On a Pleistocene human occupation at Pedra Furada, Brazil

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The last decades of fieldwork have not decisively upset the long-held view that the settlement of the Americas occurred in the very latest Pleistocene, as marked in North America by the Clovis archaeological horizon at about 11,200 years ago, and by a variety of contemporaneous South American industries. Yet there are several sites that may prove to be older, among them Pedra Furada, in the thorn forest of northeastern Brazil, a large and remarkable rock-shelter, whose Pleistocene deposits have been interpreted as containing clear evidence of human occupation.

This paper offers a considered view of Pedra Furada from three archaeologists with a wide range of experiences in sites of all ages in the Americas and elsewhere, but who also share a special interest and expertise in the issues Pedra Furada has raised: Meltzer from long study of the peopling of the Americas and the frame of thinking within which we address that issue (Meltzer 1993a; 1993b); Adovasio from his intensive excavations and analysis of the Meadowcroft Rockshelter in Pennsylvania, the prime North American pre-Clovis candidate (Adovasio et al. *1990; Donahue & Adovasio 1990); and Dillehay from his work at the Monte Verde site in Chile, a site in which extraordinary preservation has produced a rich archaeological record with radiocarbon ages in excess of 12,500 years b.p. (Dillehay 1989a; in press). At the invitation of the Pedra Furada team, the three travelled to Brazil last December to participate in an international conference on the peopling of the Americas, and see first-hand the evidence from Pedra Furada.*

Introduction and *caveats*

In a review of the problems and controversy surrounding the peopling of the Americas, Guidon & Arnaud (1991: 177) very rightly suggest, 'Working parties, meetings of specialists on site, and formal debates, should take place regularly if we are to establish an agreed basis for evaluating evidence.' It was in that spirit an invitation was graciously extended to us to visit Toca do Boqueirão da Pedra Furada and participate in the *Reunião Internacional Sobre o Povoamento das Américas* in São Raimundo Nonato, Brazil, in December 1993. It was also in that spirit we accepted the invitation.

While we returned from Brazil greatly impressed by the scope of the work at Pedra Furada, we also returned without having been convinced of the site's claims for a Pleistocene human antiquity. This is *not*, we hasten to add, a final judgement about the site; that must await the appearance of Parenti's unpublished dissertation on the material remains (Parenti 1993b), and the summary monograph(s) on the site. It does, however, reflect concerns we have about the chronology, geology, artefacts, features, and related aspects of the purported Pleistocene human occupation at Pedra Furada.

Of course, we are not experts on the data and evidence recovered from Pedra Furada; our knowledge of the site is based on presentations we heard at the Conference, two site visits (and visits to six other apparent Pleistocene sites in the region), and a cursory inspection of the recovered material, supplemented by a reading of the available site literature. Nor do we expect our opinions will be shared by our col-

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leagues (even those who viewed the site with us); we understand only too well how other individuals or groups may see the same evidence differently.

We are also well aware of the potential *appearance* of bias on our part from two of us having our own pre-Clovis candidates. We will let our paper speak for itself in this regard, but trust the issue of bias will be found to be groundless. After all, we have nothing to gain by showing Pedra Furada is — or is not — as old as it is claimed to be. This is not a competition in which only one site can 'win' and others must 'lose'. Each pre-Clovis claim is independent; the age of one has no bearing on the age of another (Meltzer 1989). It matters not to us whether the first Americans arrived 11,000, 20,000 or 50,000 years ago, or whether one or all of these sites are accepted. What matters is understanding the virtually unprecedented migration of modern humans across a rich, empty and dynamic Pleistocene landscape, of which solving the question of *when* it occurred is but the first step toward that understanding (for a discussion of these larger issues, see the papers in Dillehay & Meltzer 1991).

We would like to contribute towards that solution, for we consider ourselves to have a useful knowledge of the difficulties encountered in the excavation of potentially early records, especially in caves and rock-shelters, and in the identification of unifacial stone tool industries and possible human-made features. Adovasio and Dillehay have confronted such matters before at Meadowcroft and Monte Verde. All of us, further, are acutely aware of the long and complicated history of evaluating these sometimes controversy-laden records. Thus, our views and comments might be of some interest to our colleagues and, perhaps, of some value.

Ours is not the first commentary to be offered on this site. Several (mostly) brief assessments have appeared: some pro, some con, others withholding judgement until more first-hand information is available (e.g. Ardila Calderón & Politis 1989; Bahn 1991; 1993; Bednarik 1989; Fagan 1990; Lynch 1990; Schmitz 1987); the more partisan of these have sparked testy exchanges (e.g. Bahn & Muller Beck 1991; Fagan 1991). We have deliberately steered clear of this literature, and will neither summarize nor take sides on it. Our purpose is to provide as constructive an assessment as possible of the evidence from Pedra Furada, from our own particular vantage as participants in the debate, who have also had the opportunity to examine and discuss in detail the site's evidence now that the work and analysis are nearing completion.

Given Adovasio and Dillehay's own experience with commentators on their sites, and their natural empathy for one in Guidon's position (who, like them, never sought early sites, nor intended to get involved in the peopling of the Americas controversy), these comments on Pedra Furada are not offered lightly. Indeed, we gave the matter considerable thought before doing so. Under the circumstances, however, it seems incumbent on us to do so: this is putatively the oldest known site in the New World, and as such deserves discussion, especially by those who have had the opportunity to visit the site and view its material remains. Moreover, because of historical scepticism toward early sites (Grayson 1988; Meltzer 1989), the case for any claim can only be *strengthened* by exposing the roots of the scepticism. Finally, as Guidon has noted on several occasions, frank and (we intend) constructive discussion is the best way to bring closure for or against any purportedly early site (in this regard, Adovasio and Dillehay can testify from personal experience that Pedra Furada is *not* being singled out for unprecedented criticism). Thanks to Guidon, we began that discussion in December of 1993 in Brazil: this paper continues the process.

Brief background

Excavations at Pedra Furada took place over a decade, beginning in 1978, and to date the available primary literature on the purported Pleistocene occupation levels at the site consists of a series of relatively brief and preliminary reports on the excavation, the burgeoning radiocarbon list, general stratigraphic descriptions, comments on the lithics and features — including arguments for their human origins, and (in the more recent publications) responses to critics (e.g. Guidon 1986; 1987; 1989; Guidon & Arnaud 1991; Guidon & Delibrias 1986; Parenti 1993a; Parenti *et al.* 1990). The detailed and comprehensive reports on the site's geology,

FIGURE 1. *The Pedra Furada site area, seen from across the valley. The archaeological site is not visible in the photograph, but occurs at the base of the escarpment.*

stratigraphy and material remains have yet to appear, though (as noted) are in progress.

The site (FIGURE 1) is located in the semi-arid *caatinga* (thorn forest) of northeast Brazil (Piauí), in the re-entrant of a massive, south-facing, sandstone rock-shelter, 70 m wide, at maximum 18 m deep (the perpendicular distance from the drip-line to the rear wall, in line north–south in Guidon & Arnaud 1991: figure 2), which was filled with nearly 5 m of deposits. Those deposits slope from east to west on a 10° angle, and from the front to the rear of the shelter.

At both ends of the rock-shelter are chutes that carry material down on to the site; included in that material are quartzite cobbles which occur in a conglomerate layer approximately 100 m above the shelter floor (FIGURE 2). Along the shelter wall the chutes are marked by pronounced manganese staining, indicative of prolonged and intensive, if episodic, water flow. Visible at the base of the east (higher) end chute is a substantial talus of broken cobbles, many of which had suitably sharp edges for potential use. At the base of the west end chute, several pot-holes are visible (each is over roughly 1·5 m in diameter; one, partially obscured by a cement column put in place to support a walkway, appears to be several metres in diameter). These pot-holes undoubtedly formed as plunge pools scoured out of the bedrock.

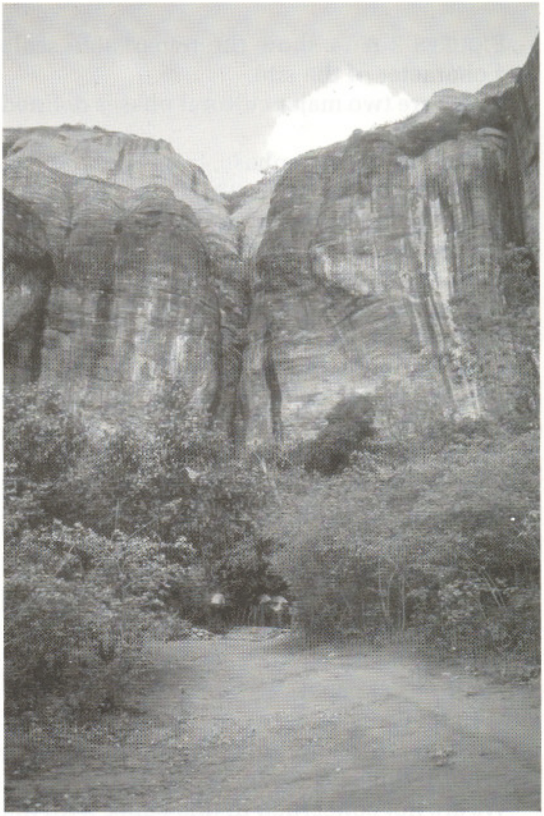

FIGURE 2. *The approach to the site itself.*

The uppermost (light-coloured) layers visible in the cliff are the conglomerate layers which are the source for the quartzite cobbles in the site.

FIGURE 3. *A horizontal view looking towards the eastern end of the site. It shows the catwalk (which is roughly at the base of the rock-art layer), the excavation areas (showing the area in which some 5 m of sediment were removed), and the remaining witness columns (fronted by cobbles). Note the bedrock on the left, and the roof block fall (roughly marking the drip-line) on the right.*

FIGURES 3 & 4 show the remarkable scale and character of the site.

There are two major cultural phases defined at the site.

The *Pedra Furada* phase, from >48,000 b.p. to 14,300 b.p. (Parenti 1993a: table 2), is characterized by the debris of artefact manufacture and simple tools made of locally occurring quartzite and quartz. There has not been a great deal of discussion regarding activities of this period, save for Guidon's (1987: 10) remark that the site was a temporary camp for 'rock painting, flaking and retouching of rock, and cooking and eating of food'. The Pedra Furada phase deposits lack bone, wood, or other organic remains, save for pieces of charcoal. There are hearths and features from this phase, though these are reportedly more diffuse and less well defined than those in the later phases of occupation.

The later, *Serra Talhada* phase, post-dates 10,400 b.p., and includes artefacts of both local quartzite and exotic chert, abundant rock art, and — we generalize from this and other sites in this area — very pronounced and well defined hearths and anthropogenic 'occupations' or living surfaces.

We are not concerned in this paper with the Serra Talhada phase material, except for occasional comparative purposes; our focus is on the Pedra Furada phase.

Although our visit to the site occurred long after excavations ceased — not the ideal time to view a site (Dillehay 1989b) — the excavation was not backfilled and two stratigraphic witness sections remain. We assume, in our comments on the stratigraphy and geology of the site, that these witness sections are representative of the site deposits. Of the several general stratigraphic diagrams that have been presented (see Bednarik 1989; Guidon 1986; Guidon & Arnaud 1991; Guidon & Delibrias 1986), none shows the complete stratigraphic sequence as interpreted at the site, and are to varying degrees only preliminary in nature; hence, we have not reproduced then here. We understand detailed and final stratigraphic sections have been prepared, and are forthcoming (Parenti pers. comm.)

Radiocarbon chronology

The excavations at Pedra Furada have produced a total of 55 radiocarbon determinations of which 46 are currently accepted (TABLE 1; Parenti 1993a; pers. comm.); 32 of these are in the Pedra Furada phase. The Pedra Furada phase is further divided into three sub-phases. The sub-phases and their ages are: PF1, from 48,000 to 35,000 b.p.; PF2, from 32,160 to 25,000 b.p.; and PF3, from 21,400 to 14,300 b.p. (see TABLE 1, and Parenti 1993a: 307–8).

These sub-phases appear to be based almost entirely on patterns in the radiocarbon sequence or, more properly, on hiatuses within that sequence. The sub-phases seem largely unrelated to the lithostratigraphy at the site (as described in publications or that we observed). This well explains why the number and radiocarbon ages for sub-phase boundaries as currently defined differ from those published earlier (compare the chronological divisions above with those in Guidon 1986 and Guidon & Delibrias 1986: 769, where the Pedra Furada phase was divided into four sub-phases with different boundary ages). Obviously, as additional radiocarbon ages were obtained, their overall pattern changed and so did the sub-phase definitions. This also explains why radiocarbon determinations previously assigned to one sub-phase are now assigned to another; Guidon & Delibrias (1986: 769), for example, assign two determinations to PF1 (Gif-6652 and Gif-6653) that are now assigned to PF2.

There is a further element of arbitrariness in the phase and sub-phase definitions: PF1 is separated from PF2 by a hiatus of 2840 radiocarbon years, and PF2 is separated from PF3 by a hiatus of 3600 years (Parenti 1993a: table 2). Yet hiatuses of comparable duration also occur *within* the sub-phases. For example, there are hiatuses *within* PF1 of 4400 and 3000 years (between 47,000 and 42,600 b.p. and between 38,000 and 35,000 b.p., respectively). Both of these are longer gaps in the radiocarbon sequence than the hiatus *between* PF1 and PF2. Why these gaps were not used as the basis for sub-phase divisions is unclear.

Because of this approach, it is difficult to accept the assertion that the sub-phases are based on '*granulométrie* et *de leur contenu en charbon*' (Parenti 1993a: 306, emphasis ours). The sub-phases are clearly *not* anchored in distinct lithostratigraphic units; for example, the base of two of the cultural sub-phases (PF1 and PF2) appear to be marked by major spalling episodes and/or erosional surfaces with lag deposits. Yet the base of the third sub-phase (PF3) and the upper boundaries of all three sub-phases are marked only by hiatuses in the radiocarbon sequence (Guidon & Arnaud 1991: figure 3). Hiatuses between the three sub-phases are not hiatuses in observed depositional processes. These sub-phases are apparently or nominally 'cultural' rather than chronostratigraphic *sensu stricto*.

The apparent granulometric underpinning for these sub-phases is problematic. According to Parenti (pers. comm. 1993), the site was

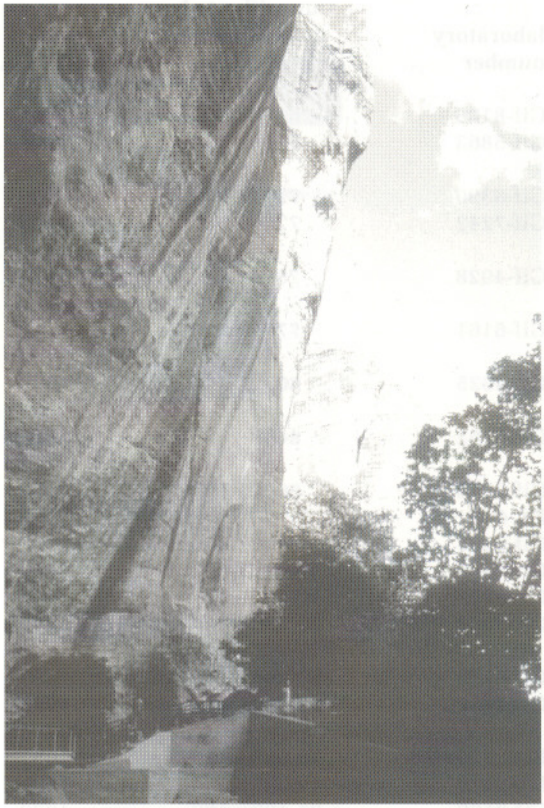

FIGURE 4. *Taken from the same position as* FIGURE 3, *this is the vertical view, to show the scale of shelter face.*

The lowermost portion of this photograph is the part shown in detail in FIGURE 3. *It still does not show the upper section of shelter wall (which is obscured by the overhang). The human figure (Adovasio), standing by the talus cone at the east end of the site, bottom centre, gives a human scale.*

excavated in arbitrary levels that were then grouped (and re-grouped) into sub-phases based on the presence of features and datable charcoal. Grain-size data was then calculated by sub-phase from discontinuous and essentially arbitrary sediment samples representing unconnected episodes or 'moments' in a lithostratigraphic continuum. The samples are therefore not necessarily related to discrete depositional events in the history of the shelter.

In effect, the criteria used to create the phases are a mix of radiocarbon determinations and a few lithostratigraphic contacts (but mostly radiocarbon determinations); yet neither alone provides a clear definition of either

laboratory number	uncalibrated determination (years b.p.)	year of research	stage	sample context[2]
Gif-8108	6150± 60	1987	ST2	East sector, unit 1
Gif-5863	6160± 130	1978	ST2	West 78 sector, unit V 60–80 cm below 0
Gif-8390	7220± 80	1987	ST2	East sector, unit 1
Gif-7242	7230± 80	1982	ST2	West 82 sector, unit III Zone A
Gif-4928	7640± 140	1978	ST2	West 78 sector, unit X 90–105 cm below 0
Gif-6161	7750± 80	1982	ST2	West 82 sector, unit II (–80 cm)/Zone A
Gif-4625	8050± 170	1978	ST1	West 78 sector, unit XII 152–171 cm below 0
Gif-6162	8450± 80	1982	ST1	West 82 sector, unit VII (–69 cm)
Gif-8350	8600± 60	1987	ST1	East sector, unit 2(1)
FZ-436	9506+135/–132	1982	ST1	West 82 sector, unit VII
Gif-8351	9800± 60	1987	ST1	East sector, unit 4(1–2)
Gif-8389	10,040± 80	1982	ST1	West 82 sector, unit VII
Gif-8352	10,050± 80	1987	ST1	East sector, unit 4(3)
Gif-5862	10,400± 180	1980	ST1	Sondage 2(80) upper part of the shelter
Gif-6159	14,300± 210	1982	PF3	West 82 sector, unit XVIII
Gif-5397	17,000± 400	1980	PF3	West 80 sector, unit 178–192 below 0
Beta-22086	18,310± 190	1987	PF3	West 87 sector, unit –1
Gif-8125	19,300± 200	1987	PF3	East sector, unit 5(3)
Gif-6160	21,400± 400	1982	PF3	West 82 sector, unit XVII
Gif-5398	≥25,000	1980	PF2	West 80 sector, unit 203–210 below 0
Gif-5648	≥25,000	1980	PF2	West 80 sector, unit 192–203 below 0
Gif-6147	25,200± 320	1982	PF2	West 82 sector, unit XX
Gif-8353	25,600± 450	1987	PF2	East sector, unit 5(3)
Gif-5963	26,300± 600	1982	PF2	West 82 sector, unit XIX (–258 cm)
Gif-6309	26,300± 800	1983	PF2	West 83 sector, unit XIX (–303 cm)
Gif-5962	26,400± 500	1982	PF2	West 82 sector, unit XIX (–268 cm)
Gif-6308	27,000± 800	1983	PF2	West 83 sector, unit XIX (–340 cm)
Gif-8354	29,740± 650	1988	PF2	East sector, unit 7(8)
Gif-6651	29,860± 650	1984	PF2	West 84 sector, unit XIX (II)

the geological units or the cultural phases as defined.

For that matter, the cause of these hiatuses (within and between sub-phases) is clouded. Given that the sequence is not tied to actual lithostratigraphic events (though such events are present), it is difficult to attribute these hiatuses to palaeoecological events in the history of the area; to structural events in the history of the shelter; or, possibly, to human activity — such as groups creating a possible use-floor. To a degree, the sequence of radiocarbon determinations may also reflect patterns in the excavation and radiocarbon sampling, and be related only tangentially to prehistoric natural or cultural activity at the site. Ultimately, the meaning and integrity of the phases and sub-phases as currently defined at Pedra Furada, and the rationale behind them, is unclear.

laboratory number	uncalibrated determination (years b.p.)	year of research	stage	sample context[2]
Gif-6041	31,500± 950	1982	PF2	West 82 sector, unit XIX (–268 cm)
Gif-6652	31,700± 830	1984	PF2	West 84 sector, unit XXI
Beta-22085	31,860± 560	1987	PF2	West 87 sector, unit –2
Gif-6653	32,160± 1000	1984	PF2	West 84 sector, unit XXIII(I)
Gif-9019	35,000>(–27,92/1000)	1988	PF1	East sector, unit 13(1)
Gif-9018	35,000>(–26,97/1000)	1988	PF1	West 88 sector, unit 13(4)
Gif-9020	38,000>(–28,02/1000)	1988	PF1	East sector, unit 13(1)
Gif-9021	38,000≥(–27,81/1000)	1988	PF1	East sector, unit 13(1)
Beta-22858	>39,200	1987	PF1	West 87 sector, unit –3
GifTan-89357	39,500± 1600	1988	PF1	West sector, unit 14(2)
Gif-7619	40,800+4420/–1850	1987	PF1	West 87 sector, unit –3
Gif-8355	41,000+3000/–2200	1988	PF1	East sector, unit 13(2)
Gif-7681	41,500+4200/–3100	1987	PF1	West 87 sector, unit –4
GifTan-89097	42,400± 2600	1988	PF1	West sector, unit 14(1)
GifTan-89354	>42,600	1988	PF1	East sector, unit 13(1)
GifTan-89098	≥47,000	1988	PF1	Trench 6, unit 9(8)
GifTan-89265	>48,000	1988	PF1	East total sector, unit 14(1)

1 In cases of discrepancy among data sources, cultural phase designations follow the most recent work (e.g. Parenti 1993a).
2 The term 'unit' in the sample context column refers to a *vertical* unit (from the Portuguese *camada* or *nível*).

not accepted				
?	8080± 120	1982	?	West excavation/3
Gif-6436	8170± 80	1978	?	Unit I
?	10,454+114/–112	1978	?	West excavation/5
Beta-22859	10,540± 350	1987	?	West sector/2
FZ-433	13,989+167/-164	1984	?	West excavation/5
Gif-6158	23,500± 390	1982	PF2	Unit XIX (–249 cm)
Gif-6654	28,600± 600	1984	?	Unit XXV
Beta-22831	>37,350	1987	?	East sector, unit 6
Gif-8124	>38,000	1988	?	Trench 6/6

? denotes information unavailable. Gif-6158 is from Guidon & Delibrias (1986: table 1). The remainder are from the unpublished 'Lista de datações dos sítios do enclave arqueológico de São Raimundo Nonato'.

TABLE 1. *Radiocarbon determinations from Pedra Furada, adapted from Parenti (1993a: table 2); for earlier, less complete listings see Guidon & Delibrias (1986: table 1), and Guidon & Arnaud (1991: table 1).[1] The latter part of the table lists the nine radiocarbon determinations that are not accepted by the site investigators. These were identified by comparing the list given in Parenti (1993a: table 2) with the unpublished master list of radiocarbon ages from the site, 'Lista de datações dos sítios do enclave arqueológico de São Raimundo Nonato' (dated 1993), provided to the authors and used with permission.*

There are no obvious or major reversals in the gross radiocarbon column; however, Parenti reports several radiocarbon determinations run by BETA Analytic were out of sequence, and a total of nine radiocarbon determinations have been rejected (pers. comm. 1993). Granting the horizontal and vertical complexity of the site, as well as the observable complexity of its apparent depositional episodes, it is vital that there be a detailed discussion of the horizontal and vertical position of the charcoal samples, particularly relative to the hearths and artefacts (accompanied by comments on why certain determinations were rejected).

The 46 accepted radiocarbon determinations from the site do represent a large corpus of radiocarbon ages; in fact, there may be more from this site in apparent stratigraphic order than

FIGURE 5. *The matrix in the witness column at the eastern end of the site. This particular section shows portions of the PF2 layer. Note the coarseness of the fabric and the several large fractured cobbles in place.*

are available from any other site in South America. By themselves, the age determinations appear to be reliable and valid. We saw no obvious sources or mechanisms of contamination. The charcoal fragments we observed clearly appear to be wood charcoal; Adovasio's very cursory examination suggested the charcoal might come from several species (but obviously such needs to be followed up).

Ambiguity arises, however, in regard to the origin of the charcoal. In such a semi-arid region, brush fires are an obvious natural source of charcoal, and we are concerned whether the charcoal is truly anthropogenic. After all, the shelter was regularly open to receive windblown charcoal from external fires, *or* possibly from fires within the shelter itself, *or* from fires occurring on the uplands above which could have been readily transported down the chutes to the site itself. Guidon & Arnaud (1991: 176) dismiss such concerns, observing that today the *caatinga* vegetation 'burns only with difficulty', and because the site charcoal is concentrated in hearths and occurs mostly inside the shelter and not outside the drip-line. We are unconvinced by this response. Even if brush fires are uncommon in the *caatinga* today, were they uncommon in the Pleistocene vegetation surrounding the site? That question has not been answered. Moreover, excavations were rather limited outside the drip-line; it is unclear from the publications or the extant sections how discrete the placement of the charcoal was within the features; and there is no reason to suppose charcoal could not have been carried toward the rear of the shelter by natural agencies.

Further, we saw little in the stratigraphy to convince us the charcoal was anthropogenic. In one of the two witness sections on the site, the Pedra Furada phase charcoal 'lenses' were thick and diffuse, quite unlike the discrete lenses and hearths visible in the younger (Holocene) Serra Talhada phase. Nor do the Pedra Furada phase charcoal 'lenses' resemble the very discrete (and occasionally quite thick) fired phenomena we have seen in our extensive experience with dry and wet caves and rockshelters where large-scale fired floors exist. In fact, the charcoal 'lenses' in the Pedra Furada phases appear like those formed by non-human agencies, such as wind and water action.

To help resolve this ambiguity over the source of the charcoal, it would be useful to know precisely how many of those radiocarbon ages are aggregate determinations representing averages from several samples of dispersed charcoal, as opposed to determinations on single chunks of charcoal. It would also be useful to know whether and how many individual features — as opposed to dispersed lenses of scattered charcoal — were radiocarbon dated.

Until all these matters are resolved, it is difficult for us to preclude the possibility the charcoal was non-anthropogenic, and introduced by natural means. What we might have at Pedra Furada is a stratigraphically correct sequence of natural fires — in which case the hiatuses in the radiocarbon sequence may indeed have palaeoecological significance.

Macrogeology, microgeology, and site emplacement processes

Although we did not observe the underlying shale bedrock, Pedra Furada seems to be a typical re-entrant rock-shelter, although a very large example. The large blocks on the sterile floor or basement of the site may represent either initial re-entrant activity or very early roof collapse, as opposed to wall attrition.

There apparently has been no effort to study the lithology of the cliff face itself, although casual examination of that face indicates distinct facies with clear granulometric 'signatures' exist within the sandstone (which appear to represent discrete point or channel bar episodes). The lithology of these facies looks sufficiently distinctive to allow 'fingerprinting' of the rock-fall episodes; doing so would have been useful for determining the source, intensity, duration and timing of specific spalling events, both major and minor, in the long history of the shelter. Such would also provide a better context for evaluating the apparent artefacts and features in the deposits.

Our observations of the witness sections indicate the matrix of the deposits is remarkably coarse (FIGURE 5). Observed clast sizes range from medium and very coarse sand through gravel, cobbles and boulder-sized materials with a curious absence of finer sand-sized and smaller materials. These observations are supported by the available published granulometric data (e.g. Guidon & Arnaud 1991) which clearly show, discontinuous though they are, a preponderance of coarse materials throughout the sequence. While the local sandstone cement is silica, and grain-by-grain attrition will therefore be lower than in corresponding calcium-carbonate cemented shelters, the absence or scarcity of 'fines' at Pedra Furada suggests the possibility the deposits may have been substantially reworked by water after deposition (see below).

The extant witness sections reveal a minimum of five major geological strata separated from one another by apparently continuous (insofar as are still visible) interfaces or contacts which are marked, in some cases, by concentrations of cobble-sized materials. These interfaces represent changes in the depositional regime of a presently unspecifiable nature. The interfaces with a significant cobble or boulder-sized component may reflect heavy spalling episodes *or*, given the radiocarbon hiatuses that correspond to these interfaces, lag deposits resulting from extensive fluvial erosion and reworking. The source of the water for this erosion is not any creek or stream; as Guidon notes (1989: 641; Guidon & Arnaud 1991: 174) the site lies 19 m above the valley floor. Instead, the source is presumably water that flushes down the chutes, especially at the eastern (up slope) end of the shelter. Judging by the manganese staining on the shelter walls, and the erosion of the cliff face, these chutes have carried large volumes of water in the past. It would useful to map the size of the catchment in the uplands that drains into these chutes.

The interface between the uppermost Pleistocene deposits (their PF3) and the lowermost Holocene units (the base of the Serra Talhada sub-phase) is relatively clear-cut in the witness section. For that matter, the Holocene deposits here and at other shelters in the region unambiguously show the appearance of human activity, marked by obvious anthropogenic surfaces.

The sources of the fill which make up the major geological units defined at the site appear to be reasonably clear-cut. A large percentage of the sediments represent direct attrition from the roof and walls of the shelter. A substantial contribution came as well from the overlying quartzite-laden gravel bars, two of which occur 100 m above the site. While there was certainly the potential for the accumulation of limited amounts of finer sediments from attrition, their near-total absence makes it difficult to establish their relative contribution to the sediment pile. Likewise, an aeolian component may be present, having come in continuously or sporadically throughout the history of the site; the amount of the aeolian contribution cannot be quantified at this time, though we suspect that, at least in volume, it

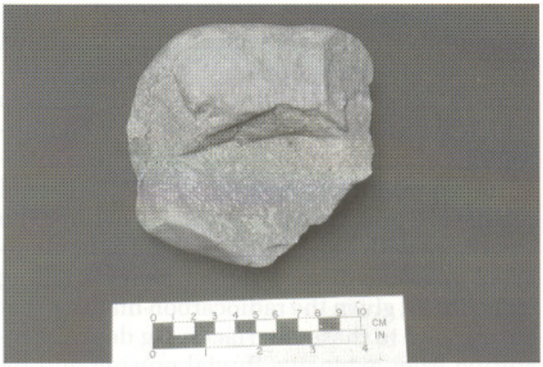

FIGURE 6. *A purported artefact from the PF 2 levels.*

FIGURE 7. *A purported artefact from the PF 2 levels.*

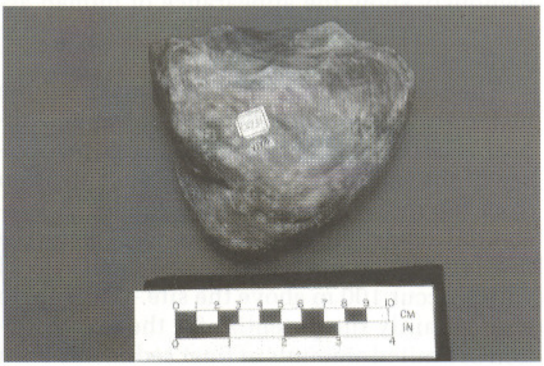

FIGURE 8. *A purported artefact from the PF 1 levels.*

is relatively minor (although perhaps important in bringing charcoal into the shelter).

Other potential sources of sediment include colluvial materials introduced via the chutes

FIGURE 9. *A purported artefact from the PF 1 levels.*

FIGURE 10. *Close-up of the edge of the specimen in* FIGURE 9.

— as opposed to rock spalls from the shelter ceiling. Both free-fall and water transport of cobble- and boulder-sized materials provide natural flaking mechanisms of considerable power, a point to which we will return.

According to the excavators, the macrostratigraphic units at the site were excavated without attention to any internal stratification. Indeed, they suggested microstratigraphy was either absent or unimportant in the formation of the sediment pile. Our own inspection of the witness section indicates that each of the major macrostratigraphic units is eminently capable of subdivision into microstratigraphic episodes or events. Perhaps this would explain why there are several thousand-year hiatuses in the radiocarbon sequence within the macrostratigraphic units (above) — those radiocarbon hiatuses may well correspond to undetected stratigraphic changes.

Were the microstratigraphy known, it would be possible to tease out discrete concentrations of putative features and artefacts. Without it, it is virtually impossible to associate any single artefact with any structure on the site or any microstratigraphic lens or possible surface. Likewise, it is impossible to link any artefact with any radiocarbon age. However, if the purported artefacts, features and radiocarbon samples were piece-plotted during excavations, it might be possible to 'reconstruct' (albeit imperfectly) the surfaces on which these materials were recovered, and so establish the randomness or non-randomness of those associations. That may buttress, but cannot prove, that these associations are more than mere geological co-occurrences within a many-thousand-year macrostratigraphic unit.

An examination of both the macro- and micro-stratigraphy in the witness section indicates there is a rearward slope to the sediments. Such a slope likely formed behind the distinctive drip-line that presumably, though not demonstrably, exists around the entire margin of the site, and as the lee side of the well-pronounced talus accumulation at the base of the east end chute. The talus does not appear to form a symmetrical cone, but instead an asymmetrical one, in which the long axis dips toward the western end of the site (because of the overall 10° slope from east to west). That long axis would be roughly bell-shaped in cross-section, and thus gravity would naturally carry a percentage of the cobbles (and other debris) that fell on to the talus toward the rear wall of the shelter. This has a bearing on the claims for artefacts.

Artefacts (FIGURES 6–10)

There is some discrepancy regarding the number of artefacts in the Pedra Furada phase; we will follow the recent counts by Parenti, which put the total at 595 specimens (Parenti 1993a: table 3; compare the larger counts in Guidon & Delibrias 1986). *All* the artefacts reported in the Pedra Furada phase are made of quartzite, the source of which is the internally stratified conglomerate gravel bar that occurs 100 m directly above the site, and was directly connected to the site via the chutes at either end of the shelter. Under the circumstances, we must ask whether these specimens are truly artefacts, as opposed to geofacts (*sensu* Haynes 1973) — naturally flaked stone created when quartzite cobbles eroded out of the conglomerate and fell 100 m to be flaked and fractured on the shelter floor. Judging by the distance of the fall, the velocity that would be reached over that distance (roughly 45 m/sec, which is considerably higher than that usually achieved by humans flaking stone, e.g. Speth 1972: 45), and the pile of flaked quartzite cobbles present in the talus and witness section, these chutes have been and are veritable geofact factories.

Unfortunately, to date there has been no explicit discussion of the criteria used in the field *during the excavations* to recognize artefacts amidst the coarse matrix of broken quartzite cobbles that comprise the site matrix, and whether those criteria were used consistently throughout the excavations. We do know, because the evidence is visible in the remaining witness sections, that such sorting decisions had to have been made almost constantly, since these alleged artefacts were selected from amidst countless broken cobbles.

To pursue this question of how 'artefacts' were sorted from non-artefacts, we were shown and subsequently made a cursory examination of the excavation backdirt piles that occur in the brush beyond the shelter drip-line. We did so to see what had been discarded as 'non-artefacts'. Picking through the backdirt revealed many stones that, put together, formed a continuous sequence from unbroken cobbles to ones slightly flaked to ones that had sharp edges and looked like chopping tools. This certainly heightened our concerns about how artefacts were defined, how they were distinguished from naturally fallen and fractured stones, and what percentage of all the broken rocks on site these alleged 'artefacts' represent. Are the 'artefacts' truly different in kind from naturally flaked rocks? Or were they merely one end of a larger continuum with those that were naturally flaked?

We do not wish to belabour the point, but some of these specimens we found in the backdirt were remarkably similar in form, size, flaking pattern, and had equally sharp edges, as many of the specimens on display at the Conference.

While there has been little or no discussion of the criteria used to sort purported artefacts from geofacts in the field, Parenti later developed an explicit set of criteria for identifying

estimated amount of time it took 1000 rocks to fall	estimated rock-fall rate (rocks/yr)	estimated number of rocks that would fall over 50,000 years
5 years	200 rocks/yr	10,000,000 rocks
10 years	100 rocks/yr	5,000,000 rocks
50 years	20 rocks/yr	1,000,000 rocks
100 years	10 rocks/yr	500,000 rocks

TABLE 2. *Models of rock-fall into Pedra Furada over 50,000 years.*

number of rocks that fell over 50,000 years	number of potential geofacts given production probability of ·01	·001	·0001	·00001	·000001
10,000,000 rocks	100,000	10,000	1000	100	10
5,000,000 rocks	50,000	5000	500	50	5
1,000,000 rocks	10,000	1000	100	10	1
500,000 rocks	5000	500	50	5	0 ·5

TABLE 3. *Possible geofact production at Pedra Furada over 50,000 years.*

sample size	probability of event	expected number in a sample = 1000	estimated sample required to detect a single specimen
1000	0·01	10 ·00	100
1000	0·001	1 ·00	1000
1000	0·0005	0 ·5	2000
1000	0·0001	0 ·1	10,000
1000	0·00001	0 ·01	100,000

TABLE 4. *Adequacy of a sample size of 1000 for detecting rare events.*

artefacts. These criteria were devised after the excavation and after the first (and more considerable) sorting of 'artefacts' from naturally flaked stone was already complete. These *post hoc* criteria were applied to a relatively small sample of specimens Parenti had in Europe for detailed study and drawing while producing his dissertation (Parenti pers. comm. 1993). Parenti was confident about the artificial status of the specimens in this collection; he was non-committal about the artificial status of those specimens not studied by him (pers. comm. 1993).

Parenti's *post hoc* criteria identified artefacts as such on the basis of

1 the number of flake scars,
2 the edge angle (<90°),
3 the pattern or 'logic' of the flake scars on the working edge, and
4 the position of the object in the rock-shelter (pers. comm. 1993).

On this last point, Parenti & Guidon argue objects near the rear of the shelter had to have been carried there by humans and were therefore manuports (pers. comm., and Guidon & Arnaud 1991: 176).

Since the identification of the Pedra Furada 'artefacts' appears to have been a two step process — 'artefacts' were selected in the field from amidst the countless rocks comprising the fill, and then a (presumably) smaller group was selected from among that initial sample — it is vitally important these two selection criteria be thoroughly explained and reconciled. To what degree, for example, would specimens initially identified as geofacts (or artefacts) be acceptable (or unacceptable) by Parenti's criteria (a question made relevant by our examination of the backdirt specimens)? And, more important, to what degree do those two separate sets of criteria *reliably* differentiate apparent artefacts from non-artefacts? Given Parenti's criteria are the more explicit and seemingly more rigorous of the two that were applied to these specimens, we will focus our discussion on them and on the actual specimens he identified as artefacts (and which were on display at the Conference).

We completely agree with Parenti and others that many of the specimens we saw on display *could be* artefacts. We agree with Pelegrin who argued at the conference that specimens like these generally would not be expected to result from natural causes. Still, he conceded — and we agree here too — that in rare circumstances naturally fallen rocks could acquire the kind of flaking seen on these specimens; in Pelegrin's estimate that would occur *less* than 1% of the time. He based this estimate on the pattern and type of flake scars observed on the specimens, and his belief in the improbability such specimens would receive multiple and apparently uniform blows from natural causes (Pelegrin pers. comm. 1993).

To counter the suspicion these specimens were merely naturally tumbled quartzite cobbles and flakes, Parenti collected and analysed 2000 stones from the talus piles that occur at the base of the east (500 stones) and west (500 stones) chutes on the site, and from a talus pile at the base of a third chute (1000 stones) just off the western edge of the site. *None* of these 2000 stones exhibited the kinds of flaking or flake patterns he observed among his sample of apparent artefacts (Parenti pers. comm. 1993).

While Pelegrin's arguments and Parenti's observations are very well taken, we must demur on several points. The issue, as Pelegrin says, is a probabilistic one: the odds may indeed be slight that nature could produce such specimens, but are the circumstances at this particular site such that even these seemingly rare events occurred often enough to produce the record of 'artefacts' that exists? Moreover, is Parenti's sample of naturally fallen stones statistically large enough to show these purportedly rare events did *not* happen?

Parenti is now gathering data on the rate of cobble-fall into the shelter, so is uncertain how long it took the original sample of 2000 naturally fallen stones to accumulate. Still, we can use that sample (or at least the 1000 stones that fell down the east and west chutes and fed directly into the site), and several inferred times of accumulation, to create models of rock-fall rate and accumulation over the 50,000 years the shelter was open (TABLE 2).

We assume, for sake of discussion, that rock-fall was relatively constant over time; this assumption is not unreasonable, since episodes of faster or slower rock-fall when time-averaged will even out. Of course, in reality there were likely distinct episodes of cobble-fall, tied to changing climatic conditions or structural instabilities in the cliff face. Any such episodes ought to be visible in geologic and stratigraphic data, and it would be useful to see whether such episodes exist and, further, whether they are correlated with the abundance of artefacts (or, for that matter, features, living-floors or radiocarbon determinations). Coarse data are apparently available on the intensity of the cobble 'rain' at the site (Parenti 1993a: 306), and ought to be so examined.

On the basis of the calculations in TABLE 2, and taking as a starting-point the probability estimate offered by Pelegrin (that nature would produce such specimens less than 1% of the time), it is clear that under certain models one would expect *large* numbers of geofacts at Pedra Furada (TABLE 3).

For example, were the probability of nature producing these geofacts 1% (·01), and were the number of rocks that fell into the shelter over the last 50,000 years as low as only 500,000 (which, given the amount of cobbles we saw in the backdirt and the remaining witness section, seems extremely low to us), 5000 geofacts would have been produced. That number is a large enough to account for all the specimens identified as artefacts at Pedra Furada.

It would also explain why no purported artefacts were seen by Parenti in the sample of 1000 in the two chutes within the site proper: at the estimated rate of production of 0·1 geofacts/yr (5000 in 50,000 years), on average only one geofact would be created each decade. If Parenti's sample of 1000 naturally fallen stones took less than a decade to accumulate, it is statistically unlikely a geofact would occur in the sample.

In fact, Parenti's sample of 1000, albeit useful as a starting point, is only adequate to detect relatively *common* events. As can be seen in TABLE 4, a sample of 1000 specimens will likely detect events that occur in probabilities larger than ·001.

However, if Pelegrin is correct and geofact production occurs less than 1% of the time, a sample of 1000 is statistically inadequate to the task of detecting such specimens. If the probability of geofact production is, for example, ·00001, then a sample of 100,000 naturally fallen rocks would be needed to ensure statistically the likelihood of detecting a single geofact. Statistics aside, it is possible geofacts could occur in smaller samples, but the odds are against it. In effect, the sample of 1000 naturally fallen rocks in the shelter cannot falsify the alternative hypothesis that the Pedra Furada specimens are geofacts.

There are several possible objections to the alternative hypothesis that these specimens are geofacts. First, Guidon argues that the specimens recovered from the rear area of the shelter had to have been carried there by people, not nature, and are artefacts by virtue of being manuports (pers. comm. 1993, and Guidon & Arnaud 1991: 176). We find this argument unconvincing, since the rocks at the rear of the shelter would have been readily transported there by nature as gravity carried them down the lee side of the long axis of the talus cone.

Second, Parenti, Pelegrin and others at the Conference suggested certain of these specimens could not be geofacts because of the large number of flakes (>3) removed from them. We cannot accept this argument either, because it assumes there were only limited opportunities for nature to flake these cobbles: when the cobbles first hit the ground after plunging down the chute; when they bounced after hitting; and when they were struck by another falling stone. Yet, while these cobbles could plunge down the chutes only once, there is no reason to suppose that once a cobble fell to the shelter floor it was not subsequently moved, or that it was not struck on several more occasions. For that matter, there is no reason to suppose only one flake was removed each time the cobble was struck. The coarse nature of the matrix comprising the shelter fill shows there was a great deal of energy in the shelter, and cobbles likely moved and were flaked repeatedly well after their initial plunge into the shelter.

A third possible objection to the hypothesis these quartzite cobbles are geofacts is that similar ones occur in the Holocene Serra Talhada phase alongside unmistakable chert artefacts, with that association in a secure archaeological context implying the quartzite specimens must be artefacts (e.g. Guidon & Arnaud 1991: 175). We also find this argument problematic for several reasons. For one, our concerns about the quartzite specimens in the Pedra Furada phase carry over to the essentially identical flaked quartzite specimens in the Holocene-age Serra Talhada phases. The Holocene phase includes specimens of flaked quartzite *and* chert; we have no doubt the Serra Talhada chert specimens, which show complicated unifacial and bifacial flaking, are artefacts. We remain to be convinced the Serra Talhada quartzite specimens are artefacts. In addition, the fact that flaked quartzite cobbles occur in the Pleistocene and Holocene levels at the site merely shows the *mechanism* producing these specimens did not change over time. It does not show what (or who) that mechanism might have been. Finally, as R.S. MacNeish observed at the Conference, if these flaked quartzite cobbles were produced by humans, they show remarkably little technological, typological or morphological change over the 50,000-year span the shelter was open: the Serra Talhada phase specimens are virtually identical to those in the Pedra Furada phase dating tens of thousands of years earlier.

It is difficult to account for an absence of culture change over 50,000 years, save to suggest that perhaps the site was used solely as a quarry, and lithic reduction strategies remained unchanged. But that supposition seems highly unlikely, if not inexplicable (granting we are dealing with *Homo sapiens sapiens*). In sharp contrast, one would expect little variation in the flaked quartzite cobbles from the Pedra Furada and Serra Talhada phases, were they all created by the same *natural* processes. Geofacts might vary owing to changes in the conglomerate layer(s) serving as the cobble source, changes in the geometry of the chute, episodic intensity of cobble fall, the density of the cobble layer below and so on, but such variation would be far less than one would expect were the cobbles flaked by 2500 generations of human hands.[1]

For the moment, then, we cannot accept the claim that the flaked quartzite cobbles at Pedra Furada are artefacts; indeed, the weight of evidence and reason forces us to assume (until proven otherwise) that these specimens are geofacts.

Features

There were four discrete types of features (structures) reported at Pedra Furada: hearths marked by the presence of heated stone or charcoal; stone-bordered hearths (in which the stone is on the surface or inset into the ground); cuvette hearths; and stone structures with no evidence of heating (Parenti pers. comm. 1993). Save for the cuvette hearths, which occur only in the Holocene layers, these features are reported from throughout the Pleistocene deposits. According to Parenti (1993b: table 14), there were 87 structures in the three Pleistocene-age levels (Parenti 1993a: 308 reports 86). As was the case with the artefacts, based on plan views of the structures that we saw illustrated at the Conference (none was visible in the witness sections), we agree that many *could be* due to human agency.

But as was also the case with the artefacts, these purported artificial features were defined against a backdrop of naturally occurring cobbles (the size of the cobbles in the features is no different from the size of the non-humanly moved or modified cobbles in the surrounding matrix). High-energy fluvial action over and through the sediment pile was more than capable of sorting natural accumulations of clasts and cobbles into arrangements that mimicked anthropogenic features.

The key issue, as with the purported artefacts, is one of definition: how were the features isolated in the field and their boundaries drawn relative to the surrounding matrix and context? Pictures of the features when they were initially uncovered would be beneficial, along with views of the profiles and cross-sections of these features. It is also necessary to address whether features contain discrete charcoal distributions and, equally important, whether charcoal also accumulated in and around clusters of unpatterned stones (that is, clusters not identified as features). If so, what did the latter look like? What was their horizontal and vertical distribution? What criteria were used to define them as non-cultural? Can any of the archaeologically excavated clusters be replicated in control areas outside the shelter or in other shelters where natural fires occur?

On a related note, sub-phases PF1, PF2, and PF3 had a total of 20, 51 and 16 features, respectively. Perhaps not coincidentally, those same sub-phases produced 196, 273 and 126 artefacts (Parenti 1993a: table 3). Clearly, the number of features and the number of artefacts co-vary through the sequence. It appears, based on limited evidence published in Parenti (1993:

1 Of course, if these specimens were artefacts, we expect some evidence of use-wear. During the Conference, one of us (TDD) had the opportunity to carry out a cursory microscopic inspection (using a portable Bausch & Lomb) of the sharp edges of 10 stone artefacts, including three specimens identified as choppers and others as large flakes. No discernible use-wear in the form of edge-crushing, flaking and micro-fracturing were revealed at 50x magnification. Granted, this was an unsystematic examination of a very small and statistically unrepresentative sample. Granted, too, it is very difficult to produce use-wear on quartzite, especially if the artefacts were used as expedient tools. However, if the choppers had been utilized, even minimally, some damage should be present along some edges (which makes this situation unlike the case of the falling rocks, in which there is the expectation that geofacts mimicking artefacts will be quite rare). This matter ought to be pursued vigorously with a larger and representative sample.

That was not possible under the Conference circumstances, but as a check on the observations, Dillehay carried out a cursory and unsystematic experimental use-wear study of five broken and sharp-edged quartzite pieces which either had been discarded by Pelegrin (who flaked several cobbles) or broken naturally. The unused edges of these non-archaeological stones first were inspected under the microscope for any damage. Undamaged edges were then used to chop and slice wood (this work was carried out in the plaza outside the Conference site). Each stone was subjected to roughly 200 chopping and 400 cutting strokes. Microscopic inspection of these experimental stones revealed noticeable damage in the form of crushing (collapsed ridges, piled grains of loosened quartz crystals, in-filling of crevices with loose grains, etc.) on the edges of the choppers, and of slight edge rounding and occasional nicking and micro-fracturing on the edges of the flakes. None of these attributes were observed on the edges of the purported artefacts from Pedra Furada.

Again, these are merely suggestive results from a very cursory study, and cannot be taken to imply use-wear was absent on all the Pedra Furada specimens. Post-depositional agents (chemical wear, water percolation, micro-exfoliation etc.) may have worn away discernible evidence on the particular specimens we examined. Use-wear on quartzite edges may also be too difficult to detect. Nonetheless, we remain puzzled that none occurred on the purported choppers — artefacts in which damage would be most severe, and thus most easily detected.

figure 1), that the sedimentation rate in the shelter co-varies with the numbers of features and artefacts as well. That is, as the amount of roof fall and colluvial debris increases, so apparently does the number of features and artefacts. The features reportedly decreased in size up through the sequence (Parenti *et al.* 1990: 36), although the published data are too limited to reveal whether the *éboulis* and colluvial clast sizes also show this pattern of size decrease through time. We cannot say, therefore, whether the *size* of these natural and purportedly artificial products co-vary as well. However, if, on closer inspection, the amount and/or size of naturally deposited material correlates with the amount and/or size of the features and artefacts, it would strongly suggest non-human agencies lay behind their production.

There has so far been relatively little discussion of the spatial patterning of features or artefact clusters in the shelter. In our experience, living-floors within shelters show use patterning, with horizontally discrete living areas or activity areas that might be tied to the microenvironment of the shelter (the preferred use-zone might correspond to the driest or warmest portions of the shelter, for example). Not all parts of a shelter, especially one as large as Pedra Furada, will be used in the same way, nor would one expect it to be used uniformly across its full extent. Were there primary use-zones within the shelter of Pedra Furada? Did those change through time? Did the kind and type of feature vary across the shelter? Do the features correlate with natural lag surfaces within the shelter — and thus become explicable by natural agencies and not artificial ones?

Such questions about intra-site spatial patterning for all phases must be resolved, not just to help clarify the origins of the structures, but also to help explain their origin. As before with the charcoal and the artefacts, we are not saying the features at Pedra Furada *are* natural, but the site geology and hydrology makes this a very likely alternative explanation, and certainly one that must be investigated and shown *not* to have been a factor.

Excavation methodology

The excavation methodology employed at Pedra Furada, and apparently at the other closed shelter and cave sites in the São Raimundo region, seems to have been directed at defining the gross geological sequence vertically, and to delimiting features horizontally. Less effort was apparently directed toward identifying or defining discrete potential living surfaces (which may have been difficult to define in the shelter), and the associations of artefacts with each other, or with features.

Significantly, all of the major excavation units at Pedra Furada originated within the drip-line. The effort was apparently not made to breach the drip-line/talus-cone deposits and the colluvial slope material beyond the shelter overhang. Doing so would have provided details on the geological history of the site, and should have enabled the excavators to distinguish more effectively between culturally modified and culturally unmodified surfaces.

Excavation methods appear to have largely employed shovels and pick mattocks rather than trowels and smaller tools; this severely handicapped the detection of microstrata during excavations. Those methods would also have made it extremely difficult to identify discrete stone-flaking episodes (as were claimed to have occurred at the site), or to link those episodes with specific floors or dated material. The possible human origins of this material would have been more convincing if it existed in conjoinable accumulations with vertical and horizontal integrity. While all objects of suspected human origin were evidently piece-plotted, dip and strike data were not systematically taken. This precludes, at the very least, the delineation of trend surfaces within the deposit.

While we are aware that some of the deposits were screened (apparently using mesh as fine as 1 mm), knowing the extent of the screening relative to all the deposits would help resolve questions regarding the site's material record. For example, we observed that most of the recovered specimens (or at least those on display at the Conference) seemed relatively large. If this is, in fact, a valid observation, it would be useful to know whether it reflects a lack of comprehensive screening, or, alternatively, prehistoric natural processes (e.g. water-sorting) or human activities (e.g. primary reduction of cobbles on site).

External comparisons

We were fortunate to have the opportunity to see additional sites in the region, which included other sandstone rock-shelters (Toca do Sitio de Meieo, Caldeirão dos Rodrigues, and the Perna sites I–III) and a limestone solution cave (Toca do Cima dos Pilão). As we understand it, the flaked quartzite cobbles so abundant at Pedra Furada were apparently not found in any of these other sites, although several of these had thick Pleistocene-aged deposits (it is appropriate to add that, unlike Pedra Furada, none of these sites had a quartzite-cobble layer high above them). At the Rodrigues shelter, for example, the apparent Pleistocene human presence was marked by two crossed and partially charred sticks in otherwise sterile deposits.

The explanation given for the absence or scarcity of Pleistocene material at these other sites is that Pedra Furada, as the largest of the shelters, would have been the primary magnet to local human occupants, at the expense of the other shelter sites. It is, of course, possible that the Pleistocene occupation was predominantly in open but now eroded sites in the valley bottom (Guidon & Arnaud 1991: 173), but so far these have not been detected.

Curiously, by Early Holocene times, the other and previously ignored cave and shelter sites all show convincing and abundant evidence of human presence in the form of artefacts, rock-art and so on.

Summary and thoughts for future inquiries

Obviously, we are sceptical of the claims for a Pleistocene human presence at Pedra Furada, and in our view the concerns raised here must be resolved before this potentially important site is accepted (at least by us). In the interests of furthering the debate in a constructive fashion, we have specific recommendations for resolving these concerns (these are in addition to those suggestions made earlier in the text). Some can be met with information that is undoubtedly already available.

1

To resolve the chronological questions, the lithostratigraphy, geochronology and cultural occupation(s) at the site ought to be rigorously (and independently) defined. Attention should be paid to the precise horizontal and vertical location of individual charcoal samples, and their precise position relative to any individual features and/or artefacts. Data ought to be provided on each of the specific age determinations: whether they came from individual charcoal samples, or from aggregated ones; whether they came from discrete hearths or thick and diffuse charcoal lenses; and whether samples from the hearths occur as discrete clusters within the hearths or whether their association is less distinct. Naturally, this information needs to be considered against the backdrop of how charcoal might have accumulated in the shelter deposits, whether by human or natural agencies. There should also be greater attention to the relationship of the radiocarbon chronology to the shelter's natural history. It might also be useful to discuss in detail the current sub-phases; what they — and the hiatuses between them — may represent and why: periods of intensive human activity? discrete depositional or erosional episodes? palaeoenvironmental cycles? This also ought to include detailed comments on the differences in the features (including the hearths) and apparent artefacts from the several sub-phases.

To a degree, concerns about the origin and integrity of the radiocarbon profile, and its relationship to human agency and natural depositional events, might need to be resolved by careful micro-stratigraphic excavation of portions of the remaining witness sections.

2

A full discussion of the field criteria for selection of artefacts is critical, as well as a discussion of how those initial criteria were similar to or differed from those later developed by Parenti.

Because specimens from Pedra Furada I seem little different from those of the Pedra Furada III phase, or, for that matter, from the Serra Talhada phase, it is important to assess in detail whether there are changes in the purported lithics through time and what the nature and significance of those changes are (this might include an examination of the arrangement of flake scars to see whether they overlap in patterned ways). Piece-plotting of the flaked quartzite cobbles and the indisputable chert artefacts in the Sierra Talhada levels would help clarify their spatial relationship, and thus perhaps the origin of the quartzite specimens in these later units.

Further, there should be more discussion of how the purported artefacts differ from the geofacts on the site. In order to help resolve this issue, there needs to be more information on the natural fall of material into the shelter from spalling events within the shelter and from cobble debris carried down the chutes. This can be done by looking long-term at larger samples of debris that enters the shelter. It would be useful to set up traps to catch and record the volume, character, size and rate of spalls and cobbles entering the shelter (see, for example, Donahue & Adovasio 1990). This, in turn, would provide a rough gauge for establishing yearly or longer rates of fall and accumulation that would help refine models of shelter formation, and the role of nature alone in creating that formation. More specifically, of course, this would help increase the sample of geofacts at this locality, and thus increase (or decrease) confidence in assertions about the purported artefacts on the site.

Confidence would be further enhanced by a detailed discussion of the relative frequency of purported artefacts on the site, and the degree to which that frequency rises or falls in consort with changes in the sedimentation. In addition, use-wear studies — granting the conditions noted above — need to be reported (or undertaken, as the need may be). What is the character of use-wear on these specimens? What accounts for the use-wear? Does it change through time? On the latter, of course, there must be a discussion of what changes if any occur through time in the specimens. If there are no changes, why not? If there are changes, what is their nature, and could such changes be explained by natural processes as well?

3

To resolve concerns over the origin and definition of the features, it will be important to reject the possibility they might be natural. This can be done by detailing the criteria by which features were recognized as humanly made; the process by which they were delineated from the surrounding matrix; their spatial patterning (or lack thereof); the degree to which charcoal and the purported artefacts are (or are not) clustered within or around their edges; and the degree to which charcoal and the purported artefacts do (or do not) occur independently of these structures. In essence, there must be greater discussion of the natural background of material on these surfaces.

At the same time, there ought to be greater attention to the potential cultural activities on the site. Was this a periodically occupied camp in which a variety of economic and technological activities occurred, and where one would expect to see, for example, an internally structured site characterized by large and small firepits, other features, clusters of lithic debris (including representative material from the entire reduction sequence, from cores to discarded tools showing use-wear), and periodic re-use of the site and site-furniture (hearth stones)? *Or* was this locality a quarry site, where one might instead expect a more ephemeral archaeological record, perhaps characterized by light scatters and occasional discrete clusters of selected types of lithic raw material associated with loosely structured hearths, and in which expended utilized flakes and tools are less common? *Or* did the activities (and hence the expected patterning in the archaeological record) vary over time? Resolving these issues is important, and incumbent on the investigators, for it will help provide the context for evaluating any archaeological record at the site.

4

Concerns about both artefacts and the features might be alleviated by using available piece-plot information to 'reconstruct' the absolute and relative position of material and thus perhaps make it possible to determine whether that material occurs in random or non-random patterns across and through the deposits. The demonstration of non-random distributions will not, itself, prove these materials have a human origin. However, the documentation of randomness will highlight the potential contribution of natural agencies in the formation of these deposits and their contents.

5

In regard to the origin of those deposits, it would be useful to excavate portions of the witness section to define carefully the micro-stratigraphy, including the dip and strike of the microstratigraphic beds, to establish trend surfaces and fabric patterns and to explore the nature of the interfaces and possible hiatuses in deposition, identify the existence of possible living surfaces and recover by the use of fine screens (and perhaps even selected flotation) smaller classes of materials, possibly including organics. As a part of this, it would be useful to measure systematically and quantitatively the actual fine sediment (silt and clay size fraction) contribution that is present in the matrix and its possible sources.

We recognize that site-wide questions about lithostratigraphy may not be resolvable with the remaining witness. However, work on the witness sections will at least resolve many key questions.

6

Finally, it might be useful to attempt to replicate the Pleistocene occupation at Pedra Furada at other sites in the region. This might be done by excavating beneath the detached roof-blocks which seal potential occupation areas beneath some of the Perna rock-shelters (especially Perna II). Careful, state-of-the-art excavations at this and other localities would seem to offer excellent potential for identifying and characterizing a Pleistocene presence in this area which must exist, if the Pedra Furada record is as claimed.

While we recognize the excavators and others may not share our concerns, we raise them here in good faith, without any intent of dismissing the work carried out at the site, and with the hope that both the concerns and the suggestions made to resolve them might be addressed in the forthcoming monograph or in future work at the site or in the region. We appreciate, of course, the complications of re-opening excavations here or elsewhere, but if more research is done at Pedra Furada or at other localities in the future, these suggestions might be considered to help resolve the ambiguities so often associated with early sites.

Ultimately, the course of action taken is the choice and responsibility of the Pedra Furada research team. If, in their opinion, the concerns raised here are not germane or significant or in need of further resolution, that is their choice. For our part, these are concerns we believe can be and should be resolved in order to convince us (and, perhaps, an even more sceptical archaeological community) that there was a Pleistocene human occupation at Pedra Furada.

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