COMMENTS

Assessing a Reassessment of Early "Pre-Littoral" Radiocarbon Dates from the Oregon Coast

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A recent discussion by Rick Minor (1995) concerned the radiocarbon and other evidence for a postulated adaptive stage in the cultural history of the Oregon coast termed "pre-littoral" by Lyman and Ross (1988) and Lyman (1991a). Although I am in general agreement with Minor's (1995:271) conclusion that "No securely dated evidence currently exists for a 'pre-littoral' cultural adaptation along the southern Northwest Coast," his discussion is inaccurate in some respects and in others misrepresents what I have written about that stage. Perhaps this inaccuracy resulted from my not being sufficiently explicit in my original papers, but regardless, I take this opportunity to explain my original thinking and to respond to some of Minor's remarks.

BACKGROUND

In 1988, Ross and I proposed a three-stage model that characterized our thoughts on the evolution of human adaptation to Oregon coastal environments (Lyman and Ross 1988). In short, the model comprised a pre-littoral stage (ca. 8,500 to 5,500 RCYBP), an early littoral stage (ca. 5,500 to 2,000 RCYBP), and a late littoral stage (beginning ca. 2,000 RCYBP), and was adapted—not adopted without change—from one originally proposed by Ross in the mid-1980s (Ross 1984, 1990) that closely adhered to a still earlier model outlined by Meighan (1965).[1] I used this model later to structure my analysis of Oregon coast archaeological remains (Lyman 1991a).

At the time, the model was the only one that sought to account for the available evidence of human occupation of the Oregon coast and also to include the entire Holocene, despite the absence of evidence of human occupation during the first half of that epoch. Most previous accounts of that time span suggested (a) Middle and Early Holocene sites were underwater or nonexistent; (b) the earliest evidence of human occupation postdated about 3,500 RCYBP; and (c) nearly all known and sampled sites reflected a "marine adaptation" (see Lyman and Ross [1988:69-71] and references therein). As Lyman and Ross (1988) discussed in some detail, these points were variously ambiguous or were debatable in the light of newly acquired evidence. Our model sought to clarify and thus remove the ambiguity and to provide a structure for future research beyond merely digging holes in sites to recover artifacts. As I noted a few years later, "the pre-littoral stage is a useful construct for modeling purposes due to the relative paucity of data concerning the actual nature of the early Holocene archaeological record on the coast" (Lyman 1991a:306).

WHAT IS A LITTORAL ADAPTATION?

There has been some confusion over what Ross and I meant in our original paper, and the confusion is not Minor's (1995) alone (e.g., Moss and Erlandson 1995a).[2] The bulk of the confusion seems to reside in what we meant when we said "pre-littoral" and "littoral" adaptations. We first distinguished a "maritime [marine] culture" as one that focused on the sea as a resource base and that had the necessary technology to regularly exploit the open sea and to "use ocean waters as a hunting and fishing area'' (Lyman and Ross 1988:96). We then indicated that a

> littoral culture [is one] which depends heavily on the sea as a source of resources, but which does not possess the sophisticated technology (seaworthy boats, for example) to use the open sea as a hunting and fishing area. . . . The people of littoral cultures inhabit a coastal environment, but do not ''go to sea'' to hunt and fish. They exploit instead the diverse coastal microenvironments: the edge of the sea, estuaries, shore rocks, offshore rocks and islands, tide pools, and surf areas where resources are plentiful form the focus of the utilized ocean environment [Lyman and Ross 1988:96].

Some researchers have suggested that the distinction between a maritime or marine culture and a littoral culture might be akin to ''splitting hairs'' (Moss 1994), but if they do, they miss the point. Those who went to sea and hunted whales, such as the Nootka, were surely marine oriented, but not everyone living on the coast hunted whales. Ross (1984, 1990) used the term ''marine,'' but when we collaborated (Lyman and Ross 1988), I wanted to use a term that distinguished those cultures found on the coast that did not go to sea from those that did.

Nowhere was it said that pre-littoral people did not exploit littoral or coastal resources. I explicitly noted, in a phrase quoted by Minor (1995:267), that people following such an adaptation exploited coastal environments and resources (Lyman 1991a:79-80). Thus, Minor (1995:267) misinterpreted me when he suggested that faunal evidence (see below) indicates ''a focus on the exploitation of marine—not 'pre-littoral'—resources.'' Pre-littoral peoples were *explicitly* conceived as having exploited littoral—Minor's ''marine''—resources but not as having focused their subsistence pursuits on such resources; they were conceived to be generalists. While not explicitly distinguished in Lyman and Ross (1988) or Lyman (1991a), the point here is that I was conceiving of ''marine'' resources as comprising those that required going to sea to procure, whereas ''littoral'' resources comprised those that could be obtained along the coast and within estuaries.

To suspect that the term ''pre-littoral,'' as used by Lyman and Ross (1988), means the same thing as Ross's (1984) earlier ''pre-marine'' term—denoting exclusive exploitation of terrestrial resources (Ross 1984:250)—would thus be a misinterpretation. Why would we change the term if the same definition applies to both? We changed the term because our knowledge and understanding had changed. First, in our collaborative view, Ross's ''pre-marine'' was an inaccurate characterization of our *tentative* conclusions regarding early coastally located adaptations. Second, his later use of that term (Ross 1990) underscored the difference in timing between when a term is coined, when it is used, and when it is published. Ross's 1990 paper was drafted (by him) in 1984–1985; Lyman and Ross (1988) was drafted (by us) in 1985–1986.

Critical terms in Minor's (1995:267) article are ''focus'' and ''intensive exploitation.'' Both modifiers are relative terms which he fails to define. Because these terms are relative, one must have a scale by which to determine the intensity or degree of focus and to distinguish a generalist adaptation or extensive exploitation strategy from a focused adaptation or intensive exploitation strategy. Such a scale was not mentioned in Lyman and Ross (1988), although I noted later (Lyman 1991a:39) the then poorly developed faunal scales of measuring subsistence intensity and degree of focus and attempted to build such scales using mammal remains as the units of the scale (Lyman 1989, 1991b). We still need such scales for other taxa. Simply listing how many specimens of several species of birds and fish were found does not demonstrate a focus on or intensive exploitation of a habitat, especially when some of those species are found to occur naturally in two or more of the distant offshore or marine, littoral, and interior (in or on rivers) contexts. Anadromous salmon and sturgeon and migratory waterfowl cited by Minor (1995:267, 271-270) as evidence of estuarine—"not 'pre-littoral' "—exploitation are prime examples of the latter problem.

STRAW MEN

Ross and I found that "Limited evidence indicates that some Oregon coast occupants were fully adapted to estuarine environments by about 5,000 [RCY]BP," although people had been on the coast 3,000 to 3,500 years prior to that time (Lyman and Ross 1988:97). We thus suggested that the then limited available evidence was "tantalizing" and "may represent the pre-littoral stage. These people probably were generalist foragers, exploiting a broad range of resources in coastal environments" (Lyman and Ross 1988: 98).

I used similar wording later (Lyman 1991a: 79-80) in a phrase cited by Minor (1995:267). But Minor overlooked the qualifying "tantalizing" and "may" in Lyman and Ross (1988), as well as the qualifier in Lyman (1991a:283) that the Neptune and Tahkenitch Landing materials "potentially" represented a pre-littoral stage. He also overlooked my more strongly worded qualification that "The archaeological record for the pre-littoral stage is clearly small. It is thus largely conjectural that the materials from 35LA3 [Neptune] and 35DO130 [Tahkenitch Landing] represent that stage" (Lyman 1991a:79). This later qualification was continued (Lyman 1991a: 80), when I noted that the available archaeological evidence "can be subsumed within this stage, [but] these materials neither help to confirm or serve to refute the stage's existence as it has been modeled" by Lyman and Ross (1988). Therefore, Minor (1995:267) constructed a straw man when he stated that I have "cited [the material from Neptune and Tahkenitch Landing] as evidence of a 'pre-littoral stage of cultural adaptation.' "

Additionally, Minor (1995:270-271) cited the fish and bird remains from Tahkenitch Landing, which dated between 8,000 and 5,200 RCYBP, as evidence for a "focus on the exploitation of resources from an estuary adjacent to the site" at this time. The critical aspect of this statement, of course, is the word "focus," because as originally modeled, a pre-littoral adaptation concerns generalists who exploited estuarine and littoral resources. Minor does not establish that exploitation at Tahkenitch Landing was "focused" on estuarine resources, yet he sees the evidence from this site as refuting the presence of a pre-littoral stage during this time, and suggests that had I "adequately considered [this fish and bird] evidence . . . it is doubtful [I would have used it] as substantiation for [my] hypothesized 'pre-littoral' stage" (Minor 1995:271). This is another straw man, because as noted in the preceding paragraph, I explicitly did not "substantiate" my hypothesis with the evidence I considered.

TERMINOLOGICAL HINDSIGHT

Certainly, in introducing the term "pre-littoral" I could have done things differently and also better. What might that have entailed? For one, I could have elaborated on why I was introducing the term. For example, it seemed to me that the pernicious use of the term "marine adaptation" to characterize human adaptation whenever a shell midden was found was getting us nowhere. What exactly comprised such an adaptation? I could have argued that human adaptations in the interior adjacent to the Oregon coast had involved the exploitation of shellfish and finny fish for virtually all of the Holocene. So what made the exploitation of such resources in littoral contexts so special? Not much that I could (or can) see. Similar technologies and human behaviors could be used effectively along riverbanks and marine coasts. Why, then, are discussions of foragers living on coasts so readily and frequently distinguished from foragers living in noncoastal settings (e.g., Perlman 1980; Yesner 1980)? What *was* different about a littoral adaptation? That it represented a broadening of the human niche seems to be the size of it. This broadening makes the pre-littoral a part of the Archaic stage of Willey and Phillips (1958). Such a continent-wide stage does not, however, capture some of the significant things that seem to have been happening in coastal contexts.

In my view, the exploitation of marine mammals, seals and sea lions in particular, was different. Pinnipeds do *not* necessarily require a procurement strategy and technology unique to the littoral (and marine) context(s) because these animals are amphibious (see Lyman 1989, 1991b, 1995 and references therein). Yet they do provide a rather different kind of resource from terrestrial mammalian species that, given their behaviors and how those behaviors influence how they can or must be exploited, could and perhaps sometimes did result in technological innovations and concomitant social change (e.g., Hildebrandt and Jones 1992; Jones and Hildebrandt 1995). This is not to say that the initiation of the exploitation of shellfish or finny fish could not produce similar results (e.g., Will 1976); rather, it is only to say that folks who had exploited either or both kinds of fish in riverine settings were, in a way, preadapted to the littoral zone in terms of these resources. They had the general knowledge and technology necessary to exploit the littoral zone, but they were not preadapted to the same degree or in the same way for exploiting pinnipeds.

There is a debate concerning Great Basin prehistory that is similar to that between Minor and myself. Grayson (1991) sought to test Bettinger and Baumhoff's (1982) model concerning the replacement of travellers by processors about 1,000 years ago. Madsen (1993) suggested there were flaws in the test given his interpretation of what the terms "travellers" and "processors" meant in the context of the model. The flaws, in Madsen's view, included the fact that Grayson (1991) used only the mammal remains in his test. Broughton and Grayson (1993) pointed out that Grayson's interpretation of the critical terms was different than Madsen's, hence his test was appropriate; therefore, given Grayson's interpretation, his test using only mammal remains was fully in line with the implications of the model.

Sound familiar? The similarity between the Grayson-Madsen debate and the Lyman-Minor debate reduces to how we might measure the "focus" and the "intensity" of resource exploitation. In the former debate, the authors called upon foraging theory, itself subject to interpretation. I interpreted the mammal remains from Oregon coast sites in light of Binford's (1980) distinction between foragers and collectors, terms readily subsumed within foraging theory and thus providing measurement scales. Minor has not told us how he measures the "focus" and "intensity" of resource exploitation other than by example, such as when he stated that an abundance of remains of animal taxa frequenting saltwater habitats denotes an adaptation "focused" on marine resources (Minor 1995:271).

With the benefit of hindsight, and the insights provided by the Grayson-Madsen debate, I could have provided a more logical argument for ignoring the fish and shellfish remains during my analysis (Lyman 1991a). Instead, what I did was to note that I had explicitly focused on the mammal remains and ignored the birds, fish, and shellfish *to retain comparability* across all assemblages I considered. And while I noted that I fully realized "the potential substantive significance of those other faunal data," at the time I only offered the weak excuse that I "lacked the time, expertise, and requisite resources for detailed study" of them (Lyman 1991a:4-5). I nonetheless am quite willing to accept Minor's conclusion that there is no evidence for a pre-littoral culture, but that is because I had concluded the same thing on the basis of the evidence I evaluated. What worried me then and still worries me today is: (a) the lack of faunal scales; (b) the semantics of "intensive" and "focus"; and (c) the occurrence of many of Minor's marker taxa in open ocean, estuarine/littoral, and riverine contexts. Because of these factors, I am not willing to agree that the 5,200 to 8,000 RCYBP faunal materials from Tahkenitch Landing represent either what Ross and I labeled an early littoral or a late littoral adaptation. There is perhaps another reason not to place strong faith in the Tahkenitch materials, as I note in the following section.

DATES AND ASSOCIATIONS

I agree with Minor (1995:271) that there presently is "No securely dated evidence . . . for a 'pre-littoral' cultural adaptation," but I add the qualification that there also is no evidence that such an adaptation *never* existed on the Oregon coast. This qualification underscores a limitation of the sample of archaeological evidence collected from the Oregon coast—a limitation that is important in other interpretive contexts as well (e.g., Lyman 1995). But it also brings up the question of the association of a radiocarbon date with an archaeological manifestation, something I explicitly considered when addressing the history of sea level fluctuation on the Oregon coast (Lyman 1991a:11). Minor made some important points in his discussion of the radiocarbon dates and their associations with early artifacts, but he makes two errors, one of lesser and one of greater significance.

The error of lesser significance involves misrepresentation of what is in the literature, and concerns the reporting of the 8,310 RCYBP date from Neptune. As near as I can determine, Minor (1995:268) is correct that this date "did not appear in the archaeological literature until listed by Lyman and Ross (1988:79)." As Minor also correctly noted, Lyman and Ross (1988) cited Barner (1982) and Zontek (1983) for the source of this date. I had, in fact, taken the date from the as-yet unpublished manuscript of Ross (1990) when it was still circulating in 1986 (cited in Lyman and Ross 1988:116). I corrected this citation later (Lyman 1991a:32)—something not indicated by Minor—when I cited "Ross, personal communication" (when Lyman [1991a] went to press, Ross's paper had not yet been published).

Minor (1995:268) noted that the early Neptune date "was apparently secured sometime in the late 1970s." This date (WSU-1644) was run in 1976, and the results were reported to Ross in August of that year (WSU Geochronology Lab, personal communication 1996). Ross (1983, 1984) and his students (e.g., Snyder 1978; Draper 1981; Barner 1982; Zontek 1983) did not mention this early date in publications appearing in 1977 and later because Ross was unsure of its significance for documenting culture history. He was unsure because it was over 16 times greater than his estimate of 500 $\pm$ 200 RCYBP (WSU Geochronology Lab, personal communication 1996), and over two and a half times greater than the oldest date with solid archaeological implications then available. This made it the oldest radiocarbon date from the Oregon coast with *possible* archaeological significance.

That brings us to Minor's error of greater significance. On the one hand, Lyman and Ross (1988:98) indicated that the 8,310 date from Neptune was "derived from organic-rich sediment beneath the shell midden deposit [and] the date was stratigraphically associated with about a dozen lithic flakes and non-diagnostic artifacts." Lyman (1991a:79) basically repeated this statement. Ross (1990:555), on the other hand, claimed that the date "came from beneath the shell midden . . . but no well established association of artifacts was noted." Minor's discussion implies that the two statements are significantly different in terms of their implications for the existence of a pre-littoral stage. He clearly favored Ross's lack of a "well established association" and thus labeled that association "suspect" (Minor 1995:269). Further, Minor (1995:269) indicated that this date "was not included in lists of radiocarbon dates from Oregon coast sites reported elsewhere by Ross (1983, 1990)." While this is technically correct—the date does not appear in Ross's (1983:214, 1990:556) "lists"—it is mentioned by him (Ross 1990:555), as Minor noted. I gain the impression from Minor's (1995) publication, then, that the history of when the Neptune date of 8,310 was derived and when it was later published, as well as how it was published, makes it irrelevant to issues of culture history. Minor's reasoning seems to be that if the date had been relevant, Ross would have published it in the 1970s, and he would have included it in his "lists." During my discussions with Ross in 1985–1986, he indicated that the reason he did not publish the Neptune date was because it was of unclear significance, not because it was irrelevant. Should future research prove that dates of similar age are in good association and are an accurate reflection of the age of the artifacts beneath the shell midden at Neptune, or should such research prove that the date is an invalid indication of the age of those artifacts, both the significance and the relevance of the date in question will be clarified.[3]

The statements of Lyman and Ross (1988), Ross (1990), and Lyman (1991a) regarding the Neptune date are different, but not in the manner Minor suggests; they differ in wording but not in meaning. Note that the Lyman and Ross (1988) and Lyman (1991a) statement indicated explicitly that the association is "stratigraphic" but Ross's (1990) statement does not. What I had in mind in the former was the fact that both the lithic tools and the dated charcoal were collected from the same stratum (a fact confirmed by Ross, personal communication 1987). Ross (1990) noted the lack of a close horizontal association between artifacts and the dated charcoal, but did not clearly imply that the two were stratigraphically associated. Although I was not explicit in either Lyman and Ross (1988) or Lyman (1991a) about this point, I clearly had in mind precisely what Minor finds fault with—the mere stratigraphic association of the date and the stone artifacts at Neptune had, in my view, unclear implications for the existence of a pre-littoral stage. This is precisely why Ross and I were cautious in 1988 in concluding that the evidence for a pre-littoral stage at this site was tantalizing but inconclusive.

Again, Minor's misinterpretation of what I said has led him astray. Sure, we wanted confirmation, but that is what prompted our re-evaluation of all available dates—previously published or not—and, for sake of completeness, we published them all, along with our evaluations. But we found no evidence to confirm the existence of a pre-littoral adaptation. If I had wanted confirmation as badly as Minor believes, I could have ignored this date and never mentioned it because it merely muddies the water.

Minor's focus on the "suspect" association of the artifacts and date at Neptune is curious because he commits precisely the error he accuses me of in his first endnote (Minor 1995:271). There, he noted that Moss and Erlandson (1994) had reported three radiocarbon dates from "a small, deflated shell scatter" known as Indian Sands (35CU67). Minor (1995:271) then stated, "This shell scatter represents the earliest evidence of molluscan resource exploitation so far identified on the Oregon coast." How, I wonder, can a site that is obviously "deflated" have produced such evidence? Might not the association of the dates with the observed lithic tools be "suspect"? The individuals who did the work that produced the dates were worried about the validity of the association (Erlandson and Moss 1996:294), but Minor did not elaborate, perhaps because to do so would reveal other weaknesses with his conclusion regarding Indian Sands.

The dated portion of the Indian Sands shell scatter measured 10 x 17 m. and had been deflating since the 1930s; the dates were derived from burned and unburned mussel shell (Moss and Erlandson 1994:58). Moss and Erlandson (1994: 103) stated that the "numerous burned mussel shells clearly indicated a cultural origin for the shell." While the taphonomic signature of burning can be so interpreted, there is no established linkage between burning and human activity—the shell may have burned by natural fires (e.g., Robbins and Stock 1990)—although the presence of archaeologically associated stone artifacts might be taken as corroborative of such a linkage. But Minor (1995:270) noted that "Fires played a prominent role in the patterning of vegetation along the southern Northwest Coast," and thus he argued that early radiocarbon dates at another coastal Oregon site seem to be from naturally burned materials. Thus, until the source of the burning of the shells at Indian Sands is clarified, this site cannot be taken as unequivocally representing "the earliest evidence of molluscan resource exploitation so far identified on the Oregon coast" (Minor 1995:271).

Even if the association of the dates and faunal remains at Indian Sands proves to be unequivocal and culturally significant, it is unclear if the exploitation of shell fish at this site represents the pre-littoral or early littoral stage. Moss and Erlandson (1995a:3) indicated, for example, that "it is not yet clear if the early Oregon coast sites at Indian Sands, Tahkenitch [Landing], and Blacklock Point [one charcoal date of 7,560 RCYBP] represent occasional or sporadic use of coastal resources by early land and river-based peoples." Assuming the associations are good and represent the age of human activities, I agree with this assessment. In this respect, it is important to also note that Erlandson and Moss (1996:291) indicated that at Tahkenitch Landing the "dates appear to be on scattered charcoal, and their precise [spatial and thus human behavioral] relationship to the recovered artifacts and faunal remains is unclear." In short, there is no consensus regarding what was going on culturally and adaptationally during the Early and Middle Holocene, nor is there a consensus on the validity of the associations between critical dates and artifacts at this site.

DISCUSSION AND CONCLUSION

Writing about and defining new terms is a tricky business. Different individuals have different dictionaries wired into their thinking and this potentially creates all sorts of problems even when an explicit definition is provided for a new term. Thus, it is not too surprising that there is a parallel between the misunderstanding of the term "pre-littoral" and the misunderstanding of the term "natural selection." In part, the two terms are readily interpreted to mean something not intended by their originators. After more than a century of argument and discussion and the emergence of a general if not universal consensus, the easily inferred meaning of the second word in "natural selection" still occasionally results in the misconception that over the long term, biological evolution is goal oriented rather than, as Richard Dawkins (1986) put it, a blind watchmaker. As might be predicted, then, after a mere decade the term "pre-littoral" is still misunderstood.

When Ross and I first proposed the three-stage model of adaptations, we were equally interested in accounting for the available evidence as we understood it and in providing a testable framework for future archaeological research. The original dates for the stages were, for example, changed in light of new evidence (Lyman 1991a). The aspect of the model that seems to have received the most criticism is the postulated pre-littoral stage. On the one hand, Minor's (1995) question concerns whether it actually existed. Other researchers, on the other hand, think Early Holocene human occupants of the Oregon coast were more tightly adapted to the littoral zone than we originally envisioned (e.g., Lightfoot 1993; Moss 1994; Moss and Erlandson 1995b). Perhaps they are correct. However, the available evidence is, in my view, still rather sparse and/or equivocal. In my view, the problem reduces to the fact that we lack agreed-upon faunal scales for measuring the difference between coastally located generalists—what we termed pre-littoralists—and coastally located specialists—what we termed early and late littoralists. Thus, most importantly, we must decide on the significance for such scales of faunal taxa that are naturally found in the open sea, along the coast and in estuaries, and in interior riverine contexts.

Minor's (1995) efforts to invalidate the conclusions he ascribed to me regarding the pre-littoral stage are unnecessary and misdirected. In misunderstanding and thus misrepresenting what I have said about that stage, he constructed straw men. In referencing faunal evidence from Tahkenitch Landing in an attempt to refute the existence of the pre-littoral stage, he failed to acknowledge the fact that various resources found in the littoral zone are also found in the open ocean and in the upstream reaches of rivers. In citing the evidence from Indian Sands in an attempt to corroborate his beliefs, he failed to consider relevant data and lost sight of the fact that radiocarbon dating is an indirect dating method that hinges on associations.

Was there a pre-littoral adaptive stage on the Oregon coast? Maybe, but I do not know and I really do not worry about it too much. To paraphrase Jesse Jennings's (1973) remarks regarding the apparent demise of his postulated Desert Culture, if the three-stage model of Oregon coast adaptations serves to stimulate research that produces more complete understanding, its passing will be accompanied by a wake rather than a dirge. Clearly, we need more data, we need more thinking and discussion, and we all need to read the literature a bit more closely and carefully.

NOTES

1. Lest I be misunderstood, I note that Ross (1975, 1983, 1984, 1990; Ross and Snyder 1986) did not reference Meighan's (1965) paper, nor did Ross's students (Snyder 1978; Draper 1981; Barner 1982; Pullen 1982; Zontek 1983). There is no evidence in the published record indicating that Ross's (1984, 1990) model was based on or somehow derived from Meighan's model. I thank Madonna Moss and Jon Erlandson for noting this possibility.

2. Lightfoot (1993) apparently was not confused by this discussion. Moss and Erlandson (1995a:3) suggested that Ross and I were proposing that pre-littoral peoples "focused primarily on the hunting of land animals, riverine fishing, or both." Close reading of what we said (Lyman and Ross 1988; Lyman 1991a) shows that this is incorrect. Moss and Erlandson (1995b:14) got it right in a more widely circulated paper.

3. Recall that Ross (1990) was written before Lyman and Ross (1988), but their order of publication was reversed. Would there be such a fuss if the two papers had been published in the order in which they were written, or if Ross had published his thoughts on the Neptune date in the late 1970s? Given Minor's interpretations of what I said about this date (Lyman 1991a), I suspect he would still have had a reason to publish his 1995 paper.

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Pre-Littoral or Early Archaic? Conceptualizing Early Adaptations on the Southern Northwest Coast

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It is gratifying to see that R. Lee Lyman (1997: 260) is in "general agreement" with the conclusion that "no securely dated evidence currently exists for a 'pre-littoral' cultural adaptation along the southern Northwest Coast" (Minor 1995: 271). That was the point of my article (Minor 1995). The reassessment of early radiocarbon dates from the southern Northwest Coast was prompted by the fact that the pre-littoral stage as defined by Lyman and Ross (1988) and Lyman (1991a) is not consistent with the archaeological evidence.

Marine resources were a focus of prehistoric subsistence activities along the southern Northwest Coast much earlier than acknowledged by Lyman and Ross.[1] The pre-littoral stage, even as a hypothetical construct, is not a useful way of conceptualizing occupation in this region between ca. 8,500 and 5,500 B.P. Available information about cultural adaptations on the southern Northwest Coast during this time span is more appropriately considered in terms of the Archaic stage concept (Willey and Phillips 1958).