

On Monte Verde: Fiedel's Confusions and Misrepresentations

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Part 1: Introduction

This website provides a detailed reply to Stuart Fiedel's review "Artifact Provenience at Monte Verde: Inconsistencies and Confusion" as well as to other commentators featured in a 23-page SPECIAL REPORT in the October 1999 issue of *Scientific American Discovering Archaeology*. His review points out some editorial errors in the lengthy second volume on the Monte Verde site (Dillehay 1997). Fiedel is most concerned with artifact provenience and the multiple and discontinuous numbers assigned to some artifacts and features. He also criticizes us for changing our ideas over the past twenty years about the nature and function of certain artifacts and activities in the site.

We argue that Fiedel's concerns reflect his misunderstanding of and inexperience with (1) long-term and interdisciplinary research design and analysis at a complicated wet site like Monte Verde and (2) the necessity to recode and reclassify artifacts for computational studies. His review is full of factual and interpretative errors and misrepresentations of Monte Verde and the research it represents. Fiedel's argument misleads the reader to believe that the research at Monte Verde was "chaotic," "inconsistent," and "confusing." If Fiedel had sent us a copy of his manuscript prior to publishing it in *Discovering Archaeology* and if it had been reviewed by peers in a technical journal, his queries would have been answered, his mistakes would have been prevented, and this new round of questions about Monte Verde might have had the positive effect of bringing to light errors and specific sections of the report that needed clarification.

In the Monte Verde reports, we provided much interdisciplinary scientific detail on the site, including specific artifact provenience numbers (not just catalog numbers). Our very openness and scrupulousness about documenting and reporting on the site has counter intuitively provided "evidence" for Fiedel's claims. He failed to cite supporting evidence that contradicts his own claims, as we show in Parts 2 and 3 below. Instead, he selected passages and data to support his position, compared preliminary statements made ten to fifteen years ago with those in the final report and accused us of changing our minds about the data, and took quotes out of context to indicate that our analysis was chaotic and inadequate.

A comment by David Hurst Thomas summarized the situation:

I am irked at the carping tone of Fiedel's commentary, and the ferreting out of meaningless conflict in interpretation over two decades of reporting on Monte Verde. Fiedel cops an attitude, which, in my opinion, is entirely inappropriate...I don't see multiple artifact numbers as much of a problem--having done this myself many times--but maybe a concordance on critical pieces would have been helpful. And I think it's a cheap shot to dredge up preliminary assessments and press reports to attack the Monte Verde project. I still think it's a good thing to change your mind (so long as you're honest about it). (Thomas 1999:2)

We want to make it clear that we view Fiedel's review as hostile. The draft that he sent to *Scientific American Discovering Archaeology* as well as to several individuals is not the one that was published in the SPECIAL REPORT--the published version was purged of the more inflammatory wording. Examples of this wording include the following: Fiedel (n.d.) stated in the draft that the report was "bloated, poorly constructed...riddled with inaccuracy, internal contradictions, and obfuscation;" and, in reference to the stone tools, "a few obviously worked, the great majority naturally fractured but allegedly used." Not only is it important for readers of the web page to be aware of Fiedel's tone, but also to know that it was his initial draft that the several commentators featured in the SPECIAL SECTION reacted to. We know that this earlier draft was also circulated to an unknown number of individuals and to at least one news reporter.

We serve on several editorial boards, including *Antiquity*, *Cambridge University Press*, *GeoArchaeology*, *Latin American Antiquity*, and others. When editorial boards receive the kind of hostile and accusatory review written by Fiedel, they don't publish it. Or, if they do, they make sure the author tones it down, and they send it to others for peer-review and to the accused. This is standard procedure in scientific reporting. This protocol was not fully followed by Fiedel or by *Discovering Archaeology*.

Why wasn't the Fiedel essay reviewed properly? We cannot escape the conclusion that the appeal of translating science for public consumption and for expanded readership was too tempting for *Discovering Archaeology* and Fiedel. There is no scholarly and scientific reason why this type of erroneous, misleading, and inflammatory review was published. We encourage constructive scientific discourse and review of major publications, but in refereed scientific journals, not popular, unjurious magazines.

Of the Paleoindian experts invited to comment on Fiedel's review, David Meltzer, Jim Adovasio, and Rob Bonnischen were cautious and saw the review for what it was. For instance, Meltzer asks

are the matters Fiedel raises in his critique inconsequential, or critical...it is important to know which is which, however, so as not to mislead--or to 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. (Meltzer 1999:16)

Adovasio (1999:20) says that "Fiedel's remarks demonstrate a near total failure to grasp the major methodological issues, let alone the tactical nuances, of excavations at a site like Monte Verde." Rob Bonnischen (1999:21) notes that "Fiedel did not come to grips with changes that occurred over the duration of the complex and evolving multidisciplinary project...which were reopened in the final publication."

Others (David Anderson, Vance Haynes, and Federick West) uncritically (and, in our opinion, incautiously) used Fiedel's review to further their negative opinion of Monte Verde. Anderson, Haynes, and West were strongly inspired by Fiedel's accusations and wrote

passionate and highly critical commentaries about Monte Verde. Haynes (1999:17-18), a member of the investigative team that accepted Monte Verde in 1997 (Meltzer et al. 1997), insisted that only six “real” artifacts were found at the site and begins to withdraw his support of the site. Anderson (1999:20) stated that the Smithsonian Institution Press should republish the second Monte Verde book, because it is full of errors and that Fiedel should be the editor of the new volume. West (1999:15) remarked that Monte Verde has always been more “news than science.” Instead of checking the “facts” in the second volume and questioning Fiedel’s allegations in his review, these commentators uncritically accepted the allegations and joined to discredit the research at Monte Verde.

Fiedel’s critique was news. Both *Archaeology* and *Science* magazines, two reputable journals in the discipline, picked up Fiedel’s story and uncritically reported it as fact. Michael Woods (1999:2), a news reporter, wrote that Fiedel’s “written critique also hinted of a conspiracy by scientists who want Monte Verde as the ultimate proof of pre-Clovis occupation of the New World.” Not all reporters, however, saw the Fiedel story this way. John Noble Wilford, a science writer for the *New York Times*, stated that

the attack on Monte Verde, published just before the conference (Clovis and Beyond conference held at Sante Fe in October) raised cries of foul. Many archaeologists complained that Dr. Fiedel’s review was biased and ignored material that did not support his critical thesis. They deplored his tactic of airing his critique in a popular magazine rather than a peer-reviewed journal. (Noble 1999:D4)

Why the excessively negative allegations in Fiedel’s review? As Deborah Tannen, a social linguist at Georgetown University, notes in her book *The Argument Culture: Moving from Debate to Dialogue*, “Allegations make the news, no matter where they come from, often without proof or even verification” (Tannen 1998: 278). Tannen’s book is concerned with the “unrelenting contention--an argument culture” in academia and the news corps that:

Valuing attack as a sign of respect is part of the argument culture of academia--our conception of intellectual interchange as a metaphorical battle... Younger or less prominent scholars can achieve a level of attention otherwise denied or eluding them by stepping into the ring with someone who has already attracted the spotlight. (Tannen 1998:367-370)

[Argument culture] urges us to approach the world--and the people in it--in an adversarial frame of mind. It rests on the assumption that opposition is the best way to get anything done. The best way to discuss an idea is to set up a debate; the best way to cover news is to find spokespeople who express the most extreme, polarized views and present them as ‘both sides;’ the best way to settle disputes is litigation that pits one party against the other; the best way to begin an essay is to attack someone; and the best way to show that you are really thinking is to criticize...When there is a

need to make others wrong, the temptation is great to oversimplify at best, and at worst to distort or even misrepresent other's positions, the better to refute--to search for the most foolish statement in a generally reasonable treatise, seize upon the weakest examples, ignore facts that support your opponent's views, and focus only on those that support your own. Straw men spring up like scarecrows in a cornfield. (Tannen 1998:268-69, 278)

David Hurst Thomas noted in his commentary on Fiedel's review that:

Ambushing the Monte Verde team in this way [referring to a quick, non-refereed publication] inevitably raises questions over Fiedel's motivations. Was the critique primarily concerned with clarifying the pre-Clovis possibilities at Monte Verde? Or was this just another carefully timed headline-grabber? Handed the way it was, who knows? (Thomas 1999:2)

Clearly, Fiedel is not a disinterested archaeologist observing a paradigm change in American archaeology. Fiedel says that

the Monte Verde site in southern Chile has been widely accepted as the long-sought proof of a pre-Clovis human presence in the Americas. This site...is a paradigm buster...it implies 'a fundamentally different history of human colonization of the New World' (Meltzer et al. 1997:662). Acceptance of the antiquity of Monte Verde ensures that other sites...will be less skeptically regarded. (Fiedel 1999:1)

Elsewhere, Fiedel has said that: "If a pre-Clovis human presence is confirmed (and I personally remain very skeptical), we will all have to go back to the drawing board" (Fiedel 1992:x).

The larger problem is that Fiedel's review seemingly placed a premium on negative criticism and misrepresentation of information rather than on scientific method and peer review. Fiedel misleads readers by distorting evidence and by ignoring facts that counter his own argument. He attempted to dismiss the entire suite of "compelling" archaeological evidence excavated at Monte Verde (Fiedel 1999:1), to cast permanent doubt on everything about the site, and to make archaeologists even more skeptical of other pre-Clovis sites even at the cost of professional protocol and, indeed, truth.

Over the past twenty years, die-hard Clovis advocates have questioned the radiocarbon dates (Lynch 1990; West 1993), the artifact context (Lynch 1990; Morlan 1984), the research methodology (Dincauze 1990), the quality of Latin American scholars working on the Monte Verde project (Anonymous 1982), and certain members of the research team. The integrity of the site has been sustained despite these constant attacks. With little else left to attack, Clovis advocates have now turned to the final report and other publications. We suspect that skeptics of Monte Verde (and other early sites) will always exist and that the

attempts to invalidate the site will continue. If attempts similar to Fiedel's review appear in the future, we will not respond to them.

In the twenty-year debate about Monte Verde, it is curious that of the 85 professional and student archaeologists who excavated at Monte Verde between 1977 and 1985, none have ever claimed that Monte Verde was an invalid site. More than 35 of these individuals had archaeological field experience prior to working at Monte Verde. We estimate that they collectively had about 25 years of professional experience at the time. Today, their collective professional experience totals more than 400 years. They are now professors at universities and museums in Chile, Argentina, Venezuela, Peru, Australia, and the United States. Several of those professionals are included as co-authors of this reply. Further, the project geologist, Pino, had worked previously at Quereo and Tagua-Tagua, two important late Pleistocene sites in Chile. Why haven't these professionals ever questioned the validity of Monte Verde? It is because they excavated the footprints, the points, the hearths, and other artifacts on the buried use-surfaces at the site, and they know the tight stratigraphic and contextual association among them.

The following Part 2 presents the general issues in Fiedel's review and our responses to them.

Part 2: General Issues: Fiedel's and Haynes's Accusations and Our Response to Them

We have divided this summary of general issues into seven topics: artifact provenience and numbers; documentation procedure and research design and analysis; artifact context and association; absence of "crucial photographs" and the primacy of the projectile point; changing interpretations about site and artifact function; delay in reporting material; and site visits at Monte Verde. Included below are sections of Dillehay's (1999) and Collins' (1999) previous short responses to Fiedel.

Artifact Provenience and Numbers

Fiedel gives the impression in his review that we did not follow standard archaeological reporting by providing *in situ* shots of "compelling" human-made artifacts and features and by adequate documentation of artifact provenience. He states that "the final report fails to provide even the most basic provenience data about key artifacts" (Fiedel 1999:2). This is not true and is a misrepresentation. The Monte Verde publications represent one of the very few times in Paleoindian studies that artifact inventory numbers specifying the precise provenience (not just the catalogue number) have been published for a late Pleistocene site. As we noted in our short rebuttal to Fiedel (Dillehay et. al. 1999), our intent was to be forthright and open, providing the reader with as much archaeological and analytical detail as possible. Evidently, we did not provide enough detail about our season-to-season field and

laboratory techniques in the combined 65 pages written about research design and the 1600 pages published in both volumes about the Monte Verde (Dillehay 1989a, 1997a).

Documentation Procedure and Research Design and Analysis

The use of multiple artifact inventory numbers and test pit and block excavation numbers appears to have confused Fiedel and led him to believe that the field work and method of data documentation was “chaotic,” “haphazard,” and “inadequate.”

The exploration of buried archaeological sites may take place in two ways. One is to lay out a checkerboard grid system across the site that corresponds to cardinal directions. This approach permits a systematic and integrated excavation of exploratory trenches and test pits that fit into the cardinal numerical system. Another is the opportunistic placement of trenches and test pits that are simply numbered sequentially as the excavation proceeds each season. Although in 1977 we laid out a 10 by 10 m grid system in Area A where bones and stones were first reported to be washed out of the creek bank (Dillehay 1985), most of our exploratory work at Monte Verde consisted of trenches and pits placed in different areas around the site to define the stratigraphy, collect sediment and other samples, and search for artifactual material. Most of these pits yielded no artifactual debris and thus were not expanded into larger block excavations. When trenches and pits yielded artifactual debris (e.g., X-15 and PZ-25), they were expanded into larger pits and, in some cases, block excavations. Geological pits and trenches were assigned letters and numbers; sample pits were given “PMs” and number designations (1,2,3...); and archaeological pits were given “PZs,” “TPs,” and “Xs.” Further, artifacts (such as X1500001 and D-10-1-1: see discussion in Part 3) that were first recovered from test pits that were later converted to block excavations required two or more numbers to reflect the changing excavation strategy. As Fiedel has noted, an artifact may bear two or more numbers to document its location within the expanding site excavation, and this may initially be confusing for readers skimming all publications.

When we first excavated Monte Verde in the late 1970s, archaeologists were just beginning to apply various computer spatial and statistical programs to the study of archaeological data. We initially did not set up the Monte Verde database for extensive exploratory computer analysis. We did so between 1983 and 1985, when newly available technology allowed us to program the data for CMAP, SYMAP, SPSS, AUTOCAD, SURFER, K-MEANS CLUSTER and other analyses. Between 1983 and 1995, we often recoded artifact and feature numbers to fit appropriate computer programs. For instance, the programs required that all artifacts be assigned the same quantity of numbers and letters. This often meant adding elements (e.g., suffixes, prefixes, and zeroes) to “fill in” the columns. Once again, artifact numbers changed, which we reported in preliminary publications. Thus, in combination, depending on its location in a geological pit, or archaeological pit, and on the necessity of adding elements (usually prefixes such as Rs or Xs and Os) to its artifact number to satisfy the requirements of various computer programs, an artifact may bear several numbers (e.g., A0100026 or A-1-26; X-1E-1 or X-E-1 or PZ-43-1 and so forth). Too,

some artifacts may have discontinuous or continuous numbers. We alerted readers of this strategy in the chapter “Research Design” in the second volume. We say:

An opportunistic sampling strategy was employed to test outside areas of unknown cultural activities...For ease of reference, the grid system established earlier was divided into a letter-number designation system for each block and each 1-by-1 m row and column subdivision...This system was tied into the N100-E100 grid system that was utilized to measure exact artifact locations for later computational manipulation. ...During the initial course of excavation all stone, bone, wood, and miscellaneous objects excavated ...were labeled sequentially...During the last two seasons, when an [sic] large amount of debris was recovered...a sequential number system was employed for each type. The use of these two cataloging systems resulted in discontinuous and continuous artifact numbers for each category. (Dillehay 1997b:60-61)

Further, we noted that

the reader should be aware that the total number of specimens for bone, wood, plant, and stone types occasionally varies slightly on tables containing quantitative data. The reason is that “missing cases” appear in the computer database for unmeasured variables, such as the length of a broken lithic or the taxon of a wood specimen, and so forth. The variations are less than 2 percent, but they exist nonetheless. (Dillehay 1997b:55)

In addition, computer programs, such as SYMAP, are designed to reflect the general distribution of artifacts, not their precise location at the scale of illustration shown in Volume 2. In this regard, where two or more artifacts of the same material category are located close together, programs such as SURFER often superimpose them. This may result in only one artifact being illustrated.

The point is that the last number given to an artifact and published in the second volume is its final designation; while the different numbers assigned to some artifacts may suggest different locational contexts to casual readers, the artifact location is consistent. This is not to deny that inconsistencies in data analysis and interpretation exist in the publications (which we believe is common in a long-term and inter-disciplinary project and in a lengthy report like Volume 2). Such inconsistencies as changing functional terms of bone, wood, and stone artifacts or changing species of wood and plant materials simply reflect reanalyze by specialists as new data or more accurate techniques are available. If Fiedel had carefully read and scrupulously quoted from the chapters on research design in all publications and had more experience in dealing with the kind of research carried out at Monte Verde, he would have understood the methods used by the research team and possibly been less confused about the numeration and location of artifacts and features. Others understood the vagaries of doing long-term, interdisciplinary research, such as John Driver (quoted in

Anonymous 1999:657), an archaeologist at Simon Fraser University, who stated in the *Science* story about Fiedel's review: "Those are common problems in large, multi-year excavations."

Artifact Context and Association

Fiedel (1999:1-2) complains that "Dillehay freely refers to objects lying more than 30 meters apart as 'associated' [both functionally and behaviorally]. Both Fiedel and Haynes question the behavioral inferences we draw from these associations. For example, with specific reference to the projectile points, Haynes (1999:17) says that:

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...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. (Haynes 1999:17)

Most late Pleistocene sites are characterized by bone beds, lithic scatters, hearths, and miscellaneous debris that are often distributed several meters across a buried site, so it is not unusual to find and to infer associated activity areas separated by 30 meters in distance, especially when (1) there are conjoining artifacts between them, (2) the types of artifacts found in different areas are of the same style and function, (3) the radiocarbon dates are virtually the same, (4) the cultural debris is embedded in the same stratigraphic layer, and (5) the areas make functional and behavioral sense.

We say in Volume 2 that

all cultural and ecofactual debris of the MV-II surface rests on or are contained within the upper 5 cm of Stratum MV-7, the creek bank [terrace], and, to a lesser extent, Stratum MV-6, the adjacent sand bars. Overlying these strata are MV-5, the basal peat layer that directly covers and seals the habitation layer, and MV-4, MV-3, MV-2, and MV-1...The MV-II surface is composed of cultural debris from a single occupation or from two sequential ones by the same group. Although no microstratigraphic layers were observed in the buried MV-7 occupational surface, except in the Wishbone Structure [which is characterized by two cultural building phases], the range of thickness of the use surface between 2 and 5 cm suggests that the total duration during which artifact "deposition" occurred was short, possibly stretching over a period of several months or a year or slightly more. (Dillehay 1997f: 230-31)

When the MV-5 fibrous peat was excavated, the cultural features and artifacts were found lying on or slightly embedded in the upper few centimeters of the underlying MV-7 sand layer. All artifacts and features were in this stratigraphic and archaeological context, so there was no need in the report to individually document the precise depth and stratigraphic location of each artifact from the ground surface.

Finally, we are unaware of any recent publication in Paleoindian studies or by Haynes and Fiedel that has ever produced the kind of documentation they demand.

Absence of “Crucial Photographs” and the Primacy of the Projectile Point

According to Fiedel and Haynes, archaeological documentation of late Pleistocene cultures is based primarily on the photographic record of *in situ* bifacial projectile points (preferably Clovis points) regardless of the presence of other cultural traits, such as hearths, footprints, wood and bone artifacts, and exotic materials.

Fiedel states that:

To compare Folsom [site in New Mexico] and Monte Verde ...we have all seen the points lying amid the ribs of *Bison antiquus* [at Folsom]. Similar photographic documentation has been standard procedure at Paleoindian sites ever since.... Where are the photographs, or even field drawings, of the Monte Verde bifaces at the moment of discovery at their various locations? (Fiedel 1999:4)

Fiedel is wrong. Photographic documentation of Paleoindian points has not been standard procedure in the discipline since the discovery of the Folsom site. For example, Meltzer (personal communication, 1999) studied 44 Paleoindian site reports published after the Folsom site and found that only 11 of the 44 publications showed *in situ* photographs of the projectile points. We also examined several recent publications on Paleoindian sites, including George Frison’s (1996) monograph on the Goshen Complex at the Mill Iron site in Wyoming, Joseph McAlvoy’s and Lynn McAlvoy’s (1997) book on the Cactus Hill site in Virginia, and Neal Lopinot’s and Jack Ray’s (1999) monograph on the Big Eddy site in Missouri. The Mill Iron report contains only ten *in situ* shots of the excavation and artifacts, and only one is of a projectile point. No *in situ* points were shown for the Cactus Hill and Big Eddy sites. Does this mean that these are invalid sites, because they do not show *in situ* photographs of projectile points? Of course not.

To compare the Folsom and Monte Verde sites is like comparing apples and oranges. The “compelling” human artifacts at the Folsom kill-site were the stone tools lying in direct association with the ribs. There were no hearths, no wood, bone or other artifacts at Folsom. Furthermore, in 1927, the only way to demonstrate and to date the antiquity of archaeological finds and their association with the bone remains of extinct animals was to show them *in situ*. There were no standard stratigraphic procedures and radiocarbon dating

methods at the time. It was thus critical at Folsom to have a site visit and to give primacy to viewing *in situ* points. The world is different now. And so is Monte Verde. It is not a kill-site like Folsom. The mastodon kill-sites associated with the majority of the bone remains of six to seven animals and probably with the point fragments and other kill-weapons are not located at Monte Verde, but elsewhere, probably in distant bogs. At a kill-site, points and butchering tools are important for establishing a linkage between human activity and the bone remains of extinct animals. At Monte Verde, the three projectile point fragments constitute only 0.3 percent of the total artifact inventory. Found at Monte Verde are cultural features and artifacts that are equally and even more convincing of human intervention than points. More importantly, it is these finds that support our inferences about the behavior of the hunter-gatherers that occupied the site.

In his commentary on Monte Verde, Haynes says he observed “six unequivocal stone tools” in the Monte Verde collection. All six represent what Haynes can identify as stone tools from the perspective of his Clovis template. He dismisses all other cultural evidence, including knotted cordage, hearths, other stone artifacts, footprints, wooden artifacts, plant remains, clear spatial patterning. But Haynes is not an archeologist: he is a geoarchaeologist. (Even when it comes to his geology, he imagines an improbable geologic scenario to explain the presence of artifacts at Monte Verde. He suggests that coastlines near the site account for the presence of several varieties of marine resources at the site.)

We did not emphasize the points at Monte Verde, because we did not consider them to be the most important artifacts in the site. From the outset of the Monte Verde project in the late 1970s, we emphasized other assemblages over the stone tools and bifaces at the site. In the introduction to Volume 2, Dillehay (1997a:2-3) barely mentions the stone tool industry, giving primary attention to the features, wood, bone, and botanical evidence and their internal spatial organization. This emphasis also extends to earlier publications: “When viewed from the perspective of the amount of labor committed to the procurement and preparation of these materials for use, the macrobotanical and wood remains constitute the most important assemblages” (Dillehay 1988:189).

Furthermore, the sequence of chapters in the second volume indicates the relative importance of the lithics to us. We placed lithic Chapters 15-16 near the end of the book, where we thought they belonged with respect to chapters on the habitation floor, the features, the plant assemblage, the wood assemblage, and other materials. We saw no need to judge Monte Verde primarily in terms of North American criteria, meaning bifaces. Besides, if *in situ* photographs of points are the main criterion for validating early sites, then why hasn't Meadowcroft Shelter been accepted by Fiedel, Haynes, and others? As Meltzer (1999:16) notes, “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.”

To conclude this discussion on points, Fiedel's and Haynes' expectation of them may be justifiable from the viewpoint of early North American archaeology but it does not apply to all early South American archaeology, where bifaces are infrequent, rare, or absent in sites. Haynes (1999:17) stated that: “While at the University of Kentucky in 1997 during the tour, Dillehay explained that [a low frequency or absence of points] is typical of early lithic

assemblages in South America. This is disputed by Lynch (1990, and personal communication, 1998).” Lynch’s (e.g., 1990) publication record on late Pleistocene South America is misleading. He is a strong Clovis advocate who has generally ignored the unifacial industries of the south. If Haynes and Fiedel had read the literature on late Pleistocene cultures in South America, they would have realized that pebble-tool and unifacial industries are just as prevalent and important as bifacial industries (e.g., Bryan 1986; Dillehay 1999; Dillehay et. al.1992; Kipnis 1998; Schmitz 1987).

Fiedel also complains that we have not published *in situ* photographs of the non-projectile point artifacts. This is not true. We have published 104 *in situ* shots of artifacts and features in all of the Monte Verde publications. Seventy-three *in situ* photos are presented in the 1997 report alone. More than 500 photographs, maps, and tables in that report document the location of artifacts and features. And the text provides detailed and repetitive descriptions of their locations. How much more detail do we have to make available beyond the 1600 pages and more than total 800 photographs, drawings, and tables already published?

Conveniently not mentioned by Fiedel is the standard we established in the Monte Verde books by publishing the provenience inventory number of artifacts in captions, texts, and tables. As mentioned above, we know of no other Paleoindian site report that documents a site in this manner. Besides, if we had not provided the artifact inventory numbers, how could Fiedel have made his accusations about artifact provenience?

For readers interested in viewing a few *in situ* photographs (Figs. 1-9) of projectile points and excavation procedures, see Part 6 below.

Changing Interpretations about Site and Artifact Function

Fiedel notes that our ideas about the function of the site and the functional terms of some artifacts are confusing, largely because the terms have changed over the years as our ideas about the site have changed. Investigators change their ideas in long-term research projects, as new techniques are applied and new data are discovered. This is the purpose of long-term research. When we first published on Monte Verde (Dillehay 1984), we attempted to be cautious and not assign too many strict functional terms, such as knives, scrapers, etc. until Collins’ morphological study, Dillehay’s micro-use wear analysis, and Rossen, Pollack, and Dillehay’s spatial studies were completed and compared to determine artifact use and context. Thus, the fact that we considered some artifacts to be bifaces in a 1984 article (Dillehay 1984) and later revised our assessment of these artifacts to be choppers, scrapers, or points in the final publication simply reflects our conservative approach to functional typologies. The same can be said of changing interpretations about activity areas in the site. We were trying to be as complete, cautious, and open as possible about the artifact assemblage and about the site analysis. And we have always thought that preliminary reports are just that, preliminary. The final report is the last word.

Delayed Reporting of Important Artifacts

Fiedel repeatedly asked in his review why we did not report the discovery of important artifacts, such as knotted reeds, points, grooved wood and so forth, prior to publication of the second volume. For example, he states that: “Although great importance is now attached to knotted cordage at the site...this material passed unnoticed or unremarked for years after its initial discovery” (Fiedel 1999:10).

Fiedel is wrong. The cordage and/or other important artifacts were discussed in preliminary publications and/or presented at professional meetings and invited talks long before publication of the second volume. Moreover, the specialists analyzing these materials were aware of them much earlier than their studies. For instance, Adovasio remarks that:

Fiedel implies that the indisputably anthropogenic knotted *Juncus* sp. Monte Verde cordage... was, mysteriously, not mentioned or extensively discussed prior to the final report. The sinister implication is that, somehow, this highly diagnostic material appeared *ex nihilo* to grace a chapter in Dillehay’s second volume. This innuendo, like so much in the Fiedel commentary, is erroneous and misleading. In fact, this author was aware of the existence of a small modified fiber assemblage from Monte Verde for over a decade before the opportunity to actually analyze this material was presented [which was in 1992]. (Adovasio 1999:20)

The 1997 Monte Verde Visit

In referring to site visits and criteria for evaluating Monte Verde, Fiedel says that when the inspection team visited the site in 1997 (see Meltzer et al. 1997), “there was no site left to see, as it had been largely bulldozed away in 1988. The guest scholars could only inspect a remnant soil stub that at most only demonstrated that the site’s natural stratigraphy corresponded to Dillehay and Pino’s descriptions” (Fiedel 1999:2).

The only figment here is Fiedel’s imagination. Most of the Monte Verde II component (dated about 12,500 years ago) had been excavated by 1985. Only a small portion of the buried deposit remained and seasonal flooding was slowly washing this out. In 1988, a portion of the embankment containing this component was further disturbed by a bulldozer collecting gravel to build a nearby road.

Fiedel (1999:5) would have the reader believe that we tried to keep people away from Monte Verde so they would not inspect the stratigraphy and the excavation. Since 1977, we invited numerous colleagues to visit the site and the laboratory at the Universidad Austral de Chile in Valdivia. This also is not true. We repeatedly sent telegrams and letters, and made phone calls to Chilean and foreign colleagues, inviting them to observe and participate in the excavation. Most invited colleagues wrote back, saying that they could not visit the site for various reasons (e.g., Lynch, personal communication 1983; Nunez, personal communication, 1984; Stehberg, personal communication, 1985; Niemeyer, personal communication, 1982). We understand that it was difficult for foreigners (and Chileans) to

visit the site. Between 1977 and 1988, Chile was under the rule of the Pinochet regime and was not an inviting country for casual travel or professional business. As for some Chilean colleagues at the time, we have been told through the years that travel to the south was too expensive and that a few chose not to visit because they wanted to avoid a possibly embarrassing situation. What if Monte Verde was not a site, as the late Junius Bird had insisted? Rather than visit to see for themselves, these colleagues avoided association and with it objective evaluation.

It is evident from his discussion and from his demeanor and tone in his review that Fiedel is convinced that Monte Verde is not a valid site. He feels compelled to offer reasons to explain the presence of human artifacts there. We believe a crucial problem with Fiedel's (n.d.) opinion of Monte Verde goes back to Bird's invited visit to the site. As we have explained in a previous publication (Dillehay 1989:x-xx), Bird was at the site for only two days. He was scheduled to arrive 2-3 weeks after excavation began, but he decided to come much earlier because he had been invited on a yachting trip in the Chilean canals. During his brief stay at the site, Bird helped excavate and screen the culturally sterile overburden. He never saw the underlying habitational surface dated to about 12,500 years ago. Although Bird correctly reported that he did not observe any artifacts at Monte Verde, he omitted mention of the full story: that he left before he could see the underlying artifact level (i.e., strata MV-6 and MV-7). He also failed to mention the overlying sterile overburden (strata MV-1 through MV-5). Instead, he led others to believe, such as Lynch (1990), that Monte Verde was not a genuine site and that the human artifacts there must be explained by other reasons.

Lynch has never visited Monte Verde, despite having been invited to the site on numerous occasions. In concurring with Bird's suggestion that Monte Verde was not a valid site, he imagined three scenarios to account for the presence of artifacts: 1) redeposition of an upstream Archaic site at Monte Verde; 2) filtering down of overlying Archaic artifacts into the lower Pleistocene level; and/or 3) the invention of the site by the research team. Fiedel follows Bird and Lynch in their thinking, and in their errors, by elaborating on these scenarios even further.

We stated in 1989 that the 1979 foot survey located "thirteen additional sites" along Chinchihuapi Creek (Dillehay 1989a:7). Two small sites were found upstream from Monte Verde. Both are intact and neither has been redeposited at Monte Verde. One site is Chinchihuapi, located on the south side of the creek, which is dated at approximately 12,600 years ago (Dillehay and Pino 1997:49). The other is an intact Archaic site buried in stratum MV-3, located on the north side of the creek, which dates between 8500 and 4500 years ago (see Pino 1989). This site contains very few lithics, all of which are made of raw materials completely different from those found in Monte Verde.

There are no Archaic materials washed out of an upstream site and redeposited in Monte Verde. There also are no overlying Archaic materials that filtered down into the deeper levels. We should add that the 1997 inspection team, which included Haynes, did not find an overlying Archaic level at Monte Verde.

Part 3: A Detailed Reply to Fiedel

The following rebuttal is organized in terms of different artifact categories, including the bone, wood, stone, and feature assemblages, and of mapping and interpretative issues. We correct major errors, omissions, and misrepresentations made by Fiedel in his attempt to dismiss all “compelling” archaeological evidence at Monte Verde. As the reader will see, Fiedel omits passages in Volume 2 to answer his own queries, misrepresents the scientific evidence to impart “inconsistencies” and “confusion” on our part, and compares statements in preliminary and final reports to create even more confusion. There are many more mistakes in his review that we choose not to correct below. Those provided are suffice to demonstrate the insignificance of Fiedel’s review.

Human Intervention and Mastodon Bones

In dismissing the human-induced cutmarks on mastodon bones, Fiedel misrepresents Pat Shipman’s analysis by selectively quoting from her chapter to fit his argument. In referring to the bones and photographs she inspected for marks, Fiedel quotes her as saying that: “I am unable to diagnose their origin with confidence because of the state of preservation of the bone (Shipman 1997:760)” (Fiedel 1999:3). He takes this sentence and extrapolates it to her entire analysis and manipulates the evidence by omitting the remainder of her conclusion:

Three specimens from Monte Verde and a series of macroscopic and microscopic photographs of additional specimens were inspected to refute or confirm the hypothesis that these bones were modified by humans. A tusk fragment showed microscope and gross modifications to its alveolar end that are consistent with [human] use-wear. Photographs of several bones that were not available for direct study showed marks that possess all of the microscopic morphology of known cut marks. Other specimens and photographs were ambiguous, showed probable trampling marks, or were too poorly preserved for reliable assessments. This study thus confirmed the hypothesis of human intervention on particular specimens--or at least failed to refute this hypothesis--while indicating that the bone assemblage as a whole had a complex taphonomic history. (Shipman 1997:765)

Her conclusion is entirely different from the impression of it that Fiedel gave the reader in his review.

Fiedel (1999:9) also questions the provenience of the soft tissues of animals recovered at Monte Verde, whether it is really “meat” found in the site, whether DNA was preserved, and whether DNA studies were performed on the tissues to identify them as mastodon soft tissue. He also asks: “If chunks of meat were lying exposed during occupation and after human abandonment of the campsite, one must wonder why they did not draw scavengers.

The beetle experts [Hoganson and Ashworth] who... studied the 956 specimens from Monte Verde puzzled over the absence of carrion or dung eaters” (Fiedel 1997:9).

In regard to the “missing” beetles, Fiedel conveniently leaves out the most important conclusion in the Hoganson and Ashworth report. They say on the same page (that contains his quote):

Why, then, are species that should have been attracted by human activity absent from the Monte Verde fossil assemblage? The majority of records obviously synanthropic insects from archaeological sites have been removed from thick midden deposits or dung-containing fills of long-term agricultural settlements (e.g., Osburne 1969, 1983). Their occurrences in specific types of deposits is not an accident... The situation at Monte Verde is quite different. None of the deposits that yielded insect fossils were those of a rich midden. The human feces, scrapes of meat, and other organic debris located during the excavation were scattered and not heavily concentrated in large pits or thick deposits. Given the distribution of the “bait,” the distribution of synanthropic insects would be decidedly nonrandom...and extraordinarily low...[and] water-logged scraps of meat and feces...would not have been attractive to the species that depend upon them...the archaeoentomologist Paul Buckland wrote (personal communication, 1986) that he was unable to provide an answer to the problem...and cautioned against making the assumption that anthropic insects had existed in the region at the time of man’s arrival and may not have arrived in the region and adapted to human populations until later in the Holocene... we might add that we have not found fossils of any potential synanthropic species in any “natural” fossil insect assemblages of the Pleistocene period we have analyzed from several samples collected throughout the region. (Hoganson and Ashworth 1989:226)

Why didn’t Fiedel inform the reader of this important concluding statement?

As for the meat, Fiedel is confused and misrepresents the evidence. First, two large chunks of meat or soft tissues were found in Monte Verde. Fiedel states that in a recent article Dillehay (1997m:30) noted that:

Outside the tent [in Zone D] we found two large communal hearths...Near the hearths, we uncovered two brown, sopping hunks of meat next to some mastodon bones. The final report mentions only one ‘single large piece of animal soft tissue, measuring approximately 25-by-25 cm in size...(Cibull and Geissler 1997:751). (Fiedel 1999:9)

Fiedel is confused. As expected, the two pieces had different measurements and contexts and were discussed as such in various publications. One specimen was recovered from Zone A, Area A, near the Wishbone Structure; the other was found near a hearth in Zone D. The latter piece was split or cut into two contiguous pieces and reported as one. Cibull and Geissler’s study examined only 38 macroscopic specimens available to them. Not all large

and small pieces of soft tissue were transported from Chile to the United States, so only Cibull and Geissler examined samples of some specimens. Given the expense in studying 935 macroscopic specimens and thousands of microscopic specimens, we subjected only 38 samples to extensive pathological, cellular, and histological analyses. Other samples were studied by means of amino acids and DNA studies, with mixed results as reported by Tuross (1997) and by Tuross and Dillehay (1995).

Fiedel is wrong in concluding that “there was no preserved DNA at Monte Verde (Tuross 1997:79)” (Fiedel 1999:9). Tuross states that DNA is present at Monte Verde, including both mastodon and human, but that the source of the DNA is not known (Tuross 1997:77-79). Amino acid detection of mastodon remains also is discussed by Tuross (Tuross 1997; Tuross and Dillehay 1995), contrary to what Fiedel says.

In dismissing the human activity associated with the mastodon bones at Monte Verde, Fiedel states:

As Dillehay (1997h:699) notes, finds of mastodon bones eroding from cut river banks in proximity to gravel deposits are not uncommon in southern Chile... A total of 414 bone fragments, nearly all from mastodon, were found at Monte Verde. To distinguish this particular bone concentration as the result of human activity, unlike those paleontological locales, requires unambiguous evidence of butchering, skinning, or modification into tools. Of the total 414 bones, Dillehay (1997h:725) recognized “clear” (as opposed to “probable” or “possible”) cut marks on only 6 mastodon bones...Another 29 bones had “probable” marks...[and] it must be emphasized that there are no whole or fragmentary bone or ivory points, awls, needles, fleshers, reamers, or ornaments at Monte Verde. There is nothing that looks even as superficially convincing as the bone pseudotools from Old Crow (e.g., Morlan 1978, Figures 5 and 6). (Fiedel 1999:3)

Here, Fiedel offers an unrealistic (and misleading) expectation of the site. He fails to point out that human intervention in the bone collection also can be documented by the minimum number of individual (i.e., MNI) animals and by the type, frequency, and spatial distribution of bone elements in the site (Casamiquela and Dillehay 1989; Dillehay 1997h). We say that: “the clear to probable cut marks on bones are expected, given that a small percentage of bones from some well-documented archaeological butchering sites show stone tool marks (Crader 1983; Guilday et al. 1962)” (Dillehay 1997h:732).

Fiedel does not mention that more than 80 percent of the mastodon bone collection are broken ribs and that at least six individual animals are represented at the site (Dillehay 1997h:746-748). Fiedel does not say that bone awls, needles, fleshers and other kinds of “pseudotools” like those found at Old Crow or elsewhere also do not occur at other late Pleistocene sites in the Americas. These artifacts are common features, however, at Archaic sites associated with bison, elk, deer and other big-game hunting.

In questioning the context of four side-by-side rib fragments in Zone A, Fiedel wonders whether a 1992 preliminary report (Dillehay 1992) on another possible group of side-by-side ribs in Zone D is the same as those in Zone A. He questions whether:

The eastern residential side [i.e., Zone D] [which] yielded... three broken ribs positioned side-by-side on the occupational floor of one structure” (Dillehay 1992:195) is the same set of side-by-side ribs found in Zone A. The 1997 report neither describes nor illustrates any other bone arrangement of this sort, so it must be the same one shown in the photograph and repeatedly ascribed to Zone A. (Fiedel 1999:9)

In 1990, when the 1992 article was presented as a paper at a conference and later submitted for publication, Dillehay thought that the three rib fragments in Zone D (Hut 8) also might have been placed side-by-side like those in Zone A, but after further study, realized that they were not as closely juxtaposed and arranged with the articulator ends reversed as those in Zone A. Thus, it was decided that the cluster of ribs in Zone D were not intentionally placed by the site’s inhabitants and did not warrant the same behavioral interpretation as those in Zone A. The three ribs of Zone D are shown and discussed (Dillehay 1997h:674-78), contrary to what Fiedel says.

Fiedel also questions whether the provenience of the side-by side ribs is in Area A or Area B in Zone A. We show in Volume 2 that the ribs stretch across both Area B (Units 12 and 13) and Area A (Unit 13) though the primary provenience is in Area B of Zone A. Figure 4.1 (Dillehay 1997b:61) does not include a Unit 13 in Area B. The “Unit 13” actually refers to A13, near the western edge of the bone scatter in Area B, Unit 12. Thus, Area B, Unit 12 and Area A, Unit 13 are contiguous and both proveniences are correct.

Exotic and Human Manipulated Plants

Curiously, once Fiedel was confident that he had successfully dismissed the “unprovenienced artifacts and inadequately mapped [tent] ‘structures’...[and] the lithics, bones, and microscopic residues,” he says that we are left with only “the peculiar suite of plant remains as the most convincing proof of a human occupation” (Fiedel 1999:3). Unsurprisingly, Fiedel discredits this evidence too. He states that “the overall distributions of the most common plant taxa, *Lycopodium* (club moss) and *Scirpus* (totora), are nearly identical (see Figures 13.3 b, c, [Dillehay and Rossen] 1997:361). Their spatial congruence thus only reflects simultaneous deposition by some single agency, not functional association in a human economy” (Fiedel 1999:3). What proof does Fiedel offer the reader to demonstrate how those two species were deposited in the archaeological record? He provides no evidence, just guesses.

Worse yet, Fiedel fails to mention in the same text other frequent exotic plants that have incongruent spatial associations. For instance, *Carex* sp. (n=1262) and *Gunnera* sp. (n=239) have numerous occurrences and have spatial distributions different from those of *Lycopodium* and *Scirpus*. So do fifty-five other plant species (compare Figs. 13.3 and 13.4). What can we conclude from this? In his attempt to discredit our findings, Fiedel

selected 2 of 72 species that have similar (“not exactly congruent,” as he says) spatial distributions to dismiss the cultural validity of entire plant assemblage in the site.

Fiedel does not and cannot effectively challenge the plant assemblage. The 72 species include exotic plants from all four directions from the site, including the aforementioned *Lycopodium* from the high Andean grasslands 50 km east of the site, and *Peumus boldus* from at least 200 km north of the site. It is not just the remarkable variety of the plant assemblage but many sub-components such as the four species of seaweeds, each from different marine habitats and seasonalities, eleven varieties of coastal sand dune and salt-marsh plants from 55 km west of the site, ten exclusively medicinal plants, and tubers, to name but a few examples, that all defy explanations of natural agency. No one with expertise in plant ecology or archaeobotany has challenged this collection.

Fiedel cites a single attribute--the physical condition of some plants--to further dismiss the assemblage. He quotes us as saying that: “the physical condition of the plant parts does not reveal much information about the nature of the cultural collection (Dillehay and Rossen 1997:357)” (Fiedel 1999:3). Fiedel does not mention that we employed several traits in combination--not just the physical condition--to analyze the plant assemblage, which is a standard procedure in archaeobotany. We say that: “Analysis of plant taphonomy, density, fragmentation [part of the physical condition], and spatial distribution can provide important data for evaluating the integrity [meaning human or natural agency in its deposition and distribution] of the archaeobotanical collection” (Dillehay and Rossen 1997:380). We also discuss why and how the spatial distribution and selection of plant parts strongly indicate “human intervention.”

In explaining his case for natural agency in the selection and deposition of exotic plants, he states that “transitory ‘nesting birds’ may have deposited them in their excrement” and left them in the site (Fiedel 1999:3). If this is the case, why isn’t the excrement of these birds found at Monte Verde where preservation of all perishable material is very good? And why aren’t the exotic plants found inside the excrement? How does he account for the presence of burned and unburned plant parts from the same and different species in the same hearth or cluster of plant parts? Do birds control fire like humans do? How does he explain the potato skin fragments and seaweeds in the cracks of wooden mortars? How could birds have brought whole, unchewed boldo leaves, measuring up to 5 cm in length to the site? Why do birds collect only the medicinal and poisonous parts of some species and bring them to the site? Why didn’t we find the same kinds of occurrences in the non-cultural areas we excavated? Why haven’t the geologists, palynologists, and phytolith experts working in the region over the past 35 years found similar exotic and selected plant species and parts in natural sites? (see Hoganson and Ashworth 1989; Heusser 1989). Why didn’t Fiedel cite our experimental and natural studies (Dillehay 1997a-m) on the selection and deposition of natural elements by birds and other animals in different depositional environments? Fiedel’s attempt to dismiss human agency in the plant assemblage doesn’t end here. He fantasizes that “mastodons migrated hundreds of miles (Hoppe et al. 1999)...which could account for the presence of some locally exotic plants (Fiedel 1999:3). How far is Fiedel willing to stretch the scientific data to support his argument? Why didn’t Fiedel turn his critical eye toward this unrealistic argument?

Wood Artifacts

Fiedel's misrepresentation of the wood artifacts is found in the following paragraph. Fiedel states that:

In photographs (Figure 8.16, Figure 8.18, Dillehay 1997e:206, and Figure 7.25, Dillehay 1997d:153), the grooved log is situated at the southeast corner of the [Wishbone] structure, at the open end of the wishbone, and the braziers A-2-5 and A-2-6 are adjacent to the inner face of the north wall. In drawn plans, however, the log is at the southwest, closed end of the wishbone (e.g., Figure 8.3), and the two braziers are in the open entrance on the east side. Even by assuming some consistent printing error that has somehow reversed all the pertinent photographs, I cannot reconcile the drawings with the photographs. (Fiedel 1999:11)

Fiedel cannot reconcile the drawings and the photos because he misidentifies the wood artifacts and their contexts. Figure 8.18 shows the Wishbone Structure after we had excavated architectural Phase 2 (in 1981 and 1983), the last use phase of the structure. The grooved log was excavated at the same time as the Phase 2 architecture and removed from the site for conservation treatment in the laboratory. Phase 1 was revealed in 1983 and excavated in 1985. The concentration of wood Fiedel identifies near the opening of the structure is not the grooved log in question but simply a different concentration of wood. The two different wood clusters in question are shown in Figures 8.1, 8.3 and 8.15. The wood pile he confuses with the grooved log is located to the left of the entrance to the structure. This pile is different in form, size, and location from the grooved log. The grooved log is located near the closed end of the structure, not the open end. Compare Figures 8.1, 8.3, 8.15, 8.16, and 8.18 (Dillehay 1997). This kind of mistakes typifies Fiedel's review.

Fiedel questions the provenience and function of the "tent stakes." In this case, he misidentifies and guesses the provenience of several stakes and then blames us for his error. That is, he says that:

The stake in the background [Fig. 7.51a, page 170; Dillehay 1997]...with a distinctive triangular shape, **appears** [our emphasis] to be the one shown in Figure 7.54f, with a provenience given as A-1-16. The foreground stake **seems** [our emphasis] to be A-1-15, shown as Figure 7.54e... Both stakes are attributed to the Wishbone Structure in Area A on page 170, it is stated [by Dillehay] that stakes were "placed against foundation timbers (Fig. 7.51a)" and on page 167, the stakes shown in this figure are intrinsic to a discussion of Hut 5 in Area D...[thus] if the two illustrated wood pieces actually derive from the Wishbone Structure, their function has been misinterpreted.. It also is noteworthy that Adovasio (1997:226)

describes “an overhand knot or clove hitch with auxiliary overhand knot tied directly to a stake that was left *in situ* at the site” (see Fig. 7.51a)...the knot seems to have been tied to A-1-15, which was removed from its original location and photographed (Dillehay 1997: Figure 7.54e). That photograph shows no knotted cordage attached. (Fiedel 1999:9-10)

This is one of the most blatant errors in Fiedel’s review. There were 82 stakes recovered from Monte Verde. Much like lithic, bone, and other tools, the wooden stakes also fit a general template and form. That is, they generally have curved bodies, cut and often burned tips, and bashed and flattened ends. Fiedel selects two stakes from the photographs of stakes in Volume 2 that appear to him to have similar forms and matches them (those shown in the foreground and background of Figs. 7.51a and with those shown in 7.54e-f, respectively), claims that these two are the same stake, and then accuses us of misrepresenting the evidence both contextually and functionally. **The two stakes in Figure 51a are not the same as those shown in Figure 7.54e-f.**

To show where Fiedel is wrong, the foreground stake in Figure 51a has a curved body and a pointed head, and it has a knotted reed tied around it. The alleged “same” stake in Figure 54e is straight, has a flat head, and is not associated with a knotted reed. (Fiedel is wrong about us removing the reed to photograph the stake. It is not the same stake and no reed was removed from it.) As for the background stake in Figure 51a, it is different from the one in Figure 54f. Although both have similar forms, so does the one shown in Figure 51b and many other stakes not shown.

In questioning the provenience of three wooden mortars and associated plant parts at Monte Verde, Fiedel mentions the different terms we applied to them over the years (i.e., “basin-shaped items,” “wooden mortar basins,” “wooden basins”). He notes that:

Dillehay reports potato fragments from “two wooden basins” (1997d:570), although Ugent (1997), the botanical analyst, refers to only one as the source of his samples. The mortars are also said to have yielded two pieces of ...seaweeds that are difficult to account for without invoking human agency (Dillehay and Rossen 1997:371). (Fiedel 1999:9)

Fiedel wonders whether the basins were located in Workshops 2 and 3 or Hut 3. He states that

the potato skins...Scirpus and Juncus seeds, appear to have been taken from a “long mortarlike implement” in the northwest corner of Hut 3 (Dillehay 1997e:188)...Its measurements are given as 120-x-18-x-6 cm (Dillehay 1997e:188), which corresponds to none of those given for the three mortars on page 162. It is in Hut 3, not Workshop 2...or Workshop 3. However, it is not depicted in any maps of Hut 3...[and] the associated lithics [manos] are unidentifiable. (Fiedel 1999:9)

Fiedel is misrepresenting the evidence. First, Ugent (1997:906) refers not to “one” mortar basin but to “a” basin. It is a generic designation like saying an area instead of Area A, for instance. Fiedel takes Ugent’s general statement out of context to imply the existence of only one mortar basin and thus to create more confusion. Second, we say on page 109 that “also present in the area [Workshop 2] were two grinding stones and a wooden mortar” (Dillehay 1997c). In referring to the mortars on page 162, we call them “wooden mortar basins.” When we discuss Hut 3, we say a “long **mortarlike** [our emphasis] implement that was found ...in the northwest corner of the hut” (Dillehay 1997e:188). Contrary to what Fiedel implies, we do not say that this implement is one of the three “wooden mortar basins.” We intentionally refer to it as a “mortarlike implement” to distinguish it from the other basins, despite the fact that it contained edible plant remains in its cracks. Because this implement was different in size, form, and context from the three mortar basins mentioned by Fiedel, we thought it warranted a separate description. Furthermore, this implement does appear in the northwest corner of Hut 3, contrary to Fiedel’s supposition (see Dillehay 1997d: Fig. 7.40).

Stone Tools and Points Take Precedence

A response to Fiedel’s comments on the lithic assemblage from Monte Verde was published in the SPECIAL REPORT (Collins 199:14-15) and there is no need to repeat it here. However, there are some important considerations to be reiterated. Monte Verde is not defined on the basis of its lithics. There are several absolutely unequivocally human-made stone artifacts from the site, and there are many more stones whose presence, form, or both cannot be parsimoniously explained as anything but the result of human behavior. In this latter group are numerous pieces that viewed individually or without their context are either equivocally of human authorship or would never be considered to be artifacts. In his study of these latter pieces, Collins was acutely aware of the responsibility being born because, as said, without its context, most of this assemblage would be overlooked archaeologically.

The crux of the responsibility that Collins felt in this study is that another discovery of such stones may lack associated features, perishable artifacts, and other external indicators of their cultural status. Thus, it was important to critically and carefully evaluate the lithics as an assemblage and establish independent criteria for evaluating the cultural status of the stones--criteria that might also apply to some future discovery. The final report (Collins 1997:383-506) demonstrated that at least 41, and perhaps as many as 56, stones are geologically exotic to the site; humans introduced these to the locality. The remaining “equivocal” stones at Monte Verde were distinct as a group from the local gravels both in lithology and in form. In kind of rock and in sizes and shapes, the stones found in the site are not a statistical reflection of the kinds, sizes and shapes of stones in the nearby stream beds. Stones with attributes like those found in the site were shown experimentally to be effective as tools. Taken together, these findings indicate that people selectively collected and brought to the site locally-available stones that possessed useful characteristics. Furthermore, many of these stones were modified before or during use and traces of use-wear could be discerned macroscopically as well as microscopically. For Fiedel or anyone to dismiss the stones from

Monte Verde as non-cultural, they must present a detailed, parsimonious, and **believable case** for natural causes that explain all of the attributes of these stones as an assemblage and in the context presented by Monte Verde. To state simply that these stones appear “dubious” as artifacts (Fiedel 1999:3) or that other than six pieces, “the items proposed as artifacts are equivocal, some highly so” (Haynes 1999:17) is not enough--in fact, that is the very heart of the problem that had to be solved. We do not see where Fiedel or Haynes has grappled with that problem nor offered a solution different than the one presented by Collins.

In dismissing the stone artifacts and their provenience, Fiedel quotes a sentence from Alan Bryan’s forward to the 1989 report: “Intentionally shaped stone artifacts were so infrequent [at Monte Verde] that it would be easy to hypothesize that all had been intruded from later occupations of the area [referring to the Archaic period]” (Fiedel 1999:4). Curiously, Fiedel does not provide the reader with the remainder of Bryan’s sentence, which is a

careful study of the stratigraphic and sedimentary context [referring to Pino’s geological work at the site] demonstrated that the artifact and bone bearing layer was overlain by an impermeable layer that not only prevented intrusion, but also had sealed the peaty cultural layer from the usual processes of decomposition and any further turbation by geological or animal action. (Bryan 1989:xv)

Why didn’t Fiedel cite the remainder of Bryan’s conclusion?

Fiedel also ponders “the long delay between the discoveries [of the projectile points] and the first publication (1988) of their photographs...the lack of reference to projectile points in several publications that appeared in the intervening period (e.g., Dillehay 1984 and 1988) [between the early publications and the 1997 report]” (Fiedel 1999:4). He continues that: “Since indubitably chipped stone was so rare on the site, the discovery of each of the three biface fragments and the basalt core must have occasioned considerable excitement” (Fiedel 1999:4). In building on Fiedel’s supposition, Haynes states: “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” (Haynes 1999:17). It is clear by these statements that points take precedence over other kinds of human evidence for documenting late Pleistocene sites. These statements reflect the profundity of North American centrism.

Consider this. If points are expected to be a rare and exciting find in early sites, then human footprints, well-constructed hearths with well-preserved food remains, worked and grooved wood, including tied stakes and burned wooden lances, chunks of meat, and the fallen remains of hut structures are even more rare occurrences and make even more exciting finds. Points are found every day in survey and excavation at many sites, and they don’t always excite all archaeologists the way they do Haynes and Fiedel. The perishables are truly rare finds in early sites. Indeed, some excitement was produced at Monte Verde during those moments when we found the footprints, meat, and other exotic artifacts and features. Little, if any, excitement was generated by the projectile point finds.

As for the earlier publications that don't mention the points, in the 1984 article, the editors of *Scientific American* requested line drawings of the stone tools after they saw the lack of detail on the stones in photographs. One projectile point fragment (A-1-26) was submitted for drawing, but it was not included in the publication because it was too small and fragmented and thus uninteresting. Moreover, it was not mentioned in the 1984 text, because Dillehay thought it was secondary to other more important cultural materials and patterns in the site.

In the 1988 publication mentioned by Fiedel, we focused on the preservation of organic remains, not the lithics, in the site. This publication resulted from a conference on wet site archaeology that was sponsored by Barbara Purdy at the University of Florida (Purdy 1988). Not only were the points not discussed in this and other early publications on Monte Verde, but other important non-perishables also were not mentioned. In fact, only seven lines in the wet site publication were given to stone tools (Dillehay 1988:195).

In turning to other errors and misrepresentations made by Fiedel in his comments on the stone tools, the following is a case whereby he mistakes different artifacts and leads readers to believe that our provenience records were wrong and confusing. He says that:

A handaxe-like artifact was found by Carlos Troncoso in 1976, amid slumped material eroded from the creek bank...Its discovery thus preceded Dillehay's involvement at the site. However, in an alternative version of the find... Dillehay reports that he and his students "uncovered ... a bifacially flaked chopper made of quartzite...an irrefutable stone tool." (Fiedel 1999:4)

Fiedel is wrong. In discussing Troncoso's work, we say that he "removed several small bone fragments and one large bifacially flaked tool made of quartzite" (Dillehay 1989a:4). We also say that: "In 1977...we uncovered...a bifacially flaked chopper made of quartzite" (Dillehay 1989a:5). These are two different tools found by two different excavators during two different seasons. The former was found by Troncoso in 1976 and is made of a laminated gray quartzite (Collins 1997:424, Fig. 14.16). The second is a pink granite (that could easily and was mistaken for quartzite in the 1989 archaeological report; see Collins 1997:428, Fig. 14.20a) that we excavated in 1977. Both were found in Zone A. We never said that the pink "quartzite" biface was the same artifact as the gray laminated biface excavated by Troncoso. In this case, Fiedel confuses the terms "quartzite" and "biface," and "pink" and "gray" in all instances to show confusion on our part in documenting the finds.

The next set of artifacts Fiedel dismisses is the flake facets on the laminated biface and other lithics and their possible correspondence with facets on worked wood, which we believe are related functionally in the site. He also questions the provenience of these artifacts. To deal with the provenience matter first, in associating the laminated bifacial chopper and its facet traits with a chopped log in Hut 3, Fiedel states that a "map [Fig. 20.1] does not accurately reproduce the appearance of Hut 3 as depicted in photographs" (Fiedel 1999:4). He is wrong again. This map does depict Hut 3 accurately and the placement of the wood in question (see Dillehay 1997e,i: Figs. 8.4 and 20.1).

Fiedel believes that the form and size of the facets on the bifacial chopper and those on different specimens of worked wood are different and that the laminated quartz biface could not have been used to chop or cut the wood. Fiedel casts doubt not only on our interpretation of artifact function and association but also on the provenience of these artifacts. For example, he notes that:

The other [wood] pieces allegedly worked by the biface are reported to be illustrated in Figure 7.13a (DW-1-4-4) and 7.23a. However, the latter figure shows a piece with a groove that is attributed not to the quartzite biface, but to a large percussion flake, specimen D-11-1-1, found 10 cm away from it... A photograph and drawing of the biface agree in showing both ends as less than 3 cm wide, so it is impossible to match them with 5 cm-wide cut facets in the wood...This casts doubts on Dillehay's other claims to have matched cutmarks to the used edges of three lithic "tools" found in close association. (Fiedel 1999:4-5)

First, Fiedel is wrong. We never say that the laminated quartz chopper was used to make the grooved specimen D-11-1-1 (compare Fig. 7.13a and Fig. 7.23a: Dillehay 1997d:145 and 149). He confuses the "bifacial chopper," which is designed to chop, with the flake (shown by the arrow in Fig. 7.23a), which is designed to gouge and scrape, and which is shown in direct association with the grooved wood. Moreover, close inspection of the two figures shows that the chopped wood with facets is in the lower right hand corner of Figure 7.23a. We do not confuse or point the reader to the grooved wood depicted in the upper half of the photograph (see Fig. 6 in Part 6 below). In fact, if we had meant the grooved wood, we would have cited Figure 7.23b, which shows a close up of these specimens and its chopped not grooved facets. Further, the chopped wood in Figure 7.23a is the same wood specimen in Figure 7.13a, contrary to what Fiedel says.

Second, Fiedel apparently knows little about stone tool technology and the marks they leave in wood. He measured the width of the "edges," not the edge-end width of the chopping tool. The edges of the bifacial chopper are indeed about 3 cm in width. However, when a tool such as the chopper cuts into wood, it is the edge that first penetrates, followed by the wider interior end or distal body, the latter of which widens the path of the cut as it penetrates deeper into the wood. Thus, the edge of a tool 3 cm in width and its adjacent interior body of 5-7 cm in width will leave a cut facet of 5-7 cm, not 3 cm in width, as Fiedel believes. If Fiedel had been careful enough to check his facts, he would have seen that the chopping facets on the wood and the distal edge-end facets on the bifacial chopper in question are both about 5 to 7 cm wide and thus correspond. Further, although it is a minor point, we do not conclude that the bifaces were used to chop all wood in the site. We infer and simply say they may have been used to produce the kinds of facets seen on most of the chopped wood. We believe these errors by Fiedel are due in part to his hasty and biased review of the final report and to his ignorance of lithic tool technology and morphology.

Fiedel questions the location and association of stone artifact D-11-1-1 and a nearby grooved branch (the same branch discussed above; see Fig. 6 in Part 6 below). He says that:

Although this branch is said to have been located 2 m east of Hearths D-9-5 and D-8-17, and a photograph (Figure 7.23a) shows it as seemingly oriented northwest-southeast at the creek edge, there is no manio piece of corresponding length, material, and orientation, mapped in this vicinity in Figure 8.2 or 8.4 ([Dillehay] 1997e:174-5), nor is there any lithic in Figure 20.1 ([Dillehay] 1997i:775) that might represent D-11-1-1. Scatterplots show no planing or gouging in this sector (Figures 15.50 and 15.52, also Tables 15.30, 1997g:609). (Fiedel 1999:8)

Fiedel is wrong. The wood is about 2 m east of Hearth D-8-17 and there is a wooden branch approximately 45 cm in length in the correct place on the maps. The wood maps should depict it as manio, but the symbol mistakenly shows luma not manio. The latter is an editorial oversight on our part (see Part 4). Given the scale of the map and the location of the lithic 10 cm from the branch, the computer superimposed the two specimens in question and the lithic unfortunately does not appear. However, this flake is one of the tools shown in the far eastern side of Figure 15.41a. Why doesn't Fiedel mention its location in this figure?

Fiedel questions the provenience of two "crude axelike bifaces." He states that

the 1989 report mentions both "large bifaces made of exotic basalt and quartzite" ... distinct from ... "bifacially flaked, willow-leaf shaped tools" (Dillehay 1989a:15). Collins (1997:401, 427) describes two "choppers," one (DW120410) from Zone DW that he regards unequivocally as artifactual, the other (B0704008, from Zone A) as a probable geofact. (Fiedel 1999:6)

Fiedel asks if we are confusing the two bifaces and what provenience they have. Fiedel dismisses one biface, specimen B0704008, as a "geofact." After dismissing this specimen, he attempts to replace it with another biface, one he attempts to "find" elsewhere in the site.

Fiedel fails to note that, on the very same page from which he took Collins' quote about the "geofact," Collins also states that "it seems best to regard this specimen [B0704008] either to have been a naturally fractured and rounded pebble selected for its size and shape characteristics, brought to the site, and lightly used, or, as previously inferred, to have been culturally flaked and used heavily enough to batter and round its edges" (Collins 1997:401). So, as it turns out, Collins did not just ponder whether this specimen was a geofact, but concluded that it was probably used and modified by humans. In trying to build a case for more inconsistencies on our part, after dismissing biface B0704008 as a geofact, and thus requiring the presence of another biface to replace it as the "second" biface, Fiedel looks for it by saying that

this leaves the two "hand axes" [bifaces] found near the Wishbone Structure to be identified. One is surely the quartzite preform. The second corresponds to none of the artifacts now attributed to Zone A [because Fiedel has now eliminated one as a

“geofact”]. The only large, flaked tool of exotic basalt, resembling a handaxe, is the core (PZ-43-3) now ascribed to the early MV-1 assemblage. (Fiedel 1999:6)

In his search for a replacement biface, Fiedel leads the reader from Zone A, where the two original bifaces are located, to Zone C across the creek, and encourages the reader to believe that we are mistaking bifaces from different zones and from different time periods. In fact, it is Fiedel who is confused by dismissing the second “biface” as a “geofact” and by replacing it with a “core” in Zone C, not Zone A.

In discrediting projectile point X150001, Fiedel questions its provenience and why it has three artifact inventory numbers. We explained the artifact numbering system in an earlier response to Fiedel (Dillehay et. al. 999) and in Part 2 above and will not reiterate it in detail here. In brief, the original number of the point was X150001. This number reflects its *in situ* provenience in Test Pit 15 that was excavated by Hernan Vidal, and Argentine archaeologist, early in the 1983 field season. Later in that season, Test Pit 15 was incorporated into an expanding Zone D and the point’s number was changed to D-10-1-1 and D-10-1-2. After extending the Zone D grid system into the Test Pit 15 area (not to be confused with a Test Pit 15 on the south side of the creek in Zone X-2), the “new” point provenience was located on the line between the arbitrary units D-10-1-1 and D-10-2-1. We then referred to the point provenience as both D-10-1-1 and D-10-1-2. Later, when we computerized the artifact inventory numbers for spatial and statistical analyses, these dual unit numbers for the same specimen were rejected by the computer, so we changed the number to D-10-1-1. As we mentioned earlier, there are other instances like this in the site and in the site report. The *in situ* position of the point is shown in Figures 1 and 2 in Part 6 below.

Fiedel says that “the 1985 work occurred only on the western side of Zone D...a photograph ... shows that features... located at the far eastern end of the excavation were exposed prior to clearing of debris from the central ‘huts’ in Area D in 1983” (Fiedel 1999: 4). He complains that Point D-10-1-1 cannot be seen in photographs of the surface of units in the D-10-1 area. And he says that the “1984 *Scientific American* article contains a map of the structure in Area D, excavated in 1983, but there is no mention of any projectile point (Dillehay 1984)” (Fiedel 1997:5).

Fiedel is wrong. We say that “the west side, Area DW, is where **most** [our emphasis] of the 1985 excavation took place and where most revisions are” (Dillehay 1997e:182). In 1985, we continued to test and reexcavate all areas. We state that: “The last field season [1985] was designed (1) to complete the excavation of the main residential zone, Areas A, D, and DW” (Dillehay 1989a:10). We also say that we excavated on the Southside of the creek during that same year (Dillehay 1997b:71-72).

As for the photographs (see Figs. 6.19 and 8.9, Dillehay 1997c,e: 103 and 182) showing the surface of the D-10-1 unit and surrounding units and not depicting the point, they depict wide angle views of general excavation features, not specific artifacts. Their scale of the photographs is simply too large to show small artifacts. And in regard to the 1984 map published in *Scientific American*, it is a schematic drawing of hut outlines in all zones and shows no artifacts. By pointing out that no projectile point is depicted in the 1984 article and

in the general maps of the 1997 report, Fiedel leads the reader to believe that we are not properly documenting the evidence. If he had not been so selective in his use of our publications, he could have corrected his own error in this and many other instances.

A related matter typifies Fiedel's play with the evidence to create more confusion. In pondering the provenience of D-10-1-1, he guesses at a blurry artifact number in one photograph of the point, designating this number as X-12-1 when, in fact, it is X1500001 (Fiedel 1999:5). By introducing another "number" into the provenience and questioning it, he creates more confusion.

In continuing his discussion of Point X1500001, Fiedel says that:

The point is not depicted in the scatterplot of basalt artifacts, in Area D ([Dillehay] 1997g:601). There are no bifaces included in the cluster analysis of lithics from Area A ([Dillehay] 1997g: 634-640; note particularly Figure 15.66, showing clusters of basalt artifacts). Table 15.32 ([Dillehay] 1997g:614) locates one biface in Hut 4, one in Workshop 1, one in the vicinity of the Wishbone Structure, and one in Areas A-C. By a process of elimination, only X1500001 could possibly be the specimen in Hut 4; but the description of this hut's contents ([Dillehay] 1997e:190) mentions no bifaces among the lithic artifacts. (Fiedel 1999:5)

He is incorrect. Figure 15.41 shows an asterisk just outside the boundary of unit D-10-1-2 where the computer analysis placed it. Also, in comparing Figures 4.1 and 15.41, the point location is correctly placed. The point occurred on the boundary between Workshop 4 and Hut 4, so statements regarding its functional association are relevant to both areas.

As for the cluster analysis, he fails to inform the reader that there also are no basalt artifacts shown for Groups 1,4,6,7,25, 26,27,28,29, and 30. The reason that these groups and the biface(s) are not depicted in the cluster figures is because this analysis did not include lithic groups with fewer than 7 specimens. Why didn't he tell the reader this important fact (or is it that he didn't understand the analysis enough to know why smaller sample groups were not included)? He also says that "although 41 lithic and ecofactual artifacts are reported to have been found in this area [Workshop 4], there is no mention of a projectile point (Dillehay 1997d:201)." The descriptive statement he cites is very general and not intended to detail all artifacts in the area. In this same general statement, not only do we not discuss the point, but we also do not mention the specific kinds of scrapers and other unifacial tools found in this area.

Fiedel (1999:6) also questions the provenience of point fragment A-1-26. This point was found near the Wishbone Structure in Zone A. As stated earlier in the section on "Wood Artifacts," the Wishbone Structure is made up of two distinct construction phases: an earlier underlying Phase 1 and a later overlying Phase 2 (Dillehay 1997e:203-10). We first found the structure in 1981, at which time we only exposed it. When we returned to the site in 1983, we excavated Phase 2 of the structure and its surrounding habitational or use surface. We excavated Phase 1 in 1985. Point A-4-06 was excavated by Jack Rossen and Gaston Munoz in 1983. It was recovered from the habitational surface near the outside northwest corner of the structure (see Figures 3 and 4 in Part 6 below).

In questioning the location of Point A-1-26, Fiedel states that: “Several photographs (e.g., Dillehay 1997e:8.15, 8.18; Dillehay 1984: 107) show the Wishbone Structure during excavation, but the A-1-26 biface is not shown. It is not depicted on maps of the Wishbone Structure)” (Fiedel 1999:6). Figures 6.1, 8.3, and 8.6 show only features, wood types, and structural remains, respectively, not artifacts, so we should not expect the point to be illustrated in them. Further, this point is approximately 2.5 cm in length and cannot be seen in the large-scale photographs, such as Figures 8.15, 8.16, and 8.18 (Dillehay 1997e:204, 206, 207; see Fiedel 1999:6), which shows all of Zone A and some of its features. As stated earlier, no specific artifacts are seen anywhere in these photographs. The captions of these photographs and drawings clearly state their individual purposes, but Fiedel distorts the evidence to make the reader believe that something should be there when it should not.

The point is shown, however, in its correct position in the northwest corner of the structure in Figures 15.39 and 20.1 (Dillehay 1997g,i:596 and 775, respectively). We point out in the text that “Five other stone tools were found nearby, including one of the broken projectile points [A1-26]” (Dillehay 1997e:204).

Fiedel commits another error in noting our cross reference to features near the point and the Wishbone Structure. He states that in one description we say the point is located “1 m to the southwest, adjacent to the north side of the western prong of the structure” and that in another description we say it is “associated with several stone artifacts” and “Hearth B-1-1” (Fiedel 1999:6). All of these proveniences are correct. In different descriptions of the Wishbone Structure, we discuss the horizontal association of the point with different features and artifact clusters, whether they be hearths, clusters of wood, bones or stones, or architectural debris.

In turning his attention to the third point fragment, Fiedel questions its association with mastodon bones and with the site in general. He says:

The provenience as reported to Kay (1997:659), is from the C-2 cutback profile in Area DW, Zone D, at the southern end of Workshop 1 near hut partitions 8 and 12; one paleocamelid scapula and three mastodon ribs are in the nearby vicinity. Those bones, it should be noted, had been found ten years earlier; and whatever ribs are meant, none are listed from the same provenience as the scapula (Dillehay 1997h: 676), nor illustrated in the plan of this vicinity (Dillehay 1997h:676, Figure 17.12). (Fiedel 1999:5)

Fiedel is mistaken. We say (Dillehay 1997b:72) that: “From 1985 to 1993, ... visits [to the site] have produced two additional bone fragments and one projectile point fragment [the point in question].” These three fragments, as well as one other small rib fragment, were found eroding from the creek bank where the scapula was recovered. So Kay, in making a passing comment on the general vicinity of the point in his use-wear chapter, is correct in associating the point fragment with the bones in Area DW. The three bones in question are thus discussed in the 1997 report and they are listed in Table XIV.1 (Dillehay 1997k), contrary to what Fiedel says. Pino and Dillehay found this point eroding from the north creek embankment in Area DW in 1993.

Fiedel complains about disagreements between Collins' morphological study of the lithics, Kay's micro use-wear study, and Dillehay's micro use-wear analysis. To Fiedel, this implies more confusion, though it is entirely normal for investigators studying the same stone tool assemblage from different perspectives to disagree on its function. Why doesn't Fiedel mention the numerous **agreements** between these same studies? Instead, he prefers to focus on ambiguities and incongruencies to cast further doubts on the project.

As for the questioned provenience of the slate perforator (Fiedel 1999:8), it was found in Zone A, Area A13-1 in 1985 when we were excavating the remaining portion (Phase 1) of the Wishbone Structure. This tool was found in floor fill by the Argentine archaeologist Herman Vidal, when we were cleaning and bagging the habitational use surface and midden in Zone A, after we had completely excavated Phase 1 of the Wishbone Structure.

Fiedel (1999:8) says that Tables 15.24, 15.25 and 15.32 do not list the perforator as a slate artifact in Zone A. This is an editorial oversight on our part for which we apologize. But why doesn't he point out that it is mentioned in other tables dealing with artifact function and context? Those tables are Tables 14.3, 14.4, 14.8, 14.14, 15.6, 15.31, 15.52, 15.33, 15.38, 15.39, and 15.40 (Dillehay 1997g). Tables 15.38, 15.39 and 15.40 are frequencies and percentages of Collins' Groups for a Five-Cluster K-Means solution in Zone A. These data accompany lithic scatter plots shown in Figures 15.68, 15.69, and 15.70. By not mentioning these latter tables, Fiedel emphasizes the absence, not the presence, of corroborating evidence.

In questioning the hafted scrapers at Monte Verde, Fiedel cites selected passages from several general articles dating back to the mid-1980s (i.e., Dillehay 1984, article in *Scientific American*). For instance, he says that: "The 1997 report contains dispersed references to these artifacts, which adds to the uncertainty about their number and provenience" (Fiedel 1999:7). He further states that we mention three hafted scrapers and that: "In those publications, we refer to the hafted artifacts as 'stone tools' and 'stone scrapers,' and the wooden hafts as 'hafts' and 'handles' (cf. Dillehay 1997d:159; 1984:112; 108, 112)." By constantly pointing out our use of "different" terms over the years, Fiedel creates more confusion and inconsistency. He fails to note that the earlier published terms are general and as our ideas about tool function became clearer as a result of more analyses, our ideas about the function of these tools changed.

In this same context, Fiedel questions the provenience and function of one scraper by saying that

specimen B-8-18...a hafted basalt scraper with bitumen and wood polish (resulting from hafting) situated on the proximal end...[and]...adhering bitumen fragment [B-8-18] shown in the accompanying microphotograph... the relevant text surprisingly reveals no knowledge that the tool previously had been stuck to a haft with this material...The photograph indicates, as does this description, that bitumen was found on the lateral edge of this artifact, not its proximal end, as stated on pages 564 and 573 (Dillehay 1997g). (Fiedel 1999:7-8)

The tool in question was both hafted with bitumen and used to scrape bitumen. The tool had bitumen only on its proximal edge, not the distal end, which is slightly broken (and not shown in Fig. 15.12 because the damaged side appears on the unphotographed side of the specimen). We never say that the lateral side is worked or has bitumen. In this case, Fiedel guesses from the position of the arrow in the Figure 15.12 that the lateral side displays use-wear when, in fact, it is the proximal side. By mistaking the lateral and proximal sides, however, he builds a case of mistaken identity on our part. Lastly, we committed an editorial oversight. B-8-18 was hafted on the wood specimen shown in Figure 7.33e, not on the one shown in 7.33g (see the errata list in Part 4).

In the 1984 *Scientific American* article, we thought that there may have been three, instead of two, hafted scrapers. After further micro use-wear and morphological studies, we realized that one specimen was not a scraper and that it was not hafted to an adjoining piece of wood. After realizing this in 1988, we no longer referred to three hafted scrapers.

Fiedel concludes his section on hafted scrapers by saying that “despite the inconsistencies, it appears that a hafted scraper may have been found in Area B...it is also stated [by Dillehay] that: ‘One specimen recovered from Area A... [has]...no description or illustration of a handle or attached stone... in the 1997 report’ ” (Fiedel 1999:8). Fiedel’s statement is true in this case: we did **not** [our emphasis] illustrate or describe this tool. However, he does not inform the reader that we did not illustrate and describe every other single stone, wood, and bone specimen found in the site. If we had done this, the report would have been impossibly long for publication.

Regarding the provenience of the MV-1 core or specimen PZ430003, Fiedel misrepresents its provenience and misleads the reader once again. Fiedel states that: “This unifacially chipped basalt core...[whose] provenience...[is] PZ-43...[was] excavated in 1979... and ... is reported to have been found in direct association with one of the three ‘features’ found in this part of the site: ‘Specifically, one hearthlike feature of charcoal was found in direct association with a split basalt core’ (Dillehay 1989a:7)” (Fiedel 1999:6).

Fiedel is mistaken. First, the units in question were excavated not only in 1979 but also in 1985 (see Dillehay 1997b:71-72; see earlier comments about the excavation seasons in this paper). Second, he confuses the “unifacially chipped basalt core,” which he says “is reported to have been found in direct association with one of the three features found in this part of the site” (Fiedel 1999:6), with a “split basalt core.” Both were found in Zone C and reported separately by Collins (1999:460-65). The split basalt specimen of Collins Group 27 is shown in Figure 14.73b. It is a single faceted stone “split into two halves.” Collins (1997: 463) states clearly that these stones are “pebbles split in half by percussion.” This is not the core found in PZ-43, which is chipped (not split), and should not be associated with “a feature in TP 43,” as Fiedel (1999:7) contends. In this case, Fiedel does not pay attention to the morphological characteristics and to the written descriptions of these two distinct pieces with distinct contexts and becomes confused.

In regard to the inventory numbers for flakes in Test Pit 45, Fiedel wonders why specimen “PZ-45, MV-8, No.1” was “labeled as No. 1,” if it had been found six years after the pit was first excavated in 1979 and another artifact found before it. The answer is simple and relates to the MV-8 number above. When this specific artifact was excavated in 1985, it

was resting on a dark bluish stain that appeared to be a slight rise in layer M-8, which lies immediately below the gray MV-7 layer containing the MV-2 artifacts (including PZ-45, MV-8, No.1) on the south side of the creek. We thus assigned it a different number, in this case MV-8, No. 1, to distinguish it from other artifacts in the pit. Once the pit was completely excavated and we dug deeper to document the underlying stratigraphy in greater depth and expanse, we discovered that the stain was not part of stratum MV-8, but simply a localized and degraded peat lens that happened to appear similar in color and texture to the MV-8 layer.

Fiedel (1999:6-8) questions our reasons for assigning discontinuous and duplicate numbers to artifacts in Zone C. As for non-sequenced artifacts in Zone C, we assigned numbers to everything found in the deeper pits of this area, including the few small natural clasts (< 2 cm in diameter) occasionally recovered in some pits, thinking that some could have been exotic manuports or perhaps artifacts. Nearly all pits in Zone C, where few materials were found, were excavated and cataloged in this manner. And duplication of some artifact numbers (e.g., PZ410002) is because some artifacts were broken and the two conjoining pieces were given the same number and later glued together.

Fiedel continues. He says that in Figure 10.33, Dillehay depicts the:

“Use layer of MV-1 component showing three lithics *in situ* [Dillehay 1997f:305].” The unit is not specified, but the lithic objects are situated near the north end, and thus, according to the plan (Figure 15.58), this can only be Unit 25. This identification is confirmed by the unique shape and medial ridge of the westernmost piece, which matches the multifaceted stone in Figure 14.76 (b) (Collins 1997:465). The latter figure even shows the label, ‘R-PZ-25 (7?).’ It seems that the photograph, Fig. 10.33, has been reversed, like so many others in the volume, since Figure 15.58 shows both numbers 7 and R7 on the eastern side of the unit. In fact, artifact R-7 is shown in the plan as situated in the southeastern corner of Unit 25, not along the north wall. (Fiedel 1999:7)

Fiedel continues with a longer commentary about this piece and others in Test Pit 25 and questions their provenience, thinking that he is building another case of inconsistency and confusion against us.

Fiedel is mistaken. There is another test pit with three artifacts located close to the north wall. This is Pit 42, which is the one depicted in Figure 10.33. It is not Pit 25. By guessing the pit number and by associating the flake in the left corner with the flake with a medial ridge shown in Figure 14.76, he misrepresents our evidence and builds a false case against us. The flake depicted in Figure 10.33 is not the same flake shown in Figure 14.76. Collins’ Multifaceted Flakes group, to which flake R-PZ-25 (7?) belongs, and the Single Faceted Flakes group are collectively comprised of 18 stones, twelve of which have medial ridges. The reader should compare Figures 14.74 and 14.76. In his haste to find our “mistakes” (Fiedel 1999:8), Fiedel makes them himself.

He questions the “three alternate designations” or artifact numbers (i.e., PZ-43-1, X-E-1, X-1E-1) for the basalt core. The Xs refer to an earlier stage of the project when the high

ridge on the south side of the creek was designated as Area E (see the later explanation in Part 2 of zone and area designations). An archaeological Test Pit 15 was located on the north side of the creek where Zone D is situated. A second Test Pit 15, this one being a geological trench that eventually yielded two artifacts (PZ150707 and PZ15063), was placed on the south side of the creek. Contrary to what Fiedel says, this pit is shown on Figure 15.58.

Fiedel concludes his comments on the south side of the creek by saying that: “The mapping of Zone C seems to have been haphazard, therefore the uncertainty regarding the core’s provenience is not surprising” (Fiedel 1999:7). We grant that some confusion is produced by the sequential numbers and different pit numbers, but this nomenclature becomes clearer when one understands the field strategy as we outline it in the second volume. Most of the inconsistency and “uncertainty” is produced by Fiedel. His mistaken identity of the chipped basalt pebble for the split pebble, of Test Pit 25 for Test Pit 42, of some features for other features, and so on produced the confusion.

Yet another case of alleged confusion on our part in the lithics relates to Collins (1997:429), who mentioned a total of “14 flakes” but who studied only the six that Dillehay showed him, although another eight were said to be found by Dillehay and Collins (1991:337; see Fiedel 1999:6). Dillehay had only six of the 14 flakes in his possession at the University of Kentucky when Collins studied them. The remaining flakes were in Chile. Although Collins never saw the other eight flakes, he did study the rest of the entire lithic collection.

Micro Use-Wear Evidence

Fiedel also questions the micro-use wear evidence at Monte Verde because the presence of exotic materials (e.g., seaweeds and other imports) implies human agency. In rejecting this evidence, he selects a specific passage to support his argument

the presence of microscopic traces of wood and other organic residues on the edges of unshaped stone “tools” seem convincing at first, but Dillehay (1997g:507) admits that, “Some are simply loose particles in the use surface that have been attracted and attached to the sharp edges of stones by electrostatic and kinematic forces in post-depositional times.” His arguably strongest cases for activity-derived residues are the stone “tools” allegedly found lodged in grooved wood pieces...However, this evidence actually is weak, because the claimed association of the lithics and grooved wood pieces is inadequately documented. (Fiedel 1999:3)

Fiedel is wrong. He misrepresents the evidence. The provenience of the artifacts, particularly the grooved pieces of wood (see discussion above), is not inadequately documented. It is his error in misidentifying the contextually associated grooved wood and flake used for gouging and planing it that led to confusion. He also fails to point out that not

all human-produced wear is determined by residue analysis. Wear patterns and spatial contexts are also important indicators of human activity.

Fiedel does not provide the reader with additional evidence in the use-wear chapter. For instance, in regard to the seaweeds, we state that “the worked edges of some lithic artifacts possessed traces of exotic seaweeds and other plant species, which are rare occurrences in use surfaces and features. The linkage between exotics and worked edges argues against the fortuitous adherence of articles” (Dillehay 1997g:513). We also say that: “Once the residues were removed from these stones, a number of edge-wear attributes were studied, including edge microchipping, striations, rounding, and polishes. Thirty-six (55.3 percent) of these showed strong agreement between residues, context, microwear, and gross morphology in estimating function” (Dillehay 1997g:595).

Context of Radiocarbon Dates

Fiedel notes that we first published radiocarbon date OXA-105 as 11,800 \pm 250 b.p. (Dillehay 1988:183) and later reported it as 12,000 \pm 250 b.p. (Dillehay and Pino 1997:44). The same is true for date OXA-381, reported initially as 12,090 \pm 150 b.p. and later as 12,450 \pm 150 b.p. (Fiedel 1999:10). Both dates were processed by John Gowlett at the Oxford Accelerator Lab. In 1988, Gowlett first reported the dates as 11,800 \pm 250 b.p. and 12,090 \pm 150 b.p., respectively. The laboratory later corrected the dates and reported them to us as 12,000 and 12,450 respectively (J. Gowlett, personal communication, 1989).

In regard to the first date, Fiedel questions its provenience. He states that its “provenience is consistently presented as Zone A, Area B, Column 9, Unit 5... However, Figure 6.2 in the 1989 report shows its location is about 2 m farther west, around unit C-7 or C-8” (Fiedel 1999:10). If the reader compares Figures 3.4 and 6.2 in the 1989 report (Dillehay 1989a-c), it can be seen that the bone in question is indeed located in the correct contextual position, contrary to Fiedel’s accusation.

Fiedel questions the location and function of the mastodon tusk tools. Here again are several errors on his part. To give one example, after referring to one tusk gouge, he states that:

The provenience of the other tusk gouge, which is said to have similar distal wear, is unstated [which is not true, because we provide proveniences for all of them in Table V.1 and/or in their artifact number]. Perhaps it is the specimen designated as T-5A...and illustrated in Figure 17.10b (Dillehay 1997h:673). However, that piece has lateral, not distal wear. (Fiedel 1997:10)

Fiedel is incorrect. It is Figure 17.10a, not Figure 17.b that he should have cited. The latter has the distal wear. Fiedel selects “b” to suggest confusion on our part.

Fiedel questions the association of radiocarbon date TX-3208, Point A-1-26, and Hearth B-1-1. He notes that the

midsection A-1-26 is reported to have been found either in the vicinity of Brazier A-1-2 or next to Hearth B-1-1 (Dillehay and Pino 1997:47-8; Dillehay 1997e:211). The latter hearth produced the sample that yielded the TX3208 radiocarbon date. This sample is said to have been “in direct contact” with a mastodon bone (Dillehay 1982:549; Dillehay and Pino 1997 989:139). However, the 1997 descriptions of Hearth B-1-1 mention associated stone artifacts and worked pieces of wood...but no associated pieces of bone. (Fiedel 1999:10)

Fiedel is wrong. First, we do not say “pieces of bone” but a “bone.” Furthermore, located within a short distance of the point midsection were the Wishbone Structure, Hearth B-1-1, and Brazier A-1-2. We say on page 212 (Dillehay 1997e:212) that: “The hearth [B-1-1] comprised charcoal fragments, seeds, burned fragments of leaves, and other miscellaneous organic remains. Other burned remains were scattered around the edges of this feature.” “Organic remains” refers to and includes small bone fragments. In fact, one bone is listed in Table XIV.1 for Hearth B-1-1 (see Dillehay 1997k:932). It is 7.63 cm long. The reader should also examine Figure 17.11, which shows a small black dot. This represents a bone contained within Hearth B-1-1 (Dillehay 1997h:675).

All of the artifacts and features Fiedel questions with regard to Point A-1-26 are located within 2 m of each other and pertain to the same limited activity area around the structure, so when we say that the point and the date are associated with different features, we mean the limited activity area around the structure. This kind of association is very common in archaeological descriptions of activity areas. The term “direct contact” means **near a feature**. It does not imply contained within a feature. Fiedel conveys a sense of contextual and association confusion by citing different passages referring to different associations and different materials without explaining that they belong to the same limited area and are functionally related.

Fiedel questions the location of the radiocarbon date TX-5375 taken from a manio stake. He states that “the provenience of the northwest corner of Area DW, about 5 m away from the closet huts (H.9 and H.12)...[has] no piece of manio wood mapped at this location” (Fiedel 1999:10-11). If Fiedel had been more cautious and looked closer at the report, he would have seen this stake in association with timbers in Figure 8.2 on page 174 of the 1997 report.

Fiedel states that the OXA-381 date was initially called a

“culturally modified log of *Nothofagus*” (Dillehay and Pino 1989: 140), and in 1997 (Dillehay and Pino 1997:48), it is a section of wood taken from “a cut, burned, and planed timber of luma...not *Nothofagus*...in Hut 3 in Zone D.” He [meaning Dillehay] also states that “the three ‘fire-drill hearths’ found in Areas D and DW are described as made of soft manio and huahuan woods”...yet]...the hearths are alternatively put in Hut 5 (not Hut 3, but also 5 m east of the sample location). (Fiedel 1999:11)

We say on page 191 (Dillehay 1997e) that the “three fire-drill fragments and their fire-drill hearths” were primarily “clustered near the opening of the unit near the hearths in Workshop 2.” The west side of Workshop 2 overlaps with and opens into Huts 3 and 5. Fiedel conveniently relocates “all” fire-drill pieces in Workshop 2 for the sake of his argument when, in fact, only three are situated there and they overlap with the entry to Huts 3 and 5. Furthermore, some of the fire-drill specimens are located in a cluster of woods on the northwest side of Workshop 2, which includes luma, huahuan, and manio. Also, the luma fire-drills are mentioned in Workshop 2 (Dillehay 1997e:199), contrary to what Fiedel says.

In questioning the provenience of the Beta-6755 radiocarbon date, Fiedel cites several passages in the 1997 report to show how confusing the data are:

The radiocarbon date of 12,230±140(Beta-6755) is reported to have been obtained for a “digging stick” with a fire-hardened tip, made of ulmo (Dillehay and Pino 1997:48). The reader is referred...to Fig.7.38...That figure, however, shows a “wooden lever made of alerce wood” that was found in Area B of Zone A. Perhaps Figure 7.36 is meant; it shows three “possible digging sticks,” the farthest left apparently being the one shown in the *National Geographic* photograph. Although the figure caption describes “possible digging sticks from various floors of Huts 1, 3, and 11”...an alternative stated location is Zone A, Area A...and the 1989 report gives the provenience... as Area D, Column 5, Unit 4...[which] puts it in Workshop 2. (Fiedel 1999:11)

There is no confusion on our part here. However, Fiedel was confused whether we are dating a **digging stick or a lever**. The single dated piece in question is possibly both, as we indicated in the 1997 book. We say that the specimen in Figure 7.38, which was dated, “had been used as a **lever to pry or as a digging stick** [our emphasis]” (Dillehay 1997d:161). The tip is slightly burned but mainly blunted. And the piece is correctly located in the entrance to Hut 3 (see Figures 4.1 in the 1997 report and 6.2 in 1989 report). Fiedel confuses this dated archaeological digging stick with a generic stick shown in the *National Geographic* (Gore 1997) magazine. The photograph in the magazine was taken of a “digging stick” to depict digging action. It does not correspond to the actual dated piece shown in Figure 7.38, and we never stated that it did. It is Fiedel who misidentifies the two different specimens in the *National Geographic* report and in the 1997 report and then accuses us of confusion.

Fiedel asked why we did not radiocarbon date the cordage from Monte Verde. We provide the answer Dillehay gave to *Archaeology*:

Fiedel obviously does not comprehend the nature of ...research at a wet archaeological site like Monte Verde. Excavation at wet sites is slow and tedious, often requiring immediate chemical treatment of such perishable remains as wooden tools, cordage, and other organic remains. Because some knotted cordage and other perishables at Monte Verde were very fragile organic materials, such as

wooden tools and chunks of charcoal in hearths, were chosen for dating. Obviously, once the cordage was contaminated by chemicals, it could not be dated by radiocarbon means. The answers to this and many other questions raised by Fiedel are in volume two of the site report. (Dillehay quote in Rose 1999:1)

The reader is referred to Figures 7-9 in Part 6 to view an *in situ* stake with cordage and photographs of the some excavation and conservation procedures in the field.

Human Footprints

Fiedel (1999:11-12) questions the location of the three footprints, saying that prints B and C are mistakenly attributed to Area DW in the captions of the photographs. This is a petty complaint. In fact, the footprints are on the border between Area D and Area DW; both references are correct as the border meanders between the two arbitrarily defined areas. He also questions the matrix of the prints, saying that they were found in “mud” and then in a “hardened muddy sand and silt” (Fiedel 1999:11). When we excavated the prints, they were embedded in a wet clay or muddy silt. After drying out during the excavation, the muddy silt became a hardened clay and silt. We also mention that the clay is imported and probably stored in the site, but Fiedel also questions this interpretation.

Because we describe different matrix conditions for the prints, Fiedel believes that “If stored, imported clay cannot be readily distinguished from muddy silt; one must wonder about the identification of other ‘caches’ of ‘exotic’ clay (Dillehay 1997c:102)” (Fiedel 1999:12). The imported clay was determined not by Dillehay but by geological and geochemical analyses made by Pino and Karathanasis, respectively. We state that

(1) their absence in noncultural areas of the excavated MV-7 stratum and in exposed profiles of trenches and creek banks, (2) their patterning in the site and association with open workshop areas and hearths, and (3) their exotic nature, as indicated by x-ray diffraction, x-ray fluorescence, and differential scanning calorimetry analyses (see Appendix I [Karanathanasis 1997]), suggest human intervention. Moreover, Pino (1989:109-120) has observed no geomorphological evidence indicating that these deposits were formed by sedimentary processes and that they occurred in the exposed surfaces of the MV-7 stratum. (Dillehay 1997c:102)

Scaling and Mapping

This section of Fiedel’s review also is full of errors and misrepresentations. We correct only a few of them here.

Fiedel (1999:12) complains about scaling and mapping problems and specifically doubts that the limited space within the Wishbone Structure was related to human activity. When we

refer to functions in an around the structure, we do not imply that all activities, such as butchering and use of medicinal plants, had to have taken place within it. Fiedel also refers to an exaggerated artist drawing of the structure in the *National Geographic* report (Gore 1997) and alludes to further scaling problems. He wonders how people could have slept or moved around the individual small hut cells inside the long tent-like structure. If he had read the report closely, he would see that our description of the hut follows that of the ethnographic Toldos tents in Argentina, which are shown in Chapter 8. The Toldos tent has a wooden frame with interior poles draped with animal skins. People could have moved freely from one area to another between and around the poles.

Fiedel also says that “the cause and confusion and contradictory statements about [artifact] provenience is unclear. Apart from the few that were recovered from bulk sediment samples...all artifacts at the site were presumably found *in situ*. They were not found in screens, because ‘constant sifting was unnecessary except as a periodic check’ “ (Fiedel 1999:12). All hearths were bagged and returned to the laboratory for bulk processing, as was a large portion of habitation midden, where we later found several artifacts. Other artifacts were covered with sediment attached to timbers. When the timbers were preserved in chemicals, transported to the laboratory, and cleaned, the artifacts were found.

He states that: “With respect to the location of the X-15-1 or D-10-1 projectile point, the mapping of Zone D is chaotic. The row-column grid system shown in 1989 Figure 3.4, Dillehay and Pino 1989:49) and 1997 (Figure 4.1, Dillehay 1997b:61) has somehow shifted to the east by about 5 or 6 meters in the larger-scale maps, such as Figure 20.1 (Dillehay 1997i:775)” (Fiedel 1999:12). This is not true and again represents a misrepresentation of the data by Fiedel. The reader should compare all these figures and others scattered throughout our book.

Fiedel asserts that: “The scatterplots (e.g., Fig.15.55a) show numerous items in the western section of the site where, to judge from Figure 20.1, no excavation was conducted.” This is just plain wrong. Figure 20.1 depicts activities only in Zone D, not Zone A, which is located to the west where excavations did occur. Zone A is represented by another map, Figure 20.2. He says that Figure 20.1 shows artifacts north of Huts 9 and 10, but the artifact scattersplots do not. This also is not true. Scatterplots 15.54 d, 15.55 a-c, 15.56 a,c and, 15.57 and others throughout the text show artifacts north of these huts (see Dillehay 1997g).

Fiedel (1999:5) doubts the size of the excavation blocks that we placed at the Chinchihuapi site, wondering whether we employed 1 by 2 m or 5 by 5 m test pits. We began our excavations at the site with 1 by 2 m test pits and ended by expanding two of them into 5 by 5 m block excavations after discovering artifacts in them. After excavating 15 cm in the 1 by 2 m pit, we found a large boulder and abandoned work there.

In regard to artifacts and ecofacts recovered from these two sites, Fiedel confuses readers. He says that we initially reported the presence of “39 plant species... at PM 3C-10 [Chinchihuapi site]” in 1989 (Dillehay and Pino 1989c:245) and later said that “No organic material was preserved” in the locality (Dillehay and Collins 1997:335). Fiedel collapses the Archaic and the Chinchihuapi sites again. Different assemblages are ascribed to the two different localities--one being the Archaic site where there is no organic material and the

other being the Chinchihuapi site where there is organic material (see Part 2 above for a discussion of these two sites).

Fiedel (1999:5-7) questions the use of different designations for excavation areas in and around the Monte Verde site, specifically the “X” site and area nomenclature. When we first worked at Monte Verde, we designated “X1” for the north side and “X2” for the south side of the creek. Later, as the project expanded in space, we changed the area nomenclature designations. That is, excavation zones within both the north side and the south side of the creek were designated A-E and the test pits within each were sequenced 1 through 15, 58, etc. Where test pit numbers do not appear on the map, such as Test Pits 4 and 15, they were converted to block excavations and to a different numbering system.

In this same discussion, Fiedel attempts to relate different spherical stones (e.g., sling and bola stones) with different catalog numbers to different archaeological sites by citing different publications that appeared between 1984 and 1997. The multiple artifact inventory numbers simply refer to different geological and archaeological pits, trenches, and blocks that were excavated within the Monte Verde site between 1977 and 1995.

He questions the location of Zone A in the site. Zone A developed from test pits to a large block excavation, as opposed to other zones, which were examined primarily by test pits that were designated either “Z” or “PZ” (referring to pozo or pit in Spanish). If test pits were productive, such as X-15 on the north side of the creek, they were converted to larger block excavations.

Fiedel accuses us of producing different representations of features, architecture, and artifacts in Zone A. The different maps and photographs simply depict two different use phases of the Wishbone Structure, each of which has a slightly different form and is associated with different materials. These differences are stated clearly in the text and in the captions. Furthermore, each map or photo is intended to show the location of the different feature, wood, lithic, bone, and plant assemblages. Other maps show all features and artifacts together. This also is standard procedure in documenting the evidence in a site.

Fiedel ends his review on an accusatory note “there is a hint that Area D at one time referred to part of the western excavation block in Zone A. The latter is labeled Figure 12.2 as: ‘Excavation Area A through D’ in the 1989 report in a chapter by Hoganson and Ashworth” (Fiedel 1999:12). This is not a hint of confusion between the two zones. When we first excavated Zone A, we divided it into Areas A, B, C and D. An unproductive geological trench (see Fig. 5.4 in Pino 1989) was placed in the area north of the main excavation block in Zone A. This area and trench was labeled “Area D” within Zone A. Although we found no archaeological material in this area, Hoganson and Ashworth did find paleobeele remains and used the designation in their map. A single and minor map depicting an “Areas A-D” designation hardly constitutes evidence to warrant the accusation made by Fiedel.

There are many more errors, misrepresentations, and inconsistencies in Fiedel’s report. We simply have addressed the major ones here. The remaining accusations are secondary and represent corollary issues that build on his previous mistakes.

Part 4: Editorial Oversights in Volume 2

We thank Fiedel for finding several editorial errors in Volume 2. As Collins (1999:14) pointed out in his commentary on Fiedel's review, many errors were corrected on page proofs at the last minute but, unfortunately, were not integrated by the Smithsonian Institution Press into the published version of the volume. We take responsibility, however, for these mistakes. We do not pretend that we have found all errors in the second volume. As more are found, they will be listed below.

Corrections to Some Errors in Volume 2.

1. Page 43, radiocarbon dates Beta-35193 and Beta-41983 were taken from strata associated with Test Pit 45, not Test Pit 42 (see Table 3.1, Dillehay and Pino 1997).
2. Page 44, Table 3.1, radiocarbon date Beta-6754, was found in Test Pit 45, not "Test Pit 5" (Dillehay and Pino 1997).
3. Page 48, column 2, paragraph 2, line 2, radiocarbon date OXA-381 was taken from luma wood, not manio wood (Dillehay and Pino 1997:48).
4. Pages 89-94, Feature 3 in Zone C was found in Test Pit 47 not Test Pit 45 (see Dillehay 1997c).
5. Page 162, column 2, last line says "They are very similar to those found by Lynch (1981:240-250)." This should read "They are very...by Lynch (1981:240-250) at Guitarrero Cave in northern Peru" (Dillehay 1997d)
6. Pages 174 and 175, respectively, Figures 8.2 and 8.4, shows two pieces of luma wood in the far eastern end of Zone D near the edge of the unit and about 2 m east of the area labeled "Hearth." The specimen closest to the hearth area should be depicted as manio wood, not luma wood (see Dillehay 1997e).
7. Page 205, column 2, paragraph 3, last sentence: "A radiocarbon date on the post was processed at 12,650+/- 130 B.P. (TX-4437)" should be struck from the paragraph (Dillehay 1997e).
8. Page 543, column 1, paragraph 2, lines 17-18 should read "(see Figs. 7.23, 7.25, 7.27)" not "(see Figs. 8.23, 8.25, 8.27)" (Dillehay 1997g).
9. Page 533, column 2, paragraph 4, lines 6-8. The polyhedral core was found in Zone A, Area A, not Zone D. The text should read "This specimen was recovered...in Area 1, Zone A..." not "Area D, Zone D" (Dillehay 1997g:553). See Figure 6 in Part 6 below.
10. Page 563. The stepped wooden handle on which scraper B-8-18 was attached is not Figure 7.33g but 7.33e (Dillehay 1997g). This scraper has bitumen on both its lateral and proximal edges. The tool was probably attached to the wood with bitumen and used to scrape bitumen as well. The distal edge also is slightly broken like that of B-11-6-31 (see Fig. 7.33g and pages 547, 564, and 573 in Dillehay 1997g).
11. Page 612, column 2, line 15, should read "was recovered recently from a cut profile in Area DW," not "was recovered... from Area D" (Dillehay 1997g).

12. Page 614. The slate perforator should be listed in Table 15.32 under “Area A-C” (Dillehay 1997g).
13. Page 677, Figure 17.13 caption (Dillehay 1997h) should read “Four *in situ* bone fragments in Zone A, Area B, Unit 13 and Area A, Unit 13...” not “Four *in situ* bone fragments in Zone A, Area A...”
14. Page 673, Figure 17.9 should read “Area B, Zone A” not “Area A, Zone A” (Dillehay (1997h).

Part 5: Conclusion

In conclusion, we believe that Fiedel attempted to discredit Monte Verde in defense of the Clovis model. If the Monte Verde volumes had been impeccable and free of any editorial oversights, we believe that he would have questioned them anyway. We never forwarded Monte Verde as a pre-Clovis site. Unfortunately, a mythology about Monte Verde has been stimulated by the Clovis and pre-Clovis debate. The site has become the lynchpin in a paradigm change. Thus, it is not surprising that some critics will challenge the site, but unscrupulous challenges are unnecessary.

Fiedel rushed to publish in an unjuried magazine a biased and error-ridden review of our work at Monte Verde. A few commentators followed his lead and added to the negative evaluation of the site. By publishing these as a SPECIAL REPORT, *Scientific American Discovering Archaeology* imparted an air of validity to their specific claims and even to Fiedel’s implication that we unscrupulously misrepresented Monte Verde. Fiedel seems to question whether any benefits to the profession were wrought from the 1997 visit to Monte Verde, but as Meltzer (1999:16-17) makes clear, there were benefits, and probably lasting ones at that.

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