asked specific questions by the archaeologist, it will not be long before even the excavation plan itself benefits from the co-operative effort. The accurate recovery of large numbers of archaeological (and palaeontological) 'objects' is necessarily an extremely slow process. For this reason, it is not surprising that the archaeologist plans an excavation to recover the greatest amount of information (often expressed merely in terms of numbers of 'finds') with the least possible effort. 'Specialists', such as sedimentologists, must usually make do with the exposures created by the archaeological plan, exposures that may not be adequate for the task in hand. The present author has quite often been in th situation where he knows that much more precise information concerning a given sedimentary phenomenon (with archaeological implications) could be recovered by extending a trench in a given direction, but that such an extension would not be concordant with the priorities of the archaeological excavation plan. Similarly, if the archaeological plan involves horizontal excavation (decapping), it may be difficult to convince an archaeologist that the answer to certain questions involves the study of vertical sections, or vice versa. By identifying and concentrating upon specific problems, it would be much easier to establish priorities that would no longer be artificially labelled as 'archaeological', 'sedimentological' or whatever. Everyone working on a site would recognise that, if a problem is sufficiently important (interesting) and the solution seems likely to be within the general technical and material capabilities of the excavation team, then the solution should be sought, even if this means excavating 'archaeologically sterile' deposits or modifying the excavation plan within 'archaeologically rich' deposits. It is also to be hoped that the funding agencies will eventually stop encouraging bad excavation technique through their insistence, not upon accurate and consolidated results, but upon 'new' (effectively superficial) and spectacular ones. There are only two real constraints upon excavation, money and peer pressure, both of which can be made to work towards better excavation given the right initial attitude.

PART II

STUDY SITES

21. SOUTH DEVON

21.1. Introduction

The region of interest is the most southerly limestone cave area in Britain. It runs from the Haldon Hills (Cretaceous and Tertiary, over 250m O.D.) south and west, between the present coast and the Dartmoor Cupola of the Armorican Granitic Batholith (rising northwards from c.300m to over 620m O.D.), and is bounded in the west, near Plymouth, by the valleys of the Tamar and Tavy.

This is a region of complex geological structure with much faulting and metamorphism, especially as one approaches Dartmoor. In the south, the area around Start Point is composed of metamorphic rocks (gneiss and schist). Then comes an east-west zone of Lower Devonian rocks (slates, siltstones, sandstones, thin limestones, conglomerates and scattered volcanics), ending roughly at a line from Plymouth to Dartmouth. Further north is the zone of Middle and Upper Devonian rocks (common slates with some mudstone, siltstone and sandstone, and scattered volcanics and conglomerates) which also include a variety of limestones, the substrates in which the majority of known caves have been formed. A narrow zone of Lower and Upper Carboniferous rocks (mostly shales) runs along the eastern edge of the Dartmoor granite. The highly contorted and faulted geology of the region, together with the extremely variable lithology, have given rise to quite an irregular, hilly landscape, with a range in altitude from close to sea level to around 200m O.D. It is worth noting that, in merely cooler periods, the coastline would not have been markedly distant from its present position (e.g. the 70m submarine contour is mostly within 10km of the present coast). Only during the periods of greatest ice build-up would the sea recede far to the south and west (e.g. the 100m submarine contour lies c.70km south of the present coast, and the 130m contour, c.140km south-west). The drainage pattern in the region is generally radial from Dartmoor, but there is a high density of minor, twisting tributaries, that have often cut comparatively deep valleys into these rocks after superimposition from a former Cretaceous cover.

This varied, yet southerly and relatively low altitude, landscape must have been attractive to both man and animals, even in cooler periods of the Pleistocene. Caves, with palaeontological and/or archaeological remains, are known from the areas around Plymouth (Stonehouse Cave, Hoe Cave, Cattedown Cave, Oreston Caves), Yealmpton (Eastern Torrs Quarry Cave, Yealm Bridge Cave), Brixham (Neale's Cave, Bench Cave (Fissure), Brixham (Windmill Hill or Windmill) Cave, Ash Hole), Torquay (Happaway Cave, Anstey's Cove Cave, Kent's Cavern (Hole)), Torbryan (Torcourt Cave, Tornewton Cave, Three Holes Cave, Pulsford Cave, Levaton Cave), Buckfastleigh (Joint Mitnor Cave, Baker's Pit Cave), Ashburton (Lemonford Cave) and Chudleigh (Cow Cave, Chudleigh Fissure, Pixie's (Pixies) Hole, Tramp's Shelter). In addition to these better known sites, a search of the early literature shows that a much greater number of cavities were found and destroyed by quarrying, before any scientific examination of their contents could be carried out. Some of the sites in the Torbryan (p.691), Buchfastleigh (p.789) and Chudleigh (p.818) areas have been re-examined by the present author.

The variety of information from these sites is much too great for a synthesis to be attempted here, although such a document is long overdue. An introduction to the voluminous literature can be had through reference to contributions in Cullingford (1953, 1962), to Sutcliffe (1969), and to numerous accounts in issues of the Transactions and Proceedings (formerly Journal) Torquay Natural History Society and of the Reports and Transactions Devonshire Association for the Advancement of Science, Literature and Art. The region is of considerable historical interest because of its association with many of the celebrities of British pleistocene research (e.g. J. MacEnery, W. Buckland, W. Pengelly, F.E. Zeuner, J.W. Jackson, A.J. Sutcliffe, etc.). Brixham Cave and Kent's Cavern were of great importance in the debate over man's contemporaneity with extinct fauna. The latter site is still perhaps the best known (although little understood) cave in Britain. If it could be studied using modern methods, it would almost certainly provide a stratotype of regional importance. However, it will retain its major scientific interest only as long as the surviving deposits are properly conserved. The present private ownership measures the site's worth in quite another fashion and it is not adverse to furthering its interests by illegal excavation (cf. Campbell & Sampson 1971; observations in 1977 by the present author). The cave is supposed to be a Scheduled Ancient Monument but the Inspectorate (pers.comm. 1978, in reply to a formal complaint by the present author) could see no cause for alarm. Apart from (Earlier and Later) Upper and Middle Palaeolithic industries, Kent's Cavern has produced some of the oldest artefacts yet to be found in this country (cf. Pengelly 1868-84; Campbell & Sampson 1971; Roe 1981).

The open-air deposits of the region have not been very thoroughly examined and much of the work is out of date. Raised beaches have been recognised, especially in the Plymouth area (cf. Zeuner 1959; Orme 1960). The work of Green (1949) on the Dart terraces is badly in need of revision, although Brunsden (1963) has collected much more relevant data. There has been much discussion concerning the formation of tors (cf. Eden & Green 1971) and terraced surfaces (cf. Waters 1962) on Dartmoor and the evacuation of the granite debris. A paper by Harrod et al. (1973) on aeolian deposits represents exciting possibilities for the future. Periglacial features in the region have been discussed by Williams (1968, 1969) and Mottershead (1971, 1976). Also, the work of the Soil Survey of Great Britain (cf. Clayden 1971), although not specifically oriented towards the Pleistocene, does provide a very useful body of data on at least the near-surface substrates in the region.

21.2. Tornewton Cave

21.2.1. Tornewton Cave - Situation and Morphology

The Torbryan Valley appears to be an ancient route of the River Ambrook, of the Dart drainage. The Ambrook now lies just over half a kilometre to the south-west in a valley that is generally c.15-20m deeper than the abandoned section. Torbryan Valley is only c.1.5km long, but it is c.150m wide in places, and the present intermittent stream is clearly a misfit. The valley floor is rather flat and the limestone outcrops (East Ogwell Limestone, Upper Devonian), on its north-east and south-west sides, are often degraded and buried under slope mantle. Only occasionally does the limestone appear as low (up to c.10m) cliffs;

it is in these rockfaces that various caves have been discovered. The two sites which have been most systematically excavated in the past have been re-examined by the author.

Tornewton Cave (SX 816674) lies some 180m south-east of Three Holes Cave (discussed in section 21.3. below), both sites being on the wooded (Dyer's Wood) south-west side of the Torbryan Valley. Although the presently accessible parts of Tornewton constitute quite a small system, the cave morphology is relatively complex; no accurate three-dimensional survey is yet available. The main cavity is developed on a roughly north-south trending fissure, which is almost vertical (in fact, the cave entrances point c.35° east of north, but exact orientations will not be used in order to facilitate description). This fissure narrows in places to such an extent that, to all intents and purposes, it may be considered to be bridged by solid rock. Thus, the Main Chamber has a vertical extent of at least 14m whilst, towards the exterior, rock closes the central part of the fissure, giving rise to the Top Entrance and to the Lower Entrance and 'Tunnel' (terminology after Sutcliffe & Zeuner 1962); at the back of the cave, the fissure continues southwards at a low level into Rolfe's Chamber (fig.27). A.P. Currant (pers.comm.) is of the opinion, based upon recent examination of the cave, that the fissure also continues behind the cemented sediments shown, in fig.27, half way up the back (south) 'wall' of the Main Chamber. Bedrock has not been reached at any point within the main cave, and the system continues to at least 2m below the general level of the valley floor. Offset slightly to the west of the fissure, a phreatic tube, with a minor vadose trench in its floor, penetrates the rock southwards and connects with the Main Chamber by an 'elbow', mid-way between the Top and Lower Entrances; this is the

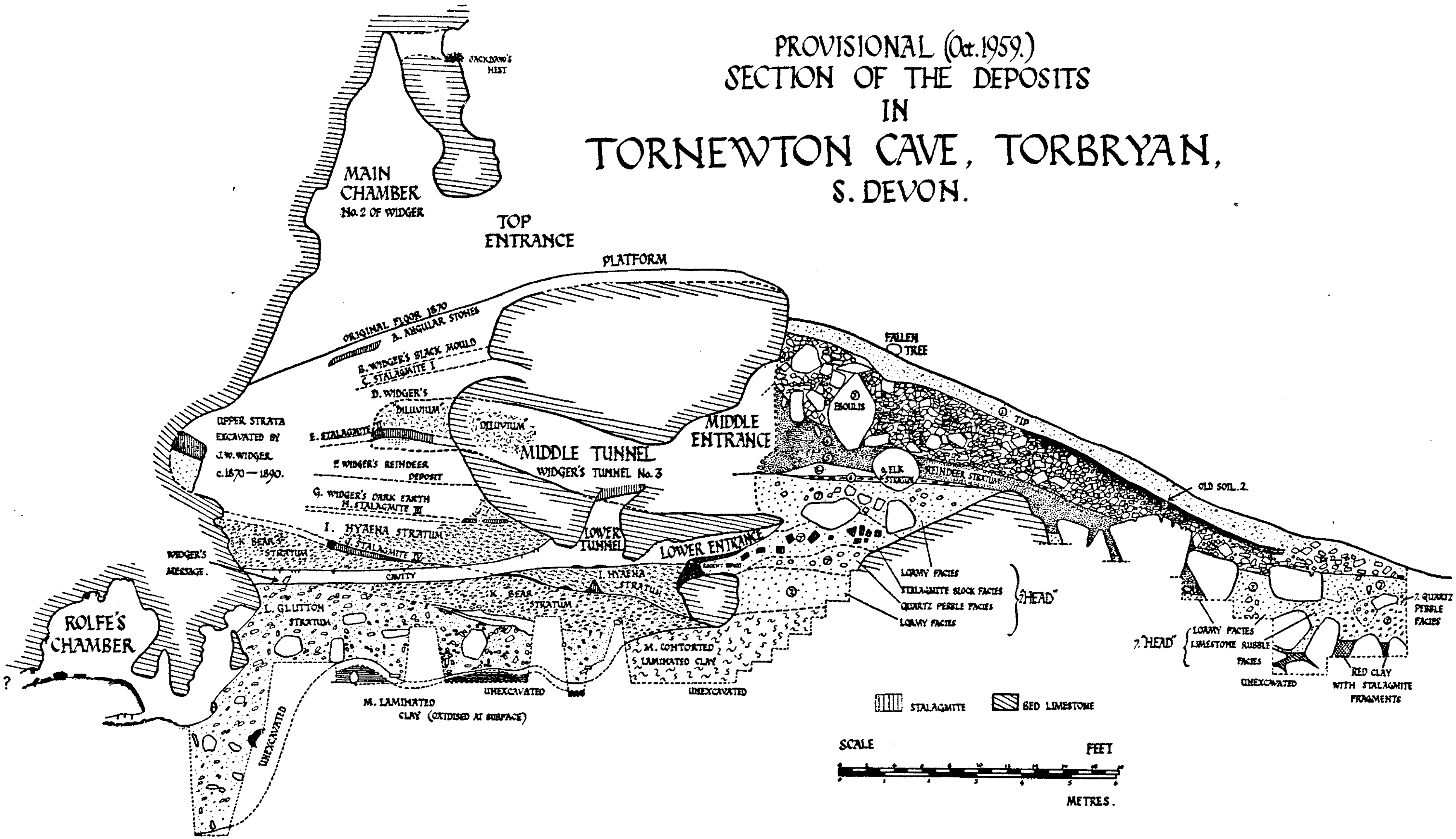


Fig.27 Tornewton Cave (after Sutcliffe & Zeuner 1962)

Middle Entrance and Tunnel (fig.27). The relationship between the three 'entrances', as seen from the exterior, is shown by Sutcliffe and Zeuner (1962, Plate 27). Yet further west is another vertical fissure, running more or less parallel to the main fissure. This second fissure, although mentioned, is not specifically named by any of the previous excavators, but it is clear that Rzebik (1968), who did not excavate but who comments on the cave's fauna, is referring to this cavity when he speaks of the "Vivian Vault". There is a tube-like connection between the Vivian Vault and the Main Chamber (fig.29). The Middle Tunnel runs between the two fissures and, apart from the eastward connection with the Main Chamber noted above, there is a small westward trending cavity, now choked with sediment, that may well connect with the northerly part of the Vivian Vault. The fissure constituting the Vivian Vault lies at a relatively low level, so that any direct, northerly connection with the exterior would be quite deeply buried beneath slope deposits. Outside the cave, the main fissure continues northwards into the open as a deep V-shaped trench in what appears to be dominantly in situ rock. The west side of this trench has cut, longitudinally, a small phreatic tube, so that the latter now appears as a 'half-tube' in the steeply inclined rockwall of the trench (fig.30).

From this description of the cave morphology it can be seen that, although the original deposits represented a sequence of considerable depth (at least 9.5m in places) and considerable horizontal extent in the north-south plane (c.27m of excavated deposits), all these sediments were very 'narrow' in the east-west plane. Within the cave, sediment bodies were rarely more than 2m wide and often considerably narrower. Outside the cave, the lower deposits in the rock-cut trench were similarly narrow whilst, towards the top of the 'V', effectively unrecorded excavation by Widger in the nineteenth century (cf. section 21.2.2.) destroyed much of the east-west extent near the rockface (outside the Middle Entrance), again leaving only a narrow sediment body for study in the 1950s. Furthermore, Widger's excavations destroyed all or most of the sediment at the Top Entrance, at the Middle Entrance, at the connection between the Middle Tunnel and the Main Chamber, and at the connection between the Vivian Vault and the Main Chamber. The excavations in the 1950s by Sutcliffe and Zeuner removed much of the sediment at the Lower Entrance. It should be noted that the earlier excavators cannot really be blamed for fragmenting the deposits in this way; caves with narrow fissures and restricted tunnels present very real difficulties and it is practically impossible to leave much material in place. Nevertheless, the cave now presents problems of stratigraphy that, sadly, are far too extreme to allow exact reconstruction. However, Tornewton is worthy of consideration since, apart from being a minor archaeological site, it certainly represents the most widely cited and discussed British palaeontological cave site.

21.2.2 Tornewton Cave - Excavations

The first excavations at Tornewton were conducted, apparently single-handedly, by J.L. Widger in the 1870s. Widger never published a detailed account of this work and those documents collated by Walker and Sutcliffe (1967) provide the only readily accessible information. In his reconstruction of the geological history of the Torbryan Valley as a whole, Widger often seems to make use of specific details relating to Tornewton Cave but it is impossible to extract any reliable information from this general text. More coherent descriptions are given in specific references to caves/tunnels, numbered 1 to 4. Walker and Sutcliffe argue that these cavities are all part of what is now known as Tornewton Cave, a proposition which seems most plausible for Nos. 2, 3 and 4. No. 1 is a tunnel left unexcavated by Widger; it cannot now be identified with certainty (cf. p.706). Note that Widger himself expressly grouped these four cavities. The following extracts represent all the available stratigraphic detail.

> No. 1 is a tunnel about twenty feet long, three feet wide, and five feet high, and is the entrance to the rocky floor of the cave. It was blocked at the mouth by boulders and diluvium, and, with the exception of the original crust lying at the bottom, was empty. These slabs were left suspended after the softer portion of the deposit was washed out, and afterwards fell to the rocky floor. [...] The contents were left undisturbed.
> No. 2 is a cave twenty feet long, four to six feet wide, and about forty feet deep. On the surface were slabs of angular stones; under this was a layer of black mould .... Under this was a white stalagmite floor about one foot thick. The next was the diluvium deposit about five feet thick. [...] Under this was another white stalagmite floor a few inches thick; and under again was the reindeer stratum, composed of red earth about six feet thick. [...] The next was a deposit of dark coloured earth about two feet thick, which emitted a nauseous gas. ... on the surface were several well-rolled quartz pebbles. Under this was the great bone bed, three feet thick. [...] Underneath again was the remaining portion of the bear deposit, which was originally about seven feet thick, but the greater portion of it had been washed out. [...] In speaking of the fine red earth of this cave, it must not be supposed that it was in a friable condition, for nearly the whole of the contents of this cave, like most of the others, were cemented by stalagmite into a solid mass, ... [...]
> No. 3 is a tunnel sixteen feet long, from two to three feet wide, and five feet high. It was filled to within a few inches of the roof with diluvium, over which was formed a thin layer of stalagmite. ... on the rocky floor were several well-rolled quartz pebbles. On making a deep cutting on the outside I found [bones]. This tunnel was the only thoroughfare to the bone bed of the cave; .... [...]
> No. 4 is a tunnel fifteen feet long, from two to four feet wide, and from six to nine feet high. Being closed anterior to the deluge, no diluvium could enter it, so that the deposits in it were as follows. The upper layer of fine red earth must have washed in through fissures in the roof. This rested on the bone bed, which was similar in character to the fossils in the cave. The next was the bear stratum, which rested on the rocky floor. This group of tunnels, Nos. 1, 3, 4, are contiguous to the hyaenas' den. (1892, quoted in Walker & Sutcliffe 1967:77-81).

The present author agrees with Walker and Sutcliffe in their suggestion that No. 2 is the Main Chamber, No. 3 is the

Middle Tunnel and No. 4 is the Vivian Vault. A schematic representation of the No. 2 sequence is shown in fig.28 of the present text. There is no reason to accept the suggestion by Lowe (1918) that the stalagmite, recorded by Widger as lying between the diluvium and the reindeer stratum in the Main Chamber (No. 2), in fact lay beneath the great bone bed; Lowe's suggestion is based merely upon a desire to recognise identical sequences in Tornewton and other caves, such as Kent's Cavern. Sutcliffe and Zeuner (1962) reported that Widger recognised another stalagmitic floor above the great bone bed (infra), but there is no mention of such a deposit in the documents assembled by Walker and Sutcliffe (1967); either Sutcliffe and Zeuner were mistaken or they had access to further unpublished documents. Widger's findings will be discussed after the stratigraphy described by Sutcliffe and Zeuner has been set out.

Lowe noted that "three enthusiastic Cave-relic hunters of Torquay" (1918:212) had dug in the Torbryan caves at some time around 1900 and had recovered flints and bones; it is not known which caves were involved, save that Torcourt (a cave further to the south-east) was certainly dug. In 1924, D.A.E. Garrod cut soundings in the Torbryan caves (Dowie 1925) but, again, it is uncertain which sites (apart from Torcourt) were examined. During the period 1936-39, A.H. Ogilvie (of the Torquay Natural History Museum) excavated, with the help of paid workmen, in Tornewton Cave, although no records survive and there was no publication. A number of bones from this excavation are housed in Torquay, finds which, when compared with provenanced material, suggest that Ogilvie worked mostly inside the cave, probably in the great bone bed and bear deposit.

Sutcliffe started examination of Tornewton as early as

1944, but most of the work was carried out in 1953-54 (outside the cave: the "Talus") and in 1957-60 (inside the cave). The following is the description of the lithostratigraphy given by Sutcliffe and Zeuner, with most of the original palaeontological and lithogenetic comments removed; their "provisional" vertical section (dated October, 1959, and which has never been revised) is here exactly reproduced as fig.27.

> I. LAMINATED CLAY (M on the section).
> The lowest deposit exposed by excavation, which could be followed continuously from the cave to the base of the talus outside, was a finely laminated yellow water-laid clay, interbedded with occasional layers of coarser grade. This stratum, which was devoid of animal remains, was examined only as far down as the water table, ... [...]
> The upper part of the "Laminated Clay" was found to have been much disturbed, especially outside the mouth of the cave, where the original stratification was hardly recognizable. Inside the cave the clay showed some brecciation and the uppermost few inches were oxidized to a rusty brown colour. The once horizontal top of the clay was of most irregular shape, being preserved several feet higher along the walls of the cave than in the centre, and plunging suddenly downwards near the back of the cave. [...]
> II. STALAGMITE FORMATION.
> In deposit III [infra] was found incorporated a large quantity of fragments of broken stalagmite and other dripstone formations. Since there were no stalagmite layers in the "Laminated Clay", these formations must originally have formed during the period of time between the laying down of this deposit and of the overlying "Glutton Stratum", in which they were incorporated. At this stage the cave apparently contained the most spectacular dripstone present at any time during its history.
> [...]
> III. The GLUTTON STRATUM (L inside the cave, absent from the talus).
> Inside the cave the "Laminated Clay" was unconformably overlain by the "Glutton Stratum", an unstratified compact earthy deposit containing abundant teeth and bone fragments, ... This deposit varied in thickness from over fifteen feet at the back of the cave, where it filled a cavity in the underlying "Laminated Clay", to nothing near the mouth of the Lower Tunnel where it thinned out entirely.
> The "Glutton Stratum" accumulated at a time when the cave was experiencing very considerable disturbance. This is indicated by the enormous quantity of broken fragments of stalagmite formations in it and by the highly irregular surface of the underlying "Laminated Clay". [The "Glutton Stratum" has a] compact texture and [a] complete lack of stratification ....
> ... it remained banked against the two arches of limestone which bridge the cave at the point where the Main Chamber joins the smaller Rolfe's Chamber, which it did not entirely fill.
> [...] The [palaeontological] remains from this stratum were all much broken and no groups of associated bones were found at any point.
> [...]
> IV. The BEAR STRATUM (K inside the cave, absent from the talus).
> The "Glutton Stratum" was immediately overlain by the "Bear Stratum", a deposit extending the full length of the Main Chamber and Lower Tunnel to a depth of two to three feet. Near the cave mouth this stratum terminated

abruptly against an arch of solid limestone which (before its removal with explosives) divided the Lower Entrance into an upper and lower opening. No corresponding deposit was found in the talus outside.

[...] The nature of the deposit ... was totally different from that beneath. Such part of the "Bear Stratum" as was not secondarily cemented by stalagmite was much less compact than the underlying "Glutton Stratum", and faint stratification was apparent in it. Occasional groups of associated bones were found, ... Furthermore, the few fragments of rock embedded in it (which were not numerous) were mostly pieces of limestone, showing that, by this time, the break up of the stalagmite formations had ceased.

[...]

V. STALAGMITIC FLOOR (J inside the cave, absent from the talus).

The "Bear Stratum" was overlain by a thin stalagmitic floor. In the Main Chamber of the cave this formed a fairly continuous sheet, a few inches thick, but in the Lower Tunnel it was represented only by isolated stalagmites, the tallest about ten inches high, which had grown up independently of one another and were not joined together at the base.

[...]

VI. The HYAENA STRATUM (= Widger's "Great Bone Bed". I inside the cave, absent from the talus).

Following the deposition of the stalagmite the cave became a hyaena den, and several feet of occupation debris accumulated throughout the length of the Main Chamber and the Lower Tunnel. Like the "Bear Stratum", this deposit terminated against the arch of limestone which bridged the Lower Entrance, and it was missing from the talus outside.

Before proceeding with the description of the "Hyaena Stratum" it is first necessary to mention a cavity which cut across this deposit and the underlying "Bear Stratum" and which at first caused some difficulty in interpreting the stratification of these deposits.

[...] As will be seen from the section, the lower deposits in the cave were able to accumulate along the full length of both the Main Chamber and the Lower Tunnel. By the time, however, that the upper part of the "Hyaena Stratum" had been laid down, the inner end of the Lower Tunnel was buried beneath the floor of the Main Chamber and subsequent sedimentation was restricted to this part of the cave and to the lateral tunnels and the talus outside. Except for a few patches of stalagmite, the "Hyaena Stratum" was the last deposit to be laid down in the Lower Tunnel.

Following the accumulation of the "Hyaena Stratum" there occurred a phase of stalagmite formation, when the deposits in the cave became partly cemented by calcite. This impregnation process was restricted to the Main Chamber, where it extended downwards to only a limited depth, beneath which the sediments, like those of the Lower Tunnel, remained unconsolidated. At a subsequent period, it would appear that the uncemented basal sediments subsided (? as a result of compaction), thus creating a space about one foot high beneath the overlying deposits, which (being securely keyed and cemented to the walls of the cave) remained suspended (see section). Small stalactites then grew upon the ceiling of this cavity. It was here, in 1877, that Widger deposited his message in a bottle.

Before it was destroyed by excavation, this "cave within a cave" appears to have extended the entire length and breadth of the Main Chamber from which it connected with the cavern space above the sediments of the Lower Tunnel.

Since the cavity traversed the cave deposits horizontally, yet the junction between the "Hyaena Stratum" and the "Bear Stratum" sloped downwards towards the front of the cave, it follows that the strata outcropping in the ceiling of the cavity became progressively younger from the back of the Main Chamber towards the Lower Tunnel. [...] The Lower Tunnel is roofed by limestone, except at one point where an unsupported stalagmite floor seals an aven.

[...]

[...] ... occasional associated groups of small bones [occurred]. Numerous well preserved coprolites were found in the "Hyaena Stratum". [...] Not only did complete coprolites occur in large numbers, but much of the deposit was composed of disseminated particles of the same substance. The upper part of the "Hyaena Stratum" even contained a local stratum of coprolitic material, ...

[...]

VII. STALAGMITE FLOOR (H inside the cave, absent from the talus).

In the Main Chamber of the cave the "Hyaena Stratum" was overlain by a stalagmite floor. This was almost entirely removed by Widger, who recorded that it was two feet thick and "fractured throughout" [cf. p.697 of the present text]. During the present phase of excavations it has been possible to examine a small remaining part of this floor, and this was found to be only an inch thick. It is suggested, therefore, that Widger's figure should probably read "two inches".

In the Lower Tunnel, the "Hyaena Stratum" was locally overlain by a few small patches of stalagmite, upon which some columns two to three inches high had grown up. These formations were considerably shattered. Since the "Hyaena Stratum" was the last deposit to be laid down in this part of the cave, these stalagmites could have formed upon it at any subsequent time up to the present day. [...]

These stalagmites are of particular interest for they were composed not only of calcite but also of a brown glassy mineral with conchoidal fracture, which has been identified by Mr. V.B. Proudfoot as the phosphate, collophane (Proudfoot, 1956). [...] The collophane occurred as a thin layer, in which the remains of a number of wood-lice had been preserved on the surface of some of the calcite stalagmites, and had also caused some local cementation of the otherwise unconsolidated cave earth beneath.

[...] The former [manganese compounds] were entirely absent and the latter [iron compounds] extremely rare [as accessory minerals to the collophane].

VIII. The HEAD (layer 7 in the talus, apparently absent inside the cave, ...).

The interpretation of the relative age of the various deposits laid down after the "Hyaena Stratum" presents considerable difficulties as most of the strata inside the cave cannot be directly related to those of the talus. It would appear, however, that the stratum of the talus known as the "Head" (layer 7) immediately post-dates the stalagmite (H) which sealed the "Hyaena Stratum". This is inferred for the following reasons.

Firstly, as the "Head" accumulated, it gradually piled up outside the Lower Entrance until this became blocked. It seems unlikely that the "Hyaena Stratum" could have been laid down after this blocking had taken place, as access would apparently then have been possible only through openings situated so high in the wall of the cave that the hyaenas would not have been able to climb out again. [...]

Secondly, a considerable number of fragments of broken stalagmite formations, partly composed of collophane, were found in the "Head". Unless there were two phases of collophane formation, then this deposit must be later than the stalagmite overlying the "Hyaena Stratum".

[...]

Since, near the mouth of the cave, the "Head" rested directly upon the "Laminated Clay", M, it also follows that the "Glutton Stratum", the "Bear Stratum" and the "Hyaena Stratum" are all missing from the talus sequence at this point. Further down the talus slope, the "Head" was found to rest upon a red clay with abundant broken fragments of stalagmite formations in it, which might be interpreted as an outlier of the "Glutton Stratum", but whether these deposits are in fact related is as yet uncertain.

Unlike the upper strata of the talus, which are laterally restricted to a narrow zone along the foot of the cliff, the "Head" extends into the floor of the valley and appears to occupy its entire width, though this has not yet been proved by continuous sectioning.

The "Head" is characterized by an almost complete lack of stratification. It was composed, however, of a number of distinct facies, with well marked boundaries, which appear to have accumulated independently and subsequently to have become mixed together in a complex manner. The principal facies are as follows:

(a) the "Limestone Rubble" facies - composed of numerous small fragments of limestone in an earthy matrix. [...]

(b) the "Stalagmite Block" facies - composed of occasional fragments of stalagmite and limestone in an earthy matrix with a high content of small fragments of slate. [...] Some of the blocks [of stalagmite] had a coating of collophane on their unbroken surfaces, showing that some collophane deposition ante-dated the deposit. [...]

At its innermost extremity, part of this facies was found to overlap onto the rock bridge which formerly divided the Lower Entrance of the cave into an upper and lower part.

(c) the "Slaty" facies - composed almost wholly of rounded cleavage fragments of slate (up to 2mm in diameter) associated with a little clay. [...]

(d) the "Quartz-Pebble" facies - composed of numerous rounded pebbles of vein-quartz and a lesser number of pebbles of decomposed local igneous rocks, contained in an earthy matrix with abundant fragments of slate. [...]

[Fig.27 shows no "Slaty facies" but it does show a "Loamy facies".]

[...]

... the boundaries between [the various facies] were found to vary from horizontal to vertical and to follow no pattern whatsoever. [...]

IX. The ELK STRATUM (layer 6 in the talus, ...).

Outside the Middle Entrance of the cave the "Head" was directly overlain by a thin richly fossiliferous horizon which has been named the "Elk Stratum" (layer 6). This deposit extended over but a small area and died out altogether a short distance from the cliff.

Although the "Elk Stratum" was found to contain faunal remains quite distinct from those in the underlying "Head" and was excavated as a distinct horizon, the matrix of these two deposits was similar and it would appear that, rather than another stratum having accumulated, the remains in the former deposit had merely been buried in the disturbed surface of the latter. The high clay content of the "Elk Stratum" and the presence in it of "numerous youthful red-brown iron concretions secondarily formed in the fabric" (Dalrymple, 1955 [cf. pp.143-151 in that document]) suggests that this deposit could be a weathering horizon on the surface of the "Head", though this is not yet proved.

[...] During this period of the cave's history man apparently made a shallow cutting through the top of the "Head" in order to facilitate access to the Middle Tunnel, and the excavated material was banked up alongside the trench in an irregular manner. A large number of bone fragments were found associated with this cutting but these occurred mainly in the banked material along its side and on the undisturbed slope below, suggesting that they were already there before the trench was cut. Very few bone fragments were found on the floor of the trench itself.

... it was found that several groups of up to five or six fragments [of bone] could each be re-assembled as single specimens. In most cases the related fragments lay fairly close together. [...]

[...]

Since the upper strata inside the cave were almost entirely excavated by Widger, it has not been possible to identify any stratum inside the cave as an underground equivalent of the "Elk Stratum". It is suggested, however, that the stratum which Widger called the "Dark Earth" (layer G) occupies such a position, for both this and the "Elk Stratum" were directly overlain by deposits which, as will be shown shortly, have been proved to be contemporaneous.

Widger recorded that the "Dark Earth" contained the remains of various animals, particulary hyaenas, and that it emitted a nauseous smell when it was dug. Only a small part of this deposit survived to be examined during the present phase of excavations and insufficient remains were found in it to determine its age. [...]

X. The REINDEER STRATUM (layers 4 and 5 of the talus = layer F inside the cave) and the GREY LOAM (layer 5a of the talus).

(a) The "Grey Loam". Following the formation of the "Elk Stratum", a thin deposit which has been called the "Grey Loam" (layer 5a) accumulated just outside the Middle Entrance of the cave. This deposit extended down the talus slope for a distance of only seven feet and is therefore relatively insignificant. It is of interest however for being a miniature talus cone, the summit of which was situated beneath the point to which the overhanging rock platform [outside the Upper Entrance] would have extended before it was cut back by the falls of rock which subsequently gave rise to layer 3 of the talus. [...] [There was a] great abundance in this deposit of small fragments of slate ....

(b) The "Reindeer Stratum". Outside the Middle Entrance of the cave, the "Grey Loam" was overlain by the "Reindeer Stratum", an almost stoneless red earthy deposit about two feet thick, which extended down the slope for a distance of approximately thirty feet, overstepping on the way from the "Grey Loam" to the "Elk Stratum" and then to the "Head".

The "Reindeer Stratum" was excavated as two layers (4 and 5 on the section), but these showed no characters whereby they could be distinguished ....

[...] Remains of only two species, the reindeer and the bovid, were abundant and these occurred in the most remarkable proportions. There were, for instance, over four hundred fragments of antlers of reindeer, but only seventeen teeth and parts of bones. The antler fragments themselves occurred in abnormal proportions, there being no straight portions of beam but an abundance of bases (mostly of naturally shed antlers) and other fragments of irregular shape. The bovid remains included nearly a hundred fragments of rib, each broken to a length of about four inches.

[...]

[...]

The "Reindeer Stratum" is one of the few deposits in the talus which can be equated with one of the strata inside the cave. The corresponding stratum is layer F. [...]

Widger reported that it [the "Internal Reindeer Stratum"] was of red colour (most of the other deposits in the cave and talus were not red) and that it "yielded nearly a hundredweight of reindeer antlers; all the sutures were smooth" (i.e. the antlers were naturally shed) "and mixed with them was a large number of rib bones. They were broken into lengths of about six inches; [...]".

[...]

... a small mass of hardened cave earth adhering to the north wall ['west' wall, in terms of the present text] of the cave [is thought to represent the only surviving part of the "Internal Reindeer Stratum"].

[...]

Since the "[External] Reindeer Stratum" occurred only as a narrow strip along the foot of the cliff, it is reasonable to suppose that it was derived from the limestone plateau above the cave .... Part of this area is still mantled at the present day with Terra Fusca (a reddish yellow soil of pronouncedly temperate character common on limestone exposures throughout the district) and it is probable that much of the "Reindeer Stratum" was derived from that source. The "Reindeer Stratum" contained numerous small concretions of haematite, which were also common in the Terra Fusca, but fragments of rocks derived from beyond the limestone outcrop were relatively scarce.

[...]

XI. STALAGMITE (layer E on the section, absent from the talus).

According to Widger, the "Internal Reindeer Stratum" was overlain by a white stalagmite floor a few inches thick. He completely removed this deposit, however, except for a local extension in the Middle Tunnel, which connects the Middle Entrance of the cave with the Main Chamber, and it could not be studied during the present phase of excavations. [...]

XII. WIDGER'S DILUVIUM (layer D, no known equivalent in the talus).

Stalagmite E was overlain by a deposit which Widger called the "Diluvium".

[...]

According to him [Widger], the "Diluvium" of the Main Chamber contained remains of rodents and bats and a few worked flints and pieces of charcoal. The deposit also extended into the Middle Tunnel ....

The "Diluvium" was entirely excavated by Widger, except for a small mass attached to the wall of the Middle Tunnel, ... This was a brown-coloured, ungraded earthy deposit containing fragments of limestone, occasional small pieces of charcoal, and a small fragment of flint. [...] Originally it [the "Diluvium"] filled the central part of the outward sloping Middle Tunnel to the roof.

[...]

XIII. STALAGMITE (layer C on the section, absent from the talus).

By the time that the last of the "Diluvium" had accumulated, the inner end of the Middle Tunnel was buried below the floor of the Main Chamber and all subsequent sedimentation was restricted to this part of the cave. According to Widger, this deposit was overlain by a white stalagmite floor about one foot thick, but he removed the whole of it and a re-examination has not been possible.

XIV. EBOULIS (layer 3 of the talus. [...]).

The deposits overlying the "External Reindeer Stratum" were totally different from those resting upon the "Internal Reindeer Stratum", and this gives rise to serious problems of correlation. ... in the talus [the "External Reindeer Stratum"] was overlain by a thick loosely packed deposit of angular blocks of limestone ....

[...]

The "Eboulis" was found to be thickest close to the Middle Entrance of the cave, which finally became blocked from the outside by this deposit, and to become progressively more attenuated towards the base of the slope, where, having overstepped from the "Reindeer Stratum" to the "Head", it died out altogether.

Except at the foot of the slope, the limestone blocks were very loosely packed and there was very little interstitial soil ....

Unfortunately the deposits at the outer end of the Middle Tunnel where the "Diluvium" and "Eboulis" might once have overlapped, were completely excavated by Widger, so that it was not possible to ascertain the relative ages of the two strata. [...]

[...]

XV. Widger's BLACK MOULD and SLABS OF ANGULAR STONES (layers B and A of the cave) and the OLD SOIL and TIP (layers 2 and 1 of the talus).

According to Widger, Stalagmite C was overlain by a layer of "Black Mould" (layer B), ... This stratum was overlain by "Angular Slabs of Limestone" which formed the floor of the cave. Both these deposits were entirely excavated by Widger and could not be re-examined, but the line of the old floor can still be discerned along the wall of the cave at the present day.

[...]

> Layer 3 of the talus was overlain by a dark humus-rich horizon (the "Old Soil", layer 2) which was apparently the actively forming soil on the slope outside the cave until it was buried at a very recent date by spoil thrown onto it from above. This soil was of the rendsina type which forms today in many places on the limestone of the surrounding district. [...] (1962:130-44)

Another fossiliferous deposit has been recognised in Tornewton:

> ... the Otter Stratum [lies] in a small chamber adjoining the Main Chamber, ...
> [...] It was composed of a mixture of broken stalagmite blocks, some containing faunal remains, in an earthy matrix, suggesting some disturbance of the deposit. (Sutcliffe & Kowalski 1976:64-5)

The present author sampled the Otter Stratum in the company of A.J. Sutcliffe in 1978; this deposit lies wholly within the Vivian Vault. The Otter Stratum is known to have been examined prior to 1968, since Rzebik (1968) reports bone specimens from this unit, as does Harrison (1980) who refers to material from the 1944-69 collections.

Certain omissions and discrepancies in the reports concerning Tornewton must now be discussed.

First, the section provided by Sutcliffe and Zeuner (1962), reproduced as fig.27 in the present text, clearly shows several stratigraphically important units that were never mentioned by these authors. The first is a remnant, shown by the hatching to be of stalagmite, which lay close to the original surface of the deposits in the Main Chamber (just left of "A. ANGULAR STONES" in fig.27); this unit will be referred to below as Stalagmite T. The second and third units, a stalagmitic formation capping a complex clastic deposit, are shown as an isolated remnant adhering to the back (south) wall of the Main Chamber; these will be referred to below as Stalagmite U and Deposit W, respectively. Sutcliffe (pers. comm. 1978) considers that these two units may be a hanging remnant of deposits older than most or all of the other deposits in the cave. The fourth unit is a complex stalagmite at the Lower Entrance, shown as growing from the rock bridge which was subsequently removed with explosives; this unit will be referred to below as Stalagmite Y. The last unit is shown as lying between the Bear Stratum and the Glutton Stratum, towards the inner end of the Lower Tunnel. In fig.27, this material has been left blank (i.e. without a compositional symbol); there is a roughly horizontal line which partially divides the schematic representation into two. This blank material has confused later authors, and Stuart (1983, Fig.4) has even compounded the initial error by arbitrarily filling the blank with a symbol indicating "silt, 'cave earth'". This unit, of unknown composition (infra), will be referred to below as Deposit Z. Note that the labels T, U, W, Y and Z imply no specific stratigraphic position or order; the letters V and X were avoided lest they be confused with the Roman numerals used by Sutcliffe and Zeuner to number stalagmites and stratigraphic 'stages'.

Certain stratigraphic problems must now be discussed. Sutcliffe and Zeuner suggest that the Head immediately post-dates H. Stalagmite (supra). The present author can see no evidence whatsoever for this suggestion. The argument concerning access for hyaenas is patently at odds with the earlier suggestion that the inner end of the Lower Tunnel was buried before the Hyaena Stratum had ceased to accumulate. The presence of 'collophane' on stalagmite fragments in the Head cannot possibly be considered to be stratigraphically diagnostic, especially since pseudo-crystalline phosphatic minerals associated with calcite are also present in the altitudinally higher Middle Tunnel (infra). The talus was dug before the interior deposits, with a three year break between the two periods of excavation. The exact point at which the talus excavation stopped is not known, nor is it known whether the second phase involved excavation into or out of the Lower Entrance. Presumably, the rock bridge was only removed when it had been cleared of surrounding sediment; if this was not so, then there is little wonder that there are stratigraphic problems. Stalagmite Y, as shown in fig.27, occupies a most equivocal stratigraphic position. Its highest portion appears to be younger than that part of the Head which it covers; the lower portion (with horizontal internal stratification?) has Hyaena Stratum banked on one side and Head banked on the other. The present author finds this configuration extremely unlikely and he has no confidence in the stratigraphic relationships suggested verbally or implied graphically. It seems highly probable that the true stratigraphic relationships were inadvertently destroyed before they could be recognised. Indeed, the fact that no exact stratigraphic relationship is described, when some relationship must (by definition) have existed, strongly supports this conclusion. The situation is further complicated by the fact that Widger made a "deep cutting" (supra) outside the Middle Entrance. By Widger's standards, the word 'deep' can hardly be interpreted as meaning less than c.1m and the subunits (facies) of the Head, that might once have been in stratigraphic contact with the Lower Tunnel deposits, are shown (fig.27) as only 1m thick. Sutcliffe and Zeuner do not record having recognised Widger's cutting. Might they not have interpreted back-fill as undisturbed Head in this highly convoluted silty deposit? Indeed, is it possible that Widger's cavity No. 1 is the Lower Entrance and Tunnel?

Fig.28 shows schematic representations of the sequences in the Main Chamber recorded by Widger, and by Sutcliffe and Zeuner. The latter authors (cf. also Walker & Sutcliffe 1967)

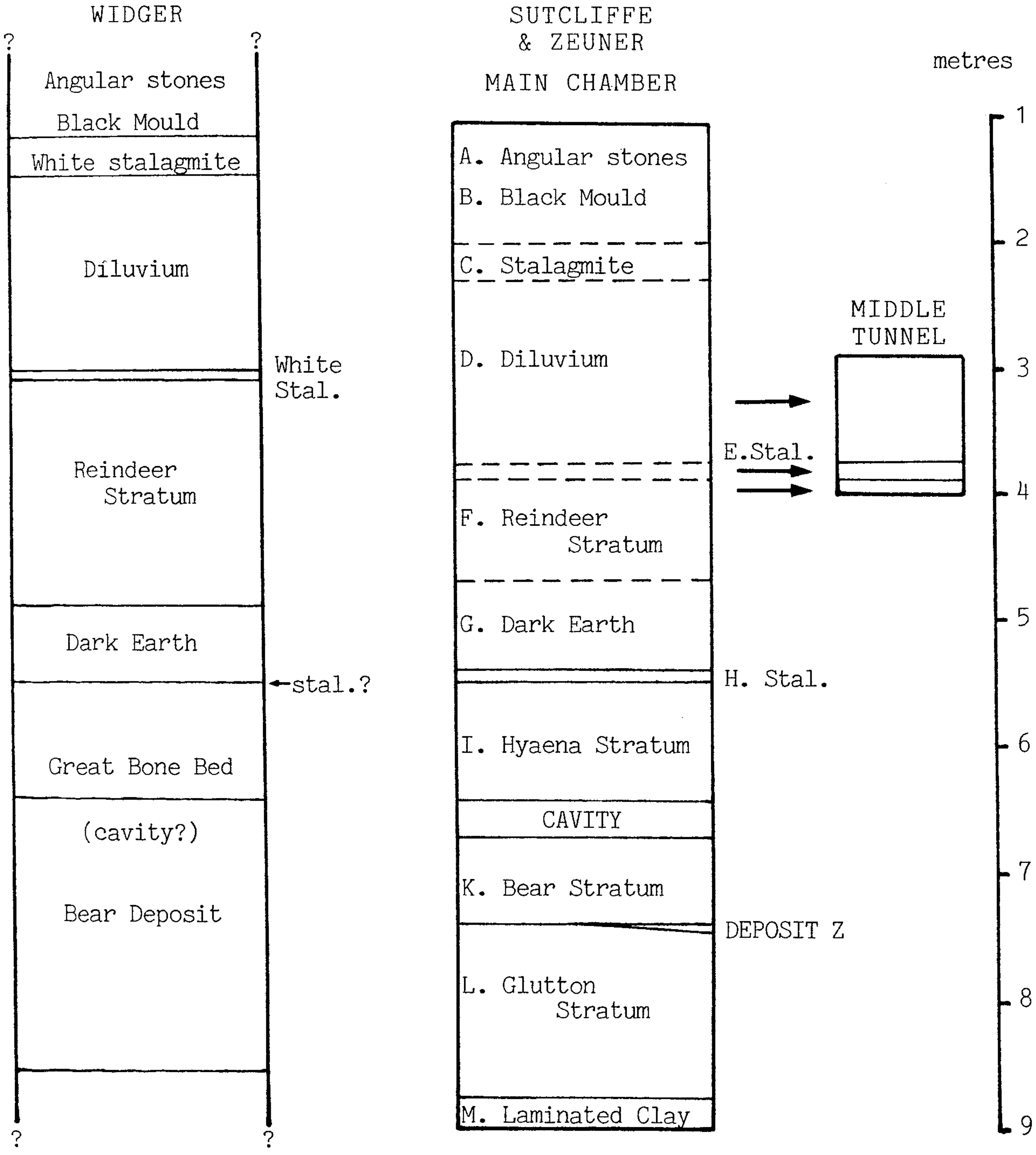


Fig.28 Tornewton Cave - Comparison of Reported Stratigraphies

correlate Widger's "bear deposit" with the Bear and Glutton Strata and the "great bone bed" with the Hyaena Stratum. These correlations seem reasonable, indeed, they are the only possible correlations. But how did Widger know that the "bear deposit" was "about seven feet thick" and why did he record that the "greater portion of it had been washed out"? Indeed, how did he deposit his message in a bottle (cf. fig.27), and how did Sutcliffe discover this message in 1954 (Walker & Sutcliffe 1967) before the major excavations in the Main Chamber? Furthermore, where did Ogilvie, and perhaps others, excavate? Somewhere in these deposits, Sutcliffe and Zeuner must have missed huge areas of disturbed sediment. The explanation for the minor (open) cavity found by Sutcliffe and Zeuner seems at least possible, but why are the sediment boundaries which cross this cavity not displaced in a step-like fashion, as would certainly be so if the lower deposits had subsided? In fig.27, these boundaries clearly imply removal of deposits, not subsidence. Are the boundaries real, or were they 'reconstructed' only after the relevant deposits had been excavated?

Above the Hyaena Stratum, Sutcliffe and Zeuner attempt to indicate the configuration of Widger's upper units. Note how, in fig.28, the thicknesses recorded by Widger (which otherwise appear reasonably accurate in this and other Torbryan caves) have been radically changed by Sutcliffe and Zeuner. This change is probably due to two factors: (a) the presence, even today (*infra*), of a thick stalagmite unit (E. Stalagmite) at the southern end of the Middle Tunnel which might appear to correlate with the stalagmite recognised by Widger in the Main Chamber, between the Internal Reindeer Stratum and the Diluvium, and (b) the fact that Widger recorded that the Middle Tunnel was filled almost to the roof with "diluvium". It will be argued below that E. Stalagmite (as it appears in the Middle Tunnel) was not recorded by Widger, and that the "diluvium" in the Middle Tunnel is not equivalent to the Diluvium in the Main Chamber. A much more coherent picture results if Widger's original deposit thicknesses are reinstated.

21.2.3. Tornewton Cave - Surviving Interior Deposits

The cave interior, as it appeared in 1978-9, is shown in fig.29, drawn in the same plane and at the same scale as in fig.27 in order to facilitate comparison. The Middle Tunnel is discussed in section 21.2.4. Very little material is left in the cave (save for the very lowest deposits); those isolated patches which do survive can usually be related to the main stratigraphy only by reference to the section published by Sutcliffe and Zeuner, that is, only by careful measurement. Sediment samples were collected at the numbered points (indicated below with the prefix 'TNW') but, because of the historical importance of this site to quaternary cave studies, the greatest care was taken to leave sufficient sediment intact for future study at every sampling point, without exception. Because no continuous stratigraphy was available, and especially because very few unit boundaries were represented in these tiny remnants, the sequence can only be described, via inference, in terms of the units recognised by previous excavators. It therefore seems acceptable to include further inference concerning lithogenesis, at this point rather than in a separate section.

Laminated Clay. Material referable to this unit was found at TNW 30 (and also at a point some 2m to the north) and at TNW 29 (and also near the bottom of the 'pit' at the entrance to

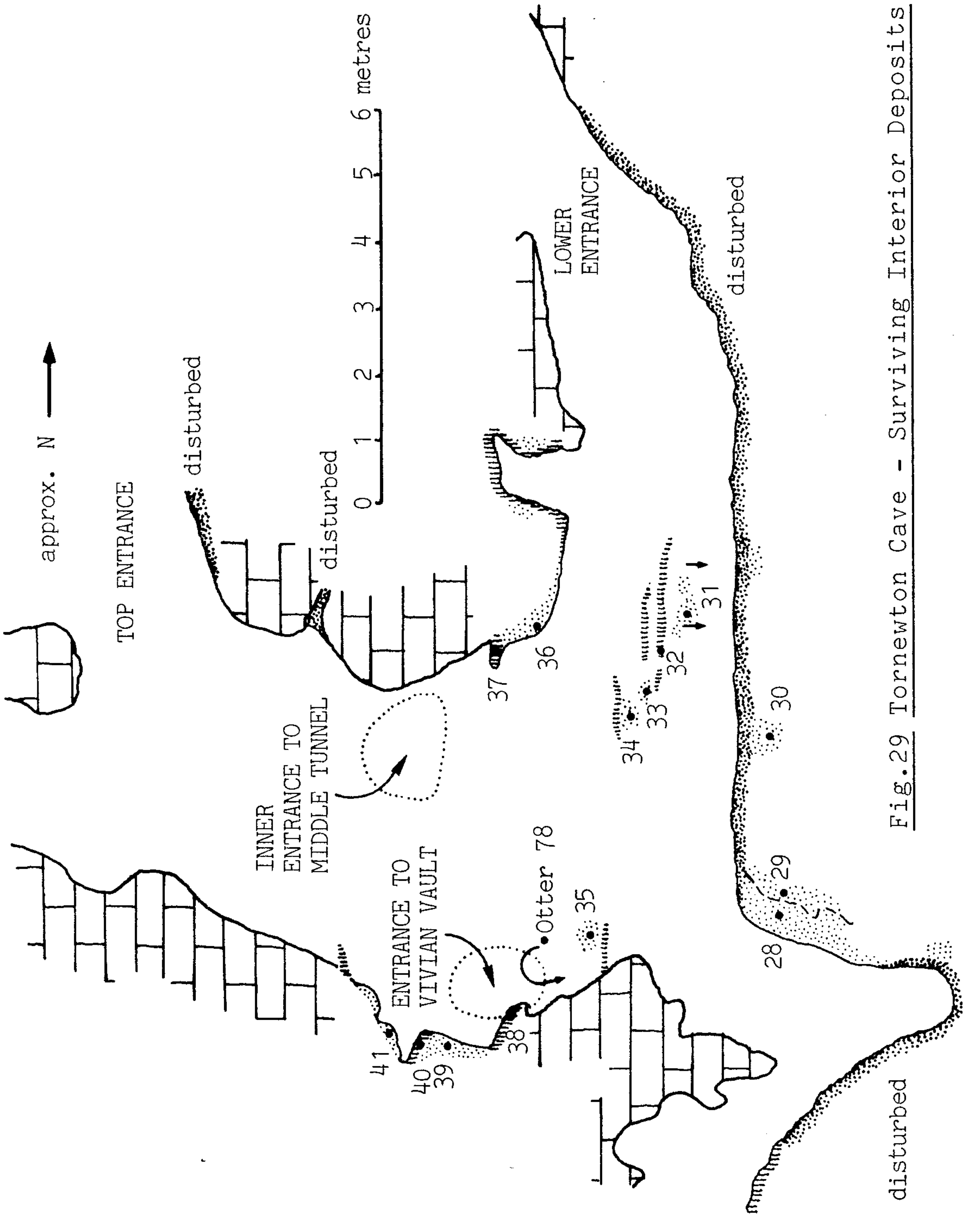


Fig.29 Tornewton Cave - Surviving Interior Deposits

Rolfe's Chamber). This is a finely bedded, non-calcareous deposit, consisting of laminations of clay and silt, with rare fine sand lenses. In the lowest exposure (in the 'pit'), there are also fine gravel lenses, with quartzite pebbles and much rounded slate debris. The matrix colour is variable, from red (10R 4/6) to yellowish brown (10YR 5/8). This is almost certainly a water-laid deposit. At TNW 30 (and at the more northerly exposure), the laminations (seen in east-west section) are sub-horizontal near the centre of the cave, but they swing round to an almost vertical orientation as they approach the east and west walls. Quite clean sand lenses display this configuration, as well as more plastic subunits, so that postdepositional deformation must have been involved. After this deformation, this deposit appears to have been radically altered from what was probably an exposed surface, since the upper 10-15cm (irrespective of present bedding angle) are heavily oxidised and weakly cemented with carbonates. Later still, the deposits were faulted (mostly along vertical planes roughly parallel to the walls with down-throw towards the centre of the cave, but with a few minor low-angle southerly thrust faults), displacing not only the stratification but also the zone of alteration (sometimes by as much as 10cm vertically). At TNW 29, the bedding is highly convoluted and there are drag folds out and down into deposit TNW 28; there is no cementation or faulting at this point.

Glutton Stratum. Material referable to this unit was found at TNW 28 and, possibly, at TNW 31 on the west wall. At TNW 28, this is a chaotic deposit, with corroded fragments of limestone and stalagmite, up to 40cm across, set at all angles in a very dense (c.1.74g/cm^3) clayey silt. The matrix is much too dense to have been disturbed by any previous excavators. The matrix colour is yellowish red (5YR 5/6). There are very common bone and slate fragments, the latter being present as coarse sand as well as fine gravel. This is almost certainly a debris flow deposit. It has obviously affected the underlying deposit at TNW 29; it seems unlikely that its emplacement was responsible for any of the phenomena noted in the Laminated Clay further to the north, although the overburden of the Glutton Stratum might have at least contributed to the faulting. The exposure at TNW 31 comprises only clasts adhering to calcite (infra); these clasts (limestone, stalagmite, bone, slate) are similar to those at TNW 28, although they are quite small.

Deposit Z. Adhering to the west wall, at approximately the position occupied by Deposit Z in fig.27, are the corroded remains of two stalagmitic formations (floors?), each c.5cm thick, with a cavity of c.0-4cm between them. The lower unit was sampled at TNW 32. The shape of these stalagmite formations closely resembles the outline and dual nature of Deposit Z in fig.27 and equivalence is therefore assumed. The two formations dip westwards by c.30° at their broken (eastern) edges, some 25cm from the west wall, but the dip rapidly increases to nearly 90° at the west wall; stalagmite is still present as far down the wall as the tips of the arrows in fig.29. Although TNW 31, which probably represents a remnant of the Glutton Stratum (supra), is cemented to the underside of the lower stalagmite (TNW 32), at no point can the calcite laminations within the stalagmite be seen to be draped over, or to be in any way affected by, the clasts of TNW 31, save that some clasts appear to have penetrated the stalagmite by pressure solution. Therefore, it cannot be demonstrated that Deposit Z formed over the Glutton Stratum.

Bear Stratum. Material referable to this unit (on grounds of measurement alone) is found at TNW 33, 34 and 35. These remnants, adhering to the west wall, are very thin and have suffered both cementation and solution to produce a highly vuggy texture. Limestone, stalagmite and slate clasts are set in a matrix of clayey silt, with areas of (infiltrated?) laminated clay, mud clasts and patches of (fine) sandier material. The colour varies with the texture but the siltier parts are yellowish red (5YR 5/6). There are many bone fragments (both megafaunal and microfaunal), of extremely varied preservation (colour, density and wear). Because these remnants are so close to the rockwall, it is inadvisable to interpret the apparently chaotic fabric as necessarily representing a primary depositional fabric. However, it should be noted that the siltier portions are almost identical (in content and texture) to the lower material assumed to represent the Glutton Stratum. Directly above TNW 34 and directly below TNW 35 are remnants of two calcitic crusts, the former stalagmitic and the latter stalactitic; these crusts might represent the floor and roof of the open "cavity" noted by Sutcliffe and Zeuner. Apart from these crusts, TNW 33, 34 and 35 show no boundaries with any other deposit.

Otter Stratum. Material referable to this unit was sampled ("Otter 78"), in the presence of A.J. Sutcliffe, from the Vivian Vault. It is almost identical to the material assumed to represent the Glutton Stratum and to the siltier portions of the material assumed to represent the Bear Stratum, save that clasts larger than 2cm are rare. The sediment body from which "Otter 78" was collected is radically undercompacted, and contains small air-holes which are not coated with clays. The fabric of the deposit is chaotic. The sediment does not adhere to the vault walls but falls away easily as it is dug. Stamping on the deposit produces a hollow echo. This is almost certainly a recent collapse deposit or, possibly, tip (anthropogenic).

J. Stalagmite. No trace of this deposit could be found save perhaps at TNW 38, where at least 8cm of stalagmite drapes the rock.

Hyaena Stratum. Material referable to this unit was found at TNW 36. The lithologic content of this deposit is almost identical to the material assumed to represent the Glutton Stratum, the Bear Stratum (siltier portions) and the Otter Stratum (with the addition of some larger clasts), including fragments of stalagmite and slate. However, there is a very much larger biogenic component, including bone (mostly megafaunal) and phosphatic aggregates (coprolites). Again, the bone shows a very wide range of preservation, including heavy, almost 'petrified' pieces and light, spongy pieces; in addition, there are many pieces showing the characteristic solutional features associated with carnivore excreta. There are also some large (up to c.12cm) clasts/aggregates, cemented by carbonates and phosphates, containing fine sediment, bones and coprolite fragments. Unlike other deposits, there is an NaOH-soluble organic component, representing c.0.7% of the material under 1mm, localised as dark amorphous patches on mineral and bone particles. The colour of this deposit is extremely variable, presenting a generally speckled aspect. The whole deposit is chaotic and variably cemented by carbonates into the narrowing fissure which represents the 'roof' of the inner end of the Lower Tunnel. It should be noted that the large 'roof' feature in the Lower Tunnel is not a rock-cut "aven", as suggested by Sutcliffe and Zeuner. This cavity is coated with crystalline calcite, of variable thickness, behind which lie very heavily altered sediments. Phosphatic material is certainly present in some quantity but it cannot be unequivocally demonstrated that these altered sediments are equivalent to the Hyaena Stratum.

Deposit W. This material, which may be obscuring a continuation of the main fissure (cf. p.692), is heavily cemented and appears to contain calcitic lenses defining several discrete units. Only one sample, TNW 39, was taken, from the least altered and cemented area. In most respects, including the biogenic component, this sediment is very similar to TNW 36, save that it has no measurable NaOH-soluble organic content (i.e. less than c.0.2%). This sediment might possibly be equivalent to the Hyaena Stratum.

G. Stalagmite. Fractured and recemented portions of a stalagmitic floor are present at TNW 37 and may possibly be referred to this unit. These fragments are not strictly *in situ* but they have material identical to TNW 36 adhering, in apparent stratigraphic contact, to their undersides. If TNW 39 is indeed part of the Hyaena Stratum, TNW 40 might be part of G. Stalagmite. The thick calcite formation, which caps the false "aven" in the Lower Tunnel, appears to be dominantly stalagmitic, although it has secondary stalactitic growths on its underside. There is a vague possibility that it may be part of G. Stalagmite.

Upper Deposits. The only surviving traces of undisturbed sediment altimetrically above G. Stalagmite adhere to the back (south) 'wall' of the Main Chamber. TNW 41 is heavily altered and vuggy. It is similar to, for instance, TNW 35, but it has less clay and a more prominent silt mode than all lower deposits. Its colour is masked by carbonates. The highest remnant is an impure calcitic ledge, which appears to have formed by massive infiltration of coarse sediment (limestone clasts) rather than as a pure speleothem; it may possibly be equivalent to Stalagmite T. These upper deposits, together with Deposit W and Stalagmite U, deserve further examination, since they are obviously quite complex and A.P. Currant (pers.comm.) feels that they may represent a coherent sequence that could possibly be related more firmly to the main stratigraphy.

21.2.4. Tornewton Cave - Surviving Deposits in the Middle Tunnel

Deposits adhering to the west wall of the Middle Tunnel are shown in the left hand part of fig.30. All these sediments are mere remnants and are cemented with carbonates. There appear to be at least two cycles of fill in this Tunnel and the deposits are best described out of stratigraphic sequence.

E. Stalagmite. This material was sampled at TNW 24, 25 and 26. TNW 26 is the main stalagmitic floor; it slopes gently upwards to the west. Underneath it are two more, perfectly distinct floors (cf. detail in fig.30). There is secondary (mostly stalactitic) calcite on the underside of TNW 25. There is airspace in places between the three floors and a little fine sediment (which may be intrusive) between TNW 24 and 25. TNW 24 appears to be in contact with the sloping rock below. All the floors have suffered re-solution; TNW 25 is the most heavily altered, having been reduced to a dense white mosaic. TNW 26 is very similar to more stalagmite which adheres to the roof of the Tunnel further north; it seems highly probable that this was once a continuous floor.

Unnamed Deposit (TNW 22). This is a very heavily indurated column of material, containing quartzite pebbles and altered limestone clasts, of sizes up to c.7cm. There is also

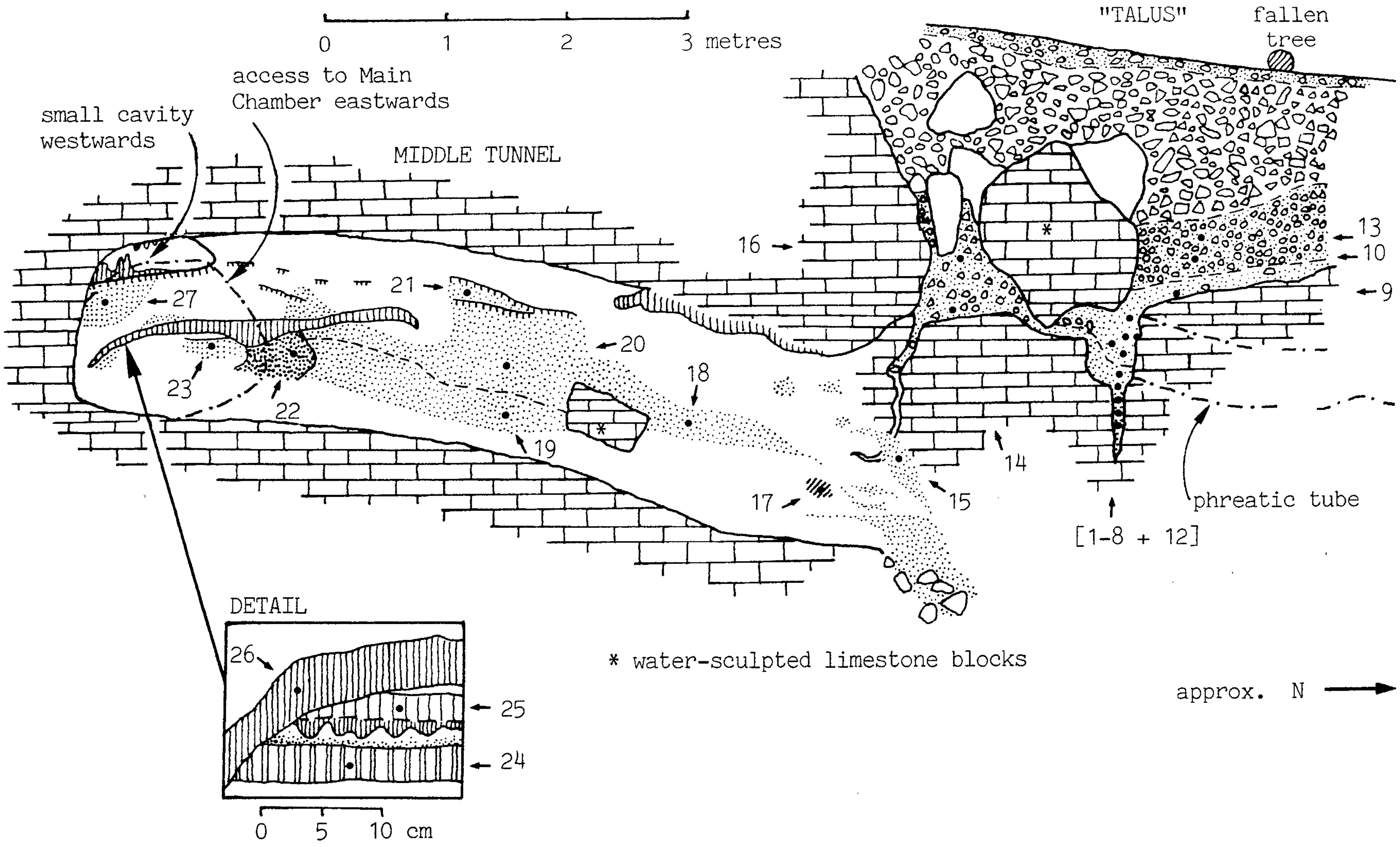


Fig.30 Tornewton Cave - Deposits of the Middle Tunnel and the Exterior

much slate debris, from fine gravel to coarse sand size. The deposit appears to be clast-supported throughout. The vuggy matrix is generally badly sorted. There are traces of weak, subhorizontal bedding. The deposit has a mottled 'yellow-red--black' colour, with iron and manganese staining, especially close to limestone clasts. The deposit is in contact with the sloping rock floor of the Tunnel and, at its upper surface, it appears to be in good stratigraphic contact with the underside of TNW 26. No contacts with the remnants of the other two stalagmites were observed.

The units E. Stalagmite and (TNW 22) would appear to represent an older sedimentary cycle and to have been massively eroded before the emplacement of the next deposits.

"Diluvium" of the Middle Tunnel. Samples TNW 18, 19, 20, 21, 23 and 27 are all very similar. They consist dominantly of a slightly clayey silt. There are quite common altered, but still angular, limestone clasts, mostly in the range 1-4cm. There are many rounded slate fragments and a few quartzite pebbles, all smaller than 2cm. The deposits are matrix-supported throughout. Larger clasts usually lie with their longest axis oriented with the general slope of the Tunnel. There are small fragments of speleothem, coprolitic aggregates, dark stained megafaunal fragments and masses of fragmented microfaunal remains, the last sometimes localised in diffuse 'pockets'. The matrix colour is generally a yellowish red (5YR 5/6), but with a tendency for the silts to be lighter (to 7.5YR 6/6) nearer the Middle Entrance and a little darker (to 5YR 4/4) towards the south end of the Tunnel. Some internal stratification may be present, although the strata would still be very similar. Between TNW 19 and 20, there is a heavily cemented zone that might indicate a boundary, as shown by the dashed line in fig.30. Between TNW 20 and 21, there is a thin calcitic crust which appears to have formed before the deposition of TNW 21. There is also a higher calcitic crust on top of TNW 21. Traces of calcitic crusts can also be seen further south and similar material caps TNW 27. This whole silty unit seems to be banked against, under or over units E. Stalagmite and (TNW 22) in such a way that it everywhere appears younger. Live, recent and subrecent mollusca are present on, and in, all the deposits of the Middle Tunnel.

One further sample, TNW 17, may be mentioned briefly. This is a small patch of material, including an 'amber' glassy substance, a deep purple glassy substance, a 'ginger' earthy substance and calcite. Test show the presence of a little iron and much phosphate. This material is present as an altered precipitate, covering the rock floor as a thin crust. It is assumed that this material is similar to the "collophane" described by Sutcliffe and Zeuner from deposits near the Lower Entrance.

21.2.5. Tornewton Cave - Surviving Exterior Deposits

Those deposits which may now be observed in the open, on the west side of the rock-cut 'trench', are shown on the righthand side of fig.30. Note that this "talus" section is set back over a metre to the west of the Middle Entrance, so that there are no stratigraphic contacts between the two areas. Behind the "talus", the bedrock rises very steeply to the west; although it might appear from fig.30 that much sediment remains, in most areas this material is only a thin remnant plastered against the rock. The remaining deposits of the "talus" cannot easily be related to the stratigraphy of Sutcliffe and Zeuner; the section now lies further to the west and all boundaries slope up quite sharply in that direction. There appear to be four main deposits, each of which will be described before discussion of how it might relate to the full stratigraphy.

The lowest deposit is represented by TNW 1, 2, 3, 4 and 5. This is a reddish yellow (7.5YR 6/6) clayey, non-calcareous silt, with a strong sand fraction composed mostly of slate fragments. Feldspar and chlorite are also present in the fine sands and coarsest silt. The deposit is massive and has slate uniformly distributed throughout, with no preferential orientation of the platy grains. There is moderately well developed columnar jointing. Open root channels are very common (with some modern roots), giving the deposit quite high macroporosity and permeability. However, the unit weight of the sediment is still relatively high at 1.63g/cm^3. There are common microfaunal remains but these are fragmentary, representing only the tougher skeletal parts. TNW 15, situated nearer the Middle Entrance, is similar in all respects, except that there are weak traces of fine lenticular or discontinuous laminar bedding. This unit appears to fit the description of the "Slaty (Loamy) facies" of the Head.

The second unit is represented by TNW 6, 7, 8, 9 and 12. There is a rather diffuse boundary with the unit below. The second unit is essentially similar to the first, save that there is more clay and a significant component of small altered limestone clasts, together with a few fragments of speleothem. The matrix colour is yellowish red (5YR 5/6). There are zones of faint bedding. Bone is present, including rib fragments of a large mammal and a shed reindeer antler base, finds which, together with the lithology, invite comparison with the External Reindeer

Stratum. TNW 14 and 16 are similar to the samples further north but they have many more, altered limestone clasts.

The third unit is represented by TNW 10 and 13. It has a very sharp, probably erosional boundary with the unit below. The third unit is again rather similar to the deeper deposits, but there is yet more clay in the reddish brown (5YR 4/3-4) matrix and angular limestone clasts (c.2-12cm) are extremely common, giving clast support. Slate is still present but bone is much rarer than in underlying deposits. This unit may also be represented to the south, close to the main rockface, but large limestone blocks make the stratigraphy in this area difficult to observe. This stony, yet comparatively matrix-rich, deposit does not match any of the units described by Sutcliffe and Zeuner.

The fourth unit is a loose. matrix-poor limestone scree (unsampled), with a reasonably sharp boundary (defined by a rise in clast size and disappearance of any significant matrix) with the unit below. This is equivalent to at least the upper part of the Eboulis.

One further sample, TNW 11, was taken from the east side of the rock-cut 'trench', at a low level which should correspond to the lower half of the Head. This is a dense silty deposit, with a few undulating bedding features. It adheres closely to the microrelief of the rock behind it. It contains two fragments of glass, both of which are certainly syndepositional with the silts themselves.

21.2.6. Tornewton Cave - Discussion

Before discussing the implications of the work carried out at Tornewton Cave over the last century, it must be pointed out that the observations of the present author are all based upon highly restricted and often degraded remnants of the former deposits. This new evidence must therefore be used with care so as not to overestimate its significance.

The most striking aspect of the present author's findings lies in the very high degree of lithologic uniformity recognised almost throughout the remaining deposits. The matrix of most clastic units is dominated by silt, and rounded slate fragments are ubiquitous. It seems very unlikely that this impression of uniformity is a mere artefact of the restricted sampling plan. Nevertheless, there would appear to be a major division into two groupings of deposits, based upon the non-carbonate component of the fine matrix (fig.31). The first (lower) grouping comprises TNW 36 (Hyaena Stratum) and TNW 28 (Glutton Stratum), along with TNW 35 (Bear Stratum) and "Otter 78" (Otter Stratum), both of which plot as very similar irregular curves to TNW 28. The second (upper) grouping comprises TNW 1 (uppermost Head), TNW 12 (? External Reindeer Stratum) and TNW 20 ("diluvium" of the Middle Tunnel), all of which are much more regular and better sorted than the first grouping. The Ipplepen Series Soil substrate will be discussed below.

The mode of emplacement and the contextual integrity of the four units of the first (lower) grouping are not always clear. Everyone is agreed that the Glutton Stratum represents a mass movement deposit of some kind.

> From its compact texture and complete lack of stratification it is inferred that [the Glutton Stratum] accumulated by a process of sludging. (Sutcliffe & Zeuner 1962:130)

> The Glutton Stratum deposits are unfortunately much disturbed, possibly by slumping, and the faunal assemblage is a curious mixture of temperate and arctic elements, strongly suggesting contamination of a cold stage fauna with material of interglacial age. (Stuart 1983:18)

The present author would add that the mass movement process could not have been merely dry or damp collapse. A fluid process, most probably debris flow, was involved. The fact that Rolfe's Chamber was not found to be filled with this material is of no great significance (contra Sutcliffe & Zeuner 1962:130) in a context where later subsidence and erosion, not to mention excavation, may have affected the deposits.

From the surviving material, the Bear Stratum appears to be texturally and compositionally similar, even almost identical, to the Glutton Stratum. However, Sutcliffe and Zeuner state that, where it was not cemented, the Bear Stratum did not contain many larger clasts (especially not stalagmite), that it was much less compact and that faint stratification was observed. Moreover, some groups of associated ('articulated') bones were found. The present author is not totally convinced that these characteristics are sufficient to dissociate the Bear Stratum (or all of this stratum) from the Glutton Stratum. Nevertheless, let us assume that the Bear Stratum (or part of it) does in fact represent a later depositional phase. How, then, were the boundaries recognised? Sutcliffe and Zeuner state that the Bear Stratum was banked against the rock bridge at the Lower Entrance and yet fig.27 contradicts this statement. Why is the lower boundary of the Bear Stratum drawn, in fig.27, as a dashed line, accompanied by question marks? Harrison records bird bones from the "Glutton/Bear Stratum transitional zone"(1980:94). Finally, the major disturbance which must have been caused by Widger was apparently not recognised by Sutcliffe and Zeuner. The present author suggests that the lower boundary of the Bear Stratum was drawn because of the occurrence of Deposit Z, which is almost certainly a dual stalagmite. Because of the failure by Sutcliffe and Zeuner to mention this deposit, it seems unlikely that it was ever present as more than a remnant anchored to the west wall. If Sutcliffe and Zeuner are correct in their suggestion that the deposits below the "cavity" have subsided, Deposit Z, which is immobile, cannot (by definition) lie stratigraphically between the Bear Stratum and the Hyaena Stratum. The present author could find no proof that Deposit Z was in primary stratigraphic contact with any other deposit. On the other hand, if the "cavity" is erosional, where did the eroded sediments go, since there is no possible exit according to Sutcliffe and Zeuner?

The integrity of the Hyaena Stratum seems better assured, at least that part of it which lies above the "cavity". J. Stalagmite lay between the Hyaena Stratum and the Bear Stratum at most points, although there is still some uncertainty nearer the Lower Entrance. The Hyaena Stratum could only have been defined by its massive biogenic component, a component which need not have been uniformly distributed throughout the lithostratigraphic unit. The reported nature of the Hyaena Stratum would certainly suggest accumulation in situ; the chaotic fabric noted by the present author is not inconsistent with the sort of localised biogenic disturbance that might well be associated with a hyaena den.

The present author cannot help but voice what seems to him to be another possible explanation for the deposits between the Laminated Clay and the "cavity". This is only a hypothesis, for which there is no concrete proof, but it would help to explain some of the anomalies in both sediments and fauna (infra). Widger suggested that the lower part of the cave was flushed (presumably inwards) after the deposition of the Hyaena Stratum, leaving this deposit hanging. If this were true, the lower deposits might merely represent 'old' sediments, formerly lying in the rock-cut trench outside the present cave, which had later been moved *en masse* into their observed position. An impression of stratification could have been created by structure in the mass movement deposits, by old material adhering to the cave walls and by disturbance and tipping by Widger. The "lower" *Hyaena Stratum* (below the "cavity") might simply have been an ancient collapse of true *Hyaena Stratum* from the false "aven" in the hanging deposits above. Naturally, this hypothesis contradicts many of the details reported by Sutcliffe and Zeuner. Therefore, it could only be elevated to a probability if it could be demonstrated that some of the faunal remains are 'post-*Hyaena Stratum*' (i.e. devensian, *infra*). It is suggested that any suspect material be radiocarbon dated.

The case of the *Otter Stratum* is difficult to assess. The material which the present author observed, and which was accepted by Sutcliffe as true "Otter Stratum", has most definitely been recently disturbed throughout. Indeed, disturbance of some kind is implied by Sutcliffe and Kowalski (1976). If "tunnel No.4" is indeed the Vivian Vault, as seems likely, Widger reported a stratified sequence of "red earth" above "bone bed" above "bear stratum", the latter resting on the rock floor. The floor of the Vivian Vault is now composed of *Otter Stratum*, not rock. The *Otter Stratum* is either mixed debris left by Widger, or it is material which later collapsed from higher in the Vault or from Widger's sections. There would appear to be no reason to suspect that the samples taken in the 60s were any less disturbed. Incidentally, if the hypothetical line indicating the southerly extension of *H. Stalagmite* in fig.27 is continued, the entrance to the Vivian Vault would not be closed at this time. If, however, Stalagmite U is part of H. Stalagmite, the Vivian Vault would certainly be closed, as implied by Widger.

Very little is known about the clastic deposits immediately above the Hyaena Stratum. Sutcliffe and Zeuner report having found a remnant of the Dark Earth, but they give no details of its composition whatsoever. One might suggest that TNW 22, in the Middle Tunnel, could represent this unit, although this would be nothing more than speculation.

The Head is a deposit of some consequence. Before discussing this further, it should be noted that the present author is by no means convinced that the Head was a unitary deposit. Thus, it does not seem entirely impossible that the lowest part of the Head was equivalent to the Bear Stratum and/or the Glutton Stratum, whether or not the above hypothesis of disturbance during the Devensian is correct. Only the uppermost part of the Head is still available for study. The general description by Sutcliffe and Zeuner is very similar to that given of the substrate of the regional Ipplepen Series Soils (brown earths) by Clayden (1971), save that stalagmite and limestone clasts appear to have been included at Tornewton (but note the possibility of intrusion of these components by the activities of Widger). Other soils in this area, such as the Ogwell Series and the Highweek Series, may also be developed upon convoluted substrates with high silt contents. Harrod et al. (1973) have studied the Ipplepen substrate and they come to the conclusion that the fine component is derived loessic material (cf. fig.31 in the present text). Sutcliffe and Zeuner indeed noted that the Head appeared to continue out into the valley. In 1979, the present author observed at least 2.5m of silt in a newly cut drain in the centre of the Torbryan Valley, just south-west of the Rectory. Similar material, but with rather

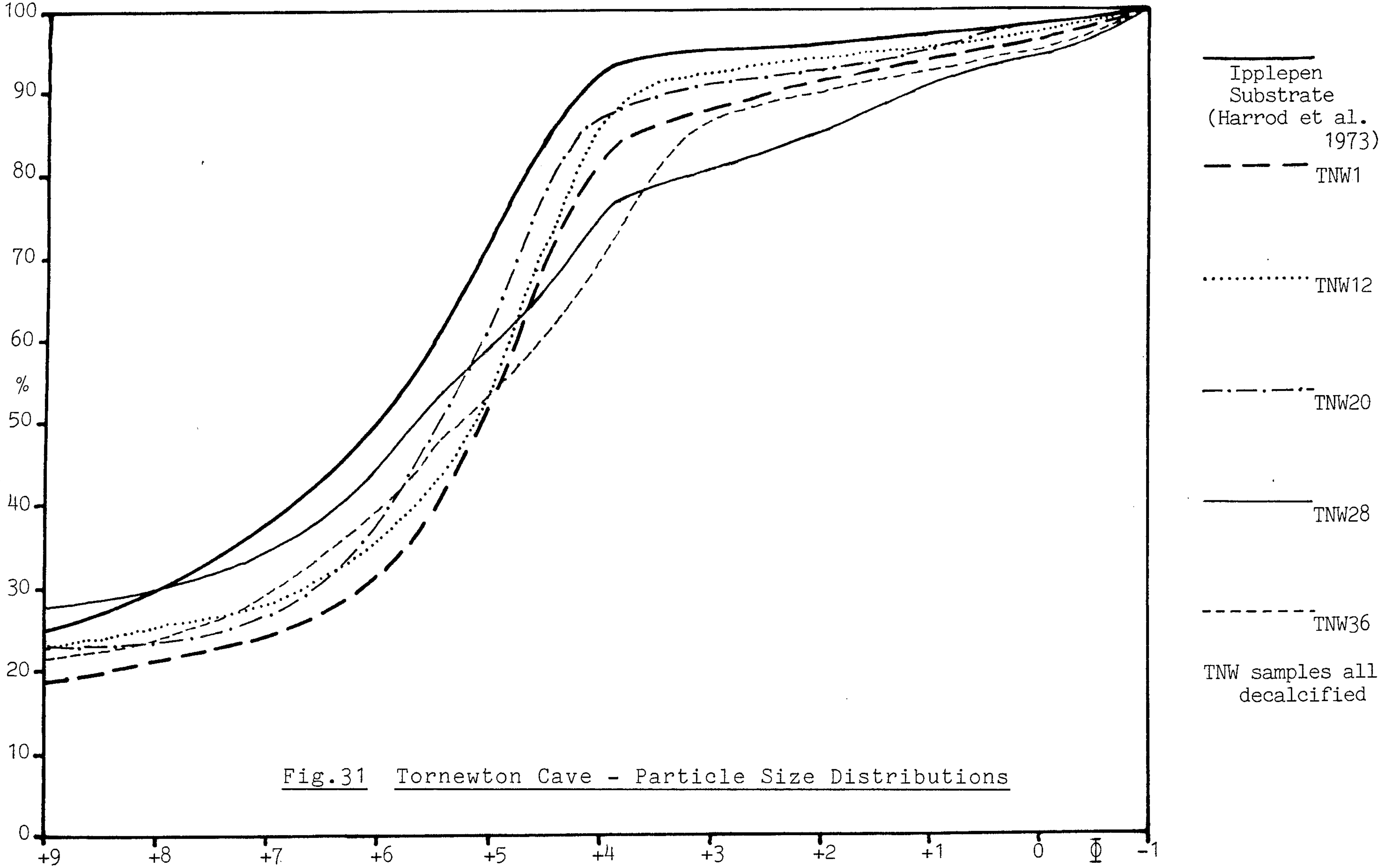


Fig.31 Tornewton Cave - Particle Size Distributions

more slate debris and more clay, is present in cuttings all along the south road out of the valley (west of Tornewton Cave), reaching an altitude of c.80m O.D. (slightly higher than the top of the limestone outcrop containing Tornewton Cave) at SX 810672. An exposure over 2m deep is present at SX 807667 and, close to the inn at Broadhampton, excavations (1979) for the foundations of a house showed particularly pure silty material to a depth of at least 1.5m. Such material is also present at least as far east as Ipplepen itself, but it seems to have disappeared as a thick characteristic unit before reaching Denbury, to the north of the Torbryan Valley, and Woodland, to the west. Further study and mapping of this slaty silt body is required but it seems likely that it will prove to be a regional unit of some importance to both cave and open-air stratigraphy.

Returning to the Tornewton Cave sequence, the lowest surviving deposit of the "talus" (cf. TNW 1, fig.30) could easily be mistaken for an in situ loess, were it not for its high unit weight, its slate content, its residual microfauna and the zones with traces of laminar bedding. If the slate component in the sands is subtracted, this unit is very similar to the Ipplepen Series Soil substrate. The choice of the term "Head" by Sutcliffe and Zeuner would appear to be most appropriate, since this unit is probably a combination of mass movement and wash deposits. Three major sources appear to be involved: material of strictly local origin (especially limestone and stalagmite clasts, if all these are original); a massive silt component of ultimately aeolian origin; and a slate component (together with shale, quartzite and a few igneous rocks). This last component must be derived from the Nordon and Gurrington Slate Province of the Upper and Middle Devonian, which almost encircles Torbryan. Surprisingly, neither silt nor slate has been found on the top of the limestone outcrop along the south-west side of the Torbryan Valley by the majority of researchers who have surveyed the area (cf. Beynon 1928; Sutcliffe & Zeuner 1962; the present author). The limestone is always thinly covered by a reddish brown (2.5YR 4/4) silty clay, lacking larger particles save for a few heavily altered limestone clasts. This material is referable to the Torbryan Series Soils of Clayden (1971) and is best classified as a local residual deposit carrying a calcareous brown earth (rendzina). There seems to be a smooth transition from Torbryan substrate to Ipplepen substrate on the gentler flanks of the limestone outcrop. However, Rosenfeld (1964) reports a slate component in the sand fraction of a "terra fusca" on top of the limestone above Three Holes Cave; the present author could only find such a component in the deposits which slope down into the valley at this point and not in those which are strictly on top of the rock outcrop. Slate, together with other exotic rocks, appears to be present throughout the Tornewton sequence; indeed, these rock types also occur elsewhere in the valley at Three Holes Cave (cf. p.766), Torcourt Cave (Dowie 1925) and Levaton Cave (Carreck 1957). The exotics could not have reached the caves over such long periods (judging from the fauna, in excess of 100ka) if they had merely been available as gravels in the valley bottom. It must be assumed that the area was more or less blanketed with an ancient slaty 'drift', most traces of which were finally stripped from the top of the Torbryan limestone at a late date in the Pleistocene. Slate often decomposes to a clayey silt, so that this might possibly be the main source for the matrix of the first (lower) grouping of units inside Tornewton. For the moment, the ultimately aeolian silt can only be differentiated by its particle size distribution (cf. fig.31). Mineralogical study would certainly prove useful (cf. Harrod et al. 1973) but, unfortunately, the present author is not equipped to extract heavy minerals of silt size. Harrod et al. suggest that the aeolian component in this region dates from the Devensian Maximum, a proposition which is concordant with the fact that the Elk Stratum at Tornewton still contains elephant (mammoth?) and woolly rhinoceros, animals that seem to have disappeared from Britain before c.15,000 B.P. (cf. Stuart 1982). However, it should not be forgotten that pre-ipswichian loessic material is known from S.E. England (cf. Catt 1977), although, in the Torbryan context, it seems unlikely that aeolian material would have remained perched on high points in the local relief for very long after its initial deposition.

The surviving deposits of the "talus" would seem to represent a gradual shift from the relatively pure exotic slate and silt components to deposits with increasing local components, culminating in the pure scree of the Eboulis. There is no major lithologic discontinuity in this sequence. The first (cf. TNW 1) and second (cf. TNW 12) units in this sequence are so similar, in all respects, to the surviving silty material in the Middle Tunnel (cf. TNW 20) that they all must surely correlate, at least in an approximate manner (i.e. they all belong to one general chronozone). If the second unit of the "talus" is indeed equivalent to the External Reindeer Stratum, then it is difficult to believe that the silty material of the Middle Tunnel is equivalent to the Diluvium of the Main Chamber. Widger recorded "diluvium" almost filling the Middle Tunnel, a deposit which was capped by a thin layer of stalagmite. This stalagmite would seem to resemble the surviving calcitic crusts (fig.30), rather than the massive, tripartite E. Stalagmite which lies mostly below the silty material.

The present author submits that Widger did not notice the eroded, older cycle deposits (E. Stalagmite and (TNW 22)) in the Middle Tunnel, save for the reference to quartz pebbles lying on the rock floor. From fig.28, it can be seen that, when Widger's original deposit thicknesses for the Main Chamber sequence are reinstated, the top of the Internal Reindeer Stratum comes very close to the top of the inner entrance to the Middle Tunnel, and that Widger's "(lower) white stalagmite" is on a level with the calcitic crusts (fig.30) in the Middle Tunnel. Widger (cf. Walker & Sutcliffe 1967) recorded bat remains from the Diluvium of the Main Chamber, as well as the fact that he did not find "fossil" bone until he had reached the Internal Reindeer Stratum. The fauna recorded by Sutcliffe and Kowalski (1976) from the "diluvium" of the Middle Tunnel is identical (eleven species) to that recovered from the External Reindeer Stratum, save for the addition of holocene mollusca (cf. p.719 in the present text) and teeth of the wood mouse (Apodemus sylvaticus), both of which could easily be recent intrusions into these remnants which have been exposed at least since Widger's excavations.

If the silty deposits of the Middle Tunnel are roughly correlative with the uppermost Head, the Elk Stratum, the Grey Loam and the External Reindeer Stratum of the "talus", it also follows that the Internal Reindeer Stratum must be placed in a similarly vague chrono-correlation. Sutcliffe and Zeuner (1962) correlate the Internal and External Reindeer Strata on the grounds of reddish colour (which is a most unreliable criterion) and also because of the shed reindeer antler bases and rib fragments found in both deposits. It would seem to the present author that this last point is probably remarkable enough to allow correlation of part of the Internal unit with part of the External unit (i.e.

there was once a continuous chronohorizon). These unusual organic components were so common that the present author was even able to find them during a mere section cleaning operation (cf. p.720). However, it still cannot be claimed that the two lithostratigraphic units are wholly or even dominantly time-correlative.

The important faunal collections from Tornewton may be briefly discussed in the light of the data presented above. The present author is primarily concerned with context; the reader should refer to the publications cited for more detailed palaeontological information. It should be noted that the Tornewton fauna has never been published in full and that considerable numbers of specimens have received no public comment.

The Hyaena Stratum would appear to be a reasonably discrete unit containing a fauna characteristic of the Ipswichian (the Upper Pleistocene 'hippopotamus' fauna; cf. p.592). The Elk Stratum fauna (which does not contain elk; cf. Stuart 1983) appears consistent with a later devensian date; the present author was unable to recognise this deposit (or horizon) in the surviving sediments. The External Reindeer Stratum, like the deposits below it, almost certainly received some input from slope wash but its fauna seems generally acceptable as a Late Devensian assemblage, although the hyaena and rhinoceros remains (a few bones only) seem out of place. Alternatively, Stuart suggests that not only the Elk Stratum but also the External Reindeer Stratum should be placed prior to 18,000 B.P. (1982) or prior to 15,000 B.P. (1983), dates which would allow the hyaena and rhinoceros to be contemporary with the other fauna of the External Reindeer Stratum, except that the mole would no longer be acceptable (Stuart 1983) because of its supposed intolerance to frozen ground. If the External Reindeer Stratum were deposited under permafrost conditions (of which there is no sign in the sediments themselves), the present author would have thought that the bank vole (Clethrionomys glareolus) and the little bustard (Otis tetrax; Harrison 1980) would also be out of place. Note that this early dating does not appear to be concordant with the archaeological remains recorded from this unit (infra).

Only the faunas of the Glutton Stratum, the Bear Stratum and the Otter Stratum remain to be discussed. The main reason for the widespread renown of Tornewton Cave lies in the apparent evidence for major climatic oscillation.

> The most important sequence of stratified cave deposits with rodent remains known from the British Isles is that of Tornewton Cave, South Devon .... Deposits in the shaft-like Main Chamber of this cave and of the talus deposits outside span a cold-interglacial-cold sequence which is unique in showing differences between the rodent faunas of the two cold stages concerned. (Sutcliffe & Kowalski 1976:62)

Sutcliffe and Zeuner (1962) refer the Glutton Stratum and the Bear Stratum (the Otter Stratum not having been specifically recognised) to the "Penultimate Glaciation". Sutcliffe (1969) refers the Glutton Stratum to the "Gipping Glaciation" and later (1974a, 1976) to the "Wolstonian Glaciation". Sutcliffe and Kowalski (1976) refer the Glutton Stratum to the "Tornewton Cave 'Glutton Stratum' Cold Stage" of the "Upper Pleistocene" (having moved the Middle/Upper Pleistocene boundary back to the end of the Hoxnian!), and the Otter Stratum to the beginning of the "Joint Mitnor Cave Interglacial" (i.e. the 'hippopotamus fauna' interglacial; cf. p.809). The stratigraphic position of the Bear Stratum is unclear, but Sutcliffe and Kowalski appear to suggest that it is intermediate between the Glutton and Otter Strata. Sutcliffe (1977:38) states that the Glutton Stratum is "(presumably) Wolstonian", the caveat being directed not at any uncertainty in the Tornewton deposits but rather at uncertainty as to the definition of the Wolstonian itself. These stratigraphic allocations are the source of the Tornewton myth, which has been perpetuated, with colourful variants, by many other authors (e.g. Kurtén 1963, 1968, 1969; Rzebik 1968; West 1968, 1977; von Koenigswald 1973; Maglio 1975; de Lumley 1976; Mottershead 1977; Stuart 1977a, 1982, 1983). Jánossy (1975) even uses the *Glutton Stratum* microfauna to help define the biostratigraphic *Solymár Phase*, claimed as valid for the whole of Europe. Atkinson and Smart are alone in suggesting that the *Glutton Stratum* "apparently belongs to the Last Interglacial (Ipswichian)"(1977:39). Finally, Stephens (1973) outdoes everyone else by allocating the *Bear Stratum* to the "Ipswichian", the *Glutton Stratum* to the "? Wolstonian" and the *Laminated Clay* (which contains no fauna or environmentally diagnostic sediments, and for which there is no dating evidence whatsoever) to an "interglacial deposit", possibly the "? Hoxnian".

The present author is inclined to write off the faunal collections from the *Glutton*, *Bear* and *Otter Strata* as mixed, presumably (mostly) pre-ipswichian (Middle Pleistocene) material, of interest only in as much as a few unusual, or even unique, taxa are individually represented. However, it seems unlikely that the Tornewton myth will disappear merely because it can be unequivocally demonstrated that the contexts of these early faunas are totally unreliable. It will therefore be necessary to go into a little more palaeontological detail.

Sutcliffe and Kowalski (1976) suggest that the *Glutton Stratum* fauna is differentiated from devensian faunas by the presence of the common hampster (*Cricetus cricetus*), of a possibly extinct hampster sometimes referred to as the dwarf or migratory hampster (variously allocated as cf. *Allocricetus bursae*, *Cricetulus bursae*, *Phodopus songorus* or *Cricetiscus migratorius*, depending upon which publication concerning Tornewton one reads), of the steppe lemming (*Lagurus lagurus*) and of the snow vole (*Microtus nivalis*). Sutcliffe and Kowalski state that these "are typical species of the 'penultimate' glaciation"(1976:63). Since no comparable faunas exist in Britain, these authors must be referring to nearby Europe, where the species cited occur throughout the Middle Pleistocene and, with the possible exception of *Allocricetus*, on into the last glaciation; therefore "typical" must not be read as meaning "diagnostic". Stuart (1982) has argued that snow vole cannot be identified from lower first molars and he has reallocated all the Tornewton specimens once referred to this species to the banal northern vole (*Microtus oeconomus*). We may also note the clawless otter (*Aonyx* (or *Cyrnaonyx*) *antiqua*), which should be pre-ipswichian. The majority of the remaining fauna from the *Glutton Stratum* would appear to indicate a generally 'cold' environment. However, also included are remains of an extinct rhinoceros (*Dicerorhinus hemitoechus*; reidentification, reported in Stuart 1983, by an unnamed researcher of the specimens previously referred to cf. *Coelodonta* sp.) and of the crossbill (*Loxia curvirostra*; Harrison 1980), neither of which should occur in a 'cold' fauna. Stuart also objects to the badger (*Meles meles*), which, for some unknown reason, he qualifies as "distinctly temperate"(1982:147). Whatever the climatic implications of badgers, it is worth while noting that these animals often dig large sets within cave sediments. Harrison (1980) identifies a new (extinct?) species, the western partridge (*Alectoris sutcliffei*), from this unit; the partridge family are temperate birds. Sutcliffe and Zeuner (1962) also recorded unspecified bats and shrews from this unit, animals which are not usually very cold-tolerant.

Concerning the Bear Stratum, Sutcliffe and Kowalski state:

> Only a few rodent remains were collected fron the Bear Stratum. The fauna does not differ in composition from that of the underlying Glutton Stratum though, with the exception of one tooth of Lagurus lagurus and four of Microtus nivalis, the typical 'penultimate glaciation' elements are lacking. Their absence could be accidental, however, since they are also rare in the Glutton Stratum. The absence of lemmings could indicate a slight amelioration of climate. (1976:64)

The identification of snow vole is not acceptable (supra), so that only one tooth of the steppe lemming is left to represent the distinct "penultimate glaciation" fauna. Note that, in addition to the steppe lemming, the Norway lemming (Lemmus lemmus) and the arctic lemming (Dicrostonyx torquatus) are present: it is assumed that "lemmings" should read "hampsters" in the above quotation. Stuart does not seem to be able to make up his mind concerning the difference between the Glutton and Bear Strata; he suggests that the faunas from the two units are similar, but that the Bear Stratum "lacks evidence of periglacial conditions" (1982:146) or, alternatively, that it lacks the "interglacial elements" (1983:18). As was noted above, birds were recovered from the "Glutton/Bear Stratum transition zone". It has been brought to the present author's attention by A.P. Currant (pers.comm.) that this "transition zone" also contained mammal remains, one of which (British Museum Natural History Catalogue Number M.49382) is a fragment of a milk tooth of Hippopotamus - the animal which more or less 'defines' ipswichian faunas!

Concerning the Otter Stratum, Sutcliffe and Kowalski state:

> This fauna appears to be a mixture from two originally separate layers. Remains from a warm period, including Microtus oeconomus, predominate. (1976:65)

Not including "Microtus nivalis" remains, the northern vole represents c.64% (n = 138) of the identified rodents from this unit, c.48% (n = 31) of those from the Bear Stratum and c.64% (n = 1660) of those from the Glutton Stratum. It is not clear what point Sutcliffe and Kowalski are trying to make here, especially since the northern vole is in fact a generally 'cool' species, occupying many different biotopes, from tundra to northerly deciduous forest (cf. Corbet 1966). Nevertheless, 'warm' elements, such as the wood mouse (Apodemus sylvaticus), and 'cold' elements, such as the common and steppe hampsters, are indeed included in the Otter Stratum. Also present are the clawless otter and a white-toothed shrew (Crocidura sp.; Rzebik 1968), the latter being referred to the Ipswichian by Stuart (1982).

Further comparison between the Glutton, Bear and Otter Strata has been attempted. Harrison (1980) notes that by far the most common bird in all three of these units, and also in the Hyaena Stratum, is the common shellduck (Tadorna tadorna), which is exclusively a coastal bird, implying the close proximity of the sea (relatively high sea level) throughout this period. Von Koenigswald (1973 and pers.comm. in Sutcliffe & Kowalski 1976) has suggested that, although the water voles from both the Glutton and Otter Strata are of the intermediate type known as Arvicola cantiana-terrestris, the Otter Stratum remains are slightly later (i.e. more like 'terrestris'). Stuart (1983) refers the water vole remains from the Glutton Stratum (he does not mention those from the Otter Stratum) to Arvicola sp., presumably because the 'cantiana' and 'terrestris' morphotypes are known to occur in single populations, with all possible transitions (cf. Chaline 1972; Stuart 1982); since the water vole is environmentally catholic, it is impossible to identify the phylogenetic stage (a parameter of the population, not of the individual) in the mixed collections from the Glutton and Otter Strata. Kurtén (1963) has made a similar assumption that the "Riss" hyaenas from Tornewton may be treated as a discrete population for biometrical purposes.

It would appear to the present author that no amount of 'explanation' will suffice to allocate the faunal material from the Glutton, Bear and Otter Strata to plausible biostratigraphic, chronostratigraphic or ecological groupings.

The archaeological material from Tornewton Cave is presumably all of Upper Palaeolithic , or later, age. Widger recovered flints from the Diluvium (Holocene ?) and Internal Reindeer Stratum of the Main Chamber, and from the "diluvium" of the Middle Tunnel. However, he did not describe individual pieces, and flints in the extant collection from the Torbryan Caves are not specifically labelled. Campbell (1977) recognises no E.U.P., only L.U.P., material from this general collection. Sutcliffe and Zeuner (1962) recorded two archaeological levels. The lowest was associated with the Elk Stratum. Here, a number of conjoinable bone fragments, some with "abraded" and polished areas, were found in sediment, interpreted as tip piled up along the side of a shallow trench which had been dug into the Head in the direction of the Middle Entrance (cf. p.701). It is most regrettable that this apparently archaeological feature was not described in proper detail, especially since some doubt must remain because of the failure to recognise, or at least to comment upon, Widger's earlier excavations outside the Middle Entrance. Also from the Elk Stratum (but not from the trench) came two unretouched flint flakes and a quartz-tourmaline pebble, all of which are accepted as archaeological by Campbell (1977), who refers them to the E.U.P., presumably because of the apparently associated fauna. Morrison (1980) follows Campbell's suggestion on this point. Note that Campbell incorrectly states that these finds came from "layer 9"; the Elk Stratum is "layer 6", but it is discussed in section IX of the 1962 text and is referred to "Stage IX" of the composite stratigraphy. The second archaeological level occurred in the External Reindeer Stratum. The unusual accumulation of shed reindeer antlers and fragmented bovid ribs (also noted by Widger in the Internal Reindeer Stratum) may well be archaeological material. The present author has not seen these finds but it is clear that they should be re-examined. Sutcliffe and Zeuner (1962) recorded four flints, which Campbell (1977) classifies as three waste flakes and a broken angle-backed point; he also lists a bone tool like a spatula, not mentioned by Sutcliffe and Zeuner. There was no flint debris (>0.5mm) in any of the present author's "talus" samples. On the grounds of the angle-backed point, Campbell suggests that this is L.U.P. material, presumably younger than 15,000 B.P. Molleson (1977) seems to follow this suggestion in connection with the human incisor from the External Reindeer Stratum. It was stated above (p.733) that this dating clashes with Stuart's reading of the fauna.

In summary, it appears that nothing very useful can be learnt from the archaeological remains at Tornewton Cave, save that man was present in the area at some time(s) during the later half of the Devensian.

21.3. Three Holes Cave

21.3.1. Three Holes Cave - Morphology

Three Holes Cave (SX 816675) lies some 180m north-west of Tornewton Cave. The general situation has already been described in section 21.2.1. The north-easterly (main) entrance of Three Holes cave opens c.4m above the level of the valley floor, thus at c.68.5m O.D. There is at least one minor fissure in the rock floor at this entrance. The rock then rises gently inwards, reaching c.70m O.D. at the limit of the excavations. Cut into the floor of the cave is a vadose trench that is c.1.5m deep and over 1m wide where it disappears, in a south-westerly direction, under the surviving deposits; this trench, which has a roughly horizontal floor, 'fades' towards the north-easterly entrance. The plan morphology of the cave is shown in fig.32. From the north-easterly entrance, the cave continues as a passage which swings round to a more southerly orientation. This passage then meets a 'chamber', which is in fact an intersection with a north-west to south-east trending fissure. It seems highly likely that the passage and vadose trench continue on towards the south-west. The cross-fissure continues upwards, its walls sloping to the north-east, and eventually connects with the surface. However, it is partially choked with large limestone blocks or bridged by rock and stalagmite, so as to give the impression of three main 'holes' in the chamber roof. The fissure continues towards both the north-west and south-east, both areas being almost blocked by sediment. There are also a number of choked avens or dome-pits in the roof, to the east side of the passage.

Considerable amounts of sediment survive in this cave, especially at and outside the main entrance and around the southern and western peripheries of the chamber. However, it was not practical to cut extensive new sections, since the exterior sediments are capped by massive and unstable boulders and the deposits in the chamber are mostly highly cemented. Sections were cleaned, but no attempt was made to cut them absolutely straight.

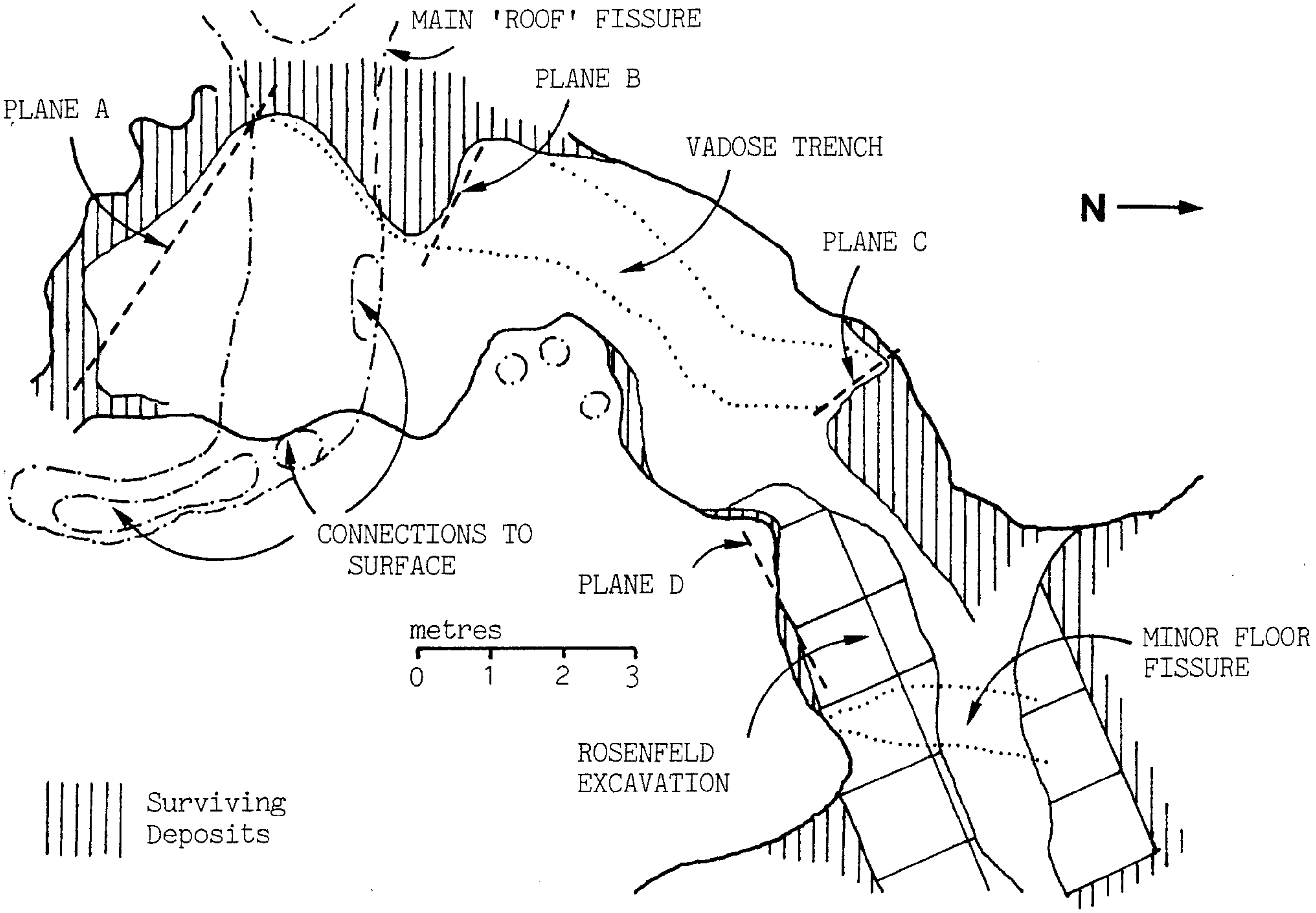


Fig. 32 Three Holes Cave - Plan

Thus, fig.34 shows a composite vertical section, looking into the system, with the upper part consisting of deposits projected onto plane A and the lower part (the vadose trench) consisting of deposits projected onto plane B. Fig.35 shows a view, looking out of the north-east entrance, projected onto plane C, and fig.36 shows a longitudinal view of the entrance deposits that remain against the south-east wall.

21.3.2. Three Holes Cave - Previous Excavations

Widger was again the first, and major excavator of Three Holes Cave, a site which he appears to have discovered at about the age of twelve (i.e. c.1835). He worked at the site intermittently for many years, but the main phase of excavation was probably around 1880. The other formal excavation of this cave was carried out by Rosenfeld (1964b), in association with F.E. Zeuner and E.N. Masson Phillips, during the period 1955-61.

Both Widger and Rosenfeld excavated in the thick slope deposits (the "talus") that almost sealed the north-eastern entrance. Here was found a stratified holocene sequence, consisting of wash deposits with incipient soils, interspersed with boulders and scree. Degraded sections of this material still survive but, because of their unstable and generally coarse nature, the present author did not investigate them. However, cursory inspection did indeed show what appears to be an irregularly accreting soil system of the rendzina type, as suggested by Rosenfeld. Furthermore, both Widger and Rosenfeld recognised occupation debris, including animal bones, flints, charcoal and pottery sherds, banked up against a crude wall, constructed across the main entrance with limestone and stalagmite blocks. It would appear that this archaeological material dates from the Late Mesolithic and/or Early Neolithic (Rosenfeld 1964b). The remainder of this section will involve discussion of the pleistocene deposits at Three Holes Cave.

There are three brief accounts of Widger's work at this site:

> ... No. 2 cavern [is] 50 feet long, from 8 to 12 feet wide and from 15 to 25 feet high. The deposits here are as follows: on top, angular stones and boulders, 2 to three feet thick; dark mould, one foot; white stalagmite floor, 10 to 12 inches; angular and rolled stones, 2 feet 6 inches; crystalline floor, 1 foot 6 inches; red clay, 2 feet; white seam clay, 3 inches; another deposit of clay of a lighter shade, 1 foot; fine sand and gravel covering the rock floor, 3 inches. (Widger 1880:249)
>
> The floor of the cavern ["No. 5"] slopes from south-west to north-east, or from back to front; at 40 feet from the entrance the roof rises to a height of about 25 feet, and the thickness and kind of the different deposits is as below beginning at the surface; large angular blocks of stone about 2 feet thick; dark mould 1 foot; white stalagmite floor 2 feet; red earth and small angular stones 2 feet; the crystalline floor 1 foot 6 inches; rolled stones 2 feet; light coloured yellow clay 1 foot; dark yellow clay 1 foot. (Widger 1881:462)
>
> We ... arrive at the entrance to a large cave - No. 7 - fifty feet long, eight to twelve feet wide, and from fifteen to twenty-five feet high. The different layers in it were as follow. On the top were large blocks of limestone from three to four feet deep and nearly touching the roof. [...] The next deposit was black mould and charcoal about one foot thick. [...] The next was a white stalagmite floor about three feet deep. Under this was the diluvium deposit, about three feet thick. [...] The next was a layer of well rolled stones from a few ounces to over twenty pounds in weight [c.9kg, equal to a sphere of limestone c.20cm in diameter]. The next was the crystalline floor about one foot thick, fractured throughout. Under this was a bed of tough clay, about three feet thick, and shaded in colour from a dark brown to a bright yellow. [...] Under this was a bed of pure river sand which rested on the rocky floor, and embedded in it were slabs of an older crystalline floor in an advanced state of decomposition. (1892, cited in Walker & Sutcliffe 1967:84).

Although the cave is given a different number in each of these three notes, the nature of the deposits and finds leaves no doubt that Widger was referring to Three Holes (cf. Walker & Sutcliffe 1967). The three sequences are schematically represented in fig.33 of the present text. The 1880 and 1881 notes are letters by Widger to J.E. Lee, a member of the Society of Antiquaries of London; Lee published these letters in order to attract support

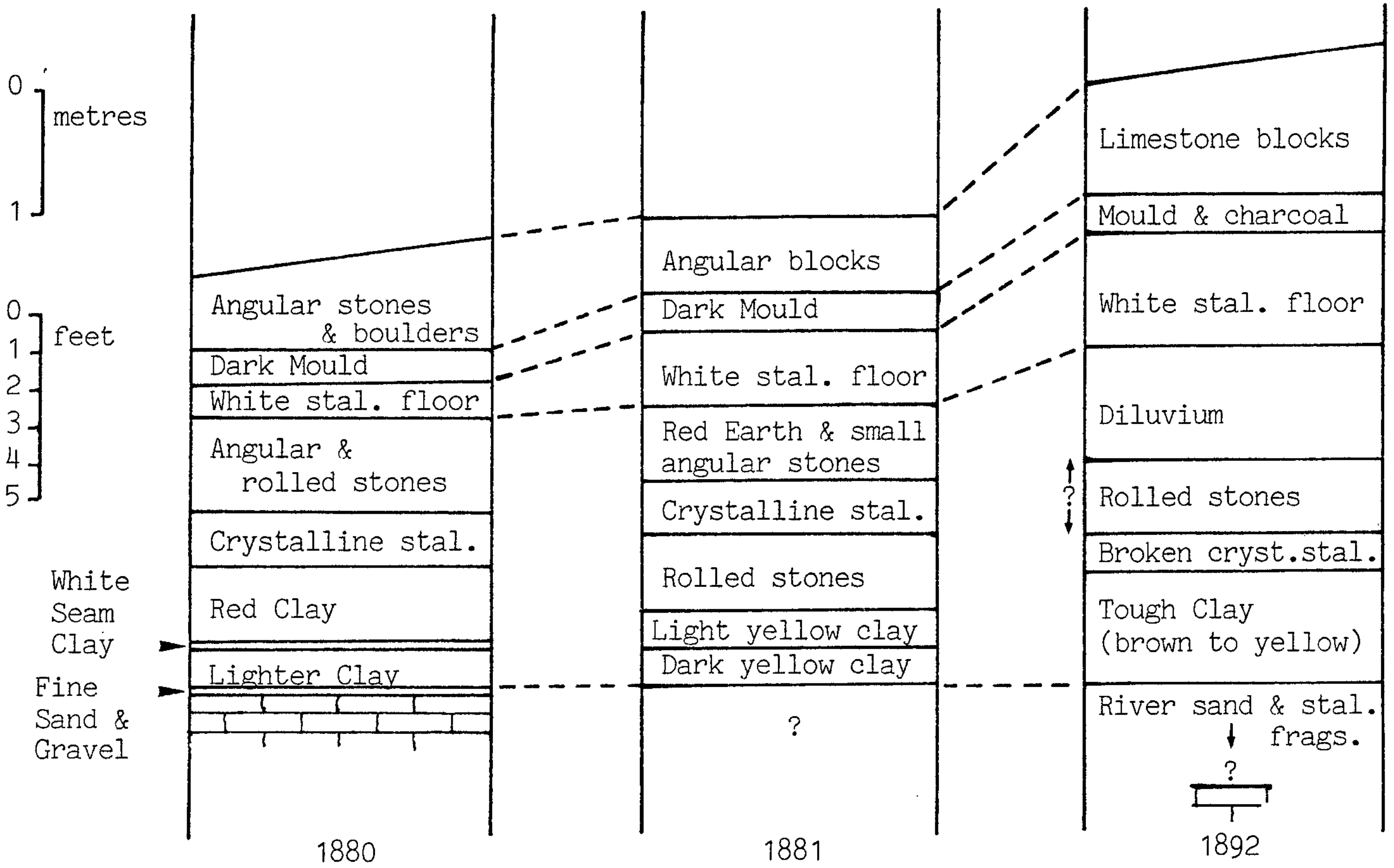


Fig.33 Three Holes Cave - Widger's Accounts

for Widger, and they have a certain immediacy which suggests that they represent the then current state of the excavations. The 1892 note was published shortly after Widger's death. Incidentally, Widger never received significant professional or financial help, despite numerous attempts to bring the Torbryan sites to the attention of celebrated researchers. Rosenfeld (1964b) dismisses all but the last of Widger's accounts of Three Holes on the grounds that the stratigraphies are inconsistent. However, it is very tempting to see the three accounts as representing three areas of the cave: just inside the main entrance (1880), in the passage (1881) and in the chamber (1892). The fact that Widger knew the height of the chamber from the start is not a problem since he recorded that one could crawl in through the roof holes and out through the main entrance before he began digging. Of course, there is no proof for this conjecture, and it is not known whether Widger excavated in any regular manner. Nevertheless, if the boundary between the basal sands and the overlying clays is taken as having been roughly horizontal (fig.33), the resulting picture is not markedly at odds with the present author's observations and there are no major inconsistencies. The angular limestone rubble and the dark mould are holocene deposits, as indicated by the artefactual and faunal contents reported by Widger. The "white stalagmite floor" probably dates from the early Holocene; the remarkable thickness reported by Widger is acceptable if cemented deposits, as well as true stalagmite, are taken into account. The most important apparent inconsistency involves the "rolled stones" and the "crystalline stalagmite floor", which seem to exchange stratigraphic position. Widger suggested that the stalagmite had been undermined and that older deposits had collapsed onto younger ones. This reading of the stratigraphy is rejected by Rosenfeld

(1964b) although she does not give any reasons for this decision. When dealing with Widger's suggestions concerning mixing and collapse of sediments one must always remember that he had the ulterior motive of explaining away any observed association between anthropogenic material and ancient ('pre-diluvial') fauna and deposits. Nevertheless, the present author will suggest that Widger was right in this case, although not in the exact details of his reconstruction of events. The unlikely thicknesses of crystalline stalagmite reported by Widger still constitute a problem. However, rather than one, there are two crystalline stalagmitic floors in this cave, and there are also bands of stalagmite fragments, both factors which might have led Widger to overestimate thicknesses, not to mention the common occurrence of very heavily cemented deposits.

Rosenfeld appears to have examined the pleistocene deposits in detail only at the north-east entrance. However, she briefly described the interior sediments and presented her ideas on the composite sequence. No really useful section drawings were published concerning these interior sediments. Rosenfeld's comments on the sediments and stratigraphy are not set down in any logical order and it has been necessary to reorganise them here in order to provide a more coherent account. Layers 1-7 in Rosenfeld's terminology are holocene, and the uppermost pleistocene deposit is layer 8. The layer numbering system is inconsistent and sometimes equivocal, and some deposits were not even numbered. The notation used for marking finds was not quite the same as that used in the publication. The deposits are described from the top downwards.

Layer (8) [finds from this unit are marked '8a' or '8']: This is a deposit of frost-shattered limestone found throughout the length of the

cave and extending beyond the present entrance beneath the talus deposits. Most of this layer had been removed by Widger, and its former surface has been reconstructed from the remains of the covering stalagmite adhering to the cave walls.
Stalagmitic Floor [above layer 8]: In the passage on the north wall, flowstone formations from the roof end abruptly in a boss or short shelf some distance above the floor of the cave. Frequently the shelf appears broken at its edges, and in some instances it lies directly above cemented angular breccia [i.e. layer 8] also adhering to the wall; in no instance does the breccia reach a height greater than that of the stalagmite. In the chamber the thick deposit of breccia below the fissure is covered by stalagmite which slopes steeply along the wall towards the back of the chamber.
It is quite clear that this stalagmite represents an old floor covering the breccia, which was broken through by Widger. A survey of the remaining shelf fragments shows a floor sloping gently down the passage, reaching nearly as far as the present [north-east] entrance. There is a steep slope from the passage up to the chamber, and the maximum height is reached below the roof fissure. [...] The faint stratification of the scree [i.e. layer 8] shows that the deposit has not been disturbed by solifluction or sludging. (1964b:15)

The double sigmoid curve [ibid., Fig. 7; replotted in the present text as fig.37, but only for material under 2mm] for the particle size distribution in level (8) shows this deposit to consist of two distinct components, and this is strikingly borne out by an examination of the sand grains of different size. The larger grades, greater than 6 mm., consist entirely of fresh angular limestone. In the grades 6 mm. to 2 mm., a few plates of rounded, iron-stained slate appear. The proportion of slate to limestone increases rapidly with decreasing grain size; slate becomes predominant in the grade 0.2 mm. to 0.06 mm. Quartzes occur only in grades less than 2 mm.; they are slightly worn and occasionally stained red with hematitic iron like the slates. Magnetite occurs in the grades less than 0.6 mm. only.
[...]
A comparison of the slate and quartz component [of layer 8] with the sand grains of the reddish clay soil (terra fusca) found on the plateau above the cave [cf. p.729 of the present text] shows great similarity, both in composition and size distribution [cf. fig.37 of the present text]. (ibid., p.23)

The Hearth Deposit [finds from this unit are marked '8h', '8 hearth', '8 black' and, possibly, '8b']: The hearth deposit in layer (8) near [but inside] the cave entrance forms an irregular band, approximately 4 inches thick. It consists of grey material less stony than [the rest of] layer (8). There is very little charcoal, but soil analysis has shown it to be richer in both phosphates and organic matter than [the rest of] layer (8). (ibid., p.15)

Layer (8t) [talus]: Below post-glacial deposits is a yellowish, very stony deposit of superficially weathered limestone scree. It merges with layer (7) [above it] but is somewhat deeper [coloured ?] and less weathered at its base. The surface of this level has a gentle gradient down towards the valley and it is most probably the external equivalent of layer (8) inside the cave, although it is a much thinner deposit [according to Rosenfeld's Fig. 3, layer 8t is c.36cm thick, as compared with layer 8 which is up to c.110cm thick just within the entrance]. (ibid., p.17)

The Bear Deposit [apart from two bear bones marked 'Bear', there are no recorded finds from this deposit]: The lower part of layer (8) is a red clayey deposit containing numerous angular limestone and stalagmite fragments. It is loosely cemented with calcite. This deposit remains horizontal beneath the roof fissure [in the chamber] whereas the limestone facies of (8) [i.e. the upper part] forms a cone beneath it [beneath the fissure]. Towards the cave entrance, the Bear Deposit becomes very thin and disappears entirely [before the actual entrance, according to Rosenfeld's Fig. 3]. (ibid., p.15)

The Bear Deposit differs from [the upper part of] level (8) in having a much greater proportion of surface material [i.e. a fine component with slate and quartz], and also in the presence of fractured stalagmite formations; .... (ibid., p.23)

Solifluction Deposit [there are no recorded finds from this deposit]: This is a very stony red clay. It is entirely unsorted and contains worn as well as angular fragments of limestone. The deposit appears to have flowed from the valley slope on the south side of the cave [in Rosenfeld's Fig. 3, this deposit is thicker on the south side of her trench], and pushed into the earlier deposits - (9t) and (10) [infra] - in the region of the cave entrance. It flows down into the rock fissure at the cave entrance [i.e. the fissure in the rock floor] and does not enter the cave passage. There is a complete lack of stratification in the deposit, and long blocks of limestone were found embedded upright, ...(ibid., p.17)

Layer(9)[a few finds marked '9']: This is a very dense laminated clay with slate sand. It varies in colour from red to yellow, ... The surface of the clay is regular and nearly horizontal; where the cave floor rises above this level, the thermoclastic deposit [layer 8] lies directly on bedrock. In the chamber the clay is confined to the rock channel and fills it to a depth of 4 feet. In the passage the red clay deposit gradually becomes thinner and near the entrance it slopes down suddenly, towards the fissure in the rock floor, where it is distorted by the solifluction deposit.
Layer (9) is completely sterile, except in areas near the cave entrance where bones ... are found in the same state of preservation as in layer (8) above it. Some of the bone in these areas is very highly worn down to smooth pebbles and thickly coated in a brown phosphatic matter which is most probably collophane. Some of the slates in the clay are similarly coated and polished, as are occasional fragments of corroded limestone in the Bear Deposit and in [the rest of] level (8). (ibid., p.16)

From the sand grain analysis of the "red clay" - layer (9) - it is clear that this is almost entirely redeposited surface material, with the addition of calcite, especially in the larger grades. The amount of calcite varies considerably throughout the deposit. The densely laminated structure of the deposit indicates sedimentation in water but the [material is of an] unsorted nature ... [cf. fig.37 of the present text]. (ibid., p.23)

Layer (9t) [finds marked '9t']: This is the earliest deposit reached below the talus. It is a dense yellow clay with a few lenses of slate sand, and it extends down the full length of the trench.
Above the fissure in the rock floor, layers (9) and (9t) become indistinguishable, and both have slumped into a cavity below. Flow marks can be seen in the clay, and tongues of the solifluction deposit embedded in the clay testify to the intense disturbance of the deposits

above the fissure. At the outer [north-easterly] limit of the solifluction deposit, the clay [layer 9t ?] becomes less dense, and blocks of limestone and stalagmite are embedded in it. There is evidence of another fissure or cavity in the bedrock here [c.3.5m north-east of the known floor fissure], but this has not yet been fully investigated. The solifluction deposit and the clay show signs of slumping here too. (ibid., p.17)

... a very dense yellow to red clay with some slate sand, level (9t). (ibid., p.13)

... layer (9t) below the talus ... is a very widespread deposit throughout the valley and has also been encountered below a cold climate deposit at Tornewton Cave farther down the valley. (ibid., p.18)

Lower Stalagmite: Several large slabs of a thick crystalline stalagmite are embedded in the clay in the rock channel [the vadose trench in the passage and chamber]. Some lie horizontally in the clay and have obviously fallen into it, others are in a vertical position near the side of the channel walls. The stalagmite is thus a fractured flowstone formation, which covered the rock floor in the chamber. It is not an undermined stalagmitic floor from an earlier phase of sedimentation as was suggested by Widger. Where the stalagmite blocks have decomposed, the clay shows a marked red colouration, and in a few instances the presence of a decomposed slab of stalagmite was inferred from the bright red colour of the deposit and a sudden concentration of calcite crystals.(ibid., p.16)

A thick flow of stalagmite covered the rock floor and channel sides in the chamber [this appears to be a reconstruction of supposed events rather than an observation].
[...]
[Later came] the sedimentation of a dense red to yellow clay, layer (9).
[...]
[There are] numerous fragments of stalagmite and limestone ... embedded in the red clay. (ibid., pp.17-18)

Layer (10): In the entrance region of the cave [but just inside], layer (9) lies unconformably on a yellow fluviatile clay and fine gravel, which is partially cemented with manganese. The surface of this fluviatile deposit has been channelled by a small [meandering; cf. ibid., p.17] cave stream, the bed of which later became filled with the red clay - layer (9). Layer (10) contains the deeply weathered slate and shale sand, associated with all other Torbryan deposits, and also fresh slate, shale, and vein quartz. (ibid., p.16)

The transition from fine gravel to clay [apparently within layer 10] shows a gradual silting up of the cave entrance ... (ibid., p.17)

Cemented Pebble Breccia: In the chamber, parts of the walls and roof are covered in a cemented breccia of limestone pebbles. This does not belong to the present series of deposits, but to an earlier phase of cave filling. It has been removed [by natural causes] almost entirely, ... (ibid., p.16).

This concludes Rosenfeld's description of the deposits which, although sometimes tortuous, equivocal and self-contradictory, bears a general resemblance to Widger's account. However, recent observation by the present author suggests that more, or different, information might have been available to Rosenfeld. In her excavated area at the main entrance, she appears to have interpreted the sequence from two long (SW-NE) vertical sections, whereas some of the stratigraphic relationships are only apparent in SE-NW cross-sections, and she presumably did not clean the old sections inside the cave sufficiently since she does not mention quite obvious features. In 1964, many archaeologists would probably have felt it unreasonable to expect them to cope with fine stratigraphic and sedimentary detail. The present author cannot stress too strongly his own belief that no archaeologist can be excused the proper responsibilities of any excavator. On the positive side, it is interesting to note that, twenty years ago, Rosenfeld indeed took the responsibility of analysing sediment samples and of publishing a general paper on sediments in archaeological caves (Rosenfeld 1964b).

21.3.3. Three Holes Cave - Recent Observations

In 1977, the cave was visited by groups from the International Speleological Congress (Sheffield) and the INQUA Congress (Birmingham). In preparation for these visits, members of the Pengelly Cave Research Association (notably A. Longman) excavated a small amount of material from the vadose trench as it enters the chamber from the passage, in order to provide a clear section; this section (at plane B on fig.34) was seen at the time of the Congress visits by the present author and was still available in 1978 when a week was spent at the cave for the purpose of studying the surviving deposits. The upper part of fig.34 is as accurate a representation of the chamber sequence as the condition of these deposits will allow. Considerable stratigraphic difficulties remain with respect to the chamber sequence; not only are the old sections unsatisfactory (finer material has often been washed out to an appreciable depth into the deposits and very recent calcite has crystallised in many places), but the sedimentary processes evidenced were extremely disruptive, producing a fragmented stratigraphic record. Sediment samples from each deposit were analysed but, until a control excavation can be undertaken, it would be premature to interpret this sequence beyond the more obvious features. Both correlation with units recognised by Widger or Rosenfeld, and recognition of the order of sedimentary events, must be postponed until p.764, since the situation is rather complex. The deposits recognised by the author will simply be numbered in a more or less arbitrary manner, with the prefix 'A' or 'B' depending upon the section in which the units occur.

A1 - The deposits subsumed under this label are not necessarily contemporary. Indeed, it is most likely that a whole sequence of units is involved, although the author was unable to recognise any really convincing internal boundaries. This material is very heavily cemented; it was difficult to penetrate the sediments even with a lump hammer and a cold chisel. Usually, all that is present are altered limestone clasts and small slate fragments, set in a solid calcitic ground mass, with a minor but extremely variable non-carbonate fine component (clay/silt/fine sand) sometimes localised in small indurated pockets. There is also some variation in cementation. For instance, the remnant near the chamber floor on the south-east side (towards the bottom left of fig.34) is even harder than other exposures, with an extremely coarse crystalline 'matrix'. This remnant also contains stalagmite clasts and decomposed bone fragments. A similarly hard

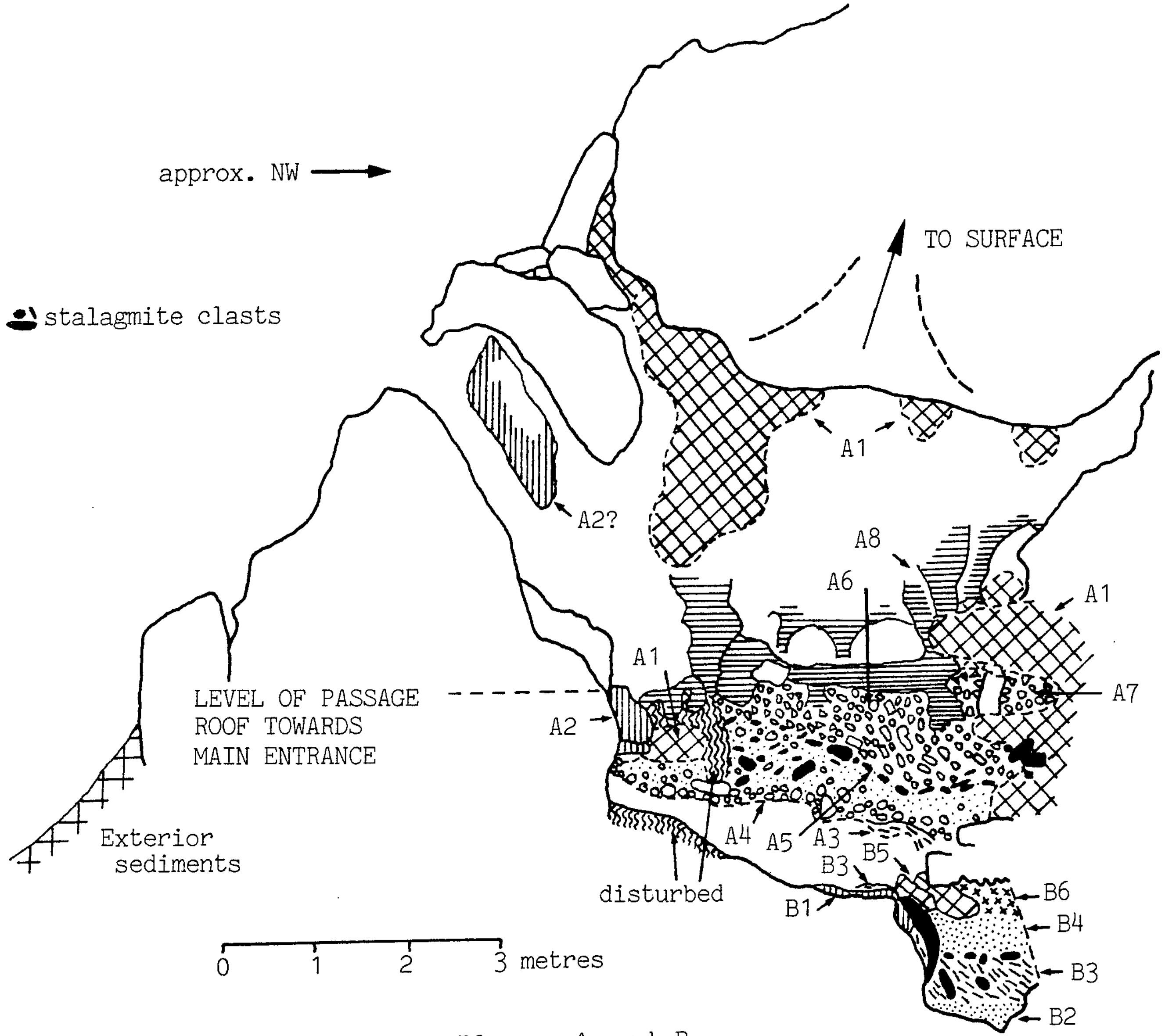


Fig.34 Three Holes Cave - Planes A and B

area of sediment, containing even larger blocks of stalagmite, is present at roughly the same level on the other (north-west) side of the chamber, but the similarity may well be fortuitous. The observed variation in all these deposits seems to be more related to the stage of diagenesis at any given point than to any stratigraphically meaningful factor. Larger particles show no markedly preferential orientation at any point in the present exposures. This cemented material occurs, anchored to the cave walls, at all heights in the chamber, save that it has never been observed directly overlying the rock floor. Furthermore, clasts of identical material, from 1-20cm across, are present in all other clastic units labelled 'A'. Whenever an in situ remnant of A1 is in contact with sediments of another unit, there appears to be a moderately sharp boundary. Moreover, the surfaces of A1 sometimes show solutional features (including vertical rills), some of which may be recent, although some are definitely more ancient since they are buried under in situ sediments. A small cavity penetrates A1 at one point (in fig.34, just to the right of the limestone block that appears as a 'pillar' between two shelves of A1, on the north-west side of the chamber); this cavity has speleothems around its periphery, and a narrow inverted channel cut into its roof, which meanders away out of sight into the deposits.

A2 - On the south-east side of the chamber, there are in situ remnants of pure crystalline speleothems. The base of what until very recently (no more than twenty years ago) must have been a large stalagmite lies, in apparent stratigraphic contact, above a decomposed remnant of stalagmitic floor. This floor seems to continue over the very hard remnant of A1, noted above, but it is difficult to be certain in this area which has suffered considerable chemical and mechanical disturbance due to water seeping out of the south-east extension of the main fissure. Bridging one of the connections to the exterior in the roof, there is a large mass of speleothem that may or may not have a rock core; the only reason to suggest that it is part of A2 is that its composition is unlike that of any other speleothem in the chamber but similar to that of A2.

A3 - Lying on the rock floor, from the centre of plane A towards the north-west, is a thin spread of dense, red (10R 4/6 to 2.5YR 4/6) silty clay, containing a few decomposing calcite crystals, although it is not generally rich in carbonates. Its upper boundary is sharp and undulating and it has undulating fissility throughout. It thickens slightly to the north-west where it begins to dip down abruptly into what is probably the vadose trench (which is obscured by sediments in the chamber itself).

A4 - Lying across the whole width of the chamber is a dense, red (2.5YR 4/6) clayey silt, which is uncemented but rich in carbonates. Very heavily altered limestone clasts are present throughout, most of them showing weak exfoliation crusts. These clasts, which are always quite rounded, are small in the top part of the deposit (c.1-5cm) but, towards the base, they become larger (up to c.20cm) and take on almost perfectly spherical forms. At the very base, especially to the south-east where the deposit lies on bedrock, these spheres are in contact (clast support) but there is absolutely no sign of mutual pressure solution and the matrix remains dense. Rounded, sometimes almost spherical, stalagmite clasts are present throughout. There is a little slate and quartzite of coarse sand grade. Towards the top of the deposit, there are increasing numbers of stalagmite blocks, some of them up to 20cm across and 10cm thick; they show varying degrees of alteration but no appreciable rounding. This deposit has a massive bone content at all levels, consisting of fragments of 'grit' size up to c.5cm, larger pieces being rare. All bone shows severe mechanical damage. The unit has no clear fabric and elongated objects are set at various angles. However, there is a general impression of 'undulations' sloping gently down towards the north-west. Note that the rock floor continues to slope sharply upwards towards the south-west (away from the observer in fig.34) under units A3 and A4.

A5 - This unit consists of stalagmite blocks, identical to those in the upper part of the underlying deposit A4. These blocks are differentiated as a unit because they form a clear and quite continuous band at the junction between A4 and A6 (*infra*). The blocks lie 'flat' at this undulating junction and are never appreciably oblique to it.

A6 - The lower part of A6 consists of a mass of angular and often tabular limestone clasts (clast support) set in a badly sorted, silty matrix. The matrix is undercompacted and there are a few small air-spaces. The lower contact with A4, between the blocks of A5, is relatively sharp, although this contact is sometimes obscured by small scale suffosion features. The limestone clasts show strong preferential orientation, especially towards the north-west side of the chamber, where they swing down at a bedding angle as high as 80°. A6 grades upwards, with no recognisable boundary as such, into a more chaotic deposit with smaller limestone clasts and increasing matrix, but still with clast support.

A7 - Any boundary between A6 and A7 is obscured by thick deposits of stalagmite A8 (*infra*). A7 is a yellowish red (5YR 5/6) silt with a high coarse sand and 'grit' content composed of slate, limestone and speleothem fragments. There are common limestone clasts (c.2-5cm), which are generally angular and sound but which nevertheless show quite strong superficial alteration. The deposit has no oriented fabric; there is partial matrix support and the matrix is 'normally' compacted.

A8 - All round the chamber, on the walls and on top of other deposits, there are thick spreads of 'chalky' speleothem. This material is altered in many places but it appears to have been a rather tufaceous formation from the outset. It contains small limestone fragments, sand and pockets of amorphous organic matter. Calcite formation is still active, and the downwards extensions over exposures of the underlying sediments, seen in fig.34, seem to be recent (i.e. 'post-Widger').

The deposits at plane B lie mostly within the vadose trench. The area between planes A and B shows no useful stratigraphic relationships; the deposits in the centre of the chamber have been removed to the rock floor and the material to the north-west (above the vadose trench) is composed entirely of large limestone blocks and heavily cemented sediment, referable to A1.

B1 - This is a 'foliated' stalagmite, consisting of lenses of calcite, alternating with thinner lenses of purple and 'amber' material with a high phosphate content (it is similar to, although thicker than, the material found on the floor of the Middle Tunnel at Tornewton; cf. p.719). It is present on the rock floor of the chamber and it swings down to cover the upper part of the south-east wall of the vadose trench.

B2 - At the bottom of the vadose trench there is a brownish yellow (10YR 6/6), non-calcareous, coarsely laminated silty clay, with a few thin stringers of slate sand. The laminations are of constant thickness but they undulate gently across the width of the trench.

B3 - This is a dense, red (2.5YR 4/6) silty clay, containing fragments of stalagmite, many calcite crystals and a few small, heavily altered limestone clasts. It is present above B1 on the floor of the chamber and it then swings down into the vadose trench, the orientation of the stalagmite clasts and the weak foliation in the clays following this down-swing. There is a minor slate and quartzite sand component, as well as some bone fragments. Towards the top of the unit in the trench, there are small pockets of non-communicating spherical vesicles (<1mm in diameter). The stalagmite clasts are similar to most other crystalline speleothems in the cave (cf. A2 and A5) but they are totally different from B1. Each stalagmite clast has one major surface that is quite smooth and hard, whilst the opposite surface is granular and shows pits and grooves. This is particularly marked on a large slab lying almost vertically near the trench wall (fig.34); in this case, it can be seen from the internal structure that the altered and grooved surface (facing into the trench) was the original upper one.

B4 - This is a brownish yellow (10YR 6/6) silty clay. There are weak laminations but there are also small, scattered particles of limestone, slate, stalagmite and bone. A redder colour is associated with pockets of calcite crystals.

B5 - These are discrete slabs of very heavily cemented sediment, similar to those occurrences grouped as A1. The laminations in B4 are not disturbed around the slabs of B5.

B6 - Filling the vadose trench to its 'roof' (*infra*) is a yellowish red (5YR 5/6) clay/silt/sand that is much less dense than the underlying uncemented sediments. The sand component contains slate, calcite crystals and other carbonate particles. There are small, moderately rounded limestone clasts and many small stalagmite fragments, together with a few pieces of bone.

The 'roof' of the vadose trench, at this point, is mostly composed of a large limestone block (not bedrock). By digging in under and around the block, it can be seen that it is set in highly cemented material. This block shows clear solution features (anastomoses) on its underside.

There are a few remaining deposits in the passage, but these are all of heavily cemented material. Indeed, some exposures contain clasts of at least one generation of older cemented material. There are a number of large stalagmite slabs in the vadose trench but most appear to have been disturbed quite recently, probably by excavation of the surrounding matrix. The apparently recent 'chalky' stalagmite ledges noted by Rosenfeld were still present, quite high on the passage walls.

The pleistocene deposits just outside the north-east entrance were all deeply buried beneath back-fill when the author examined the site. The holocene sections above this back-fill appeared rather unstable and, as a final hindrance, it rained quite heavily throughout this period. The author therefore decided to concentrate upon the remaining sediments just inside the entrance. The best series of deposits was available at plane C (fig.35).

C1 - This is a large remnant of quite heavily, but patchily, cemented material, adhering to the north-west wall. It contains many large, rounded but not spherical limestone clasts, as well as fragments of older cemented material, the latter containing stalagmite and limestone clasts. C1 has a relatively sharp boundary

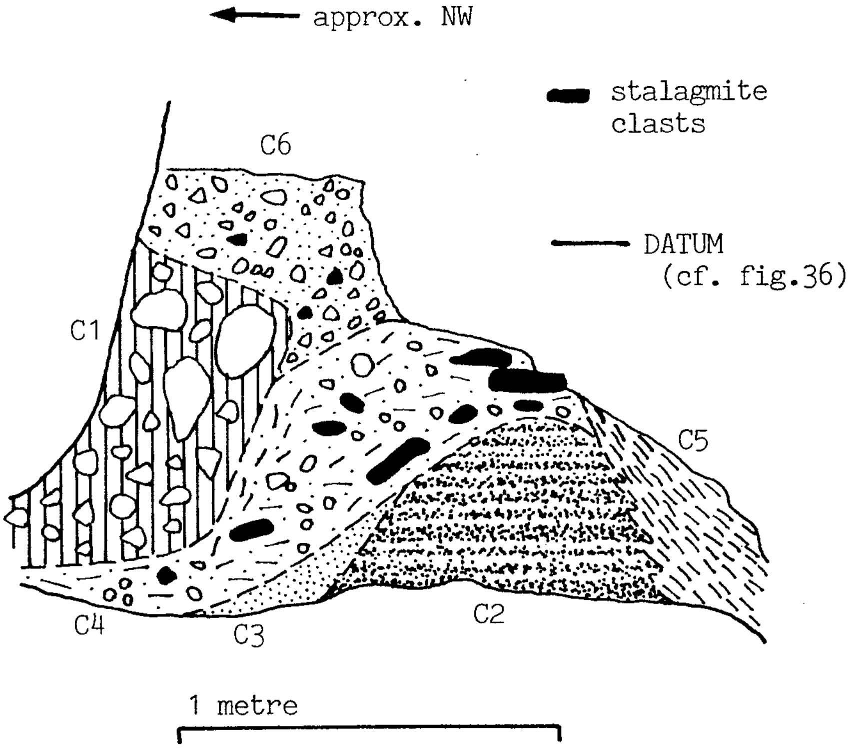


Fig.35 Three Holes Cave - Plane C

with other deposits with which it is in contact.

C2 - Lying towards the centre of the entrance/passage is a longitudinal 'ridge' of slate sand and 'grit'. This form is erosional since younger deposits lie on either side of it and the slate debris itself shows good, horizontal stratification. The 'base' colour of this deposit is strong brown (c.7.5YR 5/6) but there are speckles of yellow, ginger, red, brown, green and black, depending upon the surface condition of the slate and the presence of iron and manganese (hydr)oxides. For the first 10cm above the rock floor, this material is weakly indurated with (hydr)oxides. Slate particles are present up to c.2cm in diameter and continue down to medium sand grade; there is enough colloidal material to give the deposit weak coherence. Although this deposit is well stratified, it is remarkable that there is very little variation between the texture of individual sub-units (each bed having the same relatively wide range of particle sizes) and that there is no imbrication of the platy slate fragments. Apart from slate, there are a few very small quartzite and shale pebbles and some angular vein quartz. The stratification shows a very gentle slope down, out of the cave, but this feature may be misleading in such a small exposure. No unequivocal palaeocurrent structures could be recognised.

C3 - This is a very clayey, structureless deposit, banked up against C2 and containing a little slate debris apparently derived from C2. It has a blotchy but generally 'brown' colour (7.5YR var). All the boundaries are sharp.

C4 - Over the top of C2-3 and also against and under C1, there is a dense, very clayey silt of a generally reddish brown colour (5YR 4/4). There are a few limestone clasts (up to c.6cm), all of which are heavily altered and unsound. The clasts are rounded and some, especially the larger ones, approach spherical forms. There are many altered stalagmite fragments, from 'grit' up to slabs c.20cm long. The matrix takes on a redder colour around all types of carbonate particles. In the coarse sand and finest gravel grades there is a little slate, chert and quartzite. Small bone fragments are quite common. There are small zones of well developed vesicular structure, with the spherical vesicles up to 1mm in diameter. Near the base, most particles larger than c.5mm are oriented with their main plane parallel to the lower boundary of the deposit at any given point, but there is no preferential orientation of the long axis of elongated particles.

C5 - Towards the south-east side of the entrance is a thick deposit of relatively dense, brownish yellow (10YR 6/6) silty clay. It is vaguely laminated, but the stratification seems to have been disturbed since it dips down sharply to the south-east and it undulates gently in the SW-NE plane. The contact between C5 and C2 is sharp, but there are a number of small slump structures where the slate debris penetrates into the clay. There are also a few contorted lenses of slate debris at various levels within C5. Although the exposure was tiny, it was still enough to show that C5 also overlies C4.

C6 - The highest deposit in this section is a yellowish red (5YR 5/6), slightly clayey silt, with an important coarser fraction in the coarse sand and fine gravel range. This coarser material comprises fragments of limestone, stalagmite, slate and clasts of heavily cemented sediment. Limestone clasts are mostly angular with only a little edge-rounding, although there are a few heavily altered clasts. There is no obvious preferential orientation of larger particles and the matrix is not unduly compacted.

On the opposite, south-east side of the entrance, deposits still remain against the rock wall, giving a longitudinal section at plane D (fig.36).

D1 - This is a dense, highly convoluted silty clay with small slate gravel. The colour is generally 'yellowish' but there are many speckles and streaks of various browns.

D2 - This is a purer, non-calcareous silty clay, with clear signs of convoluted bedding. The colour is blotchy but generally brownish yellow (10YR 6/6). There is a sharp and highly convoluted boundary with D1.

D3 - This is a dense, silty clay, becoming siltier and stonier towards the north-east. Larger particles consist of angular but superficially altered limestone, a little heavily rounded limestone, decomposing stalagmite clasts, slate, clasts of indurated slate gravel, clasts of carbonate-cemented material and a little bone debris. There are a few mud clasts (up to c.1cm in diameter) and a few small zones of weakly developed vesicular structure, especially to the north-east. The colour is generally a yellowish red (5YR 5/6) but it may be much yellower or redder in patches. The boundary with D2 is sharp but highly convoluted, with pockets of one unit lying within the other.

D4 - This is a yellowish red (5YR 5/6), slightly clayey silt, with an important coarser component consisting of angular but superficially altered limestone clasts, small fragments of stalagmite, slate and clasts of carbonate-cemented material. The matrix is generally 'normally' compacted but there are undulating tongues of higher compaction and a zone of high compaction at the base. There is no obvious preferential orientation. The boundary with D3 is sharp and undulating.

D5 - This is a zone of greyer material, with many small

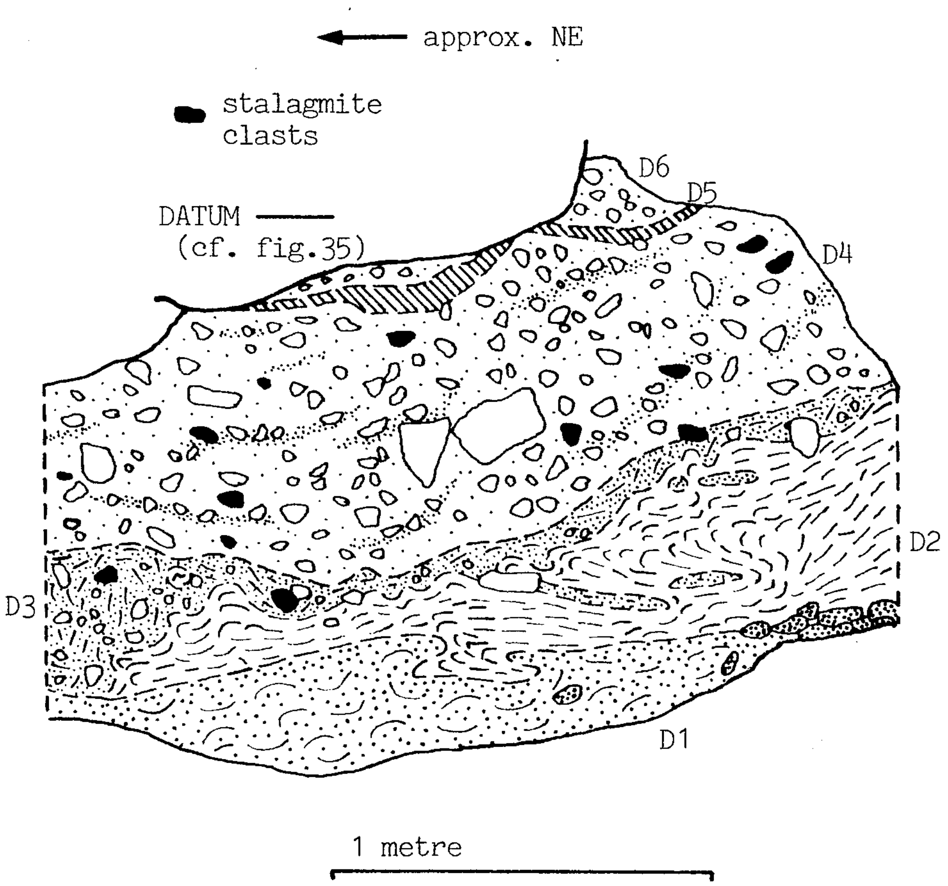


Fig.36 Three Holes Cave - Plane D

limestone clasts. There is a little fine charcoal and small bone debris. A 2mm chip of flint was found here. There is no sign of heat effects on any bone or mineral particle. The boundaries with D4 and D6 (infra) are rather diffuse.

D6 - Although the exposure was very small, D6 appears to be identical to D4.

A ledge of 'chalky' stalagmite adheres to the rock wall c.80cm above the present occurrence of D6.

21.3.4. Three Holes Cave - Interpretation of the Deposits

It is clear from the remaining deposits in this cave that the sequence of events is far from straightforward. Even with the information derived from the most recent study, it is still not possible to reconstruct this sequence with any degree of certainty. The following discussion must therefore rely heavily upon more or less probable inference. However, if appreciable deposits still survive in the north-west and south-east extensions of the main fissure, as seems likely, there may well be a possibility of solving some of the remaining problems at a later date.

Several sets of deposits within Three Holes appear to be generally 'ancient' but their stratigraphic relationships, one set to another and to apparently younger deposits, are not always clear. The first set comprises the heavily cemented material found all round the cave; specific occurrences are recorded as A1 and C1. Rosenfeld recorded this material as "Cemented Pebble Breccia". Its common occurrence as shelves may well account for the extreme thicknesses of "stalagmite" recorded by Widger. It seems highly unlikely that this is a unitary deposit, since it occurs over a vertical range of some nine metres. That similar cemented material is older than some other deposits is shown by the common clasts of 'breccia' found in many units, culminating in the large derived slabs of B5. Indeed, clasts of cemented material are sometimes contained within the 'breccia' itself. However, there is no proof that all the cemented material is older than all other deposits. It is even possible that some of the cemented material is equivalent to some of the uncemented material here recorded as separate deposits, dislocation having occurred because of subsidence of the latter.

The second set of 'ancient' deposits comprises in situ occurrences of crystalline stalagmite. The lowest, but not necessarily the oldest, occurrence is represented by B1. Rosenfeld also seems to have noted in situ stalagmite on the walls of the vadose trench and on the cave floor, but she then confused it with the large derived blocks of quite different stalagmite also found in the trench, as well as in other deposits. The second stalagmitic formation is represented by A2; whether or not the stalagmite in the roof fissure is part of this unit is open to conjecture. A2 appears to be stratified above a local occurrence of the 'cemented breccia', but it would appear to be older than all the other uncemented sediments which surround it. Clasts of good calcitic stalagmite (mostly tabular, 'floor' fragments), like A2, are present in many apparently younger deposits. No clasts of the B1 type are present, probably because this impure foliated material would be neither chemically nor physically resistant enough to survive major disturbance. It is rather tempting to refer all clasts of stalagmite to a continuous floor, now represented only by A2. One could interpret A5 as a slightly disrupted continuation of such a floor, and one could even complete the link with the clasts set in 'cemented breccia' on the north-west side of the chamber. Although such a reconstruction is not impossible, it relies entirely upon the assumption that similar calcitic material indicates a single formation. It would seem highly dangerous to base any further stratigraphic inference upon such a weak assumption.

The third set of 'ancient' deposits comprises water-laid sediments resting on the rock floor of the cave. The slate sand and fine gravel of C2 would seem to be equivalent to Rosenfeld's layer 10. Judging from the well bedded, yet homogeneous and badly sorted, nature of this material, it was probably emplaced by pulses of debris-laden water reaching a stream which normally flowed at much lower competence. The absence of imbrication and of any other current-induced features is consistent with such a mode of emplacement. Unfortunately, this absence also means that the direction of flow cannot be demonstrated. The slight rise (c.25cm) in the floor of the vadose trench as it proceeds from the north-east entrance towards the chamber is not enough to preclude in-flow through the main entrance. The stream evidenced by the sediments may have been much younger than the trench itself. Rosenfeld appears to note that layer 10 (as distinct from layer 9) graded upwards into clays, although such a trend was not apparent in C2. Widger reported "fine sand and gravel" and "river sand" (with stalagmite fragments) at the base of his excavations. If this material indeed fines upwards and perhaps inwards, it does not seem unreasonable to suggest that B2 is part of this unit.

These three components, 'cemented breccia', crystalline stalagmite, and slate sand and fine gravel with clays, therefore seem to have been available in the cave from an early date.

We now turn to the clearer sequence of deposits in the chamber. A4 shows all the signs of debris flow. The spherical shape of the limestone cobbles could not possibly be due to chemical alteration at this point in the cave, since the matrix is rich in carbonates and small bone fragments which would certainly have disappeared in an environment aggressive enough to have rounded the cobbles. Furthermore, the cobbles do not show the re-entrant solution hollows so characteristic of extreme chemical alteration. In situ alteration has indeed occurred, but it was not particularly severe. Even if spherical shape were characteristic of stream cobbles (which it is not, although isolated spherical cobbles may occur on rocky stream beds), these cobbles could not be an old lag deposit because there are areas of matrix support. The matrix is very dense throughout and, apart from the increase in stalagmite clasts towards the top of the unit, there is an apparent fining-upwards trend, as seen in this one section. The high bone content comprises material that has been mechanically fragmented, and deposited in a chaotic manner. The author interprets this deposit as a debris flow which arrived, possibly from the north-west or south-west, but more probably from the south-east. It is also possible that material entered via the roof fissures (if these were open), although this would require the size grading to have been produced by settling and it is then difficult to explain why no large clasts of stalagmite reached the base of the deposit. Limestone clasts in the source deposit(s) were already heavily altered and unsound, so that movement in the debris flow was enough to grind them to spheres. Note how the nature of this deposit corresponds to the material discussed by Kerekes and Vértes (cf. p.356). It is possible that some altered clasts were already present in the chamber and were merely rotated to produce the spherical shape. The clays of A3 need not be significantly older than A4; they might merely represent the onset of debris flow. It is possible that at least the upper part of A4 is equivalent to Rosenfeld's "Bear Deposit" (lower layer 8), since A4 indeed contains masses of bear remains (infra). Although the smaller limestone clasts at this level are not highly spherical, they are not what the present author would call 'angular', so that some doubt must remain as to the identity of the "Bear Deposit". On the other hand, no other surviving deposit in the chamber is even vaguely similar to Rosenfeld's description. Widger's "rolled stones" could well be equivalent to A4; looking at fig.34 in the present text, and allowing that Widger might also have used the term "stalagmite" to refer to very hard 'cemented breccia' as well as zones with stalagmite clasts, it is easy to see how the difficulty over the relative positions of the "rolled stones" and the "crystalline stalagmite floor" might have arisen (cf. fig.33).

Appreciable stalagmite must have been present in the system for it to have been incorporated into the debris flow of A4. However, it is not clear how the clasts of A5 relate to the debris flow event. These clasts were given separate unit status for several reasons. First, they form a relatively continuous band, right across the chamber, at the junction between A4 and A6. Second, judging from the way that calcite laminations within clasts are draped over particulate impurities, many of the clasts are the right way up. Lastly, in two cases, pairs of neighbouring clasts showed such good agreement in their internal microstratigraphy that they must be more or less contiguous fragments of the original floor. If the clasts well within A4 are also part of this floor, then A5 must pre-date A4. If two or more generations of stalagmite are present, A5 may post-date A4. The author could not find any unequivocal criteria to allow a decision as to whether or not all this calcite should be correlated, although more detailed analysis based on more extensive samples might solve this problem.

A6 certainly post-dates the formation of the A5 floor. This material is most probably equivalent to the 'middle' part of Rosenfeld's layer 8 (i.e. the lower part of her angular limestone debris) and to the lower part of Widger's "diluvium" or "red earth and small angular stones". Rosenfeld suggested that this relatively loose, coarse material is a "cone" of debris, that is, material accreting on a slope below the roof fissures. This may well be the origin of the scree, but it is clear that the deposit no longer has the fabric that would result from such a process. The limestone clasts often show near-vertical orientation of their long axis, rather than the angle of 40° or less from the horizontal that would be expected on a scree slope. Furthermore, there is no apparent imbrication or down-slope size sorting, other features which would normally occur in coarse slope deposits. This material represents a collapse, possibly involving free-fall of masses of scree from a higher level but more probably, judging from the reasonably consistent dip at least near the base of this unit, involving more gentle subsidence of an existing scree body.

We therefore have the following stratigraphic possibilities. A6 is definitely younger than A5; the fabric of A6 would suggest that it collapsed when the underlying support of A5 (as an intact floor) was removed. If A5 (as an intact floor) is older than A4, A6 may also be older than A4, given that A5 collapsed during the debris flow event rather than significantly later. Alternatively, if A5 (and thus A6) is younger than A4, a later disruptive process (on an unknown time scale) must be responsible for the collapse of A5 and A6 (*infra*). It is also possible that subsidence of these deposits continued over a long period or that it occurred as two or more discrete events.

The material of A7 differs from that of A6 in that there is much more matrix and there is no oriented fabric. Such a sediment probably represents relatively slow, low energy accumulation of material derived from various sources (the exterior, the bedrock, speleothems and 'hanging breccia', etc.), with little or no postdepositional disturbance. This would suggest that A7 is younger, not only than A6 but also than the collapse of A6. However, it would be necessary to cut deeply into the obscuring deposits of A1 and A8, and to expose the A7/6 boundary, in order to be sure of this conclusion. If A7 is 'perched' on a ledge of A1, the stratigraphy would be even more difficult to understand. A7 would appear to represent the top of the angular scree material noted by both Widger and Rosenfeld.

The stalagmite of A8 is younger than all other deposits seen by the present author; it is probably all of holocene age and some of it is very recent.

The deposits in the vadose trench at plane B are not in stratigraphic contact with those of plane A, although one assumes that, by clearing away masses of A1, the trench deposits could be exposed in plane A. The water-laid deposit, B2, has already been discussed. The 'roof' of the trench, which is composed of limestone blocks and 'cemented breccia', clearly shows the effects of a stream running in the trench at some time after the formation of the 'roof' deposits. The stream certainly filled the trench and, if there were sediments right across the chamber and passage floors, the stream may have been running in an approximation to conduit flow. This flow event may be older than, equivalent to, or younger than B2. The vadose trench itself may be considerably older than B1 and B2. The small cavity with an inverted channel in its roof, much higher in the A1 deposits, also shows the effects of the forced passage of water through the old cave fill, although the date of this event is unclear.

B3 is a mass movement deposit, representing plastic slumping of sediment into the trench from the floor of the chamber and passage to the south-east. The weak vesicular structure probably represents entrapment of air. The included material would be very similar to a mixture of A3, A4 and perhaps A5 (depending upon whether or not A5 differs from the stalagmite clasts actually within A4). The obvious possibility is that B3 represents the continuation of the A4 debris flow. There is a very steep gradient here and it is difficult to see why the whole of the trench was not filled. If the trench was indeed filled, perhaps the upper material was removed at a later date when the main flow was no longer plastic, although the fact that the extant material shows a fabric consistent with movement into the trench rather than along the trench would seem to suggest that this is unlikely. Moreover, although there are a few small, heavily altered limestone clasts, there are no large cobbles. The author is convinced that B3 contains material from A4; apart from the lithologic content, the bone debris is very similar and there may be an archaeological component (infra) common to both units. Nevertheless, the lack of cobbles in B3 remains puzzling and the problem will only be solved by following A4 into the trench at plane A. If B3 is the result of later reworking of A4 (by some process of undermining), this would provide a mechanism for the subsidence of the chamber deposits up to A6. However, if the cobbles could not get into the trench (for instance, because they were firmly wedged between the lip of the trench and A1), some other source than A4-5 must be sought for the large stalagmite fragments in B3.

B4 represents another water-laid deposit, composed mostly of silty clay, but with larger particles derived from the older units. The large slabs of B5 were in place before the sediments of B4 had accumulated up to this level, since the B4 laminations are not disturbed around B5.

B6 is neither a dominantly water-laid sediment nor a fluid mass movement deposit. It would seem to represent slow accumulation of material that fell, or was gently washed, down from the higher deposits in the chamber, perhaps causing a little further subsidence.

At least B2-4 may be generally referred to Rosenfeld's layer 9 (and perhaps 10, supra) and to Widger's basal "clay" units.

Moving to the north-east entrance area, the relationships between the deposits exposed at planes C and D, together with their correlation with the earlier excavations, must be discussed before this material can be linked with the interior sequence. As has been said above, C2 is equivalent to Rosenfeld's and Widger's basal slaty units. C1 is a local expression of the 'cemented breccia'.

The next unit in the surviving sequence is C4, C3 probably being a basal expression of this event. C4 is a mass movement deposit, probably of the debris flow variety. Judging from the large amount of stalagmite, flow was out of the cave. The vesicular structure in this deposit is most probably associated with entrapment of gases. This could have occurred during flow, or it could be the result of the decomposition of organic matter or of the ejection of formerly dissolved gases during freezing. However, there is now no trace of organic matter and there are no really unequivocal signs of frost action anywhere in the entrance deposits (infra). C4 does not seem to have been recognised by either Widger or Rosenfeld.

The next units represent something of a stratigraphic problem, although they all clearly post-date C4. C5, with its probable north-easterly continuation D2, seems once to have been a laminated, water-laid silty clay. However, it has suffered considerable plastic deformation. D1 is clearly a mixture of this material and slate debris derived from C2. Overlying D2 is a similarly convoluted unit, D3, which seems to be a mixture of D2 and C4, perhaps with an additional input of limestone clasts. The question is, when did the deformation occur and did it occur on one or more occasions. There is no certain answer to this question, but it seems best to take the minimal view that only one phase of deformation was involved. A probably disturbed remnant or lag of slate sand and gravel (cf. C2) would have been overlain by a sequence of laminated clays. These deposits were then heavily deformed and D3 was emplaced above them, all by the same process. It seems unlikely that this process was as fluid and energetic as debris flow because of the highly irregular boundary between D2 and D3. Certainly, C5 and D2 never flowed freely as most traces of laminated bedding as well as unit integrity would have been lost. D3 again contains zones of 'bubble' vesicular structure and the whole series of sediments certainly resembles a true geliflucticn deposit. All these sediments have textures that would be moderately to highly frost-susceptible, but it must be reiterated that there is no incontrovertible proof of the involvement of ground ice. This material is equivalent to Rosenfeld's disturbed layer 9, and probably to her "solifluction deposit" and layer 9t. Rosenfeld clearly implied gelifluction as the process responsible for the state of these deposits. She suggested that the "solifluction deposit" had flowed into the entrance area from the exterior slopes to the south, but the present author cannot see why she should have thought this. The fact that the "solifluction deposit" seems to have been thicker to the south is not particularly significant, given that the fissure in the underlying bedrock, into which all these sediments have subsided or flowed, is shown to be much narrower to the south on Rosenfeld's sections. The overfolding seen by the present author in plane D indicates that movement was locally out of the cave, towards the north-east, although insufficient exposures were available to reconstruct the general flow pattern. If the process responsible for the deformation and emplacement of these deposits was indeed gelifluction, there is no reason why some materials should not have encroached from the surrounding slopes. Although the present author could see no ultimate proof of gelifluction in these deposits, this site affords a very good opportunity to pursue this subject by means of further excavation into the open. If the surrounding area is covered in a mantle of similarly disrupted sediments and if there are frost-wedge casts in underlying deposits, the climatic implications would be clear. If, on the other hand, the disruption phenomena are localised in the cave mouth, waterlogging and subsidence into the underlying fissure would be a more likely cause; even the vesicular structure could be the result of the segregation of pore gases as the sediments became increasingly wetter. It is most interesting that Rosenfeld likened the sediments of her layer 9t (not seen by the present author) to those which fill the valley; the reference to Tornewton presumably indicates the Head at that site. Widger (1880) may also have been referring to this group of sediments, at least at a point just within the entrance, when he noted "red" above "lighter clay", unless the former is to be equated with C4;

it is not clear what his "white seam clay" might have been (a decomposed stalagmitic lens ?).

The last group of pleistocene deposits are represented by C6 and by D4 and D6, all these exposures probably being referable to a single major sediment body. This is certainly Rosenfeld's layer 8 (at the entrance); it may also be equivalent to Widger's "angular and rolled stones". This deposit is rather unusual. It represents generally low energy conditions and could be classified in the vague 'cave earth' category. However, it contains a very wide range of components that must certainly have been derived from older deposits further inside the cave. The 'tongues' of much denser matrix would seem to indicate minor sludging. There is no preferential fabric, and no zones of bedded wash material were observed, so that it must be assumed that the deposit accumulated quite slowly and in a relatively irregular manner. The lens of greyer material, D5, is apparently Rosenfeld's "hearth". The present author could see no evidence that this represented a strictly *in situ* hearth; there were no 'constructed' features, and the mineral and bone components showed no signs of heating. However, the present exposure must represent only the outermost trace of this material, so that a true hearth might have been present further away from the cave wall. Rosenfeld's layer 8t is not shown on her sections.

21.3.5. Three Holes Cave - Discussion

An attempt will be made in this section to construct a synthesis of the geological data and to integrate the meagre amounts of palaeontological and archaeological information available from this site.

The problems of stratigraphic interpretation in the chamber and the adjoining part of the passage have already been noted. It is even more difficult to link these areas with the deposits near the north-east entrance. A facile correlation would perhaps equate C4 with A4, the clayey deposits (C5, D1-3) with the vadose trench series (B3-6) (cf. Rosenfeld), and the upper stony material (C6, D4-6) with at least the top part of A6 and perhaps with A7 (cf. Rosenfeld). Although such a correlation seems at least possible, there is no way that the specific exposures linked in this way could be proven to represent parts of unitary sediment bodies. Fig.37 includes the present author's particle size analyses of the non-carbonate fine component of some of these deposits. Usually, this sort of information is very useful for intra-site correlation but, in this case, the author finds the data completely uninterpretable with respect to stratigraphy. Thus, the suggested correlation could only be considered as a very general chronological matching rather than as a set of precise temporal markers. The main problem is the present lack of sediments in most of the passage. The majority of the deposits at Three Holes have obviously received much material from older deposits, usually by collapse and by various degrees of fluid mass movement. However, there is no clear separation between a 'source area' and an 'area of deposition'. The author assumes that the passage was once filled with partially cemented and partially subsiding sediments, as was the chamber, so that the erosive system working along the vadose trench has incorporated older material in an anarchic manner that has inhibited the development of any regular pattern such as would allow more exact correlation of the chamber and the north-east entrance deposits.

If we turn to the palaeontological material, only a very

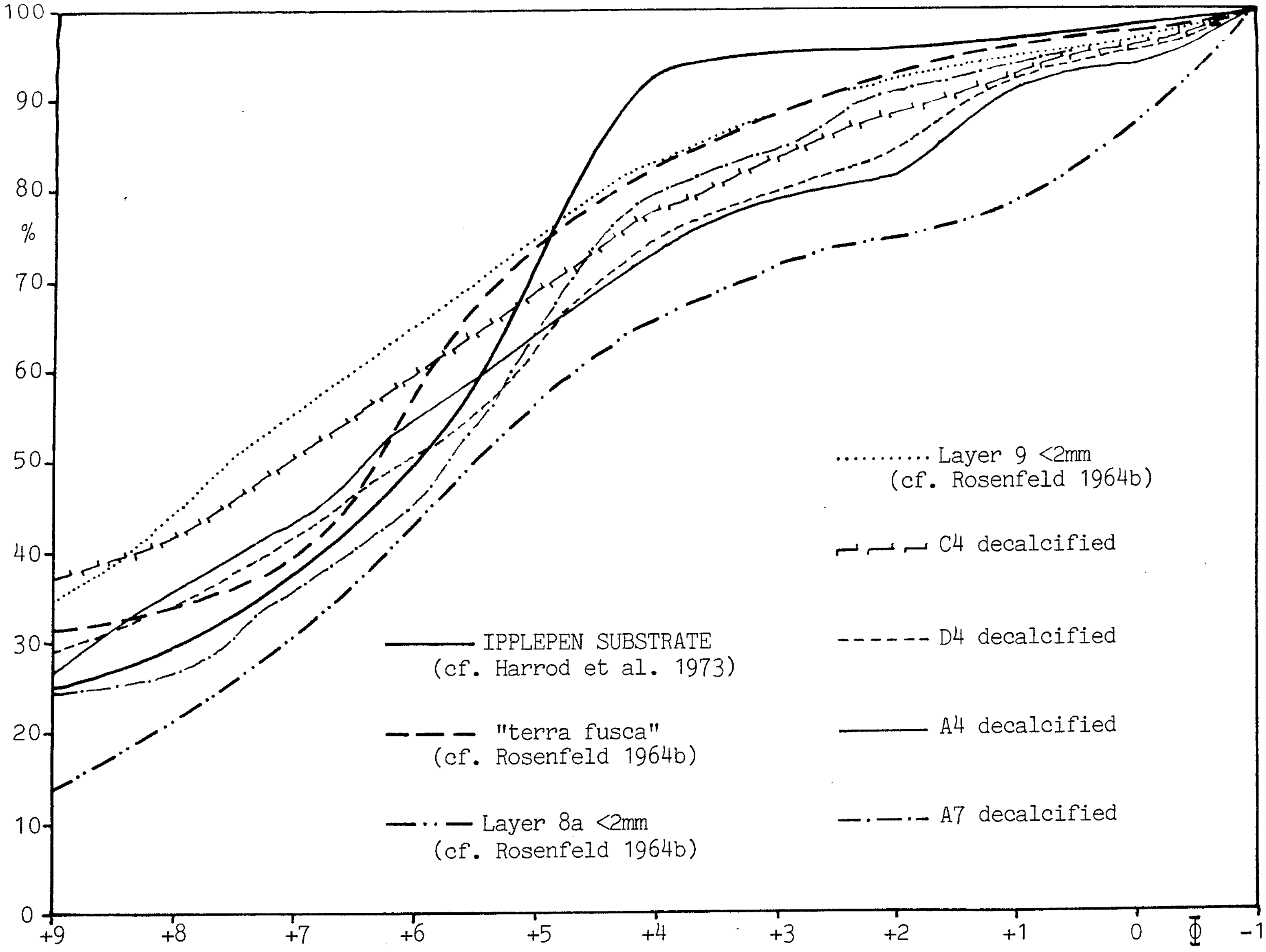


Fig.37 Three Holes Cave - Particle Size Distributions

small amount of useful information is available. Although some of Widger's finds are housed in the British Museum (Natural History), there is no record of the specific Torbryan sites from which each piece originated. Widger reported man (a jaw), bear, lynx, fox, badger, boar, bovid, horse and deer from his "diluvium", and bear from his "tough clay" (1892, cited in Walker & Sutcliffe 1967:84). However, Widger was completely untrained in faunal recognition and these identifications must be taken as unproven. Rosenfeld (1964b:20) acknowledged I.W. Cornwall and A.H. (J?) Sutcliffe as having identified the fauna from her excavations. However, only two pleistocene species are mentioned in her report, horse from the hearth (8h) and Ursus arctos mostly from her "Bear deposit". She noted that the fragmentary bone from the hearth showed angular breaks and no staining, whilst that from the "Bear deposit" was "partly worn and almost invariably stained dark brown" (ibid., p.15). Apparently mixed into layer 9 at the north-east entrance, there were more bear remains, "in the same state of preservation as in layer (8) above"(ibid., p.16). It is not clear what is meant by this last statement; Rosenfeld used the label "layer (8)" to refer to the "Bear deposit", the angular scree and the hearth.

Some of Rosenfeld's fauna is housed at the Pengelly Cave Research Centre (Buckfastleigh). The present author has no great expertise as a palaeontologist, but the following suggestions as to the animals possibly present in this collection are offered in order to indicate that Rosenfeld's fauna is more varied than her publication leads us to believe. From a remnant of layer "8" adhering to the passage wall (south-west) mid-way between the main entrance and the chamber, there appear to be remains of fox and hare. From the "Bear deposit" (presumably in the chamber), there are remains of bear. The entrance sequence appears to have contained the following animals: hearth (8h) - horse and a cervid; 8a (the stony material below the hearth ?) - hyaena, wolf, horse, reindeer; 8b ("Bear" or "black" = "hearth") - a large felid; 9 - hyaena and a very small cervid; 9t - bear, a bovid and a large felid; 10 - bear. Some of the remains, especially those of bear from most units, are indeed a relatively dark brown colour and are rather worn. The bear teeth are of further interest since, rather than having the 'primitive' features associated with arctoid bears, there are signs of more developed, 'spelaeoid' characters (in a phyletic sense - this is certainly not Ursus spelaeus). In the British context, these characters might perhaps suggest a Middle Pleistocene age. The present author recovered identifiable faunal material only from A4; it is a relatively dark brown and shows considerable mechanical damage. This very small collection is now housed in the British Museum (Natural History). A.P. Currant (pers.comm.) has noted the presence of a very large bovid, but most of the remains are of bear; the oddly spelaeoid tooth characters are also present in this material, a proposition confirmed by Currant.

The archaeological information available from Three Holes is again rather meagre, but similarly tantalising. Widger noted "one stone hammer, flint knives, bone awls, etc." (1892, cited in Walker & Sutcliffe 1967:84) from his "diluvium". Campbell (1977) was apparently able to extract some of this material from the general Torbryan collections, since he includes material recovered by both Widger and Rosenfeld in his (undifferentiated) list of L.U.P. artefacts from Three Holes. Rosenfeld recovered flint artefacts from the deposits near the main entrance. She referred this material to the L.U.P., a proposition confirmed by Campbell.

The present author examined that part of Rosenfeld's collection which is housed in the Pengelly Cave Research Centre; if any other material exists, its whereabouts are not known. Although only three certainly retouched pieces (all fragments of backed blades) were present, the L.U.P. attribution seems most reasonable. There were also 44 waste pieces and unretouched blade fragments, individually marked as coming from layers "8a", "8b", "8" or "8 black". Many of these pieces show considerable edge damage. This total of 47 pieces is rather puzzling, since Rosenfeld (1964b) recorded only 34. The present author assumes that he has not seen the other 15 retouched pieces claimed by Rosenfeld. Campbell (1977) records 20 retouched pieces and 23 waste pieces from the combined Widger and Rosenfeld excavations. One more piece from the Rosenfeld collection, with a similar surface condition to the L.U.P. flints, is marked as coming from layer 9t; it is a distal blade fragment.

A few traces of much older material have also been recovered from this site. Widger stated:

> ... dark yellow clay 1 foot. It was at the bottom of this last layer of clay I found the flint implement, which is stained by the colour of the deposit. I must state that I have always explored the different deposits separately so that there should be no mistake as to the position of the specimens; with the exception of the flint, nothing but a few phalangial bones and teeth of bears was met with at this depth. (1881:462)

In his introductory sentense to the above quoted letter from Widger, J.E. Lee noted that the object in question was a "Flint Implement of the older type". Widger refers to a "flint steinbort" from his "tough clay" (1892, cited in Walker & Sutcliffe 1967:84), which is presumably the same object. He seems to have used the term "steinbort" to cover any axe-like object. Rosenfeld equated this object with a handaxe in the existing collections:

> The handaxe is of upper Acheulean type and is made from flint or chert. It is heavily patinated. The pale ochre stain suggests that it may have been found in or on the clay fraction of layer (10). [...] The somewhat worn surface of the handaxe would indicate river transport .... (1964b:25-6)

During the early 1960s, D.A. Roe saw this handaxe; he recorded in his unpublished notes that it was a small, coarse, triangular type, with breaks at both ends (cited by courtesy of D.A. Roe). Walker and Sutcliffe (1967) note that this handaxe is marked "Higher Cave", the name Widger most often used for Three Holes. These authors describe the specimen as " a crude flint hand axe (E244) [B.M.(N.H.) catalogue number][which] is heavily patinated, stained yellow and has traces of red matrix on it." (1967:92).

The present author has only seen a cast of this handaxe. Since the piece does not appear to have been figured in any previous publication, a sketch is offered here (fig.38); only such detail as is necessary to gain a general idea of the object is shown, in case some features of the cast are misleading. The piece is certainly 'crude' in that little care has been taken in shaping the butt-end. However, slightly more refined flaking can be seen towards the tip, although the actual extremity is missing. A generally triangular shape can be reconstructed if the reduction due to damage is taken into account, although the break surfaces at the butt-end may pre-date the making of the handaxe. Thus, although the piece may not be particularly 'refined', there is no reason to suggest a markedly early date and Rosenfeld's attribution to the "upper Acheulean" would not seem unreasonable, at least in a chronological sense. Concerning the suggestion that the piece has "worn" surfaces, it would appear to the present author, judging only from the cast, that the angles are relatively sharp; there is no sign of the rounding

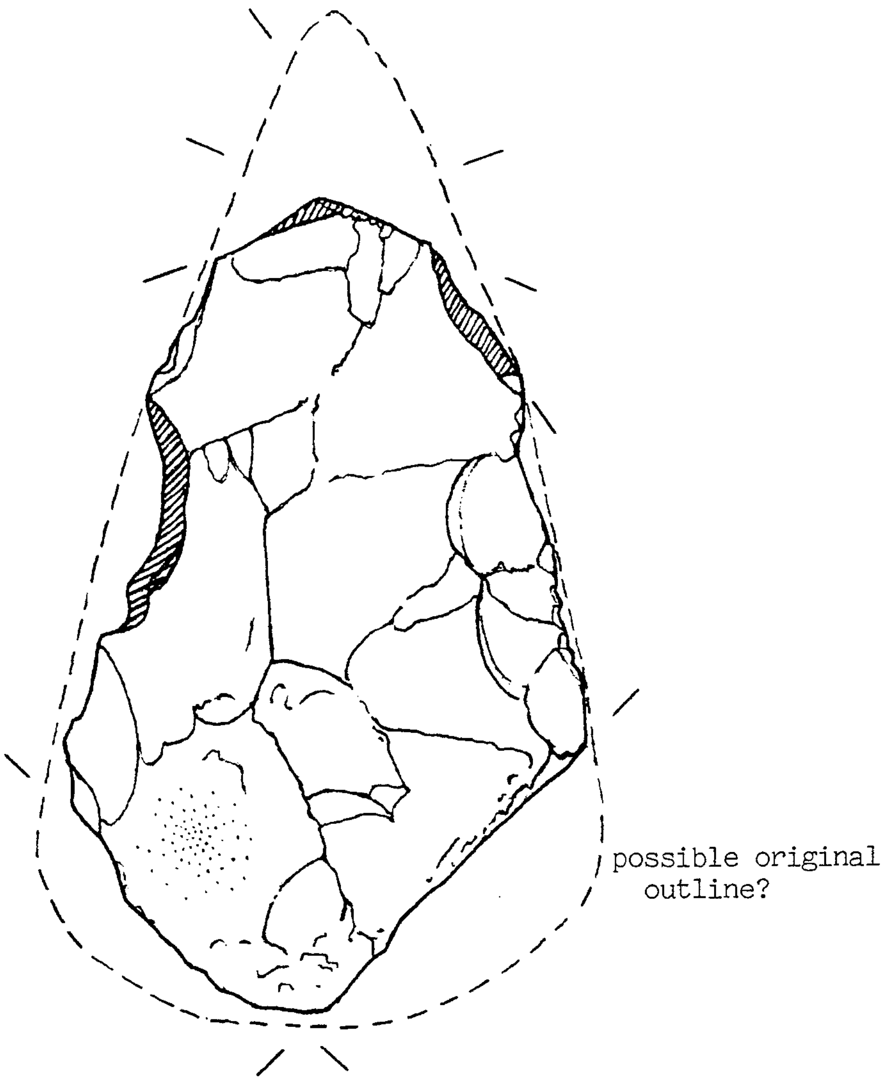


Fig.38 Three Holes Cave

Handaxe Cast

(scale 1:1)

usually associated with a mature fluvial context. However, the tool shows massive mechanical damage, from edge-crushing to snaps right through the body of the piece, suggesting a destructive environment. Such damage would not be inconsistent with debris flow.

Four other pieces are recorded from apparently 'ancient' stratigraphic positions or as being of particularly altered material. In 1892, Widger noted a single flint from his "river sand"; nothing more is known about this find. Walker and Sutcliffe (1967:91) record a "damaged and deeply patinated flint flake" in the British Museum collection, labelled "Stalagmite of Higher Cave". In the Buckfastleigh collection, there is a piece from Rosenfeld's excavation which is marked as having been found in layer 9, at the south-westerly (inner) end of her excavation trench. The piece is a proximal fragment of a laminar flake; there is a wide butt that is apparently faceted. The raw material is very coarse and it may even be a quartzite; it is unlike any other material from Three Holes. This fragment shows considerable edge damage. One more piece from the Rosenfeld collection is worthy of attention. It was accompanied by a separate note stating that it came from a remnant of layer "8" adhering to the passage wall (south-west), mid-way between the main entrance and the chamber (cf. the fox and hare remains mentioned above). It is a small 'chunk' of very heavily altered (hydrated) flint. The piece is of such a 'chalky' consistency that it has suffered much recent (post-excavation) damage and has been broken into two fragments. Little can be said about this flint, save that it is in an identical condition to the Levallois core, described below.

During sampling of unit A4, the present author recovered a large flint protruding from the underside of a disturbed slab of crystalline stalagmite. The stalagmite was removed from around the flint very carefully; it was noted that the calcite laminae ran straight up to the boundaries of the flint and were not draped over the flint. It is assumed that the flint penetrated the stalagmite by pressure solution and that the apparent association is fortuitous: the flint and the stalagmite block are merely two particles in the debris flow deposit. The flint is illustrated in fig.39. It is heavily altered, especially on those sides which were not protected by the calcite. The piece is most probably an unstruck Levallois core, an opinion shared by D.A.Roe (pers.comm.). The upper surface was best protected and shows clear preparation scars. The 'edges' and lower surface are more problematical, because of the heavy alteration, but some 'underside' preparation is still apparent. The core also shows much mechanical damage (crushing) on all salient points, damage that must have occurred prior to the reduction of the piece to its present 'chalky' consistency because the flint still gave a brittle response. This massive edge-damage makes it impossible to reconstruct the flaking order, and it cannot be seen whether or not the intended striking platform has been faceted. There is no obvious cortex on the piece and the rough texturing in fig.39 is used to show particularly degraded areas.

The present author feels justified in putting forward the following hypothesis since significant amounts of the relevant sediment bodies remain to be examined; the hypothesis is therefore open to testing. It seems possible that the six pieces (the core, the handaxe, the three flakes and the 'chunk') might be part of an assemblage, representing a discrete unit of archaeological time if not a single 'industry'. None of the pieces would be out of place in a later Lower Palaeolithic context. It also seems possible

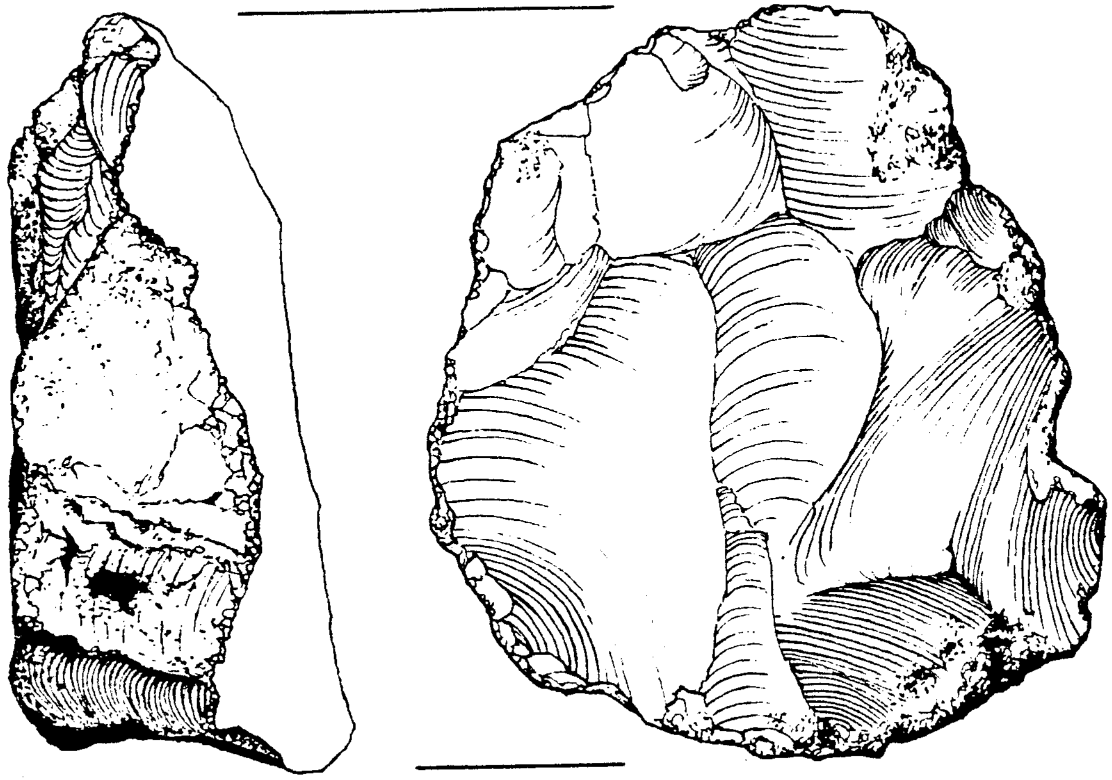


Fig.39 Three Holes Cave - Unstruck Levallois Core (scale 1:1)

that the apparently Middle Pleistocene bear remains might belong to the same rough chronozone, although it is extremely unlikely that they are part of an archaeological assemblage. It is tempting to suppose that the immediate source for all these objects, in that area of the cave that has so far been examined, is the debris flow of A4. The scattered finds in younger deposits as one approaches the north-east entrance would represent reworking of the sort which has already been noted with respect to the lithologic components. Given the uncertainty as to exact correlation, Widger's record of a flint from his "river sand" and the bear remains from Rosenfeld's layer 10 cannot be taken as evidence indicating that the A4 debris flow event pre-dates the slaty sand and gravel. Note that, even if the A4 deposit indeed contains Middle Pleistocene material, it should not be assumed that the debris flow event itself necessarily occurred at such an early date. Reasoning along the same lines, the Upper Palaeolithic material would only be of use as a clear stratigraphic time marker if Rosenfeld's hearth were indeed an in situ feature. Thus, the archaeological and palaeontological material from Three Holes is of considerable interest but the stratigraphic inference that can be drawn from it at present is no more trustworthy than that based upon the sediments.

When erosive and disruptive processes act in a normal manner, from the surface of the sediments, much mixing of originally discrete geological, archaeological and faunal assemblages may occur. However, careful examination may often permit the recognition of a logical pattern of derivation that will help to disentangle the original assemblages (cf. Pontnewydd Cave, Chapter 24). Although Three Holes Cave provides very little useful information on a specific level, it does afford a good example of the extreme disturbance caused by disruption acting dominantly from the base of the deposits. The result of such disruption is not an easily decipherable pattern but, rather, a certain 'uniform chaos', especially with respect to composition. Even in the absence of clear indications in the deposit geometry (indications which exist at Three Holes), it is proposed that this chaotic composition alone would be highly suggestive of basal disruption. At Three Holes, examination of the north-east entrance deposits would have been enough to signal the likelihood of such a process having acted further back in the cave system.

21.4. The Torbryan Valley - Conclusion

The author is of the opinion that the depositional contexts of many of the deposits in the Torbryan caves are such that previously excavated fauna and artefacts cannot be considered to be discrete, meaningful assemblages. Various forms of mass disruption (debris flow, collapse and possibly geliflućtion) have actually been demonstrated and it seems likely that other deposits, now mostly dug out, were equally mixed. It should be noted that the bedrock in Three Holes lies five metres and more above the base of the Glutton Stratum in Tornewton. It is difficult to see how basal disruption, probably involving a reactivation of the local karst, could occur in Three Holes without similar disruption occurring in Tornewton; only the uncertainty as to the dating of the Three Holes phenomena prevents the author from pressing this point further.

Despite the disturbance, the Torbryan caves certainly contain much Upper and Middle Pleistocene fauna, as well as artefacts of both Upper and Lower Palaeolithic types. In addition to the artefacts that have already been noted from Tornewton and Three Holes, there are unretouched pieces and an end-scraper from Torcourt Cave (some 200m down the valley from Tornewton), which are probably Upper Palaeolithic (cf. Campbell 1977), and a small, fairly refined ovate handaxe with a broken tip (pers.comm. D.A. Roe; the specimen is in the British Museum) from an unidentified Torbryan cave excavated by Widger. Pleistocene fauna has also been recovered from Torcourt Cave, Pulsford Cave (c.420m north-west of Three Holes) and Levaton Cave (c.700m north-west of Three Holes) (cf. Walker & Sutcliffe 1967). Although, so far, the Torbryan caves have only produced unequivocal information on a very limited scale, the chances of finding other caves in the Denbury Crinoidal Limestone, Pulsford Limestone and East Ogwell Limestone seem good. There is also at least one regional sediment body (cf. the substrate of the Ipplepen Series Soils), with further opportunities if the 'silt' and 'slate' components can be stratigraphically separated. At Tornewton, there is a vague possibility of a palaeosol (cf. the <u>Elk Stratum</u>) and, at Three Holes, there is a possibility of an interval showing 'periglacial' phenomena. Sutcliffe and Zeuner (1962) and Rosenfeld (1964b) made a number of inferences, based upon the lowest deposits at Tornewton and Three Holes, concerning the River Ambrook and the dating of the diversion of this river from the Torbryan Valley to its present, more south-westerly course. The present author believes these inferences to be invalid, both because the cave data is insufficient and also because simple river capture seems unlikely, a complex switching of routes due to massive sediment mobility (valley choking) being a more probable model in this landscape. Nevertheless, the terrace remnants in the area provide yet another possibility for increased understanding of the regional stratigraphy. Careful analysis of the landscape might allow the identification of potential cave areas that would be less likely to have suffered such extreme disruption as Tornewton and Three Holes. In any case, it should not be forgotten that precise excavation, in full awareness of the need to recognise the finest possible detail concerning the genesis of the deposits during the excavation, will often produce highly significant results, even in disturbed contexts. Thus, the Torbryan area would seem to offer considerable promise for future research.

21.5. Joint Mitnor Cave

21.5.1. Joint Mitnor Cave - Situation and Morphology

Joint Mitnor Cave (SX 742663) is developed in a limestone bluff (Ashburton Limestone, Middle Devonian) on the northern outskirts of Buckfastleigh. The bluff, which reaches c.81m O.D., overlooks the Dart Valley to the east, over 40m below; a number of streams (including the River Mardle) flow past the southern side of the bluff to join the Dart. The Dartmoor granite is only c.5km west of the site, with an intervening zone of shales, slates, grits and thin limestones (Upper Devonian and Carboniferous). Because of the proximity of the granite, the rocks of the area have suffered considerable faulting and distortion and, although Buckfastleigh is just beyond the true metamorphic aureole, there has been appreciable mineralisation of the limestone. The situation is further complicated by the presence of volcanic tuffs within the primary limestone sequence and of igneous intrusions of a later date (cf. Trewin 1978).

Joint Mitnor Cave itself is an interior section of an old karst system which was cut by quarrying during the last century. The system as a whole is quite complex and runs at various levels, traversing the entire limestone bluff. Joint Mitnor no doubt connects with the other known sections, but the connections are blocked with deposits. Thus, the only modern 'entrance' is totally artificial and of no consequence to sedimentation in the cave. The main interest of this site lies in the fact that large amounts of sediment have entered the cavity through a fissure of some sort in its roof. This fissure is still blocked with sediment and its morphology is unclear. However, it becomes impassable only c.6m below the level of the flat summit of the limestone bluff, and it is assumed that there is a connection with the surface (cf. p.817). The cave is composed of three main elements: a north-east to south-west fissure (including the roof feature noted above), a small ckoked passage leading off at a low level to the south-west, and a larger passage leading off generally westwards (cf. fig.40).

Much sediment has been excavated from the westerly part of Joint Mitnor, leaving the main sections of deposits towards the south-east side. These sections have been carefully prepared and conserved by the voluntary administrators of the site, in order to provide permanent study material. This has meant that the present author's work has been limited (most willingly) to description of these sections and to shallow sampling.

In passing, it should be noted that the William Pengelly Cave Research Centre comprises, not only this study cave, but also facilities for caving in other parts of the system and a museum. Biological experiments are conducted in the caves and a colony of bats is carefully monitored and protected, as are all fauna and

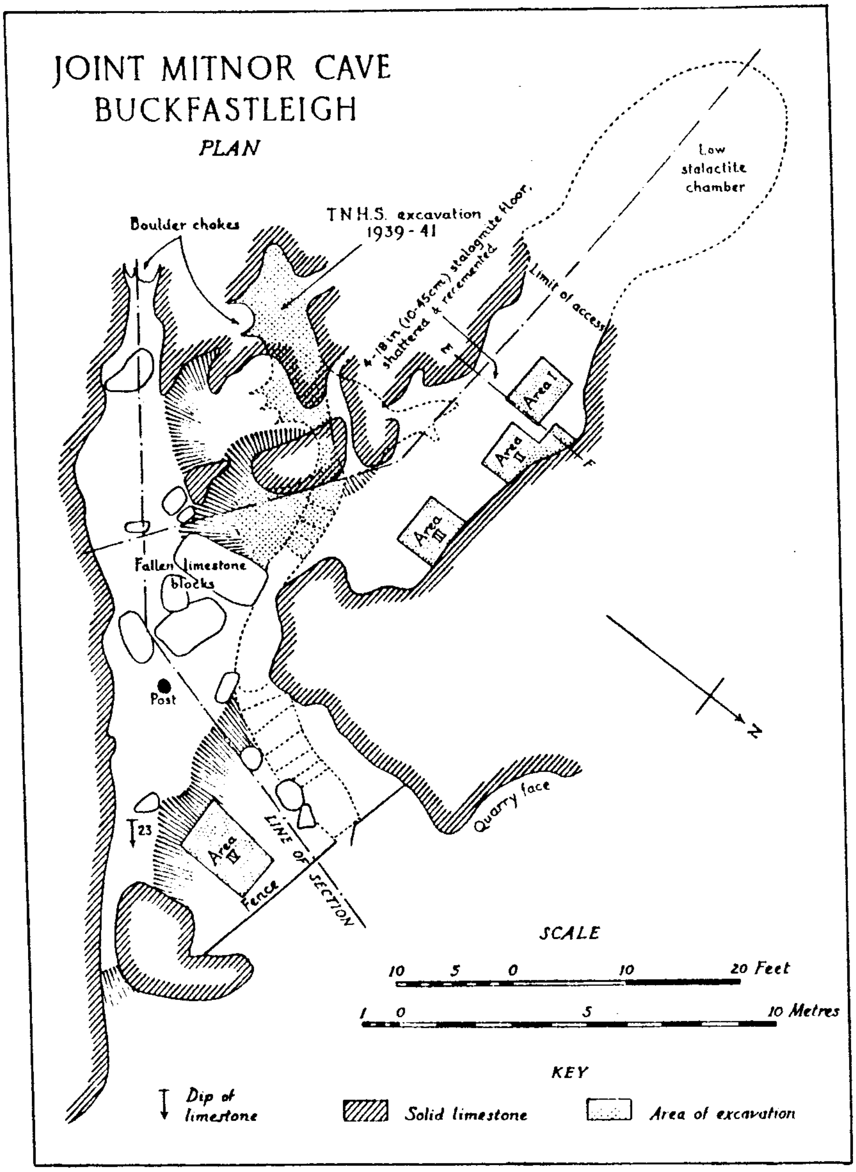


Fig.40 Joint Mitnor Cave - Plan

(after Sutcliffe 1960)

flora in the immediate area. In fact, just about every sort of non-destructive study and recreation activity is encouraged. Publications and visits are available to schools, clubs and the general public on a totally non-profit-making basis (cf. Sutcliffe 1965; Hill 1967; Maxwell 1969). The situation at Buckfastleigh contrasts violently with that at Kent's Cavern, especially since a visitor to Buckfastleigh will not be expected to swallow disingenuous fairy tales concerning the contents and history of the caves.

21.5.2. Joint Mitnor Cave - Excavations and Previous Study

The first excavations in Joint Mitnor were directed by A.H. Ogilvie (Torquay Natural History Museum), in the years 1939-41, but no substantial publication appeared. Sutcliffe (1960) took over the site and published the large faunal collection from Ogilvie's work. Apart from four small soundings, no further excavation was carried out and the stratigraphic details given by Sutcliffe are rudimentary:

> 1. THE STERILE WATER-LAID SEDIMENTS
> The lowest deposit observed in the cave is a finely-laminated water-laid clay and silt. This was encountered in the lower part of each of the trial-pits excavated, and it shows signs of intense disturbance, especially in Area IV [cf. fig.40 in the present text], which was extended down to what appears to be bed-rock. In this pit the deposit was so brecciated [?] that its origin could be recognised only because of the occurrence in it of detached blocks of laminated clay. [...]
> [...] [The maximum altitude of this deposit] is approximately 210ft. O.D. in Joint Mitnor Cave ....
> [...]
> 2. THE BONE DEPOSIT
> The water-laid deposits were overlain by a layer of loosely packed earth and angular blocks of limestone, which can be interpreted only as part of a talus cone which accumulated beneath a fissure in the roof of the cave. This deposit is apparently thickest beneath the boulder-choke [blocking the roof fissure] at the highest point of the Entrance Chamber and thins out rapidly towards the entrance of the cave where it is little more than a foot thick. [...] [The faunal remains] were certainly not carried there by water, for the angle of rest of the deposits is too steep. [...]
> [...]

> Numerous rounded pebbles occur in both the water-laid and talus deposits throughout the cave system. A selection of those from Joint Mitnor was studied by E.J. Beer, who found that many of them were derived from Dartmoor. [...]
> 3. THE STALAGMITE FLOOR
> The water-laid sediments (1) and the talus deposit (2) were locally overlain by a stalagmite floor, which reaches a thickness of about eighteen inches in the western extension of the cave. This floor had been extensively shattered by some process ... and had been partly recemented by further deposition. Near the entrance several large blocks of limestone (the largest ten feet long) had fallen from the roof of the cave onto the talus deposits. (1960:13-15)

In the period 1960-63, the study sections were cut, under the direction of L. Neale, and a more complex stratigraphy became apparent. The first reference to the new stratigraphic work that the present author has been able to find is an annotated schematic section and an excellent realistic section of the main 'talus', produced by D.B. Thomas and dated 17th. January, 1966. Sadly, neither of these documents seem to have been published, even in a polycopied form. Therefore, the most accessible description is again given by Sutcliffe:

> Figure 11 [reproduced as fig.41 in the present text] shows a section of the deposits in Joint Mitnor Cave. [...]
> The floor of the innermost [S.W.] chamber of the cave is the top of the underlying Devonian tuff, the decomposed surface of which is visible at the base of the exposed section.
> This is overlain by bed 1 - a water-laid deposit, umber coloured at the base, overlain by a red ochreous pan-like layer and laminated clay and silt. A few Dartmoor-derived pebbles are present at the bottom of bed 1. [...] Three horizontal water-levels (two calcite ridges and one notch) on the right [west] wall outside the iron door indicate former water levels in the cave at about 218-222 feet O.D. [these are not associated with sediment].
> Bed 2, visible only in the innermost chamber of the cave, lies in channels in bed 1 and probably represents sludging at the top of this deposit. [...]
> Beds 3-6 are the deposits of a talus cone beneath a now blocked shaft in the cave roof. These deposits exceed 20 feet in thickness at the back of the cave and they thin out towards the [artificial] cave mouth .... They are typical talus deposits, with steeply sloping stratification, and with ungraded earth and rock fragments mixed together; ....
> The mammalian remains are restricted to layers 3-4 ... [no lithologic description].
> [...]
> Layer 5a is composed mainly of medium sized blocks of limestone with no interstitial earthy material; 5b of small fragments of limestone; 6a is a compact cave earth with abundant fragments of local slate; 6b is a stalagmite floor; overlain by another deposit resembling 6a [=6c].
> ... these uppermost strata ... are unfossiliferous .... At the present

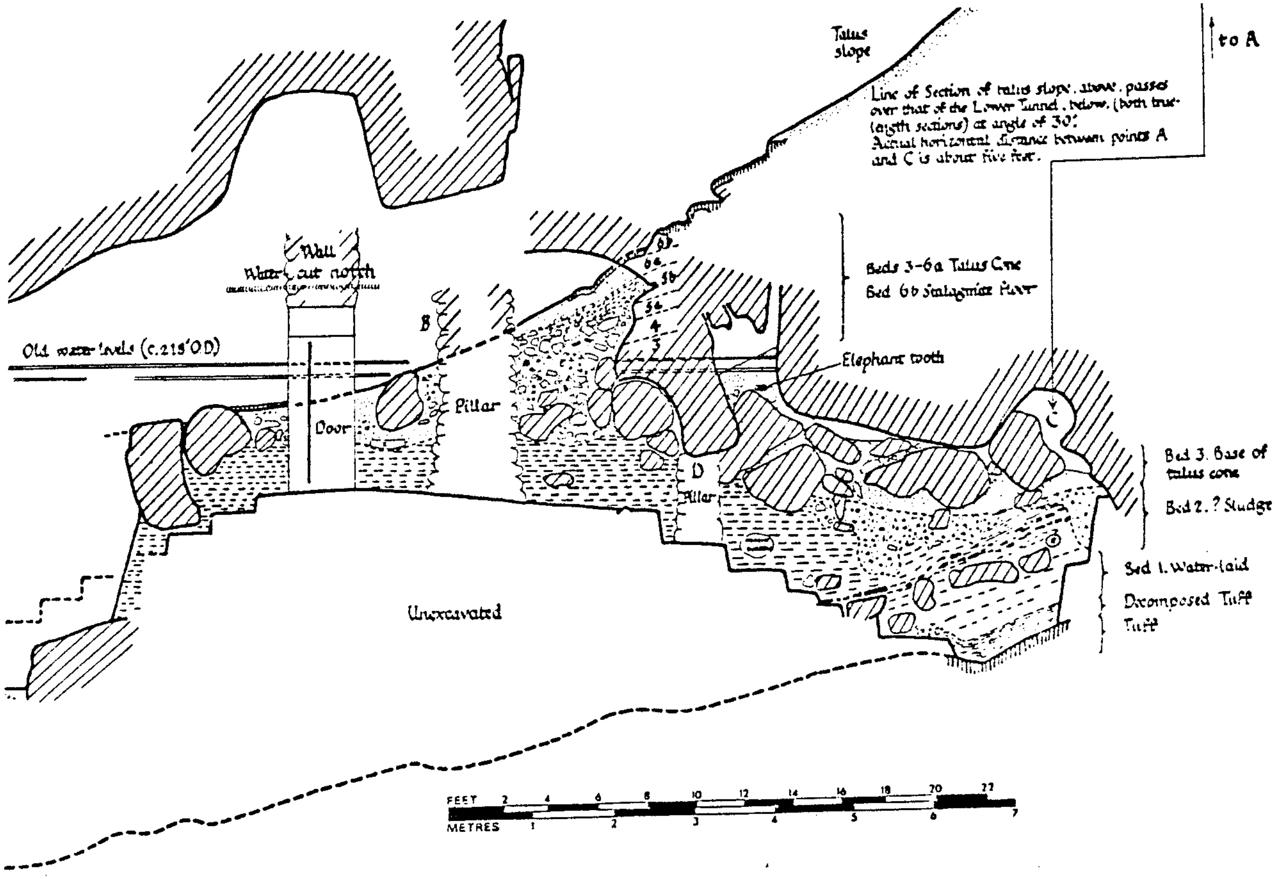


Fig.41 Joint Mitnor Cave (after Sutcliffe 1977)

> day the field above the cave is mantled with a slaty deposit .... [...] The shaft is now entirely choked and no sign of it is visible in the field above. (1974b:17-19)

A few further details are provided by Sutcliffe (1966) in a polycopied document. Dartmoor-derived pebbles are present at the top of the external sequence in the Bullycleaves Quarry (c.300m north-west of Joint Mitnor), although no in situ fluvial terrace has been recognised. Small pebbles of similar material are present at the top of bed 1 in Joint Mitnor, as well as at its base. Sutcliffe (1974b) notes that the Dartmoor-derived pebbles (in the general area, and not just within Joint Mitnor) include granite, quartz-tourmaline rock and metamorphic rocks. The "umber coloured" basal portion of bed 1 referred to in the 1974b report is presumably the "brown manganiferous deposit" of the 1966 report.

21.5.3. Joint Mitnor Cave - Recent Observations

The present author carried out a necessarily superficial examination of the Joint Mitnor deposits in 1979. Preliminary notes, written on site, were published (Collcutt 1979); certain points, especially some of the lithogenetic inferences, were ill-considered and these notes are superseded by the present discussion. The original unit numbering cannot here be maintained because several subdivisions and new units have been recognised; units in the new system will be termed '*lithozones*', with cross-references where possible to Sutcliffe's units, here termed '*layers*'. Fig.42, based on the same projection as used by Sutcliffe (cf. fig.41 in the present text), shows the configuration of the deposits in the main study sections, as recognised by the present author.

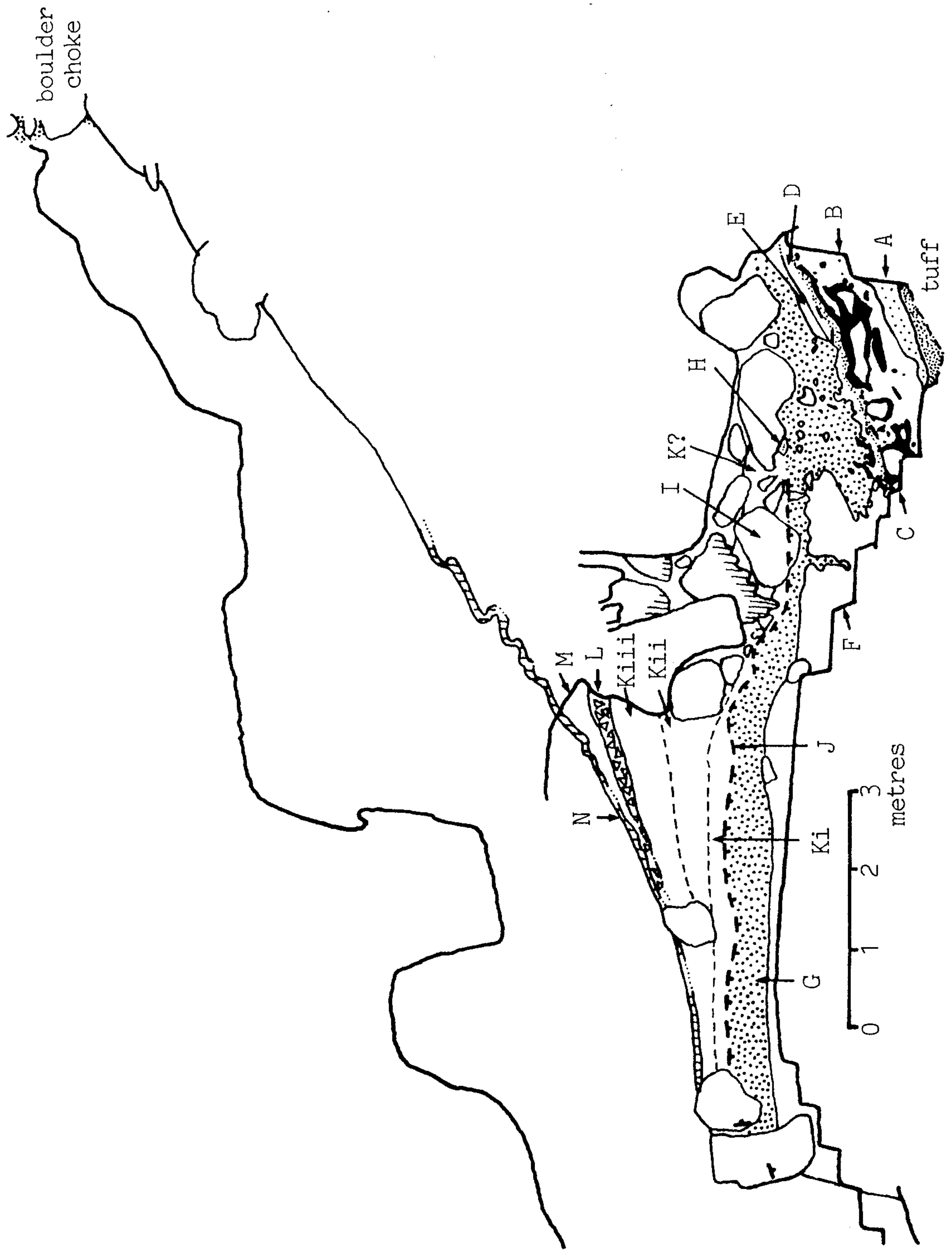


Fig.42 Joint Mitnor Cave

A lithozone. The lowest unit is a grey-green clayey material, that is compact and quite hard. After a maximum of c.25cm, it grades quickly into volcanic tuff, of which it is certainly an *in situ* decomposition product. The tuff dips down to the north-east (towards the 'entrance') by c.18° in the exposure at the 'back' of Joint Mitnor. This tuff is probably continuous with an exposure outside, in the quarry face, so that, if the tuff runs comparatively straight, the general down slope of the cave floor to the north-west would be c.12°.

B lithozone (equivalent to the lowest part of layer 1). This is a mottled but generally dark reddish brown (2.5YR 3/4) deposit. The abundant matrix is 'spongy' (i.e. although dense, it 'weeps' when compressed) and 'greasy', and contains several different types of particle which readily disintegrate to gritty or clayey residues. There are common red (10YR 4/8) vein calcite crystals and black ghosts (*infra*). Small (up to 1-2cm) rounded pebbles and coarse sand of quartzite, slate, shale and a black igneous rock (a hard aphanitic type which is probably referable to ancient tourmalinisation nearer Dartmoor) are a relatively minor constituent. The fabric is rather chaotic and, although there are zones of highly contorted laminations of silt and clay which might be approximately *in situ*, there are also obvious clasts of such laminated material, floating in less structured matrix. The boundary with A lithozone is sharp but strongly undulating.

C lithozone (equivalent to the lower central part of layer 1, including the "pan-like layer" and the "brown manganiferous deposit"). This lithozone is composed of three types of material. The first comprises large, relatively sound limestone slabs, arranged in a reasonably continuous band. Then there are areas of black to very dusky red (2.5YR 2.5/2) 'spongy' and 'greasy' material, which occur as isolated patches in B lithozone (supra) or draped around the limestone slabs of C lithozone. This material never contains inliers, even of sand size, except for limestone. Much manganese is included. At least in the more regular drapes around limestone, this material is shot through with red (10R 4/6) lines, forming dendritic patterns, the substance of which (unlike the dark mass) gives a weak HCl-reaction. Over the top of the band of blocks and dark material lies a thin zone of red to orange (high chroma) clay, containing many platy fragments of a relatively soft, red substance. These fragments split readily along their main (roughly horizontal) plane, showing surfaces with a botryoidal tendency. There are also a few dark brown laminations, patches of pure hematite and a single, thin vein of black grassy material which passes through neighbouring fragments at the same level. It will be argued below (p.806) that these phenomena are all referable to the decomposition of the original limestone blocks. The phrase 'black ghosts' will be used to refer to discrete patches of the dark manganiferous material where they occur in other lithozones.

D lithozone (equivalent to the upper central level of layer 1). This unit comprises dark reddish brown (5YR 3/3) silty clays, which are continuously and very finely laminated, although the laminations are convoluted in places. There are small (c.2mm) black ghosts throughout and a few larger, plaque-shaped ghosts at the base. The lower boundary is sharp and undulating, but not convoluted. No contact with B lithozone has been observed, there being always a thin intervening spread of the red to orange clay of C lithozone.

E lithozone (equivalent to the upper central level of layer 1). This unit comprises generally reddish brown (5YR 4/3) fine sand, silt and clay laminae. There are flecks of dark manganiferous material, especially in the finer grained laminae. There is also a metallic 'powder', probably consisting of stannite (tin pyrites), disseminated throughout the unit. The sands are mainly quartzitic but there is a comparatively high percentage of dark, opaque minerals. The laminations in the very small exposure of this unit are almost straight and parallel, but they dip down to the north-east at c.25°. The lower boundary is sharp.

F lithozone (equivalent to the uppermost part of layer 1). This is a reddish brown (5YR 4/4), comparatively fine, yet very badly sorted deposit. The fabric is dense and chaotic, with large numbers of small (c.1mm) oblate mud pellets. There are many small (<4mm) rounded pebbles of shale, slate and vein calcite (all rather unsound) as well as some of quartzite and of a black igneous rock. Contorted and discontinuous spreads of slightly larger pebbles of the same material (except for calcite) occur in places, although usually there is still matrix support. There is much manganiferous material and a few small (<5mm) black ghosts, with a thin and discontinuous band of larger ghosts at the top of the unit. The lower boundary has not been observed (cf. fig.42).

G lithozone (equivalent to layer 2). This is a dense, variegated but generally reddish brown (5YR 4/4) deposit which is quite similar to F lithozone. However, there are more common larger particles (c.5-10cm), including some limestone, either relatively sound but with thin black drapes over most surfaces or friable, light weight, rounded clasts (colour: weak red 10R 5/2) with very high porosity. Friable clasts of speleothems are quite common. The abundant matrix again contains masses of mud pellets, strong zones of which may also be associated with weak 'bubble' vesicular structure. Especially as one proceeds nearer to the 'entrance', there are also zones of large (1cm or slightly larger) mud clasts (reddish brown 5YR 5/3 or redder hue) set in a yellowish red (5YR 5/6) matrix. Black ghosts are quite common but they have extremely contorted shapes. There are small concentrations of metallic 'powder' (probably stannite) and specks of this material occur irregularly throughout the deposit. Small hematite pebbles, friable limonitic aggregates and decomposing calcite crystals give speckles and blotches of redder and yellower colour. Small exotic pebbles are still common. The lower boundary is 'subtle' but sharp; it is highly contorted, with injections of fines down into F lithozone and, towards the 'back' of the cave, material from F lithozone is totally incorporated and G lithozone shows some convoluted interpenetration with sediments as deep as B lithozone.

H lithozone (no equivalent). At one point near the 'back' of the cave, a small (c.8cm) pocket of laminated fine sand, silt and clay is wedged between the top of G lithozone and a boulder of I lithozone (*infra*). This material is almost identical to E lithozone.

I lithozone (no equivalent). Especially towards the 'back' of the cave and towards the 'entrance', G lithozone and sometimes F lithozone are penetrated by sound limestone boulders and larger slabs, often with near-vertical orientation of long axes.

J lithozone (no equivalent). This is a stalagmitic floor, originally over 10cm thick, which can be traced without interruption from the 'back' of the cave right out to the 'entrance'. The floor is sometimes present as heavily corroded but still coherent stalagmite, or as fractured blocks of such material. However, it is mostly represented by needle-like calcite crystals (2-5cm long), set vertically in a clayey matrix. In some areas, the vertical crystals are sandwiched between two zones of decomposed, horizontally laminated calcite. This floor overlies G lithozone and is cemented to the sides of the blocks of I lithozone where these project above the level of G lithozone. The lower ends of many of the thick curtain formations in the cave terminate against this floor and, even though structural links are unclear because of alteration, they may be referred to the same general phase of speleothem formation. The curtain stalagmite is usually buried in its lower parts by younger sediments (K lithozone, infra). Note that the floor, which is generally horizontal towards the 'front' of the cave, dips down into a depression towards the 'back'.

K lithozone (equivalent to layers 3-4). Away from the 'back' of the cave where large limestone boulders make observation difficult, this unit can be subdivided into three (Ki and Kii, equivalent to layer 3, and Kiii, equivalent to layer 4), with very diffuse (2-5cm thick) boundary zones between the subdivisions.

Ki. This is a dark reddish brown (5YR 3/4) clay with an admixture of silt and fine sand. Although the matrix is 'normally' compacted, it is totally composed of oblate or more irregular pellets or 'crumbs' (1-2mm diameter), compressed so as to leave only minor void space. Unlike many of the 'mud' aggregates and clasts in lower units, which usually show some laminations in their outer shells and little structure within, these 'crumbs' have a system of fine pores and are themselves composed of smaller, irregular aggregates. There are frequent, but patchy and thin, clay coatings binding the major aggregates and some thicker linings to larger voids. The matrix is quite rich in carbonates and there are rounded and altered limestone and speleothem clasts, sometimes with matrix support. Alteration is not of the 'black ghost' type but of a more normal kind, with the development of 'chalky' surfaces. There is no clear preferential orientation of clasts. There are also a number of large limestone slabs and even boulders, especially towards the 'back' of the cave. Although there are many 'speckles' (hematite, limonite, calcite crystals, etc.) giving brighter colours, the only exotic rock type which occurs in any quantity is slate (coarse sand and 'grit' up to c.5mm). There are rare, small fragments of bone, which are heavily altered and brittle (white and blue/grey mottles). Near the top of this subdivision, there are larger fragments (up to c.10cm) of a stalagmitic floor formation, which are altered but coherent. The lower boundary may undulate, but there is rarely any disturbance of the zone of calcite crystals below (_J lithozone_), although in a few places such disturbance has occurred, pulling crystals and some deformed black ghosts a little way up into _Ki_.

Kii. This subunit is generally very similar to _Ki_. However, the matrix is rather less clayey and considerably less compacted. The 'crumb' structure is present throughout, with appreciable voids between aggregates, often with clay linings. The colour becomes lighter upwards (to reddish brown 5YR 4/4), with diffuse zones of brown (7.5YR 5/4). There are speckles of unstructured carbon. Larger clasts (up to c.25cm in diameter), mostly of limestone, are similar to those in _Ki_; in addition there are a few quartzite pebbles and rare, distorted black ghosts and limestone clasts with thin black drapes. Many of the larger limestone blocks show smooth surfaces marked by minor rilling; these surfaces, and their rills, may be oriented in any direction. In the north-east to south-west plane in which the present section is cut, larger clasts show a very poorly developed tendency to dip down to the north-east at an average angle of c.10°. However, at right angles to this plane, a more consistent dip appears to have been developed (which is difficult to observe because of the need to maintain the section), down towards the north-west by as much as c.35° in places. In both planes, smaller clasts are more chaotically oriented. The deposit has only patchy matrix support. In those areas with good clast support, especially between larger limestone blocks, the matrix may be radically undercompacted (one may push a finger right into the deposit with very little effort) and there are even air-spaces up to 1cm across. Slightly altered bone fragments are quite common throughout the deposit. The lower 'boundary' is a smooth, undulating transition to Ki, with no sign whatsoever of any sharper features.

Kiii. Again, this subunit is similar to the rest of K lithozone. The colour is yellowish red (5YR 4/6) and silt and fine sand have become dominant in the matrix. The matrix is always radically undercompacted and 'crumb' structure is ubiquitous. Smaller limestone clasts are heavily rounded and are of light weight and high secondary porosity. There are more common medium sized (5-10cm) tabular fragments of limestone than below. The dip characteristics of Kii are maintained, with the north-easterly component becoming slightly more regular. There are more common distorted black ghosts and flecks of manganiferous material throughout the matrix, as well as a few, more sound clasts with thin black drapes. These 'black' altered types are of course additional to more normal limestone clasts, which are the most common types. Bone fragments are comparatively rare. The lower 'boundary' with Kii is marked by an undulating, slightly darker zone, without any sharp transitions.

It should be noted that K lithozone as a whole lacks sharp bedding features of any kind in the fine matrix. There are absolutely no laminations or even restricted lenses of discrete material. The 'crumb' structure, more or less compacted, is present throughout and there are no zones of massive structure. The whole of the deposit has a significant NaOH-soluble organic content, although its distribution is very patchy, varying from unmeasurable (less than 0.2% of the material under 1mm) to c.1.5% over very short distances. The organics are always amorphous and are present as coatings to larger particles or aggregates, or in a finely divided form within aggregates.

L lithozone (equivalent to layers 5a-b). This unit is a loose scree, with little or no matrix. At the base, there are common small limestone boulders and unaltered fragments of stalactite and curtain formations (some of which have grown on the limestone before the detachment of the latter as blocks). These elements often penetrate the surface of Kiii at a contact which dips down to the north-east at c.20°. The unit grades upwards into a medium to fine, very angular scree, with a totally chaotic and unstable fabric.

M lithozone (equivalent to layer 6a). This is a yellowish red (5YR 4/6), slightly clayey silt, with masses of slate debris in the coarse sand and finest gravel grades. Small quantities of quartzite, fine grained sandstone, a black igneous rock and decomposing granite (?) are also present in these grades. There are large amounts of small limestone clasts, with a few larger slabs, all of which are sound and generally angular, although they sometimes show minor edge rounding and their surfaces always have solutional pitting and raised calcite veins. The matrix is not well compacted; there are small zones showing 'crumb' structure and a few larger voids within denser concentrations of limestone clasts. Overall, the deposit is comparatively loose. Carbonates are present in all size grades and there are zones of crystalline cementation, mostly around limestone clasts, together with very rare rhizoliths (moulds). There is no preferential orientation and there are no internal bedding features. At the base, this material penetrates a little way between the clasts of L lithozone.

N lithozone (equivalent to layer 6b, but not necessarily equivalent to the "Stalagmitic Floor" of Sutcliffe (1960); infra). A thin and only partially continuous laminar stalagmite coats the surface of M lithozone, with a few minor stalagmitic bosses.

Above N lithozone, only thin patches of sediment survive (equivalent to layer 6c ?), all of which are highly contaminated with modern bat guano and other organics. However, at the highest accessible point, between the boulders that choke the roof fissure, there is a sediment which is a slightly clayier version of the M lithozone type. The walls of the fissure at this point, and even some of the boulders, are covered with major curtain formations.

The material described above comprises the deposits seen in the study sections towards the south-east side of the cave. Other deposits are present, but not cleany exposed, in the main western passage, but correlation across the excavated area is far from obvious. These western deposits, some of which contain bone, will not be discussed here. The thick, fractured stalagmite in this area is noted in the general discussion of stalagmite fracturing on p.286.

21.5.4. Joint Mitnor Cave - Discussion

First, the condition of the limestone in these deposits must be mentioned. The black, manganiferous ghosting of limestone has already been discussed in detail in Chapter 12. This effect is best observed in C lithozone, where the calcite veins of the limestone 'core blocks' can be followed into the surrounding mass of 'wad'. The cave walls themselves are similarly altered in the most easterly exposure of sediment at the 'entrance' (seen from outside in the quarry). However, the black ghosts are not always undisturbed alteration residues. In many of the deposits there are highly contorted masses, with no red veining, showing that lumps of the sticky 'wad' can sometimes survive redistribution. The degree to which limestone has been reduced to 'wad' may be taken as a rough measure of relative age, although changes in ground and free water regime will also have influenced the process.

The lowest deposits at Joint Mitnor (layer 1) represented by lithozones B-F, have always been known as water-laid sediments. In fact, little of this material has actually been emplaced by flowing water. B lithozone contains much material that is probably of an ultimately fluvial origin and there are some zones with laminated bedding. However, there are also discrete clasts of laminated material, often showing deformation, and the overall fabric is chaotic, both features which show that mass movement was certainly involved at a later stage. C lithozone is totally composed of the remains of limestone blocks. Apart from the black ghosting discussed above, it is clear that the red to orange clay, and the included platy fragments of a soft 'laminated' substance, are merely the product of the decomposition of an iron-rich mineral vein in the limestone. Indeed, the presence of the vein at the top surface of the original blocks explains the occurrence of the blocks in this configuration (cf. fig.42). The whole lithozone simply represents a very large slab which pulled away from the roof because of the incompetence of the mineral vein, and which fell to the floor, shattering into a 'band' of smaller blocks. Mineral veins of this type are quite common in the Buckfastleigh limestone, although no such vein outcrops in the visible walls of the cave, so that the slab must have fallen from a higher level in the fissure that is not presently accessible. The radical postdepositional alteration of these blocks is responsible for the softer parts having being confused in the past with water-laid limestone residues (cf. Sutcliffe 1974b). However, the material directly above the limestone blocks, lithozones D-E, is indeed water-laid. These laminated deposits represent quite low energy stream action, but the rapid variation in texture in E lithozone suggests similarly rapid fluctuation in flow speed. Although the exposure is very small, it seems very unlikely that the laminations in E lithozone represent cross-bedding; the extreme dip, if it is not a phreatic feature (unlikely), is therefore probably the result of tilting (subsidence) at a later date.

Judging only from the small exposure at the 'back' of the cave, lithozones B-E have a general dip of the same order as the inferred tuff band which forms the base of the cavity. However, the top of F lithozone, and what little bedding there is apparent within it, are more or less horizontal, making due allowance for later disturbance. The dense matrix, pellety fabric and generally chaotic nature of F lithozone show that it is referable to mass movement, most probably of the debris flow variety. However, the contorted lenses of fine gravel suggest that several debris flow events are represented, with intervening water flow events. The direction of flow is not apparent from the present exposures.

The sequence of lithozones B-F is therefore dominated by mass movement, not fluvial or other subaqueous, deposits. Previous chronological arguments put forward by Sutcliffe (1960, 1974b), based upon comparison with the levels of the Dart terraces in the Buckfastleigh area, are not valid. These cave deposits tell us nothing about the regional water-table, although it is unlikely that any of them were laid down under phreatic conditions. There is not even any evidence to link these sediments with the undoubted water levels, marked by solutional notches and calcite rims, to be seen at various heights on the walls near the 'entrance' and in the western passage, or with the phreatic solutional features apparent throughout the cave.

The next unit, G lithozone, is yet another debris flow deposit (cf. dense matrix, mud pellets and larger clasts, vesicular structures, etc.). Sutcliffe (1974b) recognised this unit as a "sludge" deposit at the 'back' of the cave. In fact, it is present all along the study sections, so that Sutcliffe must have included its north-east extension within his layer 1. It is interesting that even quite small limestone clasts in G lithozone retain a recognisable rock core, draped with 'wad', suggesting that this unit may be significantly younger than those below. There is considerable plastic deformation and erosion of the older deposits towards the 'back' of the cave, which seems to indicate that the G lithozone flow was moving down into the small south-west passage. However, some of this deformation could be the result of the major rockfall of I lithozone which was particularly heavy in this area. The regular top surface of G lithozone suggests that it was smoothed by sheet flow or trickle of water. It is tempting to refer the occurrence of H lithozone, in the apparently contemporary depression towards the 'back' of the cave, to such a phase, but the exposure is so small, and this relatively coherent laminated material is so like E lithozone, that it might be a remnant of older sediment.

After the fall of boulders represented by I lithozone, the cave entered a quiescent period during which the stalagmitic floor of J lithozone was formed. No traces of debris flow deposits have been recognised in younger units.

K lithozone is the main part of the celebrated Joint Mitnor "talus cone"; it contains a classic 'hippopotamus' fauna of ipswichian age. Before looking at this deposit in detail, it should be noted that it has a unique place in the history of taphonomic studies in British caves. Sutcliffe (1960) envisaged a fissure, leading to the surface above the cave, down which many different species of animal fell to their deaths below. Sutcliffe (1970) also used Joint Mitnor as the main source for a hypothetical model of bone accumulation in caves. This powerful image of a "pitfall trap" has been accepted by many later authors, either with reference to Joint Mitnor itself (e.g. Kurtén 1968; Taylor & Shackley 1973; Scott 1980; Stuart 1982, 1983) or more or less implicitly via reference to the hypothetical model (e.g. Ford 1976; Bishop 1982).

The ipswichian faunal assemblage from Joint Mitnor includes at the very least the remains of 127 individual animals of 16 different species, all from only a small part of the whole cone (Sutcliffe 1960). If this assemblage is totally, or even dominantly, the result of a pitfall mechanism, the fissure to the surface must have remained open for at least a few years. Sutcliffe notes that " the homogeneous nature of the fauna suggests that it accumulated in a relatively short time"(ibid., p.14). If the word "homogeneous" refers to the facts that the fauna is ecologically quite uniform and that most species were found at various levels in the deposits, this would seem to put an upper limit of perhaps a millennium on the duration of the accumulation.

The nature of the deposits accumulating below such a fissure will be quite variable, depending especially upon the exact geometrical relationship between the fissure opening and the surface sediment bodies and slopes. If large amounts of sediment are present as a thick cover very near the fissure opening, we might expect significant input to the cave by simple collapse, perhaps accelerated as larger animals tumble into the fissure. Collapsed sediments might survive in an extremely undercompacted form in the 'talus cone' in the cave below. However, if a fissure remains open for even a few years, wash deposits, with compact and well bedded structure, simply <u>must</u> occur. The present author has examined a large number of recent sediment cones below fissures, open to the surface or to higher passages, in caves in Britain and western Europe. Obvious wash deposits and drip craters are always present and, in addition, zones of dense, massive sediment, representing wet collapse or sludging, are quite common.

The fine matrix of <u>K lithozone</u> at Joint Mitnor lacks even minor traces of bedding or good compaction. Even the platy slate fragments are randomly oriented. Significant amounts of water never dripped, trickled or washed over the accumulating surface of the 'talus cone'. But how was water prevented from reaching the fissure if animals could fall down it?

There is absolutely no doubt in the present author's mind that <u>K lithozone</u> does indeed represent a cone of sediment that entered the present cavity through a fissure in its roof. The general morphology of the deposit, together with the reasonably consistent dip of larger clasts, could not have resulted in any other way. However, the fine matrix is strikingly anomalous. The nearest thing to this 'crumb' fabric that the author has ever seen is in old excavation tip that has lain in a generally damp, but not saturated, environment. However, K lithozone is not a very recent accumulation because the aggregates are often coated with thin clay 'skins'. Some massively organic deposits (i.e. 10-20% alkali-soluble material; cf. Badger Hole, p.956) show a vaguely similar fabric, but 'crumbs' are always associated with very well developed 'bubble' and 'tube' vesicular structure and marked cracking. In any case, had K lithozone once contained such rich organics, it seems most unlikely that the fabric could have survived their destruction. Crumb fabric is, of course, a common feature of the topsoil in limestone areas. However, even if the 'crumbs' of K lithozone are ultimately pedogenic, there should still be prominent wash deposits interstratified within the 'talus cone'. The author knows of no diagenetic process which could have produced the 'crumb' fabric in such a uniform manner throughout this deposit. There are no signs of significant carbonate mobility and there are no more unusual precipitates. If the 'crumbs' were the result of continuous percolation of water through the deposit, with a kind of 'coagulation' of the fines, one would certainly expect preferential zones of drainage and massive variation in aggregate size and development; also, individual 'crumbs' would lack significant porosity. It is therefore concluded that the 'crumbs' represent primary fabric, which is either inherited (e.g. stripping of topsoil) or, more probably, is due to the mode of final emplacement. If denser, well bedded zones had ever existed, they would have survived in some areas. It might be argued that the deposit could have been constantly disturbed by the thrashing of injured animals. Even if such a process could have systematically destroyed all traces of minor water-laid lenses and laminae, which seems most unlikely, it would also have broken down the fragile 'crumb' structure and interfered with the dip of the larger clasts. Another interesting feature of this deposit is that several 'generations' of limestone clasts appear to have been mixed together. The black ghosting could not possibly have occurred _in situ_, since perfectly sound clasts lie immediately next to large, deformed black ghosts or black-draped clasts. The origin of _K lithozone_ is obscure, but the only process which comes close to explaining all the observed attributes is more or less instantaneous damp collapse, perhaps as two or three main events. This suggests that the interpretation of the fauna as a classic pitfall assemblage may be in need of revision.

Sutcliffe (1960) published only general data on the 'talus cone' fauna. The arguments used to support the pitfall interpretation include the following:

(1) Herbivores (dominantly bison and various deer) far outnumber carnivores. From the figures cited by Sutcliffe, it can be seen that herbivores represent c.70% of the assemblage (calculated either from total specimens or from minimum number of individuals). This proportion is nearer a cross-section of the local fauna than would usually be found in 'den' caves, but there are still 28 individuals representing the larger carnivores (wolf, lion and hyaena).

(2) The cave does not appear to have been a hyaena den, since most of the bones are complete (totally complete or merely fractured but ungnawed?), there being only two gnawed fragments. The hundred or so coprolites could have been derived from dead hyaenas. This point seems most reasonable; _K lithozone_ is not at

all like any hyaena den which the present author has seen.

(3) Juvenile carnivores are rare, again militating against denning.

Sutcliffe (1966) added: (4) Some of the remains of bison, deer, wolf and hyaena show arthritic deformity, indicating older age or general infirmity, which would have made these individuals more susceptible to falling into the trap. This point seems reasonable, but such infirmity would also make these animals more susceptible to predators or any kind of accident.

Two other points, not discussed by Sutcliffe, may be deduced from the available faunal data. First, large numbers of bones and teeth appear to be 'missing'. Sutcliffe recognised a minimum of 44 individual bison (the most common species), which should give a total of c.8500 (complete) bones and teeth compared with the 1622 specimens (mostly complete ?) actually recovered. Thus, some 80% of the skeletal parts would seem to be missing. This figure rises to 90% and more for most of the other species represented. It seems most unlikely that this kind of discrepancy could be totally due to a size sorting effect on the steep slope, because it involves not only the large animals, such as elephant, but also the smaller ones, such as fox and fallow deer. Second, not only are parts missing, but the skeletal proportions also appear to be wrong. The natural proportion of teeth to all skeletal parts in hippopotamus is c.16%, but over 120 teeth out of only 374 specimens (i.e. over 32%) were recovered. In rhinoceros the natural proportion is c.13%, but over 100 teeth out of only 270 specimens (i.e. over 37%) were recovered. There is absolutely no reason to suspect that preferential chemical destruction after deposition in the 'talus cone' might have produced this pattern. Although Sutcliffe did not himself excavate most (any ?) of these remains, he does not report any mention by Ogilvie, either in person or in his notebooks, of material in articular position (but vide infra). Even making allowance for the facts that only a small part of the 'talus cone' has been excavated, and that old bones might have been disturbed by later falls of sediment and animals, these figures are extreme enough to suggest that the fauna is not composed of complete skeletons. This contrasts markedly with the normal situation in pitfall traps:

> Natural deaths may be of special significance in certain death-trap type caves, but since entire skeletons should be preserved as a result, this agency is likely to be easily recognizable. (Brain 1980:107)

The only hint of information counter to the above arguments is provided by the "artist's impression" (drawn by M. Wilson) included by Sutcliffe (1960, Plate VIII). This drawing shows complete skulls of a bear and of a hippopotamus, together with the whole carcass of a bison, resting upon the accumulating surface of the 'talus cone'. It is not clear to the present author how much artistic licence has been employed. However, at the very top of K lithozone in the extant study section, there are still over 20 in situ bones, several of which appear to be vertebrae in more or less articular position. These vertebrae are deeply embedded in the matrix and are difficult to observe; nevertheless, they could well belong to a bovid. Other bones and teeth are exposed at several points lower in the study section, but the present author could not recognise any other possible articular associations (cf. Sutcliffe 1976, Plate 13.2.).

The present author considers the question of the Joint Mitnor 'pitfall trap' to be still open. From Sutcliffe's descriptions, the faunal remains certainly do not appear to be dominantly referable to carnivore denning, and especially not to hyaena activity. On the other hand, the sediments suggest rapid collapse, possibly as more than one major event, but probably not spaced out over any significant length of time. If herbivore material is indeed present in articular position, at least at the top of the cone, this would probably indicate that a fissure was indeed open to the surface but, if only one or two animals are involved, this route might only have been available for an extremely short period. One wonders whether a pitfall or some other natural trap situation might have developed a deposit in some area nearer the surface, a deposit which later collapsed (or partially collapsed) to form the present cone, leaving a very short-lived opening to the surface. One may even speculate further by suggesting that the collapse might actually have been triggered by the weight of a bison walking onto unstable deposits outcropping at the contemporary surface. The Joint Mitnor fauna is apparently homogeneous and indicative of an ipswichian age; the composition of the enclosing sediments is compatible with an interglacial, especially with respect to the significant alkali-soluble organic content. However, a bison would not be out of place in the Devensian. It would be interesting to check whether the presently in situ remains at the top of the cone indeed represent 'articulated' portions of such an animal and whether there is any way to demonstrate that they are younger than, or at least different from, the rest of the bovid remains in the 'talus cone'.

Immediately above K lithozone lies a band of blocks, with wall and roof speleothem fragments, fining upwards into angular scree (L lithozone). This deposit need have no climatic significance and, since the upper surface of K lithozone is not compacted, it is tempting to associate the slabs and scree with the above proposed series of collapse events.

M lithozone shows no signs of bedding, but it is clearly composed of sediment very like the slaty surface deposits in the local landscape, together with an admixture of fine limestone scree. Again, this material is unlike a bed of fine sediment, formed by regular wash below a roof opening. Therefore, it too could be a collapse deposit, representing the input of masses of surface sediment which finally blocked the fissure. The stalagmite of N lithozone is the first deposit which unequivocally indicates a slowing of the sedimentation rate; indeed, it indicates virtual sealing of the cave. The author was not able to observe whether or not the stalagmite continues under the boulder and sediment choke at the highest accessible point of the 'talus cone'. This detail is stratigraphically important and a further attempt should be made to clarify the situation.

If the author is correct in suggesting a very rapid depositional sequence from K lithozone to M lithozone, with the progressive collapse of old sediments, roof and wall debris and, finally, dominantly exterior sediments, any fauna within M lithozone might help to date the collapse phase. Sutcliffe (1974b) stated that M lithozone (layer 6a) was unfossiliferous. However, the unpublished document by D.B. Thomas, referred to above (p.793), includes a note that layers 6a-c, collectively, contained bats, birds, rodents and shrews. Microfaunal remains are certainly abundant in the disturbed material observed by the present author above N lithozone, but they are mostly very recent. A sample (1kg) of the matrix of M lithozone contained only two fragments of rodent incisors and no other faunal material. However, given the interest that any dating evidence would provide, it may be worth searching larger samples of this deposit in the future.

Apart from the interest generated by its rich ipswichian fauna, Joint Mitnor Cave is also of importance to taphonomic theory.

In the past, a comparatively simple pitfall model has been proposed. The conclusions drawn from the present study of the sediments by no means contradict the whole concept of a pitfall. Nevertheless, sufficient anomalies have been recognised to suggest that the processes, or events, involved may have been rather more complex than originally supposed. The present author's treatment of the faunal material has been most superficial, based as it is on the only published accounts. More information is doubtless available in Sutcliffe's doctoral thesis (1957) which, unfortunately, the present author has not been able to consult. Most of the faunal collection, together with Ogilvie's site notes, are housed in the Torquay Natural History Museum; further study of these documents would certainly be rewarding. The site itself must still represent the richest potential source of information. D. Curry (Pengelly Cave Research Centre, pers.comm.) has informed the author that a programme of remote sensing (involving a variety of techniques, including helium tracing upwards through the roof choke) is to be set up, under the direction of P. Grainger (Department of Geology, Exeter University), in order to locate the fissure at the surface above the cave. When everything possible has been deduced about the connection to the 'talus cone' using non-destructive methods, it may prove practical to undertake further careful excavation, without endangering the study sections or the fragile ecology of the cave. As Sutcliffe has long pointed out, examination of a similar 'talus cone' in the neighbouring Reed's Cave would no doubt enlarge our understanding of this general topic. It is a great pity that so many, apparently much less equivocal pitfall traps once recorded in this country have since been destroyed (e.g. Dream Cave, Derbyshire - Buckland 1823; Milton Hill Fissure and Dulcote (Yeoman) Fissure, East Mendip -

Balch 1948; Alveston Bone Fissure, Gloucestershire - Taylor & Shackley 1973).

21.6. The Chudleigh Caves

21.6.1. Chudleigh Rocks

Chudleigh Rocks (SX 865787) are a restricted outcrop of limestone, 1km south of the town of Chudleigh. The limestone is dissected by a small stream, the Kate Brook, which rises on Great Haldon (c.240m O.D. at that point), 7km to the north-east, and which joins the Teign 0.5km downstream from Chudleigh Rocks. The short, steep-sided 'gorge' thus formed is known as the Glen. It is on the north-west side of the Glen that the limestone (reaching c.78m O.D.) appears most prominent, although this impression has been greatly accentuated by the production of 'cliffs' by quarrying over the last three centuries.

Within the Glen, the grey limestone is massive and hard but, a few hundred metres to the south-east, it is sometimes extensively dolomitised. Below the limestone are soft, greenish, fine grained rocks, with parallel fissility. A local fault, midway through the Glen, has thrown the limestone down on the north-east side. Thus, the stream enters the Glen on (harder) limestone at c.42m O.D., but leaves it on the (softer) greenish rocks at c.28m O.D., there being a nick-point (waterfall) just upstream of the fault, with a succession of drops totalling over 11m.

There is some uncertainty as to the stratigraphic position of both the limestone and the greenish rocks. Butcher and House (1972) refer to the limestone as Chudleigh Limestone and state that, although it cannot be precisely correlated with other outcrops in this part of Devon, it is mostly of Middle Devonian age (Givetian and Lower Frasnian). This limestone is mapped by the IGS (Sheet 339, Newton Abbot, 1:50,000, 1976) as Middle to Upper Devonian Chercombe Bridge Limestone, implying correlation of outcrops within a 15km long area straddling the Teign. The IGS map the greenish rocks as Kate Brook Slate of Upper Devonian (Famennian) age, whilst Butcher and House (1972) refer to them as green, soapy shales, probably of Upper Devonian (Frasnian) age. Whatever the exact relationship between these rocks, it is clear that the green slate/shale is younger than the overlying limestone, the contact being a major thrust plane.

The geology around Chudleigh Rocks is extremely complex due to faulting and folding. This has led to a variety of lithologies becoming available as source rocks for the Kate Brook catchment. Upstream of Chudleigh Rocks, the stream runs approximately at the contact between Kate Brook Slate/Shale, on the south-east side, and the Upper Carboniferous Crackington Formation with grey shales and fine-grained sandstones, to the north-west. As the terrain rises towards Great Haldon, the stream passes progressively across more Upper Devonian limestone, New Red Sandstone and Upper Greensand (Blackdown Facies: glauconite sands with pebble and shelly beds; Lower Cretaceous). The New Red Sandstone is represented by Teignmouth Breccia (either oldest Permian or youngest Carboniferous), which is a reddish brown earthy sand and fine gravel, with some large boulders (cf. Scrivenor 1948); this material is of considerable interest with respect to the Chudleigh cave sediments. On top of Great Haldon there are Buller's Hill and Tower Wood Gravels (Eocene), as well as extensive gravel, 'head' and coverloam deposits dating from the Pleistocene. Edmonds et al. (1975) note that the abundant flint in these tertiary and pleistocene gravels contains well preserved fossils which are referable to the higher biozones of the Upper Chalk (Upper Cretaceous) and they assume a former chalk cover in the area. All these lithologies have, at one time or another, reached Chudleigh Rocks via the Kate Brook Valley. To the south-east of the Rocks there is a conformable sequence from the Upper Devonian limestone to the Ugbrooke Sandstone of the Upper Carboniferous, passing via zones of nodular and shaly limestones, purple and green shales, siliceous slates, cherts and conglomerates. It would appear that the lithologies crossed by the Kate Brook (to the north-east) play a greater part in the deposits within the caves, or at least within those on the north-western side of the Glen, than do the types outcropping on the gentler slopes ("Mount Pleasant") to the south-east.

Several caves have been found and destroyed during quarrying in this area. Others are still apparent in the old quarry faces. Three extant sites are of interest here, Tramp's Cave (Shelter) on the south-east side of the Glen, and Cow Cave and Pixie's Hole on the north-west side. Only this last site has been examined in detail by the author and the description of this work will take up most of this section. The other two sites, together with the Chudleigh Fissure (a possibly extant site), are briefly discussed in section 21.6.7.

Part of Chudleigh Rocks, including the caves discussed here, has been designated as an SSSI, on the grounds of the rare fauna and flora. Pixie's Hole contains a (hard-pressed) nursing colony of greater horseshoe bats and, for this reason, visits (especially in the summer) should be restricted to that part of the cave system described in the next section. The main Chudleigh sites do not appear to be in any further danger from quarrying, although the Palace Quarry, at the north-east end of the Glen, was still being worked on a small scale in 1978. The author was unable to discover anything about the recent history of quarrying in the Rocks; indeed, it was most forcefully suggested that he should cease his enquiries. One clue regarding the clash between conservation and commercial interests is provided by a tiny plot of land, on top of the limestone directly above the easterly entrance to Pixie's Hole (infra), which is National Trust property. Although the remains of the medieval palace of the Bishop of Exeter lie close (c.13m) towards the north-west, the plot itself, which is only a few metres across, contains absolutely nothing of interest. Its purchase by the Trust can only be explained as a ploy to give more legal weight to stop quarrying of the rock below. It is a sad fact that no government agency will take action concerning limestone sites and the SSSI designation would not have afforded any protection whatsoever.

21.6.2. Pixie's Hole - Morphology

Pixie's Hole (SX 866787) is a relatively small but very complex cave system that passes through the remaining limestone block left by quarrying on both its southern and northern sides. The part of the cave of interest here is the most easterly section, which is relatively simple, but further to the west the system develops on at least four different levels, and choked passages lead off most probably to link up with other named entrances in the Rocks.

The eastern section (fig.43) is a long passage which slopes gently downwards to the north. Much of the passage, and the Pope's Chamber beyond, are developed on a near-vertical fault. After

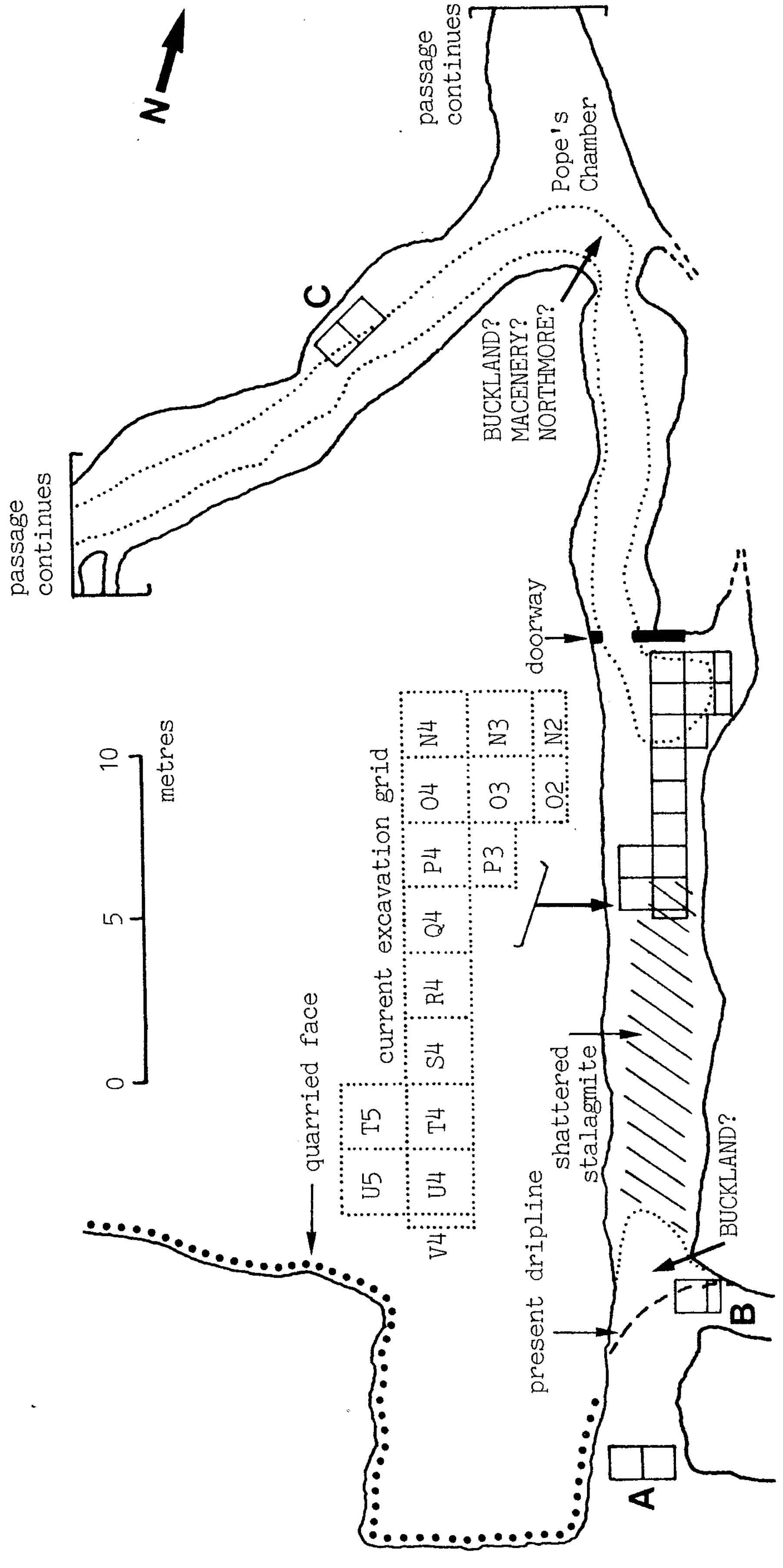


Fig.43 Pixie's Hole - Plan

c.40m, the eastern passage is joined by another passage, sloping slightly upwards to the south-west and linking with the rest of the system. Further north, the Pope's Chamber quickly narrows to a mere fissure choked with stalagmite.

The eastern passage, which contains the most important deposits, comprises three main sections. The innermost section, from the Pope's Chamber to the recent (interior) doorway, has a smooth, probably water-cut roof. It would appear that the main fault passes slightly east of this section of the passage, although it seems unlikely that a major cavity has been developed along this structural route. The second section, from the doorway to the modern drip-line, has a roof tapering to a narrow fissure along the fault. The rock is angular and fractured and, in places, large roof blocks are ready to fall. The third section, from the modern drip-line outwards, has been affected by quarrying to an unknown extent. Shot-holes are present in the rock marked "quarried face" in fig.43; these holes appear to have been cut by power tools, suggesting that at least preparations for blasting continued into this century. The large block of limestone on the eastern side of the 'entrance' may or may not be in situ bedrock; the continuation of the cave wall can be clearly seen on its western face. The unroofed (?) cavity leading up to a surface ravine just to the east (the cavity between the outer block and the main limestone mass) may be an alternative 'entrance' or it may merely have open up if and when the outer block was disturbed. Given the morphology of the valley at this point, it would seem unlikely that more than a few metres of the cave mouth have been removed by quarrying.

No excavator at Pixie's Hole has reported finding bedrock below the cave deposits, although some have dug to depths of over

3m in various parts of the system. The present author found a rock ledge (?), just to the south-east of the (interior) doorway, at a depth of c.3.5m below the highest deposits, but there is a vadose trench on the west side of the passage at this point which was not bottomed even after another 1.5m.

21.6.3. Pixie's Hole - Previous Examinations

The author cannot resist the temptation of quoting the following poem concerning Pixie's Hole.

SONNET
ON THE CAVE IN CHUDLEIGH ROCK

Within this vaulted rock, the Fairies dwell!
I fear to tread upon the enchanted ground:
For o'er this arch! and through the Cave's dark round
They've spread a charm; and wove a mystic spell!
Ev'n now perchance they scoop the concave roof!
Or pave with sparry studs the fretted floor:
Now, the unfathomed watery depths explore;
Or hang their icicles of stone aloof.
Beneath yon branchy oak, on moonshine nights
They thread the dance; or in yon streamlet lave.
Within the precincts of the hallowed Cave
They celebrate alone their festal rites:
Ah then, rash Mortals dread too near to press
For vengeful Phantoms guard the deep recess!

Rev. J. Swete, 1794.

This sonnet cannot be claimed as a masterpiece of eighteenth century literature but it does at least have the dubious honour of being one of the longer references to Pixie's Hole, despite the fact that this cave has been extensively excavated by W. Buckland, T. Acland, W.C. Trevelyan, T. Northmore, J. MacEnery and others. Pengelly (1873) collated everything that was known about the early excavations, although he did not claim to have dug at the site himself. Pengelly's publication is the synthetic source for the following brief discussion as it is much more accessible than the primary sources, some of which have not been published elsewhere.

The earliest reference (c.1797, Paddon, cited in Pengelly 1873:50-1) to the eastern passage considerably overestimates the length of the innermost, more restricted section (from the Pope's Chamber to the inner doorway). However, it is suggested that the wider section, outwards to the entrance, was about sixty feet long at that time. Also, it is stated that daylight just penetrated into the innermost section. It therefore seems probable that no great length of passage or roof has been removed by quarrying at the entrance since the eighteenth century.

Buckland was the first recorded excavator at Pixie's Hole, although it is not at all clear where and how much he dug because he never published his findings. The only general reference to the Chudleigh caves is in a footnote to his second edition of Reliquae Diluvianae (1824:69). Pengelly suggests that Buckland dug at Pixie's Hole in 1824-5 and he quotes a letter by Buckland recording finds of flint, pottery, domestic animal bones and charcoal. A magazine report of the time also includes a bizarre list of pleistocene fauna (said to have been recovered from below a stalagmitic floor) which cannot be taken as reliable (cf. Pengelly 1873). Quite a number of people appear to have worked with Buckland, although they each seem to have dug separate trenches, in an 'every man for himself' fashion. The only record of any interest is provided by T. Northmore:

> This cavern was peculiarly the residence of a British family, and Dr. Buckland in his investigation discovered what appeared to me (both from its round, or rather oval saucer-like form, and from its contents) a British kitchen. Charcoal, pottery, flint knives, etc., rewarded his research, but I deeply lament that the Professor of Geology should have destroyed this relick so valuable to the admirers of antiquity; a small portion now only remaining. It was scooped out by the Britons through the stalagmite into the mud and bones; and so well rammed or hardened at the bottom that I might almost call the floor a Devonian lime-ash. The mud in one portion of this cave is of great depth (in another portion there is none at all), but the bones lie near its surface, from about 1 to 2

> feet deep; all below seems free from bones; the cave however has not been sufficiently searched. Miss Jones found in this cavern a beautiful tooth of a bear. I sounded 6 or 8 feet of this mud without finding any bottom, but I discovered a thin black layer or regular stratum of what I take to be the black oxyd of manganese lying about 3 or 4 feet below the surface, and continuing, so far as I could judge, nearly through the whole length of the cavern. (1832, cited in Pengelly 1873:56-7)

MacEnery dug in Pixie's Hole at some time between 1829 and 1841 and recorded the following notes in his diaries:

> ... I devoted some days to the systematic exploration of its contents .... Where Dr. Buckland's party left off I continued to work, and sunk a shaft in quest of the bottom to the depth of ten feet below the surface, but it seemed to dip to an indefinite depth, following the extent of the perpendicular fissure. I had also some new excavations made near the extremity of the left branch [the passage leading to the western part of the system], and sunk a shaft there of still greater depth, till it appeared hopeless.
>
> The several excavations furnished generally nearly similar facts, with the exception of the stalagmite of the farther excavation being thinner than those near the mouth. The constant resort and burrowing of rabbits, whose presence was attended by entire skeletons lying near the surface, impeded the increase of the crust. There was but a slight skin of stalagmite. Earth more dark and loose and more abounding in bones [and] horns, which belonged chiefly to the large Stag or Red Deer. Ox, fresh and fetid.
>
> As the excavation deepened, bones became scarce, being confined, indeed, to a few solitary splinters much decayed, referrible to the Bear. The earthy deposit became, too, different; more sandy and full of pebbles, numerous nodules of flint, quartz, sandstone, and limestone rounded at the angles; the earth heavy and mouldy, but in its general characters bearing an affinity to that of the external plains of which at a former period it constituted the covering. [...] ... its principal and distinguishing feature is the extraordinary abundance of flint and gravel, and also the greater prevalence [when compared with the Torquay area] of quartz gravel and fragments, thus identifying itself with the siliceous stratum that overspreads the [exterior land] surface on all sides to a great extent;.... [...]
>
> I was not able to detect any trace of Hyaena's bone; nor do I think that the small splinters of bone found there can with any certainty be traced to [the gnawing of] that animal;.... The large bones were frequently found entire, and when broken it was generally at their weak part near the centre, as if from collision with harder bodies,.... (cited in Pengelly 1873:54-5)

The present author noted traces of disturbance and excavation throughout the eastern part of the Pixie's Hole system. A sounding was sunk at A (fig.43), which revealed c.50cm of recent wash deposits overlying at least 3m of coarse quarry rubble; this material was too unstable and dangerous to continue to a deeper level. At B (fig.43), wash deposits overlay at least 1.5m of completely disturbed stony sediment (backfill?). It is assumed that the "British kitchen" was found somewhere near this point, although it may have been deeper into the system. Just within the modern drip-line there are traces of thick, tufaceous stalagmite adhering to the west wall, c.50cm above the level of the disturbed fill which forms the present floor. Below a few centimetres of organic-rich sediment, slabs of stalagmite become more and more abundant as one proceeds inwards. By c.6m into the cave the slabs constitute a continuous 'floor', although the unit is extensively shattered up to a point c.11-12m inwards from the drip-line. This phenomenon is almost certainly the result of blasting at some time near the cave entrance.

At a point c.3m south of the interior doorway, the most obvious trenches begin. These continue for over 50m into the system. The depth, width and orientation of the trenches are extremely variable. There was no attempt to cut even approximately linear or vertical sections and there are many deep 'grubbings' into the trench walls where bones were extracted. At one point at the outer end of the trenches, there was a shallow 'niche' in the cutting wall. When the present author excavated the *in situ* sediments beyond, a large bear mandible was found, with the ascending ramus, the posterior part of the horizontal ramus and then a recent fracture, the latter aligned with the inner boundary of the 'niche'; the 'excavators' had simply exposed the tooth row and then pulled. The trenches are all partially or totally back-filled with material from the immediate vicinity, showing that the work proceeded by digging 'in front' and filling 'behind' (uncharitably known as the 'Russian technique' in some quarters). There are very common charred wooden brands that must represent the torches used to light the work. There are massive quantities of bone (including large fragments) throughout the backfill which, together with the general chaos of the digging, suggest that the work was carried out at very high speed. The author has never seen such clear evidence of the appalling standards of early cave excavations.

All the nineteenth century excavators completely missed the facts that Pixie's Hole contains a complex sequence of deposits, several rich and varied faunas, and a moderately abundant Later Upper Palaeolithic industry. That this last component was overlooked is probably most fortunate. The site has no doubt been partially protected by Pengelly's authoritative conclusion:

> With regard to the Pixies' Hole, I am at present decidedly inclined to say, ..., with Mr. Mac Enery, that "its interest is entirely confined to its possessing fossil bones and to the superstitious tales of the peasantry around". (1873:60)

21.6.4. Pixie's Hole - Recent Excavations

The archaeological material in Pixie's Hole was indeed noticed before the present author's excavations. In 1960-1, J.W. Simons (pers.comm.) recovered several flint flakes and a bone with charcoal adhering to it, from the deposit below the upper stalagmite at an exposure near the interior doorway. B. Boulton (pers.comm.) has also indicated that an end-scraper was recently found by an unknown person in the same area of the cave. The present author visited the cave in 1976, believing it to be a purely palaeontological site. Two days' work, involving cleaning of some of the old sections, showed that there was an interesting sequence with several rich faunal units. Artefacts and charcoal were also quite obvious, and a dozen pieces, including a fine burin on a truncation and a pierced stone pendant, were recovered. A small scale excavation was carried out during the two seasons 1977-8, mostly in the area just to the south of the interior doorway (cf. excavation grid shown in fig.43).

The stratigraphy recognised during the recent excavations is rather complex, due to the generally lenticular bedding of many of the deposits. Only the six squares O4-2 and N4-2 were dug to any great depth (cf. fig.43); squares P-T4 and P3 were dug to c.1m and squares U-T5 and V-U4 to c.40cm. Squares P-N4 and O-N3 had already been partially dug, presumably by the nineteenth century excavators. The restricted exposures produced by the recent work do not allow detailed interpretation of the finer lithologic divisions. Therefore, many of the deposits described below should be regarded as composite, higher level units that may be subdivided at a later date. Although an attempt has been made here, as always, to comply with the suggestions set out in the International Stratigraphic Guide (Hedberg 1976), the nature of the problem is such that only informal (casual), rather than 'preformal' (cf. p.151), lithostratigraphic classification is currently justifiable.

Silt, Sand and Gravel (SSG). The lowest deposits examined during the present work occur in what appears to be a rock-cut channel in the cave floor, aligned along the trend of the eastern passage (fig.44). The exposed rock on the east side of the channel shows extensive scalloping; the direction of the water flow which caused these features was probably inwards to the north, although the restricted exposure calls for caution. It is tempting to see this channel as a vadose trench cut into bedrock, but it is also possible that the observed rock is merely the edge of a large detached block surrounded by sediments. The sediments of SSG comprise a series of quite well bedded, but usually very badly sorted, silts, sands and fine gravels. Colours are variable but

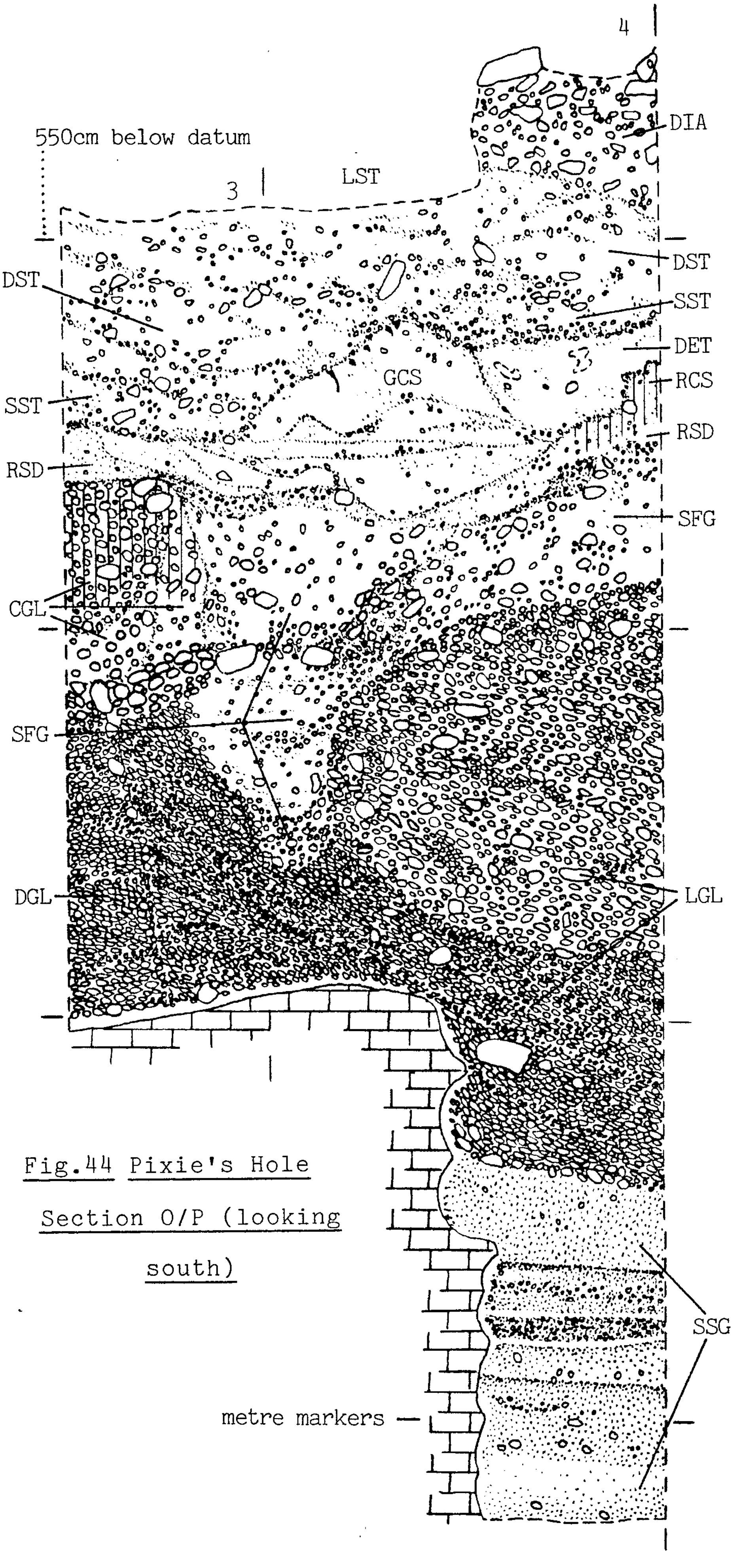


Fig.44 Pixie's Hole Section O/P (looking south)

are all within the 10YR range. Most of the sands are quartzitic, although there is much fine slate debris at all levels. The silts and accompanying clays appear to be predominantly alteration residues of slate/shale, although the fine laminations in some of these beds show that the alteration had mostly taken place before deposition. The gravels are almost exclusively composed of irregular flint debris, with angular but heavily edge-rounded forms. The beds remain well organised right up to the thin zone of clayey decomposition products that covers the limestone 'wall'. The most remarkable feature of these deposits is that, at least within the present exposure, all the beds dip down uniformly to the north by c.25°.

Dense Gravel (DGL). These deposits comprise a series of medium to fine gravels, with zones of normal grading and generally more horizontal bedding, although there is a slight dip into the cave. There is only a little coarse sand matrix but the deposits are nevertheless dense (well packed) and stand well in section. The gravel is again predominantly composed of irregular flint debris, but some more pebble-like (rounded) forms are present. These deposits occur above and to the east of the possible vadose trench (supra) and there was no observed stratigraphic contact with SSG (cf. fig.44).

Loose Gravel (LGL). This is a massive unit composed of coarsely and irregularly stratified gravel, which fills the remainder of the possible vadose trench and spreads up to and, in eroded patches, over DGL to the east. The lower part of the unit comprises beds of slightly better packed, fine to medium gravel, whilst the upper part (quite sharp transition) consists of masses of very loose, badly sorted gravel. Throughout the unit, beds dip down into the cave at c.25°, and there is also a 20-30° cross-dip, down to the west (cf. fig.44). The main component at all levels is angular, but edge-rounded flint gravel; there are also very minor components of quartzite, dark tourmalinised rocks, various light-coloured cherts, and shales (hard, dark rocks unlike the Kate Brook series). The gravels are clast-supported throughout and the sparse matrix is irregularly polymodal, usually with major modes in the medium and coarse sands, composed of flint and quartzite debris. There are also large quantities of fine, green slate debris in some zones of the matrix, irrespective of bedding; in these zones, the matrix colour is a reddish brown (5YR 5/3-4). However, in other zones, there is little recognisable slate and all coarse particles (including individual sand grains) are coated with a grey (5YR 5-6/1) clayey silt that masks all other colours. In places, it can be seen that this grey material is derived from a decomposing rock of generally pelitic type (slate/shale).

Cemented Gravel (CGL). This material occurs on the eastern side of the excavated area (mostly in squares N2-3 and 02-3; cf. figs.44 and 46). It is not clear whether this is a lateral equivalent, or partially a lateral equivalent, of SFG (infra) or a separate, older unit. CGL is composed of a series of ill-defined channel deposits, rich in coarse to medium flint gravel. Some of the flint, especially the larger particles, is quite well rounded, although the finer gravel is still very angular. The unit is very well cemented towards its top, mostly by iron (hydr)oxides but also with minor amounts of secondary carbonates. The cementation weakens progressively towards the base. The matrix is generally a silt/sand; there are some beds with matrix support but no fine structure is visible. Colour is very variable, with mottles of grey, red and yellow, as well as minor films of black probably representing manganese.

Silty Fine Gravel (SFG). This unit is composed of a complex series of channel deposits, the deepest cutting well into LGL (cf. fig.44). There is a general dip of c.17.5° into the cave. This unit is not cemented but it is otherwise quite like CGL. The matrix is generally a silt and fine sand, with flint 'grit' and a little true (i.e. plastic) clay. The colour is a very strong 'ginger' (c.5YR 4/8), and iron compounds coat many of the particles. There are moderately common brittle, almost black particles, of irregular form but with curvilinear layered internal structure; although these might represent some ultimate in situ alteration product of limestone, it seems more likely that they are reworked fragments of an ancient, impure 'ironstone' of some type. The sands, although dominantly quartzitic, contain appreciable amounts of various feldspars. The coarse component is again dominantly composed of flint. Exact quantification of the rarer 'accessory' rock types has not yet been carried out but, in addition to those types already noted in older deposits (cf. LGL), there are small particles of softer rocks (siltstones, a fine grey sandstone and a coarser reddish sandstone). Although individual channels in this unit are reasonably well defined, the deposits within each channel are not well organised and, despite common matrix support, there is no visible fine bedding and little preferential orientation of larger particles.

Concreted Stones (CST). This is a lenticular unit present only towards the northern side of the excavation (cf. figs.45 and 46). It is composed of many, highly altered limestone clasts, mostly quite large, set in a ground mass of tufaceous carbonate. There is appreciable siliceous fine sand, silt and clay dispersed within the carbonate, but not enough to constitute an autonomous matrix. There are 'lenses' of less well concreted material, with

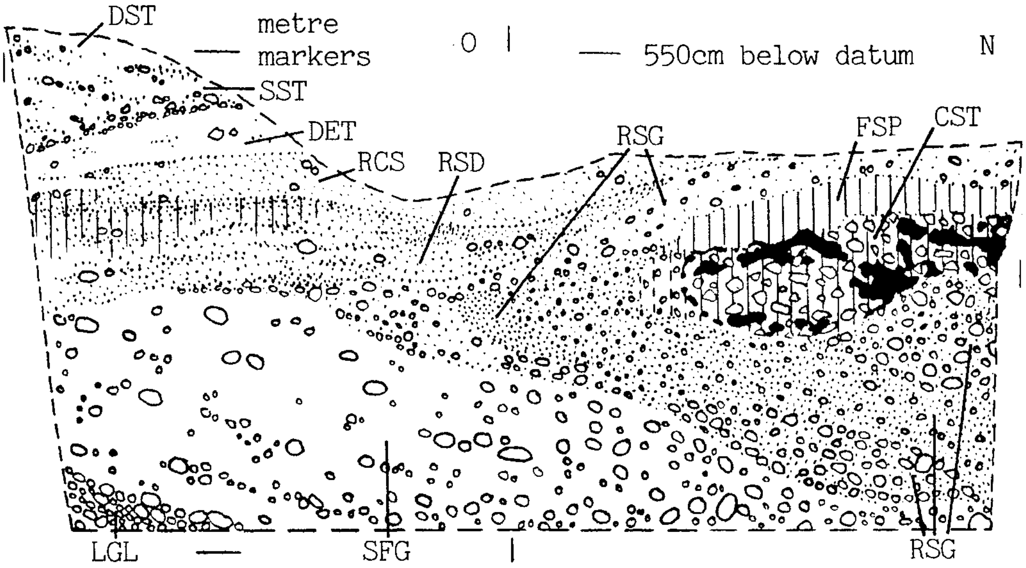


Fig.45 Pixie's Hole - Section 4/5 (looking west)

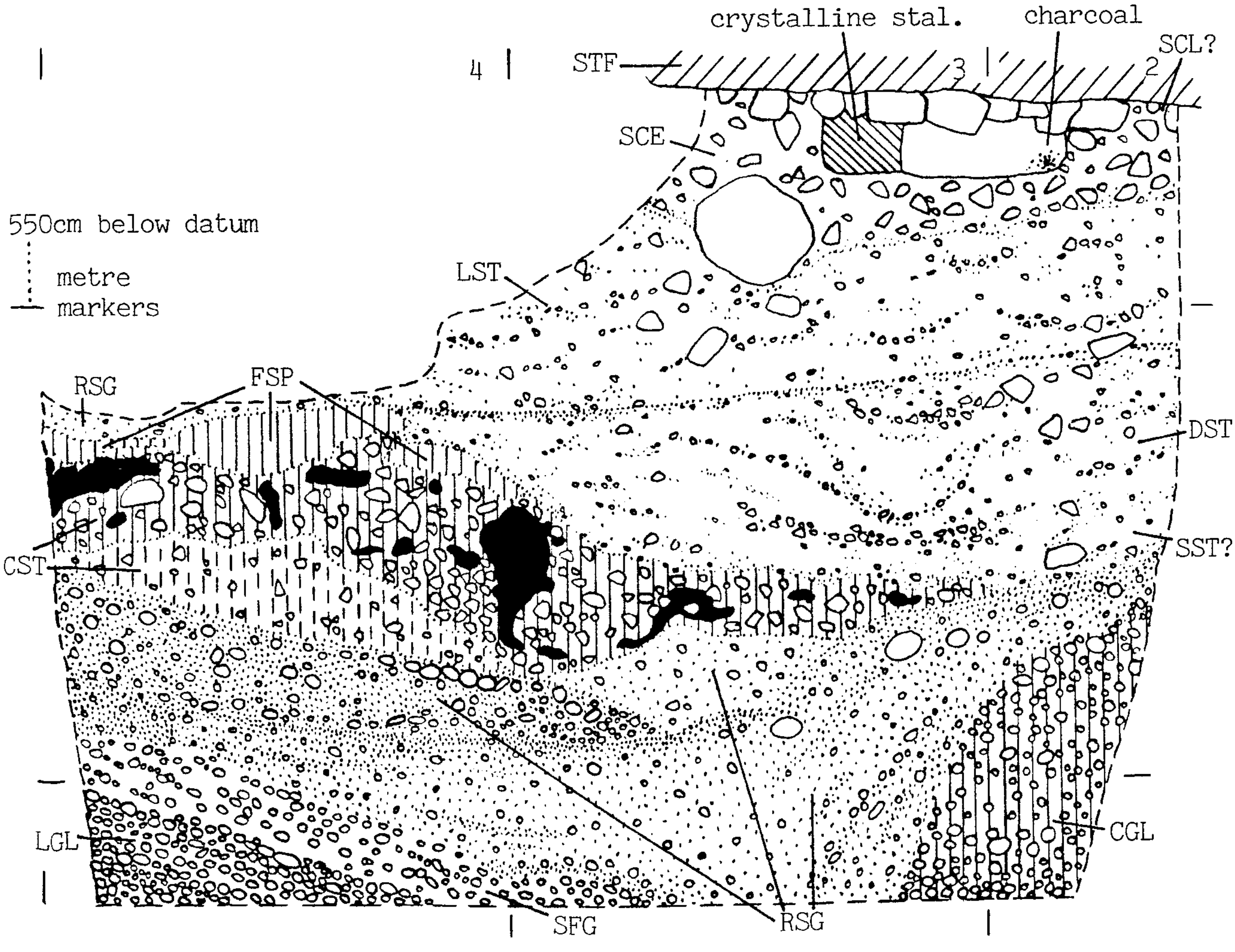


Fig.46 Pixie's Hole - Section N/M (looking north)

slightly smaller limestone clasts, towards the base of the unit but, although the boundaries are quite sharp, it is not clear whether this is a depositional or diagenetic effect. There is absolutely no organised fabric within these deposits and the carbonate shows no stratification. In the main, upper part of the unit there are large, highly irregular masses of powdery and gritty material of a very dusky red colour (2.5YR 2.5/2). Examination of this material shows it to be almost exclusively composed of comminuted bone. The bone is very brittle, and fresh breaks show a metallic, 'bluish' lustre which dulls in a few hours. The density of individual bone particles may be as high as 2.3g/cm^3. Tests for manganese give a strong positive result. There are small amounts of bone 'powder' throughout this unit (even towards its base), so that the tufaceous carbonate has a 'dirty', very pale red colour (c.2.5YR 8/2). The outer boundaries of CST are sharp (infra).

Friable Speleothem (FSP). Above CST, and cemented to it at sharp boundaries, are masses of purer and denser carbonate. Although this material is friable and shows signs of extreme alteration and recrystallisation, faint traces of stratification in some zones indicate that it is a true speleothem, probably part of a stalagmitic floor.

Red Sandy Gravel (RSG). The lenticular mass formed by CST and FSP is literally immersed in sandy gravel; the significance of this arrangement is discussed below (p.846). RSG is a series of broad channel deposits, composed of sediments that are quite similar to those of SFG, save that there is more sand and minor bedding features are more regular. Due to the lenticular nature of both SFG and RSG and to their general lithologic similarity, it is possible that the present allocation of individual beds to one or the other unit is in error in minor details. Nevertheless, the overall increase in organisation, upwards through the sequence, is clear. The matrix colour of RSG is yellowish red (5YR 5/6). The bedding shows a general dip of c.10° into the cave, although this parameter is difficult to estimate since little of the deposit was actually dug (cf. old excavations) and the dip had to be estimated from planar exposures alone.

Red Sand (RSD). This is a yellowish red (5YR 5/6) fine to medium sand, with some fine flint gravel and 'grit'. In places, roughly horizontal laminar bedding is visible, although in other zones the sand appears more massive. Iron compounds are common, giving very weak cementation, but, towards the top of the unit, there are areas of stronger carbonate cementation.

Red Clayey Sand (RCS). This is a reddish yellow (5YR 6/6) clayey deposit with sand and a little fine gravel floating in the structureless matrix. In one small area (cf. fig.45), it appears to be cemented by carbonates like RSD below it, but its highly convoluted upper boundary and rather diffuse lower boundary make it difficult to be sure of the order of events.

Grey Clay and Silts (GCS). This unit is quite complex, its lower subdivisions apparently being partly composed of material reworked from the sandy sediments below, or perhaps even of *in situ* sandy beds with massive infiltration of finer particles from above. The main unit, however, forms a discontinuous linear ridge, with internal, rather irregular stratification dipping east and west away from the accreting 'crest'. The sediment is a silty clay, of various low chroma 'grey' reds (2.5YR 3-6/2). It is dense and 'greasy', but there are traces of pellet structure (c.1-2mm in diameter). There are very small amounts of fine flint gravel and 'grit' and some unsound slate debris. Manganese is present in a finely divided form and there are many small patches of pure but structureless carbon. The unit is aligned exactly below the main roof fissure.

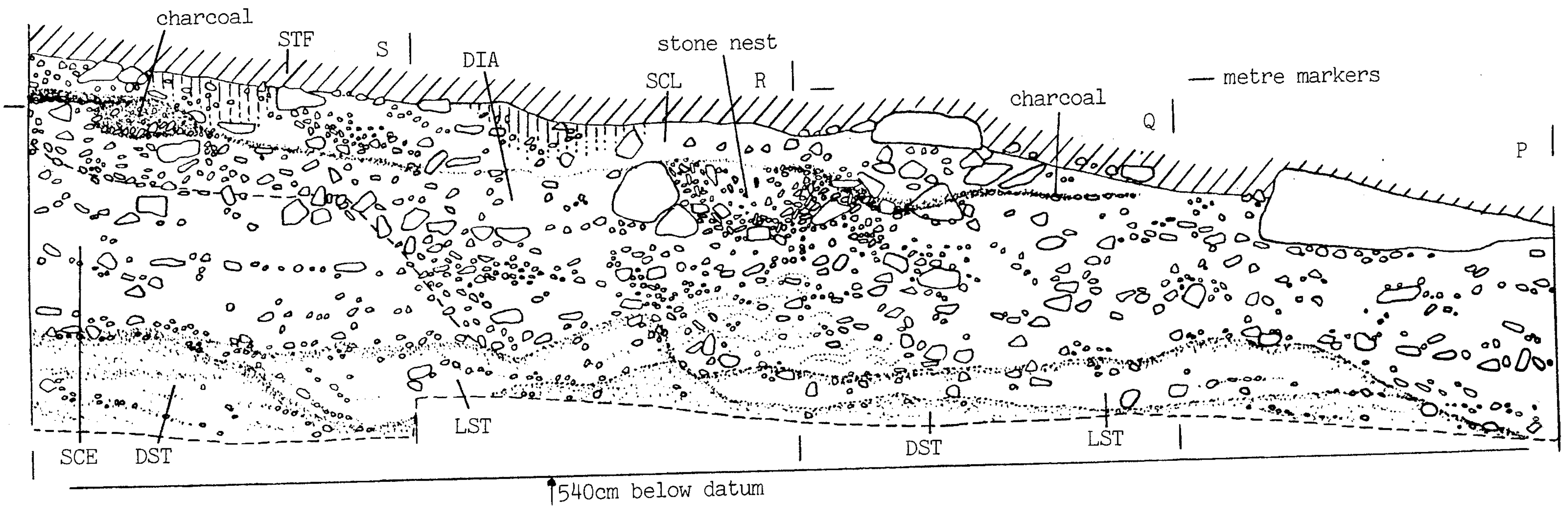
Dark Earth (DET). This is a dusky red (2.5YR 3/2) silt and clay, with the same 'greasy' feel as GCS. There are patches of dense matrix, pellet structure and rather 'fluffy' structure, organised in no obvious pattern. There are appreciable quantities of flint and quartzite 'grit', as well as a few small, very corroded and manganese-stained limestone clasts. The lower boundary with RCS is highly convoluted in a manner that suggests a geological agency. However, DET contains well defined, elongated pockets of purer silty material, very similar to the sediments overlying DET, which, improbable though it may seem in this context, look much more like small animal burrows ('mouse' sized) than simple geological features.

Stony Silt (SST), Dark Silt (DST) and Light Silt (LST). These three units will be described together since they are rather similar. The matrix is composed of silt and fine sand, with decreasing quantities of clay and manganese upwards. SST is a reddish to dark brown colour (5-7.5YR 4/4). It has a 'gritty' texture due to the common small flint and quartzite particles. There is a little limestone, which is heavily corroded and associated with dark manganese crusts. There is also some unsound slate, which gives a redder colour to the surrounding fines. DST is a dark brown colour (7.5YR 4/4). It has more limestone and fewer exotics, although the former is still heavily corroded. LST is a slightly lighter brown (7.5YR 4-5/4). Limestone is common, but it is moderately rounded and 'pock-marked', and shows frequent exfoliation crusts. Quartzite, flint and slate are rarer, but angular particles of quartzite up to c.3cm diameter still occur.

The structure of these three silty units (SST, DST and LST) is reasonably uniform. The deposits are made up of discontinuous lenticular beds, often filling minor channel-like features lined with very fine gravel spreads. Most lenses are broader than they are long and it is very difficult to follow a surface for more than a few decimetres downslope. The boundaries between the three units are often diffuse, but they seem to represent more widespread erosion surfaces. The general dip is inwards by c.5°. Organised laminated silt zones are small and very rare. There are a few clayey lenses that are usually roughly circular in plan (up to c.15cm in diameter), horizontal and very thin (<0.5mm). Isolated mud clasts (up to c.3mm diameter) are present in SST and DST. Some zones of LST have well developed pelletal structure, especially towards the north-east (e.g. squares N-02), where there is one lens, of a strong brown colour (7.5YR 5/8), which is almost totally composed of pellets. The pellets (0.25-2.0mm in diameter) are dense, with no obvious internal structure, but they are quite loosely packed in the deposits; they contain small amounts (c.0.5%) of amorphous alkali-soluble organic matter, as well as finely divided carbonates. Bone, often in a rather fragile ('flaky') condition, is common throughout the silty units; longer pieces are usually oriented parallel to the local slope.

Stony Cave Earth (SCE). This unit is only certainly present towards the outer (southern) end of the excavation (cf. fig.47) and towards the north-east end (cf. fig.46). This is a dark brown (7.5YR 4/4) clayey, calcareous silt with common limestone clasts of various sizes. The limestone is relatively sound but it is heavily edge-rounded. There are a few, reasonably continuous stone lines and quite common fine gravel (mostly limestone but with some quartzite and flint) pockets. The matrix

Fig.47 Pixie's Hole - Section 4/5 (looking west)



shows no obvious structue; it is 'normally' compacted. There is a large block of crystalline stalagmite, quite unlike any other observed formation in this part of the cave, in square N3 (cf. fig.46). Well preserved bone is common. Archaeological material occurs towards the top of the unit; this material and further sedimentological details will be discussed in section 21.6.6.

Diamicton (DIA). This unit is termed a 'diamicton' because its composition changes rapidly over short distances suggesting incomplete mixing of sources. The matrix, which is generally of a dark brown colour (7.5YR 4/4), may be clayey or silty, sometimes with contorted tongues of material with relatively sharp boundaries. Rare mud clasts, one c.2cm across (no internal structure), have been noted. Limestone clasts, with much edge rounding, are common. A few isolated limestone clasts show deep, dendritic etching on their surfaces. On the limestone as a whole there are a variety of alteration states. Even in zones with clast support, the matrix is relatively dense. The only exception to this rule is in a basin-shaped area lined with matrix-poor limestone clasts (at the R/Q boundary in fig.47). Bone in various states of preservation, but showing moderate mechanical damage, is present throughout, several long-bones being found upright. Archaeological material is also present at all levels within this relatively thick deposit. The lower boundary is sharp and easy to recognise where it is shared with beds of the silty units (DST or LST); it is strongly undulating. However, it is much more difficult to recognise where it is shared with SCE; it may appear 'knife-sharp' for 10cm and then effectively disappear for 30cm, and so on, either along or across the passage. This unit does not appear to be present on the far (eastern) side of the old excavation trench at the northerly end of the present excavations. Further sedimentological details will be discussed in section 21.6.6.

Silty Clay (SCL). This is a strong to yellowish brown (7.5-10YR 5/6) silty clay, with common, rather diffuse lenses of clayey silt. It is best developed in squares S to Q (fig.47), but it may also be present as a very thin deposit in N2 (fig.46). There is much colloidal carbonate throughout this deposit but, in some areas, there are pockets of moderately strong carbonate cementation. The fine bedding disappears completely within these cemented pockets. There are some small, heavily edge-rounded limestone clasts as well as quite frequent large blocks. Small bone particles are relatively common and there are very rare, small (<1cm) flint chips (artefacts) and specks of charcoal. The lower boundary with DIA is usually moderately sharp.

Stalagmitic Floor (STF). The highest deposit in this sequence is a massive stalagmitic floor which, where it has not been disturbed or eroded, may be over 40cm thick. The stalagmite is finely laminated but it has a tufaceous to 'chalky' consistency. On the west side of the passage, its upper surface has been eroded into a 'cascade' of solutional basins, each with a downslope rim of recrystallised calcite. The lower boundary of the stalagmite undulates and may make contact with DIA; in these areas, and where SCL is not present at all, larger flint artefacts and bones may be cemented into the base of the stalagmite.

A small sounding was also cut at point C (fig.43) in the western passage. Below the capping stalagmite, c.30cm of material very similar to DIA, but with more clay, was found; bones and two flint chips (artefacts) were recovered. Below this was a deposit similar to SFG or RSG, although there was again more clay. Its top was slightly indurated with manganese and, at this level, there were a few small lenses of finely laminated material, composed almost entirely of fine carbon particles. In the body of the gravelly deposit, there were rare, extremely rolled bones, heavily impregnated with iron compounds. Excavation was only continued for 40cm into this gravelly material.

21.6.5. Pixie's Hole - Interpretation of the Deposits

The faunal material from the present excavations is quite extensive and diverse but its study has not yet been completed (A.P. Currant, B.M.(N.H.)). For this reason, the few details that are currently available will be mentioned at the relevant points in the present section.

The first major grouping (comprising SSG, DGL, LGL, CGL, SFG, RSG, RSD and RCS) will be referred to, informally, as the 'Siliceous group'. The bedding structures of these sediments suggest that the dominant mode of emplacement was by water, although there are also signs of fast mass movement (debris flow) at various levels.

The lowest deposits, SSG, are well stratified but they are also poorly sorted and bedded at a very steep angle. The fact that beds are not disturbed near the sculpted limestone 'wall' shows that this dip cannot be due to postdepositional settling of some sort. The depositional system responsible for these beds is unclear. The most likely hypothesis presently available is that the unit is the result of pulses of sediment-laden water reaching an area of relatively still water. The high bedding angle is maintained in LGL. This thick unit is probably referable to sustained 'torrential' stream action, perhaps with phases of debris flow towards the top. However, it is difficult to be precise concerning this unit because, although it is obvious that some of the fines are derived from *in situ* slate decomposition, it is still impossible to judge how much matrix was included at the time of deposition. The whole *SSG* + *LGL* series gives the impression of very fast deposition and it may represent a very short period, or two short periods (*infra*).

The sediments of *DGL* are better organised, with weak grading and a much lower dip. These are referable to strong, but more regular stream flow. *DGL* certainly pre-dates *LGL*, but it is not clear from the present exposures whether it also pre-dates *SSG* or whether it should be inserted between *SSG* and *LGL*.

The next units, *CGL* and *SFG*, occur as a complex series of relatively narrow and deep channel deposits. The dip is again comparatively high, which is not surprising since the energy required to cut through the underlying gravels would be considerable. There is much more fine matrix than in older deposits but there is practically no sign of finer bedding structures, suggesting that erosion and deposition was by a combination of turbulent stream flow and debris flow. It is possible that *CGL* is appreciably older than *SFG*, but the cementation of *CGL* could have occurred by later mobilisation of an autochthonous component not shared by the sediments of *SFG*.

RSG seems to indicate more regular stream flow at a more normal gradient, with the partly laminated material of *RSD* representing the most mature stage. Flow probably stopped temporarily whilst *RSD* was partially cemented with carbonates. The final waning of coherent water flow in the cave is represented by *RCS*.

Overall, the Siliceous group shows a development which is quite similar to an alluvial fan sequence in the open. Such a development was obviously dependent upon the availability of masses of sediment in the Kate Brook Valley, as was indeed recognised by MacEnery. The geology of the Kate Brook catchment has been described above (section 21.6.1.). Although the lithology of the Siliceous group has not been studied in detail, there would appear to be an interesting shift midway through the sequence. The lower deposits (SSG, DGL and LGL) seem to contain two major components. The first is slate and slate decomposition products, probably referable to the local Kate Brook Slate itself. The second component comprises angular gravel, dominated by flint, and with an overall 'residual' character since only mechanically and chemically stable rocks are represented. This residual character might merely be due to destructive transport agencies, but it seems more likely that it is a primary feature of the source sediments. The author has examined the gravels, dating from the Eocene and Pleistocene, which cover the top of Great Haldon. At least superficially, there is a clear resemblance to the flinty material in Pixie's Hole, both in the form of the particles and in the strong hydration and weak iron staining of the flint. Similar cherts and quartzites are also present on Great Haldon. In addition, it is worth noting that the matrix of these exterior deposits is poor in sand, other than flint debris, and that, away from recent podzols, there is a relatively low iron concentration so that colours are generally weak.

In the upper part of the Siliceous group, the 'slate' and 'flint' components are still strongly represented, but a new component is added. A few softer rocks appear, the sand fraction (with appreciable feldspar) becomes more important, and there are large quantities of iron, as encrustations on siliceous sand grains, as sand and silt sized aggregates and as larger fragments of 'ironstone'. The author was struck by the resemblance between this material in Pixie's Hole and outcrops of the Teignmouth Breccia (New Red Sandstone) on the slopes of Great Haldon. Even the 'earthy' texture, due to the large quantities of (hyrd)oxides, is the same.

The significance of this shift is not immediately clear. Extremely local topographic factors might have been involved and the former disposition of gravel and sand bodies nearer Pixie's Hole is not known. Nevertheless, it is tempting to suggest that the Kate Brook catchment was once covered with earlier pleistocene, and perhaps tertiary, deposits and that, during a period of accelerated denudation, the stream cut down into the underlying New Red Sandstone. This hypothesis is not, at the moment, of any direct use in the interpretation of the Pixie's Hole sequence. However, the Kate Brook Valley contains a number of prominent terrace features and future study might reveal the same shift in these exterior deposits. It should be noted that the top of the Siliceous group in Pixie's Hole lies, in the present exposure, at c.56m O.D., whilst the present Kate Brook is graded to c.42m O.D. at the nick point (cf. p.818). Just up-valley (north-east) of Pixie's Hole there appears to be a relatively wide (up to c.20m) topographic 'flat' at approximately this level (the surface in fact slopes upwards to the north-west, across the width of the feature, by c.27.5° and is composed of limestone debris). There is also a lower surface, at c.47m O.D. The terrace gravels between Chudleigh Rocks and Hams Barton (cf. IGS Sheet 339, 1976) would seem more likely to be related to this lower surface; material of interest with respect to the Siliceous group at Pixie's Hole should lie at a slightly higher altitude than these mapped terrace remnants. It is clear that accurate mapping of the Kate Brook catchment will be necessary if this topic is to be pursued.

One further point is of interest here, namely the destination of all this water which was flowing into the Chudleigh Rocks system. This is an isolated body of limestone and the Kate Brook Valley more or less cuts it off from any possible phreatic link back towards the south-east. It seems possible that streams might have penetrated right through to the Teign Valley, in which case surface deposits (over shale bedrock) should be present to the west or north-west of the limestone outcrop.

At present, there is no obvious indication of the age of the Siliceous group deposits, although there is no reason why they should be particularly ancient; indeed, even an Early Devensian date would not seem impossible. The fauna recovered from the sounding at point C, and from CST (infra), may prove useful in this respect, although this sparse material appeared rather 'banal' to the present author (cf. also section 21.6.7.).

The carbonate-rich units CST and FSP may be easily identified as a 'cave breccia' capped by a stalagmitic floor. Both of these deposits show the effects of heavy alteration, probably mainly associated with the stream action evidenced in the surrounding, uppermost Siliceous group sediments (cf. p.835). However, it is not at all clear how CST and FSP fit into the sequence. The geometry of the upper beds of RSG would appear to indicate that at least part of this sandy gravel is younger than CST and FSP. This would suggest that the calcareous units are either a shelf, which was swamped by stream deposits, or a massive slab which fell into the stream. The only other deposit in the older series showing a little carbonate mobility is CGL (the cementation of RCS must post-date CST and FSP). If CST and FSP are referable to the same general period as CGL (and this is only conjecture), it must be assumed that a major break in stream deposition occurred shortly after the shift to 'New Red' rocks, that an important autochthonous deposit built up, and that this whole sequence was then almost totally removed by renewed stream action. Alternatively, CST and FSP may be approximately in the right stratigraphic position, with only partial undermining by a later stream (the uppermost RSG beds), although the extremely abrupt changes in lithofacies would then be very difficult to explain. A final possibility is that these calcareous units are the remains of an 'ancient' cave fill that is older than the whole of the Siliceous group. Clearly, further excavation, towards the north and east, will be required before this unusual 'inlier' is better understood. The majority of the identifiable faunal remains from CST appear to be referable to bear.

The ridge-like geometry of GCS almost certainly indicates that much of it entered the cave via the roof fissure immediately above. The component sediments are of a vaguely similar texture to the local slate and shale derived soil substrates (cf. Clayden 1971), but mineralogical study would be necessary to confirm the source. There is a high manganese component, not in the form of a cement, but present as small aggregates of 'wad'. This material would seem to represent stripping of 'ghosted' limestone (cf. section 12.1.). The present excavations have never been extended to the cave walls at the level of the upper part of the Siliceous group, for fear of obscuring the stratigraphy. However, it would not be at all surprising if the walls showed replacement by 'wad' at the contacts with the stream deposits. The carbon near the top of GCS, although amorphous, is almost certainly derived from charcoal. More carbon, probably deposited by sheet wash, is present at approximately the same stratigraphic level in the sounding at point C. There is absolutely no evidence for the involvement of man and this material is most probably referable to natural brush fires.

DET is composed of similar material to GCS, although there is an even higher 'wad' content. Some *in situ* ghosting can also be seen on the rare, corroded limestone clasts in this sediment. The massive and generally dense structure of this deposit, together with its convoluted lower boundary, suggest minor mass movement. The zones of 'fluffy' structure are probably due to the 'wad' component. The cave was probably extremely damp at this time.

The silty units, SST, DST and LST, show several lithologic gradients. Aggregates of 'wad' and *in situ* limestone ghosting become increasingly rarer upwards. DET, and to a lesser extent GCS, still contain fine gravel derived from exposures of the Siliceous group somewhere in the vicinity. Minor erosion of the Siliceous group appears to have occurred at the base of SST and this deposit contains much flint, slate and quartzite, as well as 'dirty' sand grains. The siliceous gravel component decreases upwards through the silty series, with a complementary increase in limestone clasts, although even the highest deposits at Pixie's Hole have minor amounts of fine flint and quartzite gravel, together with traces of some of the 'accessory' rocks noted above. The silty units all seem to have been dominantly emplaced by wash processes. Minor erosion forms, fine lag gravels, 'puddle' deposits and a few microdeltas were recognised, but the complex structure of this series deserves more detailed study since it should provide a useful general model. The energy levels seem to decrease slightly, or at least to become more regular, upwards through the sequence; more important gravel spreads (with a few mud clasts) are present nearer the base.

The silt component in these deposits is of some interest. Much of the silt in the lower levels may have been derived from the decomposition of the local limestone. However, the silt fraction in some zones of <u>LST</u> is very strong. The pelletal structure seen in these zones shows that the material was being derived, by wash processes, from a dominantly silty deposit lying somewhere nearer or outside the contemporary entrance. The low organic content (which is not high enough to indicate a particularly dense vegetation cover) also suggests derivation from the exterior. On the Haldon plateau, Clayden (1971) has recognised superficial deposits, consisting of gravelly (angular flint) loam, interpreted as due to "solifluction" (gelifluction ?), overlying "stoneless, silty drift", which may be as thick as 1m in places. More flinty gravels are present at depth. Brightly coloured podzols have been developed in this material, with the base of the upper gravelly sediments being picked out by the Bh horizons. However, there are signs of clay illuviation in the underlying "drift", which Clayden believes to be indicative of some soil development before truncation by the gravelly loam. Harrod et al. (1973) have examined the "silty drift"; they suggest that it is derived loessic material, mixed with small amounts of sand and fine gravel debris (mostly flint) from the local substratum. The present author has been unable to find such material in the immediate vicinity of Chudleigh Rocks, but this is hardly surprising given the generally high gradients and the widespread disturbance due to recent quarrying. It is tempting to refer at least the purer zones of <u>LST</u> to this "drift" material (the particle size distributions are compared in fig.48), although examination of the silt mineralogy (cf. Harrod et al. 1973) would be necessary to prove

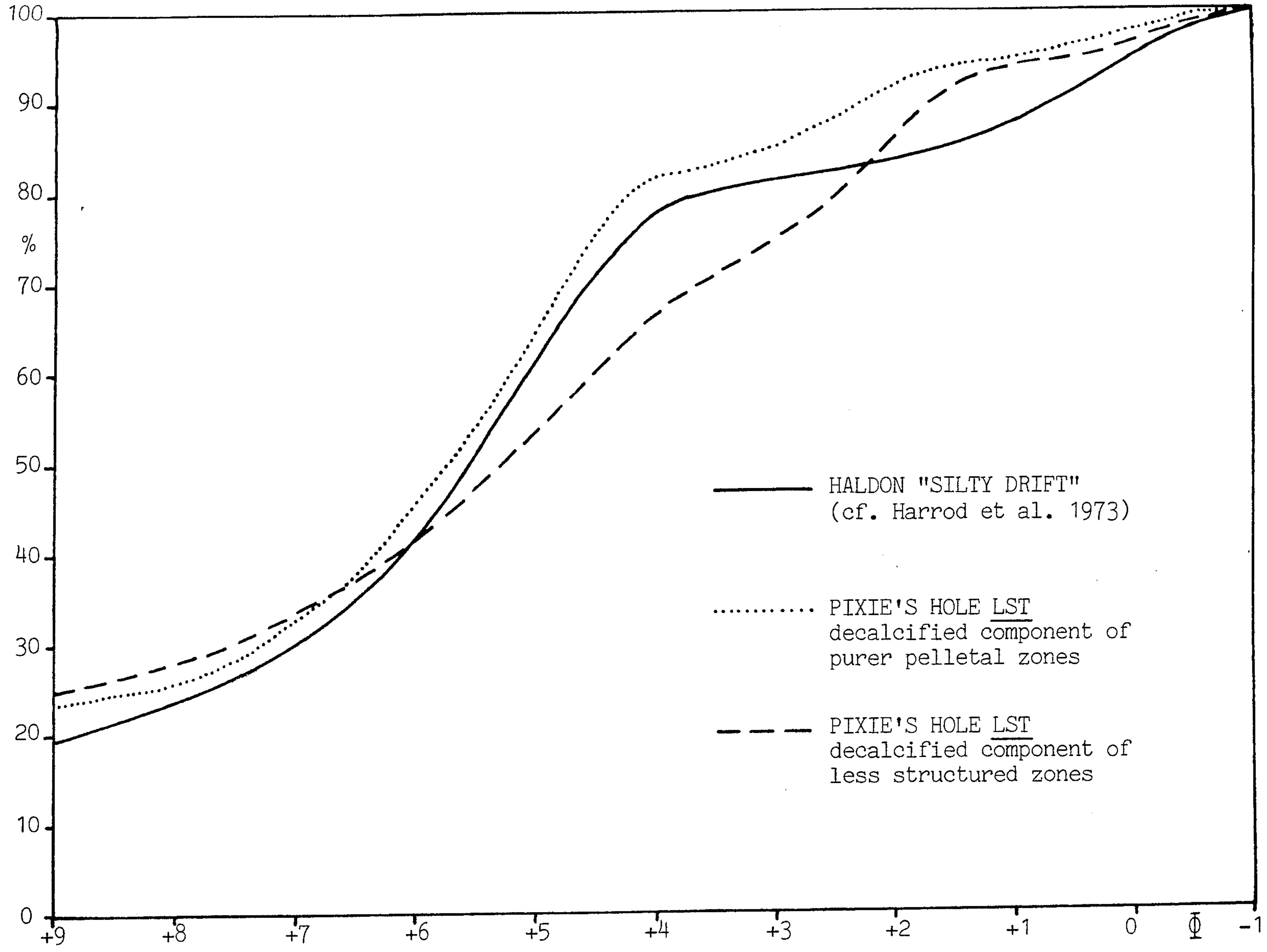


Fig.48 Pixie's Hole - Particle Size Distributions

the point. The Haldon "drift" is devoid of carbonates. The carbonates present in the pellets of LST may or may not be referable to an original loess; examination of thin sections might at least allow a decision as to whether or not the immediate source for the pellets was calcareous. Harrod et al. refer the Haldon "drift" to the same general aeolian cover as is evidenced in the Torbryan area (cf. section 21.2.6.), that is, to the Main Devensian. The presence of woolly rhinoceros (Coelodonta antiquitatis; identification confirmed by A.P. Currant) in LST is compatible with such a date (cf. Tornewton Cave, p.732).

The material of SCE is a typical 'cave earth', composed of much limestone debris, with fines added by irregular wash processes, dust input, etc. Note that the non-calcareous silt component is relatively minor, suggesting that any external "silty drift" had been largely stripped from the immediate landscape by this time. This deposit will be discussed further in the next section.

DIA is unequivocally a mass movement deposit, but insufficient details are available to be certain as to the exact processes involved. It is denser than SCE, from which it is obviously dominantly derived, although great 'swirls' of siltier material have been eroded especially from LST. The silty units also seem to have been slightly deformed in squares R-P (cf. fig.47). A few large limestone clasts in DIA show strong root etching but other clasts, immediately adjacent, lack such features and there are no rhizoliths in the matrix. Although it is by no means impossible that roots could have penetrated this far into the cave, the isolation of the etched clasts in an otherwise unaffected matrix suggests that they are derived. DIA has a generally massive structure, with a few mud clasts, but, within most of the excavated area, it does not appear to have been a markedly fluid phenomenon. However, material referable to this unit can be seen in more or less continuous sections (old trenches) deep into the system (cf. point C in the western passage); the further one proceeds into the cave, the more this deposit resembles a typical 'wet' debris flow. Near the north-western end of the present excavation, the slope of the older deposits (best seen in the silty units) seems to increase radically (to c.20° in places), so that DIA, as represented as far south as square R, appears to be a sort of 'headward erosion' feature working back from this point of high gradient and, indeed, increasing the gradient at the erosion face. There are no signs at any point that this mass movement episode involved geliflucton, even nearer the entrance; the base of the deposit is an erosion surface, not a diffuse deformation zone. However, a moderately well organised stone nest (cf. p.279), developed from the upper surface of DIA, appears to be referable to the sorting action of ground ice. The nest lies below the roof fissure, through which water would have been able to reach the sediments, and, although the nest must have formed at a point at least 20m inside the cave, the inward sloping surface would have encouraged cold external air to drain down along the passage. The emplacement of DIA and the formation of the stone nest could have been significantly separated in time or almost contemporary. The mass movement phenomena will be further discussed in the next section.

SCL is dominantly a fine sheetwash deposit. It occurred after the formation of the stone nest in the top of DIA and therefore it cannot be associated with the afterflow of DIA. The cave was evidently very damp at this time, and water dripping from the roof fissure produced localised pockets of carbonate cementation. In squares S and R, the stalagmitic floor STF seems to lie more or less conformably above SCL but, both south and north of this area, SCL has been eroded before the formation of the stalagmite.

All the calcareous deposits, from SST upwards, contain quite common faunal material, although both megafauna and microfauna have a rather 'residual' character (preponderance of harder parts) at most levels. It is probable that excavation nearer the cave entrance would produce a much richer microfaunal collection and it would be interesting to document any lateral changes in order to see how 'rapidly' the fauna is degraded by downslope movement. The present author's inexpert examination of the finds as a whole from these units would suggest that such animals as bear, hyaena, wolf, fox, cervids (certainly including reindeer and red deer), bovids (certainly including *Bos* sp.), woolly rhinoceros and horse are present. However, exact determinations, together with discussion of faunal assemblages referable to different levels, must await the completion of Currant's work on this material. Currant has indicated (pers.comm.) that these are generally 'cold' assemblages that would not be out of place in the second half of the Devensian.

21.6.6. Pixie's Hole - Archaeological Considerations

The archaeological material collected by the present author from Pixie's Hole is all referable to the L.U.P. A publication, in which full details of this material will be given, is in preparation. Suffice it to say here that the collection is quite small (although 'respectable' by British standards, unfortunately), comprising some 400 artefacts of flint and one igneous pebble drilled to form a pendant. Over 30 retouched flint tools are present, including abruptly retouched points (mostly curved backed types but with one single angle backed piece), scrapers (usually on flakes), burins (multiple and on a truncation) and a fine borer. From the moderate proportion of debitage, including three cores, it is clear that flint was knapped on site. No certainly worked bone or antler has yet been found.

The purpose of the present section is to examine the context of these finds in order to assess their general archaeological significance. This approach, not complex typological analysis or discussion of cultural affinities, should be the first step in the investigation of any site.

As was noted above, the author first recovered artefacts from the existing sections at the outermost (southern) end of the nineteenth century trenches. The present excavation was begun in square P4 and it then proceeded southwards along this metre band (towards V4). The massive deposit DIA was immediately encountered and it soon became apparent that this was some sort of mass movement deposit. Flints and tiny flecks of charcoal were recovered at all levels within this unit, with no sign whatsoever of any possibly meaningful concentrations. Nevertheless, a three-dimensional co-ordinate system, with measurement to the nearest centimetre, was adopted for all finds, both artefacts and bones; the sediments were dug in 5cm spits and quadrants of a metre, so that any sieve finds could be allocated approximate co-ordinates. No artefacts or charcoal were recovered from the underlying silty units at any time during the excavations.

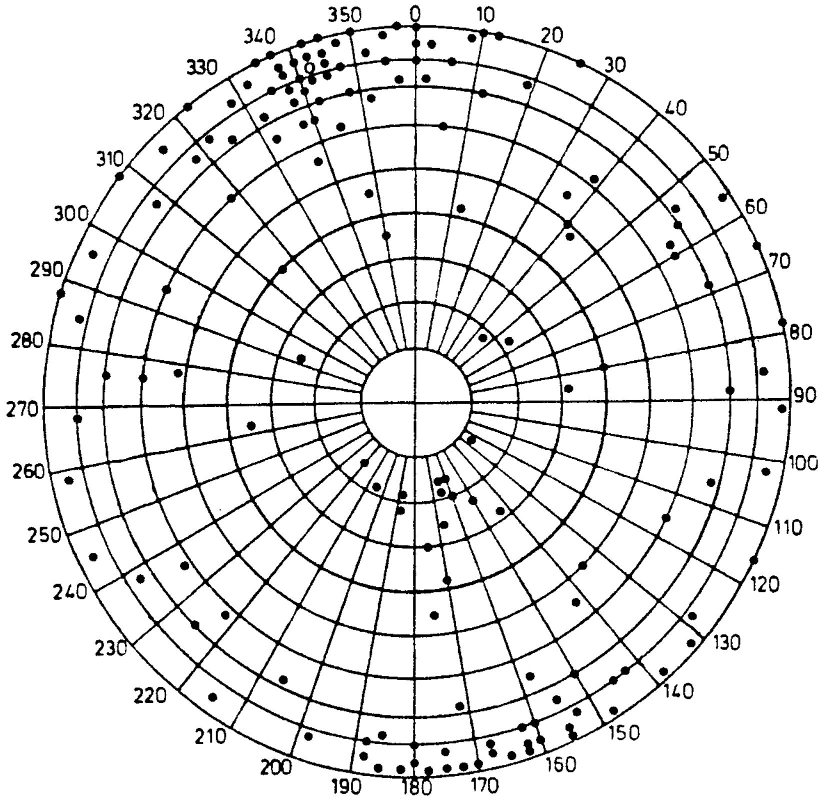
In squares Q4 and S4, quite dense spreads of degraded charcoal were found at the boundary between DIA and SCL. These were dug carefully until the lack of any traces of heating in the underlying sediments indicated that this was derived material. The fact that some of the flints already recovered from DIA showed signs of burning indicated that at least one hearth must once have been present at some point in the cave.

In addition to DIA, SCL was now producing a few small flint chips. However, in square S4, no artefacts were found deeper than c.30cm (and slightly deeper along the mid-line of the trench) into the deposits. Careful examination of the sediments allowed the undisturbed unit SCE to be identified below DIA. The 'typical' expressions of DIA and SCE were found to be sufficiently different to allow confident recognition of two separate sediment bodies, but only at very restricted points could a true boundary be easily seen. Paradoxically, when this boundary showed up, it was very sharp and smooth in plan, suggesting localised basal shearing during the movement of DIA. In fig.47, DIA appears to be significantly stonier than SCE, but this is merely a fortuitous property of the line of the section; 10cm out into the trench, the relative stoniness of the two deposits would be similar or reversed. Away from the supposed shear planes, only a 'boundary zone' (in places up to 10cm thick) could be confidently recognised actually during excavation, although very careful section cleaning and observation with a hand lens showed that a very faint, but sharp boundary was in fact present almost continuously. Some of SCE in square S4 had already been dug by this time but, judging from the co-ordinates of the finds, it was archaeologically sterile and all the artefacts recovered up to that point could be referred to DIA. The excavation season came to an end at this point.

Laboratory analysis of sediment samples from DIA and SCE indicated that, texturally and compositionally, they were very similar. The particle size distributions, with and without carbonates, showed the same, rather badly sorted curves. Only where DIA had clearly incorporated much material from the silty units (square R4 and northwards) was there any appreciable difference from SCE. Other parameters, such as limestone content (sizes, forms, alteration states) and exotic rock content, were variable but generally comparable in the two units. The situation was therefore as follows. DIA contained derived archaeological material, whilst SCE did not. However, DIA was composed of very similar material to SCE which, apart from making recognition of the boundary very difficult, suggested that the source of the artefacts was not far away, in sediments resembling SCE. The relative abundance of artefacts and the increasingly dense charcoal spreads in DIA also pointed to a close source. DIA was thinning southwards, being only c.15cm thick at the S/T junction, and its lower boundary was rising, now being only c.25cm below STF. Furthermore, preparations for the next season's excavation, involving stripping of the upper, most friable levels of STF, showed that this stalagmite sloped less markedly inwards in squares T4 and U4. Thus, the relevant stratigraphic levels were converging and, if any *in situ* archaeological material remained in the cave, it was clear that there was going to be very little 'room for manoeuvre' with respect to its recognition and differentiation from disturbed material.

Excavation was resumed during the next season. The eastern side of square S4 had not yet been excavated and the author undertook the removal of DIA in this area, and in the north-east quadrant of square T4. Stones, in the length range 4-8cm (the best represented class), were examined with respect to their orientation properties. The longest dimension and the intermediate

SQUARES S4 AND T4(part)



rings at 10° intervals of declination
o - general slope
153 clasts

Fig.49 Pixie's Hole - Clast Orientation

Debris Flow Context

SQUARES S4 AND T4(part)

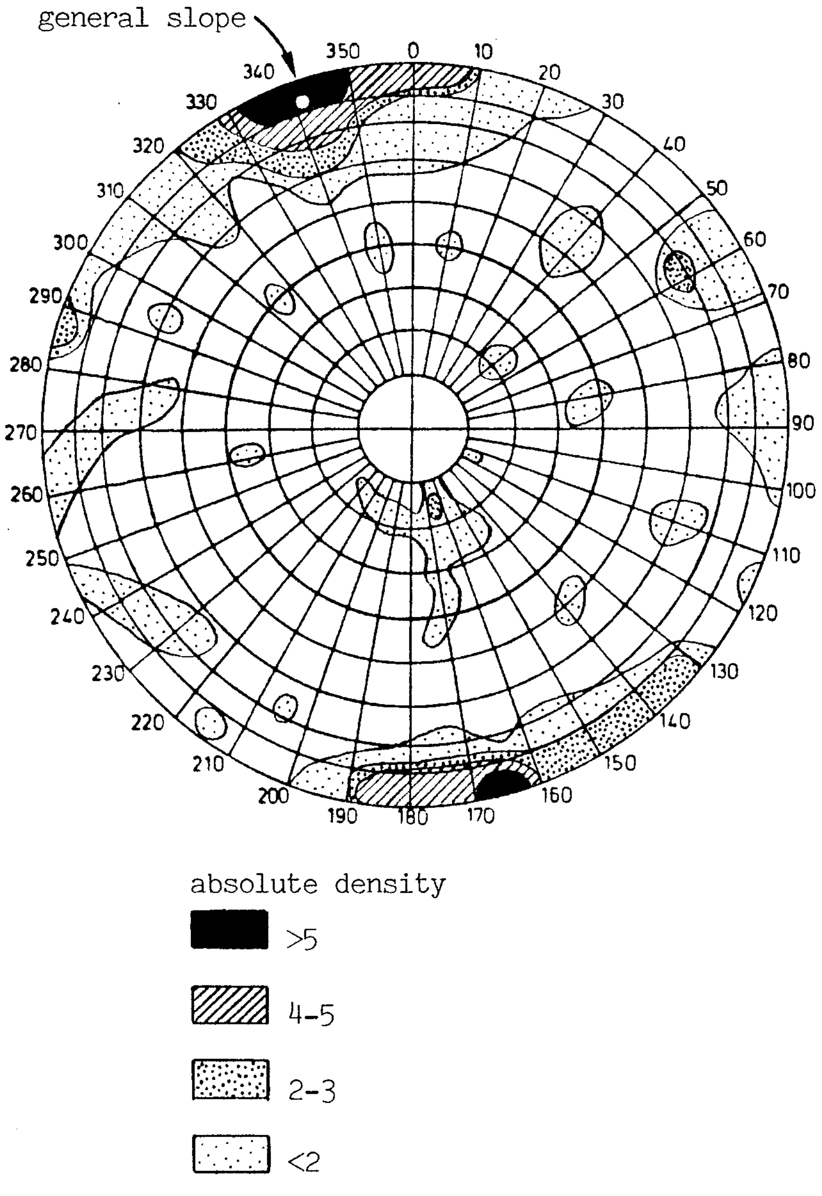


Fig.50 Pixie's Hole - Clast Orientation Contours Debris Flow Context

(data from fig.49)

dimension (width) were measured on stones *in situ*, after careful removal of as much of the matrix as possible; stones that had a length/width ratio of over 1.5 were chosen as being most likely to be representative of macrofabric. Fortunately, elongated limestone clasts are dominant in the upper units at Pixie's Hole. The longest axis of these stones was taken to indicate orientation (compass bearing). The concept of 'dip' (inclination) is rather difficult to define with irregular particles such as angular limestone clasts. In order to ensure at least operational uniformity, dip was taken to be in the vertical plane of the longest axis and the angle was measured with reference to the terminations of this axis. Orientation and dip could be measured to the nearest 2.5° with the equipment used, although this precision was perhaps a little beyond the 'practical tolerance' of the definition of parameters as measured on these irregular clasts; the reader may easily group the results reported below into 5° or even 10° classes, if desired. When orientation and dip had been measured, clasts were removed from the sediment and a point of balance, somewhere along the longest axis, was found using a knife-edge; if this point lay outside the central fifth of the clast's length, it was designated as having a 'heavy end', which was recorded as either the 'down-dip' or 'up-dip' end. This slow process was continued until approximately 150 stones had been measured. Due to the relative density and abundance of matrix, only 7 clasts (4.4% of the total), that would theoretically have fulfilled the size and elongation criteria, were abandoned due to unacceptable movement before the orientation parameters could be measured; it seems unlikely that any significant bias was introduced by this 'wastage'.

The results of this analysis are shown in figs.49-52.

SQUARES S4 AND T4(part)

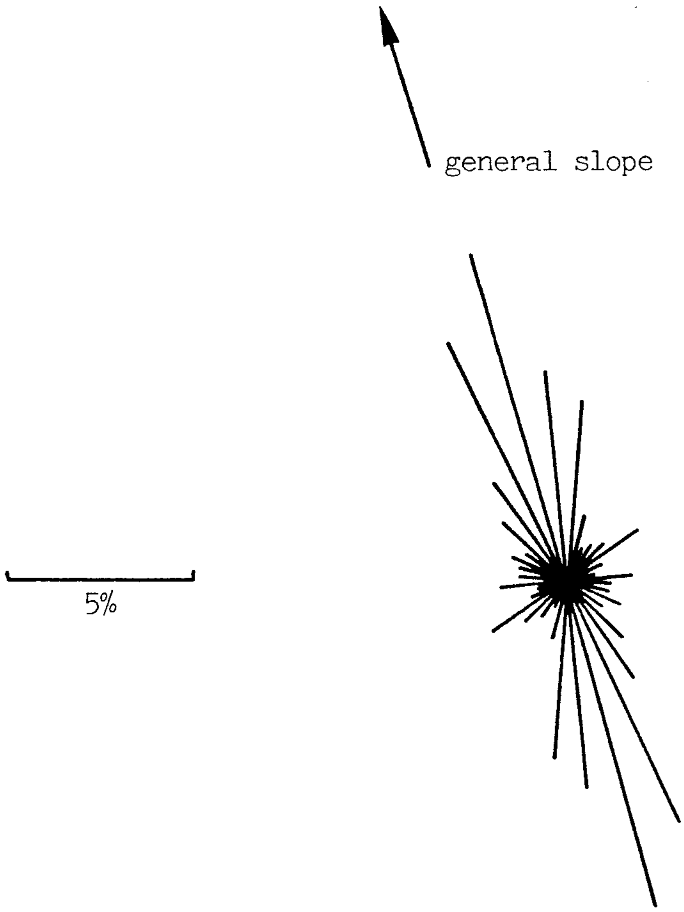


Fig.51 Pixie's Hole - Orientation Rose (Percentage)
Debris Flow Context
(data from fig.49)

SQUARES S4 AND T4(part)

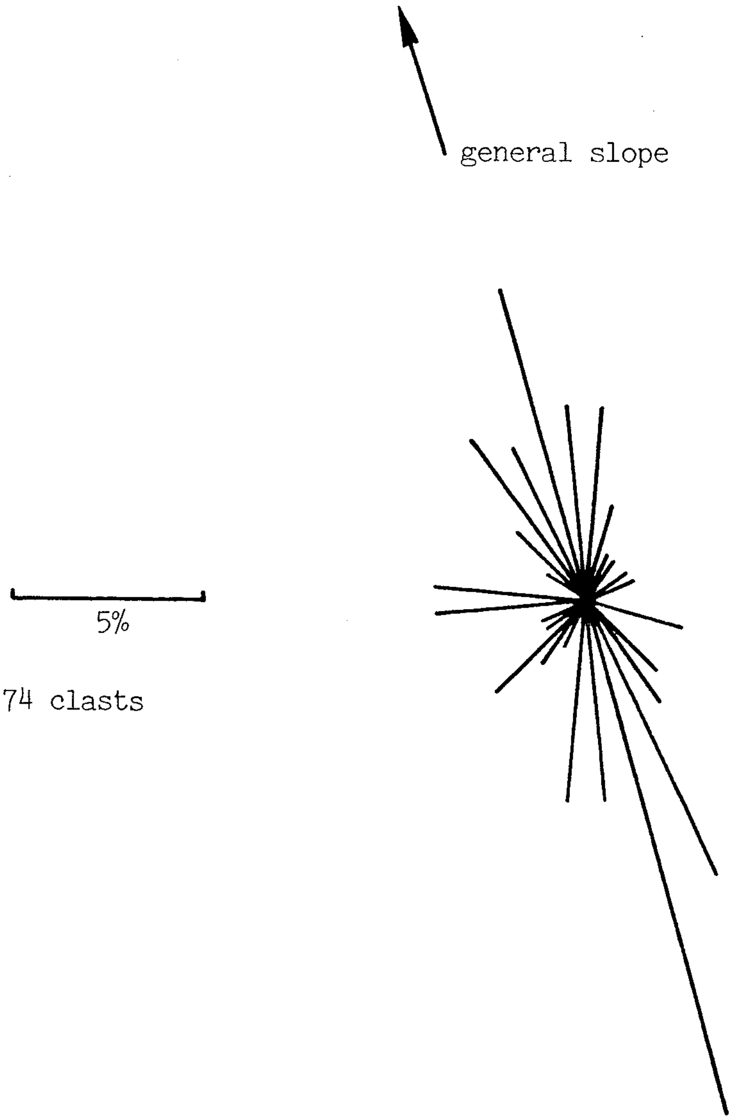


Fig.52 Pixie's Hole - Heavy End Orientation Rose
Debris Flow Context

A clear tendency for clasts to be oriented in the direction of the overall slope (parallel to the passage) can be seen, a tendency which was in fact strong enough to be quite obvious to the eye on site. Dips are distributed a few degrees on either side of the overall slope, but there are also some high angle dips. Heavy ends are equally distributed up and down slope, and there is no significant relationship with angle of dip. One other point, which was noted during this work as well as during the study of more extensive sections, is that, immediately upslope of larger limestone blocks, smaller clasts do not generally lose the normal orientation properties by 'banking up' behind the blocks. The author considers this macrofabric to be typical of moderately fluid and quite fast mass movement. As was noted above (p.852), the thickest part of DIA (squares R-P4) does not appear to represent such fluid movement, especially nearer its base where a kind of 'headward erosion' system seems to have operated; this impression (unquantified but quite obvious to the eye on site) is given by a much higher proportion of stones, towards the base, dipping sharply downslope than in squares S4 and T4.

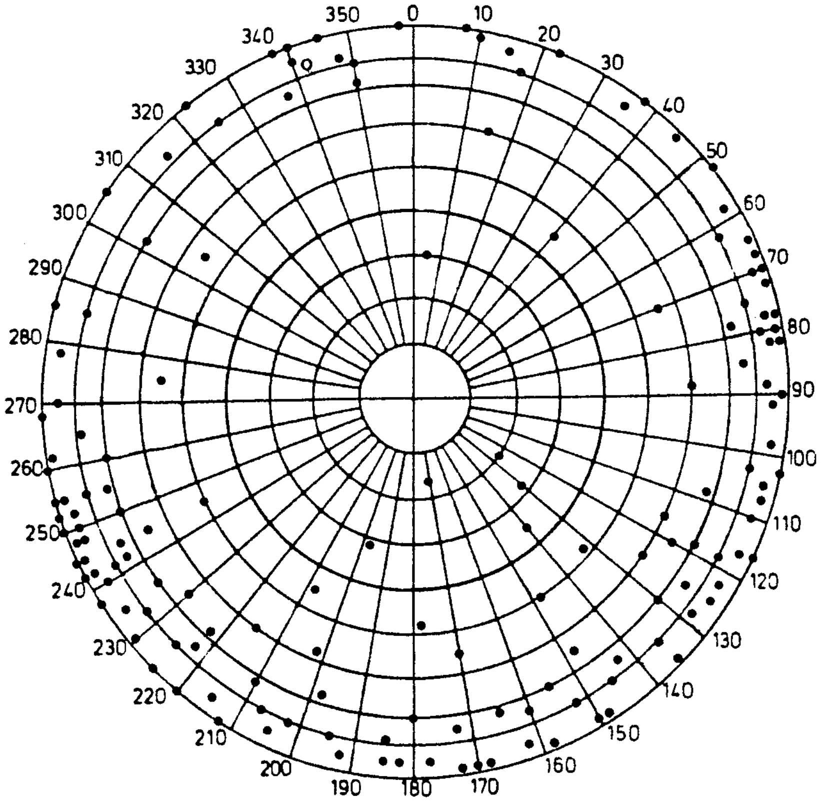
With the completion of this macrofabric analysis, it was considered that everything which could be done at the time to characterise DIA had been completed. However, the author still hopes to examine the thin section microfabric of this, and other deposits in the near future.

Excavation continued into squares T4 and U4. In addition to the co-ordinate system already in use, charcoal spreads, limestone clasts larger than c.5cm, and any other interesting details, were carefully plotted on plans. It became apparent very quickly that the predicted changes were occurring. First, most traces of DIA were lost as it angled upwards. Some thin spreads of slope-oriented stones were still apparent, but upper and lower matrix boundaries could not be accurately identified. Even the stratigraphic definition of SCL was lost. All that remained was a diffuse zone of clayier material, rich in colloidal carbonates, lying immediately below STF and penetrating down irregularly into the stonier material below. Artefacts were now being recovered only from a relatively thin band within the stony material, a band associated with ever increasing concentrations of charcoal. The stones just above, within and just below this band were clearly not dominantly slope-oriented, and the sediment as a whole appeared to be identical to SCE as defined in square S4.

Squares T5 and U5 were excavated towards the end of the season, but it was here that the orientation properties of stones were measured and we will jump ahead a little in order to discuss these findings. The same methods were used here as in the examination of DIA (supra). The sample was defined as a block of sediment (c.20cm thick) 'sandwiching' the archaeological band. It proved a little more difficult to leave stones undisturbed because the matrix was not so compact; 16 (9.5% of the total) clasts were abandoned.

It will be remembered that SCE as a whole was described in section 21.6.5. as a 'cave earth' with some irregular wash and dust input. The stratification of all the deposits at this point slopes down inwards by no more than c.7.5°, so that we would not expect SCE to show the effects of a strongly oriented energy system. The author must admit that he finds the results of the stone orientation analysis in squares T5 and U5 rather 'fluky' (cf. figs.53-56). On site, this material was clearly different from DIA but the highly organised macrofabric, apparent in the analytical results, was not nearly so obvious as a feature common

SQUARES U5 AND T5



rings at 10° intervals of declination
o - general slope
152 clasts

Fig.53 Pixie's Hole - Clast Orientation

Slope Wash Context

SQUARES U5 AND T5

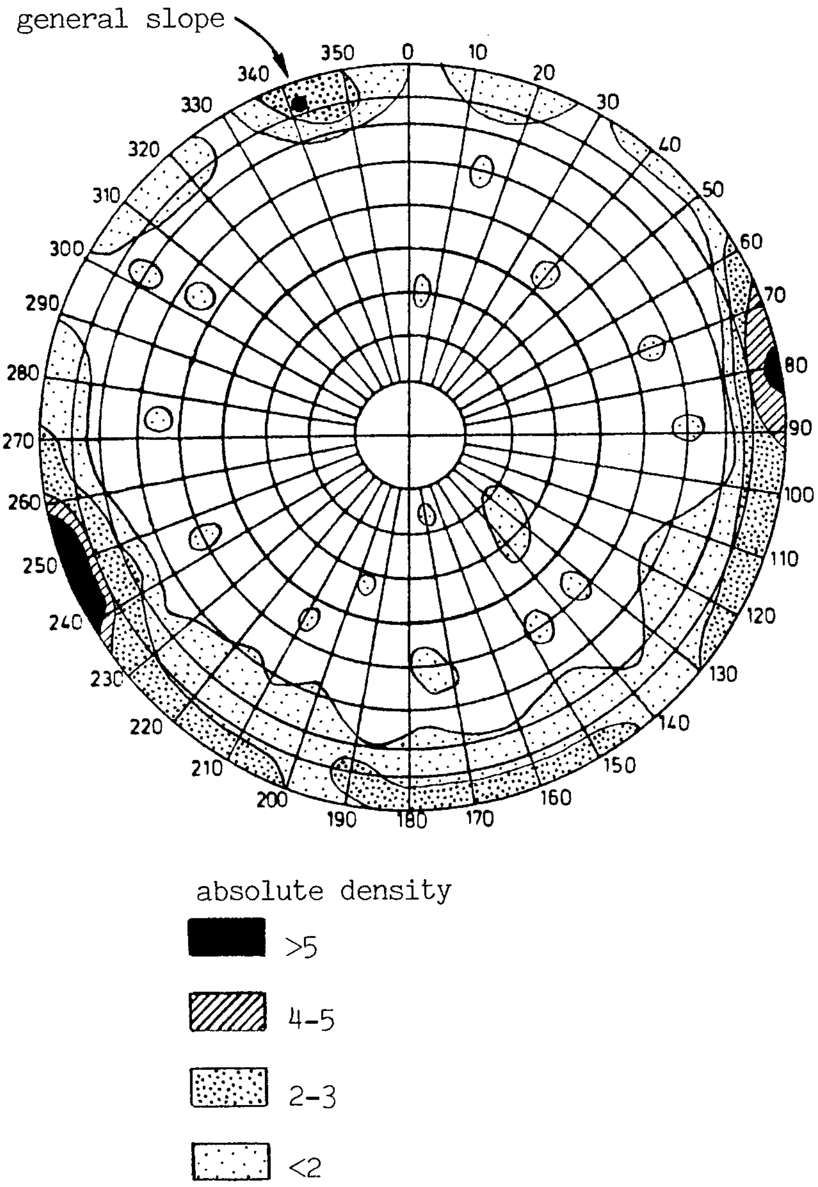


Fig.54 Pixie's Hole - Clast Orientation Contours

Slope Wash Context

(data from fig.53)

SQUARES U5 AND T5

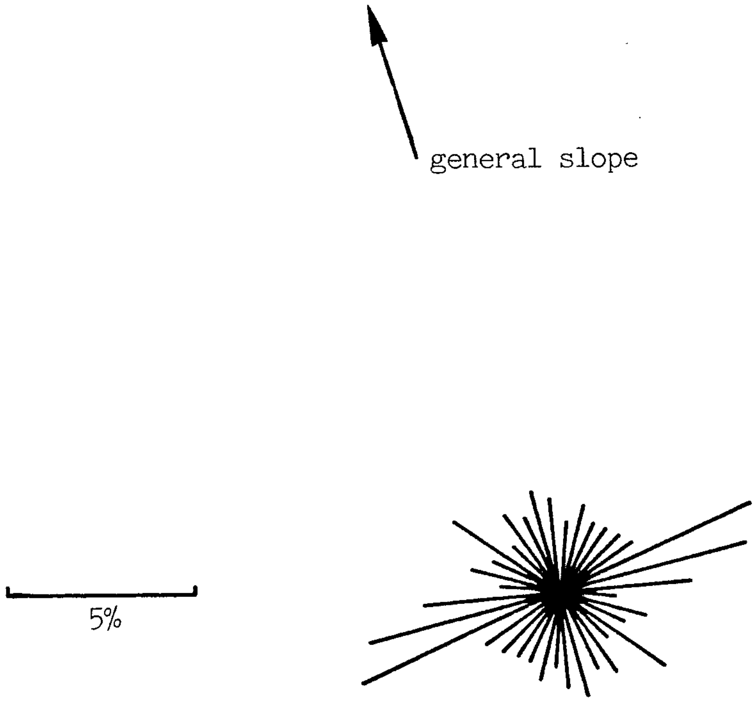


Fig.55 Pixie's Hole - Orientation Rose (Percentage)
Slope Wash Context

(data from fig.53)

SQUARES U5 AND T5

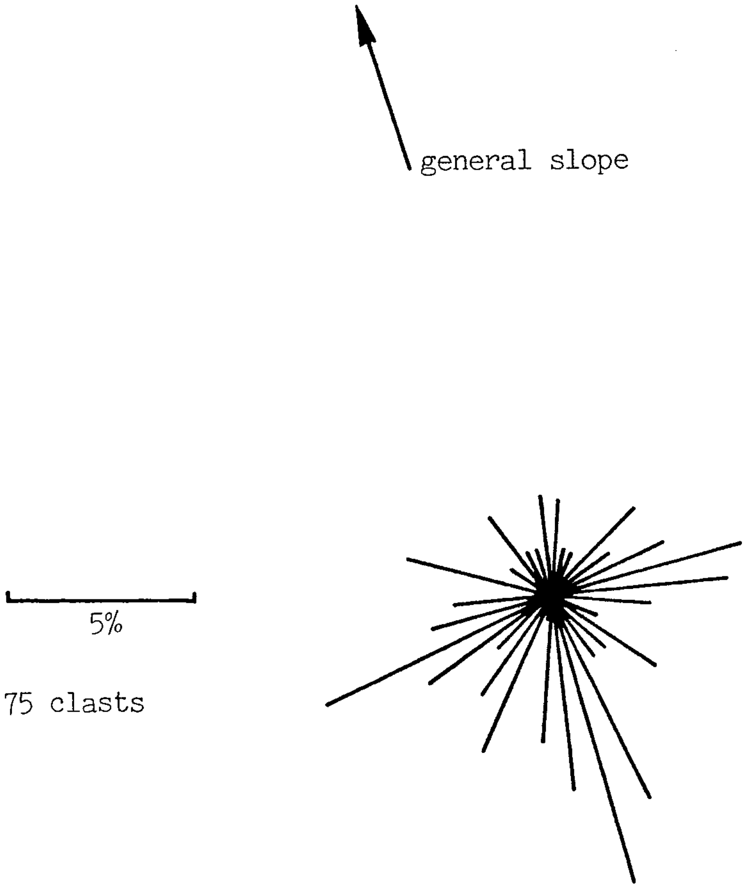


Fig.56 Pixie's Hole - Heavy End Orientation Rose

Slope Wash Context

to all zones of *SCE*. It seems doubtful whether repeated sampling would produce such clear-cut results. Nevertheless, the pre-arranged measurement procedure was rigidly adhered to during this work and the results merely reflect the fact that a particularly well structured zone was fortuitously selected for sampling. Normally one would demonstrate statistically (cf. Mardia 1972) that the documented samples of *DIA* and *SCE* differ in orientation properties, but there seems little point in this extreme case. *SCE* has a strong cross-slope orientation, with both low dips and heavy ends oriented generally counter to the slope. Furthermore, unlike *DIA*, considerable 'banking-up' of smaller clast behind larger blocks was noted. This is a 'classic' low energy slope macrofabric, showing the effects of gentle creep.

Moving, therefore, into squares T4 (part), U4, V4 (partially excavated), T5 and U5, it was clear that the context of the archaeological material had changed from a high energy environment, where any spatial information would be dominantly a function of the transport process, to a low energy environment in which some anthropogenic 'structure' might have survived. However, the whole 'living floor' problem (cf. Chapter 20) had still to be tackled and it could not be taken for granted that low energy sediments could be equated with undisturbed archaeological material. Furthermore, it could not be assumed that *DIA* would never again reappear to bite into *SCE* at some point upslope. Although this was not found to be the case in any of the squares so far examined, the undoubted passage of *DIA* across this area, probably starting from a point at least as far out as the cave entrance, must be constantly borne in mind if excavation is renewed at Pixie's Hole.

It is worthwhile imagining what the accreting surface of *SCE* would have looked like at the time of the archaeological 'presence'. This sort of sediment is a very common type in British caves; indeed, upslope of the excavated area in Pixie's Hole, the modern surface, composed mostly of old tip, provides a reasonable analogy. This is an uneven, stony surface and, although it is hard and the matrix often fills the interstices, there are many small crevices between stones down which small objects could penetrate for up to a few centimetres. Such movement would be quite quickly inhibited, however, and one would not expect the extreme dislocation that can occur in open screes. The density and coherence of the deposit would prevent the development of the sort of vertical distribution associated with trampling in finer and looser sediments. Incidentally, the limestone clasts protruding from the old tip floor, in a path down the centre of the passage, show a very strong 'lustre' caused by visitors over the last century (cf. p.679).

Fig.57 is a plan of apparently archaeologically significant material in the squares undisturbed by DIA. Note that the limit of DIA in square T4 has been placed conservatively, so that material to the south and west of the thick dashed line is definitely not part of the mass movement deposit. Much comminuted charcoal was present in these squares as a band (the density being indicated by stippling) and this material was used to help indicate which of the larger stones, in this generally stony deposit, might be of interest. Stones were marked on the plan only if they were bedded well into or just below the charcoal band, in such a way as to suggest that they would have been apparent at the surface at this time. Areas without charcoal are diagonally hatched; there is no way of knowing which stones might have been part of the surface in these areas. The result of this exercise gives only a very rough (minimal) idea of the ancient surface and it is most likely that

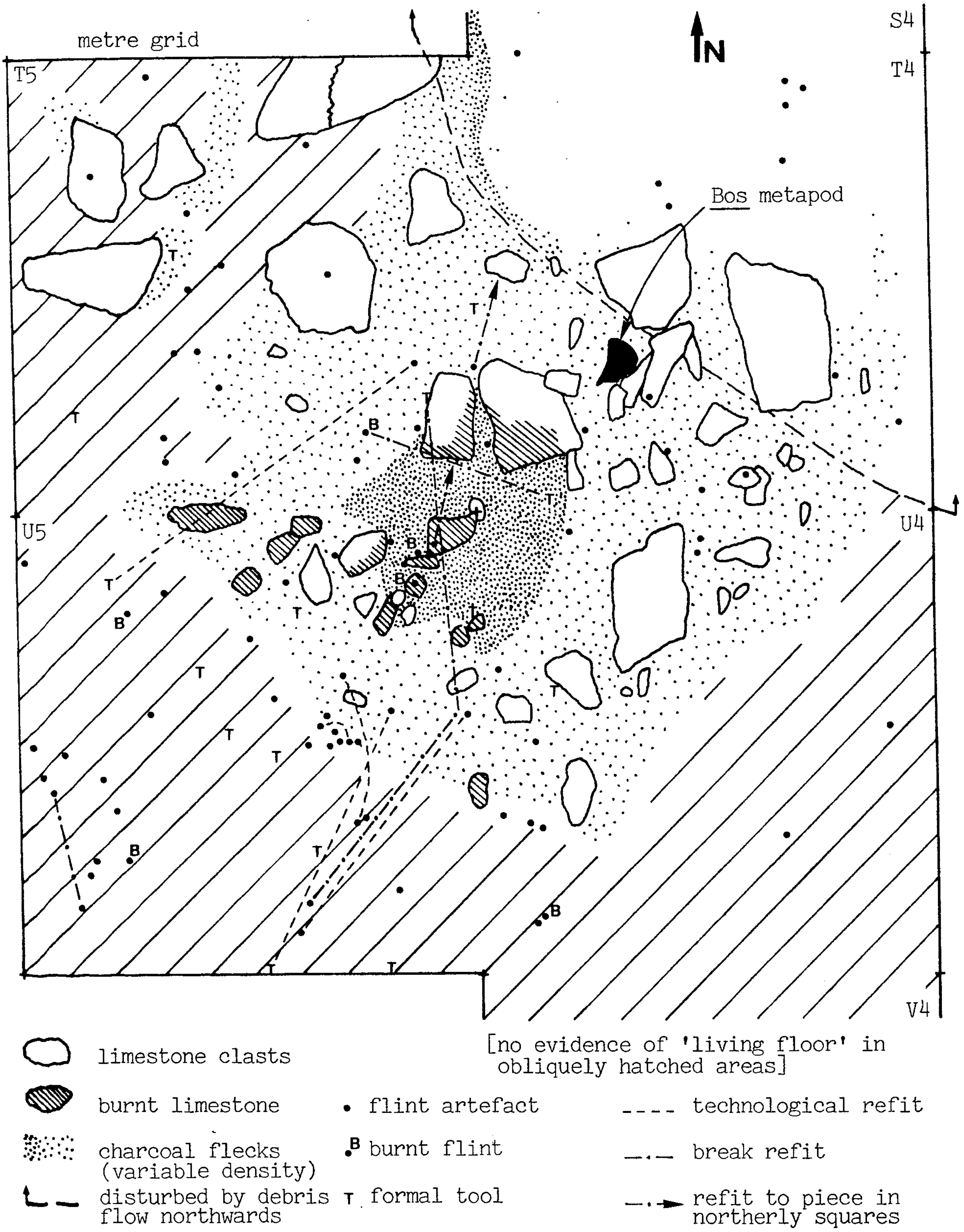


Fig.57 Pixie's Hole - Plan of Hearth Area

it was much stonier. What is clear, however, is that this surface would not have been significantly different from any other surface in SCE during the accretion of this deposit. There is absolutely no evidence for limestone constructed features or any other major anthropogenic modification of the surface. No 'lustred' stones were found, either at the reconstructed surface or below/above it.

However, there is good reason to believe that the charcoal-rich zone seen in plan in fig.57 and in section in fig.59 is an in situ hearth. The lateral boundaries of this zone were relatively sharp, as were its upper and lower surfaces. The clayey sediment below the charcoal was reddened and indurated ('baked') in very small patches, although only to a depth of 1-3mm, making it unlikely that any useful data could be recovered by techniques such as TL or palaeomagnetic analysis. Many of the small limestone clasts actually within the charcoal spread were reddened and crazed, and were sometimes split into several conjoinable pieces. The two larger blocks on the north side of the feature were also burnt and seemed to be strictly in situ. These blocks formed a 'lip' to the downslope side of the feature (cf. fig.59) but there is still no proof that they were deliberately placed in this position; they were not underlain by charcoal and it seems probable that the siting of the hearth against them was merely a matter of convenience. Some medium sized burnt stones lay on the south-west side of the feature, a position unlikely to have been due to geological disturbance. Similarly, a zone of more diffuse charcoal surrounded the feature, even on the upslope side. It seems probable that the hearth was slightly disturbed (raked, scattered, trampled, scavenged, etc.) for some reason, by either man or animal. The conclusion concerning this feature is therefore that it was a small (c.50cm diameter), casual hearth,

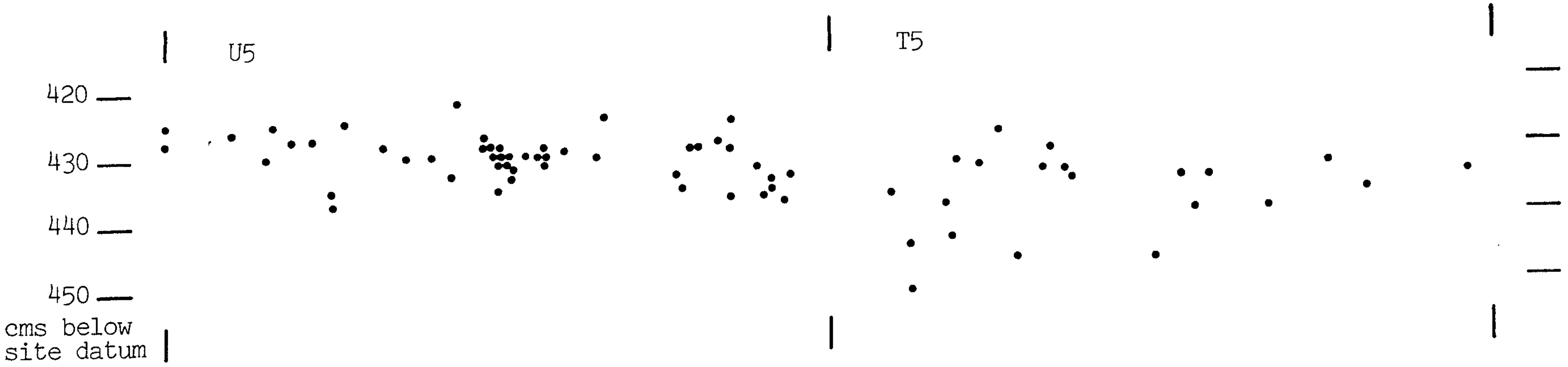


Fig.58 Pixie's Hole - Vertical Plot of Artefacts in Squares U5 and T5

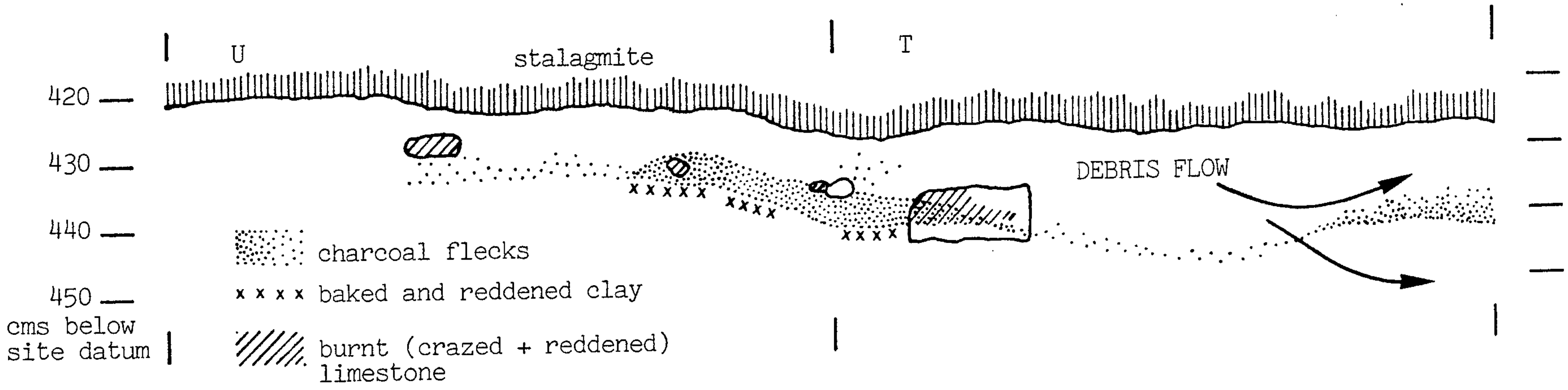


Fig.59 Pixie's Hole - Vertical Section 4/5 Across Hearth Area

not involving stone constructed or 'excavated' attributes.

As was noted above, the flint artefacts recovered from these squares occurred in a relatively thin band. It was clear, as expected, that the ancient surface was uneven, with a secondary amplitude of at least 10cm superimposed upon the general northerly slope. Fig.58 shows the flints found in squares U5 and T5 (not including sieve finds with only approximate co-ordinates), projected onto a vertical plane oriented parallel to the excavation grid (i.e. closely approximating to the line of maximum slope). The apparent thickness of the band is mostly c.15cm, with a maximum of c.25cm. However, this method of plotting slightly exaggerates the true vertical spread because of the irregularity of the surface, even within a horizontal strip only 1m wide. The author is at present working on a 'regression' model which will help to quantify the vertical spread more accurately. On site, it appeared that the flints were restricted to an undulating band no more than 15cm thick and often thinner. Furthermore, there was a clear tendency for the lowest artefacts in any area to be small chips, larger artefacts being concentrated nearer the ancient surface. The relationship between size, shape and position will be discussed further at a later date, but it is already clear that the observed distribution is not markedly at odds with the distribution to be expected if the artefacts were abandoned on the type of surface envisaged. In addition to penetration of smaller objects down crevices between stones, only slight vertical and lateral movement due to wash and creep seem likely.

With the help of R.N.E. Barton, a small number of refits between flints from the whole collection have been achieved. These can be divided into 'technological' refits, involving pieces successively removed from a core, and break refits, the breaks possibly occurring at any time from knapping onwards. Only five technological refits have so far been recognised, all pairs occurring immediately to the south and west of the hearth, with a maximum horizontal displacement between the component elements of 83cm (cf. fig.57). Three break refits also occur with both their component elements in the hearth area. Two more break refits link the hearth area with the artefacts in DIA, further to the north, and a few more break refits have both their component elements in DIA. This information, although sparse, is in full agreement with all other data in that it points to the lack of major disturbance in the hearth area.

The condition of the flint artefacts, most of which are quite small and relatively fragile, is of some interest. Preliminary examination has shown that 50% or more of the artefacts in all areas (including DIA) carry substantial break facets. This is not at all surprising in the mass movement deposit, but neither is it very surprising in the low energy slope deposit. Apart from knapping accidents, artefacts could have broken as they fell to the stony surface or as people walked over this surface. Similarly, rockfall or the shifting of larger blocks due to creep could have broken artefacts at a later date. However, judging from samples of c.100 pieces each from SCE and DIA, there is an interesting difference in the amount of major edge damage (crushing). In SCE less than 10% of pieces show such damage, whilst the proportion in DIA rises to over 30%. It is tempting to refer this difference to the change of context. A project involving precise qualification of damage types and quantification of their occurrence will shortly provide firmer information on this topic.

The nett impression given by the archaeological material from SCE in the southerly squares is that it represents the relatively undisturbed expression of a 'living floor'. The author considers that the more or less philosophical problem as to whether this material is referable to one or several closely spaced occupations is irrelevant at this point. Such considerations would only become important if detailed discussion of activity areas and behaviour patterns were to be undertaken, discussion which would be totally invalid with the present tiny sample of both artefacts and 'undisturbed space'. It should be noted that a few artefacts and charcoal flecks were recovered from the north-east end of the excavation (e.g. squares N2 and O2). This area was only dug to regularise the old sections, but the artefact-bearing deposit appears to be referable to SCE and it is possible that there is also an undisturbed surface. Merely as conjecture, one wonders what a block of crystalline stalagmite (cf. fig.46), unlike any other material in the sediments or adhering to the walls, is doing in this deposit.

Concerning the homogeneity of the flint artefacts from Pixie's Hole, it should be noted that there is no typological or technological difference between the material in DIA and that in SCE. The two linking break refits have been noted above. Two main flint types occur in both contexts: a blue/black flint that is superficially patinated to a mottled light grey, and a dark flint with a strong orange tinge that is superficially patinated to a mottled cream colour. Both these types have degraded (worn and thin) but true cortex. There are also a few chertier pieces (in both contexts), which could be distinct, but which might only represent variation within nodules of the otherwise finer textured material. This archaeological flint is radically different from the masses of flint which occur naturally in the deposits, flint which is present as angular and rolled clasts, lacking all traces of cortex, and which is internally fractured, stained and heavily hydrated (*infra*). There is therefore no pressing reason to suggest that the Pixie's Hole artefacts represent more than one assemblage, although whether this has behavioural significance remains to be seen.

If the stone artefacts and hearth feature of *SCE* can be plausibly referred to an archaeological occupation, the same is not true of the bone material in this deposit. It has already been noted that no unequivocally worked bone has yet been recovered. There is certainly plenty of herbivore bone which might belong to the archaeological assemblage, in that it does not show signs of gnawing, but it is impossible to be certain. There are also carnivores at this level, such as wolf, which are equally likely to have been responsible for the bone accumulation. The piece most likely to represent archaeological debris so far recovered is a large fragment of a *Bos* metapod (identification confirmed by A.P. Currant), found 15cm north-east of the hearth, but still clearly within *SCE*. It lay upon the charcoal spread in that area and actually contained charcoal-rich sediment washed into the medullar cavity, but it itself shows no signs of burning. One end is formed by a spiral fracture right through the shaft of the bone, there being a single point of percussion and no other obvious damage or signs of gnawing. There is a weak 'scratch' running obliquely across a tendon attachment point, which might be a butchery cut but which might equally have arisen naturally in this stony deposit. It is clear that further excavation will be necessary in order to search for bone material of a less equivocal nature.

This brings us to the dating possibilities for the Pixie's Hole occupation. The author intends to have the *Bos* metapod, noted above, radiocarbon dated, but it must be reiterated that this specimen was not in certain archaeological association. Although several burnt flint artefacts have been recovered, none of them is large enough for TL dating (which requires an absolute minimum thickness of 6mm). The charcoal from the site is all present in a highly comminuted form (as is normal in British cave sites); this conclusion was checked by flotation of samples from the densest concentrations in the hearth. The author has no confidence in radiocarbon dates on bulked samples of such fine material from limestone (i.e. carbonate-rich) caves. Some small (1-5mm) fragments of burnt bone were present in the hearth but, again, the problems of contamination would be insuperable using standard radiocarbon techniques. In any case, SCE is full of bone debris which might have been fortuitously burnt in the hearth; an accelerator date might be accurate but there would still be no certain archaeological association. Thus, the possibilities for secure dating of the occupation seem dependent upon continued excavation in the hope of finding unequivocally worked bone.

Remaining firmly at the level of generalities, it will be interesting to speculate upon what the ancient people were doing at Pixie's Hole. We know that flint was knapped at the site. Debitage, retouched tools and cores of both major flint types have been recovered. Around the hearth area much debitage was present, but not nearly enough to indicate that this was a 'preferred' knapping area. It has already been stated that the flint sources used did not include the local gravel (at least with respect to the present collection - the gravel is not, theoretically, totally unworkable). As was noted in section 21.6.5., this flint gravel is referable to tertiary and pleistocene strata in the area. IGS Sheet 339 shows Eocene or Pleistocene directly above Lower

Cretaceous Greensand in the Haldon area. However, Simpson (1969) has noted that the Haldon gravels also contain unworn flint nodules in a clayey matrix, although he does not reference either the geographical or stratigraphic position of this material. The present author examined exposures near the head of the Kate Brook in 1978. It was very difficult to see the geological strata in this area because of thick slope deposits but, at one point (approximately SX 906825), a Forestry Commission access road cutting had exposed several metres of angular flint gravel in a matrix of flint-rich sandy silt. Below this deposit, after a reasonably abrupt but convoluted boundary, lay a bed (>50cm thick) of irregular flint nodules (up to c.15cm across), all with thin but continuous cortex, set in a light coloured (various greys and pinks), massive, silty clay matrix with very little fine flint debris. This deposit appeared to represent an almost totally decalcified chalk residue, of the 'clay-with-flints' type. It may have been more or less in situ since there was no obvious oriented fabric. Exposure of the underlying strata, which should theoretically be Upper Greensand, would be necessary to check this conclusion. In any case, the flint was in excellent condition and, at least to the naked eye, was identical to the blue/black type used for artefacts at Pixie's Hole. No sign of the more orange type could be found. Flint is notoriously variable and a slight facies change, with an increase in iron compounds, is all that would be necessary to produce this second type.

The presence of good quality flint in the Kate Brook catchment (at most, only 6km from Chudleigh Rocks) may well be one of the reasons for the Pixie's Hole occupation. Although flint is not uncommon as a component in gravels throughout South Devon, to the present author's knowledge, this is the most south-westerly exposure of more or less primary context flint in Britain. The next primary source (the renowned Beer Flint) lies almost 30km to the east of Great Haldon. The Haldon nodule flint may have provided an attraction for people from much of south-west Devon, and at various different times. It would seem worthwhile searching for 'industrial' sites in the Kate Brook Valley even nearer this source. It is interesting that neolithic sites on Haldon usually contain a few artefacts made from the local, poor quality gravel but a much larger number made from Beer Flint (cf. Willock 1938); the Haldon nodule flint does not seem to have been exposed or known at that time.

Returning to Pixie's Hole, it may be said that the occupants were perhaps in the area in order to make use of the nearby flint source. However, the cave is in a reasonably 'strategic' position for any number of activities. The occupation does not appear to have been on a very large scale or to have lasted for very long, although the finds from the small area so far excavated may be misleading in this respect. What is of interest here, however, is the siting of a hearth at this particular point within the cave.

Given that no more than a few metres of passage have been destroyed at the entrance, the hearth area would probably still have been within the light zone, at least at the height of a man's head whilst sitting, although the floor level itself may have been in shadow, as it is now at all seasons. The tendency for slope deposits in descending entrance passages to have concave-down profiles is strong and often produces this 'floor shadow' effect. The hearth would have been perhaps 16m inside the cave, and centrally placed in the passage, below the very narrow roof fissure. There is absolutely no sign in the deposits that this roof fissure was even locally unchoked at the time of the occupation (cf. the contrasting situation in GCS at a lower stratigraphic level). Nor is there any sign in STF that the fissure was particularly active at that time. No air passes into or out of this fissure at the present time and the response to rainfall is minor and very slow, with up to a day's delay before the drip rate increases.

The whole slope up to the entrance cannot be reconstructed without further excavation, but it is clear that the surface continued to rise for at least a few metres laterally, south of the hearth, and it seems most probable that this tendency continued up towards the cave mouth. The exact angle of slope is not important, although it was probably over c.7.5° at most points; anything over c.2° would cause cold air to drain into the cave whenever the exterior air temperature dropped markedly below that of the interior (cf. the stone nest in the top of DIA). This feature makes Pixie's Hole, like all down-sloping passages, a rather uncomfortable place, even today. One assumes that one of the reasons for the hearth was to compensate for this discomfort. But, once a fire had been lit, would the smoke travel inwards or outwards and how fast would it clear?

Let us first assume that the passage of interest did not, at the time of the occupation, communicate with the rest of the system. During the second season of the present excavations, the author completely blocked off the eastern passage at its narrowest point, just before it reaches the Pope's Chamber (cf. fig.43), mainly to prevent visitors from falling into the deep trenches (the eastern entrance was temporarily gated), but also to observe the effect on the airflow. Unfortunately, no meteorological measuring equipment was available at the time, but the result was sufficiently marked to merit comment. The air in the passage went 'dead', save for a weak cold draught at night just above the floor and a return flow along the roof, within the first few centimetres of the fissure (cf. section 2.2.). Note that the roof slopes upwards to the entrance at only c.10° at the most and that there is c.2m of headroom above the deposits, although the cross-section of the passage is roughly triangular or domed and this headroom is available only in the centre of the passage. The introduction of burning material (care being taken not to pollute the archaeological site) merely served to increase the draught slightly, even when a substantial 'fire' was created. The increase in air movement was not enough to clear the smoke and the passage air became totally unbreathable in a matter of a minute or two. Indeed, if anything, the smoke became trapped between the moving air near the floor and roof, with the formation of smoke-filled cells rotating slowly in the vertical plane. The situation did not improve with time and the smoke took half an hour to clear after the removal of the 'fire'. This casual experiment was carried out in late summer; in winter, the result would probably be just as unpleasant. There is no reason to believe that the different climatic conditions of the later Devensian would have helped; the strong draught necessary to clear the smoke by return flow along the roof would have been 'glacial' near the floor, even if such a draught could have been created by mere gravitational drainage.

There is no direct evidence to prove that the eastern passage communicated with the rest of the Pixie's Hole system, as it does today, at the time of the archaeological occupation. However, there are certain inferential arguments that take us back at least to a time quite shortly after the occupation. It was noted by MacEnery that the capping stalagmite in the cave (cf. STF) became progressively thinner inwards. Today, there is a thick remnant of tufaceous stalagmite adhering to the west wall, just within the modern entrance to the eastern passage; it is highly probable that this is a lateral equivalent of STF. Such speleothem formation is dependent, either upon a localised source such as a spring (of which there is no sign whatsoever at Pixie's Hole), or upon a strong draught, causing rapid precipitation of the entire carbonate content of drip water by evaporation. Moving back to a time shortly after the deposition of DIA, it should be noted that there is no obvious exit from the system through which large quantities of DIA could have been removed by later erosion. If DIA blocked the lower end of the eastern passage, it must have been eroded to a minimum depth of c.1.3m before the deposition of stalagmite (cf. STF); there is not the slightest indication of such extreme erosion. We can go no further back in time, because there is always the possibility that DIA itself opened a route into the rest of the system during its emplacement. However, the argument presented above, concerning the behaviour of smoke in a single entrance, down-sloping passage, seems so compelling to the present author that he feels justified in suggesting that at least a route for appreciable air movement was indeed open at the time of the occupation.

The significance of this detail lies in the fact that Pixie's Hole is, and has always been over the recent geological past, a multiple entrance system. If it is accepted that the connection between the eastern passage and the Pope's Chamber (by far the most 'constricted' point in the system) was open, it is then certain that an air link with the upper entrances of the system was available. The nearest upper entrances lie vertically above a point a few metres beyond the 'truncation' of the south-west passage as drawn in fig.43. The lowest of these would have been at least 7m higher than the likely level of the eastern entrance at the time of the occupation, although most of the upper entrances would have lain at least 12m higher than the eastern entrance. These figures are minima because the upper entrances may have been slightly truncated by quarrying. Today, a powerful chimney effect (cf. section 2.2.) is created by what must be a very similar configuration. However, another low level entrance has been opened by quarrying , so that the airflow is now partially short-circuited; before quarrying it would have been even stronger in the east passage. Without the artificial blockage created during the second excavation season, there was a strong summer out-draught (immediately sensible to the skin) in the eastern passage, even on relatively cold and overcast days. This out-draught cleared smoke from the passage in an efficient manner, with no unpleasant build-up in the cave air. In addition to its smoke-clearing properties, the difference between this draught and that caused by gravitational drainage in a closed passage is that it was spread over the entire passage cross-section, not concentrated near the floor, so that greatly increased discharge could occur with a decreased sensation of 'cold' (chill factor). During most of the summer nights, the chimney effect weakened or stopped, although the warmth of a 'fire' was still enough to tip the balance and to re-establish a sufficient draught. However, on the few occasions when exterior temperature dropped markedly at night (late September), the chimney effect was reversed, with in-flow down the eastern passage. At such times, a 'fire' was not sufficient to prevent unpleasant accumulation of smoke. The author has not visited Pixie's Hole in the middle of winter, but it seems likely that an in-draught in the eastern passage would be the dominant flow pattern. The continuing inward slope of the passage to the north of the hearth area would require an in-draught of most uncomfortable proportions to clear smoke in an inward direction; it is, however, most unlikely that such a convectional draught could arise in this relatively small system.

The above arguments are complex and somewhat inferential. The author hopes to tighten these arguments in the near future by accurate measurement of the various meteorological parameters involved. As for the 'human' parameters, the effect of smoke in a closed space should not be underestimated. The simulated 'fires' produced by the author were fuelled with the most smoke-free material that was naturally available. If damp fuel had been used, or if fat had been dropped into the 'fire' during 'cooking', improper ventilation might have been positively dangerous. Despite the present lack of quantified data, the author suggests, merely as a working hypothesis, that the hearth in Pixie's Hole indicates occupation at some time during the 'summer half' of the year. Note that it is relative temperature (or relative virtual temperature) differences between the exterior and interior which are most important here, not absolute temperatures; the airflow model should be valid, at least in general terms, for the later Devensian. Given the small amount of information that is currently at hand, concerning the apparently 'temporary' nature of the occupation and the possible exploitation of Haldon nodule flint (presently outcropping at over 160m O.D.), a 'summer' occupation does not seem unreasonable. However, it is obvious that much stronger evidence will be required before this hypothesis can be raised to the level of a real probability.

The recent excavations at Pixie's Hole have produced some interesting information concerning Upper Palaeolithic occupation in the area, as well as a number of hypotheses which are in need of testing. Initially, excavation was planned for three main seasons. At the end of 1977, it was clear that archaeological material was definitely present in some quantity, but in a derived context. Apart from geological and palaeontological considerations, the object of the 1978 season was to collect more of this derived archaeological material and also to assess the possibility of retrieving _in situ_ material. At the end of the 1978 season, there was convincing evidence that the expression of a 'living floor' had indeed been located and that there was a very good chance that this undisturbed 'floor' continued on into the surrounding deposits. Because of this evidence, the author took the decision to cancel the last of the three planned seasons. The painful truth is that the team was not properly equipped to excavate large amounts of _in situ_ archaeological material with the level of accuracy and flexibility of response due to such an important find. The excavations had been carried out up to that point on a very low budget, most of which was provided from private sources (including the pockets of the excavators themselves). Despite attempts by the author to raise funds, it was clear that no significant amount could be found in time to ensure appreciably better quality excavation in 1979. The author could not justify continued work without, at the very least, wet sieving (dry sieving, with careful hand picking of clayier sediments, had been used up to that point, probably resulting in the loss of significant numbers of small objects), electric light (gas lamps had been used), more accurate surveying equipment (a Dumpy and line levels had been used) and more support personnel (1-4 skilled excavators and a finds assistant had been available). After a total of some five months' work, the potential of the cave had been proven and the time had come to stop. Excavation will not be resumed until the author can raise the funding appropriate to such an undertaking.

21.6.7. Other Chudleigh Sites

The present entrance to Cow Cave lies c.65m west of the eastern entrance to Pixie's Hole. However, the westernmost extension of the latter site consists of a passage which is choked with sediment at the same level as, and only c.12m from, the easternmost extension of Cow Cave, which is similarly choked with sediment. Thus, it is highly probable that the two caves connect. Cow Cave has been radically modified by quarrying, at the very least c.10m of its entrance area (to the south) having been lost. The present author found that the slope immediately outside the cave is composed of tip from the cave (old excavations, *infra*), overlying quarry rubble, to a depth of at least 2m. Today, an arched passage (c.5m wide and c.4.5m high) leads inwards (north) for c.13m, before turning eastwards and also probably westwards (i.e. a T-configuration).

The first recorded excavations at this cave were carried out by the Torquay Natural History Society, starting in 1927. Much of the entrance passage seems to have been excavated or 'mined' before this time. Beynon reported the findings up to 1932, after a total of 60 days' work at the site.

> [There] probably at one time existed a stalagmite sheet covering the deposits of the Cave. Beneath this sheet a large mass of breccia lay - an unstratified conglomeration of cemented flints and other rock fragments, set in a matrix of comminuted flint earth, mixed with soil of the Culm [Carboniferous] washed down from the Haldon and Ugbrooke slopes and cemented by the infiltration of carbonate of lime; for, from the traces of stalagmite sheet to the floor, quantities of breccia coat the walls of the three sides of the cave.
> [...]
> Below [disturbed deposits] was the yellowish Haldon soil already referred to, ... (1932:129)

The pebbles and fragments found during the excavation which are 'foreign'

> to the limestone rock of the cave are those that compose the Eocene gravels of Haldon and Ugbrooke slopes.
> Flints greatly predominate, and include fossils of sea-urchins, sponges, and shells, fine grit from Ugbrooke, pale yellowish sandstone, well-rounded quartz pebbles, and fragments of limestone. With very rare exceptions, the limestones are angular, being derived from the walls and roof of the Cavern, but all the others almost invariably are well rolled, and their surfaces are frequently smoothed and blackened. [...] Portions of large stalactites and stalagmite bosses have occasionally come to hand from the cave deposits; .... (ibid., pp.131-2)

This description suggests a deposit very like the Siliceous group in Pixie's Hole. However, the finds from this early excavation, together with later work at the site (infra), indicate that a more complex sequence was in fact present. Concerning the bones recovered, Beynon wrote:

> No bones have been found in their anatomical relations, and very few are rolled, and yet fewer gnawed, though many bone splinters have been encountered. (ibid., p.129)
>
> Many of the bones are also black. (ibid., p.131)

The species list, in order of frequency, includes bear (dominant), wolf, fox, hyaena, deer, badger, wild cat, Irish deer, ox, possible bison, and goat or sheep. Archaeological remains also appear to have been found.

> Palaeolithic man did not make the Cow Cave a permanent habitation, for no traces of an ancient hearth have been noticed. At about 10 ft. from the back wall of the Cave, in the inner chamber [at the 'node' of the T], a black streak in the Haldon deposit looked as if we had come across an ancient fireplace, but it proved to be only decayed vegetable matter. Great care had to be taken to avoid being caught by the many natural imitations of implements which were produced by pressure and fracture on flints in their travel to the Cave.
> Eight specimens of man-made implements have been recognised as such. One of these was pronounced by Mr. Reginald Smith to be a perfectly beautiful Azilian. (ibid., p.131)

Excavations continued at Cow Cave until at least 1935 (Alexander 1933, 1934, 1935), although no other detailed publication followed. Man, rhinoceros and beaver were added to the faunal list, and "one good Aurignacian flint was found which is remarkable for its perfect patination and extraordinarily porcellaneous feeling to the touch" (Alexander 1935:73-4).

During the period 1962-3, J.W. Simons conducted a careful study of the Cow Cave sections, combined with a small excavation mostly in the eastern branch. He also collated much information on the Chudleigh Caves in general and examined the extant collections in the Torquay Natural History Museum. Simons has been kind enough to allow the present author to see his manuscript and it is to be hoped that this work will be published in the near future. Suffice it to say here that an interesting series of faunal assemblages has been recorded. Simons recognises a stratigraphy which, greatly simplified, comprises a lower gravelly unit, a middle unit with many speleothem fragments set in an unstructured fine matrix, and an upper unit of 'cave earth' type. The stalagmite fragments in the middle unit were sampled for uranium dating in 1978 by H.P. Schwarcz (McMaster University, Canada). The present author's examination of Cow Cave in 1978 allowed the recognition of a stratigraphy that is in close agreement with Simons's work. Thus, the general sequence is comparable to that at Pixie's Hole. In order not to pre-empt Simons's expected publication, the following brief discussion will be limited to generalities and to details already published.

Rosenfeld makes the following comment upon the archaeology of the site:

> Six flakes, ... probably of middle Palaeolithic age, are known from the Cow Cave, in the Chudleigh Valley. Recent re-excavation of the cave by Mr. J.W. Simons yielded only one flake, but its position could be firmly established as being from the lowermost deposit, which is separated from the overlying Last Glacial reindeer stratum by a fractured stalagmite. The flakes are from prepared cores; some have facetted platforms, but none are distinctive types. (1969:133)

Campbell suggests:

> The other two Devonshire sites, Cow Cave and Tornewton Cave [cf. section 21.2. in the present text], are of uncertain affinity, although their stratigraphic evidence suggest that they might have some Earlier Upper Palaeolithic material. (1977:141)

Campbell lists only two unretouched pieces, a blade and a flake, and he references only Beynon ("1934" = 1932) and Rosenfeld (1969).

Thus, Cow Cave has been variously reported as possibly containing Middle Palaeolithic, E.U.P., "Aurignacian" and "Azilian" artefacts; it is far from clear whether the different commentators are referring to the same or to different objects. The present author has seen none of these pieces but it still seems possible to rationalise the reports. In his manuscript, Simons notes that one of the pieces found by Beynon is a backed blade of creswellian type; this is probably the "Azilian" piece previously recognised by Smith (supra). Given the finds of similar material in Pixie's Hole and Tramp's Cave (infra), it seems likely that an L.U.P. industry is present in the 'cave earth', an industry which probably acounts for the reports of "Aurignacian" and "Azilian". Campbell's proposal concerning E.U.P. material in Cow Cave is not based upon any concrete or even suggestive data; it should be abandoned unless more information becomes available. There remain only the flakes mentioned by Rosenfeld (and seen by Campbell?), probably originating from the 'gravel' unit. Rosenfeld suggests that they are "probably" Middle Palaeolithic and that they are not of "distinctive" types, and yet she implies the use of Levallois technique. It should be noted that no other archaeological text seen by the present author refers to this material. All the author can add is that he has examined flint gravel from Cow Cave, the Kate Brook Valley and Pixie's Hole (over 12m^3 at this site, all carefully sieved); the gravel is always rich in pseudo-artefacts (cf. Beynon's comment, supra) and a few good mechanical flakes were recovered that could easily be mistaken for Levallois pieces. A selection of the 'best' flakes collected by the present author was also examined by D.A. Roe and L.H. Keeley; they agreed that they were not likely to be artefacts, especially when the context is taken into account. Thus, some measure of uncertainty must remain for the present concerning the oldest Cow Cave material.

Simons has recovered rather more fauna from the 'gravel' unit in Cow Cave than the present author was able to find in the similar deposits in Pixie's Hole. The Cow Cave material also seems to be in rather better condition (unrolled) and it is to be hoped that some stratigraphic information will be forthcoming. However, the only record so far published is problematical. Sutcliffe and Kowalski state:

> The occurrence of M. [Microtus] nivalis among various rodent remains excavated by J. Simons from Cow Cave, Chudleigh, Devon ..., suggests that there may be a pre-'Ipswichian' deposit there. (1976:66)

They also indicate (ibid., p.115) that the identification in this case was by G.B. Corbet, an acknowledged authority on European mammals who should not have been mistaken. However, as has been noted in the discussion of the Tornewton fauna (p.735), Stuart (1982:37) states that "no site" discussed by Sutcliffe and Zeuner has yet produced an upper third premolar, the only tooth diagnostic of the snow vole. Stuart refers all the claimed finds to the northern vole (Microtus oeconomus). Whether or not the identification of snow vole is correct, it should still be pointed out that at no British site is there a clear indication that the proposed snow vole remains are referable to a necessarily pre-ipswichian chronozone. The present author is not aware that Simons has recovered any other species that would unequivocally indicate such a date for the 'gravel' unit at Cow Cave.

On the south-east side of the Kate Brook, almost above the waterfall, lies the site known as Tramp's Cave or Shelter. This site is formed in a steep bluff of limestone, with a south-westerly facing entrance at c.51m O.D. There does not appear to have been any quarrying in the immediate vicinity. At first sight (*infra*), this is a deep shelter, penetrating c.10m inwards from a c.6m wide entrance. Quite a large excavation has been carried out in this site and much of the accessible cavity appears to be due to this cause; digging has also occurred outwards, to several metres beyond the overhang. Rosenfeld, acknowledging Miss M. Collins for permission to publish, makes the following comment:

> In the small shelter of Tramp's Cave in the Chudleigh valley a backed blade industry with obliquely blunted blades has been recovered from a cave earth containing *Bos*, *Equus* and *Cervus elaphus*. (1969:134)

Campbell (1977, Table II.6.) notes that the excavation was carried out in 1968 and that "M. Smith, London" was responsible and is in possession of the collection. Campbell states that he has not seen the artefacts, that they number less than ten and that they are *perhaps* attributable to the L.U.P.

The present author visited the site in 1978. The sediments are very different from those in the caves on the north-western side of the Glen, indicating that the Kate Brook was an important geological 'divide' during the latest Pleistocene. No good sections were available, but there appeared to be at least 2m of rather homogeneous deposit, consisting of altered limestone scree and large blocks, set in a dense matrix of mottled but generally red (10R 4/6 or even redder hue) gritty clay. Some fine slate debris was present and, appart from degraded limestone and calcite crystals, the matrix was poor in carbonates. This material appeared to be very similar to the Waddon and Highweek Series Soil substrates (Clayden 1971) present to the north-east of the site. Judging both from the configuration of the exterior rockface and slopes and from the apparent geometry of the interior deposits, this red sediment could not have entered the site through its accessible entrance. It is therefore suggested that this site is a true cave, which continues on towards the north-east. The back of the present cavity is not bedrock; at the top of the sequence there is a jumble of massive boulders, firmly cemented by stalagmite, reaching the limestone roof.

At one point right at the back of the present cavity and c.1m into the more matrix-rich deposit, the author noted a moderately strong charcoal spread. This proved to be a mere remnant against an altered rock and it could not be decided whether or not it represented an *in situ* archaeological feature. In the small amount of sediment removed in this area were found several small, rather badly altered fragments of bone (unidentifiable, but some of them burnt) and a few flint artefacts, one of them a single angle backed point. The raw material is similar to the blue/black type at Pixie's Hole. Thus, judging from the meagre available information, it would appear that an L.U.P. industry is probably present in Tramp's Cave. Rosenfeld's use of the term "obliquely blunted blades" (traditionally applied to mesolithic material) leaves some room for doubt, but she does appear to include the site in the palaeolithic, rather than the mesolithic, part of her 1969 paper.

The last site in Chudleigh Rocks that will be mentioned here is the Chudleigh Fissure. To the present author's mind, there is some uncertainty as to the position of this fissure. It is said to be a palaeontological site, containing micromammals (cf. Sutcliffe & Kowalski 1976) and birds (cf. Bramwell 1960), indicative of a 'cold', probably later devensian faunal stage. Early references to the fissure (cf. Hinton 1926) cite A.S. Kennard as the excavator. Kennard seems to have dug in several sites in the Rocks during the first half of this century. However, the present author has never seen a published reference to the nature or location of the Chudleigh Fissure, or of any other of the sites dug by Kennard. Kennard's collection is in the British Museum (Natural History) and it would doubtless be worth searching for correspondence concerning the site.

The Chudleigh Caves have long been subject to at least local interest. Some sites have been destroyed by quarrying and brief references as to their contents cannot now be verified. Nevertheless, the surviving sites are undoubtedly of considerable importance. Indeed, the local tales suggest that even more spectacular material may remain to be found. One reliable person described to the present author a tooth, found at some time in the last few decades by an unknown visitor to one of the caves, that sounded exactly like a *Homotherium* canine!