

# Pristine Aborigines or Victims of Progress?

## The Western Shoshones in the Anthropological Imagination

by Richard O. Clemmer

In the anthropological literature, the Western Shoshones as presented by Julian Steward loom large as a group of people who adapted as best they could to scarce resources in the high interior desert areas of North America: Utah and Nevada. Steward's work has become entrenched and enshrined as unassailable, at least from a methodological point of view. I suggest that Steward's Shoshones are an example of a tradition that has become entrenched in the discipline of anthropology, resulting in its constant replication as a form of particular intellectual authority despite the development of new approaches. Attention is focused on Steward's actual data and the historical circumstances that produced them. In light of these historical circumstances, it might be more accurate to conceptualize Steward's Shoshones as "victims of progress" than as a pristine group of hunter-gather-foragers. Examination of three cases of Western Shoshone subsistence along the Humboldt River in 1828–1829, Ruby Valley and vicinity in the 1860s, and the mountains and valleys of south-central Nevada in the 1860s and 1870s supports and illustrates this point.

### The Western Shoshone Mystique

In the anthropological literature, the Western Shoshones loom large as a group of people who adapted as best they could to scarce resources in the high interior desert areas of North America: Utah and Nevada. Until Lee introduced the !Kung into anthropology (Lee 1968, 1979, 1984), the Western Shoshones were presented as the quintessential hunter-gather-foragers getting by on scarce resources. The assumption with which anthropology as a discipline greeted the introduction of the Western Shoshones into its kit bag of case studies (Steward 1938, 1955) was that, as with Radcliffe-Brown's Andaman Islanders, Malinowski's Trobriand Islanders, and Ni-muendaju's (1942) Serente, these were "pristine" peoples, unaffected by contact with the outside world until the introduction of horses and armed warfare. In fact, Steward (1955) himself certainly gave this impression: his chapter on the Western Shoshones in his highly influential *Theory of Culture Change* distinctly contrasted the "aboriginal times" of the Western Shoshones with the time when their "territory was occupied by white settlers" (1955, 112), clearly implying that the previous 12 pages of description (1955, 101–112), based on his 1938 monograph did, in fact, present Shoshones and their culture in a pristine state. Like the Andaman Is-

landers and the Trobriand Islanders, the Western Shoshones carry a revered anthropological mystique.

Steward's work has become entrenched and enshrined as unassailable, at least from a methodological point of view. Not that there have not been critics and dissenters. The original dissenters to the wider implications of Steward's theoretical statement focused squarely on the very resources of the Great Basin that Steward downplayed: riparian resources. The view that neither Steward nor Jennings (1957, 1968, 1973), who based his concept of the "desert culture" on Steward's (1938) work, did not sufficiently take lakeshore resources into account has long been held by archaeologists Heizer (1956, 1966), Napton (1969, 55), Rozaire (1963), and Swanson (1966), among others (Davis 1966; Rhode 1999, 37). They were later joined by ethnographer Stewart (1980). Other critics, particularly Rappaport (1967, 1968) and Crum (1987, 1990, 1999, 123), have criticized Steward for portraying Western Shoshone culture as primarily "gastric" and have "criticized Steward and his followers for neglecting environmental phenomena other than food resources" (Vayda and McCay 1975, 295). And finally, recent reexamination of Julian Steward as an anthropologist and as a major influence in the academic world (Blackhawk 1999; Clemmer, Myers, and Rudden 1999; Kerns 2003; Patterson and Lauria-Perricelli 1999; Pinkoski 2008a) certainly compels close attention to the spirit of the times and the state of the discipline that to a large extent earned him his important place in anthropological theory (cf. Harris 1968, 644–670).

However, my focus here is on a different issue. I wish to

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refocus attention to Steward's actual data and the historical circumstances that produced them. This refocus requires attention to one particular issue: probable availability of and access to ecological resources and consequent population levels *within the historical context in which Shoshones and their relationships to resources were first observed*, that is, between 1828, the year of first systematic intrusion and observations by Europeans, and the early 1860s, the earliest period that Steward's consultants—whom he was interviewing in 1935 and 1936—could have remembered with any accuracy. Several reexaminations of historical circumstances give reason to question the assumption of "aboriginality." This assumption underlies Steward's work and is denoted in the title of his groundbreaking ethnography, *Basin-Plateau Aboriginal Socio-political Groups*, and is reinforced in several chapters of his heavily influential *Theory of Culture Change*. In 1987 I suggested that the large numbers of emigrants trooping through Western Shoshone country with ox-drawn wagons, mules, and horses along the Humboldt River road each summer, especially between 1848 and 1860, would have severely compromised the degree to which resources would have been available to Western Shoshones. The emigrants poached game animals, shot wild fowl, secured fish, and grazed their animals wherever they found a bit of grass. In turn, Shoshones selectively took a horse or steer whenever they could do so. Despite occasionally successful retaliation by emigrants, Shoshones seem to have successfully maintained a presence along the Humboldt, although perhaps in diminished numbers. Thus, although their use and occupation of this part of their territory was not prevented, it was certainly altered. In 1999 Fowler et al. (1999) documented Western Shoshones' quick adaptation to the mining economy far to the west, in Death Valley in the 1860s, by growing crops and selling them to the miners. Steward made mention neither of the emigrants and their possible impacts nor the homesteads and cash-cropping that his consultants undoubtedly remembered and probably talked about. Nor did he mention anything about Death Valley Shoshones' involvement in the mining economy (Fowler et al. 1999, 56, 57).

I wish to extend this critical reexamination of Steward's presentation of the Western Shoshones and to suggest two things about the place of the Western Shoshones in the anthropological literature. First, if Western Shoshones are to have a place in that literature, it might be more accurate to conceptualize them as "victims of progress" (e.g., Bodley 1990) or perhaps as reluctant participants in it than as a pristine group of hunter-gather-foragers. The accuracy of Steward's ethnographic data itself must also be questioned. The data that Steward gathered, the subsistence patterns that he proposed as characteristic of the Western Shoshones, and the picture of them in the "ethnographic present" that he reconstructed cannot be taken at face value. This is so because Steward omitted so much of the historical circumstances of that "ethnographic present." Second, as Borofsky (2002) has suggested, particular traditions have become entrenched in

the discipline of anthropology that result in the constant replication of particular intellectual authorities, almost like the reenactment of rituals or the construction of a myth that reaffirm the discipline's image of itself and the social structure that maintains it. One of these traditions is that a body of ethnographic data exists that constitutes the foundation of anthropological assumptions and that this body of data can be constantly invoked and reinvoked to provide a basis for subsequent anthropological research, even when new approaches brought in from other disciplines or developed within anthropology itself urge resettling of that foundation on firmer ground. Space constraints preclude an exhaustive review of all of Steward's data here; however, I would like to examine Western Shoshone subsistence in three areas and time periods: the Humboldt River in 1826–1829; Ruby Valley and vicinity in the 1860s; and the mountains and valleys of south-central Nevada in the 1860s and 1870s, particularly the Reese River Valley and vicinity.

## Newe: The Western Shoshone People

The Western Shoshones, or the Newe, were historically Uto-Aztekan-speakers who were linguistically related to Utes and Paiutes and who made their living by gathering, foraging, hunting, fishing and doing a little farming here and there. They call themselves "Newe," "Nuwa," or "Numa," or a variation on one of those terms depending on dialect, and are identifiable as "Shoshone" on the basis of cultural and linguistic criteria. Anthropologists categorize them as either "western," "northern," or "eastern" not only on the basis of geographical location but also on the basis of shared cultural characteristics. For Eastern Shoshones, home was the Wind River Range and the headwaters of the Green and Wind rivers just to the south and west of it. They rode their horses south to the Plains to hunt bison in the late summer and early fall and into the Snake River drainage in the summer to trade with other Shoshones and with the Nez Perce. Because they were mounted, they had the largest geographical range of any of the Great Basin groups, but they were constantly on the lookout against Blackfeet, Cheyenne, and Arapahoe who clashed with them when they came down from the north to hunt the bison in Wyoming. Among their resources, they relied heavily on bison as well as on pronghorn antelope, mule deer, elk, fish, and rabbits. Their horse-based lifestyle engendered a degree of political and military cohesion that made much of their culture and social organization resemble that of the Plains groups with whom they had alliances and against whom they fought more than it did the culture and social organization of other Shoshone groups (Shimkin 1947, 245, 276–279). Northern Shoshones inhabited the Snake River drainage from the Weiser and Payette Rivers on the west and the Plains to the north of the Snake River above Salmon Falls north and east through the Bitterroot Range and the Lost and Salmon river drainages of southern Idaho and into

extreme southwestern Montana. Known as either “Sheep-eater,” “Salmon-Eater” or “Lemhi” Shoshones, they maintained diets heavy in mountain sheep, camas root, mule deer, and fish (Madsen 1980).

Western Shoshones, distinguished on the basis of dialect and culture, define their home territories in what is now northwestern Utah, southeastern Idaho, an arc stretching from northeastern to southeastern Nevada, and extreme east-central California. Steward (1938) divided Western Shoshones roughly into 18 sociopolitical groups. These groups were not bands or aggregated of bands, but Steward names these groups after the particular areas that they frequented. Thus, there are the Humboldt River groups, the Railroad Valley groups, the Ruby Valley groups, and so on. Steward did not present them as emic or self-identified groups, and the actual relationship, if any, to self-identified groups remains unknown. Concerning origins of the words “Shoshone” or “Shoshoni,” Steward (1937, 627) says: “All Shoshone call themselves num: or num,. The origin of ‘Shoshone’ is unknown though a plausible guess derives it from so (much) sonip (grass).” “Gosh-Utes” living east and south of Great Salt Lake were also Western Shoshones. Steward noted that there is little cultural or linguistic difference between Goshutes and Western Shoshones and that Western Shoshones use the term “Goshute”—meaning literally “dust-Ute”—to refer to Shoshones in western Utah who did not have abundant food sources available to them. He also distinguishes a group known as “Weber Utes” but in his more complete work (1938, 136, 220, 221) notes that there is no cultural or linguistic difference between Goshutes and Weber Utes and that the Weber Utes were therefore not Utes. He also notes a fourth group of Goshutes in Tooele Valley in eastern Utah. Two small groups in northwestern Utah/southern Idaho were designated historically as Northwestern Shoshones. Because Western Shoshones refer to themselves as “Newe,” I will use that term alternately with “Western Shoshones” in the following pages. Where reference is made to a specific self-named group within the Western Shoshone entity, I will use the Shoshone name.

Steward’s picture of aboriginal life conceptualized people who had a simple culture produced by the stress of resource scarcity. Steward also insisted that it was resource scarcity that compelled a social organization reflecting the primacy of producing food and rituals that celebrated unexpected windfalls. Starvation was common and social fragmentation was the norm. People gathered together only temporarily in winter villages where they huddled in abject apprehension of an irremediable spring dearth of resources that would send them wandering nine months out of the year in search of food, hoping for a feast of salmon or pronghorn that would provide them with fat times, however brief. For a short period between 1860 and 1870, according to Steward, some groups achieved tighter and more sustained social organization when they went on the warpath. Although some ecological work has been done among the Northern Paiute (Fowler 1972, 1982, 2000), no systematic reexamination of Western Shoshone cultural

ecology has been undertaken. We will turn first to the Humboldt River and the Western Shoshones as they were encountered by trapper Peter Skene Ogden in 1828 and 1829.

## Beaver in the Humboldt: A New Resource

### *Trapping Beaver: The “Scorched Stream” Approach*

Ogden took over the position of chief trapper for the Hudson’s Bay Company from Alexander Ross in 1825, and Ogden provides clear evidence that beaver were an important resource for Humboldt River Shoshones. He found beaver skins used for clothing and noted that virtually all the Indians he saw were wearing “shoes made of beaver skin.” He concluded that “the Indians in this river destroy a great number of beaver” (Ogden 1971, 148) and reported constant trouble with Indians stealing traps as well as horses. He insisted that in the 1828–1829 season alone they had stolen 50 traps, although Rusco (1978, 165, 167) could document specific mention of only 14 traps stolen in all four of Ogden’s expeditions. She found that of 520 encounters between trappers and “hostile” Indians in Shoshone country documented for 10 trapping expeditions over a 16-year period, only 173 were negative. Twenty-one encounters are accounted for by a firefight between Hudson’s Bay trappers—largely Flat Head and Nez Perce trappers—and intrusive Blackfeet at a rendezvous at Pierre’s Hole in 1832, and 58 were accounted for by a notorious massacre of Paiutes by the Bonneville-Walker expedition of 1833. Clearly, Western Shoshones opposed the trapping of beaver and tried to defend this important resource without directly confronting the trappers themselves and risking possible annihilation (fig. 1).

Besides the Hudson’s Bay, Northwest, Rocky Mountain, and American fur companies, at least three other fur-trapping enterprises operated in the Snake and Humboldt drainages in the 1820s and 1830s. It is known that four Rocky Mountain Fur Company trappers, including Thomas Fitzpatrick and Milton Sublette, went to the Humboldt River in 1832 to trap, but whether they got any beaver and how many is not known (Hafen 1931, 122). Trappers were expected to carry little food; Hudson’s Bay trappers explicitly were told to live off the land (Rusco 1978, 158). While a seasoned trapper might obtain an advance to outfit a pack train based on his history as a reliable fur producer or against anticipated production, by far the majority of trappers preferred to travel light, foraging as they went. This strategy meant that, in addition to trapping as many animals as fast as possible, trappers also represented potential competition with Indians for other subsistence resources. Trappers living off the land for the better part of a year not only ate beaver but also hunted deer, antelope, and

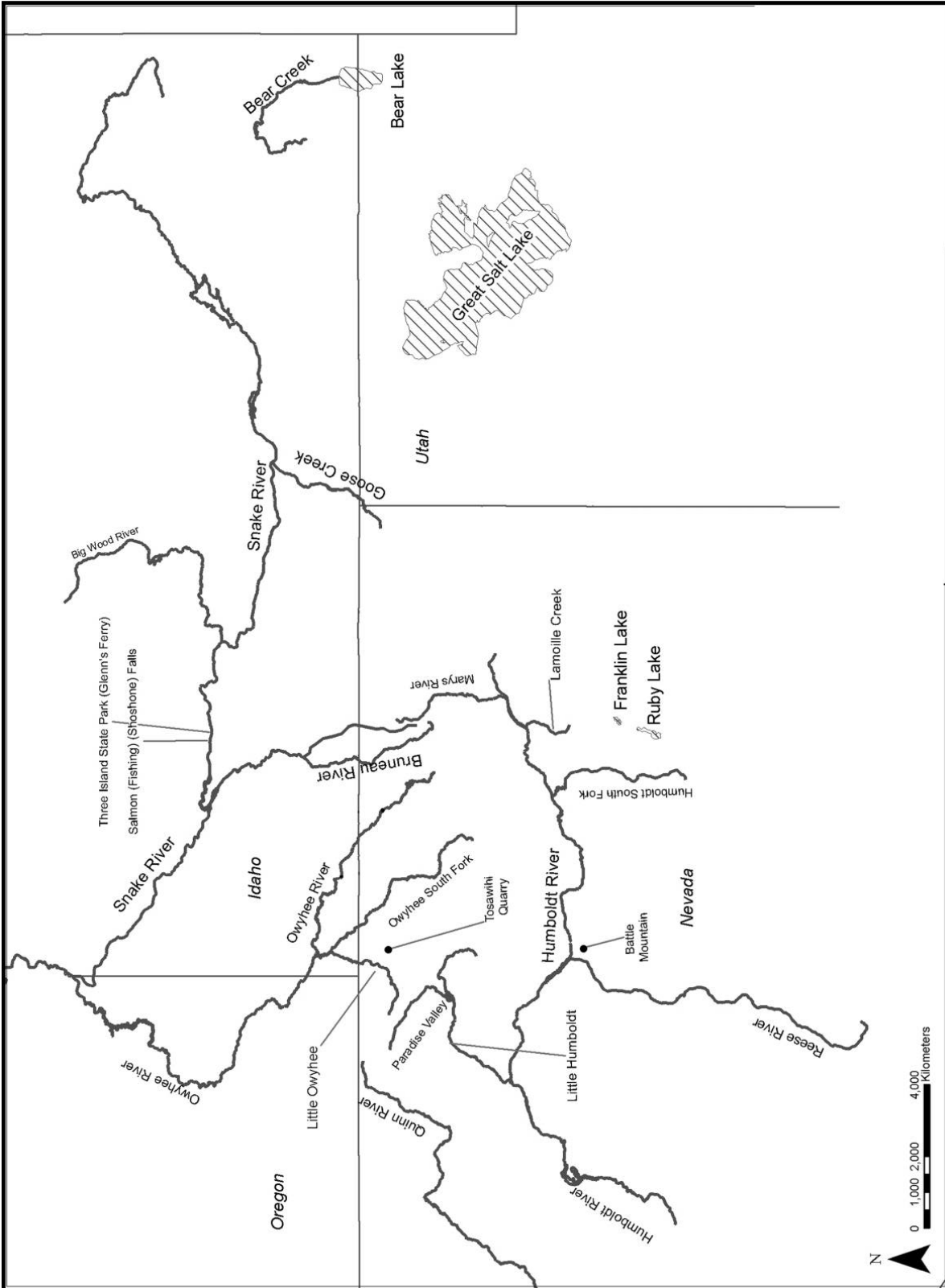


Figure 1. Snake, Humboldt, and Quinn rivers and tributary streams.

waterfowl and even gathered roots and berries when they needed to do so.

However, it was beaver that constituted the linchpin resource that provided meat, clothing, and ecological stability to drainages in Western Shoshone territory in southern Idaho and northeastern Nevada. And it was the Hudson's Bay Company's Ogden who must be credited with trying to trap out the Owyhee and Humboldt rivers. For political reasons, the Hudson's Bay policy in Snake country was to "destroy" the "rich preserve of Beaver" (George Simpson, 1824, quoted by Mackie 1997, 47). About to embark on an expedition to the Snake River drainage, Simpson, governor of the Hudson's Bay Company, wrote directly to Ogden, specifically instructing him to "devastate the country as far as beaver supply was concerned. It was sort of a 'scorched stream' policy designed to denude the country and render it unprofitable and hence unattractive to Americans" (Miller and Miller 1971, xiv). "That is, they deliberately and systematically sought to trap all of the beaver in the territory . . . to make it unprofitable for their competitors" (Rusco 1978, 159). The policy was implemented off and on in various areas under British control during Simpson's long tenure as governor (Feit 2007, 60). Ogden roamed all over the southwestern Snake and Owyhee drainages and went far south of the southern boundary of "Oregon," trespassing into the area claimed by Mexico. Altogether, implementation of Simpson's vengeful trapping policy was to have enduring effects.

In late October 1828, Ogden and his party went due south from the Malheur River at Harney Lake until they hit the Quinn River in southeastern Oregon, and then they struck southeast until they came into the Little Humboldt drainage, following the fork until they hit the main Humboldt River. On the Little Humboldt River in Paradise Valley (he thought it might be the "Sandwich Island" River, i.e., the Owyhee River), Ogden's trappers reported that beaver were not very abundant. Ogden (1971, 107) attributed their scarcity to their "wildness," caused by the fact that Indians regularly took them. Nonetheless, his trappers came in with 50 beaver. In the Quinn and Paradise river valleys, Ogden (1971, 105) found "most numerous" Indians, who later were described as mixed Paiutes and Shoshones. Along the tributary Little Humboldt, Ogden reported "the natives are most numerous again . . . the banks of the river are lined with their huts of large size" (Ogden 1971, 106). He left his trappers and explored downriver on the main Humboldt past Winnemucca (Miller and Miller 1971, 107) but did not trap. The next day, Ogden's party followed him downriver; they then reversed direction, leaving the river but going west and eventually coming again to the Humboldt River proper near present-day Golconda, about 10 miles west of the historic boundary between Shoshones to the east and Paiutes to the west. Ogden had named it "Unknown River" (Miller and Miller 1971, 107, 108) and continued to call it by that name in his journal, although he remarked later that he should

have called it "Swampy River" because of the shallow turbid areas, which were undoubtedly caused by beaver dams.<sup>1</sup>

Ogden led his party upriver, following an Indian track that he thought had been traveled by 400 horses, although he apparently did not see the horses and gave no reason for his assertion. Now definitely in Shoshone country, he encountered a camp of at least 300 Indians and remarked that "it is almost indescribable how numerous the natives are in this quarter" (Ogden 1971, 108). Clearly, once again, beaver were an important subsistence resource. Ten Indians paid them a visit "and as we had four traps stolen yesterday, I did not lose this opportunity of treating them pretty roughly, at the same time I gave them to understand if they did not cease from stealing we would shoot all we saw" (Ogden 1971, 108). Traveling further west upriver, Ogden (1971, 109, 110) reported the banks of the Humboldt "lined with Indian villages" but "at present all deserted." Three days later, on November 19, 1828, he encountered a camp of 50 tents. "Upwards of 150 Indians paid us a visit, poor miserable looking wretches with scarcely any covering, and the greater part without bows or arrows or any weapons of defence" but "fat and in good condition" (1971, 109, 110). The following day they or others came again, this time numbering 200. Near present-day Dunphy, Indians "as usual" were "most numerous about us and again have stolen six traps" (1971, 109, 110). In early April 1829, near the confluence of the Humboldt with Lamoille Creek, he found "thirty Indians employed in fishing salmon trout, about eight inches in length, remarkably fine" (Ogden 1971, 140). Either fish were very abundant or Ogden used extraordinary persuasion, because he reported that "they gave us all they had, about fifteen" (Ogden 1971, 140). Water was low in the river and they crossed it easily, going further downstream to the confluence with the North Fork, where they found another 15 Indians fishing. About 4 miles south of where Elko is today they encountered "a camp of upwards of sixty Indians, but could obtain no fish from them" (Ogden 1971, 141).

Along the Little Humboldt and the part of the Humboldt that he trapped in Shoshone country in the two seasons that he trapped it, Ogden reported taking a total of two otter and 770 beaver; he seems to have established a quota of 800 for the Humboldt and thus must have regarded it as trapped out. Trappers took male and female beavers indiscriminately. In fact, it is nearly impossible to tell a male beaver from a female until the animal is skinned. The gestation period for beavers is 110 days, and births usually occur between March and June.

1. The trappers called it "Mary's River" in honor of an Indian wife of one of the trappers in Ogden's party (Cline 1963, 116). Morgan (1943, 5) states that it was named "Mary's River" after Ogden's wife. However, this story is apocryphal; Ogden's wife was named Julia, and whether she accompanied him is a matter of conjecture (Miller and Miller 1971, xvi-xvii). John Work (1923, 298) appropriately christened it "Ogden's River" in 1831, and Washington Irving still used that name for it in 1868. Zenas Leonard in 1833 called it "Barren River." It got the name "Humboldt" from John C. Fremont in 1843, and that was the name that finally stuck, although a north branch of it is still called "Mary's River."

Thus, trapping a stream over a period of several months in the spring, as Ogden was doing on the Humboldt, could eliminate large numbers of females in various stages of pregnancy. Litters usually number between two and four in size (Hilfiker 1991, 36), and thus, assuming gender symmetry, trapping 100 beavers could hypothetically eliminate up to 250. Trapping 770 could eliminate nearly 2,000. Although heavy trapping may have temporarily increased breeding and fecundity of those beaver that Ogden did not get (Feit 2007, 68, 69), the Bonneville-Walker expedition of 1833–1834 (Irving n.d.; Leonard 1959) certainly would have harvested these temporarily larger numbers and thus perhaps permanently pulled the beaver numbers down below harvestable levels.

Utilizing the participation of the Cree farther north and east in the fur trade (Francis and Morantz 1983; Ray 1998) as an analogy to the Western Shoshone situation, a possibility that was suggested by Rusco (1978, 160, 170) needs to be entertained, that beaver indeed were decimated but by the Indians themselves. She asserts that Indians stole traps in order to acquire a new technology, the acquisition of which improved their ability to exploit certain resources and the wealth they promised, implying that the natives themselves participated in trapping out the beaver. However, as Francis and Morantz (1983, 167) note, there was not just one fur trade but many, “differing over time depending on economic situations and differing from place to place depending on geography, ecology, and relationships with the Indian people.” Unlike the situation with the Cree to the northeast, there is simply no evidence that whole families of Shoshones ever took up trapping and trading beaver as a sustained commercial enterprise, although it is possible that some Humboldt or Snake River Shoshone individuals did participate in it. Remy and Brenchley (1861, 145–147) encountered a Humboldt Shoshone who had spent “some years” with Canadian trappers hunting beaver and had returned well fed, clothed, and with horses. And apparently, 50 years earlier in 1805, William Clark had encountered a Shoshone who was visiting from “the Duck Valley country which lies somewhere in Oregon country—possibly between the forks of the Owyhee River” at Lost Trail Pass in 1805, where the Lewis and Clark expedition turned north into the Bitterroot Mountains instead of turning southwest and following the Salmon River down to the Snake (Ontko 1993, 120, n. 93). It is possible that, whoever he was, he was there to participate in the fur trade.

In *The Fur Traders and the Indian*, Lewis Saum (1965, 47, 48) says, “the far West river-and-coast tribes . . . excelled in fishing and trading. . . . From the early fur-trade perspective, they fulfilled a useful function. By 1793, however, Alexander Mackenzie [of the Northwest Company] had pushed overland to the ocean, and within a few years white traders were in the high reaches of New Caledonia [British Columbia, Washington, and Oregon] at the source of the furs.” Just how far south and east they penetrated and the extent to which Plateau Indians may have been involved in the fur trade in these very early years is unknown. John Day, who supplied the Northwest Company with pelts in the 1820s reportedly set up a

profitable fur trade with the “Snake Indians.” But Day was trapping in the John Day River and its tributaries in east-central Oregon; the term “Snake Indians” was applied indiscriminately to any and all Indians in the Snake River drainage.<sup>2</sup> If “Snake” Indians were supplying him, they were most likely Bannocks. There is apparently only one mention in trappers’ journals of Shoshones at a trade rendezvous, and this rendezvous was located very far from Western Shoshone country; it was in the territory of the Crow Indians, at Horse Creek on the Green River in 1837. The “Shoshoni or Snake Indians” (Mackie 1997, 107, 108) there were undoubtedly mainly Eastern Shoshones with beaver pelts from streams far to the east of the Humboldt. There is no record of Newe with packs of pelts to trade regularly traipsing all the way from the isolated Humboldt up to rendezvous in any great numbers, and Ogden got hardly any beaver pelts from trading there. Therefore, the scenario wherein the Newe along the Humboldt were the primary offenders in using steel traps to decimate beaver, thus eliminating their own food source, is the least likely and most speculative alternative. Rather, it is most likely that the Hudson’s Bay trappers really did trap out the Humboldt, Little Humboldt, and Quinn river valleys to the extent that, as in the Owyhee, beaver were effectively eliminated as a resource for food and clothing that Shoshones could easily obtain in those streams.

Therefore, there is good reason to postulate that non-Indians trappers sufficiently destroyed the breeding population to have eliminated the beaver as a resource for a whole generation of Humboldt Shoshone. If Ogden and subsequent trappers such as Bonneville and Walker in 1833–1834 just happened to trap more females than males, could they have substantially decimated the Humboldt beaver population? If they did so, what might have been the consequences? If they did not do so, it is difficult to account for Steward’s complete omission of beaver from his discussion of subsistence resources. In his entire discussion of resources, Steward mentions beaver only once: “Those [rodents] living in the water, the beaver . . . and the muskrat . . . were too restricted in occurrence and number to have had great importance” (Steward 1938, 40). But of course, that is just the point: had they always been so? Or was there some activity that had made them so?

#### Discussion

Ogden’s encounters with large numbers of fishing Shoshones wearing beaver clothing and footwear along the Humboldt confirms Steward’s (1938, 159) documentation of the importance of trout to Humboldt River Shoshones and also supports Steward’s surmise that the Powell and Ingalls population estimates of 1872 (Fowler and Fowler 1971; Powell

2. Local historian Gale Ontko (1993, 225–235) assumes these “Snake Indians” were White Knife Shoshones. But the White Knife Shoshones were from the Humboldt River area, moving transhumantly between the Humboldt, the Snake, and the Reese River Valley (Harris 1940).

and Ingalls 1872) were too low. John Wesley Powell, the director of the Bureau of American Ethnology and of the U.S. Geological Survey, together with his partner, special agent George Ingalls of the Bureau of Indian Affairs, did a whirlwind tour of Shoshone and Paiute country, interviewing Indians from their headquarters in the Battle Mountain Hotel; the accuracy of their data might well have been hit or miss. In fact, if anything, Ogden's observations argue for denser population concentrations along the Humboldt and its tributaries in the 1820s than Steward's estimate of around 1,100. If the banks of the Humboldt were "lined with Indian villages" in 1829 and if it was "almost indescribable how numerous the natives are in this quarter," how many would that have been? If Ogden encountered 300 Indians one day, a camp of 50 tents on another, upward of 150 Indians the next day, and 200 a couple of days later, surely Steward's (1938, 47) inference of an aboriginal population density of *one person per 3.75 square miles* must be low. In fact, it would seem that density might have been exactly the inverse: that is, *3.75 persons or more per square mile*.

It could be argued that this high density may have been seasonal. Ogden made his observations in November, when, according to Steward, people would have been establishing winter villages. To some extent, high seasonal density fits the pattern that Steward (1938, 232) projected for Western Shoshones. It is worthwhile quoting Steward's summary statement on Western Shoshone settlement patterns at length here:

The most permanent association of families was at winter encampments. These were sites where certain families habitually remained during the months when vegetable foods could not be had. Necessary conditions for such sites were accessibility to stored seeds, especially pine nuts, water, sufficient wood for house building and fuel, and absence of extremely low winter temperatures. These conditions were most often fulfilled in the mouths of canyons or within the pine nut–juniper belt in the mountains, though sometimes broad valleys near fishing streams were chosen. . . . Whether [encampments] . . . were clustered in dense colonies depended upon the quality of foods which could be gathered and stored within convenient distance of each camp.

The Humboldt riparian ecotone certainly qualifies as a broad valley near a fishing stream. Steward has no population estimates for the Western Shoshone part of the Humboldt drainage north of the Humboldt itself and has no data on villages there. However, Ogden found the Indians "numerous" along the Little Humboldt and in the Quinn River Valley. Just to the south of the Humboldt River, Steward shows population density as slightly higher than that of the Humboldt itself: 3.37 square miles per person. For "all of Nevada," including Northern and Southern Paiutes as well as Western Shoshones, he accepts an Indian agent's 1861 estimate of 7,000 (Steward 1938, 48), for a density of 15.8 square miles per person, or 0.063 persons per square mile. This may well have

been the density in 1861, but given the data from Ogden's journal, it cannot be accepted as the aboriginal density in the 1820s. If we assume that, in fact, there were no winter villages north of the Humboldt except in the Little Humboldt and Quinn river valleys, which certainly must have been good locations for winter villages, then we must assume the people from the "blank" area on Steward's population density map congregated for winter in the Little Humboldt, Quinn, and Humboldt river valleys and that people from the south located winter camps along streams and in the mouths of canyons, near fishing streams and within the pine nut–juniper belt in the Ruby Mountains and in Dixie Valley. This assumption yields a population density of 4.9 persons per square mile for the 10,800-square-mile area of the Humboldt River and the area to the north of it and east of the Little Humboldt/Quinn drainages. This is a far higher than the average of 0.063 in 1861!

There are only a few alternative explanations for these discrepancies. One possibility is that Steward's data were just not very good. On the one hand, he seems to have had good information from a couple of informants; on the other hand, he mistrusted the Powell and Ingalls censuses for some areas. A second possibility is that Ogden exaggerated population levels or simply misestimated on the high end. But Ogden was a very carefully chronicler of almost everything—from geography and weather to number of beaver trapped each day. At various points he tests what he has been told about Indians against his observations. There is no reason to not accept his estimates at face value.

Rather, the most likely explanation for the diminution of beaver and Newe is that trappers really did trap out the Humboldt, Little Humboldt, and Quinn river valleys to the extent that, like near the Owyhee, beaver were effectively eliminated as a resource for food and clothing that Shoshones could easily obtain in those streams. The difference in population densities between the figure of 4.9 square miles per person postulated here for 1829 and the figure of 0.063 that Steward accepted for 1861 in the Humboldt drainage might well reflect the impacts of trappers on beaver as a food source in the 1820s and 1830s.

#### *Steward's Assessment of Life along the Humboldt*

To what extent were the massive changes in the ecology of the Humboldt River and in Shoshones' subsistence strategies reflected in the picture that Julian Steward gives us? Steward located his ethnography of the Humboldt River at Elko, assuming the information that he got for Elko also applied to Dixie Valley, South Fork Valley, and communities along the Humboldt River for 40 or 50 miles in each direction. He listed only one informant for Elko, BG, who was born near there ca. 1875. "BG" is Bill Gibson. With his wife Jane, Julian spent a week in Elko in August 1935, interviewing Bill Gibson sporadically during that time. They found him "friendly and well informed, with an unusually keen historical sense, 'who

gave them a clear picture of surrounding (non-patrilineal) bands” (Kerns 2003, 193).

Steward also may have also gotten some information from “FSm, full-blooded Shoshoni man, born ca. 1855 in Grass Valley, south of Battle Mountain.” However, this man “lived as a child at Austin, then went to Carlin, Echo Canyon, and finally to Owyhee when the reservation was founded”<sup>3</sup> (Steward 1941, 213, 214). He does not seem to have used FSm at all for the Humboldt, but he did use “JP, full-blood Shoshoni man, born ca. 1850 or 1855 at Battle Mountain; has lived at Owyhee for many years.” Steward regarded JP as “well informed” and “one of the best informants,” although he needed an interpreter. He also used RVJ, a Ruby Valley man born in 1840 or 1845, extensively (Steward 1938, 152–64) and a population estimate from BM, born around 1855, who lived in Ruby Valley.

Steward noted that “the Humboldt Valley was a fertile region” and that “areas foraged for food were somewhat smaller and the villages somewhat larger” but that the “foods were somewhat erratic in their occurrence” (Steward 1938, 153). The villages were located at Independence Valley along Maggie Creek, Palisade, Carlin, Elko, and Dixie Valley. Three clusters of families lived in Huntington Valley, and about 100 people called the Kuivyudika (eaters of *Valeriana edulis* roots [Steward 1938, 154]) lived along Lamoille Creek at Halleck, “where fish could be taken all winter, and the Ruby Mountains provided foods in considerable variety and quantity,” including pine nuts and ground squirrels. For population figures, Steward (1938, 155–161) relied entirely on the Powell and Ingalls (1872) figures, which totaled 194 for Palisade and Carlin and 264 for Elko and Halleck, and he added another 228 for the stretch between Deeth and Wells. For Huntington Valley, RVJ, BG, and BM gave Steward estimates of a total of 41 families. For Dixie Valley, he used RVJ’s estimate of 246 persons, and in the Battle Mountain area, where he considered the Powell and Ingalls census figure of 194 as too small, he accepted an unreferenced estimate of 500 for the stretch of the Humboldt between Battle Mountain and Iron Point. Steward had only one figure from Indian agent Jacob Holeman of 600 for Thousand Springs Valley, northeast of Wells, in 1853. This area was right on the emigrant trail and would have experienced the same pressure on resources as the Humboldt at that time. Steward regarded the group as “transient,” and he was probably correct.

Because “families” constitutes a very vague measure when trying to estimate population, it is difficult to arrive at a solid figure for the Humboldt drainage; however, population was likely double Steward’s (1938, 49) estimate of 1,092. Although BG could not have remembered anything earlier than the late 1870s, RVJ’s memory may well have stretched back beyond the first intense emigrant impacts of 1848. Thus, it seems that

3. This could have been as early as 1877 but could have been as late as 1884 or 1885, when it was finally clear that the reservation would be maintained and its residents not forced to relocate to Fort Hall.

some of Steward’s population figures are accurate. His subsistence data, however, are suspect: “Along the Humboldt River . . . game was none too plentiful. A man was lucky to kill enough large game to make a complete outfit of skin clothing. Early accounts indicate that such small game as ground hogs [probably prairie dogs, or possibly rock chucks in the extreme northern parts of the drainage], gophers, and rats were perhaps economically more important.” It certainly must be questioned why it did not occur to Steward that this reliance on small animals was a fallback position. Why did Steward not consult Ogden’s documentation of his struggle with Shoshones over beaver? Steward cannot be faulted for not consulting Ogden’s 1827–1829 journals (Ogden 1971) because they had not been published and were available only in manuscript form in London. But Ogden’s 1825–1826 journal (Ogden 1950 [1826]) detailing his “scorched stream” trapping along the Snake and Owyhee was available; it had been published in the *Quarterly of the Oregon Historical Society* in 1909 (Elliott 1909).

## Seeds and Pronghorn Antelope in the Valleys and Mountains of Central Nevada: Rolling the Stages through Ruby Valley

By 1860 the Chorpenning stage company was running coaches regularly along the route between Salt Lake City and the Sierras (Settle and Settle 1955, 40). The primary station east of the Sierras was at what is now Carson City; there were three on the eastern side of the route, in Pleasant Valley, Spring Valley, and Deep Creek, and another in the middle of the route at Ruby Valley. With the stage road being pushed all the way through what was called the “Central Route” west of South Fork Valley, penetrating the Toiyabe Range and the Shoshone Mountains that summer, stages, mule trains, and the occasional emigrant wagon began using the route, taking it all the way to the Carson Valley. By the 1860s it was this Central Route that was being used exclusively. But it would be the Pony Express that would really challenge the ecological resources along the Central Route (fig. 2).

The Pony Express was a scheme developed by a brace of entrepreneurs in Lexington, Missouri: William Russell and William Waddell. In 1859 they took on another partner, Alexander Majors, and taking over the assets of an already operating freight line, Russell et al. established the Central Overland California and Pike’s Peak Express Company. On the recommendation of a couple of employees, William Russell decided that the company would capture the attention of investors by instituting a fast, reliable, frequent courier service straight from Independence, Missouri, to Sacramento. It would accomplish a spectacular victory over the U.S. mail contractor that slowly plied the stage route between the rail terminus in Missouri and Salt Lake City by getting a letter—



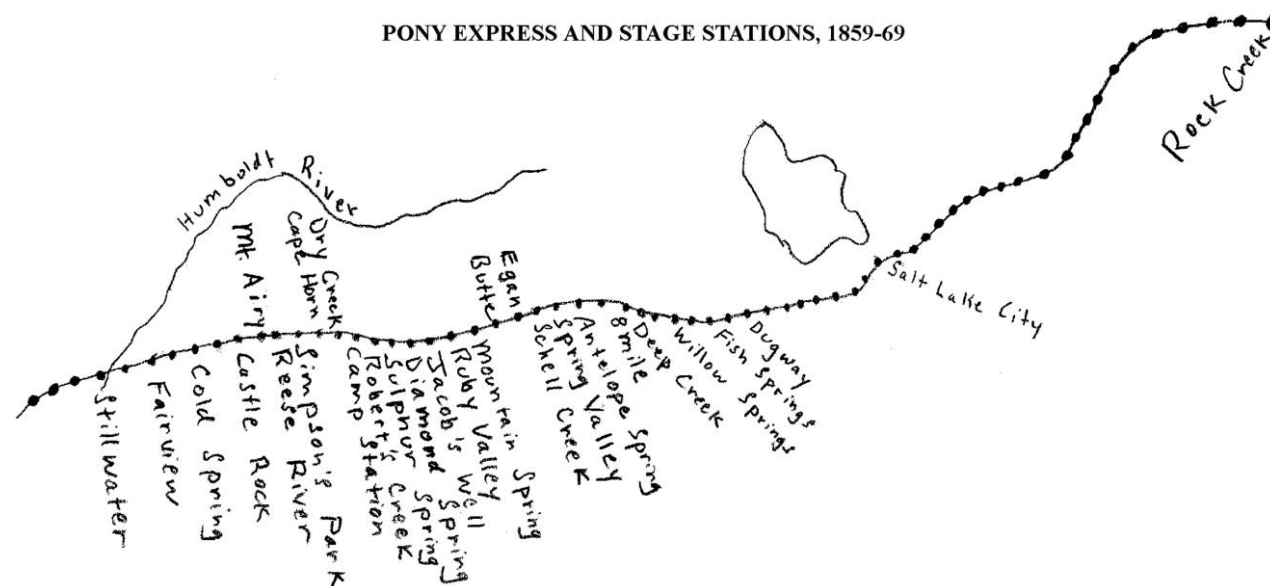


Figure 2. Pony Express and stage stations in Newe country, 1859–1869.

or anything else that anyone cared to send—in a saddlebag carried in hand-off relays by horse riders over the 900 miles from St. Joseph, Missouri, to San Francisco in 13 days (the first saddlebag actually made the trip in 11 days), guaranteed, once a week, week in, week out, winter and summer, 52 weeks a year. They would surpass the lumbering mail stages by 33 days or more, and they would run the route once a week in both directions. Charges would be a minimum of \$5.00 per half-ounce (the weight of a full-sheet letter and envelope); after August 1860 the minimum was dropped to \$2.50 per quarter-ounce; Hafen 1926, 189). A month after it started, the service was increased to twice a week.

In order to make all this happen, Russell, Majors, and Waddell needed to build way stations so that horses could be changed out at least once a day; some stations would double as stage stations where passengers could be put up if necessary and where the coaches could be serviced. The more rugged station keepers could sleep in bedrolls on dirt floors but where passengers might have to pass the night, regular bedsteads with ticking would have to be delivered. Food for riders, station masters, and horses would have to be packed in from Sacramento and Salt Lake City on a regular basis, because station masters could not abandon their posts to go out hunting. Most important, stations would have to be built close together—no more than 12 or 15 miles apart—and close to reliable water supplies. This meant either digging wells (Burton 1862, 556) or routing the vehicles through valleys where springs were located. Where no water could be located, wooden casks would have to be filled and hauled in by wagon (Burton 1862, 556). Corrals and stalls would be needed for the horses. Bags of oats could be delivered on a regular basis,

but ideally a pasture should be fenced and planted in “long forage”: hay and wheat grass (Chapman 1932, 149).

Russell, Majors, and Waddell set about hiring riders, finding the fastest horses, searching out the best locations for stations, outfitting them with supplies, and installing station masters. They set up 36 stations in Shoshone territory that were usually 10–12 miles apart (at least 9 and at most 18 miles; Egan 1917, 197–198). These first stations, thrown up hastily in the summer and fall of 1858 were little more than tents or at best stud-and-mud shacks up against a hill, with a dugout at the back (Chapman 1932, 128, 149). However, by 1860 stations had been improved. There were two types: relay stations and home stations. The home stations were entrepôts where feed for the horses and supplies for the stationmasters were stored, pack mules were permanently corralled for supplying the other 43 stage stations, and a small mess hall could provide meals to the mule skinner who periodically showed up with supplies. Some of the relay stations remained essentially a dugout roofed over with timbers (Burton 1862, 555; Chapman 1932, 128; Settle and Settle 1949, 78–79).

The home Pony Express stations also doubled as stage stations. They had at least three rooms: a kitchen with stove; a mess hall with tables of rough planks on trestles and often a fireplace; and a barracks, usually sleeping four, where the keeper, tenders, riders, mule skinner, and stage driver, and every now and then some paying stage travelers, could sleep. Sometimes this room doubled as storage room. Company wagon trains loaded with supplies including food—bacon, beans, coffee, molasses, pickles—called at monthly intervals or oftener from Salt Lake City, Sacramento, and Genoa (Chapman 1932, 149; Settle and Settle 1955, 45, 115, 116). Adding

to the hustle and bustle was the construction of the transcontinental telegraph. Because it was the shorter route between Salt Lake City and Sacramento, pole-raising took place hard on the heels of the leveling, scraping, and pounding of the Central Route. Hundreds of white pine logs were felled along the route (Read 1965, 198), just 30 miles east of where the road descended into the southern reaches of Ruby Valley.

### Discussion

Over the period of a year, the problem caused by all this activity was not so much a reduction in total resources as it was a cumulative disruption of the established flow of people in and out of resource areas. Resources were not in the places where Newe expected them to be nor were they there at the expected times; worse, the places where the resources were expected were now usurped by foreigners who coveted water, meadow, fish, and fowl for themselves. Undoubtedly Newe saw the Pony Express and stage stations as places where mounds of food were stored and not used—or used to feed animals that were not butchered for food—and that had no purpose except to support riders hurrying toward nothing or vehicles traversing their country that they were seldom invited to ride. Burton (1862, 570) found that at the stations in the Shoshone heartland, Newe men kept themselves aloof and derided the whites for their wastefulness. Indians hovered around the stage and express stations, hoping for a crumb. At first, such situations were welcomed by the station staff. An Indian or two hanging about could be pressed into service as cheap servants who would do chores around the station, tend stock, and add local color in return “for an occasional meal, their only payment” (Burton 1862, 568–570, 585, 589–590). At stations, if a squaw could be gotten to clean out the necessary, fix the meals, and provide “other services,” so much the better.

### Ecological Consequences

By 1860, the heartland of Western Shoshone territory, an area that had had only four or five small settlements with hardly a dozen non-Indians altogether as permanent residents in 1856, had acquired nearly 20 permanent settlements in the form of stage and Pony Express stations, with a minimum of 40 permanent residents. More to the point, where there had formerly been 40 or 50 domestic ungulates as permanent residents, there were now a minimum of 100. Eight six-mule teams pulled coaches through the countryside once a week, and teams of six, eight, or 10 mules came tramping through once a week with the mail when the stage coaches were not running. Once a month, mule teams pulling eight or nine wagons loaded with supplies churned the roadway; and a minimum of two horses at any one time thundered through twice a week at breakneck speed. Additionally, an undetermined number of travelers on horses and in wagons, including

some emigrants, now chose the Central Route over the Humboldt Trail (Burton 1862, 555).

Where mere footpaths existed in 1856, Army surveyors commanded by Lieutenant James Simpson laid out a wide road that 4 years later accommodated iron-rimmed wheeled vehicles and metal-shod hooves. Simpson's heavy wagons mashed the sagebrush and greasewood and ground up the grasses and forbs. The very best springs, the deepest and most reliable streams, and the most lush parts of the valley floors had been usurped for stock watering and pasturage (e.g., Meadow Creek, Egan's [Lost] Springs, Fish Springs, Willow Springs, Mountain Springs, Reading's Springs, Ibapah, Eight-Mile Spring, Spring Valley, Antelope Springs, Schell Creek, Ruby Valley, Jacob's Well, Diamond Springs, Roberts Creek, Simpson's Park, Sage Springs, Reese River; Burton 1862, 548, 550, 551, 561, 562, 564, 565, 572, 574, 590, 591; Egan 1917, 197, 198). Where good stands of native grass could be found, horses and mules were turned out to graze (Burton 1862, 556, 564, 566, 572, 583, 584). Goshute Shoshones deliberately concealed the location of one of their springs in order to protect it (Burton 1862, 556). Devil's Hole near Dug Way in the Salt Lake Desert, a natural well that Simpson (1876, 50) in 1859 implied was as drinkable as any well water, had become “half-salt” by 1860 (Burton 1862, 557). Express riders seeking firm footing through the miles of “putty-like mud” in spring and autumn rains pushed their horses into wide swaths skirting the muddied road and pounded the silty roadway into a thick carpet of fine alkaline powder in summer that swirled in clouds of gritty dust when the wind blew through the broad valleys (Burton 1862, 551, 557, 559).

Stated another way, the number of animal-days represented by domestic ungulates in Western Shoshone country had increased from about 60 per month in 1858 to 400 per month in 1860.<sup>4</sup> The importance of this increase lay not only in the space the animal took up or in the terrain that it traversed but in the changes to the configuration of resources, especially grasses and water sources, that the animals' presence generated. Streams were diverted for irrigating pastures that were planted in grasses not native to the Great Basin: hay, wheat grass, and oats (Burton 1862, 562, 563). Where pastures were not planted, animals were fed grains of these grasses. Whether in pasture or trotting along the road, the animals excreted droppings that would have gotten quickly pulverized or mashed into the mud. The varying amounts of undigested grain in the droppings would have then gotten blown into the air, washed into drainages, gobbled up by birds, ingested by rodents, rabbits, and coyotes and deposited among the *Oryzopsis* and *Mentzelia*, where they would sprout, head, and multiply. Before long, the invaders would take over, crowding out the natives.

Pronghorns would have been driven south of the road; to

4. An “animal day” is a 24-hour period in which an animal, in this case a mule, horse, or head of cattle, utilized some portion of the Western Shoshone habitat.

the north, the Humboldt traffic would have already compromised pronghorn habitats. Shoshones seeking out plots of grass seeds in their accustomed locations would not have found them. Pronghorn would be immediately more scarce. Deer, migrating to and from higher locations in spring or seeking out water sources in summer would be picked off by station keepers or scared off by clanking mule teams, thundering express riders, and the rolling dust from stage coaches, as would game birds (see Burton 1862, 560). During a particularly wet year, floods created a swamp 3 miles wide at the upper end of Steptoe Valley; stage coaches had to be unloaded so that they did not sink into the sticky alkaline soft-pan. Indians lugged passengers and baggage across the sink in sedan chairs constructed of cottonwood poles with slings of burlap sacks. The price was \$40 per trip. Whether it was the Indians or the Overland Company that got the \$40 is not recorded (Ashbaugh 1963, 300).

In the late fall of 1860 and early winter of 1861, the condition of Shoshones between Reese River and Ruby Valley mirrored the dilemma of all Indians along the Central Route. Indian agents knew that the Overland Company had monopolized the grass to feed the company's stock and pasture pony express horses, depriving the Indians "of the seed which they have heretofore used as an article of food" (Martin 1861, 134). But of course there was more to it than that; the Company had destroyed whole microhabitats. Army personnel reported from Ruby Valley that the pine nut crop had failed and thus they had no provisions stored away for winter. "All, or nearly all of their grass has been cut by the Stage Company or by citizens living on the road" (Madsen 1985, 136, 137).

By the summer of 1860, observed Sir Richard Burton, Indians had been "driven by destitution to beg at the stations" (Burton 1862, 560). On March 22, 1860, they attacked the mail stage at Eight Mile station, killing the driver. In May it was attacked again, the driver and the station cook were killed, and the mail was cut to pieces. Also in May 1860, the Deep Creek post was struck, one man was shot, and several horses were taken (Egan 1917, 256, 265). In July it was attacked again and five men were killed (Hafen 1926, 251). A confrontation at the Schell Creek station resulted in three or four Indians being killed. Later, the Schell Creek station was burned and the Butte station was attacked. However, conflicting stories abound. Two tenders at Dry Creek were killed after the stationmaster supposedly refused to give back his Indian wife to demanding Shoshones (Madsen 1985, 122, 123). But another story had it that only one man was killed after refusing to give up all of his provisions to hungry Indians (Chapman 1932, 209, 210; cf. Burton 1862, 588), and still another version blamed the altercation on "Brigham's boys," Mormons with whom the "Pony Express & Mail Route" was "well stocked" and who wanted to "excite troubles" (Brewer 1860, cited in Nevada Bureau of Land Management 1976, 30). A station keeper at Dry Creek was said to have "shot down an Indian ruthlessly and in cold blood saying that he would rather shoot a man than a dog" (Brewer 1860, cited in Nevada Bureau of

Land Management 1976, 30). But how many of these actions were retaliation and counterretaliations and how many were occasioned by local disputes is impossible to know.

A reminiscence by Overland agent Frederick William Hurst reported the winter so severe that "Indians were dying by the hundreds (Carter 1947, cited in Nevada Bureau of Land Management 1976, 44). "Contrary to instruction," he "would mix flour into a paste similar to Hawaiian poi and set it outside at night for the starving Indians" (Carter 1947, cited in Nevada Bureau of Land Management 1976, 44). Supposedly the gesture prevented the Ruby Valley station from being attacked and burned (Carter 1947, cited in Nevada Bureau of Land Management 1976, 44). Agent Wasson said that the Overland Company was issuing rations of grain to the Indians "at such points as it seemed necessary, and in quantities sufficient to prevent starvation" (U.S. House of Representatives 1862, 363), and the company even asserted that in at least one year, it had purchased \$10,000 in beef for "gratuitous distribution among the half-starved savages," in countering charges that the Overland's aggressive policy toward Indians had been the entire cause of the troubles along the route in the first place (Conkling and Conkling 1947, 31, 32).

If the Overland's employee in Ruby Valley can be believed, distributions of food were acts of charity on the part of individual agents, not a company policy, and without independent confirmation from agent Wasson, the assertion about beef cannot be credited. In fact, there is some evidence that at least some of the "rations" that were distributed originated with the Nevada Territorial Government, not with the stage company, even though it may have been stage company employees who did the distribution: Henry Butterfield, agent for the company, wrote to James Nye, Nevada's territorial governor and Superintendent of Indian Affairs ex officio on January 24, 1862:

In regard to the provisions you sent out to be distributed by Mr. Wasson was all distributed at Reese River with the exception of three sacks that I brought with me as far as Roberts Creek. It was left in charge of O.L.M. Agents to distribute it as if had ought to be or not. (Butterfield to Nye, January 24, 1862, in TERR 1862).

The irony therefore is poignant; having trampled, destroyed, usurped, subverted, wasted, and disrupted ecosystems and resources, the Overland Company was now being patted on the back for doling out a meal here and there to starving Newe.

#### *The "Indian Wars"*

In June 1860, the pony express rider between Deep Creek and Willow Springs was killed. Attacks again near Willow Springs station a year later in June 1861 resulted in a rider being badly wounded and losing his horse (Burton 1862, 560, 561), but three Goshute were killed in the process. The victorious station tenders scalped them. On July 3, 1860, two pony express men met an ambush in Ruby Valley (Settle and Settle 1955,

157, 158), perhaps by Shoshones captained by Tosawih (‘‘White Knife’’) leader Buck (Crum 1994, 22), who would later become quite prominent when the U.S. military appointed him ‘‘chief’’ in 1863. But in 1860 he was not the intruders’ favorite person; he was called a ‘‘troublesome Indian’’ by the Indian agent and in December 1860 threatened the Roberts Creek stage station. Cho-kup, another leader, intervened and calmed everyone down (Madsen 1985, 137), although initial reports said that the station had been actually attacked and the Ruby Valley station threatened (Nye 1862, 362; U.S. House of Representatives 1862, 359–366).

In September 1860, Indians successfully drove off all the horses from the Antelope Springs station (Settle and Settle 1955, 158, 159). Although Burton (1862, 565) found the Antelope Springs station burned to the ground, stages and riders still stopped there for pasturage, water, and firewood. Egan’s station was also burned in early October 1860, and the Butte station<sup>5</sup> was attacked after a retaliatory action by U.S. military had killed 18 Indians when a group of about 80 had showed up at the station demanding flour, bacon, and sugar (Fisher 1948, cited in Nevada Bureau of Land Management 1976, 52). The Egan Canyon confrontation was said to have been initiated by Paiutes, but in October, the military announced the killing of 25 Goshutes in retaliation. The Simpson’s Park station was burned but rebuilt (Burton 1862, 589; Madsen 1985, 122–125). Altogether, the Pony Express was said to have lost 150 horses and had seven stations burnt and 16 men killed (Settle and Settle 1955, 160; cf. Nevada Bureau of Land Management 1976, 56, 61). In retaliation, volunteers who were organized in California to prevent pro-anti-Union sentiments from throwing the state to the Confederacy and who found themselves with nothing to do (there was no insurgency anywhere in the west) were pressed into service to put an end to Indian attacks along the mail and stage routes once and for all. Under the command of Colonel Patrick Connor (also Conner), they marched out of Stockton, California, on July 12, 1862, and continued east, eventually taking command of the military district of Utah, which included Nevada. In June, three emigrant trains reportedly were waylaid by the Shoshones near Soda Springs, Idaho. Three white men driving two wagons reportedly were killed on the emigrant road north of Great Salt Lake (U.S. House of Representatives 1862, 349).

In September, an emigrant wagon train was attacked by Indians and 12 emigrants were killed at Gravelly Ford on the Humboldt Road. Based on reports in early 1861 that the ‘‘White Knives, occupying the country between the Upper Humboldt and the present mail road; and also from the Gose Utes, who reside east of Ruby Valley’’ (Madsen 1985, 167) planned to interrupt the mail and telegraph lines, the attack was blamed on Shoshones. Connor marched to Ruby Valley, where the Army was constructing Fort Ruby, and imple-

mented a plan to punish the perpetrators. His men were ‘‘spoiling for action’’ and Connor gave it to them: he ordered his men to ‘‘destroy every male Indian whom you may encounter in the vicinity of the late massacres’’ (Madsen 1985, 167). In Ruby Valley, Connor offered 50 dollars for each Indian implicated in the Gravelly Ford attack (Madsen 1985, 165–169). Marching on to Salt Lake City, Connor heard in January 1863 about couriers having been killed on the road between Cache Valley and Salt Lake. There was a report that miners had been killed in southern Idaho. Eight men with wagons and stock traveling the road through Cache Valley made a wrong turn and ended up on the Bear River, near an Indian camp. The Indians treated the men ‘‘uncourteously’’ and drove off their stock; the men reacted and resecured some of the stock, but in the skirmish, one of the wagoners was killed. Connor decided this was his moment. He decided to attack the Indian camp on Bear River secretly. He attacked at 6 a.m. on January 29. The unsuspecting Indians, all members of Sanpitz’ band of Northwestern Shoshones, were taken completely by surprise. Twenty-three of Connor’s troops were killed in the incident, but anywhere between 210 and 368 Indians were obliterated, perhaps as many as 70% of them women and children, although estimates vary widely because the Indian dead were left where they had fallen on the cold ground; the official body count was 224 (Madsen 1985, 178–194, 199). It was a massacre. The entire series of incidents leading up to the massacre are certainly attributable to non-Indian intrusion and to the consequent destruction of resources. One result of these events was the negotiation of several treaties, among them the Treaty of Ruby Valley with the ‘‘Western Bands of the Shoshone Nation’’ in 1863 (Kappler 1904, 851–853).

Following negotiation of the treaty, the area experienced increasing encroachment in the form of homesteading. Although the Pony Express was bankrupt by 1862, the stage company continued to run its stages through Ruby Valley and on the Central Route, upping the frequency of service. In 1865, the Overland stage station personnel began farming 1,000 acres with 90 oxen hitched up to 30 plows. The harvest was reported as 17,322 bushels of grain and 4,587 bushels of root crops. The stage station farm amounted to a homestead; all the company’s food supplies were raised on that farm until 1869. In 1867 a grist mill and a saw mill were constructed (Angel 1958 [1881] 106, 390). Thus, within 4 years of the Treaty being negotiated in Ruby Valley, grains were being grown there on a regular basis and timber was being harvested nearby. By 1868, 1,800 acres were under cultivation in Ruby Valley, mostly in grain (ARSGN 1861–1869, 126, 127; McElrath 1970, 85), and in subsequent years, the acreages ‘‘planted to grain’’ only increased (Sharp n.d., n.p.). Between 1866, when the first Texas longhorns were brought into the Humboldt drainage (Morgan 1943, 311), and the turn of the century, the number of cattle in Ruby Valley burgeoned into the thousands, with ‘‘most ranchers’’ running ‘‘around a thousand head or more’’ (Sharp n.d., n.p.). How much room

5. Popularly known as ‘‘Robber’s Roost’’ (Burton 1862, 568; Settle and Settle 1955, 141)

would there have been for native *yomba*, *wada*, sunflower, mariposa lily, or serviceberry to flourish amid domestic barley, oats, and timothy?

### *Life in the Valley*

One of the stage drivers, Ira Wines, also settled in Ruby Valley. Wines and “built a log cabin on his homestead and started raising grain for the stage horses” in spring 1866. He “planted grain and in the fall harvested his first crop.” He “tramped it out with oxen and hired squaws to fan out the chaff. . . . The Indians told him of wonderful grass” in Newark Valley “so he took some Indians with him, also scythes and rakes and driving a bull team went over there to harvest hay to sell to the Overland Stage” (Sharp n.d., n.p.). Later he either bought or homesteaded the Overland farm after it was abandoned in 1869 and built a flour mill there that used water from Overland Creek. Wines opened a general store, and after mines opened up in Eureka and Spruce Mountain, freight teams loaded up flour, bran, and “shorts” (by-products of the wheat). He accepted beef on the hoof from ranchers in exchange for grain.

Wines’s daughter Florence Sharp remembered “a large camp” of “many Indians” “about a mile” from the Overland ranch (Sharp n.d., n.p.). Aside from hiring “squaws” at harvest time, Wines seems to have had little interaction with them, although ostensibly he “was a great Friend of the Indians and acted as counselor for them when they had trouble among themselves,” threatening to call out troops from Fort Halleck, which replaced Fort Ruby in 1867, “if he had any difficulty settling any argument” (Sharp n.d., n.p.). After his first year as a homesteader, Wines does not seem to have hired Indians in any capacity. He hired a threshing crew every year with a “crew of men” and a machine operated by “five spans of horses” (Sharp n.d., n.p.). Whether the crew might have included Indians is not mentioned in Sharp’s memoir. For domestic chores, Wines hired a “Chinaman” who was cheap labor because he “would cook, wash and iron, chop wood, cut their meat, feed the poultry and pigs and in summer raise a fine garden” for a dollar a day (Sharp n.d., n.p.). In 1882 J. R. Bowman (1882–1883, 197) reported Shoshones being employed on ranches and “the squaws . . . employed in the plainest kinds of housework . . . during the last 15 years.”

### *Steward’s Ethnography*

Steward’s primary consultant for Ruby Valley was one of the few Shoshones born early enough to accurately report on the period before the substantial penetration of the area by non-Indians. Steward (1938, 144, 145) assessed the area as “exceptionally fertile” with “an exceptionally dense population” and larger villages. “Though the census data are incomplete,” he stated, “the northern two-thirds of the valley for which population estimates are available had about 420 persons in 1,200 square miles, or one person to 2.8 square miles. This

omits the inhabitants of one village, who were not known but who, if included would give a denser population,” and even Steward regarded his list of villages as incomplete. He references Simpson’s estimate of 1,500 Indians there “in 1859” but regarded it as “a temporary massing of the population from a considerable area during the war, for it would be more than one person per square mile, which is unbelievably dense for native times” (Steward 1938, 144). But what “war” was Steward referencing? The so-called Pyramid Lake War in the western part of the state that was matched by actions against the Pony Express and stage stations in the eastern part of the state did not begin until 1860. Moreover, Simpson was referring to an estimate communicated to him the previous year—in 1858—by a state company employee. Steward obviously missed that fact.

Steward (1938, 145–147) documented the links that people in five additional villages outside the valley had to settlements within it through participation in rabbit drives, festivals, and marriages. “As population was relatively dense . . . and as food distribution permitted clusters of some size, village control of a minor sort was necessary. Hence each village had a chief,” and “chieftainship was preferably patrilineal” (Steward 1938, 148). RVJ told him that people obtained horses around 1860 but usually ate them. RVJ (possibly “Ruby Valley Jim”) may have been the same person as the “Lazy Jim” who testified in Senate hearings in 1932. Born in 1840 or 1845 (Steward 1941, 214), he was Steward’s oldest consultant and perhaps the only of his consultants who truly remembered life before non-Indian intrusion. His testimony in congressional hearings in 1932 that at the time of the signing of the Treaty in 1863 he had one son (U.S. Senate 1934, 14854) squares nicely with the probability of his being born in the early 1840s. When he was 8–10 years old, he saw his first white man driving a wagon past where he hid in the willows. The white man “have wool on face . . . not soldier . . . From then they come every year. Saw first white man in fall of year . . . He come wagon,” he told the Senators through an interpreter (U.S. Senate 1934, 14856). But even this encounter could have been as early as 1846, when some emigrants were taking the Hastings Cutoff. The fact that they came “every year” after that indicates that what Jim saw was an emigrant wagon; after 1845 emigrants using the Hastings Guide and the Jefferson Map (Morgan 1963, 238–244) would specifically head for Ruby Valley with expectations of finding good resources for their taking. Thus, it is not certain that Steward was getting data on a truly aboriginal way of life and use of resources even for Ruby Valley.

## The Industrial Revolution and Urban Blight Invade the Sagebrush and Piñons

By 1875 silver, gold, and copper mines and mills were operating at numerous locations in eastern Nevada (fig. 3). The populations of these areas fluctuated tremendously with the

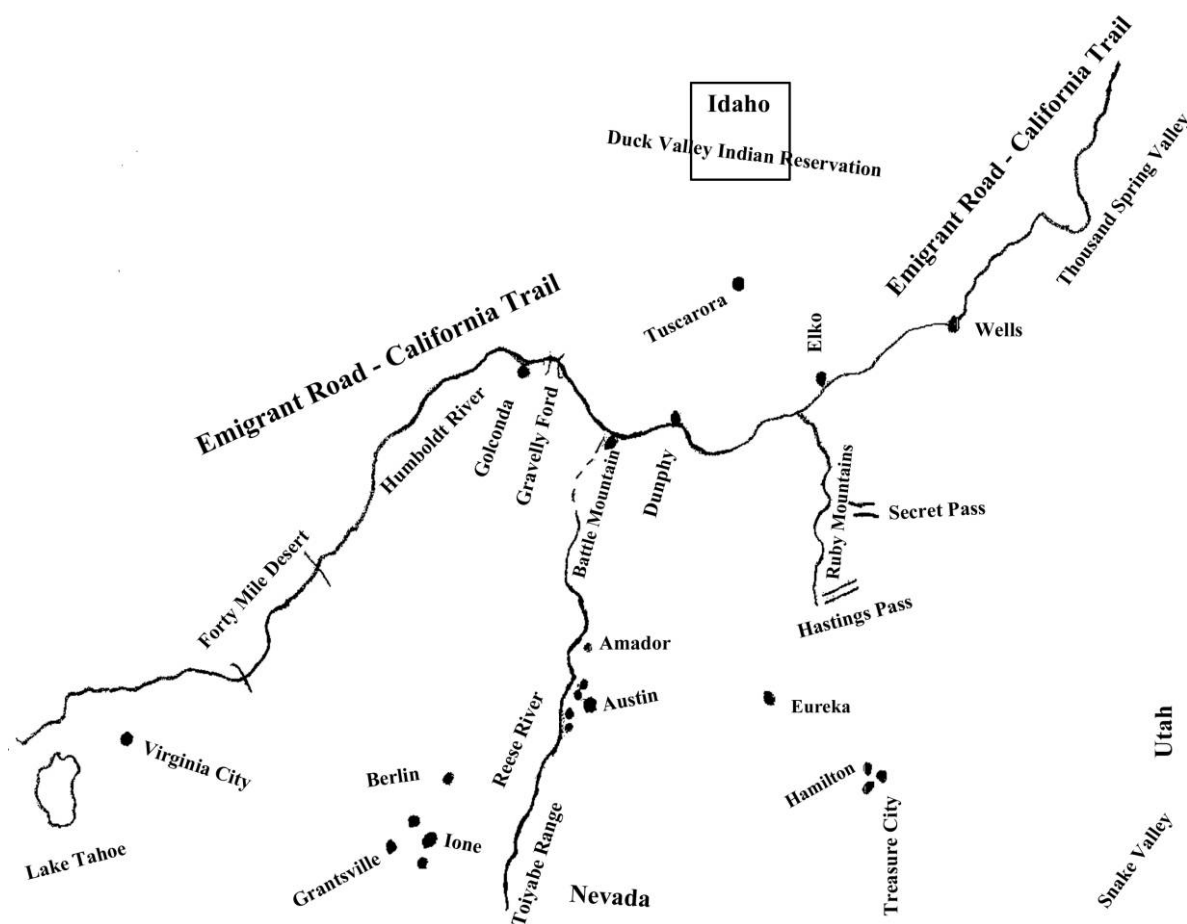


Figure 3. California emigrant road, 1846–1869, and urban settlements, 1861–1880.

discovery of new strikes. At various times during this period, the population in areas around Austin, Hamilton, Eureka, and Tuscarora swelled to between 10,000 and 40,000. Although most claims were not worked, more than 20,000 claims were staked in eastern Nevada in the 1860s and 1870s (Jackson 1963, 19; Kane 1970, 161; Lincoln 1970, 31; McElrath 1970, 85–87; Paher 1970, 167; Smith 1970, 79).

#### *Boom at Reese River*

Prospectors in the eastern parts of Nevada looked in vain for a big strike until 1862, when they found silver in the Toiyabe range just where the Overland stage route crossed the Reese River. Local legend has the discovery “born when a Pony Express pony kick[ed] over a rock” (Welcome to Austin n.d., 1), but of course the “Pony” had stopped running in 1861. Instead, the discovery began with an ex-Pony Express rider now manning a stage station who was among the many men on the lookout for ore-bearing rock. He found one, sent it to Virginia City for assaying, and, with some partners, staked a claim. Once the results of the assay were known, miners

rushed from the gold fields of California and from the Comstock Lode at Virginia City to the new strike. Eventually a reported \$50 million in bullion was taken from Austin mines (Welcome to Austin n.d., 4).

The strike’s auspicious location resulted in an instant boom. Thousands arrived in the bitter cold of January 1863. The spring thaw brought train after train of freight wagons that pounded the wagon road into powder as they brought thousands of dollars worth of supplies from Sacramento. One traveler counted the arrival in 1 day of 274 freight trains, 19 passenger wagons, 69 individuals on horses and mules, and 31 more trudging along on foot. One entrepreneur tried using camels, imported and used unsuccessfully by the U.S. Army in the Arizona Territory during the Civil War, in a commercial transport venture. In a few months, the city of Austin arose, straddling the road and eventually becoming the seat of a huge county that stretched to the Utah border.

Four thousand people were said to have descended on the area in that year. The number of ticketed stage passengers was reported at 5,840; at one time, stages ran from Austin to

19 stops to the north and to the south. Freighters who could keep their vehicles moving and who could carve out new roads that would cut travel time could turn handsome profits (Jackson 1963, 38–43). At its height, probably between 1865 and 1868, the population might have reached anywhere between 6,000 and 8,000 (Lewis 1955, 7–12, 15–17), and promoters Thompson and West (Angel 1958 [1881], 465, 469) optimistically insisted the population peaked at 10,000. Reportedly, 366 houses went up in the boom year of 1863, in addition to innumerable tents and shanties. By 1869 the Reese River District had registered over 6,000 mining claims, although only 62 had produced any significant amounts of ore (Raymond 1870, 128–131). Below Austin, a number of other shantytowns sprawled on the floor of Reese River Valley. To the south was Jacobsville, at the site of the old Pony Express station, with 300–400 people and the county seat until 1864. Just to the north were Amador and Coral City, with a combined population of 1,500. On the west side of the Toiyabe Range along Reese River and to the north of Austin was Clifton, which was short-lived; to the south was a camp at the confluence of Reese River and Big Creek, and there was a town alternately called Reese River or Canyon City, which had a hotel, two restaurants, a butcher shop, three saloons, a post office, and several hundred residents. All of these boomtowns except Austin went bust by 1866 (Ashbaugh 1963, 220; Paher 1970, 160, 380).

Construction lumber and mining timbers used at Austin and vicinity consisted of two varieties: the first-class white pine brought from Virginia City and the second-class piñon pine, native to the area and known as Reese River lumber. Cords of wood were needed to heat stores and homes and also for making the steam that powered the mining and milling machinery. The irony of all this use of wood was that it made the city of Austin a fire waiting to happen, and in 1881, it did. A light summer breeze fanned the flames, and despite firefighters' efforts, more than 15 buildings were consumed. Many of them were never rebuilt.

Six mills were put up, although most of them did not operate for any length of time. Anywhere from one to four smelting furnaces were in operation at the same time in Austin through the 1880s, using ore shipped in by rail after construction of the Nevada Central Railroad. The narrow-gauge Nevada Central was completed in 1880, running 92 miles from its terminus with the Central Pacific at Battle Mountain to Clifton, now just a collection of derelict shacks, renamed Ledlie in honor of the contractor who built it in February 1880. A mile-long feeder line snaked up to the city of Austin, initially operating with mule power but eventually utilizing a steam engine (Lewis 1955, 190–197).

Trains stopped at 10 stations along the way. They transported eastern coal and the cheaper Rocky Mountain coal and flue dust for the mills; mining timbers and lagging for the mines; and baled hay, iron, lumber, and local produce such as potatoes for businesses and households. Trains carried milled ore out of Austin. From other mining towns such as

Galena, Silver Creek, and Helena the railroad carried base metals to the Austin furnaces for smelting. Thirty miles south of Austin, gold and silver were discovered in 1864, and several small settlements—Berlin, Union, and Grantsville—were established within a 5-mile radius of Ione. Ten miles west of Ione in the Mammoth Range, where an Indian had revealed a silver ledge to whites in 1863, the town of Ellsworth had sprung up by 1871, and a 10-stamp steam-powered mill with a furnace was operating, with several Indians employed to tend the pans, settlers, and concentrators (Ashbaugh 1963, 200, 223; Paher 1970, 380).

In the 1950s, journalist Oscar Lewis<sup>6</sup> combed the dead files of the *Reese River Reveille* as well as other newspapers to construct an anecdotal narrative of life in Austin during early mining years. Over the years, the *Reveille* editorial staff seems to have been very selective about what they decided to print, trying to put a happy face on the grueling realities of life in an isolated mining town and attempting to assure readers that Austin was indeed a nice quiet city, as civilized, proper, and familiar as any other little mid-American city. Nonetheless, Lewis (1955, 99) was able to find a description by a traveling correspondent for the *New York Tribune* from fall 1865 that probably typified Austin during its boom years:

At night the brilliantly lighted gambling saloons, with open fronts, are filled with a motley crowd. Women conduct the games at several monte tables, shuffling the cards and handling the piles of silver coins with the unruffled serenity of professional gamblers; while men of all classes “fight the tiger” with the usual earnestness of that fascinating pursuit.

The town Indians subsist by begging at the kitchens of residences, hotels, restaurants, and miners' cabins, and the majority of them pass their time sleeping in the sunshine or gambling at cards. With all, gambling is the chief aim of life—whether it be Indian poker or monte, by day on the street corners, or at night in their miserable wickiups, or playing marbles “for keeps” among themselves, or with small white boys

Some of the [Indian] men work at odd jobs, such as cutting firewood, scavenger work, washing dishes in restaurants or hotel kitchens, and other odd chores, while many of the women do rough washing for families and perform various easy menial services. The women are much more industrious than the men, but all their earnings go to their lords and masters, to furnish them with gambling capital. (*Reese River Reveille* quoted by Lewis 1955, 103)

Chopping wood, or selling firewood for use in private homes seems to have been a common source of income for Shoshones and Paiutes (Lewis 1955, 106). In some cases, firewood was traded for a cup of coffee and a few pieces of bread (Lewis 1955, 104). Wood was also sold for manufacturing into charcoal.

6. Not to be confused with the anthropologist with the same name.

### *Ecological Impacts of Mining and Smelting*

In 1865 alone, 1,200,000 board feet of lumber were milled from Reese River “nut pine” for everything from constructing miners’ shacks to building hotels, saloons, laundries and stores to shoring up mine tunnels (Angel 1958 [1881], 469; Lewis 1955, 7–12, 15–17). It was also used to make charcoal. Charcoal was manufactured locally along the Nevada Central rail line and transported to Austin for use in smelters; wood was also cut along the line, transported at \$2.50–\$4 per cord (Angel 1881, 284), and sold for \$10 per cord. Nut pine was the most desired fuel (Raymond 1870, 124).

Charcoal fueled the smelters. Local historian Victor Goodwin (1966, 15) gives a good description of how charcoal was made:

The pinyons were cut during the winter months, when the sap was down, by Italian wood cutters—or Nova Scotians, around Bruffey Canyon—bucked into five-foot bolts, and left to season until the summer months. When summer came, the carbonari scraped off level places at intervals along the canyon bottoms, and on these built circular tiers of the five-foot pinyon cordwood lengths. . . . Each of these stacks was known as a “pit.” . . . After the pit was completed, the charcoal maker would drop blazing coals and kindling down the center hole. When the fire within the stack was burning well, the center opening would then be plugged at the top. It took around 30 days to reduced one of these pits to charcoal.

Just how much these activities impacted indigenous food-gathering is difficult to assess; a Paiute man, Sam, who was employed as a street cleaner in Austin told a reporter for the *Reese River Reveille* that following a major flood that demolished much of the city in 1873 because there was no longer a street to clean and thus no job for him, he would “attend the festivities of the pine-nut harvest and ‘heap catchum pine-nut’” (Lewis 1955, 219).

Of course, even the most intensive felling of piñon could not demolish all stands of it. However, whether or not Sam would find pine nuts was not just a matter of finding one of the remaining stands in the Toiyabe Range or elsewhere. A nut-bearing cone takes 2 years to mature, so any one tree can produce nuts once every 2 years. Because production of edible nuts is also dependent upon rainfall and other climatic variables, hardly any single tree produces a significant crop reliably every 2 years (Kirk 1970, 220). Steward (1938, 27) noted that “each tree yields but once in 3 or 4 years. In some years there is a good crop throughout the area, in some years virtually none. In other years, some localities yield nuts but others do not.” Ronald Lanner (1981, 70), author of the only lengthy discussion of the piñon pine, quoted Indian agent Levi Gheen in conversation with white homesteaders in Snake Valley in 1875 in illustration of this point: “For the past four years there have been no pine nuts in this country. This year

there is a great plenty and as has been the custom in former years when the pine nuts were ripe, word was went out to all the Indians to gather in parties in the mountains and have their dance and Pine-Nut Feast.”

Archaeologist David Hurst Thomas (1969, 1971, 1972, 1973), using computer simulation, postulated that Shoshones could reliably expect a harvestable crop of nuts in any particular area only once every 7 years on average. And obviously, the fewer trees there were, the less likely it was that a good crop could be found.

### *Steward’s Ethnography*

Steward recorded 25 sites of villages in the Reese River Valley and an additional 16 “winter sites . . . located at sources of water along the eastern slope of the Shoshoni Mountains.” Subsistence practices included sowing wild seeds; harvesting pine nuts—“of outstanding importance”—digging nine different roots; picking serviceberries, gooseberries, currants, and chokecherries; and communal antelope and rabbit hunts organized by specific leaders (Steward 1938, 101–105). Seed plots were burned first before they were sown, and they were owned by families, inherited patrilineally, and protected by village suzerainty. Steward (1938, 107) mentioned Tutuwa, a leader throughout the late nineteenth and early twentieth centuries, as director of festivals, celebrated in the fall when the pine-nut harvest was good and at other times, usually spring, when there was a good seed or root harvest or successful pronghorn antelope hunt. “Austin,” said Steward (1938, 107) “has long been a center of Shoshone population from a large area.” Steward drew on data from an Indian agent’s report from 1862 and also from Powell and Ingalls’s (1872) report. Steward’s primary consultants were TH, “born ca. 1870 or 1875,” and MJ and her husband JF, also born 1870 or 1875 (Steward 1941, 213, 214). Clearly, these individuals were recalling a time well after the area had been drastically affected by boom towns, mines, and mills that not only brought thousands of people tramping all over the valley and nearby hills but also drastically affected availability of water, pine nuts, and other resources. They were reconstructing a life for Steward’s ethnography that was as much an adaptation to the demography and activities of the mining economy as it was to the natural flora, fauna, and terrain.

## Conclusion

### *Why the Western Shoshones?*

The influence of Steward’s Great Basin ethnography cannot be underestimated. For example, between 1998 and 2006, four heavily used textbooks<sup>7</sup> presented Steward’s Western Shosho-

7. For example, Haviland 1999; Keesing and Strathern 1998; Schultz and Lavenda 2001; Scupin 2006.



nes as an example of the primordial hunters and foragers who eked out a living in small family groups, constantly roaming in search of windfalls and cornucopias as well as the tiniest bits of edible food. Archaeologist Lewis Binford (1962, 218, 219) initially cited Steward's (1955, 36–40) "theory and concept of cultural ecology" in his nascent call for grounding "archaeology as anthropology" in the study of processes of environmental adaptation. Binford relied first on Lee (1968) and then later on the work of David Hurst Thomas (see Thomas 1974, 1975), which in turn relied heavily on Steward's ethnography in developing his foraging model (Binford 1980), which he based firmly on ethnographic analogy. Although Binford did not mention the Western Shoshones *per se*, it was the Western Shoshones whom Steward used to explicate that theory. Binford was clearly influenced by Steward's presentation of the Shoshone as an aboriginal society in proposing that "material items" functioning together with "behavioral elements," such as a social system and an ideology, would be reflected in the archaeological record.

Archaeologist Jesse Jennings (1957, 1968) assumed this ecological predetermination and sought to find it in the archaeological record. He projected Steward's ethnographic picture of Great Basin peoples back nearly 10,000 years, suggesting an adaptation called the Desert Archaic Culture that superseded the big-game-hunting adaptation as represented in the Clovis and Folsom archaeological traditions. Archaeologist David Hurst Thomas (1969, 1971, 1972, 1973) initially also found support for Steward's model of resource exploitation and settlement patterns, but at almost the same time, Jennings (1973) abandoned the Desert Culture concept. Later Thomas (1982, 162) decided that Steward indeed had not sufficiently taken lakeshore resources into account in developing his overall model, especially criticizing the image of people on the brink of starvation as "the bias in the Basin." He attributed this image to Steward's own failure to take into account some of the data in his own ethnography in his 1955 "synthesis" (Steward 1955) and turning his "fundamental ethnographic description" of 1938 into "a general theoretical statement" 17 years later (Thomas 1982, 162). Rada Dyson-Hudson and Eric Smith used the Western Shoshone and the Owens Valley Paiute, documented by Steward (1938) and their own data on the Karimojong, to test a theory about the relationships of resource availability to economic defense of territories. A recent PhD dissertation noted Steward's work as uniformly understood to be a leading model in the discipline and often "recorded as 'foundational,'" "monumental," and a "value-free foundation for the study of indigenous societies" (Pinkoski 2008a, 179–181).

### *Victims of Progress*

Global economic forces and the extension of communication and transportation systems that satisfied entrepreneurial dreams of grandeur and nationalistic images of manifest des-

tiny were much stronger conditioners of Shoshones' adaptive strategies in the mid-nineteenth century than were the operation of ostensibly pristine environmental processes such as climate, geography, and the occurrence of indigenous flora and fauna. The ecological changes that were brought by the intrusion of trappers, emigrants, transportation corridors, and mining should prompt a reassessment of the image of the Western Shoshones as pristinely aboriginal foragers, subsisting in very small groups, eking out a living on scarce resources. In fact, they fit better with John Bodley's (1990, 4, 5) generalization about the impact of progress on tribal peoples:

In the mid-eighteenth century the industrial revolution launched the developing Western nations on an explosive growth in population and consumption called "progress," which led to an unprecedented assault on the world's tribal peoples and their resources. Within the 250 years since the world has been totally transformed, many self-sufficient tribal cultures have disappeared, and dramatic resource shortages and environmental disasters have materialized.

Unlike Wilmsen (1989; Wilmsen and Denbow 1990), I am not suggesting that Western Shoshone culture as documented by Steward represented a marginalized residue of people who formerly possessed a more powerful means of production such as iron technology or a different means of production such as animal husbandry. However, as Wilmsen convincingly argues for the !Kung, I am suggesting that Western Shoshones did formerly enjoy a more productive and abundant way of life, and along with the !Kung should not be taken as the prototype of pristinely aboriginal foragers eking out a living on scarce and undependable resources. Steward's own data, clearly from the contact era, contradict some of his generalizations that have become the hallmarks of how to characterize precontact desert-dwelling hunter-gatherer-foragers. Although it may well have been "physically impossible for families either to remain in one place for any considerable time or for more than a few families to remain in permanent association" (Steward 1938, 257), one wonders whether the impossibility to which Steward referred might have resulted from the intrusions of itinerant prospectors. It is difficult to reconcile defense of seed plots with lack of permanent association.

Undoubtedly the villages near water whose locations Steward plotted were occupied in precontact and postcontact times, but would habitation of those locations, as well as the degree of their permanence and the size of their populations, have been enhanced by the mining economy or prevented by it? The same water that Shoshones sought was also sought by the mining industry. No doubt "subsistence habits required in each region largely determined the size, nature and permanency of population aggregates" (Steward 1938, 257), but again, one must wonder whether the disappearance of large villages mentioned by Ogden along the Humboldt in 1828 and 1829 by the time Steward was doing his salvage ethnography was not due to the destruction of the beaver population and the subsequent devastation of most of the other naturally

occurring resources along the Humboldt Road 20–30 years later. The customary transhumance of the White Knives between the Snake River, the Humboldt River, Ruby Valley, and the Reese River Valley (Harris 1940, 39; Steward 1938, 167, 168) might well have been initiated as a result of resource depletion along the Humboldt.

### *The Power of Intellectual Authority*

The assumption that mobility patterns, population densities and aggregates, and resource availability among indigenous hunter-gatherer-foragers as documented by ethnographers should be enshrined and venerated in commemorative rituals of reference when a basis for generalizations is sought has more to do with the invention of traditions that undergird a discipline than with the search for trustworthy veracity. Indigenous peoples such as the Western Shoshones who do indeed maintain residency and cultural ties to specific locations within their aboriginal territories have done so under duress and often in defiance of government regulations, private industrial priorities, and a general attitude that they are rightly confined to reservations. The accuracy of Steward's ethnographic data may also be questioned. His biographer, Virginia Kerns (2003, 181, 182), not only reveals Steward's bias in having overlooked women's labor and thus implicitly presenting women as the "economic dependents of male providers" but also points out that Steward's fieldwork techniques in selecting only particular consultants whom he could interview conveniently (e.g., Kerns 2003, 193) could have resulted in some lopsided interpretations. Elmer Rusco's (1999) equally revealing assessment of Steward's fieldwork suggests that Steward simply could not be bothered with actually gathering data that might have led him to conclusions that did not square with his already-formed opinions.

However, it is more the absence of historical contextualization that is in question here. Had Steward provided that historical contextualization, the "ethnographic" Western Shoshones would have provided a much different model than the one with which they have been saddled. Bodley's (1990, 147) observation that progress in the late twentieth century was imposing "new strains on the ecosystems upon which (tribal peoples) must depend" surely applies to the historical situation of Western Shoshones. "The introduction of new technology, increased consumption . . . and the eradication of all traditional controls," observes Bodley (1990, 147), "combined to replace what . . . was a relatively stable balance between population and natural resources with a new system" that threw the system out of balance. The lack of attention to historical events that affected the Western Shoshones and their cultural ecology, which anthropologist Julian Steward studied so intensely and used to develop his "theory and method of cultural ecology," is especially disconcerting considering the impact that ecological studies have had on the discipline of anthropology. It also underscores anthropology's long-stand-

ing reluctance to engage the differential power relationships between indigenous peoples and dominant societies that result in many of those peoples having to put up with compromised ecologies and marginalized lives. This point is especially salient given the prominence that Steward gave to his theory and method of cultural ecology and to his "methodology of multilinear evolution" that framed his testimony as an expert witness for the U.S. Department of Justice in Indian Claims Commission proceedings (Clemmer 2004; Ronaasen, Clemmer, and Rudden 1999).

It might be asked why perpetuating the static picture of the ecologically hard-scrabbling Newe remains a component of "anthropological knowledge" that continues to exert intellectual authority within the discipline. What has motivated anthropology to so consistently reward reifications such as the "Appollonian Zuni" (Benedict 1934), the "fierce Yanomami" (Chagnon 1983; cf. Borofsky et al. 2005), or the famously bogus "gentle Tassaday" (Nance 1975)? The Western Shoshones might be much better presented within the discipline of anthropology not as a typical case of people eking out a living on scarce resources but rather as a prototype of indigenous people forced to adapt to the invasion of capitalist commerce and industry, subjected to environmental disasters, made to be refugees in their own land, cheated out of their treaty rights, and betrayed by the law.

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## Comments

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I have no doubt that the Western Shoshones were "victims of progress" in the sense that I earlier described the process as it operated at a global level, but did Steward not realize this, and did his ethnographic depictions and related theories ignore these facts? My reading of Steward suggests that he was not blind to the reality of Euro-American impact on the Shoshones and related peoples, but he viewed this impact in a rather positive light as inevitable, if not natural, "acculturation" and adaptation. His view of this acculturation process was also very much colored by his cultural ecology theory of cultural types and levels of integration. For Steward, what made the Western Shoshones special was his belief that they exemplified a "family level of sociocultural integration" (Steward 1955, 101–121). He felt that this cultural type was determined by how the Shoshones used their difficult "natural" environment in a way that restricted them to a low population density. Their dependence on irregularly available pine nut resources distinguished them from other low-density foraging

peoples who were able to form patrilineal bands because they regularly hunted large game.

Given Steward's detailed knowledge of Great Basin environmental conditions, it is indeed remarkable that he neglected to take historic ecological changes into account in his ethnographic reconstruction. This oversight makes sense if we consider the way his theory of acculturation distorted his treatment of the Euro-American invasion and conquest of Shoshone territory. He suggested that the "arrival" of the horse and "Whites" presented the Shoshones with the opportunity to raid "new resources" such as livestock and emigrant wagon trains, and this new opportunity encouraged them to form a new "cultural type"—the "predatory band." When the Shoshones were defeated militarily by the U.S. Army, (presumably as punishment for their raiding), they were "given reservations." According to Steward they apparently had an easier time "adapting" to their new conditions than other more complex indigenous groups because their social organization was so simple. As Steward explained, the Shoshones "did not have to experience the break-up of suprafamily-level institutions" (1955, 58). Steward described how the political transformation "wrought by the arrival of the White man" and the "White man's economy [which] made possible the amalgamation of formerly independent villages, and warfare, which was unknown in aboriginal days, provided a motive for banding together" (1937, 630).

By 1951 Steward continued to describe the "acculturation" of the Western Shoshones in equally benign terms as follows: "When white miners and ranchers entered their country a century ago, individual families readily attached themselves to white communities. When their native hunting and gathering resources were depleted, they worked for wages sufficient to maintain their very low standard of living. Later, they were given reservation, but these consisted of little more than small residence sites" (1955, 57).

This analysis all sounds rather coldly detached from the human realities of military conquest and certainly does not suggest, as Clemmer shows, that the Shoshones faced the unappealing choice between continued armed resistance and becoming impoverished dependents when invading trappers, farmers, ranchers, miners, and others rapidly usurped their most crucial resources. It is also likely that the Shoshones experienced severe depopulation due to introduced disease, in addition to the massacres that Clemmer describes.

Steward's seeming disregard of the coercive dimensions of "acculturation" as well as how the realities of conquest might have distorted his ethnography brings to mind the work of Steward's colleague Alfred Kroeber. It is also remarkable how Kroeber could maintain a similar icy objectivity in his *Handbook of the Indians of California* when he explained why he deliberately chose to omit all "accounts of the relations of the natives with the whites and of the events befalling them after such contact was established" (Kroeber 1925, VI). He explained that he was not prepared to do the necessary library work and had no predilections in that direction. It is shocking

that Kroeber could be so detached about this given that the Yurok, who he had studied in detail, experienced a 75% decline in population, from 2,500 to 610, between 1849 and 1910. This genocidal destruction was still in process when Kroeber was doing his ethnography (Buckley 1989). Kroeber, of course, was one of Steward's principal mentors, and it is perhaps not surprising that they may have shared similar orientations toward how to describe "aboriginal" culture. However, unlike Kroeber, we know that Steward was well acquainted with the ethnohistoric details, as demonstrated by his work on the Ute for the Indian Claims Commission.

Today we would be much less likely to gloss over harsh ethnohistoric realities, and the concept of "acculturation" is no longer fashionable. Most anthropologists now routinely acknowledge that almost all "natural" ecosystems are humanly modified to some degree, but it is crucial to distinguish which peoples do which modifications. It is too easy to use standard ecosystem maps to characterize the baseline physical environment of any cultural groups. The message here is that ethnographers need to be scrupulously careful to situate the peoples and cultures within their full ethnohistoric and environmental contexts.

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This article continues Clemmer's valuable reappraisal of Julian Steward's influential Great Basin research. While other scholars have identified specific flaws in Steward's Great Basin research, Clemmer raises a new one: Steward's treatment of mid-nineteenth century Western Shoshones (Newe) as pristinely aboriginal and subsequent anthropologists' acceptance of that depiction. Clemmer provides compelling evidence that Steward's Newe sources had lived during an era of nonnative intrusions that caused significant environmental impacts that altered native subsistence, settlement, and demography. Thus, it is unlikely that the data they gave to Steward reflected the precontact conditions that he depicted and that then became reified in anthropology.

Since I am neither a Great Basin expert or an ecologist, I must leave evaluation of those particulars in Clemmer's presentation to other commentators. But I do not find Clemmer's argument surprising or particularly controversial. Certainly, one of the important developments in anthropology and history since the 1960s is a greater awareness of how early and extensive the impacts of mercantile colonialism and capitalism were. We now have many studies that reveal that what previously were alleged to have been pre-colonial conditions were, in fact, outcomes of colonial processes (see, e.g., Wolf 1982). I do not say this to diminish what I see as an important contribution from Clemmer. I believe scholarship of the sort he provides here is an essential part of a living anthropology, and Clemmer's con-

tribution is all the more valuable because of the considerable influence of the work he critiques. That said, I think it is worth pointing out the irony that the historical ecology approach Clemmer employs so convincingly has its roots in the melding of Steward's cultural ecology with the more history-conscious studies performed under Steward's direction at Columbia University. Such classic works as Murphy and Steward (1956) and Sidney Mintz's and Eric Wolf's work in Steward's Puerto Rico Project helped position the study of mercantile and capitalist power more centrally within anthropology, facilitating the critique that Clemmer carries out. From this perspective, Steward is a transitional figure whose importance does not hinge upon the aspect of his Western Shoshone research addressed here. Clemmer, then, is building upon Steward's foundation rather than diminishing it.

I do have one minor quibble. While I think that Clemmer offers a useful reminder of the value of revisiting our cherished classics with an open mind, I would caution against substituting one flawed anthropological invention for another. The appeal of Bodley's "victims of progress" concept derives from the way it evokes and renews the old yet durable invented tradition of a savage—civilized dichotomy and the primitivist and progressivist philosophies of history derived equally from it (Lovejoy and Boas 1935). Although this dichotomy helped give birth to anthropology (Adams 1998; Pearce 1988), it has always presented a flawed model of human variation even in its newer iterations (Roseberry and O'Brien 1991; Trouillot 2003). Bodley's (2001, 16–19) dismissal of criticism does not address these deeper limitations, leaving his victims of progress concept vulnerable to at least some charges of romanticism. Clemmer's characterization of a Newe pre-colonial past rests on Ogden's 1828–29 journals. Whereas Clemmer uses this source effectively to show that the nineteenth century was traumatic for the Newe, Ogden's observations do not tell us how long the larger villages and conditions of abundance had existed along the Humboldt or what other kinds of traumas the Newe living there (or their predecessors) might have experienced over the vastness of pre-colonial time. Aside from this minor qualification, Clemmer has performed a notable service by reinvestigating the historical context of Steward's Newe data.

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I read this paper primarily as an archaeologist who makes extensive use of hunter-gatherer ethnography and who follows a paradigm that privileges ecology. As such, I appreciate being reminded of the potential pitfall of misinterpreting the ethnographic record out of ignorance of its historical context. This is especially important for cases where we cannot collect new ethnographic data, such as nineteenth-century Great Basin foragers. Clemmer's review of these three Western Sho-

shone cases is in the spirit of Ed Wilmsen's 1989 *Land Filled with Flies*, and Carmel Schrire's 1984 edited volume, *Past and Present in Hunter-Gatherer Studies*. Clemmer's paper, like these studies, criticizes those who assume that ethnography records behavior that is (only) a product of environment and subsistence, rather than (also) of native peoples coping with the social and environmental disruptions created by the incursion of colonial peoples.

But such warnings have been issued for the past 25 years, and frankly, I think we have learned the lesson. For example, in developing a baseline foraging model for the Carson Desert and Stillwater Mountains of western Nevada (Kelly 1985, 2001), I used only pre-1862 ethnographic data, since 1862 is when permanent Euro-American settlement began in earnest in that region. I also acknowledged that Ogden and other fur-trappers, and later California-bound immigrants, most likely decimated the game populations of the Humboldt River Valley to the north and provided an alternative in the form of draft animals. I used the ethnographic data to help construct a series of foraging models that could then be tested against archaeological data. David Zeanah made a similar (and better) effort for the same region (Zeanah 1996). Both of us sought models based on principles of human decision-making behavior vis-à-vis food resources. Yes, the late-nineteenth-century Shoshones had to cope with colonial powers, but they also had to forage, move about the landscape, choose places to live, make tools, and so on. We can use ethnographic information on such topics without assuming, as Clemmer apparently believes we do, that the Western Shoshones were "the prototype of pristine aboriginal foragers eking out a living on scarce and undependable resources." Archaeologists try to use ethnographic data as carefully as possible. Would Clemmer rather than we ignore them altogether?

The "victims-of-progress" perspective taken by Clemmer implies that the value of ethnographic data to anthropology is *only* to document the tragedy of the colonial clash of cultures. To accept that other uses produce erroneous results, Clemmer needs to show that the use of ethnographic data by ecologically minded anthropologists are wrong *because* the ethnographic data reflect the colonial clash rather than, say, food resource distribution and availability. For example, Clemmer notes that Dyson-Hudson and Smith (1978) made use of Steward's data on the Western Shoshone and Owen's Valley Paiute to test their theoretical model of territoriality. The implication is that they were wrong to do so because Steward's data are wrong. Clemmer suggests that territoriality might have instead been in response to the "intrusions of itinerant prospectors." But he provides no supporting evidence. While I appreciate Clemmer's warning, it would be more useful to show what portions of the ethnographic record primarily reflect the colonial clash rather than some other factor.

Yes, we must be careful in using ethnographic data, because those data partially reflect indigenous peoples coping with colonial powers. The data provide no stereotypes, nor even analogies. But we know that ethnographic data are best not

mined for analogies—a point made by Binford in the 1960s—but used cross-culturally to arrive at hypotheses about the principles of human behavior (Binford 2001; Kelly 1995). There are several valid perspectives for such studies, one of which is labeled “ecological.”

Finally, Clemmer asks why anthropology has a “long-standing reluctance to engage the differential power relationships between indigenous peoples and dominant societies that result in many of those peoples having to put up with compromised ecologies and marginalized lives.” I disagree; much cultural and even archaeological research over the past few decades addresses this important issue. And so I am left wondering how critiquing a long-dead anthropologist for data gathered some 80 years ago helps Clemmer achieve his laudable goal.

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Richard Clemmer provides a compelling reconstruction of the ecological damage done by intruders in three parts of Western Shoshone country in the nineteenth century. Julian Steward (1938) made passing references in *Basin-Plateau Aboriginal Socio-Political Groups* to settlers’ livestock, which grazed on the wild grasses that had formerly provided seeds for foragers. But as Clemmer shows, the damage caused by grazing livestock was just one chapter of the story. Trapping, mining, and other profit-making activities also contributed to the wholesale destruction of ecosystems in the three areas he profiles.

In a forthcoming book about Steward’s Great Basin fieldwork (Kerns, forthcoming), I cover some of the same territory. I share Clemmer’s views about the scale of environmental damage. It led to a swift and severe ecological crisis, not only for Western Shoshones but for native people throughout the entire region where Steward worked. They suffered the consequences of rapid settlement in myriad ways, and he saw the effects throughout the Great Basin in the mid-1930s. A question I ask, as Clemmer also does, is why he remained silent about the events that produced those changes.

Since my research route differed from Clemmer’s, it offers independent support for some of his major conclusions. Using unpublished records and other sources, I was able to identify most of Steward’s cultural informants by name, and to reconstruct at least the broad contours of their lives. The evidence of their individual lives, which I outline in the book, clearly demonstrates that they qualify as “victims of progress.” To borrow from Clemmer’s apt and eloquent closing words, they had all experienced “the invasion of capitalist commerce and industry” and “environmental disaster,” as well as repeated betrayals by government officials and courts of law.

Following diverse lines of evidence about individual lives—Steward’s and his wife’s as well as his cultural informants’—

also led me to conclusions that differ from Clemmer’s in certain respects. Some of the differences concern Steward’s field data and approach to fieldwork. It seems reasonable for us today to doubt that “victims of progress” make very good informants about a way of life that was largely destroyed. Yet several generations of American anthropologists engaged in so-called salvage ethnography, and they depended greatly on the memories of the survivors of “progress.” So did Steward. The evidence indicates that the men and women he questioned were recognized by other members of their communities as very knowledgeable. While they clearly were not “pristine aborigines,” most of the Western Shoshones and other elders he interviewed were highly credible informants; and the oldest among them had first-hand memories of the time before the invasion of their lands. Many elders had also listened to the memories of their parents and grandparents about that earlier time, and they passed on to Steward some of those memories. Much of the primary data that he recorded in *Basin-Plateau* was thus composed of the personal memories of living or posthumous informants, along with his own direct observations of the land, water sources, and some former village sites.

Although Steward did work with a few exceptionally old informants—some were well into their nineties—none of them lived in the three areas that Clemmer discusses. Steward’s oldest informant for Ruby Valley was BM. I learned that BM was Billy Mose, who was probably in his mid-eighties when he and Steward met in 1935. Born around 1850, he drew not only on his own memories but also on those of his father, Mose, who was born decades before the mid-century events that Clemmer recounts. Another informant for Ruby Valley, RVJ, was Ruby Valley Johnson. It appears unlikely that he was “Lazy Jim,” who had testified in Senate hearings in the early 1930s.

Steward’s particular goals as a salvage ethnographer led him to focus on the time before “progress” reached the Great Basin in the nineteenth century. Ethnographic data are always partial and generally skewed in ways that conform to the ethnographer’s research goals and theoretical perspective. Because of his behaviorist leanings, the field data Steward acquired for his ethnography centered on what cultural informants told him about observable activities and events of the distant past. (He was also recording data for Alfred L. Kroeber’s project, the Culture Element Distribution Survey, which required a broader field of vision.) It was not the case that Steward “could not be bothered with actually gathering” other data—but rather that he considered matters of cultural meaning to lie outside the realm of scientific inquiry (Kerns 2003), and most events of recent history to lie beyond his specific topic of inquiry.

Those who do not share Steward’s behaviorist premises may well regard not only his interpretations as “lopsided” but also his ethnographic data. They will likely also see the behaviorist approach, which leaves out so much of human experience, as dehumanizing in some obvious respects.

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One of the foundations of anthropology has always been fieldwork, with that detailed field investigation generating the empirical data from which theoretical explanations are developed. If, as we constantly tell students, we should constantly challenge our theoretical presumptions, then we should also periodically reconsider the classic ethnographic cases upon which those theories rest. In this article, Clemmer rightly invites us to reexamine the factual foundations of Julian Steward's sweeping theoretical construction.

Steward used his own Shoshone ethnography to characterize the family level, the simplest and lowest of his levels of sociocultural integration, within his grand scheme of multilinear cultural evolution (Steward 1955, 101–121). He had emulated the founders of both the American and British branches of anthropology who all selected to do their initial fieldwork with hunter-gatherers—Boas in 1883 with Central Eskimos, Radcliffe-Brown in 1906 with Andaman Islanders, and Malinowski in 1909 with native Australians (Boas 1964 [1888]; Malinowski 1963 [1913]; Radcliffe-Brown 1964 [1922]). This nonaccidental emphasis on hunter-gatherer ethnography made it then, as now, critical to locate these specific societies accurately within our intellectual framework.

Broadly conceived, Clemmer's article asks whether hunter-gatherers, such as the Shoshones, can legitimately be portrayed as having a fixed, aboriginal culture that lingers on the historical stage to be changed by outside influences or whether they are internally dynamic within their own cultural identity as hunter-gatherers. Of course, archaeologists working within traditional Shoshone country, as well as Eskimo territories and increasingly Australia, know the answer to this question, as their work clearly shows that none of these cultures were static in the pre-Euro-American period (reminding us, once again, why anthropology has four fields that should be included in our training, reading, and research, and in our problem conceptualization and theoretical construction).

Ethnohistory is a useful technique for filling in the empirical gap between the shallowest, controllable levels of archaeological excavation and the deepest reliable data of ethnographic recall. In recent years, ethnohistorical research of classic ethnographic areas has documented dramatic changes in native environments, cultures, social structures, and political organization that occurred upon Euro-American arrival (e.g., Lee and Guenther 1991; Oswalt 1979; Wilmsen 1989).

Even in the Great Basin where these historic changes were both late and rapid, they pose a problem for the reliability of ethnographic data. As I found in the case of Southern Paiutes, who adjoined Shoshones on the south and east, and as Clemmer found for Shoshones as well, anthropologists

arrived to do fieldwork for the foundation ethnographies more than a generation after drastic historic changes had already dramatically altered native life (Knack 2001, 367, n239). Those anthropologists were, more or less consciously, doing memory ethnography, rather than observing actively ongoing cultural behaviors.

One of the characteristics of Great Basin cultures has been their openness to rapid adoption of materials and cultural elements from other groups, whether native or nonnative. Steward himself casually included foxtail in his list of Shoshone ethnobotanical plants, and even casually noted it as an introduced species, but then continued to describe its subsistence uses along with its Shoshone name (Steward 1938, 23). In his description of Monitor Valley Shoshones, he recorded that the mining town of Belmont had become a center of a large Caucasian and Indian population after 1865 but found this relevant only in regard to rabbit drives (Steward 1938, 110).

Clemmer challenges Steward's representation of Shoshones as a pristine aboriginal culture eking out a living on scarce and undependable resources. Clemmer's data show that by 1935 when Steward arrived to do fieldwork, and indeed by the time of any reliable recall by even his oldest informants, Shoshone culture was hardly "pristine." However, his data do much to actually support Steward's second characterization of Shoshones as making do on resources that became even more scarce and more unreliable over time, even while they creatively incorporated newly introduced economic elements such as plants, animals, and wage labor.

It is curious that only the year before Julian Steward published his magnum opus on the structural evolution of cultures as described in the ethnographic present (Steward 1955), the journal *Ethnohistory* was publishing its first issue (Krech 1991). In the 55 years since, that journal has produced a steady output of fine-grained analyses of local changes in native cultures around the world during the period of and immediately subsequent to Euro-American impact.

More recently there has been a pressure to focus on the interaction between a larger context and local historical events, such as Clemmer is doing here (Schneider 1995). It then becomes logically necessary again to step away from detailed analysis of cases that occurred in a single time and place, to theoretical explanations of *why* those specific events occurred then and there. Such theories of sociohistoric processes must be placed in terms that are independent of particular case manifestations, so that those general explanations may be applied to cultures and instances occurring elsewhere and at other points in times. Theoreticians, from Kroeber (1969 [1944]) to Wolf (1982) to Jorgensen (1972; whose model was developed from Great Basin data, incidentally), have attempted to answer such grand questions. Solution to these issues remains before us.

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This article documents the pervasive effects of American expansion on the ecosystems and native peoples of the Great Basin. It effectively contrasts an account of these peoples that emphasizes history and intersocietal relations with the more closed and static view offered by Steward in his classical works on the Shoshones. This article thus serves as an important corrective and joins a large body of work on foragers that discusses their long, complex engagements with market economies and state societies or, phrased differently, with capitalism and colonialism. This work includes the debate on the San which has occupied many pages of this journal, starting with an article by Wilmsen and Denbow (1990); Grinker's work on the Pygmies of Central Africa (1994), and large bodies of research on aboriginal Australians and the Inuit.

However, there are two concerns that I would like to raise about the article: its reading of Steward's 1955 chapter is narrow, and its suggestion of an alternative in the notion of "victims of progress" is not wholly satisfactory.

The narrowness comes from its concentration on Steward's omission—certainly a serious one—of the effects of contact and invasion on the Shoshones and, correspondingly, from its neglect of Steward's intellectual ambitions in his 1955 chapter. The chapter's subtitle is "An Example of a Family Sociocultural Level of Integration." At the time he wrote, Steward had been developing the notion of the sociocultural level of integration—the idea that cultures could be located at one of a small number of levels of integration, particularly the village or band, the tribe, the chiefdom, and the state. To Steward, it was self-evident that humans lived within well-bounded cultural groups and that each culture had a higher or lower level of dominant political institutions. These concepts seem antiquated to the contemporary anthropologists, who are sensitive to the fluidity, rather than the firmness, of boundaries, and who are attentive to the connections among cultural groups.

Steward's assumptions, outdated as they may be, provide one component of his fascination with the Shoshones. (This fascination has other roots as well, including the crucial years of his youth, which he spent at Deep Springs Preparatory School in eastern California, where he developed an intimate familiarity with the Great Basin and an abiding concern for the native peoples of the region.) Steward believed that the concept of the level of sociocultural integration was strengthened by the documentation of the one extant group at the lowest level, the family; this group was the Western Shoshones. The uniquely harsh desert environment created a uniquely dispersed pattern of residence, movement, and resource collection.

Though the concept of level of sociocultural integration may seem simplistic and naive, it did perform useful service to anthropology at the time. It contributed to debates within ar-

chaeology and cultural anthropology on the rise of the state. There are significant echoes of Steward's notion of levels, and of the linkage of levels, in *Peasant Wars of the Twentieth Century* (Wolf 1969), *Caribbean Transformations* (Mintz 1974), and *Weapons of the Weak* (Scott 1985), works that devote to colonialism and capitalism the attention that Clemmer urges. More recently, the anthropological engagement with globalization draws on this notion as well, since it considers the emergence of a larger, worldwide level of integration—an idea suggested by Steward in his later work (Steward 1972). A form of this attention to colonialism and capitalism is present in a work of Steward's that appeared around the same time as *Theory of Culture Change*. "Tappers and Trappers: Parallel Process in Acculturation," (Murphy and Steward 1956) compares two native groups, the Brazilian Mundurucú and the Canadian Montagnais, who shift from autonomous subsistence to dependence on commodity trade (rubber tapping and fur trapping, respectively). This article continues Steward's assumptions about the boundedness of cultural groups and the existence of well-defined levels of sociocultural integration. These assumptions allow a thoughtful account of the history of these groups. Steward and Murphy examine tensions that the leaders in these two groups face as they are pulled between their ties to the local communities and the demands of the traders. They describe the loss of autonomous native leadership (at the village level in Brazil and the equivalent band level in Canada) as communities fragment into separate households, each dependent on its ties to an external trader.

Though the term of Bodley's that Clemmer uses approvingly, "victims of progress," might apply to these two groups, Murphy and Steward offered a more nuanced account. Rather than subsuming the particular groups into a broad category of passive victimhood, they showed the points in which native peoples choose engagement. Rather than subsuming the specific forms of commodity trade into a broad category of linear progress, they examined particular phases of market expansion. Clemmer has conducted a study of similar depth and nuance in his comparison of several different local groups of Shoshones, who encountered outsiders in different contexts and moments. Unfortunately, the potential richness of this comparison remains incompletely developed, because the lessons Clemmer draws from this material consist largely of a simplistic opposition between the failure of Steward's program and the success of Bodley's.

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I agree with Richard Clemmer who argues that Julian Steward's representation of Western Shoshone society is not supported by the data he collected nor by the circumstances in which they were produced. Steward (1976 [1941], 181) be-

lieved that “the lone family . . . was usually the maximum economic unit, and, rarely enjoying the company of other families, it was the only stable social unit being linked to other families only through loose kinship bonds.” The reasons for this in Steward’s view were ecological and economic: limited food resources that were spatially dispersed over the landscape. Clemmer and others have shown that the resources were more varied and less scarce than Steward supposed. Clemmer argues instead that Western Shoshone sociopolitical and economic organization Steward described was not primordial but rather the product of conditions created by genocidal and ethnocidal practices associated with the penetration of capitalist enterprises and massive immigration into the region in the mid-nineteenth century—the destruction of food resources by trappers, livestock grazing, mining, and roads, to name only a few. This articulation of capitalist and precapitalist modes of production also witnessed the dissolution of earlier practices and relations as well as the emergence of new forms (e.g., wage labor).

Let me elaborate a couple of points. Steward (e.g., 1938, 1951) typically employed a “building-block” model of society in which the family was the fundamental unit and other forms appeared as social complexity increased—the shift from mechanical to organic solidarity in Durkheim’s terms. He viewed households as primary units of production and consumption and did not, in my opinion, pay sufficient attention to activities, such as collective rabbit drives or annual winter ceremonies, that operated beyond the household level and that involved relations based on kinship, friendship, and the expectation of reciprocity at some future time. Participation in such activities reproduced the wider community demographically, socially, and culturally and gave meaning to the interdependence of the Shoshones. The importance of the ceremonies that brought the various camps and households together during the winter months is attested by the fact that they continued to held regularly even under the most dire of circumstances after the Basin peoples were enmeshed in capitalist social relations.

For Steward, as for Thomas Hobbes, the forms of alienation in capitalist society, which put one individual in competition with all others for scarce resources, was transhistorical and typical of all societies. While nuclear families have a certain kind of centrality and economic importance in capitalist society, they often do not have the same importance in precapitalist social formations. Thus, Steward’s assumptions about human nature and scarcity are not supported by his own data or by the accounts of other anthropologists who paid more attention to their social theoretical premises and to the sociohistorical contexts in which they worked.

A number of archaeologists appropriated and recycled Steward’s model of Great Basin society and its theoretical perspective in works that were broadly concerned with archaic societies and the transition from foraging to food-producing economies from Mesoamerica to the Andes. The problem with their approach is that they naturalized and treated as

transhistorical a form of sociopolitical organization that was arguably the creation of nineteenth-century capitalism and colonial expansion. As Matthew Spriggs (2008) has shown, the Americas are not the only place where ethnographic models derived from one region have been used to explain ancient societies known archaeologically in other regions. One should add that archaeologists are not the only practitioners of the human sciences who uncritically appropriate models and explanations from other fields of inquiry.

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In 1983 David Hurst Thomas wrote, “Despite the popularity of the Great Basin Shoshoneans in comparative studies, too few contemporary investigators bother to return to Steward’s actual data.” In this essay, Richard Clemmer states specifically that he will “refocus attention to Steward’s actual data and the historical circumstances that produced them.” In his discussion, Clemmer provides a compelling account of pressures placed on valuable resources used by indigenous peoples of the Great Basin—ultimately answering Thomas’s call. This is an important paper that describes the Great Basin and the colonial pressures endured by the Newe in detailed terms, complicating the history of the area and exploding any credibility that Julian Steward’s analyses might still retain.

Clemmer states that he wishes “to extend [a] critical re-examination of Steward’s presentation of the Western Shoshones” and to discuss their place “in the anthropological literature.” First, he debunks the representation of the Newe as a “pristine group;” rather, relying on Bodley’s notion of “victims of progress” to show their continuity and enduring agency against the affects of colonial penetration, he frames them as “reluctant participants” in colonial history. In this manner, Clemmer demonstrates that the Newe have been completely decontextualized from U.S. colonial history within the anthropological literature generally and in Steward’s work specifically. By providing a fascinating account of colonial penetration into Newe territory and detailing, in various degrees, the involvement of such institutions as the Hudson’s Bay Company, the Pony Express, the U.S. Army, and the Central Overland California and Pike’s Peak Express Company, he begins to fill this glaring gap in our knowledge.

However, this greater historical contextualization is not Clemmer’s only point; it leads him to conclude that Great Basin ethnographers’ descriptions are “enshrined and venerated in commemorative rituals of reference when a basis for generalizations are sought” which he says has more to do with framing the discipline than with any faithful representation of our history in North America. He concludes:



Indigenous peoples such as the Western Shoshones who do indeed maintain residency and cultural ties to specific locations within their aboriginal territories have done so under duress and often in defiance of government regulations, private industrial priorities, and a general attitude that they are rightly confined to reservations. . . . Had Steward provided that historical contextualization, the “ethnographic” Western Shoshones would have provided a much different model than the one which they have been saddled.

Clemmer’s contextualization includes information on a scorched earth policy, the culling and extermination of the beaver, the “obliteration” of hundreds of indigenous Americans in 1863, the overharvesting of trees and nuts, and the intrusion of multiple wagon trains through Newe territory. This is a critical demonstration that permits a stronger conclusion to be drawn than the Newe simply being victims of *progress*. These are issues of colonialism that are comparable to situations in Africa, Canada, and other parts of the world. Thus, while understanding the Newe as “victims of progress” undermines Steward, it permits the elision of our responsibilities as anthropologists and facilitates our complicity in continuing colonial projects in North America.

Clemmer’s information, contextualized within a colonial frame, calls upon the discipline (particularly in the United States) to interrogate its own historiography so that the ethical context of our work can be examined and reconsidered, particularly in light of our representation of indigenous peoples and the connection of these representations to colonial policies. I think that when we do, we will find that, as in the action anthropology of Sol Tax, we have a role to play in the just resolution of political relations between indigenous peoples and the state (e.g., S. Tax, unpublished manuscript), and then can possibly *begin* to decouple the relationship between anthropological theory and colonialism in North America.

Finally, it is clear that anthropology is a moral science and although Steward argued specifically that it is a neutral science, his student, Stanley Diamond, hammered him on this point, recognizing that “although Steward’s very decision for neutrality . . . is itself a decisive act that could have significant social consequences, our discipline offers neither proof nor assurances that one of these strategies is either more benign, more fecund, or more ‘scientific’ than the other” (Diamond in Manners 1973, 891; see Diamond 1968). Now we understand that Steward’s pronouncements are scientific only to the extent that he stripped them of the colonial context within which these data were situated and applied (Pinkoski 2008a, 2008b) and, as Clemmer’s analysis enforces, compel us to find a just place to stand in the political struggles within which we are embedded (Asch 2001; Hancock 2008)—and in doing so reclaim a path forged by Tax to stand with indigenous Americans against the injustices that entrapped them in the name of progress:

“I could not but share their dismay. What little I might do

to understand those bonds and perhaps to loosen them I had to do. But I soon learned that ultimate success or failure on my part for them, or theirs for themselves, was no measure of the pleasure of the effort, and so I have kept on supporting their long struggle” (Tax 1988, 15).

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Clemmer offers detailed evidence to confront the generally unquestioned assumption that the Shoshones were completely traditional and their environment pristine. Thereby he accomplishes what Steward neglected, ethnohistorical contextualization. Clemmer provides a great service to anthropology and, far more importantly, to the Shoshones, by challenging the primitive stereotype created by Steward and its replication to this day by many others.

At the same time, historians are careful to avoid the pitfalls of presentism; that is, judging the past on the basis of the present. Steward conducted fieldwork by relying largely on the memory culture of elderly informants with the aim of preserving a record of “traditional” culture for salvage ethnography following Franz Boas and the Boasians (Gruber 1970; Kerns 2003). It was only with Eric Wolf’s (1982) book *Europe and the People without History*, among other critical analyses, that anthropologists in general began to wake up to the larger political economy beyond the community level and to the forces of colonialism. However, Wolf’s book appeared 10 years after Steward’s death and about five decades after his fieldwork with the Shoshones. Accordingly, while Steward may be faulted in some ways for neglecting colonial impacts on the Shoshones and their habitat, more recent authors may be faulted even more who continue to depict such indigenous societies as the “primitive other” instead of people with a distinctive alternative lifestyle adapting to the challenges of their environment, society, and impinging external forces. For a similar example, see the ethnohistorical study of the regional political economy of the Yanomami by R. Brian Ferguson (1995).

There is certainly validity and utility in Clemmer’s analysis, as there is in Marc Pinkoski’s (2008a) critique of Steward’s role in the Indian Land Claims Commission, each revealing a significant yet neglected side of Steward. Both Clemmer and Pinkoski identify the evolutionism in Steward’s work, a bias that infects much anthropology to this day. Evolution is a scientific fact, whereas evolutionism can be an ideology serving colonial and other ulterior purposes. Evolutionism also feeds the “denial of coevalness” to use Johannes Fabian’s (1983) indictment of much of ethnography. That is, when indigenous peoples are misrepresented as “stone age survivors” then it is easier to pursue the illusion that we are not complicit in the colonialism and the world system that is

threatening their survival and well-being; and, consequently, some anthropologists feel free to pursue their egocentric career trajectories in the ivory tower complete with the latest theoretical fashions and fantasies to the neglect of indigenous realities and practicalities (Sponsel 2006a; Tierney 2001).

While there is plenty of room for the criticism of some aspects of Steward's work, and perhaps to some degree of his professional ethics as well, that does not diminish other aspects of his work that endure. There is far more to Steward's anthropology and cultural ecology than his Shoshone ethnography, and there is far more to ecological studies in anthropology than Steward. Yet undoubtedly Steward was the single most important pioneer in developing cultural ecology. Moreover, cultural ecology remains the foundation and core of subsequent anthropological approaches to human-environment interactions, such as historical ecology, political ecology, and spiritual ecology (Sponsel 2006b, 2007; Sutton and Anderson 2004).

In any case, Clemmer provides another clarion call to alert our profession to scrutinize assumptions about traditional cultures and pristine environments. Indeed, historical ecology would question to what degree and in what ways Shoshone science, technology, and activities may have impacted the natural resources and ecosystems in their habitat before the invasion of colonials, as for example, by fire use and management (Balee 1998). That could be a significant question as well.

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## Reply

Although it does indeed seem as if I am challenging Steward's ghost, it is not so much Steward that has been my concern. Rather, it is the degree to which our assumptions about the histories of indigenous peoples and especially their relationships to environmental resources might be influenced or compromised by flawed methodologies and assumptions from anthropology's past that continue to prowl around in the present. While the "opposition" of the concept of "pristine" aborigines against "victims of progress" might be a bit strained, Bodley, Kerns, Patterson, Pinkoski, and Sponsel have no problem with my characterizing Shoshones as victims of progress. Haley and Orlove do not like the term or my juxtaposition of that status against "pristine aborigines" as well as the concept itself, critiquing it as too simplistic. They point out that my historical-ecological approach owes a debt to the melding of Steward's cultural ecology with the "more history-conscious" approaches of Mintz and Wolf. Orlove points out the importance of the concept of "levels of sociocultural integration" in the Wolf-Mintz political-economic paradigm. I could not agree more.

So it is certainly ironic and lamentable that in 1955, after initiation of the "Puerto Rico Project" in which Wolf, Mintz,

and others cut their teeth on the colonial context within which Puerto Ricans utilized sociopolitical organization and structure to access material goods (Steward et al. 1956), and after coauthoring the "Tappers and Trappers" article with Murphy, Steward could not have revisited the Western Shoshones utilizing this reworked historical-ecological model. He could not have done so because although he had asked some of the right questions 20 years earlier, he had not asked enough of them. Instead, he was relegated basically to rehashing the more narrow "cultural ecology" paradigm that he used back in the 1930s and drawing attention only to the "family level" of sociocultural integration (despite doubts at the time that it even existed). It is even more lamentable, as Knack points out (and Patterson reinforces) that a close reading of *Basin-Plateau* reveals that Steward was well aware of the impact of mining activity around Belmont (and, I would add, Austin) on Shoshones' as well as non-Indians' settlement patterns and demography. It was not, as she points out, that Steward's ethnography was all wrong. In fact—unless Kerns's research turns up some unanticipated revelations—there was much of it that was all right. It was just incomplete and decontextualized.

It was Steward's failure, or refusal, to acknowledge the impact of nineteenth-century capitalism and colonial expansion as the context within which the "scarcity" that he thought was driving Shoshone behavior and the possible causal impact of that context that compromises the image of a "scarcity"-driven, "gastric"-compelled "aboriginal" culture. At the risk of overburdening the argument with just a little more data, I will give another example from the Treasure Hill–Hamilton area, just to the east of Austin and north of Belmont, which had become a collection point for Indians and non-Indians alike by 1869. During the decade of most intense mining, 1869–1878, the area supported 228 mines, most of them owned by companies chartered in San Francisco. On paper they boasted total operating capital of over a quarter-billion dollars (Raymond 1870, 150–173). In 1878 a San Francisco ore processing company acquired Illipah Spring and the nearby Eberhardt Mine, 3 miles east and down the hill from Treasure City on the edge of Railroad Valley, erecting steam pumping works that took 1,500,000 gallons out of the spring in a 24-hour period. At times, water flowed out of the rock at the rate of 2 million gallons per day in the dry season and 5 million per day in the early summer. The water was being used exclusively for milling (Jackson 1963, 141, 142, based on Angel 1958 [1881], 659, 660). The sustained use of this water would have surely reduced the flow of Illipah Creek to a trickle in the dry months, July through December. The plants producing important roots—wild carrot (*Perideridia*), wild parsnip (*Pastinaca*), valerian ("tobacco root"), cattail, bear grass root (*Xerophyllum tenax*), mariposa lily (Sego; *Calochortus*), broom rape root (*Orobancha*), cowparsnip (*Heraclium*), all standard foodstuffs sought by Shoshones—would have all but disappeared.

It was capitalization of mining companies, whether with

English or American money, and the dynamics of investment, capitalization, executive authority, and so on, in far away places such as San Francisco and London that resulted in the scarcity of root resources for the occupants of Railroad Valley, forcing Shoshones to find alternatives and to develop what must have been a mixed economy, continuing to hunt and forage but also seeking wage labor, however menial. Would it be useful to know how many, if any indigenous individuals were employed by the mine complex or by the miners themselves for household menial labor? Could Steward have found a few informants who might have known? Might Steward have asked his Indian informants about what they remembered about their interactions with non-Indians? Would a question about whether or not they had free access to Illipah Spring and its associated resources have placed the notion of “scarcity” into quite a different context from the one received from Steward? I think so.

Bodley targets the influence of Kroeber as well as Steward’s approach to acculturation (cf. Clemmer 1999; Rusco 1999) as the culprit lurking behind what must be regarded as an ethnographic project missing some of its parts. But revisiting the origins of “Steward’s model” (despite Thomas’s [1983] insistence that Steward did not have a model) requires more than merely “critiquing a long-dead anthropologist for data gathered some 80 years ago,” as Kelly puts it. The “model” had, in one form or another, a very long and influential life, being, as Patterson puts it, “recycled” largely by archaeologists throughout the 1940s, 1950s, and 1960s. Certainly the use of ethnographic analogy for developing propositions for interpreting the archaeological record was a methodological milestone. If the goal is to develop general models of “human decision-making behavior vis-à-vis food resources,” as Kelly avers, then yes, Shoshones did indeed have to make decisions about food resources within the colonial context. The question is just how much or how little they had to do this before the imposition of that context. Equally, Lee’s !Kung San might well have been utilizing the hundreds of resources of the Kalahari Desert in the same way that perhaps their ancestors or some other folk were doing 10,000 years ago. The issue is whether or not indigenous peoples are serving as analogies for archaeologists to develop behavioral “principles” or to understand human history and indigenous peoples’ participation in that history. It should be possible to do both.

Therefore it would seem to be incumbent on anthropology as a discipline and project to inquire about not only the origins and persistence of some long-standing assumptions, but also to ask, as Sponsel points out, how anthropologists have interfaced with the 30-year-long movement for indigenous peoples’ collective human rights. While anthropologists have had some involvement and even influence within this movement, that involvement was largely uninformed by anthropological theory and methodology, falling instead within political rubrics largely developed by NGOs. Thus, it seems worthwhile to examine pillars of anthropology such as Kroeber, Steward, Mead, Geertz, and others so that we can con-

textualize not only their work, but also our own within broader sociopolitical structures and processes that contextualized anthropology and continue to do so. Rediscovering even the “micro”-level contexts of fieldwork and interviewing, as well as early experiences, as Kerns is doing in finding Steward’s informants, can reveal some crucial assumptions that start out as personal inclinations and end up as unquestioned cornerstones of anthropological inquiry.

I agree with what Orlove and Haley seem to be implying, that it would be unwise to substitute one formulaic model for another. Steward’s behaviorist “cultural ecology” approach to Basin-Plateau cultures seems to make humans into knee-jerk responders to material conditions, despite the fact that Steward denied such a simplistic interpretation. In contrast, as Patterson points out, Shoshones seem to have exercised a good deal of agency in maintaining and even strengthening those customs that sustained solidarity in the face of the trying circumstances of capitalist intrusion. Yet Steward had trouble locating this agency, and therefore ignored it. Knack (1996, 2008) has initiated investigation of this kind of agency which is indeed part of the “richness of particular phases of the market economy,” as Orlove puts it, for the Great Basin, in the same way that Wolf and Mintz have done so for the world. I agree with Haley, Kelly, and Orlove that the challenge for a more reflective and nuanced anthropology surely is not to turn indigenous peoples from Stone Age survivors into mere victims of progress but to find the agency embedded in indigenous peoples’ institutions that they themselves developed and also in their engagement with the institutions of the market economy in the times and places that they had to do so. And to do that requires some attention to the behavior patterns taken up by the movers and shakers of those institutions of the market economy as well as to those of the peoples who are compelled to cut their way through them. It is a question of balancing attention to the modern-day equivalents of the Eberhardt Mining Company—the Rio Tintos, World Banks, and Shell Oils of the world—with attention to the behavior patterns of the local communities that are the usual focus of anthropological inquiry.

As a discipline, we seem reluctant to put a cultural face on history, to put a political-economic face on culture, and to wed the two, despite the models advanced by Wolf and Mintz and in the pages of *Ethnohistory* which, as Knack points out, has been around and publishing many such detailed studies for more than 50 years. Are we, as Sponsel suggests, indulging in career fantasies at the expense of documenting “indigenous realities and practicalities”? Is it significant that Miguel Alfonso Martínez, the United Nations’ special rapporteur for the Study on Treaties, Agreements, and Other Constructive Arrangements between States and Indigenous Populations exempted from his critique of anthropology “reflexive or post-modern anthropology” and *ethnohistorical* anthropology (Howard 2003, 207; italics mine)? It might be worthwhile to ponder the extent to which historical ecological events and capitalist intrusion sufficiently weakened or at least forced

postponement of an indigenous Western Shoshone movement to enforce treaty rights and gain real reparations such as restoration of land (rather than the monetary pittance offered by the Indian Claims Commission 100 years later; see Hall and Fenelon 2009, 146, 147; Ronaasen, Clemmer, and Rudden 1999). How many other indigenous groups might have come to the world's attention much earlier as peoples whose lives and cultures were under threat if the particular anthropologist that initially documented them (some under Steward's supervision as contributors to the *Handbook of South American Indians*) had taken a more political-economic, historic-ecologic approach? With the UN General Assembly's passage of the Declaration on the Rights of Indigenous Peoples (despite the United States' no vote on the measure), deprivation of indigenous peoples' resources has become an internationally actionable offense. Even though more than 70 years have passed since those situations affected the Newe of the 1860s and 1870s, reconstructing the ecological history that resulted in that deprivation may offer lessons about documenting and hopefully halting or reversing similar situations affecting indigenous peoples today.

—Richard O. Clemmer

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