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Huna Tlingit Traditional Environmental Knowledge, Conservation, and the Management of a "Wilderness" Park[1]

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A study of Huna Tlingit traditional gull-egg harvests in Glacier Bay National Park and Preserve, Alaska, indicates that local traditional environmental knowledge includes a sophisticated appreciation of glaucous-winged gull (*Larus glaucescens*) nesting biology and behavior—in particular, an understanding of this gull as an indeterminate layer with a modal clutch size of three. The community has applied knowledge to the design of sustainable egg-harvesting strategies. The dominant strategy is to take eggs from nests with one or two eggs but leave nests with three or more; an alternative strategy advocates partial harvests from three-egg clutches. The case study is related to a critical review of work questioning the contributions of traditional environmental knowledge to sustainable resource management, past and present. In particular we argue against a new orthodoxy that discounts the capacity of indigenous communities to conserve the natural resources of their lands.

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The value of traditional environmental knowledge as a basis for cooperative resource management engaging professional scientists employed by government land management agencies, on the one hand, and indigenous or traditional local communities, on the other, has been widely promoted in recent years (see Freeman and Carbyn 1988, Williams and Baines 1993, Stevens 1997, Feit 1998, Berkes 1999, Hunn 1999). Proponents argue that detailed, empirically validated knowledge of plants and animals and their roles within a local ecosystem is prerequisite to appreciating the impact of human harvests and designing sustainable resource management strategies. Two critiques of this approach are particularly prominent. One, which we characterize as postmodernist, sees the attempt to translate indigenous concepts in order to develop a dialogue with modern scientists and resource management bureaucrats as an extension of modernist hegemony. The other, which we identify with conservation biology, is dismissive of the possibility that indigenous, traditional, and/or small-scale subsistence communities might conserve their natural resources. It tends to define "conservation" strictly as practices designed to conserve biodiversity, either ignoring traditional environmental knowledge or judging it irrelevant to people's behavior with respect to environmental resources.

The Postmodernist Critique

Nadasdy (1999:2) asserts that efforts to integrate traditional environmental knowledge with modern science for comanagement are doomed to failure and may actually "be reinforcing, rather than breaking down, a number of Western cultural biases that in the end work against full community involvement in managing local land and wildlife." He argues that the fatal flaw of such efforts is that the terms "traditional," "environmental/ecological," and "knowledge" denote concepts alien to indigenous modes of understanding: " 'Traditional' has the effect of assuming that cultural practice is frozen at a particular point in time (usually the distant past)" (p. 4). Thus, opponents of applying traditional environmen-

tal knowledge to management decisions may argue that contemporary indigenous people have no traditional knowledge, given that they now use rifles, snow machines, and outboard motors. The term "environmental/ecological," he suggests, distorts indigenous perspectives because "implicit in their use is the notion that human beings are separate and distinct from the rest of the world" (p. 4). Finally, "knowledge" presupposes a "Western" Cartesian duality of mind versus matter and implies "knowledge as an abstract 'product' of the human intellect . . . completely separable from the cultural milieu that gives [it] meaning" (p. 5).

Nadasdy's solution is "the devolution of control over local land and resources to aboriginal communities themselves," thus relieving indigenous peoples "of the burden of having to express themselves in ways that are foreign to them to justify their views to scientists and bureaucrats" (p. 15). His critique expresses his profound distrust of the idea of effective translation of the conceptual realities of indigenous cultures into those of modern national bureaucracies. However, his "solution" imagines a world in which colonial occupation and settlement never occurred and nations willingly cede their sovereign power. In reality, indigenous communities everywhere must necessarily engage an encompassing polity that holds ultimate power. It would seem more in the interest of the survival of indigenous communities to foster a dialogue, however imperfect, with the professional scientists and resource managers entrusted by national governments with the "protection of our natural resources."

We question the assertion that the elemental terms of traditional environmental knowledge and terms such as "management" or "conservation" are "incommensurable" (Nadasdy 1999:2). For example, in the Sahaptin language of the Columbia River Basin (see Hunn and Selam 1990), "traditional" may be translated as *tamánwit*, "the Law," literally "that which is laid down," understood as principles of right conduct set forth by Coyote at the close of the Myth Age. Sahaptin-speaking Indians, as well as the great majority of anthropologists, agree that traditions are not a fixed set of directives frozen in the past but principles that guide behavior, even through the drastic changes wrought by Euro-American colonization (see Berkes 1999:3–15). Thus, contemporary Yakama, Warm Springs, and Umatilla Indians seek guidance from and find inspiration in this traditional Law both in their internal affairs and in their dealings with external forces.

A concept of "environment" is likewise explicitly recognized in Sahaptin. *Tiichám*, "land," is a close conceptual equivalent, though it is understood to refer to a landscape inhabited by animate brings with which people (*tanán-ma*) must maintain proper moral relations in perpetuity. To speak of the "land" by no means implies that people are separated from or opposed to the "land." Quite the contrary.

Finally, "knowledge" is *shúkat*, and in Sahaptin as in English it implies power. One's spirit ally is one's "knowledge." Without a systematic analysis of Sahaptin ethnoepistemology we cannot say precisely in what respects Sahaptin notions of "knowledge" differ from our own. However, to claim, as Nadasdy does, that "traditional knowledge is not really 'knowledge' at all in *the Western sense of the term*" (our italics) caricatures both "traditional" and "Western" knowledge (see Lakoff and Johnson 1999).

The Tlingit saying *aat yá ayunei*, "Respect everything provided by the Holy Spirit" (Herman Kitka, personal communication, 2002), is understood to require that one not harvest fish or deer, for example, in excess of one's needs. The Tlingit phrase *a daat khuyawdzitaakh*, "one cares for (or looks after) it," describes the responsibility of clan or house leaders (*hít s'áati*) to husband a salmon stream or other resource patch within the clan territory. In short, the relevance of traditional environmental knowledge for conservation is by no means alien to traditional or indigenous peoples' understanding. Of course, this is no guarantee that Western "experts" will properly appreciate the common ground beneath these indigenous conceptual systems and their own.

That some people misconstrue these terms or cynically manipulate them (see Hensel and Morrow 1998, Nadasdy n.d.) is not sufficient cause to abandon them. Nadasdy's critique affirms a strong form of the so-called Sapir-Whorf hypothesis of linguistic relativity. At the extreme, linguistic relativists deny the possibility of translation, abandoning each human community to live isolated in a solipsistic world that is the unique creation of its linguistic history. In our judgment, this extreme position is indefensible. We believe that those who study traditional environmental knowledge should strive to translate as faithfully as possible between the myriad indigenous and local systems of environmental understanding and modern scientific views of our common reality. We recognize that the successful integration of such knowledge with other forms of knowledge in cooperative resource management regimes is a political process in which indigenous peoples are at a distinct power disadvantage. We also recognize the danger in promoting traditional or indigenous knowledge taken out of context as guarantor of "sustainable development" (Agrawal 1995). However, we do not believe that resistance to recognizing indigenous knowledge and wisdom is reason to abandon attempts to promote its value, both intellectual and practical. We agree with Cruikshank (2001:391), who suggests that "local knowledge of the world . . . has more similarities with contemporary science than differences from it" and that "we need knowledge bridges that work from local concepts as well as from science if we are to bring broadly based human values to bear on problems" such as the conservation of biological and cultural diversity. Sillitoe (1998) makes a similar case. While recognizing "the pitfalls of ethnocentrism . . . in some indigenous-knowledge research" and the fact that "some scientists behave as if it were possible to pluck information . . . out of cultural context" (p. 228), he cautions against "the danger of taking the sociocultural embeddedness issue too far and producing ethnographic accounts which will strike scientists as esoteric records which they are unable to relate to their

work. There is a need to make the connections" (p. 229). We shall present a case study of Huna Tlingit environmental knowledge and resource use that illustrates these connections.

The Conservation-Biology Critique

A rather different challenge to the proposition that traditional environmental knowledge should play an integral role in contemporary resource management is the argument—now so widely accepted as to constitute a new orthodoxy—that indigenous ("traditional," "local," or "small-scale") communities do not, as a rule, practice "conservation" in relation to their natural environments but rather are by nature inclined to "overharvest" resources, limited only by their technological and demographic capacity (see Meilleur 1994). This conclusion has been widely promoted by Jared Diamond in a series of essays (e.g., 1986, 1988) and in his book *The Third Chimpanzee* (1992). More recently, the ethnohistorian Shepard Krech has elaborated this theme in *The Ecological Indian* (1999), in which he sets out to disabuse us of the myth of the Noble Savage. Diamond and Krech are at pains to demonstrate that indigenous peoples were quite as likely as modern peoples to degrade their natural environments, irrespective of the sophistication of their environmental knowledge. The orthodoxy of this view is suggested by the manner in which writers seek to establish their legitimacy by disclaiming any residual romanticism with regard to indigenous resource management. Fitzgibbon (1998:449), for example, opens her contribution to an influential recent collection entitled *Behavioral Ecology and Conservation Biology* with this disclaimer: "Although the idea of hunter-gatherers living in harmony with nature was popular among anthropologists in the 1960s and 1970s, this is no longer the prevailing view, and it is now clear that traditional societies often overharvest their prey."

This orthodoxy is morally, politically, and theoretically charged. The old orthodoxy is said to deny indigenous peoples agency, discounting their active and creative management of the natural environment. In contrast, the new orthodoxy may be seen as delegitimizing indigenous claims to traditional lands and livelihoods. Ideological opponents of neocolonialism and global capitalism may prefer to see indigenous communities and other small-scale subsistence societies as utopian alternatives to the mayhem of modernity. Meanwhile, as Ruttan and Borgerhoff Mulder (1999:622) note,

> Evolutionary ecologists question whether indigenous peoples are natural conservationists, their primary objection being theoretical. Individuals cannot be expected to limit present harvests of resources for the purpose of conserving them for future use if this behavior entails a cost. . . . The rationale of this critique is that such restraint is altruistic if the benefits are shared by all but the costs are borne individually.

We find this polarization of the debate unfortunate. We are particularly critical of the various ways in which proponents of the new orthodoxy have characterized the key term in the debate, "conservation." Described as altruistic, preservationist, and essentialist, "conservation" is difficult if not impossible to attain, and conservation biologists thus dismiss numerous examples of successful environmental stewardship by indigenous communities. Yet "conservation" in everyday English is a word rich with powerful moral connotations. "Conservation" is *good*; failure to conserve is *bad*. If the term is deprived of its normal content but retains its power to praise or condemn, there is danger of misunderstanding.

Krech's *The Ecological Indian* is hailed by Carolyn Merchant in the jacket copy as "a stunning, provocative reassessment of the image of the noble Indian living harmoniously within nature." In his attempt to rescue Native Americans from this stereotype, Krech argues that they more likely burned and pillaged nature to the limits of their abilities. He sets out to debunk a "myth," not to clarify a complex issue. In our judgment, the cases he offers are either (1) highly speculative reconstructions left hanging as likely possibilities, as in his discussions of the evidence for Pleistocene overkill and Hohokam self-destruction; (2) accounts of environmental mayhem promoted by colonial and neocolonial capitalist enterprise but blamed on the Indians caught up in this trade following the destruction of their traditional societies, as in his treatment of bison, white-tailed deer, beaver, and Native corporations; or (3) cases of sustainable indigenous subsistence practice misconstrued as destructive or anticonservationist, as in his analysis of Indian burning, Indian spiritual beliefs about animals, and contemporary Makah whaling. These cases prove nothing with respect to the authenticity of "the ecological Indian."

Charles Kay, in an article entitled "Aboriginal Overkill" (1994), argues that "Native Americans had no effective conservation practices, and the manner in which they harvested ungulates was the exact opposite of any predicted conservation strategy." He asserts that "Native Americans acted in ways that maximized their individual fitness regardless of the impact on the environment." This follows from his assumption that "for humans, conservation is seldom an evolutionarily stable strategy" (p. 359). Kay's evidence that elk, at least, were present in only limited numbers in much of western North America where they are now abundant seems convincing, and it is plausible that this was the consequence of heavy hunting pressure by Native Americans. However, to label a particular level of predation "overkill" presumes that we can define some optimal "natural" level of predation. While Indian elk hunting may indeed have suppressed the "natural" reproductive potential of that species, surviving elk populations proved more than adequate for millennia in the face of such hunting pressure. To call this "overkill" demonstrates Kay's preservationist bias.

Michael Alvard's (1998*a*) research with Amazonian hunters was carefully designed to distinguish optimal-

foraging strategies from conservationist ones. His data clearly indicate that the Diamante Piro of southeastern Peru do not employ a conservationist strategy in selecting prey. However, as he admits, "generations are often required for exploitive foraging decisions to deplete prey populations" (p. 492). One might add that prey populations may *never* be depleted by such "exploitive foraging" if the human population is maintained at a sufficiently low density. Smith and Wishnie (2000:509) cite evidence that subsistence hunters typically harvest but a small fraction of the available prey population surplus. If within the memory of the living (and the cultural memory of the community) a particular harvest strategy has no detectable impact on prey populations, why adopt the more costly conservationist strategy? Is one *not* a conservationist for not conserving a resource that is abundant relative to the demands placed on it? As Alvard recognizes, optimal-foraging predictions maximize short-term returns but say nothing of complementary long-term strategies that might achieve a conservationist outcome at the end of the day. For example, the strategy reported for contemporary Waswanipi Cree of rotating hunting grounds (Feit 1973), a hunter's analogue of fallow cycling of swidden fields, might allow for the long-term regeneration of prey populations depleted in the short term by an optimal-foraging strategy.

Smith and Wishnie (2000) argue that true "conservation" must be carefully distinguished from mere "sustainable habitat use," since even long-term sustainability (stable adaptations lasting centuries or millennia) may be explained in terms other than conservation, such as "low population density, low demand for a resource, or limited technology." True conservation, according to Smith and Wishnie, involves actions or practices that "(*a*) prevent or mitigate resource depletion, species extirpation, or habitat degradation, and (*b*) [are] designed to do so" (p. 501). "Design" need not be conscious—that is, it may result from a biogenetic or cultural evolutionary process—but to prove design one must rule out alternative explanations, for example, the marginal-value theorem of evolutionary ecology (pp. 501–2, 512). They argue that small-scale subsistence societies are not inclined to conserve when it is not in people's self-interest to do so. While granting that "small-scale societies have developed many practices designed to enhance livelihood, for which habitat or biodiversity conservation is a by-product" (p. 511), they suggest that such practices are not "conservation" because they were not designed to produce just that outcome and no other. Applying these criteria, they conclude that "voluntary conservation is rare" in small-scale societies.

Though Smith and Wishnie's definition of "conservation" seems reasonable, their application of the design criterion rules out many examples of sustainable harvesting that fit what most people think of as "conservation"—actions that prevent something of value from being damaged, lost, or wasted. They discount Nelson's (1982, 1983) argument that the Koyukon proscription of wasteful hunting reflects a "conservation ethic," countering that "avoiding wanton destruction of unneeded prey is not the same as restraining harvest below current desires" and that "matching harvest levels to current needs . . . does not qualify as conservation" (p. 511). In other words, conservation requires self-sacrifice; it must be altruistic.

In our view, the new orthodoxy diverts attention from a key issue: How can we avoid destroying our global environment, and what can we learn from indigenous resource management practices (and their grounding in traditional environmental knowledge) that might help us achieve that goal? Redford (1990:47) is pessimistic: "How relevant are [indigenous] methods and customs [of resource management] to situations where [low population density, abundant land, and limited involvement with a market economy] no longer exist . . . ? Techniques developed to satisfy subsistence needs are unlikely to work when surpluses are needed for cash." Granted. But if the problem is excess population, land scarcity, and the market economy, we should consider how we might work with indigenous peoples to restrain such destructive forces rather than discount their resolve to defend their traditional way of life and the land and resources essential to their survival as a people.

A preservationist bias is clear in the new orthodoxy. For example, Redford and Stearman (1993:252) argue that the interests of indigenous peoples and conservation biologists conflict, since "it is clear that if the *full range* of genetic, species, and ecosystem diversity is to be maintained *in its natural abundance* on a given piece of land, then virtually any significant activity by humans must not be allowed. . . . even low levels of indigenous activity alter biodiversity as defined above." Kay's characterization of Native American hunting as "overkill" implies that any measurable demographic impact on a prey species is excessive. Likewise, Smith and Wishnie (2000: 515) argue that "the dominant cultural meaning and practice of 'conservation' at the current historical moment focuses on preservation of biodiversity." We would counter that this is not the way the term "conservation" is most widely understood. Furthermore, as Alcorn (1993) has argued, while the preservationist ideal is clearly alien to indigenous peoples, a notion of conservation as "caring for the earth" is widely recognized. To hold indigenous peoples to the preservationist standard undermines the possibility of an effective alliance between conservation biologists and indigenous communities in defense of the environment.

Perhaps it is irrelevant whether we call the practices that have sustained the Huna Tlingit in their homeland for the past several millennia "conservation." In any case, we consider the traditional Huna Tlingit gull-egg harvest practices that we describe below an example of indigenous resource management that might be judged conservationist, with the proviso that what is to be conserved is not biodiversity in the abstract but a living community that requires as a condition of its continued existence the sustainable management of the resources on which it depends.

The Huna Tlingit Case

We report here the results of a study of the traditional harvesting of glaucous-winged gull (*Larus glaucescens*) eggs by Huna Tlingit people of southeastern Alaska, with particular emphasis on their harvests from gull colonies on the Marble Islands in Glacier Bay National Park and Preserve (GBNPP).[2] Many Huna Tlingit fondly recall how as children they traveled in late May and early June to the Marble Islands in Glacier Bay on family outings to harvest the large, rich gull eggs. Many bitterly resent the fact that these harvests are now prohibited by law, a prohibition enforced by the GBNPP administration since ca. 1960. (Glacier Bay was designated a national monument in 1925, expanded in 1939, and designated a national park and preserve in 1980. The federal presence and tourist pressure have increased gradually since its establishment [Catton 1993, 1995].) Many, if not all, Huna Tlingit value Glacier Bay as their "breadbasket" (Bosworth 1988, Thornton 1999) and the core of their ancestral homeland (Goldschmidt and Haas 1998). The gull-egg harvest issue has come to stand for the wider historic conflict between local Tlingit and the Park Service. In an effort to improve relations with its Tlingit neighbors, the GBNPP administration met with Huna Tlingit leaders in 1997. At the Huna's request, the Park Service agreed to fund this study of traditional gull-egg harvests, which the Huna hoped might support the eventual legal recognition of what they see as their right to continue their traditional harvest activities within the park.

A BRIEF HISTORY

The Huna Tlingit have occupied their traditional territory for millennia, with no evidence that they have degraded it (which is not to claim they had no impact on their environment; see Hunn et al. 2002:19–51). The local archaeological record locates cultures dating to 10,230 ± 800 B.P. within a few miles of the present village of Hoonah and clearly ancestral cultural traditions here by A.D. 1020 ± 70 (Ackerman, Hamilton, and Stuckenrath 1979). Croes (2001) has analyzed a 6,000-year-old basket recovered from the Silver Hole site on Prince of Wales Island, at the southern limit of present-day Tlingit territory. He concludes that "the basket in many ways closely resembles the ethnographic Tlingit and Haida basketry of this northern area" (p. 151) and clearly contrasts stylistically with baskets of similar age from farther south. The Huna recount oral histories indicating that they had occupied Glacier Bay before the last glacial advance of the "Little Ice Age," ca. A.D. 1100–1800 (Thornton 1995). They were in the process of reclaiming ancestral resource harvesting sites when the first Euro-American explorers arrived on the scene to witness the release of Glacier Bay from its burden of ice.

One of 13 *kwaan* or "tribes" of the Alaskan Tlingit language group or nation, the Huna include members of four major clans with original ties to Glacier Bay as well as members of a few additional clans. Clan rights over a territory generally did not preclude the harvesting of resources by other Huna residents of Huna Kawoo (Huna People's Country). As Pat Mills put it, "People from other places had to get permission, but if you were from Huna and knew the place you could just go." Relatives and friends from other communities, such as Angoon, Tenakee, and even Metlakatla, were also welcome so long as they went with a local family and there were enough eggs.

The village of Hoonah across Icy Strait from Glacier Bay has been from earliest recorded history the Huna's primary permanent settlement. Before the 20th century, they had additional winter villages, but these have been abandoned in favor of Hoonah. The historical process of residential consolidation at the site is relevant to a proper understanding of how Huna Tlingit strategies for harvesting gull eggs may have changed since the pre-European contact period and to the question of the effect of the establishment of the park and preserve on prior gull-egg harvesting practices. In the 19th century Huna Tlingit people apparently occupied as many as a dozen "villages," "settlements," and "forts" distributed throughout their recognized territory (de Laguna 1990, Goldschmidt and Haas 1998). These village sites were staging areas for subsistence harvesting. Abandonment of villages was in part a response to depopulation due to introduced disease epidemics and to involvement in the commercial fishing industry (Langdon and Brakel 2001). However, several villages and camps were abandoned as a direct result of forced exclusion by whites. In particular, fox farmers are reported to have appropriated Huna village lands and forcibly expelled residents of those villages. A fishing camp at Bartlett Cove, the site of GBNPP headquarters, appears to have been appropriated by the park administration (Langdon and Brakel 2001:104–21). Statements by many Huna Tlingit people indicate that they associate their exclusion from traditional settlements within Glacier Bay with the establishment of Glacier Bay National Monument.

The first mention of Glacier Bay in the historical record comes from the Vancouver expedition of 1792 (Vancouver 1801). Although Vancouver's shore party described it as a massive wall of ice fronting the turbulent berg-choked waters of Icy Strait, they nonetheless encountered a native group camped near the mouth of the bay and seemingly at home in that inhospitable environment (Menzies 1993:148–51). In 1878 the naturalist John Muir ventured into the bay with Tlingit hunters as

2. This study was developed in collaboration with the Huna Tlingits and personnel of Glacier Bay National Park and Preserve (GBNPP) and was funded by the U.S. National Park Service under Cooperative Agreement No. 14443 CA-9000-95-0019, Subagreement 1, Modification 3, between the U.S. National Park Service and the U.S. Geological Service Biological Resources Division, University of Washington, and in cooperation with the Huna Indian Association, Hoonah, Alaska. It was designed to document fully the traditional environmental knowledge with respect to harvests of glaucous-winged gull (*Larus glaucescens*) eggs as a basis for considering the possibility of negotiating the resumption of gull-egg harvests in the park and preserve. A full report has been submitted to GBNPP personnel and the Huna community (Hunn et al. 2002).

guides (Muir 1915). Glacier Bay captured his imagination, and his writings and public presentations prompted others to follow. Within several years of Muir's first visit, Glacier Bay became a regular stopover for steamships carrying an assortment of scientists, explorers, and venturesome tourists. All accounts by these early visitors mention the active involvement of Huna Tlingits in traditional activities throughout Glacier Bay. Camps were reported in the middle and lower reaches of the bay, and Bartlett Cove, with its village and cannery, was a regular stopover for steamships. After a lobbying effort by the Ecological Society of America and an intense political battle pitting preservationists and scientists against business interests and settlers, Glacier Bay was designated a national monument by presidential proclamation on February 26, 1925.

Tlingit society underwent profound changes during this period. The burgeoning commercial salmon industry brought sweeping changes beginning in the late 1870s. Within a few years salmon, the foundation of the Tlingit economy, was transformed into common property, and Tlingits were reduced from proud owners of streams and fish resources to wage-labor fishers and cannery workers. They found themselves increasingly cut off from many traditional subsistence sites that were being settled by non-natives or included in federal land management units such as Tongass National Forest and Glacier Bay National Monument. In the face of powerful pressure to assimilate, many Tlingits were able to meld many of these societal changes with their traditional subsistence way of life. For example, the summer's commercial fishing activities were dovetailed with subsistence fishing, hunting, and gathering outings, and the transition to gas-powered boats meant that many of the traditional locations for these activities could be reached more swiftly.

Until the late 1930s the Park Service had very little direct management involvement in the monument. By this time a host of non-native homesteaders, miners, trappers, commercial fishers, and fox farmers had moved into the region and Tlingit culture was in a state of transition. Though the Huna were clearly involved in cash-oriented activities such as trapping, seal hunting for hides and bounty, commercial fishing, and prospecting, they remained deeply connected to the Glacier Bay landscape. Park officials noted smokehouses at the mouths of productive fish streams and parties of Huna traveling to gather berries and gull eggs (see Trager 1939 and Been 1940).

Park Service officials moved to eliminate certain activities, such as the trapping and hunting of land animals, by native and non-native alike. Gathering birds' eggs, which was technically illegal under the Migratory Bird Treaty Act[3] and federal regulations, was eliminated in the monument in the early 1960s. This action strained relationships between Huna Tlingits and the Park Service, as it effectively cut the Huna off from their favorite gathering sites. Other activities, such as seal hunting for bounty and commercial fishing, were allowed to continue.

With time, even authorized uses decreased because of tension between Huna and the Park Service. However, Schroeder (1995) shows that Huna Tlingit harvesting activities continued throughout the park well into the 1980s. In summary, Huna Tlingits have utilized Glacier Bay for subsistence activities throughout the historic period despite legal sanctions. This perseverance is motivated by the deep spiritual connection of the Huna Tlingits to their homeland, their recognition that the most effective and meaningful way to maintain this integral connection is through subsistence activities, and their ability to adapt subsistence strategies and technologies to new conditions within an ever-changing social and legal framework.

HUNA TLINGIT SUBSISTENCE

For the Huna, subsistence was far more than an economic activity; it was also a "moral and religious occupation" (de Laguna 1990:209). For example, "The hunter had to purify himself [before hunting] by bathing, fasting and continence, [and] to refrain from announcing what he hoped to kill" (p. 210), "No animal . . . should be slain needlessly, nor mocked, nor should the body be wasted" (p. 209). "Fish had to be treated with respect and the offal returned to streams or burned to insure their reincarnation" (p. 210). Berries were believed to have an "inner form" or spirit (*yeik*) that had to be treated with respect (Thornton 1999:36).

Traditional practice included explicit conservation provisions. For example, "Kake people hunted sheep at three places but were careful not to visit the same place for two years, to conserve the game" (de Laguna 1990: 210). "Patchy" resources of critical importance, such as salmon spawning areas, halibut-fishing grounds, and berry patches, were owned by families that monitored them and controlled access to them. A number of key resources were cultivated by weeding (strawberries), fertilizing (berries), and transplanting (soapberries, salmon, deer) (Thornton 1999:4; Herman Kitka, personal communication, June 5, 1998; Pat Mills, personal communication, November 6, 1998). In all these activities, sharing was of the essence: "Each woman marked her fish with distinctive cuts and kept her bundles separate in the cache, taking pleasure in sharing them with housemates or visitors" (de Laguna 1990:210).

Fish were the primary resource category. According to Murdock's *Ethnographic Atlas* (1967:106), fishing (including shell fishing and marine-mammal hunting) accounted for 56–65% of Tlingit subsistence; hunting of large land animals (including trapping and fowling) ac-

3. On October 7, 1999, the U.S. Fish and Wildlife Service announced that the United States and Canada had formally agreed to a protocol amending the treaty. This agreement allows both countries to recognize and cooperatively manage subsistence uses of migratory birds and their eggs for "their own nutritional and other essential needs," including such harvests in the spring and summer, and establishes eligibility for the "indigenous inhabitants of Alaska" in specified areas.

counted for 26–35% and gathering of plants and small land animals (possibly including birds' eggs) for the remaining 6–15%. These proportions are probably systematically biased, exaggerating somewhat the importance of hunting at the expense of gathering. Thornton (1999) has shown that the quantitative contribution of a resource to Tlingit subsistence does not necessarily reflect the cultural significance of that resource for local people. For example, berries have profound spiritual and social significance for Huna Tlingit people despite their low ranking in Murdock's scale. Nevertheless, the quantitative predominance of fish in their diet is undeniable. Huna fished for five salmon species (harvested July–November, dried in fall for winter), halibut and Pacific gray cod (harvested late winter to early spring), and herring (eggs in April, rendered for oil in fall). Hunters targeted deer, mountain goats, and Dall sheep in fall, seals in summer, and bears in late winter. A great variety of shellfish was harvested in winter and spring, while greens and roots were available in late spring and many berries in fall (Newton and Moss 1984, Thornton 1999).

Seagull eggs had their place among this abundance of traditional riches. Though they were not notable in terms of their quantitative contribution to the diet or of outstanding ritual significance, they were nevertheless highly appreciated and are now fondly remembered with respect both to their having marked a turning point in the subsistence year and to the way in which they brought families together. Gull eggs were taken during a brief window of opportunity between mid-May and mid-June. Given the tight synchronization of egg laying in the gull colonies (described in more detail below), optimal harvests were possible for only a limited time. Gull-egg collecting trips heralded the arrival of good travel weather and relief from food shortages. It was a particularly exciting time for children, who participated actively in the gull-egg harvests. For many hunting-and-gathering peoples, food species symbolically represent the particular places where they are harvested, and harvest places are elements of a sacred landscape. This is especially true among the Huna Tlingit, who harvest each resource with and for family, house, clan, and tribe. Huna today view gull-egg harvesting as exceptionally important not just for its food value but for its power to define the Huna as a people and to maintain their ties to their ancestral lands and waters.

The creation of the national park and preserve has placed the majority of Huna Tlingit ancestral lands and resource harvest areas under increasingly restrictive federal control. Regarding Glacier Bay as their sacred homeland, Huna have become increasingly indignant about restrictions on subsistence and other activities within the park and monument boundaries. In the mid-1990s cultural resource management personnel at the park invited a group of Huna elders to a workshop to discuss possible collaboration on a project to document Tlingit knowledge of cultural and natural resources within the park. The Huna halted the conference proceedings when they realized that they were being asked to reveal their knowledge without being promised anything in return. In return for their cooperation they demanded that the park restore limited harvest rights for two key subsistence foods, seals and seagull eggs. Park officials agreed to work cooperatively with them toward a resolution of these issues. The present study was funded in that spirit.

Methods

During May, June, and October of 1998 we visited Hoonah and met with community leaders. On our arrival we presented our research proposal to a community meeting at the offices of the Huna Indian Association. Wayne Howell, a representative of the GBNPP cultural resources staff who had been working to improve communication between Huna people and the park administration, was also present. We emphasized the value of having an independent, academically based research team conduct the research. Though the study was funded by GBNPP at the request of Huna leaders, it was our understanding that our draft report was to be presented for review simultaneously to the Huna community and to the park administration and that, while both parties were welcome to comment and offer suggested changes, the final responsibility for the report was ours. We expressed our hope that the report might provide a detailed account of the significance of gull-egg harvests for the Huna people that might serve as a basis for discussion and negotiation between them and the park administration.

The research was approved and a list of knowledgeable local citizens provided. We interviewed all the available Tlingit individuals who were knowledgeable about traditional gull-egg harvests ($n = 45$) using an outline of topics to be addressed. Interviews were conducted by one or more of the researchers, most often in the interviewee's home. They were informal, and interviewees were encouraged to elaborate upon their experiences, perspectives, and opinions. These interviews were tape recorded with permission and the tapes subsequently transcribed. Interviewees were offered a token payment for their time. Russell later returned to Hoonah for additional interviews and to allow interviewees an opportunity to review and edit their interview transcriptions. All transcriptions were then carefully reviewed by project personnel, and representative quotes were selected for inclusion in the final report to illustrate the range of local perspectives on the issues.

We also visited GBNPP headquarters to discuss the project's design with Park Service representatives and conducted a survey of the region by air courtesy of the Park Service. Meanwhile, Thornton compiled relevant Tlingit-language vocabulary and Hunn reviewed the relevant ornithological literature, comparing the scientific literature with Huna Tlingit ethnoscientific knowledge of gull biogeography, breeding biology, and behavior, with special emphasis on the glaucous-winged gulls of the park and preserve (e.g., Baicich and Harrison 1997, Bent 1963[1921], Ehrlich, Dobkin, and Wheye 1988, Patten 1974, Verbeek 1993). Preliminary drafts of our report

(Hunn et al. 2002) were submitted to the GBNPP administration and to the Hoonah community for review.

Results

Gull-egg harvests, though of minor significance in the Huna Tlingit diet, past or present (see Newton and Moss 1984, Schroeder and Kookesh 1990), are accorded a prominent place in subsistence practice. Indeed, in the mid-20th century, at least, gull-egg harvests were considered a touchstone of Huna Tlingit identity.

THE CULTURAL SIGNIFICANCE OF GULL-EGG HARVESTING

The exceptional significance of gull-egg harvests is due in part to the fact that they marked a key seasonal transition from the relative confinement of the winter and early spring to the mobility characteristic of summer and fall, during which time the bulk of traditional subsistence products were harvested:

> And this is how we have come to love our country the way our fathers and uncles did. We also felt that we were part of somebody and somebody special when our families took us on these trips [to harvest eggs]. We were taught this is who we are and that this is how it's going to be.

> I think it was connection to your seasons, to . . . progress in your life, to continuity, to sharing in the community, to everyone coming together and, you know, doing this one thing. . . . And the difference between an egg inside Glacier Bay and an egg outside Glacier Bay is Glacier Bay is our traditional homeland. That's where our heart and soul is. That's what ties us to our land. Our food that comes out of there is directly responsible for our strength, our knowledge, our inner peace, as compared to [food] that's outside of the traditional homeland.

Gull-egg harvests also had special social and cultural significance as an activity that typically involved the whole family working together, including children as young as eight years old. It was perhaps a unique opportunity for children to learn from their parents and grandparents—in the context of the actual harvesting—both practical and moral lessons with respect to Tlingits' relationship with their natural environment:

> Gathering eggs in Glacier Bay was something especially the family looked forward to. It was like Easter. Family and cousins gathered up there and we collected eggs, and it was a joyous occasion.

> As soon [as you] were big enough, you go to your uncle, and the uncle was responsible to teach and train the kids.

> And one of our uncles' boats would take off . . . would take the whole family up to Glacier Bay to gather eggs. . . . [The children would be let off] on the hillsides with our uncles making sure we didn't go too far off the edge.

> We did not go as group of men or a bunch of people here or another bunch there. We went as family. . . .

> I remember carrying some of my little sisters and brothers on my back when we were going up there and doing this.

Although not a highly ritualized activity, egg collecting was a context in which traditional values were reinforced:

> I only remember my grandfather would put the egg up like this, looking towards Heaven and thanking the birds for the food that he found. . . . He'd call the birds just like they were people, . . . and he said, "Thank you for letting me find the egg for my meal today."

> Dad took us up there to gather eggs, and before we went to get the eggs, while we're on our way up on the boat, they would instruct us about how many eggs to take, to respect it and not try to play with it. And like I said, it was just like a spiritual food. . . .

> You did not want to even bother or touch any place, because the gull knows more about it than you do, so you always left the nest alone. You did not disturb it. You just took the eggs and stepped around it. Children were also taught to only take what was needed or what they could use.

> [Any time] that you harvest food or you're in the sacred homeland, you are being watched by every elder that is accompanying you.

> We were also told that if people broke the rules established by the elders they might not ever be asked to go again.

> The whole Glacier Bay was respected. Always when you left there, you had to pick up your garbage from wherever you're at. You put that away or you burn it in the fire. You don't just leave it.

> We consider a lot of things people. We talk to them. We believe there is a spirit. We [don't] know how he looked or anything. We only know he existed somewhere. Probably existed in the rock or in the mountains, in the animals in Glacier Bay or whatever. We do know he exists.

Though the quantities harvested were limited by the brevity of the harvest season (just a few weeks) and the scarcity of accessible colonies (in practice, by the second half of the 20th century, limited to South Marble Island),

the eggs were widely shared within extended families and the community:

> We'd halibut fish in Glacier Bay. When it was time . . . to pick eggs, they'd pick all the eggs they could. They'd pack 'em in ice and they'd bring 'em for their families. . . .
>
> The seagull egg was too important. I remember my dad bringing them home. And he'd talk about storing them in moss and grass on the boat. . . . Usually when he went up there for halibut fishing, after the halibut trip they would bring the eggs home.

HARVESTING GULL EGGS: THE DOMINANT STRATEGY

A substantial majority of respondents who specified a gull-egg harvesting strategy volunteered that they had been taught that they should harvest eggs only from nests with one or two eggs present (24 of 39 respondents, 64%). The most common strategy reported by far was to collect only from nests with up to two eggs and to take them all (16 respondents, 41%). We call this strategy A:

> What I was taught, if there was one or two eggs in there, that was good to take, you take them. If there was three or more in there, you know, they're already starting to form so the party I was with said don't touch them.
>
> We only picked one or two eggs. If there were three eggs in the nest we were told to leave it alone because there was usually birds in there.
>
> When you'd go up there to Marble Island, you walk around and look for [a nest], and then when you see it, you look at the eggs. Some of them have one egg. That's good. If it's got three to four eggs, you leave it alone.
>
> And we didn't pick any eggs off the nest that had already three eggs. If they had three eggs in there, then they had an embryo. . . . If there was one egg, two eggs, you could pick them, but if there were three eggs, then we stopped.

Two variations on this common strategy were reported by those harvesting eggs from a nest with one or two eggs. The first of these was to take only one egg from such a nest (2 respondents, 5%), and the second was to leave one egg in a nest with two eggs and not take any from a nest with one egg (2 respondents, 5%).

> We were instructed . . . that we are not even to touch nests that have three eggs in it. Nests that have two, you can take one.
>
> If there was just two eggs, leave one. Even if there was one, we were told not to touch 'em.
>
> We always left one.

Five respondents (13%) reported taking eggs only from a nest with one egg in it.

> You only pick the nest that has one egg in it. And the old timers would say . . . when you go picking seagull eggs, just like pick a nest that has just one egg in it. If there're two, there might be little chicks coming.

It was not at first obvious to us how this cultural injunction, if scrupulously respected, might have affected local gull populations in the long run. However, after consulting ornithological accounts of glaucous-winged gull breeding biology and behavior, which describe the species as an indeterminate layer (Bent 1963[1921]; Ehrlich, Dobkin, and Wheye 1988:165, 176), it became clear that this strategy was well designed to conserve local gull populations while affording a substantial and predictable harvest.

Once the female begins to lay (typically one egg every other day), she will continue laying until she has a full clutch of three eggs (less often one or two).[4] When this clutch size is achieved, her capacity to produce new eggs shuts down. "The onset of incubation [sometime after the second egg is laid] probably causes developing follicles to atrophy . . . and ovulation to cease" (Kennedy 1991:110). Experiments with various gull species have demonstrated that if eggs are removed before incubation begins, the female will continue laying. An experimental study of the closely related lesser black-backed gull (*Larus fuscus*) showed that these gulls "were capable of producing, on average, almost three times the normal clutch of three eggs." To be precise, the mean number of eggs induced was 8.59 ± 0.61 eggs over a period of 23.5 ± 1.9 days. One individual laid 16 eggs (Nager, Monaghan, and Houston 2000:1343). However, after incubation has begun, the female will not replace the stolen eggs. If all the eggs are taken or the nest is destroyed, a female may renest after an extended period of recuperation.[5]

It is likely that this egg-harvesting strategy was stressful for the females whose nests were repeatedly deprived of eggs, since egg production requires a substantial investment of energy. Continued harvests would also have delayed the onset of incubation. It has been reported that later clutches are on average smaller, perhaps as a response to the reduced period for fledging characteristic of late clutches. Thus this harvesting strategy was most likely not "harmless" to the gulls—that is, their fledging rates might have been marginally greater without the

4. Zador (2001) reports that of 291 unmanipulated glaucous-winged gull nests observed in 1999 and 2000 on South Marble Island, 68% had three-egg, 20% had two-egg, and 11% had one-egg clutches.

5. Gull population-control experiments indicate that glaucous-winged gulls will initiate a new egg-laying cycle approximately 12 days following the destruction of a completed clutch (Ickes, Belant, and Dolbeer 1998; Stephani Zador, personal communication, 2001).

harvest pressure. Nevertheless, there is no evidence that the impact of this harvest strategy on fledging success was such as to have posed a threat to the maintenance and even growth of local gull populations.

Considerable evidence suggests, furthermore, that harvesting is not a significant factor in the ultimate success of the gulls' breeding efforts. Bent (1963[1921]) has described how nesting gulls respond to disturbance at their colonies with respect to two other close relatives of the glaucous-winged gull, the Western (*L. occidentalis*) and herring (*L. argenteus*) gulls:

> "While they are somewhat wary, many allowed us to come quite close before rising from their nests. . . ." [p. 92, quoting Ray 1904]
>
> After being robbed the birds soon begin laying again and [Dawson] noted, by watching a certain nest, that an egg was laid every other day. [p. 91]
>
> Although the nest may be frequently robbed and several sets of eggs may be laid, only one brood of young is raised in a season. The normal set consists of three eggs, though two eggs often constitute a full set in the later layings. . . . [p. 92]
>
> Only one brood is raised, but when the nests are frequently robbed the birds are kept laying all summer. [p. 106]

Verbeek (1993:12) confirms these gulls' resilience in the face of human activity: "Glaucous-winged gulls are not easily disturbed at nest sites. In one study, in which nests on a roof were removed periodically . . . sixteen pairs rebuilt their nests on average 4.7 times rather than move to another site." In fact, there is some evidence that in certain circumstances harvesting of eggs may actually *enhance* colony productivity. A study by Vermeer et al. (1991) of the effects of egging on glaucous-winged gull colonies in the Queen Charlotte Islands of British Columbia concluded:

> If egging occurs throughout the laying and incubation periods, no gull chicks are produced [as occurred in one of the colonies studied]. In the three egged colonies where egging stopped midway during the laying period, gulls produced 0.86 fledglings per pair, whereas the average fledging rate for non-egged colonies was 0.77 chicks/pair. At the higher rate the gull population would be expected to increase at 2.7% per year. . . . Therefore the effects of egging, if practiced in moderation, would still allow the population to grow.

The predominant Huna harvesting strategy is consistent with the latter case.

Most Huna understand these basic facts of gull reproductive biology and behavior. The harvesting strategy described above is a self-conscious application of this traditional knowledge to produce a sustainable yield of eggs at or near the gull's reproductive capacity. In fact, one may describe the Huna egg-harvesting strategy as a form of animal husbandry (lacking, of course, the induced genetic changes involved in domestication).

VARIANT STRATEGIES

There is, however, less than perfect agreement among Huna Tlingits on the proper strategy for harvesting gull eggs. Harvesting from nests with three or more eggs was reported by 12 (31%) respondents.[6] It is noteworthy that all but three of these specified a strategy of leaving one or more eggs in the nest. We call this strategy B.

One person described a strategy involving leaving eggs in the nest on the basis of the time of egg collection in the nesting season: "In the early part [of the season] you . . . take all of them. At a later date you start becoming selective even though it's pretty hard to tell which one is which. . . . we used to take two and leave one. . . . And then you start taking only one." Strategy B was further qualified by suggesting that where there were three eggs in a nest they might already contain developing embryos. Such eggs were avoided by most, but elders were said to have a special liking for them and the exclusive right to eat them:

> So three eggs on down that's when I pick them, and out of three eggs I'll take two eggs, but when you have four eggs there's already little ones in there. Three would be the most that you picked, and you want to put those eggs [from nests with three?] in a special place for the elder.

If there were questions about whether eggs might contain embryos, they were given a "float test." If the egg sank, it was "fresh"; if it floated, it was "too far gone." This practice was described over 60 years ago by a Park Service biologist: "Some of the Indians are less destructive in collecting eggs; their practice is for each member to carry a small pail of seawater and test all eggs by placing them in this water. Those that float are replaced in the nest, and those that sink are collected" (Trager 1939:4). This practice is referred to as the "water test" by our consultants. Elder Sam Hanlon explained this process in more detail:

> And the first time that we go up there, which is the last week of May, by that time the climate is warming up so the seagulls start laying their eggs. . . . But anyway, they picked only a single egg in each nest. They can clean the whole island, you know. The next day, you would find more in that nest you just cleaned out . . . a single egg. There's still no limit. You can spend two-three days picking one egg at a time. And you can get as much as a hundred, two hundred eggs real easy. So now it comes to two and

6. Empirical research indicates that four-egg clutches are extraordinarily rare. Reports by respondents indicating taking from nests with four or more eggs may reflect distortions in information due to the passage of time (see Patten 1974, Reid 1987). Zador (2001: 24) reports one clutch with four eggs in a total of 291 nests in 1999 and 2000.

> three eggs. By this time it's June. So the climate is so warm, in a day or two the eggs that had been laid two or three days ago, they already had chicks in there. So when it comes to that, our people used to carry a bucket of warm water, and they would take one egg at a time from the nest if there's two or three, and they would put it in warm water. The temperature of the warm water should be [only] warm enough so you're not cooking the eggs. . . . And when it floated, it's telling you one thing—that there's a chick in there. So we put it back to let it hatch. Pick another one. . . . *We don't want to kill the whole population of seagulls off* [our italics].

The impact on fledging success of harvesting some but not all of the eggs from a completed clutch is not entirely clear. We know of no experimental data that specifically address this point. Stephani Zador, the author of a recent study of gull nesting in Glacier Bay, addresses this issue as follows (personal communication, 2002):

> We know that in most cases the gulls will not continue to lay eggs when they have already been incubating one or more. So, if one egg is left, they will be able to fledge one chick at the most. . . . Given that the gulls can fledge up to three chicks in a good year, if many pairs were left with only one egg to incubate and brood to fledging, it is logical that overall fledging success would be reduced. In fact, fledging success would be limited to one chick/pair, even in the best of conditions. . . . In the situation . . . [where] there were three eggs and two were taken within hours after the third was laid, the female would generally need to resume follicle growth to form the replacement egg(s). But it will not do so if meanwhile it is incubating the egg left in the nest.

However, the impact of harvesting some but not all eggs from a completed clutch should be considerably less than a simple proportion of the eggs left—that is, if two or one were left, the fledging rate would be better than two-thirds or one-third of the rate expected if the clutch had been left intact. Fledging success rates measured by Patten at the North Marble Island colony in 1972 and 1973 were 1.75 and 1.80 from nests averaging 2.80 and 2.96 eggs per clutch. Thus, the third egg is rarely successfully fledged. Patten notes that these fledging rates are nevertheless well above the estimated replacement fledging rate of 0.92 (1974:64).[7] Thus, three-egg clutches represent an investment "wasted" in poor-to-average years in anticipation of a dividend in exceptionally favorable years. Artificially reducing a three-egg clutch to two or one thus relieves the adult gulls of a significant burden of parental investment, and this should significantly enhance the probability of fledging for the remaining egg or eggs. Except in the most favorable years, it seems likely that overall fledging success rates would be reduced only marginally, if at all, by this harvesting strategy. In sum, harvesting some but not all eggs from a recently completed clutch is conservative in limiting the impact of such harvests to a marginal loss in most years.

Finally, we should note that we have confirmed a third strategy, first reported by Trager (1939:4):

> Two methods are used in taking the eggs. One is to rob only nests containing three or less eggs. The other method is more destructive. Upon landing on the island, all eggs present in the nests are destroyed. Then three or four days later, all nests are robbed of all eggs which they contain, thus eliminating the possibility of taking partially hatched eggs.

This "more destructive" alternative, which we will call strategy C, was reported by just one respondent and specifically rejected by several others. It may have been employed under special circumstances, for example, when fishing parties on the outer coast needed fresh eggs but could not time their harvests precisely. Though Trager describes this alternative strategy as "destructive" and many contemporary Huna Tlingits explicitly reject it as improper, it may nevertheless have been sustainable if practiced only occasionally. Destroying clutches that are already complete and being incubated need not result in nest abandonment. Biologists have observed relaying in colonies well advanced in incubation that had been destroyed by severe weather, predation (Shugart and Scharf 1976), or intentional nest disruption (Ickes, Belant, and Dolbeer 1998). The respondent who described this strategy clearly expected the gulls to relay, as the method was justified as a means to obtain fresh eggs. This strategy, however, is more expensive of human effort in that one must first destroy the eggs (perhaps also marking the nests so treated) and then return several days later in the hope of finding fresh ones. It would also appear incompatible with the dominant strategy described for the Marble Island colonies. It is, therefore, likely that it was not often practiced. It was certainly not part of the familial egg-harvesting tradition valued by the majority of respondents. The fact that there are differences of opinion within our sample demonstrates that a culture is a dynamic system of sometimes competing beliefs and practices but a system characterized by certain widely shared understandings.

Discussion

Some observers have suggested that Huna Tlingit egg harvests may have been responsible for observed or imputed glaucous-winged gull nesting failures in the park. The best-known and most influential of these claims is that incorporated in Lowell Sumner's "Special Report to the National Park Service on the Hunting Rights of the Hoonah Natives in Glacier Bay National Monument" (1947). The report was solicited by the Park Service in response to pressure from the Bureau of Indian Affairs to permit seal hunting in Glacier Bay. Sumner concluded

7. As noted above, Vermeer et al. (1991) report that 0.85 fledgings per pair would support increases at 2.7% per year.

that egg harvesting "would result in severe depletion" of the gull population if allowed to continue. He recommended to the park superintendent that egg harvesting be excluded from the "special privileges" of "the Hoonah natives" (p. 10) on the basis of an inference concerning the cause of an observed nesting failure:

> On June 25, 1947, the seabird nesting colony on North Marble Island was inspected by the National Park Service party. According to normal expectation, nesting activities should have been well under way at this date, with hundreds of young gulls in evidence, or at least hundreds of nests with incubation well underway. Instead, great crowds of gulls stood at empty nests, displaying the listlessness that characteristically settles upon a bird colony a few days after it has been robbed. There were no young gulls whatever, and of nests that contained eggs, only one had the full complement of three. . . . It is recognized that the Hoonah natives used to raid the bird colonies of Glacier Bay during primitive times. However, Hoonah has become an incorporated town with daily radio communication . . . and all the home conveniences of the machine age that the mail-order houses can furnish. Use of seabird eggs by such a large community can only result in eventual severe depletion. . . . The Director's authorization of January 7, 1947, listing the special privileges of the Hoonah natives, does not include the gathering of seabird eggs. It is believed that in view of present and future use of Glacier Bay National Monument, this omission is completely justified.

We believe that Sumner's inference that the colony failure must have been due to native harvesting is highly speculative. Similar inferences by Been in 1940 at Drake Island also lack empirical support. A similar reproductive failure of glaucous-winged gulls was documented in 1975 (Paige 1975), long after traditional egg harvests were prohibited. Furthermore, the 1975 failure was not restricted to a single colony but evident throughout Glacier Bay. However, we cannot rule out the possibility that Sumner's visit to the North Marble Island colony just happened to occur shortly after a strategy C harvest by Huna people. If indeed "great crowds of gulls stood at empty nests" on June 25, 1947, this would argue against a nesting failure due to a drastic crash in food supplies, as under those conditions it is unlikely that many gulls would have remained at the colony (Stephani Zador, personal communication, 2001). However, given evidence that colonies will renest in about 12 days after such a disturbance, it is quite possible that had Sumner returned to the colony two weeks later he would have found it thriving.

Our analysis of the validity of Huna Tlingit traditional knowledge of gull reproductive behavior suggests that Huna Tlingit egg harvesting did not seriously disrupt gull nesting or reduce gull reproductive success over time. As Patten (1974) noted, glaucous-winged gulls at the major Marble Island colonies were reproducing at rates well above replacement levels in the early 1970s. Assuming that the Huna Tlingits had harvested eggs from these colonies annually for some 100 years prior to Sumner's visit, it appears that their traditional harvests did not harm the long-term reproductive success of gulls in Glacier Bay.

FACTORS AFFECTING NESTING GULL POPULATIONS

The accessible nesting gull populations in lower Glacier Bay are apparently substantially smaller now than 50 or 100 years ago. It is clear that the most important factor in this decline is vegetative succession (see Cooper 1923, Lawrence 1958, Reiners, Worley, and Lawrence 1971). Three broad physiognomic community types are distinguished subsequent to the emergence of bare rock or soil from beneath the retreating glacier. The first is the "pioneer community" stage of low herbaceous and woody mat vegetation, which, other things being equal, is well suited to gull nesting. This is followed by a "willow-alder thicket" stage that would most likely preclude gull nesting, and it in turn is overgrown by a young Sitka spruce forest (Cooper 1923:225). According to Reiners, Worley, and Lawrence (1971:56), the mat community may develop 5–20 years after exposure of the substrate, the shrub-thicket stage at 20–40 years, and the spruce forest at 75–100 years. However, the rapidity of the transition varies a great deal depending on the substrate, being most rapid on slate and argillite and slowest on limestone and marble, particularly where the substrate is steep and/or has few crevices (Cooper 1923:234):

> The more favorable spots, such as level or depressed areas, or surfaces with many crevices, soon become covered with a luxuriant turf-like growth, . . . by increase of the shrubby species such areas are rapidly converted into thickets in which alder and willows are dominant, while the adjacent steeper and smoother surfaces are still bare of plants. . . . The spruces, thickly scattered upon the meadow and thicket areas, indicate the future course of development.

Cooper does not mention the Marble Islands, but as their name and location suggests, they very likely are composed of the rock surfaces most resistant to weathering and invasion. Drake and Willoughby Islands (described by Cooper as "being carved of solid rock" [1923:97]) supported glaucous-winged gull colonies until the mid-20th century but are now too overgrown. North Marble Island has undergone the same fate somewhat more recently (i.e., since Patten's studies there in 1973–74). It is uncertain if and/or when the South Marble Island colony will become overgrown with vegetation, though it has remained relatively bare for at least 160 years since the mantling glaciers retreated (Reiners, Worley, and Lawrence 1971:56).

SUSTAINABILITY OF MARBLE ISLANDS HARVESTS

It is impossible to determine exactly how many families harvested gull eggs on the Marble Islands each year and how many eggs they took. There has been an official presence discouraging use of the park for subsistence purposes for about 50 years. Many people remembered their last trip to these islands for egg gathering to have been in the early 1960s or earlier. Of 20 respondents who specified when they had last collected eggs in Glacier Bay, 5 said before the 1950s, 4 during the 1950s, 7 during the 1960s, 3 during the 1970s, and 1 during the 1980s. Thus their recollections of quantities harvested are rough approximations.

As far as current use is concerned, people in Hoonah are understandably reluctant to talk about what is defined by the U.S. government as illegal activity in a study funded by an agency responsible for enforcement of those laws. We therefore avoided asking about illegal harvesting. Consequently, we cannot estimate current levels of consumption of gull eggs in Hoonah with direct responses from interviewees. However, we believe that the consumption of gull eggs in Hoonah is now quite limited and has been since enforcement of regulations in Glacier Bay National Park became more active in the early-to-mid-1960s. Comments from interviewees that support this inference include statements from some parents that they would like their children to have the opportunity to eat gull eggs but only occasional illegal eggs came into the community.

An alternative way of estimating Huna egg harvests prior to more active law enforcement and after Huna acquisition of larger and faster boats is to calculate the quantities available for harvest, given the cultural context of Huna Tlingit egg gathering—including strategies for taking them and available technology. Such estimates are limited by the fact that the glaucous-winged gull nesting populations have not been systematically monitored at any site in the region. The number of eggs that might have been harvested is a function of the number of active nests within a certain radius of Hoonah at that time (i.e., the 1950s), the number of "surplus" eggs that a female gull might produce given what is known of gull breeding biology, and the efficiency of Huna Tlingit gull-egg harvests.

For the number of nests we have only Patten's rough estimate of 2,000 breeding birds (1,000 nesting pairs) for the North and South Marble Island breeding colonies in 1975 (reduced to ca. 350 pairs in 2000 on South Marble [Zador 2001]) and a count of 1,494 nesting birds (747 nesting pairs) tabulated at nine outer-coastal colonies by Sowls et al. (1982). However, we believe that these outer-coastal colonies were likely harvested only incidentally while pursuing other resources. The colony on Middle Pass Rock in Icy Strait is likely the only colony outside of Glacier Bay that would have contributed significantly to this harvest.

For the number of "surplus" eggs we have two estimates. Nager, Monaghan, and Houston (2000:1343) report a mean number of eggs laid per female of 8.59 based on an experimental manipulation of nesting lesser black-backed gulls. Zador (2001:23) reports considerably lower averages of 5.76 and 5.75 eggs per female for the South Marble Island colony in Glacier Bay for 1999 and 2000. If we subtract three eggs from these averages to allow for an eventual full clutch of three eggs, we may estimate "surplus" egg production—the number of eggs that might have been taken from each nest while leaving a full clutch that could then be incubated, hatched, and fledged—at 2.8–5.6 eggs per nest. This "surplus" production is likely somewhat more than a "sustainable" harvest in that the stress of replacing lost eggs and the delays incurred in completing the nesting cycle would reduce fledging success rates by some amount.

We have no quantitative data with which to estimate the efficiency of Huna Tlingit egg harvests, that is, the fraction of the "surplus" eggs that we might reasonably expect the Huna to have harvested each year. It is certain that 100% of the "surplus" eggs could not have been harvested, given that some fraction of the active nests would have been located on inaccessible terrain and that some fraction of nests and/or eggs would have escaped notice. During any given visit some nests would have been empty or would have contained complete clutches and therefore would have been passed over. The variation in harvesting strategies reported would also have tended to reduce the overall efficiency of the harvest. Finally, competition from other predators would have reduced the number of eggs available to Huna egg collectors. At the same time respondents report that harvests were organized to coincide with the most productive period of the nesting cycle, taking advantage of the tightly synchronized laying schedule in the colony. Huna also communicated with one another with respect to the condition of the colony and the success of recent harvest efforts, which could have substantially enhanced overall harvest efficiency. Thus, it is not unreasonable to expect a rather high efficiency rate for these traditional harvests. For the sake of illustration, we may calculate the number of eggs that might have been harvested sustainably at a range of efficiencies (e.g., between 30% and 70%). These limits suggest a range of estimated annual harvests of between 840 and 2,520 eggs.

Another approach to estimating the quantity of gull eggs harvested relies on general descriptions of the way in which eggs were gathered. One respondent estimated that a single person might fill a five-gallon pail with eggs during a single visit to the colony. We estimated that 30 hen's eggs fit into a gallon, which translates to 17.6 gull eggs per gallon, or 88.5 in a five-gallon pail. However, allowing for the layers of grass, Indian celery, or moss used to cushion the eggs, 60 gull eggs per five-gallon pail is a reasonable estimate.[8] If this is accurate, between 14 and 42 egg gatherers on the Marble Islands would have exhausted the supply. This suggests that a minority of

8. The Park Service intercepted an illegal harvest of eggs in the summer of 1999 in which the harvester had placed 45 eggs in a well-cushioned five-gallon pail three-quarters full (Wayne Howell, personal communication, 2000).

Hoonah families actively harvested eggs at these sites. However, since eggs were widely shared within the village, most families would have consumed at least a few eggs each year.

Catton (1997), drawing on Bureau of Indian Affairs annual statistical reports for 1943 and 1945, states that in 1943 "800 dozen" (9,600) gull eggs were harvested by Huna Tlingits from throughout their traditional use area. To support an annual harvest of 9,600 eggs there must have been a substantially larger nesting gull population in lower Glacier Bay then than at present. This is quite possible given the number of historic nesting sites in this area that are now abandoned because of vegetational succession.[9]

We are convinced that these harvests were "sustainable" by the fact that the South Marble colony has supported many hundreds of nesting gulls for as long as elders can recall and as far back as historical records exist. The Marble Islands were considered very important for nesting gulls by biologists as early as 1939 (Trager 1939) and 1947 (Summer 1947). Patten (1974) described the Marble Island colonies as "by far the largest in Glacier Bay." On the basis of the association of recently established gull colonies with the tongues of retreating glaciers, it seems likely that the Marble Islands have been important nesting sites for glaucous-winged gulls since they were first exposed by retreating glaciers ca. 1845.

It is a highly likely that Huna Tlingits harvested eggs from these colonies throughout the approximately 120 years between the emergence of the islands from the ice and the ban on egg harvesting from Glacier Bay National Monument. There is no evidence that those harvests have had negative impacts on breeding success at the colony. In fact, none of our consultants could remember any shortages of eggs for gathering, nor could they recall any notable variations in gull populations from year to year.

Conclusions

In response to those who doubt the utility of applying traditional environmental knowledge to comanagement of natural resources in protected areas, the Huna Tlingit case gives cause for optimism. Huna Tlingit people have no difficulty understanding the contemporary significance of their traditional environmental knowledge. Furthermore, their native language contains a rich vocabulary for traditional conservation principles and practices. As a direct consequence of our report, the GBNPP staff worked with Huna Tlingit leaders to organize a special egg-harvesting expedition to Middle Pass Rock in the Inian Islands, just outside the park boundary. This involved over 20 Hoonah secondary school students, who used the ethnographic material in our report—in conjunction with the advice of elders—as a guide for their collecting trip (Wayne Howell, personal communication, 2001). This proved a very positive experience for the community, and it promises to be a first step toward the Huna's goal of reestablishing this traditional subsistence practice in Glacier Bay. Though there remain many legal and administrative obstacles to achieving this goal, documenting the sophistication of Huna traditional environmental knowledge and the likely sustainability of their traditional practice should facilitate the process, with the traditional harvest strategies serving as a model for the management of the gull-egg resource.

In response to those who are convinced that "indigenous peoples" were not inclined to practice "conservation," the Huna Tlingit case provides a counterexample. The Huna observed gull nesting behavior closely and developed an understanding of gull breeding biology which they applied to the design of sustainable harvesting strategies. These appear to have functioned effectively for at least 100 years. The Huna Tlingit gull-egg harvest meets Smith and Wishnie's theoretical criteria for the occurrence of "conservation" (2000:505–6): (1) controlled or exclusive access (stable land rights), (2) distinct or confined resource populations, (3) resilient resource populations, (4) low discount rates, such that the value of sustained yield exceeds the value of immediate yield, and (5) social parameters (e.g., small group size and stable membership) and institutions (monitoring and sanctioning) that counter free-riding. Criteria 4 and 5 are characteristic of indigenous communities in general and of the Huna Tlingit community in particular. Such communities are defined by their long-standing residence in an area and their fierce determination to defend their right to remain there. These attachments guarantee a low discount rate for a resource such as nesting gulls, a resource that meets criteria 2 and 3. All that the Huna Tlingits lack today to meet Smith and Wishnie's criteria fully is control over the resource they were forced to cede to the Park Service 50 years ago. Of course, the Huna Tlingits conserved the local gull population not from any abstract concern to preserve biodiversity but rather in order to ensure that their children and their children's children might enjoy the harvest of gull eggs as they had. We would add to Smith and Wishnie's five criteria a sixth: that the community have sufficient empirically grounded understanding of the local environment to recognize how its harvests affect the survival of local populations of plants and animals. Traditional environmental knowledge is a necessary though clearly not sufficient foundation for conserving the balance between the needs of the community and the recuperative powers of the ecosystem that sustains it.

The issue now before the Park Service is not simply whether this traditional gull-egg harvest might or might not be biologically sustainable. It seems clear that a prohibition of Huna gull-egg harvests cannot be justified as necessary to protect the glaucous-winged gull population

9. Because of access difficulties, outer-coastal and Icy Strait colonies were likely harvested only incidentally while pursuing other resources and visited no more than once a year. Therefore colonies outside of Glacier Bay would not likely have contributed substantially to the 1943 harvest total.

in Glacier Bay, as gull populations throughout southeastern Alaska have been increasing (see, e.g., National Audubon Society 1997). Vegetational succession subsequent to glacial retreat in Glacier Bay seems the most significant factor affecting the long-term fate of glaucous-winged gull colonies here, and bald eagles are the most significant egg predator (Zador and Piatt 1999). However, the Park Service must answer to other constituencies in addition to conservation biologists and local Native American communities. Glacier Bay is hailed by "wilderness" advocates as one of the "crown jewels" among American "wilderness parks," a laboratory of natural regeneration. Tourists visit the park to experience "pristine wilderness" (ironically, often aboard luxurious cruise ships). Most make a close approach to the Marble Islands to admire the nesting seabirds and roosting Steller's sea lions. One may imagine their reactions were they to witness a Huna Tlingit traditional gull-egg harvest in progress there—though if they were informed in advance of the cultural value of these activities for the Tlingits and made to understand the careful conduct of the harvests they might come away with a truer understanding of the Alaskan "wilderness." What is at stake is not just a few seagull eggs but the seemingly irreconcilable competing claims to one of America's most impressive landscapes. We hope that our report will help inform activists on all sides of the importance of recognizing the legitimacy of Tlingit interests in the future of Glacier Bay.

Comments

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Postmodernists excused, most ethnographers would agree that indigenous people often have good knowledge of their environments. From my reading, those of us identified by Hunn et al. as contributing to the "new orthodoxy" do not question traditional environmental knowledge per se. We are more interested, however, in what indigenous people *do* with their knowledge. While there is some evidence that the Huna have a folk understanding of glaucous-winged gull nesting biology, it does not follow that they will necessarily use their knowledge to conserve the gull populations. I have seen Piro bow hunters in Peru and Wana blowgun hunters in Indonesia use an intimate understanding of animal alarm calls to lure entire social groups of prey (tamarins in Peru, birds in Indonesia) toward a wounded conspecific, where the animals were subsequently killed one by one. Whether foragers work to conserve their prey remains an empirical question regardless of how much they know about their resources.

Hunn et al. also criticize the "new orthodoxy" for the view that people in small-scale subsistence economies are unlikely to practice conservation. This characterization is correct as far as it goes, but it ignores the subtleties of a complex argument. It is not accurate to say that the new orthodoxy discounts the *capacity* of indigenous people to conserve. Smith and Wishnie (2000) and Alvard (1998) argue that while conservation is not predicted to be widespread in small-scale subsistence economies, there are conditions that favor it. Research is moving from simple myth-debunking to sophisticated analyses aimed at understanding the contexts that do and do not favor conservation. Lawrence Kuznar and I argue that animal husbandry is a good example of nascent resource conservation and that it arose historically in contexts that favored conservation in general (Alvard and Kuznar 2001). Interestingly, Hunn et al. refer to the Huna egg-harvesting strategy as a form of animal husbandry.

A regularly misunderstood point is that not all sustainable harvesting is evidence of conservation. Thus, although Hunn et al. argue that the Huna have functioned effectively for millennia, this is *not* evidence that they have done so by conserving their resources. The answer to the rhetorical question "Is one therefore *not* a conservationist for not conserving a resource that is abundant relative to the demands placed on it?" is yes. One may shrug and argue that if people are unlikely to overexploit because of limited technology or low consumer demand, then access to the resource should be granted, but this leaves the theoretical question of conservation unanswered.

Elsewhere I have described conservation as resource use reduced to a level below what would be fitness-maximizing in the short term and designed to encourage long-term, sustainable benefits in the future (Alvard 1998*b*). Given this definition, I view Huna selective egg harvesting as a potential case of conservation. Unfortunately, the contexts of the foraging trips are not presented in enough detail to draw conclusions one way or the other. We are led to believe that Huna foragers understood that removing "fresh" eggs would induce the birds to lay more. Hunn et al. say that this is a self-conscious application of traditional environmental knowledge to produce a sustainable yield, but this interpretation is not apparent from the quotes presented in the text. Not one of these indicates awareness that the harvesting strategy causes the gulls to continue laying. I should be clear, though, that even such awareness would indicate only that the foragers were *managing* the population—perhaps with the goal of conserving it, perhaps with the goal of maximizing short-term returns.

A designation of conservation depends not on why the "fresh" eggs were taken but rather on why the embryonic eggs were left behind. It is clear that "fresh" eggs were preferred to ones with embryos. Most of the narratives indicate that people left behind eggs that were more likely to contain embryos. The interesting question, especially for anyone who has been to the Philippines and partaken of the ubiquitous and nutritious snack called *balut* (embryonic duck), is why. If Huna regularly refrained from harvesting embryonic eggs that they might

have otherwise consumed in order to maintain the gull population over the long term, then I am willing to consider the label "conservation." Only one quote (from Sam Hanlon) suggests this. If embryonic eggs were avoided because, for example, they were less edible, contained difficult-to-digest feathers or bones, or were more costly to process, conservation is less a possibility.

The bulk of Hunn et al.'s data comes from narratives of Huna respondents. Besides the obvious problems associated with this approach, it may be particularly problematic in this case. While I understand and am sympathetic to the desire to involve Huna in the research, I am sure that these researchers appreciate the conflict of interests involved in relying on the accounts of people who have vested interests in the outcome of the research. They write that egg harvesting "defines who the Huna are as a people" and is "the touchstone of Huna identity." It is surely in the best interests of the respondents to report a benign harvest strategy and play down more destructive methods in order to gain access to Glacier Bay National Park.

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"Conservation is about people as much as it is about species or ecosystems," according to a recent editorial in a high-profile conservation journal, and "social factors are often the primary determinants of success or failure" of conservation programs (Mascia et al. 2003:1). The article by Hunn and colleagues is important and timely because it addresses these issues. It tells the story of Huna Tlingit gull-egg harvests in an Alaska national park and uses it to argue against the critics of traditional environmental knowledge, who discount the capacity for management of indigenous peoples.

In this brief commentary I will focus on the contribution of Hunn and colleagues to the wider discussion of how we might develop a cross-cultural, pluralistic definition of conservation. They hold that traditional environmental knowledge is important for conservation and value traditional knowledge for cooperative resource management that brings together government scientists and indigenous experts. They consider detailed community knowledge about the environment a prerequisite to assessing the impacts of customary harvests and designing strategies for sustainable use. This is not a view shared by the two other schools of thought they identify as "postmodernist" and "conservation-biology."

The postmodernist critique holds that cooperative resource management does not or cannot work because it creates for indigenous peoples "the burden of having to express themselves in ways that are foreign to them to justify their views to scientists and bureaucrats"; thus, the solution is "the devolution of control over local land and resources to aboriginal communities themselves" (Nadasdy 1999:15). The conservation-biology critique takes a skeptical view of the value of traditional ecological knowledge and questions whether indigenous people can ever conserve their natural resources. It is this second critique that Hunn and colleagues confront. They put Huna Tlingit conservation to the test using the criteria of Smith and Wishnie (2000:501): conservation involves actions or practices that (*a*) prevent depletion of species and degradation of environments and (*b*) are designed to do so. Their approach and results are well reasoned and convincing. The way they tackle the second criterion is about as realistic as anyone can make it, given that the "intent" of conservation is not something that can be clearly distinguished from conservation as an incidental effect (Ruttan and Borgerhoff Mulder 1999, Berkes 1999).

As for the postmodernist critique, Hunn et al. deal with it indirectly. Their case study is located in a national park. If the Huna Tlingit chose not to explain their ways and justify their views to the government scientists, the outcome would be quite predictable: their customary harvest of gull eggs would remain illegal. Nadasdy's larger argument is correct in many ways (e.g., the cultural context of indigenous knowledge) and admirable in its defense of the integrity of indigenous knowledge. Nevertheless, the case study shows that the postmodernist argument is not very useful for marginalized people negotiating for their rights. Hunn et al. differ from these critics in that they see the feasibility of a common set of operational concepts between scientists and indigenous knowledge-holders (Berkes et al. 1998) and are willing to take a chance with cooperative resource management (Singleton 1998). Such co-management rarely takes place on a level playing field; however, it creates an opportunity for transformative change not so much for indigenous groups as for our dominant society's understanding of conservation.

Our conventional views of conservation have usually been too simplistic and too Western-centric. In many developing countries, for example, local support for conservation remains weak because conservation is often seen as an elite issue insensitive to local livelihood needs. Customary harvests of indigenous groups and livelihood activities of resource-based communities may not fit well with the narrower definitions of *conservation as preservation*, but in many cases these communities are the conservationists' best natural allies (Alcorn 1993).

What would a broader definition of conservation look like? Brosius and Russell (2003:55) call for "a social definition of conservation that validates and encourages small-scale local conservation efforts, that links conservation with issues such as soil fertility degradation and loss of traditional food crop varieties." Programs that encompass not only the livelihood needs of local people but also their *knowledge* can effectively link conservationists with indigenous groups and other rural groups, thus diversifying the base of people "who speak for the earth" and building broader, pluralistic constituencies for conservation (Brosius and Russell 2003). What are the elements of such a cross-cultural approach that respects

indigenous epistemologies, and what are the ways in which the dominant preservationist ethic may be modified? It is in this context that the contribution of Hunn and colleagues is in my view particularly significant.

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Hunn et al. argue against a "new orthodoxy" that discounts the capacity of indigenous peoples to practice sustainable resource harvesting strategies. In doing so, they point out that in debunking the "ecologically noble savage" myth many writers have offered another one—"Indian as overkiller"—in its place. They are certainly correct here to question the empirical evidence of the various "overkill" arguments that have been made (see also Broughton 2002*a*: 66–68; Grayson and Meltzer 2003). In reality, the impact of indigenous human populations on the landscape is highly variable. This is perhaps nowhere better documented than in the archaeological record, where the *long-term* effects of human hunting and harvesting strategies have been examined in detail.

For example, recent fine-grained studies of the age structure and growth rates of archaeological fur seal (*Callorhinus ursinus*) remains from the Ozette site on the Washington coast show that 700 years of hunting (A.D. 1100 to 1800) had no effect on fur seal populations (Etnier 2002). Similarly, artiodactyl populations appear to have irrupted in many areas of California and the Great Basin at the end of the drought-dominated middle Holocene (8,000 to 4,000 B.P.) despite active human hunting at this time (Broughton and Bayham 2003). At the same time, many cases of substantial anthropogenic depressions of large-sized vertebrate taxa have now been carefully documented in many settings across North America (Grayson 2001; Broughton 2002*a*, *b*). Virtually all of these cases have been derived from contexts that supported large and expanding human populations. This certainly characterizes the conditions of the late Holocene in California, where the documentation of human impacts on prehistoric faunas is especially comprehensive. California ethnographies, I note, also contain many examples describing how sustainable yields were maintained by deliberate resource management practices (Anderson, Barbour, and Whitworth 1998). Demographic factors would thus seem to override emic-based attitudes and philosophies about resource conservation.

In the case that Hunn et al. offer as an example of indigenous resource conservation, the primary data are perspectives and opinions about traditional gull-egg harvesting elicited from 45 Tlingit individuals. Although many individuals reported that they would take eggs only from nests that had incomplete clutches, many other, less benign strategies were also reported. That harvesting strategies might vary in different contexts is implicitly rejected, except to explain away one particularly destructive method. The relationship between the replies given to the researchers and actual egg harvesting behavior—past, present, or future—is, of course, unknown. The respondents must also have been aware that their comments could influence whether egg harvesting would be permitted in the future. As sympathetic as I am to the Tlingit cause, this research design, unfortunately, lacks scientific rigor.

Even taking the sample of responses as a fixed and true reflection of how Tlingit egg collectors actually behave, it remains unclear how the mix of reported strategies would affect gull populations under different intensities of predation and different distributions and densities of nests in the colonies. Hunn et al. wear a well-intentioned bias on their sleeve by repeatedly playing down the potential effects of egg harvests. Most egregious is their suggestion that since the mean fledging success reported is about 1.80 birds per nest, reducing three-egg clutches to two-egg clutches is actually doing the birds a favor by removing "wasted investment." But the mean values are as high as they are because of the many nests that do successfully fledge three young. Indeed, seabird reproductive strategies are sensitively geared to interannual variation in environmental factors that influence fledging success (e.g., Ainley and Boekelheide 1990). The effects of egg or chick mortality due to the intraspecific predation that routinely follows the disturbance of colonies is also not fully addressed. Nor is any mention made of the taking of chicks, juveniles, and adult birds, a practice commonly associated with the harvesting of seabird colonies up and down the eastern Pacific Coast in historic and prehistoric times (Yesner 1981, Broughton 2002*b*).

Hunn et al. repeatedly assert that there is "no evidence" that Tlingit egg harvesting had a negative impact on the breeding success of gull colonies; it would seem more accurate to say that only anecdotal evidence exists to evaluate whether it did or did not. While respondents may not have been able to recall any shortages of eggs or variations in gull populations, memories can be selective and may not extend to prehistoric times, prior to the introduction Old World diseases, when human populations and pressure on local subsistence resources were both considerably higher.

While the time for polemics on indigenous conservation may now have passed, Hunn et al. ride the old-orthodoxy bandwagon in pleading a case for conservation where few data exist to support it.

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This paper incorporates the components of a timely study in applied anthropology: escalating antagonisms between a small indigenous community and a powerful bureaucracy, ready expertise from highly qualified schol-

ars, unanticipated and powerful research results. Following social sciences convention, the authors position their study at the centre of a debate in environmental anthropology, and my remarks address this contribution.

The history of Glacier Bay National Park and Preserve follows a pattern familiar in North America: establishment of a national monument, expansion of boundaries to include more territory, designation as a national park and preserve, inclusion in a UNESCO-designated world heritage site, and systematic prohibition of indigenous harvesting activities by people who regard these lands as ancestral territories. In the mid-1990s, park personnel invited Huna Tlingit residents to document their "traditional ecological knowledge" about an area from which they had been excluded for decades. Not surprisingly, the Huna delegation agreed to proceed only if the park restored limited harvest for seals and gull eggs. A compromise of a kind was reached when the U.S. National Park Service agreed to fund an independent study of historical gull-egg harvesting strategies conducted by an academic research team working in collaboration with both Huna and park personnel.

Hunn and his colleagues bring decades of ethnographic expertise in environmental anthropology to this project and are unusually well situated to conduct the research. They conclude that Huna residents at Glacier Bay demonstrate a sophisticated appreciation of glaucous-winged gull nesting biology and behavior that is reflected in sustainable egg harvest strategies. They anticipate that this study will generate new policy, but here the jury is still out.

Their larger concern is to address debates about local knowledge that have become polarized during the past decade. At one extreme they identify what they call a "new orthodoxy" shaped by conservation biology and rooted in a cultural ecology paradigm that hinges on optimal foraging strategies, altruism, and shifting definitions of conservation. Its proponents, they say, deny a priori the possibility that creative indigenous management techniques ever conserved natural resources in the past. Thus constructed, the benchmark for definitions of conservation veers sharply away from notions of stewardship toward ever-narrowing definitions of strict preservation.

They characterize their other pole as a "postmodernist critique," but here they may draw too firm a line between their own position and that of writers who, in their words, "misconstrue or cynically manipulate" indigenous terms to suggest incommensurability between the views of local residents and scientists. The difference between their own position and that of Paul Nadasdy, whom they identify as their chief "postmodernist" opponent, seems largely one of emphasis. They speak very generally about power imbalances, noting that "indigenous communities everywhere must necessarily engage an encompassing polity that holds ultimate power" and that Western "experts" may fail to appreciate or choose not to integrate alternative approaches to knowledge in cooperative resource management regimes. They might have added that in Alaska and northwestern Canada parks have historically drawn on science to provide justification for dispossessing native peoples of their homelands and have taken legal action to enforce this. Where Nadasdy provides ethnographic accounts of a translation process, they retreat to science as though cultural ecology could in the end be separated from political ecology.

Significantly, Nadasdy conducts research near the adjoining Kluane National Park in Canada, where prohibitions against indigenous hunting were also enforced from the 1940s. It is now encompassed by the same world heritage site that engulfs Glacier Bay. His argument seems less about incommensurability than about how local knowledge can be distorted by what he elsewhere calls "fundamentally contested terms." He points out that certain terms—"land," "hunting," "resources," and "property"—are central to negotiations surrounding land claims, wildlife management, and environmental issues but that negotiating parties usually mean very different things when they use identical English terms. Inevitably the most powerful parties take their meanings to represent common sense and build those meanings into legal agreements. This seems similar to concerns that Hunn et al. discuss with regard to how the term "conservation" has been hijacked by those who enforce (or manipulate) meanings with reference to narrow definitions of "preservation" rather than broader definitions of stewardship.

Ideally, Hunn et al. would proceed with two further steps crucial for assessing their thesis that good research can generate policy changes: first, an ethnographic account of the negotiations that follow in moving recommendations to policy and, second, a parallel study of the impact of a limited seal harvest, historically more contentious than egg harvests. The history of interactions between park administrators and Huna may be as significant to communication as "culture" in this case. These additional stages might further clarify anthropology's contributions to local-knowledge debates.

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In their detailed study of the previously neglected practice of egg collecting, Hunn et al. demonstrate the efficacy of Tlingit traditional environmental knowledge in promoting sustainable use of gull eggs. They show how Tlingit practice incorporates knowledge of gull reproductive biology and behavior as now documented by biologists. This issue is not merely of academic interest; Hunn et al. argue persuasively that Huna Tlingit should regain their rights to harvest gull eggs in what is now Glacier Bay National Park, part of their aboriginal territory. They usefully position their work within the larger theoretical debates that now challenge local traditional environmental knowledge. On one side are those

who question whether indigenous people were capable of "sustainable resource management" and point to cases of resource degradation. On the other side are those who see the concept of such management as an alien and ethnocentric projection. Hunn et al. provide a much-needed empirical corrective to this debate.

Tlingit seabird egg collecting has been poorly documented until now, mentioned only in general terms, with its importance minimized on the basis of its overall dietary contribution (e.g., Jacobs and Jacobs 1982:123; de Laguna 1972:395). Hunn et al. explain the cultural significance of gull-egg collecting as both a celebration of seasonal change and a time for families to enjoy an excursion to offshore islands. The Marble Islands are one of only a few recorded locations of Tlingit egg collecting. Goldschmidt and Haas (1998:47, 57, 64, 83) mentioned four other places where eggs were procured. Seabird eggs may have been collected in the outer Prince of Wales Archipelago; Langdon (1977:95) suggested probable Tlingit use of Noyes, Timbered, and Maurelle Islands, but no specific data are available. Yet Nelson and Lehnhausen (1983) list 91 known seabird colonies in southeastern Alaska, all potential sites of Tlingit use.

The antiquity of seabird egg use is similarly unknown. To my knowledge, seabird eggshells have never been identified in the region's archaeological sites, which span more than 10,000 years of prehistory. I have identified the bones of 11 seabird taxa found in the Cape Addington Rockshelter on Noyes Island, including short-tailed albatross, northern fulmar, storm petrel, cormorant, gull, pigeon guillemot, rhinoceros auklet, tufted puffin, Cassin's auklet, murrelet, and common murre (Moss n.d.). This range of taxa suggests offshore seabird hunting, although some of these (e.g., albatross) were not taken at colonies. The location, scale, and frequency of Tlingit seabird use across southeastern Alaska remains poorly known, emphasizing the importance of Hunn et al.'s study.

Hunn et al. argue that Tlingit subsistence practices incorporate principles of conservation. This is further evident in scores of Tlingit texts that highlight ethical responsibility toward animals. All living beings are part of the Tlingit social and moral world. Narratives show how cruelty, killing more than is needed, or ridiculing animals brings a person misfortune or even death. As one of de Laguna's (1972:824) informants said, "The old Indians never just shot animals for no purpose. They just shot what they needed, and every animal they killed, they talked to it and explained why they had to kill it. They showed the animals respect." This ethical system and the principle of trusteeship (Langdon 2000) extended to the beings Westerners term "plant or animal resources." Tlingit environmental knowledge also guards people's health and safety. For example, seasonal warnings against shellfish consumption helped the Tlingit avoid paralytic shellfish poisoning (Moss 1993). Many writers have demonstrated that subsistence practices are the foundation of Tlingit cultural identity. This conservation ethic still survives, although in the face of state and corporate manipulation (Dombrowski 2002) a number of native village corporations have clear-cut their lands, resulting in substantial damage to wildlife habitat. I argue that this is a thoroughly contemporary phenomenon that represents a radical break with the precepts of traditional environmental knowledge.

Nevertheless, what Hunn et al. have called the "new orthodoxy," the idea that indigenous people will "by nature" overharvest resources, has recently gained prominence, especially among North American archaeologists. As scholars who claim interest in long-term cultural and environmental change, many of us seem to forget that extant wildlife have evolved with over 10,000 years of Native American hunting and gathering pressure. Nelson (1998) has chronicled the adaptive virtuosity of deer, showing how their dietary breadth and reproductive capacity have led to overpopulation, disease, and starvation in the absence of hunting. Wildlife adaptations have been evolving in tandem with human hunting and gathering strategies over a long period of time, yet some scholars inspired by conservation biology seem to assume a pristine, uninhabited wilderness as a baseline. It is hard not to interpret this theoretical stance, at least in part, as a reaction to the recent successes of those working for indigenous rights.

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Hunn et al. report on a significant piece of research: Huna Tlingit strategies for harvesting gull eggs. The significance lies in several areas. The research brings important evidence into the debates about traditional environmental knowledge and the capacity of indigenous people to draw on their received knowledge in participating in dialogue about ecological futures. It has facilitated dialogue between Huna Tlingit and national parks personnel and is thus providing practical outcomes for both indigenous people and parks. It provides a detailed body of evidence concerning both sustainable-yield practice and the culture of place. By bringing their study into the debates about whether indigenous peoples are or ever were "conservationists," Hunn et al. open up the ground for a more constructive dialogue through the careful accumulation and analysis of evidence from a range of sources including Huna people and ornithologists. It is especially important, however, that this research has practical outcomes in intervening in the politics of "wilderness."

I am particularly drawn to the inclusion of quotes from the extended and open-ended interviews the researchers conducted with Huna people. In my view, one of their great contributions is that they implicitly hold up a mirror to the new orthodoxy of conservationism, which sets a very stringent standard for "conservation." In reading the Huna discussions of egging we encounter

a rich engagement that radically transcends the new orthodoxy. When Huna people talk about "how we have come to love our country," we gain a much greater sense of the density of multiple contexts and forms of connection that bring Huna people into the islands for gulls' eggs and impel them to harvest so as to continue to be able to return.

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Hunn and colleagues describe the foraging practices of Huna Tlingit of Alaska as a case in which traditional environmental knowledge guides sustainable harvests of seagull eggs and the long-term conservation of the gull population. The traditional environmental knowledge in this case is a rule-of-thumb for the number of eggs that one should take from a nest. Because glaucous-winged gulls are indeterminate layers, they are capable of laying additional eggs to replace those that have been harvested. Hunn et al. argue that when traditional egg-gathering guidelines are followed, the birds' fledging rate is little affected and the gull population is conserved. In large part I find this explanation a convincing example of indigenous conservation. This is an important contribution to the literature on traditional environmental knowledge and sustainable foraging strategies, and I applaud the authors for their work.

My objections have to do with the way they have chosen to contextualize their study theoretically. They have seriously mischaracterized the viewpoint(s) they classify as the "conservation biology critique of traditional environmental knowledge" or the "new orthodoxy," and this promotes a polarization of viewpoints that is, like many famous theoretical battles in anthropology, based on a false dichotomy. It is unnecessary because the example of Huna Tlingit gull-egg harvesting does not contradict or refute the basic tenets of "the new orthodoxy" or at least the part of this supposed orthodoxy with which I am most familiar. I do not wish to speak for the entire category, which is more heterogeneous than Hunn et al. make it appear, and I do not have adequate space to make all the appropriate distinctions. Rather, I limit my discussion to the perspectives informed by human behavioral ecology (Smith and Wishnie 2000; Alvard 1995, 1998*a*, *b*; Alvard and Kuznar 2001; Ruttan and Borgerhoff Mulder 1999; Low 1996).

The major mischaracterizations are these: First, Hunn et al. describe the "new orthodoxy" as a "critique of traditional environmental knowledge," but most of the literature they cite as supporting it does not talk about traditional environmental knowledge at all. Human behavioral ecology by no means contradicts or dismisses the idea that indigenous peoples know their environments very well and pass this knowledge on to future generations as part of their survival strategy. On the contrary, one of the core ideas in human behavioral ecology is that inherited cultural information (in addition to inherited genetic information) contributes significantly to behavioral phenotypes (Smith and Winterhalder 1992:26; Richerson and Boyd 1992; Durham 1991).

Secondly, Hunn et al. make it sound as though the human behavioral ecological viewpoint were that conservation by indigenous peoples is *impossible*. This is false. Human behavioral ecology predicts that conservation will be *rare*. This is because evolutionary theory more easily accounts for self-centered, short-term behaviors. Evolutionary theory can also explain cooperative, long-term strategies such as those involved in resource conservation, but these explanations require special circumstances and so are less likely to occur (Alvard 1998*b*, Smith and Wishnie 2000). When they do occur, they are all the more interesting to behavioral ecologists. For example, Alvard and Kuznar (2001) see animal husbandry as conservation and discuss the conditions under which immediate-gain hunters became conserving herders.

Human behavioral ecology views conservation as both a collective-action/cooperation problem and a future-discounting problem. The collective-action problem has to do with whether everyone will follow the rules. If some people decide to limit their harvesting behaviors at personal expense, what is to stop other people from cheating and illicitly harvesting the conserved bounty? The future-discounting problem has to do with the tradeoff between immediate and delayed rewards. Decision makers often prefer smaller rewards available soon to larger, delayed rewards. These two problems do not imply that a "tragedy of the commons" is inevitable. Human behavioral ecology suggests alternatives, and the Huna Tlingit example actually illustrates several of them.

The collective-action problem is easily resolved because there is a sanction against cheaters: "If people broke the rules established by the elders they might not ever be asked to go again. . . . you are being watched by every elder that is accompanying you." There may be little temptation to cheat because the rules are easy to follow and there is not much to be gained by breaking them. The future-discounting problem is also easily resolved, because there is little short-term cost to conservation. Egg foragers who conserve still end up with pretty much the same number of eggs at the end of the day. They satisfy their immediate needs while still investing in the future of the gull population.

Hunn and colleagues' diatribe against human behavioral ecological approaches to conservation is misplaced. Their own case example is largely consistent with human behavioral ecology and not a refutation of it. Human behavioral ecology can be theoretically consistent with traditional environmental knowledge and advocacy for traditional people's sovereignty.

Reply

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We appreciate this opportunity to respond to the supportive and critical observations of our commentators. They represent a balanced range of perspectives and collectively bring years of field experience and reflection to the issues we have raised, most pointedly the contentious question of how, when, and if we may expect human communities to conserve the natural resources on which they depend. We addressed our critique to two positions that we attributed—admittedly with a broad brush—to "postmodernists," on the one hand, and "conservation biologists," on the other. Cruikshank and Rose are perhaps most sympathetic to the view we have characterized as "postmodernist," while Alvard, Broughton, and Tucker might be said to represent perspectives allied with the "new orthodoxy" we have attributed to conservation biology. Broughton, Moss, and Tucker bring the archaeologists' deep time perspective to the issue, while Berkes, Cruikshank, and Rose have among them many years of experience in collaborative research with indigenous peoples. Cruikshank and Moss have extensive firsthand experience of the Tlingits and their immediate neighbors.

A great advantage of CA☆ treatment is that the responses of a range of expert commentators will expose whatever aspects of the original argument were unclear while allowing the authors to clarify misperceptions. We would like to clarify one point in particular: The majority of the commentators, including those with contrary perspectives, attribute to us the view that conservation biology's "new orthodoxy" dismisses entirely the possibility of conservation by indigenous societies. For example, Berkes cites the conservation biologists' skepticism that indigenous peoples "can *ever* conserve their natural resources," while Cruikshank characterizes the "new orthodoxy" as "rooted in a cultural ecology paradigm that . . . den[ies] a priori the possibility that creative indigenous management techniques *ever* conserved natural resources" (our emphasis). Tucker argues that we mischaracterize the conservation-biology position by suggesting that conservation biologists consider conservation by indigenous peoples "impossible."

To the contrary, we argue that conservation biologists consider conservation not impossible but certainly improbable, that for them conservation is an exception to a general rule of evolutionary biology that human beings—in common with other organisms—*ceteris paribus*, will pursue selfish, short-term interests. Assertions to the contrary are met with deep skepticism. Purported examples of indigenous conservation are subjected to intense critical scrutiny. In effect, indigenous peoples are guilty until proven innocent, guilty of failing to design a collective course of action that conserves the biodiversity on which their way of life depends. The popularity of Paul Martin's Pleistocene-overkill hypothesis—despite the lack of any credible evidence to support it—suggests the critical double standard in play. As Grayson (1984) has shown, that hypothesis has been refined to the point that it can never be falsified, but it is widely accepted by conservation biologists and the general public as proven fact.

We respect the power of evolutionary ecological theory to generate interesting hypotheses relevant to the issue of resource conservation by indigenous and other communities. We recognize the distinction between "epiphenomenal conservation" (see Hunn 1982) and "conservation by design" (Smith and Wishnie 2000). We appreciate the fact that conservation is "a collective-action/cooperation problem and a future-discounting problem" (Tucker). However, we believe that most evolutionary ecologists seriously underestimate the likelihood of conservation, particularly by indigenous communities. We disagree with the theoretical claim that conservation by indigenous people should be "rare" (Tucker) or that "conservation is not predicted [by human behavioral ecological theory] to be widespread in small-scale subsistence economies" (Alvard). Rather, we believe that indigenous communities typically exhibit precisely those characteristics Smith and Wishnie (2000) have identified as favoring conservation. Such communities are characterized by intimate, personal relationships informed by many generations of collective experience of living within the resources of a local territory. "Cheaters" are readily identified and effectively sanctioned. "Future discounting" is minimized by the deep personal attachment to their land that members of such communities will share. Conservation by indigenous communities should be seen not as exceptional but rather as the rule. What needs to be explained is the occasional failures to conserve associated with, for example, "large and expanding populations" (Broughton) or disruptions of subsistence economies by colonial and/or capitalist penetration. Examples of purported failure to conserve by indigenous communities should be subjected to the same skepticism more usually accorded assertions that indigenous communities conserve.

We have offered as an example of indigenous conservation a single, limited case study, that of Huna Tlingit seagull-egg harvest strategies. Our "research design" has been criticized as lacking "scientific rigor" (Broughton). It has been suggested that because our evidence is limited to statements by Huna Tlingit community members as to how eggs were harvested, it cannot be extrapolated to actual harvest practices (Alvard). It is suggested that the Huna Tlingit had a vested interest in demonstrating to us their conservationist sensitivities and therefore their reports are likely biased by that interest. This is tantamount to suggesting that they got together in advance of our arrival, having carefully consulted the ornithological literature on seagull breeding biology, to develop a response calculated to deceive us. This suggestion is, in our opinion, absurd. We noted that the Huna's responses varied in many significant details. We noted also

that the implications of their dominant strategy for sustainable harvests were not at all obvious to us initially. Finally, as Alvard comments, our Tlingit consultants rarely cited resource conservation explicitly as justification for their harvest strategies. To suggest that they invented this tradition for our benefit seems to us to reflect a strong inclination to discount the possibility of indigenous resource conservation. To suggest as well that they might have been motivated primarily by a desire for fresh eggs or an aversion to eggs with developing embryos (Alvard) ignores our discussion of the issue. Many Huna elders consider eggs with developing embryos to be a special treat. There is no general aversion to eating such eggs. Furthermore, if culinary preference rather than a concern for resource conservation were the motivation for their harvest strategies, it would seem far more efficient to simply harvest all the eggs and later discard those deemed objectionable. To seek to explain away our evidence as selfishly motivated affirms our point: Conservation biology discounts the likelihood of indigenous conservation because "human behavioral ecology more easily accounts for self-centered, short-term behaviors" (Tucker). This is in our view a serious limitation of human behavioral ecology.

We are well aware of the limitations of the evidence we present. However, our "research design" did not lack "scientific rigor"; it was simply not designed to provide an airtight test of a hypothesis of human behavioral ecology. Instead, we set out to document Huna Tlingit traditional environmental knowledge and its application to a practice effectively prohibited by the federal government 50 years ago. That our data shed some light on an issue of theoretical significance for human behavioral ecology was serendipitous. We also appreciate the fact that ethnographic data such as we have presented, while particularly effective in demonstrating the continuing power of primordial attachments to place for indigenous peoples, as Cruikshank, Moss, and Rose stress, cannot address the question of long-term impacts. Such impacts must be assessed archaeologically. Unfortunately, seagull-egg harvests leave few if any traces in the archaeological record (Moss). Furthermore, given the dynamism of the Tlingit environment, it is not likely that the specific environmental circumstances Huna Tlingit confronted during the past two centuries could be precisely replicated at any other time and place. Yet, the Tlingit archaeological record provides no evidence of environmental degradation attributable to Tlingit mismanagement during the past ten millennia. We are doubtful that this record is due to "limited technology or low consumer demand" (Alvard), as it is clear that it would have been a simple matter for the Tlingit to have destroyed every gull colony in southeastern Alaska if they had been of a mind to do so, while their "low consumer demand" must be seen as a strict function of their population density—a key parameter of the sustainability of subsistence economies that is outside the scope of the present argument.

If the archaeological record had provided evidence for southeastern Alaska of a "substantial anthropogenic depression of large-size vertebrate taxa," as noted by Broughton for the late Holocene in California, would we be required to conclude that ancestral Tlingit failed to conserve their resources, or might we just as well conclude that they had simply achieved sustainability at a higher Tlingit population density? Evidence of "population depression" is not evidence of "overharvesting" so long as human predator and prey populations coexist over the long term. We do not claim that Huna Tlingit seagull-egg harvests had no impact on local gull populations, just that whatever that impact might have been was sustainable for the foreseeable future.

Alvard and Tucker both take us to task for suggesting that conservation biologists deny the relevance of traditional environmental knowledge for indigenous conservation. Tucker is quite correct to note that, rather than deny the relevance of traditional environmental knowledge, conservation biology "does not talk about traditional environmental knowledge at all." That is precisely our point. We argue not that such knowledge in and of itself is sufficient to guarantee conservation but that it is a necessary foundation for conservationist practice. Furthermore, given the abundant evidence of intense attachment by indigenous community members to a way of life firmly rooted in place, combined with a worldview that imagines the local community to be part of a larger moral universe encompassing the people, plants, and animals of that place, supported by an intimate empirical understanding of the local ecology, it should follow that conservation is not a theoretical anomaly but the normative expectation.

In conclusion, we would like to recognize the concern raised initially by Nadasdy (1999) and here expressed by Berkes, Cruikshank, Moss, and Rose that traditional environmental knowledge not be exploited by the state to deprive indigenous communities of their right to control their own destinies as it has so often been in the past. However, we are uncomfortable with the pessimistic assessment of those we have characterized as "postmodernist" that no true collaboration is possible between indigenous communities and the governments that encompass them. We believe that Cruikshank overstates the case when she asserts that "inevitably, the most powerful parties take their meanings to represent common sense and build those meanings into legal agreements." It is worth noting that the language of Native American treaties is interpreted by federal courts in the United States in accordance with the legal principle that ambiguities in contract language should be resolved from the perspective of the weaker party (Cohen 1986:55). If the most powerful party "inevitably" defined the situation to its benefit, there would be no Indian treaty right to catch 50% of the harvestable salmon in Washington waters. Our research was inspired in part by the belief of certain National Park Service managers that Huna desires to resume limited harvest of certain resources within Glacier Bay National Park and Preserve can be accommodated within the spirit of the agency's preservationist mission. Thus neither party to the present conflict represents a monolithic force. There are individuals

on both sides who are disinclined to compromise but also individuals on both sides willing and able to pursue a resolution of the conflict that can preserve the fundamental concerns of both parties. A real and lasting contribution of this research may be to encourage more cooperative management of Glacier Bay National Park's resources. Berkes makes the important point that the primary benefit of such a co-management agreement and the analysis of traditional environmental knowledge that helps justify it may be in defining conservation less ethnocentrically, recognizing the illusion of a resource management policy dedicated to the goal of preserving a pristine wilderness that never was. Conservation should be defined to include people, their communities, and their livelihoods.

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