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Chapter 8 - Echoes from the ice patch: Jim Benedict's legacy and twenty-first century Rocky Mountain archaeology

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One would think the Rocky Mountains would be hard to miss: a chain of high ranges stretching from New Mexico to British Columbia that together comprise a large portion of the North American Cordillera, which in turn splits the continent from Alaska to Mexico. To a large extent, however, archaeologists through a good portion of the twentieth century did miss the Rockies, at least in terms of thinking of them as a distinct culture area with its own unique histories and adaptive strategies (e.g., Wissler 1923). But Benedict did not, and this is a large part of his legacy. He helped put Rocky Mountain archaeology on the map as its own research domain, separate from but also drawing on the archaeologies of the Great Plains and Great Basin. Without this essential contribution, most of the research in this collection of papers would at best lack empirical cohesion and at worst may not have ever come together in the first place.

Benedict's methodological and intellectual legacy, however, also extends well beyond the Rockies. By emphasizing the role mountains play in the development of regional settlement and subsistence systems, he not only set Rocky Mountain research apart as its own endeavor, he also helped develop a robust body of thinking about the ecology of worldwide high-altitude adaptations. He was also one of the pioneers in using cultural ecology to explain the development of mountain-adapted behaviors, especially in light of climate change. Lastly, he helped develop archaeology as a science concerned with processual questions through a period of time marked at either end of the political and theoretical spectrum by repeated attacks on science and the scientific method. Benedict's steadfast (but also quietly non-polemical) insistence on positivism, objectivism, interdisciplinary work, and the development of new scientific methods outshines even his regional import, as it was his steadfast scientific approach that helped lead to some of the more interesting mountain research in his time and, as seen in the variety of papers in this issue, beyond. The review that follows briefly explores this legacy as expressed in the papers comprising this special issue of *Plains Anthropologist*.

Putting rocky mountain archaeology on the map

At the risk of repeating Bender (this volume) and LaBelle and Cassells (this volume), Benedict's work in the late 1960s and 1970s, along with research by people like Frison (1965, 1967, 1971), Husted (1974), and Loendorf (1973),[1] helped develop

Rocky Mountain archaeology as its own research domain, distinct from Great Basin and Great Plains archaeology. Prior to this time, Rocky Mountain archaeology tended to be seen as peripheral to research in adjoining regions (Kornfeld et al. 2010; Larson and Kornfeld 1994; Mulloy 1958; Sharrock 1966). The impetus behind Benedict's contributions along these lines hinged on his work at a series of high-altitude sites in Colorado (e.g., Benedict 1974, 1975a, 1975b) which in turn conditioned his thinking about the Albion Boardinghouse and McKean complexes, among many other things.

Benedict attributed the aspect of the Albion Boardinghouse complex that entailed more intensive use of alpine habitats to migrants from the Plains escaping Antevs's (1948, 1955) mid-Holocene Altithermal drought or droughts (Benedict 1975c, 1979). This Rocky Mountain refugium model inextricably linked the Rockies to the Plains and at first glance might seem to support the idea that what happened in the Rockies was indeed peripheral to what happened further east. But critical to this hypothesis was the idea that the Rockies held the "missing chapters" (Benedict 1979:10) to Plains culture history and were thus essential to understanding macro-regional trends. In short, the Rockies were no longer peripheral; they were part of a larger culture-historical explanatory package.

Benedict took this type of thinking a step farther in his assertion that the widely distributed McKean complex had its origin not in the Desert West, with its broad-based, Archaic subsistence focus on diverse animals and plants (Frison 1991:21; Jennings 1957), nor in the Great Plains. Rather, Benedict (1973a) posited that this broader-spectrum (in comparison to Paleoindian) adaptation had its origin in the Rockies and then spread northeastward across the Plains and on into Canada. This inverted the prevailing view of which area, the Great Plains or the Rocky Mountains, drove the changes seen across the region during the Middle Archaic (Rennie 1994; Syms 1969; Wheeler 1952). Though he later revised his thinking on the McKean complex (Benedict 2005), his focus on how foragers exploited diverse plant and animal resources at altitude was seminal. This work set the stage for critical research by Bender and Wright (1988), Black (1991), Cannon (1996), Frison (1991), Larson (1990), Metcalf and Black (1997), and Wright et al. (1980), among many others, who saw Rocky Mountain ecosystems engendering their own unique types of intensive, oftentimes logistically organized hunting and gathering adaptations, distinct from those on the Plains or in the Great Basin.

It is this perspective that guides a substantial portion of the research in the Rockies today (e.g., Adams 2010; LaBelle and Pelton 2013; Metcalf and MacDonald 2012; Morgan et al. 2012; Scheiber and Finley 2010; Stirn 2014) and is expressed in some way in all of the articles comprising this issue. Kornfeld (this volume), for instance, argues that well-developed and place-specific high-altitude behavioral patterns were in place in Middle Park, Colorado by Paleoindian times. If true, this pushes the development of Rocky Mountain-specific adaptive patterns back in time to the terminal Pleistocene or early Holocene. Bender (this volume) takes a similar approach, arguing that land use in South Park, Colorado, centered on persistent places associated with lithic resource procurement, thus showing that another place-specific pattern developed and was maintained for many millennia in the central Rockies.

Finley et al. (this volume) make similar assertions regarding long-term continuity in mobility patterns and technological organization in western Wyoming.

Though Finley et al. (this volume) link these patterns to those of the Numa (and thus show some indication of a pan-regional approach), I have to wonder if the particularistic focus on sub-regional behavioral variability (e.g., the Rocky Mountains or specific ranges within) of the type espoused by Zeanah (2000:14) and reiterated by Bender (this volume) has gone too far. Focusing on only the specific, relying on just the literature and data from any given region, and not making comparisons to other areas might be a good way of reconstructing increasingly nuanced pictures of local culture histories, but it is not a good way to develop general theory about mountain adaptations (e.g., Aldenderfer 2006; Gremillion et al. 2014; Guillet et al. 1983; Rhoades and Thompson 1975). Strange as it may sound after extolling Benedict's considerable contributions to developing a robust Rocky Mountain-oriented archaeology, this is where I think some of Benedict's more important contributions lie: in modeling and ecological theory.

Modeling mountains, mobility, and settlement

When it comes to Benedict, mountains, mobility, and modeling, the most obvious example is found in Benedict (1992) where he develops an ecologically informed, bipartite model of seasonal transhumance where, as Bender (this volume) indicates: (1) Early Archaic people employed an "up-down," residentially mobile seasonal round extending from the high Plains to Colorado's high country; and (2) Late Prehistoric people engaged in a "rotary," mountain-oriented system where populations mapped on to diverse mountain resources, moving from pass to park to high country as different sets of resources became available. Per these models, in the Early Archaic, people were tethered to the lowlands and exploited higher altitudes on a seasonal basis. In the Late Prehistoric, people were full-time mountain residents with adaptive systems fine-tuned to the ecological constraints found in these settings. The question, then, is why switch to a solely mountain-oriented system?

Todd (this volume) presents some very interesting data that speak to what appear to be similar diachronic changes in mountain settlement systems in the Greybull River area of northwestern Wyoming. These data show far more Late Prehistoric than Paleoindian projectile points, perhaps showing intensification of mountain use over time (a pattern that is similar to how Benedict perceived Colorado's Rocky Mountain culture history). Yet the data also show a decrease in the mean elevation at which diagnostic points are found from the Early Archaic to Late Prehistoric periods, at first glance arguably implying more intensive use of high altitudes earlier in the Holocene. While I think the standard deviation of these data may be more telling than the mean, in that low standard deviations (perhaps during the early Holocene?) might imply targeting specific ecozones and high standard deviations (perhaps in the Late Holocene?) more diverse (and therefore more broad-spectrum or intensive) land use, this type of easy-to-implement but potentially very interesting analysis builds upon and stands to revise our understanding of

mountain mobility in much the same manner as Benedict's (1992) work, especially if implemented beyond the Greybull area.

The clearest example of modeling in this issue, however, is of course Bender's (this volume) piece on explaining persistent places in South Park. Another good example is from Finley et al. (this volume), who reconstruct obsidian conveyance zones in northwestern Wyoming. This research builds on a considerable body of work that links obsidian provenance data to mobility patterns (e.g., Eerkens et al. 2008; Harvey 2012; Jones et al. 2012; Smith 2010). Rather than simply reconstruct mobility, however, they attempt to explain obsidian conveyance at the macro-scale by arguing that changes in obsidian source diversity and artifact curation reflect distinct sub-regional territories that were conditioned more by ethnolinguistic boundaries than ecological constraints. Though I think more work needs to be done to develop and test alternative hypotheses, this research shows how Benedict's initial attempts at modeling transhumance in terms of ecology have evolved, and will hopefully continue to evolve, by considering how ethnicity, politics and social structure help condition mobility and settlement in greater ecological context.

Climate change and ecology matter

Benedict's ecological focus was at its core concerned with climate change, now more relevant than ever. This is expressed in his long-running concern with the Altithermal (Benedict 1970a, 1979), his employment of techniques associated with environmental archaeology and geomorphology to reconstruct past climates and environments (Benedict 1970b, 1973b, 2011; Benedict et al. 2008), and with an overarching interest in how the environment and environmental change conditioned human adaptive strategies, especially settlement and subsistence behaviors (Benedict 1975c, 1992, 1999, 2007). To my thinking, Todd's (this volume) data on projectile points and elevation might be interesting to explore in paleoclimatic context and Bender's (the volume) argument for long-term settlement pattern stability in the Colorado Rockies needs further investigation given the variability in climate in the Rocky Mountain region over the course of the Holocene (Mensing et al. 2012). In this regard, Stirn's (2014) models are also interesting from a climate change perspective, given the growing body of evidence for expansion of whitebark pine forest, the target ecozone for rock ring sites in the Wind River range during the main period of occupation for these types of sites (Losey 2013; Morgan, Losey, and Trout 2014; Morgan et al. 2012; Trout 2015).

The article by Kornfeld (this volume), however, is the one that most clearly draws on Benedict's focus on climate change and ecology on both empirical and theoretical levels. On the empirical side, this article specifically focuses on using paleoenvironmental proxy data to inform our understanding of past settlement systems. In Middle Park, CO, these data appear to show an expansion of coniferous forest at the expense of open parkland in Paleoindian times. The fact that Kornfeld also identifies intensification of use of within bone nutrients (marrow) is very interesting in this context, given the fact that closed, high-elevation coniferous forests are far less biotically productive (at least in terms of resources targeted by humans) than

open areas that provide browse and graze for ungulates. If there is indeed a causal connection here, then Kornfeld may have identified some of the earliest, environmentally meditated high-altitude resource intensification on the continent, not on plant foods but rather on large mammals, thus indicating a type of faunal intensification seen more often in Pleistocene Eurasia (e.g., Yi et al. 2013).

But it is Kornfeld's (this volume) approach to theory and modeling that I think is most important to understanding Benedict's ecological legacy. Drawing on the work of Aldenderfer (1998, 2003, 2006), which is essential to understanding worldwide high-altitude human ecology, Kornfeld clearly elucidates the increased caloric requirements of living at altitude and the limiting effects of cold, hypoxia, and pronounced seasonality on humans choosing to live in such settings (e.g., Beall 2001; Hock 1970). While noting that high altitudes can indeed be remarkably productive on a seasonal basis, Kornfeld also makes it clear that archaeological research that does not seriously consider the facts of human physiology in light of high-elevation environmental characteristics is sure to neglect a critical aspect of what actually conditioned human behavior in such settings. In short, unless the unique limitations of high-altitude environments are taken into account, we will never fully understand the technologies, work strategies, social structures and the like that developed in these settings. We will also struggle in making comparisons to other high-altitude adaptations, which is essential to developing general theory.

This leads to something I think is missing in this collection of papers: a comparative consideration of Benedict's impact beyond the Rockies. Because Benedict was chiefly concerned with ecology and climate change, his research has been cited and built upon in regions ranging from the Great Basin, to California's Sierra Nevada, to the Argentine Andes, and even to the Tibetan Plateau. Comparisons are invariably made between Benedict's work in the Rockies and Bettinger's (1991) and Thomas's work (1982) on the high-altitude villages of the western and central Great Basin. It is clear in reading the Great Basin literature that both Bettinger and Thomas were aware of and were influenced by Benedict, particularly in his consideration of climatic variability and its relationship to the intensity of high-altitude settlement. Less well known is the extent to which Benedict's models, and Rocky Mountain archaeology in general, inform reconstructions of settlement patterns and trajectories of high-altitude intensification, often in the context of climate change, in the Sierra Nevada (Leftwich 2010; Morgan 2006, 2009; Stevens 2003, 2005). The gist of a lot of this work is that prehistoric Sierra Nevada groups employed various attributes of up-down versus more logistically oriented, mountain-specific systems and that the development of more intensive, logistical systems tended to develop later in prehistory, in much the same way as is often seen in the Rockies.

Globally, similar concerns about how ecological relationships conditioned mobility and settlement are evinced in Aldenderfer's (1998) work on Andean Archaic lifeways. In this work, he draws on Benedict's (1992) model for the Colorado Rockies by noting how changes in up-down versus more permanent, intensive mountain lifeways were affiliated with changes in lower-elevation population-resource dynamics. This work, in turn, informs Aldenderfer's (2006) seminal comparative analysis and modeling of high-altitude lifeways worldwide, where he argues that economic intensification in lowland settings tended to drive more intensive and regular use

of high-altitude habitats, whether it be on the Ethiopian highlands, the Tibetan Plateau or in the Andes (see also Lozny 2013; Orlove and Guillet 1985). Questions concerning the relationship between lowland populations, their economic orientation, and variations in mountain biotic productivity drive much of the research in the Andes (Neme and Gil 2009; Neme et al. 2013; Otaola et al. 2015) and the Tibetan Plateau today (Aldenderfer and Zhang 2004; Brantingham et al. 2003, 2007; Chen et al. 2015; d'Alpoim Guedes et al. 2014).

Putting positivism into mountain archaeology and beyond

Though Benedict's contributions to developing Rocky Mountain archaeology and a robust body of thinking about the ecology of mountain settlement and mobility are clear, I think his most substantial contribution is perhaps less obvious: helping develop a dynamic, environmentally oriented, puzzle-solving normal science (see Kuhn 1996) centered on understanding human behavior in the mountains. This is what I truly like about Benedict's work: he used hard science and bravely adopted new methods to answer difficult archaeological questions. Want to know the effects of climate change at the landscape scales in which people actually operated? Assess rates of downslope soil movement in the high country (Benedict 1970b). Want to understand site chronology relative to landscape evolution? Reconstruct the geomorphic history of different site settings (Benedict 1973b, 1985, 2005). Want to zero in on site chronology with potsherds? Turn to thermoluminescence dating (Benedict 1989). Want to provide temporal context for surface assemblages and features in the absence of radiocarbon dates or diagnostic artifacts? Use lichenometry (Benedict 2009). Want to understand climate's effect on tree line absent direct measures for relict tree lines? Employ fungus (*Schlerotia* sp.) as a proxy (Benedict 2011).

There is a lot of good science in this collection of papers, no doubt a reflection of Benedict's legacy. Finley et al. (this volume), for instance, employ a robust sampling methodology and statistical tests to back up their claim for multiple, distinct obsidian conveyance zones in western Wyoming. Kornfeld (this volume) focuses on the hard data on human physiology to develop testable, comparable models of human adaptation to altitude. Black and Theis (this volume) use a comprehensive set of geologic data and a sophisticated GIS analysis of these data to make the very important assertion that we really do not know the extent of chert sources in the Rockies and therefore need to be much more careful when identifying toolstone provenance. The concluding article by Cannon et al. (this volume), however, most clearly shows a scientific approach akin to Benedict's. Relying on the robust quantitative measures developed by zooarchaeologists over the last several decades (Reitz and Wing 2008) and appropriate use of statistical significance tests (Baxter 2008), Cannon et al. convincingly assert that our understanding of prehistoric bison biogeography in the Greater Yellowstone Ecosystem (GYE) is poorly developed and that bison hunting, previously thought to be of little consequence there, was likely more intensive than we thought. Interestingly, this brings us full circle: if Cannon et al. are correct, at least this portion of the Rockies becomes more strongly linked to the Plains.

The articles in this issue thus show dynamic ways of using scientific methods to address important research questions. In several instances, this research challenges us to revise or at least re-address what we think we know about prehistoric Rocky Mountain peoples. This is of course what good science does: challenge established thinking, not through biased polemics, but by falsifiable, replicable studies. But while Benedict's contributions to developing Rocky Mountain environmental archaeology as a science are well expressed in this collection of papers, I do worry about this legacy to some degree.

My concern is based on a reluctance on the part of some to take chances to solve vexing empirical problems like Benedict did with, for example, different dating proxies, ostensibly due to the (oftentimes considerable) problems associated with these methods. For instance, lichenometry is (as Benedict himself was clearly aware) fraught with problems and really is not that great a way to tell time when compared to other methods (Benedict 2009). But in the absence of other methods (as is so often the case with surface features in the high country), is not some temporal resolution better than none at all (e.g., Morgan, Bettinger, and Giambastiani 2014)? The same might be said for obsidian hydration dating, which can provide at least a measure of (albeit oftentimes relative) chronological control absent radiocarbon or diagnostic projectile points (e.g., Beck and Jones 1995; Rogers and Duke 2014).

To conclude, as archaeology has become an increasingly well-established discipline in public, private, and academic spheres, it also runs the risk of becoming less a dynamic science than the work of engineers and technicians (see Kuhn 1996:30) when the methods developed through normal science become merely tools for accomplishing specific tasks (e.g., the job of the electrician who comes to rewire your house is a far cry from Volta's, Franklin's and later, Edison's revolutionary work with electricity). Clearly (and in contrast), it is exactly Benedict's creative approach to answering empirical questions that has far more likelihood of generating the types of anomalous data that might force us to rethink some of our cherished notions about how and why people lived the way they did in the past. If we keep looking in the same places with the same methodological approaches, it is clear what we will find out: what we already know. And that, if the papers contained in this issue are any indication, will be Jim Benedict's lasting legacy: a robust archaeological science that looks in new places for new data using novel techniques.

Notes

1 It is worth mentioning, as LaBelle and Cassells (this volume) and Benedict himself (2001) aptly point out: even this groundbreaking research built on work of those who came before.

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