

orderly, but anywhere that sizable bodies of evidence are recorded, patterning is discerned" and thus do we arrive at our constructs (West 1996).

Finally, I would like to suggest that from the outset, the Monte Verde problems were unfortunately compounded by its being treated as a piece of news rather than science. To that I shall add that while not unusual, the contribution of the *National Geographic* to this controversy is less than helpful. Fiedel is to be commended for his very important contribution.

COMMENT ON THE "REPLY TO FIEDEL, PART I" BY TOM D. DILLEHAY, ET AL., AND "REPLY TO FIEDEL, PART II" BY MICHAEL B. COLLINS

There appears to be little of substance in these responses. Fiedel's major criticisms really do not seem to be adequately addressed. For example, if artifact numbers are changed during a lengthy study, then surely a conversion table should always be supplied the reader. Collins' statement that Monte Verde "had no investigated precedents" actually strikes at the very core of the controversy. Being totally unprecedented in character and locus, there was always demanded an especially robust demonstration in all regards of its authenticity. Despite all the efforts of these investigators, this has not been forthcoming.

Clearly, an enormous amount of work has gone into this site and for this Dillehay and his associates deserve full credit. It is unfortunate for us all that the limitations of the evidence have prevented those efforts from being fully rewarded. Thus, in some minds, Monte Verde is as ambiguous now as it was at the outset.

On Monte Verde

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When my co-authors and I (Meltzer et al. 1997:660) urged readers to examine for themselves Dillehay's (1989, 1997) *Monte Verde* volumes, we meant it. Our views of the site were not intended to be a dictum slavishly followed, nor the final judgment on the matter. Fiedel took us up on our advice, obviously spent a great deal of time picking over the volume, and for that one can have no objections. I certainly don't. Yet, while I applaud his effort, I am disappointed by his method and results.

Many of the problems Fiedel ferreted out – as Dillehay and others, and Collins, note in their responses – are rooted in changing numbering systems, varying mapping protocols, the complications of producing and editing the massive results of a multi-year, multi-investigator research program, etc. Errors obviously happened in the process, and they do need to be clarified and corrected. None of us is immune to this sort of thing: take, for example, Fiedel (1999). It seems to me quite clear that Figure 1 in that paper was lifted without attribution and with little modification from Figure 3 in Kunz and Reanier 1994 (which Fiedel 1999 cites in another context). Do I think Fiedel intentionally plagiarized Kunz and Reanier's Figure 3? No. Do I think that his publishing the figure without attribution is a sign that inadvertent errors can occur, despite one's best efforts? Yes. But do I think that means I should therefore reject the arguments in his paper? Hardly. The point is, errors happen.

Dillehay recognizes that errors do happen and takes responsibility for those in the *Monte Verde* volume, but are the matters Fiedel raises in his critique inconsequential, or critical? Editorial/typographical or substantive? It is important to know which is which, however, so as not to mislead – or be misled – into confusing the trivial with the profound.

Separating the two in Fiedel's critique is no easy task, because he gives most of the problems that he sees equal weight, and because he blankets the whole in a patina of almost-conspiratorial mistrust and accusations, layered with all too-frequent snide remarks (e.g. "Dillehay's Hamlet-like agonizing"). In that, he does his critique no favors. More to the point, the most useful, productive, and constructive procedure (and certainly the most open and fair minded) would have been to first send Dillehay a draft of the criticism for comment and clarification of what were the trivial/editorial problems, and then once the minor problems were dispersed with and the major issues clarified, submit the piece to a rigorously peer-reviewed academic journal. Fiedel did not do so. I don't know why he did not, nor am I going

to adopt the negative tone which runs throughout his piece, and impute sinister motives on his part.

Yet, I don't want to "kill the messenger" either. It is entirely appropriate to question Monte Verde's evidence and interpretations, and especially matters of artifact provenience. All of us have at one point or another: indeed, the issue of artifact provenience was just one of the issues that prompted long discussions over the course of our visit to Monte Verde in January of 1997. And some of us have wavered in our views of this and other matters related to the site, as my colleague Vance Haynes notes. That's to be expected as we think more about it, and interpretations change (and on the matter of changing interpretations with new evidence and ideas, Dillehay should be granted that right no less than the rest of us, so the fact that his interpretations in the 1980s don't march in lock-step with those expressed in 1997 doesn't bother me). But, again, the discussion will only move forward if we can filter out what's important and unresolved, and what is not. Frankly, I'm not convinced a lot of the material in this critique is that profound, but will await Dillehay's promised errata pages to judge for myself. I recommend Fiedel do likewise. In the meantime, it's probably a good idea to remember that it was not too long ago that we were chiding pre-Clovis proponents for heaping up piles of dubious pre-Clovis cases, and insisting that where there's smoke, there must be fire.

In any case, Dillehay and his research team are quite capable of defending themselves on the questions of the Monte Verde data, evidence, and interpretations without my help, so I won't inject myself any further into the debate over the details. I would, however, like to make a few points about the Monte Verde site visit, since Fiedel raises it, and I was directly involved.

The trip to Monte Verde was extremely interesting, educational, and mostly enjoyable – though it also had its share of tense and downright unpleasant moments. Awkward as the latter were, they were a sure sign the visit was doing what it was supposed to do: allow a frank exchange of views. The one thing the Monte Verde trip was not was a counterpart to the 1927 Folsom site visit, but then it wasn't intended to be (as Fiedel supposes). Back in the early 1990s, Tom Dillehay, Vance Haynes, and I agreed to jointly organize a visit to Monte Verde. We all came into it with open eyes: the excavations were already completed, and much of the site was gone; though how much, of course, was not known precisely – as it often is not, but Dillehay was quite up front about this matter.

We also well appreciated that site visits may be seen by some as an anachronism: they were born at a time when determining the age and integrity of a site could be and in some ways had to be done in the field, by examining its stratigraphy, context, and geology, and the nature of the associated remains. It was a time when a photograph of a point in the ribs of an extinct bison could instantly convey its archeological age and context. As at Folsom in 1927. But let's face it: would or could a picture of one of the Monte Verde points sitting on the site surface do as much, as Fiedel imagines? I don't think so. After all, Adovasio (e.g. Adovasio and Carlisle 1984:136) has published pictures of a projectile point in pre-Clovis age levels at Meadowcroft, but so far I have not seen a massive groundswell of support for that site's claims. Today, much of the evidence from a site emerges in post-excavation analysis of radiocarbon samples, sediment chemistry, artifact sources and residues, the isotopic composition of organic remains, the detailed mapping of material and stratigraphy, and so on.

In fact, that being the case, why bother to go to the site? Because, as Haynes has argued, what you can see on site can never be fully portrayed in pictures or words, like the setting and surroundings, the complexity of the stratigraphy, the position of artifacts in the deposits and on the surfaces (if you arrive in time, of course, and we did not – but we did have a chance to directly examine many of the artifacts). You can even get a feel for how difficult the sediment was to excavate or how easy it might have been to miss potentially mixed items in the deposits. Dillehay welcomed us to do all that, and ultimately we went to Monte Verde – that it happened to be just 9 months shy of the 70th anniversary of the Folsom visit was a coincidence, though one not without historical irony.

A few months after our return, and after we'd all had a chance to mull what we'd seen, Donald Grayson and I drafted up a statement that was circulated to all of those who went on the visit (and who were not affiliated with the Monte Verde research team). All who signed on to it therefore had a chance to examine it in the cold light of day and well after any bruised egos or perceived twisted elbows had fully healed. Everyone did sign it, because all of us felt, after asking the questions Fiedel asked and a whole lot more, and being satisfied with the answers, that the site was archaeological and 12,500 years old (Meltzer et al. 1997). Many of us had lingering questions about the character of the artifact assemblages and their interpretation,

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but that did not change the central conclusion. Frankly, until I can ascertain how much of Fiedel's critique is smoke, and how much is fire, I don't see any reason to change that conclusion.

I think that, too, because one of the other principal lessons that emerged over the course of the site visit was that we weren't in Kansas anymore. South American late Pleistocene sites and assemblages look very different than what we have in North America. Theirs is not a record dominated by projectile points embedded in Pleistocene megafauna (frankly, I don't think our North American record is either, but that's certainly the image many carry). Is it therefore appropriate to concentrate a critique on those aspects of a site with which we are familiar, or fault Dillehay for not giving emphasis to classes of remains we would give emphasis to, or for not excavating the site or not writing the book the way we would have done it? Or, worse, ascribe base motives because he did not? I don't think so. On a related note, I had a chance to examine and photograph the site's lithic assemblage, including the slate perforator, in 1986, well before the assemblage was described in detail or this specimen was mentioned in print (which Fiedel puts as 1991). So I can attest that specimens such as this one had not escaped analytical attention, and indeed Michael Collins and I had a lively discussion about the slate perforator's possible function. The fact that it was not mentioned in some of the preliminary papers only means it was not mentioned in some of the preliminary papers. Lots of things were not. That's what final reports are for. The bottom line for me: The *Monte Verde* volume has errors, it has glitches. What project of this magnitude and scale doesn't? But I remain to be convinced the errors are fatal. I'm not throwing away my copy of the book just yet.

Monte Verde and the Pre-Clovis Situation in America

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Some anthropologists have used the Monte Verde site as the breakthrough that requires a new paradigm (e.g., *Mammoth Trumpet*, V. 13, n. 3, 1998), much as with the initial Folsom site discovery. As one who participated in the 1997 site evaluation, I can say it is certainly not the modern equivalent of the breakthrough created by the discovery in the 1920s of 10 or more Folsom projectile points among the skeletons of more than 20 extinct bison buried under 1 to 3 m of stratified alluvium.

In early January, 1997, the very important but controversial Monte Verde archaeological site in southern Chile was examined by 13 specialists representing several scientific disciplines (Meltzer et al., 1997). The early part of the conference was devoted to the examination of artifacts and site data, as well as orientation lectures by the principal investigators from the site. The last days were spent in the field for orientation with respect to the regional geology and archaeology, and one day examining the Monte Verde site and collecting samples for geochronological testing.

As the Monte Verde site visit was being planned, one of the sponsors stated that a consensus would be required. I found this to be an unreasonable request and replied that it was unlikely. However, by the final evening in Chile it was obvious that I was the only hold-out. I would have much preferred a chance to reread the second volume (Dillehay 1997) before making a decision on the spot. My reservations were about the provenience of the six unequivocal stone artifacts because my preliminary reading of the poor quality preprint of Volume II (Dillehay 1997) was mainly devoted to evaluating the radiocarbon dating and the stratigraphic provenience of the samples analyzed for ^{14}C . It was not until my return from the field trip that I could take the time to look up the specific provenience of each stone tool in the various site reports (Dillehay 1989, 1997). To my surprise, I found these data to be inadequate and therefore unconvincing.

At the Monte Verde site there are six stone artifacts, unquestionably the work of prehistoric people, that have reportedly been found in stratigraphic context with associated wood and charcoal that yielded eleven radiocarbon ages averaging about 12,500 B.P. (Dillehay 1997).

Three of the unequivocal artifacts are midsections of willow-leaf-shaped pro-

jectile points (X1500001, A0100026, and D-S-1), two very well made. Another artifact is a polished slate rod (A1300100). Yet another is a small ovoid tonalite solid girdled with a groove and believed to be a bola stone (B0701100). The sixth is a stream-rounded rhyolite cobble that may be a core or may have been intended as a chopping tool (A0104001). A large quartzite biface (B1005000) is not considered here because it was not found *in situ* (Dillehay 1997:612). In my opinion, all of the other items proposed as artifacts are equivocal, some highly so. Regardless of these opinions, many, if not most, of the more equivocal artifacts of wood, stone, and bone may be cultural items associated with the occupation surface as claimed. However, Fiedel describes many discrepancies in the location and mapping.

It is very important to thoroughly understand the stratigraphy of the Monte Verde site in order to evaluate the provenience of each documented object and the relationship of one to another in time and space (for a complete listing of radiocarbon dates see Dillehay 1997: Table 3.1). Figure 1 is a generalized stratigraphic cross-section of the site. While not precise, it is accurate. The youngest stratum underlying the surface of the occupation bearing these artifacts is a channel gravel (MV-6) that has no organic material suitable for radiocarbon dating but unconformably overlies an erosional surface (contact Y1 of Figure 1) on an alluvial volcanic sand (MV-7) with uncalibrated radiocarbon ages of $23,660 \pm 320$ B.P. (BETA-35193) and $27,860 \pm 2010$ B.P. (BETA-41983). The oldest stratum overlying the cultural layer is a peat deposit (MV-5) dating between 12,000 B.P. and 10,330 B.P., on the basis of seven radiocarbon ages (Figure 1). The peat covers the occupation surface and is thickest (15-30 cm) in low areas underlain by the channel gravel (MV-6) and thins out up slope away from the MV-6 channel. The basal contact, Y2, of the MV-5 peat with the MV-6 gravel joins the Y1 contact away from the channel to form unconformable contact Y1-2 (Figure 1), which is the occupation surface. All six of the unquestionable stone artifacts reportedly occurred directly below and were covered by the MV-5 peat.

In accepting this site on the basis of the six unquestionable stone artifacts, I still have reservations about the following aspects:

1. All but one of the wood artifacts that I examined are questionable and probably would not be considered artifacts out of context. Figure 7.30 (Dillehay 1997:156) is the best candidate I saw. However, I have no personal experience with crude wooden artifacts. Unfortunately, we were not able to see the longitudinally grooved log (Dillehay 1997, Figure 7.25) and the log with burnt fire-stick holes (Dillehay 1997, Figure 7.41). These appear to be the least equivocal of the wooden artifacts, but we were informed that these were lost due to poor curation in the case of the grooved log, and the bored log could not be found. In our walk through the remnants of the beach forest I noticed a weathered log segment with irregularly spaced holes much like those in photographs of the missing fire-starting log.

2. Many of the stone artifacts I examined are very questionable and would not be considered artifactual out of context. While at the University of Kentucky in 1997 during the tour, Dillehay explained that this is typical of early lithic assemblages in South America. This is disputed by Lynch (1990, and personal communication, 1998).

Nowhere in either Volume I or II by Dillehay can I find any data specifically devoted to the micro-stratigraphic provenience of each of the unequivocal artifacts. My review of the provenience of the six unequivocal stone artifacts, while similar to that of Fiedel's, is not as thorough. Fiedel has taken the time and tedious effort to thoroughly evaluate the 1300-page Volume II report, checking and counter-checking every point. Rather than repeat my findings that duplicate those of Fiedel, I simply repeat the deficiencies I see in the reporting.

The overlying strata are not discussed specifically in regard to projectile points, the polished slate rod, the grooved bola stone or the core-chopper. It would be helpful to know the depth of each occurrence below the modern surface, the thickness of MV-5 over each one and the presence or absence of intervening strata. Unfortunately, no photographs of any of these unquestionable artifacts *in situ* are shown in publications. Dillehay et al. state in their response that the reason is poor quality in making prints from "fuzzy" slides. Even so, if the artifacts are at all recognizable, the images of them in place would be of interest. Who actually excavated each artifact? Perhaps they would have taken photographs *in situ* or even made notes regarding their find which, I expect, would have raised considerable excitement at the time of discovery.

In the absence of skeptical professional inquiry, as done at Folsom in 1927 and 1928, such procedures for demonstrating the position of each artifact with respect to the basal contact of MV-5 and the overlying and underlying strata would have gone a long way toward validating Monte Verde.