Building Cultural Chronology in Eastern Washington: The Influence of Geochronology, Index Fossils, and Radiocarbon Dating

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Until the early 1950s, it was believed that the archaeological record of eastern Washington state did not exceed 4000 years. That belief changed in the middle 1950s after discovery of the Lind Coulee site (45GR97), originally dated to the Holocene on the basis of geochronological data and subsequently dated via radiocarbon to 8700 B.P. Geochronology and geochronology provided temporal control and resulted in archaeological time being measured discontinuously as stratigraphically associated sets of artifacts were stacked one upon the other. Percentage stratigraphy and frequency seriation played no role in the measurement of time or the construction of cultural chronologies. Local cultural chronologies originally constructed on the basis of chronostratigraphic marker horizons were not significantly altered in the 1960s and 1970s because few radiocarbon ages were run and the chronological validity of the ages was questioned. The increased intensity of cultural resource management in the 1980s witnessed a marked increased in the production of radiocarbon ages, but extant cultural chronologies have not been significantly altered in structure or appearance. © 2000 John Wiley & Sons, Inc.

INTRODUCTION

Although the point had been made before (e.g., Willey, 1953), several decades ago Albert Spaulding (1960) made explicit the fact that the archaeological record comprised what he called three dimensions of variation. First, it varied in formal properties, or simple form artifacts came in various sizes, shapes, colors, and compositions. Second, those forms had particular and sometimes peculiar geographic distributions. Third, artifact forms had distinct distributions in time; some seemed to occur briefly whereas others appeared to have been made over rather long time spans or during multiple time spans. Part of Spaulding’s discussion was devoted to the fact that the first two dimensions form and space were directly visible whereas the third dimension time had to be made visible analytically. It is the last fact that was squarely faced and directly addressed during the so-called culture-history period of Americanist archaeology (e.g., Lyman et al., 1997; Willey and Sabloff, 1993). During this period, relative-dating techniques such as the direct historical approach (Lyman and O’Tien, 2000), seriation, stratigraphy, and typo-
logical cross-dating (O’Brien and Lyman, 1999) were variously developed and put to extensive use by archaeologists trained and working in North America.

The history of how archaeologists grappled with making time visible is an intriguing one (O’Brien and Lyman [1999] and references therein), and it took an interesting turn after 1950, when radiocarbon dating became available. That turn, later referred to as the "radiocarbon revolution" (Renfrew, 1973; see also Taylor, 1996), prompted a major rethinking of at least some portions of humankind’s past. But the impact of this dating technique was not everywhere the same; it was not always a "revolutionary" development. In this article, I present a history of efforts to build cultural chronology in eastern Washington state, comprising part of the Columbia–Fraser Plateau culture area. Archaeological efforts to construct cultural chronologies for the region illustrate the lack of "revolutionary" impact of radiocarbon dating when other dating techniques—in this case, particularly geochronology and index fossils—have served their purposes well.

To avoid a "presentist history," I begin with a brief summary of the general historical context and structure of Americanist archaeology during the first half of the twentieth century. I then indicate how I generated a data base of known radiocarbon ages from sites in eastern Washington. This is followed by a summary of the conception of eastern Washington prehistory prior to the middle 1950s when the first "early" site was found. That particular site was thought to be "early" because of its geological context and the styles of projectile points it contained; the seminal radiocarbon age for eastern Washington came from this site and showed that it was in fact early. I document at some length how this site set the precedent for building local cultural chronologies: use geochronological evidence and particular styles of projectile points. I show that early misunderstanding of the radiocarbon dating method and problems with some ages did not hinder the perfection of the local cultural chronology because that chronology was founded in geochronology, and that only in the 1980s was radiocarbon dating used extensively to refine local cultural chronologies. Finally, I argue that the chronological methods that have been used in eastern Washington have resulted in sequences of cultures that measure time discontinuously and minimal effort has been made to measure time and culture change continuously.

BACKGROUND

Received wisdom within Americanist archaeology is that the 1910–1950 period began with the so-called stratigraphic revolution, or the use of stratigraphic excavation for purposes of measuring time (e.g., Browman and Givens, 1996). In my view, this is a mischaracterization. Before the end of the 19th century, archaeologists in general knew that the depth of an artifact beneath the ground surface was not a good indicator of that artifact’s age, and they also knew that an artifact’s stratigraphic context often did provide a good indication of its age (Lyman et al., 1997). The real revolution resided in a change in how artifacts were classified; before 1910, artifacts had often been categorized as cultural traits such as "pottery"
or "projectile points." Shifting the scale of their classification units to variants of traits, such as red-on-white pottery and side-noticed projectile points, archaeologists confirmed that they could measure the passage of time by plotting the changes in artifact variants against their stratigraphic provenience (Lyman and O'Brien, 1999). Two alternatives then presented themselves. The fluctuating frequencies of what quickly became known as "styles"—artifact variants that permitted time’s passage to be measured—could be used to mark time’s passage in a continuous manner when seriated (Kroeber, 1916; Spier, 1917) or when plotted against the stratigraphic column that produced them (Kidder and Kidder, 1917; Nelson, 1916)—the technique later referred to as percentage stratigraphy. (Percentage stratigraphy involves plotting the relative—proportional—frequency of each artifact type in each of several stratigraphically delimited assemblages against the stratum from which they were recovered.) Alternatively, styles could be defined in such a manner as to comprise index fossils that encompassed small chunks of time and allowed correlation of spatially remote collections; these kinds of styles became known as horizon styles (Willey, 1945) and measured time discontinuously.

As methods for measuring time with artifacts became better understood and refined, it was shown that if one were to measure time with observed variation in artifact form, then how time was measured depended on how that form was partitioned into artifact styles or types (e.g., Phillips et al., 1951). Time is a continuum, but we divide it up into chunks of various sizes—millennia, centuries, decades, years, months, days, hours, minutes, seconds. The size of those chunks depends on the units used in the measurement system. In the case of archaeology, the units are artifact types that encompass amounts of time that vary more or less directly with the amount of formal variation included in each type. Index fossils and horizon styles tend to comprise brief temporal periods and tend to measure time discontinuously because each style signifies a more or less unique portion of the temporal continuum; ideally, there is little chance of one horizon style temporally overlapping with another. Artifact types that measure longer temporal periods are more likely to overlap temporally but not be perfectly coincident with one another; such types measure time continuously precisely because of their overlap (Lyman and O'Brien, 2000; O'Brien and Lyman, 1999).

The choice of how artifacts were to be classified, then, influenced how time was ultimately measured—continuously or discontinuously. Americanist archaeologists do not seem to realize that they chose the latter for two epistemological reasons. First, they sought prehistoric "cultures" to stack up in a sequence or chronological order (Lyman et al., 1997). Second, the apparent reality of resulting discontinuous chronologies was reinforced by a focus on the stratigraphic context of artifacts. As visibly distinct units of deposition with chronological significance, strata became units of artifact collection and association (Lyman and O'Brien, 1999). Because (1) the depositional record was viewed as a set of nonoverlapping chunks called strata and (2) artifacts were treated as index fossils for purposes of temporal correlation of collections distributed across space, time was more often than not measured discontinuously despite the availability of methods for meas
suring it continuously (Lyman, et al., 1998). Both of these factors played a significant role in building cultural chronologies in eastern Washington. In this area, their influence was so pervasive that the advent of radiocarbon dating had little impact on perceptions of local prehistory save for refining a few temporal boundaries between prehistoric cultures within sequences established on geochronological bases. My colleague Jerry Galm (personal communication) suggests that "benign neglect" of the radiocarbon dating method also influenced perceptions of local prehistory; in my view, he is correct.

METHODS AND MATERIALS

I restrict my analysis to eastern Washington—that area of the state lying east of the crest of the Cascade Range (Figure 1). The low-lying area drained by the Columbia (and Snake and Yakima) River(s) physiographically comprises what is referred to variously as the Columbia Basin, Columbia Plateau, Southern Plateau, or simply Plateau. Histories of archaeological research in the area (Lohse and Sprague, 1998; Sprague, 1973) do not consider the influence of underpinning epistemologies, chronological methods, or radiocarbon dating on our understanding of cultural chronology. To do that requires examination of various news announcements, papers, and monographs, as well as reviews. It also requires a database comprising all radiocarbon ages and various details about each age. A description of how the database was compiled is therefore important.

I consulted the list of radiocarbon ages for the state of Washington compiled and published by the Washington state Office of Archaeology and Historic Preservation (OAHP, 1990). I first made corrections based on the original publications of many of the ages in that list. In a few cases, an incorrect laboratory number was found; in other cases, "charcoal" was given as the dated material when, in fact, the sample was "mollusk shell" or "bone." I also compiled information on all other radiocarbon ages I was aware of, many but not all of which were obtained in 1990 or later. The OAHP (1990) publication lists 513 ages from 138 sites in the 20 counties of eastern Washington. I compiled information on an additional 243 ages from 88 sites; 22 of the sites are listed in OAHP (1990), but the additional ages are not. Some of these additional ages were not included in the OAHP list apparently because they were "modern" in age or were deemed erroneous (for any of several reasons) by the excavator; others were not included because OAHP lacked the pertinent literature. I tallied all ages irrespective of age or archaeological context and association because, although I was interested in the chronological information the ages provide, for purposes of this article I was more interested in what the ages indicate about historical efforts of local archaeologists to measure time, particularly as it pertained to building a cultural chronology. For this reason, I examined all literature that included mention of a radiocarbon age. For many ages this meant only the site report; for others, this might include the site report, a published paper, a mention in the journal Radiocarbon, or some combination thereof.

I did not examine radiocarbon laboratory files; rather, I examined all reports—

short
standard
news releases, articles, conference papers and published abstracts thereof, mono-
graphs, books, contract reports — that I could locate. The published — or at least
written and circulated — record is, after all, what most archaeologists have ready
access to and is what would have had the greatest influence on ideas about eastern
Washington prehistory. The written laboratory files are generally only available to
the archaeologist who submitted the sample. For each age I recorded the material
dated, the laboratory number (not reported for several ages), the site that produced
the material, and the first written mention of the age. Several ages were listed as
“dendrocorrected” ages, but in all but one case I found the corresponding age in
raw radiocarbon years in the literature.

Although many ages were calculated using the half-life of 5568 years (or 5570,
per the instructions in Radiocarbon) originally proposed by Libby (1952), some
ages were calculated (or recalculated by the archaeologist) using the half-life of
5730 years. It is not always clear in the literature I examined which half-life an age
is based on, but when possible I used the age based on the 5568 (5570) year half-
life in analyses discussed below. Finally, some ages are reported as “adjusted” for
isotopic fractionation (e.g., Taylor, 1987:120–123), but the raw or nonadjusted age
is not always mentioned in the same literature as the adjusted age. When possible,
I tallied both the adjusted and nonadjusted ages, but I use only the latter in my
analyses. My sample comprises 756 ages from 204 sites. The facts discussed in the
three preceding paragraphs mean that not all the ages used in the analyses below
are strictly comparable in terms of what they measure. This may influence some
of the results I describe, but I presume those influences are minimal and that were
they eliminated from the data, my conclusions would not be significantly different.
All ages mentioned are given in radiocarbon years B.P. unless otherwise noted.

EARLY THOUGHTS ON CULTURAL CHRONOLOGY

To understand the impact—or lack thereof—of radiocarbon dating on our
knowledge of eastern Washington prehistory, we must first have a feel for archae-
ological conceptions of that prehistory prior to the use of radiocarbon dating. In
general, perceptions of the human prehistory of eastern Washington mirrored those
perceptions across the North American continent—there was no significant time
depth. Archaeological materials could be assigned to the historical period if they
included associated Euroamerican artifacts such as trade goods, or to the prehis-
toric period if no such artifacts were associated. For example, as archaeologist
Harlan Smith (1906b:2) remarked, “the absence of European objects in many of the
places explored establishes the fact that the remains there found antedate the
coming of white traders.” When evidence for “Early Man” was found at Folsom,
New Mexico, in 1927, the bottom dropped out of this tidy picture (Meltzer, 1983).
The convincing evidence for great antiquity in the American Southwest was the
association of artifacts with remains of an extinct form of bison, but no such evi-
dence was available in eastern Washington. Had such evidence been found—and
Charles Sternberg (1903) speculated 24 years before the Folsom discovery that it
surely would be found—it is likely that the history of eastern Washington archae-
ology would have been quite different.

A discovery of artifacts associated with the remains of extinct animals would
have done the trick. But this kind of record was unnecessary to convince people
of a deep human antiquity in eastern Washington. This is so for the simple reason
that by the early part of the 20th century it was generally agreed that mammoths,
large forms of bison, and other taxa comprising the "Pleistocene megafauna" had become extinct by the end of the Pleistocene. The remains of these beasts were known from particular distinctive strata in eastern Washington (e.g., Bryan, 1927), and the discovery of artifacts in those same strata was sufficient to ultimately convince archaeologists that people had been in eastern Washington since the terminal Pleistocene. Initially, however, given that most known artifacts were not too different from those used by extant indigenous peoples, "it was taken as established that the prehistoric cultures were relatively recent" (Campbell, 1989:54). No one worried about chronological issues because there had been no temporally lengthy occupation of eastern Washington.

In eastern Washington, researchers perceived some of the same problems with the archaeological record encountered elsewhere in North America (Lyman et al., 1997). Depth beneath the modern ground surface was not a reliable indicator of age, as Herbert Spinden (1908:178) recognized when he wrote that artifacts "found several feet below the surface . . . offer no trustworthy evidence of extreme age because of this impermanent and unstratified character of the soil in which they are found." By "impermanent" Spinden had in mind the fluvial reworking of floodplain sediments; such sediment "overlies river beaches of rounded boulders. It is light, and easily crumbles under the action of water. A large mass of such [sediment] could be shifted or built up in a short time with the river in flood" (Spinden, 1908:178). Two years earlier, Harlan Smith (1906a:552) had made similar observations based on his work in the Yakima River valley: "Definite age can not be assigned to the archaeological finds, since . . . the remains are found at no great depth or in [sediment] the surface of which is frequently shifted." But such was of no great moment because the "prehistoric culture of the region was apparently similar to that of the present natives" (Smith, 1906a:553).


In their landmark volume *Indians Before Columbus*, published just before the midpoint of the 20th century, archaeologists Paul Martin, George Quimby, and Donald Collier (Martin et al., 1947:444) made the following observation:

Archaeological research in the Plateau has not yet developed a prehistoric chronology. It is possible on the basis of presence or absence of White trade objects to distinguish historic from prehistoric sites, but at present the prehistoric sites cannot be chronologically differentiated. On the basis of available evidence archaeological remains in the Plateau do not appear to be very old, nor is there evidence of much cultural change in the prehistoric period.

In 1968, Frank Leonhardt (1968:28) indicated that in the late 1940s "the prehistory of [Plateau] was considered to have been about 2,000 years long." Several years earlier, Earl Swanson (1962a:152) suggested that "few people [in the 1950s] believed that man had been in the Columbia Plateau more than 2000 to 4000 years (for example, Shiner, 1961)." This is something of an overstatement because Shiner (1961:259) had indicated that "Paleo-Indian sites are present in the Plateau, but..."
have not been found in the McNary region" at the southern edge of eastern Washington. What then were believed to be the hallmark artifacts of the "Paleo-Indian" period were fluted points, either of the Folsom or Clovis categories (e.g., Osborne, 1956). Given the belief that only fluted points signified the Paleoindian period, and an apparent absence of such points, one of the "axioms" of local archaeology in the early 1950s was that "man has been in the Columbia Plateau no more than two thousand years" (Swanson, 1962b:1). As Swanson (1962b:1) implied, this axiom was underpinned by the "use of typology as an index of antiquity," an underpinning that Swanson indicates Richard Daugherty (e.g., 1956a) "was in the process of demonstrating...was of little significance in the identification of Early Man in the Plateau." That is, the index fossil of fluted points was not required to establish the presence of people in eastern Washington during the terminal Pleistocene and earliest Holocene, just as an association of artifacts with an extinct mammal was not required. Geochronological evidence sufficed.

Geochronology

The archaeological record is a special kind of geological record. Swanson (1962b) emphasized a geochronological approach to learning about an area's culture history through archaeological research. In a review of one of Swanson's later monographs, Douglas Osborne (1966:578) noted that "Swanson's suggestions as to geochronology and cultural change are basic." Most people working in eastern Washington in the 1950s knew about J. Harlan Bretz's (e.g., 1923a, 1923b, 1925) hypothesized "Spokane Flood," sometimes referred to as the Missoula Flood(s) after the Pleistocene lake in western Montana that was the source of the water, or the Scabland Flood after the erosional features it created in eastern Washington. The last of this set of floods was thought to have occurred about 11,000 – 15,000 years ago, even before the floods were radiometrically dated (Daugherty, 1956a; Fryxell, 1962; Mullineaux et al., 1978; Richmond et al., 1965a, 1965b). The floods had scoured much of the central part of eastern Washington but had also deposited so-called "flood gravels" that provided a chronostratigraphic marker then thought to be below the basement of the cultural sequence. Near the middle of the Holocene-stratigraphic sequence another chronostratigraphic marker existed in the form of a sometimes thick layer of volcanic ash that geologists in the 1950s thought had been deposited about 6000 years ago. The basis for that age assessment resided in pollen cores containing volcanic ash and rested on palynologist Henry Hansen's (1947) suggestion that a period of marked volcanic activity dated to the postglacial time of maximum warmth and aridity, which he estimated to have occurred during the middle Holocene, or about 8000 – 4000 years ago (Daugherty, 1956a). By the early 1960s, the age of the volcanic ash was thought to fall between 6000 and 7000 years ago (Butler, 1961; Daugherty, 1956a; Fryxell, 1965; Fryxell and Daugherty, 1962; Swanson, 1962b; Warren, 1968). Libby (1952:86) had reported ages on four samples from a tree in Oregon thought to have been "burned by the glowing pumice thrown out by the explosion of Mount Mazama"; these samples gave an
"average" age of 6453 ± 250 B.P. (C-247). Another age of 6640 ± 250 (W-858) on "charcoal found buried in pumice . . . believed to be from the Mount Mazama eruption" was reported several years later by Ruhm and Alexander (1960:161). The problem was, and is, that there are multiple volcanic ashes in various stratigraphic exposures in eastern Washington (e.g., Davis, 1985; Sarna-Wojcicki et al., 1983). Rigg and Gould (1957) muddied the waters when they suggested that what are now known to be Mazama ash deposits (Powers and Wilcox, 1964) originated from an eruption of Glacier Peak; they published two "composite" ages on peat from multiple cores in two lakes in western Washington—6950 ± 200 and 6500 ± 200 (no laboratory accession numbers were indicated by Rigg and Gould [1957], but see Broeker et al. [1956]). These ages produced what they called an average age of "about 6700 years" (Rigg and Gould, 1957:354) for the ash. This age, or "6600 B.P." (Fryxell, 1963:1288; Wilcox, 1963:810), or "6500 [B.P."] (Fryxell, 1963:16; 1965:1288), were the ones typically mentioned by archaeologists from the 1960s through the 1980s when using what was believed to be a deposit of Mazama ash as a chronostratigraphic marker (see below).

Not all archaeologists were aware of what their colleagues in geology knew or suspected. The same year that Martin et al.’s (1947) book was published, the Smithsonian Institution’s River Basin Survey began work in the Columbia Basin (Sprague, 1973). One of those projects involved excavations in the soon to be created McNary Reservoir area along the lower Columbia River. Joel Shiner (1961), under the supervision of Douglas Osborne, was responsible for much of the work involving prehistoric materials in the McNary Reservoir area. His report was submitted in 1955 but was not published until 1961 (Shiner, 1961:155, footnote 1). In that report, Shiner documented that artifacts had been found beneath a volcanic ash at two sites and that three other sites had artifacts stratigraphically above what appeared to be the same ash. Shiner (1961:173) estimated the age of the deposition of the ash, based in part on the amount of "changes in material culture" he perceived, as "closer to 2,000 [years ago] than 5,000." The volcanic ash provided a useful chronostratigraphic marker, but at about the same time that Shiner was estimating its age, geologists were learning how to identify that particular ash and how old it really was. Part of the difficulty with using Mazama ash as a chronostratigraphic horizon today resides in the fact that the eruptive activity of Mount Mazama resulted in the deposition of several ash layers between about 7000 and 6700 B.P. (e.g., Davis, 1985; Sarna-Wojcicki et al., 1983). The so-called "climatic eruption" is generally thought to date to about 6845 ± 50 B.P. (Bacon, 1983), though numerous ages suggest that many Mazama ash deposits date to ca. 6700 B.P. (e.g., Hallet et al., 1997).

Once Mazama ash could be reliably identified chemically and distinguished from other volcanic ashes (e.g., Powers and Wilcox, 1964; Steen and Fryxell, 1965), it provided a good temporal marker, or chronostratigraphic horizon, whenever it was found with artifacts above or below it. It served as well to denote the mid-Holocene climatic optimum period referred to as the Altithermal (Antevs, 1948). As researchers worked out these geological and paleoclimatological details, they were, as the
proceeding indicates, helped by the advent of radiocarbon dating. Simultaneously, that dating technique began to be exploited by archaeologists who had until that time largely relied on typological cross dating (e.g., Osborne, 1957; Shiner, 1961) or on geochronological data (e.g., Caldwell, 1956; Daugherty, 1956a; Swanson, 1962b) to determine how old their artifacts were.

After the radiocarbon method had been available for about a decade, efforts at chronological synthesis made little or no mention of the radiocarbon ages then available, probably because there were so few of them (e.g., Daugherty, 1962a, 1962b), and local archaeologists still relied heavily on geochronology to help construct site-specific cultural chronologies (e.g., Daugherty et al., 1967; Rice, 1965). By the time radiocarbon dating had been available for nearly two decades, some chronological syntheses listed ages explicitly, whereas others merely mentioned that some radiocarbon ages were available. Browman and Munsell (1969:250) listed 32 ages to help establish the temporal boundaries of the cultural periods they proposed for the Columbia Plateau, 18 of these came from sites in eastern Washington. In what is probably the single most-often cited cultural chronology for the area, Leonhardt and Rice (1970) noted that radiocarbon ages were available for the sequence of cultural phases they proposed, but they mentioned only one explicitly (see below). These observations prompt the questions of when ages were assayed, and how often they were assayed.

WHEN WERE AGES ASSAYED?

The first radiocarbon age to be produced for an archaeological collection from eastern Washington of which I am aware was based on material recovered from the Lind Coulee site (45GR17) (Figure 1). The age was reported in Richard Daugherty’s (1953) dissertation and the following year by both Libby (1954) and Daugherty (1954). Daugherty chose to excavate Lind Coulee because it seemed to represent an ancient site. When first discovered in 1947, the “nature of the deposits, the depth of the flakes and bone fragments from the surface (ca. 14 ft), and the heavily mineralized state of the bones, suggested...the possible existence of an archaeological site containing evidence of Early Man” (Daugherty, 1956a:223). While working at a nearby site in 1950, Daugherty (1956a:224) again visited Lind Coulee in an attempt “to establish with certainty whether or not an early archaeological site existed [there].” The discovery of a projectile point fragment and a scraper in the exposed coulee wall served to establish this fact, and the site was tested in 1950 and excavated in 1951 and 1952.

Given the general feeling at the time that eastern Washington’s archaeological record had no significant time depth, it is perhaps not surprising the belief that Lind Coulee was ancient and the bases for that belief were announced several times in the pages of Americanist archaeology’s leading journal, American Antiquity. There, the depth and stratigraphic context of the artifacts and bones were noted; a geologist who believed the deposits were of terminal Pleistocene age was cited as well (Burroughs, 1951; Krieger, 1952b). Projectile point styles were also said to
suggest early forms" (Krieger, 1952a:281), and it was noted that radiocarbon ages were pending (Krieger, 1952b). Lind Coulee couldn’t have been a better choice for the first radiocarbon age. Daugherty (1956a:236) noted that “on the basis of the non-stratified occurrence of the artifacts, we must consider the cultural horizon as a single unit. This interpretation is supported by an analysis of the various artifact forms.” Lind Coulee was what can be termed a single-component site and thus was relatively easy to interpret. The single component had a rather unique kind of projectile point for the area, but it was also somewhat similar to suspected ancient point styles in other areas, and geochronological data suggested significant antiquity. Daugherty (1956a:255) stated that “geological evidence indicates that the occupation of the site occurred at some time within the period from 11,000 to 6,000 years ago,” and elsewhere (Daugherty, 1956b:93) indicated that the age of the site “had been determined at between 8,000 and 10,000 years on the basis of geological, paleontological, paleoclimatological, and comparative cultural data.” Radiocarbon analysis confirmed these measures of antiquity, and thus Lind Coulee caused the bottom to drop out of the perceived shallow time depth of local prehistory.

The seminal radiocarbon assay from Lind Coulee exemplifies some of the problems that continue to plague the published radiocarbon dating record for archaeological materials in eastern Washington. It also indicates how a new, poorly understood technique for measuring time could not in any revolutionary fashion replace the tried-and-true geochronological technique for measuring time. Libby (1954:739) reported that the first Lind Coulee date was run on charcoal; Daugherty (1954:422) stated that the date was run “on burned bison bone found in direct association with artifacts.” Over the next few years, Daugherty (1956a:234; 1959:50) continued to report that this date came from “fragments of burned bison bones,” and this statement was repeated by his collaborators (Fryxell, 1963:7–8). Inspection of Libby’s files would probably be the only way to clarify this discrepancy.

There is another interesting aspect to how the Lind Coulee age was reported. Libby (1954:739) indicated that sample C-827 comprised two ages—9400 ± 940, and 8518 ± 460—for an “average” of 8700 ± 400. Daugherty (1954:422) indicated the “tests... give the following dates: 9400 ± 940, 8516 ± 460, for a weighted average of 8700 ± 400 years.” Later, Daugherty (1956a:234) simply reported that Libby’s analysis produced a “weighted age of 8700 plus or minus 400 years.” He used virtually identical wording in both another monograph published that same year (Daugherty, 1956b:83) and in a short monograph written for a nonprofessional audience (Daugherty, 1959:50). The age of the archaeological materials at Lind Coulee played an important role in developing notions of local prehistory during the 1960s, but the seminal age(s) were reported in various ways (Table I). That there were no agreed-on rules for reporting radiocarbon years or for converting them to calendar years is apparent.

The seminal age(s) from Lind Coulee was run using the solid-carbon counting technique rather than the modern gas-counting technique (see Taylor [1987:82] for discussion of the two techniques). Sprague (1973:264) suggests that because of this, in the late 1960s and early 1970s “the general consensus today is that [the date(s)]
Table I. Summary of published reports of the seminal radiocarbon age(s) from Lind Coulee (45GR857).

<table>
<thead>
<tr>
<th>Reference</th>
<th>Reported Ages</th>
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<tr>
<td>Libby, 1954:739</td>
<td>9400 ± 940 B.P.; 8518 ± 460 B.P.; average = 8700 ± 400 B.P.</td>
</tr>
<tr>
<td>Daugherty, 1954:422</td>
<td>9400 ± 940 B.P.; 8516 ± 460 B.P.; weighted average = 8700 ± 400 B.P.</td>
</tr>
<tr>
<td>Daugherty, 1956a:234</td>
<td>Weighted average = 8700 ± 460 B.P.</td>
</tr>
<tr>
<td>Daugherty, 1956b:91</td>
<td>8400 ± 940 B.P.; 8518 ± 460 B.P.; weighted average = 8700 ± 400 B.P.</td>
</tr>
<tr>
<td>Stallard, 1958:22</td>
<td>A date of approximately 8700 B.P.</td>
</tr>
<tr>
<td>Butler, 1961:39</td>
<td>Average = 8700 B.P.</td>
</tr>
<tr>
<td>Grable, 1962:19</td>
<td>7445 ± 940 B.C.; 653 ± 400 B.C.</td>
</tr>
<tr>
<td>Warren, 1968:29</td>
<td>740 ± 940 B.C.; 6308 ± 460 B.C.; weighted average = 6750 ± 400 B.C.</td>
</tr>
<tr>
<td>Browman and Munsell, 1969:250</td>
<td>6750 ± 400 B.C.</td>
</tr>
<tr>
<td>Leonhardt and Rice, 1970:25</td>
<td>6750 ± 400 B.C.</td>
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</tbody>
</table>

are too late." I have found several published statements to this effect in the literature, but the details of those statements indicate Sprague’s suggestion is incorrect. Claude Warren (1968:29) indicates that in a 1965 personal communication to him, "Daugherty now feels that the average date is too young," but Warren doesn’t indicate why Daugherty believes this. Browman and Munsell (1969:253) state that in a personal communication to them, Daugherty indicated the 8700 B.P. age is "too recent," but they don’t tell us why. Leonhardt and Rice (1970:25) provide the first indication of why the seminal age might be too young when they cite a personal communication from Daugherty and state that the "original solid carbon bone dates of 6750 ± 400 B.C. (C-827) on the Lind Coulee assemblage are now considered inaccurate. Geologic research since the Lind Coulee site was excavated suggests that it dates approximately to 9,000 or 10,000 B.C." David Rice (1972:170) basically repeats this statement and indicates that Daugherty told him that "more recent studies of the site stratigraphy suggest [the] radiocarbon dates are as much as 2500 years too late." The geochronological and stratigraphic work prompting Daugherty to reject the seminal 8700 B.P. age as too young has, so far as I can determine, never been published. This is perhaps a good thing because researchers who excavated more of the Lind Coulee site in the early 1970s (Irwin and Moody, 1976; see also Sheppard and Chatters, 1976) produced two more ages that confirm the assessment of the first "weighted average" age. One of these new ages was on "humus" (8600 ± 65, WSU-1422), and the second was on bone (8720 ± 20, WSU-1768).

Index Fossils

The seminal (solid) carbon age from Lind Coulee withstood further evaluation probably because, as Taylor (1987:83) notes, the "principal advantage of gas counting was the significant increase in practical operational efficiency over that of solid carbon counting. There was also an increase in maximum dating range and statis-
tical precision, as well as a reduction in the amount of sample material required for analysis. The suspicion that the Lind Coulee age was "too recent" seems to have resided in the concept that, according to Swanson (1962a, 1962b), Daugherty was combating—that is, archaeological types can be used as index fossils for purposes of relative dating and correlation. Daugherty (1962b:145) stated that the "earliest projectile points are lanceolate forms which in the Northwest have been called Cascade points" (Butler, 1961). A few years later it was obvious that this assessment was wrong. Excavations at Windust Caves (45FR46; Rice, 1965) and Marmes Rockshelter (45FR50; Fryxell and Daugherty, 1962; Fryxell and Cook, 1964) revealed stratified sequences of artifacts (Figure 1). Near the bottom of those sequences were Cascade points, but stratigraphically beneath them were stemmed points similar to those from Lind Coulee. No radiocarbon ages were produced from Windust Cave; in 1964 there were only two radiocarbon ages—both less than 8000 B.P.—from Marmes Rockshelter, but by 1968 there were at least 11 ages from the site, and three of them were greater than 10,000 B.P. Stemmed points (of several kinds) were stratigraphically associated with the older ages (Rice, 1969). Thus, what came to be known as the "Windust Phase"—dating as early as 10,000 B.P.—and its stemmed points became the basal culture for eastern Washington.

By the middle 1960s, an age of 8700 B.P. suggested the Lind Coulee materials were little more than a manifestation of a descendant culture rather than representative of the basal cultural manifestation. Formal similarities between Lind Coulee points and some stemmed points from the basal strata at Windust Caves and Marmes Rockshelter helped the Lind Coulee materials regain the status as the earliest cultural material in eastern Washington that they enjoyed in the 1950s. However, mere formal similarity does not necessarily signify that the "Lind Coulee assemblage is a likely ancestor for the Windust Phase" (Leonhardt and Rice, 1970:24–25). For example, D.G. Rice (1972:170) indicates that the "stemmed and leaf-shaped projectile points from Lind Coulee show strong resemblance to the Windust Phase assemblage [comprising materials from Windust Caves, Marmes Rockshelter, and Granite Point], but they are not interchangeable with the latter specimens in style." And H.S. Rice (1965:35) states "projectile points from the Lind Coulee site are quite similar to the specimens from [Windust Caves], but the Lind Coulee points are generally larger and exhibit better workmanship than that found on the Windust [Caves] specimens." No one has yet performed a detailed comparative analysis of the Lind Coulee and Windust Phase points. Nor have fluted points yet been found in a stratigraphic column also containing Windust-style points.

Why Distrust Radiocarbon Dates?

I suspect radiocarbon dating did not immediately see a great deal of use in the 1950s for three reasons. First, Daugherty (1956a:226) noted that the "reliability of certain radiocarbon dates" had to be "assumed." He apparently was cautious because an early article on the dating technique (Bliss, 1952) he referred to "points up the problem of contamination of specimens and the possible resulting errors in
dating," though he indicated that "the basic methodology is sound" (Daugherty, 1956b:92). I suspect Plateau archaeologists were aware of the concern expressed by various geologists regarding the validity of the radiocarbon method (e.g., Antevs, 1953; Hunt, 1955, 1956), though I am aware of no evidence of such an awareness. That awareness may, however, explain Bruce Stallard's assessment of a radiocarbon age from Wakeman Mound (45KL26). The age of 1089 (± 200 [M-247]) was "from what appeared to be a good sample of wood from the bottom level of the site," but it was "surprising" to Stallard (1958:36) because, according to him, typological cross dating suggested an age of 2000–2500 B.C., most likely the latter, and therefore he concluded that "the carbon date is erroneous." Stallard did not indicate his source of information regarding the age; he apparently was unaware of two other ages from the same stratum (Butler's [1960:87] Stratum A) and of similar age (1070 ± 200 [M-409]; 1090 ± 200 [M-410]). Crane and Griffin (1958b:1121) published the M-427 date in November 1958; the other two ages were published in May 1958 (Crane and Griffin, 1958a:1101).

Contamination was the first reason local archaeologists were cautious when it came to evaluating the validity of radiocarbon ages. The second reason resided in the question: Are radiocarbon ages in fact measuring the age of the target event, or something else? Plateau archaeologists were comfortable with discarding an age as inaccurate if it did not fit their notions regarding local prehistory or failed to corroborate other chronological information such as that from geological context or typological cross dating. For example, Stallard (1958:22) accepted the validity of the 8700 B.P. age for Lind Coulee because it was "in agreement with the geology of the site." The fact that the dated event is not always the target event, and therefore one must be sure the two are in direct or temporal association, was a point made abundantly clear by archaeologists Luther Cressman (1951) and Clement Meighan (1956) in significant but seldom cited early papers on the technique. The additional care during field recovery demanded by the radiocarbon method as an indirect dating technique probably contributed to its slow adoption by eastern Washington archaeologists.

The third reason radiocarbon dating probably was not used much by local archaeologists in the first two decades of its availability is that the general assumption—at least until the age from Lind Coulee (corroborated by geological evidence) became available—was that, as various remarks quoted earlier indicate, the human occupation of eastern Washington had no great time depth. Geologist Roald Fryxell (1963:1), for example, indicated that in the 1940s the answer to the question, "When did people first come to Washington state?" would have been "Three or four thousand years ago, at most." Evidence corroborating this observation is found in the remarks of Douglas Osborne (1956:38) who, after noting the seminal Lind Coulee radiocarbon age, indicated he had expected nothing earlier than mid-Holocene in age, so it came "as a distinct surprise" to him that Clovis-like fluted points had in fact been found in the Northwest—points that suggested people had been present since at least the terminal Pleistocene.
WHEN WERE RADIOCARBON AGES REPORTED?

Radiocarbon ages trickled into the literature at a gradual pace in the 1950s and early 1960s, but these were not always widely reported and thus were not well known. For instance, after the Lind Coulee age was reported, a set of ages—comprising nine individual assays on materials from Wakemap Mound—was published in two parts in 1958 by the laboratory that ran them (Crane and Griffin, 1958a, 1958b). These ages were first reported in the archaeological literature in Robert Butler’s (1960) master’s thesis but did not appear in any published and readily accessible archaeological report until 1965 (Butler, 1965). The Wakemap Mound ages are not mentioned in Warren Caldwell’s (1956) dissertation on the site, though Caldwell collected the samples and commented via personal communication on the ages (as reported in Butler [1960]).

In the 1960s, several more ages were assayed, but many of them were single ages from each of several sites. In his master’s thesis completed in 1966, David Browman (1966:125) listed 13 ages from nine eastern Washington sites; seven sites had one age, one site had two ages, and one site—Schaake Village, 45KT17 (Holmes, 1966)—had four (see also Fairhall et al., 1966). By 1967, five ages had been reported for Three Springs Bar (45PE238; Daugherty et al., 1967). Five ages for the Alderdale site in 1967 (45KL5; Cole, 1966, 1967) have been published, but two other samples from this site (350 ± 100 [Gak-1312]; sample [Gak-915] was “contaminated” and did not produce an age) have not appeared in the literature that is generally available; these two ages are discussed in an unpublished manuscript (Getting, 1986).

In 1968 four ages had been run on material from the Ryegrass Coulee site (45KT88), though only one of them is listed in the site report (Munsell, 1968); the fourth is found in a different publication (Kigoshi et al., 1969), and my perception is that it is not well known among local archaeologists. This fourth age (3940 ± 220 [Gak-1486]) compares favorably with one of the three better known ages from the site (3525 ± 145 [UW-112]).

The 1960s also saw the initiation of a dating program focused on the deep stratified deposits at Marmes Rockshelter and the adjacent floodplain sediments—a program that continued, if sporadically, for the next 25 years. The first radiocarbon age from Marmes Rockshelter was reported in 1962 (Fryxell, 1962; Sheppard et al., 1967); 20 additional ages were produced before 1970 (R. M. Chatters, 1968; Fryxell and Keel, 1969; Rice, 1969; see also Kelley et al., 1978), but only one—sample WSU-120 (Daugherty [1964] lists only the age, not the laboratory number)—was reported in the literature prior to 1965. Three ages are mentioned in Fryxell et al. (1968) but most appear in the literature of the following year (Fryxell and Keel, 1969; Rice, 1969). Some of the series of 21 ages indicated human occupation of the area more than 10,000 years ago, confirming the age of human remains dated to the late Pleistocene on the basis of geochronological observations (Fryxell and Keel, 1969; Fryxell et al., 1968). Problems with the preocene chronology of occupation of the rockshelf and the depositional sequence there prompted restudy of the then-available
ages in the 1980s (Sheppard et al., 1984), and an additional six ages were run as checks on the original 21 ages (Sheppard et al., 1987). With a total of 27 ages (one of which produced an age assay of "modern"), Marmes is not only one of the oldest sites in eastern Washington (another is the Richey-Roberts Clovis cache [Mehringer and Foit, 1990]) but the most extensively controlled chronologically on the basis of radiocarbon ages. That more ages exist for Marmes than for any other archaeological site in the area seems appropriate because it is one of very few sites with virtually the entire Holocene depositional and cultural sequence represented in its deposits.

There was a peak output of reports of ages in the 1980s as the result of several large archaeological projects that took place from the late 1970s through the mid-1980s. This peak is readily apparent in a plot of the frequency of radiocarbon ages published per five-year period beginning with the 1950–1954 period (Figure 2). To compile this graph, I used the earliest published record of which I was aware for each age. Thus many of the individual ages are plotted some years later than when they were actually run, though I presume this has little influence on the trends in dating apparent in Figure 2. The graph signifies that archaeological remains recovered and described before 1970 seldom have associated radiocarbon ages. This is perhaps why some analysts (e.g., Browman and Munsell, 1969) listed only a few ages in their syntheses whereas others listed only one (e.g., Leonhardt and Rice, 1970). But there may be another reason for the latter as well.

**Figure 2.** Frequency distribution of radiocarbon ages published per 5-year period.
EARLY PROBLEMS WITH RADIOCARBON AGES

Once the bottom dropped out of the previously perceived shallow time depth of eastern Washington’s archaeological record, the goal became one of filling in the gap between the recent materials and the ancient materials. Charles Borden (1958:1235), for example, noted that we “do not have as yet in Washington any inkling of the grand and continuous sweep of cultural development from early post-Pleistocene times onward. . . . The sites investigated in the interior of Washington to date are either only a few centuries old or, like Lind Coulee, belong in the category of Early Man. A gap of some 7,000 years remains to be filled.” These remarks echo those made with respect to other areas of the continent 20 years earlier (e.g., Kidder, 1936). But filling the gap in eastern Washington was not straightforward. As noted above, some archaeologists of the 1950s were hesitant to accept the validity of radiocarbon ages; but this hesitancy seems to have faded as time went on, and they came to better understand the dating technique. As the second decade of radiocarbon dating drew to a close, however, another cause for concern arose. What might be the problem if the age of a sample did not coincide with the presumed age of the deposit from which the sample derived? It might be that the sample was intrusive to the deposit, such as when the age was younger or older than the age of deposition suspected on the basis of geochronological or other lines of evidence. Fryxell (1962) encountered what he took to be a case of the latter—an older than expected age for the Missoula flood(s). Or it might be that the sample material could not be trusted, such as when a radiocarbon age was considerably older than the suspected age of the deposit. The second possibility became the subject of some discussion and concern in the late 1960s.

Results of excavations at Granite Point (45WT41) formed the basis of Leonhardt’s (1970) doctoral dissertation. In it, Leonhardt (1970:69) noted that excavations elsewhere had produced a radiocarbon age on charcoal and another on freshwater mussel shell, and that “the shell dated earlier than the charcoal by nearly 1,000 years.” Leonhardt gave the laboratory accession numbers for the two ages but did not report the ages or the site that had produced them. The site was 45GA17, the ages were 2230 ± 310 (WSU-465) on charcoal and 3450 ± 325 (WSU-893) on shell. The latter age is not mentioned in Schroedl (1973), though the charcoal age is. A third age, on bone collagen (845 ± 290, WSU-892), is not mentioned by Schroedl (1973) nor is it listed in OAHP (1990). All three ages are found together only in an unpublished (and not widely known) manuscript (Schroedl, n.d.) completed in about 1972 summarizing the field work and findings at 45GA17.

Leonhardt (1970:69) concluded, based on the single pair of charcoal and shell ages from 45GA17 and the geological contexts of the ages from Granite Point, that “it is wise to consider the three shell ages [from Granite Point] as limiting ages only. The actual age of associated phenomena must be considered less than the [shell] dates[s].” Leonhardt mentioned no reason for the discrepancy, though his hand-written editorial notes in Schroedl (n.d.) admonish, “I would consider the shell to be the least reliable, the bone the next least reliable.” In his dissertation,
David Rice (1972:30, 32) referred to Leonhardy’s (1970) concerns and stated that "Rubin and Taylor (1963) have noted possible problems involving 14C measurements based on shell" but did not specify what those potential problems were. Rice (1972:32) then noted that for a pair of associated ages from Marmes Rockshelter, the "shell dated 310 14C years earlier than the charcoal date, but is within one-sigma agreement. Two shell dates (WSU-209 [7400 ± 110] and WSU-210 [7870 ± 110]) are in close agreement." The former pair of ages is 8700 ± 300 (W-2208) on charcoal and 9010 ± 300 (W-2207) on shell. Rubin and Taylor (1963) suggested, although not very clearly, that mollusc-shell ages were untrustworthy. A more clearly worded discussion of the problem they address is found in an article published in the pages immediately prior to Rubin and Taylor’s (1963) article. Keith and Anderson (1963) followed the work of others and showed, using modern specimens, that shells would often produce ages that were too old by various degrees. For example, modern shells of freshwater mollusks they studied produced ages of 440 – 2300 B.P. The degree of difference in the real age and the radiocarbon-determined age depended, as Keith and Anderson (1963) suggested, not only on the bedrock-influenced mineralogy of the water in which the mollusc lived but on the organic carbon added to the water from sedimentary humus. In short, the studies of Keith and Anderson (1963), Rubin and Taylor (1963), and their predecessors suggested shell ages were potentially untrustworthy, and this, in conjunction with the inferred local geochronological sequence, is undoubtedly what concerned Leonhardy (1970), Rice (1972), and other archaeologists working in eastern Washington.

To evaluate the basis for the concern over the validity of shell ages, Sheppard et al. (1979:258) argued that “use of paired sets of radiocarbon dates provides the opportunity to evaluate whether significant systematic differences exist between various materials used for radiocarbon dating.” They used “data from the literature . . . to provide a larger and more significant sample for paired t analysis” (Sheppard et al., 1979:288). They found that “shell – charcoal [age] pairs have a small but statistically significant [difference, but that difference] is not large and shell dates may be useful for radiocarbon dating of archaeological sites” (Sheppard et al., 1979:292). The problem was examined again several years later by James Chatters (1986), who followed the procedure used by Sheppard et al. (1979) of plotting paired ages of associated samples of charcoal and shell on a scatterplot. Importantly, Chatters restricted his samples to those from eastern Washington, a wise procedure given the observations of Keith and Anderson (1963) that local hydrology seems to influence the validity of shell ages. Chatters (1986) found that if the charcoal ages were younger than about 5000 B.P., the associated shell ages tended to be older. He also found that when shell ages were plotted against charcoal ages from the same stratigraphic context, the shell ages tended to be about the same as the radiocarbon ages if the latter were older than about 6500 B.P., a result mimicked by Sheppard et al. (1987) a year later in an analysis using some of the same data. Data that did not exist when Chatters (1986) and Sheppard et al. (1987) did their analyses are now available. I used the data in Table II, and followed Sheppard et al.
Table II. Stratigraphically associated sets of shell and charcoal radiocarbon ages from eastern Washington archaeological sites (modified and updated from Chatters [1986]).

<table>
<thead>
<tr>
<th>Site</th>
<th>Material</th>
<th>Age</th>
<th>Laboratory No.</th>
<th>Reference</th>
</tr>
</thead>
<tbody>
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<td>660 ± 75</td>
<td>WSU-3034</td>
<td>Sheppard et al., 1987</td>
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<td>Sheppard et al., 1987</td>
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<td>Beta-8848</td>
<td>Chatters, 1984b</td>
</tr>
<tr>
<td></td>
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<td>Beta-8856</td>
<td>Chatters, 1984b</td>
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<td>Chatters, 1984b</td>
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<td>Ty-5914</td>
<td>Chances et al., 1989</td>
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<td></td>
<td>Shell</td>
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<td>Ty-5914</td>
<td>Chances et al., 1989</td>
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<td>45GA17</td>
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<td>WSC-465</td>
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<td>Gahn and Lyman, 1988</td>
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<td>UW-114</td>
<td>Musell, 1968</td>
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<td>UW-113</td>
<td>Musell, 1968</td>
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<td>Manma ash</td>
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<td>WSU-3036</td>
<td>Fryxell and Daugherty, 1982</td>
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<tr>
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</table>

\* New or different ages than those in Chatters (1986:91).
\* See also Kelley et al. (1978).
al.’s (1979) and Chatter’s (1986) procedure of plotting charcoal ages against their associated shell ages. I also followed Chatters (1986) and Sheppard et al. (1987) and included several shell ages from specimens directly beneath Mazama ash, using an age of 6730 B.P. for that ash (Chatters [1986] and Sheppard et al. [1987] use an age of 6700 B.P.). The age of the climatic eruption of Mount Mazama is today variously given by archaeologists in eastern Washington as 6845 ± 50—a weighted average of four ages reported by Bacon (1983)—or 6710 ± 40—a weighted average based on an analysis of 65 ages (they do not include Libby’s [1952] date) performed by Hallett et al. (1997). I use the latter estimate because of its consideration of more samples than the former and because it has been confirmed by new AMS ages (Zdanowicz et al., 1999).

For my analysis I assumed that the oldest shell and radiocarbon ages from Marmes Rockshelter were stratigraphically associated, at least at a coarse scale. These old ages from Marmes are, in fact, from different strata—the two shell ages stratigraphically overlay the two charcoal ages—but the strata involved are generally interpreted to have been deposited over a few decades given the radiocarbon ages (e.g., Sheppard et al., 1987). I also plotted every possible age pair; that is, if two charcoal ages were associated with one shell age, then I plotted each charcoal age against the shell age. The resulting scatterplot of age sets (Figure 3) tends to corroborate Chatters’ (1986) conclusions.

To enhance what is shown by the scatterplot in Figure 3, I have added two lines. The straight line is simply the diagonal, which is where the plotted points should fall if paired charcoal and shell ages measured time the same way. The curved line is the polynomial best-fit third-order regression line; it shows approximately where other plotted points would fall were they available and were the plotted points a representative sample of all such points possible. As shown in Figure 3, shell ages associated with charcoal ages younger than about 6500 B.P. tend to produce relatively older ages than the charcoal ages; the few charcoal ages older than that match fairly well with their associated shell ages, though the paucity of points between about 7000 and 10,000 radiocarbon years based on charcoal is troublesome. Why the plotted points fall the way they do is not at all clear; it may be the result of changes in stream erosional and depositional regimes after about 6500 B.P. That this occurred is suggested by various paleoclimatic data (e.g., Chatters, 1995); these changes may have resulted in the introduction of more old carbonates into the water in which the mussels live and those old carbonates being incorporated into the animals’ shells (Koith and Anderson, 1983) after 6500 B.P.

Again following Sheppard et al.’s (1979) procedure, I calculated the paired t statistic for the 18 age pairs in which the charcoal age was < 5000 B.P., and for the 10 age pairs in which the charcoal (or Mazama ash) age was > 5000 B.P. Pairs of ages in the former set are significantly different (t = 6.55, p < .0001); the pairs in the latter set are not (t = 3.02, p > .5). Whatever the cause, these statistics and the scatterplot in Figure 3 suggest chronological issues should not rest solely on radiocarbon ages derived from river mussel shells that produce ages less than 6500
years, but perhaps can rest on those falling between about 6500 and 10,000 B.P. Two thirds (58 of 87) of the shell ages in my data base produced ages > 5000 B.P.; the remainder produced ages < 5000 B.P. Most of the older shell ages seem to have been assayed for the simple reason that other dateable material was not available (Figure 4).

I suspect Leonhardy and Rice (1970) took the problem with shell ages into account when they built their cultural chronology. Both of them had recognized the potential problem in the materials they were studying—Leonhardy at Granite Point, Rice at Marmes Rockshelter. Those two sites, plus Windust Caves (Rice, 1965), provided the major basis upon which Leonhardy and Rice’s chronology was built; the first two sites contained virtually the entire cultural sequence for eastern Washington and provided charcoal and shell ages. No radiocarbon ages were (or are) available for Windust Caves, though the entire cultural sequence is represented in good stratigraphic context. Given the history of archaeological research as of 1970, what they termed the Lower Snake River Region had seen considerable archaeological research relative to other areas of eastern Washington. That would change some 15 years later when the archaeological record on the banks of the Columbia River between Wells Dam and Grand Coulee Dam was intensively studied. This prompts consideration of how ages are geographically distributed.

Figure 3. The relation between radiocarbon ages run on charcoal and on shell from the same provenience (see Table II for data). Points should fall near the diagonal line if ages in a pair are similar. The curved line is the best-fit polynomial regression line ($r = .90$).

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HOW ARE RADIOCARBON AGES DISTRIBUTED ACROSS GEOGRAPHIC SPACE?

Using the 20 counties as spatial units, the 756 ages are geographically distributed as shown in Table III. Clearly the ages are not randomly distributed; were they so distributed, there should be 37 or 38 (756 / 20 = 37.8) ages per county. Chi square analysis indicates a significant departure from this ($\chi^2 = 1032.305, p < .001$). The next thing to consider is whether all of the counties depart from the pattern expected given random chance, or if only some of the counties contribute to the departure. To evaluate this, I analyzed the residuals (see Everitt [1977] for a description of the statistical technique). Results are given in Table III and indicate that only three counties—Grant, Kittitas, and Whitman—do not contribute to the departure of the frequencies of ages per county from a random pattern. In other words, only those three counties have about as many ages as they might be expected to were the ages randomly distributed across all counties. Eleven counties have significantly ($p \leq .05$) fewer ages than they should given random chance; six counties—Chelan, Douglas, Franklin, Okanogan, Pend Oreille, and Stevens—have significantly more than they should.

The number of ages per county probably tells us less about what we know of
the cultural chronology of particular counties than about the intensity of effort to date sites in particular counties. For this reason, I tallied the number of sites with one or more radiocarbon ages per county and found that this value also varies across the counties (Table III). Given a total of 204 dated sites in eastern Washington and 20 counties, there should be about 10.2 dated sites per county. Again, chi square analysis indicates a significant departure from a random distribution of dated sites per county ($\chi^2 = 236.394, p < .001$). Analysis of residuals indicates nine counties do not contribute to this statistic (Table III). Eight counties have significantly fewer than the number of dated sites expected given random chance, and three counties—Douglas, Okanogan, and Stevens—have significantly more dated sites than chance suggests they should.

Counties with both more ages and more dated sites are Douglas, Okanogan, and Stevens. The first two counties contain sites that were the focus of two large cultural resource management projects in the 1980s. Those projects took place in the Wells Dam Reservoir and the Chief Joseph Dam Reservoir (Figure 1). Together, these projects produced a total of 141 radiocarbon ages; of the 195 total ages from these two counties of which I am aware, 72% were the direct result of these two projects. The high frequencies of ages and dated sites in Stevens County are the result of work in Lake Roosevelt—the reservoir behind Grand Coulee Dam. Much work took place there in the late 1970s and early 1980s and produced 26 ages.

Table III. Frequency of radiocarbon ages per county and frequency of dated sites per county.

<table>
<thead>
<tr>
<th>County</th>
<th>N of Ages</th>
<th>Conclusion</th>
<th>N of Dated Sites</th>
<th>Conclusion</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adams</td>
<td>1</td>
<td>Too few</td>
<td>1</td>
<td>Too few</td>
</tr>
<tr>
<td>Asotin</td>
<td>10</td>
<td>Too few</td>
<td>3</td>
<td>Too few</td>
</tr>
<tr>
<td>Benton</td>
<td>10</td>
<td>Too few</td>
<td>8</td>
<td>Correct*</td>
</tr>
<tr>
<td>Chelan</td>
<td>55</td>
<td>Too many</td>
<td>12</td>
<td>Correct*</td>
</tr>
<tr>
<td>Columbia</td>
<td>1</td>
<td>Too few</td>
<td>1</td>
<td>Too few</td>
</tr>
<tr>
<td>Douglas</td>
<td>87</td>
<td>Too many</td>
<td>25</td>
<td>Too many</td>
</tr>
<tr>
<td>Ferry</td>
<td>22</td>
<td>Too few</td>
<td>10</td>
<td>Correct*</td>
</tr>
<tr>
<td>Franklin</td>
<td>71</td>
<td>Too many</td>
<td>10</td>
<td>Correct*</td>
</tr>
<tr>
<td>Garfield</td>
<td>4</td>
<td>Too few</td>
<td>2</td>
<td>Too few</td>
</tr>
<tr>
<td>Grant</td>
<td>36</td>
<td>Correct*</td>
<td>12</td>
<td>Correct*</td>
</tr>
<tr>
<td>Kittitas</td>
<td>44</td>
<td>Correct*</td>
<td>14</td>
<td>Correct*</td>
</tr>
<tr>
<td>Klickitat</td>
<td>19</td>
<td>Too few</td>
<td>4</td>
<td>Too few</td>
</tr>
<tr>
<td>Lincoln</td>
<td>7</td>
<td>Too few</td>
<td>2</td>
<td>Too few</td>
</tr>
<tr>
<td>Okanogan</td>
<td>195</td>
<td>Too many</td>
<td>50</td>
<td>Too many</td>
</tr>
<tr>
<td>Pend Oreille</td>
<td>63</td>
<td>Too many</td>
<td>8</td>
<td>Correct*</td>
</tr>
<tr>
<td>Spokane</td>
<td>15</td>
<td>Too few</td>
<td>7</td>
<td>Correct*</td>
</tr>
<tr>
<td>Stevens</td>
<td>58</td>
<td>Too many</td>
<td>19</td>
<td>Too many</td>
</tr>
<tr>
<td>Walla Walla</td>
<td>6</td>
<td>Too few</td>
<td>2</td>
<td>Too few</td>
</tr>
<tr>
<td>Whitman</td>
<td>44</td>
<td>Correct*</td>
<td>9</td>
<td>Correct*</td>
</tr>
<tr>
<td>Yakima</td>
<td>3</td>
<td>Too few</td>
<td>3</td>
<td>Too few</td>
</tr>
</tbody>
</table>

"Conclusion" signifies the results of a statistical analysis of residuals; "correct" values are not significantly different from frequencies expected given random chance; for all others $p < .05$. 

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additional work in the mid 1990s produced 29 more ages. Thus, of the 58 total ages from Stevens County, 55 (95%) resulted from these projects.

A final way to examine the distribution of radiocarbon ages across space is to examine the number of sites with one age, the number with two ages, the number with three ages, and so on. These data are summarized in Figure 5. As might be expected, most sites with radiocarbon ages have only one or two. This means that when a site produced evidence of multiple components, the ages of those components often had to be assessed based on stratigraphy and/or on typological cross dating.

WHEN IN TIME DO RADIOCARBON AGES FALL?

The frequency of radiocarbon ages available for eastern Washington archaeological sites can be used as a proxy measure of how the temporal record has been sampled. Do we perhaps know more about some time periods than others? The frequency distribution of radiocarbon ages per 500-year increment suggests that indeed this is probable. As shown in Figure 4, we have many radiocarbon ages that are younger than 4500 B.P. but significantly fewer for earlier time periods. The overall frequency distribution of all radiocarbon ages suggests that even today, 50 years after Libby produced the first radiocarbon ages, we may be hard pressed to detect fine-scale temporal variation in archaeological materials of the late Pleisto-
cene and early and middle Holocene simply because there are few collections that have been dated by radiocarbon and that are older than 4500 radiocarbon years. Evidence that the paucity of radiocarbon ages exceeding 4500 B.P. is influencing our understanding of local prehistory may reside in the frequency of cultural units or phases identified. That is, greater temporal resolution provided by large numbers of radiocarbon ages may allow finer temporal distinctions among variants of artifact form. In short, the frequency of radiocarbon ages may vary directly with the frequency of cultural units identified. This in fact seems to be the case in eastern Washington. Thirty or more radiocarbon ages are available for each 500-year period younger than 4500 B.P.; fewer than 20 ages are available for each 500-year period older than 4500 B.P. (Figure 4). I examined eight cultural chronologies proposed for various portions of eastern Washington and tallied the number of cultural units postdating 4500 B.P. and the number of cultural units pre-dating 4500 B.P. (Table IV). Although not statistically significant (Student's t = 1.54, p = 0.15), there are on average more cultural units postdating 4500 B.P. (average = 3.38) than there are pre-dating 4500 B.P. (average = 2.28). This suggests that the frequency of radiocarbon ages per time period may be influencing our understanding of local prehistory.

There is a potential fallacy with the preceding suggestion, however. The diversity of artifact forms may, as Nelson (1969:300) suggests, have increased over time. If so, then the greater number of cultural units postdating 4500 B.P. could simply be a function of that factor rather than the increasing frequency of radiocarbon ages as we draw progressively nearer to the present. However, given how artifact types used as index fossils to signify cultural units have been constructed, this alternative is difficult to evaluate. This in turn begs the question of how artifact types have been constructed.

DISCUSSION

In a revised and updated version of a report first submitted in 1959, Osborne (1967:28) wrote, "we know little indeed of the story and chronology of the rise and fall of our Plateau artifact types." Given that the bottom of the cultural chronology dropped out in the mid-1950s and the age of the basal culture(s) of the eastern Washington sequence fell near the terminal Pleistocene, one might think things would have changed between 1959 and the late 1960s and that Osborne’s observation could be readily falsified. But it cannot. Virtually no archaeologist working in the Southern Plateau has attempted to monitor "the rise and fall of artifact types." Perhaps they had not done so as of the early 1960s because as Osborne et al. (1961:301) observed, "sites that can give information on cultural sequences in the most easily readable manner, by stratigraphy, are few here." I do not find this argument compelling, however, because when stratified sites were found and excavated, Plateau archaeologists typically plotted silhouettes of types in their average stratigraphic position (e.g., Nelson, 1969:300; Rice, 1972:134). A clear example of such is shown in Figure 6, which is taken from Rice’s (1965:108) Master’s Thesis; it

short standard
<table>
<thead>
<tr>
<th>Age</th>
<th>Grand Coulee Dam Reservoir</th>
<th>Chief Joseph Dam Reservoir</th>
<th>Wells Dam Reservoir</th>
<th>Middle Columbia</th>
<th>Yakima Valley</th>
<th>Lower Snake River</th>
<th>Plateau</th>
</tr>
</thead>
<tbody>
<tr>
<td>&lt;4500 B.P.</td>
<td>Shwayip</td>
<td>Coyote Creek</td>
<td>Cassimir Bar</td>
<td>Historic</td>
<td>Plateau</td>
<td>Nompi</td>
<td>VII</td>
</tr>
<tr>
<td></td>
<td>Sinaikst</td>
<td></td>
<td></td>
<td>Cayuse III</td>
<td>Late Cayuse</td>
<td>Late Harder</td>
<td>VI</td>
</tr>
<tr>
<td></td>
<td>Takumast</td>
<td></td>
<td></td>
<td>Cayuse II</td>
<td>Early Cayuse</td>
<td>(Early) Harder</td>
<td>V</td>
</tr>
<tr>
<td></td>
<td>Pre-Takumast</td>
<td></td>
<td></td>
<td>Cayuse I</td>
<td></td>
<td>Tucannon</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Kxoniks</td>
<td></td>
<td></td>
<td>Frenchman Springs</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Cold Springs</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>&gt;4500 B.P.</td>
<td>Shonitkwu</td>
<td>Kartar</td>
<td>Indian Dam</td>
<td>Vantage</td>
<td>Late Vantage</td>
<td>Late Cascade</td>
<td>IV</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Okanagan</td>
<td>Historic</td>
<td>Early Vantage</td>
<td>Early Cascade</td>
<td>III</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Cayuse II</td>
<td>Windust</td>
<td>Windust</td>
<td>I</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(unnamed)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Lenhardt and Rice</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Brownman and Munsell</td>
<td>(1969)</td>
</tr>
</tbody>
</table>
Figure 6. A typical diagram of a site's stratigraphic column, the silhouette of projectile point types associated with particular strata, and the breakdown into cultural units. Small numbers next to a silhouette represent the number of specimens in a type. Scale bar refers to projectile points (after Rice, 1965).
represents the earliest detailed illustration of artifact chronologies for eastern Washington of which I am aware. Earlier illustrations (Fryxell, 1963:16; Fryxell and Daugherty, 1962:19) are similar but are significantly less detailed and consist of an idealized model rather than a summary of a particular data set. The intellectual roots of this approach to measuring time are easily traced.

Recall that Swanson (1962b:1) indicated that in the middle 1950s Daugherty was “in the process of demonstrating [the axiom that] typology [as] an index of antiquity . . . was of little significance in the identification of Early Man in the Plateau.” Daugherty (1956a) adopted Americanist archaeology’s typological tenets as expressed by Alex Krieger (1944:272): “the purpose of a type in archaeology must be to provide an organizational tool which will enable the investigator to group specimens into bodies which have demonstrable historical meaning in terms of behavior patterns.” That is, an archaeological type must be an analytical device—a measurement unit—that allows the archaeologist to monitor time’s passage and cultural stability and change. To build such measurement units, Daugherty (1956a:233) employed “a slight refinement of [Krieger’s] typological concept.” His refinement involved the designation of two additional kinds of units. First, what he termed a “form” was a kind of artifact with “diagnostic formal characteristics” (Daugherty, 1956a:233). Second, if “a form is found to recur with some regularity and, as such, may have additional cultural significance, it is also designated a style” (Daugherty, 1956a:233).

It seems that Daugherty’s forms were recognizable (subjectively? intuitively?) and sortable because they had visible diagnostic attributes. Further, if multiple specimens could be sorted into the same pile because they shared or held in common attributes diagnostic of a form, then that pile comprised a style. Finally, if the temporal, spatial, and relational factors (i.e., historical factors) for a given style have been determined, such a style will then be given a type designation” (Daugherty, 1956a:233). In other words, if, and apparently only if, a style has “demonstrable historical meaning,” to borrow Krieger’s words, then it attains the status of a type.

At least one archaeologist working in the Pacific Northwest found Daugherty’s approach to classification variously unnecessary and inappropriately applied to the Lind Coulee specimens (Cressman, 1957). More importantly, Daugherty did not explicitly answer one critical question.

How is it determined if a style has historical meaning or significance? The programmatic and substantive literature indicate that a style attains type status if it occurs in a single stratigraphically distinct context (Lyman et al., 1997). This basically was the procedure employed by culture historians throughout North America during the 1950s (e.g., Phillips and Willey, 1953; Willey and Phillips, 1958); by that time they had largely abandoned methods such as frequency seriation and percentage stratigraphy for measuring time continuously (Lyman and O’Brien, 1999; Lyman et al., 1997, 1998; O’Brien and Lyman, 1999). Instead, they adopted a procedure founded on Krieger’s, or another similar concept, of the notion type, and it was the one used to help construct the seminal cultural chronologies for eastern Washington (e.g., Fryxell, 1963; Fryxell and Daugherty, 1962; Rice, 1965).
Types of artifacts—usually projectile points—used to mark the passage of time served as index fossils that allowed archaeologists to correlate components temporally and thereby to erect a chronology of cultures (e.g., Leonhardy and Rice, 1970; Nelson, 1969; Swanson, 1962b). Time was rendered discontinuous because cultural change and thus time were not measured continuously.

This was not perceived as a problem because of the focus on geochronological data for establishing a temporal framework. For example, Leonhardy (1970) followed his predecessors and focused on building a geochronologically founded cultural sequence when he excavated Granite Point. Excavations there in the late 1960s and at Marmes Rockshelter (Fryxell, 1963; Fryxell and Daugherty, 1962; Fryxell and Keel, 1969) and Windust Caves (Rice, 1965) in the mid and early 1960s, respectively, focused closely on the stratigraphic context of artifact forms that could serve as index fossils. As noted earlier, work at Marmes Rockshelter resulted in 21 radiocarbon ages by 1969; work at Granite Point produced six ages (Leonardy, 1970); no ages were assayed on materials from Windust Caves (Rice, 1965). The most often referenced and perhaps best known cultural chronology for eastern Washington is founded in large part on these three sites (Leonhardy and Rice, 1970).

That this chronology rests on the geochronology of the Lower Snake River valley was clearly demonstrated several years later (Hammatt, 1977); the near-perfect coincidence of distinct strata with the cultural phases comprising the chronology is remarkable (Figure 7).

Projectile-point types that corresponded to the depositional units had initially been recognized more than a decade earlier (e.g., Fryxell, 1963; Fryxell and Daugherty, 1962; Rice, 1965). These were the index fossils Leonhardy and Rice (1970) used to build their chronology. They explicitly followed the tenets of Willey and Phillips (1958) and Chang (1967), designating as their “basic analytical unit” the component, “defined as a configuration of artifacts and other archaeological phenomena distinct from all other such configurations within a site” (Leonhardy and Rice, 1970:2). The procedure for building the chronology involved “relating components one to another” to construct a cultural phase, which was “a polythetic set of similar components found within the region [that] represents a single segment of culture time and, hence, an archaeological stationary state” (Leonhardy and Rice, 1970:2). Implicit in their procedure was (1) the use of stratigraphic boundaries to aid the designation of components and (2) the use of artifact types similar to horizon styles, or index fossils, as the device for “relating components.” Interestingly, they explicitly recognized that their analytical protocol resulted in what appeared to be “stationary” cultural units—time was being measured discontinuously.

Thus, in several respects, the building of cultural chronologies in eastern Washington during the 1960s followed the basic procedures that archaeologists working in other areas were using at the same time. But in two important ways archaeologists in eastern Washington differed from many of their colleagues. First, they focused closely on geochronological data to help them not only build cultural chronologies, but to assess and model paleoenvironments as well. These practices would be seldom remarked across the continent until some time later; earlier ef-
Figure 7. Correlation of Lower Snake River Region chronostratigraphic units and chronology of cultural phases (after Hammatt, 1977).
forts such as those by Kirk Bryan had focused largely on establishing the deep antiquity of humans in the New World rather than working out the chronological details of their tenure there (Gifford and Rapp, 1985; Haynes, 1990). Statements on the value of geoarchaeology that appeared in the late 1970s (e.g., Gladfelter, 1977, 1981; Hassan, 1979) were old news in eastern Washington in the 1960s. By the time those statements appeared in the literature, no fewer than four Master’s theses (Amara, 1975; Cochran, 1978; Foley, 1976; Marshall, 1971) and three doctoral dissertations (Hammatt, 1977; Leonhardt, 1970; Moody, 1978) focused on or heavily based on geoarchaeology had been produced by Fryxell’s students and colleagues. For developing and teaching that focus to a host of archaeologists, Roald Fryxell (1934 – 1974) and Earl H. Swanson, Jr. (1927 – 1975) have been acknowledged (Wildesen, 1986, and Harten et al., 1980, respectively).

Second, archaeologists working in eastern Washington used not just index fossils, but assemblages of artifacts. Perhaps the earliest statement on the formulation of this approach was made by Leonhardt (1968:27), who noted, correctly in my view, that “interpretation of the kind of data we have to work with is a function of the kind of constructs we use to order the data.” By “constructs” Leonhardt meant analytical units such as types, components, phases, and the like. He went on to state “the major problem in interpreting archaeological data typical of the north-west [originated in] the kinds of lesser constructs we have been using as the basis for interpretation.... In the past there has been no concerted effort by people working in the Plateau to try to formulate consistent constructs on any level” (Leonhardt, 1968:28).

Leonhardt’s proposed solution to this dilemma consisted of two parts. First, avoid focusing solely on “artifact typology,” particularly projectile-point typologies. Second, construct “analytical devices [what he termed ‘structural units’] which will allow more precise delimitation of prehistoric groups of people, whether there are actual correspondences to ethnographic entities or not. These units are supposed to be units of culture content consisting of constellations of data which can be analyzed and compared as units” (Leonhardt, 1968:29). These “constellations of data” comprised “constellations of associated types” or “total assemblages” of artifacts rather than just types of projectile points (Leonhardt, 1968:30). On one hand, the advantage to what became known to Leonhardt’s students and colleagues as “assemblage-based archaeology” was that “pronounced temporal and geographic differences [would] become apparent” because of the larger number of kinds of artifacts compared. Comparing archaeological manifestations solely on the basis of only a few potentially shared projectile-point types, on the other hand, would suggest minimal geographic or temporal difference (Leonhardt, 1968:30; see also Leonhardt, 1970). As a result of Leonhardt’s advocacy of such, most of the archaeological phases comprising the cultural chronology originally proposed by Leonhardt and Rice (1970) for the Lower Snake River Region of southeastern Washington state were, in subsequent years, “validated” (to use the term of others) not just on the basis of projectile-point types, but assemblages—suites of associated kinds of artifacts (e.g., Bense, 1972; Kennedy, 1976; Rice, 1972; Yent, 1976).
That various of these cultural phases may not in fact represent evolutionary stable states has emerged from more recent analyses (e.g., Ames, 1982; Chatters, 1995).

I have found only one case in which a Plateau archaeologist graphed changes in artifact frequencies against time (Nelson, 1969:301), but this singular instance is not a true percentage-stratigraphy graph for several reasons. First, artifact types are graphed against cultural periods rather than against strata or other independent evidence of time's passage. Second, although it is stated that the "relative abundance of particular project point types" is plotted in the graph (Nelson, 1969:102), summing the width of the battleship curves at different positions in the graph produces inconsistent results; in other words, they don't consistently add up to 100 percent. Perhaps the first two points can be accounted for by the fact that although the graph was based "primarily upon data from [one site]," information "from other sites in the [general area] was taken into account in the preparation of [the graph]" (Nelson, 1969:102). In short, the graph seems to be an interpretation based on composite data rather than a graph of any empirical archaeological reality. This was not an uncommon occurrence in early efforts to graph the rise and fall of artifact type frequencies in other regions of North America (Lyman et al., 1998).

Simply put, percentage stratigraphy was not part of the analytical tool kit regularly used by Plateau archaeologists despite the fact that it was popular in other regions of North America and results of its application were widely published (e.g., Drucker, 1943; Evans, 1950; Ford, 1962; Phillips et al., 1951; Wilkinson, 1949). Instead, archaeologists working in eastern Washington measured time discontinuously because (1) they constructed artifact types as index fossils signifying particular cultures, phases, and the like and (2) they collected those artifacts from strata — visibly distinct units of deposition — thereby exacerbating the appearance of "archaeologically stationary states," to borrow Leonhardy and Rice's (1970) wording (Lyman and O'Brien, 1999; O'Brien and Lyman, 1999). Time simply was not perceived as a continuous dimension in the same way that it had been in other regions of North America. The advent of radiocarbon dating seems to have done nothing more than provide a way to assign absolute ages to depositional units, artifact types comprising index fossils that those strata contained, and the cultural units recognized.

CONCLUSION

The addition of radiocarbon dating to the chronology-building tool kit of archaeologists working in eastern Washington did not create a revolution in the commonly meant sense of the term. Prehistory as it was understood did not have to be re-written, although it did have to be extended farther into the past than previously thought after the seminal age at Lind Coulee was produced. The same kind of event — having the chronological base drop away — happened in 1927 without the benefit of radiocarbon dating for the North American continent when stone projectile points were found associated with latest Pleistocene — earliest Holocene—age bison remains near Folsom, New Mexico. And, the same result — filling in the
temporal gaps between the remote past and the near recent — was produced continent-wide and in eastern Washington. But archaeologists working in eastern Washington initially did not trust the few radiocarbon ages they produced, sometimes with good reason. Instead, they favored what the geological record and what their artifact types seemed to indicate. Confusion regarding precisely when particular chronostratigraphic horizons such as various volcanic ashes had in fact been deposited were resolved with radiocarbon dating and chemical techniques for ash identification, but these only made temporal estimates of those chronostratigraphic horizons more precise.

In my view, the full potential of radiocarbon dating has rarely been reached in the Plateau. This result is a function of how artifacts have been placed in time — stratigraphically — and classified — as index fossils allowing the temporal correlation of components. Thus we have stacks of phases and other cultural units such as those shown in Table IV, where any indication of change is invisible because it has been forced into the horizontal lines denoting the temporal boundaries of the units, a fact recognized elsewhere in North America over two decades ago (Plog, 1974). We know few details of the tempo and mode of change within cultural lineages (but see Chatters [1995] for an interesting effort in this respect). Radiocarbon ages have the potential to help us fill in those details by providing an absolute time scale against which frequency seriation or percentage stratigraphy data may be arrayed. If steps are taken to accomplish this, then the revolution will reside in how we structure and classify the archaeological record, not in the adoption of a new dating technique such as stratigraphic excavation or radiocarbon dating.

I thank Gerald Schroedl for providing a copy of his unpublished manuscript on 45GA17 some years ago, and Albert Oetting for recently providing a copy of his unpublished report on 45RL5. Ken Booren, Stan Gough, Rebecca Stevens, and especially Jerry Galm provided information on radiocarbon ages that are not yet in, or have just recently entered, the literature. Michael J. O’Brian provided helpful editorial comments and a key suggestion on how the focus of the paper might be perfected. I thank Ken Ames for encouragement, and Jerry Galm for comments on an early draft and his very sharp eyes.

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