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Stone Age Institute

Gosport, Indiana

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NUMBER 2

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BREATHING LIFE INTO FOSSILS:

Taphonomic Studies in Honor of C.K. (Bob) Brain

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Stone Age Institute Press · www.stoneageinstitute.org
1392 W. Dittemore Road · Gosport, IN 47433

COVER CAPTIONS AND CREDITS.

Front cover, clockwise from top left.

Top left:
Artist's reconstruction of the depositional context of Swartkrans Cave, South Africa, with a leopard consuming a hominid carcass in a tree outside the cave: bones would subsequently wash into the cave and be incorporated in the breccia deposits. © 1985 Jay H. Matternes.

Top right: The Swartkrans cave deposits in South Africa, where excavations have yielded many hominids and other animal fossils. ©1985 David L. Brill.

Bottom right: Reconstruction of a hominid being carried by a leopard. © 1985 Jay H. Matternes.

Bottom left: Photograph of a leopard mandible and the skull cap of a hominid from Swartkrans, with the leopard's canines juxtaposed with puncture marks likely produced by a leopard carrying its hominid prey. © 1985 David L. Brill.

Center: Photo of Bob Brain holding a cast of a spotted hyena skull signed by all of the taphonomy conference participants. © 2004 Kathy Schick, Stone Age Institute.

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Published by the Stone Age Institute.
ISBN-10: 0-9792-2761-5
ISBN-13: 978-0-9792-2761-5

CHAPTER 2

Rather Odd Detective Stories: A View of Some Actualistic and Taphonomic Trends in Paleoindian Studies

Gary Haynes

Abstract

During the last three decades of American Paleoindian research, some taphonomists played a mug's game while others knew all about the game's ambiguous rules. After Paleoindianists discovered a string of influential 1970s publications by researchers working mainly in Africa, they changed their attitude towards taphonomy. But many Paleoindianists idiosyncratically used taphonomy to create support for unusual propositions or to lend plausibility to off-beat theses such as an unexpectedly early human presence in the Americas, instead of testing hypotheses through taphonomic analysis. After the 1980s, taphonomic research has greatly advanced in allowing clear and definite interpretations of Paleoindian bone assemblages, but stubborn personalities and the tendency to "brand" certain sites continue to discourage the most rigorous skeptical inquiry that is taphonomy. The process of explaining archaeological contexts through taphonomy is a make-or-break step that must be applied to the earliest sites.

Introduction

This is a detective story, but a rather odd one.

C. K. Brain (1981)

America's deep prehistory is a very foreign country, and clever detectives are needed to uncover how people did things then.[1] Some Paleoindianists have been able to show us through taphonomic research what the world of foragers was like in the distant past, but not all the detectives have been equally canny. Years ago C. K. Brain said something that helped me recognize how the detective work can bedevil our imperfect minds.

I first met C. K. Brain in 1982 when I went to Africa to find field sites and agreeable governments willing to issue research permits for a planned study of elephant bones. My research plans came together in Zimbabwe instead of South Africa, so I did not see Bob Brain again until 1984, when we met in Carson City, Nevada, at a conference about animal-bone modifications. One day Dr. Brain, while eating lunch with Kate Scott and me at an A&W Root Beer stand across the street from the soon-to-be-bankrupt casino where the conference took place, warned us that "Taphonomy is a mug's game." Here's what I think he meant: Too many taphonomists were duping themselves into serving causes instead of seeking the more complex truths about site-formation processes. In fact, maybe ultimate complex truths were unattainable, which is an insight reached by other conscientious taphonomists. In spite of years of study of all the variables, no single predictor could tell us how to explain every example of bone settling, bone survival, bone subtraction from assemblages, bone marking by human butchers and feeding carnivores, or the other end-effects of taphonomic processes.

In this paper, I offer a personal view of the last three decades of American Paleoindian research, some of which was carried out by taphonomists who may never have realized they were playing a mug's game, and some of which was done by *au courant* researchers who knew all about the game's ambiguous rules. Readers will soon understand that because I was a participant in this recent period of history, my viewpoint has affected how I inter-

[1] Apologies to readers of L. P. Hartley's 1953 novel *The Go-Between* (Hartley, 2002).

pret the scientific activity and opinions of the time. But having convictions is an unavoidable part of the practice of science.

First Flush Of Ambition

I take it you are in the first flush of ambition, and just beginning to make yourself disagreeable. You think (do you not?) that you have only to state a reasonable case, and people must listen to reason and act upon at once. It is just this conviction that makes you so unpleasant.

F. M. Cornford (1908)

The starting point of my review is the middle of the 1970s, when I was a graduate student learning about the first native cultures in eastern North America. The themes that occupied my Paleoindianist colleagues and teachers were primarily (1) the timing of the earliest human colonization and (2) the technology and subsistence of the first peoples. Both themes were ripe for the application of taphonomic principles.

Contemporary developments in hominid paleontology were barely given notice in the papers and publications written by Paleoindianists of those days. Yet by 1976, thanks to the Wenner-Gren Conference and the resulting *Fossils in the Making* book, which "crystallize[d] the new science of taphonomy and [helped] to chart its future course" (Brain, 1981: ix; Brain, 1980:73), Paleoindianists discovered a string of influential publications by Andrew Hill, Kay Behrensmeyer, Pat Shipman, and others. To me in my Paleoindian program, which in those days had the status of a déclassé suburb far from the bustling metropolis of hominid evolutionary studies, the taphonomists seemed to be training through one of two axes—Harvard's or rival Berkeley's. Without mentoring or training and without peers sharing interests within my own Paleoindian program, I ingenuously entered the arena with a few observational papers about carnivore-gnawing, based on a series of studies of Pleistocene fossil bone collections, a zoo-animal-feeding study, and some actualistic work in American wildlands. These papers were often given harsh treatment by Paleoindianist referees and paleontologists trying to prevent them from being published. I look back on the taphonomists of those days as avatars of the maverick Hollywood detectives whose mulish supervisors stall their homicide investigations.

The 1970s saw the appearance of several of Brain's taphonomy papers preceding the *Hunters or the Hunted* book, as well as Rob Bonnichsen's *Pleistocene Bone Technology* monograph, in which he set out his propositions about bone-flaking in prehistory and its relationship to the pre-Clovis stage of America's colonization. In 1978, tying in neatly with Bonnichsen's proposals, the Owl Cave site in Idaho was described by a paleontologist and geologist (Miller and Dort, 1978) as an example of how prehistoric people deliberately flaked mammoth bones into tools. While the process of flaking bone was replicated and thus plausible, it still needed actualistic testing to show whether noncultural processes could be eliminated as potential causes of the same results.

Also in the 1970s, thanks to Professor C. Vance Haynes (who is not related to me) and then-graduate student Jeff Saunders, both at the University of Arizona, Paleoindianists could clearly see how relevant age-profiling can be in explaining the possible agencies that contributed animal bones to fossil sites. Saunders (1977) thought the mass mammoth site of Lehner, AZ, contained the remains of a herd of related animals killed together, because the age distribution was so similar to what is seen in modern African elephant herds. When I went to Africa a few years later to study elephant biology and behavior, I was determined to see how age profiles could vary in elephant bone assemblages when the causes of mortality varied. After a few years of fieldwork in Africa, I suggested (Haynes, 1987) that mammoth age-profiles in Clovis sites such as Lehner might reflect climatic stresses on the populations rather than mass-hunting by humans, an interpretation at odds with Saunders' (1980).[2]

A hinge point in Paleoindianists' changing attitude towards taphonomy developed during the key years 1978-1985. Within this span, perhaps 1981 was most critical: C. K. Brain's *The Hunters or the Hunted* book appeared in the same year as Lewis Binford's *Bones* book, Pat Shipman's book *Life History of a Fossil*, a *Science* paper by Stanford and colleagues about an elephant they butchered to create bone flakes—thus in their opinion proving that pre-Clovis people flaked mammoth bones in North America—and the hiring of A. K. Behrensmeyer at the Smithsonian Institution's Natural History Museum, which slimly avoided a federal hiring-freeze (Harrison, 1981). My own doctoral dissertation was completed that same year, to far less effect than the other publications. Other products of the year were the first announcement of a Clovis-associated mastodont killsite in eastern North America (Graham et al., 1981), and the first description of the Lamb Spring site in Colorado (Stanford, Wedel, and Scott, 1981) that had yielded a component of stacked and flaked mammoth bones. I was co-author of a second Lamb Springs paper the next year (Rancier et al., 1982), which added fuel to the debate about bone-flaking and the possible existence of a pre-Clovis human presence in North America.

The main use of taphonomy in Paleoindian publications—the word taphonomy being loosely and implicitly defined as bone-modification analysis—was to serve a very narrow cause, namely finding support for unusual propositions or for lending plausibility to off-beat theses such as the evidence for an unexpectedly early human presence in the Americas, based on flaked bone speci-

[2] It is worth noting that one later study of some mass mammoth Clovis sites now may indicate that the dead animals came from different source ranges and were not all related family members (Hoppe 2004).

mens. These were not really examples of detective work, as Brain had called his own taphonomic studies—they were one-sided editorials with taphonomy added to increase the plausibility. Many papers with a taphonomic bent seemed to be polemical rather than truth-seeking, intended to advance opinions without addressing the strengths and weaknesses of competing hypotheses.

Some developments in Paleoindian taphonomic work were considered pivotal at the time, but in fact they might have deflected the flow of research, like the investigative work of an obsessed but blindered detective. An example is small-scale elephant-butchering, which nearly became a cottage industry in actualistic research. The refereed *Science* paper by Stanford, Morlan, and Bonnichsen (1981) summarizing the Ginsberg experiment (also see Callahan, 1994) could not elevate the elephant-butchering projects (for example, Matyukhin, 1984; Rippeteau, 1979) from makeshift or impromptu happenings to replicative science. None of the experiments was ever written up adequately. These events achieved an almost folkloric presence as background in some of the ensuing literature about butchering marks to be found on megamammal elements and the expectable ways that prehistoric people must have sectioned huge prey carcasses. Yet these and other individual bone-modifying experiments were too easily transformable into lawlike generalizations about human behavior (as in Bonnichsen,, 1979). All too often, as shown in these examples, and following the precocious post-processual trend of the times, taphonomic studies involved a novel but reckless form of induction. Referring to the observable traces created by an individual's unmatched acts (such as Bonnichsen's bone-breaking or other archaeologists' attempts to butcher carcasses and produce cutmarks), these studies then proposed universalities about butchering practices in the past.

Of course not all work was driven by scholars trying to advance unyielding points of view. A very interesting and less slanted literature was also being produced in this period. For example, Dinah Crader in (1983) and (1984) described Bisa elephant butchering—very pertinent for Paleoindianists trying to understand mammoth-butchering—and the resulting traces of carcass sectioning and bone-processing created by people having a real economic interest in the meat and bones. When Hill (1976, 1984) described the testing of competing hypotheses about fossil animal-bone accumulations, he showed the process to be extremely challenging and requiring a rigor not seen often enough in the scientific literature. A flow was not yet underway of taphonomic writings closely relevant to Paleoindian studies, but nevertheless the 1970s and early 1980s did see a turning point in awareness of how such studies could relate to emerging interpretations.

One major emphasis in that decade was on skeletal disarticulation sequences in small and large mammals when different agencies affected the carcasses. Hill (1979) devised a statistical technique for describing the African topi sequence and modeled how the elements become scattered. Hill and Behrensmeyer (1984) soon found the disarticulation sequences to be consistent in a wide range of African mammals. A year later Hill and Behrensmeyer (1985) looked at the sequence of American bison disarticulation at the Olsen-Chubbock late Paleoindian site, and suggested that a few differences from the natural sequences they had recorded for African mammal skeletons might reflect human actions at the archaeological site. Overall, however, most human and nonhuman processes were recognized as producing very similar sequences of separation.

Influenced by this desire to know how animal skeletons are altered by different agencies in nature, and following the lead of both Brain and P. R. K. Richardson (1980), I sought data to produce flowcharts that combined information on how the grey wolf in North America damaged skeletal elements of American bison, moose, and deer and how the bones naturally disarticulated (Haynes, 1980, 1982). As far as I can tell these papers have very rarely been referenced by taphonomists and archaeologists.

Bad News

In some ways taphonomy is 'bad news' to archaeology. It shows us just how much we don't know about the archaeological record…

Sarah Colley (1990)

In 1984, a bone-modification conference was held in Carson City, Nevada, hosted by the Nevada State Museum and partly funded by Rob Bonnichsen's Center for the Study of Early Man, then located at the University of Maine, Orono. The conference scheduled the actualistic and taphonomic papers early in the program, to be followed by several half-days of presentations by people who, it appeared to me, had paid inadequate attention to the taphonomists. Even in the book that eventually resulted from the conference (Bonnichsen and Sorg 1989) some authors displayed this same selective forgetfulness. For example, early in the book (as at the conference) Oliver (1989) discussed bones showing noncultural impact marks, surface incisions, and other effects of natural processes, as did Behrensmeyer, Gordon, and Yanagi (1989), yet in the book's later articles similarly modified specimens from other sites were said to be affected by human actions only, and figures such as of tooth-marking on bones were interpreted as cultural in origin. When I mentioned the lapses in a journal review of the book, I was made to realize that I was facing lasting hostility from participants and interested parties who had staked their careers on the interpretations. Most Paleoindian researchers intended to be very selective in trying to apply the taphonomists' results for many more years to come.

At another conference held two years later at Lubbock Lake, Texas, I overheard a remark from a prominent archaeologist who advocated mammoth-bone-flaking as proof of a pre-Clovis presence in the Americas. When I approached the podium he said “Here comes a taphonomist to tell us everything we say is wrong.” I realized this was the prevailing attitude among the other participants as well—taphonomy was spoiling their stories. I was nettled at the time, but now I can point out that much of what was said really *was* dead wrong.

I remained exasperated through the mid-1980s, especially after unsuccessfully applying for NSF money to support a project to study elephant-bone-flaking. This happened in 1984.[3] An NSF proposal I submitted was returned unfunded, along with comments from anonymous reviewers who did not understand the specialized vocabulary of taphonomy or who speculated that maybe I was making up some of my results. Taphonomic studies were indeed bad news for many of these people, whose neat stories about mammoths and whose attempts to reinvent Paleoindian culture-histories were weakening.

Unhelpful And Unpleasant

…nothing is ever done until every one is convinced that it ought to be done, and has been convinced for so long that it is now time to do something else.

F. M. Cornford (1908)

In 1986, Paleoindian archaeologists George Frison and Larry Todd published a very short but influential book about a mammoth killsite at Colby, Wyoming. In one chapter Frison and Todd described an experiment with elephant bones, reminiscent of Voorhies’ (1969) and Hanson’s (1980) experiments to measure the extent to which moving water can displace different elements of mammal skeletons. Frison was a true leader in applying taphonomy to Paleoindian studies. He had come from a ranching and hunting background, and he confided to students and friends that he thought 99% of archaeologists didn’t know near enough about animal behavior to interpret human hunting behavior. He made sure his students began learning about the animals that prehistoric people hunted. He entered the taphonomic business with a flourish, becoming an ever-present voice in Paleoindian research, encouraging students and colleagues to devise methods for determining how much the animal bones in High Plains sites had been affected by human versus nonhuman processes.

Frison was (and still is) right about how little archaeologists know of animal biology and behavior, as seen in much Paleoindianist literature. Astonishingly, some archaeologists still believe that prehistoric people butchered large mammal carcasses any way they wanted to, depending on ethnic or cultural preferences presumably, without regard for efficiency or basic anatomical limitations; an example is Storck and Holland (2003: 299, 300) who suggest that even “illogical and unrealistically extravagant” proboscidean-butchering interpretations are acceptable, and that criticisms of such outlandish stories are merely “culturally relative” judgments and therefore not valid. Frison had learned from personal experience and from his intellectual control of the ethnographic literature that human butchering practices were rational, patterned, and understandable.

Frison is an example of a Paleoindianist who wisely and early paid attention to the taphonomists, even when they spoiled some of his stories. In earlier publications by Great Plains archaeologists (such as Frison, 1974) writing about prehistoric bison sites, cultural causes frequently had been assigned to bone modifications that were more likely carnivore-caused. But Frison’s experiments and his unusual curiosity opened his eyes and those of his students to the varied end effects of noncultural processes. Frison made two trips to Zimbabwe to throw spears at culled elephant carcasses and take part in large-scale elephant-butchering at the time I was doing my fieldwork there.

At one point in his writings, George Frison tried to introduce a word—“taphonomics”—which could have given a convenient name to the chapter every book should contain discussing the origins of fossil bone modifications—but it was never adopted by other authors. Frison’s experiments in bone-floating and spear-throwing produced a limited set of unreplicated data, but the work is still valuable. Thanks to Frison, we know something important about elephant-bone buoyancy, spear penetration, and especially the expected lack of cutmarks on elephant bones when they are butchered by experts.

Larry Todd also continued the taphonomic work by painstakingly documenting patterns in bison bone damage and element attrition, thus helping to clarify the prehistoric cultural and noncultural processes at work on the American High Plains (Todd, 1987; Todd and Rapson, 1988, 1999).

In 1986, Johnson and Shipman published a short description of a study that many Paleoindianist readers hoped would provide a guide for distinguishing incised bone surfaces cut by butchers from specimens cut by noncultural agents. This study was part of a family of other valuable SEM studies of the time (such as Shipman and Rose, 1983a, b, 1984). Paleoindianists made use of these works, but eventually began wondering how many hours of searching under the microscope or how many marks were examined to find the clearest matches between fossil marks and experimentally produced cuts illustrated in these guides. Paleoindianists also wondered about how the documented cutmarks had been created (were they deliberate attempts to mark bone, or were they by-products of economy-based butchering?). More discussion was needed about the range of variability in both true cuts and the fossil marks. Only the best matches and sharpest differences were featured in the widely used guides, so the ambiguity was downplayed. Paleoindianists learned to be a little more cautious over the next

[3] It was not long afterwards that I began writing sometimes biting book reviews for the journals *American Antiquity* and *North American Archaeologist*.

decade when they found that no taphonomic guide was infallible.

The 1980s and 1990s were notable for the increasing volume of publications about essential taphonomic issues. By the end of the 1980s, taphonomic research had greatly advanced in allowing clear and definite interpretations of Paleoindian bone assemblages. By then, instead of anecdotes and conjecture, we had on hand multiple empirically documented records about bone representation at different kinds of sites, bone subtraction due to scavenging carnivores, and so forth. It had begun to seem that archaeologists and paleontologists regularly applied these studies in their own research and that the research to that point had made a start in defining (even if not clearing up) the important ambiguity in fossil bone assemblages. The overlooked classics of the older literature (such as Weigelt, 1989 [original 1927 in German]) were revived in print as interest exploded in taphonomy. Solid and well reported actualistic studies inspired some Old World researchers to go to war with adversaries over the deeper meanings of early hominid bone assemblages (such as Lewis Binford and Rob Blumenschine versus Henry Bunn over Plio-Pleistocene hominid scavenging behavior [Binford, 1986; Blumenschine, 1986; Bunn and Kroll, 1986], or Curtis Marean versus Mary Stiner over Neanderthal diet [Marean, 1998; Stiner, 1994]). Cautionary tales stressed the remaining ambiguities—such as equifinality in bone survival or element distribution or surface-marking—but these were often roughly treated by critics: see the probable career-stalling responses to Rob Gargett's (1989a, b, 1999) rethinking of Neanderthal burial and Nicola Stern's (1993, 1994) cautions about assemblage structure at Olduvai Gorge. Paleoindianists also went to battle over the interpretations of assemblages such as the pre-Clovis broken bones from Old Crow, Yukon, and Lost Chicken Creek, Alaska, but these debates possessed a lower international profile because cautionary tales were often ignored or dismissed behind an authoritative sniff rather than attacked head-on with data and strong arguments.

Celebrity dissidents in Paleoindian studies seem to have developed broadly similar careerist strategies. Aggressive self-promoters rely more on a strong, thick-skinned personality able to stay the course while recounting unorthodox claims, and less on a willingness and ability to carry out adequate actualistic/taphonomic research—sustained detective work—which would uphold their questionable interpretations of the past. When asked about the finer details of their fieldwork practices or the replicability of their interpretive standards, celebrity dissidents in Paleoindian studies often may not respond, perhaps implying that they are infallible. Whenever they do claim to be responding, they mainly attack their inquisitor, which in Paleoindian studies frequently turned out to be Stuart Fiedel.[4]

Fiedel's valid querying of the Monte Verde site's ambiguities was publicly brushed aside (Dillehay et al., 1999a, b), as had been my own private questions. When I commented on a pre-publication chapter ("Zooarchaeological Remains") meant for the now well-known second volume about the site (Dillehay, 1997:661-758), the author wrote me a testy letter rejecting my questioning, and proclaimed that "after 30 years [of experience as an archaeologist] I can recognize an artifact." A peremptory dismissal of a taphonomist's caution is a common reaction, but an archaeologist's proclamation of personal skills does not obviate the need to test the reliability of interpretations.

Monte Verde's huge second volume (Dillehay, 1997) contains an impressive amount of data and interpretations, but it is more imperfect than the first volume in many ways (see Fiedel, 1999, 2000). The book is frustrating because of the frequent impossibility of figuring out where certain key items were found (such as the seemingly unmapped handful of indisputable lithic artifacts) or ambiguity about the specific items that were radiometrically dated, although it is a very large book full of outstanding information. The site yielded hundreds of minimally modified stream-rounded stones, about 400 animal bones or fragments looking like noncultural debris, diverse plant remains including wood fragments with a decidedly wave-washed look, "structures" made of what appear to be strewn wood pieces (for example, Dillehay, 1997: 775), and possible mastodont skin fragments. Overlying the materials interpreted as cultural is a peat layer that preserved the organic remains. The site is fascinating and unique, but "bizarre" would also be an appropriate word.

Paleoindianists are tough fighters when it comes to changing other people's paradigms and defending their own. Yet too often the sampling of taphonomic literature offered to support one set of interpretations is overly selective. The Monte Verde report contained a limited literature review as well as descriptions (Dillehay, 1997:695-703) of neotaphonomic experiments and observations to uphold some of the interpretations of broken bones as being culturally produced. But such experiments must be coldly evaluated, because they can be narrow and faulty if they consist of limited bone set-ups to test possibilities for bone movement and noncultural modifications.

Roosevelt (2000) observed that the discontinuous strata at Monte Verde were complex and contained possible contaminants along with the questionable "artifacts." Thus not only are the cultural materials doubted by a number of New World archaeologists, but Monte Verde's dating procedures themselves are now being questioned. Monte Verde is not the only possibly pre-Clovis site with lingering contextual problems. The Meadowcroft

[4] Unpleasant disagreements between archaeologists with different interpretations has a long history. For example, when J. L. Lorenzo claimed Irwin-Williams's field crew had fraudulently planted artifacts at Valsequillo, Mexico, in the 1960s, Irwin-Williams (n.d.:12) accused Lorenzo of "distorted personal animosity and irrational inability to change an opinion."

rockshelter site in Pennsylvania has been known for decades as a stratified site containing unquestioned lithic artifacts associated with pre-Clovis radiometric dates. Excavator James Adovasio is the site's long-suffering archaeologist who has had to face down the skeptics for nearly 30 years. He spent the 1970s and 1980s becoming a well-known dissident in Paleoindian studies, due to his advocacy of Meadowcroft's early dates, and over time his self-defense has been fine-tuned into near-churlish responses to critics (see Adovasio and Page, 2002).

The problems in this case are centered not around determining if modified bones are genuine artifacts, but around the dating itself. Two published reviews (Flannery, 2003; Roosevelt, 2000) of Adovasio's co-authored book (Adovasio and Page, 2002) about the site and its surrounding controversies alert readers to the fact that naturally occurring coal might have contaminated some of the dated materials at Meadowcroft. The process of analyzing the samples used in radiometric dating is part of a make-or-break contextual study that hasn't been done in either the Meadowcroft or Monte Verde cases. Yet while directly dating the lowermost Meadowcroft and Monte Verde organics, such as plant fiber, wood, or nutshells, might serve to test the possible contamination of sediments with dead carbon, these materials also must be proven to have cultural associations, and that requires much more taphonomic detective work.

The prevailing strategy in Paleoindian debates is mainly trying to strip opponents of respect instead of objectively answering the criticisms about dubious interpretations. Also favored is accusing critics of misunderstanding or distorting one's views, although distortions and misrepresentations are rhetorical sins that all parties commit. In the words of C. Hitchens (2004:28), writing in reply to a similar kind of response to his criticisms, "When a man thinks any stick will do, he tends to pick up a boomerang."

Brandscapes

This, like other species of patriotism, consists in a sincere belief that the institution to which you belong is better than an institution to which other people belong.

F. M. Cornford (1908)

Such personality-driven debates in place of collaborative detective work possibly arise from the unconscious process of archaeological "brandscaping," a term I borrow from modern marketing and cultural studies. This word usually means the marketing of an object by creating special spaces, designs, and associated products that consumers can identify with it. The word as I use it here refers to the transformation of what should be merely an archaeological interpretation into a career-centered cause. When an archaeological interpretation/scenario/discovery is introduced by one or a few sources, it may strike a chord with archaeologists and become a widespread belief instead of a testable (and in-need-of-testing) possibility, even though we all know that archaeological stories are unproven. Eventually, the belief comes to be shared by people who know little to nothing of the original evidence behind the interpretation. In the case of the South American site called Monte Verde, the belief that the site is reliably interpreted rests firmly in many people who may not have read the two big volumes about it. The site becomes part of a mass belief system, in which certain key concepts are always linked and firmly accepted.

Mass belief systems may not be "very deep or long lasting," but they are superpotent (Twitchell, 2003:vii). The population of believers may not share wider interests or even specific knowledge, but they understand each other because they share a branded thing, such as a Monte Verde point of view about American prehistory (*viz*., pre-Clovis populations spread across the New World with minimal visibility and little ecological impact, speaking different languages and having different geographic origins, etc.). An attack on Monte Verde is an attack on a global brand name having a huge list of consumers. To consume the Monte Verde story is perhaps to feel part of a new cognoscenti, a special class of archaeologists, a fresh generation of prehistorians who feel entitled to believe in a site where almost everything is unique, unreplicated elsewhere, different from all other sites.

This is a safe way to consume the brand's rejection of the status quo and is thus a downstream form of iconoclasty (Twitchell, 2003), or a secure way for brand adherents to feel in the know about the past's most cryptic evidence. Monte Verde is an example of a Paleoindian brandscape—a nesting collection of ideas, attitudes, and scenarios that are identifiable and coherent, and most importantly are an ensemble. Perhaps some people have decided to inhabit the Monte Verde brandscape because it is a trend—many of them do not actually calculate its strengths, only its mass appeal. Monte Verde is an object of aggressive marketing, and now it is considered unarguable truth by many people.

Good News

If we knew what we were doing,
it wouldn't be called research, would it?

Attributed to Albert Einstein.

Basic research is what I'm doing
when I don't know what I'm doing.

Attributed to Wernher Von Braun.

Although I've described examples of the biased adoption of taphonomic work in one-sided support of Paleoindian interpretive causes, nonetheless many other examples of enduring and important taphonomic studies have been done specifically for Paleoindian research. As mentioned already, Lawrence Todd in particular has quietly taken Paleoindian taphonomy to a higher level, along with a few other students who trained with George Frison or Lewis Binford. Their work has provided Paleoindianists with necessary guides to taxon-specific pattern-recognition principles, such as Burgett's (1990) study of coyote (*Canis latrans*) scavenging on bison (*Bison bison*) and elk (*Cervus canadensis*) carcasses, Matthew Hill's (2001) part-taphonomic analysis of Paleoindian diet and subsistence, or Todd's (e.g., 1983, 1987; Todd and Rapson, 1988, 1999) series of papers about quantification and precise data-recording standards. My own early publications—where I described general shapes and unquantified central frequencies of bison bones affected by a variety of noncultural processes—appear inadequate today when seen in the light of the work done by these colleagues. The detective business in Paleoindian studies hasn't always been faultless, but I think it has moved along towards maturity.

My elephant-taphonomy studies, which are ongoing to this day, are useful, I hope, but I had to self-fund much of the fieldwork after the NSF review process proved so bigoted in the mid-1980s. The experts who ignored or disliked the work in the 1980s still do, apparently, but I don't think they have read very much of it, judging from their unwillingness to cite the publications, even if only to disagree with them. To quote essayist C. Hitchens (2004:28) again: "After allowing me to shoulder my way, with many a sigh, through all [their] scurvy pages, [they] will not deign to glance in return at what I wrote."

Like scholars-in-disagreement, scholars of a later age are rarely generous towards the output of earlier generations. Many of the taphonomic publications of 25 years earlier suffer criticisms (then and now) for not addressing contemporary keynotes. Yet in spite of the sore points, many hindsight evaluations are also useful (for example, Lyman and Fox (1989) on variability in bone weathering) and do add a new, valuable dimension to the pioneering publications. I am at best a peripheral player in taphonomic dramas, but I too have learned how it can be both ego-feeding and bruising to see one's works dissected and critiqued by sharp graduate students (Graves, 2002) or colleagues (Domínguez-Rodrigo and Piqueras, 2003), but it is the price one pays for publishing in science.

More than two decades after Brain's *The Hunters or the Hunted* book, several of the 1970s-era taphonomists who set the pace for Paleoindian researchers have moved away from fundamental actualistic fieldwork. One example is Pat Shipman, now a successful science writer. Others did not so much leave the field as step laterally to be bigger-picture interpreters, a natural evolution of career trajectories. For example, Andrew Hill, who had plunged into the young and burgeoning field of taphonomy in the late 1960s, has since become a leader in hominid ecology and taxonomy. Others who were Brain's compañeros in the 1960s and 1970s taphonomic research, such as Kay Behrensmeyer (e.g., 1975), never really left the actualistic work behind but expanded upon it to show how ecosystem reconstructions can be improved through taphonomic analyses. The trend towards moving on from taphonomy has also depleted the ranks of Paleoindianists who once ran actualistic or neotaphonomic projects. In my 22 years of full-time teaching, I've had only one doctoral student in taphonomy, but I'm still doing my own taphonomic fieldwork, even if I can't interest anybody else.

The global community of taphonomic researchers has grown well beyond Paleoindianists and the specialists working in Africa. Researchers from South America and Europe in the past years have carried out their own new actualistic or taphonomic studies (for example, Mondini, 1995, 2000, 2001a,b; Wojtal, 2001; Wojtal and Sobczyk, 2003), aimed at correcting earlier studies' errors or shaping the research towards local conditions, thus keeping the field alive, to use an ironic modifier. Each new study reveals the temporal and geographical variability in taphonomic processes, thus potentially adding to Paleoindian bone assemblages a wider array of new sources of proxy information about the past.

The most encouraging trend in Paleoindian taphonomic research has been the awareness (still not fully emerged) that controversy is actually good and skepticism is even better. Archaeologist Charles Keally, comparing the nature of America's Paleoindian debates with the controversy about the nonexistence of an Early Paleolithic stage in Japan, pointed out that the Paleoindian debate has become interdisciplinary, scientific, and academic, and while criticisms are often heated, "conferences and publications purposefully include contributors from both (all?) sides" (Keally, 2001). In Keally's view, controversy is exciting and useful, most ideas are openly admitted to be only speculation, questioning is and should be common, solid scientific evidence is required [to address problems], vigorous and public debate is normal, people should be encouraged to change their minds after hearing new evidence or arguments, and scholars should enjoy having their ideas criticized. While some Paleoindianists (Adovasio and Page, 2002; Dillehay, 2000) may not appear to agree with these precepts, the current generation of taphonomists must have gotten used to them by now.

Brain's Sway

…small things [can be used to] discover great [things]…better than great can discover the small.

Francis Bacon (1973; orig. 1605)

Is the post-"Hunters or Hunted" period a case of Paleoindianists behaving as Feyerabend (1975) suggested they might want to do during a period of changing interpretations (anything goes—anarchy and intellectual dishonesty are acceptable and valid when exploring the unknown), or as Bourdieu (1977) theorized they usually would (careerism is as important in shaping scientific trends as any so-called objective search for truths), or as Kuhn (1962) had generalized (periods of paradigm shift are full of programmatic confusion and leadership struggles)?

The last quarter-century of Paleoindian studies had these and other socio-political processes taking place, but the model C. K. Brain had established was available as a filter to pass the three kinds of pettiness through. Brain had asked a question in the title of his major book —*The Hunters or the Hunted*?—and that willingness to query was intended to encourage readers to weigh evidence and seek answers, rather than to decide those answers in advance and to merely invent *ad hoc* models to support hardened opinions.

Practically speaking, Brain's book showed him engaged in multiple modes of taphonomic study. He had done feeding experiments with large carnivores, he had carried out actualistic studies, he had learned the patterns of fossil bone modifications, and he had carefully read the growing literature about other people's taphonomic research. He thus put together his case step-by-step.

Rubidge (2000:5) has pointed out that Brain enjoyed the day by day process of just doing science. He looked for answers creatively and often in the same ways the old fashioned naturalists did it, by allowing himself to veer off intellectually in many different directions when it seemed to be needed. He was not a project-driven careerist obsessed with achieving prominence in his field, which I think set him apart from many Paleoindianists.

He was inspired to do the taphonomic work first by his knowledge (and eventually doubts) about Raymond Dart's hunting-ape hypotheses (Brain, 1997), and second by his first-hand knowledge of Plio-Pleistocene fossils—accumulated through decades of "hard labor at Swartkrans" (Brain, 1973, 1974, 1976a). He was also inspired by the new ideas emerging from meetings with other researchers who had similar puzzles to solve. Especially catalytic was the Wenner-Gren conference of 1976 (Brain 1976b). Yet Brain was a true all-around naturalist —he worked as a geological scientist, a paleontologist, a lower-vertebrate zoologist, the director of a major natural history museum, a historian of science, a biographer of scientists—in short, he had no end to the shifting problems he wanted to address.

He patiently kept at the taphonomic work for over two decades—never expecting to solve the problems in a single field season or a single research process. The specifically taphonomic set of problems did not completely monopolize his attention from the late 1960s through the 1990s, but it came close to doing so.

He had the benefit of living on a continent where the most directly relevant taphonomic fieldwork could be done (such as seen in Hill, 1975; Maguire et al., 1980; Richardson, 1980). Meanwhile, Paleoindian taphonomists trying to work within North America faced a shortage of landscapes where they could study noncultural processes such as carcass-feeding by the same free-roaming carnivores that would have been present before the colonial era.

He designed and carried out many different and related projects—examining the effects of bone weathering (Brain, 1967b), collecting ethnographic data (Jenkins and Brain, 1967), observing patterns in humanly butchered remains (Brain, 1967a, 1969), experimentally feeding animal carcasses to carnivores (Brain, 1981), collecting animal bones from wild carnivore lairs (Brain, 1981), collecting bones from owl roosts in caves, and so on.

These projects were examples of actualism, neotaphonomy, and classical taphonomy (defined here as the laboratory interpretation of fossil bone histories). They formed the basis for his "rather odd detective story" about Plio-Pleistocene hominids in South Africa. He was comfortable carrying forward his line of reasoning one small maneuver at a time. He reviewed others' work, collected data, and spelled out his alternative interpretations with grace and tact.

Did Paleoindian taphonomists follow suit? I don't think we ever really did, but once in a while some scholars came close. My own body of work is incomplete and unbalanced compared to Brain's. Other Paleoindianists' work of the 1970s and 1980s also seems unfulfilled or provisional, but several classic references will never lose their usefulness. Yet, Paleoindian studies in general have stayed at an unfledged stage because American prehistorians often seek "brands" of interpretations instead of facing the complexities and ambiguities that a long-term commitment to taphonomy reveals.

I end this paper by acknowledging that Brain's contribution to Paleoindian research went beyond merely providing examples of taphonomic studies to emulate. To his greatest credit, he also showed us how to stalwartly present a case without alienating colleagues and friends.

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