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THE EMERGENCE OF COMPLEX HUNTER-GATHERERS ON THE CANADIAN PLATEAU: A RESPONSE TO HAYDEN

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Brian Hayden argues that our analysis of Keatley Creek stratigraphy and dates offers inaccurate conclusions. Although our data demonstrate that the village appeared late and was somewhat unstable, Hayden continues to support a model of early emergence and high stability. Hayden offers no new data to support his position. Consequently, we argue that there is no reason to reject our revised view.

Brian Hayden arguye que las conclusiones de nuestro análisis de la estratigrafía y fechamiento del sitio Keatley Creek, están equivocadas. Aunque nuestros datos demuestran que la aldea apareció tarde y fue algo inestable, Hayden continúa apoyando un modelo de surgimiento temprano y estabilidad plena. Hayden no ofrece nuevos datos para apoyar a su posición. Por lo tanto, mantenemos que no se debe rechazar a nuestra explicación revisada.

In 1986, Brian Hayden embarked upon what is probably the most ambitious and longest-lasting investigation of a housepit village in the Pacific Northwest region. Hayden's research at Keatley Creek has subsequently offered a number of critical contributions to the region's prehistory and to the general archaeological knowledge of complex hunter-gatherers. Most fundamentally, Hayden and colleagues (Hayden 1997a, 1997b, 2000; Hayden et al. 1996; Hayden and Spafford 1993) demonstrated the ability of household-level analysis to provide information on variability in socioeconomic organization. Hayden describes the Keatley Creek village as heterarchically organized around a series of corporate groups occupying unusually large housepits. To Hayden (1994), this form of organization was adaptive in the context of highly abundant resources, predicated only on the technological capacity of local populations to harvest and store surpluses and the presence of individual aggrandizers who could use that surplus to better their own standing by aggregating kin and clients in corporate group households within the emergent communities. Hayden asserts that since the technology for mass harvest and storage of salmon moved into the region after 3600 cal B.P. (Prentiss and Chatters 2003), it is logical to expect large villages such as Keatley Creek to have developed shortly thereafter, certainly no later than 2600 cal B.P.

It is no surprise that Hayden finds much to disagree with in our assessment of the evolution of the Keatley Creek village (Prentiss et al. 2003). Our analysis not only indicated that the village emerged at a relatively late date (about 1700 cal B.P.), but that the pattern of occupation itself was not entirely stable. To make matters worse, we suggested that it came at a time of potential shortage in access to salmon on the interior Plateau. In order to address these problems, Hayden offers three critiques of our work focusing on stratigraphy and dating, housepit continuity, and explanations for village formation.

Hayden argues that we misread the stratigraphic record at Housepit 7, and consequently formed an incorrect impression of the history of the Keatley Creek village. We argued that Housepit 7 was completed and occupied immediately following the

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occupation of a very small housepit (Subhousepit 3) around 1700 cal B.P. Once present, inhabitants excavated various deep cache pits on the west side of the house, eventually filling these with refuse derived from unknown sources. One of these pits (88-P31) bisected Subhousepit 3 and received a large number of dog bones in its fill, one of which clearly predated the Housepit 7 occupation (2326–2001 cal B.P.). In contrast to our interpretation, Hayden contends that Housepit 7 was established by ca. 2600 cal B.P., after which inhabitants periodically excavated cache pits (Pits 88-P31 and 89-P5) and small partitioned rooms (Subhousepit 3). He argues that periodic re-excavation and expansion of cache pits in the northwestern sector of Housepit 7 by early occupants makes it impossible to reconstruct stratigraphic relationships between cache pit and room features in this area.

Hayden's arguments cannot be sustained under the current evidence. The stratigraphic position of Subhousepit 3 is indisputable. Hayden's Figure 1 illustrates a profile of Pit 89-P5 showing the pit cutting through "dump" materials on its north side. This dump is actually the stratified fill of Subhousepit 3 unrecognized by the 1989 excavators. As shown in Figure 1 (this paper), both Pit 89-P5 and 88-P31 neatly bisect all dated strata from Subhousepit 3. As we pointed out in our original paper (Prentiss et al. 2003; see also Prentiss et al. 2002), Subhousepit 3 was created and occupied for long enough to form multiple floors prior to the infilling of its depression by later Housepit 7 occupants (Figure 1). A careful examination of our (2003) Figures 5 and 6 reveals that Subhousepit 3 is buried by the complete Housepit 7 stratigraphic sequence. Radiocarbon dates also confirm this chronology. We derived seven dates on features (five hearths, one large charcoal fragment, and one piece of charcoal from a post-hole) from the Subhousepit 3 strata spanning 1353 to 1815 cal B.P. The Subhousepit 3 early floor dates (1815 [1677, 1628, 1528] 1353 cal B.P.) slightly predate the earliest date on an *in situ* hearth below the outer north rim of Housepit 7 (1710 [1614] 1518 cal B.P.), though they fall substantially within the same 2 sigma range. They substantially predate the oldest *in situ* feature date from definitive early Housepit 7 strata (1511 [1405] 1299 cal B.P.) located in levels above Subhousepit 3.

Hayden asserts that Subhousepit 3 is too small to be any form of living structure, suggesting that it served instead as a room. As demonstrated by our stratigraphic analysis, the early floors in Subhousepit 3 could not have been a room. Unfortunately, the full diameter could not be accurately measured due to truncation on its south side by cache pits and east side by Hayden's 1987 test trench. We estimate that the original structure may have been as much as 2.5–3 m in maximum diameter. This would not be an unreasonable size for a typical small hunter-gatherer lodge, tent, or hut in various contexts around the world (e.g., Bartram et al. 1991; Binford 1991; Yellen 1977). Indeed, Alexander (1992) documents similar sized mat lodges and menstrual huts used in residential contexts from the ethnographic record of the Canadian Plateau. Harris (2004) recently argued that on the basis of the layout of features (hearths, postholes, and a cache pit), faunal and floral remains, and lithic artifacts, Subhousepit 3 was most likely a small semi-subterranean mat lodge structure used in a cold-season residential context.

Hayden views projectile points as indicators of a Shuswap Horizon age (ca. 2400–3500 B.P.) for the first occupations of Housepit 7, asserting that they nonrandomly occur in early housepit strata. Shuswap points are present in layers associated with Subhousepit 3 and early Housepit 7. One large specimen was found *in situ* on the ca. 1700 cal B.P. floor of Subhousepit 3. We consider early projectile points to be a notoriously unreliable source of dating. Our radiocarbon dating on *in situ* features clearly demonstrates that projectile points typical of various earlier ages frequently appear in later contexts. It is not surprising that they would nonrandomly occur in housepits as people who were interested in quality stone tools lived in these places and may well have collected old tools and potentially even imitated older styles. Excavation of the Housepit 7 crater also could have disturbed a scatter of Shuswap horizon artifacts from an earlier camp. We have documented a disturbed Lochnore phase (ca. 4000–6000 B.P.) camp on the west side of Housepit 7 and its artifacts are also embedded within early Housepit 7 sediments.

Moving on from the advent of the aggregated village, Hayden is concerned that we are insufficiently cautious in our conclusions regarding breaks in the record of occupation at Housepit 7. The issue of unbroken occupational continuity at Housepit 7 is critical to Hayden, who uses this as the corner-

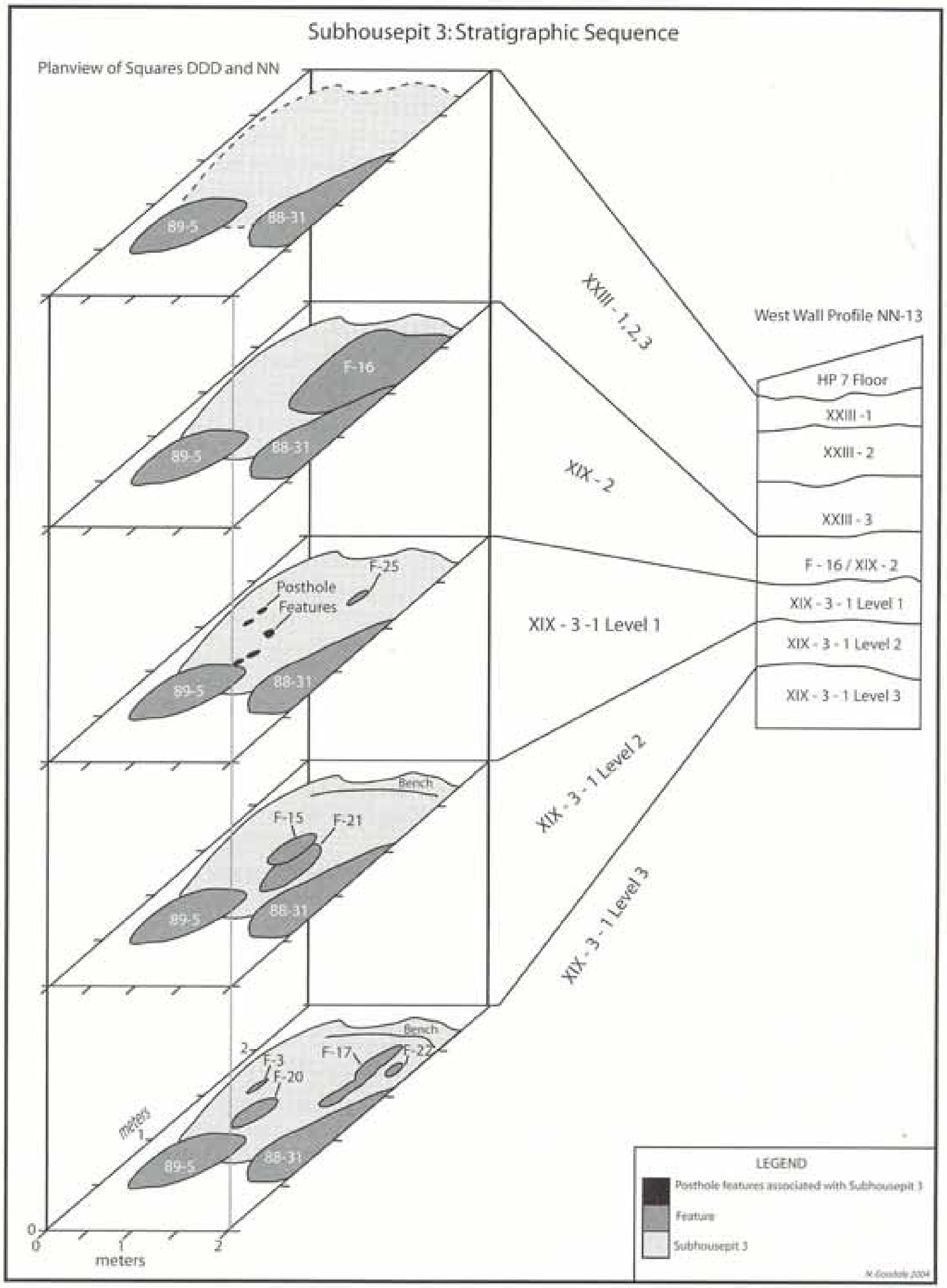

Figure 1. Plan views and basic profile of major Subhousepit 3 strata illustrating cutting and filling on south side by pits 89-P5 and 88-P31 that extend downward from the Housepit 7 final floor (XXIII=Upper Subhousepit 3 fill, XIX-2=midden material plus F-16 hearth; XIX-3-1=early Subhousepit 3 floors).

stone of his argument for corporate group households as the peak adaptive pattern in the Mid-Fraser between ca. 2600 and 800 cal B.P. Unfortunately, the data do not entirely support his view. So, Hayden points out that while we can recognize breaks in the record from our excavations, other unexcavated parts of the rim could hypothetically fill in those gaps. This seems unlikely, given the current evidence, which suggests that not only did short breaks occur in the radiocarbon record, but that they are also associated with major shifts in occupational activities. Following the initial construction of Housepit 7 after 1700 cal B.P., the first break occurs (at ca. 1350–1450 cal B.P.) prior to the establishment of Subhousepit 1, a likely room attached to the larger house. The second break occurs between the formation of Rims 3 and 4 beginning at ca. 1250–1350 cal B.P. The next most recent group of dates is associated with the final Housepit 7 floor and falls in the range of 800–900 cal B.P. This is significant because abandonments of other villages in the Mid-Fraser (e.g., Bridge River) occurred at similar dates (ca. 1100–1200 cal B.P.). Further, rim 4 strata in Housepit 7 show significantly expanded quantities of woody roof debris, other botanicals, mammal bones, and prestige items (Burns 2003; Godin 2004; Lyons 2003). Clearly, something of demographic and cultural significance occurred at this time. It is still possible that despite possible short breaks in the record of occupation, the same corporate group returned to live at Housepit 7. We have found no evidence of change in frequencies of lithic raw material types or other data that could support an alternative interpretation to the Hayden et al. (1996) conception of a long-lived corporate group.

Hayden disputes our comments on relationships between the dates of early housepit villages and climatic changes. We suggested that the Mid-Fraser villages may have emerged at a time of shortage in access to salmon associated with warm and dry climatic conditions. He offers in replacement the familiar scenario that large villages with social inequality could only arise and exist under conditions of plenty. Hayden suggests that this conclusion is supported by pollen evidence for little change since 3,200 years ago and new paleoclimatic data from glacial moraines in the British Columbia Coast Range indicating wetter conditions developing after 1700 cal B.P.

Hayden relies on a single pollen study (Mathewes and Pellatt 2000) to argue that no significant climatic change has occurred over the past 3,200 years on the Canadian Plateau. This is highly problematic, given the fact that pollen alone is a poor indicator of short-term fluctuations in climate and it is even worse when based on one data set. In contrast, we cited studies of pollen, fire history, and fisheries (e.g., Chatters et al. 1995; Chavez et al. 2003; Hallett et al. 2003; Mann et al. 1998) from throughout the greater Pacific Northwest region indicating dry conditions at the advent of the Mid-Fraser villages. Nowhere in our paper did we say that we favored only fire histories for paleoenvironmental reconstruction.

Hayden introduces exciting new research in Coast Range glacial chronology (Reyes 2003; Reyes and Clague 2004) suggesting a wetter period between 1700 and 1400 cal B.P. Here, he seems to imply that even if our date for initial village emergence is correct, then at least it did not occur for the reasons we suggested. Clearly there is much to learn about microclimatic variability within the region. However, it is well known that regional warm and dry conditions probably peaked between 2400 and 1600 cal B.P. (Chatters 1998). Warm and dry conditions during this time would have increased erosion and associated sedimentary bedloads in river systems, thereby decreasing salmon productivity well past the period of peak drought (Chatters et al. 1995). We would expect winter moisture to increase in the Coast Range following the latter period (known elsewhere as the "Roman Drought") before reversing during the Little Climatic Optimum of ca. 1300–700 cal B.P. Thus, the new glacial chronology actually lends support to our arguments.

Finally, Hayden argues that reduced salmon populations would not have been conducive to support of large villages in the Mid-Fraser canyon. As we pointed out (Prentiss et al. 2003:731), regional reduction in salmon productivity does not necessarily mean that some patches might not have been comparatively productive. Kew (1992) demonstrates that in the poorest years, the Mid-Fraser can be 400 percent more productive than the next best drainage on the Canadian Plateau. Under sustained low productivity conditions, this would place an extraordinary value on the fishing sites of the Mid-Fraser rapids, and, consequently, it would offer enormous payoffs for investments in packed communities and ultimately socioeconomic complexity. We thank Hayden for his important research in Mid-Fraser archaeology and the opportunity to debate these issues.

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