# SUBJECTIVITY AND THE RECORDING OF PALAEOLITHIC CAVE ART

by

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## ABSTRACT

*This chapter aims to review some of the changes in approach which have taken place in the study of palaeolithic parietal art over the past twenty years. In so doing, it highlights the nature of strategies involved in such studies, and uses the Spanish decorated cave of Hornos de la Peña as its primary example. The review reveals a general lack of precision in much work on parietal art, stemming from indecision about the nature of any "art system of communication" which may have been involved. In this context, therefore, it may not be surprising to find much interpretation still concerned with species identification on the one hand and with distributions of motifs in the caves on the other. It concludes by emphasizing the intimate relationship between the investigator's own preconceptions and the research methodology adopted.*

## PREAMBLE AND HISTORY

It is more than twenty years ago that, together with Andrée Rosenfeld, I first reviewed palaeolithic cave art studies (Ucko and Rosenfeld 1967) and more than ten years ago that I tried again (Ucko 1977 : 8). Since then there have been many important, exciting and new discoveries, as well as advances in certain photographic and other methods of recording. However, when one turns to interpretation, most publications appear to assume that we all know what we are doing and that we have good theoretical grounds for so doing. I doubt that we really do and I shall argue here that even recording techniques and conservation activities *must* depend on a solid framework of method and theory.

I directed research in the cave of Hornos de la Peña (Santander) from 1971 till 1973 (Ucko 1987). At the time the team-work approach adopted was very unusual; I was assisted by an outstanding team : Ms E. Dowman (then of the Institute of Archaeology, London), Dr R. Layton (Department of Anthropology, University of Durham), Dr A. Rosenfeld (Department of Anthropology, University College London) and Professor M. Weaver (Heritage Foundation of Canada). Also unusual were at least some of the stated aims of the project; I quote them in summary from an application for funds :

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### Speleological Investigations

1. to establish the entrance(s) of the cave;
2. to establish the relationship between original floor levels and access to the cave;
3. to establish the relationship between the original floor levels and the specific art works;
4. to establish the exact plan of the cave (and exploration of any extensions to the cave discovered during the investigation).

### Technological Investigations

1. to investigate the conditions relating to differential preservation within particular areas of the cave;
2. to investigate the conditions relating to differential preservation of different parts of a particular representation;
3. to determine and establish objective criteria for the differentiation of natural rock fissures from artificially engraved lines.

### Surveying Investigations

1. accurate recording of all the representations by tracing;
2. accurate recording of all the representations by photography;
3. construction of a model of the cave to enable research in laboratory;
4. to establish the relationship between cave features and art works;
5. to establish the relationship between cave areas and art work (Leroi-Gourhan 1965; Ucko and Rosenfeld 1967);
6. to establish the relationship between floor heights and art localities;
7. to establish the relationship between cave entrances and art works;
8. to define the criterion of visibility of art works.

### Primitive Art Investigations

1. to consider the spatial distribution of art works within the cave;
2. to consider the spatial distribution between art works;
3. to consider the role of orientation within the art works;
4. to identify and consider the role of superpositioning within the art works;
5. to consider the role of relative size of representations;
6. to consider the role of style and techniques within the art works;
7. to consider the variable of date within the art works;
8. to carry out experiments of lighting within the cave – the replication of palaeolithic lighting with special reference to shadows and natural features in the cave;
9. to assess the relative visibility of representations within the cave;
10. to define the visual context of each representation;
11. to define what constitutes "related" representations or lines;
12. to consider the iconographic content of the art;
13. to carry out a comparative analysis of the content and style of the Hornos de la Peña representations.

The reason that we chose these items to record was *because we thought we knew what the relevant questions were which had to be answered.* It is relatively obvious that specific points which are considered worth recording at any particular moment are always those which we think are not obviated by the "known", but it is exactly what we consider "known" which so often turns out to be based on the most fragile of information. This approach is in sharp contrast to, for example, an art-historical or literary approach to the study of human endeavour, where basic methodologies and aspirations of the research are apparently unchanging and the assumed "clues" to understanding are taken to be relatively inviolable. Most of the work on palaeolithic (at least cave) art at the time of the Hornos enquiries sat firmly within the comfortable assumptions of art historical analysis rather than being concerned with the formulation of research aims and methods appropriate to the study of palaeolithic art as a system of communication and information sharing.

As we shall see below this situation has greatly changed over the past ten years in the works of a limited number of researchers, but these are exceptional in recognising that methodology cannot be usefully pursued outside of the framework of a research design and research strategy.

It can, of course, be argued, as it certainly was by us during our work at Hornos de la Peña, that one cannot begin to formulate hypotheses and research strategies until one knows what is actually contained within a cave. But our experiences in Hornos de la Peña show how difficult this apparently easy demand can be. Let me therefore present a few examples of specific problems :

1. Del Rio (1906 : 50) reported that he thought he saw the representation of a bison within a maze of superimposed lines in an area of the cave (Fig. 1, his C) where we found nothing remotely resembling a bison. The presence or absence of such a representation significantly alters any distributional interpretation along the lines of Leroi-Gourhan. It is important to realise that Breuil also did not record any bison in the area described by Del Rio. Quite apart from the implications for a Leroi-Gourhan-type analysis, this remark by Del Rio raises the question as to whether or not representations were clearer in 1906 than in 1911, let alone nowadays.

Del Rio gives much more detail (1906 : 51-2) about a representation of a deer, including its measurements and line qualities and the fact that it was carved in low relief, supposedly in the entrance area. The entrance area has been subjected both to natural flaking and to human and animal interference. We were quite unable to match the line qualities given by Del Rio with anything still surviving in the entrance and, as far as our research is concerned, there *is* no deer in the entrance chamber. One might simply suspect physical destruction of a representation or a mistaken identity by Del Rio either of various amorphous lines (Ucko 1987 : pl. 1) or of an "apparent" horse (Ucko 1987 : Pl. 2, Fig. 3 a-d) published by Breuil, were it not for the fact that both Del Rio and Breuil had *misplaced* the representation of a deer (Ucko 1987 : Pl. 4, Fig. 4) *which is in fact deep down in the cave*, and placed it in the entrance region (at 2 in Fig. 2). Unfortunately, the whole question of location in this cave remains one of the obscurities of previous publications, for a cervid (Del Rio, Breuil, and Sierra 1911 : Fig. 93, 2; Ucko 1987 : Figs 7a-b) was published as being in Chamber D (Fig. 3, Location 2) but is in fact in Chamber E (Fig. 3, Location 42) and this reflects the unreliability of the existing published data base.

These examples show how difficult it is to correct the data base when a cave has been left open to the ravages of one and all. As will be seen later, it is known from Hornos de la Peña that people scratched representations on the walls at different times and local tradition has it that the "amorphous" lines in our area 44 were known to Del Rio but not published by Breuil. After extensive research (including washing of the wall which appeared to have been smeared with a mud wash), we concluded that this area contained lines which were of a different quality from recent initials and that there was no reason against assuming that some, at least, of the lines could be of considerable antiquity, whether made by man or other beast.

2. It was only after the completion of our lengthy period of research and our extensive exploration of both old and newly-discovered galleries, and despite the fact that the cave had been relatively well published by Breuil, that we realised that we might have missed one whole category of palaeolithic art even if it had been present in the cave. Because of our preconceptions in the early 70s about palaeolithic art, we concentrated on walls and roofs (the floors having been totally disturbed by previous visitors) and we were "programmed" to look for representations of a size known to us from previous experiences of palaeolithic art. It was only after this period that Lorblanchet introduced us to the minute and finely-engraved representations of Ste-Eulalie (Lorblanchet *et al* 1973). There is little doubt that despite three seasons work, we could well have missed this sort of representation.

3. One zoomorphic representation stands out at least as clearly as any other in Hornos de la Peña; a representation whose precise species is hard to specify (Ucko 1987 : Fig. 14, Pl. 9). This figure had not been published or mentioned previously in secondary descriptive accounts of the cave contents. Despite intensive investigation, we remain unclear as to its date .The patination of some lines might suggest that it is not extremely recent and some lines run over old stalagmite but, even so, we have not determined any way to assign it (or part of it) to any of the specific periods represented by archaeological debris in the cave. The patination of the various lines making up this representation are not similar, and it is indeed conceivable that lines, such as at least one "leg", were added to the basic shape at different times. It is equally possible that all of the lines, except those of the "head" are of relatively recent origins (and see Delluc and Delluc 1971 for similar problems in other caves).

4. On a particular crumbling stalagmitic column far inside the cave of Hornos de la Peña is a complex of lines which preoccupied us throughout our research. The lines, located on a decaying brown stalagmite, were first thought to represent a possible engraving of a bird, an identification later tentatively switched to a deer with head stretched upwards (Ucko 1987 : Fig. 5). Our interest in this "representation" was heightened in the context of the "missing deer" identified by Del Rio, and as described above. We carried out extensive binocular examination of the quality of the lines which revealed that they were not all cut to the same depth into the stalagmite and also that many of them petered out and did not join each other, as first thought on the basis of a superficial visual impression. This analysis was then followed up by moulding and we have concluded that there is nothing definite to indicate that all except possibly one of the lines are not natural.

These examples serve to reinforce Clottes' recent (1989) warning about the unreliable nature of many palaeolithic cave art inventories and the inevitable harmful consequences to interpretation that such inaccuracy implies. Much of what follows is the result of failure by our team to achieve

some of the proposed research aims set out above at the beginning of this paper, together with an attempt to learn from our experiences.

## THE POINT AT ISSUE

The fundamental question is "Why do we choose to record what we do when dealing with palaeolithic cave art ?" There are those who claim that we can relax because our techniques are being "depersonalised" (Delluc, Delluc, *et al* 1981 : 16-19); they are not, and they can never so become.

Let me try to put this fundamental point more explicitly and with some of its complex implications :

a) A tracing by Breuil, a photograph by Vertut, a sketch by Lemozi, or a replica by Ligot and Lehanier, are all the result of subjective choice, albeit in varying degrees and involving different technical choices, but all are subjective and partial (distorting) records only (see Rigaud *et al* 1984 for many examples). This is strikingly true even of Lascaux II which is so often acclaimed as a "faithful copy" whereas, in fact, it only seeks to reproduce about a third of the cave's depth and only part of its content (Dauvois and Vézian 1979); it is self-evidently ludicrous to claim that the replication is so good that it is almost possible to claim that "Lascaux has reopened" (Delluc and Delluc 1984 : 84).

Let us now turn to our work at Hornos de la Peña and compare it to Breuil's. We found that his drawing (Ucko 1987 : Fig 29a) of "a (nother) complete primitive ox... is... roughly drawn; it has only one straight horn pointing forwards and one leg per pair, no tail and an arrow stuck in its flank" (Breuil 1952 : 353), omitted the middle section of the line which traverses the supposed animal body (Ucko 1987 : Fig. 29b), thus transforming the upper end into a horn, the lower end into an arrow. In fact it is true that from certain angles in the cave the central part of this line *is* almost invisible [and does not show up in photographs (Pl. 1)] but from other angles it is quite clear (Pl. 2.). Breuil was clearly influenced in his perception (i.e. "choice") of this representation by his pre-supposition that palaeolithic art was to do with sympathetic hunting magic, as, of course, he and others have been in their identification of "breath" and "tongues" on various animal representations (Breuil 1952 : 339; Del Rio, Breuil and Sierra 1911 : 102).

It is vital to recognise that this "biasing" of the record is evident not only at the level of a specified particular image, in terms of which line is chosen to be included or omitted from a particular trace or sketch (or given especial lighting or shadow in a photograph), but just as much at the more general contextual level of how much of a "surround" is included in a trace or sketch or photograph.

In Hornos de la Peña, during experiments with fat lamps, we found that it was more difficult to distinguish between natural and artificial lines on the cave walls, that deep engraved lines or cracks stood out well, that many features of the cave surrounds combined in the flickering light of the burning fat to give an impression of movement to the lines on the walls, and that shadows from protrusions normally came from the top, as the lamps cast no downward light and lamps were always placed on the floor when activities were carried out.

Despite important discussions over the last ten years by Lorblanchet for Marcenac (Rigaud *et al* 1984 : 34), Sainte-Eulalie (Lorblanchet *et al* 1973), and by Vialou for Aldène (Vialou 1979), about the use of natural rock features, bear scratches, etc., in the "composition" of palaeolithic cave art, there is no general concensus amongst researchers as to what a *comparative* data base of palaeolithic art should specifically consist of – and yet this can be claimed to be one of the essential dilemmas of palaeolithic art studies (Dauvois and Vézian 1979). Is the data base to include only the artificial man-made lines within a cave or also all the possibly "associated" natural features and, if so, what will be the usable definition of "possibly associated" ? At Hornos de la Peña, it is clear that Breuil did not distinguish between the artificial and natural when the "feature" made "anatomical sense" to him, as in the case of eyes and pupils.

Indeed in at least one case we suspect that Breuil's identification of an ibex (Ucko 1987 : Fig. 28) incorporates an area of probably bat scratches (and see Clot and Cantet 1974 : 82) as its beard (Fig. 11 A-C). If the data base is to include the use of non-man-made items when they are deemed to have formed part of a palaeolithic "composition", the content of the data base is bound to remain to a large degree subjective and unstandardised. Vialou's publication of Aldène (Vialou 1979 : 64-69) demonstrates this point excellently with regard to bear scratches, the use of natural lines, and assumed mammoth iconography.

There is a similar lack of precision in works which acclaim advances in photographic techniques, and analyses by, and through, photography. None ask what is the "unit" which they are seeking to analyse or "record" (further discussion below). Similarly, almost all discussions of methods of conservation and how caves should be treated in the context of public tourism all assume that we all know what image, or complex of images, area of a wall, or part of the totality of the cave should be so "conserved" and which parts need not be so treated. An important consequence of this approach is the assumption that we are all agreed on what is important for the public "to be told" and understand about palaeolithic art. The suspicion remains, however, that no one has formulated the question for debate and that conservation (and even recording ?) is usually undertaken as a response to disaster (e.g. Lascaux, Altamira, Niaux, etc.) and choices made simply for pragmatic tourist-based reasons. For example, it is more than likely that if a Niaux II were to be reconstructed (along the lines of Lascaux II) it would certainly be the Salon Noir which was reconstructed simply on grounds of tourist interest and "aesthetic" beauty (and therefore protection) without any overt discussion of what the public *should* be shown of Niaux, of which the Salon Noir is only one small part of a coherent whole (Vialou 1981 : 19). After all it could well be argued that the public should be "educated" to appreciate the existence of what Vialou (1981 : 22) calls "Magdalenian space" and this, it could be claimed, could be much better demonstrated in quite different parts of Niaux. It is heartening, therefore, to find the very recent suggestion (Rouzaud *et al* 1989 : 65) that La Magdeleine cave should be fitted out so that visitors will see the art works at the same physical eye level as did their originators.

There is often a similar lack of precision by those who stress the need for multi-inter-disciplinary team research in caves. Are they talking about the sort of team research as referred to for Pech Merle (Rigaud *et al* 1984 : 35) or la Magdeleine (Rouzaud *et al* 1989), and as carried out at Hornos de la Peña, which in the 1970s consisted of six academics selected for their particular expertise in photography, moulding techniques, excavation experience, research into "primitive art", etc. together with students to carry out speleological investigations and to assist with recording work ? Or are they in agreement with the despairing words of Pfeiffer, already

criticized by me (Ucko 1984), that "it is time for something more systematic; visits to a set of selected sites by a working group made up of not only an archaeologist and an artist but also of a dancer, psychologist, architect, acoustical engineer, a producer or professor of drama involved in staging and stage effects, and a zoologist with an experience of observing the behaviour of wild animals" ?

As Robert Layton put it in one of the formulations of the Hornos research project (and see Rouzaud *et al* 1989), we need to be very precise about what experiments with different lighting (or sounds) can achieve. On the positive side, the use of different lighting procedures in caves can act as a corrective to simply following the influences of Breuil's published drawings and perceptions. Different lighting conditions may also reveal different lines and emphases from those studied by previous researchers. In our experiments at Hornos de la Peña we tried to capitalize on these questions by dividing students into those who had, and those who had not, previously seen publications of palaeolithic art and each category of student produced sketches of cave wall areas lit respectively by oil or fat lamps, acetylene, and fluorescent tubes. At no point, nor in any way, was the assumption made that we thought that we were looking at the art "through the eyes of palaeolithic man", since what he perceived depended on both physiological development and the results of cultural conditioning, both of which may have been very different from our own; and in this we differ both from the Dellucs (1981 : 16) and from Dauvois and Vézian (1979), all of whom appear to believe in the possibility of recreating palaeolithic perspectives (and see Layton 1987 : 211). Candles and acetylene lighting were often used by Breuil, and such lighting conditions can be simulated by us; in addition we have his sketches and tracings. This does not, however, mean that we can ever exactly replicate even his individual perceptions.

Many of the above remarks reflect, I think, the continuing level of unreality in many of the palaeolithic art studies of today. The lack of precision which I have reviewed perhaps could be claimed to coincide with a period of stagnation in palaeolithic art studies were it not for the exceptional studies of the past ten years or so, to which I have already referred above. Despite these exceptional publications, however, it remains true that most practitioners of palaeolithic cave art studies appear to lack clarity in formulating what they think the study of prehistoric art is all about, and therefore there is a lack of precision in assessing what problems (which may be diverse and even heterogeneous but which need nevertheless to be clearly defined) are pertinent and of interest. The lack of public discussion of these basic points of principle in palaeolithic cave art studies is in sharp contrast to the recent burst of sophisticated reviews of other prehistoric rock art studies (Lewis-Williams 1983; Meighan 1981 : 110) and their aims, some of which have taken note of the rather longer-lived and very theoretically oriented investigations of living "primitive" art systems (Merlan 1989). Let me attempt to make some of these dilemmas both explicit in general terms and also pertinent to our previous work in Hornos de la Peña.

Methods of study adopted in the analysis of palaeolithic cave art are guided by the researcher's theoretical orientation. The first priority of any theory must be to determine what are viable questions to ask of palaeolithic art, and which questions, however interesting, cannot be answered. As Layton (1987 : 212-3) has pointed out, the fact that cave art is rich in its varied motifs, and remains where the artist(s) put it suggests that several useful questions should be able to be asked with respect to it; however, not all are equally easily answered.

Let me take, as the first parameter of enquiry, or hypothesis, that the "sets of paintings (or engravings in caves) constitute performances or texts created within (an) artistic system" and that, to a certain extent at least, "the complexity of their structuring is a matter for empirical analysis" (Layton 1987 : 213). This is, in fact a very similar starting point to several other authors (Sauvet, Sauvet and Wlodarczyk 1977; Vialou 1982 : 42) and, as has already been seen, it was also our original parameter of research for our investigations at Hornos de la Peña.

We all know that in fact assessing such assumed structures is not easy; (a) differential preservation may have destroyed some of the original corpus; (b) there may have been complementary subsystems which leave no record (e.g. ritual).

b) Classifying depictions as representations of "the same motif" (e.g. "bison", "claviforms") is often difficult. It is good to see that Sauvet *et al* (1977 : 548) have made this situation explicit; what they have not stressed is that current practice must of itself result in the arbitrary, and hence subjective, imposition of "categories" of "sign types" onto the data.

The treatment of each cave as a single unit of analysis is also questionable, and less generally recognised; if we take, for example, Leroi-Gourhan's interpretation of Hornos de la Peña, we find that he suggested that different areas of concentrated engravings stand out both from each other and possibly also from a scatter of less visually spectacular motifs. If such a situation were indeed the case it might reflect complementary cultural themes or it might mean that the motifs were entirely unrelated to each other; in the latter case, any treatment of the cave as a meaningful single unit would surely be unwarranted. The same point has also to be made at a different level of analysis. Long ago Rosenfeld and I argued (1967 : 224) the need for the study of groups of caves as complexes; the Dellucs' study (Delluc and Delluc 1983) of three decorated caves near Domme raises again, in an acute form, the question whether the distribution of art within the single cave is really the appropriate dimension within which to try to establish regularities and irregularities of art motifs.

At this stage it must be made explicit whether or not we think that we can answer the question, "What was the specific content of the belief system" ? Fascinating though a positive answer to this question would be, it probably is impossible to deduce the belief system because it was the product of a particular, unique cultural trajectory through time. Indeed, in this context comparative surveys of non-palaeolithic rock art do not help for, contrary to the partial résumé of convenient practices produced by the Sauvets (Sauvet, Sauvet and Wlodarczyk 1977 : 552), they in fact show a multiplicity of functions which rock art may serve as well as a huge variety of non-sacred "messages" which can be conveyed by art "motifs" (Ucko 1977 : 42; Merlan 1989). Moreover, in the general context of the development of human cultural systems, the specific content seems less important than the structural complexity of the system. I am therefore arguing that, within the particular parameter of art as a communication system, which I have chosen to explore first, the most useful questions to ask in the analysis of palaeolithic art are those concerned with identifying the size of the corpus of motifs which constituted the "vocabulary" of the art, the frequency with which they were used, and any regular patterns in their distribution across a region which may reveal structured rather than random use.

It is easy to see how this approach to the study of palaeolithic cave art leads to significant differences between this article and those conventional discussions which claim the need to be

able to assign animals to subspecies in order to understand the Upper Palaeolithic environments and ecologies. The well known and large literature attempting to assign palaeolithic representations of animals to species and subspecies does so without much success and inevitably leaves several "animal" representations defying such classification (Layton 1987 : 219; Clottes 1989). Recently, Vialou (1982) has shown how more sophisticated attempts to identify animal species even at Niaux have also failed for at least three representations. There are two possibly more important related questions to be asked : first, is this really what a study of palaeolithic art should be concerned with and, second, is a clear differentiation of animals into species and sub-species what the palaeolithic artists were concerned with ? Are we agreed as to our own etic interests, and were the emic aims really to differentiate species and sub-species by certain diagnostic features ? I do not believe that the Sauvets (Sauvet and Sauvet 1979 : 341) are right in simply claiming that palaeolithic artists always kept species-diagnostic features present, even in their simplest works. On the contrary, as Vialou (1979 : 212) and the Dellucs (1974) have shown, many "animal" representations remain species non-diagnostic even when the most objective "tests" (derived from palaeolithic art canons) are applied to them : "Magdalenian art styles... are very diverse. Some are close to zoological reality while others demonstrate stylistic freedom" (Vialou, *ibid.*). In this case, the Sauvets appear to fall within the general trap of seeking to find the sort of evidence with which they can work; the emic is being made to conform to the etic. Vialou's (1979 : 60-63) recognition of a category of "indeterminate" representations at Aldène is refreshing in this context as is the Delluc' (1974 : 37-38) acceptance that E11 at Villars cannot be certainly assigned as either feline or horse. Indeed, are we certain that the investigation of the area between (and possibly overlapping) the emic and etic is really appropriate in this palaeolithic context for the recognition of species-diagnostic animal traits ? (and see Clottes 1989; Lorblanchet 1989; and especially Merlan 1989 : 22-4).

None of these considerations seem to be discussed by those who simply assume that zoological interpretation and classification has some sort of objective significance. In fact we need to remember that some ten years ago a zoologist pointed out, with regard to a group of Australian rock art, that "out of 22 items... my diagnosis was wrong in 15 (and only) superficially right in 7" (Macintosh 1977 : 194). Those studying palaeolithic art with the above approach seem to remain unaware of the point stressed by me (Ucko 1977 : 16) that what is considered to be "zoological observation" is, like the study of palaeolithic art, a matter of subjective choice. The profoundly negative results of an overtly and more or less exclusive zoological approach to palaeolithic art recording and interpretation at a specific level can be noted at Hornos de la Peña with regard to Breuil's subjective comments about the particular *quality* of the representation of an animal hoof (Del Rio, Breuil, Sierra 1911 : 96) and, at a general level, in the way that intentional palaeolithic ambiguity may be being forced into a western scientific straitjacket (and see below).

To continue with Hornos de la Peña (although the same is true of many other caves), Breuil and Saint-Périer (1927 : 146-147) referred to the several finger tracings (Pl. 3) as simple decorative motifs in meander form, and pointed out that they could have been inspired by the form of "serpents"; in other parts of the cave also, Breuil *et al* suggested the interpretation of specific representations (Pl. 4) as possible snakes (Del Rio, Breuil and Sierra 1911 : 96). It would be tempting indeed to try sub-species identification of the possible snakes of Hornos de la Peña but we should ask (a) whether snakes actually existed in the Upper Palaeolithic or not, (b) are we really certain that we can distinguish the intended representation of a snake from other

images which are treated as "signs", and (c) if representations are assumed to be snakes, why not worm representations ?

This same set of concern can be approached from the opposite direction. As Rosenfeld and I have pointed out (Ucko and Rosenfeld 1972), "If a representation cannot be recognised as a known animal, or if the head of a figure cannot be assigned to a particular species, there is no *a priori* reason why it should be thought to represent an anthropomorph with a mask. If the head (or whole body) appears to us to look like a camel or other animal which cannot have existed in Europe according to our present knowledge of fauna in Upper Palaeolithic times there are three possible interpretations which we may adduce : (a) that the resemblance to the camel or etc. is purely coincidental, (b) that our assessment of Pleistocene fauna is incorrect, or (c) that the palaeolithic artists had gained the knowledge of this animal either previously or in other areas" (and see Del Rio, Breuil and Sierra 1911 : 98). When we get to this level of species interpretation it is not entirely clear to me why Breuil's interpretation of certain lines cut in the clay at Hornos de la Peña as a possible "isard" (Ucko 1987, Fig. 8) because it "has only a very small head" and despite "the absence of horns" (Breuil 1952 : 353; Del Rio, Breuil and Sierra 1911 : 93) should be any more correct than a student's view of the same lines with electric light as a seal (Layton ms), an interpretation which incorporates a few natural cracks in the clay surface as fat folds behind the head and ignores the small mark which Breuil interpreted as an eye. The same sort of problem, and its consequent influence on the subjective nature of the published record, can be seen in the discussion by Del Rio *et al* (1911 : 96) of the "non-bovid" ears of another representation.

This discussion highlights the continuing naive treatment in most publications of questions of style, whereas a a few recent publications have started sophisticated discussion of this concept. Strictly speaking, identifying what individual motifs represent is the step which first leads to the establishment of the "meaning" of the art. The motifs are signifiers and the concepts that they represent are the signified. Most appear to consider this step self-evident, i.e. they consider the representational quality of motifs to be transparently obvious. However, some are much more obvious than others and we would be foolhardy to assume that we have satisfactorily "understood" palaeolithic art *style*, or to forget that the resemblance between the visual motif and the natural model is never more than partial and is always a matter of convention. Because most of us appear to consider some palaeolithic art representations to be "naturalistic", are we therefore being logical in our apparent assumption that diagnostic zoological features of an animal (species) will be present overall within the art works of palaeolithic peoples ? One thing is indeed clear and that is that if the answer is "no", then a statistical analysis based only on those representations/features which are recognised by us as animals [let alone specific species (subspecies)] may be highly misleading with regard to any statements about the ecology of the period.

To assume that what is "naturalistic" (usually undefined) to our 20th century eyes reflects a concern for anatomical accuracy by artists of another culture (whether contemporaneous or prehistoric) is at least dangerous if not totally naive. The situation is somewhat complex, for the idea of "realism" is derived from the art itself and not from actual knowledge of the prehistoric animals themselves. Thus when people describe a representation as "realistic/naturalistic" they usually mean that the representation is detailed, and detail is, of course, not necessarily

synonymous with realism; nor, of course, should lack of detail be equated with chronological age (Delluc and Delluc 1973 : 205).

Lack of clarity about this whole complex problem in the context of palaeolithic art studies has led to the repetition of all kinds of fanciful and totally unjustified theories and interpretations about humans/anthropomorphs, their role in ritual, and even about the nature of cognitive development in the Ice Age (Vialou 1983 : 96). Some time ago Rosenfeld and I (Ucko and Rosenfeld 1972) tried to put the problems of the correct identification of "anthropomorphs", into two wider contexts ; (1) the anatomical and/or stylistic features which might allow us to accurately recognise the intended anthropomorph, and (2) the implications for the interpretation of the function(s)/meaning(s) of palaeolithic cave art of various possible categories of anthropomorphic representations. It is striking how, until recently (Layton 1987; Lorblanchet 1989 : 8) most people have shied away from the many points of method and principle raised in that review when they have come to discuss and describe their own recent discoveries (whether on La Marche plaquettes, within overall reviews of Aurignacian representations, or in the context of specific cave content). However, the questions posed in 1972 do not simply go away, and there is continued evidence of the subjectivity of many such prior identifications (e.g. Roussot, Frost and Daubisse 1984; Delluc and Delluc 1974 : 30, 52-3; Vialou 1982 : 29), as well as the dubious nature of new ones (Delluc and Delluc *et al* 1981 : 38-39, 71-72).

Even Lorblanchet appears to accept assumptions regarding what is "human" in art, so that he feels that he can, apparently, make decisions about what is "human", what is "animal", and what is non-representational, as well as their respective associations, on the Hieroglyphic ceiling at Pech Merle (Clottes 1983 : Fig. 21, 484-485; but see Clottes 1989 and Lorblanchet 1989). At Hornos de la Peña there are several panels where even Breuil (Del Rio, Breuil and Sierra 1911 : 91, 104) gave up, although the lines on the wall could as well have depicted anthropomorph or bird or animal or "sign" (Ucko 1987 : Fig. 28). Confusion of lines, in the context of Hornos de la Peña anthropomorphs, leads me inevitably to the best known figure of this cave.

Whatever else one may wish to say about the lines in this area of the cave the so-called anthropomorph (Pl. 5) is undoubtedly a very different creature from the one that is normally reproduced in publications (Fig. 5) and described by Breuil (1952) as a "human figure... resembling a monkey... (with)... false tail. This strange person... (whose)... head, with its rather flattened top, is worthy of neanderthal man." Breuil continued his publication "I left out the erect sex, as I was not sure, when I copied it, that it belonged to this figure. A little later I realised that it did belong. Such is the Man of Hornos, the only one in the caves, perhaps the god of this cavern..." (1952 : 355; Del Rio, Breuil and Sierra 1911 : 98).

The last few have shown (Vialou 1979 : 23-24; Delluc and Delluc *et al* 1981 : 37-39; Lorblanchet *et al* 1973 : 30, 270-275) a much more critical awareness of the problems of correctly identifying "chronology" by a study of superpositioning and/or quality of lines. For Airvaux and Pradel (1984 : 213), admittedly speaking about plaquettes, it is normally not possible to establish chronology through a study of superpositioning. Our studies at Hornos concentrated on such analyses (binocular and through mouldings) and would confirm the rarity of being able to say anything definitively. What does seem relatively clear, however, about the "anthropomorph" is that the so-called penis must have been completed after the nearby horse which in turn was finished after the lines were drawn for the body of the "anthropomorph". Even

if the superpositionings represent only a few minutes of difference in time, it is at least interesting (in the same sense that it is interesting to see how the Aldène "circle" was built up (Vialou 1979 : 38-39) to know that the "anthropomorph" was not originally conceived as an ithyphallic individual (if that is what was finaly intended) but that the so-called penis was added to the basic body form at a late stage of the engraving process. I am sure that it is not merely coincidence that the moment "anthropomorphs" are under consideration even those researchers who most attempt to be "objective" about palaeolithic cave art begin to adopt subjective terminology. Thus, the Sauvets describe such representations as "ruling" over the engraved animals (Sauvet, Sauvet and Wlodarczyk 1977 : 557) and Vialou thinks that "it" "dominates" them (Vialou 1981 : 138) ; none gives any convincing reasons for this interpretation. Continued use of such undefined terms as "anthropomorph", together with subjective statements about their function, by palaeolithic art specialists shows their continuing unconcern about the fundamental characteristics of art as a communication system where the important methodology consists of identifying regularities of motif use. What we term "naturalism" and "schematisation" are more likely to be associated with the nature of the *use* of art in different contexts and with the intentional use of ambiguity of representation rather than with anything else. This statement is based on the findings of several art studies of modern "primitive" art systems (Biebuyck 1977 : 65 ; Layton 1985 ; Layton 1987) and the possible existence of intentionally ambiguous representations in palaeolithic cave art which is beginning to be acknowledged (Layton 1987 ; Ucko 1987; Vialou 1983 : 84; Ucko and Rosenfeld 1972; Delluc and Delluc *et al* 1981 : 49-51).

The continuing (but see Lorblanchet 1989) naive differentiation of animal and human representations into "naturalistic", "realistic", and "other" provides fodder for the next obvious deficiency of many of the approaches to palaeolithic art – namely the continuing assumption that there is indeed a meaningful categorical break between such images and those so conveniently cast aside under the rubric of "signs". If "signs" are not conceptually animals or anthropomorphs, which of them are "naturalistic" and which "schematic"/unfinished/ etc ? What is the definition of a "sign" ? Are *all* "punctuations" "realistic/naturalistic" and, indeed, are they all really one and the same "category" of sign, whether painted or "engraved" (Vialou 1982 : 31-33) ? Finally, what does acceptance of a category of signs imply in terms of our interpretations ? Although Sauvet *et al* (1977 : 555) have discussed some of these points briefly in the context of handprints, general discussion is still lacking.

We need to be explicit about the assumptions that we are carrying into our work of recording and analysis. When we look at animal representations we apparently tolerate a considerable variation in the mode of portrayal; some outline figures seem as readily identifiable as are solid polychromes. We are assuming that the significance of differences in the shape of heads, horns backlines etc., can be appreciated by us because we assume that we know that these are features diagnostic of bison, deer or horse. However, when we look at what are conventionally nowadays classified as "signs" – i.e. where there is little or no "clue", at least to our eyes, of what the representation is supposed to signify – researchers have tended to appeal to supposed typological derivation (e.g. Leroi-Gourhan and Breuil) or cave location (Leroi-Gourhan). For Hornos de la Peña, at least, claims that signs begin or end a figurative unity seem inapplicable and of no help in the recognition either of sign types or so-called cave panels.

The problem with Hornos de la Peña is that we have discovered a large number of apparently non-zoomorphic lines (such as loops (Pl. 6) and circles) and complexes of lines

(Pl. 7 and Ucko 1987 : Pl. 11 in Ucko location 108), but that we do not know how to date them (see below, and also see Delluc and Delluc 1983 : 57). For Breuil there really was no apparent problem for he tended only to see animals, and omitted many areas where there are definite, apparently aged, engraved lines that do *not* look like animals. Sometimes, however, in his account he did note that there were many non-zoomorphic finger tracings which, except in one case, he did not reproduce. Also, where he saw a mass of superimposed lines, Breuil tended to isolate those configurations within them which could be made to resemble animals. In several more recent researches, recording has been extended to include only those non-zoomorphic lines which are supposed to conform to a standardized inventory of repeated "sign" elements. Works such as those of Lorblanchet *et al* 1973 : 310-313), and Vialou (1979 : 45) which give detailed recordings of all apparently non-figurative representations without attempting to fit them into any typology, are still few and have yet to be assimilated into general theoretical discussions. However, Vialou (1982 : 27) has begun to confront the problems.

If we exclude from the contents of Hornos de la Peña all those apparently man-made non-zoomorphic representations which, either because of lack of patination, superficiality of line, "freshness", or obvious character as an initial or name, are not securely or easily datable to the Palaeolithic we are left with items whose distribution in the cave is scattered and their possible "association" with zoo/anthropomorphs various and apparently non-standardised (Fig. 6).

All this, of course, again raises essential questions about aims and methodology of research. What we consider to be "simple" (or the simplest of) motifs are those most difficult to classify and, as I have suggested elsewhere (Ucko 1977 : 9), probably the most likely to have been used (or "invented") most often in different places and at different times, and possibly with varying "significations". As Layton (1987 : 220) asks : (1) Have we the right to consider all rectangular "grids" (Pls. 8, 9) (and see Sieveking 1981 : 327; Lorblanchet *et al* 1973 : 30, 310-313; Roussot-Larroque and Roussot 1973; Sauvet and Sauvet 1983 : Fig. 14) as representative of a single motif-type ? (2) Is it not possible, or even likely, that the "grid" was spontaneously rediscovered at different times during the palaeolithic, a question which Sauvet *et al* (1977 : 548-551) have also been investigating ? (3) Is the palaeolithic "grid" really not a representation of animal/anthropomorph/artefact/plant, but really a separate signifier of a different (more complex) concept ?

We arrive now at the third major set of criticism about the ongoing work of palaeolithic art investigation. Despite the length of time that has elapsed since Leroi-Gourhan's first publications, it is striking that most subsequent publications still seem simply concerned to point out the individual addition to a particular cave record and the reinterpretation of a particular image within a cave, or with criticising his original simplistic interpretation of cave art as an expression of sexual dichotomy, as opposed to really tackling the essential problems of his revolutionary work. Notable exceptions to this observation are the sensitive publications by Lorblanchet *et al* (1973 : 313-317) for Sainte-Eulalie, the Dellucs (1974 : 59-63) for Villars, Vialou (1982) for Niaux, and Rouzaud *et al* (1989) for La Magdeleine. Nevertheless is remains a striking fact that there are no attempts at all to deal with, or discuss, Leroi-Gourhan's more recent and more complex hypotheses (1982); this may be because the details of his analyses are now effectively dismissed by most colleagues who treat them with silence and some of whom are now trying out quite different "schemeta" of their own (Vialou 1983 : 38-45).

The real problems at issue for the recording of palaeolithic art are both quite different and much more subtle. Do we accept the assumption that the *number* of representations of a particular image is irrelevant, and that a simple count of presence or absence of image is all that is relevant ? Do we accept that *size*, *colour*, or *technique* is irrelevant to either the emic intention or the etic understanding of the palaeolithic art communication system ? Do we accept that "*finished*" or "*unfinished*", "*naturalistic*" or "*schematic*", placed high or low on a wall or "*hidden behind*" a boulder, a simple presence or absence within an arbitrarily designated "area" is sufficient to be the basis of our comprehension of the underlying art system ? Leroi-Gourhan's so-called statistics were attacked by Rosenfeld and myself as soon as his book was first published (1967 : 196-217) and have been commented on and criticised significantly by others also (Parkington 1969). If *any* of the above questions were to be answered in the negative, the assumption of "regularities" in cave distributions might well collapse. Almost no one since Leroi-Gourhan (but see Layton 1987 : 23) appears to be willing to address themselves seriously to this overall question. The major exceptions include Vialou (1983 : 93-94), who does present some discussion of the problem as well as some new "statistical" (= numerical) analysis and who does, for Niaux, make some use of individual "motifs" and their colouring (Vialou 1989 : 35), and Rouzaud *et al* (1989) who have recently attempted to infer relationships between individual representations within a cave in a quite new – and minutely detailed – way. Also exceptional are the Sauvets who have attempted (1979 : 341, 346, 349, 352) to tackle these questions, but their decision to treat numbers, size, and technique of representations as at least secondary in importance to what they call the "theme of the assemblage" is simply a matter of assertion, not adequately demonstrated by the statistical tests which they adduce on the basis of a somewhat questionable data base.

Nevertheless the Sauvets'attempt to link their method and findings to linguistic analysis of communication systems is admirable. Furthermore the Sauvets *do* attempt to use the number of individual representations of species to deduce interspecific relations. The problem at issue here is, however, whether it may still be premature to accept the validity of their thematic approach at all.

Let me therefore return again to Hornos de la Peña. By 1911 Breuil had identified 27 discrete configurations, or motifs, in the cave (Fig. 2). All but one (meanders at location 23 of Ucko's survey) of these he identified as animal, part of animal, or anthropomorphic representations (Fig. 7). At the end of our work at Hornos de la Peña we had identified some 140 discrete configurations (Fig. 3). In a few cases we had found additional representations of animals which Breuil would certainly have accepted as palaeolithic, but in most cases the additional configurations were indecipherable and some at least could have dated from a variety of periods. In some instances we question the accuracy of Breuil's animal identification and details. For example, Breuil *et al* identified "a very rough engraving of the head of a bison reduced to an "accolade" line going from the muzzle to the top of the forehead, and to a strongly incurving horn, a rudimentary ear and a small 'fusiform' eye" (Ucko 1987 : Fig. 26a; 29 : 93). Our examination of the area (Fig. 3, location 112 and Ucko 1987 : Fig. 26b) showed that the situation was very different from that published, and Breuil had clearly omitted several lines and altered the position of others. The line which Breuil took as part of the back of the bison is in fact one of the strokes making up the letter M, a letter which shows up clearly in photography (Ucko 1987 : Pl. 16) (and compare the Dellucs' problems at La Martine regarding Defoix's 1669 inscription (Delluc and Delluc 1983 : 21, Fig. 14)). Superpositioning is relatively clear – most of

the lines of the M predate the line taken by Breuil to be part of the bison. Various details as recorded by Breuil are distorted, particularly the so-called "oval-shaped eye" which is an arbitrary selection from three curving lines. The front part of Breuil's bison face is also an arbitrary selection but this time it incorporates several natural cracks.

The only other detailed analysis of this cave is that by Leroi-Gourhan which features a supposed ibex in the open-air entrance region, presumably signifying, to Leroi-Gourhan, the essentially "male" nature of the area when coupled with the engraved horse; both he presumably saw as "opposed" to the bison which Breuil recorded on a block in the entrance, but which has long disappeared. Unfortunately Breuil's published plan was in error and Breuil's cervid is, in fact, far inside the cave (see above) and no ibex exists in the entrance region ! Confused by Breuil's admittedly simplified published plan of the cave, Leroi-Gourhan claimed (1965 : 248, 318) a discrete physical area as a "second sanctuary"; no such discrete area can be objectively distinguished and in any case it is impossible to match Leroi-Gourhan's description of the supposed contents of this "part" of the cave either with Breuil's account or with our own reanalysis of it. He does not publish his normal type of distribution plan for Hornos and in fact refers to its distribution of art as "disconcerting".

I return to my starting point : why have individuals chosen to record the things they have ? Is it, for example, because Leroi-Gourhan never did the necessary work to correct and identify each individual addition to the published record that he therefore *had* to adopt the view that *one* representation of an animal species was the equivalent of *many* such representations ? It is not clear why we should continue to believe that it is sufficient to count the *frequency* of particular animal *themes* on the assumption that they can be determined by simply noting *absence* or *presence* of *motifs*. The recording of the distribution of representations of animals at Hornos de la Peña by "motif" versus "theme" can be shown to change our comprehension of the data. This whole question is of course of significance to more than representations of "animals/anthropomorphs", for it is clear that the same set of assumptions underlie many people's recording and analysis of "signs". As we all know, the analysis of the distribution of themes or motifs within caves depends also on the way that cave "areas" are defined. Space does not allow me to reiterate points made elsewhere (Ucko and Rosenfeld 1967) about the subjective nature of our categorisation and delimitation of "panels", etc., concepts which may well not have been part of the palaeolithic mental system (Sauvet and Sauvet 1979 : 340).

I want to move, as the last general area of my review, to another question concerning the caves used by palaeolithic artists, but to a point of much wider significance than the definition of cave "panels", etc. Almost everyone appears to assume that we know what was the palaeolithic emic view of *a* or *the*, cave. Many who deal with conservation appear to assume that we know what the significance of "cave" was within palaeolithic ideology, and virtually everyone who tackles the question of cave contexts at all, assumes that "difficulty of access/approach/working area" would have been similarly regarded in the Ice Age. In fact this approach contrasts markedly with our normal treatment of palaeolithic techniques and technologies, for few have posed the question, "Why did all palaeolithic artists not confine themselves to painting or finger-tracing, as both of these techniques were presumably less arduous than engraving or low-relief ?" Few, if any of us, have taken the step of arguing simply on the basis of "difficulty" that engravings and low-relief works can be automatically excluded from a possible category of "doodle", so that they must reflect a desire for permanence of record...

In fact, we know in all cultures that all social boundaries are not always coterminous with actual physical conditions or situations. "Difficulty" of context is, by and large, a socially and culturally defined phenomenon. What right have we to assume that an etic classification of "cave area" is a useful reflection of emic attitude ?

In order to stress the subjectivity of many of the current approaches to "the cave", I now describe the history of Hornos de la Peña, adopting a rather different style from the norm : as far as we know the first "human" explorers of the cave that we now call Hornos de la Peña were those living some 30,000 years ago. Why they first went into the interior of this (and other) caves is not known.

As far as we can tell from the archaeological record, prehistoric humans continued to go into this cave for some 20,000 years and there is some suggestion that people entered at least the entrance regions of the cave in the Neolithic and Bronze/Iron age periods (c. 3000–2000 B.C.). The next thing that we know, from literature, about Hornos de la Peña is that H. Alcade Del Rio squeezed through a narrow aperture to get into the hidden parts of the cave in 1903 AD. His view of the cave's contents, judging from his publications, was partial (Del Rio 1906) and far removed from our own. Subsequently, during 1906 and 1910 AD, H. del Rio and L. Sierra were joined by H. Breuil, H. Obermaier and J. Bouyssonie, and excavations were carried out in the "entrance" area. Local rumour states that a joke was played on Del Rio; a friend of his used a nail to draw a representation of an animal somewhere inside the cave ! During this time also the art was studied and the final image of Hornos de la Peña established according to Breuil's vision of it (Del Rio, Breuil and Sierra 1911 : Fig. 83-4).

From this period on, the cave was known to those interested in palaeolithic archaeology and access was open to all. Local tradition says that at least goats (and almost certainly cows also) were occasionally herded in the mouth of the cave and that considerable defacement of the rock surfaces (whether inside the cave or only in the entrance chamber) occurred. Those who entered the cave certainly included some specialists, for a plan of the cave made in 1919, but there is also strong local rumour that many stalagtites were removed from Hornos de la Peña in order to decorate the thermal baths nearby. Other rumours mention the attraction of fountains in the cave and suggest that people went into the caves to get the excellent water. In 1924 AD a door was erected, perhaps on the direct orders of King Alfonso XIII and the cave was declared an architectural/artistic monument on the 25th April in that year.

Local information is unequivocal that the cave of Hornos de la Peña was used as a refuge during the Civil War from aeroplanes apparently directing attacks at an airfield at Los Corrales, and reports state that some people stayed "in" the cave for several days, whereas most only went there for a day or a few hours at a time. Local repute also reports that the people sheltering in the entrance area of the cave entered the actual cave out of boredom and to explore. There is no information regarding the fate of the door erected in 1924. It is possible to surmise, that as the area was one which had to react to major thrusts of Franco's advance, and as most of the local people were united in opposing Franco, that the door may well have been broken open or that it may have been opened by a local sympathiser.

To the Spanish Government in 1953 the cave was important enough for the Antiquities Department to take control of it and to carry out works for its upkeep. Since then official guided tours are mounted on request and there have been sporadic visits by photographers, speleologists and archaeologists, culminating in our team research between 1971 and 1973.

In the current vogue of attempting to define "areas" of caves as "entrance" regions, "central" regions, and so on, there is little reason to be sanguine that we can really assume a constancy *of attitude* to such "areas". Even within a "cultural" group of researchers such as our own, attitudes to the cave varied from "the urge to explore and find new niches, chambers and corridors" to utter terror with consequent urination ! It would surely be too simple to suggest that all members of the French culture in the 20th century share Leroi-Gourhan's view of the essential femininity of caverns. Perhaps more seriously, it can be seen simply from the evidence presented above that the attitudes of Del Rio, Breuil, the shepherds and our own research team *cannot* have been the same, as their respective knowledge of the different areas, extent and plan of the cave, varied (Fig. 8). All peoples' attitudes to things are at least in part conditioned by their usage of them. The history of the use of this cave suggests that it was both varied and compartmentalized differently at different times.

I wish to underline the importance of two points for they remain those which seem not to have been adequately recognised. The literature of the past few years shows a remarkable sophistication of published cave plans indicating such elements as floor levels and cavern roofs, potential cave entrances, and possible relationships between cavern systems (Delluc and Delluc 1974 : 6, 16; Delluc and Delluc 1983) meeting many of the requests urged in Ucko and Rosenfeld many years ago (1969 : 108-14, 196-7). Despite this sophistication of recent publications and despite a whole book devoted to Palaeospeleology (Rouzaud 1978), few authors except Clot and Cantet (1974 : 96), appear to have confronted the first critical question which concerns the validity of an at last tripartite classification of decorated palaeolithic caves (e.g. Rouzaud (1978 : 123) does not deal with this question at all and although he refers to our book he does not mention this point and he does not include it in his bibliography, while the Dellucs (1983 : 70) stick to Laming-Emperaire's oversimplified classification based entirely on which regions of the cave received daylight, even when their own analysis of Comarque (Delluc and Delluc 1981 : 78-86) shows it to be quite unsatisfactory). The second point at issue is a somewhat different one – sophistication of description and detail of accurate planning is inevitably accompanied in the above recent literature by an assumption that palaeolithic *attitudes* to cave areas (niches, low passages, etc.) would have been the same as those of present day (at least) speleologists. Even terms such as "niche", and descriptions such as "difficult of access" (Delluc and Delluc 1974 : 6; Rouzaud 1978 : 124, 135-136; Vialou 1979 : 29, 36) unwarrantably presuppose a communality of perceptions and reactions by those of the past with those of today.

## CONCLUDING REMARKS

Part of the fascination to us all of cave and rock art studies is that we have before us the physical positioning of the art. However the chequered history of use of Hornos de la Peña must serve as a warning for, in this case, the "canvas" has been "slashed" and "violated" almost beyond recognition. Two illustrations demonstrate the necessity for the recording of every detail of "use" of a cave *throughout* its (especially modern) history.

As mentioned earlier, one of the aims of our team research at Hornos de la Peña was to investigate the visibility of representations and to this end we closely examined stalagmite and stalactite breakage both in the regions where there was evidence of art activity and in areas where there was none. In general our findings were inconclusive for (1) where there were breakages

they did not appear to affect visibility and (2) many of the breakages could have been modern. There is, in any case, some question as to whether any findings could have been conclusive for the construction of stairs and the digging of "floors" could have been accompanied by stalagmite/stalactite breakage and the local history of this particular cave attests to its plundering for stalagmitic concretions to "decorate" nearby tourist and health resorts. Much the same unsatisfactory situation applies to the Dellucs' study of Villars (1974 : 10-11) [and see their comments on La Martine (1983 : 30, note 9)].

In several localities in the cave of Hornos de la Peña are natural "holes" in the ceilings which are filled with mud. Several of these mud-filled cavities are many metres higher than the tallest human standing upright on either the modern or the ancient floor level. A few of these cavities have had holes "jabbed" into the mud either with finger(s) ore more probably with some implement(s) (Pls. 10, 11). Other caves are reported to have similar clay-filled natural holes, some also with artificial punctures and it is argued (Clot and Cantet 1974 : 90-91) that they are likely to be of palaeolithic date. It is as inconceivable to us today to understand the nature of such action be it that such activity took place 30,000, 3000, 300 or 3 years ago (and we are unable to prove at which date this activity did in fact take place).

Investigation of the "use" of a cave, not only through the study of footprints, but also by studying breakage of concretions, re-use/retouch of artistic representations, etc. is a means of discovering such social usage and contexts – and is a thoroughly legitimate parameter of enquiry. Unfortunately, in the case of Hornos de la Peña, continual neglect of the evidence contained in the cave has led to the contextual evidence having become virtually indecipherable (Hornos de la Peña is, of course not exceptional in these regards (see e.g. Villars : Delluc and Delluc *et al* 1974 : 46-47, note 9; Montespan : Bégouën and Clottes 1988). It is clear that the history of interest in Hornos de la Peña is a complex one [but not more so than some other caves like La Martine (Delluc and Delluc 1983 : 9, notes 2, 3)] and this was, indeed, one of the reasons for the choice of this cave for study – how successful would we be in the recognition and determination of the age of engraved lines ? At Hornos de la Peña there are the occasional indicators such as the newly discovered "bearded (rein) deer with open mouth" (Fig. 9, Ucko 1987 : Pl.8) partially under (datable ?) stalagmitic flow and the (non-existent ?) "bison head" identified by Breuil but really consisting of an amalgamation of pre-1911 AD initials and natural cracks (Ucko 1987 : Fig. 26). However it must remain true to say that where a cave has remained open, and where previous unrecorded conservation and preservation activities have been carried out, so the problems of being able to correctly date particular engraved lines become acute (see e.g. the whitewashed area of Hornos de la Peña; Vialou (Vialou 1979 : 71) for Aldène; and the Dellucs (1983 : 21) for La Martine and (1981 : 76-77) for Comarque.

These particular problems of Hornos de la Peña serve to highlight the advantages and deficiencies of specific techniques of recording but they also focus attention on many of our preconceptions about palaeolithic art in general. In some cases the points raised are relatively obvious ones. Thus the fact that the newly discovered reindeer (Fig. 9), mentioned above, was under stalactitic flow may incline one to assign a palaeolithic date to "signs" under stalactite but the fact that there are such non-natural marks under such concretions does not, of course, determine whether they were made by man or by bear/bat/ etc.

Less obvious, perhaps, and certainly a result that we did *not* expect was the recognition that despite our own experience we continued to be uncertain in some cases as to what were natural and what were artificial lines. We had devised a technique of recording by tracing which clearly differentiated between lines which we took to be artificial and those that we took to be natural, and we carried out some random cross-checking of our tracings by having the same cave representation recorded on more than one occasion. Throughout the two seasons of intensive tracing work there were always some lines about which it was difficult to be precise [and see the problems at La Magdeleine (Rouzaud *et al* 1989) and Bois du Cantet (Clot and Cantet 1974 : 75)], but it was only in comparing the tracings of several seasons that it became clear that confident ascription to both "artificial" and "natural" had occurred about the *same* cave lines (Ucko 1987 : Fig. 28 d, e, and see Rouzaud *et al* 1989). Subsequent microscopic analysis of the lines in question did not solve the problem.

It would be tempting, and also reassuring, to be able to claim that such uncertainties applied only to rare examples of faint lines in inaccessible areas of the cave often covered by subsequent stalagmite flow. Unfortunately this is not so, for uncertainty still surrounds one of the most accessible of representations in the daylit entrance of Hornos de la Peña. Below the evident left hoof of an animal (Ucko 1987 : Pl. 1) are two lines about which we are still undecided as to whether they are *artificial* (and weathered) or weathered *natural* cracks. Nor are problems confined always to engravings, for we find it impossible without destructive sampling, to always distinguish between faded black paint and natural stain (Clottes 1988 : 296 for similar problems at the cave of Mayrière Supérieure; Delluc and Delluc (1983 : 34) for even worse problems at La Martine). In Hornos de la Peña, where paintings were evidently few (Pl. 12), there are numerous black marks whose nature still remains obscure but most of them are located in Gallery B which also contains a relatively clear painted depiction of a horse (Pl. 13). Clottes (personal communication, 1984) maintains that experts are still unable, in the case of Roquecourbère, to distinguish with any real degree of certainty between the "modern" (forged) line and the really old. The same is true of the engraving in Hornos de la Peña described above (Ucklo 1987 : Fig. 14) which, at least today, is amongst the most evidently visible of all the representations. Is it really possible that neither Del Rio nor Breuil ever saw it and, if they did, why did they not bother to even mention it in their publications ? There is undoubtedly the need for a sympathetic study of those representations which the older authorities, such as Breuil, failed to recognise or accept as palaeolithic (see e.g. Delluc and Delluc 1971 : 245).

In the specific case of Hornos de la Peña there is an added major problem, for we suspect that misguided "conservation" activities in the past have resulted in the (unrecorded ?) smearing of walls containing disfiguring "modern" inscriptions with a mud and "plaster" wash, thus making the identification of line qualities even more difficult than before. Of our c. 140 discrete configurations, mentioned above, we would not be surprised if up to 90 were caused by human activity *since* 1906 and many of these "scratches" were almost certainly due to careless exploration and ill-conceived conservation measures.

The final point which I would have liked to attempt to disentangle was, therefore, how far the choice of the unit of analysis at Hornos de la Peña from 1971 to 1973 as the individual *motif* as *forced* upon us as the parameter of investigation (because of the treatment that the cave had suffered over the thousands of years) and how far it was chosen because we started with a research design that set out to test that prehistoric cave art was a system of communication with

one or more structures of its own which allowed messages to be "read" and "conveyed" to those of the same cultural and perceptual background. Retrospectively this is almost impossible to do, for our aims went much wider than this single parameter of enquiry. What *is* clear, however, is that the analysis of palaeolithic cave art as a communicating system of ordered *themes* does not require the same sort of detailed recording as does the analysis of such a system which is assumed to be based on ordered *motifs* (which is the parameter which we chose to adopt at Hornos de la Peña). It is equally true that the level of detailed recording required by those who believe that recognition of different styles of *individuals* is possible in cave art is infinitely greater than that demanded by the *above* parameters of enquiry (whatever the chosen unit(s) of analysis). It is certainly the very accurate recording of *all* individual *artificial* markings on cave walls/floors/roofs which can give the best evidence of actual use of the art (especially evidence of "retouching" whether in palaeolithic cave art in general (Rouzaud *et al* 1989; Marshack 1984; Lorblanchet *et al* 1973), in Hornos de la Peña itself (Del Rio, Breuil and Sierra 1911 : Pls. 7, 8, 56), or in other rock art complexes (Ucko 1977). There is also no doubt that it is only through the continual accurate checking of individual representations within so-called "categories" of artistic expression that the occasional "revelation" may occur with regard to assumed prehistoric technological activity [whether this be about palaeolithic negative hand prints or about Australian freehand painting versus composite stencil work on rocks (Walsh 1983)].

I have tried to show in the preceding pages that strategies for research, and the assumed relevant accompanying skills and levels of recording, are most often based on the assumptions of the investigators regarding the significance of the art works which are being investigated. I have also suggested, through the example of Hornos de la Peña, that, at the same time, particular research strategies are sometimes forced on the researcher by the nature of the data itself as well as by considerations of time and expense involved. In the case of Hornos de la Peña our original set of parameters for investigation involved enormous expense and time. Practical aspects of money and time also pose a major dilemma for palaeolithic art studies in general and undoubtedly continue to encourage investigations which do not focus on the very accurate recording of all individual lines or the attempted determination of what is natural and what is artificial/human or artificial/animal. There is a certain poignant irony to my conclusion that *most* of the ongoing work on palaeolithic cave art is not carried out in depth, that such partial recording work is best suited to research strategies which assume that palaeolithic art was a communication system based on *thematic* content, but/and that most of those who are actually carrying out such recording work do not in fact necessarily believe at all that palaeolithic cave art was a system designed for public communication and consumption.

Whatever else may have been elucidated, I think that one message is beyond dispute – it is certainly timely that we take control of our destinies as experts about palaeolithic cave art. To do so our main job in the future may have to be to persuade and convince administrators, Ministers of Culture, UNESCO, and other agencies, of the length of time needed to *adequately* record palaeolithic parietal works of art. We do not deserve to succeed in this endeavour until we can *agree* on the *variety* of our aims and until we can specify the necessary levels of details required by our recordings relative to each of those research strategies which we can confidently associate with the particular aims of specified palaeolithic cave art enquiries.

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This article originated in a draft manuscript written in two parts, the first by myself (Department of Archaeology, Southampton) and the second at my request by Dr Robert Layton (Department of Anthropology, University of Durham); it was, at that time, intended only to be a half-hour verbal communication for a colloquium in Périgueux, France, in 1984. However, by the time of the colloquium itself I had expanded my part of the manuscript into a full-length article. Some 50 copies of the article, translated rapidly into French, were circulated at the session on "Techniques et Méthodes". I had also prepared a short version of the paper, in English and French, which was presented to the colloquium. Subsequent to the colloquium, Robert Layton has published his views elsewhere (1985, 1987) and I received various comments and letters about my presentation at the Périgueux meeting. As a result I significantly altered various parts of the previously circulated short article and added various bibliographical references. This was then published in Spanish in *Cien años despues de Sautuola*, edited by M. González Morales, 1989, Santander, Diputación Regional de Cantabria. This present article is a slightly updated and revised version of the Spanish publication, prepared especially at the request of Jean Clottes for inclusion in English in this volume. I welcomed the above request since the distribution of the Spanish publication seems to have been limited in the extreme, and the recognition of the effects of subjectivity in the recording and interpreting of data remains of the utmost importance to future research undertakings.

I would like to thank especially the following for their comments since the Périgueux colloquium : Prof. A Beltrán, Dr. J. Clottes, Ms. J. Hubert, Dr. M. Lorblanchet, Dr. G. Sauvet, Prof. F. Soleilhavoup, Prof. M. Weaver. I also wish to acknowledge those who assisted me during the preparation of the original version of this chapter, especially Prof. De Bellos, Dr. M. Lorblanchet and Dr. S.J. Shennan. In particular I acknowledge my great debt to Dr. R. Layton and Ms. J. Hubert, many of whose comments and views have been incorporated in the main body of this text, and I must also mention Mr. M. Curtis, without whose assistance all the notes of the work at Hornos de la Peña would have remained in chaos.

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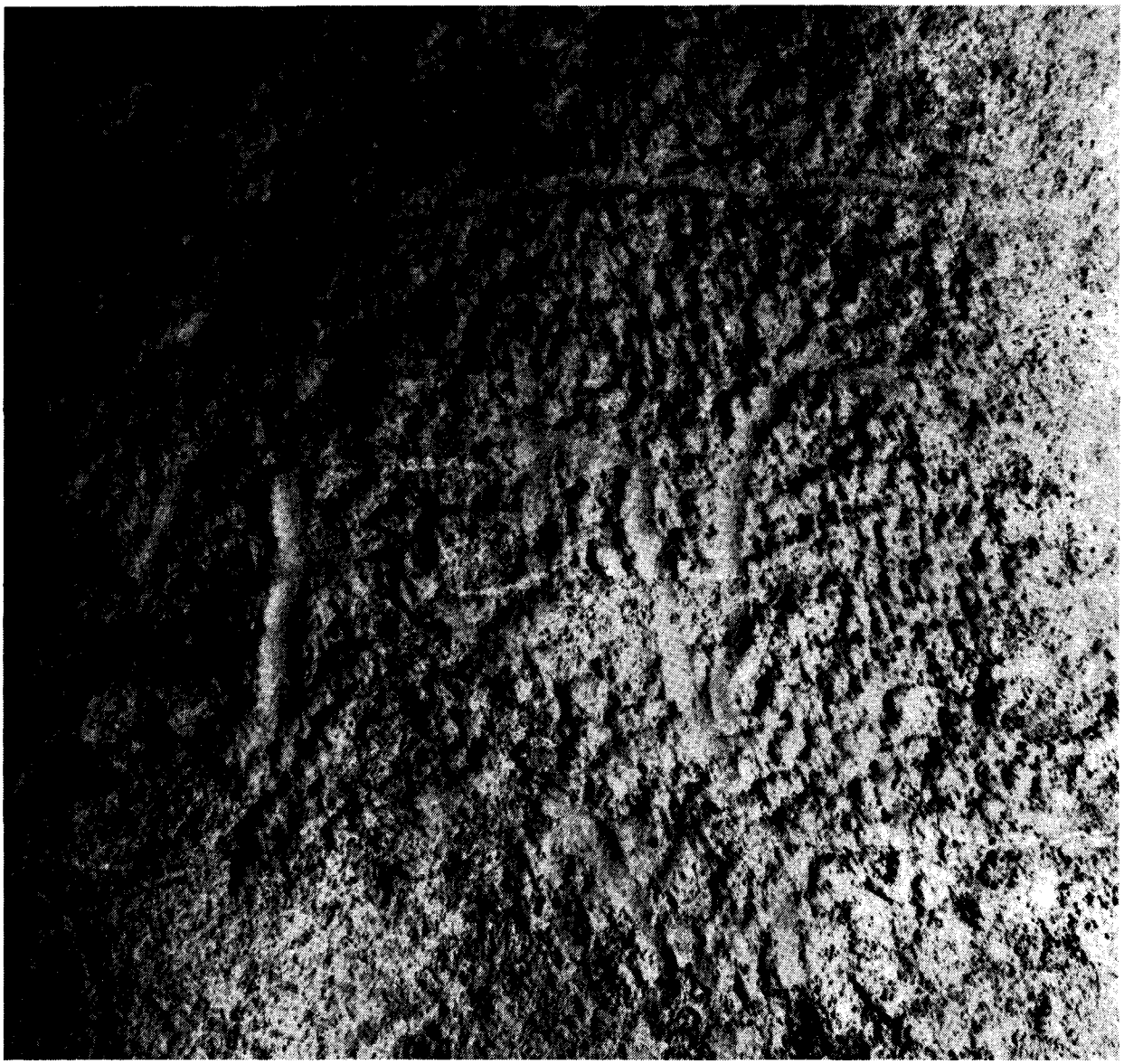
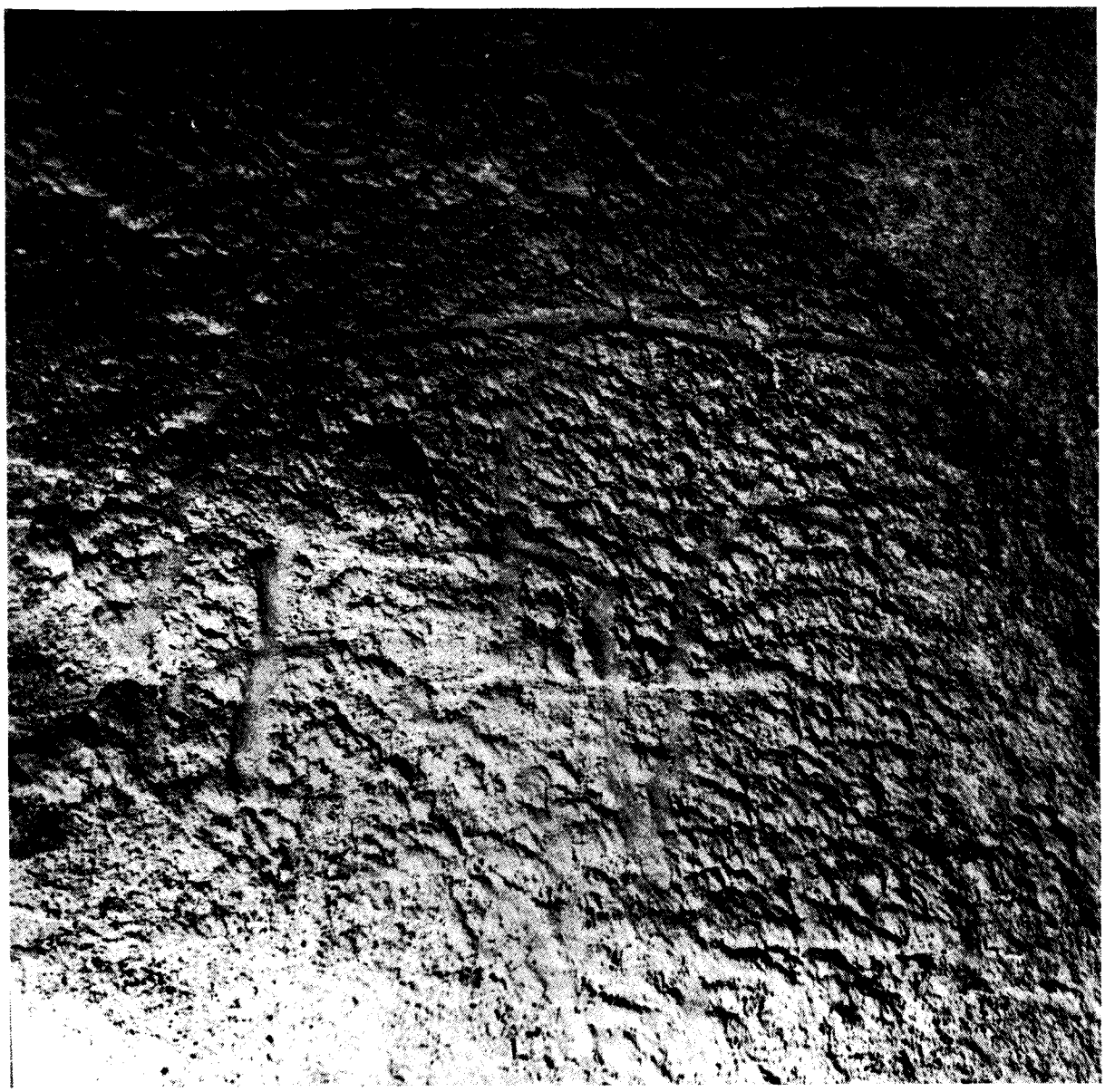


Pl. 1
Finger tracing of "ox", with photograph apparently showing "arrow" stuck in its flank (Ucko location 128).



Pl. 2
Finger tracing of "ox", with photograph apparently showing line crossing through flank and continuing outside the line of the body.

Pl. 3
Finger-traced meanders (Ucko location 11).

Pl. 4
Engraved "snake" (Ucko location 102).

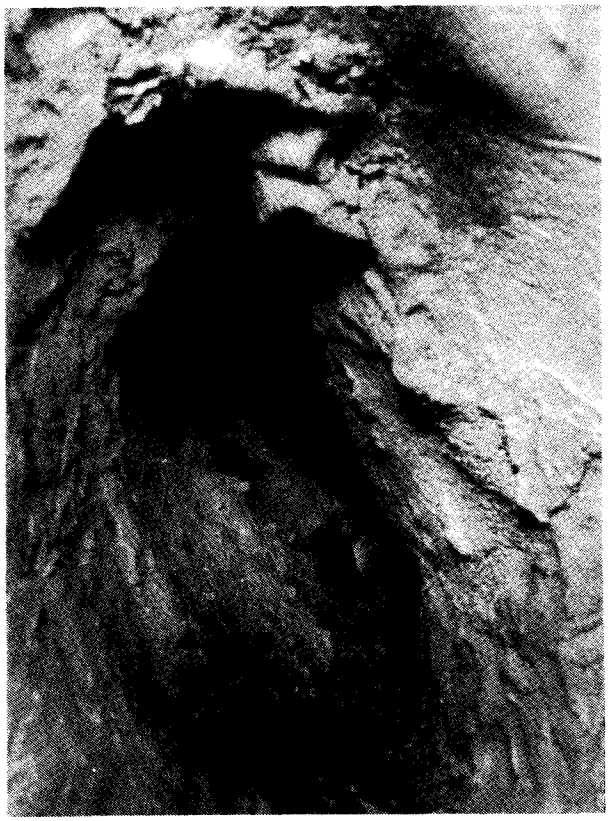
Pl. 5
Engraved "anthropomorph" and some of the lines in the area (Ucko location 96).

Pl. 6
Engraved "loop" (Ucko location 19).

Pl. 7 – Engraved complex of apparently non-representional lines (Ucko location 93).

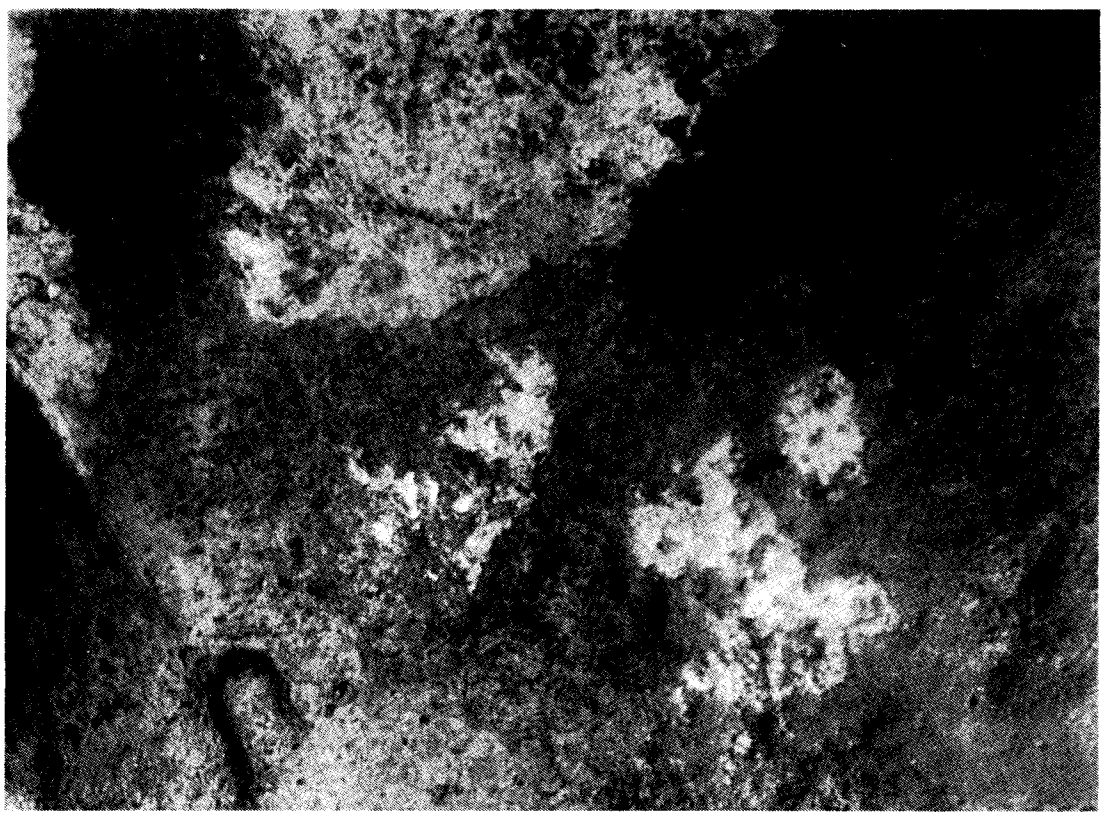
Pl. 8
Finger-traced and engraved "grid" (Ucko location 60).

Pl. 9
Finger-traced "grid" surrounding natural cavity (Ucko location 101).



Pl. 10 & 11
Natural cavities filled with mud into which holes have been apparently "jabbed" (Ucko location 57 and 107).

Pl. 12
Possible painting of dorsal line of an animal and small "tectiform" (Ucko location 26).



Pl. 13
Black painted horse (Ucko location 137).

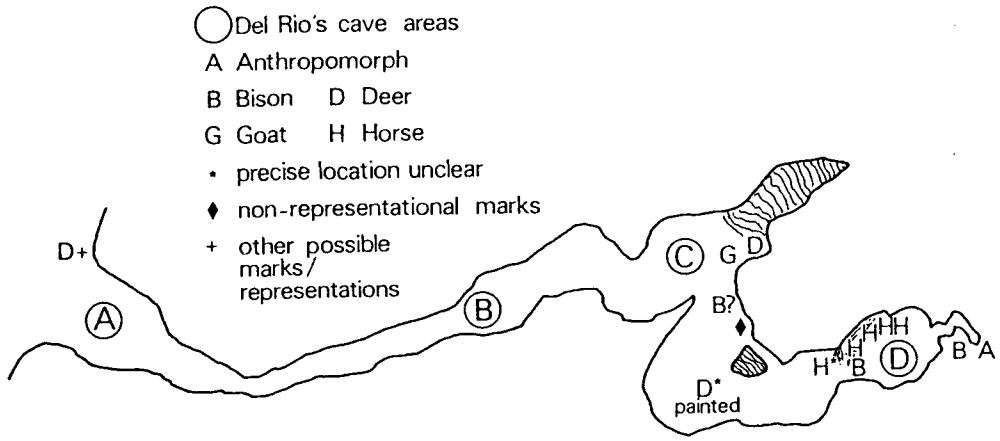



Fig. 1
Del Rio's distribution of animal and anthropomorph representations (1906, Pl. I).

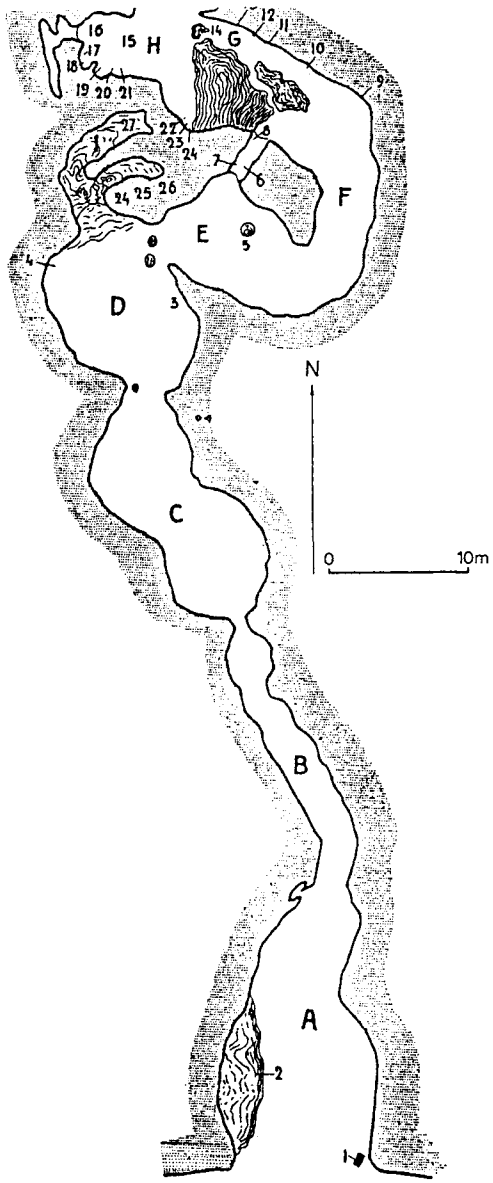



Fig. 2
Breuil's location of representations (Del Rio, Breuil and Sierra 1911, Fig. 83).

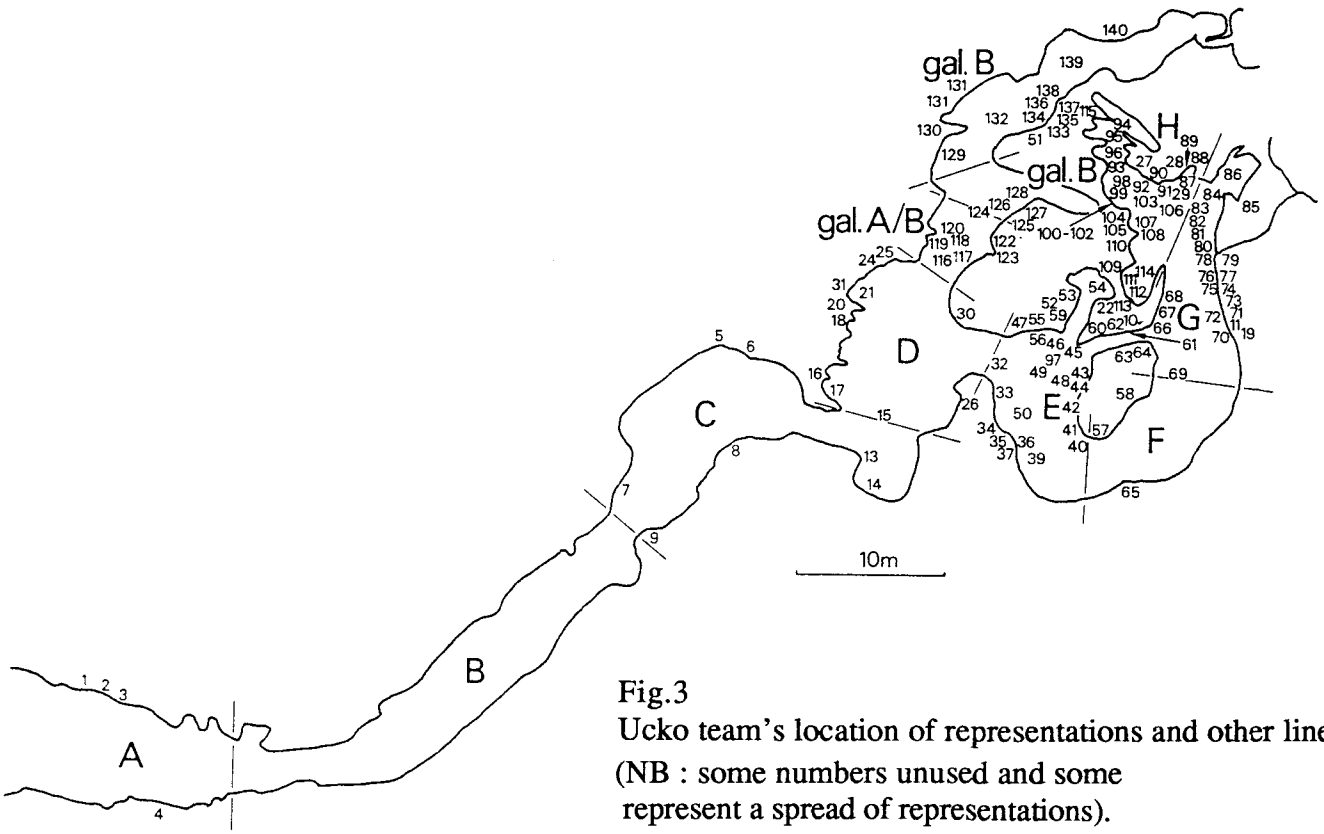



Fig.3
Ucko team's location of representations and other line
(NB : some numbers unused and some
represent a spread of representations).

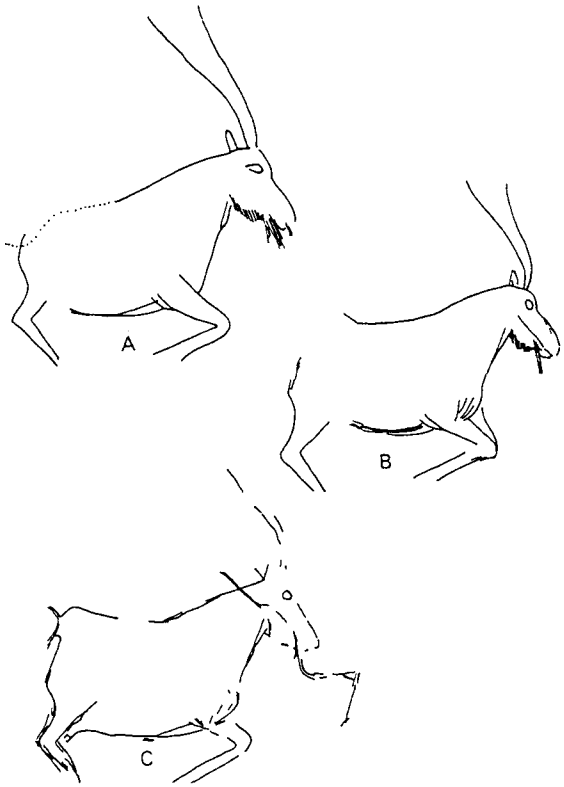



Fig. 4
A & B : two interpretations by Breuil (Del Rio, Breuil and Sierra 1911) of an ibex with beard.
C : Ucko team's tracing omitting beard because of identification of the lines as bat scratches.

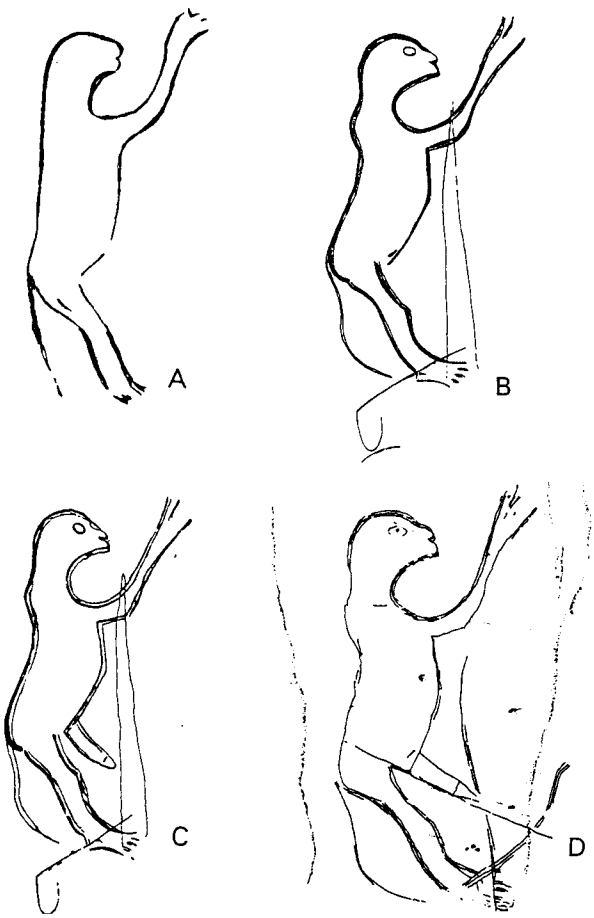



Fig. 5
Engraved "anthropomorph". A : Del Rio 1906. B : Del Rio, Breuil and Sierra 1911. C : Breuil 1952 and D : Ucko team.

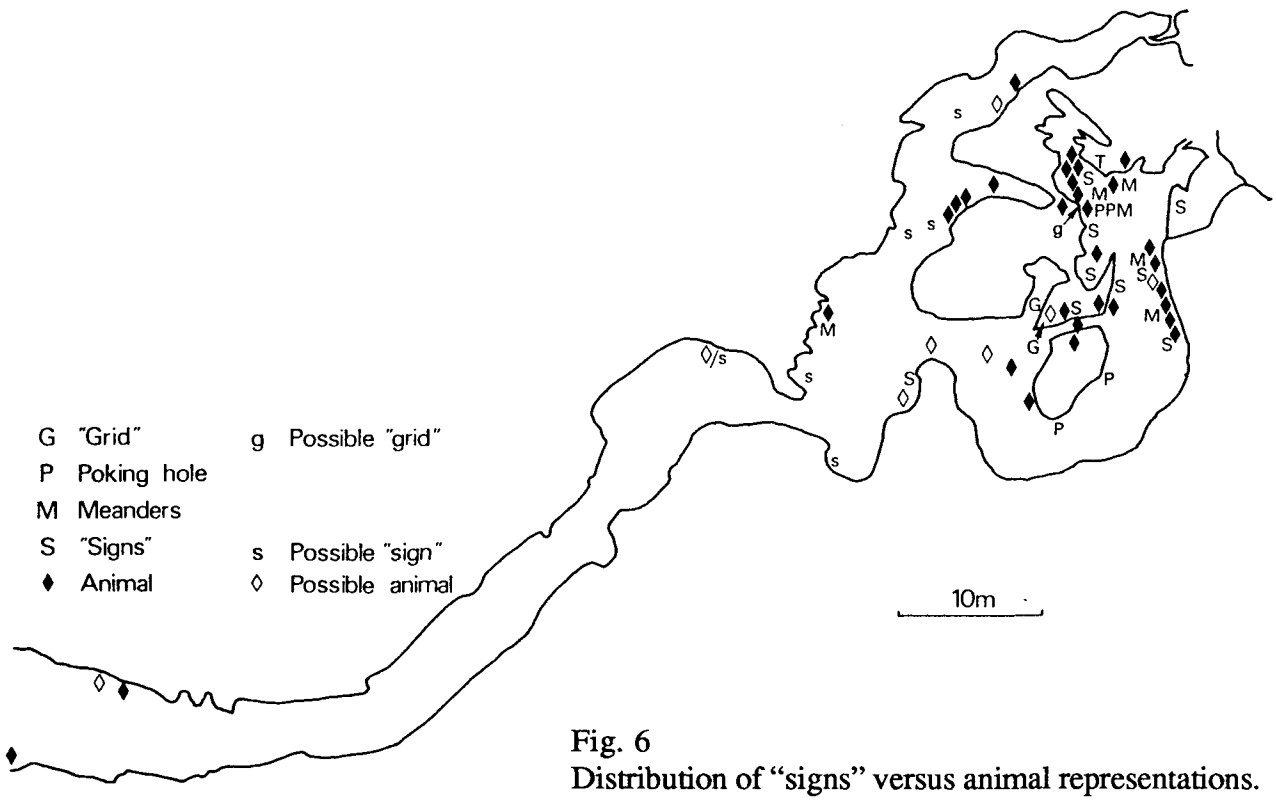



Fig. 6
Distribution of "signs" versus animal representations.

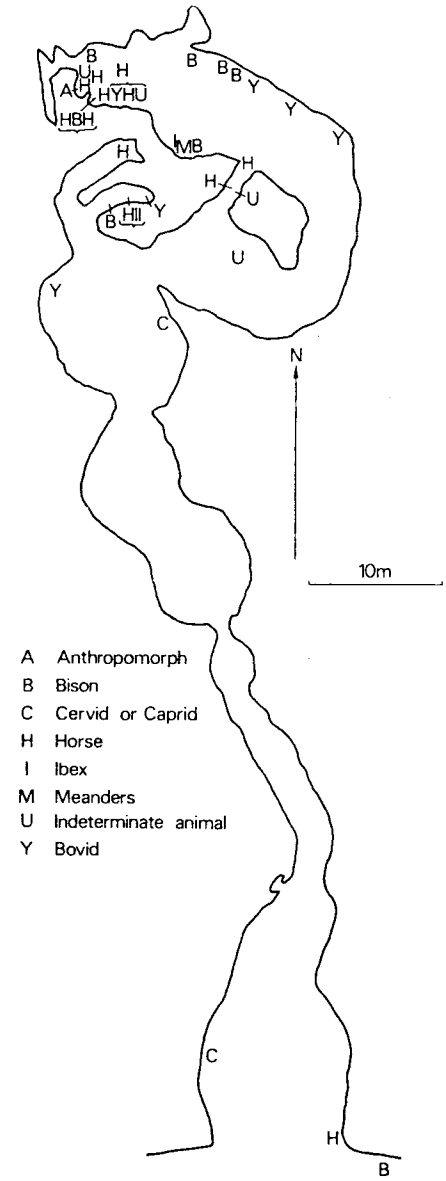



Fig. 7
Distribution of Breuil's identifications.

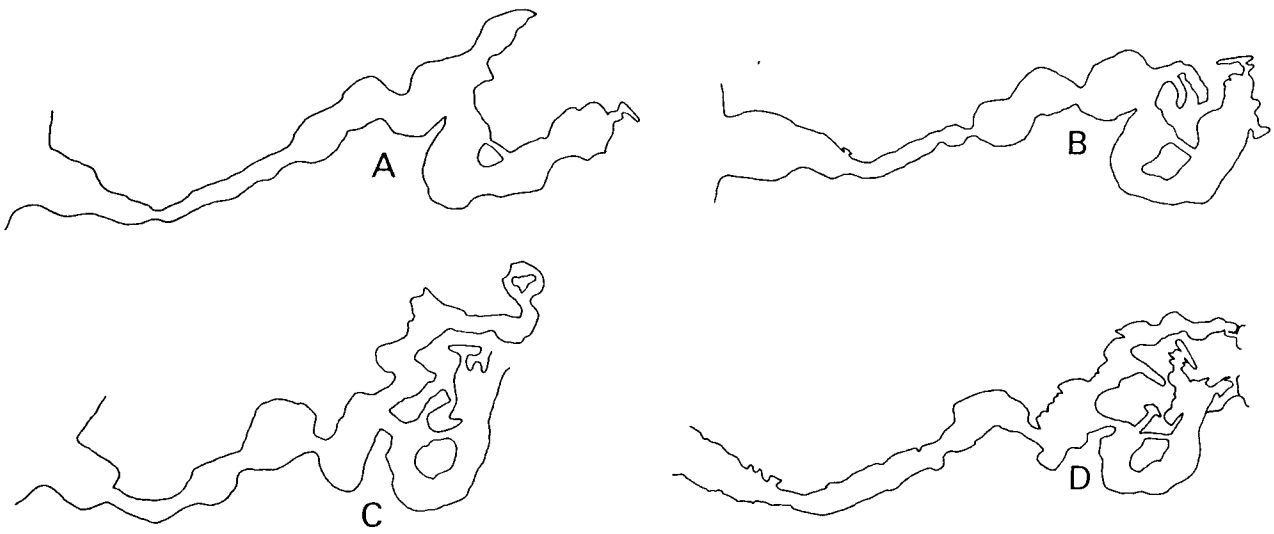



Fig. 8
A : 1906 Plan (Del Rio 1906). B : 1911 plan (Del Rio, Breuil and Sierra 1911). C : 1919 plan (unpublished). D : Ucko team plan (1971-1973).

Fig. 9 : Engraved (rein)deer with open mouth (Ucko location 67).